Evaluating School Starting Age Rules: Evidence from selected EU countries Group 24

April 18, 2024

Abstract

In this analysis we evaluate school-entry age rules for several countries and its effect on test-scores measured by the PISA survey. We selected a subset of countries due to their variation in school-entry age and their preventive measures and policies in the mitigation of early school dropouts. By looking at a regression discontinuity in time we see that birth-month significantly affects school performance. However, due to several features of the data no definitive causal inference can be made. Above all, the identification may be endogenous, for example high educated individuals will time their children's birth to give them an advantage and can therefore bias the results. Therefore, it is critical to also consider these results considering children from a low socioeconomic background. We therefore look at grade repetition and find that the probability of grade repetition is lower for students born after the cut-off month for school-entry. We note that while our results point to the fact that starting age rules affect grades, families with higher socioeconomic status are in a stronger position to time childbirth. This might increase inequality and should therefore be studied more extensively to develop according policies.

JEL Classification: I21, I26

Keywords: education, school-entry age, regression discontinuity design, PISA

1 Introduction and Background

What is the best age for children to start school? As children grow older, could there be consequences of school entry age when students are tracked through exams such as the PISA? To answer these questions, a large body of education economics literature evaluates the effects of school entrance age on students' achievements. Within education systems, a single yearly cut-off date ensures that all children born within the same cohort start school simultaneously.

Educational economists have analyzed for several countries the effects of school-entry age on school educational achievements and individual labor market performance (Fuchs & Wößmann, 2008; Sprietsma, 2010). One line of empirical literature focuses on the relationship between school entrance age and scholastic results. Many of these studies use the school entry date and variations in the birth date of students as an exogenous variation in the entry to school. They do so to analyze the performance of children that are in the same grade but have different birth dates (Crawford et al., 2010; Datar, 2006). These studies find that children who are younger when they start kindergarten compared to older children, are disadvantaged. This is observed by Crawford et al. (2010) in UK and Strøm (2004) in Norway. Elder and Lubotsky (2009) show that these younger entrants have a higher probability of grade repetition. Puhani and Weber (2008) show that in Germany older students from the cohort are more likely to attend a more academic secondary school track. An exception to these studies is Leuven et al. (2005) who observe that in the Netherlands allowing disadvantaged children to start one month early increases their test scores slightly.

The other stream of literature studies the educational outcomes on earnings in the labor market. Studies that observed the impact of birth month on earnings and on labor-market performance show mixed results. Fredriksson and Ockert (2005) observe a negative effect in Sweden. In contrast, in Norway Black et al. (2011) find that starting school younger has no significant effect on scholastic achievements but a small positive effect on income levels in the labor market.

In the European Union, there are a variety of educational policies regarding the compulsory school starting age (European Commission, 2022). Currently, the age requirements from Hungary and France is three, and for Croatia and Estonia the minimum age is 7 years old. Regarding the school leaving age we also observe some variation, with the leaving ages ranging from 15 to 19 years of age. While most countries in the EU set their compulsory education requirements based on the age of the student (for eg. be 16 year old to drop out of high school), Netherlands and Hungary have additional requirements, such as the completion of certain certifications (European Commission, 2022). For our analysis, we consider the following countries: Germany, Hungary, Luxembourg, Netherlands, Romania. We selected these countries as they have a track record of providing reliable and complete data and they capture enough regional variation in school starting age and compulsory education law. When choosing the countries for our sample we initially included UK, France, Spain and Belgium. However, we soon noticed that the latter three countries have a cut-off date of birth year in January while the other countries have the cut-off month ranging between August to October so for empirical convenience we retained finally the Netherlands, Luxembourg, Germany, Romania and Hungary. To tackle early school leaving, the countries in our samples also have policies implemented, for example in Hungary the Life Course Survey anually tracks students careers and the Tanoda Center provides additional support for disadvantaged children. In Luxemburg, the preventive program Action Locale Pour Les Jeunes contacts young school drop outs to motivate their educational perspectives. In the Netherlands, there is the Families and Schools Together, early tracking, Dropout Covenants which is a financial initiative by the Ministry of Education to reduce the number of dropouts, the Dropout Explorer that offers reliable data on drop out rates at various levels. In Germany, preventive initiatives among others, include early tracking and Bildungsketten which guides individuals in their transition from school, vocational education and training. (Lyche, 2012).

2 Descriptive Statistics

2.1 PISA dataset

The PISA study is a program to measure the educational achievement of 15-year-olds across different OECD members and associated countries. Our dataset consist of variables collected from three rounds of PISA studies, more specifically 2015, 2018, and 2022. The aim of the PISA study is to collect representative data that allows for cross-country analysis of the respective education systems. Therefore, instead of focusing on a singular grade, PISA maximises comparability between countries by sampling 15-year-old students. Students are tested on math, science and reading skills with the aim to accurately discern the subjects competencies in analyzing, comprehending and reasoning within that field. These skills are examined through a wide variety of questions and problem sets to ensure that a comprehensive analysis of the students cognitive and educational capabilities is captured. The PISA datasets also measure a wide-variety of control variables such as socio-economic and demographic data, but also psychological well-being and attitudes towards school. This enables us to adequately identify the effects of shifts in education policy. We added additional variables not present in the original dataset from the original PISA database, such as the ISCED-0 variable, truancy.

Several student variables are taken into account. Birth year and month are the key variables, since they decide the cutoff for the Regression Discontinuity in Time (RDiT) design analysis. School starting age is the other important variable since students differ in the age they start school conditional on the cutoff age. Other student variables are also taken into account such as gender, immigration status, whether socio-economic status the occupation of the father and mother. We had hoped to take into account siblings but the data has no information on siblings in the first 2 waves, therefore we have excluded this variable. To make between country comparisons we use the international grade as a means of standardization. However, not all countries are comparable and there is some variability in the grade the students is by age of 15. In Romania most students are in grade 9 by the age of 10, this is a similar situation for Hungary. In the Netherlands, most students are in grade 10 by the age of 15 while the second most are in grade 9. Luxembourg has a similar distribution of students attending grade 9 and 10, as does Germany. For the outcome variables we consider the PISA test scores and whether the student has repeated a grade.

2.2 Test scores

Figure 1 shows a map of the average test scores (math, science, reading) mapped to their respective country. Scores are averaged over the three available waves and all available respondents in the dataset. Darker colours indicate higher scores. As such, it is observed that students in Northern European countries perform well in PISA tests. Poland and the Czech Republic have high scores for math, reading and science tests. On the contrary, countries in the Balkans, on averages, show lower test scores for math, reading and science.

Table 1 and table 2 respectively show the descriptive statistics for math and reading over the different waves. We consider the subset of countries: Germany, Hungary, Luxembourg, Netherlands, Romania and the United Kingdom. Table 3 gives a graphical representation of the percentiles for math scores, averaged over the three waves and regarding Germany, Hungary, Luxembourg, Netherlands, Romania and the United Kingdom. There is a clear difference in the distribution of test scores considering Romania compared to other, possibly an indication that students perform worse in mathematics compared to the other countries in the sample.

Table 1: Descriptive statistics - Math

	Round	Obs	Mean	Std. dev.	Min	Max
Germany	2015	6,504	508.74	83.40	239.28	765.73
	2018	5,451	502.24	90.13	224.77	744.79
	2022	6,116	477.74	90.20	238.31	753.26
Hungary	2015	5658	484.73	86.09	211.60	770.33
	2018	5132	488.10	84.18	185.44	754.69
	2022	6198	479.72	88.52	223.71	746.95
Luxembourg	2015	5,299	486.44	87.96	241.56	760.14
	2018	523	483.81	92.13	209.67	750.05
	2022		-	-	-	-
Netherlands	2015	5,385	513.90	86.49	232.84	806.53
	2018	4,765	514.10	90.23	167.12	782.63
	2022	5,046	491.04	10.30	232.28	774.95
Romania	2015	5	444.00	78.99	214.37	691.04
	2018	5075	429.99	85.83	152.98	697.55
	2022	7364	435.55	93.45	155.70	761.60
United Kingdom	2015	14,157	491.50	81.07	114.47	754.24
	2018	13,818	495.17	81.91	182.20	754.22
	2022	12,972	481.82	91.52	182.77	834.49

Table 2: Descriptive statistics - Reading

	Round	Obs	Mean	Std. dev.	Min	Max
Germany	2015	6,504	512.15	92.88	179.66	763.59
	2018	5,451	500.23	10.28	198.37	816.44
	2022	6,116	483.04	98.93	176.51	785.72
Hungary	2015	5,658	477.25	89.79	173.31	724.49
	2018	5,132	482.90	93.84	199.22	728.05
	2022	6,198	479.98	93.41	151.88	756.95
Luxembourg	2015	5,299	482.28	10.10	185.72	742.00
	2018	523	470.51	10.56	179.12	777.60
	2022		-	-	-	-
Netherlands	2015	5,385	504.95	95.81	189.43	769.87
	2018	4,765	479.82	10.28	169.78	768.53
	2022	5,046	456.80	10.86	156.93	733.11
Romania	2015	4,876	434.33	87.22	144.93	722.93
	2018	5,075	428.28	92.48	129.55	702.31
	2022	7,364	436.35	93.52	140.91	712.39
United Kingdom	2015	14,157	495.12	87.08	173.62	804.88
	2018	13,818	499.58	94.61	184.62	794.63
	2022	12,972	490.44	97.04	154.40	816.35

Table 3: Descriptive statistics: Percentiles

	Germany	**Hungary**	**Luxembourg**	**Netherlands**	**Romania**	**United Kingdom**
1%	297.92	294.60	292.32	300.52	253.91	292.38
5%	345.17	338.04	337.91	344.73	298.02	348.87
10%	376.65	366.17	366.82	376.41	324.59	380.36
25%	433.89	423.45	417.34	437.34	372.42	438.52
50%	498.89	486.09	486.21	511.92	433.62	503.03
75%	559.98	545.68	552.83	578.27	497.02	564.88
90%	610.62	594.99	602.60	626.24	552.96	617.34
95%	637.64	623.21	629.14	651.14	585.65	646.48
99%	685.87	670.71	674.97	693.09	639.80	698.34

Note: this table presents the percentiles of the distribution of math scores, averaged over the three waves. Each column represents a separate country

Figure 1: Average test scores by country

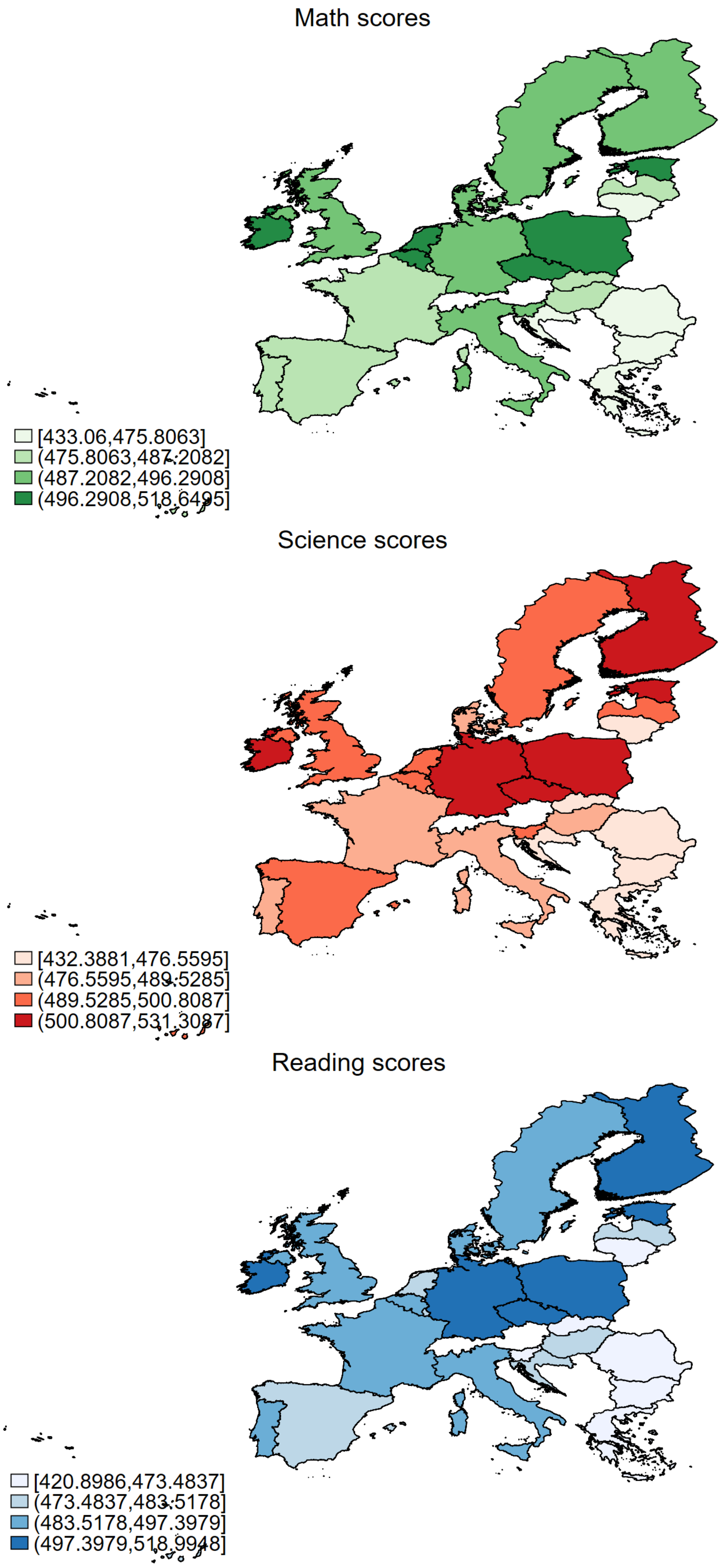


Figure 2: Evolution of school starting age: ISCED 0

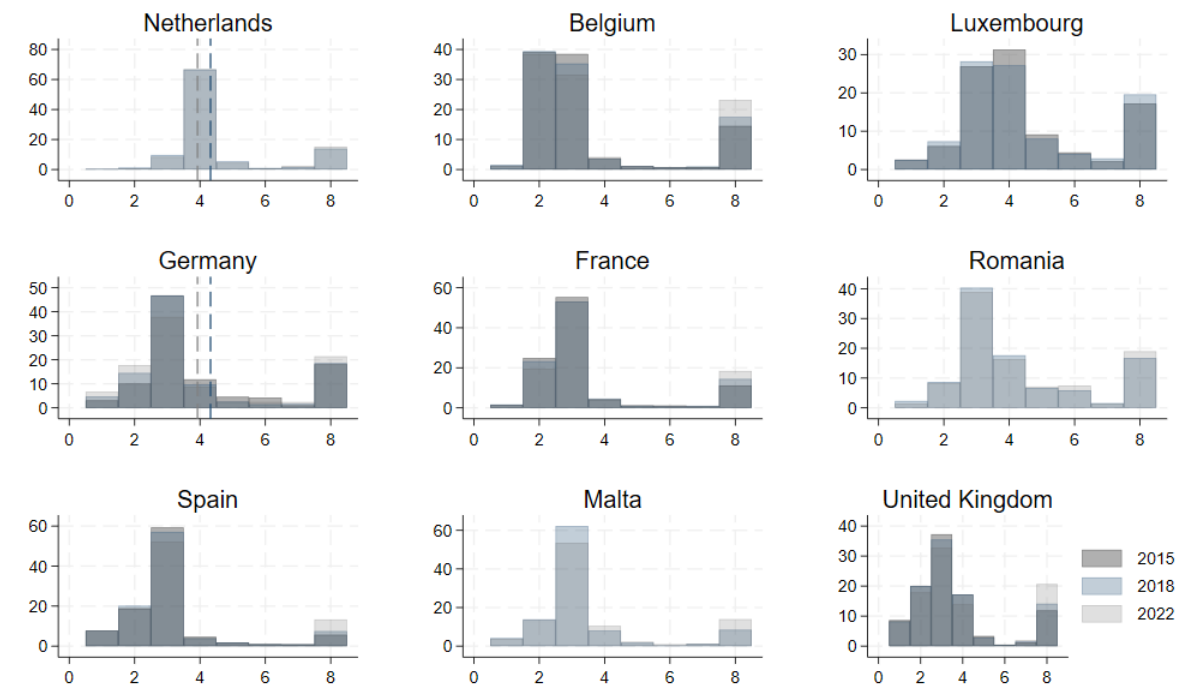
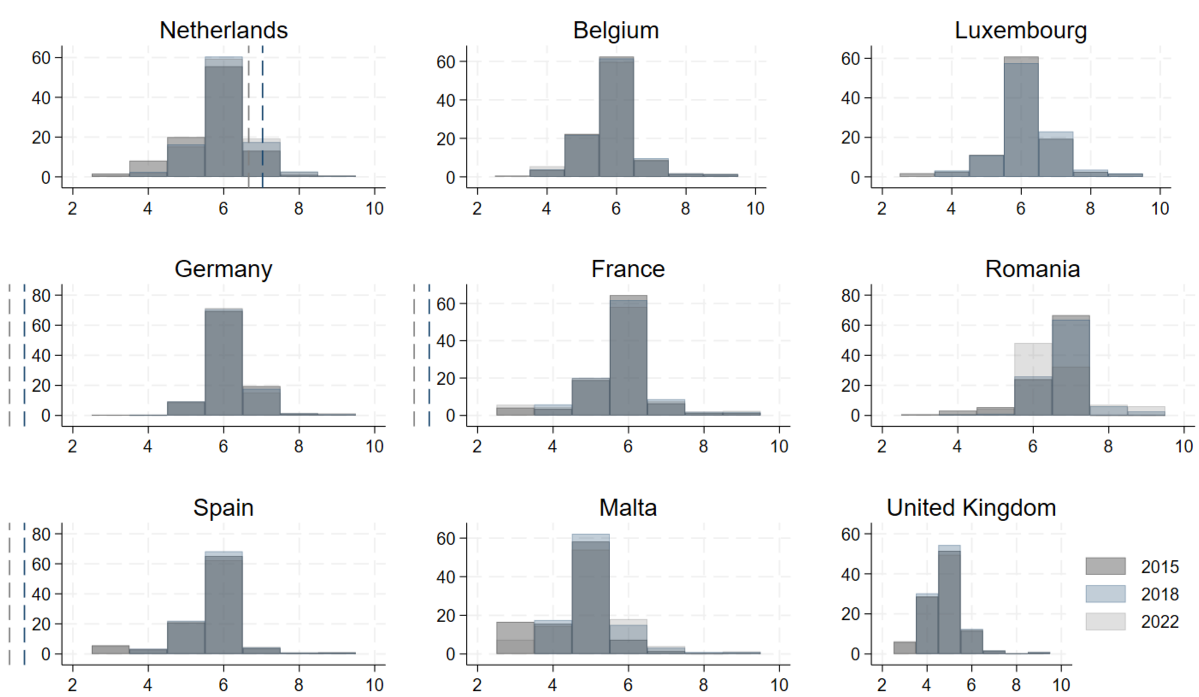


Figure 3: Evolution of school starting age: ISCED 1



2.3 School starting age

Furthermore, it is of particular particular interest to examine in which countries there is a change in the school starting age. Figures 2 and 3 plot the empirical distribution of school starting age for nine European countries. The graphs show school starting age for ISCED 0 and ISCED 1 education.

For ISCED 0 level education, we do not have observations for 2015 in The Netherlands, Romania and Malta. For other countries, data is available for all three waves.

For ISCED 1 level education, on average, school starting ages are distributed around a mean of six years in the countries included in the image. Importantly, in Romania most students are in grade 9 by the age of 15, this is a similar situation for Hungary. In the Netherlands, most students are in grade 10 by the age of 15, with the second largest group being in grade 9. Luxembourg has a similar distribution of students attending grade 9 and 10, as does Germany. Therefore, we split up our results in grade 9 and 10 and every country is regarded separately by the how the students are distributed.

3 Empirical Strategy

3.1 Potential Endogeneity

With the cross-sectional PISA dataset, it is not possible to observe all student, parental, and school characteristics, this can cause potential endogeneity through omitted variable bias. In this paper, we attempt to minimize the omitted variable bias through the use of RDD measurement techniques, which is further explained in the next section.

Furthermore, comparing the performance results over different waves of the PISA data cannot be interpreted causally, because many events that happened could affect the school performance. Referring back to our previous example, Romania, the Figure 3 clearly indicates a reduction in school starting age going from 2018 to 2022. This indicates a policy change in school starting age going from seven to six years. However, during this period, the Covid-19 pandemic also hit Europe, including Romania. Therefore, any changes in the test results might not be attributable in the policy change from 2018 to 2022.

Additionally, families from a higher socioeconomic status might time the birth of their children because they are aware that their child will perform better than their cohort. Therefore, we do an additional analysis considering the correlation between children from a low socioeconomic and repeating grades. For this study we only consider the Netherlands following Leuven et al., 2005.

3.2 Regression Discontinuity in Time

To resolve for this endogeneity, we leverage month of birth as an exogenous source of variation in the data. For example, in The Netherlands, a child has to attend school when by October 1st, the child has turned 6 years old. If the child only turns 6 after October 1st, school start is postponed by one year. Similar regulation exists for different countries, as represented in Table 4.

Differences in birth month provide a source of exogenous variation in starting age. This enables us to compare the average outcome of students born right before the cutoff month (who will start their schooling career early, and are thus *young* within their cohorts) with the average outcome of students right after the cutoff month (who will start their schooling career late, and are thus *old* within their cohorts).

We can expect that this source of variation is exogenous, because the *young* students born just before the cutoff should not differ in other characteristics to the *old* students born just after the cutoff. This allows us to estimate the average treatment effect as

$$ATE = E[Y_i(1) - Y_i(0)|X = c] \tag{1}$$

where $E[Y_i(1)|X]$ is the expected outcome for the *old* students and $E[Y_i(0)|X]$ is the expected outcome for the *young* student. The regression specification for this Regression Discontinuity Design is as follows:

$$y_{i,t} = \beta_0 + \beta_1 Post_i + f(Month_i) + f(Month_i \times Post_i) + \varepsilon_{i,t}. \tag{2}$$

In this equation β_1 is the coefficient of causal interest, specifying if a child was born before or after the country-specific cutoff. It takes a value of one if the child is born after the cutoff, and a value of zero otherwise. We allow for non-linearities in the effects of birth-month, denoted by the functional form $f(Month_i)$ that is also allowed to differ before and after the cutoff month. Standard errors are estimated using heteroskedasticity robust estimation of the variance-covariance matrix.

Table 4: Cutoffs for RDiT

Country	**School starting rule**	**Cutoff month**
The Netherlands	6 years old on October 1st	October
Germany	6 years old on September 1st	September
United Kingdom	5 years old on 31st	December/March/August
Luxembourg	4 years old on 1st	September
Hungary	6 years old on 31st	August
Romania	6 years old on 31st	August

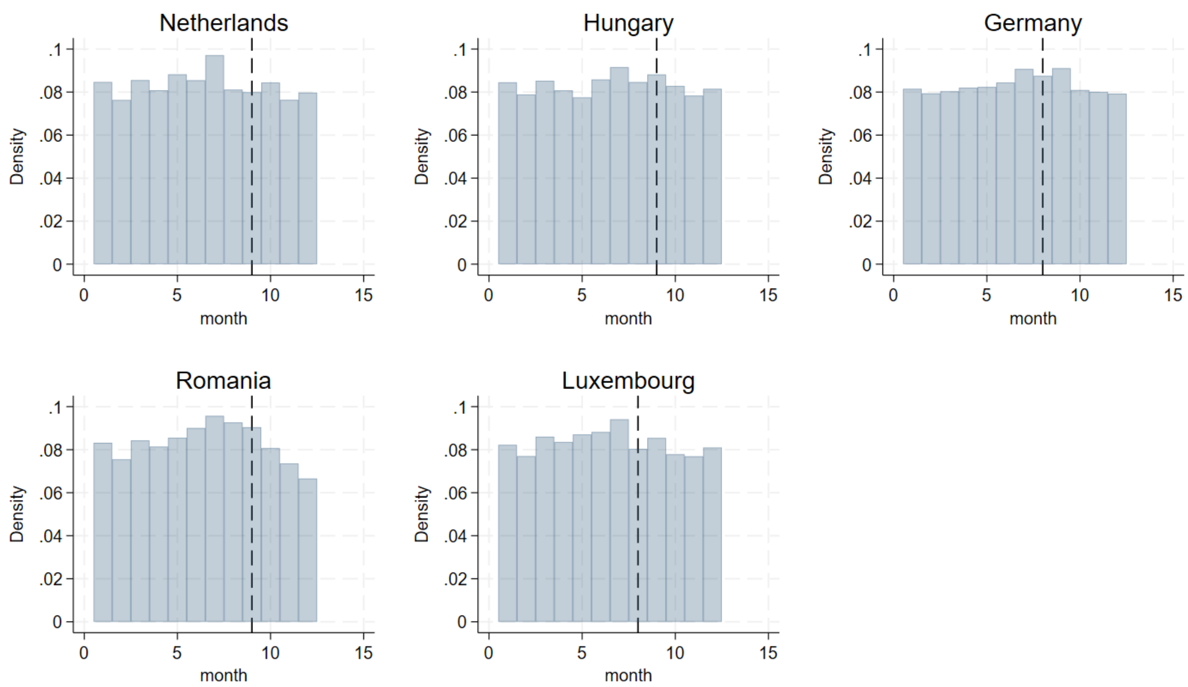
In a first robustness check, we verify the validity of our results by adding control variables:

$$y_{i,t} = \beta_0 + \beta_1 Post_i + f(Month_i) + f(Month_i \times Post_i) + \boldsymbol{\delta x'_{i,t}} + \varepsilon_{i,t}. \tag{3}$$

The vector of control variables $\boldsymbol{x'_{i,t}}$ includes socio-economic status, parental education, the availability of ICT resources at home, migratory background, relative grade in the cohort and truancy behaviour. This set of control variables is chosen to represent the family and socio-economic background of the student and his or her behavioural characteristics.

The validity of Regression Discontinuity in Time depends on the absence of bunching of birth months around the threshold. Figure 4 gives an describes the distribution of months of births over the year, and is indicative for the absence of bunching around the threshold for education. However, as indicated by **?**, using birth months as a source of variation might suffer from remaining endogeneity concerns. This phenomenon is referred to a *redshirting*, and indicates parents' explicit preferences in the months they would like to give birth to their children.

Figure 4: Bunching



Note: This Figure shows the distribution of birth months around the year. Vertical lines indicate the cutoff months for school year enrollment. Data is taken from the 2015, 2018 and 2022 waves.

4 Results

4.1 Math scores

Regression Results for the base specification are given in Table 5. A robustness check, including student specific control variables is included in Table 6. The base results indicate an overall positive effect of being born after the cutoff month for school enrollment, and thus for being an older student within the cohort. If a child is born after the cutoff month, on average, the PISA math scores increase with 17.288 to 31.030 points. This increase is significant at the 0.1% significance level for 9th grade students and at the 1% significance level for 10th grade students. Results also indicate economic significance, since approximately 50% of the observations ranging between PISA scores of 400 and 600. When examining countries individually, effects of being born post cutoff month remain significant. Variation across countries, however, exists. For example, results in Germany are less or insignificant, while the results for Hungary and Luxembourg are more in line with the general conclusion.

Table 5: Regression Results: wave 2015 & 2018

	(1) All-9th	(2) All-10th	(3) DEU-9th	(4) DEU-10th	(5) ROU-9th	(6) ROU-10th	(7) HUN-9th	(8) HUN-10th	(9) LUX-9th	(10) LUX-10th	(11) NLD-9th
Post	31.030***	17.288**	18.650*	7.801	-20.409	35.420***	57.641	37.982***	21.545**	27.091***	-3.071
	(10.20)	(2.96)	(2.27)	(0.80)	(-0.96)	(5.32)	(1.84)	(5.94)	(2.61)	(3.94)	(-0.33)
Month	1.318**	3.767***	1.500**	0.576	-3.100*	-1.520	-0.400	2.198**	-0.203	0.368	1.463**
	(3.00)	(3.86)	(2.66)	(0.39)	(-2.55)	(-1.41)	(-0.33)	(2.97)	(-0.36)	(0.44)	(3.13)
Interaction	1.169	0.253	-6.822	6.647*	17.329	2.257	-7.338	1.342	4.113	3.804	12.054*
	(1.16)	(0.14)	(-1.86)	(2.15)	(1.53)	(1.06)	(-0.78)	(0.62)	(1.22)	(1.33)	(2.43)
Constant	461.401***	485.126***	551.189***	427.312***	439.322***	471.782***	522.887***	443.866***	541.744***	467.426***	553.865***
	(266.05)	(125.50)	(219.47)	(77.68)	(72.64)	(116.61)	(78.63)	(146.38)	(238.06)	(137.05)	(253.42)
N	13169	3297	4338	1525	1187	2683	1491	3070	3415	2594	4611
F	297.312	76.954	7.273	18.239	3.164	36.372	4.933	133.639	18.053	48.786	20.544
r2	0.063	0.066	0.005	0.034	0.007	0.039	0.004	0.118	0.013	0.054	0.011

$^{*}p < .10, ^{**}p < .05, ^{***}p < .01$

Table 6: Regression Results: math, wave 2015 & 2018 including controls

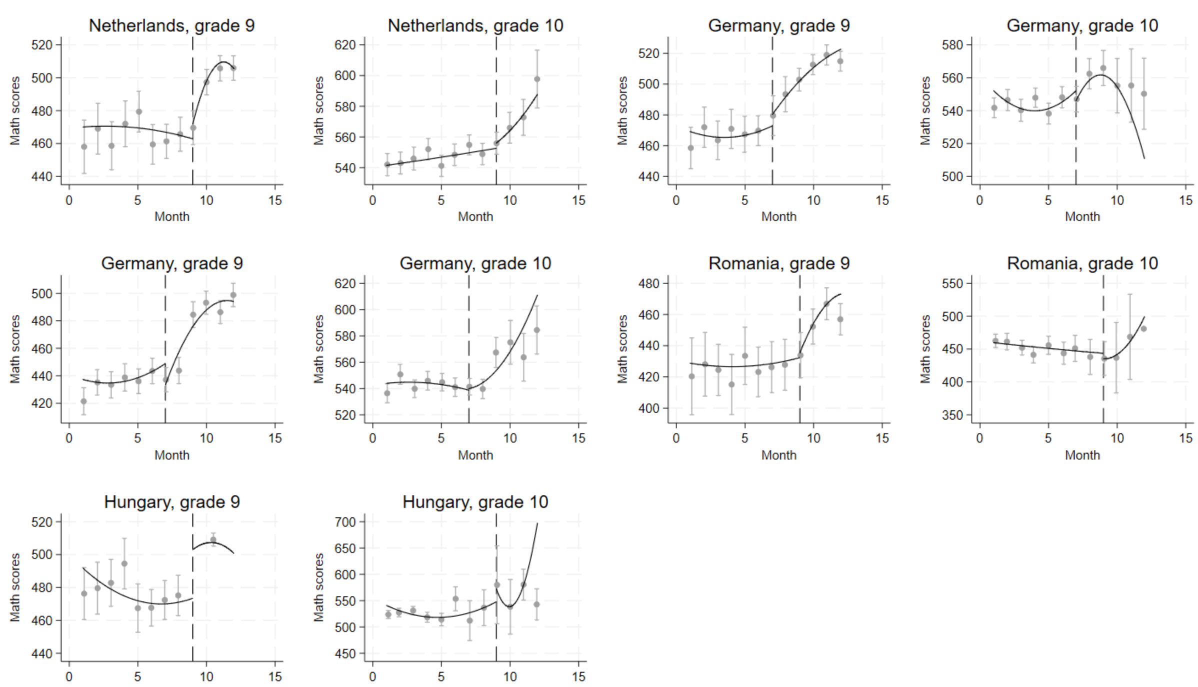
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	All-9th	All-10th	DEU-9th	DEU-10th	ROU-9th	ROU-10th	HUN-9th	HUN-10th	LUX-9th	LUX-10th	NLD-9th
Post	20.407**	16.608	-2.771	-8.493	-39.754	22.571*	14.360	20.843*	5.132	24.031*	0.477
	(2.59)	(1.30)	(-0.18)	(-0.52)	(-1.48)	(2.36)	(0.40)	(2.29)	(0.50)	(2.28)	(0.04)
Month	1.750	3.285	2.380*	4.169	-0.106	2.196	-0.852	1.792	1.167	-1.067	0.923
	(1.32)	(1.52)	(2.31)	(1.45)	(-0.06)	(1.29)	(-0.53)	(1.54)	(1.49)	(-0.78)	(1.38)
Interaction	-1.835	-3.941	-5.133	1.933	15.000	-2.408	5.174	4.662	1.679	2.981	5.831
	(-0.72)	(-1.05)	(-0.82)	(0.38)	(1.06)	(-0.76)	(0.46)	(1.55)	(0.40)	(0.68)	(0.89)
Parental Education	0.180	0.016	0.517**	0.439	0.493	0.501*	0.229	0.455**	0.309*	-0.089	0.174
	(1.22)	(0.07)	(2.72)	(1.59)	(1.46)	(2.36)	(0.85)	(2.66)	(1.97)	(-0.44)	(1.10)
ICT Resources	-1.525	-7.504	-2.346	7.932	8.500	-3.090	-1.722	-0.092	-3.948	-1.989	-4.964*
	(-0.60)	(-1.90)	(-0.69)	(1.45)	(1.37)	(-0.88)	(-0.36)	(-0.04)	(-1.82)	(-0.58)	(-2.20)
Social Status	27.189***	28.569***	19.708***	31.298***	22.738*	35.623***	36.663***	16.334***	18.416***	22.135***	28.406***
	(6.93)	(5.21)	(4.19)	(4.11)	(2.58)	(5.89)	(4.94)	(4.06)	(5.25)	(4.46)	(6.69)
Relative grade	-3.098										
	(-0.83)										
Truancy	-26.082***	-26.911***	-23.332***	-12.116**	-19.092***	-22.297***	-28.417***	-19.160***	-24.667***	-14.642*	-36.606***
	(-7.49)	(-4.61)	(-4.87)	(-3.00)	(-5.02)	(-4.19)	(-4.42)	(-5.74)	(-5.13)	(-2.34)	(-6.67)
Constant	479.890***	491.535***	534.581***	595.383***	537.889***	490.608***	511.997***	461.198***	561.478***	463.605***	593.991***
	(23.61)	(17.95)	(25.09)	(22.71)	(5.69)	(25.15)	(11.14)	(37.09)	(50.63)	(23.22)	(22.80)
N	3880	671	1127	446	419	884	722	1164	1458	715	1861
F	40.134	18.773	21.355	.	.	52.107	28.176	54.847	34.682	13.410	31.667
r2	0.223	0.216	0.162	0.311	0.263	0.346	0.267	0.275	0.177	0.131	0.153

$^{*}p < .10, ^{**}p < .05, ^{***}p < .01$

Figure 5 graphically represents the distribution of math scores for children in each month of birth. Data spans the waves of 2015 and 2018. Because children making the PISA-test are in different grades, and this can affect their performance, figures are represented separately for children in Grade 9 and Grade 10 at the time of making the PISA test. The vertical line in the Figure represents the legal cutoff month for school enrollment, and serves as the discontinuity in the RDD. As stated in our prior results, children born after the cutoff month, have higher results for the PISA test. This observation holds consistently for the Netherlands, Germany, Luxembourg, Romania and Hungary, and the results are observed for students in grade 9 and 10.

We do not always observe a jump immediately at the cutoff month. For example, in The Netherlands the jump in grades is negligible sharply at the cutoff. Rather, we find an overall increased student performance for students born after the cutoff months. This observation might be due to the fact that national regulation is not always followed up to strictly. As a minimal example, a child born in the Netherlands just after the October 1st might still start schooling this academic year, instead of waiting for an entire year. This makes the cutoff rule fuzzy, and further research might benefit from applying a Fuzzy Regression Discontinuity Design.

Figure 5: Results Base Specification: wave 2015 & 2018



4.2 Grade Repetition

We corroborate our main findings for grade repetition. In line with previous results, we expect that being an *older* student in the class cohort, reduces the probability of having repeating the grade, just like it improved math scores. Since the outcome variable is binary (taking a value of one if the student has repeated a grade and a value of zero if the student has not repeated), the regression takes the form of a probit regression, with as specification:

$$\begin{aligned} P(y_i = 1) = \Phi(Z_i) \\ Z_i = \beta_0 + \beta_1 Post_i + f(Month_i) + f(Month_i \times Post_i + \boldsymbol{\delta x'_{i,t}}) \end{aligned} \tag{4}$$

where $\Phi(Z_i)$ represents the cumulative standard normal distribution. The control variables included in $\boldsymbol{x_{i,t}}$ are the same as in the main analysis. Results are represented in Table 7. Aggregating over all countries in the analysis, we indeed find a significant and negative effect for being porn after the cutoff month, and enrolling education only a year later. Being an *older* student within a cohort, reduces the probability of repeating a grade. This conclusion holds for students taking the PISA-test in 9th and 10th grade. Furthermore, the coefficient sizes in a similar range. At the level of individual countries, being born in the months after the cutoff month is not always an indicator of reduced probability of grade repetition. Whenever the coefficient is statistically significant, which it is for Hungary and Luxembourg, it carries a negative sign, confirming our initial intuition.

Table 7: Regression Results: grade repetition, wave 2015 & 2018 including controls

	(1) All-9th	(2) All-10th	(3) DEU-9th	(4) DEU-10th	(5) ROU-9th	(6) ROU-10th	(7) HUN-9th	(8) HUN-10th	(9) LUX-9th	(10) LUX-10th	(11) NLD-9th
Post	-0.822***	-0.831**	-0.172	-0.150	0.000	0.423	0.000	-2.583***	0.000	-1.081***	0.612
	(-4.20)	(-2.98)	(-0.39)	(-0.25)	(.)	(0.99)	(.)	(-10.10)	(.)	(-4.26)	(1.39)
Month	-0.209***	-0.317***	-0.022	-0.095	-0.017	-0.285***	-0.257***	-0.015	-0.145*	-0.159***	-0.046
	(-8.78)	(-5.57)	(-0.37)	(-1.65)	(-0.33)	(-6.73)	(-3.87)	(-0.54)	(-2.14)	(-3.90)	(-1.38)
Interaction	-0.048	0.071	0.042	-0.110	0.000	-0.052	0.000	0.026	0.000	-0.164	-0.048
	(-0.58)	(0.64)	(0.55)	(-0.48)	(.)	(-0.26)	(.)	(0.31)	(.)	(-1.50)	(-0.21)
Parental Education	0.005	0.002	-0.001	0.006	-0.004	-0.001	-0.016	0.001	0.014	0.007	-0.005
	(1.42)	(0.35)	(-0.23)	(0.54)	(-0.41)	(-0.19)	(-1.32)	(0.23)	(1.21)	(1.38)	(-0.70)
ICT Resources	0.207***	0.104	0.142	-0.061	0.904	-0.010	-0.564***	0.122*	-0.546**	0.161*	0.018
	(3.39)	(1.01)	(0.81)	(-0.42)	(1.96)	(-0.07)	(-3.31)	(2.11)	(-3.28)	(2.05)	(0.16)
Social Status	-0.131	-0.060	-0.097	-0.524*	-1.555***	-0.107	0.187	-0.113	-0.264	-0.228	0.157
	(-1.36)	(-0.40)	(-0.52)	(-2.25)	(-4.41)	(-0.54)	(0.69)	(-1.21)	(-0.99)	(-1.64)	(0.87)
Relative grade	-0.527***										
	(-6.23)										
Truancy	0.164*	0.380***	0.000	0.263*	-0.327	0.124	0.352	0.210*	0.212	0.045	0.428***
	(2.39)	(3.68)	(.)	(2.25)	(-1.14)	(0.84)	(1.21)	(2.27)	(1.27)	(0.29)	(3.55)
Constant	-0.239	-0.333	-2.167***	-2.596***	-3.561***	-1.255	-3.903***	0.370	-4.419***	-0.133	-1.808**
	(-0.63)	(-0.59)	(-4.87)	(-4.10)	(-5.64)	(-1.78)	(-4.28)	(1.22)	(-4.81)	(-0.25)	(-2.76)
N	3873	670	1006	442	398	872	687	1161	1330	713	1861

$^{*}p < .10, ^{**}p < .05, ^{***}p < .01$

4.3 Heterogeneity

Finally, we examine heterogeneity in effects, focussing specifically on the Netherlands. In Table 8 we make a distinction between two groups of children, based on their socioeconomic status (PISA escs-variable). We classified a student as having low socioeconomic status if the escs variable took a value below 0, while escs-values above are indicative of high socioeconomic status. Columns 1 and 4 replicate the results for the entire sample, while columns 2 and 4 (3 and 5) present the results for the low (high) socioeconomic classes. Results provide evidence of a differential effect between these two groups, that is most pronounced and significant for students in 9th grade. These results are in line with Leuven et al. (2005), who find small detrimental effects for children from poor socioeconomic status.

Table 8: Regression Results: heterogeneity based on socioeconomic background, wave 2015 & 2018 including controls

	(1) All-9th	(2) Low-9th	(3) High-9th	(4) All-10th	(5) Low-10th	(6) High-10th
Post	-1.081***	-0.419	-1.488***	0.612	-0.149	1.362*
	(-4.26)	(-0.99)	(-4.52)	(1.39)	(-0.29)	(2.23)
Month	-0.159***	-0.161**	-0.179**	-0.046	0.079	-0.101**
	(-3.90)	(-2.66)	(-3.08)	(-1.38)	(1.03)	(-2.59)
Interaction	-0.164	-0.426*	-0.003	-0.048	0.128	-0.336
	(-1.50)	(-2.19)	(-0.02)	(-0.21)	(0.74)	(-1.00)
Parental Education	0.007	0.003	0.009	-0.005	-0.012	-0.001
	(1.38)	(0.37)	(1.21)	(-0.70)	(-1.21)	(-0.10)
ICT Resources	0.161*	0.048	0.199	0.018	-0.206	0.052
	(2.05)	(0.41)	(1.96)	(0.16)	(-0.88)	(0.40)
Social Status	-0.228	-0.346	-0.220	0.157	0.379	0.326
	(-1.64)	(-1.47)	(-0.82)	(0.87)	(1.06)	(1.06)
Truancy	0.045	-0.112	0.167	0.428***	0.000	0.546***
	(0.29)	(-0.49)	(0.71)	(3.55)	(.)	(3.93)
Constant	-0.133	-0.314	0.423	-1.808**	-1.516**	-2.521**
	(-0.25)	(-0.43)	(0.62)	(-2.76)	(-3.16)	(-2.72)
N	713	267	446	1861	438	1390

$^{*}p < .10, ^{**}p < .05, ^{***}p < .01$

5 Conclusion

In our analysis we investigated whether the age of the student at the time of school entry affects children's school performances. To do so we used three waves of the PISA-2015, 2018 and 2022. We focused our analysis on five countries within the EU region. To deal with the endogeneity problem in the dataset we have employed the regression discontinuity design strategy. In our descriptives, we find that the school starting age shows variation across the sample but the mean school starting age is 6 years. Our findings show that the age effects are strong: the outcome variable i.e. math and reading scores are significant and positive for when the birth date of the students is after the cut-off month averaged over all selected countries. We observe that the probability of repeating a grade is lower if the birth date of the child is after the cut off month. This probability is more pronounced for children that have a weaker socioeconomic background. Therefore, the school entry age and the educational policy measures are important factors in a student's scholastic performance. The main policy implication of our analysis is that younger students will probably have lower educational outcomes with respect to their older peers. However, we see that the choice to postpone the entry of the child into a child into a school is related to its socio-economic background and ability of inter-generational mobility. A very good public policy would then be to direct resources towards younger students in the cohort such as attention via help of a teaching assistant, inclusion of parents to overcome scholastic problems and to tract students from a younger age and for a longer duration.

References

Black, S. E., Devereux, P. J., & Salvanes, K. G. (2011). Too young to leave the nest? the effects of school starting age. *The review of economics and statistics*, *93*(2), 455–467.

Crawford, C., Dearden, L., & Meghir, C. (2010). When you are born matters: The impact of date of birth on educational outcomes in england. *DoQSS Working Paper*, (10-09).

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

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

European Commission. (2022). *Compulsory education in europe – 2022/2023*. Publications Office of the European Union. https://doi.org/doi/10.2797/235076

Fredriksson, P., & Ockert, B. (2005). Is early learning really more productive? the effect of school starting age on school and labor market performance. *IZA Discussion Paper*, (1659). https://ssrn.com/abstract=760728

Fuchs, T., & Wößmann, L. (2008). What accounts for international differences in student performance? A re-examination using PISA data. *Empirical economics*, *32*(2-3), 433–464. https://doi.org/10.1007/s00181-006-0087-0

Leuven, E., Lindahl, M., Oosterbeek, H., & Webbink, D. (2005). New evidence on the effect of time in school on early achievement. *Scholar Working Paper*, *47*(04), 1–25.

Lyche, C. S. (2012). TACKLING EARLY SCHOOL LEAVING IN EUROPE: DIFFERENT POLICIES. *CESifo DICE Report*, *3*, 81–82. https://www.ifo.de/DocDL/dicereport312-db3.pdf

Oosterbeek, H., ter Meulen, S., & van der Klaauw, B. (2021). Long-term effects of school-starting-age rules. *Economics of Education Review*, *84*, 102144. https://doi.org/https://doi.org/10.1016/j.econedurev.2021.102144

Puhani, P. A., & Weber, A. M. (2008). Does the early bird catch the worm? In C. Dustmann, B. Fitzenberger, & S. Machin (Eds.), *The economics of education and training* (pp. 105–132). Physica-Verlag HD. https://doi.org/10.1007/978-3-7908-2022-5_6

Sprietsma, M. (2010). Effect of relative age in the first grade of primary school on long-term scholastic results: international comparative evidence using PISA 2003. *Education economics*, *18*(1), 1–32. https://doi.org/10.1080/09645290802201961

Strøm, B. (2004). *Student achievement and birthday effects* [Unpublished manuscript, Norwegian University of Science and Technology].