Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background, and Peers*

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Abstract

Using data from two cohorts of students, we present evidence that children who are relatively old when they enter kindergarten score higher on achievement tests and are less likely to repeat grades or suffer from learning disabilities than their younger classmates. These differences are driven by the accumulation of skill prior to school entry. The test score effects appear during the first few months of kindergarten, before much learning has taken place in school, and are especially pronounced among children from upper-income families. We do not find that the relationship between entrance age and outcomes reflects a heightened ability to learn or greater physical maturity among older children, the most common interpretations of the entrance age effect. The evidence also shows that having older classmates improves a child's test scores but increases the probability of grade repetition and learning disability diagnoses.

I. Introduction

During the past thirty years, a steadily increasing fraction of children has entered kindergarten after their sixth birthday instead of the more traditional route of beginning at age five. In October 1980 9.8 percent of five-year-olds were not yet enrolled in kindergarten; by October 2002 that figure had risen to 20.8 percent.[1] Much of this increase stems from changes in state education laws that require children to have reached their fifth birthday before a specific day to be eligible to begin kindergarten each fall. For example, in Illinois a child must turn five years old by September 1, 2006 to be eligible to enroll in kindergarten in the fall of 2006. These policy reforms are based in part on the idea that children who are older when they enter kindergarten are better equipped to learn in school.

Figures 1 and 2 present evidence that changes in kindergarten statutes have substantially increased average entrance ages. Figure 1 shows the population-weighted fraction of states with entrance cutoffs in six selected categories. In 1975, six states had cutoffs of September 14 or earlier, while 14 states had relatively late cutoffs between November 30 and January 1. An additional 15 states did not have any uniform state regulation and instead left such decisions up to individual school districts. From the mid-1970s to the mid-1990s, many states either moved their kindergarten birthday cutoff from December to September or instituted a September cutoff when there previously was no statewide mandate. By 2004, 29 states had cutoffs of September 14 or earlier, five states had cutoffs between November 30 and January 1, and only eight states had no uniform state law. Figure 2 displays the fraction of children behind the median grade for their age in 1954-1995 birth cohorts, calculated from the October Supplements to the Current Population Survey.[2] The most dramatic increases occurred among the 1969 to 1984 birth cohorts, who would have been affected by entrance cutoffs from roughly 1974 to 1989. It is clear from Figure 2 that increases in entrance ages translate into increases in average ages in later grades, so holding eventual attainment constant, recent cohorts will also tend to enter the labor market at older ages.

The idea that older children do better in school is the basis for past policy changes that led to this dramatic increase in kindergarten entrance age. The popular conception is that

[1] These figures come from tabulations of the 1980 and 2002 October Supplements to the Current Population Survey.
[2] In practice, the "fraction of children behind the median grade" is just the proportion of 5 year olds who are not yet enrolled in kindergarten, the proportion of 6 year olds not yet enrolled in first grade, and so on.

entrance age matters because it ensures that a child has the skills and maturity to learn in school. For example, children should have academic skills, such as the ability to count to ten and know the letters of the alphabet; social skills, such as the ability to share with other children and follow teachers' directions; and the maturity necessary to be apart from their parents. A flurry of recent studies from a number of countries indicates that children who are relatively older when they begin kindergarten tend to do better in elementary and secondary school.[3]

Our work is the first of which we are aware to examine the mechanisms underlying the relationship between entrance age and educational outcomes. We present evidence that older children perform better on reading and math achievement tests and are less likely to repeat early grades because they have learned more basic skills prior to entering kindergarten, either from their parents or in a structured pre-school setting. We also find that older children are less likely to be diagnosed with Attention Deficit Disorder (ADD) or Attention Deficit-Hyperactivity Disorder (ADHD), consistent with the notion that referrals to specialists are based in part on a child's maturity or school performance relative to classmates. A child's physical maturity, as measured by height, does little to explain the entrance age effect, and we also fail to find evidence that older children have a greater ability to learn in school. In our interpretation of the evidence, policies that delay kindergarten entry but do nothing to address pre-kindergarten learning are not likely to be successful in raising the achievement level of children from families that provide poor learning environments, especially children from low-income households.

We use two sources of data, the Early Childhood Longitudinal Study-Kindergarten cohort (ECLS-K) and the National Educational Longitudinal Survey of 1988 (NELS:88). The ECLS-K is a nationally representative sample of kindergarteners in the fall of 1998, when autumn cutoffs were more common. The NELS:88 is a nationally representative sample of eighth graders in the spring of 1988. Most of these students were born in the last quarter of 1973 or the first three quarters of 1974 and entered kindergarten when entrance cutoffs were generally later in the calendar year. Compliance rates with entrance cutoffs are high in both data sources, implying that entrance age cutoffs have a powerful effect on the timing of kindergarten entrance.

[3] There is a large literature about the effects of entrance age on school performance, surveyed thoroughly in Stipek (2002). de Cos (1997) also provides a survey and background in response to California state Assemblymember Kerry Mazzoni at a time when legislators were debating a bill to move the California cutoff from December 2 to September 1. Additional recent studies are listed in footnote 4 below.

We exploit the fact that entrance cutoffs generate individual-specific entrance ages among compliers that are arguably exogenous with respect to school performance. For example, a child born in October who lives in a state with a December 1 cutoff may begin kindergarten in the fall that he turns five years old. An otherwise similar child that lives in a state with a September 1 cutoff would have to wait an additional year and begin kindergarten in the fall when he turns six years old. Variation in birthdates throughout the calendar year among children who live in the same state and face the same entrance cutoff generates additional variation in age at kindergarten entry. Using these two distinct sources of variation in entrance age, we use children's predicted kindergarten entrance age if they were to begin school when first allowed by law as an instrumental variable for children's actual kindergarten entrance age in models of reading and math test scores, grade progression, and diagnoses of a variety of learning disabilities.[4] We are thus able to characterize how entrance age influences academic outcomes among the large fraction of children who comply with state entry laws.[5]

Two empirical findings point to pre-kindergarten preparation, rather than learning during kindergarten, as the mechanism underlying the entrance age effect. First, our baseline models indicate that being a year older at the beginning of kindergarten leads to a 0.53 standard deviation increase in reading test scores and a 0.83 standard deviation increase in math scores during the fall of kindergarten, a point in time so early in the academic year that very little learning has taken place in school. The entrance age effects tend to diminish as children progress through school but are sizable even in eighth grade. Second, we present compelling evidence that entrance age effects are larger among children from high socioeconomic status families than among poorer children. This pattern is consistent with a relatively high rate of accumulation of human capital among high-income children in the years prior to kindergarten, and suggests that policies intended to raise average entrance ages will exacerbate socioeconomic differences in achievement in early grades.

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[4] Previous authors who have used variation in birth date and/or school entry cutoffs as an exogenous source of variation in entrance ages include Angrist and Krueger (1991, 1992), Bedard and Dhuey (2005), Cascio and Lewis (2006), Datar (2006), Fertig and Kluve (2005), Fredriksson and Öckert (2005), Leuven *et al.* (2004), Mayer and Knutson (1999), McCrary and Royer (2005), and Strøm (2004).

[5] That is, we are estimating local average treatment effects (LATE). Compliance with state kindergarten entry laws is very high: among children with a uniform statewide entrance cutoff, 92.8 percent in the ECLS-K and 87.5 percent in the NELS:88 enter kindergarten in the year they are assigned by law. The concluding section of the paper discusses implications of our results for those who do not comply with state laws.

Finally, we present evidence that the age of a child's peers also has important effects on test scores, grade progression, and diagnoses of learning disabilities. Differences in entrance cutoffs across states generate potentially exogenous variation in the average age of kindergarten students within a school. We use this variation to show that, conditional on a child's own age, having older classmates tends to raise reading and math achievement but also increases the probabilities of repeating a grade and receiving a diagnosis of a learning disability. For example, we estimate that raising average entrance ages by moving a kindergarten cutoff from December 1 to September 1 increases grade repetition rates by 2.8 percentage points among children whose own entrance age is unaffected but who are now younger relative to their classmates. Among children forced to delay entry by a year, retention rates decrease by 14.5 percentage points. These negative peer effects likely arise from the fact that grade progression and the decision to refer a child to a behavioral specialist are partly based on judgments about how a child compares to his or her classmates, rather than based solely on an absolute standard.

The following section describes the data we use and provides an intuitive description of our identification strategy. Section III formally describes our estimation strategy, discusses our underlying identification assumptions, and presents baseline estimates of the relationship between entrance age and child outcomes. In Section IV we discuss the difficulty in separately identifying the effects of kindergarten entrance age, the child's age at the time of the test, and schooling level. Section V presents evidence of heterogeneous entry age effects, and section VI presents models distinguishing the effects of an individual student's age and the average age of his classmates. Section VII concludes. An Appendix contains additional sensitivity analyses.

II. Data and Descriptive Analysis

We analyze two sources of data: the Early Childhood Longitudinal Study-Kindergarten cohort, a nationally representative survey of kindergarteners in the fall of 1998, and the National Educational Longitudinal Study of 1988, a nationally representative survey of eighth graders in the spring of 1988. This section describes the data and sample construction and provides a descriptive analysis of the relationship between kindergarten entrance laws and entrance age.

II.A. The Early Childhood Longitudinal Study (ECLS-K)

ECLS-K is a National Center for Education Statistics (NCES) longitudinal survey that began in the fall of 1998. The NCES initially surveyed 18,644 kindergarteners from over 1000 kindergarten programs in the fall of the 1998-1999 school year. Individuals were re-sampled in the spring of 1999, the fall and spring of the 1999-2000 school year (when most of the students were in first grade), and again in the spring of 2002 (when most were in third grade). Children's parents, teachers, and school administrators were also interviewed. We use a base sample of 14,333 children who have data from at least two different interviews and non-missing kindergarten entry cutoff information. [6]

Kindergarten entrance age is computed as the child's age on September 1 of the year he or she began kindergarten. Although the ECLS-K contains information on kindergarten cutoff dates at the school level, as reported by a school administrator, we opt to use kindergarten cutoffs that are set as part of state law.[7] School level cutoffs, especially for private schools and for all schools in states without a uniform statewide cutoff, are potentially correlated with the socioeconomic status of parents or the ability level of children. Statewide cutoffs are less likely to suffer this source of bias (we return to the issue of the exogeneity of state cutoffs in the Appendix). We assign to each child the kindergarten cutoff in his or her state of residence in the fall of 1998. Some states do not have uniform state cutoffs and thus we exclude children living in those states from our analysis (see Figure 1). We compute predicted entrance age, our key instrument, as the child's age on September 1 in the year he or she was first eligible to enter kindergarten according to the state cutoff.

Our key outcomes are children's performance on math and reading tests administered in each wave and indicators that a child is retained in grade or diagnosed with a variety of learning disabilities. We use item response theory (IRT) test scores to facilitate comparability of scores across individuals and over time. This method of test scoring accounts for the fact that the difficulty level of exam questions depends on how well a student answered earlier questions on

[6] Further information about the ECLS-K sampling design can be found on the NCES website: http://nces.ed.gov/pubsearch/pubsinfo.asp?pubid=2004089. We use the ECLS-K Longitudinal Kindergarten-Third Grade Public Use Data File and the ECLS-K Restricted Use Geographic Identifier file. We do not use the ECLS-K sample weights.

[7] State of residence in the ECLS is listed in the base year ECLS-K Restricted Use Geographic Identifier file. State kindergarten cutoffs were matched to ECLS-K respondents and obtained from individual state statutes as well as from the Education Commission of the States (ECS).

the test, and on past test performance. Our measure of grade retention is an indicator that the child was in either first or second grade during the spring 2002 interview, when he or she would have otherwise been in third grade. Finally, parents are asked in each survey whether their child has been diagnosed with any of a series of learning disabilities, including attention deficit disorder (ADD), attention deficit-hyperactivity disorder (ADHD), autism, and dyslexia. We create indicators for whether a child was diagnosed with any learning disability, diagnosed with either ADD or ADHD, or diagnosed with a disability other than ADD or ADHD in any survey period.

II.B. The National Educational Longitudinal Study of 1988 (NELS:88)

NELS:88 is an NCES survey which began in the Spring of 1988. 1032 schools contributed as many as 26 eighth grade students to the base year survey, resulting in 24,599 eighth graders participating. Parent, student, and teacher surveys provide information on family and individual background and on pre-high school achievement and behavior. Each student was also administered a series of cognitive tests to ascertain aptitude and achievement in math, science, reading, and history. We again use standardized item response theory (IRT) test scores. Our central outcome measures are the eighth grade reading and math test scores and an indicator of whether an individual repeated any grade up to eighth grade. While the NELS:88 is based on a choice-based sampling design, our results are largely insensitive to the use of sample weights, so we present unweighted estimates throughout.[8]

Unlike the ECLS-K data, in the NELS:88 we do not know where a student lived when he or she entered kindergarten, nor the year they actually began kindergarten. We assign them the state cutoff in effect at the time of their kindergarten entry in their 1988 state of residence and calculate predicted entrance age in a similar manner to that in the ECLS.[9] This assignment induces some measurement error in predicted entrance age, but not actual entrance age, resulting in a decrease in the precision of 2SLS results.[10] The consistency of the estimates is not affected.

[8] The sampling scheme in the NELS:88 is further explained on the NCES website: http://nces.ed.gov/surveys/nels88/

[9] State of residence in the NELS can be inferred from detailed information on zip code characteristics of the eighth grade school on the NELS:88 Restricted Use files. State kindergarten cutoffs were matched to NELS respondents and obtained from individual state statutes and the Education Commission of the States (ECS).

[10] Lincove and Painter (2006) also discuss this source of measurement error.

We assume children began kindergarten in the fall of 1979 if they had not skipped or repeated grades prior to the eighth grade interview. The NELS includes retrospective reports on grade progression, which we use to calculate the year of kindergarten entry for kids who skipped or repeated a grade.[11] Kindergarten entry age is computed as the child's age on September 1 in the year he or she entered kindergarten. Some states do not have uniform state cutoffs, so we exclude children living in those states from our analysis.

II.C. Kindergarten Entry Laws and Children's Entrance Age

This section provides an intuitive description of our strategy to identify the effect of kindergarten entrance age on educational outcomes. Compliance rates with kindergarten entrance laws are high, so the laws exert a powerful influence on children's entrance age. Figure 3 shows the relationship between state cutoffs and the average date of birth of children in the NELS:88 sample. The size of the data point is proportional to the number of children in each cell, with September 1 being the largest cell. Later cutoffs induce nearly month-for-month increases in the average entrance age of children, with kindergarteners who live in states with December 1 cutoffs being three months older on average than kindergarteners who live in states with a September 1 cutoff.

Figure 4 shows the relationship in the NELS:88 between birth month, actual entrance age, and a child's entrance age if he or she entered in the year first allowed by law, in states with a September 1 cutoff. Recall that we define "predicted entrance age" as a child's entrance age if he or she started in the year first allowed by law. [12] Among children born before September 1, variation in birth month is associated with a nearly month-for-month decrease in actual entrance age. Children born in September, however, are born after the cutoff and are required to wait until the following fall to enroll in kindergarten. Hence, predicted entrance age jumps by 11 months between kids with August birthdays and kids with September birthdays. Non-compliance on

[11] Recall bias in retrospective reports will induce a mechanical relationship between entrance age and grade retention, biasing OLS estimates of the effect of entrance age on retention. Consistency of our 2SLS strategy will not be affected. The NELS:88 includes both parental and student reports of grade retention, and the results reported below are largely insensitive to whether we use parental reports, student reports, or only cases in which the two reports agree (which occurs in 94.1% of cases).

[12] For example, a child born on October 1, 1993 would be 4 years and 11 months old on September 1, 1998, the assumed beginning of the school year. If his state cutoff was November 1, he could enter kindergarten in the fall of 1998, and his "predicted entrance age" would be 4 and 11/12, or 4.92. If his state cutoff was September 1, he would have to wait until the fall of 1999 to enter kindergarten, and his "predicted entrance age" would be 5.92.

either side of the entrance cutoff reduces the size of the discontinuity in average entrance age to less than eleven month (a similar pattern is found in ECLS-K). Our instrumental variables estimates below use predicted entrance age as an exogenous source of variation in actual entrance age. Figure 4 suggests this first-stage relationship is strong. The t-statistics associated with predicted entrance age in our first-stage regression (described in more detail below) are 48.2 in the ECLS and 50.9 in NELS:88, with partial R^2 values of 0.36 and 0.21, respectively.

Figure 5 shows the reduced-form relationship between predicted entrance age and the average percentile score on the fall 1998 reading and math tests in the ECLS-K. Both test scores have a strong, positive relationship with predicted entrance age. The oldest entrants, with predicted entrance ages of six years and two months, score at roughly the 65th percentile on the reading test and the 73rd percentile on the math test, on average. The youngest entrants are at approximately the 40th percentile on both tests, on average. Reduced-form regressions of test scores on predicted entrance age produce coefficients of 10.4 percentile points and 17.1 percentile points for the reading and math tests, respectively. The relationship between test scores and predicted entrance age appears nearly linear, and we cannot reject linearity at conventional significance levels.

This variation in predicted entrance age is driven by two sources: differences in birthdates within a state and differences in cutoff dates across states. Figure 6 shows both sources of variation by showing the relationship between calendar month of birth and math percentile scores in states with cutoffs of either August 31 or September 1, and cutoffs of either December 1 or 2. In states with a December 1 or 2 cutoff, the oldest children in the class are born in December, but after the cutoff, and the youngest children are born in November. Math test scores are steadily declining from one birth month to the next in these states, but with a sharp increase in scores between November and December births. November births score about 13.2 percentile points less than their classmates with December births, implying a one-year entrance age effect of 14.4 (= 13.2 / (11/12)). By contrast, in states with an August 31 or September 1 cutoff, children born in August are the youngest in the class, while those born in September are the oldest. In these states, there is a clear discontinuity in test scores between kids born in August and those born in September, with the 14.1 percentile point differential corresponding to a one-year entrance age effect of 15.4.

A second type of comparison evident in Figure 6 is between children born later in the calendar year in the two groups of states. For example, children born in October in states with a December 1 or 2 cutoff will tend to enter kindergarten when they are four years and ten months old, whereas children born at the same time but who live in states with an August 31 or September 1 cutoff will enter a year later, when they are five years and ten months old. The difference in reading test scores between these two groups is 17.0 percentage points. This "between-group" estimate is slightly larger than the "within-group" estimates of 14.4 and 15.4 above and is similar to the pooled estimate of 17.1 from Figure 5.

The test score comparisons in Figures 5 and 6 provide the essence of the identification strategy described below. Within a state, or within a group of states that share the same kindergarten cutoff, variation in birthdays throughout the year generates variation in entrance ages. Children born just before the cutoff will tend to be young upon entry and children born just after the cutoff will tend to be relatively old upon entry. Moreover, differences in cutoffs across states imply that children born on the same day, but who live in different states, will end up entering kindergarten at different ages. Our main results below use these two sources of variation simultaneously. We also use the two sources separately to perform a sensitivity analysis and overidentification tests in the Appendix.

III. Using Predicted Entrance Age to Identify the Entrance Age Effect

III.A. Specification and Identification Issues

We begin with a simple specification of the effect of entrance age on child education outcomes. We instrument actual entrance age with the age a child would enter if he or she began kindergarten when first allowed by state law. The baseline model is given by the system

(1) $$Y_i = \alpha EA_i + X_i\gamma + \varepsilon_i$$

(2) $$EA_i = \beta PEA_i + X_i\delta + \nu_i$$

where i indexes children, Y_i is the outcome of interest, EA_i and PEA_i are actual and predicted entrance age, and X_i represents a vector of demographic, family background, city type, region, and child characteristics that may influence outcomes and actual entrance age. ε_i represents unobserved determinants of outcomes, including child ability, and ν_i represents unobserved determinants of children's entrance age, which may also include a child's ability and maturity, as well as parental characteristics.[13] The coefficient α represents the effect of entrance age on outcomes. For now, we assume that α does not vary across children. OLS models of equation (1) will deliver consistent estimates of α if $Cov(\varepsilon_i, EA_i) = 0$, a condition which is not likely to be satisfied because parents choose whether to start their children in kindergarten on time, delay entry, or enter early based on children's maturity and ability. Two-stage least squares (2SLS) estimates of α will be consistent if $Cov(\varepsilon_i, PEA_i) = 0$.

Our vector of covariates includes indicators for gender, race, and ethnicity. We also condition on family structure, the marital status of the child's primary caregiver, Census region, urbanicity, parental education, household income, family size, and quarter of birth. Since we analyze several years of data from the ECLS, our covariates for these models reflect characteristics in each year. The covariates in the NELS:88 refer to characteristics when the child was in eighth grade.

As mentioned above, consistent estimation of α is based on the exogeneity of two distinct sources of variation in predicted kindergarten entrance ages: differences in months of birth across children and differences in kindergarten cutoff dates across states. Bound and Jaeger (2000) discuss a large body of evidence showing correlations between season of birth and family background, education, and earnings, especially among older generations of Americans. Although we find only small, statistically insignificant associations between family background and children's quarter of birth, we include quarter of birth indicators in all outcome models. The inclusion of these indicators does not substantially affect our parameter estimates, nor does including a linear trend in calendar month of birth or individual month of birth indicators. The identification strategy would also be invalid if parents sort into states based on kindergarten cutoffs or if states choose their cutoffs based on factors correlated with average characteristics of children in the state. Our main results include Census region indicators, which control for regional variation in child ability. The results are also robust to the inclusion of state fixed

[13] Since actual entrance age and predicted entrance age will always differ by a whole year (or two in rare cases), one can think of $X_i\delta + \nu_i$ in equation 2 as being a linear approximation to a function that takes the value of one if the child delays entry by a year, zero if he enters on time, and negative one if he enters early.

effects, which leverages only within-state variation in birthdates. The Appendix explores the robustness of our results to these and other specification issues.

Since the sampling frames in our data are a kindergarten entry cohort and an eighth grade cohort, rather than birth cohorts, our models compare children born in different years who entered school at the same time. For example, among October births in the ECLS data, those who enter kindergarten at age 4 years and 10 months were generally born in October 1993, while those who enter at age 5 years and 10 months were generally born in October 1992. If we instead had a birth cohort, such as a sample of all children born in October 1992, variation in entrance age would be driven by children who enter kindergarten in different years (i.e. those who enter in 1997 versus those who enter in 1998). These two methods, following birth cohorts versus following kindergarten entry cohorts, will not necessarily produce similar estimates if birth year or entry year has independent effects on outcomes.

Finally, note that 2SLS estimation of equations (1) and (2) does not require full compliance with entry cutoffs, nor does it require that non-compliance be random. Full compliance would imply equality of entrance age and predicted entrance age, so OLS and 2SLS would deliver identical results. Similarly, if non-compliance were random, OLS estimation of equation (1) would be consistent and there would be no need for 2SLS.

III.B. Basic Specification Results

Tables 1 through 3 present OLS and 2SLS estimates of the effect of school entrance age on test scores, grade retention, and learning disability diagnoses in the ELCS. Table 1 shows results from models of reading test scores from fall 1998, spring 1999, spring 2000, and spring 2002. For a child that follows the normal grade progression, these test dates correspond to the fall and spring of kindergarten, the spring of first grade, and the spring of third grade. Column (1) shows results from an OLS regression of reading test scores on entrance age without any control variables. Being a year older at kindergarten entry raises test scores by 3.79 points, which is 14 percent of the average score of 27.5 and 38 percent of the standard deviation of scores. The estimate is quite precise, with a standard error of 0.31.[14] Column (2) includes the full set of control variables. The OLS estimate is essentially unchanged, which indicates that entrance age

[14] All standard errors are adjusted for clustering at the school level.

is largely uncorrelated with observable determinants of test scores. Columns (3) and (4) present 2SLS estimates with and without control variables. Both of these estimates are larger than the corresponding OLS estimates, indicating delayed entry being more common among students who would otherwise have low test scores and early entry is more common among high-scoring students. The 2SLS estimate with controls indicates that being a year older at kindergarten entry increases test scores by 5.28 points, with a standard error of 0.47. This estimate corresponds to 19 percent of the mean test score and 53 percent of the standard deviation in scores. Finally, in column (5) we express the reading test score as a percentile within the ECLS sample (ranging from 1 to 100 with a mean of 50). The 2SLS estimate that includes control variables shows a 16.68 percentile point effect of entrance age, with a standard error of 1.28.

Models of spring 1999, 2000, and 2002 reading test scores indicate a lasting effect of entrance age on achievement, though the effect declines over time. An additional year of age at entry is associated with increased percentile reading scores of 19.33 points in spring 1999, 14.08 points in spring 2000, and 11.08 points in spring 2002. All of these effects are precisely estimated, with t-ratios of 7.9 or higher. Note that the raw IRT scores are measured on the same scale in all survey periods, so test scores increase on average from 27.5 in fall 1998 to 107.5 in spring 2002 and become more dispersed over time. As a result, a given percentile-point effect in will correspond to a larger IRT score effect in later years than in fall 1998.

To put the size of these effects into perspective, the coefficients on log family income and mother's education in IRT test score models are approximately one and 0.8, respectively, in fall 1998. Therefore, an additional year of age at kindergarten entry increases average fall kindergarten reading scores by more than five times as much as raising family income by one log point and by 6.6 times as much as a one year increase in either parent's education.

Table 2 presents estimated effects of entrance age on math test scores in the ECLS. The estimates imply large positive effects, with 2SLS estimates in model (5) indicating that an additional year of age at the time of kindergarten entry is associated with a 24.03 percentile point increase in initial math scores and an 11.54 percentile point increase in math scores in the spring of 2002. In the first two years of the survey, the effect of entrance age on math scores is larger than the effect on reading scores, and the effects are of equal size in the spring of 2002.

Table 3 presents estimates of the effect of entrance age on the probability of repeating a grade in school and receiving a learning disability diagnosis. The grade repetition measure is taken from the spring 2002 interview, when on-track children should be in third grade, and is equal to one for children who are in first or second grade. 2SLS estimates show that children who enter at older ages are significantly less likely to repeat a grade. Being a year older at entry reduces the probability of grade retention at any point between kindergarten and second grade by 13.1 percentage points, a strikingly large effect relative to the sample probability of 8.8 percent.

Each survey period, the NCES asks parents of ECLS children whether their child had "been evaluated by a professional in response to {his/her} ability to pay attention or learn." Parents who answered in the affirmative were asked if they received a diagnosis, and what the diagnosis was. The most common diagnoses are dyslexia and learning disabilities, ADD, ADHD, and a developmental delay. We analyze an indicator variable that is equal to one if the child was diagnosed in any round of the survey with any type of condition, and we also consider ADD and ADHD diagnoses separately from diagnoses of other learning disabilities. The results for the overall disability measure are presented in the second row of Table 3. The baseline diagnosis rate is 8.8 percent, and the 2SLS estimate of the effect of entrance age indicates that being a year older at the time of kindergarten entry reduces the probability by 2.5 percentage points, which represents both the effect on being referred to a specialist and the effect of receiving a positive diagnosis. The remaining rows of the table show that ADD and ADHD diagnoses account for the entire entrance age-disability gradient, with other learning disabilities having essentially no relationship with entrance age.

A large literature has documented the association between ADD and ADHD diagnoses and a child's "season of birth". The results of Table 3 are insensitive to the inclusion of controls for season or month of birth, implying that it is not season of birth, per se, but a child's exogenously determined age of entry into kindergarten that influences ADD and ADHD diagnoses.[15] An additional year of age at entry decreases the probability of an ADD or ADHD diagnosis by 67 percent (= -0.029 / 0.043) relative to the baseline diagnosis rate. This sharp gradient may reinforce the popular notion that ADD and ADHD diagnoses are more subjective than diagnoses of mental retardation and learning disabilities such as dyslexia. Some ADD and ADHD diagnoses may simply reflect a lack of emotional maturity among young kindergarten entrants; alternatively, the oldest children in a class may be under-diagnosed because their disabilities are masked in comparison to the behavior of younger classmates. Distinguishing

[15] Goodman et al (2003) survey the recent psychological literature and reach a similar conclusion.

between these hypotheses is beyond the scope of this paper, but the results suggest that future research into the mechanisms of ADD and ADHD diagnoses may prove fruitful.

Finally, Table 4 presents estimates of the effect of kindergarten entrance age on eighth grade reading and math test scores and on the probability of grade retention between kindergarten and eighth grade in NELS:88. Column (1) shows OLS estimates without controls that indicate a negative relationship between entry age and reading and math scores. The addition of controls in column (2) lessens the magnitude of the effect. In 2SLS models in columns (3) and (4), the effect of entrance age is positive and statistically significant. The 2SLS models with controls indicate that being a year older at kindergarten entry leads to 6.21 and 3.78 percentile point increases in reading and math scores, respectively. The last row of Table 4 shows results for models of grade retention, with the 2SLS estimate in column (4) indicating that being a year older at entry reduces the probability by 15.5 percentage points. As in the ECLS-K, this effect is dramatic relative to the sample mean of 21.4 percent.

A comparison across the columns of Table 4 indicates a negative association between entrance age and both observable and unobservable determinants of test scores in the NELS:88. Unlike in ECLS-K, the OLS estimates are sensitive to the inclusion of additional covariates, with the implied benefit of entrance age increasing when a detailed set of control variables is included in the models. This pattern implies that actual entrance age is negatively related to observable characteristics that promote educational success, consistent with the notion that voluntary delayed kindergarten entry is common among those with fewer academic skills. One might suspect that the same correlation pattern would exist between entrance age and unobservable parental and child inputs, and the 2SLS estimates confirm this suspicion.[16] In all outcome models, the 2SLS point estimates imply larger beneficial effects than the corresponding OLS estimates.

In all of the 2SLS models presented above, the insensitivity of the estimates to the inclusion of a rich set of covariates provides some reassurance about the validity of predicted entrance age as an instrument. If predicted entrance age were "as good as randomly assigned," we would expect to see no correlation between it and any observable measures, which is what the similarity of models (3) and (4) implies. In the Appendix and Appendix Table 1, we further

[16] See Altonji et al (2005) for a formal description of the data generating process that generates similar selection on observable and unobservable dimensions.

assess the validity of our identification strategy in two ways. First, we examine models that use only variation in birth dates or variation in cutoff dates, but not both, as a source of identification. Second, we estimate models that use the discontinuity in predicted entrance ages for those born within one month of their state's cutoff date as the sole source of variation in predicted entrance ages. We find that the results of Tables 1 through 4 are insensitive to these alternative specifications, suggesting that these baseline estimates identify a causal effect of entrance age on early educational outcomes.

To summarize, being older at kindergarten entry substantially increases achievement test scores. The large effects on third and eighth grade scores found here and in Bedard and Dhuey (2005) have origins at the very start of kindergarten. We have also shown that entrance age affects the probability of repeating a grade and the probability of receiving a learning diagnosis. A comparison of OLS and 2SLS results indicates that entrance age is negatively correlated with unobservable determinants of outcomes, particularly in NELS:88. It is difficult, however, to draw firm policy conclusions without knowing the mechanism driving these effects. In the following three sections we attempt to shed some light on the reasons for the positive association between entrance age and achievement.

IV. The Effects of Age at Kindergarten Entrance, Age at Test, and Educational Attainment on Test Scores

In models of test scores presented thus far, we have followed previous literature by interpreting estimates of α in equation (1) as measuring the causal impact of kindergarten entrance age on educational achievement. In this interpretation, children who enter school at older ages perform well on achievement tests because they learn more in kindergarten and in subsequent grades than their younger classmates do. This view implies that the timing of entry into kindergarten has long-lasting effects on the level of human capital gained in school, which perhaps justifies delaying the entry of younger children and those who are physically, emotionally, or cognitively immature. We will refer to this mechanism as the "entrance-age effect" from this point forward. However, this is not the only interpretation of the relationship between entrance age and academic achievement. Children's human capital and cognitive development generally increase each year regardless of whether they are in school, as they learn from their parents, pre-school teachers, and other caregivers. Even if kindergarten did nothing to develop human capital, it is likely that older entrants would do well on achievement tests simply because they are relatively old when they take the tests. To the extent that the relationship between entrance age and achievement reflects this "age-at-test effect," rather than the entrance-age effect, beginning school at a young age is simply correlated with lower achievement within a class and not a direct cause of it. In this subsection, we formalize this distinction and discuss its implications.

A generalized version of equation (1) that includes the effects of both entrance age and age at test is given by

$$(3) \qquad Y_{it} = c_t + \alpha_{1t} EA_i + \alpha_{2t} AGE_{it} + X_{it}\gamma_t + \varepsilon_{it},$$

where AGE_{it} represents the age of a child in calendar years when he or she is administered a test. The models presented in Tables 1-4 are stratified by calendar time, and we have made this explicit in equation (3) by including t subscripts for all time varying variables and coefficients. The evolution of c_t over time reflects the year-to-year change in average test scores once children enter school, i.e., the causal affect of schooling on test scores. In this framework, children's achievement grows by α_{2t} each year prior to entering kindergarten as parents and preschools develop children's human capital. At the beginning of kindergarten, older children enjoy an advantage over their younger classmates simply because they have had more time to acquire human capital. Equation (3) also highlights the idea that children who are older when they enter kindergarten may acquire human capital at a faster rate than their younger classmates, reflected by α_{1t}. We could further generalize equation (3) by allowing nonlinearities in the effects of entrance age and age at test within a survey period, but we present this simple linear form to fix ideas. We explicitly allow the entrance-age effect and the age-at-test effect to vary over time; the age-at-test effect may decline over time since a one-year gap in age may represent a large degree of learning among children in kindergarten but a trivially small difference among high school students. One might also suspect that the entrance-age effect falls over time as children who were poorly prepared for kindergarten make up for their academic deficiencies in other ways.

It is not possible to separately identify the entrance-age effect from the age-at-test effect since a child's entrance age (EA_i) and age at the time of the test (AGE_{it}) differ by a fixed amount for all children within each survey period. For example, for a test given at the beginning of kindergarten, entrance age and age at test are identical; for a test at the beginning of first grade, entrance age and age at test differ by one.[17] Thus, all of our reported coefficients in Tables 1-4 should be interpreted as the combined effect of entrance age and the age at the time of the test, $\alpha_{1t} + \alpha_{2t}$.

Despite this identification problem, we can make meaningful headway in identifying the relative importance of entrance age versus a child's age at a particular test date. Most children took the fall 1998 test in October or November, when they had been in kindergarten for only one or two months, so differences in cognitive ability that existed prior to kindergarten are likely driving these differences in average test scores between young and old children. As a result, the relationship between entrance age and average test scores among children in fall 1998 presumably captures only the age-at-test effect, α_{2t}. Column (5) of Table 1 shows a slight increase in the gradient between entrance age and reading test scores between the fall of 1998 and the spring of 1999, consistent with older kindergarteners learning more during kindergarten than their younger classmates, but the gradient then falls over the next two years. There is no widening of the math test score gap in kindergarten at all. These patterns are consistent with a small (possibly zero) entrance-age effect and an age-at-test effect that declines over time. For the most part, older children in early grades do relatively well because they had more preparation and cognitive abilities prior to entering school, not because they learned more once in school.

Models of eighth grade tests in Table 4 show positive effects of age at kindergarten entry on both reading and math scores. In principle, these effects could represent either persistent age-at-test effects or entrance-age effects. A third possibility is that success in early grades leads to success in later grades, regardless of the reason. If "learning begets learning", as emphasized in Heckman (2000), Carneiro and Heckman (2003), and Bedard and Dhuey (2005), then strong performance in kindergarten leads to strong performance in first grade, and so on, so children who succeed in early grades will end up succeeding in later grades. A similar pattern will emerge if schools use formal or informal ability tracking in early grades and if students learn more when grouped in classes with better students. In sum, it is difficult to interpret why kindergarten

[17] Identification would be possible if one were willing to put additional structure on the effect of schooling, c_t. For example, if grade retention were randomly assigned and did not affect human capital accumulation, one could compare children that entered kindergarten at the same time but progressed into first grade in different years. Both intuition and the findings of the previous sections indicate these assumptions do not hold in practice.

entrance age is related to school performance in later grades. It is clear, however, that test score gaps observed in eighth grade have origins at the start of kindergarten that likely reflect human capital children acquired prior to entering school.

The estimates in Tables 1-4 can also be used to compare the relative effectiveness of time in school versus delayed entry as mechanisms to improve test scores. Table 1 shows that average reading test scores increase from 38.9 to 68.0 when measured from spring 1999 to spring 2000 in the ECLS-K, a gain of 29.1 points; average math test scores increase by 23.0 points over the same period. These gains are considerably larger than the corresponding estimates of the combined entrance-age effect and age-at-test effect, $\alpha_{1t} + \alpha_{2t}$. The ratio of $\alpha_{1t} + \alpha_{2t}$ to the year-to-year increases in average test scores is 0.28 (= 8.17 / 29.1) for reading scores and 0.43 (= 9.98 / 23.0) for math scores.[18] This implies that children's test scores increase more quickly at young ages when they are in school than when they are not. Therefore, although a state's decision to increase the average entrance age will raise average test scores at each grade level, the policy change would lower average test scores at each age level since children will have been in school fewer years at each age.

To summarize, we conclude that among children who comply with their state entrance age policy, those who are relatively older at the start of kindergarten tend to do better on reading and math tests than their younger classmates. The fact that this gap in scores is evident from the very beginning of kindergarten indicates that it largely reflects differences in skills acquired prior to school, rather than reflecting a causal impact of being older at entry on subsequent learning. In the following section, we present additional evidence in support of this interpretation. We will continue to refer to differences in test scores between children who begin kindergarten at different ages as the effect of entrance age, though this section has made clear that this reduced-form effect combines both an "entrance-age effect" and an "age-at-test effect" that are conceptually distinct.

[18] Note that these ratios identify a sharp upper bound on $\alpha_{1t} / \Delta c_t$ under the assumptions that all three parameters are nonnegative, and α_{1t} and α_{2t} are constant over time; this bound holds when α_{2t} is zero for all t. Datar (2006) estimates models of test score growth in an attempt to separate entrance age effects from age at test effects, but the inferences from these growth models hinge on α_{2t} being constant over time and on the functional form of outcome models being correctly specified. We view equation (3) as an approximation and are reluctant impose additional assumptions to identify the parameters.

V. Heterogeneity in the Relationship between Entrance Age and Educational Outcomes

A large body of research shows significant differences in early school performance across socioeconomic stratum and racial groups.[19] Some of these differences are attributable to differences in home environments, parental behaviors, and enrollment in preschool programs. To the extent that high-SES families provide their children with home environments that develop basic skills relatively quickly, children's pre-kindergarten experience will have a larger effect on test scores among rich children than among poor children. In this section, we test this prediction and find evidence that the entrance age effect is substantially larger among children from higher socioeconomic status families.

Understanding differences in the effect of entrance age across the socioeconomic spectrum is important in its own right. Many scholars and policymakers argue that increasing kindergarten entrance age (either by parents voluntarily redshirting their children or via moving the entrance age cutoff earlier in the year) may be an effective policy to raise achievement, especially among low-income children and children who otherwise may be at risk of doing poorly in school. These discussions regarding entrance age have not specifically focused on what children would be doing during that year if not enrolled in kindergarten, even though authors such as Currie (2001) have highlighted the long-term consequences of human capital accumulation prior to school entry. If children are not engaged in activities that build their human capital, delayed entry will likely have no effect on children's eventual kindergarten performance. Below, we find large entrance age effects among rich children and much smaller effects among poorer children, implying that increasing the overall entrance age will have the perverse effect of exacerbating socioeconomic differences in school performance.

To investigate differences in the effect of kindergarten entrance age on children from different family backgrounds, we begin by classifying children into one of four quartiles based on their observable characteristics. Specifically, for ECLS-K children we regress the fall kindergarten reading score on all of the exogenous covariates included in equation (1). These variables represent children's gender, race and ethnicity, parental income and education, family

[19] See Duncan and Brooks-Gunn (1997), Mayer (1997), Lubotsky (2001), Carneiro and Heckman (2003), Fryer and Levitt (2004), among many others. The OLS and 2SLS models described in Section III.B. indicated that children from richer families, and those with better-educated parents, tended to score higher on the fall kindergarten exams and repeat grades less often.

structure, region, and urbanicity. We restricted the regression sample to those born more than three months before or after the kindergarten cutoff date to minimize omitted variables bias due to the exclusion of entrance age in these models. We then generate a predicted test score for all children in the data based on the coefficients from this model and children's observable characteristics and classify children into quartiles based on this "family background index". More precisely, this index ranks children according to who is likely to perform well on achievement tests based on their family background. A similar index is created for children in the NELS:88 based on their eighth grade reading test score.

Table 5 provides descriptive information about children, their families, and their school performance across the four quartiles, with quartile 1 representing those with the lowest "family background index" and quartile 4 representing the highest. Panel A shows that the family background index is strongly positively correlated with fall kindergarten reading test scores (by construction), maternal education, and family income; and negatively correlated with the probability a child is in first or second grade in the spring 2002 interview and the probability a child is raised in a one-parent household in the ECLS-K. Although it is not surprising the variables listed in Panel A are correlated with the family background index (with the exception of grade repetition, the variables are used to construct the index), the correlations indicate our index is strongly correlated with familiar measures of family background.

In Panel B of Table 5 we investigate whether there is evidence in the ECLS-K to support the notion that children from richer families receive greater parental inputs into their human capital. We use measures of children's reading activities as a proxy for parents' direct human capital input (these measures are not part of the family background index). In the ECLS-K fall kindergarten parental interview, the child's primary caregiver – the mother for most sampled children – is asked "Now I'd like to talk with you about {CHILD}'s activities with family members. In a typical week, how often do you or any other family member do the following things with {CHILD}?" and is presented with a list of activities, such as reading books, telling stories, and playing sports. The primary caregiver may respond with "not at all," "once or twice," "3 to 6 times," and "every day." The first column of Panel B shows the frequency with which parents in each family background quartile respond that they read books to their child every day during a typical week. 36.5 percent of parents in the poorest quartile report they read to their child every day, while 60.2 percent of parents in the richest quartile report doing so. The second column shows that 27.7 percent of parents in the poorest quartile report they "talk about nature or do science projects" with their child at least three times per week, compared to 39.9 percent of parents in the richest quartile. The remaining columns indicate that children from richer family backgrounds also tend to have more children's books in the home (whether purchased or borrowed from the library), and these children are also more likely to look at picture books every day. The fraction of children that read (or pretend to read) to themselves is roughly constant across family background quartiles.

These correlations between family background and home reading activities during the fall of kindergarten suggest that rich children experience a more enriching home environment that stresses building reading skills and vocabulary. Tabulations by Todd and Wolpin (2005) of the National Longitudinal Survey of Youth-Children show that white parents of pre-kindergarten children are more likely than black or Hispanic parents to engage in similar skills-building activities. Lubotsky (2001) uses similar data and finds significant correlations between parental resources and a variety of parental behaviors that build children's skills among families of school-age children. Although it is difficult to firmly establish a causal link between reading and other skill-building activities in the home and performance on reading and math tests in school, our reading of the evidence is that poor parents are less likely to develop their children's human capital prior to kindergarten to the extent done by richer parents. Richer children appear to learn more prior to entering kindergarten than do poorer children, implying that being older at kindergarten entry is likely to have a larger effect on rich children's test scores than it does for poor children's scores. We turn next to direct evidence on whether the data support this prediction.

Table 6 explores the variation in the effect of entrance age across the four family background quartiles. Each entry in the table represents a 2SLS estimate of α from the system in equations (1) and (2). Each model of test scores indicates that entrance age effects rise with socioeconomic status. For example, the effect of being a year older at kindergarten entry raises the fall 1998 reading test scores of the poorest quartile of students by 2.80 points and raises the scores of the richest quartile by 10.59 points. This four-fold increase in magnitude is statistically significant and is considerably larger than the 50 percent increase in average reading scores across the quartiles, implying that the entrance age effect relative to the mean test score also rises. The remaining results in panel A show large differences in entrance age effects on reading test scores through third grade, and the results in panel B show similarly large effects for math test scores.

Panel C shows the effect of entrance age on grade retention and diagnoses of learning disabilities in the ECLS-K, and panel D shows the effects on grade retention and eighth grade test scores in the NELS:88. For the grade retention and diagnoses of learning disability measures, the baseline averages are considerably different across the quartiles, so below the coefficients and standard errors we display the ratio of the coefficient to the baseline rate for each cell (in brackets). These models point to larger grade retention effects of entrance age relative to the baseline rate for richer children. The effect of being a year older at kindergarten entry lowers the probability of repeating a grade between kindergarten and second grade by 21.4 percentage points among the poorest quartile in the ECLS-K. This group had a baseline retention rate of 17.4 percent, so the ratio of the effect size to the baseline rate is -1.23. Among the richest quartile, the point estimate is12.0, which is 3.27 times their baseline retention rate of 3.7 percent. We find no pattern for the probability of being diagnosed with a learning disability. The effect of entrance age on eighth grade test scores is also larger for richer children, with the estimated coefficient rising from 0.68 among the poorest quartile to 3.51 points among the third quartile. The effect among the richest quartile is 2.31, which is smaller than the effect among the third quartile and perhaps reflects sampling variation.[20]

The estimates in Table 6 are consistent with the idea that older children do better in school because they have had more time to build skills prior to entering kindergarten. An alternative reason why children's entrance age influences school performance is that age is strongly associated with physical maturity, and relatively mature children may be better equipped for the physical and mental rigors of school. One convenient measure of physical maturity is height, and Panel E of Table 6 shows 2SLS estimates of the association between height (measured in inches) and entrance age for the full ECLS-K sample and separately by family background quartile. The results show that each year of age is associated with being 2.3 inches taller in fall 1998 and 2.2 inches taller in spring 2002 (these estimates are consistent with the 8.4 inch gain in average height in the three and a half years between the two survey rounds). More importantly, the next four columns indicate that the relationship between entrance age and

[20] We also estimated a more parsimonious model that pooled all observations and included an interaction between entrance age and the family background index. The coefficient on the interaction term is 0.15, with a standard error of 0.07.

height is the same across all four family background groups. The coefficients range from 2.2 to 2.5 inches per year, though the differences across quartiles are not statistically significant in either survey period. We interpret this evidence to mean that physical maturity does not play any role in explaining the wide variation in the association between entrance age and educational outcomes across socioeconomic groups. Moreover, since the heterogeneity in Panels A through D is of such a large magnitude, it is likely that physical maturity is not a driving force in any of the entrance-age effects (or age-at-test effects) found above.[21]

VI. Peer Effects in Kindergarten Entrance Age – Relative or Absolute Age Effects?

In this section, we investigate whether entrance age laws matter because they influence an individual child's age or because they influence the average age of the class (and a student's age relative to the class average), or both. The story described in the introduction is that a child's entrance age influences their school performance because older children are better able to learn or have already learned some basic skills prior to kindergarten entry, but there are several reasons why the class average age may matter as well. First, an older class may have fewer disruptions or allow a teacher to focus on more advanced material.[22] Second, the achievement or behavior of older students may have a positive spillover effect on younger students. Alternatively, a child's own age may matter only through its effect on the child's location in the classroom age distribution. A five year old may struggle if he is the youngest in a class with a curriculum targeted at older students, but the same child may do well if placed in a class with a younger average age.

The distinction between the impacts of a child's absolute entrance age and his age relative to classmates is important for the design of education policy. If relative age is the dominant factor, then changes in entrance age cutoffs will simply change which children are the youngest in the class and which are the oldest, without any aggregate effect on test scores. The

[21] The models presented in Table 6 treat children's height as endogenous, but one could assume height is exogenous and include it as an independent variable in models of test scores. In unreported models, we find that height has no substantive effect on the estimated entrance age effects or the pattern of results across family background quartiles. In phone interviews, Puhani and Weber (2005) find that 21 out of 22 school headmasters believe maturity is primarily driving the association between entrance age and outcomes. This belief is difficult to square with the overall pattern of results in Table 6.

[22] Lazear (2001) presents a model where student disruptiveness influences student learning and the optimal class size.

only aggregate effects of these policy changes would involve children born late in the calendar year necessarily remaining out of school an extra year, perhaps imposing additional childcare costs on parents.

To model the independent effect of classmates' average entrance age, we augment equation (1) with $\overline{EA}_{s,-i}$, the average entrance age in school *s* over all sampled children from a school except child *i*, and $\overline{X}_{s,-i}$, a vector of the school average covariates. The model of child outcomes thus becomes

$$(4) \qquad Y_{is} = \varphi_1 EA_{is} + \varphi_2 \overline{EA}_{s,-i} + X_{is}\gamma + \overline{X}_{s,-i}\theta + \varepsilon_{is} .$$

Unobserved determinants of outcomes are likely to influence individual entrance ages and school averages, so we instrument both measures with predicted individual entrance age and the school average if all students perfectly complied with statewide kindergarten entrance policies. Identification of both φ_1 and φ_2 is possible because the model of equations (1) and (2) is overidentified – variation in absolute age at entry depends on entrance cutoffs and individual birthdays, while variation in school averages is generated by variation in average birthdays across schools and variation across schools in the entrance age cutoffs.[23] In practice, almost all of the variation in predicted school average entrance ages is due to variation across states in entry cutoff dates, so the estimates of φ_1 and φ_2 are largely insensitive to fixing average birth dates across schools within a state.

Before proceeding, we note that the statistical model in equation (4) also captures the idea that peers matter because a child's performance is influenced by his or her age relative to the class average age. To see this, note that the model given by:

$$(5) \qquad Y_{is} = \delta_1 EA_{is} + \delta_2 (EA_{is} - \overline{EA}_{s,-i}) + X_{is}\gamma + \overline{X}_{s,-i}\theta + \varepsilon_{is}$$

[23] Since the ECLS-K and NELS:88 do not collect information on all students in each school, the estimates of predicted and actual school average entry ages contain sampling error. Since the noise is presumably uncorrelated with the true values, estimates of φ_2 reported below are biased toward zero. In ECLS-K and NELS:88 schools that have more than one kindergarten or eighth grade class, students are drawn from all classes.

is equivalent to that in equation (4), with $\varphi_1 = \delta_1 + \delta_2$ and $\varphi_2 = -\delta_2$. Put differently, without putting additional structure on the data, we cannot decipher whether peers matter because of direct spillovers from older students to younger ones (or vise versa), or because teachers design curriculums to best teach the average child. Thus, we proceed with estimates of equation (4) but note that a positive effect of the school average entrance age (φ_2) corresponds to a negative effect of a child's age relative to the school average.

Tables 7 and 8 present 2SLS estimates of equation (4) using the ECLS sample. Table 7 shows that conditioning on the school average entrance age does not substantially alter inferences about the effects of individual entrance age. Specifically, being a year older at kindergarten entry is associated with a 5.26 point increase in reading test scores, which is nearly identical to the 5.28 point increase measured in Table 1. The effect of the class average age is 2.73, with a standard error of 1.18. The spring kindergarten and first grade models imply an even larger effect of the class average, with coefficients of 4.60 and 4.39, respectively. Since there is a much smaller degree of variation in class average entry ages than in individual entry ages, the standard errors on the class average effects are relatively large. Nevertheless, the effect of the class average age on reading scores in the first two years are statistically different from zero. The effects on math scores in the spring of kindergarten and first grade are 2.76 and 3.21, respectively, both statistically significant.

Table 8 presents estimates of the effect of individual and school average entrance ages on the probability of being held back in school and being diagnosed with learning disabilities in ECLS-K and eighth grade outcomes from NELS:88. As in the test score models, inclusion of school average age does not markedly change the point estimates on individual entrance age relative to those reported in Table 4. In contrast to the results for test scores, in the top panel there is a modest *negative* peer effect on the probability of a learning disability diagnosis and repeating a grade. An increase in a class's average age by a quarter of a year – for example, by moving the entrance cutoff from December 1 to September 1 – increases the probability of being diagnosed with a learning disability by 1.2 percentage points (4.8 divided by 4) and increases the probability of repeating a grade by 0.7 percentage points (2.8 divided by 4) among children whose own entrance age is not influenced by the policy change (those born between December 1 and August 31, assuming full compliance with the law). While these estimates are somewhat noisy, and in the case of grade retention, statistically insignificant at conventional levels, they are suggestive that the beneficial effects older classmates exert on test scores do not extend to grade progression or diagnoses of learning disabilities.

The bottom panel of Table 8 shows a similar story for outcomes in NELS:88. The estimates imply that classmates' average entrance age may modestly increase test scores, although the point estimates are small and the standard errors relatively large. The negative effect of peers' age on grade progression is large and statistically significant. Assuming full compliance with state laws, a change in the kindergarten cutoff from December 1 to September 1 would increase the grade repetition rate by 2.8 percentage points (11.3 divided by 4) among children born between December 1 and August 31. For children born between September 1 and November 30, the grade repetition probability would decrease by 14.5 percentage points, which combines the effect of an additional year of one's own age at entry (representing a 17.3 percentage point decline) and the effect of increasing the class average age by three months (again, a 2.8 percentage point increase).

In summary, a child's age at entry into kindergarten and the average age of his classmates both appear to boost achievement test scores, at least in kindergarten and first grade. These findings suggest that changes in entrance age cutoffs do have aggregate effects on test scores within a grade level. Perhaps more importantly, even with these gains in human capital accumulation, a child's likelihood of repeating a grade or receiving a learning disability diagnosis increases with the average age of his or her peers. These findings reinforce concerns that the detection of some learning disabilities can be quite subjective. To the extent that learning disability diagnoses or grade repetition are undesirable educational outcomes, these findings also represent a novel contribution to the debate on peer effects – relatively advanced peers can prove detrimental to a child's outcomes that are determined by teachers' and administrators' comparisons of one student to another.

VII. Conclusions and Policy Implications

We have presented evidence that among children who comply with kindergarten entrance age cutoffs, those who are older upon kindergarten entry outperform their younger classmates on reading and math achievement tests. Our baseline estimates indicate that being a year older at the beginning of kindergarten leads to a 0.53 standard deviation increase in average reading scores and a 0.83 standard deviation increase in average math scores during the fall of kindergarten. Although these are large effects, they are smaller than the average increase in achievement children experience between kindergarten and first grade, implying that policy changes that increase entrance age will lower average test scores at each age level. The entrance age effects diminish as children progress through school, but we still find noticeable effects in eighth grade. We also find that an additional year of age at the start of kindergarten decreases the probability of being held back prior to third grade by 13 percentage points, a dramatic effect relative to the 8.8 percent sample grade repetition rate, and reduces the probability of being diagnosed with ADD or ADHD by nearly three percentage points.

Our evidence is consistent with the notion that older children excel because they have accumulated more human capital prior to entering kindergarten than have younger children. The effects of entrance age are particularly pronounced for children of high-income parents, reflecting the greater level of investments that relatively wealthy parents tend to make in their children prior to kindergarten. We do not find support for the alternative hypotheses that the entrance age premium reflects differences in physical maturity or in the capacity to learn once in school.

We also find that the average age of a child's classmates positively influences test scores while simultaneously *increasing* the likelihood that a student repeats a grade in school or receives a learning disability diagnosis. In one interpretation of this pattern, high-performing peers positively influence a student's achievement, but school and parental decisions regarding grade retention and referrals to behavior professionals are partly based on a student's age or performance relative to his or her classmates. Most strikingly, our estimates from NELS:88 imply that a change in the entry cutoff from December 1 to September 1, resulting in a three-month increase in average entrance ages, would increase the likelihood of grade retention between kindergarten and eighth grade by 2.8 percentage points among children whose own entrance age is unaffected.

If the benefits of delayed enrollment result from human capital accumulation prior to kindergarten, policy debates regarding kindergarten entrance age must also ask what children will be doing if not in school. Our estimates imply that moving a state cutoff from December to September will raise average entrance ages and average achievement in early grades, but such a change will also exacerbate socioeconomic differences in achievement because the test scores of high-income children will tend to increase more than that of low-income children. If the goal of policy is to raise the achievement of the children most susceptible to falling behind, a policy focused solely on entrance ages is likely to fail since at-risk children receive the least investment prior to entering school.

Decisions by parents to voluntarily delay their child's entry into kindergarten have recently received attention as a means to improve the eventual performance of the most at-risk children. Our 2SLS estimates measure local average treatment effects of entrance age for children whose entrance age is influenced by state entry cutoffs.[24] Since beginning kindergarten earlier or later than proscribed by law represents non-compliance with state kindergarten cutoffs, by definition, the existence of heterogeneous treatment effects implies that 2SLS estimates may be highly misleading about the average causal effects of voluntarily starting kindergarten early or late. Nevertheless, it seems clear that children who receive little cognitive stimulation at home are poorly served by staying out of school an additional year prior to kindergarten.

Finally, delayed entry into kindergarten imposes additional childcare costs on parents and reduces future earnings of the child, because an extra year of preparation for kindergarten delays entry into the job market by one year (if eventual educational attainment and the retirement date are not affected). The potential long-run benefits of being older at kindergarten entry must be weighed against these costs to know whether, and for whom, a policy to encourage delayed entry is worthwhile.

[24] Local average treatment effects are discussed in Imbens and Angrist (1994) and Angrist and Imbens (1995), among others.

Appendix: Sensitivity Analysis

The identification strategy pursued in Section III is based on two distinct sources of variation in predicted kindergarten entrance ages: differences in months of birth across children and differences in kindergarten cutoff dates across states. Although the insensitivity of the 2SLS estimates to the inclusion of controls suggests that predicted entrance age is “as good as randomly assigned” and can therefore be used to identify local average treatment effects of kindergarten entrance age on child outcomes, there are some potential threats to validity. A child’s month of birth may be correlated with unobservables that influence outcomes, as authors such as Bound and Jaeger (2000) have argued. Alternatively, state-level cutoffs may be endogenous, as states choose their kindergarten cutoff date taking into account the socioeconomic status of families in the state, or the school performance of children compared to those in other states. We explore these possibilities by examining models that use only variation in birth dates or variation in cutoff dates, but not both, as a source of identification. Additionally, we estimate models that use the discontinuity in predicted entrance ages for those born within one month of their state’s cutoff date as the sole source of variation in predicted entrance ages.

Appendix Table 1 explores the robustness of our main results to different identification assumptions. For purposes of comparison, column (1) of the table replicates the 2SLS estimates from Tables 1-4, using specifications that include the covariates. The models in column (2) add a full set of indicator variables for a child’s month of birth, so that the entrance age effect is identified solely from variation in state laws. A comparison of the estimates in columns (1) and (2) reveals that the addition of the birth month indicators has essentially no effect in either the ECLS-K or NELS:88. For example, the coefficient on entrance age in models of Fall 1998 reading scores does not change at two digits of precision, while the Spring 1999 coefficient increases from 8.17 to 8.34. One exception is that the effect of entrance age on diagnoses of learning disabilities falls from -0.025 to -0.009, the latter not being statistically significant.

Column (3) presents estimates from models that include fixed effects for state of residence, forcing identification to come from within-state variation in birth months across students. Again, relative to column (1), the estimates change only modestly – less than 20 percent of the baseline coefficient in all cases. Note the entrance age effect on diagnosis of a learning disability in these models is -0.035, with a standard error of 0.013.

The robustness of the point estimates to these alternative specifications provides some reassurance that predicted entrance age is a valid exclusion restriction in models of education outcomes. Either the estimates in columns (1) through (3) are all relatively free of bias or the magnitude of the bias resulting from within-state variation is roughly equivalent to the magnitude of bias resulting from across-state variation. Although this alternative explanation is unlikely in our view, we turn next to an additional specification using a restricted sample that can deliver consistent estimates even in the presence of an association between outcomes and both birth month and state cutoff mandates.

Estimates Based on the Discontinuity Sample

Recall that kindergarten entry laws induce a discontinuity in the relationship between date of birth and predicted kindergarten entrance age. For example, in a state with a September 1 cutoff, those born in early September are likely to enter kindergarten a full year later than those born just days earlier, in late August. This discontinuity is a large source of identifying information in estimates of equation (1), so estimates based only on those born close to the cutoff retain identifying power while avoiding two potential sources of bias. Specifically, even if month (or season) of birth directly affects outcomes, this association will not lead to bias as long as children born close to the cutoff date are similar along unobservable dimensions. These models also include indicators for each cutoff date, thereby sidestepping concerns about the endogeneity of state laws by forcing identification to come from within-state (or within groups of states with identical cutoff dates) variation in entrance ages.

The fourth column of the table presents estimates of model (1) applied to a "discontinuity sample" of children born within one month of their state's kindergarten cutoff date.[25] As a result

[25] We do not pursue a more comprehensive regression discontinuity (RD) strategy in the spirit of Hahn, Todd, and van der Klaauw (2001), McCrary and Royer (2005), and van der Klaauw (2002) for four reasons. First, the sample sizes in both NELS:88 and ECLS-K are not large enough to meet the demands of RD designs, which are commonly applied to large data sources such as the U.S. Census of Population or state birth records. Second, NELS:88 only reports a child's month of birth, rather than the exact date, so a RD strategy based on exact date of birth is not possible in these data. Third, across a wide variety of outcomes we cannot reject the assumption of linear entrance age effects, in which case the entrance age effect from a full RD specification would be identical to those in our

of using only two months of birth dates in each state, the estimation samples used in column (4) are roughly one-sixth the size of the full sample estimates given in the other three columns (for example, for ECLS-K Fall 1998 reading scores, the sample size decreases from 11,592 to 1689)) and thus coefficients are less precisely estimated. In most models the point estimates change only modestly relative to column (1). There are a few notable exceptions: The estimate for ECLS-K spring 2002 reading scores is 5.75, suggesting a slightly smaller effect than the full sample estimate of 7.41. The effect on diagnoses of learning disabilities decreases from -0.025 to -0.044, and in NELS:88, the effect on eighth grade math scores increases from 1.34 to 2.32. The "discontinuity sample" estimates are smaller in absolute value than the corresponding full sample estimates for seven of the fourteen outcomes and larger for the remaining seven outcomes, which suggests there is not a clear direction of bias in the baseline models. In all cases, the differences between columns (1) and (4) are not statistically significant and do not change the qualitative inferences based on the full models presented above. These patterns provide reassurance about the validity of the identification strategy pursued above, and we view the full sample estimates as our preferred set of results.[26]

baseline models. Finally, the insensitivity of point estimates to limiting the sample to those born within a month of the cutoff dates suggests that there is not much value-added in a full RD design.

[26] We have performed several additional robustness checks, including estimating all dichotomous outcome models as IV-probit models, stratifying the samples based on gender, and eliminating children attending private schools from the analysis. None of these alternative specifications had a substantive impact on the results. Additional results are available upon request.

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Table 1: The Effect of School Entrance Age on Reading Test Scores, ECLS

Test date	Mean of IRT test score S.D. N	Models of IRT reading test score by estimation method OLS (1)	 OLS (2)	 2SLS (3)	 2SLS (4)	Test score percentile 2SLS (5)
Fall 1998	27.5	3.79	3.69	4.15	5.28	16.68
	10.0	(0.31)	(0.29)	(0.49)	(0.47)	(1.28)
	11592	0.018	0.212	0.018	0.209	0.248
Spring 1999	38.9	5.07	5.05	6.20	8.17	19.33
	13.4	(0.40)	(0.39)	(0.64)	(0.62)	(1.33)
	11975	0.018	0.192	0.017	0.187	0.211
Spring 2000	68.0	7.60	7.17	8.11	10.67	14.08
	20.7	(0.59)	(0.55)	(0.95)	(0.89)	(1.22)
	12046	0.017	0.219	0.017	0.216	0.213
Spring 2002	107.5	7.09	5.26	6.54	7.41	11.08
	20.2	(0.72)	(0.60)	(1.03)	(0.88)	(1.27)
	10336	0.016	0.285	0.016	0.284	0.285
Covariates?		No	Yes	No	Yes	Yes

Note: The entries for each model are the coefficient, standard error in parentheses, and the regression r-squared. Standard errors are adjusted for clustering at the school level. Covariates are described in the text.

Table 2: The Effect of School Entrance Age on Math Test Scores, ECLS

Test date	Mean of IRT test score S.D. N	Models of IRT math test score by estimation method OLS (1)	 OLS (2)	 2SLS (3)	 2SLS (4)	Test score percentile 2SLS (5)
Fall 1998	21.5	5.90	5.07	6.62	7.41	24.03
	8.9	(0.29)	(0.27)	(0.44)	(0.42)	(1.17)
	12313	0.056	0.288	0.056	0.281	0.302
Spring 1999	31.6	7.34	6.04	9.17	9.98	25.05
	11.5	(0.38)	(0.34)	(0.56)	(0.52)	(1.20)
	12469	0.052	0.260	0.049	0.248	0.256
Spring 2000	54.6	8.81	7.00	9.72	10.34	18.44
	16.0	(0.49)	(0.46)	(0.72)	(0.69)	(1.20)
	12283	0.039	0.243	0.039	0.238	0.237
Spring 2002	84.6	6.85	5.03	6.43	7.27	11.54
	17.9	(0.58)	(0.52)	(0.86)	(0.74)	(1.20)
	10411	0.019	0.259	0.019	0.258	0.258
Covariates?		No	Yes	No	Yes	Yes

Note: The entries for each model are the coefficient, standard error in parentheses, and the regression r-squared. Standard errors are adjusted for clustering at the school level. Covariates are described in the text.

Table 3: The Effect of School Entrance Age on Grade Retention and Learning Disabilities, ECLS

Dependent variable	Mean N	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)
In 1st or 2nd grade in Spring, 2002	0.088 10431	-0.112 (0.010) 0.020	-0.112 (0.011) 0.071	-0.116 (0.013) 0.020	-0.131 (0.015) 0.071
Diagnosis of learning disability/ADD/ADHD/etc.	0.088 12860	0.008 (0.008) 0.000	0.005 (0.009) 0.042	-0.026 (0.011) 0.000	-0.025 (0.012) 0.041
Diagnosis of ADD or ADHD	0.043 12860	-0.004 (0.006) 0.000	-0.011 (0.006) 0.030	-0.021 (0.007) 0.000	-0.029 (0.009) 0.030
Diagnosis of non-ADD/ADHD learning disability	0.045 12860	0.012 (0.005) 0.000	0.014 (0.005) 0.014	-0.004 (0.007) 0.000	0.001 (0.008) 0.014
Covariates?		No	Yes	No	Yes

Note: Entries include the coefficient, standard error, and r-squared from each regression. Standard errors are adjusted for clustering at the school level. Covariates are described in the text.

Table 4: The Effect of School Entrance Age on 8th Grade Outcomes, NELS:88

Dependent Variable	Mean S.D. N	Estimation method OLS (1)	 OLS (2)	 2SLS (3)	 2SLS (4)	Test score percentile 2SLS (5)
8th grade reading score	50.2	-1.07	-0.34	2.33	2.27	6.21
	10.1	(0.19)	(0.15)	(0.50)	(0.50)	(1.40)
	16213	0.000	0.228	0.000	0.217	0.215
8th grade math score	50.4	-0.92	-0.29	1.61	1.34	3.78
	10.2	(0.22)	(0.17)	(0.54)	(0.50)	(1.42)
	16211	0.002	0.276	0.000	0.271	0.271
Retained in any grade K-8	0.214	-0.078	-0.092	-0.171	-0.155	
	-	(0.011)	(0.011)	(0.019)	(0.022)	-
	16585	0.007	0.105	0.000	0.101	
Covariates?		No	Yes	No	Yes	Yes

Note: Entries include the coefficient, standard error, and r-squared from each regression. Standard errors are adjusted for clustering at the school level. Covariates are described in the text.

Table 5: Child and Household Characteristics by Family Background Quartile, ECLS-K

Panel A:

	Average child and household characteristics				
	Fall Kindergarten Reading Test score	Repeated a Grade, K-2	Mother's Education	Family Income[a]	Single Parent Household
Quartile 1	22.4	0.174	11.7	19,600	0.403
Quartile 2	25.5	0.084	12.9	35,000	0.274
Quartile 3	28.4	0.048	13.9	50,000	0.157
Quartile 4	33.6	0.037	15.6	80,000	0.036
Overall	27.5	0.088	13.6	45,000	0.214

Panel B:

	Average child and household characteristics				
	Parent reads to child everyday	Parent talks to child about nature 3+ times per week	Child reads to self every day	Number of childrens books in home	Child reads picture books everyday
Quartile 1	0.365	0.277	0.359	48.6	0.445
Quartile 2	0.411	0.306	0.337	69.6	0.484
Quartile 3	0.448	0.324	0.337	86.4	0.511
Quartile 4	0.602	0.399	0.381	103.5	0.610
Overall	0.461	0.328	0.356	77.3	0.514

Note: Family background quartile is defined in the text. N=11,592

[a] Median family income

Table 6: 2SLS Estimates by Family Background Quartile

A. ECLS-K Reading test	Full Sample	Family background quartile 1	2	3	4
Fall 1998	5.28	2.80	3.57	5.59	10.59
	(0.47)	(0.63)	(0.81)	(0.83)	(1.41)
Spring 1999	8.21	6.11	6.67	8.23	14.00
	(0.62)	(0.88)	(1.09)	(1.13)	(1.86)
Spring 2000	10.67	9.05	8.46	12.41	15.16
	(0.89)	(1.56)	(1.75)	(1.90)	(2.27)
Spring 2002	7.24	3.18	6.41	7.22	10.99
	(0.88)	(1.95)	(1.83)	(1.63)	(1.87)
B. ECLS-K Math test					
Fall 1998	7.41	5.12	5.67	8.28	12.42
	(0.42)	(0.60)	(0.67)	(0.93)	(1.21)
Spring 1999	10.00	7.90	7.61	10.98	14.94
	(0.52)	(0.80)	(0.87)	(1.07)	(1.49)
Spring 2000	10.30	8.98	8.21	11.11	13.79
	(0.69)	(1.20)	(1.30)	(1.44)	(1.78)
Spring 2002	7.20	6.45	5.60	5.35	10.30
	(0.74)	(1.50)	(1.71)	(1.57)	(1.63)

Table 6 (continued): 2SLS Estimates by Family Background Quartile

C. ECLS-K Other outcomes	Full Sample	Family background quartile 1	2	3	4
In 1st or 2nd grade in Spring 2002	-0.132	-0.214	-0.135	-0.087	-0.120
	(0.015)	(0.038)	(0.029)	(0.026)	(0.026)
	[-1.577]	[-1.232]	[-1.609]	[-1.808]	[-3.269]
Diagnosed with a learning disability/ADD/ ADHD/etc.	-0.027	-0.038	-0.006	-0.053	-0.012
	(0.012)	(0.026)	(0.028)	(0.030)	(0.022)
	[-0.295]	[-0.332]	[-0.056]	[-0.698]	[-0.190]
D. NELS:88 Outcomes					
8th grade reading test	2.27	0.68	2.43	3.51	2.31
	(0.50)	(0.65)	(0.96)	(0.97)	(1.22)
8th grade math test	1.33	0.38	1.37	2.22	1.02
	(0.50)	(0.65)	(0.93)	(0.98)	(1.18)
Retained in any grade K-8	-0.156	-0.185	-0.187	-0.108	-0.112
	(0.022)	(0.046)	(0.045)	(0.039)	(0.032)
	[-0.739]	[-0.497]	[-0.757]	[-0.734]	[-1.332]
E. ECLS-K height in inches					
Fall 1998	2.31	2.32	2.49	2.33	2.21
	(0.10)	(0.19)	(0.19)	(0.23)	(0.23)
Spring 2002	2.24	2.29	2.36	2.28	2.33
	(0.13)	(0.25)	(0.25)	(0.31)	(0.31)

Notes:
1) All models include covariates. Standard errors are adjusted for clustering at the school level. Terms in brackets are the ratio of the coefficient to the baseline probability.

2) Average height is 44.7" in fall 1998 and 53.1" in spring 2002.

Table 7: The Effect of Individual and Class Average School Starting Age on Test Scores, ECLS-K

Test date and subject	Sample size	2SLS Estimates	
		Individual entrance age	School average age
Fall 1998 IRT reading score	11576	5.26 (0.47)	2.73 (1.18)
Fall 1998 IRT math score	12295	7.58 (0.40)	0.65 (0.88)
Spring 1999 IRT reading score	11957	7.83 (0.61)	4.60 (1.70)
Spring 1999 IRT math score	12451	9.75 (0.50)	2.76 (1.18)
Spring 2000 IRT reading score	12032	10.28 (0.83)	4.39 (2.49)
Spring 2000 IRT math score	12269	10.08 (0.65)	3.21 (1.70)
Spring 2002 IRT reading score	10323	7.19 (0.87)	2.34 (2.09)
Spring 2002 IRT math score	10398	6.87 (0.75)	3.63 (2.04)

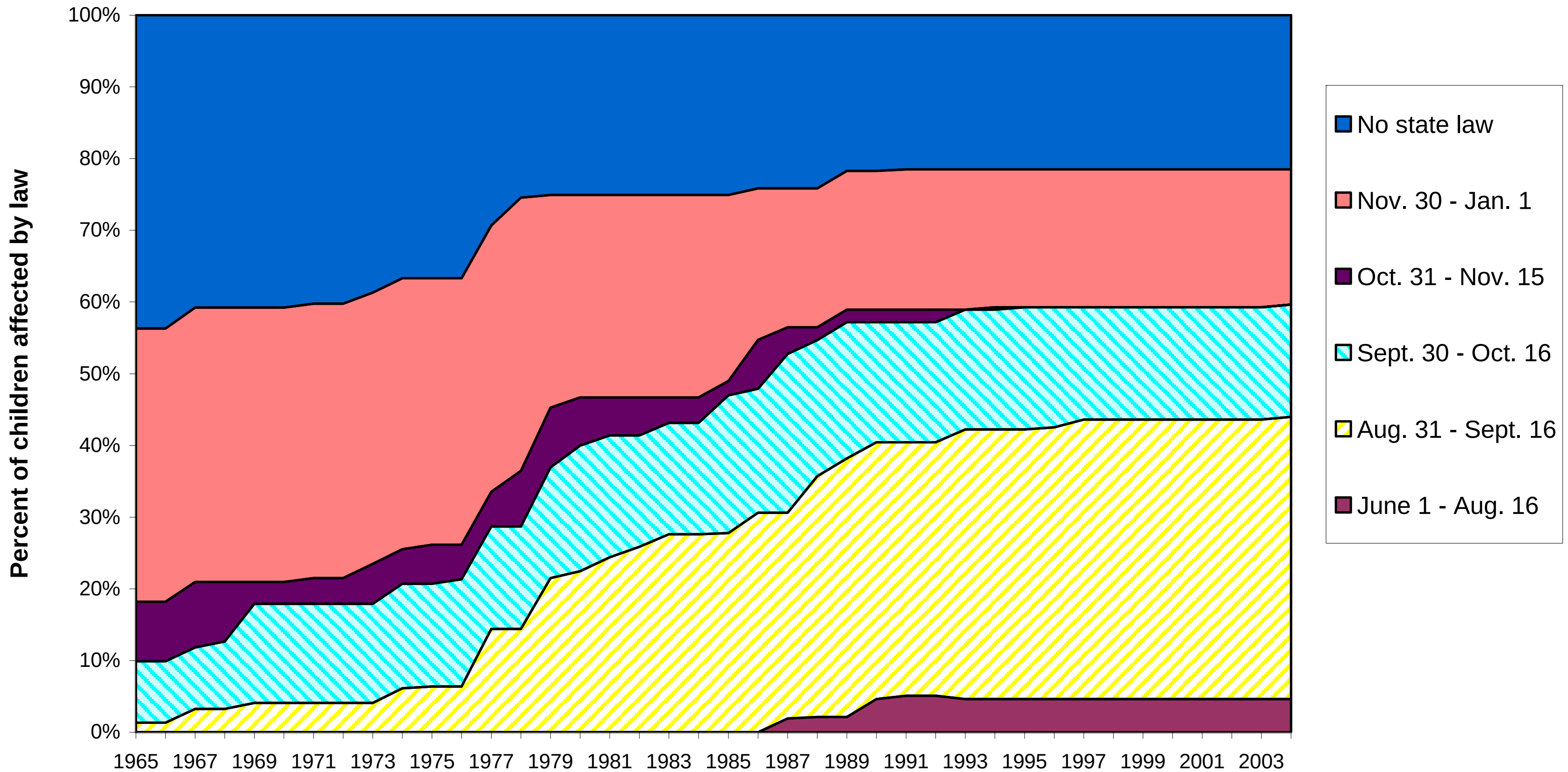
Note: All models control for individual covariates and the school average covariates. Entries include the coefficient and standard error. Standard errors are adjusted for clustering at the school level.

Table 8: The Effect of Individual and Class Average School Starting Age on Grade Retention, Learning Disabilities, and 8th Grade Test Scores, ECLS-K and NELS:88

	Sample size	2SLS Estimates	
		Individual entrance age	School average age
A. ECLS Outcomes			
In 1st or 2nd Grade in Spring 2002	12377	-0.141	0.023
		(0.014)	(0.038)
Diagnosed with a learning disability/ADD/ ADHD/etc.	12840	-0.034	0.048
		(0.013)	(0.028)
B. NELS:88 Outcomes			
8th grade reading score	16209	2.13	0.92
		(0.50)	(1.35)
8th grade math score	16206	1.26	0.89
		(0.46)	(1.48)
Retained in Any Grade K-8	16579	-0.173	0.113
		(0.021)	(0.058)

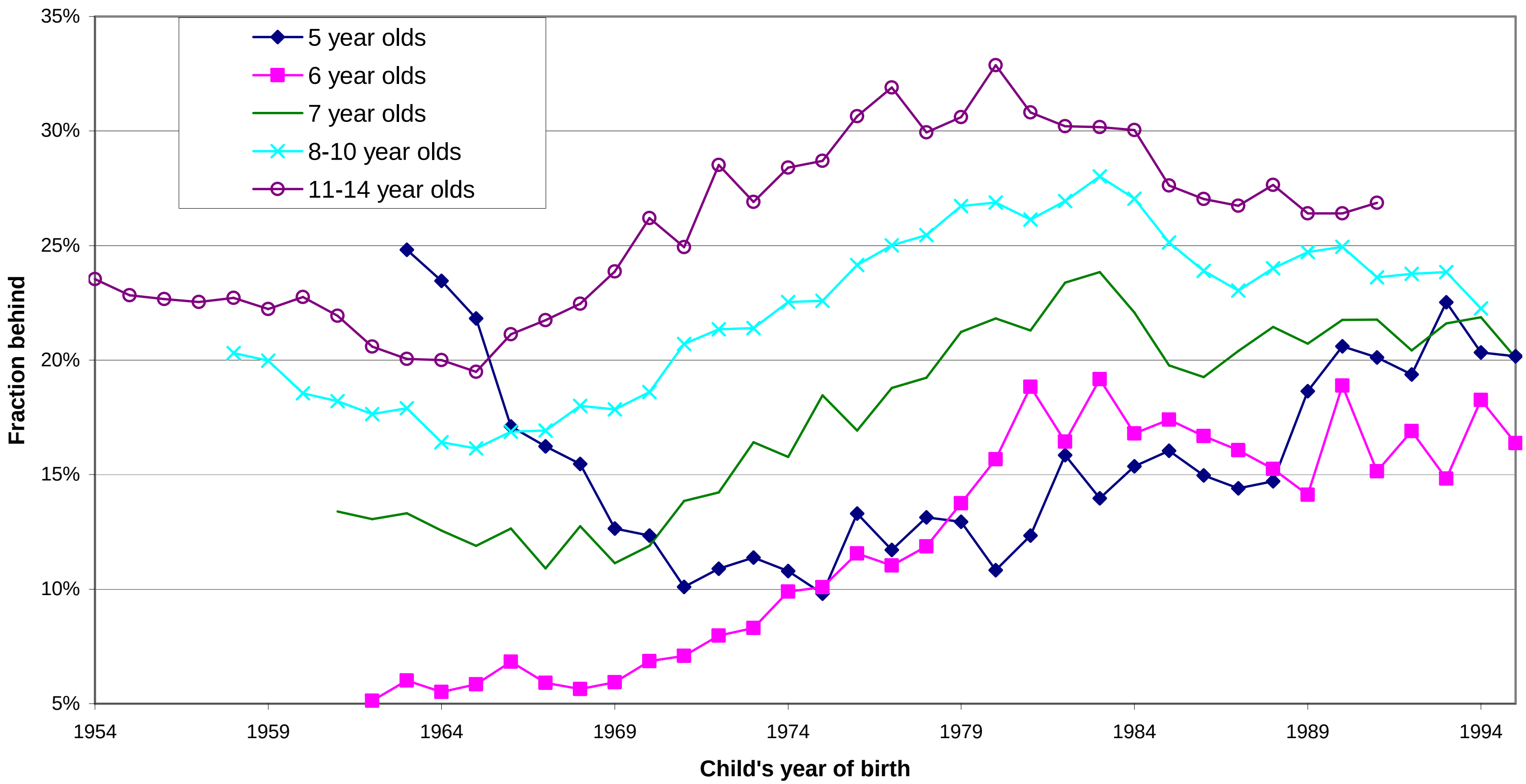
Note: All models control for individual covariates and the school average covariates. Entries include the coefficient and standard error. Standard errors are adjusted for clustering at the school level.

Figure 1: State Entrance Age Cutoffs, 1965-2005



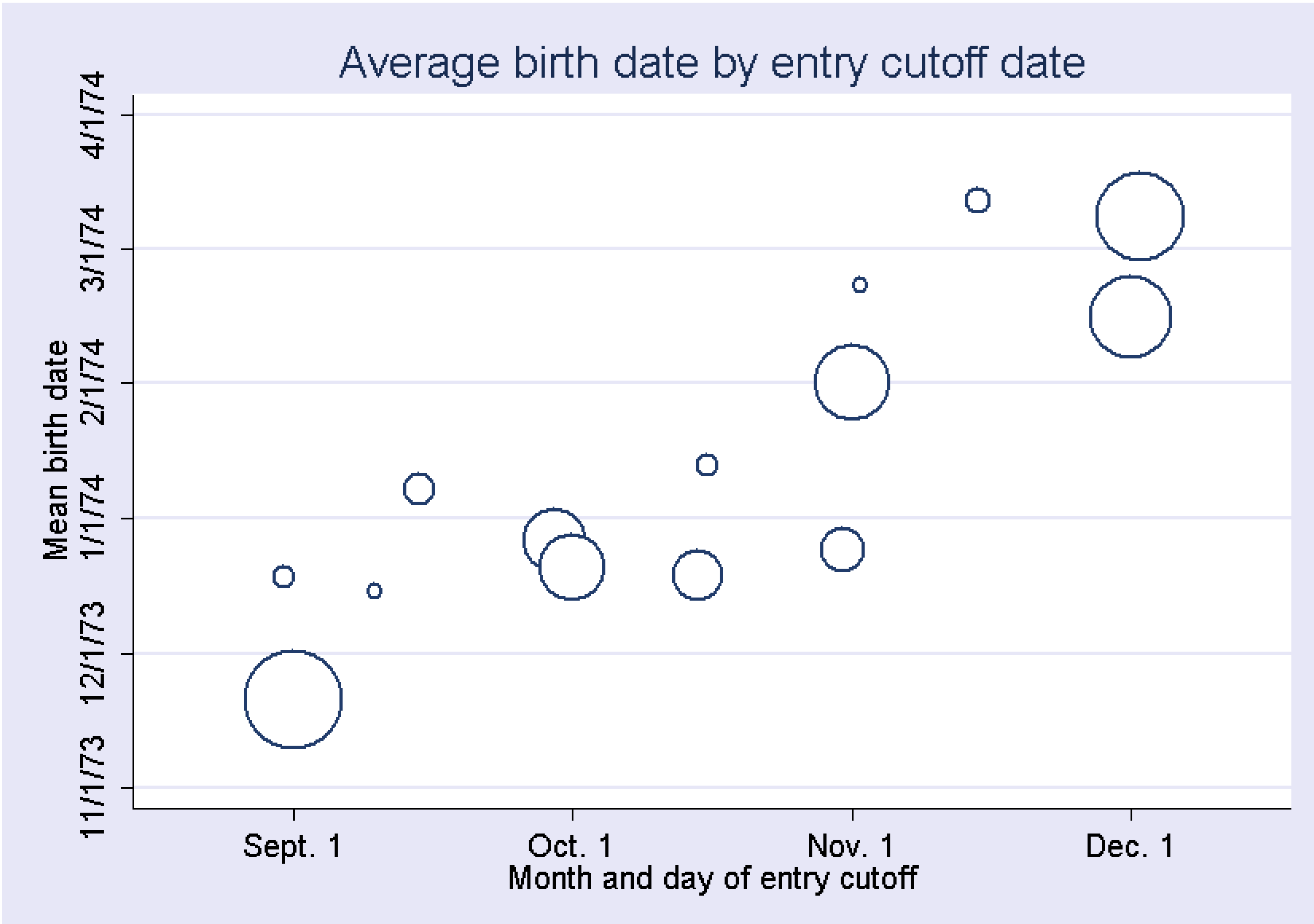
Source: Authors calculations from state kindergarten statutes. States are weighted by the number of children four to six years old in the state in 1990.

Figure 2: Fraction of Children Behind Median Grade for Age (students who are not enrolled are considered behind)



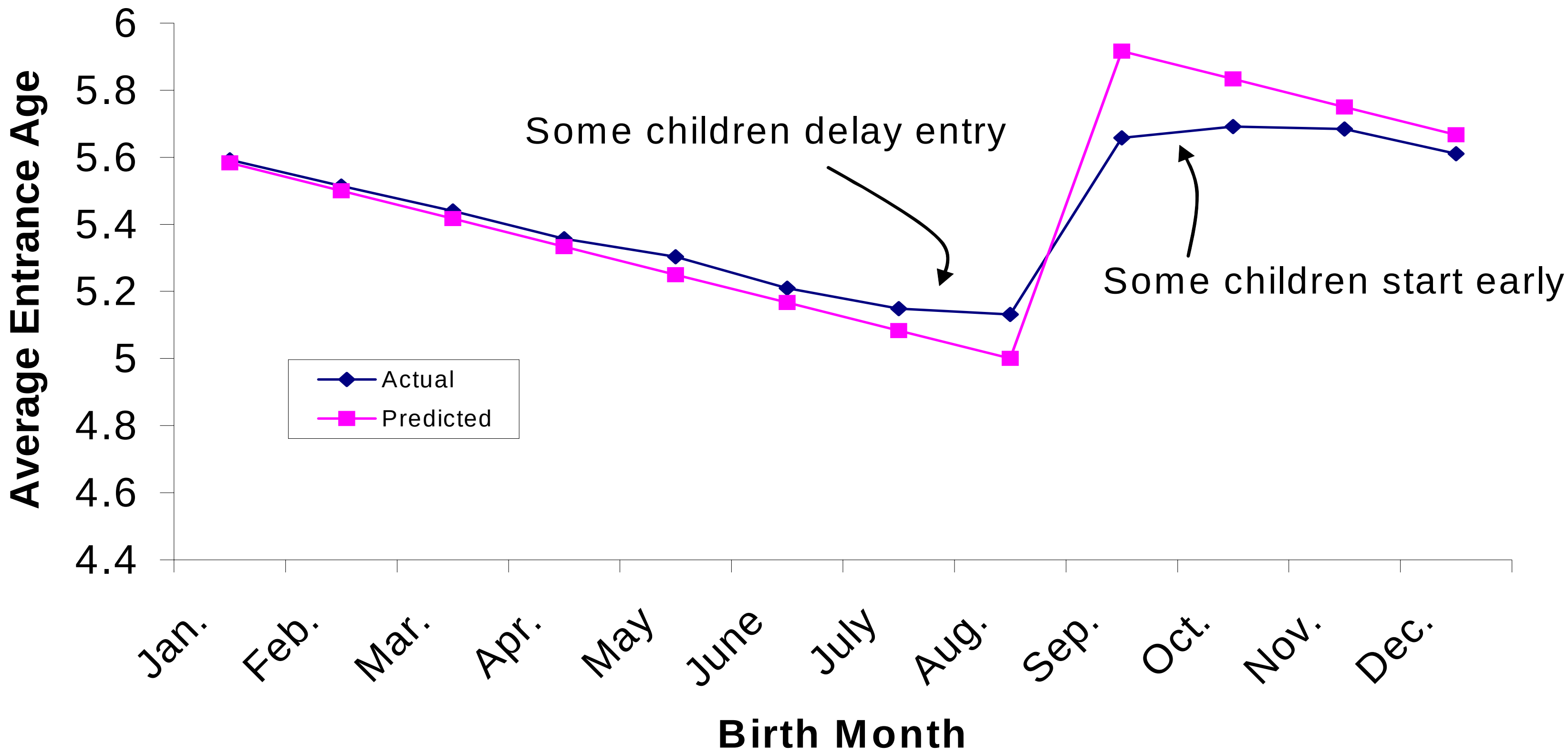
Source: Authors tabulations from the October Supplements to the Current Population Survey.

Figure 3



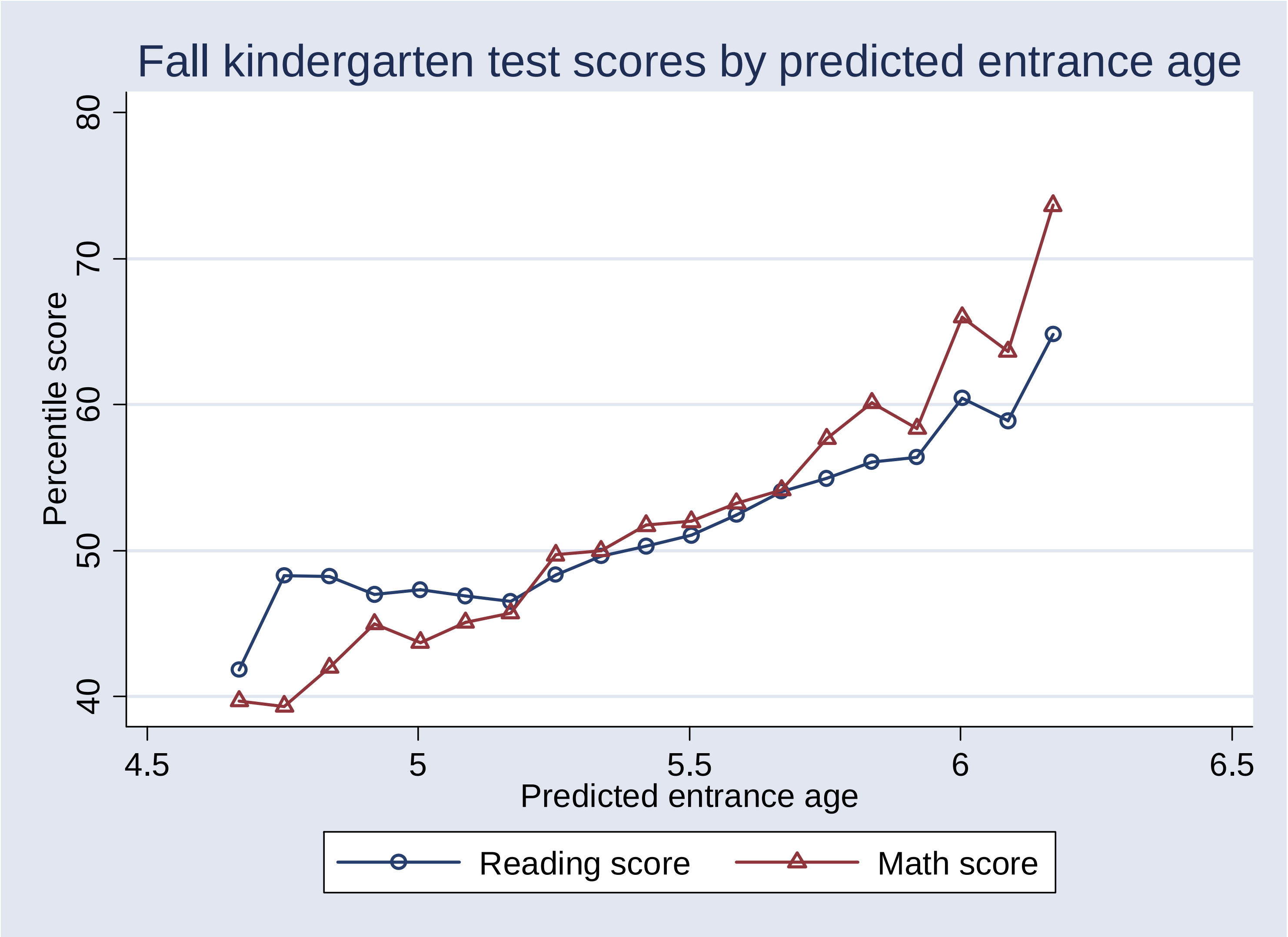
Source: Authors' tabulations from NELS:88

Figure 4: Actual and Predicted Average Entrance Age by Birth Month in States with a September 1 cutoff



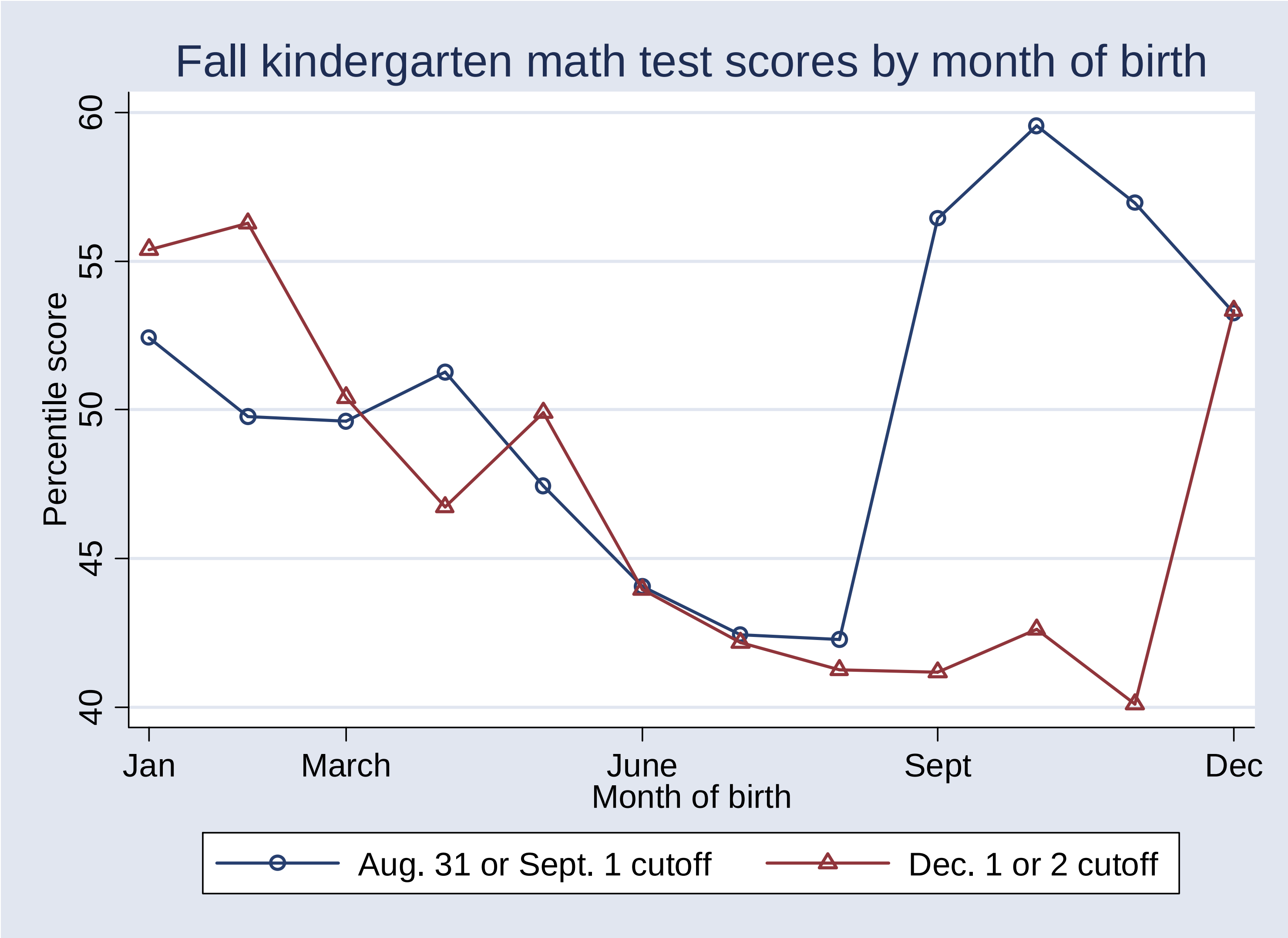
Source: Authors' tabulations from NELS:88

Figure 5



Source: Authors' tabulations from ECLS-K

Figure 6



Source: Authors' tabulations from ECLS-K

Appendix Table 1: Sensitivity of 2SLS Results to Controls for State of Residence, Birth Month, and Limiting the Sample to Those Born within a Month of School Entry Cutoff Dates

	Model			
Dependent Variable:	(1)	(2)	(3)	(4)
A. Reading Scores				
Fall 1998	5.28	5.28	5.00	4.83
	(0.47)	(0.49)	(0.49)	(0.88)
	0.209	0.209	0.220	0.204
Spring 1999	8.17	8.34	7.68	7.26
	(0.62)	(0.66)	(0.65)	(1.20)
	0.187	0.186	0.203	0.181
Spring 2000	10.67	10.86	10.08	10.17
	(0.89)	(0.96)	(0.90)	(1.58)
	0.216	0.217	0.231	0.230
Spring 2002	7.41	7.76	7.07	5.75
	(0.88)	(0.93)	(0.90)	(1.60)
	0.284	0.285	0.301	0.290
B. Math Scores				
Fall 1998	7.41	7.46	7.45	7.89
	(0.42)	(0.44)	(0.42)	(0.80)
	0.281	0.281	0.290	0.295
Spring 1999	9.98	10.18	9.48	10.41
	(0.52)	(0.54)	(0.52)	(0.92)
	0.248	0.247	0.259	0.250
Spring 2000	10.34	10.62	9.85	10.36
	(0.69)	(0.74)	(0.67)	(1.12)
	0.238	0.238	0.251	0.284
Spring 2002	7.27	7.37	6.57	6.37
	(0.74)	(0.78)	(0.79)	(1.46)
	0.258	0.258	0.275	0.300
Discontinuity Sample	No	No	No	Yes
Quarter of birth	Yes	No	Yes	Yes
Month of birth	No	Yes	No	No
Four Census regions	Yes	Yes	No	Yes
State	No	No	Yes	No
Other covariates	Yes	Yes	Yes	Yes

Notes: The entries for each model are the coefficient, standard error in parentheses, and r-squared. Standard errors are adjusted for clustering at the school level. Covariates are described in the text. Sample sizes in column (4) are 1689, 1750, 1754, 1531, 1772, 1807, 1781, and 1546 for Fall 1998, Spring 1999, Spring 2000, Spring 2002 reading scores and Fall 1998, Spring 1999, Spring 2000, and Spring 2002 math scores, respectively.

Appendix Table 1 (continued): Sensitivity of 2SLS Results to Controls for State of Residence, Birth Month, and Limiting the Sample to Those Born within a Month of School Entry Cutoff Dates

	Model			
Dependent Variable:	(1)	(2)	(3)	(4)
C. ECLS outcomes				
In 1st or 2nd grade in	-0.131	-0.142	-0.138	-0.133
Spring, 2002	(0.015)	(0.016)	(0.015)	(0.023)
	0.071	0.072	0.086	0.112
Diagnosis of learning	-0.025	-0.009	-0.035	-0.044
disability/ADD/ADHD/etc.	(0.012)	(0.013)	(0.013)	(0.021)
	0.041	0.044	0.044	0.056
D. NELS:88 outcomes				
8th Grade Reading Score	2.27	2.39	2.07	2.53
	(0.50)	(0.54)	(0.51)	(0.77)
	0.217	0.216	0.223	0.244
8th Grade Math Score	1.34	1.64	1.36	2.32
	(0.50)	(0.54)	(0.48)	(0.78)
	0.271	0.270	0.279	0.288
Held back prior to 8th grade	-0.155	-0.154	-0.181	-0.152
	(0.022)	(0.024)	(0.021)	(0.035)
	0.101	0.102	0.102	0.143
Discontinuity Sample	No	No	No	Yes
Quarter of birth	Yes	No	Yes	Yes
Month of birth	No	Yes	No	No
Four Census regions	Yes	Yes	No	Yes
State	No	No	Yes	No
Other covariates	Yes	Yes	Yes	Yes

Notes: The entries for each model are the coefficient, standard error in parentheses, and r-squared. Standard errors are adjusted for clustering at the school level. Covariates are described in the text. Sample sizes in column (4) are 1805, 1547, and 1863 for "repeated kindergarten", "In 1st or 2nd grade in Spring 2002", and "LD diagnosis", and 2553, 2546, and 2609 for 8th grade reading and math scores and "Held back prior to 8th grade", respectively.