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Long-Term Effects of School-Starting-Age Rules

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ABSTRACT

Long-Term Effects of School-Starting-Age Rules*

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 therefore have more labor market experience and higher earnings at any given 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.

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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; Elder & Lubotsky, 2009; Lubotsky & Kaestner, 2016; McEwan & Shapiro, 2008; Mühlenweg & Puhani, 2010; Puhani & Weber, 2008; Schneeweis & Zweimüller, 2014). A few studies have examined whether the early advantage of older students carries over to labor market outcomes in adulthood. Black, Devereux, and Salvanes (2011) and Larsen and Solli (2017) find only modest effects on long-term outcomes in Norway, Fredriksson and Öckert (2013) conclude that prime-age earnings are unaffected in Sweden, and Dobkin and Ferreira (2010) find no effects on wages and employment in two states in the US (Texas and California).

A possible reason for the absence of long-term effects in Norway, Sweden, and the US is that these countries 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.

The closure of the gap between the oldest and youngest students in a cohort can be at-

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[1] Other countries that use early ability tracking include Austria, Belgium, Czech Republic, Germany, Hungary, Turkey and Singapore (Organisation for Economic Co-operation and Development, 2013).

tributed to large streams between the vocational track and the college and university tracks. Over 80% of the initial gap is closed due to students streaming up from the vocational education 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: Dutch educational system
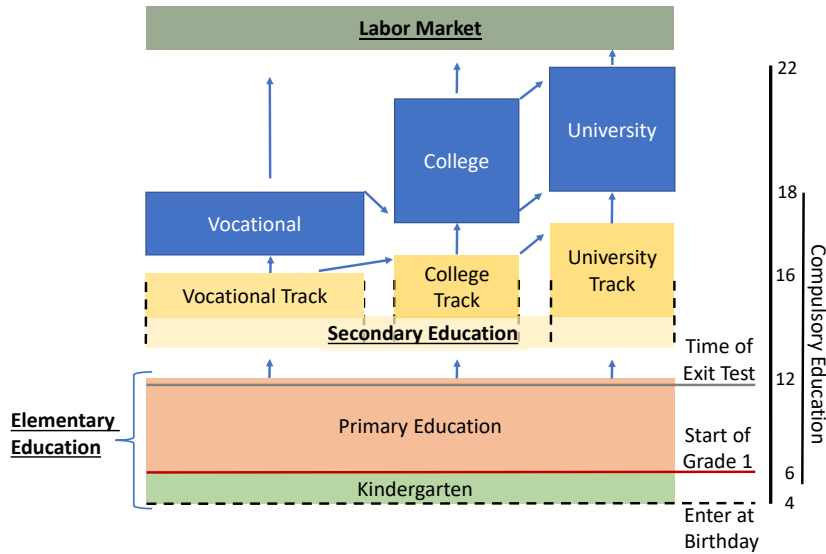
Note. The vertical bar on the right indicates students' age at nominal study duration. Arrows represents possible upward transitions.

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. School 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. 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 also 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.

3 Data

The data from Statistics Netherlands allow us to construct a detailed picture of the effects of the month of birth during a person's lifetime. Statistics Netherlands gathers administrative records from different agencies. The basis of our compiled data set comes from the municipalities who record the birth date of each person and to which family s/he belongs, to these records, we match the rest of the data sets on the basis of a unique identifier. From the records of the testing bureau, we get the performance on the exit test from primary school, and the track recommendation from the primary school teacher. The enrollment data from the ministry of education gives us the third year secondary education track and the tertiary education entry. The degree registry from the ministry of education gives us the secondary education diploma(s) and tertiary education degree(s). Using the tax records from the tax authority, we compile the earnings before taxes for the ages from 18 to 65.

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[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).

Because data from education and tax records are only available from 1995 onward, there are no birth cohorts for which we observe the complete sequence of outcomes. Figure 2 pictures which outcomes we observe for which birth cohorts. The horizontal axis in Figure 2 indicates birth cohorts, and the orange line shows if the outcome displayed on the vertical axis is available. In the results section, we will inquire whether effects on the same outcome are homogeneous across (groups of) cohorts. That is, we will examine whether the effects of birth month on track in grade 9 differ for cohorts 1994-2002 and cohorts 1989-1993. Likewise, we will examine whether effects on enrollment in tertiary education differ for cohorts 1989-1993 and cohorts 1983-1989.

The administrative records from the municipality link children to their parents. This gives information about the social-economic status (SES) and ethnicity. We construct an SES-indicator based on the parental earnings distribution of 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 birthplace of the parents. If one of the parents is born in a non-western country, the child is labeled as from non-western descent. We restrict the sample to children born in the Netherlands such that we are sure that the cut-off rule applies.

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 from 1994 to 2006. All surveys have information on birth month, but only the 1998 and 2000 surveys have information on the birthday of children.

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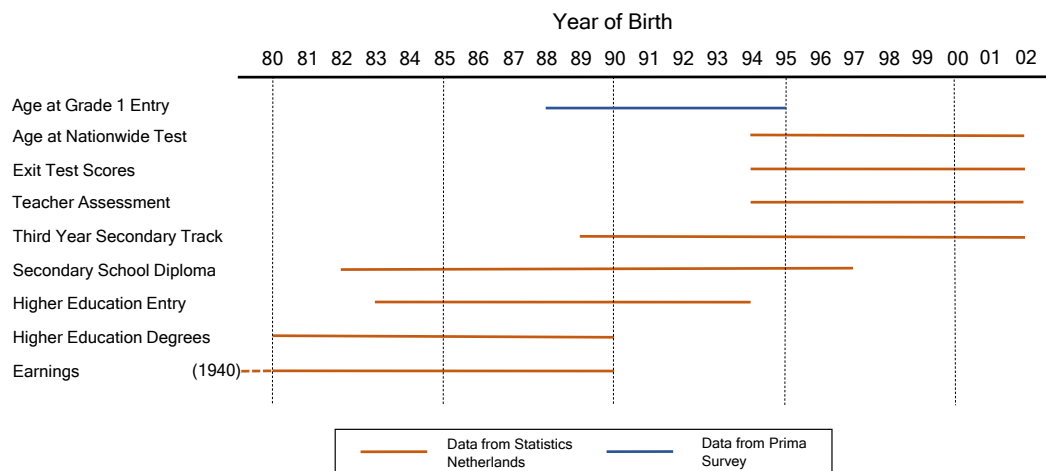
Note. The horizontal bars represent the data availability of birth cohorts for the variable specified on the vertical axis. Earnings data is available for birth cohorts starting in 1940.

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 for individual i from cohort t and being born in October instead of September (Old_i).

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

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 1st.

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 and Öckert (2013)) or from a regression of relative age in class on being born in October (as in Bedard and Dhuey (2006)). We therefore have a sharp regression discontinuity 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 and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014), we find that the optimal bandwidth (almost always) lies between 14 and 30 days on both sides. Therefore, we present RDD-estimates based on 14-days, 30-days, and optimal bandwidths in the main text. In the graphs, we will also show results for all birth months.

The interpretation of estimates of δ in Equation (1) as causal effects of being old when supposed to start school depends on the assumption that birth dates are as good as randomly assigned relative to the threshold. To assess this assumption, 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; $p = 0.31$ for the one month window and $p = 0.45$ for the two weeks window.

Figure 3 shows the density plots of birthdays. The left-hand plot shows the density of births over the entire year. It shows that there is some seasonal variation in the number of births over the months, but not around the threshold. The right-hand plot shows the density of birthdays in the four months surrounding October 1st. It shows that the density of birthdays seems smooth around the threshold. However, using a McCrary test, we find a discontinuity of 0.045 (in log differences) with a standard error of 0.006. In Appendix B, we show that the McCrary test also finds discontinuities on 14 other days in the four months around the cut-off. If we

apply the McCrary test on separate years, we only find significant results in 3 out of the 24 years.

Figure 3: Distribution of Birthdays
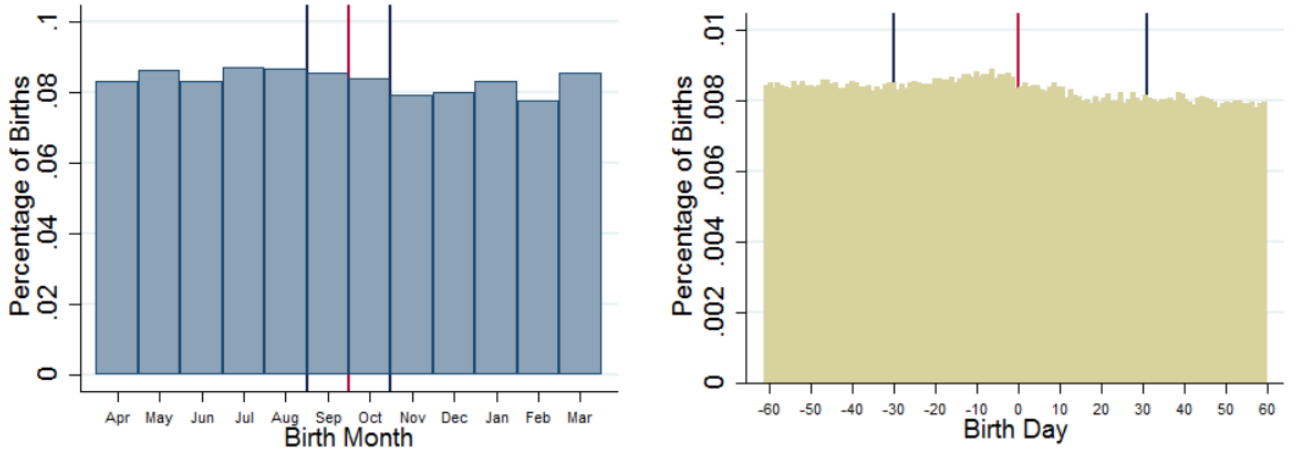
Note. The left-hand panel shows the distribution over the entire year. Each bar represents the percentage of births in that month. The right panel shows the distribution of births in the 4 months around October 1st. Each bar in this panel represents the percentage of births each day. Both panels are based on the cohorts born in the Netherlands between April 1977 and March 2002

Table 1: Balancing table

	Fraction women	Fraction SES	Fraction Non-West.	Age of Father
	30 day interval on both sides of the cut-off			
Old_i	0.0041	−0.0005	−0.0014	−0.0104
	(0.0022)	(0.0019)	(0.0015)	(0.0233)
Obs.	786.579	786.579	786.579	786.579
Birth year fixed effects	√	√	√	√
	14 day interval on both sides of the cut-off			
Old_i	0.0032	−0.0010	−0.0029	−0.0161
	(0.0033)	(0.0029)	(0.0024)	(0.0343)
Obs.	367.274	367.274	367.274	367.274
Birth year fixed effects	√	√	√	√

Note. 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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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.

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. Students born in July to September postpone enrollment in grade 1 and children born in October and November start one year earlier than they are supposed to.[4] 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.

Figure A2 in Appendix A 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.[5] The results focus on the probability to enter or finish college or

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[4]Figure A1 in Appendix A depict the daily average starting age around the threshold for children born in September and October.

[5]The corresponding graphs are presented in Appendix C.

Figure 4: School starting age by month of birth

Discontinuity at threshold = 0.41 (se = 0.02)
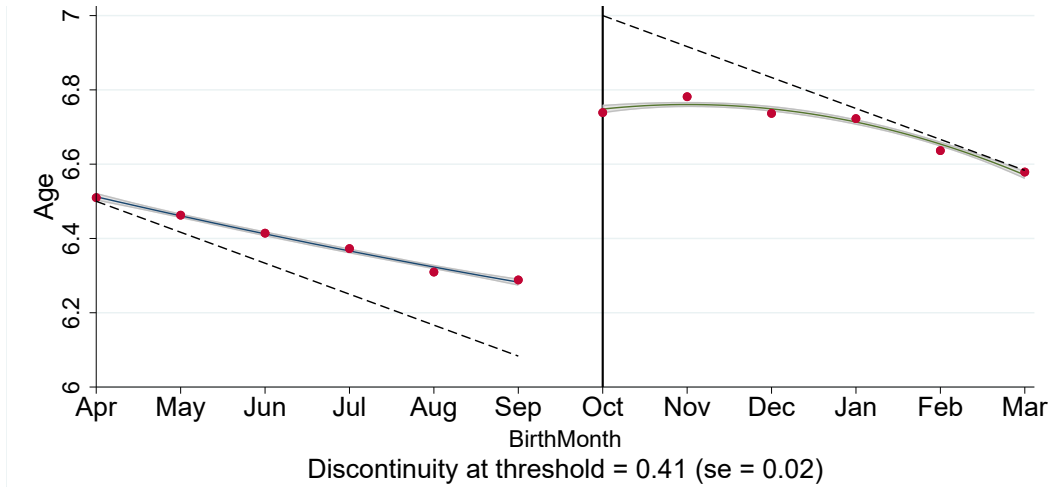
Note. Data from Prima cohort survey. Bi-annual cohorts were born between April 1988 to March 1995, 94,935 observations. The red dot represents the monthly average, and the two solid lines represent the best fit from a second-degree polynomial regression from each side of the cut-off. Although hardly 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.

university education instead of vocational education.[6] 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 3.7 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%.[7] 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.

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 4 percentage points.[8] Because teachers know students month of birth (birthdays are celebrated in school), they can in principle adjust for students' age-at-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.

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[6]Appendix D reports results for the university education cut-off. These results are similar to the results discussed in this section.

[7]For university education this is 3.1 to 3.6 on a base rate of 18.5 %, see row 2 of Table D1

[8]"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".

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.

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 ($p = 0.000$). 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 1.4 percentage points.[9] 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.

To understand what drives the closure of the initial gap between students born in October and September, Table 3 analyzes the up and down switches that occur at different stages. Each row shows for a particular stage which percentages of the students born in September and October move up or down in the system. The penultimate column shows the difference in percentages between September and October.[10] The bottom two rows in the table report the totals of up and down transitions. This shows that 82% of the track changes that close the gap between students born in September and October are due to upward changes. We also see that transitions in later stages are more important for closing the gap. This latter finding 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.

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 in before October 1st earn 409 euro's per year more than people born after the threshold. This annual difference shrinks to 288 euro's at age 40 and has completely disappeared at ages 50 and 60.

Table E1 in Appendix E 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

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[9]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.

[10]The percentages of Table 3 do not exactly match the ones from Table 2. Table 2 uses an RD design whereas Table 3 shows simple averages.

Table 2: Educational career

	$\hat{\delta}$			Optimal bandwidth	Mean	Cohort
	Bandwidth					
	14 days	30 days	Opt.			
Standardized Exit-test score	0.078*** (0.009)	0.095*** (0.014)	0.081*** (0.014)	15 days	0.000	1994 - 2002
Exit-test score = College or University	0.037*** (0.007)	0.048*** (0.005)	0.037*** (0.007)	15 days	0.503	1994 - 2002
Teacher Assessment = College or University	0.047*** (0.008)	0.054*** (0.004)	0.054*** (0.007)	18 days	0.431	1994 - 2002
Grade 9 track = College or University	0.037*** (0.007)	0.042*** (0.006)	0.033*** (0.007)	12 days	0.427	1994 - 2002
	0.043*** (0.009)	0.044*** (0.006)	0.041*** (0.008)	19 days	0.405	1989 - 1993
School diploma = College or University	0.019*** (0.009)	0.024*** (0.006)	0.022*** (0.007)	19 days	0.392	1989 - 1993
Enrollment into College or University	0.003 (0.008)	0.008* (0.006)	0.005 (0.007)	19 days	0.521	1989 - 1993
	0.010 (0.007)	0.013*** (0.005)	0.013** (0.006)	22 days	0.480	1983 - 1989
College or University degree	0.007 (0.005)	0.003 (0.007)	0.008 (0.007)	17 days	0.384	1983 - 1989

Note. 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 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 number of observations for the optimal bandwidth varies with the number of days. The optimal bandwidths are calculated using the algorithm developed in Imbens and Kalyanaraman (2012). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Ability track changes

Track change	Transition From	To	Sep. %	Oct. %	Diff. %	Cohort
Up	Exit test = vocational track	Grade 9 track = college/university track	6.93	6.27	0.66	1994-2002
Down	Exit test = college/university track	Grade 9 track = vocational track	8.73	8.76	-0.03	1994-2002
Up	Grade 9 track = vocational track	Secondary School diploma = college/university track	4.24	3.21	1.03	1989-1999
Down	Grade 9 track = college/university track	Secondary School diploma = vocational track	1.43	1.79	-0.36	1989-1999
Up	Secondary School diploma = vocational track	Entry college/university	15.90	13.46	2.44	1989-1993
Down	Grade 9 track = college/university track	Entry vocational college	2.86	2.92	-0.06	1983-1989
Down		Not finishing college/university	11.66	12.09	-0.43	1983-1989
Total						
Up					**4.13**	
Down					**-0.88**	

Note. 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.

to 212 euros and peaks at age 25 with 775 euros. 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. At age 40, the difference is 1% (see column (4) in Table E1). Combining the estimates at different ages, we can calculate the net present value of being born in October instead of September. At a discount rate of zero this value equals -10,970 euros (s.e. 2,599), this shrinks to -5,013 euros (s.e. 1,150) at a discount rate of 0.05.[11]

Figure 5: Earnings at different ages
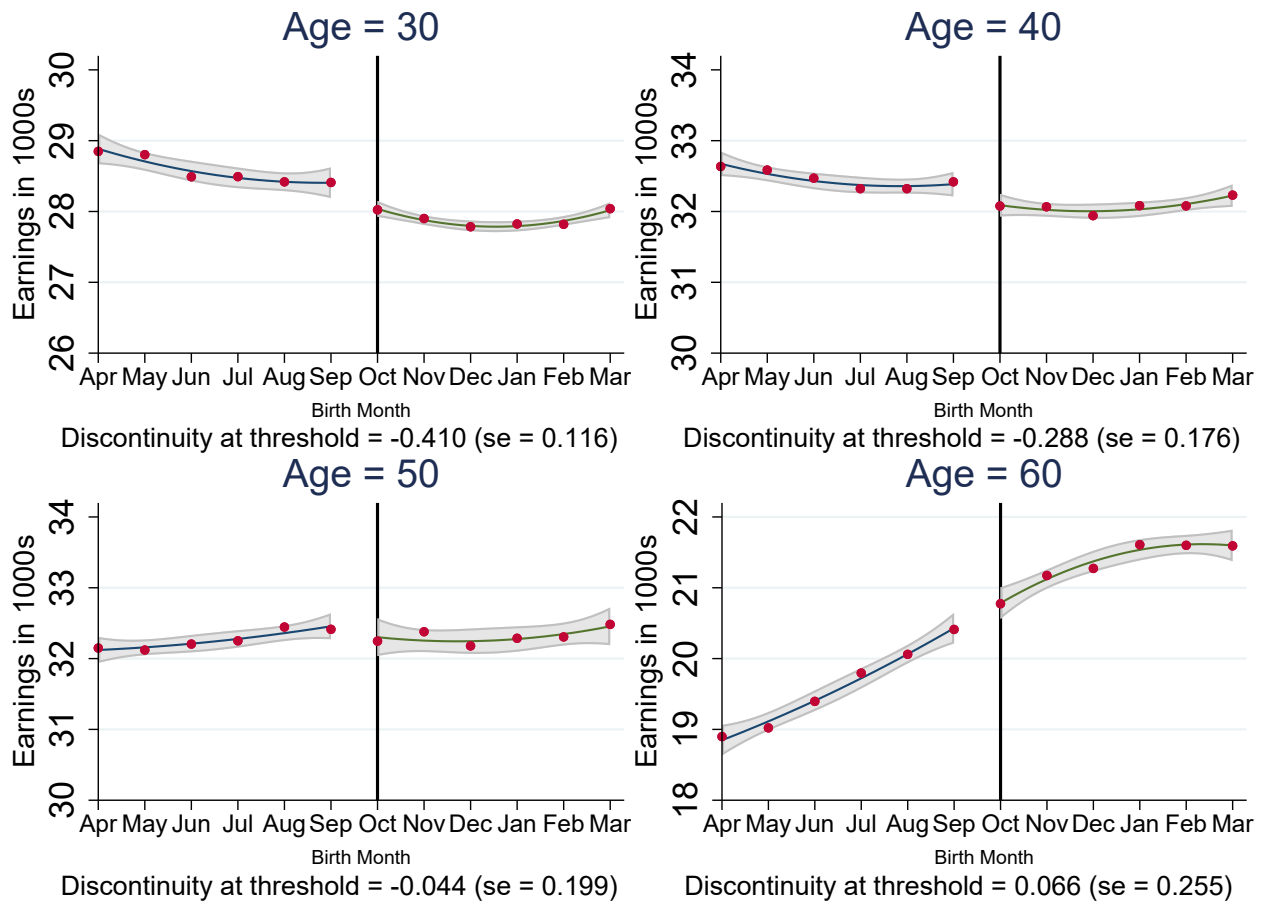
Note. Earnings are income from (self-)employment. Each red dot is a monthly average. We include zero earnings in this average. The grey area represents the 95% confidence interval of a second polynomial regression, separately estimated left and right of the cut-off. Each panel uses a different cohorts. The cohorts and the number of observations can be found in table E1. The local linear regression discontinuity estimates are based on a 30-day bandwidth at both sides of the cut-off.

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

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[11]The final column of Table E1 shows that there is no persistent effect of month of birth on the probability of having positive earnings.

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 compared 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 6 shows that being born in September instead of October raises experience by 3.6 months (0.3 of a year).

Figure 6: Labor market experience

Discontinuity at threshold = -0.30 (se = 0.04)

Note. 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 polynomial regression, separately estimated left and right of the cut-off. Both panels use the same cohort from April 1981 to March 1986, 512,590 observations. The local linear regression discontinuity estimate is based on a 30-day bandwidth at both sides of the cut-off.

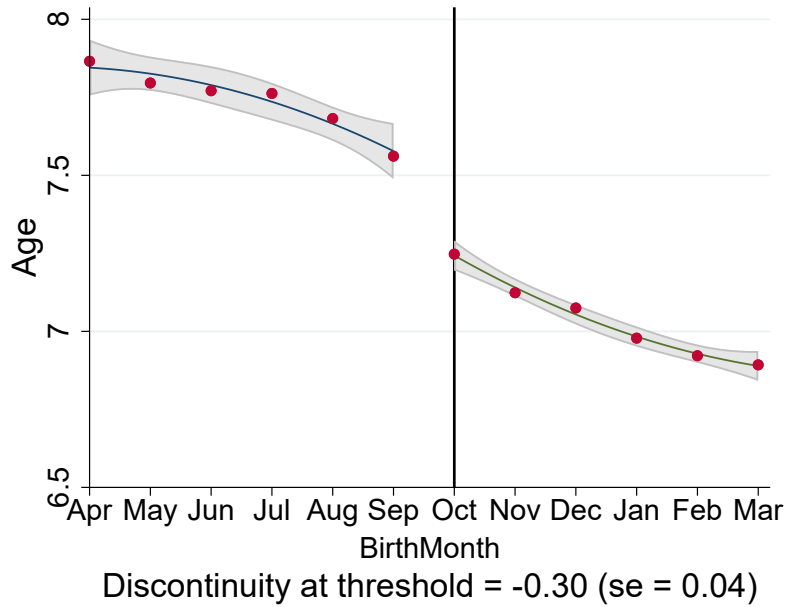Table 4 shows estimates of the earnings equation. Using the experience coefficients from column (2), we find that 0.3 years of experience increase earnings by 250 euros. The reduced form effect of being born after instead of before the threshold on earnings at age 30 equals 409 euros. While not identical these results suggest that experience is indeed the channel through which month of birth affects earnings.

Table 4: Earnings equation for 30 year olds

Outcome	Log Earnings at 30	Earnings at 30
Years of education	0.060***	2,248.45***
	(0.000)	(11.29)
Experience	0.070***	2,723.02***
	(0.001)	(27.18)
Experience^2	−0.003***	−122.00***
	(0.000)	(1.75)

Note. 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, 512,590 observations, constant included in regression.

6 Heterogeneity

The effects of being born before or after the school entrance 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, or males and females may react differently to placement in a lower track.

Columns (1) to (3) 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 are dissimilar for the effects on earnings at age 30. 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. If we would have expected that non-western families were less skilled in navigating the educational system[13], one should expect large positive effects on both outcomes. 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) of Table 5 show that the effects of being born in October instead of September on placement in 9th grade and on the final degree obtained are the same for boys and girls. There are no substantial gender differences in the effects. Only at age 30 the effect on earnings is twice as high for men than for women, which reflects the gender earnings gap at

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[12]Fredriksson, Öckert, and 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.

[13]These families might, for instance, might not know that with a GPA of at least X at stage Z, one is allowed into the higher track.

this age.

Table 5: 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	0.03*** (0.01)	0.05*** (0.01)	0.04*** (0.01)	0.04*** (0.00)	0.02* (0.01)	0.03*** (0.00)	0.05*** (0.00)
Mean	0.33	0.38	0.58	0.45	0.31	0.40	0.45
College or University degree	0.02* (0.01)	0.00 (0.01)	0.00 (0.01)	0.01 (0.01)	−0.02* (0.01)	0.00 (0.01)	0.00 (0.01)
Mean	0.25	0.29	0.49	0.34	0.23	0.30	0.36
Earnings at 30	−110.76 (251.36)	−261.97 (156.39)	−953.51*** (270.68)	−382.53*** (121.42)	−606.78 (413.73)	−573.41*** (172.76)	−256.16* (143.33)
Mean	25,204.93	27,704.01	32,696.00	28,657.11	23,808.93	33,410.82	22,858.91

Note. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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. Because people born just before the threshold were relatively young in school enter the labor market almost four months earlier, they have at any given age more labor market experience and therefore until age 40, higher earnings. Our findings show that early ability tracking does not need to harm the long-term outcomes of children that for exogenous reasons were placed in a lower track, provided that the system offers students a second chance (cf.

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Dustmann, Puhani, and Schönberg (2017) and Biewen and Tapalaga (2017)).

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Appendix A

Figure A1: Entry to grade 1 - daily bins
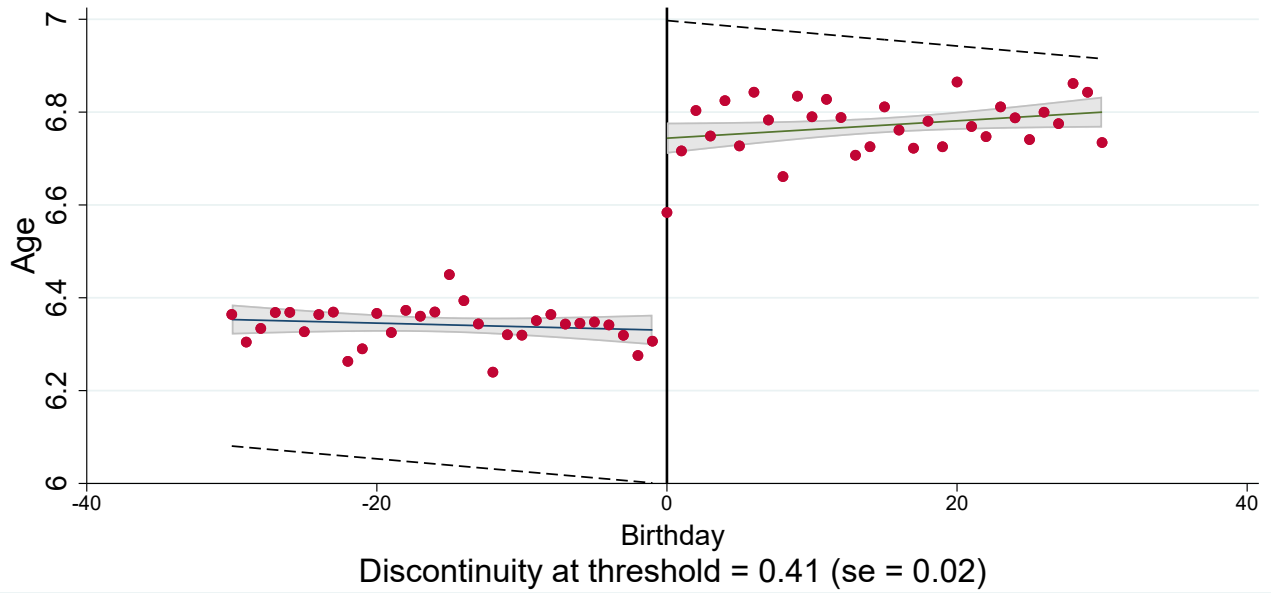
Note. Data from Prima cohort survey 1998 and 2000, 5,747 observations. The red dot represents the daily average, the two solid lines represent the best fit from a linear regression from each side of the cut-off. The grey area is the 95% confidence area of the solid line. The dashed line indicates what 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 cut-off.

Figure A2: Starting age by birth month - heterogeneity

(a) Gender

Discontinuity at threshold male = 0.39 (se = 0.03)
Discontinuity at threshold female = 0.43 (se = 0.03)

(b) SES

Discontinuity at threshold low SES = 0.45 (se = 0.04)
Discontinuity at threshold middle SES = 0.38 (se = 0.04)
Discontinuity at threshold high SES = 0.39 (se = 0.05)

(c) Ethnicity

Discontinuity at threshold west. = 0.40 (se = 0.03)
Discontinuity at threshold non-west. = 0.43 (se = 0.04)
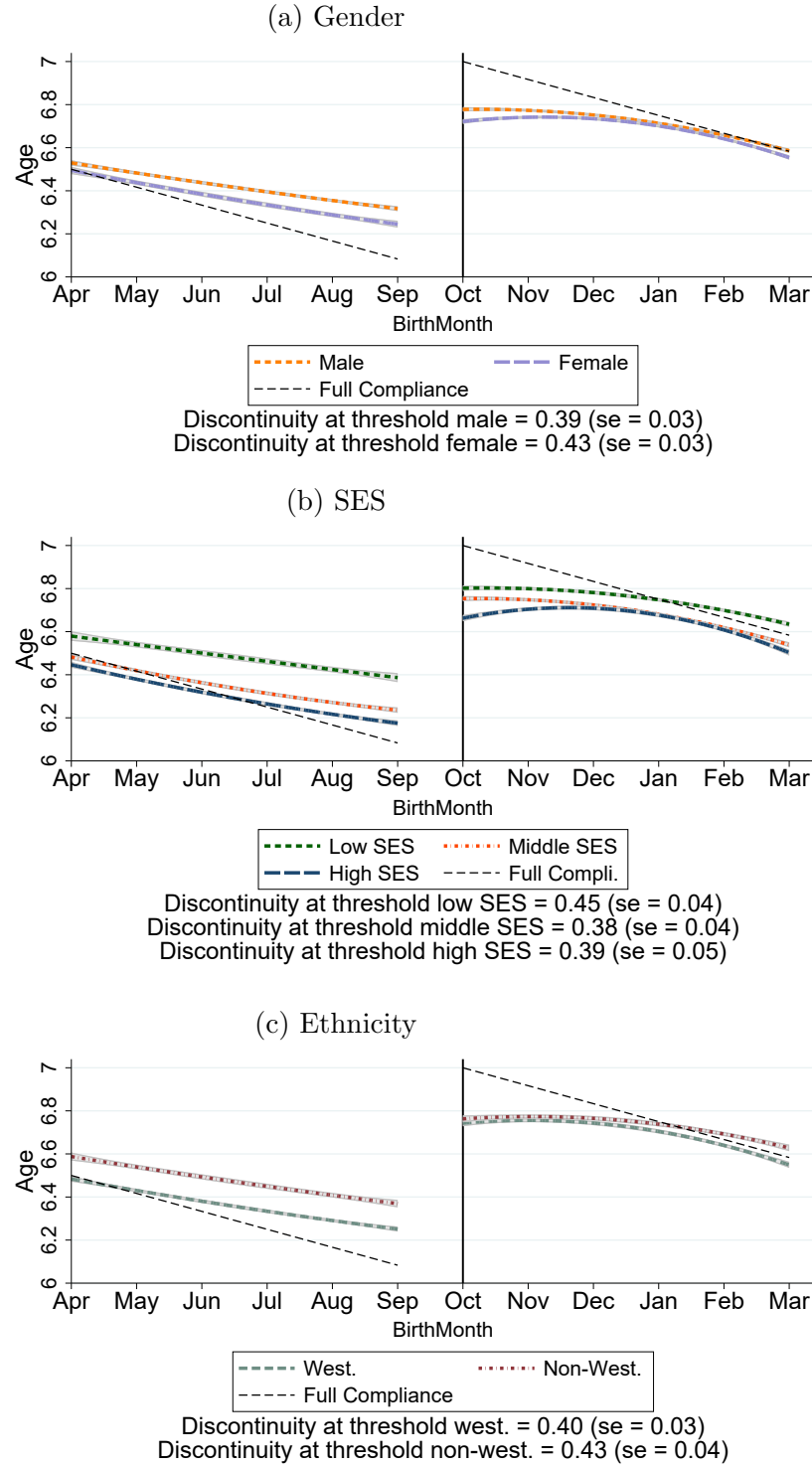
Note. Data from Prima cohort survey. Bi-annual cohorts born between April 1988 to March 1995, 94,935 observations. The curved lines represent the best fit from a quadratic regression from each side of the cut-off. The grey area is the 95% confidence area of the solid line. The linear black dashed line 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 B

Figure B1: McCrary test with different cut-off dates or on individual year
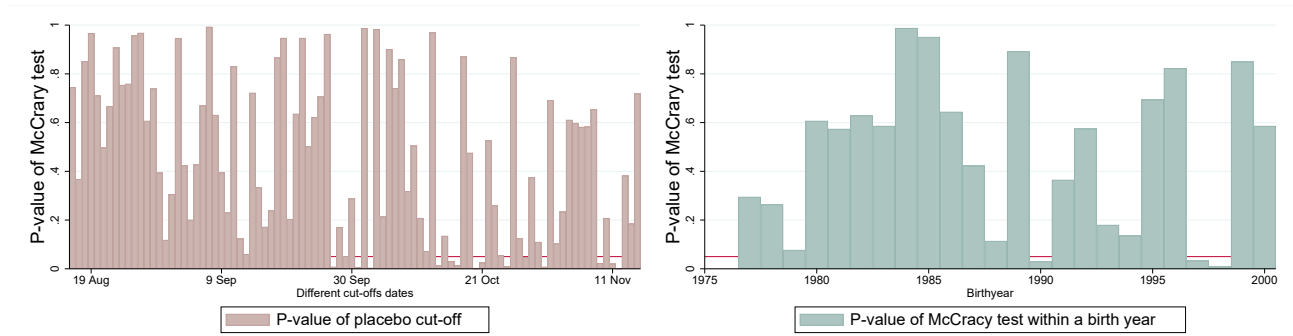
Note. The left panel shows that the first of October is not the only date at which the McCrary test finds a discontinuity. Each bar in this graph is the p-value from a McCrary test that has a different cut-off date. The cut-off dates are specified on the horizontal axis. Right panel shows that within a year, only in 3 out of 23 years the McCrary test finds a discontinuity. Each bar is a p-value from a McCrary test in each individual year. On average each McCrary test in this panel uses about 60,000 observations. The years used for the particular McCrary test are specified on the horizontal axis.

Appendix C

Figure C1: Teacher assessment, exit test, and actual third year secondary school track - Vocational track against College and University Track
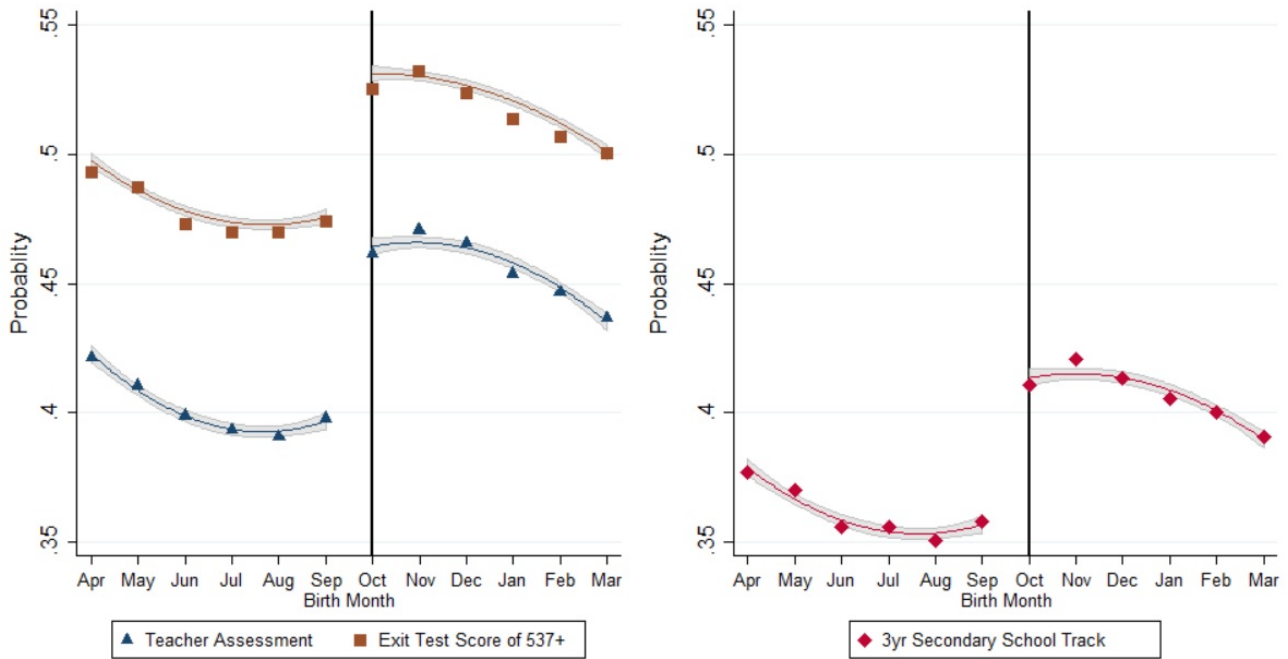
Note. The left figure shows whether the teacher assessment is college or university track or not and the probability that the exit-test score is equal to college or university education assessment or not. The right panel shows the probability that the track allocation in the third year of secondary school (grade 9) is equal to college or university track. Both panels use the 1994 -2002 birth-cohorts.

Figure C2: Third Year Secondary School Track to Final degree - Vocational track against College and University Track
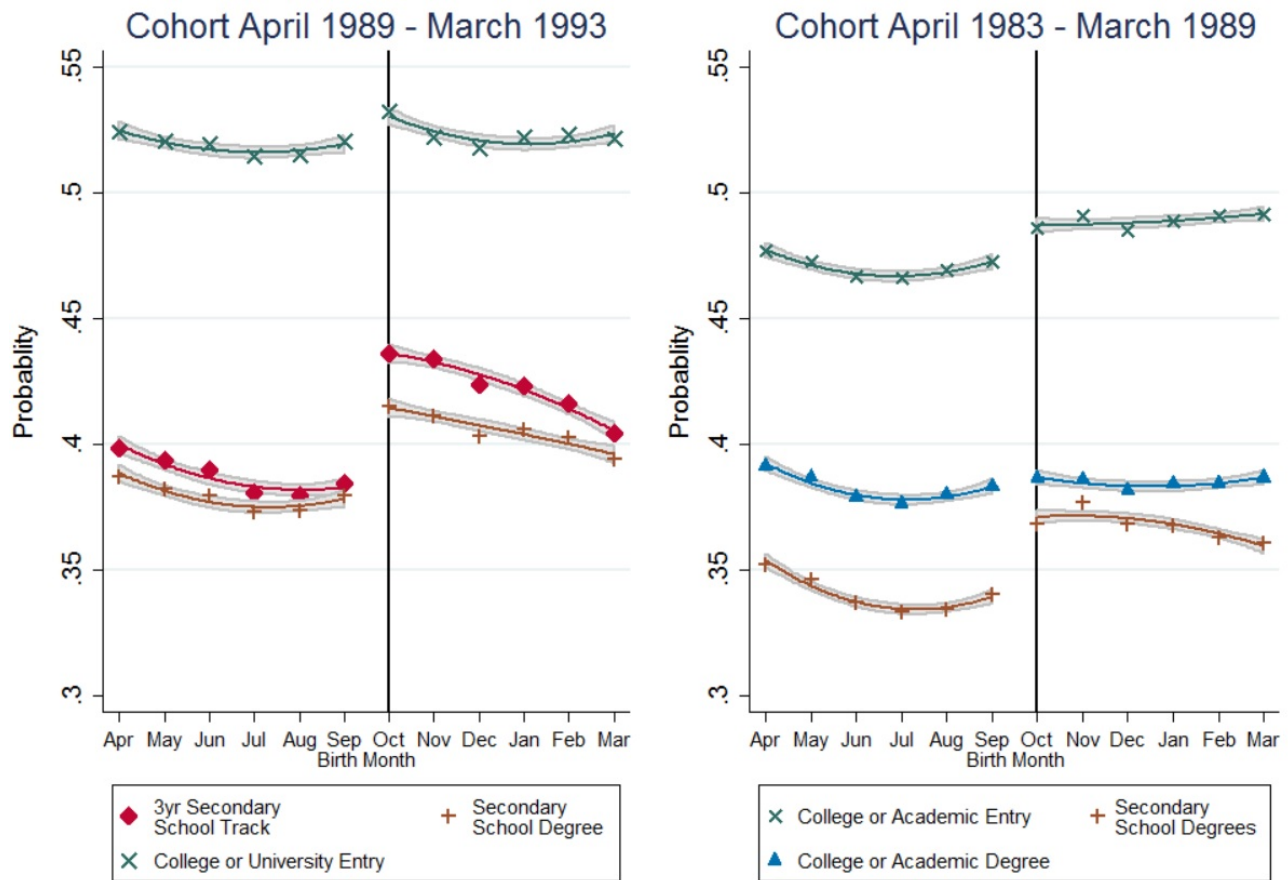
Note. The left panel shows the probability that the track allocation in the third year of secondary school (grade 9) is equal to college or university track, the probability of a college or university track secondary school degree, and the probability that a students attends a college or university. 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 D

Table D1: Educational career: university education cut-off

	$\hat{\delta}$			Optimal bandwidth	Mean	Cohort
	Bandwidth					
	14 days	30 days	Opt.			
Standardized Exit-test score	0.078*** (0.009)	0.095*** (0.014)	0.081*** (0.014)	15 days	0.000	1994 - 2002
Exit-test score = University	0.032*** (0.004)	0.036*** (0.006)	0.031*** (0.005)	15 days	0.185	1994 - 2002
Teacher Assessment = University	0.031*** (0.006)	0.035*** (0.004)	0.033*** (0.005)	20 days	0.141	1994 - 2002
Grade 9 track = University	0.032*** (0.005)	0.037*** (0.003)	0.027*** (0.006)	11 days	0.221	1994 - 2002
	0.032*** (0.008)	0.035*** (0.007)	0.035*** (0.005)	18 days	0.212	1989 - 1993
School diploma = University	0.015*** (0.007)	0.021*** (0.004)	0.020*** (0.005)	18 days	0.176	1989 - 1993
Enrollment into University	0.006 (0.007)	0.012** (0.004)	0.005 (0.007)	16 days	0.178	1989 - 1993
	0.008 (0.007)	0.009*** (0.004)	0.007 (0.004)	20 days	0.160	1983 - 1989
University degree	0.009 (0.005)	0.008** (0.004)	0.007 (0.004)	20 days	0.152	1983 - 1989

Note. 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 and 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix E

Table E1: Reduced form estimates (δ) of being relative old in school

Age (1)	N (2)	Earnings (3)	log(Earnings) (4)	I[Earnings = 0] (5)
18	499,765	-212.26*** (18.94)	-0.07*** (0.01)	0.01** (0.00)
19	496,976	-260.30*** (32.08)	-0.05*** (0.01)	0.00 (0.00)
20	492,352	-423.39*** (44.00)	-0.05*** (0.01)	0.00 (0.00)
21	487,677	-603.81*** (52.78)	-0.06*** (0.01)	0.00 (0.00)
22	481,992	-659.95*** (71.20)	-0.06*** (0.01)	0.00 (0.00)
23	476,594	-712.03*** (73.87)	-0.05*** (0.01)	0.00 (0.00)
24	469,418	-752.03*** (76.94)	-0.06*** (0.01)	0.00 (0.00)
25	465,224	-774.84*** (87.15)	-0.05*** (0.01)	0.00 (0.00)
26	462,354	-536.48*** (90.70)	-0.03*** (0.01)	0.00 (0.00)
27	463,491	-453.41*** (97.04)	-0.02*** (0.01)	0.00 (0.00)
28	467,896	-430.04*** (102.15)	-0.01** (0.00)	0.00 (0.00)
29	474,674	-326.27*** (115.49)	-0.01 (0.00)	0.00 (0.00)
30	483,654	-409.74** (116.33)	-0.01** (0.00)	0.00 (0.00)
31	492,358	-303.06** (121.58)	-0.01* (0.00)	0.00 (0.00)
32	501,368	-309.35** (128.50)	-0.01* (0.00)	0.00 (0.00)
33	510,495	-309.35** (134.52)	-0.01** (0.00)	0.00 (0.00)
34	519,433	-346.62** (142.77)	-0.02** (0.01)	0.00 (0.00)
35	528,877	-209.66 (146.79)	-0.00 (0.01)	0.00 (0.00)
36	540,232	-324.68** (155.72)	-0.01* (0.01)	0.00 (0.00)
37	551,418	-304.27** (161.94)	-0.01 (0.01)	0.00 (0.00)
38	561,661	-384.02** (168.30)	-0.01 (0.01)	0.00 (0.00)
39	571,371	-295.41* (170.80)	-0.01* (0.01)	0.00 (0.00)
40	581,919	-288.04 (176.15)	-0.01* (0.01)	0.00 (0.00)
41	590,665	-288.12 (200.77)	-0.01* (0.01)	0.00** (0.00)
42	598,618	-361.15** (178.88)	-0.01 (0.01)	0.00** (0.00)
43	601,728	-321.85* (184.24)	-0.01** (0.01)	0.00 (0.00)
44	601,263	-141.67 (182.82)	-0.01 (0.01)	0.00 (0.00)
45	598,124	-34.48 (189.46)	-0.00 (0.01)	0.00* (0.00)
46	592,833	3.65 (197.11)	-0.01 (0.01)	0.00 (0.00)
47	589,332	113.11 (193.02)	-0.00 (0.01)	0.00 (0.00)
48	585,781	105.23 (196.33)	-0.00 (0.01)	0.00 (0.00)
49	581,017	-43.73 (202.68)	0.00 (0.01)	0.00 (0.00)
50	575,606	44.94 (199.39)	-0.00 (0.01)	0.00 (0.00)
51	570,680	-608.22 (494.50)	-0.00 (0.01)	0.00 (0.00)
52	567,386	-226.13 (201.14)	-0.01 (0.01)	0.00 (0.00)
53	567,960	-402.40** (201.19)	-0.01* (0.01)	0.00 (0.00)
54	554,430	-276.54 (204.96)	-0.00 (0.01)	0.00 (0.00)
55	516,826	-295.05 (210.43)	-0.00 (0.01)	0.00 (0.00)
56	479,664	-172.88 (227.59)	0.00 (0.01)	0.00 (0.00)
57	441,851	-439.89* (254.53)	0.00 (0.01)	0.00 (0.00)
58	405,536	-27.33 (234.61)	0.00 (0.01)	0.00 (0.00)
59	370,600	275.44 (273.89)	0.00 (0.01)	0.00 (0.00)
60	335,910	66.19 (254.69)	0.03*** (0.01)	0.00 (0.00)
61	301,738	115.15 (275.07)	0.02 (0.01)	0.00 (0.00)
62	267,961	407.86 (262.89)	0.03 (0.01)	0.00 (0.00)
63	233,935	625.57 (230.80)	0.02 (0.02)	-0.00 (0.00)
64	200,630	237.36 (224.60)	0.03 (0.02)	-0.00 (0.00)
65	167,925	246.32 (223.33)	0.06** (0.03)	0.00 (0.00)

Note. 30-day interval on each side of the cut-off. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$