Impacts of School Entry Age on Academic Growth through 2nd Grade:

A Multi-State Regression Discontinuity Analysis

Angela Johnson
Megan Kuhfeld
NWEA

Abstract

The belief that additional time allows children to become more ready for school has affected public policy and individual practices. Prior studies estimated either associations between school entry age and academic growth or causal effects on achievement measured at one or two points. This paper contributes novel causal evidence for the impacts of kindergarten entry age on academic growth in the first three years of school. We embed regression discontinuity into a piecewise multilevel growth model and apply it to rich assessment data from three states. Being a year older leads to higher initial achievement and higher kindergarten growth rates but lower growth rates during 1st and 2nd grades. Effects do not differ by gender or race.

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The belief that additional time allows children to become more ready for school has affected both public policy and individual practices. Many states have shifted cutoff dates to increase the age at which children are permitted to enroll (Education Commission of the States, 2018; Elder & Lubotsky, 2009). Parents are also choosing to keep their children who have reached the legal age of school entry out of school for one or more years, a practice commonly known as academic redshirting (Bassok & Reardon, 2013). Children gain school readiness skills during the extra time spent before school, through pre-kindergarten (preK), childcare experiences, and maturation. This allows older students to start kindergarten at a higher level of achievement than younger students. The question is whether entering school older results in lower rates of learning while in school; if so, the initial advantage of older entry age is unlikely to be sustained.

The prevailing assumption behind raising the age of school entry is that being older and more mature helps children derive more benefit from schooling (Meisels, 1999). States increase school entry age based on a perceived absolute advantage to older entry age, the idea that maturity results in more learning. Assuming years of schooling are the same, if older entrants learn at a higher rate than younger entrants while in school, then entering school older would result in greater human capital accumulation in the long run (Elder & Lubotsky, 2009). In addition to this absolute advantage, parents may perceive a relative advantage of being older than one’s classmates: that school staff may allocate academic opportunities in ways that favor higher-achieving, better-behaved children (Schanzenbach & Larson, 2017).

Despite the common assumption that entering school older is better and the policies associated with this idea, existing research has found mixed results on the effect of school entry age on achievement status (e.g., Cook & Kang, 2018; Suziedelyte & Zhu, 2015), and evidence is lacking on rates of learning. If older school entry age indeed leads to more learning, raising entry age would be worthwhile. If, however, younger children show faster gains once in school, then initial differences in achievement would fade over time, resulting in limited benefits overall for older entry age (Elder & Lubotsky, 2009). Students' rate of growth during each year in school is key to understanding the impact of entry age, but the extant evidence is limited.

The benefits of raising school entry age merit interrogation in light of the associated public and private costs. During the extra year spent out of school, children require care, either in the form of public pre-K or private childcare. The financial burden of these programs must be borne by the state or the family. Given the high costs for one additional year of out-of-school care, state policy and family decisions for school entry age should be informed by credible causal estimates on the rate of learning in school, which determines whether initial advantages of older school entry age is sustained in the long run. To our knowledge, no research has provided this evidence. Furthermore, disparities in students' academic skills by race/ethnicity and socioeconomic status (SES) have been well documented at school entry (von Hippel et al., 2018; Kuhfeld et al., 2020). Little research has documented whether the potential initial benefits of older school entry age differ by race/ethnicity or SES, or whether there is heterogeneity in the degree to which these benefits persist across the early grades.

To address these important gaps in the research, this study seeks to answer two research questions:

(1) What is the impact of being a year older at kindergarten entry on students' academic growth during the first three years of school?

(2) Does the impact of being a year older at kindergarten entry on academic growth vary by student characteristics (e.g., gender, race/ethnicity) or by state?

Research on School Entry Age

Earlier observational studies tended to find that children who entered school older scored higher on cognitive, academic, and behavioral measures compared to children who entered younger (see Stipek, 2002 for a review), but the differences diminish in higher grades. A related line of inquiry predicted redshirting, or raising entry age by parent choice, and found small associations between redshirting and downstream outcomes (e.g., Fortner & Jenkins, 2017; Graue & DiPerna, 2000). One concern is that older entrants and younger entrants can differ in systematic ways, and the direction of the bias is ambiguous. Entering school older may reflect socioeconomic advantage or developmental challenge. These factors are unobservable but likely to influence downstream outcomes. To deal with this, a body of quasi-experimental research used national or state cutoff dates to identify exogenous variations in school entry age and isolate the age effect (e.g., Bedard & Dhuey, 2006, 2012; Lenard & Peña, 2018; Datar & Gottfried, 2015). Some of these studies relied on the season or month of birth as an instrument for age measured at school entry. This approach would effectively address omitted variable bias if the instrument is uncorrelated with other unmeasured determinants of outcome. However, as seasonal birth rates have been shown to vary based on family background characteristics (Buckles & Hungerman, 2013), the design may still produce biased estimates.

Two lines of studies come closest to addressing academic growth and the long-term impact of school entry age but with little overlap: (a) causal studies that applied regression discontinuity (RD) designs to achievement measured at one or two points in time; and (b) studies that used vertically-scaled measures to predict growth over time in the absence of causal inference. To our knowledge, no research has examined the causal effect of school entry age on academic growth.

Regression Discontinuity (RD) Studies

Recent studies have privileged the use of the RD design, which improves upon other quasi-experimental methods by leveraging the jump in the average age of students at the state's cutoff date. States set cutoff dates on or before which a child must turn five to enter school. Students born on or just before the cutoff date are presumably similar on average to students born just after the cutoff date in background characteristics. But the former group, having just turned five, are permitted to enter school while the latter must wait until the following year when they are close to six years old. Any differences in downstream outcomes between the two groups can be interpreted as the causal effect of being a year older at school entry. The state policy creates exogenous variation in school entry age at a single cutoff date. This makes the RD less vulnerable to biases due to seasonal birth endogeneity since the RD estimates the local average treatment effect at a single point.

Results from RD studies on the effects of delayed school entry varied substantially by context. Dee and Sievertsen (2018) used data from Denmark and found that being a year older at school entry reduces inattention and hyperactivity measured at ages seven and 11. Applying a similar design, Depew and Eren (2016) found reductions in juvenile crime for black females. In contrast, a study of Australian children showed that older entry age is detrimental to cognitive skills (Suziedelyte & Zhu, 2015). Cook and Kang (2018) examined data from North Carolina and found that being a year older at school entry raised math and reading scores. Jenkins and Fortner

(2019), likewise using North Carolina data, also concluded that older entry age yielded (in this case small) benefits to students' test scores in the spring of 3rd grade, as well as reduced probabilities of being identified as having a disability.

The RD literature improves upon other studies in terms of addressing selection into entering school at an older age. However, the extant RD studies share two key limitations. First, the achievement outcomes in previous RD studies were only measured at one or two points in time, and the measures were not vertically scaled. No research has examined the causal effect on academic growth. This limitation means that we do not know whether any initial causal effects of older entry age on achievement fade as students progress through school. This is an important question, given the cost associated with an additional year of out-of-school childcare. Second, US studies used data from only one state (e.g., Cook & Kang, 2018). They yielded mixed findings, suggesting that the unique context of each study may limit the generalizability of the results, as school entry age policies differ by state. We know of no study that compared effects across states or across schools within a state.

Studies on Academic Growth

Studies that examined academic growth using descriptive and instrumental variables approaches presented mixed findings. We know of only two that used vertically-scaled measures to study the relation between school entry age and academic growth over time. One paper used survey data on 900 children in the US and applied hierarchical linear modeling (HLM) to Woodcock-Johnson test scores (NICHD Early Child Care Research Networks, 2007). It found that children who entered kindergarten at a younger age had higher initial scores, but children who entered at older ages experienced greater increases overtime, outperforming younger entrants in 3rd grade. Datar (2006) used the nationally representative Early Childhood

Longitudinal Study-Kindergarten Class (ECLS-K) survey data and an instrumental variable approach to estimate the relation between kindergarten entry age and gains in math and reading test scores between the fall of kindergarten and the spring of 1st grade. She found that older kindergarten entrants had significantly higher initial test scores, as well as steeper score trajectories during the first two years of school. These results, along with mixed findings from the RD literature, highlight the need to further examine the effects on not only achievement but growth over time by applying more rigorous methods to vertically-scaled achievement measures.

Contributions of the Current Study

We employ a multilevel, fuzzy RD framework to examine the causal impact of entering kindergarten a year older on academic growth in the first three years of school. Causal estimates on growth patterns in these early grades will provide important insights on how any advantage held by older students is maintained or fades away as students progress through school. We use rich data from NWEA's MAP Growth K-2 assessment, which has been administered in a consistent format longitudinally across multiple states. Our analytic sample includes 30,552 students across three states who were born within 30 days of the school entry cutoff date. We ask whether children who turned five years old around the same time but entered school one year apart had different academic growth trajectories. Our focus is on the extent to which school entry age affects the rate at which students learn in each school year, a question immediately relevant to education policy.

This paper makes four important contributions to the literature. First, our fuzzy RD deign credibly identifies the causal effect, at a single state-policy cutoff date, of entering a year older on growth rates in the first three years of school. Second, using repeated measures of achievement within year, we estimate students' growth trajectories separately for kindergarten,

1^{st} grade, and 2^{nd} grade, unmasking the differences between grades that earlier studies pooled. Datar (2006) only observed student achievement in the fall of kindergarten and the spring of 1^{st} grade. The NICHD (2007) study tested children at 54 months then at the end of 1^{st} and 3^{rd} grades. As a result, these two studies extrapolated children's growth trajectories using only two or three data points measured 18 or more months apart. Our study greatly improves upon these studies by modeling growth within as well as across school years, providing a more detailed illustration of growth over time and the impact of school entry age. This design allows to us to identify key developmental periods in which older students' initial advantage may fade or grow, as well as unpack whether the initial advantage operates in a similar manner while students are in school versus on summer break. Third, in addition to local average treatment effects for the average student, we estimate between-school variations in the effect of older entry age and explore school-level covariates that contribute to the variations. This cannot be accomplished with standard RD models.[1] By using a multilevel RD framework, we can explicitly examine whether the advantage of entering school at an older age operates in a similar or different manner across various school contexts. Finally, we expand the observed time window of growth from the first two years to the first three years of school for the main sample and one additional year for a subsample. This is a significant extension, considering the importance of growth in this critical period of early learning.

Data

Data Source

The data used in this study came from the Growth Research Database (GRD) at NWEA. The GRD contains longitudinal test score data from students in thousands of public school districts across the country that partner with NWEA for a variety of purposes (to monitor growth throughout the school year, teacher/school evaluation, as an indicator for intervention or special programming, etc.). In this study, we focus on three states: two Midwestern and one Southern. We chose these three states because the GRD contained data on a sizable proportion of the schools serving kindergarten students in each state during the study period (36%, 59%, and 77% of schools serving kindergarten in the three states, respectively). It should be noted that the schools in the NWEA sample are not randomly selected within each state. Districts and schools select into administering the MAP Growth assessment to their students. Most schools that partner with NWEA test the majority of students within a grade (an average of 80% of enrolled students). A comparison of the schools in our sample with the public schools serving kindergarten through 2nd graders in each state is available upon request.

Measures

We examine students' reading and mathematics scores on NWEA's MAP Growth assessment. Each test is aligned to state content standards and takes approximately 40 to 60 minutes depending on the grade and subject area. The MAP Growth assessments are computerized, adaptive tests typically administered in the fall, winter, and spring. In the early grades, MAP Growth includes developmentally-appropriate items, interactive elements, and audio supports to engage and accurately assess early learners. Test scores are reported on the RIT scale, where RIT stands for Rasch Unit and is a linear transformation of the logit scale units of the Rasch item response theory model.

In addition to assessment scores, we also have students' race/ethnicity, gender, school calendars (e.g., start and end dates), and students' birthdate. We use school calendar, test dates, and birthdates to calculate age at school entry and how many months students have been in school prior to testing. Schools set their own testing windows and there is typically a fair amount of variation in how many weeks into the school year that students are first assessed. Furthermore, we use a set of school characteristics reported by the Common Core of Data (CCD) from the National Center for Education Statistics (NCES). The CCD variables used in this study include school percentage of Free or Reduced- Price Lunch (FRPL) receipt, percentage of White students in the school, and percentage of Black students in the school. We use these school-level covariates in the multi-level models employed to estimate growth.

Sample

We focus on students in three birth year cohorts, following students who were born in calendar years 2009 to 2011 and entered kindergarten between the fall of 2014 and the fall of 2017. There is a total of nine possible observations per test subject per student across the fall, winter, and spring of K, 1st, and 2nd grades. Students born in calendar year 2009 form a subsample and were followed to the spring of 3rd grade. Table 1 provides a visualization of the birth year cohorts. For example, students in the 2009 birth cohort who turned five on or before September 1st, 2014 were eligible to enter kindergarten in 2014-15 whereas students who turned five after September 1, 2014 were not be eligible to enter kindergarten until 2015-16. The comparison of interest is between students within the same birth year who were born just prior to the cut-off (and entered school at around five-years-old) and those born just after the birthday cut-off (and entered school at almost six-years-old). In any given school year, the two groups were in different grades (e.g., in 2015-16, younger entrants in the 2009 birth cohort were in 1st grade, while older entrants in the same birth cohort were in kindergarten). In the last birthyear cohort, born in calendar 2011, includes students who entered school at age 5 and were followed to the spring of 2nd grade spring and students who entered school near or at age 6 and were followed to the spring of 1st grade. The latter group was excluded from the analysis on growth during 2nd grade (results are robust to additionally excluding their birthyear peers who entered school at age 5).

[INSERT TABLE 1 HERE]

In total, we have data for 181,876 students across the three states who took the MAP Growth reading assessment or math assessment in the fall of kindergarten between academic years 2014-15 and 2017-18. We restrict the sample for analysis by dropping 6,105 students whose school did not match to NCES public school records, 214 students who attended schools with fewer than 10 students represented in the MAP growth data for the study grades and years, and 118 students with missing demographic data. Finally, we retain students whose birthdates were within 30 days of the states' cutoff date, following previous RD studies (e.g., Dee & Sievertsen, 2018). The analytic sample includes data for 30,552 students in 1,305 schools. Table 2 presents demographic information for reading and math test takers separately. The reading sample is 49% female, 44% White, 23% Black, 16% Hispanic, and 17% other race/ethnicity. The math sample is similar in composition. A comparison of the schools in our sample with the public schools serving kindergarten through 2nd graders in each state is available upon request.

[INSERT TABLE 2 HERE]

Research Design

We first descriptively compare the math and reading trajectories of the students who were born just before and just after the cutoff by plotting the mean achievement level of each group within the fall, winter, and spring of kindergarten, 1st grade, and 2nd grade. These analyses allow for general understanding of trends prior to specifying more sophisticated statistical models.

Then, we estimate the causal effect of being a year older on test scores at kindergarten entry and on growth rates in the first three years of school. We employ a "fuzzy" RD design, which incorporates a two-stage least squares approach, with students' date of birth as the running variable (Lee & Lemieux, 2010). States set a cutoff date on or before which students must turn five to enter kindergarten. We center the students' date of birth such that $Days_i$ = 0 for students born on the first day after the cutoff. If parents follow their state's cutoff dates for kindergarten entry, children would enter kindergarten when they are five years old. Students born just before the cutoff would have just turned five; students born just after the cutoff would enroll the following year, just before turning six. Thus, we expect to observe a discontinuity, or "jump", in the kindergarten entry age of children born around the cutoff date. The RD design exploits this discontinuity in entry age to estimate the causal effect of entering a year older.

Our reduced-form equations model academic outcomes as a flexible function of the running variable and an indicator for having a birthdate after the cutoff:

$$y_i = \alpha_0 + \alpha_1 \mathbf{1}(Days_i \geq 0) + f(Days_i) + \rho' X_i + \varepsilon_i \qquad (1)$$

where y_i represents the outcomes of interest (initial test score in the fall of kindergarten year; growth rates estimated using multi-level modeling, described later); $Days_i$ represents the distance between the student's birthdate and the cutoff date; and X_i is a vector of student-level covariates. α_1 is the parameter of interest and the estimated causal effect of being a year older at kindergarten entry. For the first outcome, initial test score, this parameter represents the gap in academic achievement level at kindergarten entry between five- and six-year-old students. For the second set of outcomes, academic growth, this parameter represents the difference between the growth rates of students who enter kindergarten at five versus six years old.

Validity of the RD Design

The validity of the design hinges on whether the cutoff induced variation in kindergarten entry age and whether assignment to either side of the cutoff was "as good as random" (Lee &

Lemieux, 2010). We first test if there is a discontinuity in the age of entry at the cutoff. As shown in Online Appendix OA1, having a birthdate after the state cutoff significantly increased the age at which children entered kindergarten by approximately three quarters of a year. Then, we check that students with birthdates around the cutoff are similar on observable pretreatment characteristics. Using an RD model with linear splines, we test if the densities of students' gender and race/ethnicity are continuous (see Appendix A1 Panel A). Unfortunately, we do not observe students' socioeconomic status and were unable to use it to test for balance or as a control; the percentage of students eligible for free or reduced-price lunch (%FRPL) is not statistically significant at the 0.05 level. Prior literature shows that parents are very unlikely to precisely manipulate their children's birthdates (Dickert-Conlin & Elder, 2010). We perform density tests and verify that there is no evidence of precise manipulation of the running variable in our data (McCrary, 2008). Results are reported in Online Appendix OA2. We also verify that attrition is balanced by checking the availability of test scores in the springs of kindergarten, 1st grade, and 2nd grade (Appendix A1 Panel B). These checks provide some reassurance that the RD design is valid. Another concern regarding the RD design is the appropriate choice of bandwidth and functional form. We address this by taking a local linear regression approach. For brevity, we describe below our preferred RD model specification, which includes linear splines and uses data within a bandwidth of 30 days.[2] To test sensitivity, we compare estimates across a variety of bandwidths and estimates from linear and quadratic RD specifications.

Having conducted checks for the validity of the RD design, we use an RD model with linear splines to estimate the impact of being a year older on test scores in the fall of kindergarten. We use two approaches to estimate the impact on initial test score. Our first approach follows the standard RD framework, using the students' kindergarten fall term test score as the outcome. This approach treats the students' test score as their achievement level at the start of kindergarten, regardless of when testing occurred in the fall. The limitation of this approach is that some students in our sample have been exposed to up to two months of instruction prior to their fall test, which confounds estimates of achievement differences at school entry. Our second approach incorporates RD into a multilevel growth model. This approach accounts for individual differences in testing dates in both the estimation of (a) initial test scores at school entry and (b) growth rates across the school year. As described in additional detail in the following section, the intercept parameter in the multilevel growth model is an extrapolation of student achievement to the first day of school, allowing for an estimate of the impact of entering school a year older that is uncontaminated by differences in school exposure We present both sets of results for comparison.

Multilevel Growth Model

We use a piecewise multilevel growth model (e.g., Downey, von Hippel, & Broh, 2004; von Hippel, Workman, Downey, 2018). In this set-up, MAP Growth test scores (level 1) observed in each term are nested within students (level 2) and schools (level 3). We include any student who has at least one MAP Growth score, even if he or she did not test in all nine waves. In these models, school affiliation is considered time-invariant. In the situation where a student switched schools during the study years, students are assigned to the school in which the student entered kindergarten because transferring to another school may be endogenous. We use school characteristics reported for academic year 2013-14. Our examination of the multiple years of school characteristics available from the CCD indicate that school composition tends to be highly stable over time (Chingos, 2020). For schools with missing data for the 2013-14 school year, we use characteristics for an adjacent school year (e.g., 2014-15).

Rather than examining overall growth trajectories across years, our piecewise growth model specification allows us to separately examine differences in academic growth estimates between younger and older students in kindergarten, 1st grade, and 2nd grade as well as the summers after kindergarten and 1st grade. Seasonal patterns of learning, where gains during the school year are followed by flattening or dropping of test scores during the summer, have been observed across a range of datasets (von Hippel & Hamrock, 2019). Additionally, average growth rates have been found to decelerate across school years (Bloom et al., 2008; Thum & Kuhfeld, 2020), which means that estimating a single overall school-year growth rate will mask systematic differences in learning rates across grade levels. For these reasons, researchers interested in modeling growth across multiple timepoints during the school year typically rely on a multilevel piecewise growth models that accounts for variation in testing date within the school year and allows for separate growth terms per school year and summer (e.g., Quinn et al., 2016). By separately specifying growth terms for each school year, we can test whether the potential advantage of entering school a year older is constant across grades or if it begins to fade as students progress through school. This model estimates students' academic growth as a linear function of their "months of exposure" to each school year and summer break. Months of exposure is calculated based on a student's school start and end dates and the test administration dates (see Online Appendix OA3 for details). For example, a student testing at the end of August in 1st grade may have 9.3 months of exposure to kindergarten, 2.7 months exposure to summer following kindergarten, and one week of exposure to 1st grade.

We first estimate the monthly learning rates during each school year and summer from kindergarten to 2nd grade. At level 1, the growth model is:

$$y_{tij} = \pi_{0ij} + \pi_{1ij}G0_{ij} + \pi_{2ij}S1_{ij} + \pi_{3ij}G1_{ij} + \pi_{4ij}S2_{ij} + \pi_{5ij}G2_{ij} + e_{tij}. \quad (2)$$

We view each test score y_{tij} as a linear function of the months that student *i* in school *j* has been exposed to kindergarten ($G0_{ij}$), 1st grade ($G1_{ij}$), and 2nd grade ($G2_{ij}$); and the number of months that the student has been exposed to the summer after kindergarten ($S1_{ij}$) and 1st grade ($S2_{ij}$). As von Hippel and colleagues (2018) note, this model "implicitly extrapolates beyond the test dates to the scores that would have been achieved on the first and last day of the school year" (p. 335). The intercept (π_{0ij}) therefore is the predicted score for student *i* in school *j* testing on the first day of kindergarten, even if the student tested four weeks into the school year. The slopes ($\pi_{1ij}, \dots, \pi_{5ij}$) are the monthly learning rates of student *i* during each school year and summer.

At level 2, we include an indicator for having a birthdate after the state cutoff ($A_{ij} = 1$ if students were born after the cut-off, zero otherwise), a measure of the distance between student's birthdate and the state's age cutoff date (Days_{ij}), and the interaction between these terms ($A_{ij} * \text{Days}_{ij}$). The inclusion of these terms allows us to capture differences between younger and older kindergarten entrants with respect to the intercept (estimated test scores at kindergarten entry) as well as the school year/summer learning rates. This level-2 equation is analogous to an RD model with linear splines in the traditional RD framework. As in equation (1), the coefficient for the A_{ij} term is the estimated effect of being a year older at kindergarten entry on learning rates. Additionally, we include dummy variables at level 2 for cohort ($C2_{ij}$ and $C3_{ij}$) to allow for cohort differences in the estimation of the intercept and growth terms (where the 2009 birth year is the reference group). Random effects are included at both the student- and school-level to capture variation in the intercept and slopes across levels of the model. In Equation (3) below, we display the specification of the student-level random intercept term (π_{0ij}). Omitted here for brevity (see Appendix A2 for the full set of student- and school-level equations), the same specification is used for each of the student slope terms ($\pi_{1ij}, \ldots, \pi_{5ij}$). Lastly, state fixed effects (dummies) are included at level 3 of the model.

Level-2 Model (student (i) within school (j)): (3)

$$\pi_{0ij} = \beta_{00j} + \beta_{01j}\mathrm{A}_{ij} + \beta_{02j}\mathrm{Days}_{ij} + \beta_{03j}\left(\mathrm{A}_{ij} * \mathrm{Days}_{ij}\right) + \beta_{04j}\left(C2_{ij}\right) + \beta_{05j}\left(C3_{ij}\right) + r_{0ij}$$
$$\vdots$$
$$\pi_{5ij} = \beta_{50j} + \beta_{51j}\mathrm{A}_{ij} + \beta_{52j}\mathrm{Days}_{ij} + \beta_{53j}\left(\mathrm{A}_{ij} * \mathrm{Days}_{ij}\right) + \beta_{54j}\left(C2_{ij}\right) + \beta_{55j}\left(C3_{ij}\right) + r_{5ij}$$

This model described above represents our baseline model (which we will refer to as Model I). We also test whether the estimated effect of entering kindergarten a year older is sensitive to the inclusion of key student and school-level covariates. In Model II, we add indicators for gender ($Female_{ij}$) and race/ethnicity ($Black_{ij}, Hispanic_{ij}$, and $OtherRace_{ij}$), where younger male White students are the reference group. Model III extends Model II to include three grand-mean centered school-level covariates: (a) school percentage of FRPL receipt, (b) percentage of White students, and (c) percentage of Black students. All models were estimated using full-information maximum likelihood estimation in HLM Version 7 (Raudenbush, Bryk, & Congdon, 2013), which uses all available test scores in estimation (whether or not a given student was observed in all waves).

Effect Heterogeneity

Extant literature suggests that the treatment effects of entering kindergarten a year older may not be homogenous (e.g., Jenkins & Fortner, 2019). We examine the heterogeneity of our treatment estimates by estimating three additional growth models that test for interactions between entering school a year older (A_{ij}) and key covariates. These additional models build upon Model II, which already contains dummy variables for birth year cohort, state, race/ethnicity, and gender. First, we interact having a birthdate after the cutoff with the state dummy variables to see if the effect is constant across the three states in our study (Model IV).

Second, we test whether the effect of entering school a year older is different between boys and girls (Model IV). Lastly, we test interactions between entering a year older and race/ethnicity to estimate differential treatment effects between racial/ethnic subgroups (Model VI). Online Appendix OA4 shows the full specification for Models IV through VI.

Results

K-2 Academic Achievement Trends

[INSERT FIGURE 1 HERE]

Figure 1 presents the average trajectory of math and reading test scores from the fall of kindergarten to the spring of 2^{nd} grade for students with birthdates within 30 days of the cut date. These results are pooled across cohorts and states (separate state plots available upon request). We present group means across time on the RIT scale as well as standardized difference scores within each timepoint. For reference, the average standard deviation (SD) in the fall of kindergarten is 10.06 RIT points in math and 9.42 RIT points in reading.

In the fall of kindergarten, there is a sizable gap in test scores favoring students who enter school a year older over students entering at around five (0.66 SD in math and 0.57 SD in reading). These gaps mostly hold steady in kindergarten (the reading gap even widens) but shrink during 1^{st} and 2^{nd} grade. By the end of 2^{nd} grade, the advantage of being older has almost halved in math (to 0.37 SD) and shrunk considerably in reading (to 0.35 SD), though both gaps remain sizable. Both groups of students show learning gains during the school year followed by a flattening or drop in test scores during the summer (i.e., summer learning loss). This pattern, also observed with MAP Growth data by Kuhfeld (2019) and Atteberry and McEachin (2019), among others, necessitates the piecewise growth structure specified in our multilevel growth models. In the remainder of this paper, we focus on initial achievement and growth during academic years since (a) effects on summer learning rates were imprecisely estimated and (b) schools have more influence over student learning during the year than during the summer. Estimated effects of being older on summer learning rates can be found in Online Appendix OA5.

RQ1. What is the impact of being a year older at kindergarten entry on students' academic growth during the first three years of school?

Using the standard RD approach, we estimate the impact of being a year older on students' observed test scores in the fall of kindergarten (Appendix A1 Panel C). The mean observed score for students who enter at five years old is 139.5 RIT for math and 136.7 for reading. The estimated impact of entering kindergarten a year older is 5.19 RIT for math and 4.05 for reading. In the absence of repeated measures, the observed scores can serve as a proxy for students' achievement level prior to kindergarten entry. However, these scores likely have been affected by days of instruction prior to testing and be higher than scores students would have received had they been tested on the first day of kindergarten. This may bias the estimates on growth. We therefore privilege the results presented in Table 3, which use estimated initial achievement prior to kindergarten instruction as the outcome.

[INSERT TABLE 3 HERE]

Table 3 presents the reduced form estimates from the first set of hierarchical linear models (Models I-III) applied to examine the impact of being a year older on students' initial achievement and academic growth across the first three years of school. For parsimony, only the key parameters of interest pertaining to initial achievement and growth are included (the full set of coefficients is available upon request). The baseline model (Model I) indicates that as expected, the estimated initial scores prior to instruction, (137.8 RIT for math and 134.8 for reading), are lower than observed scores. Older students enter kindergarten with a significant advantage over younger students (5.47 RIT points in math, 4.10 in reading) and show significantly higher growth rates during kindergarten (0.14 RIT points per month in math, 0.24 per month in reading). However, the advantage seems to flip in 1st grade, with older students showing significantly lower monthly growth rates in math and reading in both 1st and 2nd grade. In 1st grade math, the average younger entrant gained 2.34 RIT points per month, and the average older entrant gained 0.15 RIT points less per month than the younger entrants. These findings hold when student and school covariates are included (Model II and Model III, respectively).

Figure 2 shows the changes at the birthdate cutoff in initial test score at kindergarten entry and in growth rates over the first three years of school. Data are pooled across states and birth year cohorts (see Online Appendix OA6 for separate figures by state). Each black circle represents the average initial score or average monthly growth for students in the corresponding centered birthdate bin (containing 350 to 700 students). Consistent with the results in Table 3, in both math and reading, the monthly learning rates for older students were significantly higher than younger students during the kindergarten year but significantly lower in 1st and 2nd grade.

[INSERT FIGURE 2 HERE]

The bottom of Table 3 presents the estimated student- and school-level random effects SDs, which allow us to examine whether there is significant between-school variation in the effect of being older on initial status and growth. In math, there is significant between-school variation in the advantage of being older at entry (SD=1.25) as well as the monthly growth rates in kindergarten (SD=0.19) and 2nd grade (SD=0.12). For kindergarten growth, since the overall advantage of being older is only 0.14 RIT points, this degree of between-school variation indicates that in some schools being older has a net zero or negative impact. In reading, between-school variation in the impact of being older is only statistically significant in kindergarten.

Growth in 3rd Grade (Cohort 2009)

We followed the 2009 cohort for one additional year. In both reading and math, the older students showed significantly lower growth rate in 3rd grade, indicating the initial advantage of being older continues to shrink as students move through school (Appendix Table A3).

RQ2. Does the impact of being a year older at kindergarten entry on academic growth vary by student characteristics (e.g., gender, race/ethnicity), or by state?

[INSERT TABLE 4 HERE]

Table 4 presents the findings examining whether the impact of being a year older at kindergarten entry on academic growth varies by student characteristics or state. We do not see strong evidence of state-level differences in the impact of being older on initial status or growth in math, though there are some significant interactions by state in reading. For example, State 2 shows a smaller advantage (-1.37 RIT points) of being older in students' initial reading scores than State 3 (the reference state). In addition, the impact of entering school older on students' academic growth across the three school years observed in State 3 is absent in State 2.

The results from Model V indicate that while the effect of being older at school entry on initial test scores is slightly larger for girls than boys (0.59 RIT points in math, 0.93 in reading), the impact of being older on growth rates across kindergarten through 2nd grade does not significantly differ by gender. Lastly, we examined interactions between being older and students' race/ethnicity (Model VI). Hispanic students experienced a significantly larger effect of being older on initial achievement (0.96 RIT points in math, 1.23 in reading) but similar effects of being older on growth rates as White students. The only significant interaction for Black students is related to the impact of being older on kindergarten growth rates, where older Black students had larger advantages (0.12 RIT points in math, 0.15 in reading) compared to White students.

Robustness Checks

In addition to checking that our results are robust to the inclusion of student- and school-level covariates (Models II and III), we perform two other sensitivity tests. First, we vary the RD bandwidth from 5 to 45 days around the birthdate cutoff. Results are very similar across the bandwidths (see Online Appendix OA7). Second, we include quadratic splines in level 2 of our model and find that the addition of quadratic terms, which had insignificant estimates, does not improve model fit (results available upon request). These analyses provide reassurance that the findings from our preferred linear model and bandwidth of 30 days are robust. Finally, we ran the models using two sets of placebo birthdate cutoffs; estimates are close to zero and of small practical importance (Online Appendix OA8).

Discussion

This study demonstrates a novel approach to estimating the causal impact of kindergarten entry age on academic growth by integrating the RD design with multilevel growth modeling. We report four main findings. First, being a year older at kindergarten entry has significant positive effects on initial math and reading achievement, as well as monthly growth rates during the kindergarten school year. Second, the effects of being older at kindergarten entry on 1st and 2nd grade math and reading growth are negative and significant. In other words, the initial gaps between older and younger students start to close as students progress through the early grades because after kindergarten, older students grow at slower rates compared to younger students. Third, while there are some heterogeneous effects on initial test score, the impact of being older on growth, especially growth after kindergarten, generally does not differ by gender or ethnicity.

Finally, while the effect of being older on initial test scores and growth varied significantly across different school contexts in math, it was mostly consistent across school settings in reading.

The gap we observe in initial test score between older and younger kindergarten students is consistent with previous literature. For example, Datar (2006) and Stipek (2001) similarly found that children who entered kindergarten at an older age had higher initial test scores upon entry. Relatedly, being older has been shown to raise achievement measured at a fixed point relatively early in a student's academic career. Employing a fuzzy RD framework, Cook and Kang (2018) found that an extra year of age has significant causal effects on end-of-3rd-grade test scores: .36 SD in reading and .30 SD in math. Our descriptive findings are consistent with these estimates, showing that a gap of similar magnitude (.37 SD for math, .35 SD for reading) remains between older and younger students in achievement level by the end of 2nd grade.

However, our results on early growth trajectories contrast with earlier studies and add novel evidence to this line of inquiry. Datar (2006) and the NICHD Early Child Care Research Network (2007) also used vertically-scaled measures and found that the achievement of older kindergarten entrants grows faster during the early grades. In contrast, we found that older students only learned faster than younger students during kindergarten; in fact, they grew significantly more slowly during 1st through 3rd grade. The previous papers were not able to distinguish kindergarten year growth from other grade levels because they only observed achievement data before or immediately after kindergarten entry and then again at the end of 1st grade. In modeling growth within and across grades, our findings illuminate a richer trajectory and make important distinctions between grades that allow policymakers and educators to design programs and policies targeted at the appropriate grade levels.

More generally, our findings suggest that the extant evidence on the advantages of older school entry age ought to be interrogated. The decline we see in 1^{st} and 2^{nd} grade in the older-age premium is consistent with the findings of other studies that examined outcomes measured in later grades. For instance, Bedard and Dhuey (2006) found that the 8^{th} grade achievement gap between students who entered school older and younger was much smaller than the gap in 4^{th} grade. Fletcher and Kim (2016) also found that the impact on math and reading achievement of increasing entry age was large in 4^{th} grade, much smaller in 8^{th} grade, and negligible in 12^{th} grade. The gap reduction that begins in 1^{st} grade may continue into later grades, but more rigorous research is required to clarify effects on longer term outcomes.

The existing literature reports an array of outcomes, from achievement in early grades to juvenile incarceration to adult wages. But almost all studies on school entry age, like Bedard and Dhuey (2006) and Fletcher and Kim (2016), observed outcomes that were not vertically-scaled and, if repeated at all, measured at least a few years apart. Thus, the current knowledge is built on inconsistent measures taken across disparate student ages, grade levels, and geographical regions, and consumers of the research are having to base their practices on forced connections between distinct findings that may not be comparable or generalizable. Policymakers and educators need more actionable evidence drawn from repeated, vertically-scaled measures of student achievement over time. A viable first step is to examine the growth trajectories of students from 4^{th} grade to high school, bridging the gaps in our understanding of academic development in the middle grades. Research is also needed to improve measurements of academic engagement and socioemotional development. Dee and Sievertsen (2018) showed that entering school a year older resulted in lower levels of inattention and hyperactivity. Datar and Gottfried (2015) also found that older school entry age improved social behavioral outcomes, measured in kindergarten, 1st, 3rd, and 5th grade. Establishing more reliable and comparable measures will allow the tracking and modeling of these important "non-academic" outcomes across time to further understand the effects of policy and practices.

There are multiple possible explanations for the initial advantage of being older. As Jenkins and Fortner (2019) elaborate, students who just miss the birthdate cutoff have an extra year in which they may experience high-quality early education that better prepare them to enter school. Additionally, another year of development may result in improved executive functioning that translates to a higher ability to focus on the MAP Growth assessments in the early grades. However, it is less clear what might explain the fade-out of the age advantage in 1st through 3rd grade. It may be that teachers in these grades focus more attention on catching up the younger students. Alternatively, the maturational advantage in kindergarten mat begin to narrow in later grades independent of teacher attention (e.g., developmental differences are larger between five and six-year-olds than between seven and eight-year-olds). It is also possible that younger students benefit from having older peers who are higher-achieving and better-behaved. Research on peer effects found that holding the child's own age constant, being relatively young in the class led to higher test scores in kindergarten and 8th grade (Cascio & Schanzenbach, 2016). Relative age may be influential in reducing the gap over time. Our findings beg future research to explore the mechanisms behind the causal link between age and growth trajectories.

Limitations

This study is not without limitations. As with all RD studies, our estimates are based on data from students who were born on days close to the states' school entry cutoff. The findings may not generalize to students born in other times of the year. For instance, the effects of entering school one year older may be different for students entering school at 5.5. versus 6.5

years old. Another limitation of the RD design is that estimates are only defined for the complier population, thus the findings may not generalize to always-takers ("redshirters") and never-takers (students who would have entered school before or at 5 years old regardless of policy).[3] Further, since our data contained only a few student covariates, we were unable to examine how the impact of kindergarten entry age on growth differs by socioeconomic status or other subgroup membership, such as English Learners. Future research should examine whether there are additional benefits of being older for these groups.

Further research also is needed to disentangle the factors that drove the initial achievement gap between older and younger students. Children with pre-kindergarten experience are likely to enter school with more skills than children without. Without data on pre-kindergarten attendance or childcare participation, we were unable to distinguish between the effects of relative maturity and hold-out year experience on initial achievement. Our sample of schools within each state is not a random subset of schools serving kindergarteners and may differ from all public schools in the state. Second, we cannot yet track student cohorts beyond 3rd grade. It is important to know whether the gap between younger and older students continues to narrow after 3rd grade. Finally, we are only able to follow students who enrolled in public schools and took the MAP Growth assessments. The results may be susceptible to bias if the probability of enrolling in a school that administers MAP Growth jumps at the birthdate cutoff, though we are not aware of a theoretical basis for this concern.

Concluding Remarks

The age of school entry matters because of its implications on students' academic readiness and subsequent experiences and on the private and public costs associated with delaying schooling. This study contributes novel causal evidence by showing that initial advantages to entering school a year older fade as older students grow at significantly lower rates after kindergarten. Based on these results, we recommend that policymakers and families consider these fade-out effects before adopting policies and practices that raise school entry age.

Disaggregating growth trajectories by grade level is especially critical to addressing equity among subgroups of students. A few studies have investigated effect heterogeneity for school entry age by gender and race, but due to the lack of comparable repeated measures, none has been able to identify the grade level(s) in which the differential effects manifest. For instance, Jenkins and Fortner (2019) examined 3rd grade test scores and found that practices that increased students' age at school entry differentially benefitted low-income students but disadvantaged non-White students. How different subgroups grow during each of the early grades and how age contributes to the growth patterns remain unclear. One might conclude for example, that low-income students should be redshirted at higher rates since, judging from 3rd grade test scores, they benefit more from older entry age than higher income students (Jenkins & Fortner, 2019). However, it is possible that older age has a positive differential effect on low-income students' growth during kindergarten but a negative differential effect in the years that follow, such that in the long run the benefits disappear. Without observing the subgroups' growth trajectories within and across years, heterogeneous effects on achievement measured in one point in time should be interpreted with caution.

We show that initial achievement gaps between older and younger kindergarten entrants exist across subgroups but growth rates over the first few years of school do not vary significantly by subgroup status. This is an important insight unfound in previous research. To design policies aimed at providing equitable opportunities to all student subgroups, knowledge of both achievement levels and development over time is required. This study demonstrates that research designs combining the strengths of causal inference and multilevel growth modeling offer great potential for identifying the impact of policies on student achievement and growth.

Notes

[1] This is a key advantage of multilevel RD over the standard RD approach. While clustering errors in the standard RD model can account for the nested structure due to both repeated assessments within individuals and the nesting of students in school settings, the standard RD model cannot estimate between-group variation.

[2] As detailed in the "Multilevel Growth Model" section, our model estimates the outcomes (growth rates) simultaneously as the treatment effects. As a result, we are unable to calculate optimal bandwidths for growth rates. The optimal bandwidth for the kindergarten fall test score was 29 days (Cattaneo et al., 2018).

[3] In Online Appendix OA9, we present graphical evidence on the distinction between the compliers and always-takers and never-takers in our sample.

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Table 1

Visualization of the Birth-Year Cohorts and the School Years in which Students were Assessed

		Birthday Before Cut Date									Birthdate after Cut Date			
Cohort	Year of Birth	Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	**Cut Date**	Sep.	Oct.	Nov.	Dec.
1	2009				K: 2014-15 1st: 2015-16 2nd: 2016-17 3rd: 2017-18					**9/1/2014**		K: 2015-16 1st: 2016-17 2nd: 2017-18 3rd: 2018-19		
2	2010				K: 2015-16 1st: 2016-17 2nd: 2017-18					**9/1/2015**		K: 2016-17 1st: 2017-18 2nd: 2018-19		
3	2011				K: 2016-17 1st: 2017-18 2nd: 2018-19					**9/1/2016**		K: 2017-18 1st: 2018-19		

Note. Cohort in our study is defined by students' birth year (2009 to 2011). In this set-up, the students with birthdates right after the Cut Date are the "treated" group, as they enter school almost a year older than the students whose birthdates fall in the month right before the Cut Date. Due to the unavailability of treated group outcome data for Cohort 3, we omit the Cohort 3 dummy coefficients from Table 3, "Growth in 2nd Grade" results.

Table 2

Demographic Characteristics of the Sample for Students Within 30 Days Bandwidth

	All 3 States	State 1	State 2	State 3
Reading (BW = 30)				
Female	0.49	0.49	0.49	0.49
White	0.44	0.4	0.47	0.51
Black	0.23	0.19	0.13	0.33
Hispanic	0.16	0.21	0.11	0.09
Other race	0.17	0.20	0.28	0.08
Born after entry cutoff	0.55	0.53	0.56	0.58
Cohort 1 - 5th birthday in 2014	0.29	0.29	0.32	0.27
Cohort 2 - 5th birthday in 2015	0.35	0.34	0.35	0.36
Cohort 3 - 5th birthday in 2016	0.36	0.37	0.33	0.37
N. Students	26,172	14,714	3,008	8,450
N. Schools	1,155	706	146	303
Math (BW = 30)				
Female	0.49	0.49	0.49	0.49
White	0.42	0.36	0.47	0.51
Black	0.24	0.21	0.14	0.33
Hispanic	0.18	0.24	0.11	0.09
Other race	0.16	0.18	0.28	0.08
Born after entry cutoff	0.55	0.53	0.56	0.58
Cohort 1 - 5th birthday in 2014	0.29	0.31	0.32	0.26
Cohort 2 - 5th birthday in 2015	0.35	0.34	0.35	0.37
Cohort 3 - 5th birthday in 2016	0.36	0.35	0.33	0.37
N. Students	30,128	17,166	3,118	9,844
N. Schools	1,298	809	149	340

Note. BW=bandwidth. There is a slightly higher percentage of students in the “Born after entry cut-off” group because this group contains an extra day ($0 \leq Days_{ij} \leq 30$) compared to the “Born before or on entry cutoff” group ($-30 \leq Days_{ij} < 0$).

Table 3

HLM results for the first three model specifications

Variable	Math (I)	Math (II)	Math (III)	Reading (I)	Reading (II)	Reading (III)
			Starting RIT			
A	**5.47 (0.23)**	**5.51 (0.22)**	**5.50 (0.22)**	**4.10 (0.23)**	**4.13 (0.23)**	**4.12 (0.23)**
Control Group Fall K Score	137.8 (0.30)	140.2 (0.30)	139.4 (0.30)	134.8 (0.27)	135.9 (0.28)	135.5 (0.29)
State 1	0.61 (0.30)	0.92 (0.25)	1.78 (0.27)	0.98 (0.26)	1.14 (0.23)	1.74 (0.27)
State 2	2.65 (0.58)	1.88 (0.48)	1.28 (0.41)	1.76 (0.49)	1.35 (0.43)	0.91 (0.37)
Cohort 2	-1.32 (0.15)	-1.32 (0.15)	-1.33 (0.15)	-0.95 (0.16)	-0.97 (0.16)	-0.98 (0.16)
Cohort 3	-1.85 (0.16)	-1.86 (0.16)	-1.87 (0.16)	-1.14 (0.17)	-1.17 (0.17)	-1.17 (0.17)
Female		0.75 (0.11)	0.74 (0.11)		1.45 (0.11)	1.45 (0.11)
Black		-5.73 (0.19)	-5.01 (0.22)		-3.69 (0.20)	-3.19 (0.22)
Hispanic		-6.20 (0.23)	-5.62 (0.25)		-4.87 (0.22)	-4.44 (0.24)
Other race		-2.02 (0.24)	-1.76 (0.24)		-1.35 (0.23)	-1.15 (0.23)
% FRPL			-6.88 (1.37)			-5.51 (1.43)
% White			4.73 (0.57)			4.00 (0.60)
% Black			1.08 (0.56)			*1.04 (0.64)*
			Growth in Kindergarten			
A	**0.14 (0.03)**	**0.14 (0.03)**	**0.14 (0.03)**	**0.24 (0.03)**	**0.24 (0.03)**	**0.24 (0.03)**
Control Group K Growth	2.28 (0.03)	2.41 (0.04)	2.42 (0.04)	2.15 (0.04)	2.26 (0.04)	2.26 (0.04)
State 1	-0.06 (0.03)	-0.08 (0.03)	-0.08 (0.03)	-0.12 (0.03)	-0.13 (0.03)	-0.12 (0.04)
State 2	-0.12 (0.05)	-0.15 (0.05)	-0.14 (0.05)	-0.15 (0.05)	-0.19 (0.05)	-0.20 (0.05)
Cohort 2	0.10 (0.02)	0.10 (0.02)	0.10 (0.02)	0.07 (0.02)	0.07 (0.02)	0.07 (0.02)
Cohort 3	0.18 (0.02)	0.19 (0.02)	0.19 (0.02)	0.06 (0.02)	0.06 (0.02)	0.05 (0.02)
Female		-0.11 (0.01)	-0.11 (0.01)		0.02 (0.01)	*0.02 (0.01)*
Black		-0.20 (0.02)	-0.23 (0.02)		-0.27 (0.03)	-0.28 (0.03)
Hispanic		-0.04 (0.02)	-0.06 (0.03)		-0.18 (0.03)	-0.19 (0.03)
Other race		*-0.02 (0.02)*	-0.03 (0.02)		*-0.05 (0.03)*	-0.06 (0.03)
% FRPL			-0.06 (0.12)			-0.29 (0.14)
% White			-0.15 (0.06)			*0.00 (0.07)*
% Black			*0.05 (0.07)*			*0.02 (0.09)*
			Growth in 1st Grade			
A	**-0.15 (0.03)**	**-0.15 (0.03)**	**-0.15 (0.03)**	**-0.14 (0.03)**	**-0.14 (0.03)**	**-0.14 (0.03)**
Control Group 1st Grade Growth	2.34 (0.03)	2.46 (0.03)	2.46 (0.03)	2.18 (0.03)	2.26 (0.03)	2.24 (0.04)
State 1	-0.04 (0.03)	-0.05 (0.03)	-0.04 (0.03)	-0.04 (0.03)	-0.05 (0.03)	-0.04 (0.03)
State 2	0.00 (0.04)	-0.03 (0.04)	-0.05 (0.04)	0.07 (0.04)	0.04 (0.04)	0.03 (0.04)
Cohort 2	0.03 (0.02)	0.04 (0.02)	0.04 (0.02)	-0.06 (0.02)	-0.06 (0.02)	-0.06 (0.02)
Cohort 3	0.09 (0.02)	0.09 (0.02)	0.09 (0.02)	-0.07 (0.02)	-0.07 (0.02)	-0.07 (0.02)
Female		-0.14 (0.01)	-0.14 (0.01)		*-0.01 (0.01)*	*-0.01 (0.01)*

Black		-0.15 (0.02)	-0.15 (0.02)		-0.18 (0.02)	-0.15 (0.03)
Hispanic		-0.06 (0.02)	-0.06 (0.02)		-0.11 (0.03)	-0.08 (0.03)
Other race		*-0.01 (0.02)*	-0.02 (0.02)		*-0.03 (0.03)*	*-0.02 (0.03)*
% FRPL			-0.24 (0.11)			*-0.02 (0.11)*
% White			*-0.02 (0.05)*			0.16 (0.06)
% Black			*-0.03 (0.06)*			*-0.01 (0.08)*
Growth in 2nd Grade						
A	**-0.11 (0.03)**	**-0.11 (0.03)**	**-0.11 (0.03)**	**-0.10 (0.04)**	**-0.10 (0.04)**	**-0.10 (0.04)**
Control Group 2nd Grade Growth	1.68 (0.03)	1.74 (0.03)	1.73 (0.03)	1.77 (0.04)	1.83 (0.04)	1.81 (0.04)
State 1	*-0.01 (0.02)*	-0.01 (0.02)	0.02 (0.03)	-0.10 (0.03)	-0.10 (0.03)	-0.08 (0.03)
State 2	*0.06 (0.04)*	0.06 (0.04)	0.07 (0.04)	*0.04 (0.05)*	0.04 (0.05)	0.05 (0.05)
Cohort 2	*0.01 (0.02)*	0.01 (0.02)	0.01 (0.02)	*0.00 (0.02)*	0.00 (0.02)	0.00 (0.02)
Female		-0.06 (0.01)	-0.06 (0.01)		0.01 (0.02)	0.01 (0.02)
Black		-0.06 (0.02)	-0.08 (0.02)		-0.13 (0.03)	-0.12 (0.03)
Hispanic		*-0.02 (0.02)*	0.00 (0.02)		-0.08 (0.03)	-0.05 (0.03)
Other race		-0.06 (0.02)	-0.06 (0.02)		-0.12 (0.03)	-0.11 (0.03)
% FRPL			-0.15 (0.11)			*0.07 (0.13)*
% White			0.11 (0.05)			*0.13 (0.06)*
% Black			0.15 (0.05)			*0.10 (0.07)*
Student-level Random Effect SD						
Starting RIT	8.26	8.06	8.06	7.13	6.96	6.96
Growth in K	0.64	0.63	0.63	0.63	0.63	0.63
Growth in 1st	0.51	0.51	0.51	0.44	0.44	0.44
Growth in 2nd	0.21	0.21	0.21	0.57	0.57	0.57
School-level Random Effect SD						
Starting RIT	4.72	3.74	3.34	3.92	3.33	3.04
Starting RIT - A	1.25	1.26	1.25	1.15	1.20	1.20
Growth in K	0.43	0.42	0.42	0.42	0.41	0.40
Growth in K - A	0.19	0.18	0.18	0.19	0.19	0.19
Growth in 1st	0.34	0.34	0.34	0.32	0.31	0.30
Growth in 1st - A	0.14	0.14	0.14	*0.14*	*0.14*	*0.14*
Growth in 2nd	0.27	0.27	0.26	0.25	0.25	0.25
Growth in 2nd - A	0.12	0.12	0.12	*0.12*	*0.12*	*0.12*

Note. Robust Standard Errors in parentheses. Italicized parameters are not statistically significant. Each panel-column represents a separate regression with the panel title as the dependent variable. Model specifications are detailed in Appendix A4. Control group (students who entered kindergarten at 5 years old) estimates are presented for context. State 1 and State 2 are dummy variables. State 3 is the omitted category. Cohort 2 and Cohort 3 refer to students born in calendar years 2010 and 2011. Students born in 2009 are the omitted category. Female, Black, Hispanic, and Other race are student-level controls. % FRPL (free or reduced-price lunch eligibility), % White, and % Black are school-level controls. For brevity, summer loss estimates are excluded from this table but included in Online Appendix OA6. Some predictors of secondary interest (including C and A*C) were included in each model but excluded here.

Table 4

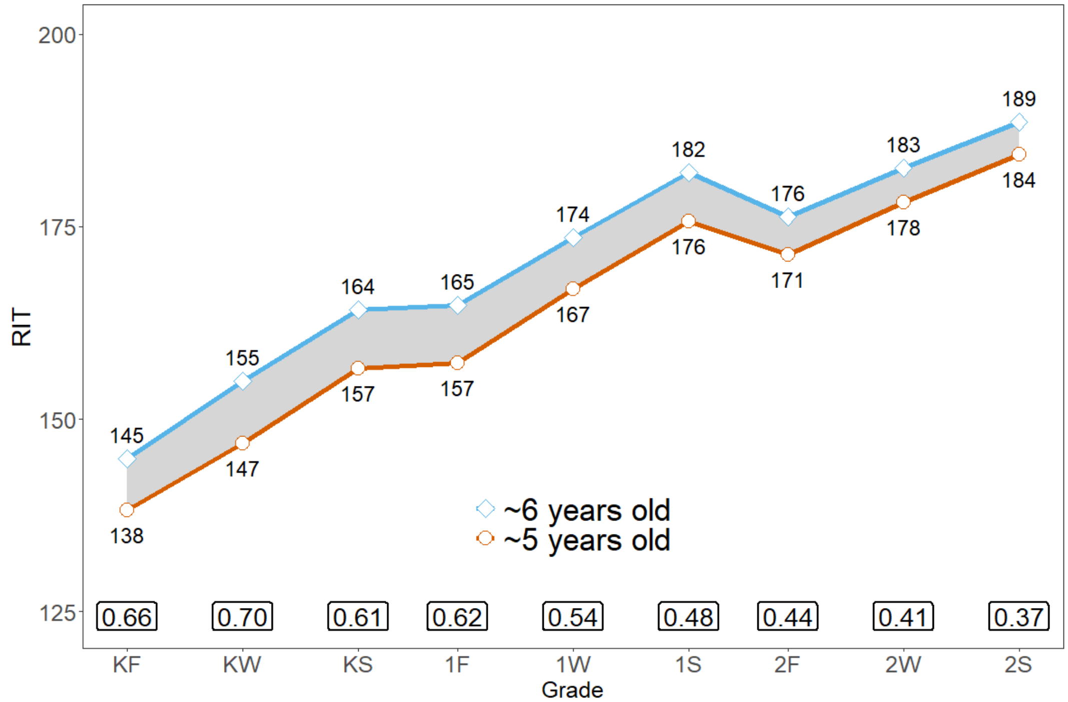
Results for the Growth Models Examining the Potential Interactions between Being Older and a set of Student Characteristics

	Math			Reading		
Variable	(IV)	(V)	(VI)	(IV)	(V)	(VI)
	Starting RIT					
A	5.72 (0.28)	5.22 (0.25)	5.30 (0.25)	4.32 (0.26)	3.67 (0.25)	3.90 (0.25)
A*State 1	*0.06 (0.26)*			*-0.06 (0.26)*		
A*State 2	-2.34 (0.43)			-1.37 (0.42)		
A*Female		0.59 (0.21)			0.93 (0.22)	
A*Black			*0.15 (0.24)*			*0.14 (0.26)*
A*Hispanic			0.96 (0.30)			1.23 (0.30)
	Growth in Kindergarten					
A	0.15 (0.03)	0.15 (0.03)	0.09 (0.03)	0.34 (0.04)	0.23 (0.03)	0.21 (0.03)
A*State 1	*-0.01 (0.03)*			-0.10 (0.04)		
A*State 2	*-0.07 (0.05)*			-0.31 (0.06)		
A*Female		*-0.02 (0.02)*			*0.02 (0.03)*	
A*Black			0.12 (0.03)			*0.15 (0.04)*
A*Hispanic			0.11 (0.03)			*0.03 (0.04)*
	Growth in 1st Grade					
A	-0.18 (0.03)	-0.14 (0.03)	-0.14 (0.03)	-0.21 (0.04)	-0.13 (0.03)	-0.13 (0.03)
A*State 1	*0.04 (0.03)*			0.09 (0.03)		
A*State 2	*0.10 (0.05)*			0.18 (0.06)		
A*Female		*-0.02 (0.03)*			*-0.01 (0.03)*	
A*Black			*-0.05 (0.03)*			*-0.01 (0.04)*
A*Hispanic			*0.00 (0.04)*			*-0.04 (0.04)*
	Growth in 2nd Grade					
A	-0.07 (0.03)	-0.11 (0.03)	-0.13 (0.03)	-0.11 (0.05)	*-0.08 (0.04)*	-0.12 (0.04)
A*State 1	-0.07 (0.03)			*0.00 (0.04)*		
A*State 2	*0.05 (0.05)*			*0.08 (0.06)*		
A*Female		*-0.01 (0.03)*			*-0.04 (0.04)*	
A*Black			*0.07 (0.03)*			*0.03 (0.04)*
A*Hispanic			*0.00 (0.04)*			*0.06 (0.05)*

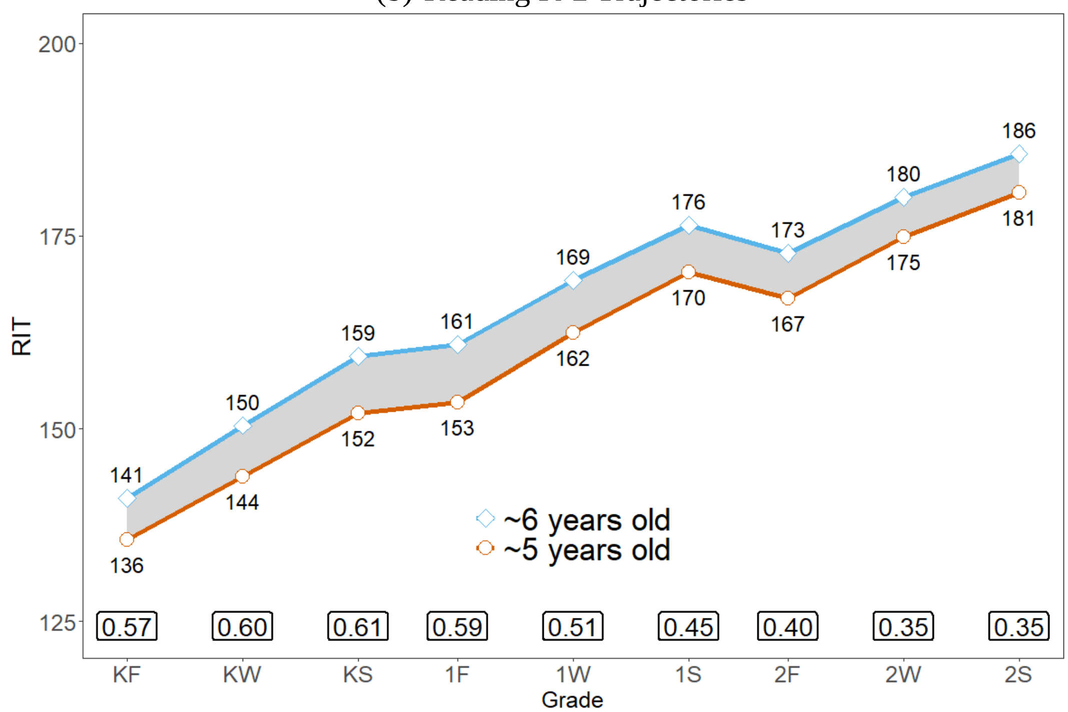
Note. Robust Standard Errors in parentheses. Italicized parameters are not statistically significant. Each panel-column represents key parameter estimates from a separate regression with the panel title as the dependent variable. Model specifications are detailed in Online Appendix OA1. State 1 and State 2 are dummy variables. State 3 is the omitted category. Female, Black, and Hispanic are student-level controls. For brevity, summer loss estimates are excluded from this table but included in Online Appendix OA6. Estimates of secondary interest (main effects) are suppressed.

Figure 1. K-2 Growth Trajectories

(a) Math K-2 Trajectories



(b) Reading K-2 Trajectories



Notes: Average trajectories (pooled across states and cohorts) of the students with birthdates within 30 days prior to cut date (circles) and students with birthdates within 30 days after the cut date (diamonds). Group means (rounded) are presented next to the lines, and standardized mean differences between the groups in each term (standardized by the pooled standard deviation calculated within each grade/term pair) are reported at the bottom of each figure.

Figure 2. Estimated Effects on Test Scores

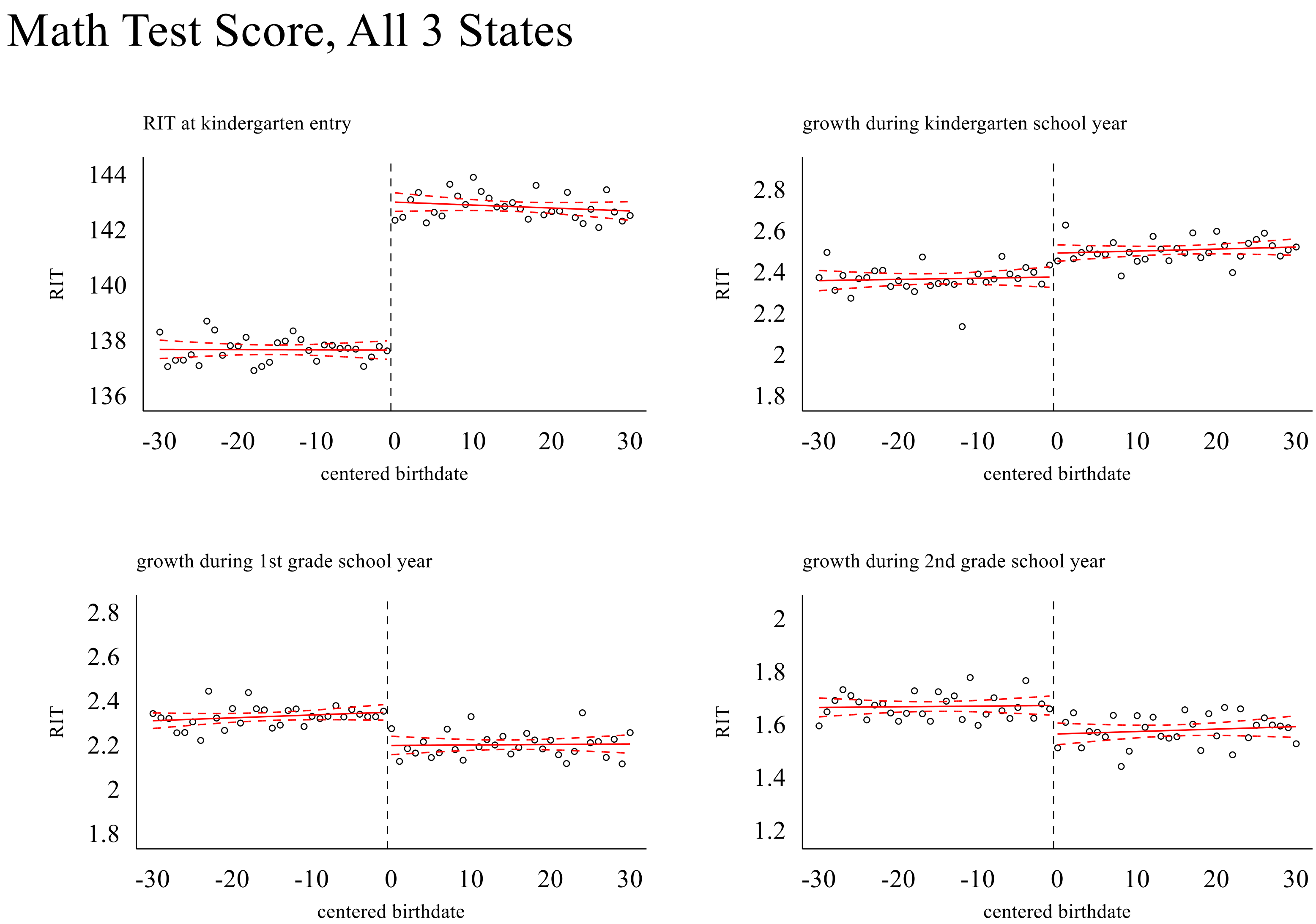
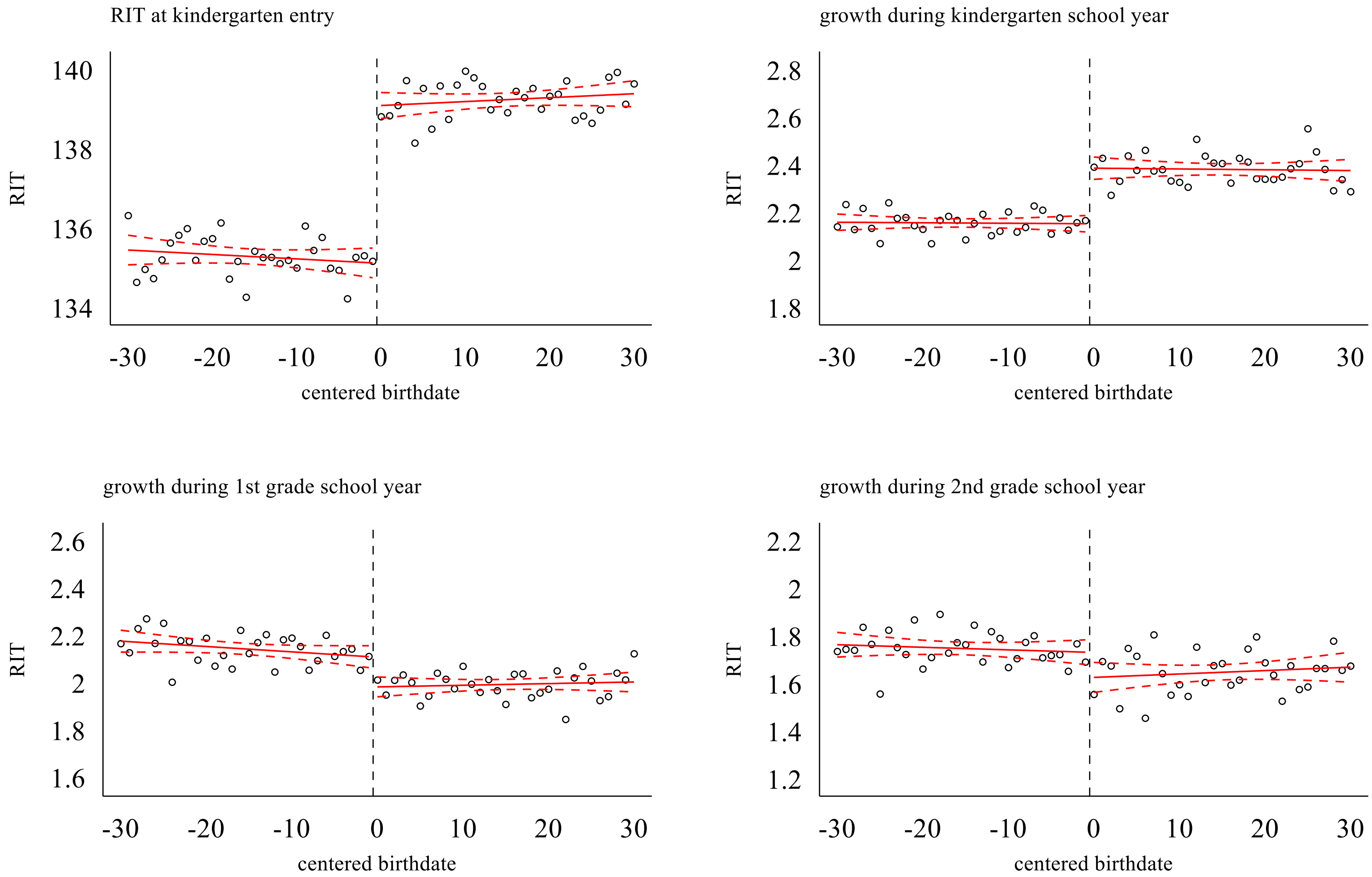


Figure 2 (continued)

Reading Test Score, All 3 States



Appendix

A1. Student Characteristics and Achievement in Fall of Kindergarten

Panel A: Student and School Demographics

Characteristics	(1) Female	(2) Black	(3) Hispanic	(4) Other Race	(5) School % Black	(6) School % White	(7) School % FRPL
Days ≥ 0	-0.004	0.005	-0.003	-0.003	0.005	0.008	-0.005*
	(0.012)	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)	(0.003)
Observations	30,552	30,552	30,552	30,552	30,552	30,552	30,552
R^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Panel B: Sample Attrition

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
Missing Test Term	K Spring	G1 Spring	G2 Spring	K Spring	G1 Spring	G2 Spring
Days ≥ 0	-0.012	0.004	0.003	-0.007	-0.011	-0.011
	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)
Observations	30,128	30,128	30,128	26,172	26,172	26,172
R^2	0.000	0.000	0.000	0.000	0.000	0.000

Panel C: Student Achievement in the Fall of Kindergarten

	(1) Math	(2) Reading
Days ≥ 0	5.187***	4.050***
	(0.242)	(0.243)
Control Group Mean	139.52	136.73
State Dummies	yes	yes
Cohort Dummies	yes	yes
Student Covariates	no	no
School Covariates	no	no
Observations	30,128	26,172
R^2	0.061	0.044

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Days ≥0 is a binary variable indicating student was born after the date of school entry cutoff. In Panel A, each column represents a regression discontinuity model with the column title as the dependent variable, linear splines, and no additional controls. Panel A, Column (1) - (4) are student-level demographic variables with binary outcomes. Panel A, Columns (5) - (7) are characteristics of students' first school from NCES Common Core of Data in 2013-2014. In Panel B, each column represents a regression discontinuity model with the column title as the dependent variable, linear splines, and no additional controls. The outcome in Panel B are "missing test score in given test term"; 1 if missing; 0 if available. In Panel C, the outcome is the observed test scores in RIT points. Consistent with Model I, in Panel B, the RD model includes linear splines, cohort dummies, and state dummies (coefficients suppressed) but no student or school covariates.

A2. Model specification

Model 1:

Our first model estimates the monthly learning rates during each school year and summer from kindergarten to second grade. At level 1, the growth model is:

$$\mathrm{y}_{tij} = \pi_{0ij} + \pi_{1ij}G0_{ij} + \pi_{2ij}S1_{ij} + \pi_{3ij}G1_{ij} + \pi_{4ij}S2_{ij} + \pi_{5ij}G2_{ij} + e_{tij}.$$

For details on how each of the level-1 predictors ($G0_{ij}$ through $G2_{ij}$) were calculated, see Online Appendix OA2. Each level-1 term is regressed on a set of student-level characteristics, including an indicator for having a birthdate after the state cutoff (A_{ij}), a measure of the distance between student's birthdate and the state's age cutoff date (Days_{ij}), and the interaction between these terms ($A_{ij} * \mathrm{Days}_{ij}$), and dummy variables at level 2 for cohort ($C1_{ij}$ and $C2_{ij}$). Additionally, student-level random effects were included to allow for random intercepts and slope terms within each grade/summer.

Level-2 Model (student (i) within school (j)):

$$\begin{aligned}
\pi_{0ij} &= \beta_{00j} + \beta_{01j}\mathrm{A}_{ij} + \beta_{02j}\mathrm{Days}_{ij} + \beta_{03j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{04j}(C3_{ij}) + \beta_{05j}(C3_{ij}) + r_{0ij}\\
\pi_{1ij} &= \beta_{10j} + \beta_{11j}\mathrm{A}_{ij} + \beta_{12j}\mathrm{Days}_{ij} + \beta_{13j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{14j}(C3_{ij}) + \beta_{15j}(C3_{ij}) + r_{1ij}\\
\pi_{2ij} &= \beta_{20j} + \beta_{21j}\mathrm{A}_{ij} + \beta_{22j}\mathrm{Days}_{ij} + \beta_{23j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{24j}(C3_{ij}) + \beta_{25j}(C3_{ij}) + r_{2ij}\\
\pi_{3ij} &= \beta_{30j} + \beta_{31j}\mathrm{A}_{ij} + \beta_{32j}\mathrm{Days}_{ij} + \beta_{33j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{34j}(C3_{ij}) + \beta_{35j}(C3_{ij}) + r_{3ij}\\
\pi_{4ij} &= \beta_{40j} + \beta_{41j}\mathrm{A}_{ij} + \beta_{42j}\mathrm{Days}_{ij} + \beta_{43j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{44j}(C3_{ij}) + \beta_{45j}(C3_{ij}) + r_{4ij}\\
\pi_{5ij} &= \beta_{50j} + \beta_{51j}\mathrm{A}_{ij} + \beta_{52j}\mathrm{Days}_{ij} + \beta_{53j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{54j}(C3_{ij}) + \beta_{55j}(C3_{ij}) + r_{5ij}
\end{aligned}$$

At the school-level (level 3), we include state dummy variables and random effects for the intercept and growth terms. Additionally, random effects were included to allow the effect of being older A_{ij} to vary randomly between schools (e.g., random effects u_{01j} to u_{51j}). All other school-level covariates were treated as fixed.

Level-3 Model (school (j)):

$$\begin{aligned}
\beta_{00j} &= \gamma_{000} + \gamma_{001}(State1_j) + \gamma_{002}(State2_j) + u_{00j}\\
\beta_{10j} &= \gamma_{100} + \gamma_{101}(State1_j) + \gamma_{102}(State2_j) + u_{10j}\\
\beta_{20j} &= \gamma_{200} + \gamma_{201}(State1_j) + \gamma_{202}(State2_j) + u_{20j}\\
\beta_{30j} &= \gamma_{300} + \gamma_{301}(State1_j) + \gamma_{302}(State2_j) + u_{30j}\\
\beta_{40j} &= \gamma_{400} + \gamma_{401}(State1_j) + \gamma_{402}(State2_j) + u_{40j}\\
\beta_{50j} &= \gamma_{500} + \gamma_{501}(State1_j) + \gamma_{502}(State2_j) + u_{50j}\\
\beta_{01j} &= \gamma_{010} + u_{01j}\\
\beta_{11j} &= \gamma_{110} + u_{11j}\\
\beta_{21j} &= \gamma_{210} + u_{21j}\\
\beta_{31j} &= \gamma_{310} + u_{31j}\\
\beta_{41j} &= \gamma_{410} + u_{41j}\\
\beta_{51j} &= \gamma_{510} + u_{51j}\\
\beta_{11j} &= \gamma_{110}\\
&\vdots\\
\beta_{55j} &= \gamma_{550}
\end{aligned}$$

Model 2:

Model 2 expands upon Model 1 by adding student-level gender and race/ethnicity indicators.

Level-2 Model (student (i) within school (j)):

$$\mathrm{y}_{tij} = \pi_{0ij} + \pi_{1ij}G0_{ij} + \pi_{2ij}S1_{ij} + \pi_{3ij}G1_{ij} + \pi_{4ij}S2_{ij} + \pi_{5ij}G2_{ij} + e_{tij}.$$

Level-2 Model (student (i) within school (j)):

$$\begin{gathered}
\pi_{0ij} = \beta_{00j} + \beta_{01j}\mathrm{A}_{ij} + \beta_{02j}\mathrm{Days}_{ij} + \beta_{03j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{04j}(C2_{ij}) + \beta_{05j}(C3_{ij}) + \beta_{06j}(Female_{ij}) \\
+ \beta_{07}(Black_{ij}) + \beta_{08j}(Hispanic_{ij}) + \beta_{09j}(OtherRace_{ij}) + r_{0ij} \\
\pi_{1ij} = \beta_{10j} + \beta_{11j}\mathrm{A}_{ij} + \beta_{12j}\mathrm{Days}_{ij} + \beta_{13j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{14j}(C2_{ij}) + \beta_{15j}(C3_{ij}) + \beta_{16j}(Female_{ij}) \\
+ \beta_{17}(Black_{ij}) + \beta_{18j}(Hispanic_{ij}) + \beta_{19j}(OtherRace_{ij}) + r_{1ij} \\
\pi_{2ij} = \beta_{20j} + \beta_{21j}\mathrm{A}_{ij} + \beta_{22j}\mathrm{Days}_{ij} + \beta_{23j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{24j}(C2_{ij}) + \beta_{25j}(C3_{ij}) + \beta_{26j}(Female_{ij}) + \\
\beta_{27}(Black_{ij}) + \beta_{28j}(Hispanic_{ij}) + \beta_{29j}(OtherRace_{ij}) + r_{2ij} \\
\pi_{3ij} = \beta_{30j} + \beta_{31j}\mathrm{A}_{ij} + \beta_{32j}\mathrm{Days}_{ij} + \beta_{33j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{34j}(C2_{ij}) + \beta_{35j}(C3_{ij}) + \beta_{36j}(Female_{ij}) \\
+ \beta_{37}(Black_{ij}) + \beta_{38j}(Hispanic_{ij}) + \beta_{39j}(OtherRace_{ij}) + r_{3ij} \\
\pi_{4ij} = \beta_{40j} + \beta_{41j}\mathrm{A}_{ij} + \beta_{42j}\mathrm{Days}_{ij} + \beta_{43j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{44j}(C2_{ij}) + \beta_{45j}(C3_{ij}) + \beta_{46j}(Female_{ij}) \\
+ \beta_{47}(Black_{ij}) + \beta_{48j}(Hispanic_{ij}) + \beta_{49j}(OtherRace_{ij}) + r_{4ij} \\
\pi_{5ij} = \beta_{50j} + \beta_{51j}\mathrm{A}_{ij} + \beta_{52j}\mathrm{Days}_{ij} + \beta_{53j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{54j}(C2_{ij}) + \beta_{55j}(C3_{ij}) + \beta_{56j}(Female_{ij}) \\
+ \beta_{57}(Black_{ij}) + \beta_{58j}(Hispanic_{ij}) + \beta_{59j}(OtherRace_{ij}) + r_{5ij}
\end{gathered}$$

Level-3 Model (school (j)):

$$\begin{gathered}
\beta_{00j} = \gamma_{000} + \gamma_{001}(State1_j) + \gamma_{002}(State2_j) + u_{00j} \\
\beta_{10j} = \gamma_{100} + \gamma_{101}(State1_j) + \gamma_{102}(State2_j) + u_{10j} \\
\beta_{20j} = \gamma_{200} + \gamma_{201}(State1_j) + \gamma_{202}(State2_j) + u_{20j} \\
\beta_{30j} = \gamma_{300} + \gamma_{301}(State1_j) + \gamma_{302}(State2_j) + u_{30j} \\
\beta_{40j} = \gamma_{400} + \gamma_{401}(State1_j) + \gamma_{402}(State2_j) + u_{40j} \\
\beta_{50j} = \gamma_{500} + \gamma_{501}(State1_j) + \gamma_{502}(State2_j) + u_{50j} \\
\beta_{01j} = \gamma_{010} + u_{01j} \\
\beta_{11j} = \gamma_{110} + u_{11j} \\
\beta_{21j} = \gamma_{210} + u_{21j} \\
\beta_{31j} = \gamma_{310} + u_{31j} \\
\beta_{41j} = \gamma_{410} + u_{41j} \\
\beta_{51j} = \gamma_{510} + u_{51j} \\
\beta_{11j} = \gamma_{110} \\
\vdots \\
\beta_{59j} = \gamma_{590}
\end{gathered}$$

Model 3:

Model 3 expands upon Model 2 by adding a set of school characteristics at level 3.

Level-2 Model (student (i) within school (j)):

$$\mathrm{y}_{tij} = \pi_{0ij} + \pi_{1ij}G0_{ij} + \pi_{2ij}S1_{ij} + \pi_{3ij}G1_{ij} + \pi_{4ij}S2_{ij} + \pi_{5ij}G2_{ij} + e_{tij}.$$

Level-2 Model (student (i) within school (j)):

$$\begin{aligned}
\pi_{0ij} &= \beta_{00j} + \beta_{01j}\mathrm{A}_{ij} + \beta_{02j}\mathrm{Days}_{ij} + \beta_{03j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{04j}(C2_{ij}) + \beta_{05j}(C3_{ij}) + \beta_{06j}(Female_{ij}) \\
&\quad + \beta_{07}(Black_{ij}) + \beta_{08j}(Hispanic_{ij}) + \beta_{09j}(OtherRace_{ij}) + r_{0ij} \\
\pi_{1ij} &= \beta_{10j} + \beta_{11j}\mathrm{A}_{ij} + \beta_{12j}\mathrm{Days}_{ij} + \beta_{13j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{14j}(C2_{ij}) + \beta_{15j}(C3_{ij}) + \beta_{16j}(Female_{ij}) \\
&\quad + \beta_{17}(Black_{ij}) + \beta_{18j}(Hispanic_{ij}) + \beta_{19j}(OtherRace_{ij}) + r_{1ij} \\
\pi_{2ij} &= \beta_{20j} + \beta_{21j}\mathrm{A}_{ij} + \beta_{22j}\mathrm{Days}_{ij} + \beta_{23j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{24j}(C2_{ij}) + \beta_{25j}(C3_{ij}) + \beta_{26j}(Female_{ij}) + \\
&\quad \beta_{27}(Black_{ij}) + \beta_{28j}(Hispanic_{ij}) + \beta_{29j}(OtherRace_{ij}) + r_{2ij} \\
\pi_{3ij} &= \beta_{30j} + \beta_{31j}\mathrm{A}_{ij} + \beta_{32j}\mathrm{Days}_{ij} + \beta_{33j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{34j}(C2_{ij}) + \beta_{35j}(C3_{ij}) + \beta_{36j}(Female_{ij}) \\
&\quad + \beta_{37}(Black_{ij}) + \beta_{38j}(Hispanic_{ij}) + \beta_{39j}(OtherRace_{ij}) + r_{3ij} \\
\pi_{4ij} &= \beta_{40j} + \beta_{41j}\mathrm{A}_{ij} + \beta_{42j}\mathrm{Days}_{ij} + \beta_{43j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{44j}(C2_{ij}) + \beta_{45j}(C3_{ij}) + \beta_{46j}(Female_{ij}) \\
&\quad + \beta_{47}(Black_{ij}) + \beta_{48j}(Hispanic_{ij}) + \beta_{49j}(OtherRace_{ij}) + r_{4ij} \\
\pi_{5ij} &= \beta_{50j} + \beta_{51j}\mathrm{A}_{ij} + \beta_{52j}\mathrm{Days}_{ij} + \beta_{53j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{54j}(C2_{ij}) + \beta_{55j}(C3_{ij}) + \beta_{56j}(Female_{ij}) \\
&\quad + \beta_{57}(Black_{ij}) + \beta_{58j}(Hispanic_{ij}) + \beta_{59j}(OtherRace_{ij}) + r_{5ij}
\end{aligned}$$

Level-3 Model (school (j)):

$$\begin{aligned}
\beta_{00j} &= \gamma_{000} + \gamma_{001}(State1_j) + \gamma_{002}(State2_j) + \gamma_{003}(\%FRPL_j) + \gamma_{004}(\%White_j) + \gamma_{005}(\%Black_j) + u_{00j} \\
\beta_{10j} &= \gamma_{100} + \gamma_{101}(State1_j) + \gamma_{102}(State2_j) + \gamma_{103}(\%FRPL_j) + \gamma_{104}(\%White_j) + \gamma_{105}(\%Black_j) + u_{10j} \\
\beta_{20j} &= \gamma_{200} + \gamma_{201}(State1_j) + \gamma_{202}(State2_j) + \gamma_{203}(\%FRPL_j) + \gamma_{204}(\%White_j) + \gamma_{205}(\%Black_j) + u_{20j} \\
\beta_{30j} &= \gamma_{300} + \gamma_{301}(State1_j) + \gamma_{302}(State2_j) + \gamma_{303}(\%FRPL_j) + \gamma_{304}(\%White_j) + \gamma_{305}(\%Black_j) + u_{30j} \\
\beta_{40j} &= \gamma_{400} + \gamma_{401}(State1_j) + \gamma_{402}(State2_j) + \gamma_{403}(\%FRPL_j) + \gamma_{404}(\%White_j) + \gamma_{405}(\%Black_j) + u_{40j} \\
\beta_{50j} &= \gamma_{500} + \gamma_{501}(State1_j) + \gamma_{502}(State2_j) + \gamma_{503}(\%FRPL_j) + \gamma_{504}(\%White_j) + \gamma_{505}(\%Black_j) + u_{50j}
\end{aligned}$$

$$\begin{gathered}
\beta_{01j} = \gamma_{010} + u_{01j} \\
\beta_{11j} = \gamma_{110} + u_{11j} \\
\beta_{21j} = \gamma_{210} + u_{21j} \\
\beta_{31j} = \gamma_{310} + u_{31j} \\
\beta_{41j} = \gamma_{410} + u_{41j} \\
\beta_{51j} = \gamma_{510} + u_{51j} \\
\beta_{11j} = \gamma_{110} \\
\vdots \\
\beta_{59j} = \gamma_{590}
\end{gathered}$$

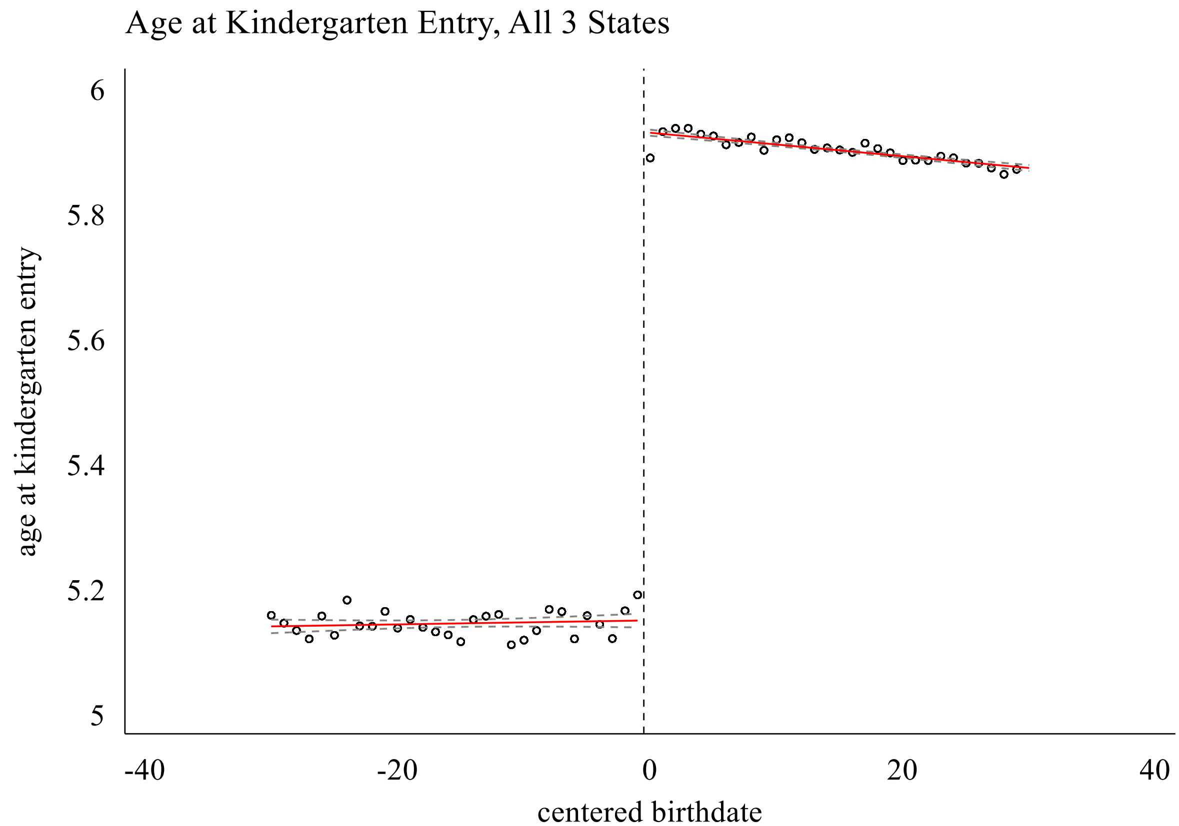
A3. Third Grade Results for Cohort 2009

	Math	Reading
Control Group Fall K Score	139.34 (0.40)	136.48 (0.43)
A	4.25 (0.45)	3.20 (0.48)
Control Group K Growth	2.24 (0.04)	2.12 (0.05)
A	0.21 (0.05)	0.24 (0.06)
Control Group Summer After K Growth	-1.21 (0.12)	-0.94 (0.13)
A	-0.05 (0.14)	0.00 (0.17)
Control Group 1st Grade Growth	2.28 (0.04)	2.18 (0.04)
A	-0.14 (0.05)	-0.17 (0.06)
Control Group Summer After 1st Growth	-2.78 (0.14)	-2.19 (0.16)
A	-0.53 (0.16)	-0.27 (0.20)
Control Group 2nd Grade Growth	1.66 (0.04)	1.60 (0.06)
A	-0.11 (0.04)	0.02 (0.07)
Control Group Summer After 2nd Growth	-2.12 (0.12)	-1.36 (0.15)
A	0.09 (0.14)	-0.03 (0.18)
Control Group 3rd Grade Growth	1.58 (0.03)	1.42 (0.04)
A	-0.16 (0.04)	-0.21 (0.06)

Notes: Robust standard errors in parentheses. Estimation uses Model I, which includes state dummies. Each column is a separate regression. Coefficient of secondary interest are suppressed for brevity. Sample is restricted to students born in calendar year 2009. Control group represents students who entered kindergarten close to five years old. A is the estimate for the impact of being a year older.

Online Appendix

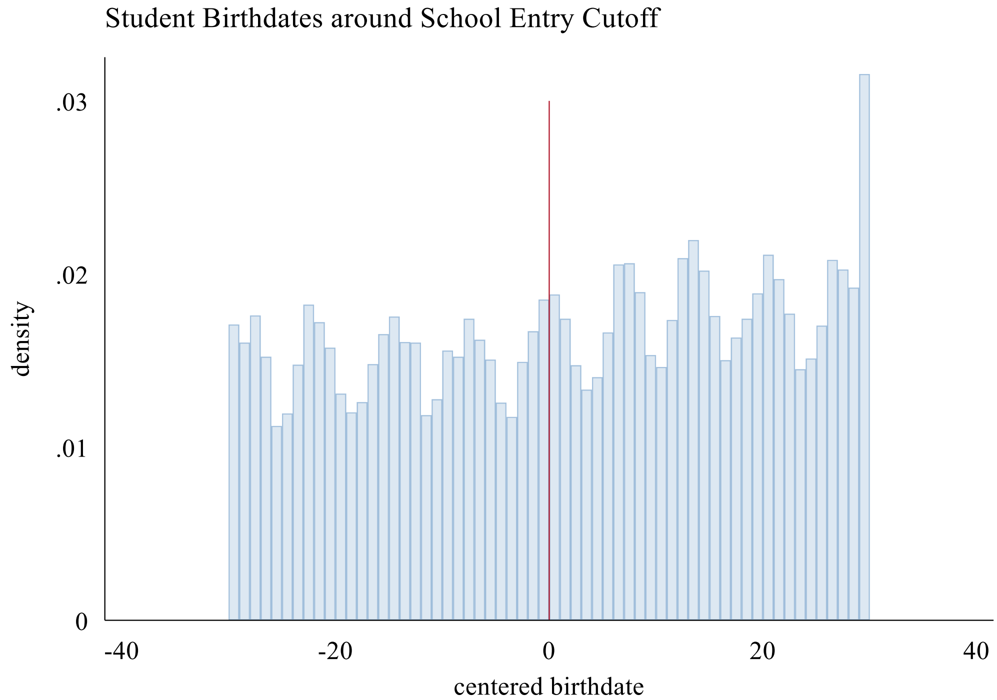
OA1. First Stage Figure and Table

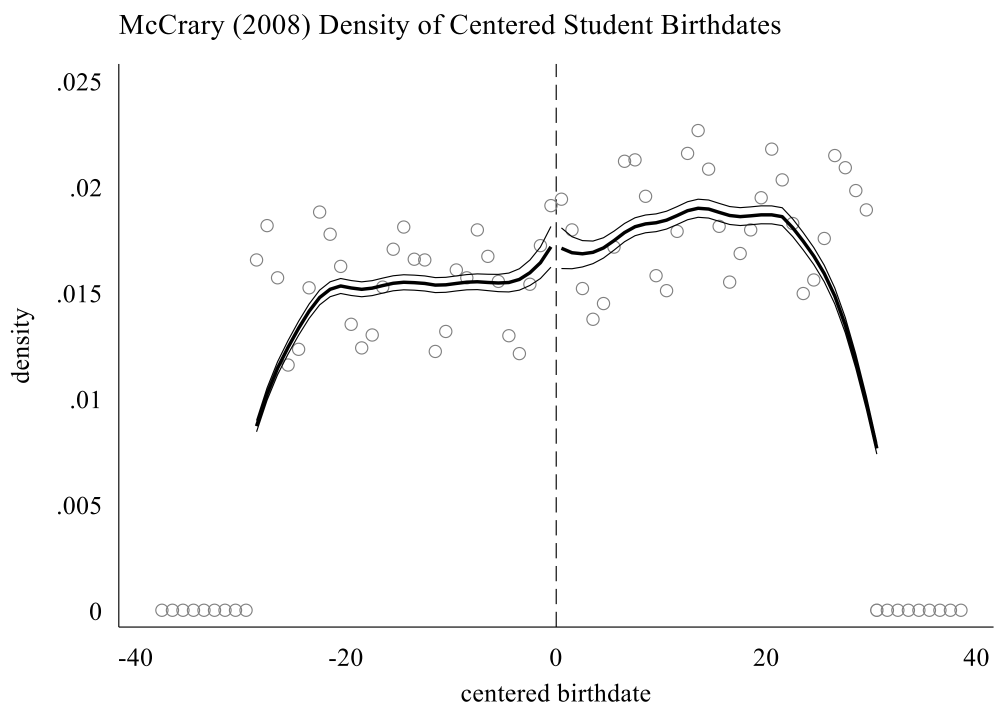


Predictor	(1) bandwidth 5	(2) bandwidth 10	(3) bandwidth 20	(4) bandwidth 30
Days ≥ 0	0.729***	0.751***	0.767***	0.780***
	(0.019)	(0.013)	(0.009)	(0.007)
Constant	5.251***	5.235***	5.212***	5.201***
	(0.020)	(0.013)	(0.009)	(0.007)
Observations	5,176	10,356	20,440	30,552
R^2	0.642	0.658	0.684	0.693
F-stat	1431	3561	7930	13303
F-stat p-value	0	0	0	0

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Each column represents a regression with bandwidth indicated in the column title. Days ≥ 0 is a binary variable indicating student was born after the date of school entry cutoff. Model also includes distance from the cutoff date, the interaction between the distance for the cutoff date and the indicator for being born after the cutoff, and student-level demographic variables.

OA2. Density Check Figures





Discontinuity Estimate = -0.017 (0.045)

OA3. Calculating Months of Exposure to School

To set up the design matrix for this seasonal learning model, we needed to calculate three sets of time variables: (a) number of months in school prior to testing, (b) total number of months spent in school across the whole school year, and (c) months of summer vacation. Time before testing was calculated as the difference between the school start date and test administration date for each student. The number of weeks before testing ranged from zero to 17 weeks in the fall and 18 to 36 weeks in the spring, with the average student testing in week four in the fall and week 33 in the spring. The total number of months in school is calculated as the end date subtracted by the school start date. The months of summer vacation is the fall school start date subtracted by the prior year spring end date. These three sets of values are used to fill in the design matrix (as shown in Table A10). For example, if a student tests in the fall of first grade, he or she has been exposed to all of kindergarten (typically 9.5 months), a couple months of summer vacation after kindergarten, and one or two months of first grade. Since he or she has not been exposed to another summer vacation or 2nd grade, the values for those predictors are set to zero

Table OA3

Coding of Monthly Exposure Rates for an Average Student Testing Between Kindergarten and 2nd Grade

							Monthly Exposure Design Matrix					
Grade/Term	School Start Date	School End Date	Test date	Exposure to Summer	Exposure to School Year Prior to Testing	Exposure to Full School Year	Int.	G0	Sum1	G1	Sum2	G2
Fall K	8/20/2015	6/12/2016	9/1/2015	NA	0.39	9.58	1.00	0.39	0.00	0.00	0.00	0.00
Spring K	8/20/2015	6/12/2016	5/1/2016	NA	8.23	9.58	1.00	8.23	0.00	0.00	0.00	0.00
Fall 1st	8/8/2016	6/4/2017	10/1/2016	1.84	1.74	9.68	1.00	9.58	1.84	1.74	0.00	0.00
Spring 1st	8/8/2016	6/4/2017	4/1/2017	1.84	7.61	9.68	1.00	9.58	1.84	7.61	0.00	0.00
Fall 2nd	8/14/2017	6/5/2018	9/5/2017	2.29	0.71	9.33	1.00	9.58	1.84	9.68	2.29	0.71
Spring 2nd	8/14/2017	6/5/2018	5/15/2018	2.29	8.84	9.33	1.00	9.58	1.84	9.68	2.29	8.84

OA4. Model Specification

Models 4-6 expands upon Model 2 by adding a set of interaction terms at level 2. We illustrate Model 5 (gender interactions) below but the logic holds for Models 4 & 6 as well.

Level-2 Model (student (i) within school (j)):

$$\mathrm{y}_{tij} = \pi_{0ij} + \pi_{1ij}G0_{ij} + \pi_{2ij}S1_{ij} + \pi_{3ij}G1_{ij} + \pi_{4ij}S2_{ij} + \pi_{5ij}G2_{ij} + e_{tij}.$$

Level-2 Model (student (i) within school (j)):

$$\pi_{0ij} = \beta_{00j} + \beta_{01j}\mathrm{A}_{ij} + \beta_{02j}\mathrm{Days}_{ij} + \beta_{03j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{04j}(C2_{ij}) + \beta_{05j}(C3_{ij}) + \beta_{06j}(Female_{ij}) + \beta_{07}(Black_{ij}) + \beta_{08j}(Hispanic_{ij}) + \beta_{09j}(OtherRace_{ij}) + \beta_{010j}(\mathrm{Days}_{ij} * Female_{ij}) + r_{0ij}$$

$$\pi_{1ij} = \beta_{10j} + \beta_{11j}\mathrm{A}_{ij} + \beta_{12j}\mathrm{Days}_{ij} + \beta_{13j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{14j}(C2_{ij}) + \beta_{15j}(C3_{ij}) + \beta_{16j}(Female_{ij}) + \beta_{17}(Black_{ij}) + \beta_{18j}(Hispanic_{ij}) + \beta_{19j}(OtherRace_{ij}) + \beta_{110j}(\mathrm{Days}_{ij} * Female_{ij}) + r_{1ij}$$

$$\pi_{2ij} = \beta_{20j} + \beta_{21j}\mathrm{A}_{ij} + \beta_{22j}\mathrm{Days}_{ij} + \beta_{23j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{24j}(C2_{ij}) + \beta_{25j}(C3_{ij}) + \beta_{26j}(Female_{ij}) + \beta_{27}(Black_{ij}) + \beta_{28j}(Hispanic_{ij}) + \beta_{29j}(OtherRace_{ij}) + \beta_{210j}(\mathrm{Days}_{ij} * Female_{ij}) + r_{2ij}$$

$$\pi_{3ij} = \beta_{30j} + \beta_{31j}\mathrm{A}_{ij} + \beta_{32j}\mathrm{Days}_{ij} + \beta_{33j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{34j}(C2_{ij}) + \beta_{35j}(C3_{ij}) + \beta_{36j}(Female_{ij}) + \beta_{37}(Black_{ij}) + \beta_{38j}(Hispanic_{ij}) + \beta_{39j}(OtherRace_{ij}) + \beta_{310j}(\mathrm{Days}_{ij} * Female_{ij}) + r_{3ij}$$

$$\pi_{4ij} = \beta_{40j} + \beta_{41j}\mathrm{A}_{ij} + \beta_{42j}\mathrm{Days}_{ij} + \beta_{43j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{44j}(C2_{ij}) + \beta_{45j}(C3_{ij}) + \beta_{46j}(Female_{ij}) + \beta_{47}(Black_{ij}) + \beta_{48j}(Hispanic_{ij}) + \beta_{49j}(OtherRace_{ij}) + \beta_{410j}(\mathrm{Days}_{ij} * Female_{ij}) + r_{4ij}$$

$$\pi_{5ij} = \beta_{50j} + \beta_{51j}\mathrm{A}_{ij} + \beta_{52j}\mathrm{Days}_{ij} + \beta_{53j}(\mathrm{A}_{ij} * \mathrm{Days}_{ij}) + \beta_{54j}(C2_{ij}) + \beta_{55j}(C3_{ij}) + \beta_{56j}(Female_{ij}) + \beta_{57}(Black_{ij}) + \beta_{58j}(Hispanic_{ij}) + \beta_{59j}(OtherRace_{ij}) + \beta_{510j}(\mathrm{Days}_{ij} * Female_{ij}) + r_{5ij}$$

Level-3 Model (school (j)):

$$\beta_{00j} = \gamma_{000} + \gamma_{001}(State1_j) + \gamma_{002}(State2_j) + \gamma_{003}(\%FRPL_j) + \gamma_{004}(\%White_j) + \gamma_{005}(\%Black_j) + u_{00j}$$
$$\beta_{10j} = \gamma_{100} + \gamma_{101}(State1_j) + \gamma_{102}(State2_j) + \gamma_{103}(\%FRPL_j) + \gamma_{104}(\%White_j) + \gamma_{105}(\%Black_j) + u_{10j}$$
$$\beta_{20j} = \gamma_{200} + \gamma_{201}(State1_j) + \gamma_{202}(State2_j) + \gamma_{203}(\%FRPL_j) + \gamma_{204}(\%White_j) + \gamma_{205}(\%Black_j) + u_{20j}$$
$$\beta_{30j} = \gamma_{300} + \gamma_{301}(State1_j) + \gamma_{302}(State2_j) + \gamma_{303}(\%FRPL_j) + \gamma_{304}(\%White_j) + \gamma_{305}(\%Black_j) + u_{30j}$$
$$\beta_{40j} = \gamma_{400} + \gamma_{401}(State1_j) + \gamma_{402}(State2_j) + \gamma_{403}(\%FRPL_j) + \gamma_{404}(\%White_j) + \gamma_{405}(\%Black_j) + u_{40j}$$
$$\beta_{50j} = \gamma_{500} + \gamma_{501}(State1_j) + \gamma_{502}(State2_j) + \gamma_{503}(\%FRPL_j) + \gamma_{504}(\%White_j) + \gamma_{505}(\%Black_j) + u_{50j}$$
$$\beta_{01j} = \gamma_{010} + u_{01j}$$
$$\beta_{11j} = \gamma_{110} + u_{11j}$$
$$\beta_{21j} = \gamma_{210} + u_{21j}$$
$$\beta_{31j} = \gamma_{310} + u_{31j}$$
$$\beta_{41j} = \gamma_{410} + u_{41j}$$
$$\beta_{51j} = \gamma_{510} + u_{51j}$$
$$\beta_{11j} = \gamma_{110}$$
$$\vdots$$
$$\beta_{59j} = \gamma_{590}$$

OA5. Summer Learning Loss Estimates for Models 1-6

Summer learning loss estimates corresponding to the HLM results presented in Table 3

	Math			Reading		
Variable	(I)	(II)	(III)	(I)	(II)	(III)
			Growth in Summer after Kindergarten			
A	**-0.14 (0.08)**	**-0.15 (0.08)**	**-0.15 (0.08)**	**-0.11 (0.09)**	**-0.11 (0.09)**	**-0.11 (0.09)**
Control Group	-1.47 (0.09)	-1.59 (0.09)	-1.61 (0.10)	-1.17 (0.10)	-1.30 (0.10)	-1.33 (0.11)
State 1	0.39 (0.07)	0.37 (0.07)	0.29 (0.08)	0.48 (0.07)	0.49 (0.08)	0.43 (0.09)
State 2	1.08 (0.10)	1.07 (0.10)	0.96 (0.11)	0.82 (0.09)	0.84 (0.10)	0.76 (0.10)
C2	-0.06 (0.05)	-0.06 (0.05)	-0.06 (0.05)	0.12 (0.06)	0.11 (0.06)	0.11 (0.06)
C3	-0.42 (0.06)	-0.42 (0.06)	-0.42 (0.06)	-0.13 (0.06)	-0.13 (0.06)	-0.13 (0.06)
Female		0.16 (0.04)	0.16 (0.04)		0.16 (0.04)	0.16 (0.04)
Black		0.09 (0.06)	0.28 (0.07)		0.15 (0.08)	0.30 (0.10)
Hispanic		0.08 (0.07)	0.11 (0.08)		0.01 (0.09)	0.05 (0.10)
Other Race		0.15 (0.06)	0.19 (0.06)		0.09 (0.07)	0.13 (0.07)
% FRPL			0.37 (0.26)			0.10 (0.29)
% White			0.13 (0.15)			0.08 (0.16)
% Black			-0.69 (0.20)			-0.52 (0.25)
			Growth in Summer after 1st Grade			
A	**-0.58 (0.09)**	**-0.58 (0.09)**	**-0.58 (0.09)**	**-0.33 (0.12)**	**-0.33 (0.12)**	**-0.33 (0.12)**
Control Group	-3.17 (0.11)	-3.77 (0.11)	-3.73 (0.12)	-2.73 (0.12)	-3.12 (0.13)	-3.07 (0.13)
State 1	0.40 (0.10)	0.36 (0.10)	0.25 (0.11)	0.50 (0.10)	0.49 (0.10)	0.40 (0.12)
State 2	1.54 (0.14)	1.64 (0.13)	1.65 (0.14)	1.23 (0.15)	1.25 (0.15)	1.19 (0.16)
C2	-0.35 (0.06)	-0.36 (0.06)	-0.36 (0.06)	-0.12 (0.07)	-0.13 (0.07)	-0.13 (0.07)
C3	-0.78 (0.08)	-0.78 (0.08)	-0.77 (0.08)	-0.40 (0.10)	-0.42 (0.11)	-0.42 (0.11)
Female		0.36 (0.04)	0.36 (0.04)		0.19 (0.06)	0.19 (0.06)
Black		0.92 (0.08)	0.98 (0.08)		0.64 (0.10)	0.69 (0.11)
Hispanic		0.84 (0.08)	0.84 (0.09)		0.47 (0.12)	0.42 (0.12)
Other Race		0.40 (0.09)	0.41 (0.09)		0.48 (0.11)	0.48 (0.11)
% FRPL			1.32 (0.40)			-0.05 (0.46)
% White			0.03 (0.20)			-0.45 (0.24)
% Black			-0.33 (0.21)			-0.52 (0.29)

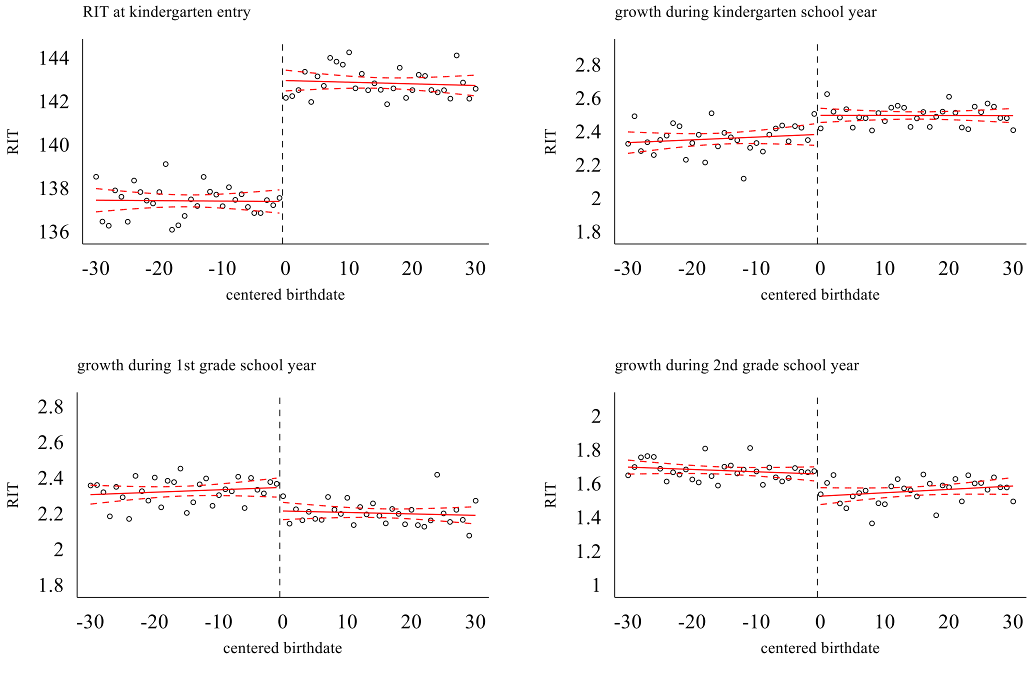
OA5. Summer Learning Loss Estimates for Models 1-6 (continued)

Summer learning loss estimates corresponding to the HLM results presented in Table 4

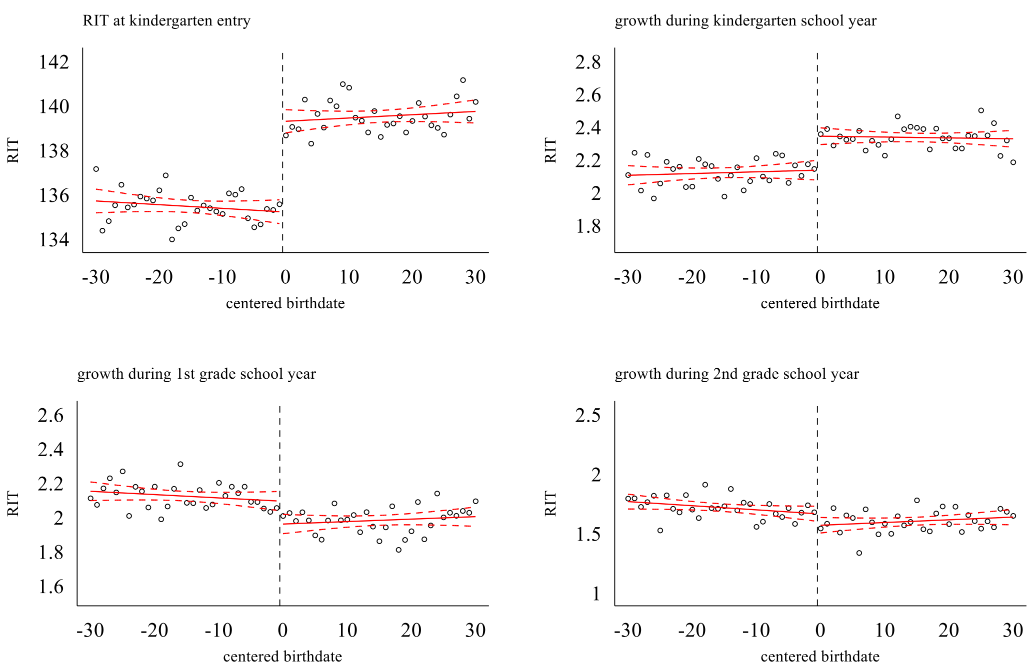
	Math			Reading		
Variable	(IV)	(V)	(VI)	(IV)	(V)	(VI)
Growth in Summer after Kindergarten						
A	-0.23 (0.10)	-0.14 (0.09)	-0.16 (0.08)	-0.30 (0.11)	0.01 (0.10)	-0.12 (0.10)
A*State 1	0.05 (0.09)			0.19 (0.10)		
A*State 2	0.33 (0.12)			0.53 (0.13)		
A*Female		-0.02 (0.07)			-0.24 (0.08)	
A*Black			0.03 (0.10)			-0.23 (0.11)
A*Hispanic			0.04 (0.10)			0.34 (0.12)
Growth in Summer after 1st Grade						
A	-0.67 (0.11)	-0.56 (0.10)	-0.51 (0.10)	-0.29 (0.14)	-0.28 (0.14)	-0.32 (0.13)
A*State 1	0.09 (0.11)			-0.08 (0.13)		
A*State 2	0.27 (0.15)			0.00 (0.18)		
A*Female		-0.04 (0.09)			-0.10 (0.12)	
A*Black			-0.11 (0.11)			0.02 (0.15)
A*Hispanic			-0.22 (0.12)			-0.13 (0.17)

OA6. Discontinuity Figures by State

Math Test Score, State 1

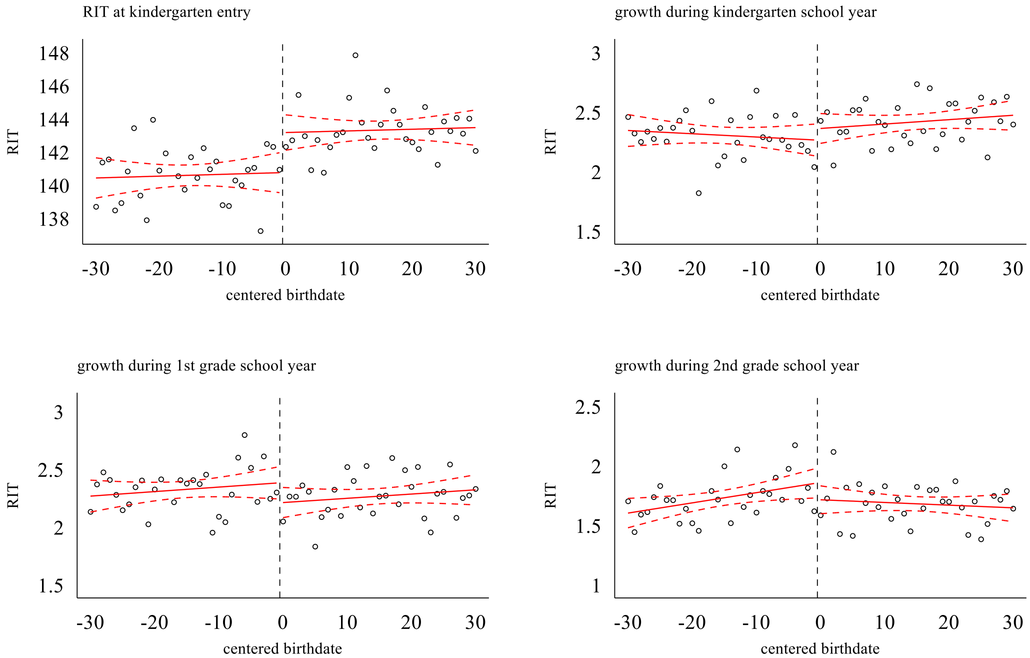


Reading Test Score, State 1

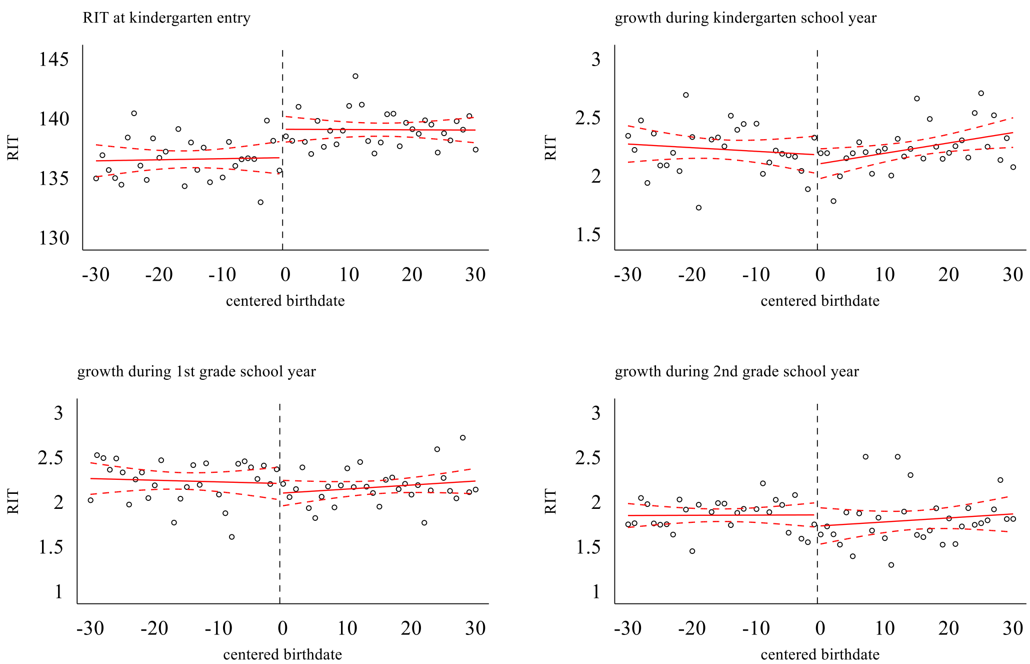


OA6. Discontinuity Figures by State (continued)

Math Test Score, State 2

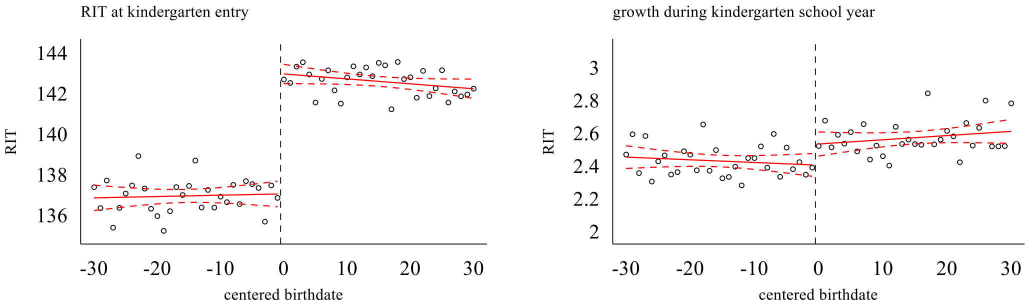


Reading Test Score, State 2

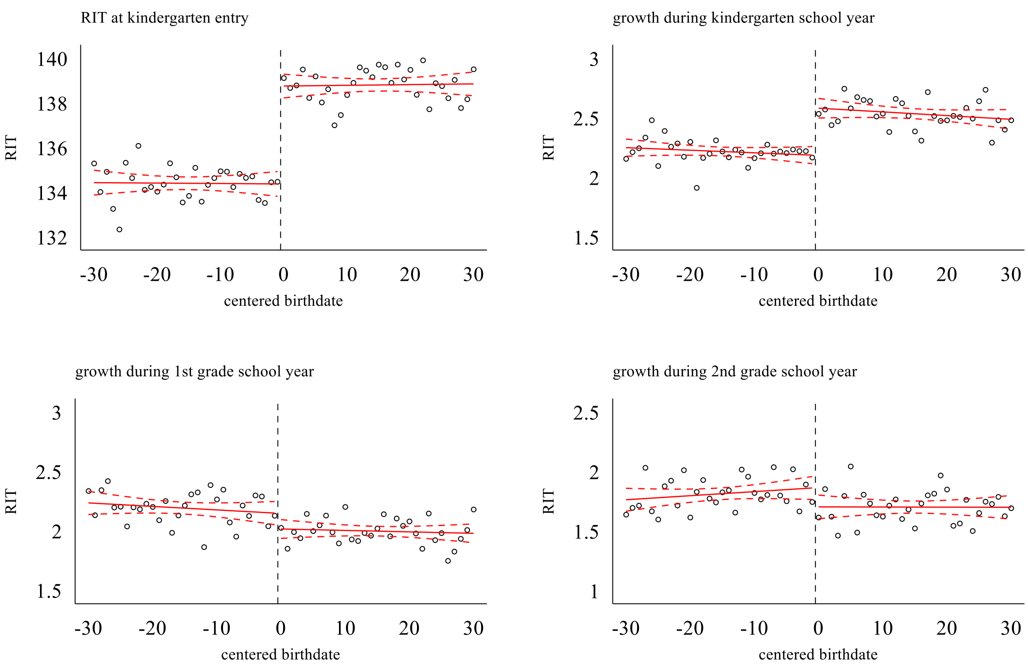


OA6. Discontinuity Figures by State (continued)

Math Test Score, State 3

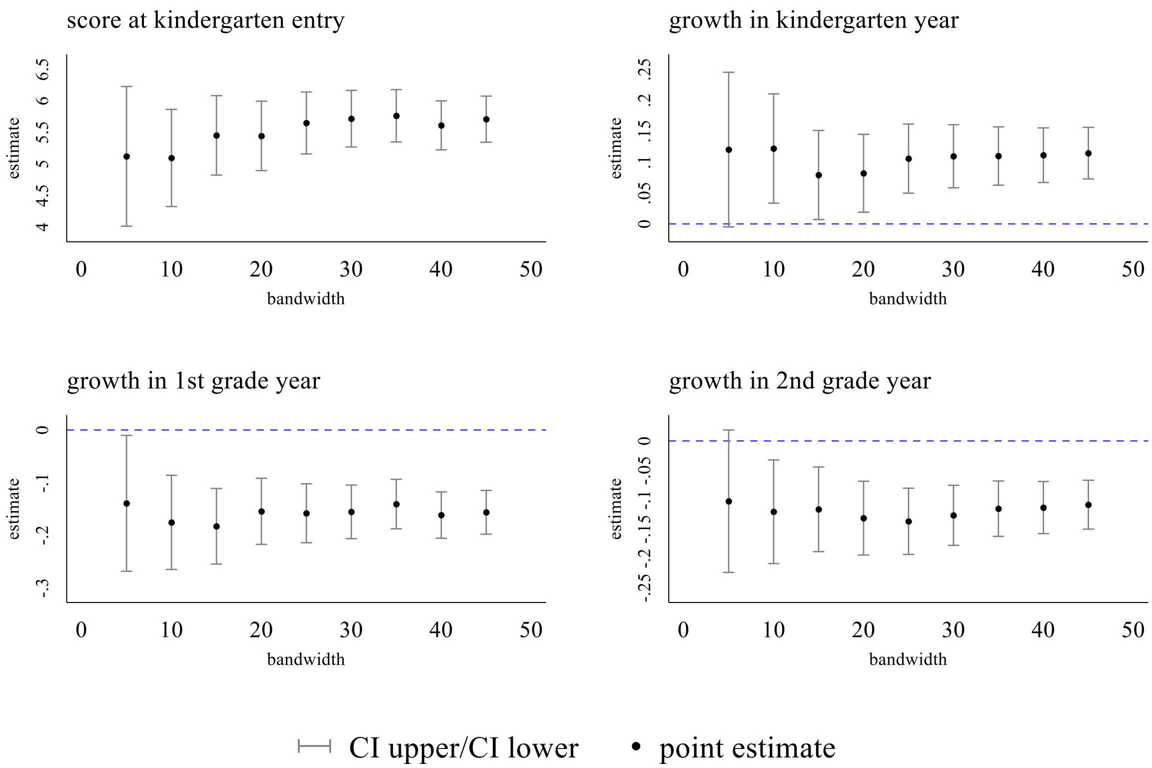


Reading Test Score, State 3

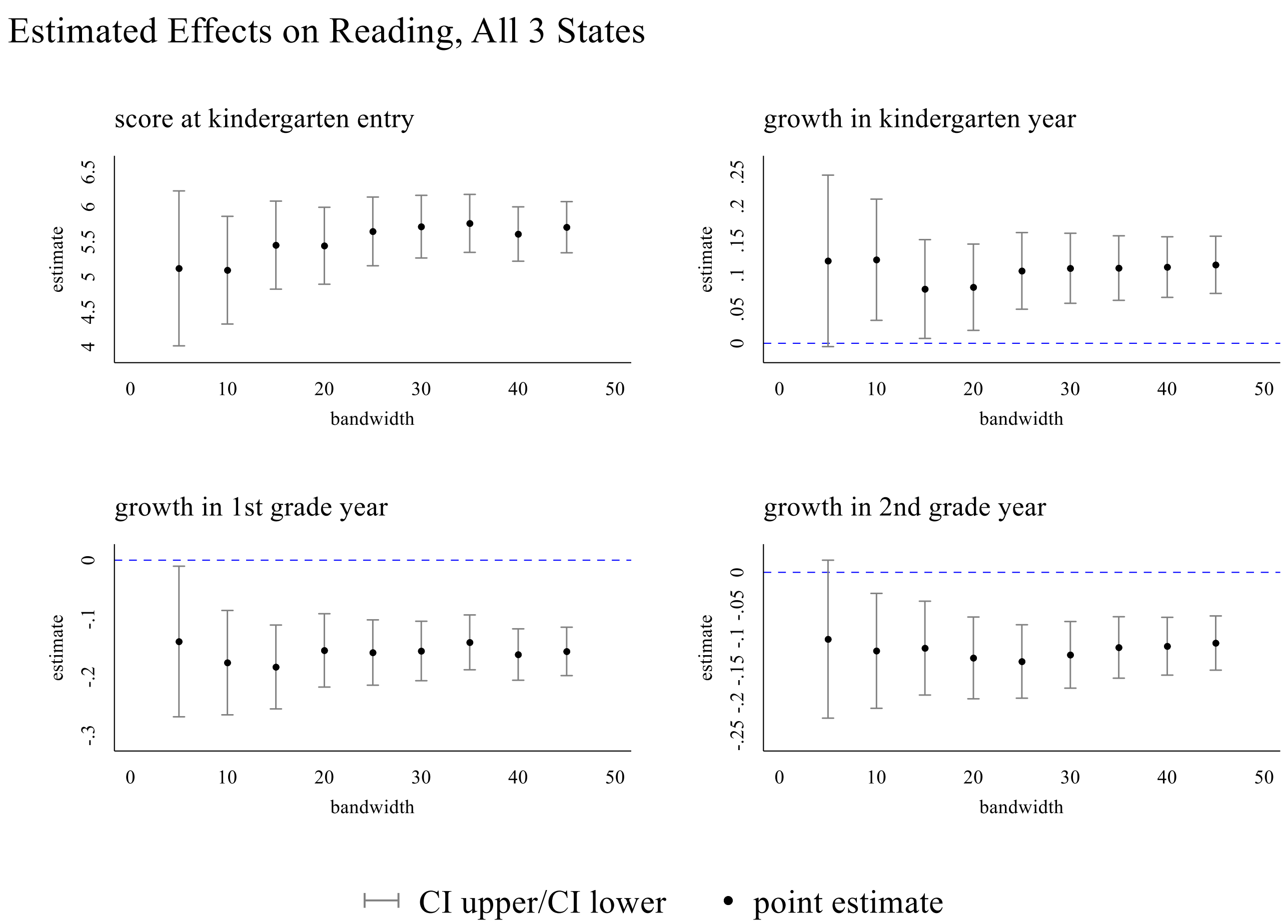


OA7. Robustness Across Bandwidths

Estimated Effects on Math, All 3 States



OA7. Robustness Across Bandwidths (continued)



OA8. Placebo Threshold Tests

As an additional test of validity for the RD design, we estimated treatment effects using placebo thresholds (10 days before the true birthdate cutoff and 10 days after the true cutoff). Results are as follows. All estimates on growth are close to zero. A few estimates are statistically significant (likely due to samples with more than 30,000 students) but practically small (in fall of kindergarten 1SD is approximately 10 RIT).

	Math		Reading	
	(1)	(2)	(3)	(4)
	-10 days	+ 10 days	-10 days	+ 10 days
Intercept	1.25	1.24	0.99	1.02
	(0.25)	(0.24)	(0.26)	(0.24)
K Growth	0.08	0.02	0.08	0.05
	(0.03)	(0.02)	(0.03)	(0.03)
G1 Growth	-0.03	0.00	-0.03	-0.02
	(0.03)	(0.03)	(0.04)	(0.03)
G2 Growth	-0.05	0.00	-0.08	-0.01
	(0.03)	(0.03)	(0.04)	(0.04)

Notes: Robust standard errors in parentheses. Estimation uses Model I, which includes state dummies. Each column is a separate regression. Estimate presented for the impact of being a year older. Coefficient of secondary interest are suppressed for brevity.

OA9. Effects Heterogeneity

In the presence of effect heterogeneity, our estimates would only be defined for the "compliers" in the sample, or children who choose the school entry age assigned to them by their state's cutoff date. We follow Dee and Sievertsen (2018) and distinguish compliers from "always-takers" and from "never-takers' in the intent-to-treat design. Students who choose a school entry age that is different from the age assigned by the state's cutoff date may be distinct from students who follow the policy in important ways. For example, if redshirted children come from more socioeconomically-advantaged families than children who enter school on time, the impact of entering a year older on redshirted children may be different from the impact on others. We interrogate generalizability by examining if the students who followed the state's cutoff date differ from those who delayed or entered early (Dee & Sievertsen, 2018; Bertanha & Imbens, 2014).

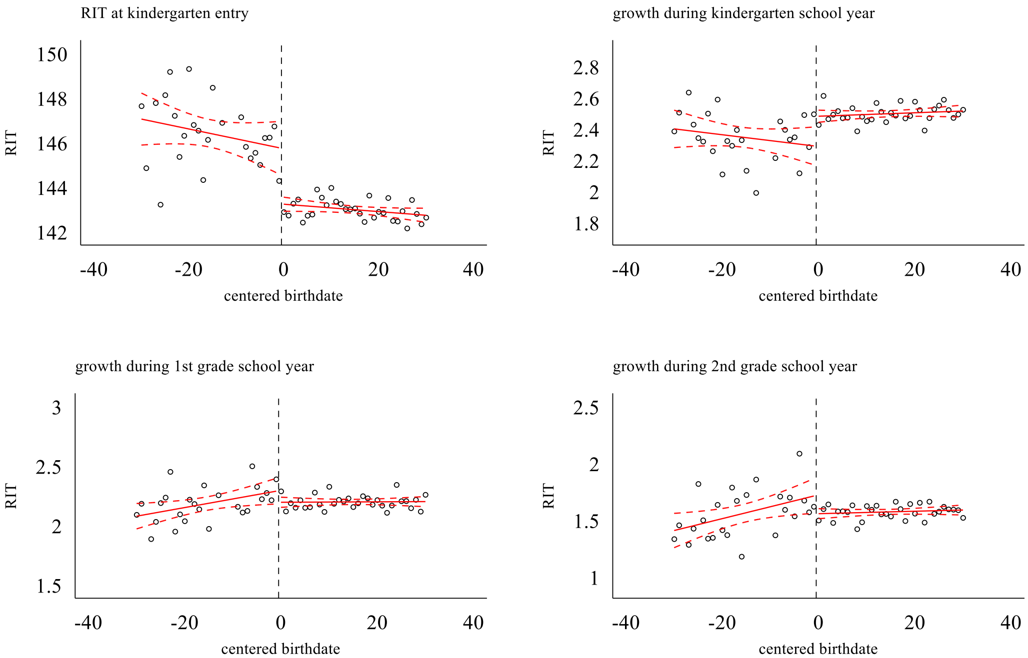
We make the reasonable assumption that no students were "defiers" who would have chosen to disobey their state's cutoff regardless of the age of entry it assigned (i.e., would have entered at six years old had the cutoff mandated entering at five, but would have entered at five years old had the cutoff mandated entering close to six). Defining treatment as entering kindergarten a year older, we refer to students who, regardless of the policy cutoff, would have entered kindergarten close to five years old as "never-takers" and students who would have entered close to or older than six as "always-takers".

We conduct this analysis graphically by plotting the initial achievement and learning rates of two subsamples. First, we focus on a subsample of students who entered kindergarten after turning five years 11 months old and compare the outcomes of students born on or before the cutoff date (always-takers, N=1,824) and students born after the cutoff date (compliers and always-takers). This comparison provides an indication for whether "redshirters" are distinct from students who enter a year older because of the state's cutoff date. Then, we focus on a subsample of students who entered kindergarten before turning five years one month old and compare the growth rates of students born on or before the cutoff date (compliers and never-takers) and students born after the cutoff date (never-takers, N=448). This comparison provides an indication for whether students who would always choose to enter kindergarten around five years old are fundamentally different from students who enter young because of the state's cutoff date. Significant differences would suggest limited generalizability of the estimated local average treatment effect to students who would always choose to enter school young or old.

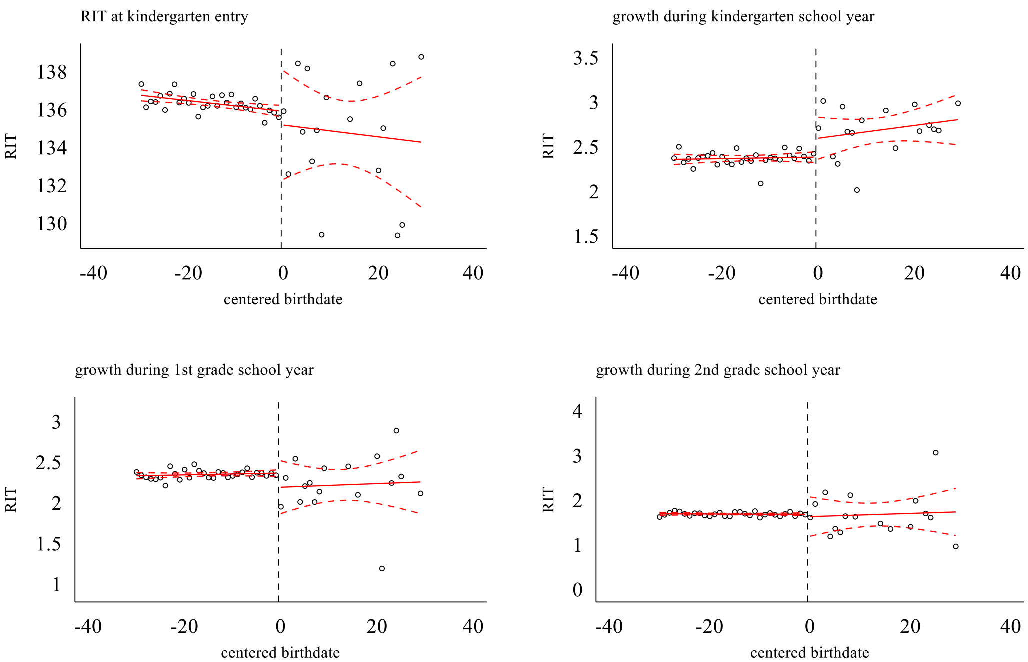
As the graphs show, always-takers (redshirted children) have higher achievement at kindergarten entry and somewhat higher growth during kindergarten than compliers and always-takers born after the cutoff, but their growth rates during 1st and 2nd grade were indistinguishable. Never-takers (children who would have always entered kindergarten young) were indistinguishable from compliers and never-takers born before the cutoff in all measures. We interpret these results as suggestive evidence that the negative estimated effects on growth rates in 1st and 2nd grade are generalizable to students who choose to enter school older or younger than their policy-mandated entry age.

OA9. Effects Heterogeneity (continued)

Compliers and Always-takers vs. Always-takers, Math

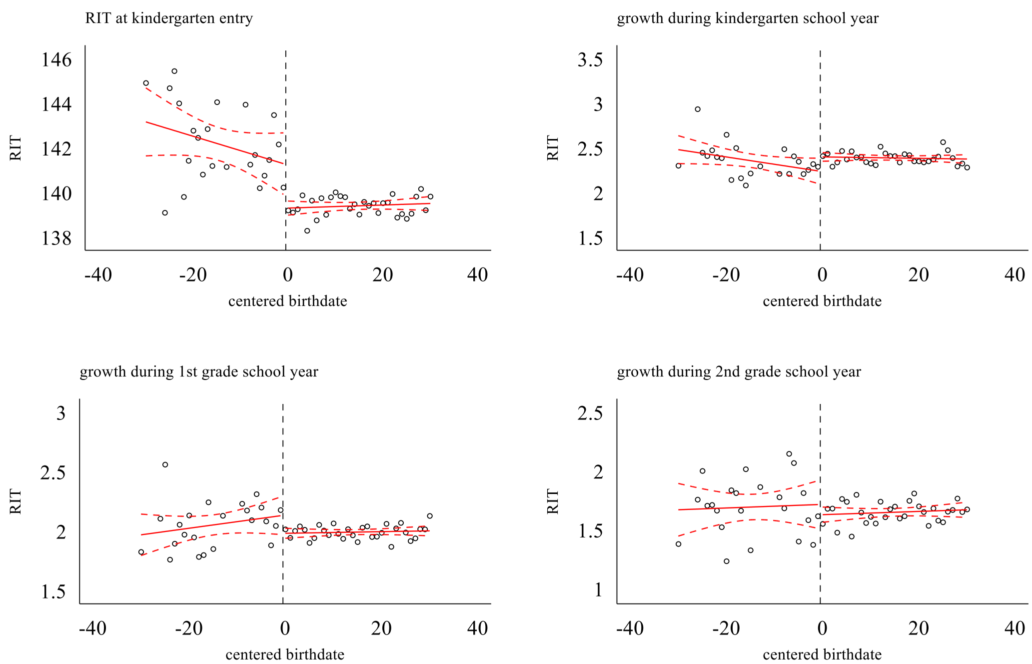


Compliers and Never-takers vs. Never-takers, Math

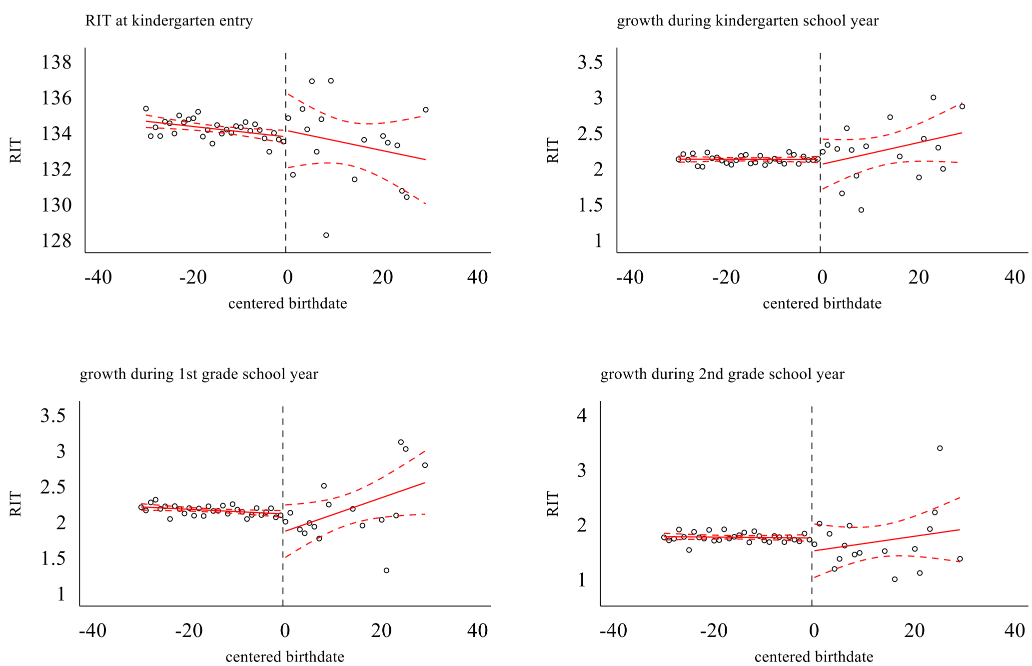


OA9. Effects Heterogeneity (continued)

Compliers and Always-takers vs. Always-takers, Reading



Compliers and Never-takers vs. Never-takers, Reading



OA10. Test Score Distributions by School Year for Students Born 60-90 Days Before or After Policy Cutoff

To see if achievement shows a time trend, we examine test scores for two school-entry-year cohorts of students who tested in the same grade level in the same academic year. Students included in this analysis were born 60 to 90 days before or after their state's school entry cutoff date. Since their centered birthdates were outside the bandwidth, these students were excluded from the main analysis. The columns "all" show summary statistics for students born before and after the cutoff. The column "born before cutoff" shows students born before the cutoff and entered school either at 5 years and 2 or 3 months or, in a relatively small number of cases, at 6 years and 2 or 3 months of age. The column "born after cutoff" shows students born after cutoff and entered school at 5 months and 9 or 10 months of age. Within each column and test term (e.g., K fall, all students), the two cohorts of students have similar test score means and standard deviations. This provides some reassurance that any differences we observe in achievement between students who enter school in different school years are not due to time trends.

Panel A: Math		all			born before cutoff				born after cutoff		
	N	mean	sd	birthyear	N	mean	sd	birthyear	N	mean	sd
K Fall											
2015-2016	11007	141.6	10.4	2009; 2010	5682	139.5	9.9	2009	5325	143.9	10.6
2016-2017	10908	141.2	10.4	2010; 2011	5545	139.5	9.9	2010	5363	142.9	10.6
K Winter											
2015-2016	8855	150.7	12.2	2009; 2010	4571	148.3	11.8	2009	4284	153.3	12.0
2016-2017	8848	150.0	12.1	2010; 2011	4505	148.1	11.9	2010	4343	152.0	12.1
K Spring											
2015-2016	10127	159.9	12.8	2009; 2010	5214	157.6	12.9	2009	4913	162.4	12.2
2016-2017	10388	159.6	12.9	2010; 2011	5304	157.7	12.8	2010	5084	161.7	12.7
G1 Fall											
2016-2017	8330	161.2	12.6	2009; 2010	4279	159.1	12.4	2009	4051	163.5	12.5
2017-2018	8924	160.8	12.8	2010; 2011	4538	158.8	12.6	2010	4386	162.8	12.6
G1 Winter											
2016-2017	7666	169.8	12.4	2009; 2010	3941	167.8	12.4	2009	3725	172.0	12.1
2017-2018	8349	170.1	12.9	2010; 2011	4266	168.4	12.8	2010	4083	171.9	12.9
G1 Spring											
2016-2017	8917	178.5	13.4	2009; 2010	4595	176.6	13.4	2009	4322	180.6	13.1
2017-2018	9256	178.5	13.9	2010; 2011	4702	177.1	13.7	2010	4554	180.1	13.9

Panel B: Reading		all			born before cutoff				born after cutoff		
	N	mean	sd	birthyear	N	mean	sd	birthyear	N	mean	sd
K Fall											
2015-2016	9309	138.3	9.8	2009; 2010	4816	136.7	9.2	2009	4493	140.1	10.1
2016-2017	9763	137.9	9.6	2010; 2011	4963	136.5	9.2	2010	4800	139.4	9.8
K Winter											
2015-2016	7556	147.1	11.3	2009; 2010	3917	144.9	10.9	2009	3639	149.4	11.4
2016-2017	7824	146.1	11.1	2010; 2011	3995	144.5	10.8	2010	3829	147.7	11.3
K Spring											
2015-2016	8612	155.3	12.7	2009; 2010	4454	153.1	12.4	2009	4158	157.8	12.6
2016-2017	9343	154.6	12.7	2010; 2011	4776	152.7	12.3	2010	4567	156.6	12.8
G1 Fall											
2016-2017	7122	157.1	13.0	2009; 2010	3673	154.8	12.4	2009	3449	159.5	13.3
2017-2018	7899	157.0	12.8	2010; 2011	4006	155.0	12.4	2010	3893	159.1	12.9

G1 Winter											
2016-2017	6478	165.5	13.8	2009; 2010	3351	163.0	13.6	2009	3127	168.1	13.5
2017-2018	7312	165.6	13.5	2010; 2011	3741	163.8	13.3	2010	3571	167.5	13.5
G1 Spring											
2016-2017	7552	173.0	14.0	2009; 2010	3904	170.9	14.0	2009	3648	175.3	13.7
2017-2018	8236	172.7	13.9	2010; 2011	4179	171.1	13.7	2010	4057	174.4	13.9