Economics of Education Review 84 (2021) 102144

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Long-term effects of school-starting-age rules☆

Hessel Oosterbeek[a], Simon ter Meulen[a], Bas van der Klaauw[b]

[a] University of Amsterdam, School of Economics, Roetersstraat 11, Amsterdam, WB, 1018, Netherlands
[b] VU University Amsterdam, Department of Economics, De Boelelaan 1105, Amsterdam, HV, 1081 Netherlands

A R T I C L E I N F O

JEL classification:
I21
I24
I26

Keywords:
Relative cohort age
School starting age
Early tracking

A B S T R A C T

To study the long-term effects of school-starting-age rules in a setting with early ability tracking, we exploit the birth month threshold used in the Netherlands. We find that students born just after the threshold perform better at the end of primary school than students born just before it. This translates into increased placement in high ability tracks in secondary education. This difference diminishes gradually during subsequent stages, and we find no effect on the highest attained educational level. Those born just before the threshold enter the labor market somewhat younger and have therefore more labor market experience and higher earnings at any age until 40. We conclude that early ability tracking does not harm long-term outcomes of children who were, for exogenous reasons, placed in a lower track.

1. Introduction

A robust finding in the economics of education literature is that children who are the oldest in their cohort perform better on school-based achievement tests than their younger classmates. This difference has been attributed to the hard-to-disentangle effects of school starting age, relative age in class, and age-at-test (Bedard & Dhuey, 2006; Cook & Kang, 2020; Elder & Lubotsky, 2009; Leuven, Lindahl, Oosterbeek, & Webbink, 2010; Lubotsky & Kaestner, 2016; McEwan & Shapiro, 2008; Mühlenweg & Puhani, 2010; Peña, 2020; Puhani & Weber, 2008; Schneeweis & Zweimüller, 2014). There is less consensus about the longer-run effects. Black, Devereux, & Salvanes (2011) and Dobkin & Ferreira (2010) find for Norway and the US no effect on final degree, while Fredriksson & Öckert (2013) find an increase in years of education. None of these papers find that the early advantage of older students carries over to labor market outcomes in adulthood.

A possible reason for the absence of strong long-term effects is that the countries that have been studied postpone ability tracking to the end of lower secondary education. Postponed tracking gives relatively young children more time to catch up with their older classmates. It is still an open question whether long-term effects are also absent in settings where ability tracking occurs at a younger age. In this paper, we address this question by analyzing the long-term effects of the school-starting-age rule in the Netherlands. This rule stipulates that children who turn six years old before October 1st can enroll in first grade at the start of the school year (in August or September), while children who turn six years old on October 1st or later can only enroll in first grade at the start of the next school year. Ability tracking in the Netherlands occurs at age 12 when children transfer from primary school to secondary school.[1] Track placement is to a large extent determined by students' performance on a nationwide exit test for which students' scores are not adjusted for their age.

Using Dutch register data and a sharp regression discontinuity design, we find that children born just after October 1st have a substantial advantage at the end of primary school compared to students born just before October 1st. The difference on the test that is used to assign students to ability tracks is on average 0.08 SD. This translates into a 4 percentage point difference in the probability to be assigned to and enter the college or university track instead of a vocational track in secondary education. This difference should be compared to a base rate of 0.5. The difference diminishes gradually during the subsequent educational stages. The difference in the probability to obtain a diploma from the college or university track is only 2 percentage points. And the difference in the probability of enrolling in a professional college or a university is only 1 percentage point. There is no difference in terms of the final degrees that students obtain.

☆ We gratefully acknowledge valuable comments from two anonymous referees and from seminar participants in different places. The non-public micro data used in this paper are available via remote access to the Microdata services of Statistics Netherlands (CBS). Oosterbeek received support from the Research Council of Norway Toppforsk grant no. 275906. Van der Klaauw acknowledges financial support from a Vici-grant from the Dutch Science Foundation (NWO).

E-mail addresses: h.oosterbeek@uva.nl (H. Oosterbeek), s.r.termeulen@uva.nl (S. ter Meulen), b.vander.klaauw@vu.nl (B. van der Klaauw).

[1] Other countries that use early ability tracking include Austria, Belgium, Czech Republic, Germany, Hungary, Turkey and Singapore (OECD, 2013).

https://doi.org/10.1016/j.econedurev.2021.102144
Received 16 November 2020; Received in revised form 16 April 2021; Accepted 7 June 2021
Available online 15 July 2021

The closure of the gap between the oldest and youngest students in a cohort can be attributed to large streams between the vocational track and the college and university tracks. Over 70% of the initial gap is closed due to students streaming up from the vocational track to the college and university tracks. The remainder is due to students streaming down from the college and university tracks to the vocational track. The tracking system in the Netherlands is thus flexible enough to adjust for initial placement differences due to an exogenous factor (month of birth).

For earnings, we find that those born before the threshold outperform those born after the threshold. This is because those born before, enter the labor market at a younger age, which translates into more labor market experience in adulthood. These earnings effects are present until age 40. This is longer than what other studies have found.

This paper proceeds as follows. The next section briefly describes the educational system in the Netherlands. Section 3 describes the data. Section 4 introduces the empirical approach and assesses its validity. Section 5 presents and discusses the main results, and Section 6 reports heterogeneity. Section 7 summarizes and concludes.

2. Education system in the Netherlands

Figure 1 gives an overview of the Dutch education system. Elementary school in the Netherlands consists of two to three years of kindergarten and six years of primary school. Kindergartens admit children on a rolling basis, children can start the day they turn 4 years old. Children that are 6 years old on October 1st are supposed to transfer to primary school at the start of the school year.[2] Children who turn 6 years old on October 1st or later are expected to enroll in primary school in the next school year. Schools had some discretion to deviate from the "October 1st" threshold and in recent years some schools started to apply less strict threshold rules.[3]

In grade 6, at the end of primary education, students take a nationwide exit test. The performance on this test serves as the leading indicator for the ability track of the student in secondary education. However, the primary school teacher has the discretion to overrule the exit test.

We distinguish three ability tracks in secondary school that differ in how academically demanding they are. The vocational track lasts four years and gives access to subsequent vocational programs. The college track takes five years and gives access to professional colleges. The university track takes six years and gives access to university education.

Some secondary schools teach students from different tracks in the same class with the same study materials for one or two years, meaning that a student can enter another track in the second or third year of secondary school than the one s/he was initially assigned to. Upstreaming and downstreaming is possible at later moments in secondary and higher education. The most common form of upstreaming occurs after a student obtained the diploma from a lower track. A common form of downstreaming occurs when a student performs poorly at their current level.

[2] A school year runs from August/September to June/July.

[3] The first entrance threshold for primary education was implemented in 1920 (Koninklijk besluit nr 37, 1920, Art.1). The next rule was formalized in 1937 (Koninklijk besluit nr 36, 1937, Art.1). This rule included the prosecution of non-complying schools (Inspectie Onderwijs, 1936). In 1985 kindergarten and primary education merged into elementary education. The new education law did not specify any entrance threshold, which was unnoticed until at least 8 years later (van Dam, 1994). Afterward, some schools started to apply the rule with some discretion. However, in 2013 the majority of schools still used the October 1st threshold (Smeets & Resing, 2013).

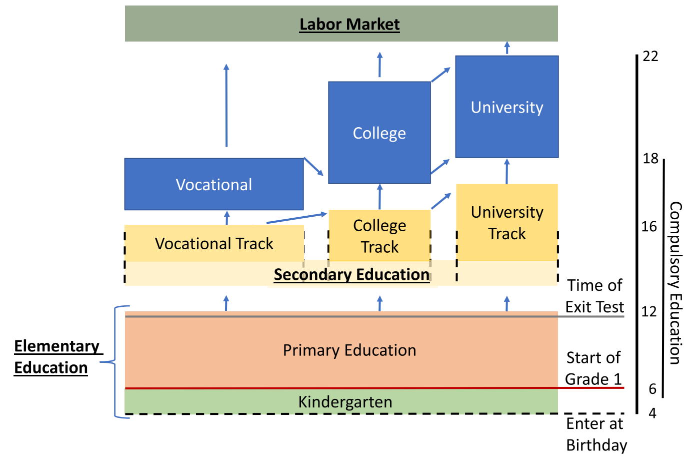

Fig. 1. Dutch educational system.
Notes: The vertical bar on the right indicates students' age at nominal study duration. Arrows represents possible upward transitions.

3. Data

In the empirical analysis we use administrative data available at Statistics Netherlands. These data come from various registers and can be merged using a unique personal identifier. The records from the municipalities contain for each individual the day of birth, country of birth, gender and the link to family members. The records of the nationwide testing agencies provide the results of the exit test from primary education and the track recommendation for secondary education given by the primary school teacher. The enrollment data from the Ministry of Education contain the third year secondary education track and the tertiary education entry. The degree register of the Ministry of Education contains the secondary education diploma(s) and tertiary education degree(s). We use these administrative data to describe individuals' educational careers.

Earnings data come from the tax records that the tax authority provides to Statistics Netherlands. We use earnings before taxes for the years 1999 to 2015 to construct annual earnings at the ages between 18 and 65. For example, for age 30, we use the birth cohorts 1969 to 1985. Figure A1 in the Appendix gives a schematic overview of the cohorts that are used for which ages. To compare annual earnings observed in different years we adjust all earnings to price levels in 2015. All earnings regressions include fixed effects for the year of observation.

The administrative records from the municipality link children to their parents. This gives information about the social-economic status (SES), ethnicity and the age of the parents. We construct a SES-indicator based on the parental earnings distribution in 1999 (net of age effects). The bottom quartile is the Low-SES group. The second and third quartiles form the Middle-SES group, and the top quartile is the High-SES group. Ethnicity is determined on the basis of the country of birth of the parents. If one of the parents is born in a non-western country, the child is labeled as being from non-western descent. We restrict the sample to children born in the Netherlands to make sure that the Dutch school-starting-age rule applies. We use gender, SES, ethnicity and the age of the father to show the balancing around the threshold and to study heterogeneous treatment effects.

The data at Statistics Netherlands contain no information about children's actual school starting age. Therefore, we use the school starting age from the so-called Prima Survey. The Prima Survey is a bi-annual longitudinal survey that gathers educational information from students in a representative sample of 600 primary schools (around 10% of the schools) from 1994 to 2006. All surveys have information on birth month, but only the 1998 and 2000 surveys have information about the day of birth.

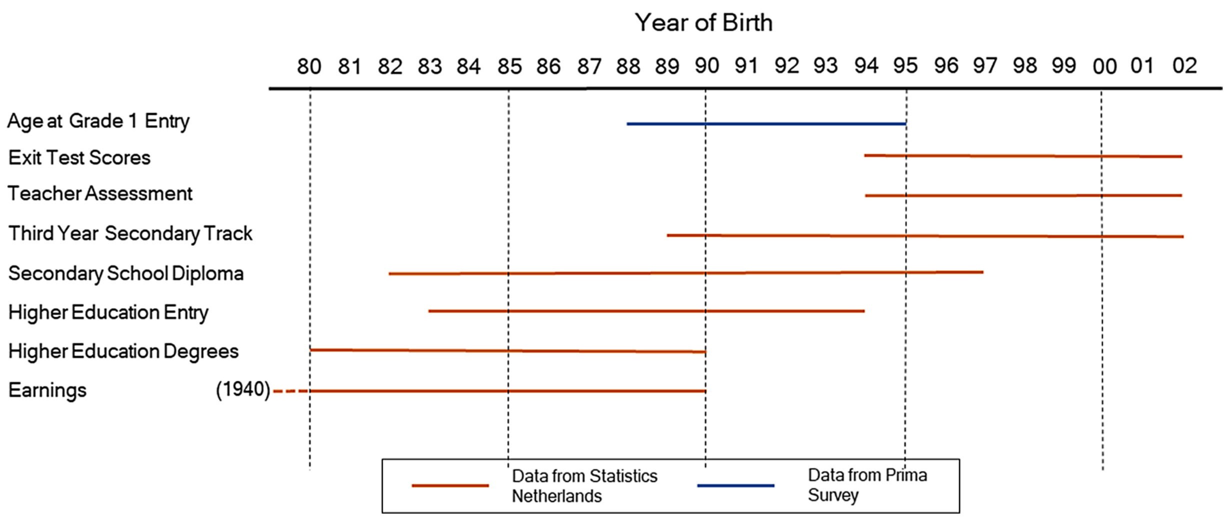

Fig. 2. Data availability.
Notes: The horizontal bars represent the data availability of birth cohorts for the variable specified on the vertical axis. Earnings data is available for birth cohorts starting in 1940.

The observation period differs between the different data sources. There is no birth cohort for which we observe the complete sequence of outcomes. Figure 2 shows which outcomes are observed for which birth cohorts. The horizontal axis in Figure 2 indicates birth cohorts. The orange lines show for which birth cohorts the outcomes displayed on the vertical axis are available. In the results section, we will inquire whether effects on the same outcome are homogenous across (groups of) cohorts. That is, we will examine whether the effects of birth month on track in grade 9 differ for cohorts born between 1994 and 2002 and cohorts born between 1989 and 1993. Likewise, we will examine whether effects on enrollment in tertiary education differ for cohorts 1989-1993 and cohorts 1983-1989.

4. Empirical approach

We are interested in the effects on education and labor market outcomes of being born in October and therefore being the oldest in a cohort versus being born in September and therefore being the youngest in a cohort. We assume a linear relationship between outcome variable y_{it} for individual i from cohort t and being born in October (Old_i) instead of September.

$$y_{it} = \alpha_t + \delta \cdot \text{Old}_i + f(\text{Birthday}_i) + \varepsilon_{it}, \tag{1}$$

The effect of being born in October instead of September on outcomes is captured by δ, the parameter of interest. $f(\text{Birthday}_i)$ is a function that controls for direct effects of the running variable birthday on outcomes. α_t are birth cohort fixed effects, with cohorts centered around October 1.

We focus the analysis on reduced-form effects. We abstain from scaling these effects by a first-stage effect from a regression of school starting age on being born in October (as in Fredriksson & Öckert (2013)) or from a regression of relative age in class on being born in October (as in Bedard & Dhuey (2006)). We therefore have a sharp regression discontinuity (RD) design. In the main text, we present results based on a restricted sample of persons born in September and October and from a specification that controls linearly for the day of birth. Using the algorithm developed by Imbens & Kalyanaraman (2012) and Calonico, Cattaneo, & Titiunik (2014), we find that the optimal bandwidth (almost always) lies between 14 and 30 days on both sides. Therefore, we present RD-estimates based on 14-days, 30-days, and optimal bandwidths in the main text. In graphs we will also show results for all birth months.

The validity of the RD design depends on the independence assumption that birth dates are as good as randomly assigned relative to the threshold. A first piece of evidence in support of that, would be the absence of bunching near the threshold. The left graph in Figure 3 shows the density of month of births over the year. There are systematic fluctuations over the year, in particular from July to December there is a declining trend in the number of births. The right graph of Figure 3 shows the density plots of days of birth around the cut-off. At the end of September more children are born than early October. This is a general pattern that is observed for many countries. The peak in the number of birth late September relates to conception late December during the Christmas and New Year period. The McCrary test rejects the absence of bunching around the threshold with a p-value of 0.013, the log difference in observations is 0.043. We therefore reject the absence of bunching around the threshold.

Bunching invalidates the RD design if observations at the two sides of the threshold are systematically different. To assess this, Table 1 reports estimates from Equation (1) for pre-determined characteristics as outcome variables. This shows that there is no relationship between being born one month (two weeks) after versus before the threshold and students' gender, SES, ethnicity, and the age of the father. We also tested whether these four characteristics jointly predict that a student is born in the month (two weeks) after versus before the threshold. An F-test does not reject the null of no effect; $p = 0.30$ for the one month window and $p = 0.47$ for the two weeks window.[4]

5. Results

We present the results in three parts. We start with results about the effects of month of birth on actual school starting age. Subsection 5.2 presents results for various steps in students' educational career and Subsection 5.3 presents results for earnings.

[4] In addition, we use the student characteristics to predict the main outcomes and test if predicted outcomes are discontinuous at the threshold. To predict the outcomes we interact all student characteristics and allow for birth-year fixed effects. Figure C1 shows that for all predicted outcomes there are no significant differences at the threshold.

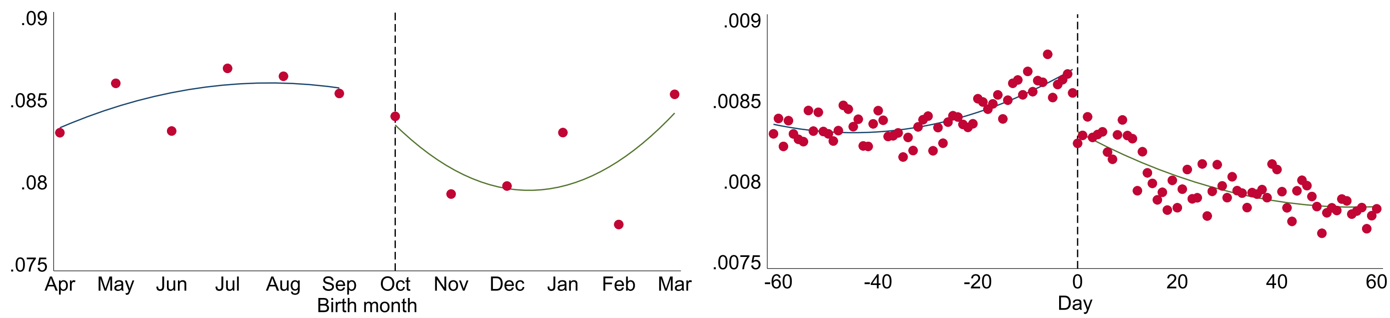

Fig. 3. Distribution of birthdays.

Notes: The left graph shows the density of birth over the months of the year (0 is October). The right graphs shows the density of days of birth around the cut-off (0 is October 1). The lines represent a second degree polynomial, separately estimated left and right of the cut-off. The figure is based on the cohorts born in the Netherlands between April 1977 and March 2002.

Table 1
Balancing table

	Gender	SES	Etnicity	Age of Father
	30 day interval on both sides of the cut-off			
Old	0.0040	-0.0006	-0.0014	-0.0104
	(0.0022)	(0.0020)	(0.0015)	(0.0233)
Obs.	785,142	785,142	785,142	769,574
Birth year fixed effects	√	√	√	√
	14 day interval on both sides of the cut-off			
Old	0.0031	-0.0009	-0.0028	-0.0162
	(0.0033)	(0.0029)	(0.0023)	(0.0343)
Obs.	366,589	366,589	366,589	359,539
Birth year fixed effects	√	√	√	√

Notes: The upper-panel uses a 30-days interval on both sides of the cut-off. The lower-panel uses a 14-days interval around the cut-off. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$

5.1. School starting age

Figure 4 plots the relation between months of birth and school starting age. The horizontal axis is centered at the entrance threshold of October 1st. The dashed lines show what the relation would look like if all students complied with the school starting age rule. In that case the jump at the threshold would be equal to one full year. The solid lines show results from local linear regressions based on actual school starting age. The jump at the threshold based on actual school starting age equals 0.41 of a year (5 months). Comparison of the dashed and solid lines reveals that non-compliance comes from two sources. Some children born in July to September postpone enrollment in grade 1 and some children born in October and November start one year earlier than they are supposed to.[5] The estimate of 0.41 means that one should multiply the reduced form estimates presented next by 2.5 to interpret these as causal effects of starting school a year earlier. For the interpretation of the estimated treatment effects, it should be stressed that children who do not enroll in primary school stay in kindergarten. So the treatment of enrolling in primary school at an older age also means having spent more time in kindergarten.

Figure B2 in Appendix B shows graphs with separate lines by gender, SES and ethnicity. While the jumps at the threshold are very similar for male and female students, the patterns of non-compliance are different. Girls born before October 1st are less likely to postpone enrollment than boys born before October 1st. At the same time, girls born after October 1st are more likely to start early than boys born after October 1st. Similar differences in compliance are present for students from different social backgrounds. Children from higher SES families are less likely to postpone entrance when born before October 1st and more likely to start early when born after October 1st than children from lower SES families. Children from non-western descent are more likely to postpone entrance when born before October 1st than native children. Because there is no difference in the shares of children that start early when born after October 1st, the jump at the threshold is larger for native students (0.42) than for students from non-western descent (0.34).

5.2. Educational career

Table 2 reports the effect of being born just after instead of just before October 1st on education outcomes at different stages.[6] The results focus on the probability to enter or finish college or university education instead of vocational education.[7] Since it is not possible to estimate effects for all the different stages for the same cohort, we present results separately for overlapping cohorts. This is indicated in the final column of the table.

The first row of Table 2 shows that being born after rather than before the threshold increases performance on the exit test from primary school by around 0.08 SD-units. This translates in the next row into a 4.0 to 4.8 percentage points increase in the probability to have a score high enough for the college/university track. This effect should be compared to a base rate of 50%.[8] Since the score on the test is not adjusted for students' age-at-test, this estimate is the combined effect of age-at-test, school starting age and relative cohort age during primary school. Our estimated effect on the exit test translates to a local average treatment effect of about 0.2 of a standard deviation, which is similar to what Bedard & Dhuey (2006), Peña (2020) and Cook & Kang (2020) find.

The third row shows the effect of being born after instead of before the threshold on the recommendation of the primary school teacher. Being born in October instead of September boosts the probability to be recommended the college or university track about 5 percentage points.[9] Because teachers know students month of birth (birthdays are celebrated in school), they can in principle adjust for students' age-at-

[5] Figure B1 in Appendix B depict the daily average starting age around the threshold for children born in September and October.

[6] The corresponding graphs are presented in Appendix D.

[7] Appendix E reports results for entering university education instead of college or vocational education. These results are similar to the results discussed in this section.

[8] For university education this is 3.5 to 3.7 on a base rate of 18.6 %, see row 2 of Table E1.

[9] "Being born in October instead of September" should be understood as short-hand for "Being born just after the October 1st threshold instead of being born just before the October 1st threshold".

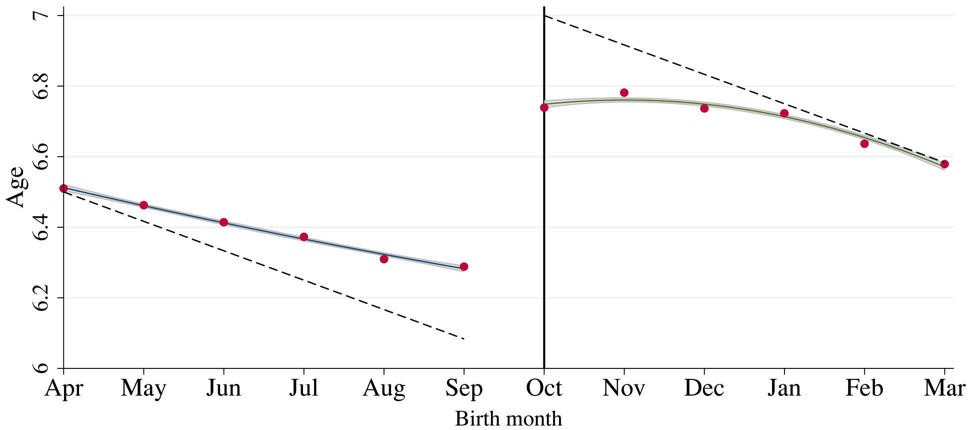

Fig. 4. School starting age by month of birth. **Notes:** Data from Prima cohort survey. Bi-annual cohorts were born between April 1988 to March 1995, 94,935 observations. The red dot represents the monthly average, and the two solid lines represent the best fit from a second-degree polynomial regression from each side of the cut-off. Although hardly visible, the grey area is the 95% confidence area of the solid line. The dashed line indicates what the starting age would have been in case of full compliance. The local linear regression discontinuity takes a 30-day bandwidth at both sides of the cut-off and only uses data from Prima cohort survey 1998 and 2000, 5,774 observations.

Table 2
Educational career

	$\hat{\delta}$			Optimal bandwidth	Mean	Cohort
	Bandwidth					
	14 days	30 days	Opt.			
Standardized Exit-test score	0.082***	0.097***	0.085***	16 days	0.000	1994 - 2002
	(0.013)	(0.009)	(0.013)			
Exit-test score = College or University	0.040***	0.048***	0.043***	17 days	0.503	1994 - 2002
	(0.007)	(0.005)	(0.006)			
Teacher Assessment = College or University	0.050***	0.057***	0.055***	21 days	0.430	1994 - 2002
	(0.008)	(0.005)	(0.006)			
Grade 9 track = College or University	0.037***	0.041***	0.037***	14 days	0.455	1994 - 2002
	(0.006)	(0.004)	(0.006)			
	0.048***	0.046***	0.042***	23 days	0.426	1989 - 1993
	(0.008)	(0.006)	(0.007)			
School diploma = College or University	0.024***	0.025***	0.021***	20 days	0.392	1989 - 1993
	(0.008)	(0.005)	(0.007)			
Enrollment into College or University	0.008	0.010	0.003	21 days	0.524	1989 - 1993
	(0.008)	(0.005)	(0.007)			
	0.009	0.011*	0.012*	22 days	0.495	1983 - 1989
	(0.007)	(0.005)	(0.006)			
College or University degree	0.006	0.004	0.006	21 days	0.388	1983 - 1989
	(0.007)	(0.005)	(0.006)			

Notes: The exit test score is standardized such that it has a mean of zero and a standard deviation of 1. An exit test score that is equal to a college or university track assessment is a score of 537 or higher. Grade 9 track is the track a student attends in the third year of secondary school. A school diploma is a diploma that a student obtains in secondary school. Column *Opt.* uses the bandwidths that are displayed in the column *Optimal bandwidth*. The *Mean* of the outcome variable is calculated over all birthdays in the cohort range. The number of observations for the 30 and 14-day bandwidths, respectively are 184,082 and 86,025 for the 1994 - 2002 cohort, 131,609 and 61,151 for the 1989 - 1993 cohort, and 178,772 and 83,759 for the 1983 -1989 cohort. The number of observations for the optimal bandwidth varies with the number of days. The optimal bandwidths are calculated using the algorithm developed in Imbens & Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$

Table 3
Ability track changes

Track	Transition		Sep.	Oct.	Diff.	Cohort
change	From	To	%	%	%	
Up	Exit test = vocational track	Grade 9 track = college university track	7.62	6.83	0.79	1994-2002
Down	Exit test = college university track	Grade 9 track = vocational track	8.81	8.82	-0.01	1994-2002
Up	Grade 9 track = vocational track	Secondary School diploma = college university track	3.97	2.97	1.00	1989-1999
Down	Grade 9 track = college university track	Secondary School diploma = vocational track	1.71	2.08	-0.37	1989-1999
Up	Secondary School diploma = vocational track	Entry college university	18.56	16.72	1.84	1983-1989
Down	Grade 9 track = college university track	Entry vocational college	0.48	0.99	-0.51	1983-1989
Down		Not finishing college university	10.87	11.39	-0.52	1983-1989
Total						
Up					**3.63**	
Down					**-1.41**	

Notes: An exit test score that is equal to a college or university track assessment is a score of 537 or higher. Grade 9 track is the track a student attends in the third year of secondary school. A school diploma is a diploma that a student obtains in secondary school.

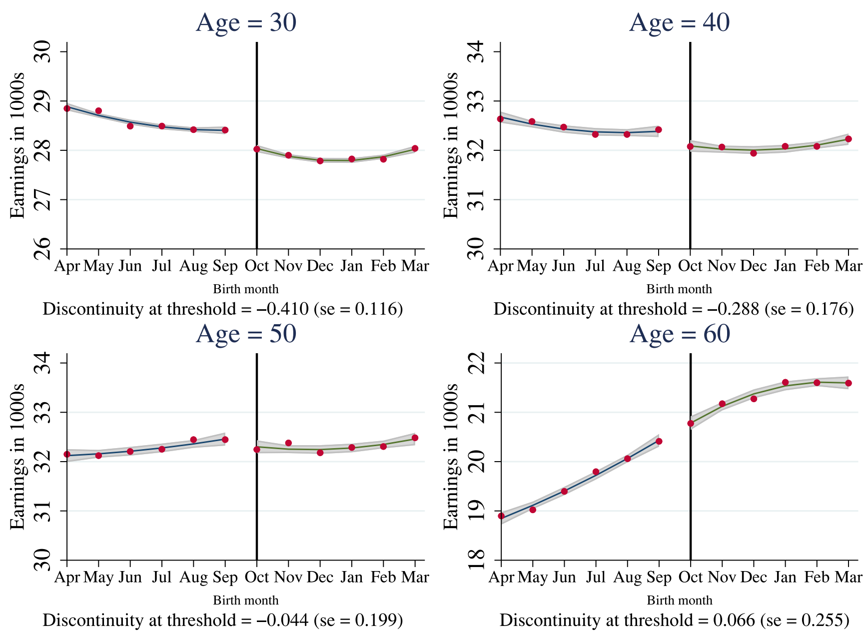

Fig. 5. Earnings at different ages.
Notes: Earnings are income from (self-)employment. Each red dot is a monthly average. We include zero earnings in this average. The grey area represents the 95% confidence interval of a second-degree polynomial regression, separately estimated left and right of the cut-off. Each panel uses a different birth-cohort. Age 30 uses birth-cohorts 1969 - 1985 with a total of 483,654 observations. Age 40 uses birth-cohorts 1959 - 1975 with a total of 581,919 observations. Age 50 uses birth-cohorts 1949 - 1965 with a total of 575,606 observations. For age 60, we use the birth-cohorts 1939 - 1965, a total of 335,910 observations. The local linear regression discontinuity estimates are based on a 30-day bandwidth at both sides of the threshold.

recommendation. The fact that the effects on the teacher recommendation and the exit test are very similar suggests that the effects of school starting age and relative cohort age dominate the effect of age-at-test.

The next two rows apply to the track in which students are observed in grade 9 (three years into secondary education). Student born in October are in 9th grade 4 percentage points more likely to be in the college or university track than students born in September. This effect is slightly smaller than the difference in track placement corresponding to the primary school exit test or the teacher recommendation. The similarity of the two estimates for different cohorts (1994-2002 and 1989-2003) indicates that the effect is rather constant over time. Our estimates have a similar magnitude as found by Puhani & Weber (2008) and Mühlenweg & Puhani (2010) who study attending the highest secondary school track in Germany.

In the next row, we see that being born in October increases the probability to obtain a secondary school diploma at the college or university level by 2 percentage points, relative to a base of 39%. This effect is significantly smaller than the effects measured at the end of primary school and in grade 9 ($p < 0.001$). When looking at enrollment in college or university we see that the gap between October and September born students becomes even smaller. Being born in October increases the probability to enroll in college or university by only 1 to 1.2 percentage points.[10] Finally, when we consider obtaining a college or university degree, we end up with a quite precisely estimated effect of zero. Hence, despite their higher achievement at the end of primary school and their placement in higher tracks in secondary education, students born in October are equally likely to obtain a college or university degree as students born in September. Black et al. (2011) and Dobkin & Ferreira (2010) also find no effect on the final degree for Norway and the US, while Fredriksson & Öckert (2013) find an increase of about 0.14 years of education in Sweden.

To understand what drives the closure of the initial gap in education outcomes between students born in October and September, we consider track switches that occur during secondary school. It is both possible to transfer to a higher track (Up) and to a lower track (Down). Table 3 shows the incidence of the different track switches for students born in September and October as well as the difference in incidence between both groups. For example, the first row shows the transfer from an exit test advising the vocational track to being enrolled in the college or university track in grade 9. About 7.6% of the students born in September make this transfer up, while this is about 6.8% for the students born in October.[11] The differences between students born in September and October for upward switches are always positive, while the differences for downward switches are always negative. This shows that students who start early are more likely to move up and less likely to move down at all education stages. Differences in transitions in later stages contribute most to the closure of the gap. This suggests that postponement of tracking to one or two years into secondary school will not contribute much to a reduction of month of birth effects. The bottom two rows in the table report the totals of upward and downward transitions. This shows that 72% of the track changes that close the gap between students born in September and October are due to upward changes.

5.3. Earnings

Figure 5 presents the relation between month of birth and earnings at ages 30, 40, 50 and 60. At age 30 people born before October 1st earn 410 euro per year more than people born after the threshold. This annual difference shrinks to 288 euro at age 40 and has completely disappeared at ages 50 and 60. Dobkin & Ferreira (2010) and Larsen & Solli (2017) do not find any effects on earnings at age 30 or later, while Black et al. (2011) and Fredriksson & Öckert (2013) find effects that are slightly smaller than our estimated effects.

[10] Column 4 of Table 2 indicates that close to 40% of a cohort gets a secondary school diploma at the college or university level while around 50% enrolls in college or university. The difference is due to students who graduate from the vocational track and later on enroll in college or university.

[11] The percentages of Table 3 do not exactly match the ones from Table 2. Table 2 reports RD estimates whereas Table 3 shows simple averages.

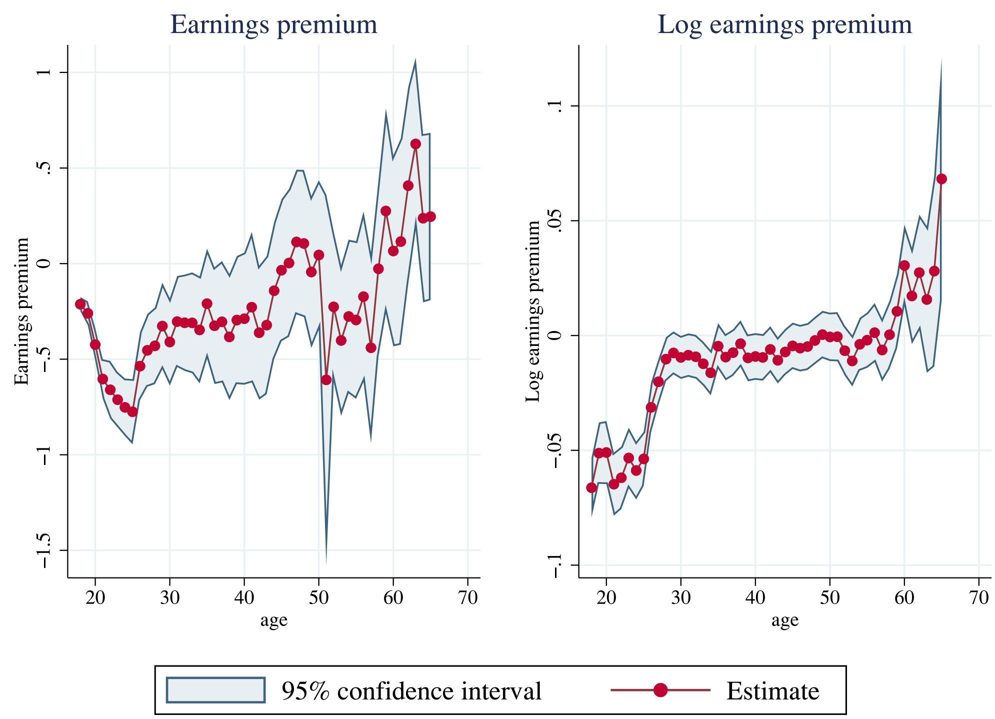

Fig. 6. Life time earnings.
Notes: Earnings are income from (self-)employment. Each red dot is an individual RDD estimate for a particular age. To the left panel, we include zero earnings. The grey area represents the 95% confidence interval the estimate. Each Ages uses a different cohorts, and therefore a different number of observations. The local linear regression discontinuity estimates are based on a 30-day bandwidth at both sides of the threshold.

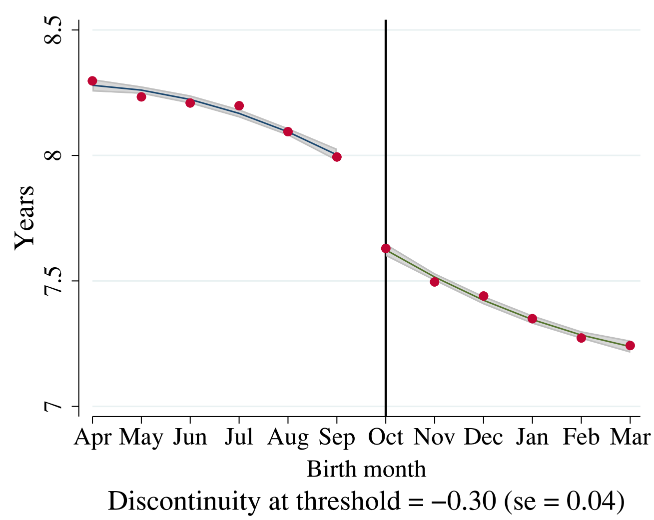

Fig. 7. Labor market experience.
Notes: Labor market experience is measured as the number of years a person earns above minimum wage at age 30. Each (red) dot is a monthly average. The grey area represents the 95% confidence interval of a second-degree polynomial regression, separately estimated left and right of the cut-off. The figure uses the cohort from April 1981 to March 1986, 853,974 observations. The local linear regression discontinuity estimate is based on a 30-day bandwidth at both sides of the threshold and uses 99,439 observations.

Figure 6 reports estimates of the month of birth effect on earnings for each separate age from 18 to 65. At age 18 the earnings gain for people born in September amounts to 212 euro and peaks at age 25 with 775 euro. After that, the earnings difference becomes smaller and is after age 40 often not significantly different from zero. This is partly due to the larger standard errors for older age groups. The peak at age 25 corresponds with a 5% difference in earnings. Between age 30 and 40, the difference is 1%. Combining the estimates of different ages, we can

Table 4
The main outcomes for different social-economic groups

	SES			Ethnicity		Gender	
	Low (1)	Middle (2)	High (3)	West. (4)	Non-W. (5)	Male (6)	Female (7)
Grade 9 = College or University	0.021*	0.048***	0.041***	0.045***	0.018*	0.032***	0.050***
	(0.008)	(0.005)	(0.007)	(0.004)	(0.010)	(0.006)	(0.006)
Mean	0.368	0.408	0.613	0.478	0.330	0.430	0.481
College or University degree	0.015	-0.003	0.001	0.006	-0.017	0.002	0.003
	(0.009)	(0.006)	(0.009)	(0.005)	(0.013)	(0.006)	(0.007)
Mean	0.292	0.149	0.553	0.401	0.311	0.340	0.441
Earnings at 30 in 1000s	-0.299	-0.268	-0.765***	-0.370***	-0.683	-0.573***	-0.249
	(0.239)	(0.149)	(0.255)	(0.121)	(0.413)	(0.172)	(0.143)
Mean	25.190	27.574	32.438	28.690	23.894	33.465	22.886

Notes: Each regression takes a 30 day interval on each side of the cut-off. Grade 9 track is the track a student attends in the third year of secondary school. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$

calculate the net present value of being born in October instead of September. At a discount rate of zero this value equals -10,970 euro (s.e. 2,599), this shrinks to -5,013 euro (s.e. 1,150) at a discount rate of 0.05.

The likely explanation for the negative effects of being born in October on earnings is that those who start school at an older age will – given the same education attainment – also leave the education system at an older age and therefore enter the labor market at an older age. The earnings difference between people born at different sides of the threshold would then reflect the return to the additional labor market experience that people born in September have in comparison to people born in October.

To inquire whether a difference in labor market experience can indeed explain the difference in earnings, we first estimate the effect of being born in October instead of September on experience at age 30 and then regress earnings at age 30 on experience (squared) and years of schooling. Experience is measured as the amount of time that someone has at least earned the full-time minimum wage, and years of schooling is the nominal duration of the highest level attained. Figure 7 shows that being born in September instead of October raises experience by 3.6 months (0.3 of a year).

Table F1 in Appendix F shows estimates of a Mincer earnings function for 30-year-olds. Using the experience coefficients from column (2) in Table F1, we find that 0.3 years of experience increase earnings by 250 euro. The reduced form effect of being born after instead of before the threshold on earnings at age 30 equals 410 euro. While not identical these results suggest that experience is indeed the channel through which month of birth affects earnings.

6. Heterogeneity

The effects of being born before or after the school starting date may differ between different groups of students. For instance, high and low SES families may have different capacities to overcome adverse shocks, migrant students may find it harder to navigate the educational system, and girls may react differently to placement in a lower track than boys.

Columns (1) to (3) of Table 4 show that birth month effects on education outcomes are very similar for children from different SES groups. This is somewhat surprising as we could suspect that high SES families are better in responding to an educational shock.[12] The birth month effects on earnings at age 30 differ across SES groups. For people from low and middle SES families, there is no significant effect of birth month on earnings, while this effect is substantial for people raised in high SES families. People from high SES families are likely to have steeper experience profiles than people from middle and low SES families.

Column (4) and (5) report effects by ethnicity. These show that for children from non-western descent, the impact of birth month on track placement is smaller, and when born in October, they are somewhat less likely to obtain a college or university degree. With regards to earnings, results are less clear, mostly due to the higher variance in the earnings of people from non-western descent.

Columns (6) and (7) show that the effects of being born in October instead of September on placement in 9th grade and on the final degree obtained are similar for boys and girls. Only the effect on earnings at age 30 is twice as high for men than for women, which reflects the gender earnings gap at this age.

7. Conclusion

The formation of school classes based on birth cohorts implies that within a class children differ up to one year in age. A large literature has shown that older students do better in school than younger students. The sparse evidence on the long-term effects of month of birth indicates that these effects are quite small. This evidence is, however, limited to settings where ability tracking occurs relatively late.

We find that also in a setting with early ability tracking, being born just after instead of just before the entrance threshold has only small effects on long-term outcomes. There is a substantial effect on students' achievement at the end of primary school and on the subsequent placement in ability tracks. This gap in favor of older students, diminishes gradually during the next education stages and there is no effect on the probability to obtain a college or university degree. People born in September enter the labor market on average four months earlier than people born in October of the same year. This gives those born in September at any given age more labor market experience and, until age 40, higher earnings than those born in October. Our findings show that early ability tracking does not need to harm the long-term outcomes of children who for exogenous reasons were placed in a lower track, provided that the system offers students a second chance (cf. Dustmann, Puhani, & Schönberg (2017), Biewen & Tapalaga (2017) and Biewen & Thiele (2020)).

Appendix A

[12] Fredriksson, Öckert, & Oosterbeek (2016), for instance, show that only high-income parents spend more time helping their children with their homework in response to a larger class size.

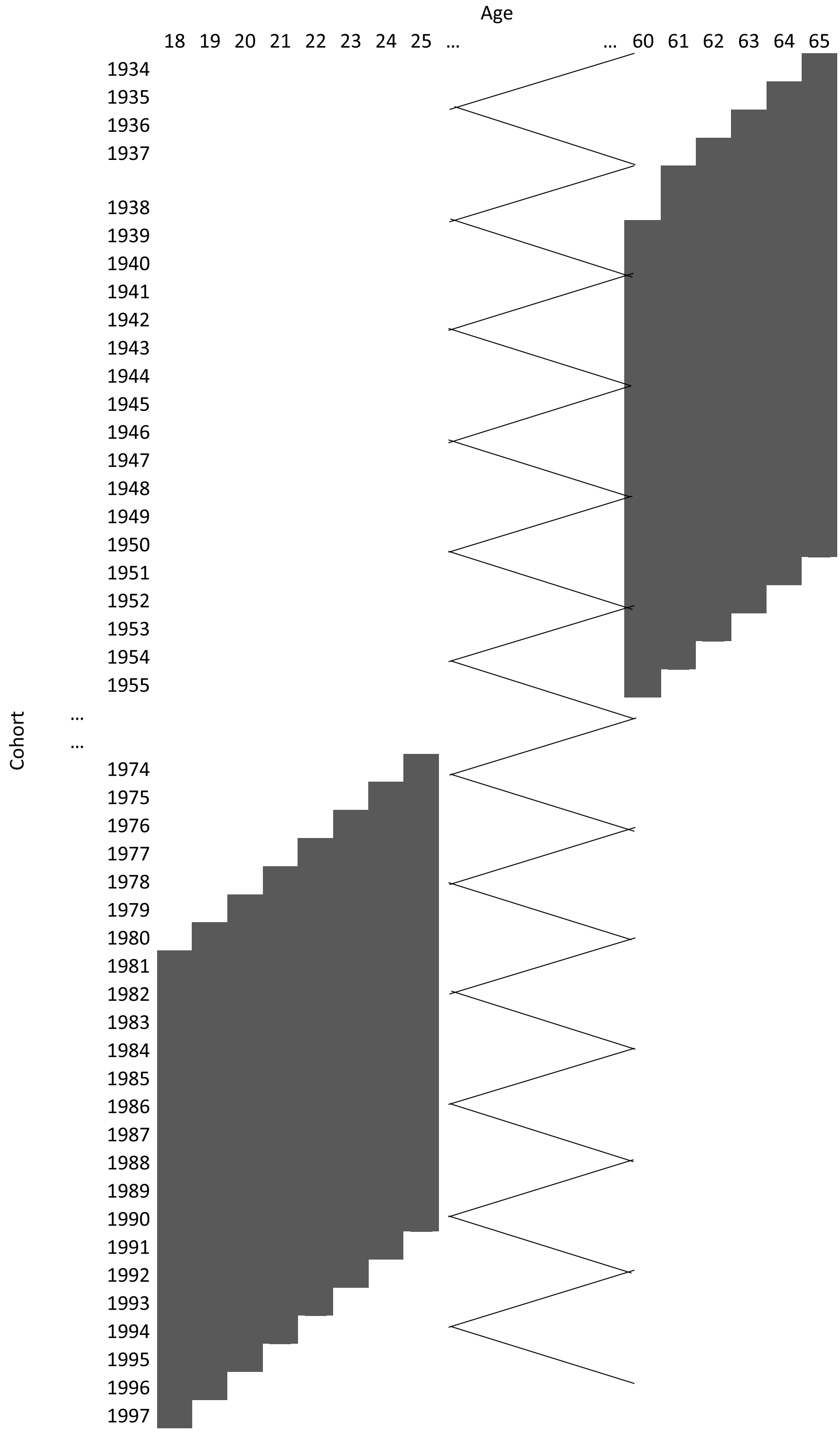

Fig. A1. Earnings data.
Notes: The shaded area represents one cohort (vertical axis) of observations available for earnings at the age that is specified on the horizontal axis.

Appendix B

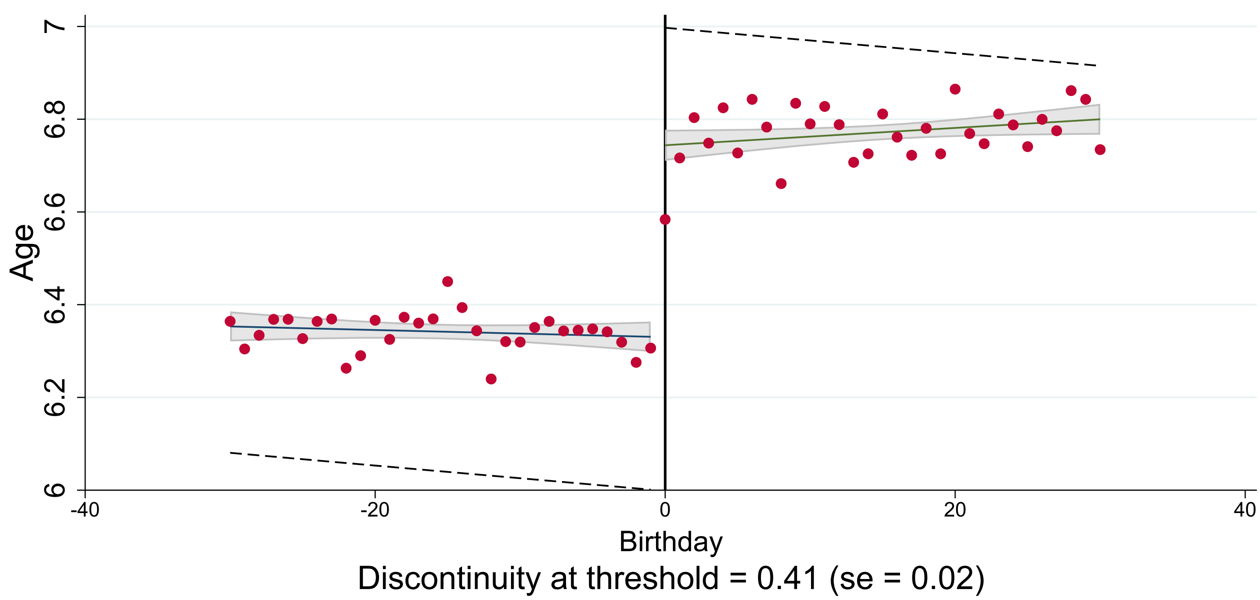

Fig. B1. Entry to grade 1 - daily bins.
Notes: Data from Prima cohort survey 1998 and 2000, 5,747 observations. The red dot represents the daily average, the two solid lines represent the best fit from a linear regression from each side of the cut-off. The grey area is the 95% confidence area of the solid line. The dashed line indicates what the starting age would have been in case of full compliance. The graph and the local linear regression discontinuity take a 30-day bandwidth at both sides of the threshold.

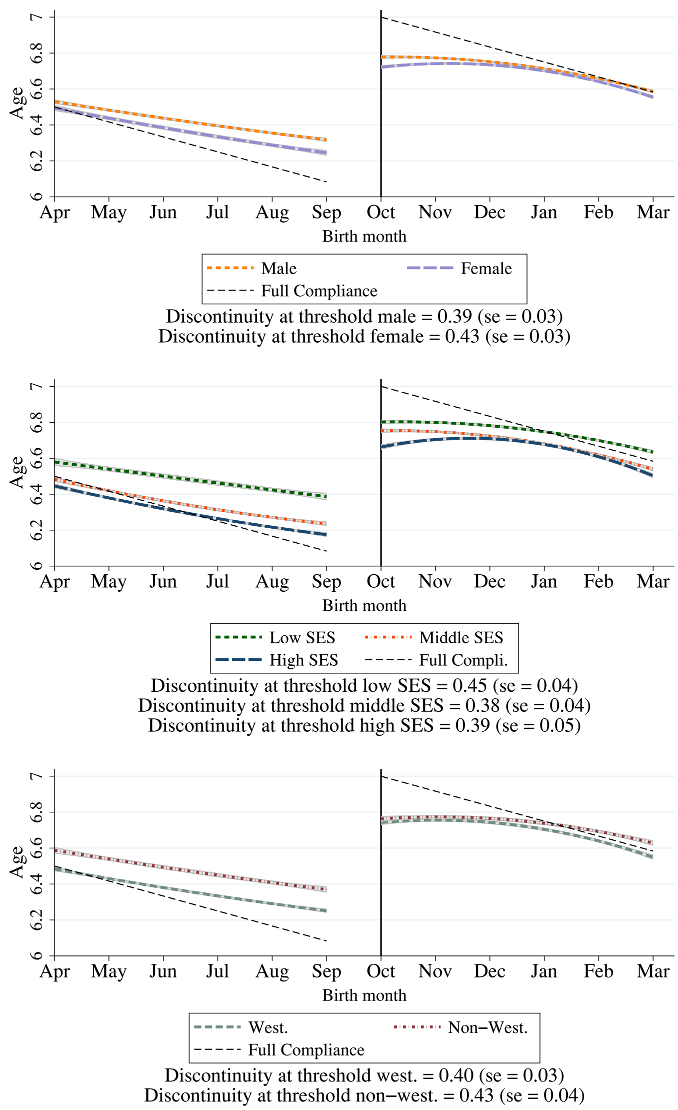

Fig. B2. Starting age by birth month - heterogeneity.

Notes: Data from Prima cohort survey. Bi-annual cohorts born between April 1988 to March 1995, 94,935 observations. The curved lines represent the best fit from a quadratic regression from each side of the cut-off. The grey area is the 95% confidence area of the solid line. The linear black dashed line indicates what the starting age would have been in case of full compliance. The regression discontinuity printed below the figures use a 30-day bandwidth at both sides of the cut-off. The local linear estimates use data from Prima cohort survey 1998 and 2000, 5,704 observations.

Appendix C

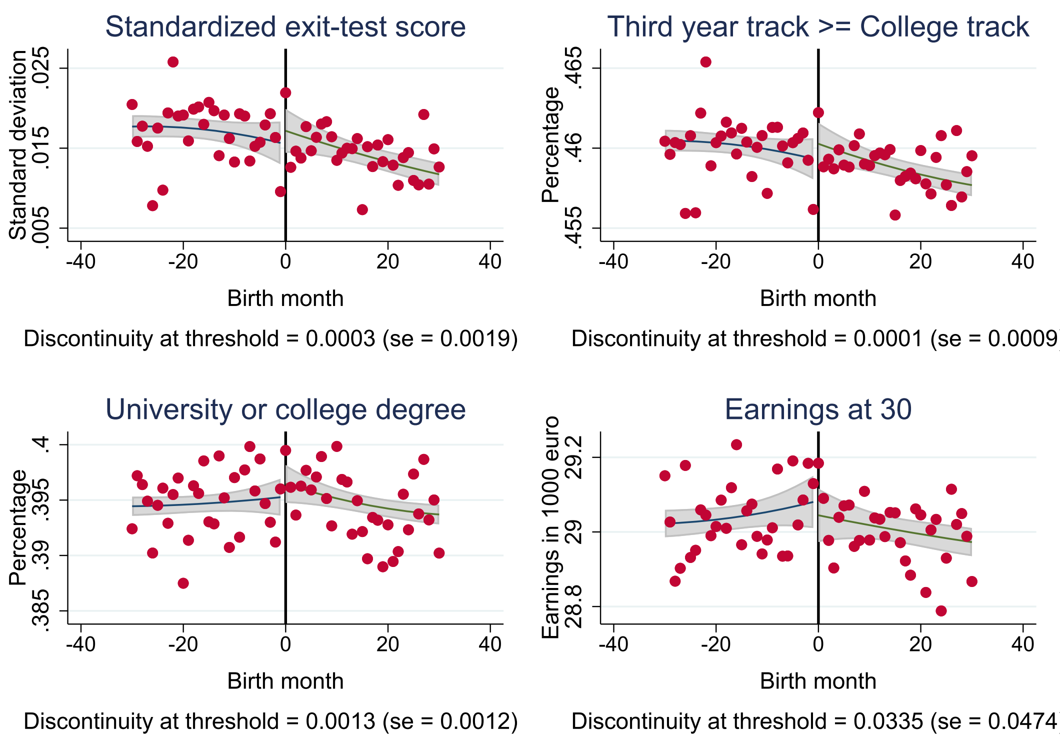

Fig. C1. Exogeneity test based on predicted outcomes.

Notes: Predicted outcomes are based on a model with birth year fixed effects, the student characteristics High-SES, female, non-western descent, age of father at birth and all interactions between these student characteristics. The left upper panel shows the predicted exit-test scores based on the four months around the first of October for cohorts born between 1994 and 2002, 264,103 observations. The right upper panel shows this relationship for the third-year track position of the students. This panel uses the same observations as the left upper panel. The left lower panel shows the relationship between the predicted university and college degrees and the cut-off. This panel uses the four months around the cut-off for cohorts born between 1983 and 1989, a total of 175,033 observations. The right lower panel shows predict deflated earnings at 30 around the cut-off. This panel uses 224,684 observations from cohorts born between 1969 and 1985. Earnings are income from (self-)employment. We include zero earnings in the average in this panel. Each red dot is a daily average. The grey area represents the 95% confidence interval of a second-degree polynomial regression, separately estimated left and right of the cut-off.

Appendix D

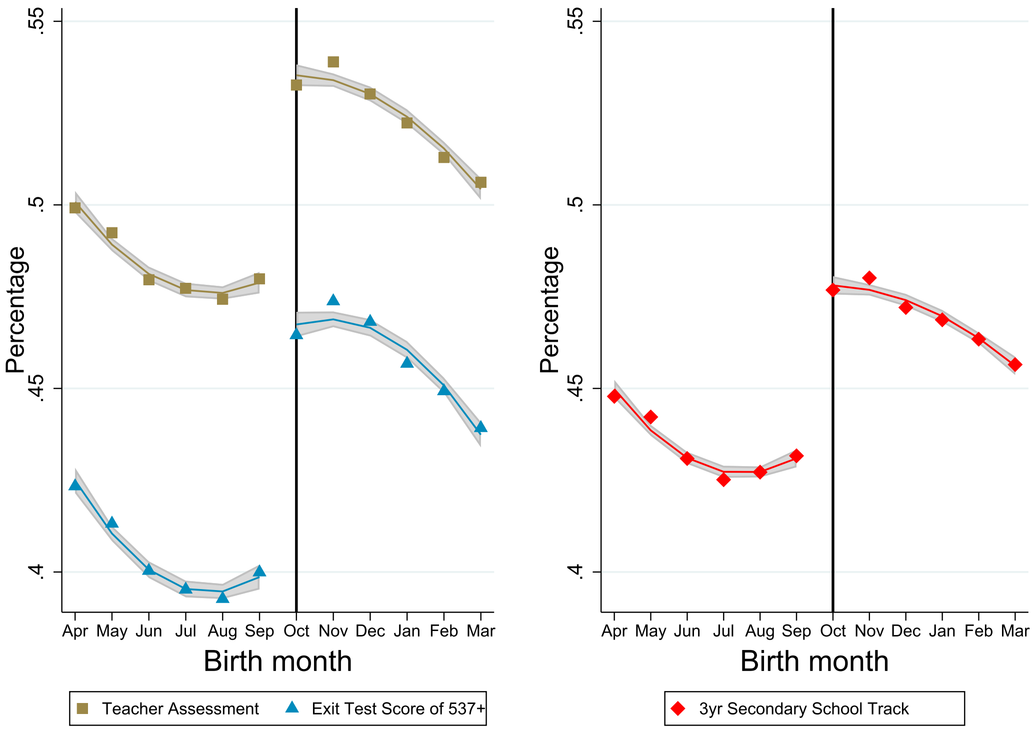

Fig. D1. Teacher assessment, exit test, and actual third year secondary school track - Vocational track against College and University Track.
Notes: The left figure shows whether the teacher assessment is college or university track or not and the probability that the exit-test score is equal to college or university education assessment or not. The right panel shows the probability that the track allocation in the third year of secondary school (grade 9) is equal to college or university track. Both panels use the 1994 -2002 birth-cohorts.

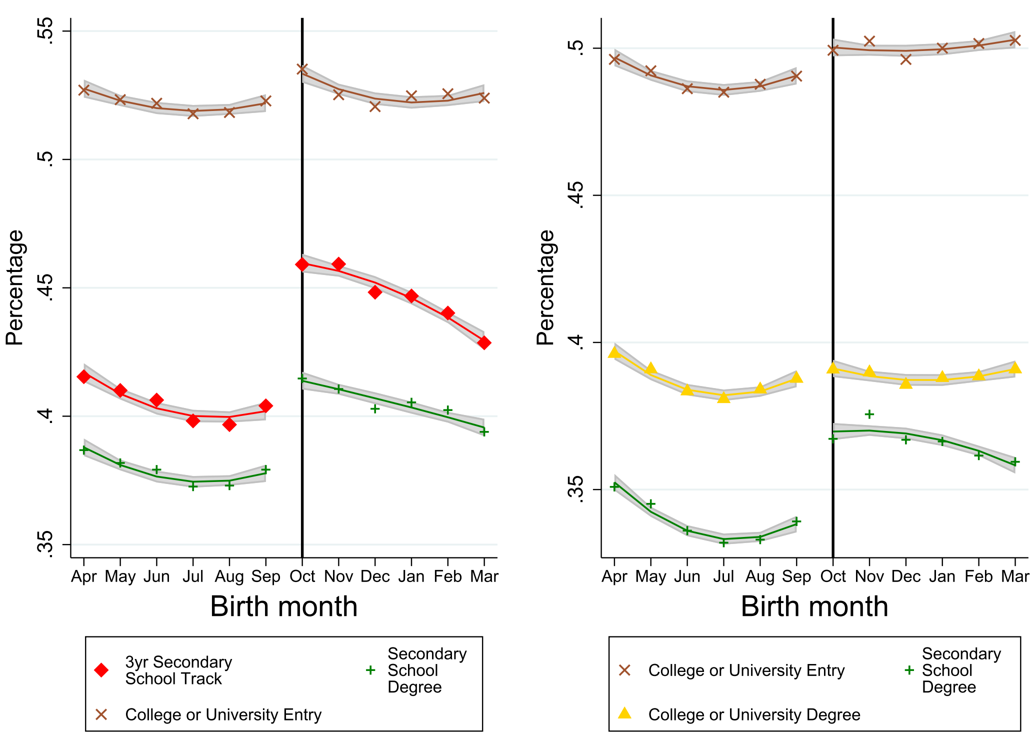

Fig. D2. Third Year Secondary School Track to Final degree - Vocational track against College and University Track.
Notes: The left panel shows the probability that the track allocation in the third year of secondary school (grade 9) is equal to college or university track, the probability of a college or university track secondary school degree, and the probability that a students attends a college or university. This panels uses the 1989 - 1993 birth-cohorts. The right panel shows probability of a college or university track secondary school degree, the probability that a students attends a college or university and the probability that the student walks away with a college or university degree. This panel uses the 1983 to 1989 cohort.

Appendix E

Table E1
Educational career: university education vs college/vocational education

	$\hat{\delta}$			Optimal bandwidth	Mean	Cohort
	Bandwidth					
	14 days	30 days	Opt.			
Standardized Exit-test score	0.082 ***	0.097***	0.085***	16 days	0.000	1994 - 2002
	(0.013)	(0.009)	(0.013)			
Exit-test score = University	0.033***	0.037***	0.035***	17 days	0.186	1994 - 2002
	(0.005)	(0.004)	(0.005)			
Teacher Assessment = University	0.031***	0.036***	0.035***	23 days	0.141	1994 - 2002
	(0.005)	(0.004)	(0.004)			
Grade 9 track = University	0.032***	0.037***	0.032***	14 days	0.221	1994 - 2002
	(0.005)	(0.003)	(0.005)			
	0.033***	0.036***	0.036**	20 days	0.212	1989 - 1993
	(0.007)	(0.005)	(0.006)			
School diploma = University	0.017**	0.021***	0.019***	23 days	0.176	1989 - 1993
	(0.006)	(0.004)	(0.005)			
Enrollment into University	0.007	0.011*	0.010	19 days	0.191	1989 - 1993
	(0.006)	(0.004)	(0.005)			
	0.006	0.006	0.006	22 days	0.186	1983 - 1989
	(0.005)	(0.004)	(0.004)			
University degree	0.009	0.008*	0.007	21 days	0.151	1983 - 1989
	(0.005)	(0.003)	(0.004)			

Notes: The number of observations for the 30 and 14-day bandwidths, respectively are 181,096 and 83,068 for the 1994 - 2002 cohort, 130,716 and 60,042 for the 1989 - 1993 cohort, and 175,898 and 127,275 for the 1983 -1989 cohort. The optimal bandwidths are calculated using the algorithm developed in Imbens & Kalyanaraman (2012). Column *Opt.* uses the bandwidths that are displayed in the column *Optimal bandwidth*. The *Mean* of the outcome variable is calculated over all birthdays in the cohort range. The exit test score is standardized such that it has a mean of zero and a standard deviation of 1. An exit test score that is equal to an university track assessment is a score of 545 or higher. Grade 9 track is the track a student attends in the third year of secondary school. A school diploma is a diploma that a student obtains in secondary school. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$

Appendix F

Table F1
Earnings equation for 30 year olds

Outcome	Log Earnings at 30	Earnings at 30
Years of education	0.060***	2,247.67***
	(0.000)	(9.95)
Experience	0.071***	2,724.50***
	(0.001)	(31.17)
Experience^2	-0.003***	-121.77***
	(0.000)	(2.07)

Notes: Earnings are from employment or own employment. Years of education are the nominal years of education needed for the final degree. Experience is measured as the years with earnings above full-time minimum wage earnings. The table uses all available 30 year-olds from the cohort 1981-1986, 504,007 observations, constant included in regression. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.005$

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