The Impact of School Entry Age on Student Achievement: Evidence from Nebraska

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Abstract

Using administrative data with the exact date of birth from the Nebraska Department of Education, I assessed the impact of waiting an additional year to start kindergarten on the socioeconomic achievement gap. Applying a fuzzy regression discontinuity design, I found a positive effect of waiting on test scores. However, the positive impact diminished over time, and the diminishing effect was more pronounced for children in disadvantaged households. This suggested that the decision to delay kindergarten entry might worsen the socioeconomic achievement gap. However, using an exogenous change in the kindergarten entry policy in Nebraska, I found no impact of moving the kindergarten cut-off earlier on the achievement gap. A reduction in the practice of redshirting might have driven that result.

Keywords: entry age, kindergarten cut-off, achievement gap, Nebraska, achievement scores

1 Introduction

School readiness holds considerable importance in determining academic achievement. Given the absence of a clear measure to define school readiness, the age at which a child enters school is commonly used as a proxy for their readiness. A simple strategy to improve achievement scores was to raise the average age of admission to school (Stipek, 2002).

Numerous studies investigated the effects of school entry age on test scores. While analyzing the influence of entry age provided valuable insights, the emphasis was mainly on the average impact. However, the decision to delay kindergarten entry could affect high-scoring students differently compared to those with lower scores. Furthermore, this impact varied across demographic characteristics (Elder & Lubotsky, 2009). Although many studies demonstrated the diminishing effect of entry age as students progressed through school, little attention was paid to whether this fading pattern exhibited heterogeneity based on their background. Considering all these factors, it was crucial to address some additional questions that had not yet been explored: For whom did the kindergarten entry age matter? Did the impact show a fading pattern, and did the diminishing rate exhibit uniformity across different demographic groups?

My analysis was based on student-level administrative data from the Nebraska Department of Education (NDE). It was longitudinal data with the precise date of birth of the students. This paper focused on nine cohorts, and the cohorts had begun kindergarten between the academic years 2007-2008 and 2015-2016. I used a fuzzy regression discontinuity framework, which leveraged exogenous variation in school starting age imposed by state law to estimate the impact of waiting an additional year to start school on test scores from 3rd grade through 8th grade. I found that waiting an additional year to start kindergarten resulted in a 0.3 standard deviation increase in test scores.

I also checked if there was any evidence of heterogeneity in impact for each grade by demographic characteristics, precisely if the impact varied by gender, socioeconomic status, or race. The advantage of waiting was more evident in the advantaged groups, aligning with the findings of Elder & Lubotsky (2009). Although the achievement gap between older and younger students gradually decreased, the rate of reduction was notably faster for children from disadvantaged backgrounds.

In addition to that, I investigated the impact of waiting on the entire distribution of test scores. The average effect on test scores did not reveal the considerable heterogeneity in the causal effect of waiting on the test score distribution, which might be interesting from a policy perspective. Of particular interest was whether waiting helped the students at the bottom of the score distribution. This paper offered answers to these distributional questions. Applying the IV quantile approach, I found that waiting to improved test scores regardless of the position in the score distribution. However, waiting resulted in a sizable increase in test scores, especially at the upper end of the distribution compared to the lower end, implying that top performers in the class accrued more benefits from waiting.

The distinct magnitude of the impact of waiting across different demographic groups might contribute to an acceleration of the socioeconomic and racial achievement gaps. Considering the change in kindergarten entry policy in Nebraska, I explored how moving the kindergarten cut-off earlier in the calendar year affected the achievement gap between advantaged and disadvantaged groups. Surprisingly, I found that the reform of the policy did not have a substantial impact on the achievement gap. This result could have been driven by a growing gap resulting from waiting and a reduction in the practice of redshirting. Importantly, my results remained consistent across various specifications.

This paper was related to three different streams of literature. First, I added to the literature on kindergarten eligibility and its impact on different academic achievements. Most studies studied the association between school entrance ages and various educational outcomes. Although usually a higher entry age gave a comparative advantage in school, ranging from higher test scores (Datar, 2006; Attar & Cohen-Zada, 2018; Elder & Lubotsky, 2009; Cook & Kang, 2016; Fletcher & Kim, 2016) to a lower probability of grade retention (Cook & Kang, 2016). A higher entry age also led to a higher probability of high school dropout and a lower accumulation of human capital, as entering school late caused one to reach the mandatory school attendance age earlier (Cook & Kang, 2016). Some studies investigated the impact on other outcomes, such as adult wages (Bedard & Dhuey, 2007), juvenile crimes (Depew & Eren, 2016), and felony offenses (Cook & Kang, 2016). All of these studies estimated the mean effect of school entry age on the outcomes. Unfortunately, the conventional approach did not take into account the heterogeneity of impact across the score distribution. To overcome that, I incorporated the quantile approach in the school readiness literature, which had not been studied before. I found that waiting to enter kindergarten increased the achievement score, consistent with previous works on birthday effects and academic performance. This quantile approach additionally enabled us to observe considerable heterogeneity throughout the distribution. The impact of waiting was greater for those who were at the top of the score distribution.

This paper contributed to the school readiness literature by comparing the progress of various subgroups based on demographic characteristics throughout different grades. Some studies found that the impact of entry age did not persist for long and the achievement gap between older and younger children began to fade away (Lincove & Painter, 2006; Elder & Lubotsky, 2009). Considering the gap in the magnitude of impact among various groups, I investigated the pattern of fading away of entry-age effects across different demographic groups as they progressed through school. My findings revealed that the age effect persisted longer among children of high-income families than among other groups, exacerbating the achievement gap. This happened because of inadequate support for the development of children from low-income households. Thus, incorporating these distinct trajectories, we gained insight into the underlying mechanisms that contributed to this phenomenon.

This paper also contributed to the literature on the achievement gap. Previous studies on achievement gap mainly explored the effects of factors such as school standards, cultural norms, and childhood environment on the achievement gap (Autor et al., 2016; Nollenberger et al., 2016; Chetty et al., 2016). Some studies also investigated the impact of entry age on the achievement gap. For example, Oshima & Domaleski (2006) identified the existence of an achievement gap up to grade 5. My work was closely related to the research of Lenard & Peña (2018). They used an instrumental variable strategy based on policy reform in North Carolina and found an increase in the minority-majority achievement gap (based on race) in waiting. However, Lenard & Peña (2018) focused on a single school district and grade three test scores. I contributed to this literature by specifically focusing on the socioeconomic and racial achievement gap and incorporating data from multiple grades for analysis.

The rest of the paper proceeded as follows. In the next section, I provided details of the institutional background, data, and descriptive statistics. Section 3 described the intuitions behind the research design. Section 4 explored the empirical strategy. Section 5 presented my results, followed by the impact of the policy reform in Section 6. Section 7 discussed robustness checks and sensitivity analyses. In the end, I concluded in Section 8.

2 Settings and Data

2.1 Admission Process

Before the 2012-13 school year, the cut-off date for kindergarten enrollment in Nebraska was October 15. This meant that children had to turn five years old on or before October 15 to be eligible for kindergarten. However, the provision of early entrance had always been included in the Nebraska statute. Before the reform, children born between October 16 and February 1 were allowed to start school, provided that they met certain requirements. [1] However, starting from the 2012-13 school year, the cut-off date was changed to July 31, allowing children to enter kindergarten if they reached the age of five on or before that date. Children born on or after August 1 will be deferred and start kindergarten at almost the age of 6. However, as mentioned before, there are opportunities for early entry. Children who turn five between two cut-off dates, precisely from August 1 to October 15, might also be considered eligible if they demonstrate satisfactory performance on a school assessment. This assessment evaluates whether a child possesses the necessary skills to handle kindergarten-level tasks (Neb. Rev. Stat. § 79-214). Regulations on determining maturity depend a lot on parents' discretion. Parents may accelerate kindergarten entry if they feel the child is skilled enough to start early. In contrast, if parents suspect any developmental delay, they might hold their children back even if they are eligible to enter. In particular, the reason for red-shirting or green-shirting is not possible to determine (Fertig & Kluve, 2005). This leads to variations in enrollment practices.

2.2 Sample

The current study used student-level administrative data collected by the Nebraska Department of Education (NDE). The sample spanned data from 2007-2008

[1]"A board can admit a student even if he/she is born after the cut-off if parent or guardian request and provides an affidavit stating that (a) the child attended kindergarten in another jurisdiction in the current school year, (b) the family anticipates relocation to another jurisdiction that would allow admission within the current year, or (c) the child has demonstrated through recognized assessment procedures approved by the board that he or she is capable of carrying the work of kindergarten or the beginner grade" (Neb. Rev. Stat. § 79-214)

to 2018-2019. Due to COVID, I restricted my sample to 2018-2019. However, it included all first-time kindergarten entrants from the 2007-2008 to 2015-2016 school years who enrolled in the public schools of Nebraska. The data were limited to only students from public schools in Nebraska, as they were required to follow the state's kindergarten entry regulations [2]. The kindergarten entry cohort ranged from approximately 21,000 to 22,000 each year. I excluded 14,126 observations as they had incongruous birth dates across different grades. As it was longitudinal data, I was able to follow each kindergarten entrant for a few years until grade eight[3]. One limitation of the data was that it excluded children studying at private schools. Furthermore, I could not observe if any student moved out of the state. I restricted the sample's entry age to 57 to 79 months as of August 1 of their kindergarten entry year, as in Nebraska, where the compulsory school age was six by January 1 (Neb. Rev. Ann. § 79-201). I also dropped those students who I could not observe in kindergarten. Of my sample of first-time kindergarten entrants, 48% were female. Regarding ethnicity, the percentages were 70% White, 7% Black, 18% Hispanic, and 2% Asian, respectively. 48% of my sample was eligible for free and reduced-fee lunch (FRL), while almost 12% were eligible for special education. While nearly 13% of the students were enrolled in ELL (English Language Learner) classes in kindergarten, less than 1% were homeless in kindergarten. My sample was evenly distributed around the cut-off point, as indicated in Table 1.

[2] In Nebraska, 85.67% of kindergarten-going children attended public school. (Based on the calculation from IPUMS USA)

[3] The dataset was not balanced, and I was unable to track all students across all grades.

I assigned each child the kindergarten cut-off applicable in the year they reached the age of five. I computed 'waiting to enter kindergarten', the key explanatory variable based on the information if the student's entry was delayed from the year they turned five to the year they turned six. The primary factors on which I focused were the children's achievement scores in math and reading, administered at each grade level from three to eight. I standardized scores according to the assessment system, grade, and year. It is important to note that during the period of my analysis in Nebraska, the assessment system shifted from NeSA (Nebraska State Accountability Test) to NSCAS (Nebraska Student-Centered Assessment System). According to NeSA, the scale scores ranged from 0 to 200. Scores below 84 were considered below-standard performance, scores between 85 and 135 indicated standard performance, and scores above 135 indicated above-standard performance. The change occurred from the 2017-2018 academic year. NSCAS used different scoring scales for math and reading. Math scores fell between 2220 and 2890, while reading scores ranged from 1000 to 1550. NSCAS offered an alternative NSCAS for special education. According to NSCAS, English proficiency was determined based on the ELA (English Language Arts) score, which consisted of three components: reading comprehension, reading vocabulary, and writing. Due to the inaccessibility of separate subcategories, I considered their overall ELA score to gauge their reading proficiency. I measured the indicators of being gifted by assigning a value of one if a student was selected for the gifted program within grade four; otherwise, I assigned a value of zero. I did not consider a student gifted if they entered the program after grade four, as there were very few cohorts available in upper grades. Within my sample, approximately 15 % of the students were identified as gifted. However, following the implementation of the policy reform, there was a slight decrease in the rate of gifted identification. My measure of grade retention was an indicator that a student had repeated any grade from kindergarten to grade 4. In this sample, only a very few students repeated a grade. I defined an indicator for special education, which was set to one if a student was diagnosed with any form of disability by the end of grade 4. Almost 23 % of the sample received special education. The summary statistics for these variables were presented in Table A1.

Table A2 presented summary statistics of mean achievement scores at specific points of the score distributions. It displayed the mean and standard deviation for both standardized math and standardized reading scores, along with individual-level controls. In particular, individuals within the lowest 25 % of the score distribution exhibited a higher likelihood of meeting the criteria for the FRL program (FRL-eligible), identifying as Black or Hispanic, and residing in metropolitan areas (which, in this context, include Lincoln and Omaha). In contrast, for those within the top 25 % of both score distributions, there was a probability exceeding 80 % that they were White. This suggested a variance in the distribution across different percentiles, showcasing some degree of heterogeneity.

3 Kindergarten cut-off, Waiting, and Test Scores

This section delved into the rationale behind my research design. The aim was to investigate how a child's birthday position relative to the cut-off date was associated with the likelihood of waiting, and, in turn, how this influenced test scores.

3.1 The First Stage

Figure 1 showed the relationship between birthdates throughout the year and the probability of treatment. I defined treatment following the definition of Ricks (2022) and Barua & Lang (2016). Given this, the family could enroll the children in kindergarten when they reached the age of five or could choose to wait until the following year. I used the decision of waiting to enter kindergarten as treatment.

The treatment of 'waiting to enter kindergarten' offered several advantages compared to other approaches. Numerous studies employed various modes of age as treatments, such as predicted age (Elder & Lubotsky, 2009), relative age (Peña, 2017; Thompson et al., 2004), and entry age (Angrist & Krueger, 1992; Shapiro, 2023; Fertig & Kluve, 2005), among others. In contrast, my treatment avoided the ambiguities associated with using the entry age or predicted age as a treatment. Furthermore, disentangling different age effects, like relative age and absolute age, was not easily achievable. By using 'wait to enter kindergarten' as treatment, there was no need to separate these age effects (Ricks, 2022). Moreover, this treatment did not violate the monotonicity assumptions that other age-based treatments failed to satisfy (Barua & Lang, 2016).

The kindergarten cut-off in Nebraska had always been October 15 before the enactment of the new law. Children born after October 15 were required to wait an additional year before starting kindergarten. Figure 1 (a) showed a sharp increase in the probability of waiting, rising from 0.53 to 0.98 on October 15, before the reform.

Interestingly, since the 2012-13 academic year, Nebraska has had two cut-offs for kindergarten enrollment. The cutoff was moved from October 15 to July 31, and at the same time, there is a provision for early entry, which allows assessment-based enrollment for children born between these two cutoff dates. Figure 1 (b) depicted the scenario after the policy reform. The probability of waiting then jumped on 31 July, although the increase was less than 1, indicating the prevalence of non-compliance on both sides of the cut-offs. However, the likelihood of waiting still increased on October 15. Based on the magnitude of the jump in the probability of waiting at both cut-offs, I concluded that in Nebraska, most parents tended to adhere to the kindergarten entry policy and the recommendation by waiting until the following Fall if their children's birthday fell after the soft cut-off, which is now July 31.

3.2 The Reduced-Form Relationship

Figure 2 depicted the reduced-form relationship between the date of birth of the children and their standardized test scores. This connection mirrored the pattern seen in the probability of waiting, as shown in Figure 1. In Panel A, we observed this relationship prior to the policy change. When the cut-off was October 15, the oldest students in the class were born late October or November. Similarly, the youngest students were born in the first part of October. Math test scores remained relatively consistent until mid-October, after which there was a noticeable increase in math scores. Students born just after the October cut-off exhibited a math score that was 0.11 σ higher than their peers born just before the cut-off. Similar patterns were observed for reading scores. In contrast, under the new kindergarten entry policy with two cut-offs, one on July 31 and another on October 15, a distinct trend emerged. A significant surge in math scores was observed at the July cut-off point. However, at the second cut-off in October, there was a decline in math scores. The largest benefit from waiting was observed for those born after July 31 and who were recommended to wait. The advantage of waiting was somewhat reduced for those born after October 15, although the impact of waiting was still notable compared to those who did not need to wait. The magnitude of impact was almost identical for the reading scores. These reduced-form relations did not account for non-compliance, which leads to a bias in the magnitude of discontinuity around cut-off points, tending towards zero (Attar & Cohen-Zada, 2018).

4 Empirical Strategy

Due to concerns arising from the manipulation of entry age, the literature on kindergarten entrance historically used IV estimates to capture the impact of entry age. The predicted age (Elder & Lubotsky, 2009), the number of days between the child's 5th birthday and the kindergarten cutoff date (Datar, 2006), and the quarter of birth (Angrist & Krueger, 1992) were some of the instruments in the literature on kindergarten cut-off. However, any instrument that used variation in the month or quarter of birth would not satisfy the monotonicity assumption to identify the Local Average Treatment Effect (LATE) (Barua & Lang, 2016). Because of this, I used a binary instrument equal to one if the student was required by the state law to wait an additional year to start kindergarten or zero otherwise, following the work of Barua & Lang (2016).

My goal was to estimate the return from waiting an additional year to enter kindergarten versus not waiting. Ordinary Least Squares (OLS) could not yield an unbiased estimate as parents could select the treatment.

The randomly assigned cut-off solved the problem of selection bias. As the treatment effect was only identifiable at the cut-off, only LATE could be produced.

4.1 Impact of Waiting on Achievement Scores: RD Approach

The kindergarten entry policy provided a natural framework for Regression Discontinuity Design (RDD). Due to non-compliance on both sides of the cut-off, RDD took a fuzzy form.

The intuition behind using this strategy was that students with birthdays very close to the cut-off, on both sides, were comparable in terms of observable and unobservable characteristics. The estimates I obtained were known as the LATE for children whose admission decision depended on which side of the cut-off their birthday fell, namely the group of compliers. The assigned variable for treatment,

D_i took the value of one if they started kindergarten at age six or more and zero otherwise. I estimated the following equation for educational outcomes at cut-off:

$$Y_i = \alpha + \beta D_i + \tau X_i + \epsilon_i \tag{1}$$

where Y_i were the educational outcomes for the individual i, D_i was an indicator that took the value one if the individual i's kindergarten entry was delayed from the year he turned five to the year he turned six X denoted personal characteristics, including sex and race. β captured the effect of waiting an additional year to start kindergarten on educational outcomes, holding ϵ_i and X_i constant. The estimator in the model mentioned above might be biased because parents could influence D by speeding up or red-shirting their children's school entry. To overcome this, I projected Y_i and D_i as follows:

$$Y_i = \mu Z_i + \pi(birthday - cut - off) + \delta Z_i(birthday - cut - off) + u_i \tag{2}$$

$$D_i = \theta Z_i + \sigma(birthday - cut - off) + \eta Z_i(birthday - cut - off) + v_i \tag{3}$$

where Z_i characterized the incentives behind the delay in kindergarten entry for individual i. I defined Z as below:

$$Z = \mathbb{1}[r > cut - off] \tag{4}$$

where r indicated the running variable, which in this case was 'date of birth'. I considered the cut-off to be 15 October for the years before the policy change and

31 July after the reform. Due to the variability in the cut-offs, I normalized the cut-off around zero and adjusted the running variable accordingly. Z captured whether a student was recommended to delay kindergarten entry. Here, $\mu = \theta\beta$ by linear projection. Under smoothness of the running variable and assuming $\theta \neq 0$, β was identified as the ratio of discontinuity at Y_i to that of D_i.

During the estimation of equations 2 and 3, I restricted my sample to children with birthdays within a relatively narrow window around the kindergarten cut-off to avoid influence of children whose birthdays were far from the cut-off. I used 45 days of data bandwidth for analysis. The traits of individuals within 45 days around the cut-off were very similar. Estimates were not sensitive to the choice of bandwidth. For this design to be effective, Z could not be correlated with unobserved characteristics. This implied that Z influenced Y solely through D within the specific narrow window around the cut-off. This estimate captured the average treatment effect among children whose school entry decision was induced by the instrument.

4.1.1 Identification

To produce consistent estimates, the identification strategy required some conditions to hold. First, the monotonicity condition had to be satisfied. The monotonicity assumption required that crossing the kindergarten cut-off did not make an individual less likely to wait an additional year to start kindergarten. What this implied was that if a child waited a year when they were born before the cut-off, they would also have waited if their date of birth was just after the cut-off. My setting seemed to ensure the absence of defiers in the sample.

Second, the date of birth around the cut-off could not be manipulated. If parents manipulated the date of birth to expedite the school entry of their children, the distribution of the running variable at the cut-off would have been discontinuous. Since parents were unlikely to determine the exact date of birth, children born before and after the cut-off were similar in terms of observable and predetermined characteristics (McCrary & Royer, 2011). However, there was the possibility of differential selection in public schools before and after the cut-off. If there had been a tendency among parents to use private schools as a means to bypass the entrance age requirement (Taveras, 2021) of the school entry policy, the condition of continuity at the cut-off might have been violated. To address this concern, I checked the evidence of discontinuity at the cut-off.

I ran a rddensity test that used a local cubic estimator to find evidence of discontinuity (Cattaneo et al., 2018). This test did not provide statistically significant evidence of discontinuity around the cut-off (Figure A1). The density test result suggested a very similar admission rate in public schools in Nebraska among children born before and after the cut-off. The distribution of date of birth was also consistent with the above findings. Figure 3 displayed a histogram depicting the birthdates of all children and appeared to show a smooth distribution near the cut-off point.

Third, to ensure the validity of the RD estimates, it was recommended to test the balance of observable covariates around the cut-off. The goal was to assess whether the observations just above the cut-off were similar to those under the cut-off point. In Table 2, I provided estimates obtained from the test of discontinuities in predetermined covariates by applying both parametric and nonparametric approaches. In both cases, I estimated if there was any discontinuity in characteristics around the cut-off. I employed bandwidths of 45 and 30 days for each covariate. Using 45 days of bandwidth, I did not find any evidence of discontinuity for any covariates. However, with a 30-day bandwidth, I observed only one significant jump at the threshold for the black covariate. The findings were also reflected in Figure 4.

In my sample, I did not have children whose parents used private schools to bypass the kindergarten threshold and then transitioned their children back to public schools in subsequent grades.

There was another visual test for manipulation, which could be used when there might have been some discontinuity in the running variable for exogenous reasons. According to Lee & Lemieux (2010), a sufficient condition for an unbiased RD estimate required smoothness of the conditional joint distribution of the running variable.

Unless other approaches required only the continuity of the running variable, this test required the ratio of conditional and unconditional densities of the running variable to be continuous. If there was some discontinuity in the running variable due to some exogenous factors in the determination of the treatment, that discontinuity had to be balanced to make the ratio continuous (Zimmerman, 2014).

Figure A2 showed the density ratios for six groups: White, Black, Hispanic, female, FRL eligible students, and students living in metropolitan areas. Each point indicated the ratio of the proportion of children with specific characteristics to the proportion of all observations within the specific bin. In accordance with a valid RD design, each density ratio remained continuous around the threshold.

4.2 Impact of Waiting on Achievement Scores: Quantile Approach

To estimate the effect of waiting on the distribution of achievement scores at each point in the score distribution other than the mean, the quantile regression framework had to be applied. At the θth quantile, the basic quantile model was expressed as:

$$Y_{i\theta} = \beta_\theta D_i + \epsilon_{i\theta} \tag{5}$$

where $Y_{i\theta}$ denoted the θ quantile of Y given D. β_θ was the parameter of interest that indicated the coefficent of interest at θ quantile. It captured the difference in the conditional θ quantiles of the outcomes Y_1 and Y_0. This model was estimated by minimizing the following equation according to Koenker & Bassett Jr (1978) as follows:

$$min \sum_{i:y_i>=\beta_\theta D_i}^{N} \theta|y_i - \beta_\theta D_i| + \sum_{i:y_i<\beta_\theta D_i}^{N} \theta|y_i - \beta_\theta D_i| \tag{6}$$

The value of the coefficient, β varied based on the specific quantile being studied. The Quantile Treatment Effect (QTE) represented the impact of treatment D at different points of the marginal distribution of potential outcomes. As D was endogenous in this case, the conditional quantile of the outcome given the treatment was not equal to the quantile of the potential outcome (Chernozhukov & Hansen, 2004). Conventional quantile regression failed to produce an unbiased estimate and called for quantile regression with instrumental variables. To solve this endogeneity problem, I utilized the instrumental variable quantile regression (IVQR) model proposed by Chernozhukov & Hansen (2004). The model required the instrument to affect outcomes only through treatment. Under some conditions, the IVQR model satisfied the following conditional probability:

$$Pr\{y - D\beta_{\theta} <= Z' * 0|Z\} = \theta \tag{7}$$

β_{θ} had to be estimated in such a way that the coefficient of Z approached zero. In particular, I used smoothed estimating equations (SEE) outlined in Kaplan & Sun (2017). This estimator applied the kernel method to smooth the original estimating function.

Using the IVQR framework was justified when impacts demonstrated evidence of heterogeneity across the distribution. It could be tested with a post-estimation command used after SEE estimation. That test supported the adaptation of the quantile approach[4](Table A4).

[4]If Kolmogorov–Smirnov statistic was greater than the critical value, we could reject the null hypothesis. The null hypothesis was a constant effect throughout the distribution

5 Results

5.1 RD Approach

This section presented the effect of waiting an additional year on educational outcomes. Regressions were estimated on the sample of students from public schools in Nebraska. I started by providing evidence that children born within 45 days before the cut-off were similar to the ones born after 45 days of the cut-off. Table A3 presented prevalence statistics for six covariates, namely, female, FRL eligible, metro, White, Black, and Hispanic, demonstrating the evidence that 'before' and 'after' groups were close to each other, and it could be expected that the children were assigned at random to these two groups. Table 2 also provided additional evidence to support the use of 45 days of bandwidth for analysis.

The impacts of waiting an additional year to start kindergarten on standardized test scores could be assessed parametrically or nonparametrically. Although the parametric method was my main approach, I also incorporated a nonparametric approach that used an inference procedure developed by Calonico et al. (2014). Table 3 presented the impacts on the standardized math and reading scores, spanning from grade three through eight, utilizing nonparametric and parametric approaches. In the first and third columns, I presented results obtained from utilizing bandwidth 45, and in the second and fourth columns, utilizing bandwidth 30. The results were not that sensitive to the choice of estimation approach and bandwidth selection. On the other hand, Table 4 presented the sensitivity to the estimation method and the observable characteristics. In the first row, I presented the results obtained by the OLS analysis, which indicated a substantial and favorable impact of waiting a year to start kindergarten on test scores. Specifically, the coefficient of 0.457 suggested that waiting a year led to an improvement of 0.457 σ in math scores. As the decision to wait suffered from endogeneity issues, I addressed that concern using two-stage least squares (2SLS) strategy. In the second row, I presented the results from the 2SLS estimation without any additional controls. However, as demonstrated in Table A2, it became apparent that high-performing students often came from affluent backgrounds and were predominantly of White ethnicity. Therefore, I considered the inclusion of controls for these demographic characteristics to potentially regulate the impact of waiting a year to start kindergarten. In the third row, I provided the 2SLS estimates with individual-level controls and go further by accounting for factors that remained consistent within a school district. This was achieved by introducing school district fixed effects in the fourth row. Row 4 presented the 2SLS estimates with both individual-level controls and school district fixed effects. In particular, the 2SLS estimates remained largely consistent across rows 2, 3, and 4. This suggested that the effects of delaying kindergarten were not strongly correlated with observable characteristics of the children and the school districts they attended. However, in contrast to the 2SLS estimates, the OLS estimates demonstrated a more substantial positive impact of waiting on test scores. Comparison of OLS and 2SLS estimates, respectively, from rows (1) and (2) confirmed evidence of positive selection on gains, implying that students who were more likely to wait had more significant treatment effects, which was contrary to the findings of Elder & Lubotsky (2009).

The relationship between waiting a year and achievement scores gradually diminished over time as students progressed through the grades. For the math score, the decision to wait improved the test score by 0.341 σ in grade 3. By the time students reached eighth grade, the positive effect reduced to 0.256 σ. The same pattern held for reading scores. However, the influence of delaying school entry did not completely fade. Even in eighth grade, this influence remained substantial and statistically significant for math and reading scores.

5.2 Heterogeneity Analysis

To address the research question regarding potential variations based on gender, race, or socioeconomic status, I employed the same fuzzy RDD without any additional controls. This approach allowed me to estimate LATE for distinct subgroups, including male and female students (by gender), individuals from metropolitan and non-metropolitan areas (by metropolitan status), those eligible and ineligible for FRL programs (by socioeconomic status), and participants from different racial backgrounds, namely White, Black, and Hispanic. I used equations 2 and 3 to derive these estimates. Figures 5, 6, 7, and 8 displayed plots containing estimated coefficients and their corresponding robust confidence intervals, which I arranged side by side for easier comparison. Upon examining the magnitude of the impact, I observed that waiting an additional year had a greater effect on improving girls' test scores compared to boys. The diminishing effect of waiting was particularly noticeable among boys, where the fade-out occured more rapidly. Conversely, for girls, the fade-out rate was considerably slower. Children from both metropolitan and non-metropolitan areas showed similar responses in terms of the magnitude of impact and the fading out of this impact over time. However, the impact varied significantly across different socioeconomic statuses. Notably, the effect was more pronounced among children from affluent backgrounds, consistent with the findings of Elder & Lubotsky (2009). Elder & Lubotsky (2009) attributed this stronger entry age effect to the faster accumulation of human capital among high-income children before starting kindergarten. I also found that the benefit of an additional year of waiting diminished rapidly for students eligible for FRL program. When comparing estimates across racial groups, I found that the response to entry age was relatively stronger among White children, while the advantage of waiting faded out at a faster rate for Black and Hispanic children. This observation aligned with the pattern seen in the comparison between FRL-eligible and FRL-ineligible groups. The differential rate in the pace of fading out implied some presence of Matthew effect in age at entry literature, even though the gap between older and younger children narrowed overtime[5]. I also examined whether these variations in impact were statistically significant and presented the results in Table 5. I observed that gender, FRL eligibility, and race contributed to the heterogeneity in impact. When evaluating the impact of waiting while interacting with the White category, I excluded 'Asian' from the analysis. Since Asians exhibited the highest average scores, considering them as non-White would not have provided an accurate representation of the true impact of waiting and could

[5]Sociologist Robert K. Merton coined the term "Matthew Effect" to refer to his theory of cumulative advantage in science. The phenomenon was named after a verse in the Gospel of Matthew (13:12), which states that "for whoever hath, to him shall be given, and he shall have more abundance: but whoever hath not, from him shall be taken away even that he hath." (Delis et al. (2022))

potentially biased the results downward (Table A5). However, the effects were consistent for both math and reading scores.

5.3 Quantile Approach

The RD estimates of Section 5.1 confirmed the significant impact of waiting on the achievement scores. The impact could have been more pronounced for certain students, while less pronounced for others. Therefore, I investigated the heterogeneity in the effect of waiting on scores using quantile regression. Specifically, to address the endogeneity of the treatment variable, I employed the IVQR model. In this section, I presented the causal impact of waiting to enter kindergarten across the achievement score distribution. The results obtained from OLS, 2SLS, ordinary quantile regression (QR), and instrumental variable quantile regression (IVQR) differed from each other.

Panels A and B of Table 4 showed OLS estimates for math and reading scores, respectively. Waiting an additional year before entering kindergarten resulted in an increase of 0.457σ in math scores and 0.452σ in reading scores. These point estimates indicated a score gain compared to those who did not wait. However, OLS estimates may have overstated the actual causal impact of waiting on standardized scores. To address this concern and the potential influence of other factors, I examined the 2SLS model. In this model, the estimates for the effect of waiting were approximately 30% smaller than the OLS estimates, although they remained statistically significant. The OLS and 2SLS estimates both provided a conditional mean effect of waiting an additional year to start kindergarten on achievement scores, although the distributional aspects of these estimates remained undiscovered. Quantile regression estimates in Table 6, on the other hand, provided OLS-like estimates at various points along the distribution. In this analysis, waiting an additional year before starting kindergarten was associated with a positive and statistically significant impact on scores in all quantiles. The magnitude of the impact varied, ranging from an increase of 0.347 σ to 0.527 σ for math scores and from 0.379 σ to 0.481 σ for reading scores. As the median point estimate exceeded the mean estimate, the distribution of math and reading scores skewed toward the left.

In Panel B of Table 6, for waiting an additional year to start kindergarten, the instrument I used was the indicator of whether the birthday fell after the kindergarten cut-off. Whereas the 2SLS estimator captured the mean effect of waiting, IVQR provided 2SLS-type estimates at various points of the score distribution. Consistent with the mean effect between OLS and 2SLS, the estimates of the IVQR model were considerably lower than their OLS counterparts. The IVQR estimate at the 90th percentile of the math score distribution was 0.358 σ, while the corresponding QR estimate was 0.537 σ. Figure 9 demonstrated the causal effect of waiting on scores by plotting estimates from the QR and IVQR models. QR estimates in (i) of Panels A and B of Figure 9 showed significant variability throughout the score distribution. The OLS estimates were only consistent with QR estimates around the 30th and 20th percentiles for math and reading, respectively. Below that range, point estimates were much smaller in magnitude than OLS estimates. However, after that, the range estimates were significantly larger than the OLS estimates. (ii) of Panels A and B presented the IVQR estimates. The pattern of IVQR estimates was quite similar for reading scores, as well. Below the 25th percentile, the IVQR estimates were smaller than the 2SLS estimates. IVQR estimates were harmonious with the 2SLS estimates from the 25th to the 70th percentiles. Beyond that point, IVQR exceeded the 2SLS estimate. However, at higher quantiles, the smaller number of observations widened the standard errors, making the IVQR estimates not significantly different from the 2SLS estimates. IVQR estimates outgrew 2SLS between approximately the 70th and 85th percentiles.

6 The Effect of Changing Kindergarten cut-off on Achievement Gap

After the enactment of No Child Left Behind in 2001, more emphasis was placed on improving test scores and accountability than ever before. To ensure better performance in inter-state comparison of scores, many states moved the kindergarten cut-off earlier (Cannon & Lipscomb,2008). In the Fall of 1968, 96 % of six-year-old and five-year-old were enrolled in 1st grade and kindergarten, respectively, or above. In 2005, the ratio had fallen drastically to 84 % (Deming & Dynarski, 2008). Following this trend, Nebraska increased the minimum kindergarten entrance age by 2.5 months. Moving the cut-off earlier in the calendar year increased the school entry age by making more students wait. Although the entrance age effect was positive among students from families with lower socioeconomic status, the overall waiting a year risked exacerbating the socioeconomic achievement gap in academic performance.

In Section 5.2, I identified variations in the effects of waiting on various groups of children, such as male-female, FRL eligible-FRL ineligible, and White-non White. I found that the impact of waiting another year to start kindergarten was greater among children from privileged families. Moreover, quantile IV regressions revealed a stronger impact among students from the upper quantile of the score distribution. As students from high-income families were more prevalent in the top quantile, this confirmed heterogeneity in impact across socioeconomic groups. This disparity in impact raised another research question: did policy reform amplify or diminish the achievement gap between these different groups?

To investigate how advancing the kindergarten cut-off date impacted socioeconomic or racial achievement gaps, I focused my analysis on children whose eligibility for kindergarten admission was directly influenced by the policy change. These children were born between August 1 and October 15 and were no longer eligible to start kindergarten the year they turned five unless they passed the early entrance assessment test. My specification was difference-in-difference, which compared the standardized scores of FRL-eligible children to those who were not FRL-eligible. Simultaneously, it compared the educational outcomes of White children with non-White children. The estimation model utilized two sources of variation. The first difference came from the gap between the FRL eligible/White and FRL ineligible/non-White group. The second difference came from comparing test scores before and after the policy change. I estimated the following equations for this group of students:

$$Y_{idt} = \alpha + \beta FRL_i + \gamma post_t + \delta post_t x FRL_i + \mu_d + \tau_t + X_i + \eta_{idt} \tag{8}$$

$$Y_{idt} = \alpha + \beta White_i + \gamma post_t + \delta post_t x White_i + \mu_d + \tau_t + X_i + \eta_{idt} \tag{9}$$

In this equation, Y_{idt} represented the test score of an individual i in the time period 't' in the school district 'd'. FRL_i in equation 8 was an indicator that signals whether the individual is eligible for the FRL program. On the other hand, $White_i$ in Equation 9 took the value one if the student was White and zero otherwise. Furthermore, $post_t$ was an indicator that determines whether the year of admission to kindergarten fell before or after the policy change. μ_d and τ_t represented school-district and year fixed effects, respectively. X_i denoted individual-level controls, such as race and gender. The parameter of interest, represented by δ, indicated whether the policy reform improved or deteriorated the achievement gap among the children directly affected by the change. While examining the racial achievement gap, I excluded Asians for the same logic, for which I dropped them from the heterogeneity analysis in section 5.2.

This research design identified the effect of changing the kindergarten cut-off on the achievement gap under the assumption that there existed a parallel trend in test scores between FRL eligible/White and FRL ineligible/non-White groups of students before the policy reform. To get confirmation of that, I utilized the following event-study model:

$$y_{idt} = \alpha + \sum_{k \neq -1} \beta_k D_{idt} + \phi X_i + \eta_d + \gamma_t + \epsilon_{idt} \tag{10}$$

where the parameter of interest was β, which showed the impact of policy reform on educational outcomes. D was a binary variable that equals one if the treatment was active for the individual i in the school district d and year t. I included event-time dummies -5 to +3 in the event study graph. I excluded $k = 1$ as the coefficients were estimated relative to the standardized scores of a year before the policy change. The event study graphs (Figures A3, A4) confirmed the parallel trend assumption in the pre-reform period, as the time trends in the outcome were very similar among treated and nontreated children.

The results from Figures 10 and 11 demonstrated the impact of policy reform on the socioeconomic and racial achievement gap, respectively. For children born within two cut-off dates, changing the cut-off date reduced the extent of the achievement gap among them. However, moving the cut-off earlier appeared to neither enhance nor reduce the existing socioeconomic achievement gap in math scores, though it successfully reduced the gap in reading scores. The reform decreased the advantages of White children over non-White children, thus lessening the racial achievement gap.

7 Robustness Checks

Figure A5 illustrated that the results were not sensitive to the selection of the bandwidth. Across the range of 15 to 75, the estimated coefficient remained largely unaffected by the choice of bandwidth. I also performed an additional falsification test by examining for any discontinuity at various dates around the cut-off. However, I did not find any evidence of a discontinuity in the outcome variable on those randomly selected dates (Figure A7).

There were concerns about potential data attrition. To investigate this, I predicted the probability of missing exam scores. The results in Table A6 showed that crossing the threshold was not associated with missing values of exam scores until grade 6. However, from grade 6, there was a noticeable reduction in the likelihood of missing test scores at the cut-off point. It is important to note that these estimates, although statistically significant, were of minimal magnitude. Despite these findings, the results appeared to be relatively unresponsive to data attrition. In Figure A6, I presented the results while considering this attrition issue. The base category and the category including only cohorts with scores from all grades produced almost identical estimates. However, the estimates from the balanced panel were slightly lower, but still closely aligned with those from the base category.

8 Conclusion & Discussion

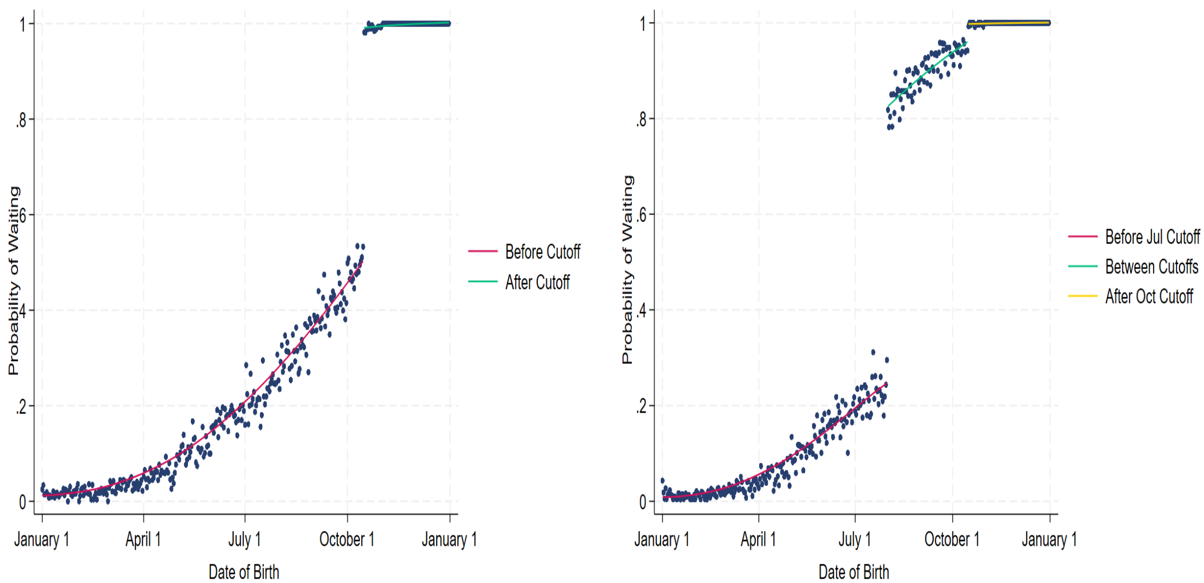
My results suggested that waiting an additional year to start kindergarten boosted the achievement test scores. In particular, this delay improved both math and reading scores by more than 0.3 standard deviations. These results aligned well with previous studies (Bedard & Dhuey, 2006; McEwan & Shapiro, 2008; Puhani & Weber, 2008; Peña, 2017). Although the advantageous impact diminished over time, it remained notably stronger compared to the findings of Elder & Lubotsky (2009) in later grades and was comparable to Cascio & Schanzenbach (2016). For example, the effect on math scores declined from 0.341 standard deviations to 0.256 standard deviations, which was quite strong and significant. Furthermore, the observed improvement was greater for students placed near the top of the score distribution as opposed to those at the lower end.

It is crucial to highlight that the effects of entry age varied among individuals. I observed a more pronounced diminishing impact among students from disadvantaged backgrounds, whereas students from advantaged backgrounds experienced the benefits of entry age for a more extended period. This observation aligned with the findings of Elder & Lubotsky (2009),and supported the notion of skill accumulation before starting kindergarten. Structural barriers disproportionately hindered students in disadvantaged groups from gaining the advantages of delayed entry (Ricks, 2022). When this policy change focused only on kindergarten entry age, it contributed to a widening achievement gap, as at-risk children were already lagging behind in terms of human capital. To narrow this gap, it was imperative to eliminate barriers and increase investments in high-quality pre-kindergarten and kindergarten programs (Ricks, 2022; Cannon & Lipscomb, 2008). Considering the substantial financial burden on disadvantaged households associated with holding back children for a year, the policy has not proven successful in reducing the achievement gap in their favor (Gelbach, 2002; Cascio, 2006).

Although advancing the kindergarten cut-off date was likely to improve academic achievement, policymakers needed to weigh the associated costs carefully, particularly for disadvantaged households. Instead of just delaying entry, it was crucial to explore potential connections between school entry age, childcare costs, and the achievement gap to formulate a more effective and equitable policy for everyone.

9 Figure

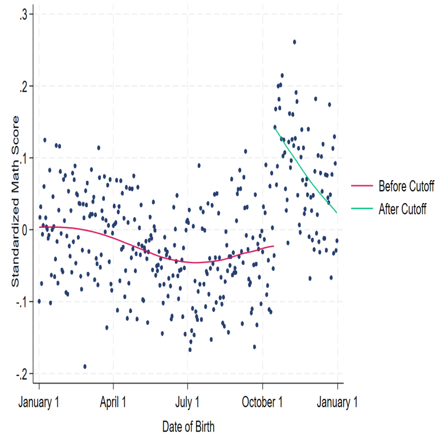
Figure 1: 1st Stage: Before and after policy change



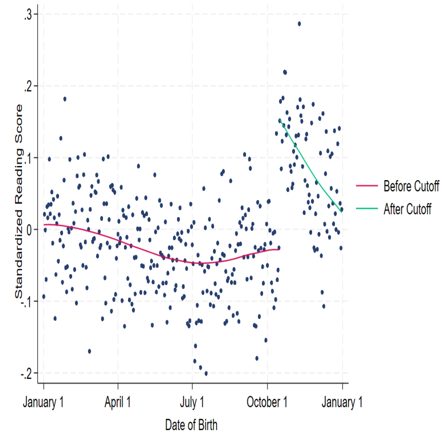
Panel A: Before policy change | Panel B: After policy change

Notes: This figure presents a pattern in waiting over the date of birth . Panel A displays the plot of the first stage before the policy reform, whereas Panel B represents the plot after the reform. Both scatter plots illustrate the likelihood of delaying entry into kindergarten based on birthdate, along with their corresponding best-fit lines. In both graphs, points are daily averages. Waiting is defined as one if an individual's school entry is delayed from the year in which he turns 5 to the year he turns 6. The sample consists of all students who started kindergarten between 57 and 79 months old, for whom I have access to the year of kindergarten entry.

Figure 2: Reduced-form relation: Before and after policy change

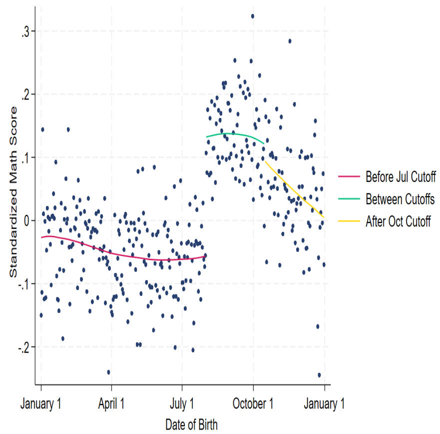


(i) Math score

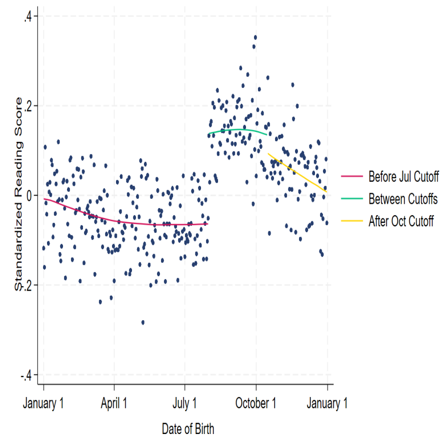


(ii) Reading score

Panel A: Before policy change



(i) Math score

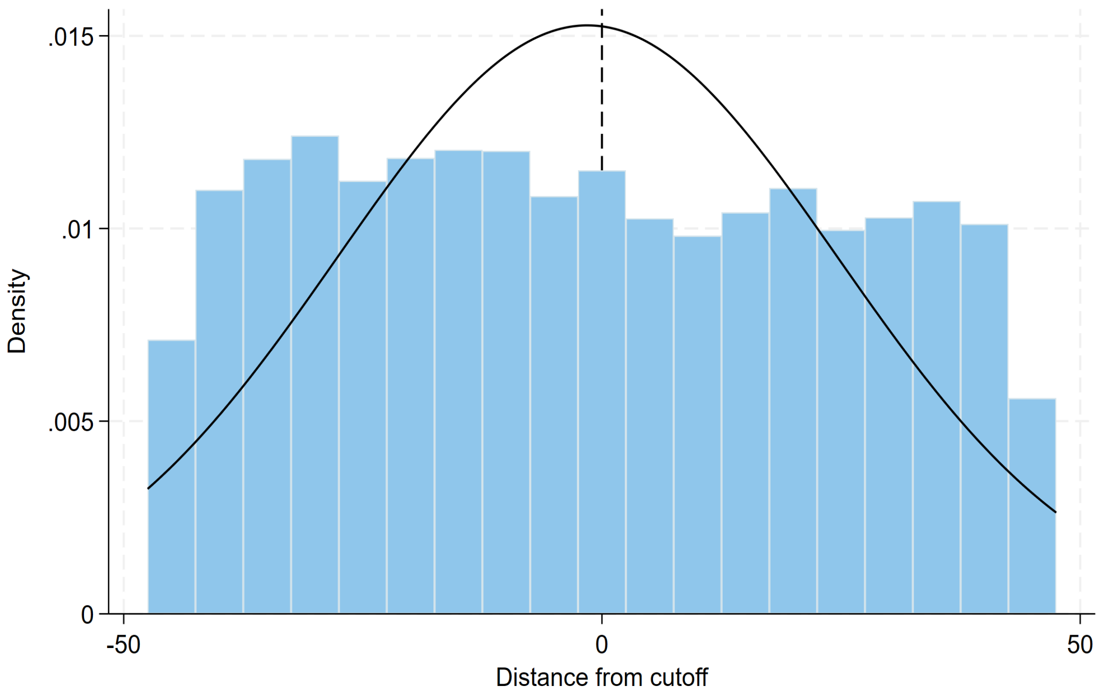


(ii) Reading score

Panel B: After policy change

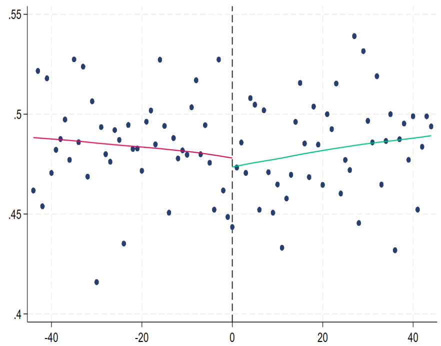
Notes: This figure presents a pattern in standardized test scores of grade 3 over the date of birth, along with their corresponding best-fit lines. Panel A displays the plot of the reduced form before the policy reform, whereas Panel B represents the plot after the reform. In both panels, (i) shows a pattern in math test scores, and (ii) shows a pattern in reading test scores. In both graphs, points are daily averages. All the scores are standardized by assessment name, grade, and year. The sample consists of all students who started kindergarten between 57 and 79 months old, for whom I have access to the year of kindergarten entry.

Figure 3: Histogram of date of birth

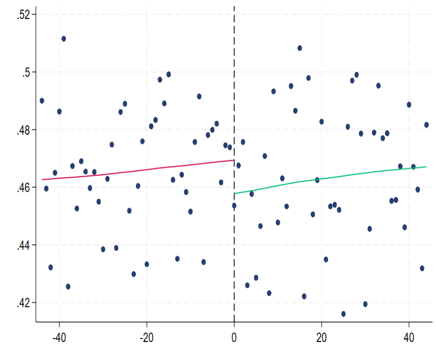


Notes: Histogram of the date of birth of sample students born 45 days around the cutoff. Separate columns are shown for each bin, where bin width is 5 days. The sample consists of all students who started kindergarten between 57 and 79 months old, for whom I have access to the year of kindergarten entry.

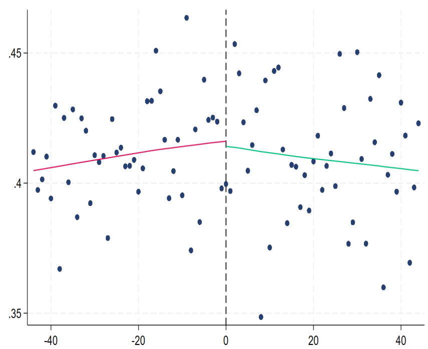
Figure 4: Covariate balance test



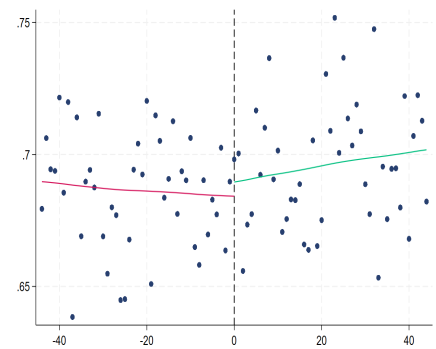
(a) Female



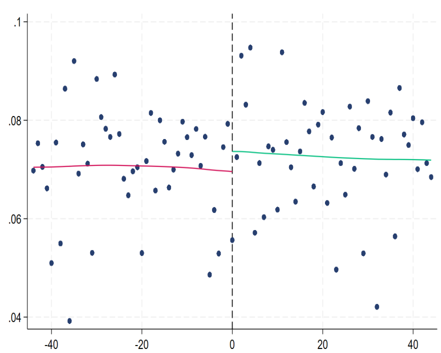
(b) FRL Eligible



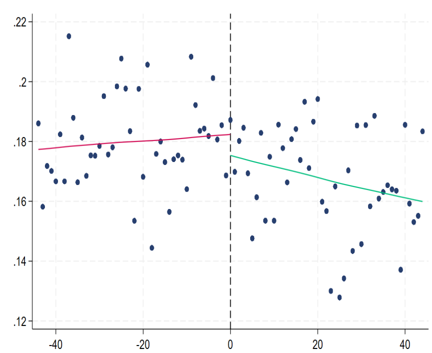
(c) Metro



(d) White



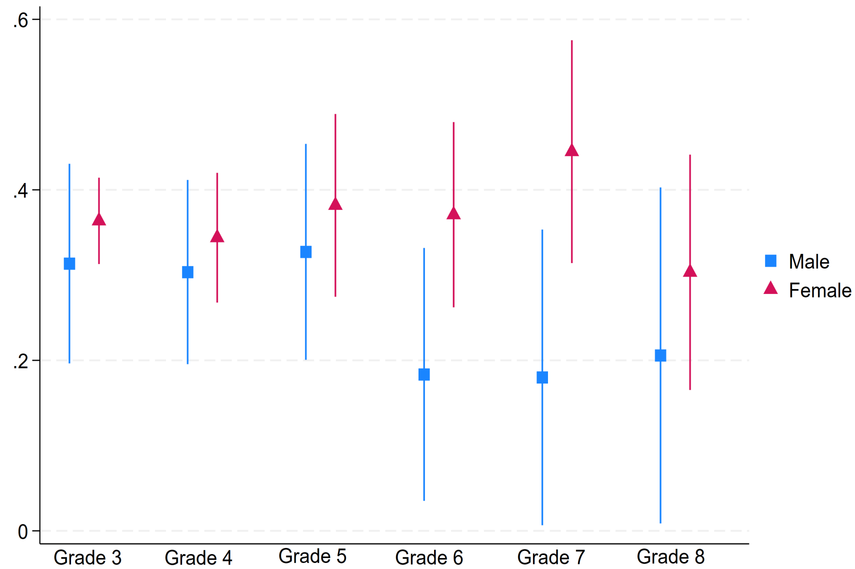
(e) Black



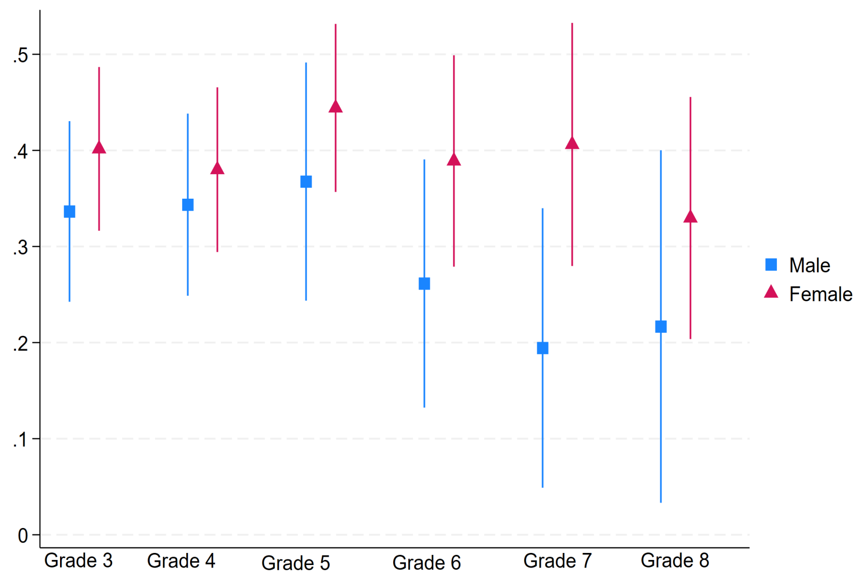
(f) Hispanic

Notes: Scatter plots represent means of demographic characteristics by distance relative to the cut-off. 45 days of bandwidth has been taken. All points indicate a daily average of relevant covariates. Lines are local polynomial smooth plots of fitted values.

Figure 5: Estimates by grades: Gender wise



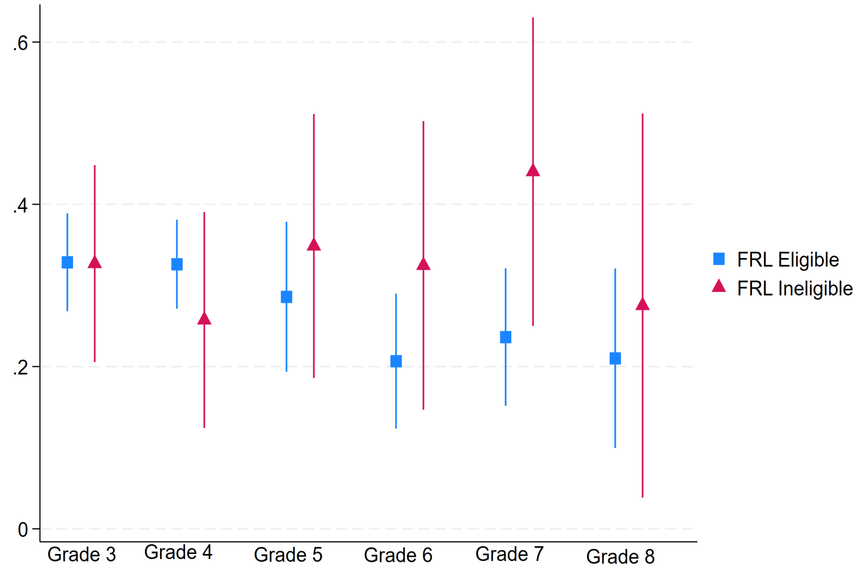
Panel A: Standardized math score by grade



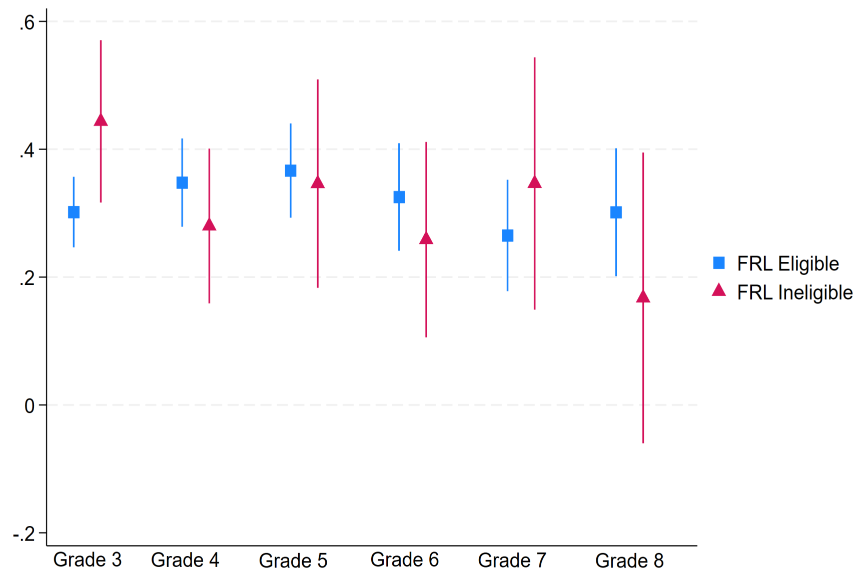
Panel B: Standardized reading score by grade

Notes: All treatment effects were estimated separately for male and female students using the 2SLS specification, considering 45 days of bandwidth around the cut-off. The treatment is waiting for an additional year to start kindergarten. Standard errors are clustered at the school district level. All regressions are run without controls. Test scores are standardized by assessment system, year, and grade.

Figure 6: Estimates by grades: FRL eligibility wise



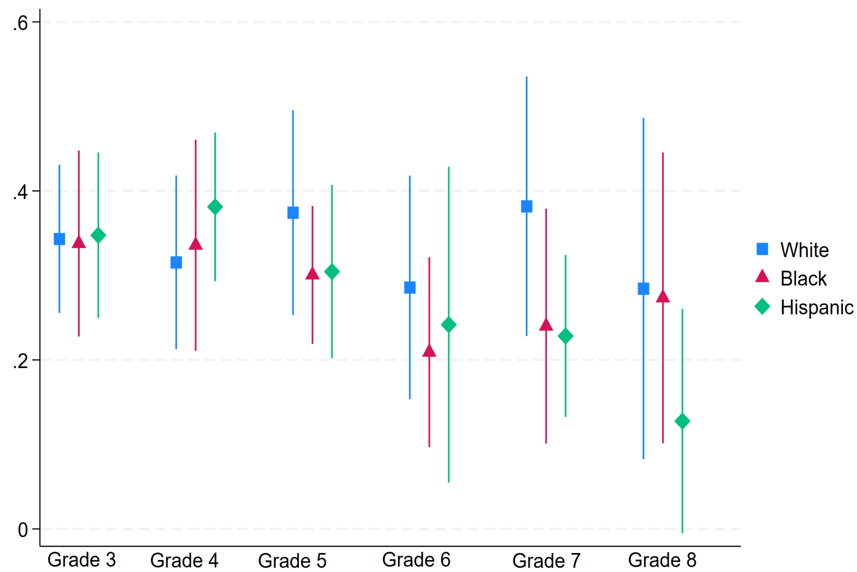
Panel A: Standardized math score by grade



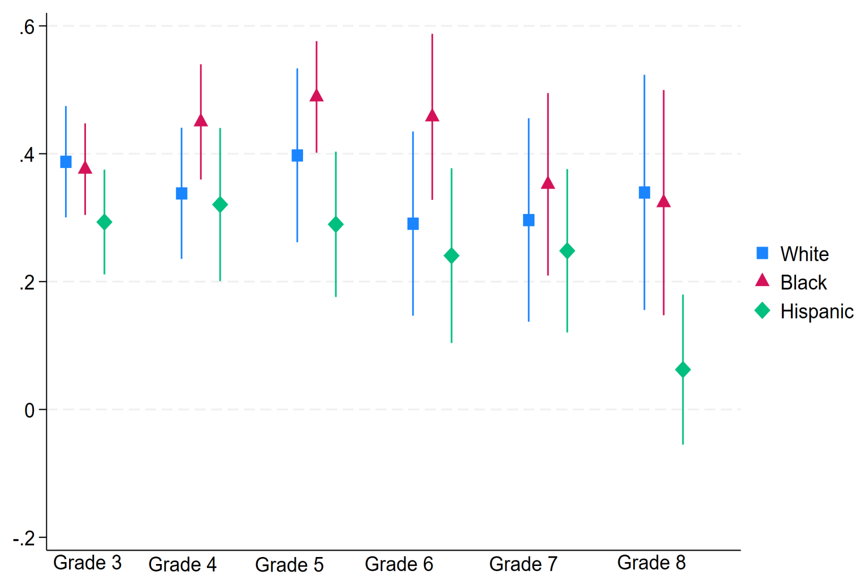
Panel B: Standardized reading score by grade

Notes: All treatment effects were estimated separately for FRL eligible and FRL ineligible students using the 2SLS specification, considering 45 days of bandwidth around the cut-off. Treatment is waiting for an additional year to start kindergarten. Standard errors are clustered at the school district level. All regressions are run without controls. Test scores are standardized by assessment system, year, and grade.

Figure 7: Estimates by grades: Across races



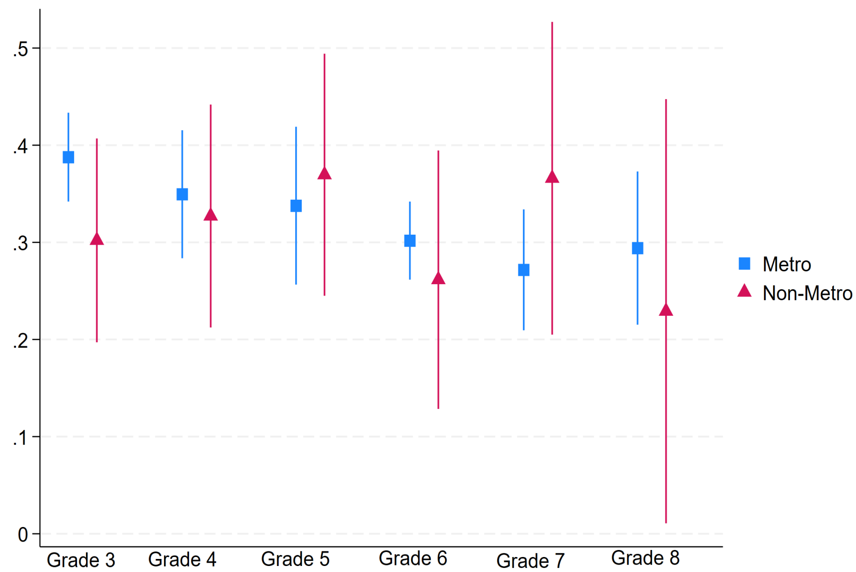
Panel A: Standardized math score by grade



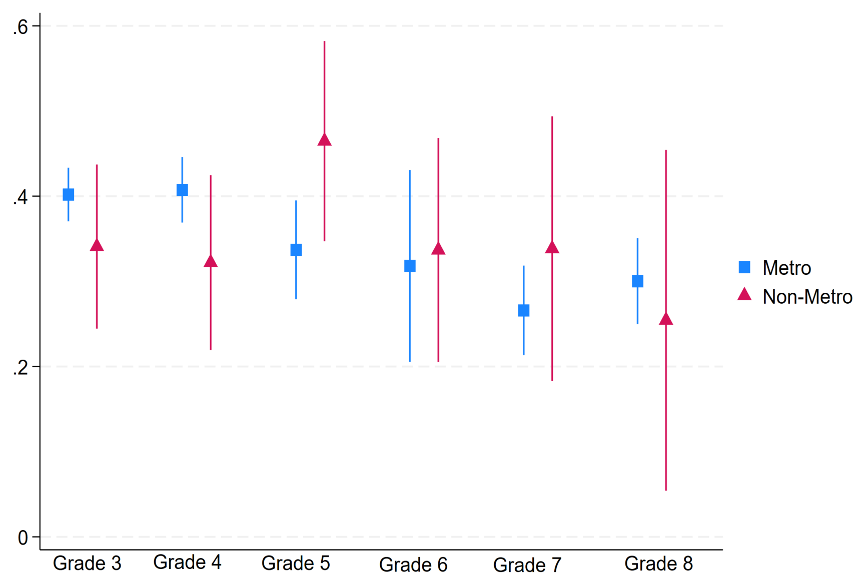
Panel B: Standardized reading score by grade

Notes: All treatment effects were estimated separately for White, Black, and Hispanic students using the 2SLS specification, considering 45 days of bandwidth around the cut-off. Treatment is waiting for an additional year to start kindergarten. Standard errors are clustered at the school district level. All regressions are run without controls. Test scores are standardized by assessment system, year, and grade.

Figure 8: Estimates by grades: Location wise



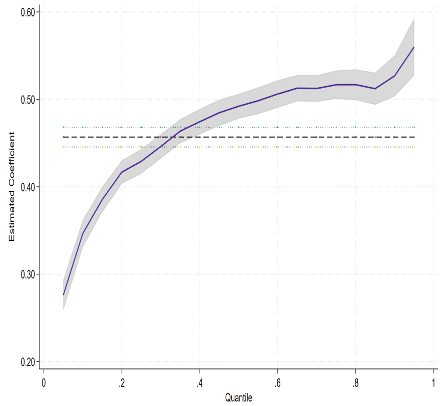
Panel A: Standardized math score by grade



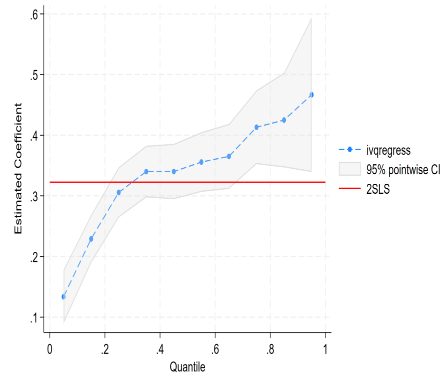
Panel B: Standardized reading score by grade

Notes: All treatment effects were estimated separately for students living in metropolitan and non-metropolitan areas using the 2SLS specification, considering 45 days of bandwidth around the cut-off. Treatment is waiting for an additional year to start kindergarten. Standard errors are clustered at the school district level. All regressions are run without controls. Test scores are standardized by assessment system, year, and grade. Only Lincoln and Omaha are considered metropolitan areas in this analysis.

Figure 9: Quantile regression estimates

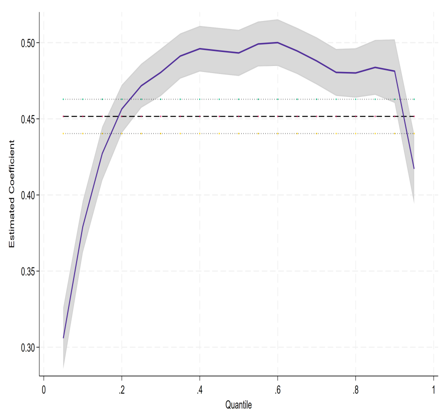


(i) QR estimates

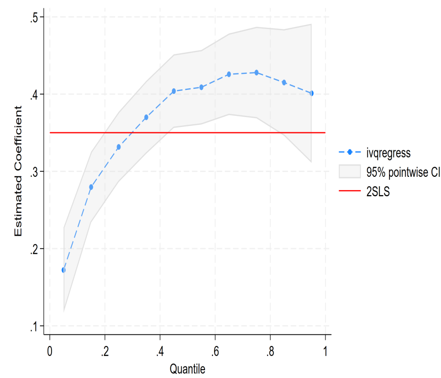


(ii) IVQR estimates

Panel A: Standardized math score



(i) QR estimates

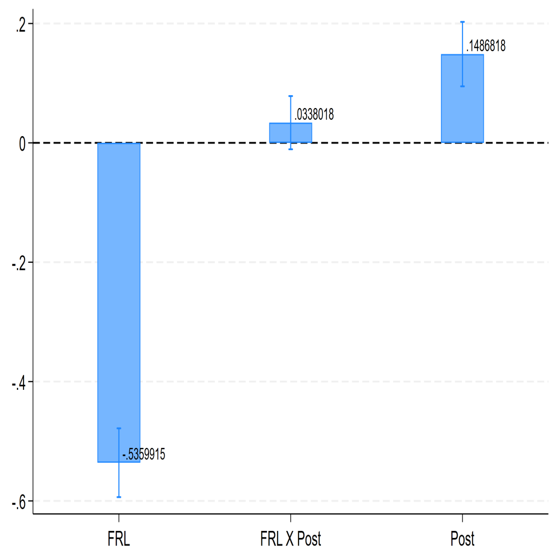


(ii) IVQR estimates

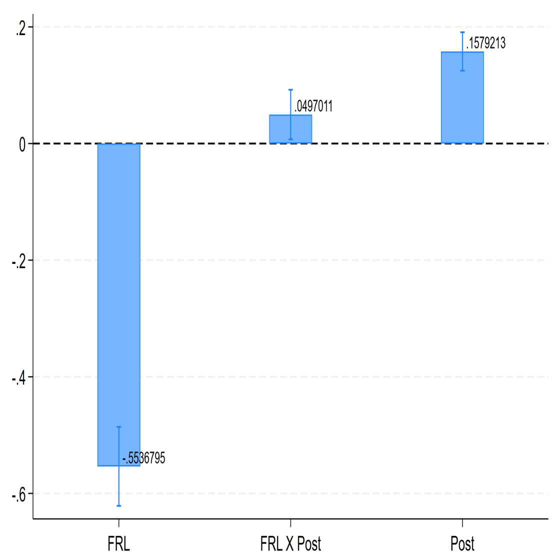
Panel B: Standardized reading score

Notes: This figure represents QR and IVQR estimates for standardized test scores. Panel A displays estimates for math scores and panel B displays estimates for reading scores. In both panels, (i) shows estimates from quantile regression (OLS) and (ii) shows estimates from IVQR. Coefficient estimates are on the vertical axis, and the quantile index is on the horizontal axis. Treatment is waiting for an additional year to start kindergarten. All regressions are run without controls. The shaded region indicates 95% confidence interval. Robust standard errors are used for inference purposes.

Figure 10: Socioeconomic achievement gap in test scores



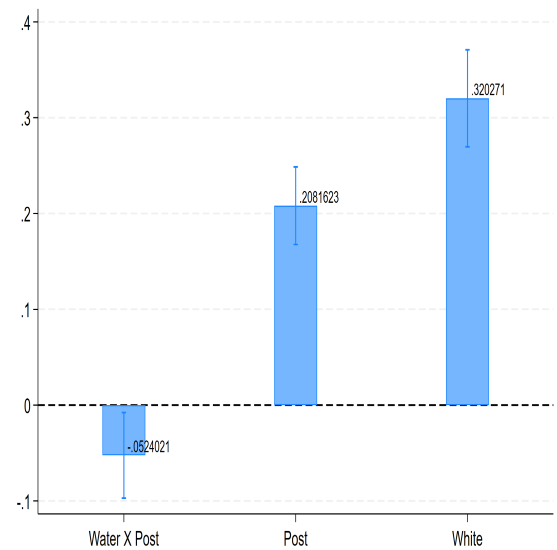
Panel A: Math score



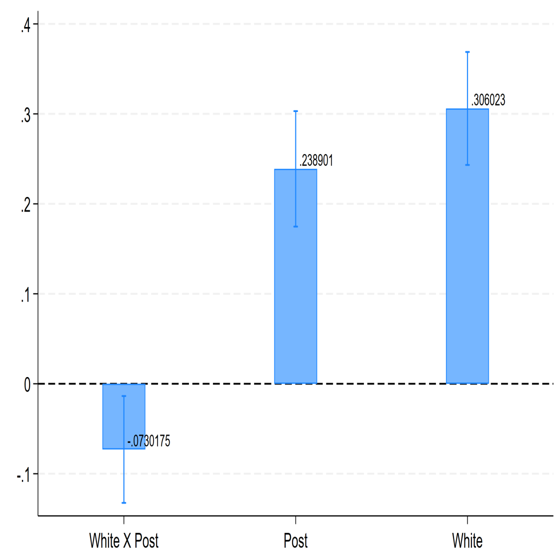
Panel B: Reading score

Notes:Both panels show estimates from equation 8. The shaded region indicates a 95% confidence interval. The sample sizes for Panel A and Panel B are 161,421 and 160,906, respectively.

Figure 11: Racial achievement gap in test scores



Panel A: Math score



Panel B: Reading score

Notes: Both panels show estimates from equation 9. The shaded region of (ii) indicates a 95% confidence interval. he sample sizes for Panel A and Panel B are 158,066 and 157,564, respectively.

10 Table

Table 1: Descriptive statistics

	Before change in cut-off		After change in cut-off	
	Before Oct cut-off	After Oct cut-off	Before Jul cut-off	After Jul cut-off
Age in months	63.74	67.82	64.05	68.39
	(4.33)	(1.09)	(3.51)	(2.66)
Female	0.49	0.48	0.48	0.48
	(0.50)	(0.50)	(0.50)	(0.50)
FRL eligible	0.46	0.47	0.48	0.47
	(0.50)	(0.50)	(0.50)	(0.50)
EL eligible	0.13	0.12	0.14	0.12
	(0.33)	(0.32)	(0.34)	(0.33)
White	0.71	0.71	0.67	0.67
	(0.46)	(0.46)	(0.47)	(0.47)
Black	0.07	0.08	0.07	0.07
	(0.26)	(0.27)	(0.25)	(0.25)
Hispanic	0.17	0.16	0.19	0.18
	(0.37)	(0.37)	(0.39)	(0.39)
Asian	0.02	0.02	0.02	0.02
	(0.14)	(0.14)	(0.15)	(0.15)
Metro	0.41	0.41	0.41	0.41
	(0.49)	(0.49)	(0.49)	(0.49)
Observations	84,662	21,245	51,065	34,428

Notes: The sample consists of public-school students in Nebraska who started kindergarten between 2007-2008 and 2015-2016. All students are included in the sample, disregarding the choice of bandwidth. All the students in the sample have started kindergarten at the age of 57 to 79 months. Total number of observations is 191,400.

Table 2: Validity of the regression discontinuity design

	Parametric estimation		Non-parametric estimation	
	BW=45	BW=30	BW=45	BW=30
Female	-0.000827	-0.00529	-0.01005	-0.00949
	(0.00930)	(0.0114)	(0.01006)	(0.01231)
FRL Eligible	-0.0101	-0.0157	-0.01637	-0.01779
	(0.00928)	(0.0114)	(0.01005)	(0.0123)
White	0.00541	-0.0103	0.00479	0.00593
	(0.00860)	(0.0106)	(0.00935)	(0.01145)
Black	0.00531	0.0126**	0.00426	0.00544
	(0.00480)	(0.00592)	(0.00519)	(0.00633)
Hispanic	-0.00883	-0.00267	-0.00516	-0.00634
	(0.00705)	(0.00869)	(0.00771)	(0.00945)
Metro	-0.00118	0.00433	-0.0023	0.00232
	(0.00916)	(0.0113)	(0.00992)	(0.01215)

Notes: Each covariate is regressed on the indicator for crossing the cut-off, distance from the cut-off, and interaction between the indicator for crossing the cut-off and distance from the cut-off following equation 2. Here, "BW=45" and "BW=30" specifications use observations within 45 and 30 days around the cut-off, respectively. Non-parametric estimation uses local polynomial RD point estimators with robust bias-corrected confidence intervals and inference procedures developed by Calonico et al. (2014).

Table 3: Estimates using different methods

	Non-parametric estimation		Parametric estimation	
	Math score			
	BW=45	BW=30	BW=45	BW=30
All Grades	0.278***	0.242***	0.323***	0.242***
	(0.0236)	(0.0409)	(0.0286)	(0.0409)
Grade 3	0.305***	0.275***	0.341***	0.257***
	(0.045)	(0.060)	(0.035)	(0.040)
Grade 4	0.291***	0.291***	0.338***	0.272***
	(0.067)	(0.067)	(0.0352)	(0.0447)
Grade 5	0.302***	0.272**	0.355***	0.298***
	(0.0566)	(0.0759)	(0.0406)	(0.0563)
Grade 6	0.250***	0.220**	0.280***	0.194***
	(0.0634)	(0.0852)	(0.0381)	(0.0517)
Grade 7	0.262***	0.218**	0.318***	0.210***
	(0.0708)	(0.0958)	(0.0515)	(0.0750)
Grade 8	0.172**	0.115	0.256	0.130
	(0.0823)	(0.111)	(0.0607)	(0.0821)
	Reading score			
	BW=45	BW=30	BW=45	BW=30
All Grades	0.3178***	0.2941 ***	0.3501***	0.2884***
	(0.0235)	(0.0314)	(0.0258)	(0.0406)
Grade 3	0.3397***	0.3244***	0.3684***	0.3077***
	(0.045)	(0.0596)	(0.0305)	(0.0409)
Grade 4	0.3498***	0.3448***	0.3613***	0.3171***
	(0.0504)	(0.0673)	(0.0309)	(0.0441)
Grade 5	0.370***	0.348**	0.405***	0.363***
	(0.0561)	(0.075)	(0.040)	(0.0623)
Grade 6	0.308***	0.278**	0.3279***	0.281***
	(0.0637)	(0.0855)	(0.0434)	(0.0519)
Grade 7	0.259***	0.244**	0.302***	0.224***
	(0.070)	(0.0949)	(0.0466)	(0.0755)
Grade 8	0.1817**	0.0778	0.273***	0.111
	(0.0811)	(0.1094)	(0.0545)	(0.0869)

Notes: Each outcome is regressed based on the fuzzy regression discontinuity design. All the treatment effects are estimated through the parametric approach that use the 2SLS specification. Non-parametric estimation utilizes local polynomial RD point estimators with robust bias-corrected confidence intervals and inference procedures developed by Calonico et al. (2014). Here, both approaches use either ”BW=45” or ”BW=30” bandwidths with observations within 45 and 30 days around the cut-off, respectively.

Table 4: Impact of waiting on standardized scores

			Math Score				
Model	All Grades	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
			Panel A: No Controls				
OLS	0.457***	0.479***	0.466***	0.445***	0.432***	0.490***	0.429***
	(0.0400)	(0.0339)	(0.0398)	(0.0367)	(0.0461)	(0.0630)	(0.0662)
2SLS	0.323***	0.341***	0.338***	0.355***	0.280***	0.318***	0.256***
	(0.0286)	(0.0352)	(0.0352)	(0.0406)	(0.0381)	(0.0515)	(0.0607)
F-Stat	165.774	255.004	217.128	176.365	139.714	92.006	85.693
			Panel B: Individual level controls				
2SLS	0.296***	0.331***	0.304***	0.310***	0.247***	0.308***	0.235***
	(0.0259)	(0.0344)	(0.0313)	(0.0382)	(0.0387)	(0.0469)	(0.0565)
F-Stat	164.172	249.507	215.173	176.316	138.934	90.516	84.520
			Panel C: Individual level controls + School-district FE				
2SLS	0.284***	0.331***	0.300***	0.295***	0.230***	0.272***	0.204***
	(0.0245)	(0.0338)	(0.0305)	(0.0362)	(0.0378)	(0.0417)	(0.0530)
F-stat	168.831	252.741	220.556	183.094	145.729	91.840	86.316
Observations	187,746	45,183	39,279	33,468	27,822	23,508	18,486
			Reading Score				
Model	All Grades	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
			Panel A: No Controls				
OLS	0.452***	0.464***	0.462***	0.451***	0.438***	0.470***	0.428***
	(0.0387)	(0.0346)	(0.0365)	(0.0344)	(0.0430)	(0.0591)	(0.0598)
2SLS	0.350***	0.368***	0.361***	0.405***	0.328***	0.302***	0.273***
	(0.0258)	(0.0305)	(0.0309)	(0.0400)	(0.0434)	(0.0466)	(0.0545)
F-stat	166.567	255.795	216.044	179.087	140.834	92.037	85.949
			Panel B: Individual level controls				
2SLS	0.324***	0.358***	0.326***	0.361***	0.294***	0.293***	0.252***
	(0.0254)	(0.0310)	(0.0314)	(0.0379)	(0.0447)	(0.0448)	(0.0511)
F-stat	164.574	250.602	212.381	177.236	139.838	91.093	84.663
			Panel C: Individual level controls + School-district FE				
2SLS	0.319***	0.357***	0.333***	0.348***	0.281***	0.279***	0.244***
	(0.0237)	(0.0308)	(0.0292)	(0.0344)	(0.0465)	(0.0421)	(0.0477)
F-stat	169.247	253.843	217.702	183.808	146.650	92.535	86.516
Observations	187,117	45,073	39,163	33,367	27,730	23,408	18,376

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: All treatment effects were estimated using the 2SLS specification. Treatment is waiting for an additional year to start kindergarten. Test scores are standardized by assessment system, year, and grade. Individual-level controls are female, FRL eligibility, White, Black, and Hispanic. 45 days of bandwidth has been used here for selecting observations close to the cutoff. Standard errors are clustered at the school-district level.

Table 5: Impact of waiting on standardized score: Heterogeneity analysis

	Math score		Reading score	
	Sub-sample	Interaction	Sub-sample	Interaction
	Panel A: Female			
Waiting	0.374***	0.292***	0.396***	0.310***
	(0.0394)	(0.0359)	(0.0361)	(0.0330)
Waiting x Female		0.0608**		0.0804***
		(0.0301)		(0.0290)
Female		-0.0725***		0.129***
		(0.0242)		(0.0236)
Observationss	90,856	187,746	90,613	187,117
	Panel B: FRL Eligible			
Waiting	0.280***	0.336***	0.321***	0.355***
	(0.0221)	(0.0406)	(0.0238)	(0.0368)
Waiting x FRL		-0.0643**		-0.0498*
		(0.0296)		(0.0284)
FRL		-0.627***		-0.631***
		(0.0615)		(0.0618)
Observations	85,773	187,746	85,394	187,117
	Panel C: White			
Waiting	0.334***	0.279***	0.352***	0.320***
	(0.0478)	(0.0226)	(0.0439)	(0.0247)
Waiting x white		0.0692**		0.0310
		(0.0281)		(0.0311)
White		0.558***		0.541***
		(0.0603)		(0.0636)
Observations	132,134	183,806	131,757	183,194
	Panel D: Metro			
Waiting	0.331***	0.324***	0.349***	0.358***
	(0.00950)	(0.0378)	(0.00562)	(0.0363)
Waiting x metro		-0.00326		-0.0162
		(0.0322)		(0.0305)
Metro		-0.0359		0.0191
		(0.174)		(0.164)
Observations	74,180	187,746	73,867	187,117

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All treatment effects were estimated using the 2SLS specification considering 45 days of bandwidth around the cutoff. All the regressions are run without any controls. Standard errors are clustered at the school-district level. Treatment is waiting for an additional year to start kindergarten. Test scores are standardized by assessment system, year, and grade. Panel C regressions exclude Asian from the sample.

Table 6: Impact of waiting on standardized score: Quantile approach

Outcome	Conditional quantile treatment effects				
variable	0.10	0.30	0.50	0.70	0.90
Panel A	Quantile regression				
Math score	0.347***	0.446***	0.492***	0.512***	0.527***
(0.00824)	(0.00723)	(0.00740)	(0.00778)	(0.0113)	
Reading score	0.379***	0.480***	0.493***	0.488***	0.481***
(0.00899)	(0.00785)	(0.00754)	(0.00768)	(0.0106)	
Panel B	IVQR				
Math Score	0.188***	0.332***	0.353***	0.394***	0.433***
	(0.0205)	(0.0211)	(0.0242)	(0.0284)	(0.0452)
Reading Score	0.213***	0.351***	0.403***	0.441***	0.407***
	(0.0239)	(0.0230)	(0.0241)	(0.0288)	(0.0421)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Each row indicates a separate regression. All treatment effects were estimated considering 45 days of bandwidth around the cut-off. All the regressions are run without any controls. Treatment is waiting for an additional year to start kindergarten. Panels A and B show results using OLS and 2SLS specifications, respectively. IVQR estimates are obtained using smoothed estimating equations (SEE) outlined in Kaplan & Sun (2017). Test scores are standardized by assessment type, grade, and year.

Table 7: Difference-in-difference estimates of the impact of policy change on achievement gap

	Panel A: SES achievement gap	
	Math score	Reading score
FRL	-0.536***	-0.554***
	(0.0293)	(0.0344)
FRL X Post	0.0338	0.0497**
	(0.0226)	(0.0216)
Post	0.149***	0.158***
	(0.0274)	(0.0167)
Year FE	Yes	Yes
School-district FE	Yes	Yes
Observations	161,421	160,906
R-squared	0.202	0.202
	Panel B: Racial achievement gap	
	Math score	Reading score
White	0.320***	0.306***
	(0.0257)	(0.0319)
White x Post	-0.0524**	-0.0730**
	(0.0227)	(0.0302)
Post	0.208***	0.239***
	(0.0206)	(0.0326)
Year FE	Yes	Yes
School-district FE	Yes	Yes
Observations	158,066	157,564
R-squared	0.150	0.147

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows difference-in-difference estimates of the effect of policy change on the achievement gap. Post is a dummy variable taking a value of 1 for KG entry years starting from 2012 and 0 otherwise. FRL is a dummy variable taking the value of 1 if the child is FRL eligible. Non-White is also a dummy, equal to 1 if the child is non-White but not Asian. All the regressions are run with controls (e.g. gender and races). Standard errors are clustered at the school-district level. Test scores are standardized by assessment system, year, and grade. Panel B regressions exclude Asian from the sample.

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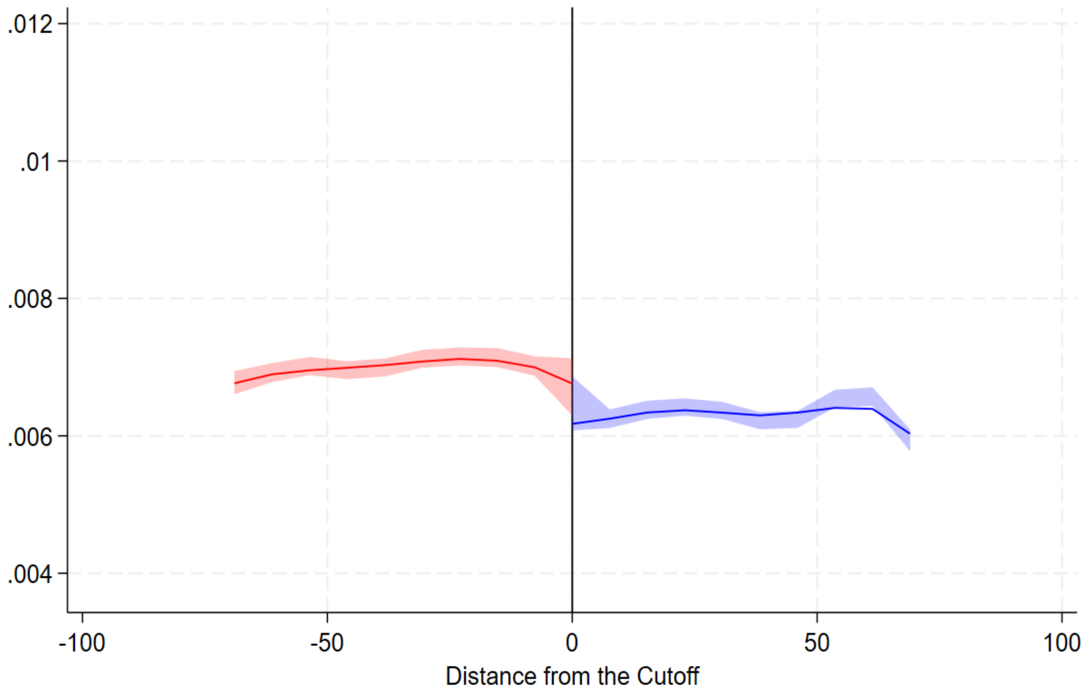
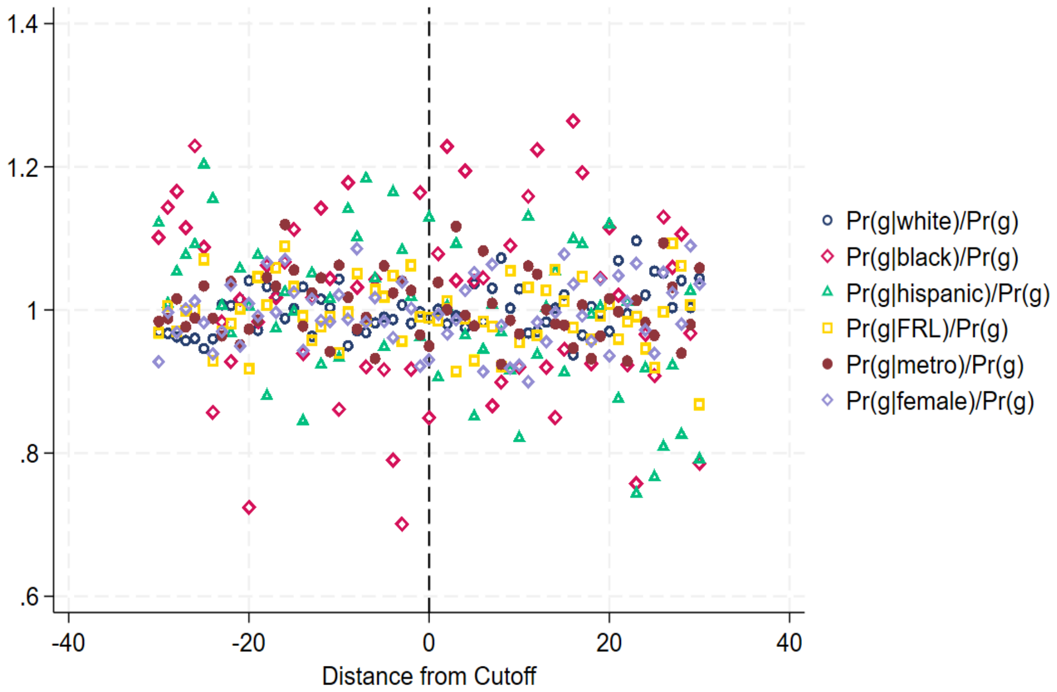
Figure A1: Manipulation test



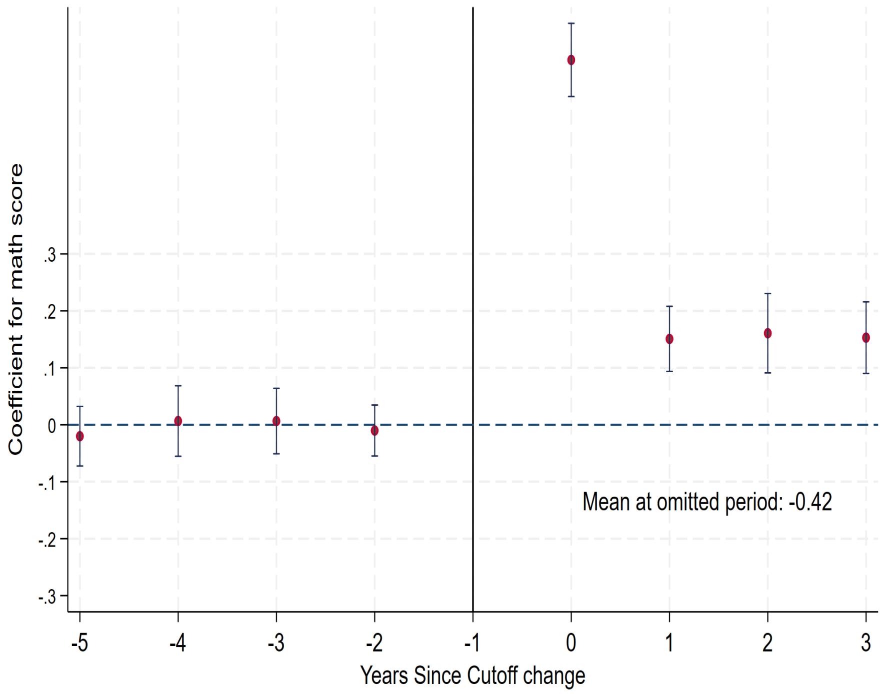
Notes: rddensity plot with local quadratic approximation producing T statistic=0.3095 (p=0.7569), implying no statistical evidence of systematic manipulation of the running variable.

Figure A2: Conditional density test

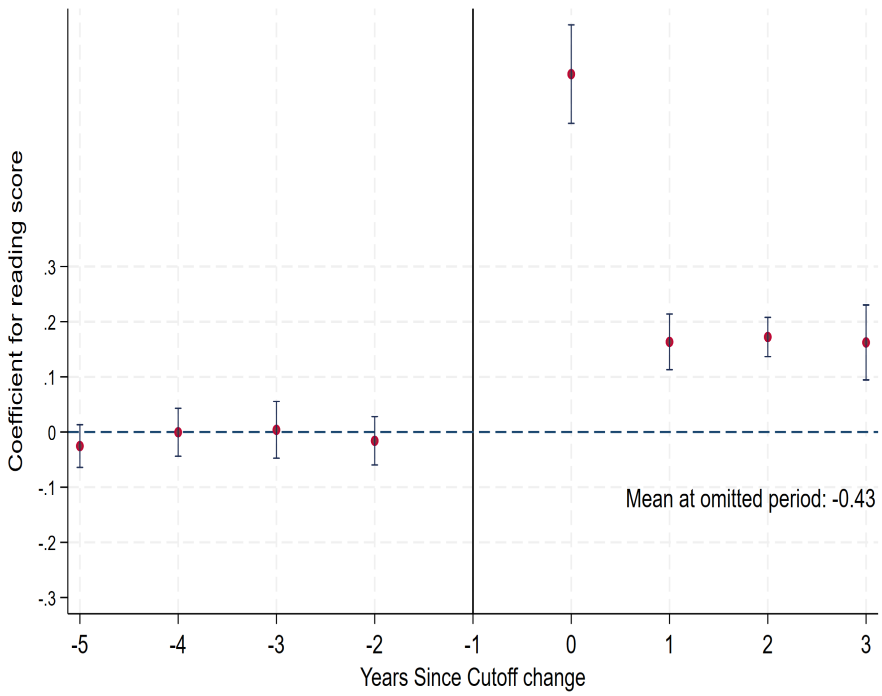


Notes: These are ratios of conditional to unconditional birthdate densities by the distance from the cut-off for six groups: White, Black, Hispanic, female, FRL-eligible students, and students living in metropolitan areas. Lincoln and Omaha are considered metropolitan areas. Densities are calculated with a bin width of 1 day.

Figure A3: Effects of KG cut-off change on standardized scores of FRL eligible children



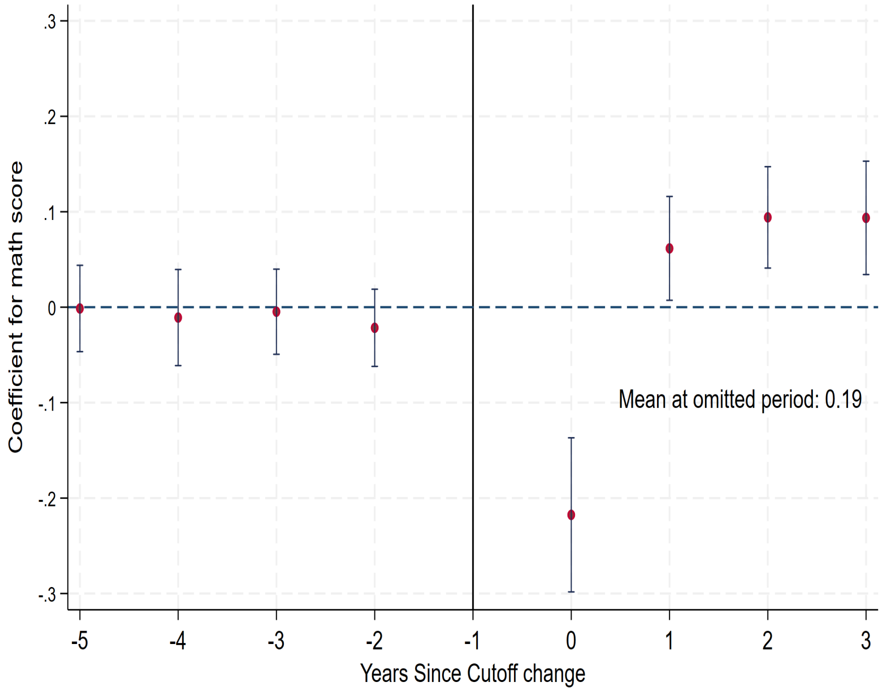
Panel A: Event study: Math score



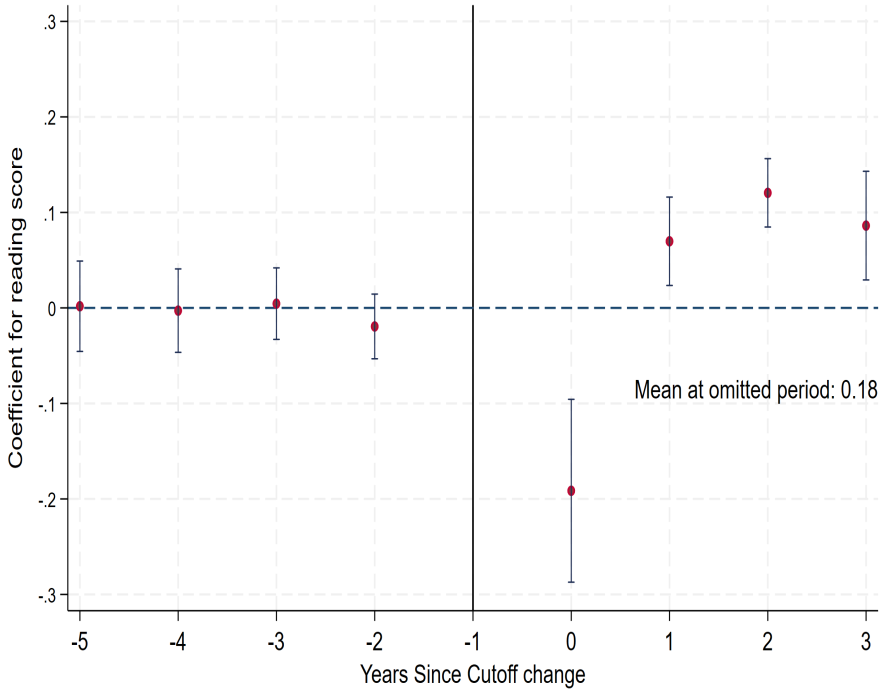
Panel B: Event study: Reading Score

Notes: This figure plots the estimates of the effect of the KG cut-off change on the standardized scores of FRL eligible children. The figure depicts the coefficients and 95% confidence intervals. Test scores are standardized by assessment type, grade, and year. Standard errors are clustered at the school-district level.

Figure A4: Effects of KG cut-off change on standardized scores of White children



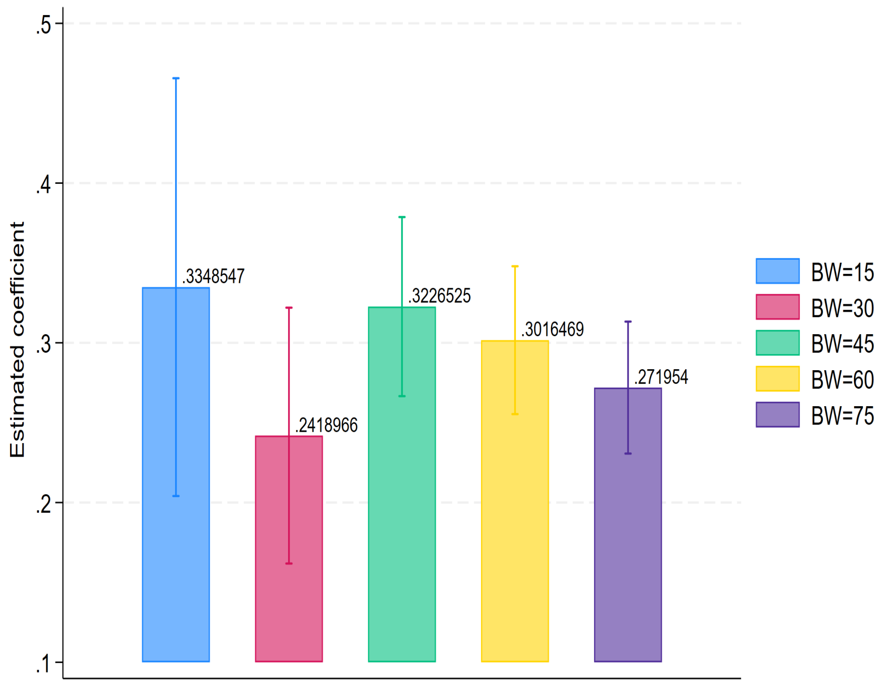
Panel A: Event study: Math score



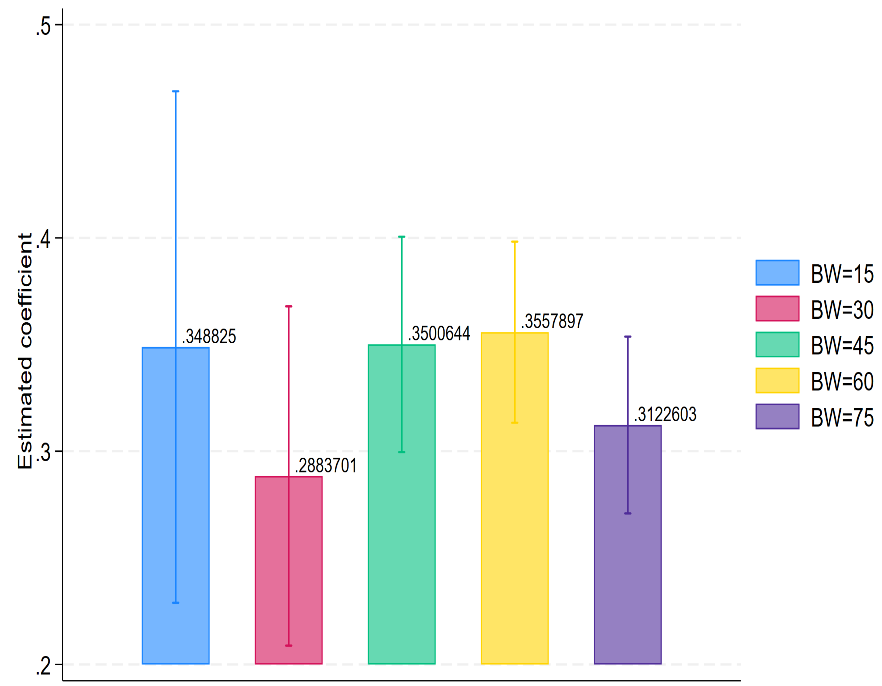
Panel B: Event study: Reading Score

Notes: This figure plots the estimates of the effect of the KG cut-off change on the standardized scores of White children. The figure depicts the coefficients and 95% confidence intervals. Test scores are standardized by assessment type, grade, and year. Standard errors are clustered at the school-district level.

Figure A5: Checking bandwidth sensitivity



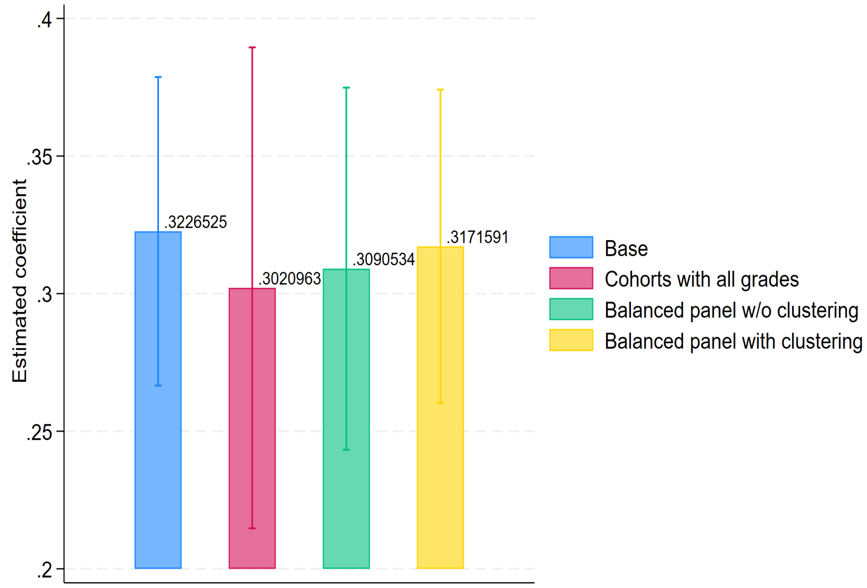
Panel A: Bandwidth sensitivity: Math score



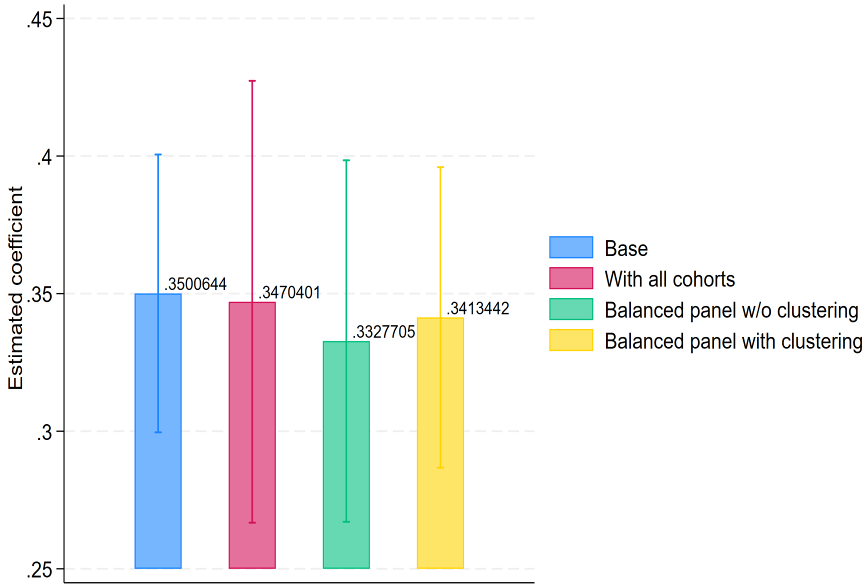
Panel B: Bandwidth sensitivity: Reading Score

Notes: Each bar indicates the estimated coefficient from a separate regression with different bandwidths using the 2SLS specification. The treatment is waiting for an additional year to start kindergarten. These regressions do not include any controls. Test scores are standardized by assessment type, grade, and year. Standard errors are clustered at the school-district level.

Figure A6: Checking sensitivity to attrition



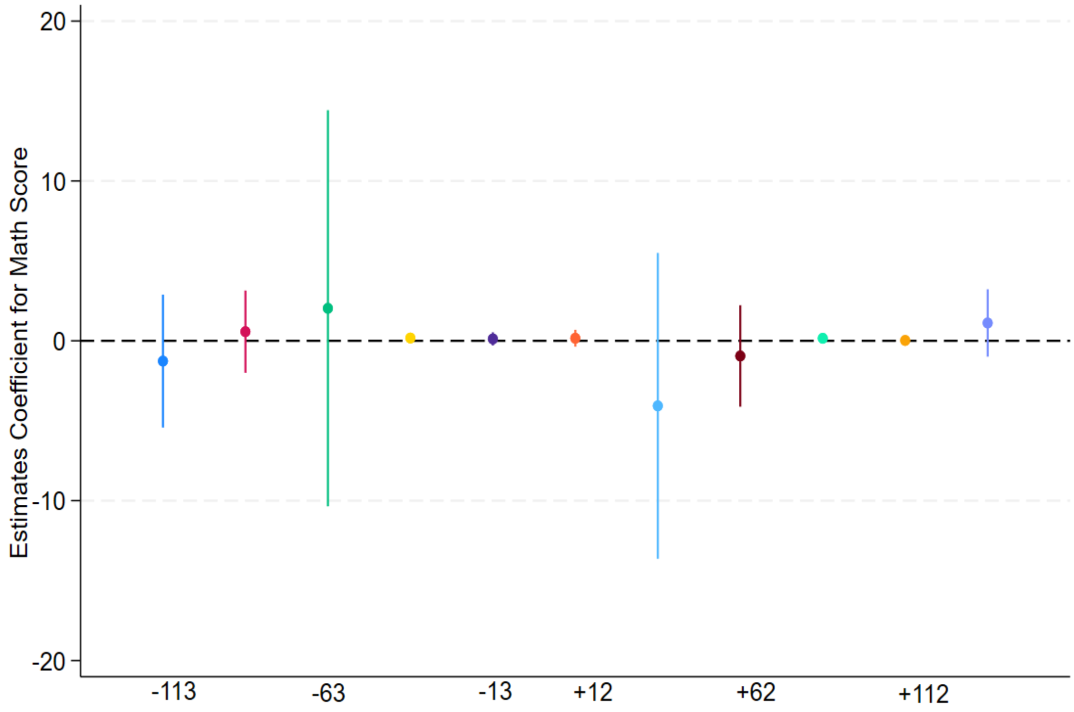
Panel A: Sensitivity to attrition: Math score



Panel B: Sensitivity to attrition: Reading score

Notes: Each bar indicates the estimated coefficient from a separate regression considering 45 days of bandwidth around the cutoff using a 2SLS specification. Each represents a coefficient obtained from running a regression with a different sample. The blue bar represents the base category. The pink bar shows regression results for cohorts with scores from all grades, particularly children who started kindergarten in 2007-08 and 2008-09. The green bar portrays results for a balanced panel without clustering, while the yellow bar incorporates clustering at the school district level. Treatment is waiting for an additional year to start kindergarten. These regressions do not include any controls. Test scores are standardized by assessment type, grade, and year.

Figure A7: Checking sensitivity to different cut-offs



Notes: The figure shows the estimated coefficient for math score at pseudo cutoff around the original cutoff using the 2SLS specification. All coefficients are estimated considering a 45-day bandwidth. The treatment is waiting for an additional year to start kindergarten. These regressions do not include any controls. Test scores are standardized by assessment type, grade, and year. Standard errors are clustered at the school-district level.

Table A1: Descriptive statistics

	Before change in cut-off		After change in cut-off	
	Before Oct cut-off	After Oct cut-off	Before Jul cut-off	After Jul cut-off
Waiting	0.18	1	0.09	0.95
	(0.38)	(0.05)	(0.29)	(0.22)
Gifted	0.17	0.20	0.11	0.14
	(0.38)	(0.40)	(0.31)	(0.34)
Special Educ	0.24	0.22	0.24	0.22
	(0.43)	(0.42)	(0.43)	(0.41)
Repetition	0.06	0.02	0.03	0.01
	(0.24)	(0.13)	(0.18)	(0.10)
Observations	84,662	21,245	51,065	34,428

Notes: The sample consists of public school students of Nebraska who started kindergarten from 2007-08 to 2015-16. All the students in the sample started kindergarten at the age of 57 to 79 months. All the scores are standardized by assessment name, grade, and year. Gifted, Special Educ, and Repetition are coded as one if this identification occurs by grade 4. The total number of observations is 191,400.

Table A2: Summary statistics by quantiles

Panel A: Standardized math score							
Score interval	Math score	Female	FRL eligible	White	Black	Hispanic	Metro
Q1	-1.156	0.488	0.716	0.493	0.155	0.273	0.475
	(0.373)	(0.50)	(0.451)	(0.50)	(0.362)	(0.445)	(0.50)
Q2	-0.354	0.507	0.530	0.667	0.068	0.207	0.381
	(0.178)	(0.50)	(0.50)	(0.471)	(0.251)	(0.405)	(0.486)
Q3	0.269	0.497	0.385	0.762	0.043	0.142	0.371
	(0.192)	(0.50)	(0.487)	(0.426)	(0.202)	(0.349)	(0.483)
Q4	1.209	0.448	0.231	0.842	0.021	0.077	0.417
	(0.466)	(0.497)	(0.421)	(0.364)	(0.144)	(0.266)	(0.493)
Panel B: Standardized reading score							
Score interval	Reading score	Female	FRL eligible	White	Black	Hispanic	Metro
Q1	-1.193	0.423	0.712	0.514	0.139	0.269	0.449
	(0.396)	(0.494)	(0.453)	(0.50)	(0.464)	(0.443)	(0.497)
Q2	-0.331	0.476	0.531	0.660	0.074	0.209	0.381
	(0.193)	(0.499)	(0.499)	(0.474)	(0.261)	(0.406)	(0.486)
Q3	0.318	0.505	0.390	0.751	0.048	0.146	0.382
	(0.190)	(0.50)	(0.488)	(0.432)	(0.213)	(0.353)	(0.486)
Q4	1.177	0.537	0.228	0.839	0.025	0.075	0.431
	(0.399)	(0.499)	(0.420)	(0.367)	(0.158)	(0.264)	(0.495)

Notes: Test scores are standardized by assessment system, year, and grade. The proportions of individual-level controls, such as female, FRL eligibility, White, Black, Hispanic, and metro (Lincoln and Omaha are considered as metro), are calculated by dividing the sample into four quantiles based on math score (Panel A) & reading score (Panel B) and then calculating the average for all students within the quantile.

Table A3: Symmetry around the cut-off

Covariate	Before cut-off	After cut-off	P value of difference
Female	0.4839	0.4833	0.9022
FRL Eligible	0.4658	0.4627	0.5007
White	0.6866	0.6964	0.0219
Black	0.0707	0.0731	0.3180
Hispanic	0.1801	0.1664	0.0001
Asian	0.0214	0.0221	0.5935
Metro	0.4117	0.4088	0.5274

Notes: The first column shows the average of each covariate before the cut-off, while the second column presents the average after the cut-off. 45 days of bandwidth were used around the cut-off. The third column indicates the p-value of the difference between column 1 and column 2.

Table A4: Test for constant effect

Sample	Dependent variable	K-S statistic	Critical value	Result
All	Std math	7.931	2.354	Reject null
All	Std reading	7.221	2.613	Reject null

Notes: All the provided statistics are outcomes derived from the post-estimation command following SEE (Smoothed estimating equations) estimation. The null hypothesis is that the treatment effect is constant for all the estimated quantiles. Critical values are reported at a 95 % confidence level.

Table A5: Test scores by race

	White	Black	Hispanic	Asian
Math	0.1662	-0.6587	-0.3931	0.3964
	(0.0025)	(0.007)	(0.0044)	(0.0163)
Reading	0.1585	-0.5822	-0.3893	0.3080
	(0.0025)	(0.0074)	(0.0045)	(0.0152)

Notes: Average math and reading scores for different demographic groups considering the full sample.

Table A6: Checking attrition at each grade

	Missing Score					
Model	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
2SLS	-0.000590	0.000675	-0.00100	0.000848	-0.00936***	-0.00774***
	(0.00323)	(0.00306)	(0.00287)	(0.00265)	(0.00246)	(0.00221)

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: A missing score is defined as one if an individual's score is missing for that particular grade. Missing scores for each grade are regressed on crossing cutoff, distance from cutoff, and interaction between crossing cutoff and distance from cutoff, following equation 2. A bandwidth of 45 days has been used to select observations close to the cutoff. These regressions do not include any controls.