The Scandinavian
Journal of Economics

Scand. J. of Economics 116(2), 394–428, 2014
DOI: 10.1111/sjoe.12046

Early Tracking and the Misfortune of Being Young*

Nicole Schneeweis

University of Linz, A-4040 Linz, Austria
nicole.schneeweis@jku.at

Martina Zweimüller

University of Linz, A-4040 Linz, Austria
martina.zweimueller@jku.at

Abstract

Recent research 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. In this study, we estimate the causal relative-age effect on track choice in Austria, a country where students are first tracked in grade 5 (at the age of 10 years), and again in grade 9. We find a strong positive relative-age effect on track choice in grades 5–8. The age effect persists beyond grade 8 for students from less-favorable socioeconomic backgrounds and students in urban areas.

Keywords: Educational tracking; relative-age effect; school choice; socioeconomic gradient

JEL classification: *I*21; *I*24; *I*28

I. Introduction

Although all European countries allocate their students to various educational tracks at some stage of secondary education, in some countries, the decision about which track each student is to attend is made at a relatively early stage of the education process (e.g., at the age of 10 years in Austria and Germany).[1] The early segregation of students into academic and vocational schools is controversial, because there are strong arguments both in favor of and against early tracking. One concern is that early tracking might

*For helpful discussions and comments, we would like to thank Ludger Wößmann, Guido Schwerdt, Rudolf Winter-Ebmer, Martin Halla, two anonymous referees, and (seminar) participants in Stockholm, Munich, Linz, London, Tallinn, and Vienna. The usual disclaimers apply. Financial support from the Austrian Science Fund (NRN S103) and the municipality of Linz is gratefully acknowledged. The administrative data were provided by the municipality of Linz.

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

be inefficient, because some students end up in the wrong track whenever track choice is based on a noisy signal of ability (Brunello *et al.*, 2007).

The body of literature on relative-age effects in educational achievement shows that the amount of such noise relates to the age at which students are graded or take a test.[2] Younger students perform significantly worse, compared to older peers within the same grade (Bedard and Dhuey, 2006; Datar, 2006; Fredriksson and Öckert, 2006; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009). The position of a student within the age distribution of a grade is determined by the school enrollment cut-off date and the birth month of that student. Therefore, the noise can be considered randomly allocated via birth month. Existing evidence suggests that relative-age effects fade with the duration of schooling (Elder and Lubotsky, 2009; Black *et al.*, 2011), but this evidence is based on data from countries where tracking is postponed to higher grades, or where 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 might be preserved or even reinforced on account of 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).

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 (i.e., in grade 5) and again tracked at the age of 14 (i.e., in grade 9). The second tracking might mitigate the relative-age effect, if students who were initially assigned to the low track because of an age-related disadvantage manage to upgrade to the high track in grade 9. We use administrative data from a major Austrian city for the 1984–2006 period and data from the Programme for International Student Assessment (PISA) for the years 2003 and 2006, in order to estimate whether older students are more likely to attend high-track schools than their younger peers. Because the observed age of a student is endogenous as a result of grade retention and the possibility of enrolling 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 of a grade are almost 40 percent less likely to attend high-track schools than their oldest peers. In grade 9, when the second tracking occurs, we find no significant relative-age effect, on average. These results are in line with recent research on the effects of school entry age on track choice in Germany (Mühlenweg

[2] The concept of "relative age" was first introduced by Allen and Barnsley (1993). It refers to age differences among individuals who are grouped by cohort (based on a specific cut-off date).

[3] For instance, US high-school students are divided into ability groups within schools, based on their prior scholastic performance.

and Puhani, 2010). We contribute to this body of literature by studying differences among socioeconomic groups and between rural and urban areas. We find that relative-age effects differ among groups. The assertion 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 more-favorable parental backgrounds, it even increases for children with less-favorable parental backgrounds. Thus, within an education system that features early tracking, relative-age effects reinforce existing socioeconomic inequalities.

In comparing urban and rural areas, we find a significant relative-age effect in urban areas, in both grades 8 and 9; in contrast, relative age has no effect in rural areas, in either of these two grades. Heterogeneity in the estimated effect between rural and urban areas can be explained by differences in the supply of academic schools for students in grades 5–8. Because the supply of academic schools is rather small 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 or at the bottom of a cohort's ability distribution, and most for students at the middle of the ability distribution, a small supply of academic schools for students in grades 5–8 might 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 there actually occurs in grade 9. This postponement might be responsible for the absence of relative-age effects in grade 9 in rural areas.

The structure of this paper is as follows. In Section II, we provide an overview of the related body of literature. We give information on the relevant aspects of the Austrian education system and on the data used herein in Sections III and IV, respectively. In Section V, we outline our identification strategy and discuss potential threats to identification. In Section VI, we present our estimates of the relative-age effect on track choice following the first and second tracking, we highlight the heterogeneity of the effects among socioeconomic groups and between urban and rural areas, and we provide some sensitivity checks. We also provide some descriptive evidence on potential labor-market consequences. Finally, in Section VII, we conclude and discuss the policy implications of our findings.

II. Related Body of Literature

The current study relates to the body of literature on the effects of early tracking on efficiency and equality of opportunity, and to the body of literature on relative-age effects on school performance and track choice.

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

Exploiting the results of a randomized experiment in Kenya, Duflo *et al.* (2011) have provided evidence that tracking primary-school students by prior achievement increases the test scores of students in high-achievement and low-achievement classes, because homogeneous classrooms allow teachers to focus their teaching. However, they admit that these results can be obtained only in developing countries, where students are very heterogeneous and classes are large. In contrast, developed countries are characterized by smaller classes, fewer achievement differences, and a higher level of resources. Galindo-Rueda and Vignoles (2004) have analyzed the gradual abolition of selective grammar schools in the UK. Using the political affiliation of the county as an instrument of comprehensive-school attendance, they have found some evidence that high-ability students perform worse under a comprehensive-schooling system, and that the performance of low- or middle-ability students was not affected by ability tracking.[4] To the best of our knowledge, there is no other direct evidence of efficiency gains as a result of early tracking in developed countries.

However, other studies have shown that early tracking is not efficient and does not render strong results. Meghir and Palme (1995) have found that the introduction of compulsory comprehensive schooling in Sweden induced, on average, an increase in schooling beyond the compulsory level and an increase in earnings for students with unskilled fathers. The mean effect on earnings for all students was positive but not significant. In a

[4] These results have been challenged by Pischke and Manning (2006), who have shown that this identification strategy cannot remove the selection bias between students who attend comprehensive and selective schools.

recent study, Pekkala Kerr *et al.* (2013) have investigated the impact of the Finnish comprehensive-school reform in the 1970s on cognitive skills. They have found small positive effects on mean achievement in verbal reasoning tests, as well as positive effects on the achievement of students with low-educated parents in arithmetic and logical reasoning tests.

The body of literature on peer effects gives indirect evidence of the optimal allocation of students. If peer effects are non-linear – such that low-ability students would benefit from high-ability students, and the latter would be less affected, or not at all, by low-ability peers – heterogeneous classrooms should be more efficient, because they lead to higher aggregate achievement. In contrast, if high-ability students are more sensitive to peers, aggregate achievement is maximized when classrooms are homogeneous. In the literature, there is mixed evidence of peer effects, in particular with respect to non-linearity. While some studies have shown that students from less-favorable social backgrounds and low-achieving students are most affected by their peers (e.g., Schindler-Rangvid, 2003; Schneeweis and Winter-Ebmer, 2007; Gould *et al.*, 2009; Lavy *et al.*, 2011), other authors have found no non-linearity (e.g., Hanushek *et al.*, 2003; Ammermüller and Pischke, 2009). However, Carrell and Hoekstra (2010) have found that peers from troubled families strongly impair the cognitive achievements of children from high-income backgrounds and the disciplinary behavior of children from low-income backgrounds.[5]

Overall, there are two channels by which tracking can enhance efficiency: through non-linear peer effects (i.e., to the benefit of high-ability students), or through teacher effects. Both channels suggest that tracking should occur as early as possible. However, tracking also exacts a cost. Ideally, track choice should be based on a student's innate ability, but in reality, a student's ability is unobserved and track choice is based on an imperfect measure of ability (i.e., prior educational achievements, such as grades or test scores). Psychologists argue that the correlation between childhood 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 ultimate cost of tracking is the potential to misallocate students to the "wrong" track; such a cost is expected to be higher when track choices are made earlier in a student's schooling.[6] Allen and Barnsley (1993) have argued 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.

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

[6] Brunello *et al.* (2007) have described this trade-off in a theoretical model and have denoted the counteracting effects as "specialization" and "noise" effects.

Because school enrollment is based on a certain cut-off date, the birth month of a student determines his or her position in the age distribution of his or her cohort (grade). Recent research has shown that this position relates to a student's achievement. For example, Bedard and Dhuey (2006) have shown for a number of OECD countries that younger students perform significantly worse than their older peers in grades 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) have been able to disentangle these effects and they have found a strong positive age-at-test effect and a small negative effect of starting school one year later. Elder and Lubotsky (2009) have shown that the age effect tends to be smaller in higher grades.

In this paper, we aim to emphasize the following point: if students are separated into different educational tracks very early, differences in age-related achievement probably translate into age-related differences in track choice, irrespective of the exact origin of the age effect. Moreover, if differences in age-related achievement are less important in higher grades, early tracking might contribute to a persistence in those differences, whereas later selection could increase educational attainment and earnings.

Related research has been undertaken by Fertig and Kluve (2005), Jürges and Schneider (2011), and Mühlenweg and Puhani (2010) in Germany. Jürges and Schneider (2011) have used data from the German PISA 2000 extension study and have shown that age at track choice has a sizeable positive effect on the probability of attending a high-track school in grades 5, 7, and 9. Fertig and Kluve (2005) have used survey data and have found no significant effect of enrollment age on track choice for students enrolled in the late 1960s and 1970s. Of these studies, the one that most closely relates to our research is the study of Mühlenweg and Puhani (2010), who have estimated relative-age effects using administrative data from the German state of Hessen. They have found 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 on account of the potential to revise tracks after grade 10. Actually, they show that the younger students are more likely to upgrade to a higher secondary track, but not to the highest one. While the highest-track schools provide unrestricted access to all forms of tertiary education, the higher-track schools provide unrestricted access only to "Universities of Applied Science" and restricted access (by subject of specialization) to universities.

Our contribution to the literature highlights the heterogeneity in the relative-age effect among socioeconomic groups and between urban and

[7] Similar results have been obtained by McEwan and Shapiro (2008) in Chile.

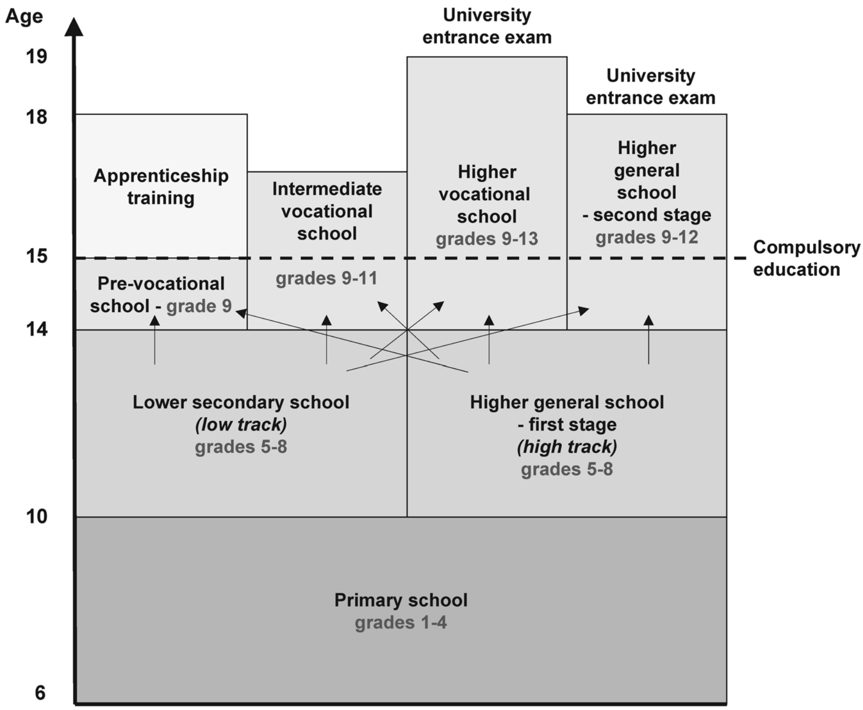

Fig. 1. The Austrian education system

rural areas. We find that only students with high parental socioeconomic status (SES) are able to benefit from the opportunity to revise their initial track choice later. 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 SES are typically not able to catch up. The age effect can even increase, because older students are more likely than their younger peers to change from the low track to the high track.

III. Institutional Background

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

In Austria, primary school starts at the age of six and takes four years. School enrollment is based on a cut-off date; children are first 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. Because children can differ in terms of maturity, these enrollment rules are not strictly enforced; for example, children who turn six between September 1 and December 31 can enroll early if their parents apply for early enrollment, if the health officer of the school confirms that the child is mature enough, and if the primary school principal agrees (early enrollment). However, if it turns out that a six-year-old child is not mature enough, he or she will need to attend the pre-primary class, instead of the first grade of primary school, and enroll in grade 1 a year later (late enrollment). Furthermore, if a student's achievement is insufficient in more than two subjects, he or she will need to repeat a 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 oriented curriculum, employ teachers with higher qualifications, and pay higher wages.

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

In grade 9, students in Austria are again given the opportunity 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 who intend to pursue apprenticeship training. Intermediate vocational schools provide professional training and conclude, after three years, with a final exam. 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).

[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".

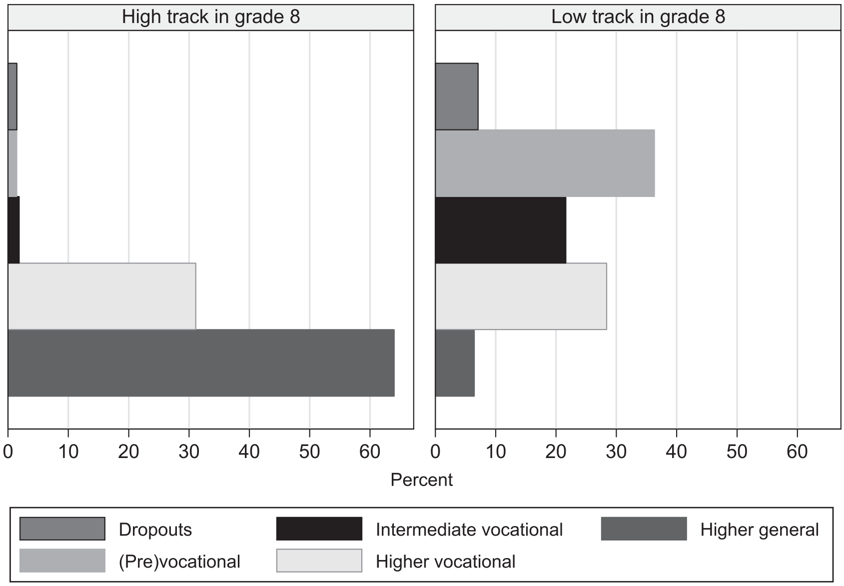

Fig. 2. Post-grade 8 transition of students from high and low-track schools
Data source: Statistics Austria, Education documentation (Schulstatistik) 2008, grade–8 students in 2005/06

Although the first occurrence of tracking is very early in Austria, the education system provides some flexibility by allowing students to revise their track decision in grade 9. For example, it is possible for students from low-track schools to prepare for university entrance qualifications by choosing a higher vocational or higher general school (high track) in grade 9. Depending on their grades, these students might need to pass an exam to be admitted to a high-track school. However, the difference in the quality of education between high-track and low-track schools in grades 5–8 often hampers the transition of low-track students to high-track schools in grade 9.

The majority of Austrian students attend a low-track school in grades 5–8. For example, in the 2005–2006 school year, about 71 percent of Austrian students attended a low-track school in grade 8 (data from Schulstatistik (Education documentation), Statistics Austria, Vienna). Figure 2 shows, separately, the transition after grade 8 of students from high-track and low-track schools. The school choices of high-track and low-track students are very different: while about 96 percent of high-track students choose a track that leads to university entrance qualifications (i.e., typically 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.

IV. Data and Samples

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

Administrative Data

The administrative student-level data cover all resident students who attended grade 5 in public or private (mostly confessional) schools in Linz between 1984–1985 and 2001–2002.[9] The data allow us to observe each student until grade 8.[10] The data provide information on some basic individual-level student characteristics (e.g., year and month of birth, gender, and language) and the students' school careers (school type, school, and grade).

Summary statistics are provided in the Appendix (Panel A of Table A1). Over the entire period, about 45 percent of students attended high-track schools in grade 5. Because some students changed to the low track, the percentage is somewhat lower in grade 8, indicating that the rate of downward mobility exceeds that of upward mobility. About 80 percent of students enrolled according to the cut-off rule, 2 percent enrolled early, and 18 percent enrolled late. On average, students are about 10.67 years old (observed age) when they make their track choice.[11] In line with the school enrollment rule and in the absence of grade repetition, the average age at track choice should be 10.46 years (assigned age). The difference between the observed age and the assigned age of a student is a result of late and early enrollment, on the one hand, and grade repetition, on the other hand. Figure 3 shows that the share of students who enrolled late was highest among students born in August (i.e., children born shortly before the cut-off date of September 1). About 50 percent of all August-born children delay enrollment by at least one year. In contrast, early enrollment is seen only among children born soon after the cut-off date.

PISA Data

We use survey data from the PISA studies 2003 and 2006 to estimate the relative-age effect in grade 9. PISA does not sample entire grades; rather,

[9] Our sample consists of 27 public schools and six private schools.
[10] For some students, we also observe data with respect to grade 9, but only for those who have not repeated a grade and have not attended a pre-primary class. This is because the single purpose of the data collection is to report on nine years of compulsory schooling, which includes repeated grades and pre-primary education.
[11] Actually, students are half a year younger: the track choice is made after the first term of grade 4, because it is then that admission for grade 5 starts.

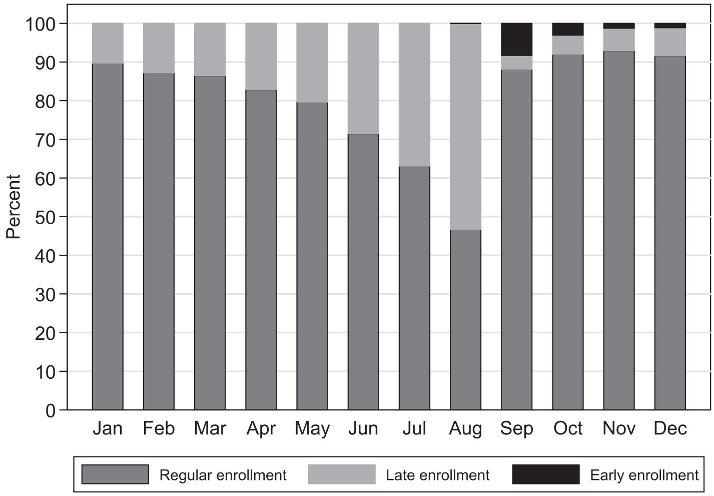

Fig. 3. Regular, late and early enrollment by birth month
Data source: Administrative student-level data for Linz, grade-5 students between 1984-85 and 2001-02

Table 1. *Composition of the PISA sample*

		Grade 8	Grade 9	Grade 10	Total
Born Jan–Aug	Percent	1.1	20.2	78.6	100
	N	63	1,118	4,345	5,526
		(late/repeat)	(late/repeat)	(regular)	
Born Sep–Dec	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

Data Source: PISA waves 2003 and 2006

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

The Austrian PISA study contains a number of questions on the school careers of 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 of education to date, 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' ages at the first and second track choices (i.e., after grades 4 and 8). Our estimation sample, then, consists of all students observed in grades 8, 9, and 10 for whom we can reconstruct these variables.[12]

We do not observe the choice of second track for students who are still in grade 8. This group of students is presumably negatively selected with respect to educational achievement, because they have either repeated a grade or delayed enrollment (see Table 1). Because the majority of these students are born between September and December, to exclude those observations from our analysis would lead to an 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. However, by assigning the low track, we pursue a conservative strategy and might underestimate the true effect.

A further complication arises, given that PISA samples only students in educational programs. 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 a downward bias in our estimates, because these students are more likely to be among the younger students (i.e., 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 A1). About 29 percent of Austrian students attend high-track schools in grade 8. This percentage is lower than that for the sample from the city of Linz, because of differences between urban and rural areas in their respective supplies of high-track schools for grades 5–8. In grade

[12] For 2.9 percent of the sample, we do not have enough information to reconstruct school careers. These observations are excluded from the analysis. A comparison of the share of students in low-track 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 show that our estimation sample is representative of the population of students.

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

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.

V. Identification Strategy

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

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

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

Here, $\mathit{High\ track}^*_{ig}$ 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. $\mathit{Observed\ age}_{ig}$ 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 the 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 relative-age effect on track choice in grade g.

The variation in observed age arises from the following three sources: the distribution of births over the calendar year, the non-compliance of some students with the school enrollment cut-off date rule, and grade retention. As our data suggest, some students enroll early and thus are among the youngest within a grade, whereas students who enroll late or have repeated a grade are among the oldest in a grade. Because 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 provide 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 exogenous variation in observed age at track choice coming from variations in birth month; that is, we use the assigned age at track choice as an instrument for observed age.[14] The assigned age is the age a student would be 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

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

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

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

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 cut-off date rule, net of grade retention, and late or 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 within a heterogeneous treatment effects framework (Angrist *et al.*, 1996), implying that we can identify the causal relative-age effect only for compliers (i.e., we estimate a local average treatment effect). Compliers are students who are among the oldest within a grade, but only because they were born soon after the cut-off date (e.g., in September); they would have been among the youngest in their cohort had they been born before the cut-off 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 be random and not related to other (unobserved) determinants of academic achievement, in particular, student ability and parental socioeconomic background. At least, we must assume that parents do not schedule births so that they fall either before or after the cut-off date. If, for example, high-ability parents are more likely to have 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).

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

Because our administrative data contain no information on parental characteristics, we cannot determine 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 cut-off date rule is not strictly enforced (i.e., where late or early enrollment is possible). As a robustness check, we include the quarter of birth in our regressions and show the results for a restricted sample, including only students born in August and September. In the PISA sample, we control for an index of parental SES and for the highest level of parental education. The distribution of parental education and the index of parental SES across birth months (see Figure A1 in the Appendix) generate no clear pattern of seasonality. Furthermore, Table A2 in the Appendix presents estimates from separate regressions of the instrument on background characteristics, such as student gender, immigrant background, residential neighborhood, and socioeconomic background. Overall, we find no significant evidence of a violation of the random-assignment assumption.

Third, while the instrument might not affect some students (i.e., always-takers and never-takers), all students affected by the instrument must be affected in the same direction; we thus assume monotonicity. The monotonicity assumption implies that no one makes a decision that contradicts his or her assignment (i.e., there are no defiers). If monotonicity is violated, the instrumental variables (IV) estimate is an average of the effect for compliers and the effect for defiers; if those effects differ, the IV regression yields a biased estimate of the causal effect for compliers. The nature of 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 the context of the current study, the monotonicity assumption is not fulfilled, because our treatment variable is multivalued and the instrument shifts the age of all students.[15] This implies that neither always-takers nor never-takers exist, and the instrument does not affect all students in the same way. According to the school enrollment rule, being born in September should make a student, on average, 11 months older at track choice than he or she would be if he or she had been born in August. However, some August-born students enroll one year later than they should, which makes them one month older than they would have been, had they been born in September and had they complied with the rule. While these students would be always-takers if we used a binary treatment, in our setting, they are defiers. A similar argument can be made with respect to

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

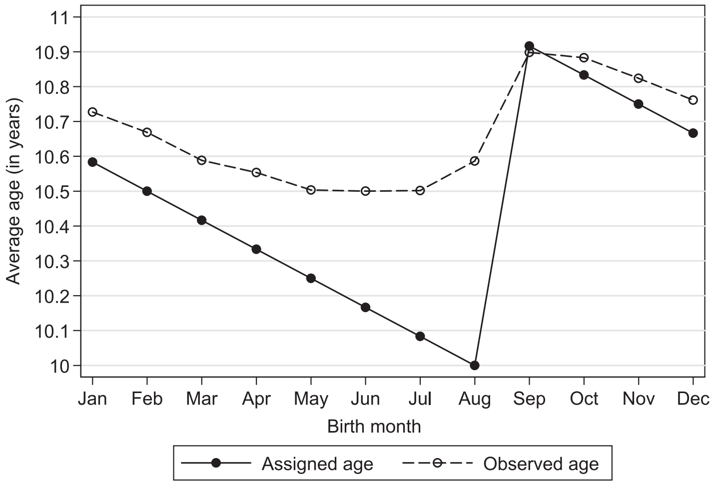

Fig. 4. Assigned age and observed age by birth month
Data source: Administrative student-level data for Linz, grade-5 students between 1984-85 and 2001-02

grade-repeaters and students who enroll early. Violation of the monotonicity assumption implies that the IV regression yields an unbiased estimate of the local average treatment effect only if the relative-age effect is linear and homogeneous 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 the next section, following a discussion of the main results.

VI. Results

Based on our administrative data from Linz, first we present some descriptive evidence of the existence of the first stage, that is, the relationship between assigned and observed age and for a positive relationship between assigned age and the probability of attending 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 a result of non-compliance with the cut-off date rule and grade retention in primary school. As expected, the highest deviation is found among students born slightly before the cut-off date, because

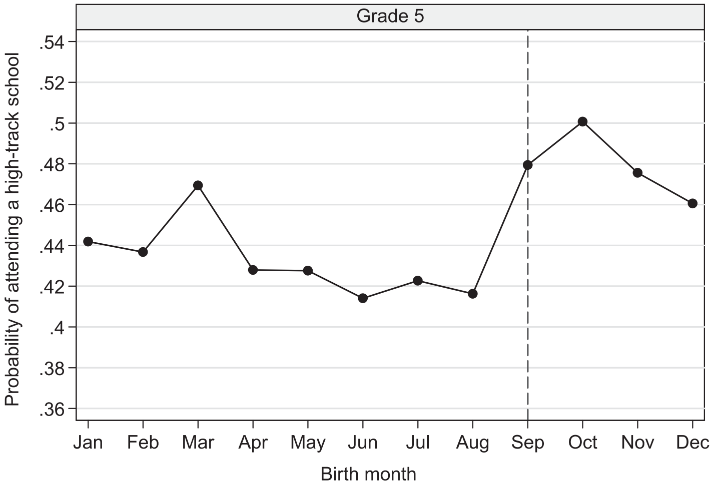

Fig. 5. Attendance of a high-track school by birth month
Data source: Administrative student-level data for Linz, grade-5 students between 1984-85 and 2001-02

these students are more likely to enroll late than students born after the cut-off date. Nevertheless, there is still a clear discontinuity in observed age at track choice, between August-born and September-born students.

The reduced-form relationship between birth month (assigned age) and the probability of attending a high-track school, as presented 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. Although a considerable number of August-born students enroll late – thus increasing their age at the time of track choice, relative to their peers – these students are still significantly less likely to choose a high-track school in grade 5.

Baseline Results: Administrative Data

Table 2 presents our baseline IV probit estimates of the relative-age effect on the probability of attending 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 sufficiently correlates

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

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
Percent 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 and 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 errors are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. Control variables are sex, immigrant background, and year dummies.
[a]Probit IV 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.

with observed age. The IV probit estimate for grade 5 suggests that being 11 months older at track choice increases a student's probability of attending a high-track school by 17.5 percentage points; this 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 changes with each grade. Given that mobility between school tracks is possible, we would expect the effect to diminish at ever-higher grades, if small differences in age become less and less important as school careers progress. Because of the more academic curriculum and the formal admission requirements of 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 needing to fulfill 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

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

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.

The reduced-form estimates show the net impact of the cut-off date rule on the probability of attending a high-track school. Being born in September instead of August leads to a 7.7 (6.5) percentage point difference in the probability of attending a high-track school in grade 5 (grade 8). The reduced-form estimates are lower, because non-compliance partially offsets the disadvantage created by the education system for children born slightly before the cut-off date; for example, delaying enrollment by one year or attending a pre-primary class instead might help those students 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 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.

Baseline Results: PISA Data

Do students assigned to the low track on account of an age-related disadvantage manage to upgrade to a high-track school in grade 9? Because upward mobility is common after grade 8 (see Figure 2), the second tracking might lead to a correct allocation of talents. We would expect the relative-age effect to disappear, if students who ended up in the low track because they were younger at track choice can compensate for that initial disadvantage and be more likely to change to high-track schooling 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 of attending a high-track school in grades 8 and 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 full sample of Austrian students (Columns 1 and 2), we find that the probability of attending a high-track school in grade 8 is 12.4 percentage points higher for students who are 11 months older at track choice in

[18] Our administrative 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 correlates with age and birth month.

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

Table 3. *PISA sample: track choice in grades 8 and 9*

	All students		Low SES		High SES		Urban areas		Rural areas	
	Grade 8 (1)	Grade 9 (2)	Grade 8 (3)	Grade 9 (4)	Grade 8 (5)	Grade 9 (6)	Grade 8 (7)	Grade 9 (8)	Grade 8 (9)	Grade 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
Percent 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
Std 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 errors (in parentheses) account for complex survey design. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. Control variables are gender, 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 SES that is lower (higher) than the sample mean. The index is missing for 3 percent of the sample; these observations are excluded in Columns 3–6.

[a]Probit IV 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.

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 (Mühlenweg and Puhani, 2010; Jürges and Schneider, 2011). However, as Columns 3–10 of Table 3 show, we also find that the relative-age effect differs among socioeconomic groups and between urban and rural areas.

We use the International Socioeconomic Index of Occupational Status (ISEI) score (Ganzeboom *et al.*, 1992) to break down our sample in terms of the SES of the students' parents. The score varies from 16 to 90 and has a mean of 48 (and a median of 46). The high SES sample includes all students who have at least one parent with a score of 49 or higher. The IV estimate of the relative-age effect in grade 8 is remarkably similar among 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 actually reinforces the relative-age effect.

Table 4 shows whether track upgrading or track downgrading could explain 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 among grade-9 students with more-favorable parental backgrounds is mainly the result of a disproportionate upgrading of younger low-track students to high-track schools (13.4 percentage points) and a somewhat higher likelihood that older high-track students will downgrade to the low track (1.7 percentage points). Because we find no 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 that of Mühlenweg and Puhani (2010); however, it holds only for students with high parental SES. The increase in the relative-age effect among students from less-favorable parental backgrounds is because older students are 13.5 percentage points more likely to change from the low track to the high track in grade 9. Again, the change is a result of

[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 4. *Upgrading and downgrading between grades 8 and 9*

	All students	Low SES	High SES	Urban areas	Rural areas
Panel A: Upgrading to:					
a high-track school[a]	−0.024	0.135**	−0.134***	0.004	−0.057
	(0.042)	(0.063)	(0.047)	(0.054)	(0.064)
a 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:					
a low-track school[c]	0.022**	0.021	0.017**	0.046**	−0.002
	(0.009)	(0.016)	(0.008)	(0.020)	(0.014)
a low-track school or	0.073**	0.039	0.089**	0.138***	0.010
a higher vocational school[d]	(0.030)	(0.042)	(0.038)	(0.044)	(0.034)
Observations	8,136	3,964	3,940	4,508	3,628

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

upgrades to higher vocational schools. In addition to having a lower likelihood of attending high-track schools in grades 5–8, younger students with low parental SES also have a lower likelihood of upgrading to the high track in grade 9.

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. 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] For students who attend schools in rural areas, in neither grade 8 nor grade 9 do we find any relative-age effect on the probability of attending a high-track school. In urban areas, older students are 18.4 percentage points more likely to attend high-track schools 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

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

in the last column of Table 2. As Table 4 shows, the decrease in the relative-age effect between grades 8 and 9 in urban areas is because older students are 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 a higher general school to a higher vocational school.

The heterogeneity in the estimated relative-age effect between rural and urban areas can be explained by the considerably smaller supply of high-track schools for grade 5–8 students 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 low-track schools.[22] Given that high-ability students are more likely to attend high-track schools, a small supply of high-track schools implies that most students at the middle and 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 small supply of high-track schools might account for the absence of the relative-age effect in grade 8 in rural areas.

A small 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 heterogeneous, compared to a situation where the supply of high-track schools is large. Rural low-track schools are quite similar to comprehensive schools, thus implying that the first tracking in rural areas actually occurs in grade 9. This postponement might 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 might misclassify some students because we know only the locations of the schools they attended in grade 9, we are confident that classification error cannot explain the magnitude of our estimates – in particular, that of the difference in effects between urban and rural areas. Our results would be biased if older rural students were more likely than younger rural students to change to high-track schools in urban areas or if younger rural students were more likely than older rural students to change to low-track schools in urban areas. If one of these scenarios were true, we would expect differences in the distribution of students across birth months, between the urban and rural samples.

[22] Data are from Schulstatistik (Education documentation), Statistics Austria, Vienna. The authors' own calculations are used for the 2009–2010 school year.

[23] It could be argued that comprehensive schools are more efficient only in rural areas, because students are more homogeneous there. In fact, we find the opposite pattern: the standard deviation of the index of parental SES is higher for students in rural areas, compared to those in urban areas (see Table 3).

Table 5. *Sensitivity checks: random assignment*

	Administrative data[a]				PISA data[b]	
	Restricted sample		+ Quarter of birth		+ SES and 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 errors are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.
[a]Administrative student-level data for the city of Linz. The sample consists of students observed in grade 5 between 1984 and 2001. The restricted sample includes only students born in August or September. The same students are observed in grades 6–8. Control variables are gender, immigrant background, and year dummies.
[b]Data from PISA waves 2003 and 2006. Control variables are sex, immigrant background, pisa wave dummy, the index of parental 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.

However, we find no evidence that the proportion of older students is larger in the urban sample.

Sensitivity Analysis

In this section, we address sensitivity testing with respect to the random-assignment assumption and the monotonicity assumption.

Using our administrative data, we perform two sensitivity tests to show that we are not confounding the causal relative-age effect with season-of-birth effects. Recent research suggests that the season of birth might correlate with family background. Buckles and Hungerman (2013) have found that in the US, children born in the first quarter are more likely to have a less-favorable family background. To address 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 soon before or after the cut-off date (i.e., students born in August or September). The estimated coefficients from the restricted sample are very similar to those from the sample that includes all birth months. Furthermore, the inclusion of quarter-of-birth dummies does not significantly change our results. Because our PISA data provide information on parental characteristics, we re-estimate our baseline model, which has been augmented by control variables for the highest parental SES (ISEI; Ganzeboom *et al.*, 1992) and the parents' highest education level (measured in ISCED categories; UNESCO, 2006). The results do not differ significantly, thus indicating that our instrument does not correlate with parental background.[24]

As discussed in Section V our instrument does not affect all students in the same way (i.e., the monotonicity assumption is violated). We propose the following sensitivity check: we redefine our treatment and assignment variable, such that the monotonicity assumption is fulfilled, and then we analyze whether our results show the same patterns as those seen in our baseline regressions.

Our redefined assignment variable (*old*) is a binary variable that is equal to one if a student is born within the first six months following the cut-off date (i.e., between September and February), and zero otherwise. The first group includes students who would be among the older students within the grade, if they complied with the school enrollment cut-off 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 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. Because 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 differs from our baseline estimates, which 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 of attending a high-track school. In contrast, the coefficients presented below are based on a 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 six months.

Although the magnitude of the estimated effect is not comparable to our baseline results, the conclusions are the same. Older students are 5.3

[24] See also Table A2 in the Appendix for the results of regressions of background characteristics on the instrument.

[25] Note that our baseline estimates are obtained from probit IV models; the results from linear probability models are similar.

Table 6. *Sensitivity checks: monotonicity*

	Administrative data[a] Linz		PISA data[b]									
			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 errors are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

[a]Administrative student-level data for the city of Linz. The sample consists of students observed in grade 5 between 1984 and 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 SES 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 cut-off 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 cut-off date, and zero otherwise. F-statistics on the excluded instrument are given 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 cut-off date, and zero otherwise.

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 and 3.6 percentage points in grades 8 and 9, respectively.

In comparing urban and rural areas and students from more-favorable and less-favorable parental backgrounds, we find patterns similar to those seen in our baseline regressions, presented in Tables 2 and 3.[26]

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 enroll in low-track schools during grades 5–8. Additionally, after the second tracking in grade 9, younger students do not fully catch up. We find substantial age effects among students from lower socioeconomic backgrounds and from urban areas.

The educational disadvantage experienced by relatively younger students most likely perpetuates as they enter the labor market. To the best of our knowledge, no study has investigated 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 both Austria (Fersterer and Winter-Ebmer, 2003) and Germany (Dustmann, 2004; Mühlenweg and Puhani, 2010).

We present in Table 7 additional descriptive evidence from Austria. We run weighted ordinary least-squares regressions for individuals who are 25–55 years old, using consistent cross-sections of data from the Mikrozensus 1991, 1993, 1995, and 1997. The Mikrozensus is a 1-percent representative sample of the Austrian population, and it includes information on education, labor market status, and net earnings. Education is measured in terms of the highest educational qualifications attained (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 separately the different types

[26] The results from regressions that use assigned age as an instrument for being among the older students are somewhat larger, but show a pattern similar to that seen in the estimates presented in Table 6.

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

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)
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 individuals who are 25–55 years old, 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 are included in all regressions, and an indicator for part-time work (<35 hours per week) is 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 are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

[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).

of high-track education (Panel B). Finally, we distinguish individuals living in rural and urban areas (Panel C).

High-track schooling is positively associated with labor market outcomes, as seen in increased employment, a lower risk of unemployment, and higher wages. On average, the earnings of workers with a high-track education are roughly 30 percent higher than those of workers with a low-track education. As can be seen in Panel B, most of these results are found for all types of high-track education, although they are most pronounced for individuals holding a university degree; the wages of these individuals 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 experienced by younger students in urban areas and from lower socioeconomic backgrounds is likely to have major consequences on their lifetime earnings.

VII. Conclusions

We have studied the secondary-school track choices of Austrian students. By international standards, educational tracking occurs very early in Austria: students there must choose at age 10 between an academic track and a vocational track. We argue that in education systems where first tracking occurs early, track choice is strongly influenced by factors other than innate ability; we also provide evidence that relative age at track choice is one such factor.

By 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 body of literature not only by providing evidence from another country that has early tracking, but also by investigating heterogeneous age effects among different groups of students.

The results of our study show that in grades 5–8, younger students are significantly less likely to attend a high-track school than their older peers. Beyond grade 8, students again need to choose between different educational tracks. The second tracking might 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 find there to be no significant relative-age effect in grade 9; however, we find evidence of important heterogeneities in the relative-age effect, with respect to parental background and school location.

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. Within this latter 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 levels attend a high-track school in grade 9. We conclude that in an education system that features early tracking, relative-age effects reinforce existing socioeconomic inequalities.

Furthermore, for students who attend schools in rural areas – in either grade 8 or 9 – we find no relative-age effect on the probability of attending a high-track school. In urban areas, however, younger students are less likely to attend high-track schools than older students in grades 8 and 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 relates to the small supply there of high-track schools for grades 5–8. Because 80 percent of students in rural areas attend local low-track schools until grade 8, these schools are almost like comprehensive schools; in fact, students there are first tracked in grade 9. This postponement might 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 were to occur later, the difference in the relative-age effect between urban and rural areas gives some indirect evidence that this could be the case.

Unlike 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; our conclusions do not materially change.

Appendix

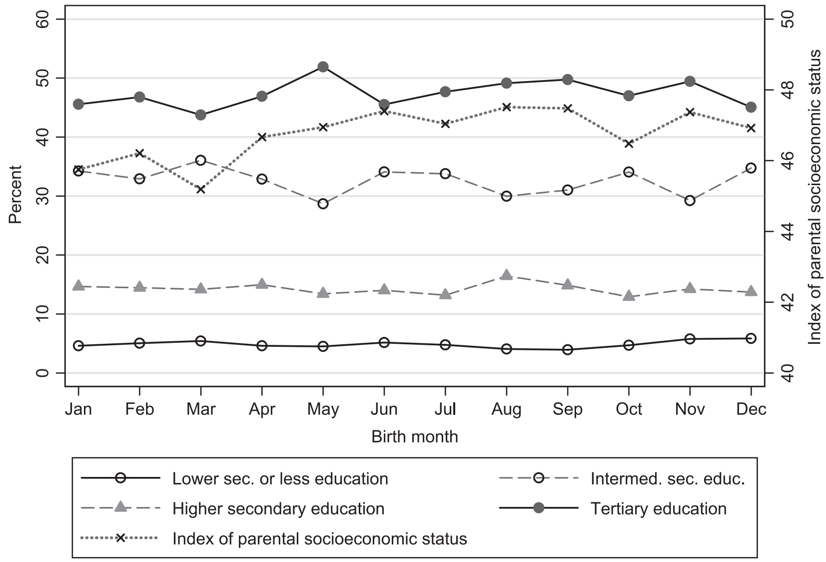

Fig. A1. Distribution of parental SES across birth months
Data source: PISA waves 2003 and 2006

Table A1. *Summary statistics*

	Mean	Std 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 (grades 2–4)	0.04	
Repeated class (grades 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	

Continued

Table A1. *Continued*

	Mean	Std dev.
Enrollment:		
Regular	0.79	
Late	0.17	
Early	0.04	
Repeated class (grades 2–4)	0.02	
Repeated class (grades 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 SES[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 and 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 (Ganzeboom *et al.*, 1992) of either parent or the ISEI score of the only available parent. The score 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 A2. *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

Continued

Table A2. *Continued*

	Coeff.	SE	Mean
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 SES	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 A1 and Section IV for a description of the samples and variables. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. [a]Results are similar when we aggregate the 24 statistical districts to seven quarters.

References

Allen, J. and 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*, 649–659.

Ammermüller, A. and Pischke, J.-S. (2009), Peer Effects in European Primary Schools: Evidence from PIRLS, *Journal of Labour Economics 27*, 315–348.

Angrist, J. D., Imbens, G. W., and Rubin, D. B. (1996), Identification of Causal Effects Using Instrumental Variables, *Journal of the American Statistical Association 91*, 444–472.

Barua, R. and Lang, K. (2009), School Entry, Educational Attainment and Quarter of Birth: A Cautionary Tale of LATE, NBER Working Paper 15236.

Bauer, P. and Riphahn, R. T. (2006), Timing of School Tracking as a Determinant of Intergenerational Transmission of Education, *Economics Letters 91*, 90–97.

Bedard, K. and Dhuey, E. (2006), The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects, *Quarterly Journal of Economics 121*, 1437–1472.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2011), Too Young To Leave The Nest? The Effects of School Starting Age, *Review of Economics and Statistics 92*, 455–467.

Brunello, G. and Checchi, D. (2007), Does School Tracking Affect Equality of Opportunity? New International Evidence, *Economic Policy 22* (52), 781–861.

Brunello, G., Giannini, M., and Ariga, K. (2007), The Optimal Timing of School Tracking, in L. Wößmann and P. Petersen (eds.), *Schools and the Equal Opportunity Problem*, MIT Press, Cambridge, MA, 129–156.

Buckles, K. and Hungerman, D. M. (2013), Season of Birth and Later Outcomes: Old Questions, New Answers, *The Review of Economics and Statistics 95*, 711–724.

Carrell, S. E. and Hoekstra, M. L. (2010), Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids, *American Economic Journal: Applied Economics 2*, 211–228.

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

Datar, A. (2006), Does Delaying Kindergarten Entrance Give Children A Head Start?, *Economics of Education Review 25*, 43–62.

Duflo, E., Dupas, P., and 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*, 209–230.

Elder, T. E. and Lubotsky, D. H. (2009), Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background, and Peers, *Journal of Human Resources 44*, 641–683.

Fersterer, J. and Winter-Ebmer, R. (2003), Are Austrian Returns to Education Falling over Time?, *Labour Economics 10*, 73–89.

Fertig, M. and Kluve, J. (2005), The Effect of Age at School Entry on Educational Attainment in Germany, IZA Discussion Paper 1507.

Fredriksson, P. and Öckert, B. (2006), Is Early Learning Really More Productive? The Effect of School Starting Age on School and Labor Market Performance, Working Paper 2006:12, Institute for Labour Market Policy Evaluation (IFAU, Uppsala).

Galindo-Rueda, F. and Vignoles, A. (2004), The Heterogeneous Effect of Selection in Secondary Schools: Understanding the Changing Role of Ability, IZA Discussion Paper 1245.

Ganzeboom, H. B. G., de Graaf, P. M., and Treiman, D. J. (1992), A Standard International Socioeconomic Index of Occupational Status, *Social Science Research 21*, 1–56.

Gould, E. D., Lavy, V., and Paserman, M. D. (2009), Does Immigration Affect the Long-Term Educational Outcomes of Natives? Quasi-Experimental Evidence, *Economic Journal 119* (540), 1243–1269.

Hanushek, E. A. and Wößmann, L. (2006), Does Educational Tracking Affect Performance and Inequality? Differences-in-Differences Evidence across Countries, *Economic Journal 160* (510), C63–C76.

Hanushek, E. A., Kain, J. F., Markman, J. M., and Rivkin, S. G. (2003), Does Peer Ability Affect Student Achievement?, *Journal of Applied Econometrics 18*, 527–544.

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

Hoxby, C. M. and Weingarth, G. (2006), Taking Race out of the Equation: School Reassignment and the Structure of Peer Effects, Unpublished manuscript, Harvard University.

Jürges, H. and Schneider, K. (2011), Why Young Boys Stumble: Early Tracking, Age and Gender Bias in the German School System, *German Economic Review 12*, 371–394.

Lavy, V., Paserman, M. D., and Schlosser, A. (2011), Inside the Black of Box of Ability Peer Effects: Evidence from Variation in High and Low Achievers in the Classroom, *Economic Journal 122* (559), 208–237.

McEwan, P. J. and Shapiro, J. S. (2008), The Benefits of Delayed Primary School Enrollment. Discontinuity Estimates Using Exact Birth Dates, *Journal of Human Resources 43*, 1–29.

Meghir, C. and Palme, M. (1995), Educational Reform, Ability, and Family Background, *American Economic Review 95* (1), 414–424.

Mühlenweg, A. M. and Puhani, P. A. (2010), The Evolution of the School-Entry Age Effect in a School Tracking System, *Journal of Human Resources 45*, 407–438.

Pekkala Kerr, S., Pekkarinen, T., and Uusitalo, R. (2013), School Tracking and Development of Cognitive Skills, *Journal of Labour Economics 31*, 577–602.

Pekkarinen, T., Uusitalo, R., and Pekkala Kerr, S. (2009), School Tracking and Intergenerational Income Mobility: Evidence from the Finnish Comprehensive School Reform, *Journal of Public Economics 93*, 965–973.

Petersen, A. C. (1983), Pubertal Change and Cognition, in J. Brooks-Gunn and A. Petersen (eds.), *Girls at Puberty: Biological and Psychosocial Perspectives*, Plenum Press, New York, 179–198.

Pischke, J.-S. and Manning, A. (2006), Comprehensive versus Selective Schooling in England and Wales: What Do We Know?, NBER Working Paper 12176.

Schindler-Rangvid, B. (2003), Educational Peer Effects. Quantile Regression Evidence from Denmark with PISA 2000 Data, PhD thesis, Aarhus School of Business.

Schneeweis, N. and Winter-Ebmer, R. (2007), Peer Effects in Austrian Schools, *Empirical Economics 32*, 387–409.

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

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

First version submitted February 2011;
final version received October 2012.