Parental Love Is Not Blind: Identifying Selection into Early School Start

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Abstract

In many countries, parents can choose whether their children start elementary school one year early. Like many other educational decisions, this choice has lasting consequences that depend on each child's characteristics. Which children are sent to school early? We propose a novel methodology to identify the sign and strength of selection into early starting. We exploit a feature of the Italian education system: only children born between January and April can start elementary school one year early. We use data on standardized tests taken by all students in Italy. We find robust evidence of positive selection: early starters would have obtained scores 0.2 standard deviations higher than the average student, had they started regularly. Additionally, we use this methodology to compare the effect of early starting on selected and average students. We find that the penalty associated with early start is lower for selected students born in March and April.

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1 Introduction

Parents make many decisions regarding the education of their children. They decide whether to enroll children on early formal child care, which school they attend, whether they participate in extracurricular activities, and also school starting age. These are crucial choices, with effects that may depend on each child's specific characteristics. Then, it is important to understand how parents choose. In particular, how do parents take into account their children's characteristics when making these decisions?

We focus on how parents choose the school starting age. In several countries parents can choose that their children start elementary school one year early. The literature on early child development shows that this decision has lasting consequences. On the one side, early starters enter the labor market one year early, which increases their returns from human capital (see for example Black, Devereux, and Salvanes [2011]). Moreover, children from disadvantaged backgrounds could benefit from attending school rather than staying at home. On the other side, early starters have worse academic performance (see for example Bedard and Dhuey [2006]). The relative importance of these factors and hence the effect of starting early can depend on each child's characteristics.

The main difficulty in understanding which students are selected into early start is that this decision itself affects students' outcomes. To see this, consider children who differ in their underlying unobservable ability. We would like to know whether high ability children are selected into early start. Everything else equal, a higher ability student has higher test scores. However, age also affects scores: an extra month of age leads to higher test scores. In order to compare the underlying ability of those who start one year early relative to those who do not, a naïve approach would be to compare the test scores of these two groups. Unfortunately, the difference in scores has two sources: 1) the difference in ability and 2) the difference in age. Students who start one year early are effectively twelve months younger than regular students born in the same month. Being twelve months younger has a strong negative effect on test scores. Therefore, we could mistakenly conclude that early starters are low ability students. Similar problems would arise in the study of other educational decisions. For instance, the enrollment on early child care also affects the outcomes of children, making it hard to identify how they were selected.

Our strategy to identify the characteristics of students who start early is based on 1) a feature of the Italian education system and 2) an empirical regularity on how age affects test scores. First, children in Italy can start elementary school at age five instead of age six *only if they are born between January and April*. Children born between May and December must start school at age six. Thus, within the same class, children can be born up to sixteen months apart. Second, there is a pattern on how age affects test scores: the increase in average test score from one extra month of age is constant. Fredriksson and Öckert [2014] show this empirical regularity for Sweden, while Crawford, Dearden, and Meghir [2010] do it for England, and Black, Devereux, and Salvanes [2011] for Norway. We document this also for our Italian dataset: the effect of age-in-months on average test scores is linear for children born between May and December.

Our methodology to identify which students start school early has three steps. First, we use the subsample of children born from May to December to estimate age-in-months effects on test scores. Children born in these months must start school at the age of six. Hence, differences in average test scores across months of birth are exclusively due to differences in age when taking the test. Second, we use the estimated age-in-months effects to compute the average test scores for children born between January and April, had *all* students started regularly. In practice, we extrapolate the linear trend found for children born between May and December to those born from January to April. Finally, we compute the average test scores that early starters would have obtained, had they started regularly. To do so, we only need test scores from regular starters and the proportion of early starters. If the average test scores that early starters would have obtained are higher than those of the average student in the population, we conclude that there is positive selection. Our measure of the strength of selection is the difference between the average test score of early starters, had they started regularly, and the average test score in the population, had all students started regularly.

We use data on standardized tests administered to all students in Italy. These tests cover two subjects (Italian and mathematics), are designed by an agency of the Italian

Government (the National Institute for the Evaluation of the School System - INVALSI), and are mandatory for all students. Students are tested in second, fifth, eight and tenth grades of compulsory schooling. In our analysis we use information on test scores, month and year of birth, and grade for the academic years 2011–12 to 2016–17.

Our main result is that early starters are positively selected. Had they started regularly, early starters would have been at the top of the grade distribution of their cohort: they would have obtained scores 0.2 standard deviations higher than the average student. We also show that the negative effect of starting one year early on test scores is smaller for March-April born early starters as compared to the average child.

Two considerations become necessary for the correct interpretation of these results. First, from a theoretical perspective, when they decide whether to send their children to school early, parents optimize some objective function, taking into account the information on their children characteristics that is available to them. The selection of children into early start is a result of this optimization problem. We observe neither the objective function of parents nor the information that they have. Instead, we use information on whether parents make their children start school one year earlier and children's test scores to learn how selection takes place. Second, the decision to send children to school early has several consequences including effects on test scores at all educational levels and labor market outcomes. In this paper we focus exclusively on test scores during compulsory education. Addressing whether the decision is optimal overall is beyond the scope of this paper.

1.1 Related Literature

Several papers focus on the effect of relative age within the classroom. There is consensus that older students obtain higher scores. Bedard and Dhuey [2006] find that the oldest students score 4–12 percentiles higher than the youngest students in grade four; and 2–9 percentiles higher in grade eight. The evidence of the effects of relative age on labor market outcomes and on education attainment is mixed. Black, Devereux, and Salvanes [2011] find that older students have lower earnings at the beginning of their careers. This effect disappears by age 30. On the other side, Black, Devereux, and Salvanes do not find any effect of relative age on educational attainment. Instead, Dobkin and Ferreira [2010] find that younger individuals are more likely to complete more years of schooling. They do not find any effect on wage and employment.[1]

A different line of research focuses on the effects of starting school one year early or one year late. Ordine, Rose, and Sposato [2018] use the same data source as we use in this paper. They study whether students who have the option of starting early perform better than those who do not have that option. Ordine, Rose, and Sposato find that those who take the option to early start perform better than the regular students who do not have the option (those born between May and December). Cook and Kang [2018] use data from North Carolina and show that white males with lower academic abilities from high educated families are more likely to delay their entrance to school. They find that postponing school entrance decreases the male-female achievement gap by 11% for white students.

The main challenge for the identification of the effect of school starting age on outcomes is that school starting age may be endogenous. Parents may take into account their children's characteristics when making this decision. For instance, if parents perceive that their children are more mature and talented than the average child of their age, they may make their children start one year early. There are two main techniques to solve for endogeneity. First, a common identification strategy is to instrument the actual starting age with the age at which children would have started school if they had followed the standard entry rule. Second, some papers use regression discontinuity designs that exploit school entry cutoff dates and compare the performance of children born at both sides of the cutoff.

Papers that estimate the effect of early school start do not focus on uncovering the pattern of selection. However, one could gain some insight about the sign of selection

[1] There is a large literature on the effect of relative age on these outcomes. For additional evidence on the effect of relative age on test scores see Dobkin and Ferreira [2010], Strøm [2004], Fredriksson and Öckert [2014], Crawford, Dearden, and Meghir [2010], Black, Devereux, and Salvanes [2011], McEwan and Shapiro [2008], and Cook and Kang [2018]. For additional evidence on the effect of relative age on labor market outcomes and educational attainment see Dhuey and Lipscomb [2008], Bedard and Dhuey [2006], Puhani and Weber [2008], Mühlenweg and Puhani [2010], Pellizzari and Billari [2012], Zweimüller [2013], Fredriksson and Öckert [2014], and Ponzo and Scoppa [2014].

by comparing naïve OLS estimations (affected by selection) to other estimators which are unaffected by selection (i.e. IV or RDD estimates). This approach works when the bias of the OLS estimations can be attributed entirely to selection.[2] An IV estimate that exceeds the OLS estimate indicates the presence of negative selection into early start. However, IV estimates inform only about the sub-sample of the population of students whose school entry age behavior is affected by the rule (the "compliers"). Similarly, RDD estimates refer only to the subpopulation of students born close to the cutoff date. As a result, the comparison of OLS and IV/RDD estimates is not informative of selection in the population as a whole. What is more, this comparison provides evidence only about the direction of selection. In contrast, we measure the sign and strength of selection for the whole population and for different months of birth.

Our paper also relates to the literature about parental's perception of their children's type. Two recent papers focus on whether parents are informed about the academic performance of their children. Kinsler and Pavan [2016] use a US longitudinal survey. They find that parents beliefs about their child's skills relative to children of the same age are determined by their child's skills relative to children of the same school. They also find a positive relationship between perceived children's abilities and parents' investment in human capital (in particular they focus on remedial education). Dizon-Ross [2017] analyzes data from a field experiment in Malawi. Dizon-Ross provides evidence that parents, especially the poorer and less educated, have distorted beliefs about their children's performance at school. In turn, these inaccurate perceptions prevent parents from making the optimal investment in human capital. Once provided with the right information about their children performance at school, they update their beliefs and they invest more efficiently in their education.

The remainder of this paper is organized as follows. We present the data and institutional background in Section 2. In Section 3 we describe our methodology and in Section 4 we present our results. Section 5 discusses several robustness checks. We conclude in Section 6.

[2]This assumption is appealing in our setup as reverse causality and measurement error are unlikely to affect OLS estimates.

2 Data and Institutional Framework

We use standardized test score data from the National Institute for the Evaluation of the School System (INVALSI). Education is compulsory in Italy between ages 6 and 16. The education system is divided in elementary school (five years), middle school (three years) and secondary school (five years).[3] Students take standardized tests in the second and fifth year of elementary school, then three years later in the third year of middle school and finally two years later in the second year of secondary school. INVALSI provides data from academic years 2009–10 to 2016–17.

The INVALSI data contains test scores from two subjects (Italian and mathematics) and indicates the number of correct answers. We standardize scores by subject, academic year, and grade to have zero mean and unit variance (as in Angrist, Battistin, and Vuri [2017]). The data set also includes students' characteristics (among them: gender, whether they attended daycare, and whether they attended kindergarten) and parental characteristics (among them: migrant status, level of education, and occupation).

We make a series of exclusions to arrive at the sample that we use for our analysis. First, information on students' month of birth is not available for academic years 2009–10 and 2010–11. Since this information is crucial to identify selection, we only include academic years 2011–12 to 2016–17 in our sample. Second, selection into early starting takes place right before the first year of elementary school. Since our objective is to identify selection, we focus on the second year of elementary school, the closest to this decision. Moreover, the effects of an extra month of age on scores are stronger in second grade than in later grades. Finally, for later grades early starters may appear as regular starters if they repeated a grade while grade repetition is extremely uncommon in second grade. [4]

Next, we include in our sample only regular and early starters. We say a child is *regular* when he turns seven the year he starts grade 2. Instead, a child is an *early starter* if he satisfies two conditions: he turns six the year he starts grade 2 and he is born between January and April. We then exclude students from three groups. First, those who turn eight or more the year they start grade 2 (1.61% of total students in grade 2). Second,

[3] We provide further institutional details in the Appendix (A.2).

[4] See Section 5.2 for a discussion on other grades.

those who turn five or less the year they start grade 2 (less than 0.01% of total students in grade 2). Third, we also exclude students who turn six the year they start grade 2, but are born between May and December (0.40% of total students in grade 2).

The resulting data set includes $2,800,777$ observations. The average student answers correctly 62.3% and 61.1% of the questions in the Italian and mathematics tests, respectively. In our sample 31.8% of children are potential early starters, since they are born between January and April. Of those, only 26.2% start early. In this way, 8.33% of the observations in our sample correspond to early starters. Of all students in grade 2 who are born in January, 43.4% of them are early starters. This proportion decreases to 28.3% for February, 19.2% for March, and finally 13.2% for April.

Table 1 presents average key characteristics of students and their parents. We describe separately the group of early starters - column (I), the group of regular starters born between January and April - column (II), and the group of regular starters born between May and December - column (III). Early starters are more likely to be female (54% instead of 47% for regular starters born in the same months), less likely to be foreign-born (1.4% instead of 3.1%), and less likely to have foreign-born parents. They are less likely to have attended daycare (35% instead of 38%) or kindergarten (88% instead of 91%). Finally, early starters have a higher proportion of parents with university degrees (26% instead of 18% for mothers, and 21% instead of 14% for fathers).[5] In general, early starters have observable characteristics associated to better performance in tests. Nevertheless, there might be several unobserved characteristics that offset the effect of the observable ones. We aim at measuring the total selection.

Average test scores exhibit some common patterns for all academic years and for both subjects. To illustrate these patterns consider the test scores in Italian for the cohort 2016–17, as an example. Figure 1 presents average test scores by month both for regular and early starters. Circles (in red) represent average test scores for regular starters, while triangles (in green) represent average test scores for early starters. The thick line fits average test scores of regular starters born between May and December. Average test scores ex-

[5] Table 6 in the Appendix provides further information for these groups; it describes the education and labor market status of parents.

Table 1: Descriptive Statistics by Group. Grade 2. All Cohorts

Characteristics	(I) Early Starters	(II) Regular (Jan–Apr)	(III) Regular (May–Dec)
Male	0.455	0.528	0.507
	(0.498)	(0.499)	(0.5)
Foreign-born	0.014	0.031	0.026
	(0.118)	(0.174)	(0.16)
Foreign-born mother	0.104	0.152	0.143
	(0.306)	(0.359)	(0.35)
Foreign-born father	0.081	0.123	0.116
	(0.273)	(0.329)	(0.32)
Attended daycare	0.347	0.378	0.364
	(0.476)	(0.485)	(0.481)
Attended kindergarten	0.876	0.912	0.903
	(0.329)	(0.283)	(0.296)
Mother university degree	0.259	0.184	0.199
	(0.438)	(0.387)	(0.399)
Father university degree	0.208	0.138	0.152
	(0.406)	(0.345)	(0.359)

Notes: This table presents averages and standard deviations (in parentheses).

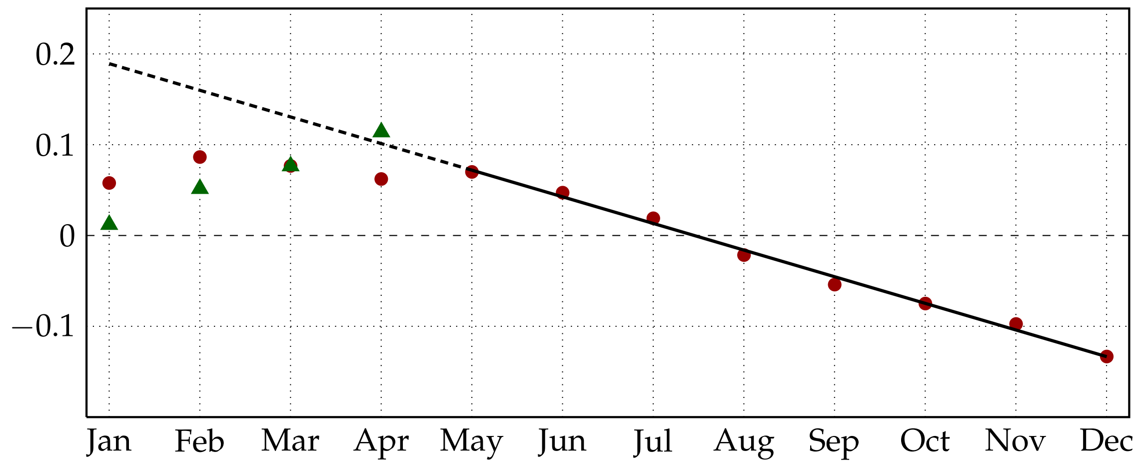
hibit a linear decrease from May to December. However, average test scores for regular starters born between January and April lie below this linear trend. This underperformance of regular starters is preliminary evidence of the presence of positive selection into early start.

3 Methodology

Our first objective is to identify which students are selected into early starting: do higher ability students start early? In other words, had they started regularly, would selected students have obtained grades from the top of the distribution? Unfortunately, we do not observe their counterfactual scores. Moreover, the scores we do observe from these students include a strong age effect: selected students are effectively 12 months younger than non-selected students born in the same month because they start early.

We focus on counterfactual average scores, and account explicitly for the age effect. To do so, we express test scores $T^t(m, x)$ as a function of m (age-in-months) and x (any

Figure 1: Average Test Scores of Regular and Early Starters. Italian. 2016–17



Notes: Circles (in red) represent average test scores for regular starters, while triangles (in green) represent average test scores for early starters. The thick line fits average test scores of regular starters born between May and December. The dashed line its extrapolation for January-April born students.

other individual characteristics that determine scores). The superscript t indexes academic years. A student with characteristics (m, x) who starts regularly obtains scores $T^t(m, x)$, while one who starts early obtains scores $T^{t-1}(m - 12, x)$.

Students may belong to one of three groups $G \in \{S, NS, U\}$, where S denotes students who are *selected* into early starting, NS denotes students who are not selected into early starting (that is, they start regularly) and $U = S \cup NS$ denotes all students. In order to construct our counterfactual scores, we consider two possible treatments $T \in \{E, R\}$, where E denotes early starting, and R denotes starting regularly. Then, expected average scores for different groups are given by:[6]

$$A^t(G, T, m) = \begin{cases} E\left[T^t(m, x_i) \mid i \in G, m\right] & \text{if } T = R \\ E\left[T^{t-1}(m - 12, x_i) \mid i \in G, m\right] & \text{if } T = E \end{cases}$$

The strength of selection is given by $A(S, R, m) - A(U, R, m)$: the difference between the average test score of early starters, had they started regularly, and the average test score in the population, had all students started regularly. Although we do not observe

[6]For notational simplicity we drop the superscript t from $A^t(G, T, m)$. In what follows we denote average scores as $A(G, T, m)$.

these magnitudes, we can indirectly infer them.

Our methodology relies on a key identifying assumption, that average test scores in the population are linear in age-in-months: $A(U, R, m) = \alpha + \beta m$. As discussed in the introduction, there is evidence for many countries with different school starting age cut-offs that this is the case. Moreover, we provide evidence that average scores are linear in age-in-months also in our data set in Section 5.1. Our methodology follows three steps.

Estimating age-in-months effects. In our first step we estimate the linear age-in-months effect on test scores on the subsample of regular students born between May and December using the following equation:

$$T_i^{st} = \alpha^{st} + \beta^{st} m_i^t + \varepsilon_i^{st} \qquad \forall\, s, t, \text{ and for } m \in [5, 12] \tag{1}$$

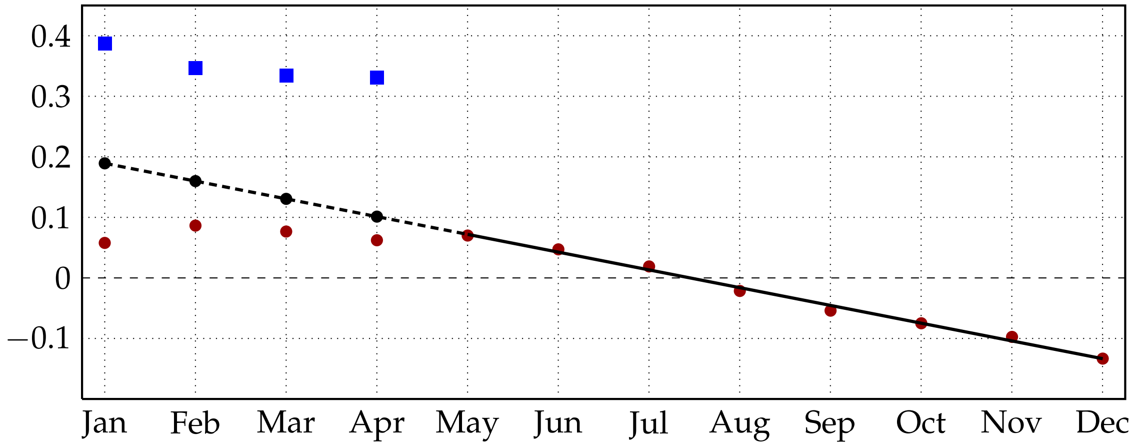
where T is the standardized test score of student i in subject s (Italian or mathematics) in year t (from academic year 2011–12 to 2016–17) and m stands for age-in-months. Our coefficient of interest β measures the effect of an extra month of age on test scores. We estimate equation (1) separately for each subject s and for each academic year t.

Predicting average test scores. In our second step we compute the predicted average test scores of students born between January and April, had all students started regularly. We use the estimated coefficients $\hat{\alpha}$ and $\hat{\beta}$ from equation (1) to compute $\widehat{A}(U, R, m) = \hat{\alpha} + \hat{\beta} m$.

Figure 2 illustrates our methodology. This figure presents again average test scores in Italian for the cohort 2016–17. In our first step, we estimate equation (1) and obtain the thick black line in Figure 2. This line fits average test scores for regular starters born between May and December. In our second step we extrapolate this linear trend to the months between January and April. In this way we obtain the predicted average test scores $\widehat{A}(U, R, m)$, had all students started regularly. These are shown with black circles in Figure 2.

Calculating counterfactual test scores for early starters. In our third step we calculate the scores that early starters would have obtained, if they had not been selected into early starting. Our methodology allows for the indirect identification of $A(S, R, m)$. The aver-

Figure 2: Estimated Average Test Scores of Early Starters, Had They Started Regularly. Italian. 2016–17



Notes: Circles (in red) represent average test scores for regular starters. The thick line, estimated from equation (1), fits average test scores of regular starters born between May and December. Circles over the fitted line (in black) show predicted average test scores $\widehat{A}(U,R,m)$. Squares (in blue) represent the average test scores that early starters would have obtained had they started regularly, $\widehat{A}(S,R,m)$, as computed from equation (2).

age test score $A(U,R,m)$ of all students, had all of them started regularly, is a weighted average of the scores of those selected, and those not selected:

$$A(U,R,m) = P_S(m)A(S,R,m) + [1 - P_S(m)]\, A(NS,R,m).$$

where $P_S(m)$ denotes the proportion of students selected into early starting. We observe both $P_S(m)$ and $A(NS,R,m)$ in our sample. We compute $\widehat{A}(U,R,m)$ in our second step. Then, the predicted average test score $\widehat{A}(S,R,m)$ of early starters born in m can be easily expressed as

$$\widehat{A}(S,R,m) = (P_S(m))^{-1} \left[\widehat{A}(U,R,m) - (1 - P_S(m))\, A(NS,R,m)\right]. \qquad (2)$$

The blue squares in Figure 2 represent the predicted average test score $\widehat{A}(S,R,m)$ of early starters, as computed from equation (2).

Our measure of the strength of selection is $\widehat{A}(S,R,m) - \widehat{A}(U,R,m)$: the difference between the (predicted) average test score of early starters, had they started regularly, and the (predicted) average test score in the population, had all students started regularly.

This measure is the vertical distance between the squares in blue and the circles in black in Figure 2.

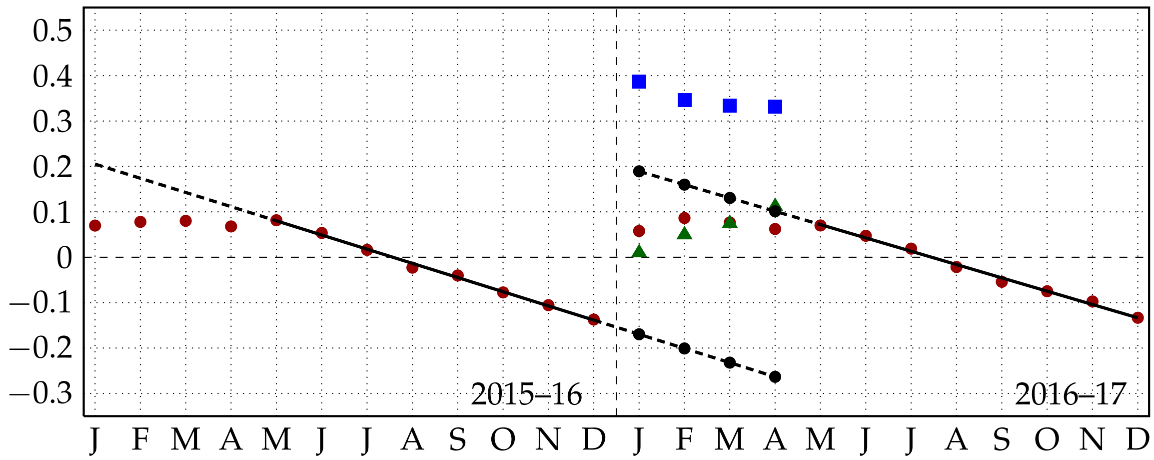
We next focus on the penalty from starting school early that is the difference between the performance of students in the scenarios in which they start early and regularly. As explained in our review of the literature, this difference has been shown to be negative on average in Italy as well as other contexts. We check whether this penalty is different for the group of early starters and that of the average student in the population. The penalty, in terms of test scores, from starting early is given by $A(S, E, m) - A(S, R, m)$ for those selected into early starting. Instead, for an average student in the population, the penalty is given by $A(U, E, m) - A(U, R, m)$.

Figure 3 illustrates our methodology to measure the penalty from starting early. This figure presents average test scores in Italian for two successive cohorts. Red circles represent average test scores for regular starters. Those in the left panel correspond to the academic year 2015–16, while those in the right panel correspond to the academic year 2016–17. Green triangles represent the actual scores $A(S, E, m)$ that early starters obtain. We compare them to the average test scores $\widehat{A}(S, R, m)$ of early starters, had they started regularly. As in Figure 2, these are shown with blue squares. The vertical difference between the green triangles and the blue squares represents the penalty for early starters. Instead, the effect on scores for the average student in the population is given by the difference between the (estimated) average test score $\widehat{A}(U, E, m)$, had all students started early, and the (estimated) average test score $\widehat{A}(U, R, m)$ had all students started regularly. This difference is represented by the vertical distance between black circles in Figure 3.

4 Results

We follow the three steps described in the previous section and illustrated in Figure 2. We first present our estimates for the effect of an extra month of age on test scores (our first step) from equation (1). Table 2 reports these estimates for each subject s and for each academic year t. The estimated age-in-month effect on test scores ranges between -0.29 and -0.35. The linear equation (1) fits the average test scores for regular starters

Figure 3: Early Starters: Actual Scores vs. Estimated Scores Had They Started Regularly. Italian. 2016–2017



Notes: The left panel shows test scores obtained by children who became 7 years old (the regular age of enrollment in 2nd grade) in 2015, i.e. children born in 2008. The right panel shows test scores obtained by children who became 7 years old in 2016, i.e. children born in 2009. Circles (in red) represent average test scores of regular starters. The thick line on the left panel fits average test scores of regular starters born between May and December 2008. The thick line on the right panel fits average test scores of regular starters born between May and December 2009. Squares (in blue) represent the average test scores that early starters born in 2009 would have obtained had they started regularly, as computed from equation (2). Triangles (in green) represent the actual average test scores of early starters born in 2009.

extremely well in all academic years and for both mathematics and Italian.[7]

Table 2: Estimates of $\hat{\beta}$ (Linear Age-in-Month Effect on Test Scores)

	2011–12	2012–13	2013–14	2014–15	2015–16	2016–17
Mathematics	−0.030	−0.034	−0.033	−0.034	−0.031	−0.035
Italian	−0.031	−0.033	−0.031	−0.035	−0.031	−0.029

Notes: This table presents the estimates of the linear age-in-month effects $\hat{\beta}$ for all academic years, for Italian and mathematics. All shown estimates are statistically significant: p-values < 0.001.

In our second step we directly compute the predicted average test score $\widehat{A}(U, R, m)$ in the population, had all students started regularly (the black circles in Figure 2). Finally, in the third step, we use equation (2) to compute the predicted average test score $\widehat{A}(S, R, m)$ of early starters, had they started regularly (the blue squares in Figure 2). Figure 2 shows that the difference $\widehat{A}(S, R, m) - \widehat{A}(U, R, m)$ is positive for test scores in Italian

[7]We perform several tests for linearity in Section 5.1.

Table 3: Estimates of the Strength of Selection

(A) - Mathematics Scores						
	2011–2012	2012–2013	2013–2014	2014–2015	2015–2016	2016–2017
January	0.187	0.178	0.179	0.191	0.209	0.204
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
February	0.24	0.281	0.172	0.215	0.259	0.207
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
March	0.213	0.21	0.138	0.18	0.238	0.212
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
April	0.156	0.246	0.098	0.191	0.258	0.226
	(0.003)	(0.000)	(0.017)	(0.000)	(0.000)	(0.000)
(B) - Italian Scores						
	2011–2012	2012–2013	2013–2014	2014–2015	2015–2016	2016–2017
January	0.156	0.172	0.168	0.209	0.191	0.198
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
February	0.168	0.287	0.178	0.252	0.243	0.187
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
March	0.151	0.271	0.149	0.224	0.232	0.204
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
April	0.136	0.318	0.121	0.248	0.267	0.23
	(0.005)	(0.000)	(0.004)	(0.000)	(0.000)	(0.000)

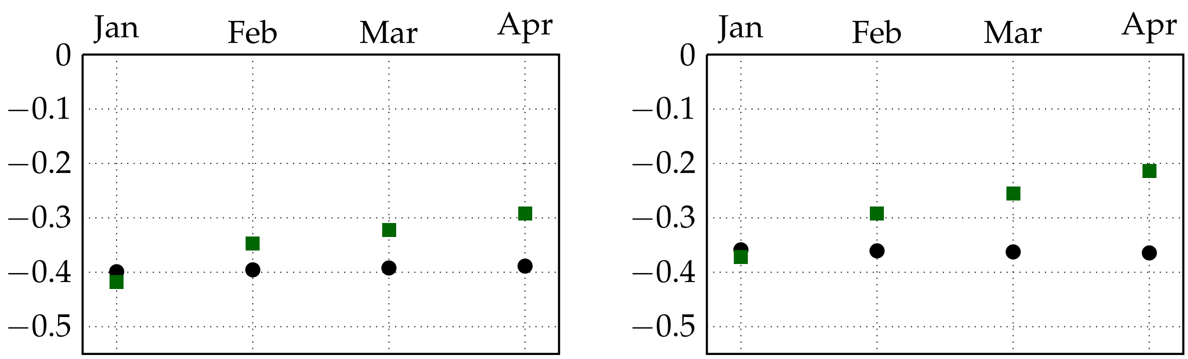
Notes: The strength of selection is measured by the difference between the (estimated) average test score of early starters had they started regularly and the (estimated) average test score in the population had all students started regularly. Standard errors are computed using bootstrap. p-values are reported in parentheses.

in the academic year 2016–17. This is evidence that early starters are positively selected for all months (January to April).

We show that there is positive selection in all academic years, for all months. Table 3 presents the strength of selection for all months (January to April), for all cohorts (2011–12 to 2016–17), and for both subjects. Estimates are positive and significant in all cases. The estimated strength of selection ranges between 0.098 and 0.318.

We next present our results on the penalty from starting early. Our estimate for the penalty for an early starter is given by the difference between $A(S, E, m)$ (the green triangles in Figure 3) and $\widehat{A}(S, R, m)$ (the blue squares in Figure 3). We plot this difference with green squares in Figure 4. Instead, our estimate for the penalty for an average student is given by the difference between $\widehat{A}(U, E, m)$ (the black circles over the left line) and $\widehat{A}(U, R, m)$ (the black circles over the right line in Figure 3). We plot this difference with black circles in Figure 4.

Figure 4: The Effect of Starting Early for Early Starters vs. the Average Student. 2016–2017



(a) Mathematics

(b) Italian

Notes: Squares (in green) represent differences between the actual average test scores obtained by early starters born in 2009 (represented by the green triangles in Figure 3) and their (estimated) average test scores had they started earlier (blue squares in Figure 3). Circles (in black) represent differences between the (estimated) average test scores in the population had all students started early (the black dots over the fitted line on the left panel in Figure 3) and the (estimated) average test scores in the population had all students started regularly (the black dots over the fitted line on the right panel in Figure 3).

Table 4 presents our estimates for the differences in the effect of early starting for selected students and the average student in the population. The estimates are given by $\left[A(S, E, m) - \widehat{A}(S, R, m)\right] - \left[\widehat{A}(U, E, m) - \widehat{A}(U, R, m)\right]$. A positive difference implies that the penalty from early starting is lower for selected students. As Table 4 shows, the results are mixed for children born in January and February. There is no conclusive evidence that selected students from those months are affected by starting early differently than a random student from the population. In contrast, estimates for students born in March and April are positive and significant in 16 out of 20 cases, while non-significant in the other 4 cases. This shows that the penalty from early starting is significantly lower for selected students born in March and April who are the youngest ones.

Table 4: Differences in the Effect of Early Starting for Selected and Average Students

	2012–2013	2013–2014	2014–2015	2015–2016	2016–2017
(A) - Mathematics Scores					
January	0.034^{**}	-0.012	-0.001	-0.021	0.023
	(0.017)	(0.401)	(0.927)	(0.224)	(0.169)
February	0.005	0.091^{***}	0.042	0.001	0.084^{***}
	(0.842)	(0.000)	(0.119)	(0.984)	(0.001)
March	0.168^{***}	0.209^{***}	0.127^{***}	0.094^{***}	0.143^{***}
	(0.000)	(0.000)	(0.001)	(0.006)	(0.000)
April	0.171^{***}	0.294^{***}	0.18^{***}	0.119^{**}	0.154^{***}
	(0.005)	(0.000)	(0.001)	(0.023)	(0.001)
(B) - Italian Scores					
	2012–2013	2013–2014	2014–2015	2015–2016	2016–2017
January	-0.028^{**}	-0.031^{**}	-0.068^{***}	-0.017	-0.015
	(0.045)	(0.022)	(0.000)	(0.300)	(0.374)
February	-0.099^{***}	0.053^{**}	-0.052^{**}	-0.008	0.039^{**}
	(0.000)	(0.026)	(0.041)	(0.728)	(0.096)
March	0.032	0.147^{***}	0.023	0.067^{**}	0.066^{**}
	(0.427)	(0.000)	(0.557)	(0.037)	(0.045)
April	-0.005	0.211^{***}	0.061	0.094^{**}	0.072^{**}
	(0.935)	(0.000)	(0.224)	(0.037)	(0.083)

Notes: This table presents our estimates for the differences in the effect of early starting for selected students and the average student in the population. A positive difference implies that the penalty from early starting is lower for selected students.

5 Robustness Checks

5.1 Linearity

Incomplete. We compare our linear regression to one with one dummy per month (between May and December). Our linear model performs best in terms of goodness of fit using both the Akaike and Bayesian Information Criteria.

We also perform the linearity test proposed in Gupta [2018]. For this, we sequentially add higher order polynomial terms in month of birth in equation (1). We then tested the joint significance of those terms. All coefficients were jointly non-significant at conventional levels.

5.2 Information from Other Grades

Incomplete. Table 5 reports the estimates of equation (1) for each subject s and for each academic year t, for all grades. We also show tables containing estimates of the strength of selection and the differences in the effect of early starting for selected and average students (*to be added*).

Table 5: Estimates of $\hat{\beta}$ (Linear Age-in-Month Effect on Test Scores). All Grades

(A) - Mathematics Scores						
	2011–12	2012–13	2013–14	2014–15	2015–16	2016–17
Grade 2	-0.030	-0.034	-0.033	-0.034	-0.031	-0.035
Grade 5	-0.019	-0.023	-0.023	-0.022	-0.021	-0.021
Grade 8	-0.008	-0.011	-0.013	-0.014	-0.012	-0.014
Grade 10	-0.005	-0.006	-0.006	-0.006	-0.008	-0.007
(B) - Italian Scores						
	2011–12	2012–13	2013–14	2014–15	2015–16	2016–17
Grade 2	-0.031	-0.033	-0.031	-0.035	-0.031	-0.029
Grade 5	-0.022	-0.023	-0.024	-0.022	-0.025	-0.023
Grade 8	-0.015	-0.017	-0.017	-0.018	-0.018	-0.020
Grade 10	-0.007	-0.009	-0.008	-0.009	-0.010	-0.010

Notes: This table presents the estimates of the linear age-in-month effects $\hat{\beta}$ for all academic years, for Italian and mathematics. All shown estimates are statistically significant: p-values < 0.001.

6 Discussion

There is a broad literature on policy evaluation. The objective in this literature is to measure the effect of a treatment (an intervention, a policy, or a program) on some outcomes of interest. Examples include the impact of job training on labor outcomes, the effect of retirement on individual well-being, and the impact of parental leave on children's outcomes. These papers are after an estimate of the effect for the average individual (or firm, schools, etc.) in the population. However, effects often depend on individual characteristics. Policy makers care about the effect of the treatment on those who actually get treated. They also care about who are the beneficiaries of the treatment. Whenever individuals can self-select into treatment, the estimate of the average treatment effect does not reflect these magnitudes. This makes the study of selection crucial in many setups.

In this paper we propose a new methodology to test for the presence of selection into treatment and measure the strength of selection. Our methodology can be applied to setups in which two requisites are satisfied. First, there is a well-known functional relationship between an exogenous running variable and the outcome of interest. Second, access to treatment depends exogenously on the running variable. Examples include: (i) the impact of university scholarships awarded to students with a high-school grade over a certain threshold on the probability of university grade completion; (ii) how incentives to hire young individuals (those with ages lower than a certain threshold) affects labor outcomes; and (iii) the impact of retirement on health if retirement benefits are linked to age.

We apply our methodology to the case of school starting age in Italy. In this context, the functional relationship between students' age in months and test score is linear. Moreover, only students born in certain months can start school one year earlier. We find that early starters would have obtained scores 0.2 standard deviations higher than the average student, had they started regularly. Early start implies a penalty in scores for any student (since students who early start become effectively younger). However, we find that this effect is attenuated in practice because of selection. This corroborates that in the case of school starting age, learning about selection improves our understanding of the actual consequences of early school start.

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A Appendix

A.1 Other parental characteristics

Table 6 describes the education and labor market status of the parents of three groups of students (early starters, regular starters born between January and April, regular starters born between May and December). Parents of early starters are more educated (they are 7 percentage points more likely to have a university degree), more likely to be unemployed and less likely to be blue-collar workers (around 7.5 percentage points).

Table 6: Education and Employment Status of Parents. Grade 2. All Cohorts

Characteristics	Early Starters	Regular (Jan–Apr)	Regular (May–Dec)
Mother. Highest Degree Attained			
Elementary school	0.026	0.023	0.024
Middle school	0.246	0.270	0.267
Vocational school	0.045	0.090	0.077
Music or arts high school	0.021	0.025	0.024
High school	0.402	0.408	0.409
University	0.259	0.184	0.199
Mother. Labor Market Status			
Unemployed	0.061	0.057	0.060
Houseworker	0.399	0.316	0.344
Blue-collar worker	0.078	0.152	0.133
Father. Highest Degree Attained			
Elementary school	0.031	0.028	0.029
Middle school	0.304	0.356	0.346
Vocational school	0.053	0.099	0.087
Music or arts high school	0.017	0.017	0.017
High school	0.387	0.362	0.37
University	0.208	0.138	0.152
Father. Labor Market Status			
Unemployed	0.069	0.051	0.056
Houseworker	0.005	0.006	0.006
Blue-collar worker	0.246	0.321	0.308

Notes: This table presents the proportion of parents by education attained and labor market status.

A.2 Institutional Background

The Italian education system is divided into elementary school (grades 1 to 5), middle school (grades 6 to 8) and high school (grades 9 to 13). Education is compulsory between the age of six (grade 1) and sixteen (grade 10). After middle school, students start high schools and follow one of three tracks (vocational school, technical school, lyceum). School year starts mid-September and finishes mid-June. The enrollment in elementary school is regulated by the Legislative Decree no. 59 issued on February 2004. According to this law, the standard rule is that children start elementary school the year they turn 6 but if they are born between January and April they can access school one year in advance (the year they turn five). Public schools have to accept al those who opt for early starting independently of their month of birth.

A.3 Selection on Unobservables

Incomplete.

A.4 Descriptive Characteristics by Month of Birth

Incomplete. The validity of our identification strategy relies on the fact that parents do not manipulate the date of conception in order to have the opportunity to anticipate their children's school start. We find evidence that the number of children born is very stable across months of birth and if anything the number of children born per day in May as compared to April is slightly higher.