המכון למחקר כלכלי בישראל
על-שם מוריס פאלק בע"מ (חל"צ)
The Maurice Falk Institute for
Economic Research in Israel Ltd.

# The Effect of School Entrance Age on Educational Outcomes: Evidence Using Multiple Cutoff Dates and Exact Date of Birth

by

Itay Attar and Danny Cohen-Zada

**Discussion Paper No. 18.01**

**May 2018**

**בנין פרץ נפתלי, קמפוס האוניברסיטה העברית, הר הצופים, ירושלים 9190501**
**Jerusalem, Israel01The Hebrew University Campus, MT. Scopus, 91905**
**www.falk.huji.ac.il**

# The Effect of School Entrance Age on Educational Outcomes: Evidence Using Multiple Cutoff Dates and Exact Date of Birth

By

Itay Attar and Danny Cohen-Zada*

**Abstract**

Using Israeli data, we estimate the effect of school entrance age (SEA) on student outcomes. Unlike much of the recent literature, our unique identification strategy separates the SEA effect from date of birth effects. We find that delaying school entry by one year increases fifth grade test scores in Hebrew by 0.29 standard deviations and in math by 0.16. Interestingly, while the advantage in Hebrew decreases in eighth grade, in math it almost doubles. We show that although the bias induced by failing to control for date of birth effects is generally rather small, in some cases it is quite notable. This bias could have mistakenly led us to conclude that the SEA effect on math test scores slightly decreases from fifth to eighth grade while it actually substantially increases.

**JEL Classifications**: I21, J24

**Keywords:** School entrance age, student outcomes, date of birth

The Maurice Falk Institute for Economic Research in Israel Ltd.

Jerusalem, May 2018 • Discussion Paper No. 18.01

* Email addresses: danoran@bgu.ac.il; itayattar86@gmail.com

I am grateful to Todd Elder for many helpful discussions and valuable suggestions. Comments from the associate editor, Kawaguchi Daiji, and the anonymous referees significantly improved the paper. I also thank Moshe Justman, Shirlee Lichtman-Sadot, Oren Rigbi, Analia Shlosser and participants at the Annual Meeting of the Israeli Economic Association for their valuable comments.

This work was supported by grants from the Maurice Falk Institute for Economic Research in Israel and from the Spencer Foundation (grant #201800087).

## 1. Introduction

At what age should children start school? This is a question that has long perplexed not only parents but policymakers and researchers as well. Indeed, child development researchers have argued that children's social, emotional, intellectual and physical maturity levels are important factors of school success. This view has led several states in the US to move their entry cutoff date to earlier in the school year, thereby raising the kindergarten entrance age (Bedard and Dhuey 2006, Elder and Lubotsky 2009, Stipek 2002). It has also induced more and more parents to voluntarily postpone their child's entry into school to the following year (McEwan 2008, Stipek 2002, Graue and DiPerna 2000, Paul 2010). However, as the decision to postpone school entrance involves large economic costs of childcare, delaying entry to the labor market, and lower educational attainment, it is justified only if it leads to better educational outcomes. Therefore, a vast literature has investigated the causal effect of entrance age on educational and economic outcomes.[1]

Previous studies that examined the effects of school entrance age (SEA) generally acknowledged that this variable is endogenous due to three main reasons. First, parents do not strictly follow the school entry rule and their choice of whether to delay or expedite school entry is determined based on the characteristics of the child. For example, if the child is talented and emotionally and intellectually mature, a parent will tend to expedite his entrance to school although his date of birth is after the entrance cutoff. On the other hand, if there are developmental problems school entry might be postponed, although his date of birth is before

[1] Overall, the results have been quite mixed. On the one hand, Angrist and Krueger (1991) show that a higher school entrance age leads to lower educational attainment and Black et al. (2011) provide evidence that starting school older leads to lower earnings. On the other hand, many studies find that children who enter school at an older age outperform their younger peers, at least in the lower grades (Bedard and Dhuey 2006, Datar 2006, McEwan and Shapiro 2008, Elder and Lubotsky 2009, Dhuey et al. 2017). Another related issue concerns the persistence of the SEA effect. While Bedard and Dhuey (2006) and Fredriksson and Ockert (2014) show that the school entrance age effect is long-lasting and significant, Elder and Lubotsky (2009) demonstrate that it dissipates as early as the first grades of elementary school. For a more comprehensive review of this literature see Pena (2017).

the cutoff. Second, in some countries, grade retention of underperforming students is quite common. Third, variation in entrance age stems also from variation in dates of birth throughout the calendar year, and the choice of date of birth is commonly correlated with unobserved characteristics of the parents.[2]

To control for the endogenous parental choice regarding the timing of school entry, most studies used a quasi-experimental approach that instruments actual entrance age with the "assigned" age at which the child could first legally enter school (Bedard and Dhuey 2006, Black et al. 2011, Datar 2006, Elder and Lubotsky 2009, among many others).[3,4] However, this approach has been criticized because the instrument violates the monotonicity assumption required for interpreting the estimates as the local average treatment effect (Aliprantis 2012, Barua and Lang 2016, Fiorini and Stevens 2014).[5] In addition, it does not address the concern that dates of birth are non-random and are likely to be correlated with student outcomes. In fact, Cascio and Lewis (2006) show that the season of birth has a direct effect on test scores and thus argue that controlling for detailed date of birth in a model of test scores might be not only appropriate but also important for drawing conclusions.

In order to address these issues, some studies exploited a regression discontinuity approach that concentrates on children born around school entrance cutoff dates (McEwan and Shapiro 2008, Black et al. 2011, Crawford et al. 2014, Pena 2017). However, this too

[2] Bound and Jaeger (2000) provide extensive evidence that the season of birth is correlated with family background, education and earnings, and Buckles and Hungerman (2013) document that women who give birth in the winter are younger, less educated, and less likely to be married. McEwan and Shapiro (2008) show that scheduled births cause the frequency of birthdate distribution to decline during weekends and that mothers of Sunday births have 0.18 less years of schooling relative to Monday births. Using our dataset, Appendix A provides similar evidence from Israel on the non-randomness of dates of birth.

[3] Alternatively, one can instrument actual entrance age with a dummy variable indicating whether the child's date of birth is before or after the school entrance cutoff.

[4] Another interesting approach was to concentrate on educational systems where delaying school entry and repeating grades are not permitted and thus school entry age is likely to be exogenous (Kawaguchi, 2011).

[5] This violation derives from the fact that for compliers (counterfactually) shifting a child's date of birth after the cutoff increases the school entrance age, while for non-compliers it reduces it. Consequently, the instrumental variable indicating whether the child's date of birth is before or after the entrance cutoff is not monotonically related to the actual school entrance age.

may be problematic. First, it has been shown that birthdates may be nonrandom even within a narrow interval around entrance cutoff points because parents may purposely shift their child's date of birth from one side of the entrance cutoff date to the other in order to influence their children's school entrance age (Shigeoka 2013, Kim 2017, Pena 2017).[6] Importantly, such shifts may invalidate the regression discontinuity approach even if they are made for any other institutional motivation and without any intention of influencing their children's school entrance age. For example, Dickert-Conlin and Chandra (1999) show that the probability that a child is born in the last week of December rather than the first week of January is positively correlated with tax benefits, as parents giving birth to children in December receive tax credit points for the full calendar year. This causes the timing of births not to be uniformly distributed over the two-week period surrounding the end of the year, which may invalidate the regression discontinuity approach if the school entrance cutoff coincidentally falls at the end of the year. Second, only if the functional form of the trend in date of birth is correctly specified would this strategy capture the effect of entrance age at the discontinuity. Third, due to data limitations, several of these studies were not able to include any trend in date of birth (Black et al. 2011, Elder and Lubotsky 2009) and in this case, as shown by Fiorini and Stevens (2014), monotonicity significantly fails to hold even in a regression discontinuity framework.

The previous school entrance rule in Israel (which was in effect until 2015) provides a rare opportunity to estimate the effect of entrance age on outcomes while separating it from date of birth effects. This rule determines that the school entry cutoff date is always on the same Jewish calendar date – the first day of the fourth Jewish month of "Tevet." Thus, since

[6] To illustrate, in a recent article in the New Yorker (http://www.newyorker.com/tech/elements/youngest-kid-smartest-kid - last accessed 22.5.16) a Harvard sociologist who when expecting her first child was concerned that her due date was too close to January 1st, an age cutoff for school entrance, argued that she was determined "to keep him in" until after January first in order that he be the oldest in his class and not the youngest.

the Jewish lunar year is about eleven days shorter than the solar cycle, in different years this same Jewish cutoff date is mechanically converted into different Gregorian cutoff dates, that are spread throughout December. As a result, children born on the same date of the year and who are also educated in the same country have a different school entrance age for a reason unrelated to their educational strength: They simply face a different entrance cutoff date, which implies that some of them are situated before the relevant cutoff and are allowed to enter school in the current year, while others are situated after it and have to wait until the following year.

This unique setting allows us to exploit a novel identification strategy that relies on variation in cutoff dates across years while holding the date of birth constant.[7] Accordingly, to examine whether school entrance age affects student outcomes, we use data on the exact Gregorian date of birth and employ a difference-in-difference (DID) approach which estimates the impact of entrance age by comparing changes over different years between children born on different dates of the year. Since not all parents comply with the school entry law, to control for imperfect compliance we use an indicator for the child's date of birth being before or after the cutoff point as an instrumental variable for the actual school entrance age. In addition, to deal with the potential concern that mothers who give birth on different days of the week differ in their unobserved characteristics, we also include a set of day-of-week fixed effects in several of our estimations. Our identification strategy is valid under the assumptions that parents do not precisely time births around the Jewish entrance cutoffs for reasons that may or may not be related to the school entrance age, and that

[7] Since our identification strategy keeps the date of birth constant, it also has the advantage that it strictly satisfies monotonicity. To show this, if we counterfactually shift the location of a child from before the cutoff point to after it while holding the date of birth constant, for compliers such a shift would increase the child's school entrance age by exactly one year while for never-takers and always-takers it would not have any effect on the school entrance age. Thus, under the reasonable assumption that there are no defiers, the instrumental variable indicating whether a child's date of birth is before or after the entrance cutoff is monotonically related to the actual school entrance age.

differences in Jewish birthdates do not have other implications at the institutional level. We provide evidence on these issues both in Section 4 and in Appendix B.

Our identification strategy has one limitation. Since it analyzes test scores of children in the same grade, school entry age and testing age are almost perfectly correlated. Thus, like most previous studies, we are able to only identify the combined effect of school entrance age and age-at-test. Separating these two effects requires two exogenous sources of variation and was accomplished in only a few studies. For example, Black et al. (2011) exploited both the variation in school starting age arising from the school entry cutoff date, and variation on age-at-test arising from the difference in test dates among military enlistees in Norway. Pena (2017) used a difference-in-difference approach that compares test scores of students in states where the cutoff date was changed with those of neighboring states in which students took the same tests at the same time, but where the cutoff date did not change. These studies consistently find that the effect of age-at-test is positive and the pure effect of entrance age is negative.[8]

Our difference-in-difference results show that delaying school entry by one year increases fifth grade test scores in Hebrew by about 0.29 standard deviations (SD) and in math by about 0.16 SD. In terms of persistency of effects, the advantage in Hebrew test scores decreases in eighth grade to 0.21 SD, and in math it substantially increases to 0.29 SD. These findings are robust to different specifications and identification strategies. Similar results are obtained when we exploit a regression discontinuity design (RDD) that focuses on a narrow interval of +/-28 days around the school entrance cutoff. As in Israel tracking begins as early as seventh grade for math but not for Hebrew, our findings are consistent with the literature showing that the effect of entrance age endures longer when children are assigned

[8]Also related, Carlsson et al. (2015) exploit a quasi-random assignment of test dates among military enlistees in Sweden to separate age-at-test effects from the effect of years of schooling.

to tracks at an earlier age (Muhlenweg and Puhani 2010, Fredrickson and Ockert 2014) and that younger students are significantly less likely to attend a high track school relative to their older peers (Scneeweis and Zweimuller, 2014).[9] Our results also show that adding date-of-year fixed effects to the estimation almost completely eliminates the quite substantial amount of selection that exists in the specification without these fixed effects. Furthermore, both our DID and RDD estimates indicate that although the bias induced by failing to control for date of birth effects is rather small, in some cases it is quite notable. This bias could have mistakenly led us to conclude that the SEA effect on math test scores decreases slightly from fifth grade to eighth grade while it substantially increases.

Interestingly, we also find that although girls have a much lower tendency for delayed entry to school than boys (Graue and Diperna 2000, O'Donnell and Mulligan 2008, Bassok and Reardon 2013) and also mature earlier than boys (Renwick 1984, Lim et al. 2013, Shaywitz et al. 1995, Bishop and Wahlsten 1997), SEA effects are not significantly smaller for them. Similarly, we do not find strong evidence that the effect of entrance age on test scores is significantly different among children whose parents belong to different education quartiles.

## 2. Data

Our data contains administrative records collected by the Israeli Ministry of Education for the years 2002-2006 on fifth and eighth grade students living in Jewish localities.[10] Each record

[9] According to the Director-General's Circular of the Ministry of Education (No. 8 of 1994), starting from the second half of seventh grade a school may decide to separate pupils by academic ability for mathematics and English (but not for other subjects) based on test scores in internal exams given by the school. A survey conducted by the Division of Evaluation and Measurement of the Ministry of Education in the 2008/09 school year indicates that 60% of the Hebrew-speaking schools have decided to use tracking as early as seventh grade and 75% as early as eighth grade. Unfortunately, the Ministry of Education does not document whether or not a *specific* school uses tracking in math. Thus, we are unable to examine whether children who enter school at an older age are more likely to be tracked to a higher level and whether the effect of the school entrance age is indeed larger among schools that use tracking.

[10] The education system in Israel consists of three tiers: primary education (grades 1-6), middle school (grades 7-9) and high school (grades 10-12).

includes information on the student's exact date of birth, year of school entrance, gender, parental education, number of siblings, continent of birth, father and mother's continent of birth, and dummy variables for whether the student and each of his parents were born in Israel. In addition, the records also report whether the student attended a religious public school ("Mamlachti Dati" in Hebrew) or a secular public school ("Mamlachti"). These data were linked to math and Hebrew test scores in the Growth and Effectiveness Measures for Schools - Meitzav in Hebrew (GEMS), which is an Israeli national exam conducted once a year for fifth and eighth grade pupils. This exam is administered by the Division of Evaluation and Measurement of the Ministry of Education to a representative 1-in-2 sample of all elementary and middle schools in Israel, so that each school takes part in the GEMS once every two years. The exam is performed at the midterm of each school year for math, science, Hebrew, and English. In general, all students except those in special education classes are tested. In order to facilitate comparability of scores across individuals and over time, we standardized the raw test scores (1-100 scale) by grade, subject and year, setting the mean to zero and the standard deviation to one. In addition, as alternative measures we also used percentile test scores.

To illustrate the structure of the data, Figure 1 presents a timeline with the Gregorian entrance cutoff dates of the different years and the periods around each cutoff. Because the school cutoff points are spread over the entire month of December, we define period as the calendar year of birth starting from June 15$^{th}$ prior to the entrance cutoff through June 14$^{th}$ after the entrance cutoff. Thus, we situate December in the middle of the period (each period contains 5.5 months before December and 5.5 months after December). The bottom panel of the figure presents the expected year for taking the GEMS by year and date of birth relative to the entrance cutoff point. For example, students born before the cutoff of 1992 and after the cutoff of 1991 are expected to take the fifth grade exam in 2003 and the eighth grade

exam in 2006. Similarly, students born after the cutoff of 1992 and before the cutoff of 1993 are expected to take the fifth grade exam in 2004 and the eighth grade exam in 2007. Years colored in red indicate that we do not have data on test scores for these students. For example, since our dataset runs from 2002 to 2006 we lack information on test scores of eighth grade students born after the cutoff of 1992 because they were tested in 2007. In each analysis we use only periods for which we have information on students located at both sides of the entrance cutoff. To illustrate, when estimating the effect of entrance age on test scores in fifth grade, we use only the 1991-1994 cutoffs (periods 4-7) and do not use data on period 3. Thus, we omit from the analysis those children belonging to period 3 who were born after the 1990 cutoff and were tested in 2002. Otherwise, we would create a selection problem. A child born after the 1990 cutoff will be included in our sample only if he complied with the school entry rule and didn't expedite his entry to school. In addition, a child born before the 1990 cutoff will be included in our sample only if he delayed school entry but not if he complied with the school entry rule. For the same reason, when estimating the effect of entrance age on test scores in eighth grade we use only the 1988-1991 cutoffs (periods 1-4) and omit those students belonging to period 5 who were born before the 1992 cutoff and were tested in 2006. Overall, our dataset includes 130,010 observations on fifth graders and 127,294 on eighth graders.

Of all these observations, the reported birthdate is January 1st for 1,775 children. This number is almost three times higher the number of births on December 31st (606 births) and about two and a half times higher the average number of births during all the other dates of the year (700 births). Given that in none of the years in our dataset was January 1st the school entrance cutoff date, we suspect that reported births for this date are not reliable. Indeed, our data show that 955 births on January 1st represent children who are new immigrants from Ethiopia**.** In stark contrast, the average number of births per day among Ethiopian immigrants

for all the other dates of the year is only 8.3 births. Thus, it seems obvious that these 955 births were merely dumped into the January 1st category in the data because the date of birth was not actually known. Although the reported births of January 1st for non-Ethiopian immigrants seem to be very reliable, we decided to take a more conservative approach and drop all January 1st births from the dataset (734 fifth graders and 1,041 eighth graders). In addition, we also omit 43 observations of fifth graders and 21 observations of eighth graders because they had an extremely high school entrance age of above nine years old, and 16 observations of fifth graders and 11 of eighth graders because they had an extremely low entrance age of below 4 years old, which leaves us with 129,217 observations on fifth graders and 126,220 on eighth graders. Of these students, 9,100 fifth graders were tested only in math and 8,245 only in Hebrew, and 9,740 eighth graders were tested only in math and 11,022 only in Hebrew. All the other students were tested in both subjects.

Data on the year at which the student started school is available for about 93% of the students, which allows us to identify whether they were held back a grade or skipped a grade. Of these students, the percentage of students that were held back a grade between first grade and fifth grade is 1.71% and the percentage of students that skipped a grade was 0.74%. Similarly, the percentage of students that were held back a grade between first grade and eighth grade was 2.5% and the percentage that skipped a grade was 0.33%. Because these rates are very low, to avoid sample selection, we prefer to keep the 7% of the students whose year of school entry cannot be observed in the analysis by approximating for their entrance age based on observed age at the beginning of the school year in which they were tested and under the assumption that they weren't held back or skipped a grade. This approximation should introduce a measurement error in only about 0.2% of the total number of observations. Furthermore, to take into account that for these cases the entrance age is approximated, we

include a dummy variable in our regressions that indicates whether the entrance age is accurate or approximated**.**

When we use a regression discontinuity approach we focus on a narrow interval of 28 days before and after each cutoff date, which leaves us with a working sample of 20,086 fifth grade students and 18,910 eighth grade students. Summary statistics of the datasets for fifth grade and eighth grade are provided in Table 1 and Table 2, respectively.

## 3. Empirical strategy

Studying the causal effect of entrance age is a challenging task. A “naïve” approach of correlating entrance age with test scores will yield biased and inconsistent estimates because, as mentioned earlier, it is likely that a non-random sample of children will be enrolled in school at an earlier age by their parents. The "ideal" experiment to deal with the endogeneity problem is one in which the entrance age of different children can be randomized, an option that is obviously unfeasible.

As an alternative, we exploit the fact that, similar to school entrance rules in other OECD countries (Bedard and Dhuey 2006; Black et al. 2011), the rule in Israel requires children to start school only if they were born on or before a specific cutoff date. Figure 2, which presents the predicted entrance age and the actual entrance age as a function of the running variable (location of birthdate relative to the cutoff), shows that although compliance with the entry rule is imperfect, there is still a sharp discontinuity of the actual entrance age at the cutoff point.[11] Thus, a dummy variable for being located before or after the cutoff point can serve as an instrument for the actual school entrance age. Moreover, one unique feature

[11] We define predicted entrance age as the age at which a child enters school if he starts school in the first year allowed by law. For example, according to the Israeli entrance law, a child is eligible to start school on September 1st, 2000, if he was born before December 3rd, 1994 (see Figure 1). Thus, a child born exactly on December 3rd, 1994 is first allowed to enter school on September 1, 2000 when his predicted entrance age is 5.744 years old, while a child born on December 4th, 1994 (one day after the cutoff date) is not allowed to start school in September 2000, but only in September 2001. Thus, his predicted entrance age is 6.743 years old.

of the Israeli system is an exogenous source of variation in entrance age deriving from an exogenous variation in Gregorian cutoff dates across school years because the official starting date is determined according to the Jewish calendar (Figure 1). Such exogenous variation exists only because in different periods the same Jewish cutoff date is mechanically converted into different Gregorian cutoff dates ranging from December 3rd to December 28th.[12] As a result, children born on the same date of the year and educated in the same country with the same culture and institutions have a different school entrance age only because they face a different school entrance cutoff date and not because of any reason related to their educational strength. Thus, we exploit the variation in cutoff dates across school years given the same country. This approach is quite similar to that of Bedard and Dhuey (2006) who instead used the exogenous variation in cutoff dates across countries given the same year to estimate the entrance age effect. Our strategy may be even more valid than theirs because the variation in cutoff dates across school years is totally mechanical, while the variation in cutoff dates across countries might be due to endogenous decisions made by the different countries.

Formally, our initial goal is to estimate the following test score equation:

$$Y_{ip} = \alpha_0 + \alpha_1 \cdot SEA_{ip} + \beta \cdot X_{ip} + \tau_p + \varepsilon_{ip}, \qquad (1)$$

where $Y_{ip}$ is the test scores of child $i$ born in period $p$; $SEA_{ip}$ is the observed entrance age of the child; $X_{ip}$ is a covariate set that includes detailed family backgrounds and demographics (gender, father's years of schooling, mother's years of schooling, number of siblings, indicator

[12] The Jewish or Hebrew calendar is a lunisolar calendar used predominantly to determine the dates of the Jewish holidays. It is considered to be lunisolar because although it is mainly based on the lunar cycle it nevertheless makes some adaptations based on the solar cycle. According to this calendar, each Jewish month starts with the beginning of the moon's cycle and thus runs roughly for 29.5 days. This implies that the length of a regular Jewish year that has twelve months is 354 days, which is about eleven days shorter than the solar cycle. To ensure that the religious holidays will occur during the same season every year (as required by Jewish law), the Jewish calendar uses the 19-year Metonic cycle to bring it into line with the solar cycle with the addition of an intercalary month seven times per 19 years (in the 3rd, 6th, 8th, 11th, 14th, 17th, and 19th year of each cycle).

for a religious public school, indicator for child born in Israel, indicators for the child's continent of birth, indicators for whether the mother and father were born in Israel, indicators for each of the parents' continent of birth, and a dummy variable indicating whether the entrance age is missing and thus approximated based on the age at the beginning of the school year in which the child was tested); and $\tau_p$ is a set of period fixed effects.

Because the cutoff date of a given year is exogenous and children born before the cutoff date start school a year earlier than those born just a few days later, the common strategy is to instrument $SEA_{ip}$ using the indicator $After_{ip}$ for whether child $i$ was born after the cutoff date of period $p$. The validity of this instrumental variable approach requires that the exact date of birth be random and not correlate with child outcomes. However, since exact date of birth is potentially endogenous, most previous studies addressed this issue by controlling only for the year of birth and for a linear trend in month of birth (only a few included a trend in date of birth). We improve this strategy by including fixed effects for each date of the year ($\varphi_d$) and each day-of-week ($\sigma_w$). This improved strategy is made possible because our data includes multiple cutoff dates and also information on the child's exact date of birth. Formally, our estimation takes the following form:

$$Y_{idwp} = \alpha_0 + \alpha_1 \cdot SEA_{idwp} + \beta \cdot X_{idwp} + \varphi_d + \tau_p + \sigma_w + \varepsilon_{idwp} \,, \qquad (2)$$

where the variable $After_{dp}$ serves as an instrument for the actual school entrance age (the endogenous variable). This instrument is valid because conditional on the date and period fixed effects, it is arguably unrelated to the characteristics of the child or the parents. In addition, it is also strongly correlated with the actual entrance age. Thus, this estimation allows us to identify the average treatment effect of entrance age on test scores among compliers.

Since we also want to examine whether the entrance age effect differs by gender and by parent's education we estimate the following specification which allows us to account for heterogeneity in the entrance age effect:

$$Y_{idwp} = \alpha_0 + \alpha_1 \cdot SEA_{idwp} + \alpha_2 \cdot Z_{idwp} + \alpha_3 \cdot Z_{idwp} \cdot SEA_{idwp} + \beta \cdot X_{idwp} + \varphi_d + \tau_p + \sigma_w + \varepsilon_{idwp}, \quad (4)$$

where, the variable $Z_{\text{idwp}}$ can be either gender or the quartile of the parents' education, and the vector $X_{idwp}$ includes all the other family and demographics controls. The indicator for being born after the cutoff date, $After_{dp}$, and its interaction with $Z_{\text{idwp}}$ serve as instruments for the two endogenous variables (*SEA* and its interaction term with $Z_{\text{idwp}}$).

As a last step, we estimate the effect of *SEA* using a regression discontinuity approach in which we estimate the same DID specification (equation 3) but focus on a narrow interval of twenty-eight days around each entrance cutoff date.[13] We also compare our RDD estimates to those obtained from alternative specifications used in previous studies that instead of including fixed effects for each date of the year, included either a quadratic trend of the running variable or didn't include any trend because they were unable to observe the children's exact date of birth.

## 4. Validity of the instrument

A threat to the validity of our identification strategy is if parents precisely schedule their Hebrew birth date to fall on one side of the cutoff or another, as this may introduce sharp differences in unobserved characteristics that affect student outcomes. We use three strategies to show that this is not a major concern. First, we conducted two surveys to show that Israeli parents do not generally use this calendar in their everyday life and are not even aware that

[13] We chose an interval of exactly 28 days in order to obtain an identical distribution of day-of-week at both sides of the entrance cutoff point.

the entrance cutoff date was set according to the Jewish calendar. Thus, precise birth timing near Jewish cutoff points is not likely to occur. A description of these surveys appears in Appendix B. Second, we perform a density test and show in Figure 3 that there is no suspect jump in the number of births around the discontinuity. Third, in Table 3 we provide evidence that, conditional on date-of-year fixed effects, the observed characteristics are balanced around the entrance cutoff dates. Columns 2 and 4 show that only two covariates are significant among fifth graders and no covariate is significant among eighth graders (Columns 6 and 8).

To assess the importance of controlling for date-of-year fixed effects we also estimated a set of univariate regressions in which the dependent variable is each of the background characteristics listed in the first column of Table 3 and the explanatory variable is the instrument. The results (not reported in the table because of space limitations) indicate that the instrument is strongly correlated with many student characteristics. Among fifth graders, ten variables were found to be significantly correlated with the instrument at the 5% level and three additional variables at the 10% level. Similarly, among eighth graders, 16 variables were significant at the 5% level and an additional four at the 10% level. These results strongly indicate the necessity to include date-of-year fixed effects in order to control for the fact that date of birth is non-random.

Several previous studies that used data on the entire year tried to reduce the amount of selection stemming from the non-randomness of date of birth by including month fixed effects in their set of controls. In Columns 1 and 5 of Table 3 we assess the amount of selection in such a specification by reporting the coefficient on *After* in models that include month fixed effects instead of date-of-year fixed effects. The results show that the amount of selection is non-negligible. For example, among fifth graders, twelve student characteristics were found to be significantly correlated with the instrument (Column 1). Similarly, Column

5 shows that seven characteristics were found to be significant among eighth graders. Other studies in this literature attempted to minimize the amount of selection on the instrument by concentrating on a discontinuity sample of one month on either side of the cutoff point. To assess the amount of selection when using this strategy, we focus on a discontinuity sample of 28 days on either side of the cutoff point, and report the slope from univariate regressions of each of the background characteristics on the instrument (Columns 3 and 7). The results indicate that the amount of selection is still far from negligible. Seven covariates are significant among fifth graders and eight covariates among eighth graders.

According to our strategy, when we add a set of date-of-birth fixed effects to the estimation, none of the covariates are significant among eighth graders and only two are significant among fifth graders (Columns 4 and 8). Thus, we can conclude that including the date-of-birth fixed effects in the estimation substantially decreases the amount of selection. Even more importantly, this analysis shows that, conditional on the date-of-birth fixed effects, observed student characteristics do not vary sharply around the entrance cutoff dates, providing evidence that unobserved characteristics that affect the outcome are also likely to vary smoothly around these cutoff dates.

## 5. Results

### 5.1 The reduced form relationship between entrance age and student outcomes

Figure 2 shows a sharp discontinuity in the actual entrance age at the entrance cutoff and Figure 4 indicates that this discontinuity is mirrored by a sizeable discontinuity in normalized fifth grade test scores in both Hebrew and math. It is important to note that Figure 4 does not take into account imperfect compliance and thus biases the size of the discontinuity at the cutoff point towards zero. A similar result, but with a smaller discontinuity, is found for the eighth grade test scores in Figure 5. However, estimating the effect of entrance age by

comparing the outcomes of students located before and after the cutoff point may be problematic if parents endogenously time their date of birth with respect to the cutoff point and their choice is correlated with their unobserved characteristics.

To address this concern, we now estimate the causal impact of entrance age on test scores by comparing changes over different periods between children born on different dates. In other words, we compare children located before and after the cutoff point, conditional on fixed effects for each period, each date-of-year and each day-of-week. Recall that children born on the same date of the year in different periods may be located on different sides of the cutoff point because they face a different Gregorian cutoff date. In Figures 6 and 7, for each date between December 3rd and December 28th, we compare the normalized test scores in math and Hebrew of children located after and before the entrance cutoff date. The height of each bar in the histogram corresponds to the difference between the normalized test scores of children born after the relevant entrance cutoff and those who were born before it. These figures show that both for fifth and eighth grade almost all the bars have a positive height, indicating that children born after the entrance cutoff achieved higher test scores relative to children born before the entrance cutoff. Like Figures 4 and 5, these graphs ignore imperfect compliance and thus bias the entrance age effect towards zero.

Columns 1 and 2 of Table 4 present reduced form estimates for Hebrew and math, respectively. The results indicate that being born after the cutoff point significantly increases fifth grade test scores in Hebrew by 0.09 SD and in math by 0.05 SD. Interestingly, the effect on eighth grade test scores decreases in Hebrew to 0.06 SD while it increases in math to 0.08 SD. The table shows similar results in terms of percentile points: being born after the cutoff point increases fifth grade test scores in Hebrew by 2.87 percentile points and in math by 1.83 percentile points. Again, while in Hebrew the effect on eighth grade test scores decreases to 1.70 percentile points, in math it increases to 2.53 percentile points.

In order to test our identifying assumption that the trend in test scores over the different periods is date-invariant, we next conduct two placebo tests. In the first, we move each cutoff point from December to the same day of the month in March and exclude children born before January. For example, if the entrance cutoff is December 8$^{th}$ we use a placebo cutoff for March 8$^{th}$. Similarly, in the second placebo test, we move each cutoff point to the same day of the month in September and exclude those children who were born after November. The results reported in Columns 3-6 of Table 4 indicate that the impact of being born after the placebo cutoffs is not significant in any of the estimations.

### 5.2 The effect of entrance age on test scores

The reduced form specifications presented above ignore imperfect compliance with the school entrance law. For this reason, if entrance age affects student test scores, these estimates represent a *lower-bound* effect of the actual entrance age. Therefore, we estimate equation (2) and report the results in Table 5. For comparison purposes, in Column 1 we present the results from naïve OLS estimations which show that entrance age is negatively associated with both Hebrew and math test scores. The IV estimate with controls (Column 2) indicates that entering school a year later increases fifth grade test scores in Hebrew by 0.29 SD and in math by 0.16 SD. Interestingly, in eighth grade the effect in Hebrew decreases to 0.21 SD, while in math it almost doubles to 0.29 SD. The first-stage results of each of the specifications of Tables 5 and 6 are reported in Table C1 in Appendix C. It is clear that the instrument is very strong as the F-statistics on the excluded instrument consistently and considerably exceeds the Stock and Yogo (2005) instrument threshold. The table also shows that being located after the cutoff point increases the actual entrance age by between 0.26-0.38 years.[14]

[14] In order to address more carefully the issue of grade retention, we tested the robustness of our main results in Column 2 to two alternative estimations. In the first one we omitted from the analysis those students who were

Since the instrument is correlated with all the day-of-week fixed effects (Appendix A), failing to control for them may potentially lead to biased estimates. In order to assess the size of the bias from such a specification, we estimated the same equation as in Column 2 but now omit the day-of-week fixed effects. The results, presented in Column 3, indicate that the estimates remain almost unchanged. To show that our estimates are not sensitive to the set of controls, we next estimate the same equation but omit the entire set of control variables (except for the date-of-year and day-of-week fixed effects which are included in all estimations as part of our identification strategy). The results are presented in Column 4 of Table 5 and show that the entrance age estimates remain almost unchanged.

In order to highlight the importance of disentangling the effect of entrance age from date of birth effects, we re-estimate our basic specification but now omit the date-of-year and day-of-week fixed effects. This specification may yield biased estimates not only because of omitted variables but also because they violate the required monotonicity assumption. The results, reported in Column 5 of Table 5, indicate that except for fifth grade test scores in math the estimates of the entrance age effect are not particularly sensitive to omitting the date-of-year fixed effects. However, when omitting these fixed effects for fifth grade test scores in math the entrance age effect increases from 0.16 to 0.27 SD. Thus, while our preferred estimates (Column 2) imply that the entrance age effect in math almost doubles from fifth grade to eighth grade, according to the regressions without date-of-year fixed effects, it slightly decreases. The estimates on Hebrew test scores are much less sensitive to the omission of these fixed effects.

---

held back or skipped a grade. In the second, we included them in the analysis but also added controls for being held back and for skipping a grade. We preferred not to add these controls in our main specification because being held back or skipping a grade may be outcomes of the school entrance age (Elder and Lubotsky, 2009), in which case they are considered 'bad' controls (Angrist and Pischke, 2008). Both specifications yield results which are very similar to those reported in Column 2. These results are available from the authors upon request.

Next, we report estimates where instead of date-of-year fixed effects we control only for a trend in month of birth – a specification that is quite common in the literature (Column 6). The results indicate that the entrance age effect on Hebrew test scores does not differ substantially from our preferred estimates in Column 2. However, in math, the entrance age effect on fifth grade test scores increases from 0.16 SD to 0.23 SD and on eighth grade test scores decreases from 0.29 to 0.21 SD. Although each of these estimates is only moderately biased, the two biases together could have mistakenly led us to conclude that the SEA effect on math test scores decreases slightly from fifth grade to eight grade while it actually substantially increases.

We also test the sensitivity of our estimates to using a regression discontinuity design. In this analysis, we estimate the same equation (2) but now focus on a narrow interval of +/- 28 days around each of the cutoff points. The results, presented in Column 7, show that entering school a year later increases fifth grade test scores in Hebrew by 0.23 SD and in math by 0.17 SD. In eighth grade the effect on Hebrew test scores decreases to 0.15 SD and becomes insignificant while in math it increases to 0.24 SD. Thus, our regression discontinuity estimates agree with the DID estimates in Column 2 that the entrance age effect on math test scores increases from fifth grade to eighth grade while that the estimates on Hebrew test scores decrease.

Because we used different periods for estimating fifth and eighth grade test scores (periods 4-7 for fifth grade and periods 1-4 for eighth grade), it is possible that the difference in the size of the estimated entrance age effect stems from the differences between the cohorts. In order to reduce the size of the cohort effect, we use data on students born in the same period for analyzing both fifth and eighth grade test scores. As mentioned in the data section, the only complete period for which data on student test scores are available for both fifth grade and eighth grade is Period 4, which allows us to exploit only a regression

discontinuity approach and not a difference-in-difference one. Moreover, it should be taken into account that even this kind of analysis will not completely eliminate the existence of a cohort effect because the students from Period 4 who were tested in fifth grade are not necessarily the exact same students who were tested in eighth grade (the exam was administered to a representative 1-in-2 sample every year). The results of a regression discontinuity estimation that includes only period 4 are reported in Column 8. They confirm our finding that the effect on Hebrew test scores reduces from fifth to eighth grade while the effect on math test scores substantially increases.

Previous studies could not include date-of-year fixed effects in their RD estimations either because they did not have multiple cutoff points or because they did not observe the child's exact date of birth. As an alternative, a minority of them that did have information on the children's exact date of birth (but did not have multiple cutoff points) included a quadratic trend of the running variable – the child's date of birth relative to the cutoff point. Other studies did not control for any trend of the running variable, which may lead to biased estimates not only because dates of birth are not random but also because they violate the monotonicity assumption (Fiorini and Stevens, 2014). To assess the size of the bias induced by omitting date-of-year fixed effects, we estimate two additional specifications. In Column 9 we omit the date-of-year fixed effects but include a quadratic trend of the running variable instead, while in Column 10 we do not include any trend. The results show that the effect of entrance age on math test scores slightly decreases from fifth to eight grade while our preferred specification shows that it moderately increases from 0.17 SD to 0.24 SD. Furthermore, when we compare the results in Columns 9 and 10 to our preferred estimates in Column 2 we find that the biases are even more substantial. The effect on fifth grade math test scores increases from 0.16 SD to 0.24 SD in the specification with a quadratic trend of the running variable and to 0.25 SD in the specification with no trend (Columns 9 and 10).

Similarly, the entrance age effect on eighth grade test scores in math drops from 0.29 SD to 0.21 SD in the specification with a quadratic trend and to 0.24 SD in the specification with no trend. Thus, while Column 2 indicates that the entrance age effect almost doubles from fifth grade to eighth grade, Columns 9 and 10 show that it slightly decreases.

Table 6 presents results from the same specifications as in Table 5 but the dependent variable is measured in terms of percentile scores. We can see that the effect of entrance age on fifth grade test scores in Hebrew is 9.21 percentile points and 6.1 percentile points in math. In eighth grade, the effect on Hebrew test scores slightly decreases to 6.09 percentile points while in math it increases substantially to 9.5 percentile points. The RD estimates indicate a similar pattern.

### 5.3 Effect heterogeneity

The literature shows that the practice of parents to voluntarily postpone their child's entry into school for a year is more prevalent among boys than among girls (Graue and DiPerna 2000, O'Donnell and Mulligan 2008, Bassok and Reardon 2013). This finding is consistent with evidence that girls mature and become ready for school earlier than boys (Lim et al. 2013, Shaywitz et al. 1995, Bishop and Wahlsten 1997, Renwick 1984). For example, Renwick (1984) finds that boys are more likely to "not be ready for school" than girls, and that boys expressed themselves less clearly and had more difficulty writing their names, recognizing numbers and letters and tying their shoelaces. Lim et al. (2013) shows that girls' brains mature faster and work more efficiently than boys' due to more connections across the two hemispheres of the brain.[15] To examine whether the effect of school entrance age is different for boys and girls, we estimate equation (4) where $Z_{idwp}$ indicates gender. Here we have two

[15] See also the following articles on this issue
http://www.theguardian.com/education/2015/oct/13/boys-trail-girls-literacy-numeracy-when-starting-school
http://healthland.time.com/2013/12/19/why-girls-brains-mature-faster-than-boys-brains/

endogenous variables: the school entrance age and its interaction with gender. Thus, in order to estimate this equation, we need to have two instruments. Therefore, we use the indicator for being born after the cutoff date and its interaction with gender. We estimate this equation both on the entire sample and also only within a narrow interval of +/- 28 days around the entrance cutoffs. The results, presented in Panel A of Table 7, do not provide strong evidence that the effect of entrance age among boys is significantly larger than among girls. The interaction term is positive and significant in only one of the eight specifications and only at the 10% significance level.

In addition, we also examined whether the effect of entrance age is different for parents that belong to different education quartiles. We again estimate equation (4), but now $Z_{idwp}$ denotes the education quartile of the parent. The results, presented in Panel B of Table 7, do not provide strong evidence that the entrance age effect is significantly different among parents that belong to different education quartiles.

### 5.4 Magnitude of the entrance age effect from a comparative perspective

The range of values that we find for the entrance age effect in math and Hebrew for fifth and eighth grade is well within the range of earlier studies. For example, our findings that entering school a year later increases fifth grade test scores in Hebrew by 0.29 SD (9.21 percentile points) and in math by 0.16 SD (6.10 percentile points) are similar to those of Bedard and Dhuey (2006) who found that the entrance age effect on fourth grade test scores is between 2-9 percentile points. In addition, they are only slightly smaller than the estimates of Elder and Lubotsky (2009) who find that entering school a year later increases reading test scores in fifth grade by 10.98 percentile points and in math by 9.04 percentile points. On the other hand**,** they are quite substantially smaller than those of McEwan and Shapiro (2008) who found that the entrance age effect on language fourth grade test scores is 0.38 SD and in math 0.29 SD.

Our range of values for the entrance age effect in eighth grade is also within the range of previous studies. Entering school a year later increases Hebrew test scores by 0.21 SD (6.09 percentile points) and math test scores by 0.29 SD (9.50 percentile points). Our results for Hebrew test scores are very similar to those of Elder and Lubotsky (2009) who found that entrance age increases Hebrew test scores by 6.21 percentile points. In math, the size of our estimates is substantially larger than those of Elder and Lubotsky (3.78 percentile points) but still lower than those of McEwan and Shapiro (0.43 SD). Finally, our finding that the effect of entrance age becomes only larger in eighth grade is consistent with those of McEwan and Shapiro (2008).

## 6. Concluding remarks

In this paper we exploited a unique identification strategy that allows us to estimate the causal effect of entrance age on test scores while isolating it from date of birth effects. We show that the induced bias from failing to control for date of birth effects can be quite substantial. We find that school entrance age has a sizeable effect on fifth grade test scores in Hebrew and math. Entering school a year later increases fifth grade test scores in Hebrew by 0.29 SD and in math by 0.16 SD. Moreover, the entrance age effect generally persists into eighth grade and remains substantial. In Hebrew it decreases to 0.21 SD and in math it increases substantially to 0.29 SD. As in Israel tracking begins in seventh grade only for math, this finding is consistent with the literature showing that the effect of entrance age endures longer when children are assigned to tracks at an earlier age (Muhlenweg and Puhani 2010, Fredrickson and Ockert 2014). Thus, in countries where tracking begins in early grades, age-related differences in student outcomes will most likely tend to persist into adulthood, so that the decision to start kindergarten at an older age could be a worthwhile investment. Furthermore, if countries want to avoid exacerbation of entrance age effects, they might consider postponing tracking to later stages of the education process. Another

important result is that entrance age effects are not significantly larger for boys than for girls. This result implies that parents of girls should be as careful as parents of boys about early entrance into school. Finally, we find that the effect of entrance age does not differ significantly among parents that belong to different education quartiles. All these findings are highly relevant for both parents and policy makers when deciding on the timing of school entrance.

## References


Aliprantis, Dionissi. 2012. “Redshirting, Compulsory Schooling Laws, and Education Attainment." *Journal of Educational and Behavioral Statistics* 37 (2): 316–338.

Angrist, Joshua D., and Alan B. Krueger. 1991. “Does Compulsory School Attendance Affect Schooling and Earnings?” *Quarterly Journal of Economics* 106 (4): 979–1014.

Angrist, Joshua, and Jorn-Steffen Pischke. 2008. “Mostly Harmless Econometrics: An Empiricist’s Companion.” Princeton University Press.

Barua, Rashmi and Kevin Lang. 2016. “School Entry, Educational Attainment and Quarter of Birth: A Cautionary Tale of a Local Average Treatment Effect." Journal of Human Capital, 10 (3): 347-376.

Bassok, Daphna, and Sean F. Reardon. 2013. “Academic Redshirting in Kindergarten Prevalence, Patterns, and Implications.” *Educational Evaluation and Policy Analysis* 35 (3): 283–297.

Bedard, Kelly and Elizabeth Dhuey. 2006. “The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects.” *Quarterly Journal of Economics* 121 (4): 1437–1472.

Bishop, Katherine M., and Douglas Wahlsten. 1997. “Sex Differences in The Human Corpus Callosum: Myth or Reality?” *Neuroscience and Biobehavioral Reviews* 21 (5): 581 – 601.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. 2011. “Too Young to Leave the Nest? The Effects of School Starting Age.” *Review of Economics and Statistics* 93 (2): 455–467.

Bound, John, and David A. Jaeger. 2000. ‘‘Do Compulsory Attendance Laws Alone Explain the Association between Earnings and Quarter of Birth?’’ *In Research in Labor Economics: Worker Well-Being*, ed. Solomon W. Polacheck, 83–108. New York: JAI.

Buckles, Kasey S. and Daniel M. Hungerman. 2013. “Season of Birth and Later Outcomes: Old Questions, New Answers.” *Review of Economics and Statistics* 95 (3): 711–724.

Carlsson, Magnus, Gordon B. Dahl, Björn Öckert, and Dan-Olof Rooth. 2015. “The Effect of Schooling on Cognitive Skills.” *The Review of Economics and Statistics* (97): 533–547

Cascio, Elizabeth U. and Ethan B. Lewis, 2006. “Schooling and the AFQT: Evidence from School Entry Laws.” *Journal of Human Resources* 41 (2): 294–318.

Cascio, Elizabeth U. and Diane W. Schanzenbach. 2013. “The Impacts of Expanding Access to High-Quality Preschool Education.” *Brookings Papers on Economic Activity* (1): 127–179.

Crawford, Claire, Lorraine Dearden, and Ellen Greaves. 2014. “The drivers of month-of-birth differences in children's cognitive and non-cognitive skills.” *Journal of the Royal Statistical Society: Series A (Statistics in Society)* (177): 829–860.

Datar, Ashlesha. 2006. “Does Delaying Kindergarten Entrance Give Children a Head Start?” *Economics of Education Review* 25 (1): 43–62.

Dickert-Conlin, Stacy, and Amitabh Chandra. 1999. ‘‘Taxes and the Timing of Births.’’ *Journal of Political Economy* 107 (1): 161–77.

Dhuey Elizabeth, David Figlio, Krzystof Karbownik and Jeffery Roth. 2017. "School Starting Age and Cognitive Development." NBER Working paper No. 23660.

Elder, Todd E., and Darren H. Lubotsky. 2009. “Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background, and Peers.” *Journal of Human Resources* 44 (3): 641–683.

Fiorini, Mario and Katrien Stevens. 2014. “Monotonicity in IV and fuzzy RD designs - A Guide to Practice,” Working Paper, University of Sydney.

Fredriksson, Peter and Björn Öckert. 2014. “Life-cycle Effects of Age at School Start.” *Economic Journal* 124 (579): 977–1004.

Graue, M. Elizabeth, and James DiPerna. 2000. “Redshirting and Early Retention: Who Gets The Gift of Time and What Are Its Outcomes?” *American Educational Research Journal* 37 (2): 509-534.

Kawaguchi, Daiji. 2011. “Actual Age at School Entry, Educational Outcomes, and Earnings.” *Journal of the Japanese and International Economies* 25 (2): 64–80.

Kim, Taehoon. 2017. “Age Culture, School-Entry Cutoff and the Choices of Birth and School-Entry Timing in South Korea.” Unpublished draft

Lim, Sol, Cheol E. Han, Peter J. Uhlhaas and Marcus Kaiser. 2013. “Preferential Detachment During Human Brain Development: Age- and Sex-Specific Structural Connectivity in Diffusion Tensor Imaging (DTI) Data.” *Cerebral Cortex* 25 (6): 1477-1489

McEwan, Patrick J. and Joseph S. Shapiro. 2008. “The Benefits of Delayed Primary School Enrollment: Discontinuity Estimates Using Exact Birth Dates.” *Journal of Human Resources* 43 (1): 1–29.

Mühlenweg, Andrea M. and Patrick A. Puhani. 2010. “The Evolution of the School-Entry Age Effect in a School Tracking System.” *Journal of Human Resources* 45 (2): 407–438.

O'Donnell, Kevin, and Gail Mulligan. 2008. “Parents Reports of the School Readiness of Young Children from The National Household Education Surveys Program of 2007: First look.” *Washington, DC: National Center for Education Statistics*.

Paul, Pamela. 2010. “The Littlest Redshirts Sit Out Kindergarten.” *New York Times*, August 20, 2010. http://www.nytimes.com/2010/08/22/fashion/22Cultural.html?_r=0

Pena, Pablo A. 2017. “Creating Winners and Losers: date of birth, relative age in school, and outcomes in childhood and adulthood” *Economics of Education Review* 29 (56): 152–176.

Renwick, Margery. 1984. “To School at Five: The Transition from Home to Pre-School or School”. Wellington: New Zealand Council for Educational Research.

Schneeweis, Nichole and Martina Zwemuller. 2014. “Eearly Tracking and the Misfortune of Being Young”, *Scandinavian Journal of Economics* 116(2): 394-428.

Shaywitz, Bennet A., Sally E. Shaywitz, Kenneth R. Pugh, Todd R. Constable, Pawel Skudlarski, Robert K. Fulbright, Richard A. Bronen, Jack M. Fletcher, Donald P. Shankweller, Leonard Katz and John C. Gore. 1995. “Sex Differences in the Functional Organization of the Brain for Language” *Nature* 373 (6515): 607 – 9.

Shigeoka, Hitoshi, 2015. “School Entry Cutoff Date and the Timing of Births,” NBER Working Paper No. 21402.

Stock, James H. and Motohiro Yogo. 2005. “Testing for Weak Instruments in Linear IV Regression.” Ch. 5 in J.H. Stock and D.W.K. Andrews (eds), Identification and Inference for Econometric Models: Essays in Honor of Thomas J. Rothenberg, *Cambridge University Press*.

Stipek, Deborah. 2002. “At What Age Should Children Enter Kindergarten? A Question for Policy Makers and Parents.” *Social Policy Report* 16 (1): 3–16.

*Figure 1. Time line with the cutoff points of each period and the expected years for taking the GEMS*

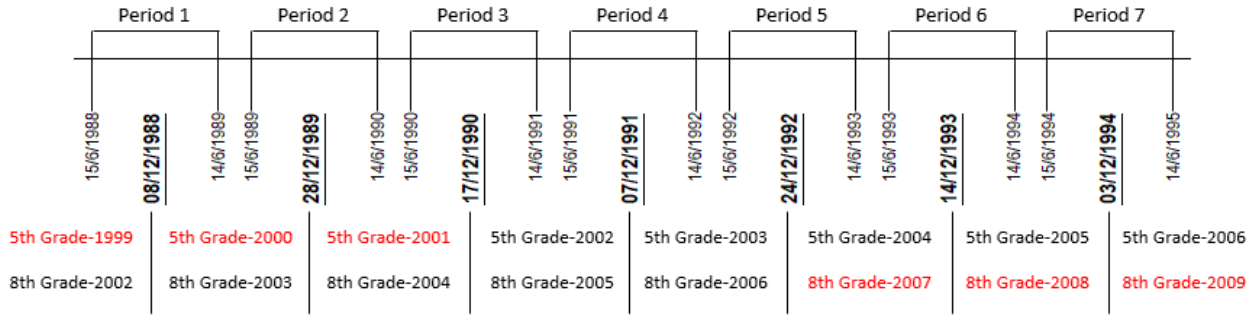


*Figure 2. Average predicted and actual entrance ages by age relative to cutoff*

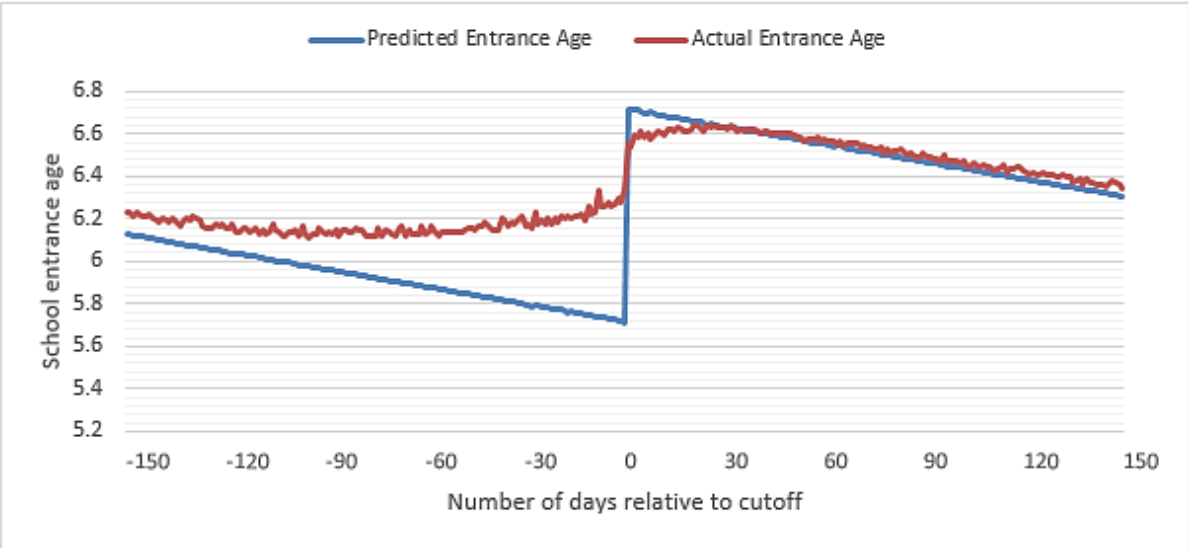


*Figure 3. Number of births by age relative to cutoff*

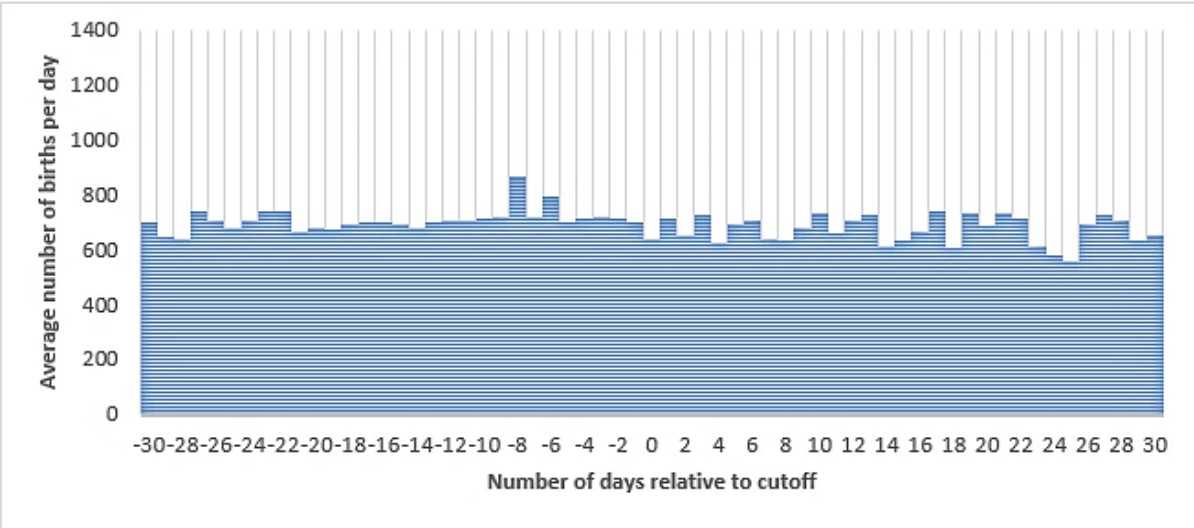

*Figure 4. Fifth-grade normalized Hebrew and math test scores by age relative to cutoff*

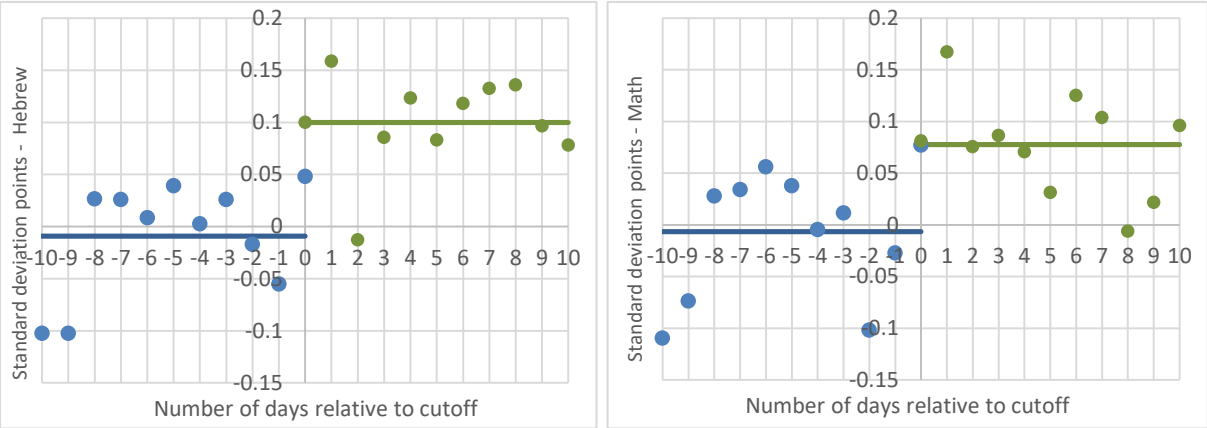


*Figure 5. Eighth-grade normalized Hebrew and math test scores by age relative to cutoff*

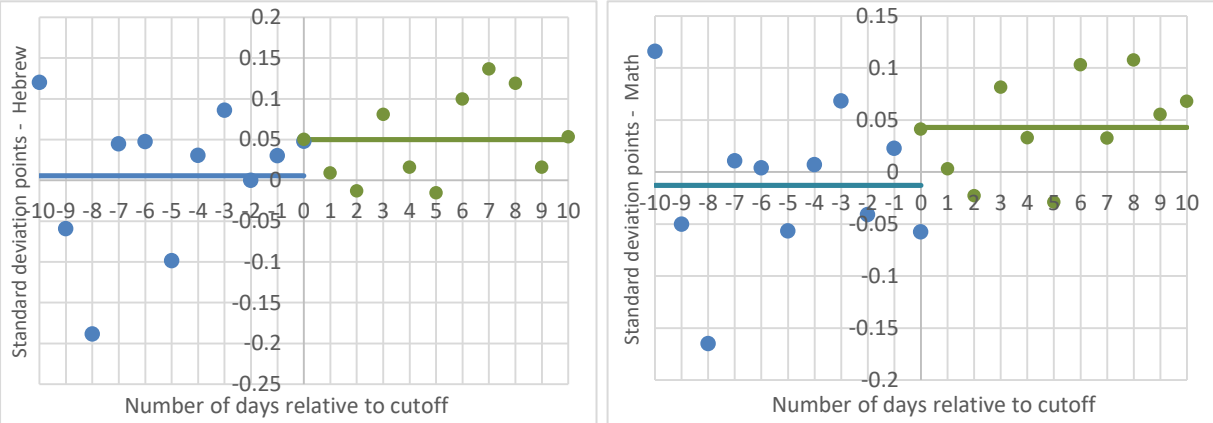


*Figure 6. Difference in normalized test scores between children located before and after the cutoff points (5th grade)*

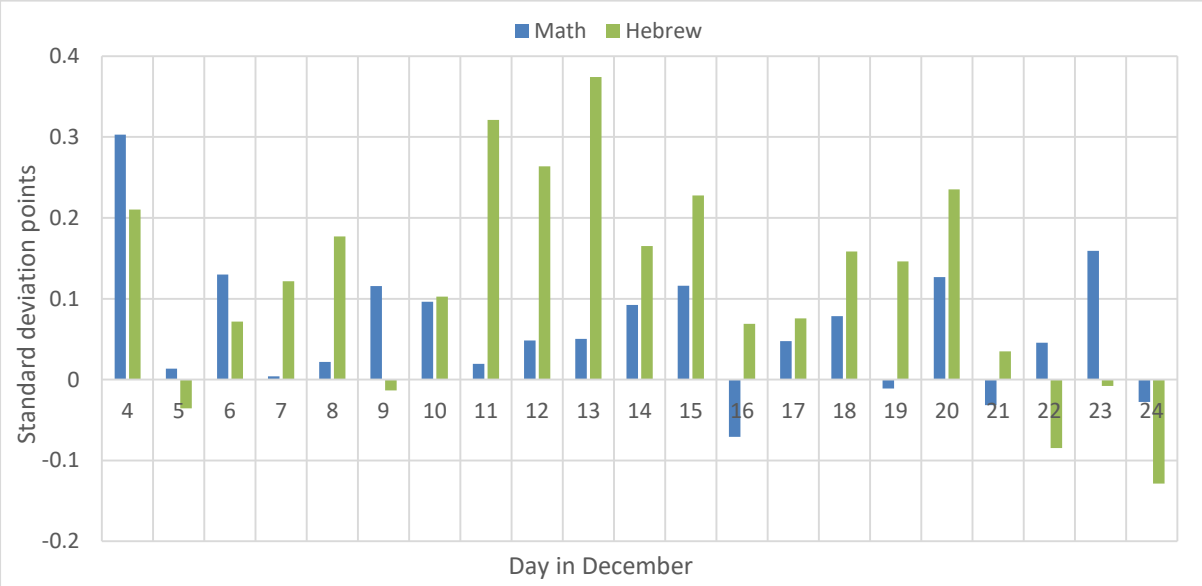

*Figure 7. Difference in normalized test scores between children located before and after the cutoff points (8th grade)*

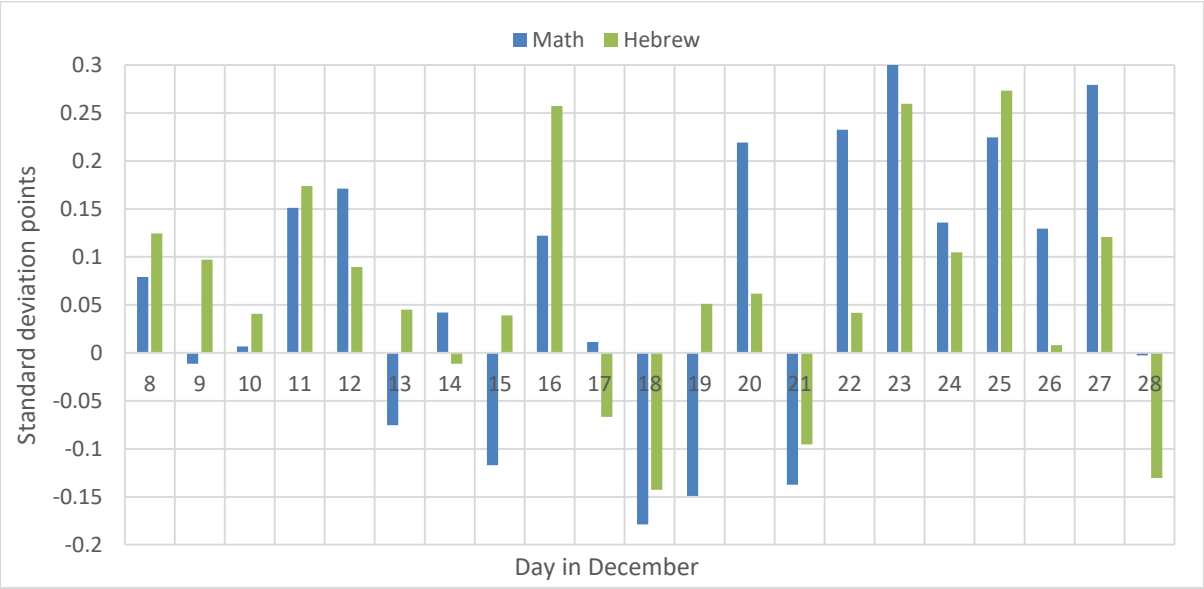

TABLE 1 – SUMMARY STATISTICS (5TH GRADE SAMPLE)

| | *DID Sample* | | | *RDD RC±28* | | |
|---|---|---|---|---|---|---|
| Variables | Obs | Mean | SD | Obs | Mean | SD |
| ***Outcome Variables*** | | | | | | |
| Normalized math score in 5th grade | 120,972 | 0.01 | 0.99 | 18,802 | 0.03 | 0.99 |
| Normalized Hebrew score in 5th grade | 120,117 | 0.01 | 0.99 | 18,632 | 0.04 | 0.99 |
| Percentile math score in 5th grade | 120,972 | 50.16 | 28.77 | 18,802 | 50.73 | 28.87 |
| Percentile Hebrew score in 5th grade | 120,117 | 50.25 | 28.81 | 18,632 | 51.12 | 28.86 |
| ***Age Variables*** | | | | | | |
| After cutoff | 129,217 | 0.48 | 0.50 | 20,086 | 0.49 | 0.50 |
| Entrance age | 129,217 | 6.33 | 0.37 | 20,086 | 6.42 | 0.46 |
| ***Background Variables*** | | | | | | |
| Father's education (1-8 Years) | 129,217 | 0.04 | 0.19 | 20,086 | 0.04 | 0.19 |
| Father's education (9-12 Years) | 129,217 | 0.50 | 0.50 | 20,086 | 0.51 | 0.50 |
| Father's education (13-16 Years) | 129,217 | 0.24 | 0.43 | 20,086 | 0.24 | 0.42 |
| Father's education (17-19 Years) | 129,217 | 0.08 | 0.26 | 20,086 | 0.07 | 0.26 |
| Father's education (20+ Years) | 129,217 | 0.03 | 0.18 | 20,086 | 0.04 | 0.19 |
| Mother's education (1-8 Years) | 129,217 | 0.03 | 0.17 | 20,086 | 0.03 | 0.17 |
| Mother's education (9-12 Years) | 129,217 | 0.50 | 0.50 | 20,086 | 0.50 | 0.50 |
| Mother's education (13-16 Years) | 129,217 | 0.28 | 0.45 | 20,086 | 0.27 | 0.45 |
| Mother's education (17-19 Years) | 129,217 | 0.08 | 0.27 | 20,086 | 0.07 | 0.26 |
| Mother's education (20+ Years) | 129,217 | 0.02 | 0.14 | 20,086 | 0.02 | 0.14 |
| Number of siblings (0-1) | 129,217 | 0.33 | 0.47 | 20,086 | 0.33 | 0.47 |
| Number of siblings (2-3) | 129,217 | 0.47 | 0.50 | 20,086 | 0.46 | 0.50 |
| Number of siblings (4-5) | 129,217 | 0.06 | 0.24 | 20,086 | 0.07 | 0.25 |
| Number of siblings (6-7) | 129,217 | 0.01 | 0.11 | 20,086 | 0.01 | 0.11 |
| Number of siblings (8-11) | 129,217 | 0.00 | 0.05 | 20,086 | 0.00 | 0.05 |
| Number of siblings (12+) | 129,217 | 0.00 | 0.01 | 20,086 | 0.00 | 0.01 |
| Male | 129,217 | 0.50 | 0.50 | 20,086 | 0.50 | 0.50 |
| Mamlachti | 129,217 | 0.77 | 0.42 | 20,086 | 0.76 | 0.43 |
| Father born in Asia or Africa | 129,217 | 0.23 | 0.42 | 20,086 | 0.23 | 0.42 |
| Father born in Australia | 129,217 | 0.00 | 0.03 | 20,086 | 0.00 | 0.03 |
| Father born in Europe | 129,217 | 0.05 | 0.23 | 20,086 | 0.05 | 0.22 |
| Father born in North America | 129,217 | 0.02 | 0.12 | 20,086 | 0.02 | 0.12 |
| Father born in South America | 129,217 | 0.01 | 0.12 | 20,086 | 0.01 | 0.12 |
| Father born in Israel | 129,217 | 0.59 | 0.49 | 20,086 | 0.58 | 0.49 |
| Mother born in Asia or Africa | 129,217 | 0.22 | 0.41 | 20,086 | 0.22 | 0.42 |
| Mother born in Australia | 129,217 | 0.00 | 0.03 | 20,086 | 0.00 | 0.03 |
| Mother born in Europe | 129,217 | 0.06 | 0.24 | 20,086 | 0.06 | 0.23 |
| Mother born in North America | 129,217 | 0.02 | 0.13 | 20,086 | 0.02 | 0.13 |
| Mother born in South America | 129,217 | 0.02 | 0.12 | 20,086 | 0.01 | 0.12 |
| Mother born in Israel | 129,217 | 0.61 | 0.49 | 20,086 | 0.60 | 0.49 |
| Student born in Asia or Africa | 129,217 | 0.05 | 0.23 | 20,086 | 0.05 | 0.22 |
| Student born in Australia | 129,217 | 0.00 | 0.02 | 20,086 | 0.00 | 0.03 |
| Student born in Europe | 129,217 | 0.04 | 0.19 | 20,086 | 0.04 | 0.19 |
| Student born in North America | 129,217 | 0.01 | 0.12 | 20,086 | 0.01 | 0.12 |
| Student born in South America | 129,217 | 0.00 | 0.07 | 20,086 | 0.00 | 0.07 |
| Student born in Israel | 129,217 | 0.89 | 0.32 | 20,086 | 0.89 | 0.31 |

TABLE 2 – SUMMARY STATISTICS (8TH GRADE SAMPLE)

| | *DID Sample* | | | *RDD RC±28* | | |
|---|---|---|---|---|---|---|
| Variables | Obs | Mean | SD | Obs | Mean | SD |
| ***Outcome Variables*** | | | | | | |
| Normalized math score in 8th grade | 115,198 | 0.02 | 0.99 | 17,266 | 0.02 | 1.00 |
| Normalized Hebrew score in 8th grade | 116,480 | 0.03 | 0.98 | 17,419 | 0.02 | 0.99 |
| Percentile math score in 8th grade | 115,198 | 50.45 | 28.76 | 17,266 | 50.54 | 28.81 |
| Percentile Hebrew score in 8th grade | 116,480 | 50.62 | 28.70 | 17,419 | 50.65 | 28.74 |
| ***Age Variables*** | | | | | | |
| After cutoff | 126,220 | 0.47 | 0.50 | 18,910 | 0.48 | 0.50 |
| Entrance age | 126,220 | 6.32 | 0.37 | 18,910 | 6.41 | 0.46 |
| ***Background Variables*** | | | | | | |
| Father's education (1-8 Years) | 126,220 | 0.04 | 0.20 | 18,910 | 0.04 | 0.21 |
| Father's education (9-12 Years) | 126,220 | 0.49 | 0.50 | 18,910 | 0.50 | 0.50 |
| Father's education (13-16 Years) | 126,220 | 0.25 | 0.43 | 18,910 | 0.25 | 0.43 |
| Father's education (17-19 Years) | 126,220 | 0.08 | 0.27 | 18,910 | 0.07 | 0.26 |
| Father's education (20+ Years) | 126,220 | 0.03 | 0.18 | 18,910 | 0.03 | 0.18 |
| Mother's education (1-8 Years) | 126,220 | 0.03 | 0.18 | 18,910 | 0.04 | 0.18 |
| Mother's education (9-12 Years) | 126,220 | 0.49 | 0.50 | 18,910 | 0.50 | 0.50 |
| Mother's education (13-16 Years) | 126,220 | 0.29 | 0.45 | 18,910 | 0.28 | 0.45 |
| Mother's education (17-19 Years) | 126,220 | 0.08 | 0.28 | 18,910 | 0.08 | 0.27 |
| Mother's education (20+ Years) | 126,220 | 0.02 | 0.15 | 18,910 | 0.02 | 0.15 |
| Number of siblings (0-1) | 126,220 | 0.31 | 0.46 | 18,910 | 0.31 | 0.46 |
| Number of siblings (2-3) | 126,220 | 0.41 | 0.49 | 18,910 | 0.40 | 0.49 |
| Number of siblings (4-5) | 126,220 | 0.05 | 0.22 | 18,910 | 0.06 | 0.23 |
| Number of siblings (6-7) | 126,220 | 0.01 | 0.10 | 18,910 | 0.01 | 0.10 |
| Number of siblings (8-11) | 126,220 | 0.00 | 0.05 | 18,910 | 0.00 | 0.05 |
| Number of siblings (12+) | 126,220 | 0.00 | 0.01 | 18,910 | 0.00 | 0.01 |
| Male | 126,220 | 0.50 | 0.50 | 18,910 | 0.49 | 0.50 |
| Mamlachti | 126,220 | 0.80 | 0.40 | 18,910 | 0.79 | 0.40 |
| Father born in Asia or Africa | 126,220 | 0.28 | 0.45 | 18,910 | 0.29 | 0.45 |
| Father born in Australia | 126,220 | 0.00 | 0.03 | 18,910 | 0.00 | 0.03 |
| Father born in Europe | 126,220 | 0.06 | 0.24 | 18,910 | 0.06 | 0.24 |
| Father born in North America | 126,220 | 0.02 | 0.12 | 18,910 | 0.01 | 0.11 |
| Father born in South America | 126,220 | 0.02 | 0.12 | 18,910 | 0.02 | 0.13 |
| Father born in Israel | 126,220 | 0.58 | 0.49 | 18,910 | 0.58 | 0.49 |
| Mother born in Asia or Africa | 126,220 | 0.26 | 0.44 | 18,910 | 0.27 | 0.44 |
| Mother born in Australia | 126,220 | 0.00 | 0.03 | 18,910 | 0.00 | 0.03 |
| Mother born in Europe | 126,220 | 0.06 | 0.24 | 18,910 | 0.06 | 0.24 |
| Mother born in North America | 126,220 | 0.02 | 0.14 | 18,910 | 0.02 | 0.13 |
| Mother born in South America | 126,220 | 0.02 | 0.13 | 18,910 | 0.02 | 0.13 |
| Mother born in Israel | 126,220 | 0.63 | 0.48 | 18,910 | 0.63 | 0.48 |
| Student born in Asia or Africa | 126,220 | 0.10 | 0.31 | 18,910 | 0.11 | 0.31 |
| Student born in Australia | 126,220 | 0.00 | 0.02 | 18,910 | 0.00 | 0.02 |
| Student born in Europe | 126,220 | 0.04 | 0.19 | 18,910 | 0.04 | 0.14 |
| Student born in North America | 126,220 | 0.02 | 0.13 | 18,910 | 0.01 | 0.12 |
| Student born in South America | 126,220 | 0.01 | 0.08 | 18,910 | 0.01 | 0.07 |
| Student born in Israel | 126,220 | 0.83 | 0.37 | 18,910 | 0.83 | 0.38 |

TABLE 3 – BALANCE TEST ON BACKGROUND CHARACTERISTICS

| | *5th Grade* | | | | *8th Grade* | | | |
|---|---|---|---|---|---|---|---|---|
| | *All Year Sample* | | (±28 days) | | *All Year Sample* | | (±28 days) | |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Father's education (1-8 Years) | -0.002 | 0.001 | 0.001 | 0.001 | -0.004 | 0.002 | -0.003 | 0.002 |
| Father's education (9-12 Years) | 0.002 | -0.008 | 0.003 | -0.008 | -0.003 | -0.010 | -0.003 | -0.010 |
| Father's education (13-16 Years) | **0.015**$^{**}$ | 0.017 | 0.000 | 0.017 | 0.003 | -0.008 | -0.002 | -0.008 |
| Father's education (17-19 Years) | -0.006 | 0.001 | 0.004 | 0.001 | 0.003 | 0.001 | 0.002 | 0.001 |
| Father's education (20+ Years) | 0.000 | 0.000 | -0.004 | -0.004 | 0.004 | 0.005 | **0.005**$^{*}$ | 0.005 |
| Mother's education (1-8 Years) | **-0.006**$^{**}$ | 0.000 | 0.001 | -0.001 | **-0.004**$^{*}$ | -0.001 | -0.003 | -0.001 |
| Mother's education (9-12 Years) | 0.007 | 0.001 | **0.013**$^{*}$ | 0.000 | 0.003 | -0.012 | 0.004 | -0.012 |
| Mother's education (13-16 Years) | **0.010**$^{*}$ | 0.005 | -0.007 | 0.005 | 0.001 | 0.007 | 0.004 | 0.007 |
| Mother's education (17-19 Years) | -0.004 | -0.002 | -0.003 | -0.002 | 0.001 | -0.003 | 0.001 | -0.003 |
| Mother's education (20+ Years) | 0.003 | 0.000 | 0.001 | 0.000 | 0.002 | 0.003 | 0.002 | 0.003 |
| Number of siblings (0-1) | 0.001 | 0.012 | 0.002 | 0.012 | 0.007 | 0.019 | 0.007 | 0.019 |
| Number of siblings (2-3) | -0.001 | 0.002 | -0.005 | 0.002 | -0.004 | -0.001 | -0.010 | -0.001 |
| Number of siblings (4-5) | -0.005 | -0.007 | -0.001 | -0.007 | 0.004 | -0.002 | 0.001 | -0.002 |
| Number of siblings (6-7) | 0.000 | -0.003 | 0.000 | -0.003 | 0.000 | -0.001 | -0.002 | -0.001 |
| Number of siblings (8-11) | **0.001**$^{**}$ | 0.001 | 0.001 | 0.001 | 0.000 | 0.000 | -0.001 | 0.000 |
| Number of siblings (12+) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Male | -0.007 | -0.014 | -0.009 | -0.014 | 0.008 | -0.011 | 0.005 | -0.011 |
| Mamlachti | **0.012**$^{**}$ | 0.007 | 0.002 | 0.007 | -0.003 | 0.002 | 0.007 | 0.002 |
| Father born in Asia-Africa | **-0.017**$^{***}$ | 0.001 | **-0.020**$^{***}$ | 0.001 | **-0.015**$^{**}$ | -0.009 | **-0.016**$^{**}$ | -0.009 |
| Father born in Australia | **-0.001**$^{*}$ | -0.001 | **-0.001**$^{**}$ | -0.001 | 0.000 | 0.000 | 0.000 | 0.000 |
| Father born in Europe | -0.002 | -0.009 | -0.005 | -0.009 | -0.002 | -0.002 | 0.004 | -0.002 |
| Father born in North America | -0.001 | -0.003 | 0.000 | -0.003 | 0.001 | 0.002 | **0.004**$^{**}$ | 0.002 |
| Father born in South America | 0.000 | 0.000 | 0.002 | 0.000 | -0.001 | -0.001 | 0.000 | -0.001 |
| Father born in Israel | 0.001 | 0.014 | **-0.048**$^{***}$ | 0.014 | **-0.020**$^{***}$ | 0.003 | 0.005 | 0.003 |
| Mother born in Asia-Africa | **-0.017**$^{***}$ | 0.001 | **-0.024**$^{***}$ | 0.001 | **-0.011**$^{*}$ | -0.005 | **0.024**$^{***}$ | -0.005 |
| Mother born in Australia | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Mother born in Europe | -0.004 | -0.009 | **-0.006**$^{*}$ | -0.009 | -0.003 | -0.002 | 0.004 | -0.002 |
| Mother born in North America | -0.003 | -0.005 | -0.002 | -0.005 | 0.002 | 0.003 | **0.004**$^{**}$ | 0.003 |
| Mother born in South America | 0.000 | 0.002 | 0.003 | 0.002 | 0.000 | 0.003 | 0.001 | 0.003 |
| Mother born in Israel | 0.003 | 0.011 | **-0.044**$^{***}$ | 0.011 | **0.013**$^{**}$ | -0.008 | **0.015**$^{**}$ | -0.008 |
| Student born in Asia-Africa | **-0.007**$^{**}$ | -0.006 | -0.002 | -0.006 | **-0.013**$^{***}$ | -0.007 | **-0.019**$^{***}$ | -0.007 |
| Student born in Australia | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Student born in Europe | **-0.006**$^{**}$ | **-0.016**$^{**}$ | -0.003 | **-0.016**$^{**}$ | -0.002 | -0.004 | 0.002 | -0.004 |
| Student born in North America | 0.002 | 0.003 | 0.001 | 0.003 | 0.000 | -0.003 | -0.001 | -0.003 |
| Student born in South America | **0.002**$^{*}$ | 0.002 | 0.001 | 0.002 | 0.000 | 0.002 | 0.000 | 0.002 |
| Student born in Israel | **0.009**$^{*}$ | **0.017**$^{**}$ | 0.002 | **0.017**$^{**}$ | **0.016**$^{***}$ | 0.012 | **0.017**$^{***}$ | 0.012 |
| Observations | 129,217 | 129,217 | 20,086 | 20,086 | 126,220 | 126,220 | 18,910 | 18,910 |
| ***Date-of-year fixed effects*** | ***No*** | ***Yes*** | ***No*** | ***Yes*** | ***No*** | ***Yes*** | ***No*** | ***Yes*** |
| ***Month fixed effects*** | ***Yes*** | ***No*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** |

Note: * denotes significance at the 10% level, ** denotes significance at the 5% level, *** denotes significance at the 1% level.

TABLE 4 - DIFFERENCE–IN-DIFFERENCE REDUCED FORM ESTIMATES ON TEST SCORES

| | *Base Results* *Cutoffs in December* | | *Placebo 1* *Cutoffs in March* | | *Placebo 2* *Cutoffs in September* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Normalized score in 5th grade | Hebrew | Math | Hebrew | Math | Hebrew | Math |
| After cutoff | 0.09*** | 0.05** | -0.02 | 0.01 | 0.01 | -0.02 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Observations | 120,117 | 120,972 | 47,655 | 47,920 | 52,300 | 52,756 |
| Percentile score in 5th grade | Hebrew | Math | Hebrew | Math | Hebrew | Math |
| After cutoff | 2.87*** | 1.83** | -0.62 | 0.77 | 0.13 | -0.28 |
| | (0.74) | (0.73) | (0.75) | (0.77) | (0.73) | (0.77) |
| Observations | 120,117 | 120,972 | 47,655 | 47,920 | 52,300 | 52,756 |
| Normalized score in 8th grade | Hebrew | Math | Hebrew | Math | Hebrew | Math |
| After cutoff | 0.06** | 0.08*** | 0.04 | 0.01 | -0.03 | 0.00 |
| | (0.02) | (0.03) | (0.03) | (0.03) | (0.02) | (0.03) |
| Observations | 116,480 | 115,198 | 45,696 | 45,276 | 51,346 | 50,744 |
| Percentile score in 8th grade | Hebrew | Math | Hebrew | Math | Hebrew | Math |
| After cutoff | 1.70** | 2.53*** | 1.40 | 0.04 | -0.33 | 0.22 |
| | (0.70) | (0.72) | (0.79) | (0.79) | (0.70) | (0.71) |
| Observations | 116,480 | 115,198 | 45,696 | 45,276 | 51,346 | 50,744 |

Note: Standard errors clustered at the school level are in parentheses. In all regressions we control for the children's background characteristics described in the text. * denotes significance at the 10% level, ** denotes significance at the 5% level, *** denotes significance at the 1% level.

TABLE 5 - DID AND RDD ESTIMATES OF THE ENTRANCE AGE EFFECT ON NORMALZIED TEST SCORES

| | DID (July-May) | | | | | | RDD (±28 days) | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | *OLS* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Normalized Hebrew score in 5th grade | | | | | | | | | | |
| Entrance age | -0.07*** | 0.29*** | 0.29*** | 0.30*** | 0.27*** | 0.27*** | 0.23** | 0.31*** | 0.27*** | 0.32*** |
| | (0.01) | (0.08) | (0.08) | (0.09) | (0.02) | (0.02) | (0.10) | (0.07) | (0.09) | (0.04) |
| F-Statistics on excluded instrument | - | 1452.3 | 1452.3 | 1377.1 | 24642.7 | 16695.2 | 519.8 | 1036.5 | 676.5 | 4120.3 |
| Observations | 120,117 | 120,117 | 120,117 | 120,117 | 120,117 | 120,117 | 18,632 | 4,804 | 18,632 | 18,632 |
| Normalized Hebrew score in 8th grade | | | | | | | | | | |
| Entrance age | -0.16*** | 0.21** | 0.21** | 0.21** | 0.15*** | 0.15*** | 0.15 | 0.22*** | 0.05 | 0.20*** |
| | (0.01) | (0.09) | (0.09) | (0.10) | (0.02) | (0.02) | (0.12) | (0.07) | (0.10) | (0.04) |
| F-Statistics on excluded instrument | - | 1047.2 | 1049.8 | 986.9 | 22973.5 | 14716.1 | 355.3 | 901.8 | 486.6 | 3512.9 |
| Observations | 116,480 | 116,480 | 116,480 | 116,480 | 116,480 | 116,480 | 17,419 | 4,180 | 17,419 | 17,419 |
| Normalized math score in 5th grade | | | | | | | | | | |
| Entrance age | -0.10*** | 0.16** | 0.16** | 0.15* | 0.27*** | 0.23*** | 0.17* | 0.25*** | 0.24*** | 0.25*** |
| | (0.01) | (0.08) | (0.08) | (0.09) | (0.02) | (0.03) | (0.10) | (0.08) | (0.09) | (0.04) |
| F-Statistics on excluded instrument | - | 1352.7 | 1348.3 | 1278.7 | 24759.0 | 16790.9 | 515.7 | 1044.6 | 663.5 | 4074.2 |
| Observations | 120,972 | 120,972 | 120,972 | 120,972 | 120,972 | 120,972 | 18,802 | 4,849 | 18,802 | 18,802 |
| Normalized math score in 8th grade | | | | | | | | | | |
| Entrance age | -0.16*** | 0.29*** | 0.29*** | 0.26** | 0.21*** | 0.20*** | 0.24** | 0.34*** | 0.21** | 0.24*** |
| | (0.01) | (0.09) | (0.09) | (0.10) | (0.03) | (0.03) | (0.12) | (0.09) | (0.10) | (0.04) |
| F-Statistics on excluded instrument | - | 936.9 | 939.4 | 898.8 | 21992.9 | 14009.1 | 331.9 | 850.1 | 467.8 | 3400.0 |
| Observations | 115,198 | 115,198 | 115,198 | 115,198 | 115,198 | 115,198 | 17,266 | 4,181 | 17,266 | 17,266 |
| ***Date-of-year fixed effects*** | ***No*** | ***Yes*** | ***Yes*** | ***Yes*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** |
| ***Day-of-week fixed effects*** | ***No*** | ***Yes*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** |
| ***Quadratic trend of the running variable*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***Yes*** | ***No*** |
| ***Linear trend for month of birth*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** | ***No*** |
| ***Controls*** | ***Yes*** | ***Yes*** | ***Yes*** | ***No*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** |

Note: Standard errors clustered at the school level are in parentheses. In all regressions, we control for the children's background characteristics described in the text.
* denotes significance at the 10% level, ** denotes significance at the 5% level, *** denotes significance at the 1% level.

TABLE 6 - DID AND RDD ESTIMATES OF THE ENTRANCE AGE EFFECT ON PERCENTILE SCORES

| | DID (July-May) | | | | | | RDD (±28 days) | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | *OLS* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Normalized Hebrew score in 5th grade | | | | | | | | | | |
| Entrance age | -1.18*** | 9.22*** | 9.23*** | 9.62*** | 8.95*** | 9.30*** | 7.47** | 9.83*** | 8.55*** | 10.11*** |
| | (0.28) | (2.38) | (2.38) | (2.50) | (0.63) | (0.72) | (3.03) | (2.10) | (2.66) | (1.11) |
| F-Statistics on excluded instrument | - | 1452.3 | 1452.3 | 1377.1 | 24642.7 | 16695.2 | 519.8 | 1036.5 | 676.5 | 4120.3 |
| Observations | 120,117 | 120,117 | 120,117 | 120,117 | 120,117 | 120,117 | 18,632 | 4,804 | 18,632 | 18,632 |
| Normalized Hebrew score in 8th grade | | | | | | | | | | |
| Entrance age | -3.88*** | 5.94** | 5.97** | 6.20** | 5.47*** | 5.50*** | 4.37 | 8.09*** | 2.60 | 6.54*** |
| | (0.28) | (2.55) | (2.54) | (2.83) | (0.66) | (0.72) | (3.35) | (2.24) | (2.89) | (1.05) |
| F-Statistics on excluded instrument | - | 1047.2 | 1049.8 | 986.9 | 22973.5 | 14716.1 | 355.3 | 901.8 | 486.6 | 3512.9 |
| Observations | 116,480 | 116,480 | 116,480 | 116,480 | 116,480 | 116,480 | 17,419 | 4,180 | 17,419 | 17,419 |
| Normalized math score in 5th grade | | | | | | | | | | |
| Entrance age | -2.23*** | 6.15** | 6.16** | 5.83** | 8.72*** | 7.46*** | 6.05** | 8.44*** | 7.15*** | 8.60*** |
| | (0.29) | (2.46) | (2.46) | (2.54) | (0.67) | (0.74) | (2.94) | (2.21) | (2.60) | (1.08) |
| F-Statistics on excluded instrument | - | 1352.7 | 1348.3 | 1278.7 | 24759.0 | 16790.9 | 515.7 | 1044.6 | 663.5 | 4074.2 |
| Observations | 120,972 | 120,972 | 120,972 | 120,972 | 120,972 | 120,972 | 18,802 | 4,849 | 18,802 | 18,802 |
| Normalized math score in 8th grade | | | | | | | | | | |
| Entrance age | -4.36*** | 9.45*** | 9.53*** | 8.50*** | 6.45*** | 6.26*** | 7.63** | 10.82*** | 6.38** | 7.34*** |
| | (0.32) | (2.81) | (2.80) | (2.93) | (0.76) | (0.79) | (3.61) | (2.59) | (3.12) | (1.18) |
| F-Statistics on excluded instrument | - | 936.9 | 939.4 | 898.8 | 21992.9 | 14009.1 | 331.9 | 850.1 | 467.8 | 3400.0 |
| Observations | 115,198 | 115,198 | 115,198 | 115,198 | 115,198 | 115,198 | 17,266 | 4,181 | 17,266 | 17,266 |
| ***Date-of-year fixed effects*** | ***No*** | ***Yes*** | ***Yes*** | ***Yes*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** |
| ***Day-of-week fixed effects*** | ***No*** | ***Yes*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***Yes*** | ***No*** |
| ***Quadratic trend for date of birth*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***Yes*** | ***No*** |
| ***Linear trend for month of birth*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** | ***No*** |
| ***Controls*** | ***Yes*** | ***Yes*** | ***Yes*** | ***No*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** |

Note: Standard errors clustered at the school level are in parentheses. In all regressions, we control for the children's background characteristics described in the text.
* denotes significance at the 10% level, ** denotes significance at the 5% level, *** denotes significance at the 1% level.

TABLE 7 - HETEROGENEOUS ENTRANCE AGE EFFECTS BY GENDER AND PARENTS' EDUCATION

| | *DID* | | *RDD (with Date FE)* | |
|---|---|---|---|---|
| | Panel A – Heterogeneous effect by gender | | | |
| Normalized score in 5th grade | Hebrew | Math | Hebrew | Math |
| Male | -0.26 | -0.25 | -0.28 | 0.02 |
| | (0.28) | (0.30) | (0.56) | (0.60) |
| Entrance age | 0.29*** | 0.14* | 0.23** | 0.17* |
| | (0.07) | (0.07) | (0.09) | (0.09) |
| **Male * Entrance age** | **-0.01** | **0.05** | **0.00** | **0.00** |
| | **(0.05)** | **(0.05)** | **(0.09)** | **(0.09)** |
| Observations | 120,117 | 120,972 | 18,632 | 18,802 |
| Normalized score in 8th grade | Hebrew | Math | Hebrew | Math |
| Male | -0.93*** | -0.08 | -0.40 | 0.07 |
| | (0.31) | (0.29) | (0.54) | (0.56) |
| Entrance age | 0.16** | 0.29*** | 0.14 | 0.26** |
| | (0.08) | (0.09) | (0.11) | (0.12) |
| **Male * Entrance age** | **0.09*** | **0.00** | **0.01** | **-0.03** |
| | **(0.05)** | **(0.05)** | **(0.08)** | **(0.10)** |
| Observations | 116,480 | 115,198 | 17,419 | 17,266 |
| | Panel B – Heterogeneous effect by parents' education | | | |
| Normalized score in 5th grade | Hebrew | Math | Hebrew | Math |
| Parents quartile | -0.44*** | -0.36*** | -0.17 | -0.17 |
| | (0.13) | (0.13) | (0.24) | (0.19) |
| Entrance age | 0.21*** | 0.11 | 0.27** | 0.20 |
| | (0.08) | (0.08) | (0.13) | (0.14) |
| **Parents quartile * Entrance age** | **0.03** | **0.02** | **-0.01** | **-0.01** |
| | **(0.02)** | **(0.02)** | **(0.04)** | **(0.04)** |
| Observations | 120,117 | 120,972 | 18,632 | 18,802 |
| Normalized score in 8th grade | Hebrew | Math | Hebrew | Math |
| Parents quartile | -0.10 | -0.03 | 0.03 | 0.08 |
| | (0.13) | (0.14) | (0.24) | (0.23) |
| Entrance age | 0.27*** | 0.39*** | 0.27* | 0.40*** |
| | (0.11) | (0.10) | (0.13) | (0.16) |
| **Parents quartile * Entrance age** | **-0.03** | **-0.04*** | **-0.05** | **-0.06** |
| | **(0.02)** | **(0.02)** | **(0.04)** | **(0.04)** |
| Observations | 116,480 | 115,198 | 17,419 | 17,266 |

Note: Standard errors clustered at the school level are in parentheses. In all regressions, we control for list of background characteristics of the children described in the text. * denote significance at the 10% level, ** denotes significance at the 5% level, *** denotes significance at the 1% level.

## Appendix A: Evidence that dates of birth are not randomly distributed

In this appendix, using our dataset, we provide three indications that a child's date of birth is endogenous. First, Figure A1 shows that the number of births in each of the months between July and September is much larger than between January and May. Also, the number of births per day in the middle of June is extremely small. Second, Figure A2 shows that, probably due to scheduled births, the number of births on each of the weekend days is much lower than the number of births on each of the other days of the week (weekends in Israel include Friday and Saturday, with Friday being a half-day in elementary schools and many workplaces). Dickert-Conlin and Chandra (1999) and McEwan and Shapiro (2008) similarly found that the frequency of births declines during weekends. Third, in order to provide direct evidence that parents' choice of date of birth is correlated with their characteristics, we estimated several characteristics of the parents as a function of a set of month fixed effects, where January is the omitted category. The results, reported in Table A1 (standard errors clustered at the school level are in parentheses), indicate that there are substantial parental differences by month of birth. For example, fathers and mothers of children born between March and May are between 0.11-0.17 years more educated relative to those of children born in January. Differences in other characteristics are revealed as well. In addition, Table A2 reports differences between parents of children born on different days of the week. Here the differences are less substantial and significant, but some degree of selection exists. For example, fathers and mothers of Sunday births are significantly more likely to be born in Israel. Thus, if enrollment cutoffs coincidentally fall near specific days of the week, it may introduce correlation between our instrument and student characteristics even in the absence of strategic birth timing (McEwan and Shapiro, 2008). In fact, the first column of Table A2 shows that our instrument indicating whether the date of birth is located before or after the cutoff point is strongly associated with all of the day-of-week fixed effects. Thus, failing to

control for day-of-week fixed effects may potentially lead to biased estimates. Taken together, we provide strong evidence that a child's date of birth is an endogenous variable.

*Figure A1. Birth distribution by month*

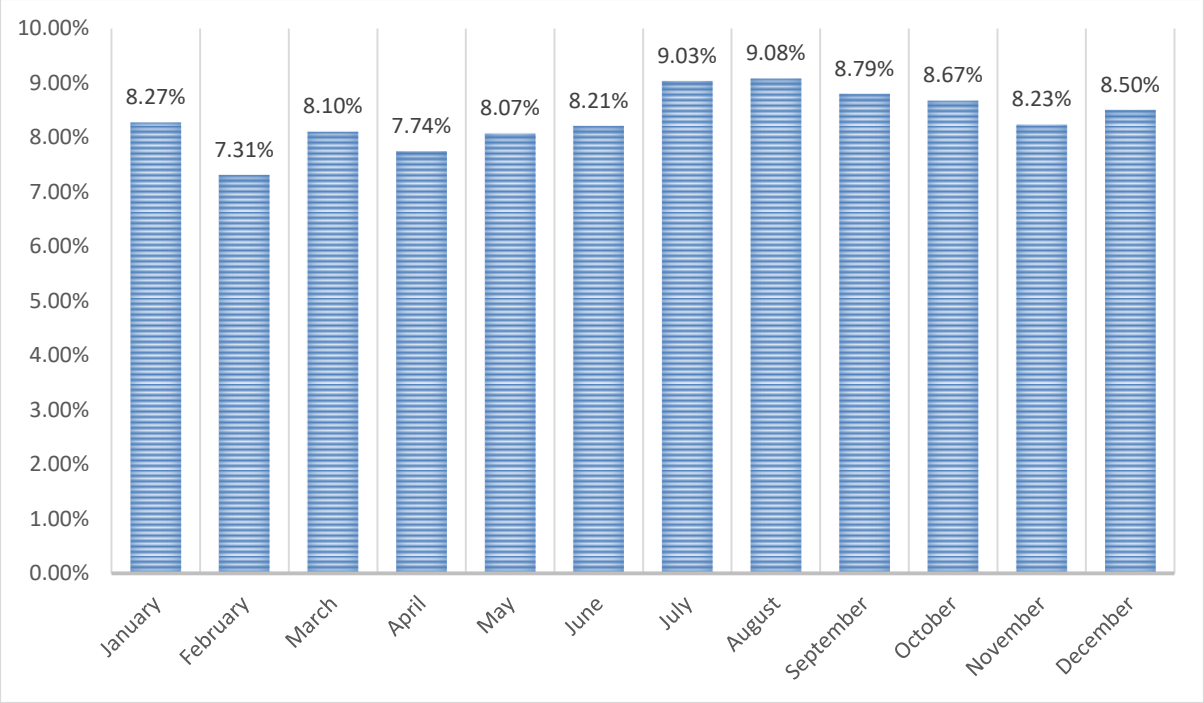


*Figure A2. Birth distribution by day of week*

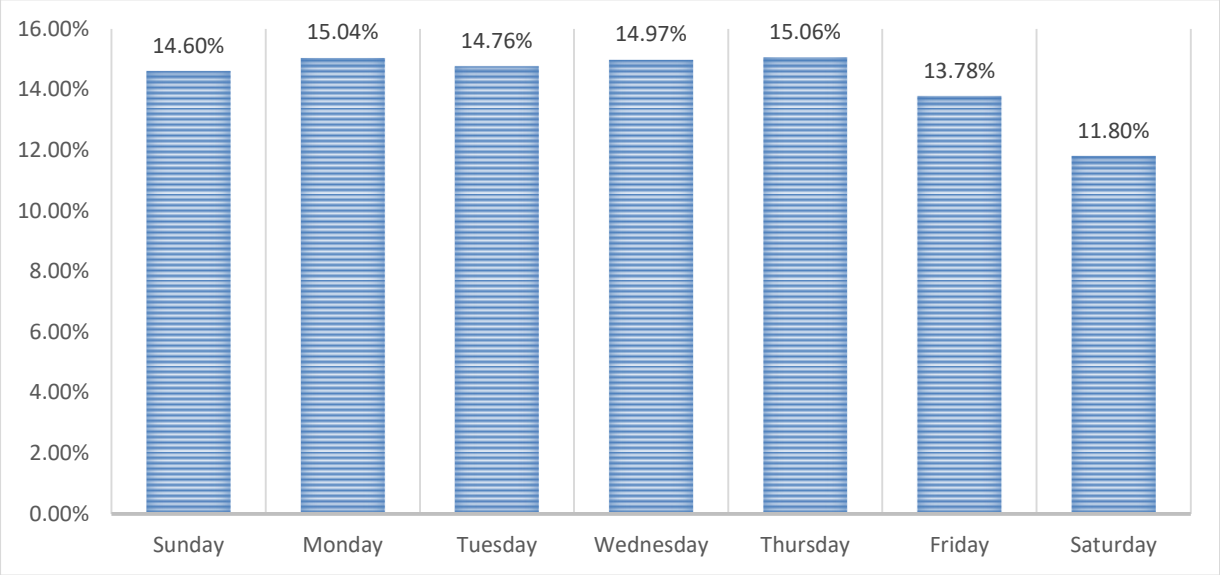

TABLE A1 – DIFFERENCES IN BACKGROUND CHARACTERISTICS BY MONTH OF BIRTH

| *Month* | Father's Education | Mother's Education | Number of Siblings | Father Born in Israel | Mother Born in Israel | Student Born in Israel |
|---|---|---|---|---|---|---|
| February | -0.14 | -0.01 | -0.03*** | 0.00 | -0.01* | -0.01* |
| | (0.03) | (0.03) | (0.01) | (0.01) | (0.00) | (0.00) |
| March | 0.13*** | 0.13*** | -0.02 | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| April | 0.11*** | 0.16*** | -0.02 | 0.00 | -0.01*** | -0.01*** |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| May | 0.14*** | 0.17*** | -0.01 | 0.01** | 0.02*** | -0.01** |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| June | 0.07** | 0.09*** | -0.02* | 0.00 | 0.01 | -0.02*** |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| July | 0.02 | 0.04 | -0.01 | 0.00 | 0.01 | -0.02*** |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| August | -0.02 | -0.01 | 0.00 | 0.00 | 0.01 | 0.00 |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| September | -0.02 | 0.04 | 0.01 | 0.00 | 0.01** | -0.01* |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| October | -0.01 | 0.04 | 0.01 | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| November | -0.01 | -0.01 | 0.01 | 0.00 | 0.01 | -0.01*** |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |
| December | 0.01 | 0.02 | 0.01 | -0.01* | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.01) | (0.00) | (0.00) | (0.00) |

Note: Standard errors clustered at the school level are in parentheses. ** denotes significance at the 5% level, *** denotes significance at the 1% level.

TABLE A2 – DIFFERENCES IN BACKGROUND CHARECTARISTICS BY DAY OF WEEK

| *Weekday* | After Cutoff | Father's Education | Mother's Education | Number of Siblings | Father Born in Israel | Mother Born in Israel | Student Born in Israel |
|---|---|---|---|---|---|---|---|
| Monday | -0.01*** | 0.02 | 0.01 | 0.00 | -0.01*** | -0.01** | -0.02*** |
| | (0.00) | (0.02) | (0.02) | (0.01) | (0.00) | (0.00) | (0.00) |
| Tuesday | -0.01** | 0.00 | 0.01 | 0.01 | -0.01*** | -0.01*** | -0.02*** |
| | (0.00) | (0.02) | (0.02) | (0.01) | (0.00) | (0.00) | (0.00) |
| Wednesday | -0.01** | 0.02 | 0.03 | -0.01 | -0.01*** | -0.01*** | -0.02*** |
| | (0.00) | (0.02) | (0.02) | (0.01) | (0.00) | (0.00) | (0.00) |
| Thursday | -0.01*** | 0.01 | 0.02 | -0.01 | -0.01*** | -0.01*** | -0.02*** |
| | (0.00) | (0.02) | (0.02) | (0.01) | (0.00) | (0.00) | (0.00) |
| Friday | -0.01* | 0.03 | 0.02 | -0.01* | -0.02*** | -0.01*** | -0.03*** |
| | (0.00) | (0.02) | (0.02) | (0.00) | (0.00) | (0.00) | (0.00) |
| Saturday | -0.01** | -0.01 | -0.01 | 0.00 | -0.02*** | -0.02*** | -0.03*** |
| | (0.00) | (0.02) | (0.02) | (0.01) | (0.00) | (0.00) | (0.00) |

Note: Standard errors clustered at the school level are in parentheses. * denotes significance at the 10% level, ** denotes significance at the 5% level, *** denotes significance at the 1% level.

## Appendix B: The Jewish calendar

In this appendix we present the results of two surveys that we conducted in order to demonstrate that Israeli parents do not generally use the Jewish calendar in their everyday life and are not even aware that the entrance cutoff date is set according to this calendar. This may be due to the fact that in the recent decades, the use of this calendar has steadily declined in favor of the internationally accepted Gregorian calendar, mainly as a result of globalization. Today, its use is restricted predominantly to determine the dates of the Jewish holidays, while its use for civil purposes is quite negligible.

One survey used the services of Sekernet (a marketing survey organization) and was conducted among a representative sample of 200 parents with children already in school, and a second among 159 economics students at Ben-Gurion University. In the first survey, when we asked "What is the date today?" or "What is the birthdate of your firstborn child?" all the respondents answered with the Gregorian date. It was also revealed that only 4% of the respondents schedule meetings only according to the Jewish calendar relative to 85% who schedule meetings only according to the Gregorian one (the rest use both calendars). In order to directly assess whether people know that the school entrance cutoff date is the first day of Tevet we asked: "What was the school entrance cutoff date of your first-born child?" Not even one respondent provided the correct Hebrew cutoff date, and furthermore, only 5% provided a Jewish date, 61% a Gregorian date (20% of which reported December 31$^{st}$, thinking the school entrance cutoff date would naturally be at the end of the year), and 34% stated that they don’t remember. In the second survey, only 52% of the students knew their own Jewish birthdate, 6% knew their mother's Jewish birthdate and 7% knew their father’s Jewish birthdate. In contrast, 100% knew their own Gregorian birthdate and 95% knew each of their parents' Gregorian birthdate. Taken together, the results of the two surveys clearly indicate that for civil purposes, most people predominantly refer to the Gregorian calendar

and not the Jewish one. Also, most parents are not even aware that the school entrance cutoff date is the first day of Tevet. Thus, we can conclude that it is very unlikely that parents would manipulate their children's Jewish date of birth in order to influence the school entrance age, and they would not have any other incentive to do so.

# Appendix C: First stage estimates for Tables 5 and 6

TABLE C1 – FIRST STAGE ESTIMATES FOR TABLES 5 AND 6

| | DID (July-May) | | | | | RDD (±28 days) | | | |
|---|---|---|---|---|---|---|---|---|---|
| | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* | *IV* |
| Variables | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Normalized Hebrew score in 5th grade | | | | | | | | | |
| Entrance age | 0.31*** | 0.31*** | 0.31*** | 0.29*** | 0.34*** | 0.30*** | 0.39*** | 0.31*** | 0.38*** |
| | (0.01) | (0.01) | (0.01) | (0.00) | (0.00) | (0.01) | (0.01) | (0.01) | (0.01) |
| Observations | 120,117 | 120,117 | 120,117 | 120,117 | 120,117 | 18,632 | 4,804 | 18,632 | 18,632 |
| Normalized Hebrew score in 8th grade | | | | | | | | | |
| Entrance age | 0.28*** | 0.28*** | 0.28*** | 0.27*** | 0.34*** | 0.27*** | 0.38*** | 0.28*** | 0.37*** |
| | (0.01) | (0.01) | (0.01) | (0.00) | (0.00) | (0.01) | (0.01) | (0.01) | (0.01) |
| Observations | 116,480 | 116,480 | 116,480 | 116,480 | 116,480 | 17,419 | 4,180 | 17,419 | 17,419 |
| Normalized math score in 5th grade | | | | | | | | | |
| Entrance age | 0.30*** | 0.30*** | 0.30*** | 0.29*** | 0.34*** | 0.30*** | 0.39*** | 0.31*** | 0.38*** |
| | (0.01) | (0.01) | (0.01) | (0.00) | (0.00) | (0.01) | (0.01) | (0.01) | (0.01) |
| Observations | 120,972 | 120,972 | 120,972 | 120,972 | 120,117 | 18,802 | 4,849 | 18,802 | 18,802 |
| Normalized math score in 8th grade | | | | | | | | | |
| Entrance age | 0.27*** | 0.27*** | 0.27*** | 0.26*** | 0.33*** | 0.26*** | 0.37*** | 0.27*** | 0.37*** |
| | (0.01) | (0.01) | (0.01) | (0.00) | (0.00) | (0.01) | (0.01) | (0.01) | (0.01) |
| Observations | 115,198 | 115,198 | 115,198 | 115,198 | 115,198 | 17,266 | 4,181 | 17,266 | 17,266 |
| ***Date-of-year fixed effects*** | ***Yes*** | ***Yes*** | ***Yes*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** |
| ***Day-of-week fixed effects*** | ***Yes*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***Yes*** | ***No*** |
| ***Quadratic trend of the running variable*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***No*** | ***Yes*** | ***No*** |
| ***Linear trend for month of birth*** | ***No*** | ***No*** | ***No*** | ***No*** | ***Yes*** | ***No*** | ***No*** | ***No*** | ***No*** |
| ***Controls*** | ***Yes*** | ***Yes*** | ***No*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** | ***Yes*** |

Note: Standard errors clustered at the school level are in parentheses. In all regressions, we control for the children's background characteristics described in the text.
* denotes significance at the 10% level, ** denotes significance at the 5% level