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Abstract

Empirical evidence suggests that relative age, which is determined by date of birth and the school entry cutoff date, has a causal effect on track choice. Using a sample of male labor market entrants drawn from Austrian register data, I analyze whether the initial assignment to different school tracks has persistent effects on educational attainment and earnings in the first years of the career. I estimate the reduced-form effect of the school entry law on starting wages and find a wage penalty of 1.1–2.0 percent for students born in August (the *youngest*) compared to students born in September (the *oldest*). The analysis of educational attainment suggests that significant differences in the type of education exist. Younger students are more likely to pursue an apprenticeship and less likely to have higher education. After five years of labor market experience, the wage penalty amounts to 0.8–1.1 percent, suggesting a persistent (albeit decreasing) negative effect of the school entry rule on labor market outcomes in an early tracking system.

JEL Classification: I21, J24, J31
Keywords: School entry law, early tracking, educational attainment, earnings, labor market entrants.

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I Introduction

The growing public interest in international comparisons of student achievement (e.g. PISA, TIMSS) has fueled the academic debate on the impact of organizational aspects of the education system. One of these organizational aspects is the timing and extent of tracking, i.e. the allocation of students into differing-ability schools or classes. The economic literature on tracking has focused on equality of opportunity and efficiency aspects. While there is clear evidence that early tracking reinforces the role of parental background and limits intergenerational mobility in education and earnings, there is no consensus on whether early tracking is efficient or not.[1] One of the main arguments in favor of early tracking is that teachers are more effective in teaching homogenous classrooms. On the other hand, less gifted students may benefit from the presence of more gifted peers. In that case, delaying the separation into different tracks could be more efficient.

Recently, economists have emphasized that early tracking may be inefficient because track choice is influenced by factors other than innate ability, for instance the age of a student. Brunello, Giannini, and Ariga [2007] argue that the allocation of students to tracks is based on a noisy signal and that the size of the noise is decreasing with the age at first selection. Allen and Barnsley [1993] and Bedard and Dhuey [2006] stress that the long-run cost of the misallocation rise with the difference in the rates of human capital accumulation between tracks.

In Austria and Germany, students are allocated to academic and vocational schools at the age of ten.[2] Recent empirical studies for these countries show that the youngest students within a school cohort (grade) are significantly less likely to attend the academic track compared to their older peers [Jürges and Schneider, 2011; Mühlenweg and Puhani, 2010; Schneeweis and Zweimüller, forthcoming].

[1]See for instance, Bauer and Riphahn [2006], Ammermüller [2005], Pekkarinen, Uusitalo, and Pekkala [2009], Brunello and Checchi [2007], Dustmann [2004], Malamud and Pop-Eleches [2011] and Hanushek and Wößmann [2006] for evidence on equality of opportunity aspects, and Duflo, Dupas, and Kremer [2011], Galindo-Rueda and Vignoles [2004], Pischke and Manning [2006], Meghir and Palme [2005], Guyon, Maurin, and McNally [2010], Hall [2012], Malamud and Pop-Eleches [2010] and Pekkarinen, Uusitalo, and Kerr [2009] for evidence on efficiency aspects.

[2]In some German states students are tracked at the age of 12. Within OECD countries, the age at first tracking varies considerably: In Hungary, the Czech Republic, Slovakia and Turkey students are first tracked at the age of 11, in Belgium and Mexico at the age of 12 and in Chile, Luxembourg and the Netherlands at the age of 13. All other OECD countries track their students at the age of 14 to 18 [Brunello and Checchi, 2007].

This paper focuses on the effects of the school entry law on educational attainment and starting wages in an early tracking system. The school entry law assigns students to different school entry cohorts based on their date of birth and generates an age difference of 11 months between children born in August and children born in September. This age difference leads to differences in the probability of choosing the academic track at age 10 [Schneeweis and Zweimüller, forthcoming]. August-born children are less likely than September-born children to attend the academic track in grades 5–8. Since the academic track offers a more advanced curriculum, a higher peer quality and teachers with higher qualifications (and salaries), the school entry law implies that younger and older students are exposed to schools of different quality for at least four years of their school career. Furthermore, younger students are less likely to choose a higher secondary school at the age of 14.

In this paper, I analyze whether the initial assignment to different school tracks has effects on educational attainment and wages in the first years of the career. The analysis is based on a sample of male labor market entrants drawn from the *Austrian Social Security Database* and followed over time. I estimate the reduced-form effect of the school entry law on starting wages and short-run wage profiles and find a wage penalty of 1.1–2.0 percent for students born in August (the *youngest*) compared to students born in September (the *oldest*). I estimate a reduced-form approach without specifying any channel through which wages could be affected, but—to a certain degree—the data allows to analyze differences in the quantity and quality of education directly. The estimation results suggest that there are significant differences in the type of education but not in the years of education. Younger individuals are significantly more likely to pursue an apprenticeship and are less likely to have at least higher general or vocational education and to hold a university degree.

The analysis of short-run wage profiles reveals a persistent wage penalty for August-born workers that amounts to 0.8–1.1 percent after five years of labor market experience. A more detailed analysis reveals that the estimated effects on (starting) wages differ along the conditional wage distribution with higher wage penalties in the middle and the higher end than in the lower end of the distribution. Overall these findings are consistent with prior evidence for Austria [Schneeweis and Zweimüller, forthcoming].

II Related literature

An increasing body of empirical research documents that the position of a student in the age distribution within a grade or class has a causal effect on school performance [e.g. Bedard and Dhuey, 2006; Datar, 2006; Fredriksson and Öckert, 2006; McEwan and Shapiro, 2008]. Since part of the variation in age is due to parental discretion in the timing of school entry, the causal effect is identified by using only the exogenous variation in age that stems from the distribution of births over the calendar year and the school entry cutoff date. The birth month of a student determines his or her relative age within a grade because school entry is based on a specific cutoff date. If the entry rule is strictly followed, students born just after the cutoff date are almost one year older than students born just before the cutoff date. Using birth month relative to the cutoff date as an instrument for a student's age in a given grade, these studies identify the causal effect of relative age[3] on school performance for compliers.[4] Compliers are students who are among the oldest within a grade only because they are born after the cutoff date and would be among the youngest if they were born before the cutoff date.

One key criticism in this literature is that this identification strategy does not allow to discriminate between the school entry age effect and the test age effect as long as students' performance is measured when they are still in school.[5] A notable exception are Black, Devereux, and Salvanes [2011], who disentangle these effects by using IQ scores of 18-year-old Norwegian students measured outside of school. They provide compelling evidence for a positive test age effect, whereas starting school one year later has a small negative effect on IQ scores. Elder and Lubotsky [2009] find that the relative age effect fades with the duration of schooling, which is interpreted as evidence that the test age effect dominates. They also find higher effects for children with more favorable parental background, sug-

[3] 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).

[4] See Angrist, Imbens, and Rubin [1996] for the interpretation of causal effects in a heterogenous treatment effects framework.

[5] The school entry age effect is the causal effect of starting school at an older age. The test age effect is due to the fact that some children are older when educational achievement is measured. The school entry age effect is perfectly collinear with the test age effect because students who start school later are also older when their educational achievement is measured.

gesting that the relative age effect partly reflects differences in skill accumulation prior to kindergarten enrollment.

Irrespective of their actual origin, relative age effects may be perpetuated by the education system, if, for example, students are separated into different educational tracks very early. In Austria and Germany, lower secondary students are physically segregated into academic (high track) schools or vocational (low track) schools at the age of 10 (in grade 5). Schneeweis and Zweimüller [forthcoming] show that older students are 13–17 percentage points more likely than their younger peers of the same grade to attend a high track school in grade 8. Mühlenweg and Puhani [2010] and Jürges and Schneider [2011] find similar results for Germany, where lower secondary education lasts until grade 9 (or grade 10 in some states). Although students may revise their track choice in any grade, upward mobility is limited because of differences in the curricula between high track and low track schools. After graduating from a low track school, a considerable number of students does revise the initially chosen track by upgrading to a general or vocational high track school. While Mühlenweg and Puhani [2010] show that the relative age effect disappears because students have the possibility to revise their track choice, Schneeweis and Zweimüller [forthcoming] still find an age penalty for the youngest Austrian students.[6]

The evidence on age-related differences in long-term outcomes such as educational attainment or earnings is mixed. Using German survey data on the school entry cohorts 1966–1980, Fertig and Kluve [2005] find no significant relationship between school starting age and educational attainment, neither for West Germany where students were tracked at age 10, nor for East Germany where education was comprehensive until the age of 16. Likewise, Black, Devereux, and Salvanes [2011] find no evidence that starting school later has a positive effect on educational attainment in a sample of Norwegian students born 1962–1980. In contrast, Fredriksson and Öckert [2006] find a positive effect on years of schooling and the probability of attending college for the Swedish population born 1935–1974. Their results suggest that the effect is higher for cohorts that attended a selective school system than for cohorts that were in the comprehensive school system that was introduced gradually between 1950 and 1967.

[6] Note that the elimination of the age effect is only due to students upgrading to vocational high track schools and not to general high track schools [Mühlenweg and Puhani, 2010].

The latter two studies also look at the long-run impact on earnings. Fredriksson and Öckert [2006] show that the age effect on earnings is negative for younger cohorts and positive for older ones. This finding is consistent with a concave experience-earnings profile. Although starting school later may have a positive effect on educational attainment, it definitely reduces labor market experience at a given age. The opposing results for younger and older cohorts may reflect that small differences in labor market experience are more important in earlier stages of the working life. Alternatively, it may be that the age effect is stronger for older cohorts because they attended the selective school system.[7] Black, Devereux, and Salvanes [2011] track different cohorts from age 24 to 35 and estimate separate regressions by age. They find a negative effect on earnings at younger ages and no effect at older ages, supporting the hypothesis that the cohort difference is due to the concave experience-earnings profile. However, their sample does not include individuals who were separated into different educational tracks early in the school career.

Dustmann, Puhani, and Schönberg [2012] analyze the long-term effects of the type of middle school attended in grade 5 to grade 9/10 on labor market outcomes in Germany. They distinguish between a direct effect that is due to differences in middle school quality and an indirect effect via completed education and show that the type of middle school affects neither educational attainment nor labor market outcomes of men aged 30 and above.[8] These results are in line with Mühlenweg and Puhani [2010] who find that relative age effect disappears after the second tracking because students have the possibility to revise their track choice. In contrast, Schneeweis and Zweimüller [forthcoming] provide evidence that the relative age effect does not fully disappear after the second tracking in Austria.

In this paper, I estimate the reduced-form effect of the school entry law on educational attainment and earnings based on a sample of male labor market entrants in Austria, a

[7] Fredriksson and Öckert [2006] are not able to discriminate between these two explanations because cohort effects cannot be distinguished from experience effects in cross-sectional data.

[8] Grenet [2009] provides evidence for France where first tracking occurs at the age of 14. He finds that younger students (born in December) are more likely to be held back in school, are more likely to choose a vocational qualification, are more likely to be unemployed and have somewhat lower wages than their older peers (born in January). For the US, Dobkins and Ferreira [2010] find no effect of birth month on labor market outcomes (employment and wages) and other long run outcomes (home ownership and house prices) although there is evidence that the youngest students obtain more education than their older peers which is due to the interaction of school entry laws with compulsory schooling laws [Angrist and Krueger, 1991].

country where first tracking occurs very early by international comparison. By focusing on the outcomes of labor market entrants the study avoids confounding the estimated effect with differences in labor market experience and shows how the effect evolves as these workers gain labor market experience.

This study is also related to the literature on the effects of postponing or reducing tracking on educational achievement and labor market outcomes. Recent research by Malamud and Pop-Eleches [2010] and Hall [2012] show that the adoption of a more comprehensive schooling system had no discernible impact on university enrollment, wages, family income and the incidence of unemployment (and non-employment). However, in both countries (Romania and Sweden) students attended a comprehensive school system until the age of 14 (or 16) already before the respective reform.

III Institutional background

Figure 1 shows the structure of the Austrian education system which is characterized by early tracking, a multitude of different educational tracks and a strong vocational orientation.

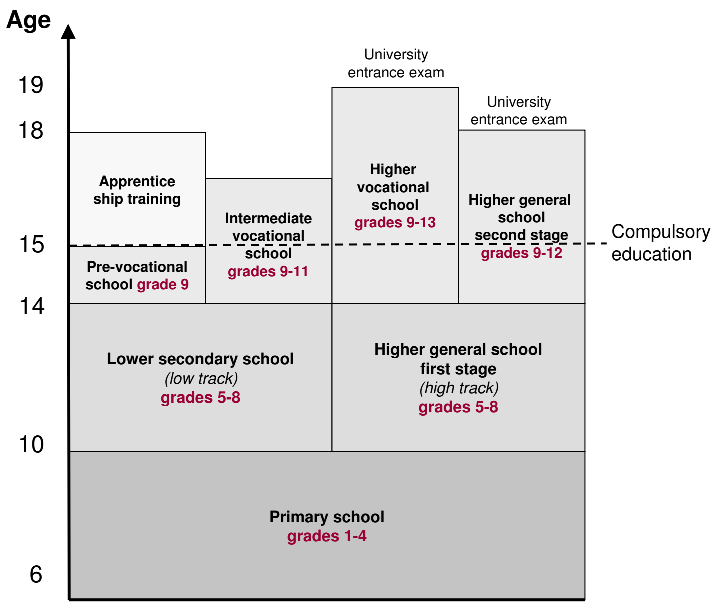

FIGURE 1. — The Austrian Education System

Austrian children enter primary school in a given year if they turn six before September 1 of that year. Children who turn six thereafter have to delay enrollment by one year. Accordingly, being born in August implies being among the youngest students within a cohort whereas being born in September implies being among the oldest students. Since children may differ in maturity, these entry rules are not strictly enforced. For instance, children born between September 1 and December 31 may enroll early under certain circumstances.[9] On the other hand, children who are not mature enough have to attend the pre-primary class instead of the first grade of primary school. Furthermore, if a student's achievement is insufficient in more than two subjects he or she has to repeat the grade. Since late enrollment (and grade retention) is much more common among students born before the cutoff date, the average age difference between September-born and August-born students is about 5 months (instead of 11 months under strict enforcement and without grade retention).[10]

Compulsory education lasts nine years. After four years of primary school, students are physically separated into academically-oriented higher general schools and vocationally-oriented lower secondary schools. The admission to a higher general school depends on primary school grades in the core subjects (Mathematics, German writing and reading) or an entrance exam. Furthermore, primary school teachers give recommendations but these are not binding.[11] Both types of education last four years but differ in many aspects, such as the curricula, and the qualifications and salaries of teachers. Formally, students have the possibility to switch between tracks, however, the more advanced curriculum in the academic track inhibits upward mobility from the vocational to the academic track.

In grade 9, the Austrian education system offers a variety of educational tracks: pre-vocational schools, a range of intermediate and higher vocational schools and the second stage of higher general schools.[12] Most students who attended a higher general school until grade 8 stay there for another four years or change to a higher vocational school. Both

[9] The parents have to apply for early enrollment, the health officer of the school has to confirm that the child is mature enough and the primary school principal has to agree.

[10] See Schneeweis and Zweimüller [forthcoming].

[11] The majority of Austrian students attends a lower secondary school, e.g. in the school year 2006/07 about 67 percent of Austrian students attended a lower secondary school in grade 8 [Statistik Austria, 2008].

[12] There are several types of intermediate and higher vocational schools with different professional orientations, e.g. business, technical, tourism, teacher training, agricultural.

school types provide university entrance qualifications upon completion. Higher vocational schools intend to prepare students for a profession in addition to providing higher general education. The majority of lower secondary school students attends either a pre-vocational school or an intermediate vocational school after grade 8. While intermediate vocational schools last three years and provide professional education, pre-vocational schools are one-year-schools that provide the last year of compulsory education and prepare students for apprenticeship training.

Apprenticeships last between two and four years and consist of professional training at a firm combined with vocational and general education at a part-time vocational school. Apprenticeship training is required for a multitude of occupations ranging from traditional crafts professions to sales, administrative and technical professions.[13] The acquired skill level strongly depends on the type of occupation. While part of these occupations can be considered as low-skilled, other—more forward-looking and medium-skill—occupations offer fairly good labor market and advancement opportunities.

IV Empirical framework

In the first part of this section the estimation model and the identification strategy are described (section IV.1). Then, I introduce the data and the estimation sample, and provide descriptive statistics (section IV.2). In contrast to previous studies the reduced-form effect of the school entry rule (birth month) on earnings and educational attainment is estimated for a sample of male workers who enter the labor market for the first time. The advantage of this strategy is that earnings differences between the youngest (i.e. August-born) and the oldest (i.e. September-born) individuals of a labor market entry cohort do not reflect differences in labor market experience but rather differences in the quantity and quality of education or skills acquired before entering the labor market.

[13] There are about 260 apprenticeship occupations.

IV.1 Econometric model

The estimation model is based on the reduced-form approach suggested in Bedard and Dhuey [2006]:[14]

$$Y_i = \alpha_0 + \alpha_1\, Assigned\ age_i\ + \alpha_2\, X_i + \alpha_3\, t_i + \epsilon_i \tag{1}$$

where Y_i is the outcome of individual i (e. g. the starting wage), X_i is a vector of exogenous variables, t_i is the year in which individual i entered the labor market and ϵ_i is the error term. The variable of interest, *Assigned age*$_i$, is a discrete variable ranging from 0 for individuals born in August to 1 for individuals born in September. The difference in assigned age between August-born and September-born individuals corresponds to 11 months. Assigned age is obtained through a simple transformation of an individual's birth month:

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

where $b_i = \{1, 2, ..., 12\}$ is the birth month of individual i.[15]

Estimation model (1) imposes a linear relationship between assigned age and the starting wage of an individual. As a robustness check, I follow Dobkins and Ferreira [2010] and estimate a regression discontinuity model. A general representation of this approach is given by

$$Y_i = \beta_0 + \beta_1\, Old_i\ + f(dist_i) + \gamma_1\, X_i + \gamma_2\, t_i + \eta_i,$$

[14] Bedard and Dhuey [2006] use assigned age as an instrument for observed age to estimate relative age effects on test scores for a number of OECD countries.

[15] The estimation is based on the individual's birth month and not the exact birth date because this information is not available in the data.

where $dist_i$ is centered at the discontinuity and measures the distance between an individual's birth month b_i and the cutoff date $b_0 = 9$.[16] $f(dist_i)$ is an unknown smooth function of $dist_i$.

$$dist_i = \begin{cases} b_i - b_0 & \text{if} \quad 3 \leq b_i \leq 12 \\ b_i - b_0 + 12 & \text{if} \quad 1 \leq b_i \leq 2 \end{cases}$$

Old_i is a binary variable that is one if the distance is greater than or equal to zero, i. e. if individual i is born between September 1 and February 28, and zero otherwise.

$$Old_i = \begin{cases} 1 & \text{if} \quad dist_i \geq 0 \\ 0 & \text{if} \quad dist_i < 0 \end{cases}$$

By imposing a linear trend that is allowed to have different slopes before and after the cutoff date, the regression discontinuity model can be written as

$$Y_i = \beta_0 + \beta_1\, Old_i \; + \beta_2\, dist_i \; + \beta_3\, dist_i \times Old_i + \gamma_1\, X_i + \gamma_2\, t_i + \eta_i, \tag{2}$$

where $dist_i$ ranges between -6 and 5 and $dist_i \times Old_i$ ranges between 0 and 5.

Both models are parametric models that are estimated with Ordinary Least Squares.[17] However, the interpretation of the coefficients differs between the two models: While in the first model, α_1 measures the effect of an age difference of 11 months, the estimated coefficient of interest in the RD model (β_1) corresponds to the age difference exactly at the cutoff (i. e. 12 months).

The standard errors are clustered at the birth month level. As emphasized by Lee and Card [2008], the discreteness of the assignment variable leads to random specification errors with a group structure. Conventional standard errors ignore this group structure and may therefore overstate the precision of the estimated effect.

[16] This normalization is necessary in order to ensure that the treatment effect at the cutoff date is measured by β_1 in a model with interaction terms [Angrist and Pischke, 2009]. The definition of $dist_i$ for January and February differs from the definition for March to December in order to center the discontinuity. This is equal to redefining the year of birth to run from March to February rather than from January to December, so that the discontinuity is at the middle of the re-defined year [c. f. Black, Devereux, and Salvanes, 2011].

[17] As noted by Lee and Card [2008], non-parametric models cannot be estimated when the assignment variable is discrete.

The main outcomes in this analysis are the starting wage and the wage in each of the five years after the labor market entry. The model estimates the reduced-form effect of assigned age/old according to the school entry law on the (starting) wage of an individual without specifying any channel through which the (starting) wage could be affected. Since the school entry rule states that August-born kids have to enroll one year earlier than September-born kids, they are less mature at school entry and less mature when they have to decide which track to attend at age 10 (first tracking) and at age 14 (second tracking). As discussed in section II, this difference in maturity potentially entails differences in school quality in grades 5–8 (direct effect) and differences in educational attainment (indirect effect). I assess the importance of the indirect effect by analyzing different measures of educational attainment.

The administrative data does not contain information on the actual age at school entry or track choice. Hence, there is no possibility to estimate a two-stage least squares regression with assigned age/old as an instrument for the actual age at school entry (or track choice).[18] Fortunately, the reduced-form effect is also informative and policy-relevant as it shows whether the combination of early tracking and the school entry rule leads to inferior outcomes for relatively younger students. Since the mapping from assigned age/old to the actual age (the first stage) is not one (due to late enrollment, early enrollment and grade retention), the estimated effect is the intention-to-treat effect and may encompass compensating effects. For instance, the age effect may be partly offset for students who repeat a grade or who do not comply with the school entry law and enroll one year later or earlier than they should according to the law.[19] Whether grade retention has positive or negative effects on student performance is still under debate.[20] Most recently, Manacorda [2012] shows that grade retention in junior high school increases school dropout and Jacob and Lefgren [2009] find negative effects on high school completion only for students who were retained in higher grade levels.

[18] Based on the estimates obtained in Schneeweis and Zweimüller [forthcoming] I could estimate a two-sample two-stage least squares regressions. However, if both effects—the direct and the indirect one—exist, two instrumental variables would be needed to separately identify both effects.

[19] As expected, late enrollment and grade repetition is much more frequently observed among the youngest students and early enrollment is almost exclusively observed among students born between September and December. See Schneeweis and Zweimüller [forthcoming].

[20] See the evidence summarized in Jacob and Lefgren [2009] and Manacorda [2012].

Another channel through which the (starting) wage could be affected are non-cognitive skills. Dhuey and Lipscomb [2008] and Thompson, Barnsley, and Battle [2004] provide evidence that older students have more leadership experience and self-esteem, which could have a positive effect on labor market outcomes. In contrast, Pellizzari and Billari [2012] argue that these psychological effects could decrease the amount of time that relatively younger students devote to social activities because they have a lower return to these activities. Using Italian data, they find that relatively younger students have less active social lives, devote more time to studying and perform better at university.

The estimated effect does not reflect ability differences if the birth month of an individual is randomly assigned and therefore, not related to unobserved characteristics that are correlated with wages (e.g. ability, parental background). Buckles and Hungerman [2008] show that children born in spring outperform children born in winter and have a better family background, suggesting that quarter of birth is not a valid instrument. In the Austrian case children born just below and just above the cutoff date are born within the same season, hence, these objections are less evident. I cannot formally test this identification assumption because the administrative data does not include any measure of parental background or student's ability. Instead, I provide some graphical evidence based on additional data from PISA (Programme for International Student Assessment, waves 2003 and 2006) and the Austrian birth register. Figures A.1 and A.2 in the appendix do not indicate a clear pattern of seasonality or birth timing.[21] As a further sensitivity check I include quarter of birth indicators in the regressions. Estimations of assigned age/old on background characteristics (e. g. foreign background) also show no evidence of non-random sorting.[22]

IV.2 Data

The empirical analysis is based on administrative employment records from the *Austrian Social Security Database* (ASSD). The ASSD is a matched firm-worker database which combines detailed longitudinal information on employment (including basic employer in-

[21] In a related paper, Schneeweis and Zweimüller [forthcoming] show that controlling for an index of parental socioeconomic status and parental education has no impact on the reduced form effect of assigned age on track choice in grades 8 and 9.

[22] These results are available upon request.

formation) and earnings since 1972. Since these data are administrative records to verify pension claims it contains complete and precise information about employment histories and earnings. The longitudinal dimension of the data allows to identify all labor market entrants (including self-employed workers, civil servants and farmers) and to track the labor market histories of these individuals over time. The limitations of the data are the lack of information on working hours and top-coded wages.[23] However, in the analysis of labor market entrants the top-coding of wages is a minor problem because only 0.05 percent of the workers in the final sample have top-coded wages in the entry year. The figures for the subsequent years range between 0.3 and 3.2 percent.[24]

The data on wages is provided by employer and year. Wages for self-employed workers and farmers are only continuously available since 1997 and wages of civil servants are not recorded at all in the ASSD. Wages of civil servants are obtained from the *Austrian Tax Register* which covers gross wages for all dependent employees in the private and the public sector and is available from 1994 to 2005.[25] Due to these data restrictions, the estimation sample consists only of individuals who entered the labor market between 1997 and 2000, and their wages are observed up until five years after their labor market entry. The starting wage refers to the first regular employment spell, whereas subsequent wages are measured as average earnings over all employment spells in a given year.

Besides employment and wage information, the database also includes some information on demographic characteristics, such as birth year and month, sex, citizenship and educational attainment. There is no comprehensive measure of educational attainment, but the database includes information on whether an individual has pursued an apprenticeship training or holds a degree (BA, MA/MSc or PhD). For a subsample (about 65 percent of the total sample), more detailed information on educational attainment is available from the *Austrian unemployment register* and can be linked to the ASSD.

[23]See Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf, and Büchi [2009] for a description of the database.

[24]Wages are top-coded because there is a maximum contribution basis according to which the social security contributions are calculated. In 2000, the maximum contribution basis amounted to €43,953 [Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf, and Büchi, 2009].

[25]Wages are top-coded only in the ASSD but not in the tax register. To make the earnings data comparable I apply the same (yearly) thresholds to the gross wages of civil servants from the tax register data.

IV.2.1 Definition of job starters

The labor market entry spell is defined as the first regular employment spell of an individual that is not a marginal employment spell[26] and lasts either for at least 60 days in a row[27] or for 180 days with the same employer but periods of non-employment in between. Apprenticeship spells are not considered as entry spells, but rather as part of the education of an individual. The entry spell of an apprentice is the first employment spell—according to the criteria above—that is not an apprenticeship spell.[28]

The main focus of the analysis is on male labor market entrants because the analysis of females involves several problems. First, due to the lack of information on working hours, the labor supply is only observed at the extensive margin and not at the intensive margin. Therefore, I can not distinguish between full-time work and part-time work, which is particularly a problem when analyzing female wages because females have a high probability of working part-time due to family responsibilities.[29] This may even complicate the analysis of starting wages because females may have children before they first enter the labor market. Second, females are more likely to temporarily leave the labor market when they have children which complicates the analysis of their wage profiles. Neither of these factors would represent a serious threat to the identification strategy unless the probability to have a child in a given year is correlated with month of birth. Since September-born females are on average 6 months older than August-born females at the time of the labor market entry they might have a higher likelihood of already having children at that point in time. Using additional data on the incidence of child birth I do find evidence for a higher likelihood of giving birth to a child before first entering the labor market for September-

[26]Marginal employment is employment with a yearly gross income below the minimum level for social security contributions. In 2000, the minimum contribution basis amounted to €4,046.

[27]An employment spell during the summer (June-September) that lasts 60 days or longer is not considered as first regular employment spell if this is the only employment spell in that year because this spell is most likely only a summer job of an individual that is still in education.

[28]The definition of the entry spell is somewhat arbitrary. I also use other thresholds to test the sensitivity of the results (30 days/150 days and 90 days/180 days). Results do not change and are available upon request.

[29]In 2004, about one third of all employed females aged 15–34 worked part-time, i.e. 35 hours or less per week [Statistik Austria, 2004].

born females. If these women are more likely to work part-time, the wage estimates include the labor supply response at the intensive margin and underestimate the true effect.[30]

I only consider individuals who are Austrian citizens because migrants may have already started or even finished their education in their home country and there is no information on the year of migration in the database. Moreover, the estimation sample is restricted to individuals entering the labor market before they turn 31. A labor market entry above the age of 30 may indicate that the individual has already worked abroad, is still in education or is unable to find stable employment. Since I cannot distinguish between those explanations I drop these individuals from the estimation sample.[31] Figure 2 shows the distribution of the entry age for male labor market entrants. More than two thirds of all individuals enter the labor market between the age of 18 to 21, i.e. when they have finished secondary education or an apprenticeship training (and the compulsory military service).

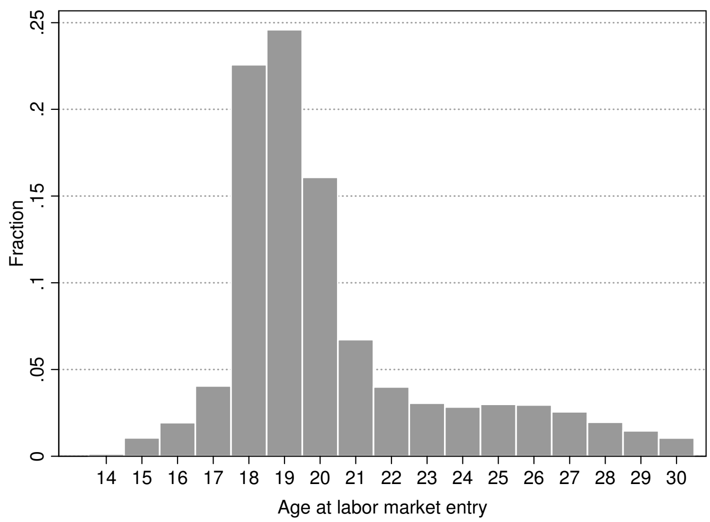

Figure 2. — Distribution of Age at Labor Market Entry

[30]Nevertheless, I have carried out the whole analysis for the female sample. Due to considerations of space, these are not presented in the paper but are available upon request. In section V, I briefly report on the results after presenting the results for the male sample.

[31]About five percent of male labor market entrants is older than 30 years and one percent is older than 40 years. Results based on an extended sample including all workers up to age 40 are similar to those presented below. These results are available upon request.

IV.2.2 Definition of variables and descriptive statistics

The estimation sample consists of 169,987 male labor market entrants. Summary statistics are provided in Table I. The average labor market entrant is almost 21 years old and holds his first job for less than two years. On average, male workers earn a starting wages of 56.45 Euros per day. The starting wage refers to the first regular employment spell and is measured as the daily wage in prices of 2008, i.e. the real wage per day actually worked. Wages are measured in the first year after labor market entry for apprentices who stay at the same firm after finishing their apprenticeship training. For this group, measuring the starting wage in the year of labor market entry would lead to an underestimation of starting wages because wages are measured per employer and year and the remuneration during the apprenticeship is lower than the starting wage.[32]

Table I. — Summary Statistics

Variable	Mean	SD
Real daily starting wage	56.45	21.86
Real daily starting wage (log)	3.94	0.52
Age at labor market entry	20.90	3.16
Duration of entry job (years)	1.71	2.58
Academic degree[a]	0.09	
Skilled worker	0.44	
Unskilled worker	0.10	
Apprenticeship training (at least 2.5 years)	0.46	
Any apprenticeship training	0.51	
Length of apprenticeship period (years)[b]	3.11	0.73
Higher general/vocational school or more[c]	0.26	
Assigned age	0.49	0.31
Old	0.49	

Notes: N=169,987. [a] The degree refers to the year 2008. [b] All individuals who had any apprenticeship training. [c] This information is only available for 109,963 individuals (65 percent of the sample).

Table I also provides summary statistics for different measures of educational attainment. 51 percent of male workers have some apprenticeship training and 9 percent hold an academic degree (MA/MSc or PhD). Based on the age at labor market entry and the information about apprenticeship training in the administrative data, a rough measure of a worker's skill level can be computed. The medium-skilled group includes all workers

[32]In the estimations below, I control for a dummy variable that indicates whether the starting wage is measured one year after labor market entry. However, the results are not sensitive to the exclusion of this variable.

who have an apprenticeship training that lasted at least 2.5 years. This restriction ensures that these workers most likely have completed their apprenticeship training.[33] The skilled group consists of workers who entered the labor market at age 19 or above and have no apprenticeship training. Workers are considered unskilled if they entered the labor market before the age of 19 and have not completed an apprenticeship. According to this definition, 44 percent of male labor market entrants are skilled workers, 46 percent have completed an apprenticeship and 10 percent are unskilled workers. The last measure of educational attainment is derived from detailed categories of completed education (ranging from compulsory schooling to the type of tertiary education) and shows whether the labor market entrant has at least finished a higher general/vocational school. However, this information is only available for a subsample of 109,963 individuals (65 percent) who have registered at the public employment office after finishing education.[34]

Table II shows the distribution of the type of the first job spell and the average starting wage for each type. The majority of male individuals starts in a blue-collar job (56.5 percent) or a white-collar job (38.5 percent). About 1.5 percent directly enter a public sector job and 2.4 percent and 1.1 percent start as self-employed or farmer, respectively. Individuals in public sector, white-collar and blue-collar jobs earn higher starting wages than self-employed workers or farmers.

Table II. — Real Daily Starting Wage by Type of Entry Job

	Obs.	%	Mean	SD
White-collar	65,475	38.5	57.6	24.9
Blue-collar	96,094	56.5	56.5	17.5
Civil servant	2,580	1.5	69.7	20.3
Self-employed	4,053	2.4	39.1	38.9
Farmer	1,785	1.1	29.2	21.0
Total	169,987	100.0	56.5	21.9

[33] Results are qualitatively similar if only apprentices with at least 2 or 3 years of apprenticeship training are considered as medium-skilled.

[34] Since workers have to register at the public employment office to be observed in the subsample, this sample may be negatively selected. However, selection may not be such a problem because all individuals who do not start working immediately after finishing education have to register at the public employment office in order to be covered by the health insurance of their parents. Otherwise they have to pay on their own for health insurance.

V Results

The first part of the analysis focuses on the reduced-form effect of the school entry law (assigned age/old) on starting wages (section V.1) and educational attainment (section V.2). In the second part, I show how the effect evolves as workers gain labor market experience and provide further results on the labor supply response, job mobility and the employment history (section V.3).

V.1 Starting wages

Table III presents regression results for estimation models 1 and 2. The dependent variable is the real daily log wage in the first regular employment spell as defined in section IV.2.1. The base specification in the first column suggests that being born in August instead of September results in a wage penalty of 1.6 percent (model 1). Controlling for season of birth effects (column 2) does not significantly change the estimated coefficient and the estimates from the regression discontinuity model are also very similar.

Table III. — The Effect of the School Entry Law on Starting Wages

Model 1:			
Assigned age	0.016***	0.014***	0.018***
	(0.003)	(0.003)	(0.003)
Adjusted R^2	0.023	0.023	0.159
Model 2:			
Old	0.019***	0.011***	0.020***
	(0.004)	(0.002)	(0.003)
dist	-0.001	0.002**	-0.002*
	(0.001)	(0.001)	(0.001)
dist × Old	0.001	-0.008***	-0.001
	(0.001)	(0.002)	(0.001)
Adjusted R^2	0.023	0.023	0.159
Year of labor market entry	Yes	Yes	Yes
Quarter of birth	No	Yes	No
Type of entry job	No	No	Yes
Observations	169,987	169,987	169,987

Notes: The dependent variable is the real daily log wage in the first regular employment spell as defined in section IV.2.1. All regressions include a dummy variable that indicates whether wages are measured one year after labor market entry and further control variables as indicated in the bottom of the table. See section IV.1 for a definition of models 1 and 2. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months).

Figure A.3 in the appendix gives a graphical representation of the results of estimation model 2. The figure shows that there is a discontinuity in starting wages at the cutoff date. The estimates from the regression discontinuity model are robust to including quadratic terms in *dist* and $dist \times Old$, and to variations in the window width around the cutoff date (i. e. estimation samples that include only +/–4, +/–3, +/–2 or +/–1 months around the cutoff). Moreover, regressions based on two sub-samples with "placebo" cutoffs at June 1 and December 1 show no significant differences in starting wages between August-born and September-born individuals.[35]

Table A.I in the appendix presents additional results from quantile regressions. Estimates are reported for the 10th, the 25th, the 50th, the 75th and the 90th percentiles of the conditional starting wage distribution. The estimates suggest that there is no significant wage penalty for August-born workers in the lowest decile. The effect is highest in the 25th percentile (about 3 percent) and declines monotonically to 2.1 percent in the 90th percentile.

Table IV shows whether the type of entry job is related to the assigned age of a labor market entrant. The estimated marginal effects are obtained from a multinomial logit regression with the type of entry job as dependent variable. September-born males are significantly less likely than August-born males to work in a blue-collar job and significantly more likely to work in a white collar-job. This difference in the type of entry job may be responsible for the observed wage penalty for August-born males. Blue-collar workers typically perform manual labor and are directly involved in the production process. In contrast, white-collar workers are non-manual workers in supervising, administrative, sales or service jobs. On average, white-collar jobs require more qualifications and pay higher wages.

Column 3 in Table III shows the estimated effect of the school entry rule conditional on the type of entry job. Since the conditional effect is very similar to the unconditional effect, the difference in the type of entry job does not seem to be responsible for the observed wage difference between August-born and September-born workers.[36]

[35] These results are available upon request. Thanks to an anonymous referee for suggesting these robustness checks.

[36] However, this coefficient should be interpreted with caution because the type of entry job can be considered as a "bad" control [Angrist and Pischke, 2009].

Table IV. The Effect of the School Entry Law on the Type of Entry Job

	Model 1: Assigned age	
White-collar	0.009**	0.013***
	(0.004)	(0.003)
Blue-collar	-0.013***	-0.017***
	(0.004)	(0.003)
Civil servant	0.002	0.001***
	(0.001)	(0.000)
Self-employed	0.001	0.001**
	(0.001)	(0.001)
Farmer	0.001	0.002***
	(0.001)	(0.001)
Year of labor market entry	Yes	Yes
Quarter of birth	No	Yes
Observations	169,987	169,987

NOTES: This table shows the estimated marginal effects of *Assigned age* on the type of entry job from multinomial logit models. See section IV.1 for a definition of model 1. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

V.2 Educational attainment

Are these age-related differences in starting wages accompanied by differences in the quantity or quality of education? Table V shows results for different measures of educational attainment (as explained in section IV.2.2). Each row presents the estimated coefficients from four different regressions with either *Assigned age* (model 1) or *Old* (model 2) as the variable of interest. The first row shows how the age at labor market entry—a proxy for years of education—is affected by the school entry rule. Differences in the entry age reflect differences in the school entry age, the grade retention rate and educational attainment. September-born labor market entrants are only about 0.6–0.7 years older than August-born entrants. Due to a higher probability of August-born students to defer enrollment or to repeat a grade, the observed age difference is less than a year on average [Schneeweis and Zweimüller, forthcoming]. Therefore, an age difference of 0.7 years does not suggest that September-born individuals attain significantly more years of education. The reason may be that very different types of education have the same duration in Austria. For example, students who attend a higher general or vocational school and students who pursue an apprenticeship both finish their education at the age of 18–19.[37]

Table V. The Effect of the School Entry Law on Educational Attainment

	Model 1: Assigned age		*Model 2: Old*	
Age at labor market entry	0.611***	0.592***	0.685***	0.671***
	(0.046)	(0.011)	(0.012)	(0.014)
Any apprenticeship training	-0.027***	-0.031***	-0.037***	-0.040***
	(0.007)	(0.003)	(0.003)	(0.002)
Skilled worker[a]	0.049***	0.051***	0.063***	0.066***
	(0.008)	(0.003)	(0.002)	(0.002)
Apprenticeship training (≥2.5 years)[b]	-0.031***	-0.033***	-0.040***	-0.042***
	(0.006)	(0.004)	(0.004)	(0.003)
Unskilled worker[c]	-0.018***	-0.018***	-0.023***	-0.024***
	(0.003)	(0.002)	(0.003)	(0.004)
Academic degree	0.003	0.007***	0.006**	0.008***
	(0.003)	(0.001)	(0.002)	(0.001)
Higher general/voc. school or more[d]	0.023***	0.026***	0.031***	0.036***
	(0.007)	(0.004)	(0.004)	(0.003)
Education missing	0.003	0.008***	0.009***	0.012**
	(0.003)	(0.002)	(0.002)	(0.004)
Dist & dist × Old	No	No	Yes	Yes
Year of labor market entry	Yes	Yes	Yes	Yes
Quarter of birth	No	Yes	No	Yes
Observations	169,987	169,987	169,987	169,987

Notes: Each cell represents a separate regression of the dependent variable (see first column) on *Assigned age* (model 1) or *Old* (model 2) and control variables as indicated in the bottom of the table. See section IV.1 for a definition of models 1 and 2. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.
[a] Reference group: unskilled workers and workers with apprenticeship training (at least 2.5 years).
[b] Reference group: skilled and unskilled workers.
[c] Reference group: skilled workers and workers with apprenticeship training (at least 2.5 years).
[d] Reference group: less than higher general/vocational school. N=109, 963.

The remainder of Table V shows regression estimates of the impact of the school entry law on the type of education. Each cell reflects results from a separate regression of a binary variable indicating that the worker has obtained a certain education level on either *Assigned age* or *Old*. Being born in September instead of August decreases the probability of having some apprenticeship training by 2.7–4.0 percentage points (depending on the specification). The effect is even higher when only completed apprenticeships, i.e. apprenticeship that lasted at least 2.5 years are considered. Moreover, September-

[37] Alternatively, it could also be that the age at labor market entry is simply a biased measure of years of education, for instance if education is correlated with job search duration.

born individuals are more likely to be skilled workers (4.9–6.6 percentage points) and less likely to be unskilled workers (1.8–2.4 percentage points), and—in three out of four specifications—are significantly more likely to hold a degree (0.6–0.8 percentage points). Results from a multinomial logit model (presented in Table A.II in the appendix) confirm the monotonic relationship between assigned age and the worker's skill level.

The analysis of the subsample for which a more detailed measure of educational attainment is available complements the results described above. September-born individuals are 2.3–3.6 percentage points more likely than August-born individuals to have completed at least a higher general or a higher vocational school. The estimated effects on educational attainment are in line with Schneeweis and Zweimüller [forthcoming], who show that August-born individuals are considerably more likely to attend a pre-vocational or an intermediate vocational school than September-born individuals.

The last row of Table V shows that the probability that detailed information on educational attainment is missing is related to an individual's month of birth. Detailed information on educational attainment is only available for individuals who have registered at the public employment office after finishing education. September-born workers are about 1 percentage point less likely to register than August-born workers (given a mean of 65 percent), indicating that the selection into the sub-sample is not completely random. Table A.III in the appendix shows that the effect on the starting wage in the subsample is comparable to the estimated effect for the total sample presented in Table III. Nevertheless, these results should be interpreted with some caution.

V.3 Short-run wage profiles

How does the effect of the school entry law evolve as workers gain labor market experience? Table VI presents results for up to five years after labor market entry. Again, both models described in section IV.1 are estimated. After the first year of labor market experience the wage penalty for August-born male workers has decreased to 1.0–1.5 percent, after the second year to 0.7–1.6 percent. In the third and the fifth year, wages for August-born are only significantly lower in the RD specification (0.8–1.1 percent).

Quantile regressions presented in Table A.IV in the appendix show that the effect is not constant along the wage distribution: Again, estimates for the 10th, the 25th, the

50th, the 75th and the 90th percentiles of the conditional starting wage distribution are reported. Consistent with the results for starting wages (see Table A.I) the estimated effect is mostly not significant in the lowest decile. In the 25th percentile, the effect is steadily declining until—five years after job entry—August-born individuals face no significant wage penalty compared to September-born individuals. Similarly, in the highest decile, there is a tendency that the effect is declining and becoming less significant with increasing labor market experience. In contrast, in the 50th and the 75th percentile, the effect remains at 1.0–2.2 percent even after five years of experience.

Table VI. The Effect of the School Entry Law on Subsequent Wages

	Model 1: Assigned age		*Model 2: Old*		*Mean (SD)*	*Obs.*
1^{st} year after job entry	0.010**	0.013**	0.012**	0.015***	4.10	161,917
	(0.004)	(0.004)	(0.004)	(0.004)	(0.47)	
2^{nd} year after job entry	0.007	0.010*	0.013***	0.016***	4.19	157,239
	(0.004)	(0.005)	(0.002)	(0.002)	(0.47)	
3^{rd} year after job entry	0.004	0.006	0.008***	0.011***	4.23	155,858
	(0.003)	(0.004)	(0.002)	(0.002)	(0.50)	
4^{th} year after job entry	0.000	0.004	0.001	0.004	4.27	154,800
	(0.003)	(0.003)	(0.004)	(0.004)	(0.52)	
5^{th} year after job entry	0.005	0.008	0.008*	0.011***	4.30	153,777
	(0.005)	(0.005)	(0.004)	(0.003)	(0.54)	
Dist & dist × Old	No	No	Yes	Yes	-	-
Year of labor market entry	Yes	Yes	Yes	Yes	-	-
Type of job	No	Yes	No	Yes	-	-

NOTES: Each cell represents a separate estimation of the wage after x years of potential labor market experience (see first column) on *Assigned age* (model 1) or *Old* (model 2) and control variables as indicated in the bottom of the table. See section IV.1 for a definition of models 1 and 2. Each estimation includes only employed individuals. Therefore, the number of observations varies between the years. Wages are measured as average real daily wages over all employment spells in a given year conditional on being employed. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

Since the analysis of wage effects is based on the subsample of employed individuals, Table A.V in the appendix shows whether individuals differ in their labor supply response at the extensive margin. The results suggest that the probability of having any employment spell in a given year is not significantly related to month of birth. However, September-born males have about 6–8 more employment days than August-born males in the first and second year after labor market entry. This difference declines to 1–2 days after 3–5 years. These results suggest that September-born males are somewhat more attached to the labor force in the first years after labor market entry, which could imply that they had a better job match in the first place and therefore, are less likely to change their employer in the short run. Job mobility is considered as an important element of early career development and wage growth [Topel and Ward, 1992]. Table A.VI in the appendix shows that September-born workers are 1.7–1.8 percentage points more likely to change their employer in the first year after labor market entry. Therefore, a better job match in the first place may not explain why September-born workers earn higher wages in the beginning of their professional career.

Table A.VII in the appendix looks at the employment history of the labor market entrants. The employment history includes all employment and unemployment spells prior to the first regular employment spell. According to the definition of labor market entrants in section IV.2.1, the employment history should predominantly include short-term or temporary employment spells, summer jobs and student jobs. The first row presents results for the incidence and duration of any kind of employment, including white-collar & blue-collar employment, temporary employment, apprenticeship training, self-employment and employment as farmer. The results suggest that there is no difference in the probability of employment between August-born and September-born workers but August-born workers have spent more days in employment before their first regular employment spell. The analysis of (the duration) of apprenticeships shows that this effect is due to August-born individuals being more likely to have apprenticeship training. Excluding apprenticeships from past employment shows that the sign of the relationship reverses. Now, September-born workers have more prior working experience than August-born workers; however, this comparison is somewhat unfair because August-born also gained working experience through apprenticeship training.

The analysis of unemployment experiences and employment in subsidized jobs is more conclusive. September-born individuals are less likely to be found in subsidized employment and are less likely to be registered as unemployed (or seeking employment) before their first regular employment spell. Moreover, they spend less days in unemployment and subsidized employment than August-born individuals. This evidence suggests that September-born workers earn higher wages and have fewer problems with finding employment because they have a higher education level. However, even if a lower education level is responsible for August-born workers' difficulties in finding employment, part of the wage effect may be due to a decrease in their reservation wage in response to these difficulties.

Overall, the analysis suggests that the school entry law entails differences in school quality and educational attainment between children born before the cutoff date and children born after the cutoff date and that these differences had a lasting impact on labor market outcomes.

Despite the concerns explained above, I have carried out the same analysis for the female sample.[38] These results suggest that the main conclusions, i. e. that the school entry rule has an effect on educational attainment and earnings in an early tracking system also hold for the female sample. Strikingly, the estimated effects on educational attainment are very similar to those estimated for the male sample. However, September-born females are more likely to have children before first entering the labor market, suggesting that they are also more likely to work part-time. The wage effect is not significant (or even negative) for women with earnings in the 10th percentile of the wage distribution and increases along the wage distribution. This finding indicates that the child effect comes from women with low earnings potential. The quantile regressions for subsequent wages show a persistent earnings gap for women with earnings above the 50th percentile of the wage distribution.

VI Conclusions

Empirical studies have shown that the allocation of students to educational tracks is influenced by the position of a student in the age distribution within a cohort. The reason is that the track choice is based on a noisy signal of ability, the size of which is correlated with age. Therefore, early tracking is considered as particularly harmful and could have long-run effects on educational attainment and earnings. By international standards, first tracking occurs very early in Austria and Germany. Recent empirical studies for these countries show that the youngest students within a school cohort (grade) are significantly less likely to attend the academic track compared to their older peers [Jürges and Schneider, 2011; Mühlenweg and Puhani, 2010; Schneeweis and Zweimüller, forthcoming].

[38] These results are available upon request.

In this paper, I estimate the reduced-form effect of the school entry rule, which assigns students to different school types based on their date of birth, on earnings and educational attainment for a sample of individuals entering the Austrian labor market between 1997 and 2000. The advantage of this strategy is that earnings differences between the youngest (i.e. August-born) and the oldest (i.e. September-born) individuals of a labor market entry cohort do not reflect differences in labor market experience but rather differences in the quantity and quality of education or skills acquired before entering the labor market.

The empirical analysis shows that September-born males earn 1.1–2.0 percent higher wages than August-born males in the year of labor market entry. The estimated effects differ along the conditional starting wage distribution with higher wage penalties in the middle and the higher end than in the lower end of the distribution. On average, the wage penalty amounts to 1.0–1.6 percent in the first and second year after entry and between 0.8 and 1.1 percent in the third and fifth year. Again, quantile regressions reveal heterogenous effects along the wage distribution.

The analysis of differences in the quantity and quality of education shows that September-born males are less likely to have apprenticeship training, and consequently are less likely to work in a blue-collar job and more likely to work in a white-collar job. Furthermore, they are more likely to have at least higher general/vocational education and to hold a degree. Altogether, September-born individuals seem to have obtained a higher education level than August-born individuals.

Further analysis of the labor supply response and job mobility do not indicate that the wage penalty is explained by differences in the labor supply at the extensive or intensive margin or differences in match quality. There is some evidence that August-born workers had more difficulties in finding stable employment which is consistent with August-born workers having lower qualifications.

Overall, the analysis suggests that the school entry law has persistent negative effects on educational attainment and earnings for children born before the cutoff date. These findings are in line with the results on track choice in grade 5–8 and 9 obtained by Schneeweis and Zweimüller [forthcoming]. Moreover, the analysis shows that the possibility to revise the track choice after some years does not necessarily remove the disadvantage of those children—as shown by Mühlenweg and Puhani [2010] for Germany.

Appendix

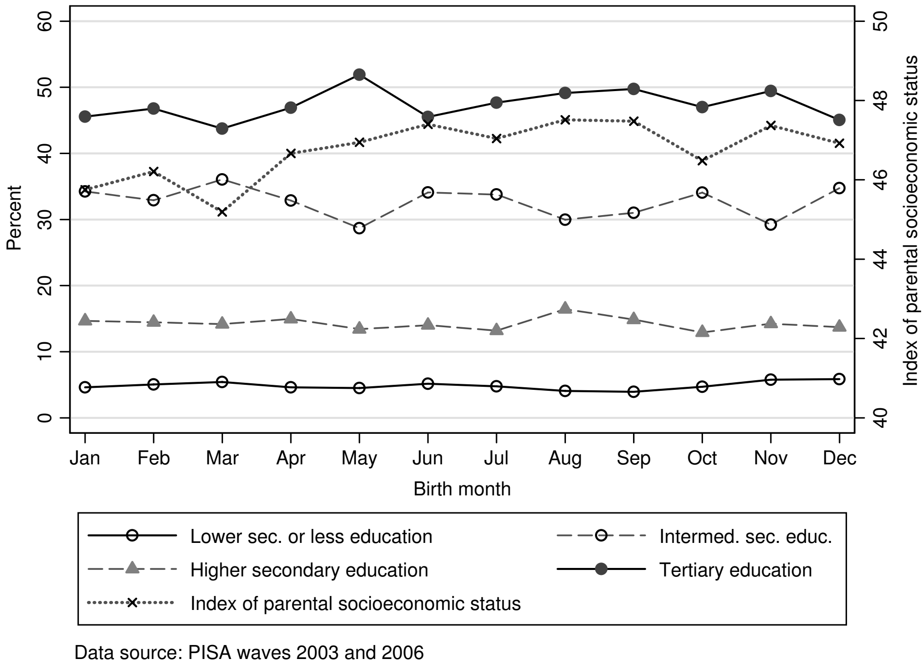

Figure A.1. Distribution of Parental Socioeconomic Status across Birth Months

Notes: This figure is based on data from PISA (Programme for International Student Assessment) waves 2003 and 2006 and shows the distribution of parental education and the index of parental socioeconomic status across birth months for students born in 1987 and 1990 [Schneeweis and Zweimüller, forthcoming].

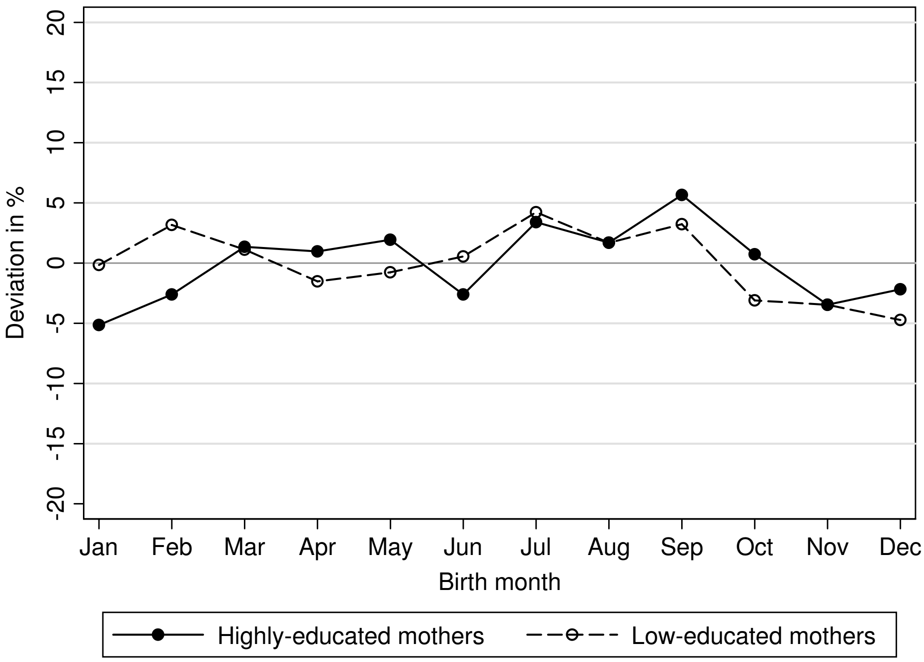

FIGURE A.2. Distribution of the education of the mother across Children's Birth Months

NOTES: This figure is based on data from the Austrian birth register and shows the monthly deviation (in percent) in the number of births with respect to a uniform distribution over the year for all Austrian births over the period 1984–1990.

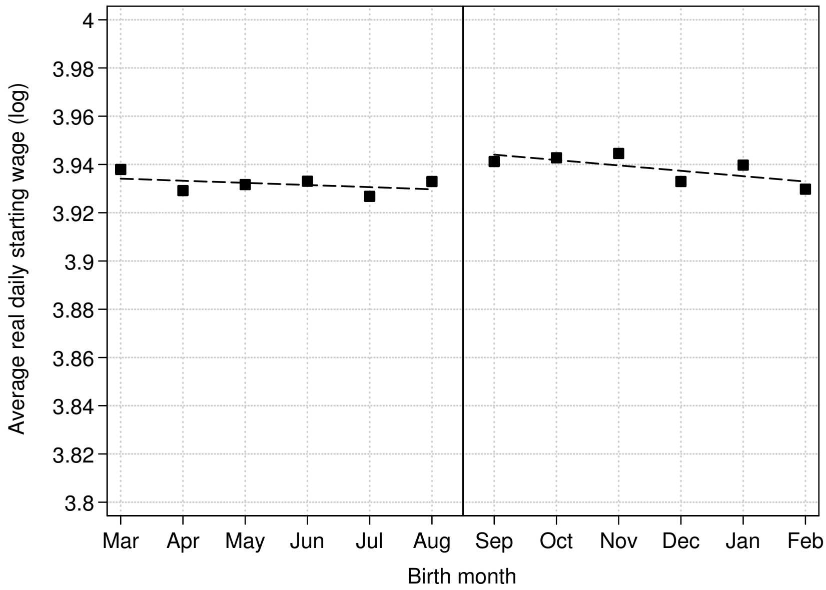

FIGURE A.3. Starting Wage by Birth Month - Linear Trend

NOTES: Figure A.3 shows the average unconditional log real daily starting wage for each birth month. The dashed line represents a linear trend estimated separately before and after the cutoff date.

Table A.I. Quantile Regressions: The Effect of the School Entry Law on Starting Wages

	Q10	*Q25*	*Q50*	*Q75*	*Q90*
Model 1:					
Assigned age	-0.002	0.028***	0.025***	0.017***	0.015***
	(0.011)	(0.004)	(0.003)	(0.003)	(0.004)
Model 2:					
Old	0.006	0.030***	0.029***	0.021***	0.021***
	(0.013)	(0.005)	(0.004)	(0.004)	(0.007)
dist	-0.002	-0.002*	-0.003***	-0.002***	-0.003**
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
dist × old	0.001	-0.001	-0.000	0.000	0.001
	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)

	P-values: Model 1	*P-values: Model 2*
Q10 = Q25	**0.001**	**0.034**
Q25 = Q50	0.397	0.786
Q25 = Q75	**0.018**	0.084
Q25 = Q90	**0.029**	0.229
Q50 = Q75	**0.009**	**0.015**
Q50 = Q90	**0.026**	0.200
Q75 = Q90	0.618	0.979

NOTES: The dependent variable is the real daily log wage in the first regular employment spell as defined in section 4.3. All regressions include a dummy variable that indicates whether wages are measured one year after labor market entry and year of labor market entry dummies. See section 4.1 for a a definition of models 1 and 2. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

Table A.II. The Effect of the School Entry Law on the Skill Level

	Model 1: Assigned age	
Unskilled worker	-0.018***	-0.018***
	(0.002)	(0.003)
Apprenticeship training (≥2.5 years)	-0.031**	-0.034***
	(0.004)	(0.005)
Skilled worker	0.049***	0.052***
	(0.004)	(0.005)
Year of labor market entry	Yes	Yes
Quarter of birth	No	Yes
Observations	169,987	169,987

NOTES: This table shows the estimated marginal effects of *Assigned age* on the worker's skill level from a multinomial logit model. See section IV.1 for a definition of model 1. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

Table A.III. The Effect of the School Entry Law on Starting Wages (Sub-sample[a])

Model 1:			
Assigned age	0.014***	0.013***	0.017***
	(0.002)	(0.001)	(0.002)
Adjusted R^2	0.026	0.026	0.142
Model 2:			
Old	0.015***	0.013***	0.018***
	(0.002)	(0.001)	(0.001)
dist	-0.000***	0.000	-0.001***
	(0.000)	(0.001)	(0.000)
dist × Old	-0.003***	-0.003*	-0.002***
	(0.001)	(0.001)	(0.000)
Adjusted R^2	0.026	0.026	0.142
Year of labor market entry	Yes	Yes	Yes
Quarter of birth	No	Yes	No
Type of entry job	No	No	Yes
Observations	109,963	109,963	109,963

Notes: The dependent variable is the real daily log wage in the first regular employment spell as defined in section IV.2.1. All regressions include a dummy variable that indicates whether wages are measured one year after labor market entry and further control variables as indicated in the bottom of the table. See section IV.1 for a definition of models 1 and 2. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

[a] Results are based on a subsample for which information on educational attainment is available. See notes in Table V for further details.

Table A.IV. Quantile Regressions: The Effect of the School Entry Law on Subsequent Wages

	Q10	*Q25*	*Q50*	*Q75*	*Q90*
1^{st} *year after job entry*					
Assigned age	0.003	0.019***	0.012***	0.015***	0.019***
	(0.011)	(0.004)	(0.002)	(0.003)	(0.005)
Old	0.011	0.019***	0.015***	0.018***	0.026***
	(0.015)	(0.005)	(0.003)	(0.004)	(0.006)
2^{nd} *year after job entry*					
Assigned age	-0.024**	0.008**	0.012***	0.016***	0.020***
	(0.010)	(0.003)	(0.003)	(0.004)	(0.005)
Old	-0.022*	0.008*	0.014***	0.022***	0.028***
	(0.012)	(0.004)	(0.004)	(0.004)	(0.008)
3^{rd} *year after job entry*					
Assigned age	-0.016	0.008**	0.013***	0.015***	0.009*
	(0.011)	(0.004)	(0.003)	(0.004)	(0.006)
Old	-0.019	0.010**	0.018***	0.021***	0.021***
	(0.014)	(0.004)	(0.003)	(0.005)	(0.007)
4^{th} *year after job entry*					
Assigned age	-0.018	0.008**	0.011***	0.016***	0.010
	(0.011)	(0.004)	(0.003)	(0.005)	(0.006)
Old	-0.029**	0.006	0.014***	0.021***	0.017**
	(0.013)	(0.005)	(0.004)	(0.005)	(0.007)
5^{th} *year after job entry*					
Assigned age	-0.003	0.006*	0.010***	0.014***	0.010*
	(0.013)	(0.004)	(0.003)	(0.005)	(0.006)
Old	-0.007	0.005	0.013***	0.022***	0.015*
	(0.016)	(0.005)	(0.004)	(0.006)	(0.009)

NOTES: This table shows results from quantile regressions of the wage after x years of potential labor market experience (see first column) on *Assigned age* (model 1) or *Old*, *dist* and *dist* × *old* (model 2) and dummies that indicate the year of labor market entry. See section IV.1 for a definition of models 1 and 2. Each estimation includes only employed individuals. Therefore, the number of observations varies between the years. Wages are measured as average real daily wages over all employment spells in a given year conditional on being employed. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

Table A.V. Labor Supply: Employment Probability[a] and Duration of Employment[b]

	Model 1: Assigned age		*Model 2: Old*		*Mean (SD)*		*Observations*[c]	
	Pr(Work)	Days emp.	Pr(Work)	Days emp.	Pr(Work)	Days emp.	Pr(Work)	Days emp.
1^{st} year after job entry	-0.001	6.973***	-0.001	8.043***	0.96	265 (117)	169,469	161,917
	(0.001)	(0.828)	(0.001)	(0.543)				
2^{nd} year after job entry	-0.001	5.690***	-0.000	5.939***	0.93	297 (103)	169,597	157,239
	(0.002)	(0.928)	(0.002)	(0.771)				
3^{rd} year after job entry	0.001	1.251	-0.001	1.467*	0.92	309 (96)	169,639	155,858
	(0.002)	(0.724)	(0.003)	(0.770)				
4^{th} year after job entry	-0.002	1.344*	-0.001	1.513**	0.91	315 (92)	169,677	154,800
	(0.003)	(0.694)	(0.005)	(0.529)				
5^{th} year after job entry	-0.004	2.344**	-0.004	2.558**	0.91	319 (89)	169,539	153,777
	(0.003)	(0.998)	(0.003)	(0.907)				
Dist & dist $\times$ Old	No	No	Yes	Yes	-	-	-	-
Year of labor market entry	Yes	Yes	Yes	Yes	-	-	-	-

NOTES: Each cell represents a separate estimation of the dependent variable as indicated in the second row and measured in a given year on *Assigned age* (model 1) or *Old* (model 2) and and control variables as indicated in the bottom of the table. See section IV.1 for a definition of models 1 and 2. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

[a] The dependent variable measures the probability to have any (non-marginal) employment spell in a given year.

[b] The dependent variable measures the days of employment for employed individuals in a given year.

[c] The number of observations depends on the number of (employed) individuals with non-missing wages.

Table A.VI. Job mobility

	Model 1: Assigned age	*Model 2: Old*	*Mean*	*Obs.*[a]
1^{st} year after job entry	0.018***	0.017**	0.29	161,917
	(0.004)	(0.006)		
2^{nd} year after job entry	0.008*	0.007	0.52	157,239
	(0.004)	(0.005)		
3^{rd} year after job entry	0.005*	0.003	0.63	155,858
	(0.003)	(0.003)		
4^{th} year after job entry	0.005**	0.005*	0.70	154,800
	(0.002)	(0.002)		
5^{th} year after job entry	0.002	-0.000	0.75	153,777
	(0.002)	(0.002)		
Year of labor market entry	Yes	Yes	-	-
Dist & dist × Old	No	Yes	-	-

NOTES: Each cell represents a separate estimation of a binary variable indicating whether an individual works for another employer than in the entry job after x years of potential labor market experience (see first column) on *Assigned age* (model 1) or *Old* (model 2) and and control variables as indicated in the bottom of the table. See section IV.1 for a definition of models 1 and 2. Each estimation includes only employed individuals. Therefore, the number of observations varies between the years. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

[a] The number of observations depends on the number of (employed) individuals with non-missing wages.

Table A.VII. Employment history

	Model 1: Assigned age (1)	(2)	*Model 2: Old* (3)	(4)	*Mean (SD)*
Any dependent employment	0.003**	0.001	0.003**	0.002	0.92
	(0.001)	(0.001)	(0.001)	(0.002)	
Dur. of dep. emp. (days)	-28.399***	-29.067***	-36.700***	-36.565***	699 (564)
	(6.028)	(3.472)	(3.021)	(3.252)	
Any apprenticeship training	-0.027***	-0.030***	-0.037***	-0.040***	0.52
	(0.007)	(0.003)	(0.003)	(0.002)	
Dur. of appr. training (days)	-35.639***	-38.874***	-46.959***	-49.559***	580 (598)
	(7.223)	(4.075)	(3.277)	(2.932)	
Any dep. emp. (w/o appr.)	0.050***	0.046***	0.057***	0.057***	0.67
	(0.004)	(0.003)	(0.004)	(0.004)	
Dur. of dep. emp. (days)	7.440***	10.303***	10.622***	13.371***	120 (216)
	(1.855)	(1.855)	(1.394)	(0.769)	
Any subsidized employment	-0.001	-0.005***	-0.003	-0.006***	0.03
	(0.002)	(0.001)	(0.002)	(0.002)	
Dur. of subsidized emp. (days)	-0.763*	-1.679***	-1.286***	-2.130***	6 (50)
	(0.388)	(0.180)	(0.411)	(0.273)	
Any registered unemployment	-0.004	-0.006*	-0.008**	-0.012**	0.34
	(0.004)	(0.003)	(0.003)	(0.005)	
Dur. of registered unemp. (days)	-3.005**	-4.397***	-5.269***	-6.440***	66 (170)
	(1.248)	(0.657)	(0.955)	(1.147)	
Dist & dist × Old	No	No	Yes	Yes	
Year of labor market entry	Yes	Yes	Yes	Yes	
Quarter of birth	No	Yes	No	Yes	
Observations	169,987	169,987	169,987	169,987	

NOTES: This table presents an analysis of the employment history prior to an individual's first regular employment spell (as defined in section IV.2.1. Each cell represents a separate estimation of the dependent variable (see first column) on *Assigned age* (model 1) or *Old* (model 2) and and control variables as indicated in the bottom of the table. See section IV.1 for a definition of models 1 and 2. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are birth months). ***, ** and * indicate significance at the 1-percent, 5-percent and 10-percent level.

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