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Abstract

This paper uses a large multi-country database with data from the OECD PISA program to disentangle the effects of birthdate on educational performance. As far as age effects are concerned, we conclude that children are disadvantaged because they are the youngest in class (relative age effect), not because they are young *per se*. Our findings go against delaying mandatory school entry as a general policy, as there is no gain from a rise in entry age - keeping age differences among students constant - to make up for the shortening of length of schooling. Such an evidence that postponing school entry postpones learning is more marked for children belonging to disadvantaged households. In contrast, the relative age effect does not interact with family background, and remains stable across school entry age cohorts. The size of this effect, measured at the age 15 is not large, but its interaction with early grade retention and tracking may enhance long-term effects. Finally, we do not detect an association between birthdate and achievement originating in unobservable characteristics of students.

JEL: I21, I28, J24

1. Introduction

Diverse strands of literature belonging to different disciplines have produced evidence that has a bearing on the association between birthdate and educational achievement. Some of this literature has been triggered by the observation by parents and teachers that the youngest children in class tend to be at disadvantage vis-a-vis their older peers. Indeed, the interaction between birthdate and school entry rules brings about differences in ages of children within grades spanning up to one year. Children born in the months just before the cut-off date for school entry tend to have poorer performance. These will be the students born towards the end of the year in countries that have December cut-offs, or those born over Summer in countries that have late Summer cut-offs.[1]

Nevertheless, the precise mechanisms underlying the impact of season of birth on achievement through school entry age are unclear. Do younger entrants do worse because they are young in absolute terms, because they are the youngest in class, or both? The answer to this question has obvious policy implications. If it is just relative age that matters, policies to delay enrolment in mandatory school - as implemented in some states in the US - bring no gain to offset potential losses for students. Nevertheless, it could still make sense for parents to hold back their children - providing the others comply - so that

1. Note that such seasonal patterns depend on the precise outcome variable that is being considered. For the US, many studies following the seminal work by Angrist and Krueguer (1991, 1992) associate individuals born in Winter with the worst outcomes, on the basis compulsory school attendance (see below).

they are the oldest in class. If it is absolute age that matters, children should be granted the necessary time to mature and reap the most benefits from their schooling.

However, other explanations for an association between performance of students and birth season have been put forward. For instance, prenatal exposure to certain conditions could bring about differences in intellectual ability of children across birth cohorts. In particular, it has been argued that the lower achievement of Summer-born children in the UK (see Sykes *et al.* (2009) - who are also the youngest in their academic year) could stem from higher incidence of viral infections during their mothers' pregnancy. More generally, work from medicine, biological sciences and psychology has documented an association between certain diseases or personality traits and birthdate.

Family is an additional channel through which birth quarter could impact on educational performance. There has been work connecting birth season and the socioeconomic background of children, with recent evidence associating births in warmer seasons with favoured families. Given that family background is a well known determinant of educational attainment, such connection could also contribute to a pattern of variation in the latter across birth seasons.

Our paper attempts to address the channels linking birthdate and academic performance in a common framework. We draw evidence from a large database with information from the four cycles of the OECD PISA program between 2003 and 2012. In contrast to most of the previous literature that carried out single-country analyses, we rely on a pooled cross-country dataset (comprising over 40 countries) to disentangle and evaluate the importance of such channels. In this context, the strategy to tackle endogeneity of school entry age in the educational production function builds on our ability to distinguish the students who entered school at the assigned age and attend the expected grade from those who entered school earlier or later than prescribed or had a regular entry but failed at least one grade.

Our main results are the following. While relative entry age has a positive impact on performance at the time of PISA tests (around age 15), absolute entry age has a nil to negative impact. When the two variables are interacted, the effect of relative age remains approximately stable across cohorts of absolute entry age and we do not find either evidence of an interaction of relative age with social background. In contrast, relative age has a stronger role in the countries where early tracking is present. Overall this evidence suggests that the relative age effect stems from competitive interaction among students and seems to be relatively immune to underlying conditions such as absolute maturity of children and socioeconomic context. In contrast, *absolute* entry age interacts with socioeconomic status, the role of family for performance being comparatively larger for late entrants.

We do not find evidence of pure birth season effects, once key variables such as entry age and completed grade have been controlled for. At the same time, our results document a relationship between family background and birthdate, in that children from families with higher socioeconomic status are disproportionately born during warmer seasons. Nevertheless, we also present indirect evidence that the impact of birth season on performance that materialises through the socioeconomic status of students is negligible.

Finally the evidence produced in this paper has a bearing on the long-standing debate about the validity of birthdate and compulsory school regulations either as an instrument for schooling in the returns-of-education equation, or as an instrument for entry age in the educational production function. As said our evidence dismisses the relevance of pure birth season effects, and plays down the importance of correlation between birth season and socioeconomic status, as drawbacks of this instrumentation strategy. At the same time, however, we present evidence of an important role of the interaction between birthdate and school entry regulations in selection of students into grade retention, early and late entry. Such role calls into question the validity of that same strategy.

The paper unfolds as follows. Section 2 gives an overview of the aforementioned channels between quarter of birth and educational performance, by way of a literature review. Section 3 explains how we tackle a key empirical issue in this context: endogeneity of entry age in the educational production function. Section 4 describes the data used, the computation of main variables and presents some descriptive evidence. Section 5 presents some preliminary regressions for the impact of socioeconomic background on the birthdate of students. Section 6 draws the core empirical results, regarding the impact of absolute and relative entry ages, pure birth date effects and the interaction between entry age and other variables such as socioeconomic background. Before concluding (section 8), section 7 documents the role of the gap between birth month and cut-off month on the probability of selection of students into grade retention and early and delayed entry.

2. Channels between season of birth and school performance: A brief literature review

As a whole, three main channels through which seasonality of birth could impinge on academic performance have been identified.

1. **The Age Channel.** Children are born at different times throughout the year, while typically school begins at a defined calendar date, with an annual admissions policy. Therefore, there will be very different levels of maturity within the same class, which arguably can turn out into a disadvantageous position for younger children. Studies that look at performance by age cohort at an early stage usually point in that direction. For instance, Sharp (1995) conducted a study in England for children between age 6 and 7, gathering data from 14 local education authorities and found that the youngest in the year group had a lower performance compared to the oldest children. These results held across English, Mathematics and Science. At the same time, there is evidence that such initial disadvantage progressively fades away as children get older (Sharp and Hutchison (1997), Hutchison and Sharp (1999)). It has been hypothesized that younger children have worse academic performance due to reduced maturity or cognitive development when they enter school, leading to a slower accumulation of skills vis-a-vis their older peers. This is the reasoning behind the rise in school entry age documented by Deming and Dynarski (2008) for the US, attributable to changes in entry regulations and mostly to parents initiative to delay enrolment with the expectation of getting their children into an advantageous position. A large body of

academic and non-academic literature has investigated this topic, focusing on the impacts on educational achievement and later outcomes. Much of this literature, however, does not explicitly distinguish between absolute age and relative age i.e. whether the effect is driven by being older and mature *per se* or relative to one's classmates (see below for the exceptions). In this case, the estimates are likely to capture a mixture of both effects - for these analyses generally use single-country samples in which students are likely to be younger or older in both senses.

Focusing on work that takes into account the endogeneity of entry age (or current age), several studies have found positive effects of entry age on achievement. Fredriksson and Oeckert (2005), for Sweden, conclude that older entrants perform better and also that children from families with weaker educational tradition have more to gain from starting school later. This study considers separately absolute and relative age and concludes that both impact positively achievement, but the first variable is the key channel. Strom (2004), for Norway, Datar (2006) and Robertson (2011), for the US, Jurges and Schneider (2005) and Puhani and Weber (2006), for Germany, Kawaguchi (2009), for Japan, McEwan and Shapiro (2008), for Chile, Smith (2009), for Canada, Crawford *et al.* (2007), for the UK, Ponzo and Scoppa (2014), for Italy, Zweimueller (2013) and Schneeweiss and Zweimueller (2014), for Austria, and Nam (2014), for Korea, all found a positive effect of entry age on academic performance. Bedard and Dhuey (2006) and Sprietsma (2010) present evidence for groups of countries (considered separately) using, respectively, TIMSS and PISA data, and establish a positive link between age of students and achievement that they interpret as a relative age effect. There are, however, differences among these studies as far as the outcome variables are concerned and thus the results presented have different implications in terms of persistence of the effects. Some contributions document positive effects on later and post- school outcomes, namely Kawaguchi (2009) (completed years of education and earnings), Bedard and Dhuey (2006) (likelihood of attending university), Crawford *et al.* (2007) (scores at age 16 and higher education participation), and Zweimueller (2013) (higher education participation, occupation and earnings). However, Robertson (2011) concludes that the effects fade away over the longer term, and Nam (2014) finds no impact at the end of secondary education and on later outcomes.

In contrast to this evidence, Fertig and Kluwe (2005), for Germany, and Dobkin and Ferreira (2010), for the US, conclude for little or no causal relationship between entry age and educational attainment (the latter study also as regards labour market outcomes). Black *et al.* (2011), for Norway, document small negative effects of starting school older on IQ scores at age 18, as well as on earnings at age 30. Elder and Lubotsky (2008), finding a quick and sharp decline in the advantage of older children at the beginning of kindergarten, assign that initial advantage to skills accumulated *prior* to school entry. This latter study considers explicitly absolute and relative age, concluding also that having a lower relative age may lead to better test scores, but simultaneously to greater grade retention. Cascio and Schanzenbach (2007), using US data, find that being relatively younger within the same cohort is beneficial, as having older classmates on average improves educational outcomes up to eight years after kindergarten. Peña

(2017), for Mexico, decomposes an overall positive effect of being older into a positive impact of age at test and a negative one of the position of students within the age distribution.[2]

2. **The Socioeconomic Channel** - The hypothesis that social background factors may be associated with season of birth has been studied for many years. Warren and Tyler (1979) were among the first authors to associate family background with seasonality of birth. This study - for the US - reported a higher proportion of births to lower status women, especially in the non-Caucasian group, during August (see e.g. Bobak and Gjonca (2001), Kesterbaum (1987) and Seiver (1989) for work in the same vein). The discussion about the use of season of birth as an instrument for schooling in a returns-of-education equation (Bound *et al.* (1995) and Bound and Jaeger (2000)) has reopened this debate. If season of birth is correlated with family background, then it suffers from the same problem as the instrumented variable. Recently this topic has been studied in Buckles and Hungerman (2013) who tested the hypothesis that children born at different times in the year are conceived by women of different socioeconomic strata. They reached evidence that seasonality in birth is driven by high socioeconomic status women planning births away from

2. Sykes *et al.* (2009) conducted a comprehensive literature review on the relationship between age and school performance and identify a channel we do not consider: the length of schooling effect. If all students have the same age when they enter school (i.e. through a staggered admissions policy), younger ones within the same class may be in a disadvantageous position, because they have the least amount of time spent in school.

Winter.

3. **The Medical Channel.** This hypothesis has been raised in biology, psychology and medicine and states that season of birth may affect children's intellect or potential ability. More specifically such work has documented an association between birthdate and certain diseases or personality traits (e.g. autism, dyslexia, extreme shyness, risk for suicide). For instance, Livingston *et al.* (1993) reported a peak of dyslexia for children born in Summer months in the US. Lerchl (2004) in a study for Germany concluded that life expectancy is significantly higher for people born between October and December. Mechanisms underlying this channel include higher prenatal exposure to certain diseases (see Orme (1962), Orme (1963), Sham *et al.* (1992), Almond (2006)) or weather conditions (Gortmaker *et al.* (1997)). This sort of literature is rather heterogenous and thus the predictions as to the incidence throughout the year of the various phenomena vary substantially. Nevertheless, such phenomena could impact on students' ability to learn and, by implication, educational achievement.

3. Key aspects of the approach followed in this paper

3.1. Evidence based on a pooled multi-country database

A key feature that distinguishes our paper from the previous literature is the fact that it is based on *pooled* multi-country data. Bedard and Dhuey (2006) and Sprietsma (2010) also used data from international examination programs, but conducted country-by-country analyses. We rely on cross-country variation to differentiate the effects of relative age vis-a-vis absolute age,[3] on the one hand, and vis-a-vis season of birth, on the other. Take the example of a country in which students eligible to enrol in a given school year are those who reach age 6 until the end of December. Suppose the school year starts at the beginning of October: the youngest students in relative terms are those born in the last quarter of the year, who will typically also be the youngest in absolute terms (still enroling at age 5). However, when data for many countries with different cut-offs and starting age regulations are put together, all cohorts of relative age will be represented for each absolute enrolment age and season of birth.

Furthermore, given the «fixed-age» nature of the PISA program, multi-country data are also useful to separate out the impacts of absolute entry age and schooling which are correlated. Say, students enroling at the prescribed age from a country with school entry at 7 will have less schooling than those from a country with entry age at 6. Therefore, one must hold schooling fixed, when measuring the impact of entry age. We thus control for completed grade and the number of months in the current grade in the regressions (as explained in section 6.1), taking mainly advantage of two sources of variation as far as on-track students are concerned. Firstly, in some countries, completed grade varies among those students because the time-span (usually a 12-month period) for the sampling of students in PISA does not coincide with the intake for a given school year. Secondly, tests may not be taken precisely at the same date by all students, within or between cycles, or the school year begin at a different

3. Given that we infer relative age in an indirect way, as explained in section 4.2.

date, which is reflected upon the months in the current grade. This last sort of variation is much enhanced by the use of cross-country data. Also in this respect our approach contrasts with that in Sprietsma (2010) who confines the sample to the students enroling in the same school year.

In the context of a multi-country sample, we must take into account that test scores are affected by many factors that are country-specific, for example, economic development, educational systems and policies, weather and geography, and parenting styles. We consider country fixed-effects in order to capture such factors, as described below. By the same token, we consider PISA-cycle fixed-effects to capture changes in scores from one PISA cycle to the other, holding the rest constant.

3.2. Dealing with endogeneity of age in the educational production function

The key difficulty in the estimation of the impact of entry age or actual age on achievement stems from endogeneity of these variables caused by correlation with omitted determinants of achievement, notably student ability. Less able children may show less maturity or developmental problems and are more likely to have their school entry delayed. If age at test - instead of age at entry - is the variable considered, such an effect is more marked because repeaters are on average older than non-repeaters when taking the test. Conversely, children abler than average may appear intellectually more mature and enroll earlier than prescribed. Most studies enumerated section 2[4] tackled such endogeneity by instrumenting observed entry age (or age at test) by means of a measure of *assigned* entry age, computed on the basis of information about school cut-off rules and birth dates.

Our empirical strategy to estimate the impact of entry age on attainment takes advantage from our being in position to distinguish the students who entered school at the assigned age and attend the expected grade (*on-track* students) from the students who entered school earlier or later than prescribed or had a regular entry but failed at least one grade (*off-track* students) - see section 4.1. We can flag early and late enrolment and grade failure through the inclusion of binary variables and indirectly control for the negative co-movement between age and ability.[5]

We also include family regressors and birth season variables in the educational production function to capture the two other channels - family and pure birth season effects - described in section 2. Indeed, the omission of those (if correlated with entry age) may create an endogeneity issue similar to the one with ability. The arguments put forward by Bound *et al.* (1995) and Bound and Jaeger (2000) to question the validity of using birthdate as an instrument

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4. Those for Norway and Japan are an exception as the authors argue that this sort of endogeneity does not arise, given that cut-off rules are fully enforced and students are not retained. This claim is supported by the figures for these countries shown in our Appendix.

5. By implication our results should be read as applying to students who entered school when prescribed and progressed continuously up to the time of PISA assessment. For robustness sake, we also present results for a sample confined to that group of students (representing about 85 percent of the total).

for schooling, also apply to using assigned age as an instrument for observed age. Besides the socioeconomic channel described above, other literature has put forward reasons for a correlation between family background and school entry age. Puhani and Weber (2006) suggest that ambitious parents may press for early enrolment of their children, while Deming and Dynarski (2008) raise the possibility (for the US) that richer families may hold their children out of school for an additional year in order to let them gain maturity.[6]

Incidentally, in this paper (section 7) we provide evidence that birthdate impacts on the selection of children to early and delayed enrolment and grade retention. This is another source of association between assigned entry age and attainment, which also questions the exogeneity of assigned age whenever grade retention and early and late entry are not controlled for.

Given the set of controls included, we can make a case for the exogeneity of actual entry age (absolute and relative) in the education production function and proceed with estimation by OLS.

4. PISA database, computation of the main variables and some descriptive evidence

We consider data for four consecutive PISA cycles: 2003, 2006, 2009 and 2012 that were put together into a single database. There are maths, reading and science tests in each cycle. We use maths scores in the analysis, but the results

6. If there is birth date targeting by some parents to ensure that their child is among the oldest in class, as studied by Shigeoka (2015) for Japan, even assigned age can be affected by this sort of endogeneity.

- both descriptive and econometric - remain very similar when reading scores are used (these are available upon request). Further, the databases include individual, socioeconomic and school data, computed on the basis of student and school questionnaires. The first step consisted in ensuring comparability of such data across the years (e.g. making sure each variable had the same categories in every year and recoding variables in order to have the same identification). With a few exceptions which will be noted, all the variables used are available for every PISA cycle. In addition a few missing values for the socioeconomic regressors have been imputed through a regression procedure.[7]

4.1. Identification of students on-track and off-track

The starting point for identifying the students who attend the expected grade, given their country and birthdate, and those who do not because they entered later or earlier than expected or otherwise failed a grade, was to determine the cut-off date for each country in the initial sample. This task was difficult because there is a considerable leeway for parents and teachers in the application of school entry regulations in most countries. Sometimes it is the law itself that grants such a leeway: for instance, in Portugal it is mandatory for pupils to enrol in the first grade for children born until September 15 of the year they reach age six, while for those reaching six thereafter, but before December 31, enrolment may by postponed by one year if parents and teachers consider it appropriate. Even when the laws appear clear-cut, there is some flexibility in

7. Core imputation variables were country, gender, age and difference to modal grade; see Pereira (2010), Appendix 2, for more details on this procedure.

almost all countries. We follow an empirical approach to determining cut-off dates,[8] and its degree of enforcement that can only be assessed by looking at the data.

We proceeded in the following way. For each country, we tabulated the distribution by grade of the students born in a given month and year, excluding those who reported they had repeated a grade at least once (and those for whom such information was missing). Information on grade failure - broken down by retention at ISCED 1 and 2, once or two times or more - is available for all PISA cycles except 2006. This cycle could not be used in this analysis of cut-offs, but this is not an issue as cut-off rules have been stable over time in almost all countries analysed. The sequence of 12 consecutive months that encompass the bulk of students attending a given grade defines the standard time-span for the intake for each school year in a country. The last month of this period is the cut-off month. In a country that enforces strictly the cut-off rule, such 12-month intake period will comprise nearly 100 percent of the non-repeater students attending a given grade, and this percentage will still be high in the countries where a cut-off date is enforced to a large extent.

With cut-off rules and birthdate information in hand, we know the prescribed grade for each student in our sample. The non-repeater students attending that grade were assumed to be on-track - i.e. to have enrolled at the prescribed date and progressed without being withheld - when taking the PISA test. A minority of students attending the expected grade but who were

8. This leads to virtually identical conclusions as the institutional information about cut-offs, for the limited set of countries for which such an information is available.

retained, and thus presumably enrolled earlier than required, were excluded from the sample (more generally, we dropped the students for whom both grade failure and early or late entry applied). If the student was above the expected grade, we assumed that he or she enrolled before the mandatory date. Those below grade were assigned to delayed school entry or grade failure on the basis of information about grade retention. In keeping with the stated above, the students who were two grades below the expected one and had been retained only once, and thus presumably entered school later than prescribed, were dropped. Moreover, we did not considered students with enrolment anticipated or postponed by more than 1 year or who had failed more than 2 grades, in order to rule out abnormal situations and mistakes in the data.

Given that we rely on information about grade retention and such information is not available for the 2006 PISA cycle, this cycle was entirely dropped in the sample used to study the impact of birthdate on achievement, though it was still used in the sample used to asses the impact of socioeconomic background on birthdate.[9] We present in an appendix tables with country data for the distribution of on-track and off-track students.

9. For the remaining cycles, when the grade retention indicator was missing and the student was below the expected grade, we had to drop the observation as well - because we could not tell late entry from grade failure. If the student was in the expected grade, we left the observation and assumed that the student had never been held back, because the loss of information in doing otherwise seemed more problematic than to have a small number of students misclassified (less than 10 percent of students in the expected grade for whom the indicator is available report to have repeated - a figure that may also include mistakes in the data).

4.2. Computation of absolute and relative entry age

We now explain the computation of entry age variables. The remaining variables used will be described when we present the econometric specifications.

We computed relative entry age in an indirect way, using the birthdate and cut-off rules. As far as regular entrants are concerned, pupils born in the cut-off month were given relative age 0, pupils born in the month before were given relative age 1 and so on up to 11 (the approach of Bedard and Dhuey (2006) and Sprietsma (2010)). In case of early or late enrolment, the indicator was shifted accordingly: for students who entered in the school one year before (after) the prescribed date that figure was shifted by -12 (+12) months.

Absolute entry age - defined as the age at the beginning of the first grade - was computed putting together the birthdate and age at test for each student with institutional information collected by us about the starting month of school year in each country (see the appendix). Figure 2 shows the distribution by entry age of students entering at the prescribed date (including on-track students and repeaters), and students with anticipated and delayed entry.

4.3. Sample used and results concerning seasonality of achievement

We restrict the sample to the countries where cut-off dates are enforced to a large extent, so that the age distribution of students enrolling in a given school year roughly spans one year - and the relative age indicator is more likely to capture the actual age rank of the student. This is important given that we do not observe actual relative ages of peers attending the same class or school (Cascio and Schanzenbach (2007)). We used two criteria for country

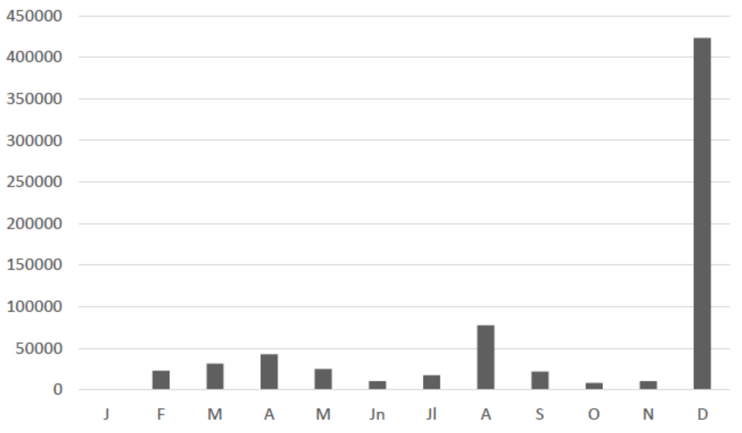

FIGURE 1: Distribution of students by cut-off dates

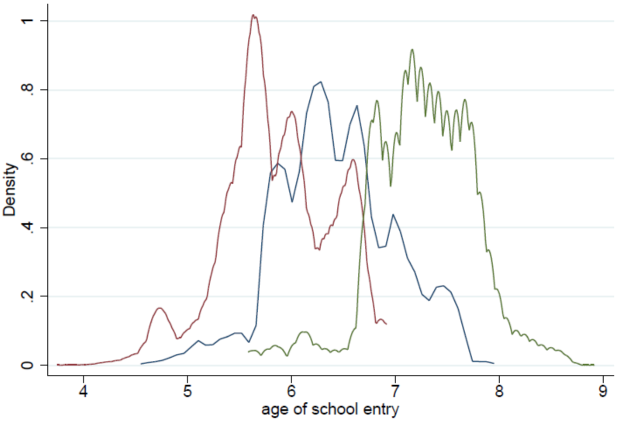

FIGURE 2: Distribution of students by entry age - regular (blue), anticipated (red) and postponed (green) entry

selection (i) a discernable cut-off date pattern, that is, one could identify the abovementioned 12-month sequence, and (ii) at least 80 percent of students entering at the prescribed date. These two criteria led us to discard 30 countries out of the initial 77 with data for at least one PISA cycle. Countries with regional cut-off rules had to be discarded as well, unless PISA data allowed to allocate students to the relevant regions, as it happens for Australia and UK (but not, for instance, for Germany and Canada). The tables in the appendix present country data for cut-off months and enforcement rates, as following from our analysis. Figure 1 shows the distribution of students in accordance with the cut-off dates of the respective country.

We thus base the analysis on data from 47 countries. The relevant sample for the regressions measuring the impact of socioeconomic background on students' birth season comprises 954,450 students. The identification of the students on-track and off-track leads, as explained, to the loss of a number of observations (in particular, the full 2006 cycle), so that the remainder of the analysis is based on 691,181 students of which 83.2 percent were on-track, 10.2 percent failed a grade, and 4.5 and 2.1 percent enrolled, respectively, later and earlier than prescribed.[10]

Figure 3 shows the profile of average math scores[11] by birth month (percentage, comparing to January). We also present the same results confining the sample to on-track students (Figure 4), as achievement differs among on- and off-track students and the distribution of individuals by birth month within these groups is not uniform (see section 7). Note that in computing these profiles, we already control for the interaction between countries and PISA cycles, in order to ensure that the PISA sampling process is not introducing an additional source of correlation between birth month and performance. For instance, if students are not sampled uniformly across birth months in a given country/cycle, this may bring about correlation between birth date and

10. Note that we exclude from the sample the students for whom grade failure overlaps with early or late entry.

11. Student scores are computed as an average of the respective 5 plausible values.

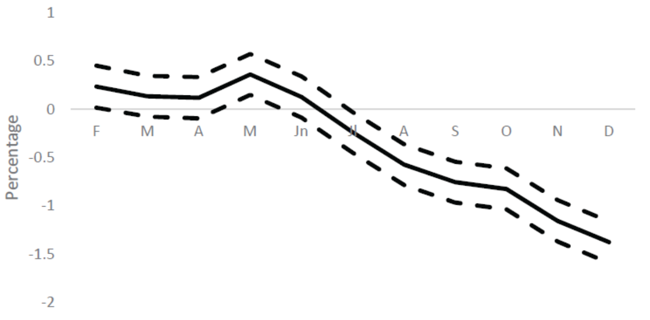

FIGURE 3: Average performance by birth month (difference to January) - all students

Note: Coefficients of monthly dummy variables in a regression where math scores (in logs) are the dependent variable, controlling for the interaction between country and pisa cycle. The chart shows the point estimate and the 95 percent confidence interval.

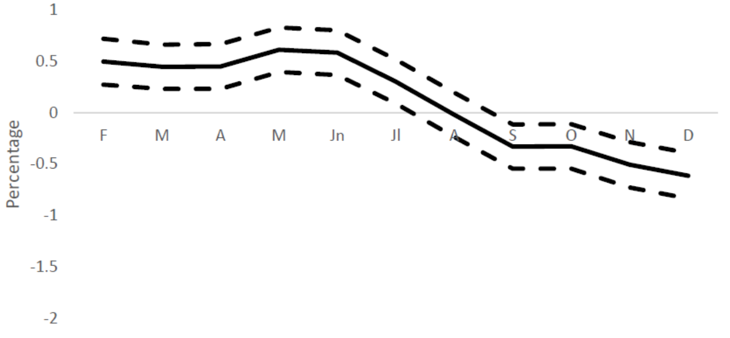

FIGURE 4: Average performance by birth month (difference to January) - on-track students

Note: Coefficients of monthly dummy variables in a regression where math scores (in logs) are the dependent variable, controlling for the interaction between country and pisa cycle. The chart shows the point estimate and the 95 percent confidence interval.

performance (via correlation between country/cycle and performance). The mentioned controls are intended to rule out such effects.

The charts indicate that there is an association between birth month and performance in PISA data - students born at the end of the year (late Autumn) perform worse than their peers. The maximum gap is vis-a-vis the students born at the beginning of the year and stands at around -1.5 percent. The fact that students born in December have the worst outcome suggests a relationship with cut-off dates - given that Figure 1 shows that the December date is predominant in this respect - and a potential role of relative age. When the sample is confined to on-track students, the gap becomes more compressed as late entrants and repeaters, who perform below average, are over-represented in the months prior to cut-off. The profiles in the two charts are nevertheless similar.

It is worth noting that the relationship between birth month and the outcome variable in the charts is the opposite to the one in Angrist and Krueguer (1991) and related literature which document that in the US students born at the beginning of the year can drop out after completing less schooling and consequently have lower wages. The difference to our results stems basically from the fact that we look at performance while at school, a different outcome variable. Nevertheless, even for length of schooling, the results in Angrist and Krueger are likely to be specific to the US, where compulsory schooling laws are defined in terms of age, whereas in most countries these are defined in terms of completed grades (as already noted e.g. by Black *et al.* (2011)).

5. Birthdate and socioeconomic background

Our econometric analysis addresses firstly the relationship between seasonality of birth and socioeconomic background, documented in works such as Warren and Tyler (1979) and recently reintroduced by Buckles and Hungerman (2013). This literature - while finding evidence of that relationship - has not reached unanimous conclusions as to its pattern. Buckles and Hungerman (2013) conclude that higher socioeconomic status women plan births away from

Winter, which is generally in line with the idea that individuals born in Winter in the US have worse outcomes. However, in contrast, earlier work by Warren and Tyler (1979) found that lower status women tended to give birth to children during Summer. Our database provides an adequate setup to study this issue on a cross-country basis, given that we have data on birthdates and socioeconomic characteristics of almost 1 million students from a large set of countries (see the appendix). We assess the impact of family variables on the probability of being born during the warm (vs. cold) season, in accordance with the findings of previous literature.

We run probit regressions pooling country data with the dependent binary variable given by birth during the warm season (ws_{ijt}, where i indexes the student, j the country and t the PISA cycle). The sample is based on all PISA cycles from 2003 to 2012. The warm season corresponds to the period between April and September in the Northern Hemisphere countries and between October and March in the Southern Hemisphere.[12] Family variables ($Family$) include binary variables for the number of books at home, parental occupation[13] and parental education. They include further native (vs. immigrant) status and regressors for with whom the student lives, the latter being unavailable for the 2006 PISA cycle. Interactions between country ($Country$) and PISA

12. Note that in section 6, when studying impact on attainment, we define birth season dummies in a different way, making them coincide with the three school terms.

13. Due to changes in ISCO coding, introduced in 2007, we had to match the old and the new codes between PISA cycles in order to have consistent data about parental occupation over time.

cycle ($Cycle$) fixed-effects are added as control variables to all regressions. The benchmark regression equation is

$$P(ws_{itj} = 1|\boldsymbol{x}_{itj}) = \Phi(\boldsymbol{x}_{itj}\boldsymbol{\beta}) = \Phi(\beta_0 + Family_{itj}\boldsymbol{\beta}_1 + Country_j * Cycle_t).$$

Table 1 presents the results as marginal effects. Regressions (1), (2) and (3) include the socioeconomic status variables i.e. books at home, parental occupation and parental education standing alone, and regression (4) all these variables together. The first three regressions show the same pattern: the probability of a student being born during the warm season increases with the socio-economic status of parents. Moreover, across the different socio-economic status proxies, the differences in this probability are statistically significant for almost every category vis-a-vis the omitted one. When all socio-economic status variables are put together, parental occupation and books at home no longer provide statistically significant results, or move to the brink of non-significance. In contrast, education dummies retain the statistical significance and their impact on the probability of being born during the warm season hardly changes in comparison to regression (3). Therefore, after controlling for parental education, occupation and number of books at home are largely redundant.

Explanatory Variable	(1)	(2)	(3)	(4)	(5)
Books at home					
≤ 25					
25 – 200	0.300**			0.216*	0.282**
	0.12			*0.13*	*0.14*
> 200	0.329**			0.199	0.093
	0.15			*0.16*	*0.19*
Parental Occupation					
Blue Collar / Low-Skilled					
Blue Collar / High-Skilled		0.290		0.260	0.212
		0.18		*0.18*	*0.21*
White Collar / Low-Skilled		0.296*		0.184	0.092
		1.75		*1.05*	*0.48*
White Collar / High-Skilled		0.348**		0.143	0.091
		0.16		*0.17*	*0.19*
Parental Education					
Up-to-Primary					
Lower Secondary			0.830***	0.809***	0.872***
			0.29	*0.29*	*0.34*
Upper Secondary			0.940***	0.877***	0.935***
			0.26	*0.26*	*0.30*
Tertiary			1.112***	1.027***	0.981***
			0.20	*0.27*	*0.31*
Other Variables					
Monoparental Family					-0.260
					0.18
No parents at home					-0.006
					0.31
Native	0.172	0.194	0.165	0.123	0.150
	0.20	*0.20*	*0.20*	*0.20*	*0.23*
Students	954450	954450	954450	954450	721836

TABLE 1. Estimated impacts on the probability of birth during the warm season

Note: Marginal effects estimated on the basis of probit regressions (contrasts of preditive margins, in percentage), controlling for the interaction of country and pisa cycle fixed-effects. Standard deviations in italics; significance at *10 percent, **5 percent and ***1 percent.

Overall our findings are in line with Buckles and Hungerman (2013), given that students with higher socio-economic background are more likely to be born during the warm season. Additionally, the fact that the proxies of education rather than the proxies of wealth are the driving factor is suggestive that it is the ability of timing births away from winter that matters instead of the variation in birth time preferences across socioeconomic strata. It may be that families from all backgrounds prefer to have their children in warmer seasons, but low-educated parents are worse at achieving that. It is also interesting to note that the probability of having children during the warm season is higher by roughly one percent for the three education cohorts above up-to-primary education. Therefore, the ability of timing births rightly seems to fall clearly behind only for parents with the lowest education level, not being very different for the remaining cohorts.

Regression (5) adds the variables capturing family composition and is run on the basis of a reduced sample, without the 2006 PISA cycle. These additional variables are not significant. We do not find evidence that children living with only one of the parents or none of them - who could disproportionately have been born by teenagers or unmarried mothers - are less likely to be born during the warm season. It is however possible that the family environment when PISA tests are taken (age 15) already bears a not so strong relationship with that at conception due, say, to parent's divorce in the intervening period.

6. Birthdate and achievement

6.1. Specification

This section investigates the importance of the different channels through which birthdate may affect achievement. The outcome variable is PISA math scores, y_{ijt}, taken in logs, of student i, from country j, assessed in PISA cycle t. Given that the dependent variable is measured in logs, the coefficients can be read as percentage impacts. The sample excludes now the 2006 PISA cycle (see section 4.3).

The dependent variables include, in the first place, absolute and relative age of school entry (*AbsAge* and *RelAge*). Absolute age is computed on the basis of information by student, as described above, not of institutional information at the country level - otherwise one could not disentangle its impact from the remaining country-specific ones, captured by the respective fixed-effects. This variable enters the regression in the form of indicator variables for enrolment at age 5 or younger, age 6, and age 7 or older. Most students in each country spread between two cohorts of absolute entry age. Relative age is measured in months and varies between 0 and 11, respectively, for the youngest and the oldest students who entered school at the prescribed age (see section 4.2 for more details).

Family is a vector including gender and the socioeconomic regressors available for all the PISA cycles: books at home, highest parental education and occupation and immigrant status. The inclusion of these regressors guarantees, in particular, that birthdate dummies will not capture the impact of socioeconomic background on achievement, given the correlation of such background and birthdate (as documented in section 5). *Grade* is a vector of dummies for the grade completed by the student, including, in addition, the number of months in the current grade. Given the inclusion of grade, the coefficient of absolute entry age will not be affected by the correlation between that variable and the grade the student attends.

The regression comprises ability controls in the form of dummies for late entry, grade failure and early entry ($Offtrack$). Countries differ a lot in terms of degree of enforcement of cut-off dates, as well as retention practices (see appendix). That is, being a repeater in a country with a negligible retention rate will signal, on average, a lower ability level than being a repeater in a country where, say, on average around 1/5 or 1/4 of all students fail a grade, at least once, up to the completion of lower secondary education. Therefore, we interact off-track dummies with country fixed-effects ($Country$).

In order to capture pure birth-season effects, we created birth-season dummies ($BirthSeason$) that group children in accordance with the three-term structure that underlies the school year in most countries (borrowing from Sykes *et al.* (2009)). The seasons are Autumn-Winter (omitted group), Winter-Spring and Spring-Summer, corresponding, in the Northern Hemisphere, to September-December, January-April and May-August.[14] Evidence presented in section 7 indicates that the birth month is strongly correlated with early and late entry and grade failure. Therefore, we interact birth-season and off-track dummies in order to better account for such correlation, notwithstanding the inclusion of off-track dummies interacted with country fixed-effects. A final set of controls consists in the interaction between country and PISA cycle ($Cycle$) fixed-effects. The benchmark regression equation is

14. In the Southern Hemisphere, we took for the same seasons, respectively, the periods March-June, July-October and November-February.

$$lny_{ijt} = \beta_0 + AbsAge_{itj}\boldsymbol{\beta}_1 + RelAge_{itj}\beta_2 + Family_{itj}\boldsymbol{\beta}_3 + Grade_{itj}\boldsymbol{\beta}_4$$

$$Offtrack_{itj} * Country_j\boldsymbol{\beta}_5 + BirthSeason_{ijt}\boldsymbol{\beta}_6 +$$

$$+ BirthSeason_{ijt} * Offtrack_{itj}\boldsymbol{\beta}_7 + Country_j * Cycle_t\boldsymbol{\beta}_8 + \varepsilon_{ijt}.$$

6.2. Impact of school entry age on achievement

Table 2 shows, in the first place, the results of the estimation of two preliminary specifications that are restricted versions of the equation above. Regression (1) includes birth season effects interacted with off-track dummies only, besides the two other groups of control variables: interaction of country and PISA cycle fixed-effects and interaction of off-track dummies and country fixed-effects. Regression (2) includes, in addition, gender and socioeconomic variables. Birthdate effects are clearly significant in the first regression and estimates indicate that on-track students born in Autumn-Winter (the omitted group) perform worse than their counterparts born in the two other seasons, by around 1 percent. The results are much in line with the seasonality of attainment shown in Chart 4. The important result coming from the second regression is that the estimates of the birth season effects hardly change when family background is controlled for. The association between family and birthdate, documented in section 5, is not strong enough for making a sizeable difference as far the measured impact of birth season on performance is concerned.

	(1)	(2)	(3)	(4)	(5)
Relative age (mts.)			0.09***	0.10***	0.09***
			0.01	*0.01*	*0.01*
Entry age 6			-0.07	-0.16*	-0.06
			0.09	*0.1*	*0.09*
Entry age 7 or above			-0.62***	-0.84***	-0.83***
			0.16	*0.17*	*0.15*
Winter-Spr. (ontrack)	1.04***	1.02***	-0.05	-0.05	0.01
	0.06	*0.05*	*0.07*	*0.08*	*0.07*
Spring-Sum. (ontrack)	0.81***	0.75***	0.02	0.08	0.07
	0.06	*0.05*	*0.07*	*0.07*	*0.07*
8th grade completed			8.10***	5.00***	7.93***
			0.14	*0.31*	*0.13*
9th grade completed			12.60***	9.45***	12.33***
			0.2	*0.35*	*0.19*
10th grade completed			17.16***	13.85***	16.88***
			0.28	*0.41*	*0.27*
11th grade completed			24.29***	21.03***	23.40***
			0.68	*0.75*	*0.59*
Current grade (mts./9)			1.28**	1.19**	1.33***
			0.51	*0.54*	*0.51*
Books at home: 25-200		7.31***	7.41***	7.59***	7.44***
		0.05	*0.05*	*0.05*	*0.05*
Books at home: > 200		13.20***	13.35***	13.60***	13.40***
		0.06	*0.06*	*0.07*	*0.06*
Delayed entry					-2.59***
					0.4
Grade failure	(interacted with country dummies)				-10.40***
					0.39
Anticipated entry					2.81***
					0.41
Students	691181	691181	691181	575116	691181

Table 2. Estimated impacts on performance - benchmark specification

Note: Dependent variable is math scores (average of the 5 plausible values) in logs; coefficients expressed in percentage. The sample in regression (4) comprises ontrack students only. Additional regressors: interaction of country and pisa cycle fixed effects - all regressions; gender, parental education, parental occupation, monoparental family, absence of parents at home and immigrant dummies - regressions (2) to (5). Standard deviations in italics; significance at *10 percent, **5 percent and ***1 percent.

The estimates for the benchmark specification are shown in column (3) and, on the basis of a sample restricted to on-track students in column (4), now dropping the interactions with off-track dummies. In regression (5), we come back to the full sample but do not interact off-track dummies with country fixed-effects, so that one can estimate «overall» coefficients for such dummies and assess the plausibility of signs and magnitudes.

Before going into the interpretation of individual coefficients, we note that results in regressions (3) and (4) are rather close,[15] suggesting that indirect ability controls avoid any substantial bias brought about by the inclusion of off-track students in the main sample. The coefficients of off-track dummies in regression (5) are statistically significant and have the expected signs given correlation with student ability. The coefficients of delayed entry and grade failure are negative, the second one having a larger size, presumably reflecting the fact that grade failure, particularly as students become older, signals a greater lack of ability. The early entry coefficient is, by contrast, positive and of a similar magnitude as the late entry one.[16] These coefficients actually concern the students born in Autumn-Winter (the omitted group), given that we keep the interaction of off-track with birth season dummies in regression (5). If we remove such an interaction as well, the estimates - not shown - remain rather similar (coefficients of 2.2 for anticipated entry, -10.9 for grade failure and -3.1 for delayed entry) and statistically significant.

15. Except for grade that shows a much stronger impact in regression (3), due to the very different level of achievement in the omitted group - 7th or a lower grade completed - in each of the two regressions. This group has around 27000 students in the benchmark regression, against 3000 in the regression confined to on-track students.

16. Note that the off-track dummies will also capture the impact of the events they flag, except as regards changing the grade the students attends, given that this variable is controlled for in the regression.

We first look at the impact of relative age. Results indicate a long-term impact of initial maturity differences of students that at age 15 translates into a difference in performance around 1 percent between the youngest and the oldest students. Such an evidence is in line with studies that found persistence of birthdate impacts until secondary education (e.g. Crawford *et al.* (2007), Bedard and Dhuey (2006) and Sprietsma (2010)), and supports its interpretation as resulting from age rank of students vis-a-vis the peers. At the same time, it deviates from papers such as Cascio and Schanzenbach (2007) and Elder and Lubotsky (2008) that found negative impacts of relative age.

As far as magnitudes are concerned, Bedard and Dhuey, using as well an international student assessment database, although country by country, document effects that stand (expressed as a percentage of the average score) at 2 to 7 percent in the 4th grade, and 1 to 5 percent in the 8th grade. We get a smaller impact but our setting is also quite different, particularly in that we consider simultaneously absolute and relative age and a much wider set of countries. Further we are assessing the impacts at a later stage of schooling (mostly 10th grade and, to a lesser extent, 9th).

The contribution of relative age to performance falls clearly behind that of belonging to an advantaged household - which, as measured by the number of books at home, is almost 8 percent for the intermediate cohort (25 to 100 books) and over 13 percent for the highest (more than 100 books). This result seemingly plays down the relevance of relative age effect in the longer term from the policy viewpoint, comparing with other issues such as the role of socieconomic status. Nevertheless, in countries where the relative age effect interacts with practices such as early grade retention and tracking, as documented below, long-term consequences are enhanced and the issue gains added policy relevance.

We now turn to the results for absolute age. The estimated impact of entering at 6 is statistically not different from that of entering at age 5 or before (omitted group),[17] at a conventional significance level; in contrast, we estimate that entering later, at age 7 or after, is slightly detrimental to performance, featuring a negative impact of about half percent. This evidence stands in contrast to papers such as Fredriksson and Oeckert (2005) and Peña (2017) that concluded for a positive impact of absolute age. While relative age is positively associated with performance, an increase in absolute entry age has either no impact or a small adverse one as it reaches 7. Children are thus disadvantaged not because they are young, but because they are among the youngest. We come back to this issue below, considering the interaction between relative and absolute age.

Our results bear on the debate about postponing enrolment of children eligible to attend school, supposed to allow them to gain maturity and be better prepared to learn. Deming and Dynarski (2008) document for the US a rise in the actual age children enter school, in about one third attributable to changes in legislation in many States and in about two-thirds to individual

17. We experimented with separating out the effects of entering at age 4 vs. 5, but the difference between these two was not significant

decisions of parents and teachers.[18] The evidence we get speaks against delaying mandatory school entry as a general policy, as no gain is to be expected from a rise in entry age, keeping age differences among students constant, while the completion of an additional grade brings about a gain around 5 percent, or even more as one moves up the completed grades. As regards postponing school entry as an individual decision, again the 1 percent gain in performance from belonging to the oldest students in class (i.e. the relative age effect) seems modest in comparison to the gain stemming from attendance of a higher grade. Our findings are thus quite consistent with the idea that postponing school entry postpones learning.

6.3. Pure season of birth effects

The evidence coming from this first set of regressions does not favor the medical hypothesis stated in section 2, as the performance of children born in Winter-Spring and Spring-Summer does not differ from that of their peers born in Autumn-Winter (omitted group) - once the effects of school entry regulations and completed grades are held constant. We do not detect an association between birthdate and educational achievement brought about by unobservable characteristics of students - and one could expect our sample (over 750 000 students) to be a large enough one for such effects to show up, should they exist. Nevertheless, our results, while not supportive of the medical hypothesis,

18. There are, however, countries where the opposite trend prevails: Puhani and Weber (2006) report an increase in school entry age in recent years in Germany, as some Federal States have brought forward the respective cut-off dates.

do not necessarily rule it out. The absence of a measurable effect on educational performance could reflect, for instance, the fact that the prevalence of the various phenomena studied by this literature is not synchronized across the year.

6.4. Interaction between age of school entry, family background and tracking

We now deepen the analysis by considering interactions between relative and absolute entry age, on the one hand, and between these variables and family background and tracking. Given that our interest essentially focuses on students who entered school at the prescribed time and progressed continuously up to the time of the PISA assessment, we now consider on-track students only (sample corresponding to regression (4) in Table 2). Moreover, we bring into the analysis an indicator for tracking that uses information from Brunello and Checchi (2006). This indicator takes on the value 1 when the country has formal tracking prior to the age of 15 (when students are assessed by PISA). As such information is not available for all the countries in the sample, the regression including the tracking indicator is run on a smaller number of countries.[19] Table 3 shows the coefficients of interest only but, as before, we control for season

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19. More specifically, the indicator is 1 for the countries that have less than 15 as the age of first selection into tracks, according to Table 1 of Brunello and Chechi. The countries covered are Australia, Austria, Belgium, Bulgaria, Chile, Czech Republic, Germany, Denmark, Spain, Finland, France, United Kingdom, Greece, Hungary, Iceland, Italy, Japan, South Korea, Luxembourg, Latvia, Netherlands, Norway, New Zealand, Poland, Portugal, Slovakia, Slovenia and Sweden.

of birth, gender, other family regressors than books at home, completed grade, months in current grade and interaction between country and PISA cycle.

	(1)	(2)	(3)
Entry age 6	-0.16	-0.14	-0.24**
	0.15	*0.14*	*0.11*
Entry age 7	-1.41***	-1.59***	-0.71***
	0.26	*0.21*	*0.19*
Relative age (mts.)	0.09***	0.11***	0.09***
	0.02	*0.02*	*0.01*
Relative age * entry age 6	0.02		
	0.02		
Relative age * entry age 7 or above	-0.01		
	0.03		
Relative age * books 25-200		-0.01	
		0.02	
Relative age * books > 200		-0.02	
		0.02	
Relative age * tracking			0.09***
			0.02
Books 25-200 * entry age 5 or under	7.36***	7.39***	
	0.12	*0.13*	
Books > 200 * entry age 5 or under	13.63***	13.69***	
	0.14	*0.15*	
Books 25-200 * entry at 6	7.42***	7.50***	
	0.07	*0.12*	
Books > 200 * entry at 6	13.22***	13.35***	
	0.08	*0.14*	
Books 25-200 * entry at 7 or above	8.27***	8.37***	
	0.11	*0.17*	
Books > 200 * entry at 7 or above	14.74***	14.91***	
	0.14	*0.2*	
Books 25-200 *no tracking			7.66***
			0.08
Books > 200 * no tracking			13.89***
			0.09
Books 25-200 * tracking			8.53***
			0.1
Books > 200 * tracking			14.65***
			0.11
Students	575116	575116	427868

TABLE 3. Estimated impacts on performance - interaction between variables

Note: Dependent variable is math scores (average of the 5 plausible values) in logs; coefficients expressed in percentage. The sample comprises on-track students only. Additional regressors: season of birth, gender, parental education, parental occupation, monoparental family, absence of parents at home and immigrant dummies, completed grade, months in current grade and interaction between country and PISA cycle. Standard deviations in italics; significance at *10 percent, **5 percent and ***1 percent.

Regression (1) in Table 3 investigates the interaction between relative and absolute age, specifically, the possibility that the effects of these variables reinforce each other (suggested e.g. by Sykes *et al.* (2009)). If this were the case, educational disadvantage of younger children should be larger in the countries where formal education starts earlier, and a way of attenuating it would be to postpone the start of compulsory education. The evidence we get, however, does not back this supposition. When we allow the effect of relative age to be differentiated by cohorts of absolute entry age, the average impact of being one month older relative to peers in class does not statistically differ as school entry age goes up (note that the coefficient of non-interacted relative age in this regression captures the effect for children entering at 5 or under). This suggests that the disadvantage faced by the youngest students in class does not stem, say, from a lack of maturity in general or readiness to tackle curricula. Regulations increasing compulsory entry age will not mitigate the relative age effect.

Some authors have suggested that the relative age effect stems from competition among peers, by analogy with sports - the setting where the existence of birthdate effects has been first noted and is best documented (e.g. Bell *et al.* (1997), Musch and Grondin (2008)). There is evidence from this field that the greater the degree of competition and selection involved in a given sport, the larger the observed relative age effect. Our results could square with such an explanation - which in terms of policy implications speaks against an excessive reliance on ranking practices at early stages of child's learning trajectory.

Regression (1) also assesses the variation in the impact of family background by cohorts of absolute entry age (for each of these cohorts, the coefficients of interacted books at home are capturing the effects vis-a-vis the lowest socioeconomic stratum - less than 25 books). Such an impact remains similar for children entering at 5 and under and 6; however, for children entering at 7 or above, belonging to higher socioeconomic strata has a stronger effect on performance. This result complements the evidence that postponing school entry postpones learning, presented in section 6.2, indicating this effect to be more marked for students from disadvantaged families (in line, for example, with Elder and Lubotsky (2008)). When school entry is delayed, children from such families, stay longer in an environment that is less stimulating from the intellectual viewpoint and where they have less educational possibilities outside formal schooling. Nevertheless, this issue seems to arise only in the event of school entry at a relatively later stage.

Regression (2) interacts relative age and family background (the coefficient of non-interacted relative age now capturing the effect for children belonging to the lowest socioeconomic cohort), keeping the interaction of that background with absolute age. The impact of relative age does not appear to be sensitive to changes in the socioeconomic strata from which students come. This reinforces the idea that the relative age effect is not primarily associated with the capacity to learn, in which case one could expect the intervention of family to mitigate it, but instead with competition at school and interaction among peers.

Previous discussion points, in particular, to a probable interaction between relative age effects and tracking, as a mechanism of ranking and selection of children into different paths, occurring in some countries at an early stage (end of primary education). Such an interaction was suggested, but not formally investigated, by Bedard and Dhuey (2006), as one of the factors that could explain the persistence of relative age effects into adolescence. Schneeweiss and Zweimueller (2014) investigate this issue for Austria - a country with early tracking - and concludes for a strong impact of relative age on assignment of students to tracks. Regression (3) provides evidence on this specific issue by interacting relative age (and family background) with the tracking indicator described above.[20] This interaction is statistically significant and suggests an effect of being relatively younger which is almost twice than in countries where there is no tracking before age 15, adding up to a gap in performance between youngest and oldest students in class over 2 percentage points. Our findings thus indicate that tracking tends to reduce equality of opportunity by preserving the role of early maturity differences throughout the educational trajectory, having a similar impact as parental privilege (Brunello and Checchi (2006)). Albeit not central to this paper, we also provide evidence about the interaction between tracking and family background. As expected, the impact on performance from belonging to a favored family is greater in countries with tracking (by about 1 p.p. over around 8 and 14 percent in countries without it, respectively, for the cohorts 25-200 and more than 200 books at home).

20. Note that the effect of the non-interacted tracking indicator is captured by the country fixed-effects.

7. Birthdate, anticipated and delayed enrolment and grade retention

In this section we look at the association between birth month and anticipation or delaying of school entry and grade failure, by documenting some factors affecting the probability of such events (Sprietsma (2010) carries out a similar exercise for grade failure). This is interesting on its own, given that our evidence is presented on the basis of a large multi-country database. In addition, we provide independent evidence to the debate about the inadequacy of quarter of birth as an instrument for school entry age.

We start with by presenting the distribution by birth month of students on-track and off-track (Table 4). A comparison of the figures in the table with the cut-off patterns in Figure 1 indicates a higher proportion of children with anticipated enrolment born just after the months in which cut-off dates peak and, conversely, a higher incidence of delayed enrollment for children born in the period prior to (and including) those months. Exceptions to cut-off regulations do not occur uniformly throughout the year, but are much confined to births around the cut-off dates. This is most visible around December - the month in which cuts-off concentrate, but also around August and April-May. The distribution by birth month of students who failed a grade shows, by comparison, a much less marked pattern.

We now move to a more formal exercise and run probit regressions for, respectively, the probability of anticipated or delayed entry (*ent*) and grade

	On-track	Off-track Anticipated	Off-track Delayed	Off-track Retention	Total
J	8.2	18.0	6.8	7.7	8.3
F	7.6	10.9	7.6	7.0	7.6
M	8.6	7.9	8.8	8.2	8.6
A	8.3	6.7	9.8	7.8	8.3
M	8.6	10.3	8.4	8.3	8.6
Jn	8.4	8.9	7.4	8.2	8.3
Jl	8.9	5.0	8.2	9.1	8.8
A	8.7	7.1	7.6	9.1	8.6
S	8.7	7.6	6.0	8.9	8.6
O	8.5	7.0	7.4	8.6	8.4
N	7.8	5.0	9.4	8.2	7.9
D	7.8	5.6	12.7	8.8	8.1
	100	100	100	100	100
Students	575116	14426	31374	70265	691181

TABLE 4. Distribution of students on-track and off-track by birth month

Note: Students for whom grade failure overlaps with early or late entry excluded from the sample.

failure (*ret*).[21] The key explanatory variable in this regression is the gap between the birth month and the month the student is eligible to enroll in a given school year, according to the rules in each country. This is *assigned* relative age (*AssRelAge*) and enters the regression in the form of a set of dummy variables. It corresponds to *actual* relative age, appearing in the previous regressions, for the students who entered school at the prescribed date (i.e. students on-track and repeaters), but not for early and late entrants. In the regressions for the probability of grade failure we have absolute entry age (*AbsAge*) as an additional regressor, and we look separately at ISCED 1 or ISCED 2 (but not at both levels simultaneously), as the role of relative entry age is conceivably more important in the first case. In all regressions

21. Separate regressions are run for each of these events, the sample comprising on-track students plus the students affected by each of them.

we include gender and the usual family variables (Fam), and the interaction between country ($Count$) and PISA cycle (Cyc) fixed-effects. The regression equations are

$$P(ent_{itj} = 1|\boldsymbol{x}_{itj}) = \Phi(\beta_0 + AssRelAge\beta_1 + Fam_{itj}\boldsymbol{\beta}_2 + Count_j * Cyc_t);$$

$$P(ret_{itj} = 1|\boldsymbol{x}_{itj}) = \Phi(\beta_0 + AssRelAge\beta_1 + AbsAge\beta_2 + Fam_{itj}\boldsymbol{\beta}_3 + Count_j * Cyc_t).$$

Table 5 shows the results for the main variables (average marginal effects). The gap between birth and cut-off months has an important impact on the probability both of anticipated and delayed entry. Children born earlier within the potential intake group are more likely to enter before the prescribed date, but this pattern is clearly more marked for those who have birthday 9 to 11 months before the cut-off month. Conversely children born later - particularly, in the cut-off month or just before it and who would be the youngest in class should they enroll according to regulations - are much more likely to have their entry postponed to the next school year. These patterns are explained by the combined effect of maturity exhibited by children, as a function of date of birth, and regulations being more prone (and teachers and parents more open) to exceptions for the students born in the months around the cut-off date.

As far as the probability of grade retention is concerned, the impact of the gap to cut-off date is still statistically significant and shows a pattern similar to that for delayed entry, but comparatively of a smaller size. Such an impact

	Anticipated entry	Delayed entry	Grade failure ISCED1	ISCED2
Female	0.74***	-1.9***	-1.44***	-2.79***
	0.04	*0.06*	*0.05*	*0.06*
Gap to cutoff				
1 month	-0.03	-6.35***	-1.21***	-0.36**
	0.05	*0.23*	*0.18*	*0.17*
2 months	0.03	-10.52***	-2.08***	-0.51***
	0.05	*0.22*	*0.17*	*0.16*
3 months	0.14***	-13***	-2.69***	-0.74***
	0.05	*0.21*	*0.18*	*0.19*
4 months	0.29***	-14.21***	-3.24***	-0.87***
	0.05	*0.20*	*0.19*	*0.19*
5 months	0.44***	-15.06***	-3.66***	-1.04***
	0.05	*0.20*	*0.18*	*0.19*
6 months	0.62***	-15.46***	-3.98***	-0.99***
	0.06	*0.20*	*0.19*	*0.2*
7 months	1.02***	-16.05***	-4.39***	-1.29***
	0.06	*0.19*	*0.18*	*0.19*
8 months	1.85***	-16.57***	-4.91***	-1.71***
	0.07	*0.19*	*0.18*	*0.19*
9 months	3.15***	-16.79***	-5.04***	-1.75***
	0.09	*0.19*	*0.18*	*0.19*
10 months	5.73***	-16.87***	-5.21***	-1.9***
	0.11	*0.19*	*0.18*	*0.2*
11 months	12.68***	-16.88***	-5.08***	-1.97***
	0.15	*0.19*	*0.19*	*0.2*
Entry age 6			*0.37****	*0.09*
			0.10	*0.14*
Entry age 7 or above			*1.07****	*-0.56***
			0.21	*0.26*
Books at home: 25-200	0.17***	-1.33***	-2.73***	-3.06***
	0.05	*0.08*	*0.07*	*0.08*
Books at home: > 200	0.98***	-1.87***	-3.72***	-4.66***
	0.07	*0.09*	*0.08*	*0.09*
Students total	539833	539922	540797	555715
Students treated	*14426*	*31374*	*25345*	*35870*

TABLE 5. Estimated impacts on the probability of anticipated entry, delayed entry and grade failure

Note: Marginal effects estimated on the basis of probit regressions (contrasts of preditive margins, in percentage). In the regressions for grade failure at ISCED1 or ISCED2, the students who failed grades at both levels are not considered. Additional regressors: parental education, parental occupation, monoparental family, absence of parents at home and immigrant dummies; interaction of country and pisa cycle fixed-effects. Standard deviations in italics; significance at *10 percent, **5 percent and ***1 percent.

operates through the relative age effect (studied in the regressions above) which influences achievement that in turn determines grade retention. However, when children are already attending school, relative age is only one of several factors affecting achievement. The estimated impacts are as expected stronger for grade failure at ISCED1, but are still visible at ISCED 2. In general, the role of relative age in the probability to repeat at early schooling is evidence that teachers do not take (or take only to a limited extent) children's relative levels of maturity into account when deciding whether to hold them back.

Given that assigned age has a role in the selection of students into early and late entry or grade retention and each of these events affects student performance in future, then assigned age will fail the exclusion restriction in a regression with performance as the dependent variable, when such events are are not controlled for. This calls into question the use of assigned age as an instrument to correct for endogeneity of actual entry (or current) age.

Students older when entering school are more likely to fail a grade at ISCED 1. Such an impact is the opposite to the one for relative entry age, an evidence that is in line with the results from achievement regressions above. In contrast, the corresponding coefficients for retention at ISCED 2 are either non-significant (age 6) or on the brink of that (age 7 or above).

Family background has a statistical significant effect on the probability of selection into each of the three events considered. Children from disadvantaged families are more likely to enrol later than the prescribed date, and the other way around for children from wealthier families. This could reflect the impact of family on the development of children prior to schooling, or a preference by wealthier families for a quicker enrolment of their children (as suggested by Puhani and Weber (2006)). Such an evidence is at odds with the trend of late enrolment of children from advantaged households in the US (Deming and Dynarski (2008)). It is however possible that the first factor noted - the developmental impact of family prior to enrolment - predominates over the other factors. As far as grade failure is concerned, results in Table 5 indicate a role of sudents' socieoeconomic background more important than for delayed enrolment, suggesting that family matters particularly when it comes to formal learning.

8. Conclusions

Our paper aimed at addressing the channels connecting birthdate and academic performance in a common framework. In particular, we attempted to disentangle age effects from pure birth season effects, as well as the impacts of absolute entry age and relative entry age.

Age effects essentially materialise through the relative entry age channel, stemming from competitive interaction among students. There are no gains in performance from a rise in absolute entry age - keeping age differences among students constant - which speaks against delaying mandatory school entry as a general policy. Relative age interacts with tracking, but not with the absolute maturity of children and socioeconomic context. Absolute entry age interacts with socioeconomic status, the role of family for performance being comparatively larger for late entrants. Our results do not support the existence of pure birth season effects.

There is evidence of a relationship between family background and birthdate, in that children from families with higher socioeconomic status are disproportionately born during warmer seasons. Nevertheless, we also present indirect evidence that the impact of birth season on performance that materialises through the socioeconomic status of students is negligible.

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Appendix: Country data

Country	Sample birthdate and family background	Sample birthdate and achievement	Beginning of school year (month)	Cut-off month	Cut-off enforcement (%)
Argentina	14,814	9,223	February	June	88.34
Australia	48,703	34,870	January	(+)	87.70
Austria	20,830	15,064	September	August	86.34
Belgium	34,269	23,272	September	December	88.29
Bulgaria	14,216	9,511	September	December	93.25
Chile	17,540	11,395	February	May	85.24
Costa Rica	9,179	8,228	February	October	92.72
Czech Republic	23,355	17,270	September	July	81.72
Denmark	21,376	17,183	August	December	86.37
Spain	81,347	60,661	September	December	98.12
Estonia	14,307	9,392	September	December	90.22
Finland	25,148	20,206	August	December	95.70
France	13,209	12,801	September	December	92.28
United Kingdom	27,057	19,985	September	(++)	96.18
Greece	19,493	14,333	September	(+++)	96.18
Hong Kong	18,595	12,808	August	December	86.46
Croatia	15,181	9,830	September	March	91.41
Hungary	18,573	13,646	September	April	80.70
Iceland	14,157	10,434	August	December	99.44
Israel	15,252	10,432	September	November	87.89
Italy	94,728	68,966	September	December	90.64
Jordan	19,997	12,970	September	December	97.37
Japan	23,096	17,144	April	March	100.00
Rep. of Korea	20,641	14,974	March	Frebruary	97.21
Lithuania	13,789	8,957	September	December	83.42
Luxembourg	18,369	13,067	September	August	89.24
Latvia	17,997	12,974	September	December	84.66
Rep. of Moldova	5,193	4,986	September	December	83.34
Malta	3,452	3,405	September	December	98.87
Mauritius	4,653	4,433	January	December	96.00
Malaysia	10,195	9,799	January	December	96.53
Netherlands	18,069	12,326	September	September	89.07
Norway	18,007	13,344	August	September	99.36
New Zealand	13,480	8,634	February	April	89.59
Poland	19,416	13,845	September	December	99.19
Portugal	20,683	14,619	September	December	93.65
Shanghai (China)	10,291	9,819	September	August	89.81
Perm (Russia)	1,760	1,737	September	December	82.34
Romania	14,967	9,579	September	December	92.43
Serbia	15,003	10,158	September	December	97.71
Slovakia	21,300	16,064	September	August	86.75
Slovenia	18,579	11,840	September	December	94.65
Sweden	18,369	13,682	August	December	97.79
Taipei	20,649	11,783	September	August	97.62
Tunisia	18,662	11,853	June	December	85.15
Uruguay	21,763	15,057	March	April	89.74
Vietnam	4,741	4,622	August	December	96.68

Country	School entry (%)				Regular entry age		
	Regular		Irregular				
	On-track	Retained	Anticipated	Delayed	P25	P50	P75
Argentina	69.80	25.41	1.55	3.23	5.92	6.17	6.42
Australia	83.25	5.62	3.52	7.61	5.75	6.00	6.42
Austria	82.68	4.79	1.77	10.75	6.33	6.67	6.92
Belgium	69.26	22.49	1.09	7.16	6.00	6.25	6.50
Bulgaria	90.94	3.01	4.38	1.67	6.92	7.17	7.42
Chile	77.06	11.20	2.73	9.01	6.00	6.25	6.50
Costa Rica	65.82	27.64	3.25	3.29	6.50	6.83	7.08
Czech Republic	80.14	1.92	4.06	13.87	6.42	6.67	7.00
Denmark	84.07	3.53	1.47	10.94	7.00	7.17	7.50
Spain	71.08	27.85	0.04	1.03	6.00	6.25	6.50
Estonia	88.11	3.14	6.56	2.19	7.16	7.42	7.67
Finland	93.29	3.07	0.42	3.22	6.92	7.17	7.42
France	63.35	32.15	3.24	1.25	5.92	6.17	6.42
United Kingdom	97.04	0.57	0.31	2.08	5.08	5.33	5.67
Greece	94.52	3.04	0.18	2.25	5.92	6.17	6.42
Hong Kong	78.61	11.30	0.44	9.65	5.92	6.17	6.42
Croatia	91.34	0.39	2.82	5.45	6.75	7.00	7.25
Hungary	77.21	4.40	1.97	16.42	6.67	6.92	7.17
Iceland	99.42	0.01	0.57	0.00	5.92	6.08	6.33
Israel	86.76	1.70	3.07	8.47	6.08	6.33	6.58
Italy	86.32	5.12	2.17	6.39	6.00	6.25	6.50
Jordan	94.41	3.55	0.00	2.04	5.92	6.17	6.42
Japan	100.00	0.00	0.00	0.00	6.17	6.58	6.75
Rep. of Korea	97.32	0.31	0.33	2.04	6.33	6.50	6.83
Lithuania	82.47	2.00	9.90	5.63	6.92	7.17	7.42
Luxembourg	60.90	32.63	2.26	4.21	6.25	6.50	6.83
Latvia	81.44	5.56	4.31	8.69	7.00	7.25	7.50
Rep. of Moldova	84.34	1.20	4.97	9.49	7.00	7.25	7.50
Malta	86.81	12.28	0.00	0.91	5.00	5.25	5.50
Mauritius	56.73	41.51	0.16	1.60	5.33	5.58	5.75
Malaysia	99.95	0.00	0.05	0.00	6.33	6.50	6.75
Netherlands	68.37	23.89	2.78	4.96	6.17	6.42	6.75
Norway	99.84	0.00	0.16	0.00	5.92	6.17	6.33
New Zealand	88.72	1.82	5.49	3.97	5.00	5.25	5.50
Poland	95.91	3.35	0.38	0.35	7.00	7.25	7.42
Portugal	69.35	26.80	0.79	3.05	6.00	6.25	6.50
Shanghai - China	85.67	5.43	4.49	4.41	6.25	6.58	6.83
Perm - Russia	80.89	2.30	4.78	12.03	7.00	7.33	7.50
Romania	91.78	1.08	4.78	2.36	7.00	7.17	7.42
Serbia	97.32	0.49	1.85	0.33	6.92	7.17	7.42
Slovakia	85.08	2.31	4.18	8.42	6.33	6.58	6.83
Slovenia	95.41	0.30	4.29	0.00	5.92	6.17	6.42
Sweden	95.43	2.39	1.70	0.48	6.92	7.17	7.33
Taipei	97.37	0.34	1.96	0.33	6.25	6.50	6.75
Tunisia	52.61	40.55	5.53	1.31	5.67	5.92	6.17
Uruguay	62.83	32.71	2.35	2.11	6.17	6.42	6.58
Vietnam	92.04	5.45	0.00	2.51	5.83	6.08	6.42