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ABSTRACT

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

Using Israeli data, we estimate the effect of school entrance age (SEA) on student outcomes. Unlike much of the recent literature, our identification strategy strictly satisfies the monotonicity assumption required for interpreting our estimates as the local average treatment effect (LATE), and also separates the effect of SEA from date of birth effects. We find that delaying school entry by one year increases fifth grade test scores in Hebrew by 0.34 standard deviations and in math by 0.19. Interestingly, while the advantage in Hebrew slightly decreases in eighth grade, in math it almost doubles. We also show that by failing to control for date of birth fixed effects we would have erroneously concluded that the SEA effect on math test scores decreases slightly from fifth grade to eight grade while it actually substantially increases.

JEL Classification: I21, J24

Keywords: school entrance age, student outcomes, date of birth

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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, thus raising the kindergarten entrance age (Bedhard and Dhuey 2006, Elder and Lubotsky 2009, Stipek 2002). It has also influenced more and more parents to voluntarily postpone their child's entry into school for a year – a practice known as redshirting (McEwan 2008, Stipek 2002, Graue and DiPerna 2000, Paul 2010). In fact, Elder and Lubotski (2009) report that while in October 1980, only 9.8 percent of five-year-old children were not yet enrolled in kindergarten, by October 2002 this share had risen to 20.8 percent. 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. For this reason, a vast literature has investigated the causal effect of entrance age on educational and economic outcomes.

Overall, the results have been quite mixed. On the one hand, Angrist and Krueger (1991) show that a higher school entrance age (SEA) 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 found 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). Another related issue concerns the persistence of the SEA effect. Bedhard and Dhuey (2006) show that the SEA effect is long-lasting and significant. The oldest children in their study not only scored between 2-9 percentiles higher than the youngest ones, but also were found to be more likely to attend university. Similarly, McEwan and Shapiro (2008) found that a one-year delay in school entrance increases eighth grade test scores by as much as 0.3 standard deviations, and Fredriksson and Ockert (2014) found that such a delay increases educational attainment in the long run by 0.16 years. In contrast, Elder and Lubotsky (2009) demonstrated that the positive entrance age effect dissipates as early as the first grades of elementary school. Dobkin and Ferreira (2010) found that while entering school early increases educational attainment, it also decreases academic performance while in school and does not affect outcomes in the labor market. They thus conclude that the overall net effect of entering school early is close to zero. Other studies have shown that the positive effect of entrance age endures longer when children are assigned to tracks at an earlier age (Muhlenweg and Puhani 2010, Fredrickson and Ockert 2014). The literature is also mixed with regard to heterogeneous effects. Dater (2006) found that the benefits from delaying entrance to kindergarten are significantly greater among poor children, but Elder and Lubotsky (2009) showed that they are greater for children of high income parents. In regard to gender, McEwan and Shapiro (2008) provided evidence that the effect of school entrance age on test scores is larger among boys than girls, but Datar (2006) and Puhani and Weber (2007) showed that it is higher for girls.

Previous studies that examined SEA effects generally acknowledge that entrance age is an endogenous variable. To deal with this concern they used school entry cutoffs as an exogenous source of variation in entrance age. More specifically, a dummy variable indicating whether the child's birth date is before or after the entry cutoff date was used as an instrument for the actual entrance age.[1] As required, this instrument is strongly associated with the actual entrance age because children who turn six after the cutoff date must delay enrollment by one

[1] As an alternative instrumental variable, some used the age at which a child enters school if he starts school in the first year allowed by law.

year and thus enter school when they are a year older.[2] However, the causal interpretation of the estimates in these studies relies on two assumptions which may not hold in practice: monotonicity and randomness of dates of birth, as explained in the following.

Monotonicity requires that all children who are affected by the instrument must be affected in the same direction (Imbens and Angrist 1994; Angrist and Imbens, 1995; Angrist et al. 1996). It has been shown that this assumption is especially crucial when the gain from the treatment is heterogeneous across the population and people sort themselves into treatment based on this gain (Heckman et al. 2006). This is exactly the case in estimating the effect of entrance age on outcomes: the gain from entering school older is heterogeneous across the population and parents take into account this gain when making their school entry decisions. However, recent studies show that strategic behavior by a non-negligible share of parents to voluntarily postpone their child's entry to school creates violations of the monotonicity assumption (Barua and Lang 2011, Aliprantis 2012, Fiorini and Stevens 2014). These violations derive 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.[3] 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. In this case, the obtained estimates cannot be interpreted as the LATE. Interestingly, by performing a stochastic dominance test, Fiorini and Stevens (2014) showed that the violation of the monotonicity assumption is significant even in a regression discontinuity framework. They suggested that monotonicity violations may be decreased by either shrinking the RD sample to

[2] Some of these studies included in their analysis all months of birth while others used a regression discontinuity approach in which they focused on a narrow interval of one month on either side of the entrance cutoff date. Since only a small minority could observe the child's exact date of birth they were not able to narrow the sample any further around the entrance cutoff date, nor could they include a trend of the running variable (date of birth).

[3] According to the LATE theorem, children who do not simply comply with the entrance rule but rather enter school in the current year or in the subsequent year regardless of where they are located relative to the cutoff are termed 'never takers' and 'always takers", respectively. For these children, counterfactually shifting their date of birth to after the cutoff date only reduces their school entrance age.

include only children born very close to the cutoff date, thus leaving us with a much smaller sample, or alternatively by including a trend in date of birth. Although the latter solution allows exploiting a larger sample by including observations that are farther from the cutoff date, this strategy would capture the effect of entrance age right at the discontinuity only if the trend in date of birth is correctly specified.

The second assumption requires that children's date of birth should be random throughout the year or at least throughout the narrow interval used by the researcher around the entrance cutoff date. However, there is growing evidence that this is not usually the case. For example, Bound and Jaeger (2000) provide extensive evidence that season of birth is correlated with family background, education and earning. In a recent paper, Buckles and Hungerman (2013) document, for example, that women giving birth in the winter are younger, less educated, and less likely to be married. Moreover, the identifying assumption that birth dates are random may fail to hold even within a narrow interval around the entrance cutoff point. To illustrate, 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 to 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.[4] Also, 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. Thus, since dates of

[4] Another reason that the date of birth is endogenous even within a narrow interval around the entrance cutoff is that parents may purposely schedule births after cutoffs if they want their children to enter school when they are older and before cutoffs if they want them to enter when they are younger. A recent article in the New Yorker illustrates this possibility http://www.newyorker.com/tech/elements/youngest-kid-smartest-kid (last accessed 22.5.16). It talks about 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. She says that she was "determined to keep him in until after January first" because she wanted the child to be the oldest in his class and not the youngest.

birth are not randomly assigned, it is necessary to isolate the effect of entrance age from date of birth effects. In fact, Cascio and Lewis (2006) provide evidence that season of birth has a direct effect on test scores and thus argue that controlling for detailed date of birth effects in a model of test scores might not only be appropriate but also important for drawing conclusions.

To address these issues, in this paper, using data on Israel, we estimate the casual effect of entrance age on student outcomes by exploiting a novel identification strategy that, unlike much of the recent literature, strictly satisfies the monotonicity assumption and also cleanly disentangles the effect of entrance age from date of birth effects.[5] We derive our strategy from the fact that while the school entry law in Israel determines a fixed cutoff date every year according to the Jewish calendar, since 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. 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 reasons unrelated to their educational strength. Rather, 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 next year. Thus, 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 the 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

[5] We discuss this issue in length in the empirical section.

fixed effects in several of our estimations. Our strategy is valid under the assumption that parents do not precisely time births around the Jewish entrance cutoffs. In Sections 2 and 3 we provide strong evidence to support this assumption. Importantly, we show that because our unique identification strategy uses variation in cutoff dates across years while holding the date of birth constant, the monotonicity assumption is not violated. Moreover, our strategy is even preferable to those suggested by Fiorini and Stevens (2014) for solving the monotonicity violations. First, it leaves us with a much larger sample that includes year-round birthdates. Second, it allows controlling for date of birth non-parametrically and thus does not rely on the correctness of the functional form as when using a trend in date of birth.

Our difference in difference results show that delaying school entry by one year increases fifth grade test scores in Hebrew by about 0.34 standard deviations (SD) and in math by about 0.19 SD. In terms of persistency of effects, the advantage in Hebrew test scores decreases in eighth grade to 0.25 SD, and in math it substantially increases to 0.34 SD. These findings are robust to different specifications and identification strategies. Furthermore, according to our regression discontinuity design estimates (RDD), the effect of delaying school entry is quite persistent not only for English but also for Hebrew test scores. Delaying school entry by one year increases Hebrew fifth grade test scores by 0.27 SD and eighth grade test scores by 0.25 SD. In math, it increases fifth grade test scores by 0.21 SD and eighth grade test scores by 0.29 SD. 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 to tracks at an earlier age (Muhlenweg and Puhani 2010, Fredrickson and Ockert 2014). Our results also show that adding date of birth fixed effects to the estimation is important not only for satisfying the monotonicity assumption but also for almost completely eliminating the quite substantial amount of selection that existed before adding the fixed effects. Furthermore, both our DID and RDD estimates indicate that failing to control for date of birth effects could have led us to erroneously conclude that the SEA effect on math test scores decreases slightly from fifth grade to eight grade while it actually substantially increases.

Our results also shed light on the specific mechanism by which school entrance age affects test scores. In all samples of children in the same grade, including ours, school entry age and testing age are almost perfectly correlated. Thus, as children who enter school later are also older when they take tests, typical estimates of entrance age effects were only able to capture the combined effect of school entrance age and 'age at test.' Black et. al (2011) is the only exception in that they did succeed in isolating the effect of entrance age from the effect of 'age at test'. Nevertheless, as argued by McEwan and Shapiro (2008), the effect of 'age at test' plausibly dissipates over time, because a year of maturation represents more learning among younger children compared to older children. Thus, both their and our findings that the combined effect is stable (if not growing over time) provide evidence that 'age at test' cannot fully explain the higher test scores of children who entered school older.

Interestingly, we also find that although girls have a much lower tendency to have 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 girls than for boys. In addition, we show that while the effect of entrance age on fifth grade test scores is not significantly different among children whose parents belong to different education quartiles, the effect on eighth grade test scores is substantially weaker among children of more educated parents.

The rest of the paper is organized as follows. Section 2 provides a background on the relationship between the Jewish and Gregorian calendars. Section 3 describes the data and the empirical strategy. In section 4 we present the results. Section 5 concludes with a brief summary and policy implications.

2. Background: The Jewish and Gregorian calendars

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, as required by the Jewish law, the religious holidays will occur during the same season every year, 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).

Up until 2015, the school entry cutoff date in Israel was always set to be on the same Jewish calendar date – the first day of the fourth Jewish month of "Tevet." This implies that in different years this same Jewish calendar cutoff date was mechanically converted into different cutoff dates according to the Georgian calendar which range from December 3rd to December 28th.

In the last decades, largely due to globalization, the use of the Jewish calendar has steadily declined in favor of the internationally accepted Gregorian calendar and today its use for civil purposes is quite negligible. This feature is important for our identification strategy as it implies that people are much less likely to manipulate their Hebrew date of birth relative to the Gregorian one. To demonstrate that most people in Israel don't refer to the Jewish calendar in their everyday life, we ran two surveys: one, which used the services of Sekernet (a marketing survey organization), was conducted among a representative sample of 200 parents with children already in school, and a second which was conducted among 159 Economics students at Ben-Gurion University. The following results show that this is indeed the case. 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 set meetings only according to the Jewish calendar relative to 85% who set 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 31st, thinking the school entrance cutoff date would naturally be at the end of the year), and 34% reported 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, and rarely use the Jewish calendar in their daily lives. 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 manipulate their Jewish date of birth in order to influence the school entrance age of their children. In the empirical section we provide direct evidence on this issue.

3. Data and empirical strategy

3.1 Data

Our data contains administrative records collected by the Israel Ministry of Education on fifth and eighth grade students for the years 2002-2006. Each record contains information on the student's exact date of birth, 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" in Hebrew). These data were linked to test scores in the GEMS (Growth and Effectiveness Measures for Schools - Meitzav in Hebrew). The GEMS is an Israeli nationwide exam conducted once a year for fifth and eighth grade pupils. It includes a series of tests administered by the Division of Evaluation and Measurement of the Ministry of Education. 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) to a mean of zero and a standard deviation of 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. It is noticeably problematic to include students born in June in the analysis because it is not clear whether they should be considered as being born after the entrance cutoff of one year or before the entrance cutoff of a consecutive year. For this reason, we omitted all the students born in June and set each period to run from July 1st prior to the entrance cutoff through May 31st after the entrance cutoff.[6]

[6] Since the cutoff points are spread over the entire month of December, if we included June students in the analysis and thus let each period to run from July 1st to June 30th we could obtain cases in which a specific date in June would belong to one period while it is actually closer to the entrance cutoff date of another period. For example, a child born on June 30th 1989 would belong to period 1 although the distance between this date and the cutoff of period 1 (204 days) is larger than the distance of this date from the cutoff of period 2 (181 days).

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, we lack information on test scores of eighth grade students born after the cutoff of 1992 because they were tested in 2007 (our dataset runs from 2002 to 2006). In each analysis we use only periods for which we have information on students located at both sides of the entrance cutoff. Thus, when estimating the effect of entrance age on test scores in fifth grade, we use only the 1991-1994 cutoffs (periods 4-7) and omit those children belonging to period 3 who were born after the 1990 cutoff and were tested in 2002. Similarly, 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 cutoff of 1992 and tested in 2006. Overall, we have information on 113,523 fifth grade students and 113,319 eighth grade students. When we use a regression discontinuity approach we focus our analysis on a narrow interval of 30 days before and after each cutoff date, which leaves us with a working sample of 21,231 fifth grade students and 20,914 eighth grade students.

3.2 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 it is likely that a non-random sample of children will be enrolled in school at an earlier age by their parents. The non-randomness of the school entrance age stems from two reasons. First, the decision whether to delay school entry is endogenously determined based on the characteristics of the child as well as those of the parents. 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 the cutoff. Second, variation in entrance age also stems from variation in the date of birth throughout the calendar year, and since the choice of day of birth is commonly correlated with unobserved characteristics of the parents (Dickert-Conlin and Chandra 1999, Bound and Jaeger 2000, McEwan and Shapiro 2008, Buckles and Hungerman 2013) school entrance age may be endogenous as well.

Using our dataset, we provide four indications that a child's date of birth is endogenous. First, Figure 2 shows that the number of births on January 1 is more than twice the number of births of December 31st, despite the fact that in none of the years in our dataset was January 1st the school entrance cutoff date. Thus, the large number of births on January 1st is either because parents mistakenly thought that this is the school entrance cutoff date or because they wanted to give birth on a "catchy" date. In countries where the entrance cutoff date is January 1st (like the US, France and Norway), such attempts of parents to target their date of birth to this date will invalidate any regression discontinuity design. Second, the number of births in each of the months between July and September is much larger than between January and May (see Figure 3 which depicts the distribution of births throughout the year). Also, the number of births per day in middle June is extremely small. Third, Figure 4 shows that, probably due to scheduled births, the number of births on each of the weekend days is much smaller 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 birthdate distribution decline during weekends. Fourth, in order to provide direct evidence that parents' choice of date of birth is correlated with their characteristics, we estimated several major 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 3 (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 April are about 0.2 years more educated relative to those of children born in January. Similarly, the number of siblings of children born between February and August is between 0.04 to 0.07 smaller relative to children born in January. Substantial differences in other characteristics are revealed as well. In addition, Table 4 reports substantial differences between parents of children born on different days of the week. For example, fathers of Friday births are 0.05 years more educated relative to fathers of Sunday births. Thus, if enrollment cutoffs coincidentally fall near specific days of the week, it will 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 4 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 in the estimation may potentially lead to biased estimates. Taken together, we provide strong evidence that a child's date of birth is an endogenous variable.

The "ideal" experiment to deal with the endogeneity problem is one in which the entrance age of different children can be randomized, an option which is obviously unfeasible. As an alternative, we use an exogenous source of variation in entrance age that is derived from exogenous variation in school entrance cutoff dates across different periods. Such exogenous variation exists only because in different periods the same Jewish cutoff date is mechanically converted into different Gregorian cutoff dates which range from December 3rd to December 28th. As a result, children born on the same date of the year, the same day of the week 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, some of them are located before the relevant cutoff and are allowed to enter school in the current year, while others are located after the relevant cutoff and thus have to wait until the next year. To illustrate, Figure 5 depicts the predicted entrance age according to the school entrance law in the different periods.[7] We can see that children who were born on the same date of the year between December 3rd and December 28th are expected to enter school at different ages according to whether they are located before or after the entrance cutoff. On the other hand, all children born before December 3rd or after December 28th have the same predicted entrance age because their date of birth is located on the same side of the cutoff point regardless of the period to which they belong.

As children born on the same date of the year in different periods enter school at a different age, if all children perfectly "comply" with the entrance cutoff date, the empirical set up for estimating the causal effect of entrance age would naturally be analyzed using a DID approach in which the outcome is compared across periods and between children who were born on different dates of the year. This estimation takes the following form:

$$Y_{idp} = \alpha_0 + \alpha_1 \cdot After_{dp} + \beta \cdot X_{idp} + \varphi_d + \tau_p + \varepsilon_{idp} , \qquad (1)$$

where Y_{ipd} is the test scores of child *i* born on date d of period *p*; $After_{dp}$ is an indicator variable for whether date d of period p is located after the entrance cutoff; X_{idp} is a set of characteristics of the child and his family (gender, father's years of schooling, mother's years

[7] 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 schooling, number of siblings, indicator 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, and indicators for each of the parents' continent of birth); φ_d is a set of date of the year fixed effects; and τ_p is a set of period fixed effects.

However, as there is evidence that mothers who give birth on different days of the week may be different in their unobserved characteristics, we need to extend equation (1) to include day-of-week fixed effects. This equation takes the following form:

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

where σ_w is a set of day-of-week fixed effects.

Under perfect compliance, the parameter α_1 would reflect the causal effect of entrance age, where the identifying assumptions are that the trend in normalized test scores over the periods is date-invariant and that parents do not manipulate the Hebrew date of birth so as to be located before or after the cutoff point. An additional identifying assumption is that the relationship between the instrument and the actual school entrance age is monotonic. However, our data indicate that compliance with the entrance cutoff date is only partial and therefore employing a simple DID specification while ignoring imperfect compliance would result in a downward biased estimate of the entrance age effect. Figure 6, which presents the predicted entrance age and the actual entrance age as a function of age relative to the cutoff date, shows that while compliance is indeed imperfect, there is still a sharp discontinuity of the actual entrance age at the cutoff point. It also shows that parents of children born after the cutoff point are much more likely to comply with the treatment assignment rule relative to children born before the cutoff point. This indicates that compliance is imperfect mainly because parents of children born before the cutoff point choose to voluntarily postpone the entrance of their children to school. We can also see that this practice is more frequent among parents of children born closer to the cutoff point.

To deal with the imperfect compliance, we use the exogenous assignment to the treatment by the entrance cutoff date as an instrumental variable for the actual treatment (i.e., entrance age). Specifically, the indicator for whether date d of period p is after the entrance cutoff date, $After_{dp}$, serves as our instrumental variable for the actual entrance age. This instrument is not weak because even with imperfect compliance there is still a sharp discontinuity of the actual entrance age at the cutoff point (see Figure 6). This approach allows us to estimate the average treatment effect of entrance age on test scores among the two kinds of compliers:

(i) Children who entered school at a relatively young age but who would not have done so had they been born after the cutoff date.

(ii) Children who entered school at a relatively older age but who would not have done so had they been born before the cutoff date.

Formally, our estimation takes the following form:

$$Y_{idwp} = \alpha_0 + \alpha_1 \cdot SEA_{idwp} + \beta \cdot X_{idwp} + \varphi_d + \tau_p + \sigma_w + \varepsilon_{idwp} , \qquad (3)$$

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.

In order to examine whether the entrance age effect is stable over different periods, we estimate an additional specification that allows the treatment effect of entrance age to differ in various periods. This specification takes the following form:

$$Y_{idwp} = \alpha_0 + \sum_{j=1}^{k} \alpha_j \cdot SEA_{idwp} \cdot P_j + \beta \cdot X_{idwp} + \varphi_d + \tau_p + \sigma_w + \varepsilon_{idwp} , \qquad (4)$$

where P_j is an indicator for whether the child's birth date is located in period j. The interaction terms between $After_{dp}$ and P_j serve as instrumental variables for the endogenous interaction terms between SEA_{idwp} and P_j. In this specification, the coefficients α_j reflect the causal effect of entrance age in period j.

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 account for heterogeneity in the entrance age effect:

$$Y_{idwp} = \alpha_0 + \alpha_1 \cdot SEA_{idwp} + \alpha_2 \cdot Z_{idwp} + \ \alpha_3 \cdot Z_{idwp} \cdot SEA_{idwp} \cdot \ \beta \cdot X_{idwp} + \varphi_d + \tau_p + \sigma_w + \varepsilon_{idp} \qquad (5)$$

In this equation, the variable Z_{idwp} can be either gender or the quartile of the parents' education. The indicator for being born after the cutoff date and its interaction with Z_{idwp} serve as instruments for the two endogenous variables (*SEA* and its interaction term with Z_{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 thirty days around each entrance cutoff date. 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.

3.3 Validity of the instrument

3.31 Excludability

Although our estimation controls for date of birth effects, a threat to the validity of our identification strategy still exists if parents systematically time their Hebrew birth date with respect to the entrance cutoff date. As we already discussed in Section 2, this behavior is very unlikely to occur because Israeli parents do not generally use this calendar in their everyday life and are not even aware that the entrance cutoff date is set according to the Jewish calendar. In order to provide direct evidence on this issue, we perform a density test and show in Figure 7 that there is no suspect jump in the number of births around the discontinuity.

3.32 Monotonicity

In discussing whether monotonicity holds in our setting it is crucial to take into account that our identification strategy includes fixed effects for each date of the year, which implies that the counterfactual for a child born on a given date in December and located before the entrance cutoff is a child born on the same date but located after the entrance cutoff. Thus, we next consider two children born on the same date but who are located on two different sides of the entrance cutoff. To illustrate, for children born on December 8th we follow Fiorini and Stevens (2014) and distinguish between four types of children based on actual and counterfactual entrance age behavior: compliers, never takers, always takers and defiers (see Table 5). The number on the left in each cell represents the entrance age of children located before the cutoff and on the right, the entrance age of children located after the entrance cutoff. The table presents two possible options for each child. Children born before the cutoff can decide to comply and enter school on time or not to comply and enter school late. Similarly, children born after the cutoff can decide to comply and enter school on time or not to comply and enter school early. As it is very unlikely that a child located before the cutoff who entered school late would instead enter school early had he been born after the cutoff point, we ignore this possibility of defiers and remain with only three types of children: compliers, always takers and never takers. We show that monotonicity holds for each of them.

Type A represents the compliers: children located before the entrance cutoff who enter school on time and who would also enter school on time had they been located after the entrance cutoff. We can see that the entrance age of the children located after the entrance cutoff is higher by exactly one year. Type B represents children located before the cutoff who enter school on time, but who would enter school early had they been located after the cutoff point. These children are the "never takers" as they always decide to enter school when they are young. The table shows that they have exactly the same actual entrance age (5.7 years). Similarly, type C represents children born before the entrance cutoff who enter school late, but who would enter school on time had they been located after the entrance cutoff. These children are the "always takers" as they always enter school when they are older. Again, these two counterfactuals have the same entrance age (6.7 years). In summary, in all cases, the actual entrance age is either larger or the same if children are located after the entrance cutoff relative to if they are located before it.

In stark contrast, as shown by Fiorini and Stevens (2014), monotonicity would fail to hold if one uses the common regression discontinuity design that compares children located on the two sides of the entrance cutoff unconditional on date-of-birth fixed effects. For example, if the entrance cutoff is January 1st and one compares children located on different sides of this cutoff, type B and Type C children would have a lower entrance age (by about one month) if they were located after the entrance cutoff relative to if they were located before it, thus violating the monotonicity assumption.

It is noteworthy that in our identification strategy the monotonicity assumption holds even if we use data on the entire year and not only in a narrow interval around the entrance cutoffs. This is important not only for internal validity but also for external validity as children born at different points in the year are slightly different and effects of starting age may vary. Although we can never identify the average treatment effect (ATE), the full sample estimates are clearly much more representative relative to estimates based on a discontinuity sample.

4. Results

4.1 Selection

We begin by providing evidence that the instrument (*After*), which indicates whether the date of birth is located before or after the cutoff point, is orthogonal to student characteristics only when conditioning on exact date of birth. To show that the amount of selection is non-negligible when failing to control for date of birth fixed effects, we estimated a set of univariate regressions in which the dependent variable is each of the background characteristics listed in the first column of Table 6 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, 14 student characteristics were found to be significantly correlated with the instrument at the 5% level and two additional variables at the 10% level. Similarly, among eighth graders, 16 variables were significant at the 5% level and an additional five variables at the 10% level.

Several previous studies that used data on the entire year included month fixed effects in their set of controls in order to reduce the concern of selection. In Column 1 we assess the amount of selection in such a specification by reporting the coefficient on *After* in models that also include month fixed effects. The results show that although the amount of selection decreases substantially, it is still non-negligible. For example, among fifth graders, nine student characteristics were found to be significantly correlated with the instrument. Similarly, Column 5 shows that four characteristics were found to be significant among eighth graders. In order to assess the importance of controlling for date of birth fixed effects, we replace the month fixed effects with date of birth fixed effect. The results show that now only one covariate is significant among fifth graders and no covariates are significant among eighth graders (Columns 2 and 6).

Previous studies in this literature also 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 concentrate on a discontinuity sample of 30 days on either side of the cutoff point, and report the slope from univariate regressions of each of the background characteristics on the instrument, for fifth graders and eighth graders, respectively (Columns 3 and 7). The results indicate that the amount of selection is still far from negligible. Nine covariates are significant among fifth graders and ten covariates among eighth graders. In comparison, when adding a set of date of birth fixed effects to the estimation, none of the covariates are significant among eighth graders and only one is significant among fifth graders (Columns 4 and 8). Taken together, we can conclude that including the date of birth fixed effects in the estimation substantially decreases the amount of selection.

4.2 The reduced form relationship between entrance age and student outcomes

Figure 6 shows a sharp discontinuity in the actual entrance age at the entrance cutoff, which is mirrored by a discontinuity in student test scores. Figures 8 and 9 indicate that there is a sizeable discontinuity at the cutoff date in normalized fifth grade test scores in both Hebrew and math. It is important to note that this graph 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 (Figures 10 and 11). However, estimating the effect of entrance age by comparing the outcomes of students located before and after the cutoff point, as commonly done in previous studies, 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 use a unique identification strategy that estimates the causal impact of entrance age on test scores by comparing changes over different periods between children born on different dates. In other words, it compares 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 12 and 13, for each date between December 3rd and December 28th, we compare the normalized test scores in math and Hebrew of children located before and after 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 8-11, these graphs ignore imperfect compliance and thus bias the entrance age effect towards zero.

Columns 1 and 2 of Table 7 present the results from estimating equation (2) for Hebrew and math, respectively. The results indicate that being born after the cutoff point significantly increases fifth grade test scores in Hebrew by 0.10 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 3.11 percentile points and in math by 1.90 percentile points. Again, while in Hebrew the effect on eighth grade test scores decreases to 1.77 percentile points, in math it increases to 2.64 percentile points.

In order to test our identifying assumption that the trend in test scores over the periods is date-invariant we next conduct two placebo tests. In the first placebo test, 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 8th we instead use a placebo cutoff for March 8th. 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 7 indicate that the impact of being born after the placebo cutoffs is not significant in any of the estimations.

4.3 The effect of entrance age on test scores

Recall that 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 effect. To address this issue, we estimate equation (3), and report the estimates of the entrance age effect in Table 8. 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 0.34 SD and in math by 0.19 SD. Interestingly, in eighth grade the effect in Hebrew decreases to 0.25 SD, while in math it almost doubles to 0.34 SD. The first-stage results of each of the specifications in Table 8 and 9 are reported in Table A1 in the appendix. It is clear that the instrument is very strong as the F-statistics on the excluded instrument consistently 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 0.24-0.37 years in the different specifications.

As we showed in section 3.2 that the instrument is correlated with all the day-of-week fixed effects, 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 despite the potential concern the estimates remain almost unchanged.

To show that our estimates are not sensitive to the set of controls included in the estimation, 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, presented in Column 4 of Table 8, show that the entrance age estimates are almost unchanged, the largest change being merely a small decrease of the entrance age effect from 0.34 to 0.3 SD.

In order to highlight the importance of disentangling the effect of entrance age from date of birth effects, we now re-estimate our basic specification but now omit all the date-of-year and day-of-week fixed effects (the difference between this column and column 3 is only the addition of date of birth fixed effects). This specification, however, 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 8, 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 birth fixed effects. However, for math fifth grade test scores the entrance age effect increases from 0.19 to 0.36 SD when omitting these fixed effects. 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.

We also test the sensitivity of our estimates to using a regression discontinuity design. In this analysis, we estimate exactly the same equation (3) but now focus on a narrow interval of 30 days around each of the cutoff points. The results, presented in Column 6, show that entering school a year later increases fifth grade test scores in Hebrew by 0.27 SD and in math by 0.21 SD. In eighth grade the effect on Hebrew test scores slightly decreases to 0.25 SD while in math it increases to 0.29 SD. Thus, our regression discontinuity estimates agree with the DID estimates that the entrance age effect on math test scores increases from fifth grade to eighth grade and that the estimates on Hebrew test scores decrease slightly.

Previous studies could not include date-of-year fixed effects in their RD estimations either because they didn't have multiple cutoff points or because they didn't observe the child's exact date of birth. As an alternative, a minority of them, which 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. Estimations that did not account for any trend may yield biased estimates not only because they do not control for date-of-birth effects, but also because they violate the monotonicity assumption (Fiorini and Stevens, 2014). To assess the size of bias induced by omitting date of birth fixed effects, we estimate two additional specifications. In Column 7 we omit the date-of-year fixed effects but include instead a quadratic trend of the running variable, while in Column 8 we do not include any trend. The results show that the entrance age effect on eighth grade test scores in Hebrew dropped from 0.25 SD to 0.14 SD and became insignificant when a quadratic trend is included instead of the date of birth fixed effects. In addition, Columns 7 and 8 show that the entrance age effect on math test scores slightly decreases from fifth grade to eight grade while our preferred specification show that it substantially increases from 0.21 SD to 0.29 SD. Furthermore, when we compare the results in Columns 7 and 8 to our preferred estimates in Column 2 we find that the biases are even more substantial. The entrance age effect on eighth grade test scores in math dropped from 0.34 SD to 0.24 SD in the specification with a quadratic trend and to 0.25 SD in the specification with no trend. Similarly, the effect on fifth grade math test scores increased from 0.19 SD to 0.26 SD when either a quadratic trend of the running variable or no trend is included instead of the date of birth fixed effects (Column 7 and 8). Thus, while Column 2 indicates that the entrance age effect almost doubles from fifth grade to eighth grade, Columns 7 and 8 show that it slightly decreases.

Table 9 presents results from the same specifications as in Table 8 but the dependent variable is measured in terms of percentile scores. We can see that the entrance age effect on fifth grade test scores in Hebrew is 10.9 percentile points and in math about 6.8 percentile points. In eighth grade, the effect on Hebrew test scores slightly decreases to 7.19 percentile points while in math it increases substantially to 11.1 percentile points. The RD estimates are similar.

In order to examine whether the entrance age effect is stable over different periods, we estimate equations (4). The results indicate that the entrance age effect is quite stable over the different periods. For example, the effect on math test scores in fifth grade varies narrowly between 0.19-0.24 SD and in eighth grade between 0.29-0.37 SD. Similarly, the effect on Hebrew test scores in fifth grade varies between 0.29-0.40 SD and in eighth grade between 0.17-0.26 SD. Most of the coefficients are significant at the 5% level and the others are close to significant.[8]

4.4 Effect Heterogeneity

The literature shows that redshirting 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

[8] For space limitations we do not present these results here. However, they are available from the authors upon request.

two hemispheres of the brain.[9] To examine whether the effect of school entrance age is different among boys and girls, we estimate the following specification:

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

where Z_{idwp} indicates gender. Here we have two endogenous variables: the school entrance age and its interaction with gender. Thus, in order to estimate equation (6) we need to have two instruments. We use the indicator for being born after the cutoff date and its interaction with gender as our instruments. We estimate this equation both on the entire sample and also only within a narrow interval of +/- 30 days around the entrance cutoffs. The results, presented in Panel A of Table 10, do not provide strong evidence that the effect of entrance age among boys is significantly larger than among girls. Although the interaction term is positive in seven out of eight estimations, it is significant only in one of them and only at the 10% significance level. This result implies that early entrance to school may be just as precarious for girls as for boys.

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 (6), but now Z_{idwp} denotes the education quartile of the parent. The results, presented in Panel B of Table 10, show that while the effect of entrance age on fifth grade test scores in both Hebrew and math is not significantly different among parents that belong to different education quartiles, in eighth grade the effect is weaker among more educated parents. The fact that parental education has a differential effect in eighth grade but not in fifth grade could be a result of the effectiveness of assistance from parents with different educational levels. While all our parents have similar mastery of a fifth-grade curriculum, by the eighth grade parents with relatively higher

[9] 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/

education can better assist their children. Therefore, in eighth grade, higher parental education mitigates the SEA effect.

4.5 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 grade and eighth grade is well within the range of earlier studies. For example, our findings that entering school a year later increase fifth grade test scores in Hebrew by 0.34 SD (10.9 percentile points) and in math by 0.19 SD (6.8 percentile points) are similar to those of Elder and Lubotsky (2008) 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. Similarly, Bedard and Dhuey (2006) found that the entrance age effect on fourth grade test scores is between 2-9 percentile points. Finally, McEwan and Shapiro (2008) similarly 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 0.25 SD (7.19 percentile points) and math test scores by 0.34 SD (11.13 percentile points). Our results for Hebrew test scores are very similar to that of Elder and Lubotski (2008) 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 Lubotski (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).

5. Concluding remarks

In this paper we exploit a unique identification strategy that, unlike much of the existing literature, allows us to estimate the causal effect of entrance age on test scores while isolating it from date of birth effects and also strictly satisfying the monotonicity assumption. We show that the induced bias from failing to control for date of birth effects can be quite substantial. We also 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 0.34 SD and in math by 0.19 SD. Moreover, the entrance age effect generally persists into eighth grade and remains substantial. In Hebrew it negligibly decreases to 0.25 SD and in math it increases substantially to 0.34 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 tracks start 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. In addition, 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 on eighth grade test scores is larger for less educated parents. All these findings are highly relevant for both parents and policy makers when deciding on the timing of school entrance.

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

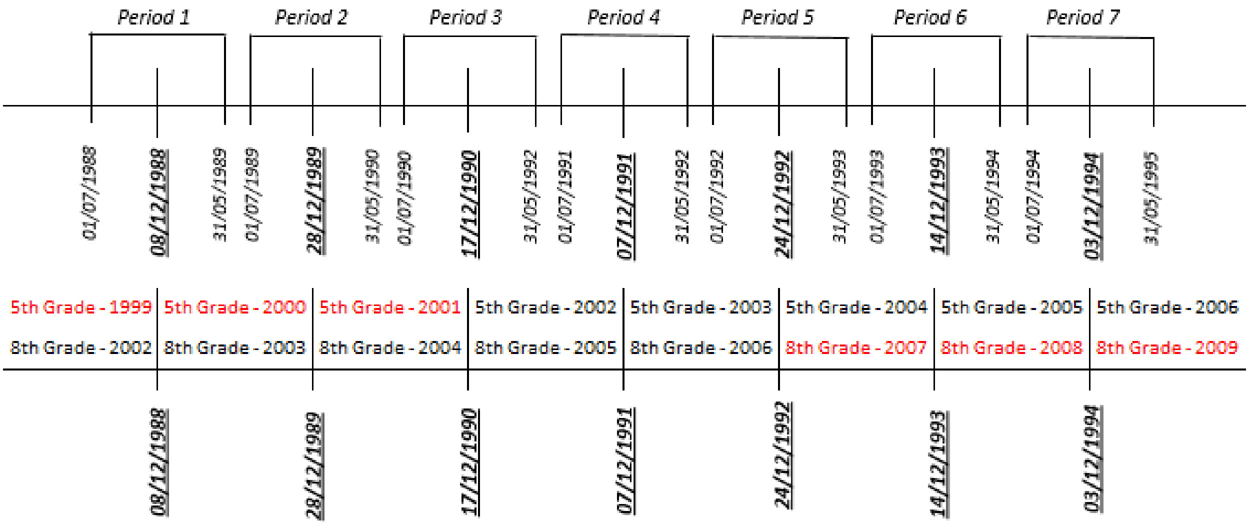

Figure 2: Number of births by day in December and January

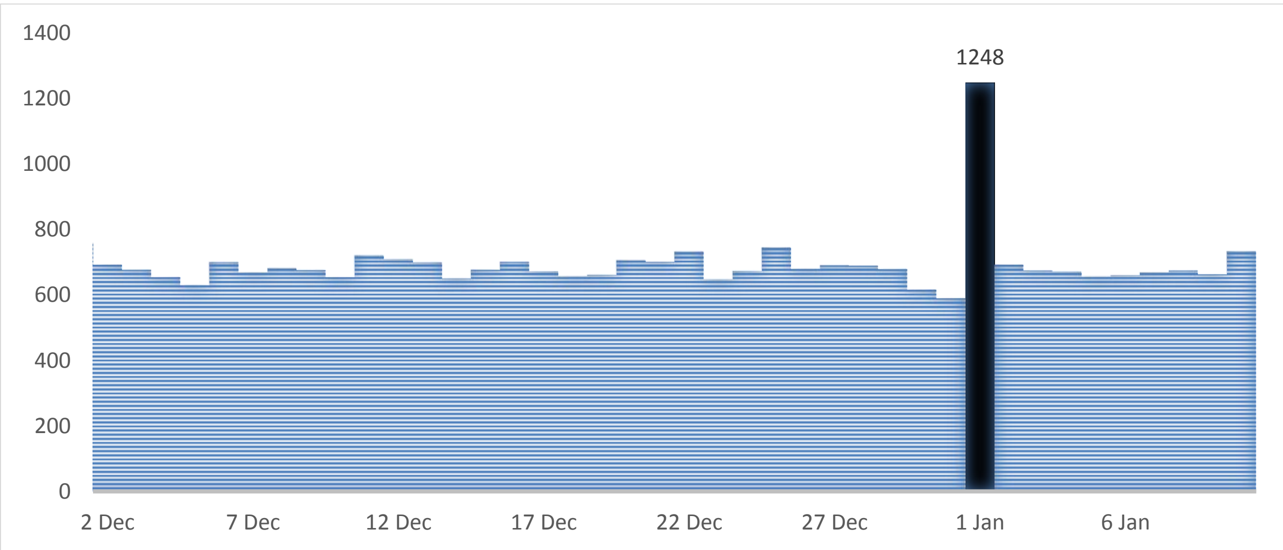

Figure 3. Birth distribution by month

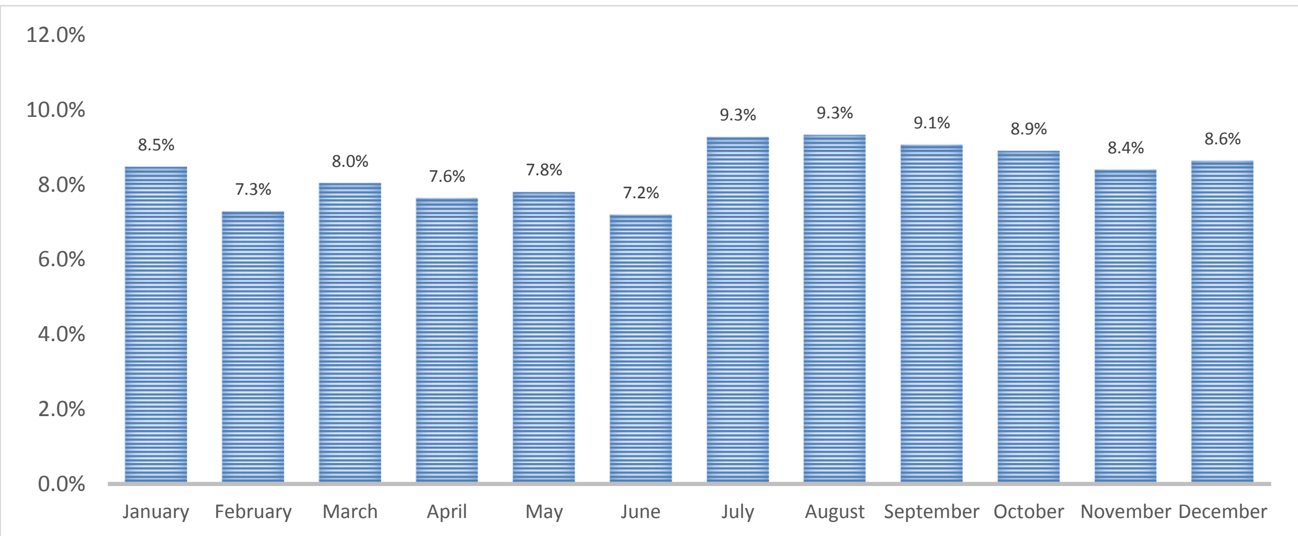

Figure 4. Birth distribution by day-of-week

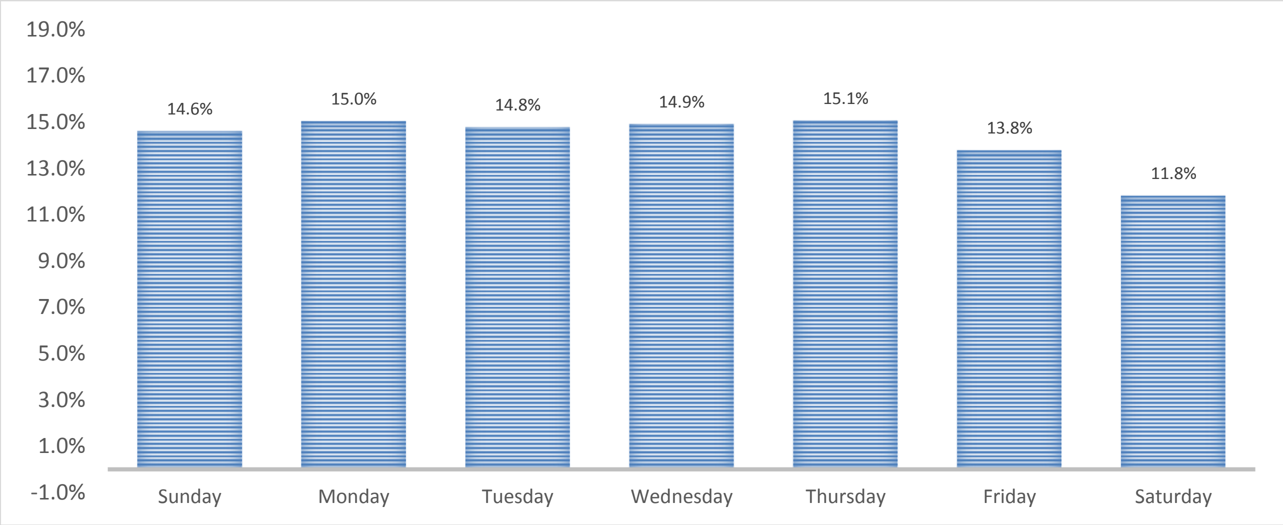

Figure 5. Predicted entrance age by day in December

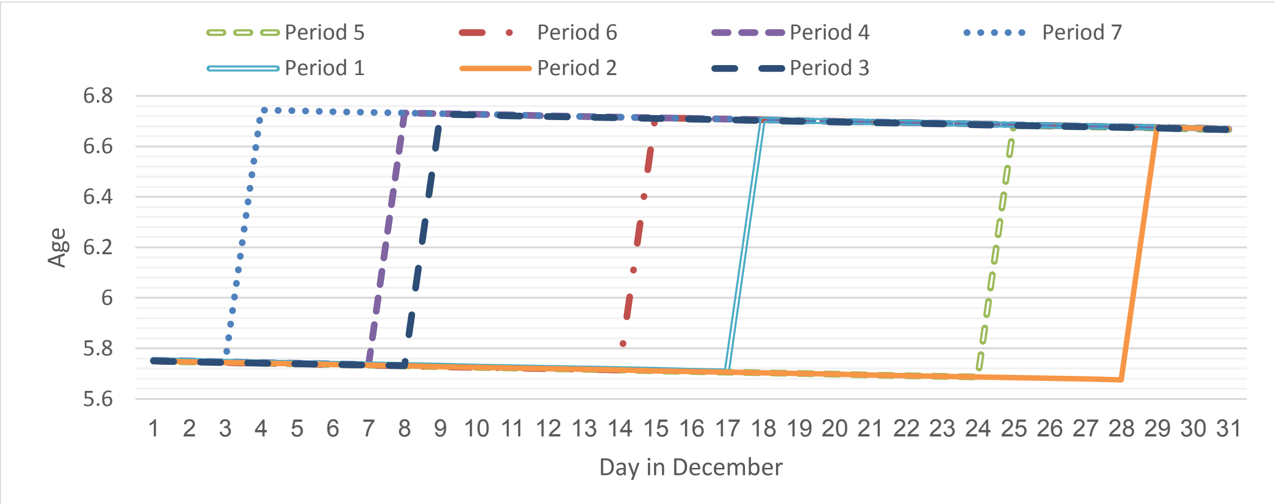

Figure 6. Average predicted and actual entrance ages by age relative to cutoff

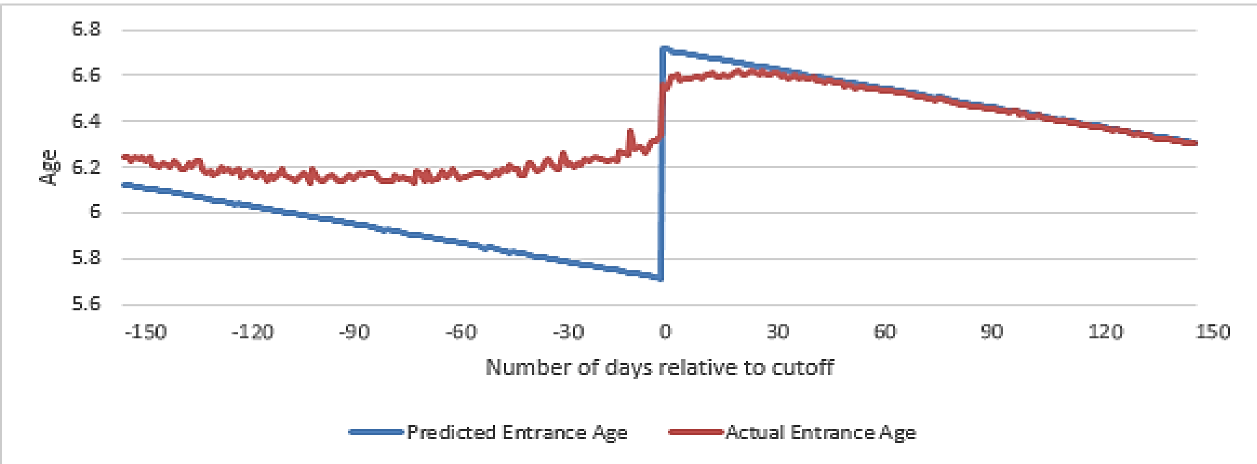

Figure 7. Number of births by age relative to cutoff

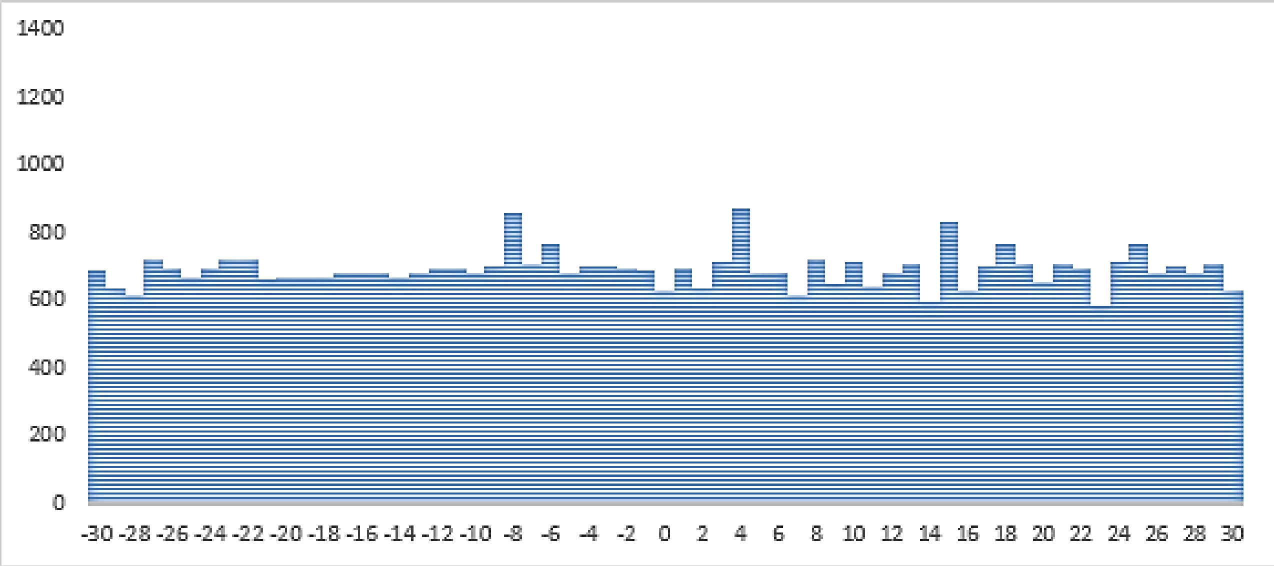

Figure 8. Fifth grade normalized Hebrew test scores by age relative to cutoff

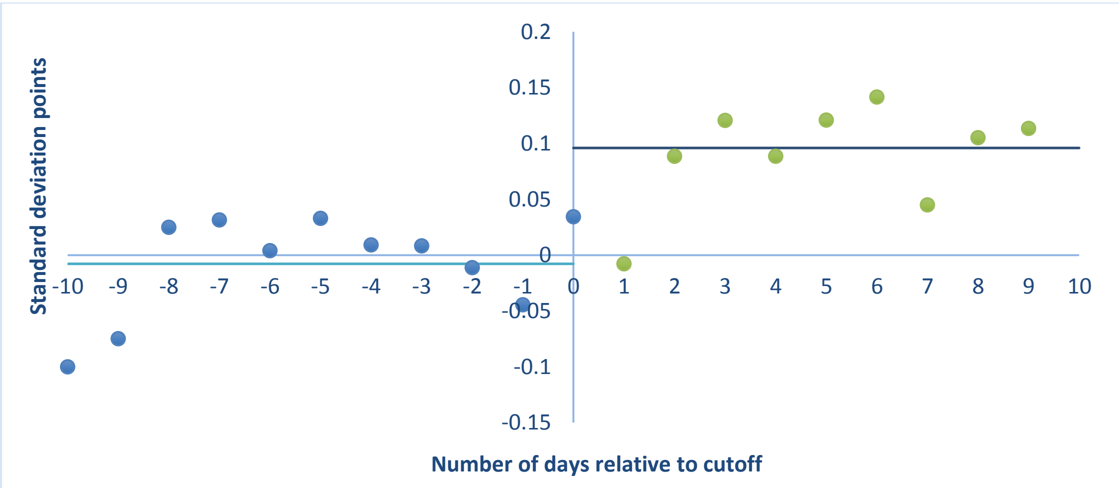

Figure 9. Fifth grade normalized math test scores by age relative to cutoff

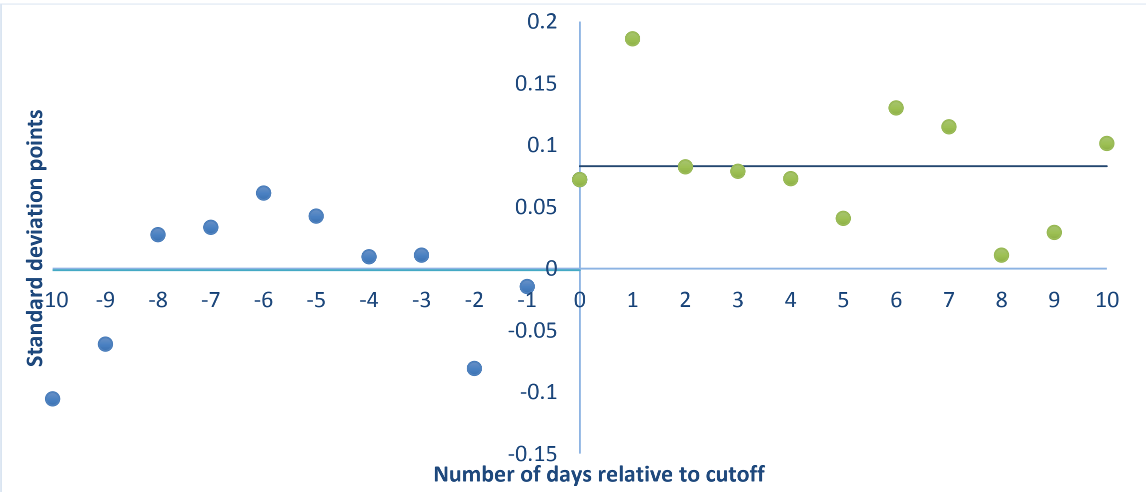

Figure 10. Eighth grade normalized Hebrew test scores by age relative to cutoff

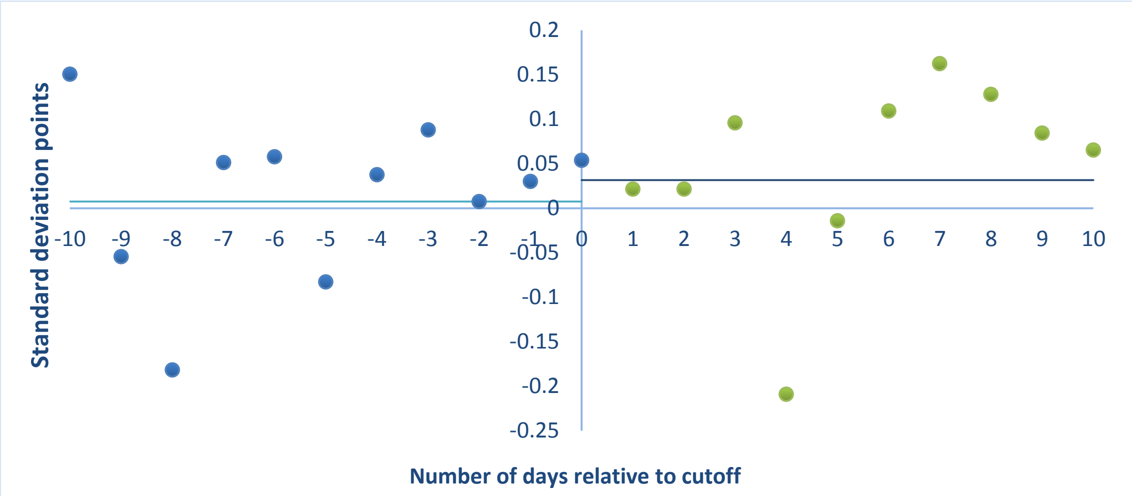

Figure 11. Eight grade normalized math test scores by age relative to cutoff

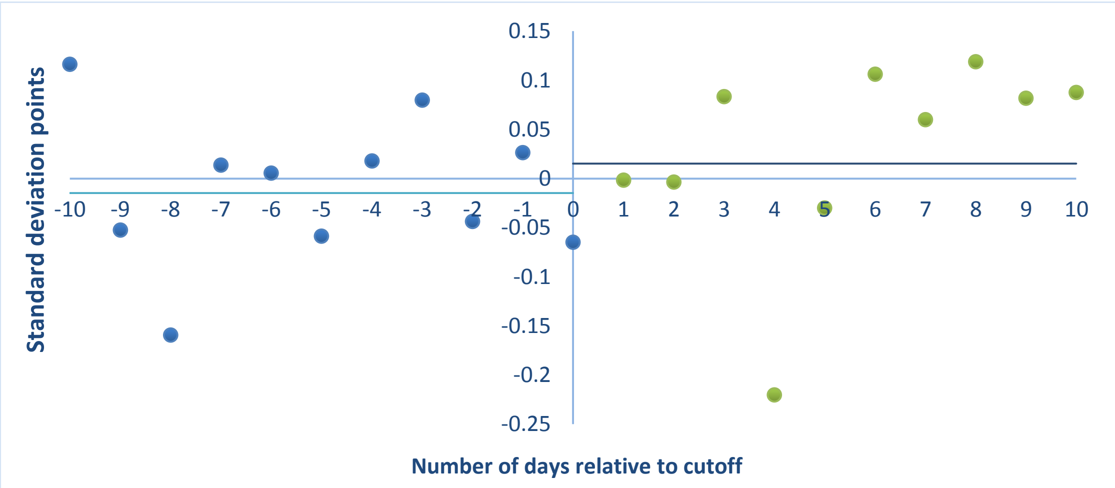

Figure 12. Difference in normalized test scores between children located before and after the cutoff points (5th grade)

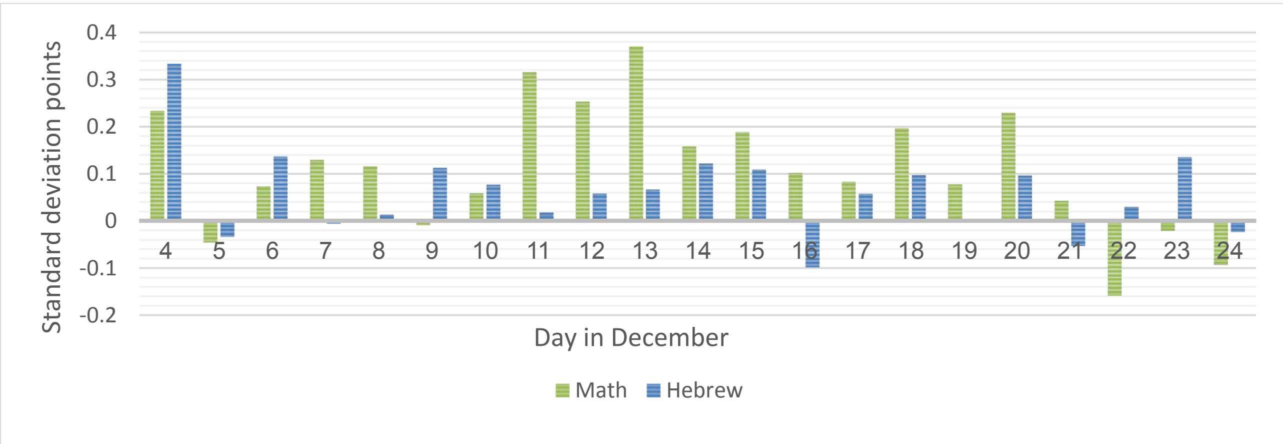

Figure 13. Difference in normalized test scores between children located before and after the cutoff points (8th grade)

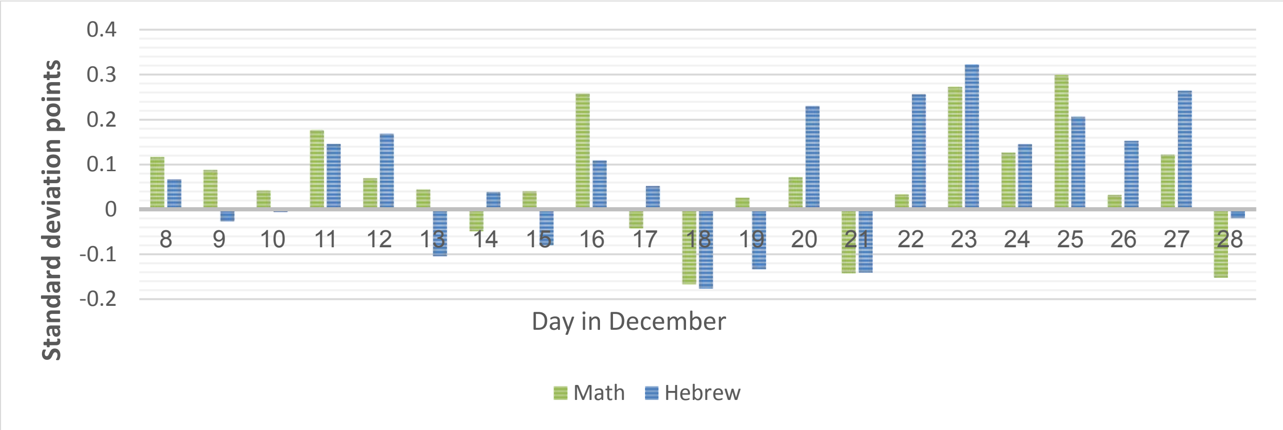

TABLE 1 – SUMMARY STATISTICS (5TH GRADE SAMPLE)

	DID Sample			*RDD RC±30*		
Variables	Obs	Mean	SD	Obs	Mean	SD
Outcome Variables						
Normalized math score in 5th grade	107,662	0.02	0.99	20,146	0.03	0.99
Normalized Hebrew score in 5th grade	106,951	0.03	0.98	19,990	0.04	0.98
Percentile math score in 5th grade	107,662	50.38	28.72	20,146	50.70	28.80
Percentile Hebrew score in 5th grade	106,951	50.59	28.75	19,990	50.99	28.84
Age Variables						
After cutoff	113,523	0.48	0.50	21,231	0.49	0.50
Entrance age	113,523	6.33	0.35	21,231	6.42	0.44
Background Variables						
Father education (1-8 Years)	113,523	0.04	0.19	21,231	0.04	0.19
Father education (9-12 Years)	113,523	0.50	0.50	21,231	0.50	0.50
Father education (13-16 Years)	113,523	0.24	0.43	21,231	0.23	0.42
Father education (17-19 Years)	113,523	0.08	0.26	21,231	0.07	0.26
Father education (20+ Years)	113,523	0.03	0.18	21,231	0.04	0.13
Mother education (1-8 Years)	113,523	0.03	0.17	21,231	0.03	0.17
Mother education (9-12 Years)	113,523	0.49	0.50	21,231	0.50	0.50
Mother education (13-16 Years)	113,523	0.28	0.49	21,231	0.27	0.44
Mother education (17-19 Years)	113,523	0.08	0.27	21,231	0.08	0.28
Mother education (20+ Years)	113,523	0.05	0.22	21,231	0.05	0.22
Number of siblings (0-1)	113,523	0.33	0.47	21,231	0.33	0.47
Number of siblings (2-3)	113,523	0.47	0.50	21,231	0.47	0.50
Number of siblings (4-5)	113,523	0.06	0.24	21,231	0.07	0.25
Number of siblings (6-7)	113,523	0.01	0.11	21,231	0.01	0.11
Number of siblings (8-11)	113,523	0.00	0.05	21,231	0.00	0.05
Number of siblings (12+)	113,523	0.00	0.01	21,231	0.00	0.01
Male	113,523	0.50	0.50	21,231	0.50	0.50
Mamlachti	113,523	0.76	0.43	21,231	0.75	0.43
Father born in Asia or Africa	113,523	0.23	0.42	21,231	0.23	0.42
Father born in Australia	113,523	0.00	0.03	21,231	0.00	0.03
Father born in Europe	113,523	0.05	0.22	21,231	0.05	0.21
Father born in North America	113,523	0.02	0.12	21,231	0.01	0.12
Father born in South America	113,523	0.01	0.12	21,231	0.01	0.12
Father born in Israel	113,523	0.59	0.49	21,231	0.57	0.49
Mother born in Asia or Africa	113,523	0.22	0.41	21,231	0.22	0.41
Mother born in Australia	113,523	0.00	0.03	21,231	0.00	0.03
Mother born in Europe	113,523	0.06	0.23	21,231	0.06	0.23
Mother born in North America	113,523	0.02	0.13	21,231	0.02	0.13
Mother born in South America	113,523	0.02	0.12	21,231	0.02	0.12
Mother born in Israel	113,523	0.62	0.49	21,231	0.60	0.49
Student born in Asia or Africa	113,523	0.05	0.22	21,231	0.05	0.23
Student born in Australia	113,523	0.00	0.02	21,231	0.00	0.02
Student born in Europe	113,523	0.04	0.18	21,231	0.04	0.19
Student born in North America	113,523	0.02	0.12	21,231	0.01	0.12
Student born in South America	113,523	0.00	0.07	21,231	0.01	0.07
Student born in Israel	113,523	0.89	0.31	21,231	0.89	0.31

TABLE 2 – SUMMARY STATISTICS (8^{TH} GRADE SAMPLE)

	DID Sample			*RDD RC±30*		
Variables	Obs	Mean	SD	Obs	Mean	SD
Outcome Variables						
Normalized Math score in 5th grade	103,723	0.03	0.99	19,129	0.01	1.00
Normalized Hebrew score in 5th grade	104,836	0.04	0.97	19,332	0.03	0.98
Percentile Math score in 5th grade	103,723	50.68	28.71	19,129	50.32	28.84
Percentile Hebrew score in 5th grade	104,836	50.97	28.62	19,332	50.71	28.71
Age Variables						
After cutoff	113,319	0.47	0.50	20,914	0.49	0.50
Entrance age	113,319	6.32	0.35	20,914	6.43	0.43
Background Variables						
Father education (1-8 Years)	113,319	0.04	0.20	20,914	0.05	0.21
Father education (9-12 Years)	113,319	0.49	0.50	20,914	0.49	0.50
Father education (13-16 Years)	113,319	0.25	0.43	20,914	0.17	0.37
Father education (17-19 Years)	113,319	0.08	0.30	20,914	0.07	0.26
Father education (20+ Years)	113,319	0.03	0.12	20,914	0.03	0.18
Mother education (1-8 Years)	113,319	0.03	0.18	20,914	0.04	0.19
Mother education (9-12 Years)	113,319	0.49	0.50	20,914	0.49	0.50
Mother education (13-16 Years)	113,319	0.29	0.45	20,914	0.19	0.39
Mother education (17-19 Years)	113,319	0.08	0.27	20,914	0.18	0.38
Mother education (20+ Years)	113,319	0.06	0.24	20,914	0.01	0.10
Number of siblings (0-1)	113,319	0.31	0.46	20,914	0.00	0.02
Number of siblings (2-3)	113,319	0.41	0.49	20,914	0.59	0.49
Number of siblings (4-5)	113,319	0.05	0.22	20,914	0.16	0.37
Number of siblings (6-7)	113,319	0.01	0.10	20,914	0.03	0.17
Number of siblings (8-11)	113,319	0.00	0.05	20,914	0.00	0.05
Number of siblings (12+)	113,319	0.00	0.01	20,914	0.00	0.01
Male	113,319	0.49	0.50	20,914	0.49	0.50
Mamlachti	113,319	0.80	0.40	20,914	0.79	0.41
Father born in Asia or Africa	113,319	0.28	0.45	20,914	0.16	0.37
Father born in Australia	113,319	0.00	0.03	20,914	0.00	0.03
Father born in Europe	113,319	0.06	0.23	20,914	0.06	0.23
Father born in North America	113,319	0.02	0.12	20,914	0.01	0.11
Father born in South America	113,319	0.02	0.12	20,914	0.02	0.13
Father born in Israel	113,319	0.59	0.49	20,914	0.57	0.49
Mother born in Asia or Africa	113,319	0.26	0.44	20,914	0.16	0.37
Mother born in Australia	113,319	0.00	0.03	20,914	0.00	0.03
Mother born in Europe	113,319	0.06	0.24	20,914	0.06	0.23
Mother born in North America	113,319	0.02	0.14	20,914	0.02	0.13
Mother born in South America	113,319	0.02	0.13	20,914	0.02	0.13
Mother born in Israel	113,319	0.63	0.48	20,914	0.62	0.48
Student born in Asia or Africa	113,319	0.10	0.30	20,914	0.09	0.29
Student born in Australia	113,319	0.00	0.02	20,914	0.00	0.02
Student born in Europe	113,319	0.03	0.18	20,914	0.04	0.19
Student born in North America	113,319	0.02	0.13	20,914	0.01	0.12
Student born in South America	113,319	0.01	0.08	20,914	0.01	0.07
Student born in Israel	113,319	0.84	0.37	20,914	0.82	0.38

TABLE 3 – DIFFERENCES IN BACKGROUND CHARACTERISTICS BY MONTH OF BIRTH

Month	Father Education	Mother Education	Number of Siblings	Father Born in Israel	Mother Born in Israel	Student Born in Israel
February	0.04	0.06*	-0.07***	0.02***	0.01	0.02***
	(0.03)	(0.03)	(0.01)	(0.01)	(0.00)	(0.00)
March	0.19***	0.20***	-0.05***	0.03***	0.02***	0.03***
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
April	0.20***	0.27***	-0.06***	0.02***	0.01**	0.01***
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
May	0.25***	0.28***	-0.04***	0.03***	0.02***	0.03***
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
June	0.14***	0.14***	-0.07***	0.01*	0.01	0.01*
	(0.04)	(0.03)	(0.01)	(0.01)	(0.00)	(0.00)
July	0.03	0.05	-0.04***	0.00	0.01	-0.01*
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
August	-0.02	0.01	-0.04***	0.01**	0.01	0.01***
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
September	0.01	0.04	-0.02	0.01***	0.01**	0.01*
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
October	0.01	0.03	-0.03**	0.01*	0.00	0.01***
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
November	0.01	0.04	-0.04***	0.01***	0.01	0.01***
	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)
December	0.04	0.05*	-0.02*	0.00	0.00	0.01***
	(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 4 – DIFFERENCES IN BACKGROUND CHARECTARISTICS BY DAY-OF-WEEK

Weekday	After Cutoff	Father Education	Mother Education	Number of Siblings	Father Born in Israel	Mother Born in Israel	Student Born in Israel
Monday	-0.01***	0.02	0.00	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	0.00	0.00	-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	0.04*	-0.02*	-0.01***	-0.01**	-0.02***
	(0.00)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)
Thursday	-0.01***	0.02	0.01	-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	0.05*	0.03	-0.02*	-0.01***	-0.01***	-0.03***
	(0.00)	(0.03)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)
Saturday	-0.01***	0.00	-0.02	0.00	-0.01***	-0.02***	-0.03***
	(0.00)	(0.03)	(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.

TABLE 5 - ACTUAL AND COUNTERFACTUAL ENTRANCE AGE FOR A CHILD BORN ON DECEMBER 8

	After cutoff	
Before cutoff	Early	On time
On time	Type B = (5.7, 5,7)	Type A = (5.7,6.7)
Late	Unlikely	Type C = (6.7, 6.7)

TABLE 6 – BALANCE TEST ON BACKGROUND CHARACTERISTICS

	5th Grade				*8th Grade*			
	All Year Sample		(±30 days)		*All Year Sample*		(±30 days)	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Father education (1-8 Years)	0.001	0.001	0.004	0.001	-0.004	0.003	**0.006***	0.003
Father education (9-12 Years)	-0.008	-0.011	-0.009	-0.011	-0.005	-0.010	**-0.018****	-0.010
Father education (13-16 Years)	0.011	0.016	-0.001	0.016	-0.009	-0.007	-0.010	-0.007
Father education (17-19 Years)	0.002	0.001	0.003	0.001	0.004	0.001	0.000	0.000
Father education (20+ Years)	-0.002	-0.002	-0.004	-0.002	0.004	0.005	**0.004****	0.005
Mother education (1-8 Years)	-0.002	0.000	0.003	0.000	-0.003	-0.002	**0.005***	-0.002
Mother education (9-12 Years)	0.003	0.001	0.000	0.001	0.000	-0.011	**-0.012***	-0.011
Mother education (13-16 Years)	0.007	0.002	-0.001	0.002	0.000	0.008	-0.002	0.009
Mother education (17-19 Years)	-0.006	-0.002	-0.004	-0.002	0.001	-0.003	-0.010	-0.003
Mother education (20+ Years)	0.001	0.005	-0.001	0.005	-0.001	-0.007	-0.004	-0.008
Number of siblings (0-1)	0.004	0.011	-0.005	0.011	**0.021****	0.017	0.000	0.017
Number of siblings (2-3)	-0.007	-0.002	-0.004	-0.002	**-0.018***	0.001	-0.010	0.001
Number of siblings (4-5)	-0.002	-0.005	0.004	-0.005	**0.008***	-0.001	**0.009****	-0.001
Number of siblings (6-7)	0.000	-0.002	0.001	0.002	-0.002	0.000	-0.001	0.000
Number of siblings (8-11)	**0.002***	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	**-0.017***	-0.016	-0.011	-0.016	0.006	-0.011	0.005	-0.011
Mamlachti	0.008	0.008	-0.004	0.008	0.001	0.003	-0.004	0.003
Father born in Asia-Africa	**-0.018****	0.012	-0.009	0.012	-0.005	-0.006	0.007	-0.006
Father born in Australia	**-0.002****	-0.001	**-0.001****	0.000	0.000	0.000	0.000	0.000
Father born in Europe	-0.004	-0.004	-0.005	-0.004	0.000	-0.002	0.000	-0.002
Father born in North America	-0.003	-0.004	**-0.003***	-0.004	0.002	0.002	**0.004****	0.002
Father born in South America	0.003	0.005	**0.003***	0.004	0.000	-0.001	0.000	-0.001
Father born in Israel	**-0.018***	-0.002	**-0.048****	-0.002	0.001	0.002	**-0.017****	0.002
Mother born in Asia-Africa	**-0.017***	0.004	-0.008	0.003	-0.004	-0.004	0.005	-0.004
Mother born in Australia	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mother born in Europe	-0.007	-0.002	**-0.007****	-0.002	-0.004	-0.002	0.000	-0.002
Mother born in North America	-0.003	-0.005	**-0.004****	-0.005	0.003	0.002	**0.003***	0.002
Mother born in South America	0.003	0.003	0.002	0.003	0.000	0.004	0.000	0.004
Mother born in Israel	**-0.021****	0.004	**-0.047****	0.004	0.001	-0.009	-0.007	-0.009
Student born in Asia-Africa	0.001	-0.001	**0.012****	-0.001	-0.006	-0.004	**0.014****	-0.005
Student born in Australia	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Student born in Europe	**-0.009****	**-0.010****	-0.004	**-0.010****	-0.002	-0.004	-0.003	-0.003
Student born in North America	0.001	0.002	0.000	0.002	-0.003	-0.003	-0.002	-0.003
Student born in South America	**0.003***	0.002	0.001	0.002	-0.001	0.002	0.000	0.001
Student born in Israel	0.006	0.006	**-0.009****	0.006	**0.012***	0.011	-0.009	0.010
Observations	113,523	113,523	21,231	21,231	113,319	113,319	20,914	20,914
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: 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.

TABLE 7 - DIFFERENCE–IN-DIFFERENCE REDUCED FORM ESTIMATES ON TEST SCORES

	Base Results		*Placebo 1*		*Placebo 2*	
	Cutoffs in December		*Cutoffs in March*		*Cutoffs in September*	
	(1)	(2)	(3)	(4)	(5)	(6)
Normalized score in 5th grade	Hebrew	Math	Hebrew	Math	Hebrew	Math
After Cutoff	0.10***	0.05**	-0.03	0.01	0.01	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	106,591	107,662	45,440	45,651	51,350	51,809
Percentile score in 5th grade	Hebrew	Math	Hebrew	Math	Hebrew	Math
After Cutoff	3.11***	1.90***	-0.77	0.64	0.20	-0.30
	(0.75)	(0.74)	(0.77)	(0.82)	(0.74)	(0.78)
Observations	106,591	107,662	45,440	45,651	51,350	51,809
Normalized score in 8th grade	Hebrew	Math	Hebrew	Math	Hebrew	Math
After Cutoff	0.06***	0.08***	0.04	0.00	-0.03	0.00
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Observations	104,836	103,723	44,443	43,999	50,693	50,134
Percentile score in 8th grade	Hebrew	Math	Hebrew	Math	Hebrew	Math
After Cutoff	1.77***	2.64***	1.23	-0.08	-0.45	0.16
	(0.71)	(0.74)	(0.81)	(0.78)	(0.70)	(0.71)
Observations	104,836	103,723	44,443	43,999	50,693	50,134

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

	DID (July-May)					RDD (±30 days)		
	OLS	*IV*	*IV*	*IV*	*IV*	*IV*	*IV*	*IV*
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Normalized Hebrew score in 5th grade								
Entrance Age	-0.07***	0.34***	0.34***	0.36***	0.36***	0.27***	0.32***	0.33***
	(0.01)	(0.09)	(0.09)	(0.10)	(0.02)	(0.10)	(0.09)	(0.04)
F-Statistics on excluded instrument	-	1247.5	1246.8	1195.8	21963.2	528.1	715.0	4476.9
Observations	106,591	106,591	106,591	106,591	106,591	19,990	19,990	19,990
Normalized Hebrew score in 8th grade								
Entrance Age	-0.16***	0.25**	0.25**	0.25**	0.27***	0.25*	0.14	0.22***
	(0.01)	(0.10)	(0.10)	(0.11)	(0.03)	(0.13)	(0.11)	(0.04)
F-Statistics on excluded instrument	-	855.6	857.9	821.4	17921.1	357.6	455.8	3367.5
Observations	104,836	104,836	104,836	104,836	104,836	19,332	19,332	19,332
Normalized math score in 5th grade								
Entrance Age	-0.10***	0.19**	0.20**	0.18*	0.36***	0.21**	0.26***	0.26***
	(0.01)	(0.09)	(0.09)	(0.10)	(0.02)	(0.10)	(0.09)	(0.04)
F-Statistics on excluded instrument	-	1173.7	1173.0	1124.9	21975.1	517.6	716.1	4464.9
Observations	107,662	107,662	107,662	107,662	107,662	20,146	20,146	20,146
Normalized math score in 8th grade								
Entrance Age	-0.19***	0.34***	0.35***	0.30***	0.31***	0.29**	0.24**	0.25***
	(0.01)	(0.11)	(0.11)	(0.11)	(0.03)	(0.13)	(0.12)	(0.04)
F-Statistics on excluded instrument	-	781.2	784.0	761.7	17276.5	337.1	449.0	3269.6
Observations	103,723	103,723	103,723	103,723	103,723	19,129	19,129	19,129
Date of year fixed effects	***No***	***Yes***	***Yes***	***Yes***	***No***	***Yes***	***No***	***No***
Day-of-week fixed effects	***No***	***Yes***	***No***	***Yes***	***No***	***Yes***	***Yes***	***No***
Quadratic trend of the running variable	***No***	***No***	***No***	***No***	***No***	***No***	***Yes***	***No***
Controls	***Yes***	***Yes***	***Yes***	***No***	***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 9 - DID AND RDD ESTIMATES OF THE ENTRANCE AGE EFFECT ON PERCENTILE SCORES

	DID (July-May)					RDD (±30 days)		
	OLS	*IV*	*IV*	*IV*	*IV*	*IV*	*IV*	*IV*
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Normalized Hebrew score in 5th grade								
Entrance Age	-1.29***	10.89***	10.90***	11.49***	11.34***	8.87***	9.81***	10.69***
	(0.31)	(2.68)	(2.68)	(2.80)	(0.66)	(3.16)	(2.73)	(1.10)
F-Statistics on excluded instrument	-	1247.5	1246.8	1195.8	21963.2	528.1	715.0	4476.9
Observations	106,591	106,591	106,591	106,591	106,591	19,990	19,990	19,990
Normalized Hebrew score in 8th grade								
Entrance Age	-4.21***	7.19**	7.22**	7.20**	8.95***	6.45*	4.73	7.09***
	(0.30)	(3.00)	(2.95)	(3.28)	(0.77)	(3.66)	(3.21)	(1.17)
F-Statistics on excluded instrument	-	855.6	857.9	821.4	17921.1	357.6	455.8	3367.5
Observations	104,836	104,836	104,836	104,836	104,836	19,332	19,332	19,332
Normalized math score in 5th grade								
Entrance Age	-2.50***	6.84**	6.93***	6.52**	11.32***	7.42**	8.30***	8.66***
	(0.32)	(2.70)	(2.70)	(2.80)	(0.71)	(3.06)	(2.60)	(1.08)
F-Statistics on excluded instrument	-	1173.7	1173.0	1124.9	21975.1	517.6	716.1	4464.9
Observations	107,662	107,662	107,662	107,662	107,662	20,146	20,146	20,146
Normalized math score in 8th grade								
Entrance Age	-5.30***	11.13***	11.21***	9.93***	9.34***	9.23**	7.49**	7.89***
	(0.34)	(3.25)	(3.24)	(3.38)	(0.87)	(3.86)	(3.42)	(1.32)
F-Statistics on excluded instrument	-	781.2	784.0	761.7	17276.5	337.1	449.0	3269.6
Observations	103,723	103,723	103,723	103,723	103,723	19,129	19,129	19,129
Date of year fixed effects	***No***	***Yes***	***Yes***	***Yes***	***No***	***Yes***	***No***	***No***
Day-of-week fixed effects	***No***	***Yes***	***No***	***Yes***	***No***	***Yes***	***Yes***	***No***
Quadratic trend of the running variable	***No***	***No***	***No***	***No***	***No***	***No***	***Yes***	***No***
Controls	***Yes***	***Yes***	***Yes***	***No***	***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 10 - 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	-0.64*	-0.48	-0.39	0.26
	(0.34)	(0.37)	(0.58)	(0.50)
Entrance Age	0.32***	0.16*	0.27***	0.22**
	(0.08)	(0.08)	(0.09)	(0.09)
Male * Entrance Age	**0.05**	**0.08**	**0.01**	**-0.03**
	(0.05)	**(0.06)**	**(0.09)**	**(0.09)**
Observations	106,951	107,662	19,990	20,146
Normalized score in 8th grade	Hebrew	Math	Hebrew	Math
Male	-1.31***	-0.46	-0.30	0.22
	(0.41)	(0.40)	(0.62)	(0.64)
Entrance Age	0.19**	0.32***	0.25**	0.32***
	(0.09)	(0.10)	(0.11)	(0.12)
Male * Entrance Age	**0.15****	**0.06**	**-0.01**	**-0.05**
	(0.06)	**(0.06)**	**(0.09)**	**(0.10)**
Observations	104,836	103,723	19,332	19,129
Panel B – Heterogeneous effect by parents' education				
Normalized score in 5th grade	Hebrew	Math	Hebrew	Math
Parents Quartile	-0.20	-0.07	-0.06	-0.10
	(0.12)	(0.12)	(0.20)	(0.20)
Entrance Age	0.33***	0.23**	0.31**	0.23*
	(0.09)	(0.09)	(0.12)	(0.13)
Parents Quartile * Entrance Age	**0.00**	**-0.02**	**-0.02**	**-0.02**
	(0.02)	**(0.02)**	**(0.03)**	**(0.03)**
Observations	106,951	107,662	19,990	20,146
Normalized score in 8th grade	Hebrew	Math	Hebrew	Math
Parents Quartile	0.14	0.18	0.22	0.16
	(0.13)	(0.15)	(0.22)	(0.22)
Entrance Age	0.38***	0.50***	0.39***	0.43**
	(0.11)	(0.11)	(0.16)	(0.17)
Parents Quartile * Entrance Age	**-0.06*****	**-0.07*****	**-0.07****	**-0.07***
	(0.02)	**(0.02)**	**(0.04)**	**(0.03)**
Observations	104,836	103,723	19,332	19,129

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

TABLE 1A – FIRST STAGE ESTIMATES FOR TABLES 8 AND 9

	DID (July-May)				RDD (±30 days)		
	IV	*IV*	*IV*	*IV*	*IV*	*IV*	*IV*
Variables	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Normalized Hebrew score in 5th grade</u>							
Entrance Age	0.29^{***}	0.29^{***}	0.28^{***}	0.28^{***}	0.29^{***}	0.30^{***}	0.37^{***}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	106,591	106,591	106,591	106,591	19,990	19,990	19,990
<u>Normalized Hebrew score in 8th grade</u>							
Entrance Age	0.25^{***}	0.25^{***}	0.25^{***}	0.27^{***}	0.24^{***}	0.24^{***}	0.32^{***}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	104,836	104,836	104,836	104,836	19,332	19,332	19,332
<u>Normalized math score in 5th grade</u>							
Entrance Age	0.28^{***}	0.28^{***}	0.27^{***}	0.28^{***}	0.28^{***}	0.30^{***}	0.37^{***}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	107,662	107,662	107,662	107,662	20,146	20,146	20,146
<u>Normalized math score in 8th grade</u>							
Entrance Age	0.24^{***}	0.25^{***}	0.24^{***}	0.26^{***}	0.24^{***}	0.24^{***}	0.32^{***}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	103,723	103,723	103,723	103,723	19,129	19,129	19,129
Date of year fixed effects	***Yes***	***Yes***	***Yes***	***No***	***Yes***	***No***	***No***
Day-of-week fixed effects	***Yes***	***No***	***Yes***	***No***	***Yes***	***Yes***	***No***
Quadratic trend of the running variable	***No***	***No***	***No***	***No***	***No***	***Yes***	***No***
Controls	***Yes***	***Yes***	***No***	***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.