Kindergarten Entrance Age and Children's Achievement

Impacts of State Policies, Family Background, and Peers

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ABSTRACT

We present evidence that the positive relationship between kindergarten entrance age and school achievement primarily reflects skill accumulation prior to kindergarten, rather than a heightened ability to learn in school among older children. The association between achievement test scores and entrance age appears during the first months of kindergarten, declines sharply in subsequent years, and is especially pronounced among children from upper-income families, a group likely to have accumulated the most skills prior to school entry. Finally, having older classmates boosts a child's test scores but increases the probability of grade repetition and diagnoses of learning disabilities such as ADHD.

I. Introduction

At what age should children begin kindergarten? During the past 30 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

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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-mandated cutoff dates 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 have turned five years old by September 1, 2007 to be eligible to enroll in the fall of 2007.) In addition, many parents of children born in the months before the cutoff choose to hold their children out of kindergarten for a year. These children would have been the youngest in their kindergarten class if they began when first allowed to enroll, but instead are among the oldest children in the class that begins the following academic year. These policy reforms and parental choices are largely based on research showing that children who are older when they start kindergarten tend to do better in early grades, perhaps justifying the large price these children pay through delayed entry into the labor market.[2]

The evidence in this paper presents a contrarian view: Age-related differences in early school performance are largely driven by the accumulation of skills prior to kindergarten and tend to fade away quickly as children progress through school. Rather than providing a boost to children's human capital development, delayed entry simply postpones learning and is likely not worth the long-term costs, especially among children from poorer families and those who have few educational opportunities outside of the public school system.

Parents, educators, and researchers have long understood that older children tend to do better across a variety of measures than younger children within the same grade. In the early 1930s, the Summit, New Jersey school system was interested in determining which students to admit into first grade. To help answer this question Elizabeth Bigelow (1934) studied the achievement of 127 fourth graders in the school system, found that children who were older when they began first grade were less likely to repeat one of the first three grades, and also tended to score higher on the Modern School Achievement Test. These achievement differences have been validated recently with arguably better samples and statistical techniques. It is perhaps surprising that there is little research about the mechanisms that lead these gaps to emerge, whether delayed entry passes a reasonable cost-benefit test, and what the implications are for education policy and parental decisions.

Despite our poor understanding of the consequences of delayed kindergarten enrollment, states have been moving their entry cutoffs earlier in the fall in order to raise the average entrance age. 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

1. These figures come from tabulations of the 1980 and 2002 October Supplements to the Current Population Survey.

2. The *New York Times Magazine*, for example, recently profiled the policy discussions and current research (Weil 2007). The large literature about the effects of entrance age on school performance is surveyed in de Cos (1997) and Stipek (2002). Additional recent studies are discussed below.

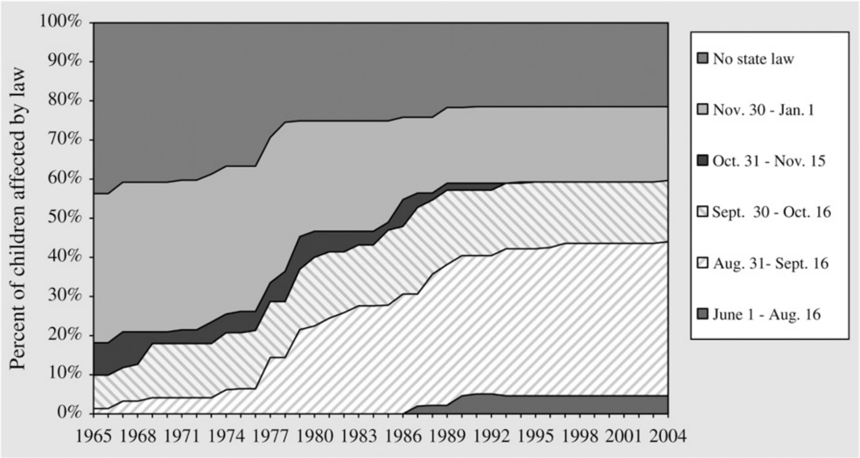

Figure 1
State Entrance Age Cutoffs, 1965-2005
Source: Authors' calculations from state kindergarten statutes. States are weighted by the number of children four to six years old in the state in 1990.

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.

There are two broad reasons why older children do better than their younger classmates, each with different policy implications. The conventional wisdom is that older children are more likely to have the necessary skills and maturity to succeed in school and therefore learn more in each grade. An important implication of this interpretation is that age-related differences in educational outcomes will tend to persist or grow as children progress through school, so that the decision to begin kindergarten at an older age could be a worthwhile investment: Older entrants potentially learn more in school, go further in school, and enter the labor market with more human capital than they otherwise would have.

An alternative view is that age-related differences in early school performance stem solely from prekindergarten learning. Once children begin kindergarten, according to this view, older and younger children tend to learn at the same rate. The skill differences that existed prior to kindergarten tend to fade away as they come to represent a smaller fraction of children's overall stock of knowledge and skills. Parents who hold their child out of school, or states that adjust their entrance requirements to raise the average entrance age, will raise achievement in early grades with little or no long-term benefit to compensate for the high cost paid in terms of lost future working years, additional childcare costs, and potentially reduced educational attainment.[3]

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), to shed light on the mechanisms underlying the relationship between school performance and the age at which children begin kindergarten. Compliance rates with state entrance cutoffs are high in both data sources, implying that the changes in state laws have had powerful effects on the timing of kindergarten entrance. We exploit the fact that these cutoffs generate individual-specific entrance ages that are arguably exogenous with respect to school performance to measure the relationship between entrance age and outcomes.[4] 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, while an otherwise similar child that lives in a state with a September 1 cutoff would have to wait an additional year to enter kindergarten. 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. Based on 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 including Attention Deficit / Hyperactivity Disorder (ADD/ADHD).[5]

Three empirical findings point to prekindergarten 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 could have taken place in school. Second, we present compelling evidence that entrance age effects are much larger and more persistent among children from high socioeconomic status families. This

3. Angrist and Krueger (1992) point out that younger entrants reach the compulsory schooling age having completed more schooling than older entrants. Their empirical analyses of the 1960 and 1980 Censuses indicate younger entrants complete more schooling than older entrants. Dobkin and Ferreira (2006) reach a similar conclusion with more recent data. By contrast, Bedard and Dhuey (2006) and Fredriksson and Öckert (2005) show the opposite, while Fertig and Kluve (2005) find no relationship between entrance age and schooling.

4. Most state cutoffs have provisions that allow children to begin a year earlier than proscribed by law if granted permission by local school administrators. Permission is generally not needed to delay kindergarten entry. State entrance laws apply only to public schools; children who attend private schools are not bound by the state cutoffs.

5. 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); Mayer and Knutson (1999); Leuven et al. (2004); Strøm (2004); Fertig and Kluve (2005); Fredriksson and Öckert (2005); Bedard and Dhuey (2006); Cascio and Lewis (2006); Datar (2006); McCrary and Royer (2006); and McEwan and Shapiro (2008). Our results below are a cautionary note to researchers who use entrance age as an exogenous source of variation in years of completed schooling since entrance age is also associated with school performance, grade progression, and diagnoses of learning disabilities, all of which may directly impact later outcomes conditional on educational attainment.

pattern is consistent with a relatively fast rate of accumulation of human capital among high-income children in the years prior to kindergarten.

Finally, as children progress through school, achievement gaps between older and younger children tend to fade away, consistent with the gaps being relics of prekindergarten learning. If older children were able to learn at a faster rate, one would expect the achievement gaps to widen from one year to the next. By third grade, there is no statistically significant association between entrance age and test scores among the poorest children. Although the entrance age effects among the most advantaged children persist until at least eighth grade, they are only a fraction of the gradient seen in kindergarten.

Even though test score gradients fade away over time, teachers, parents, and school administrators appear to make important decisions based on these early differences: Being a year younger at entry raises the probability of repeating kindergarten, first, or second grade by 13.1 percentage points, a sizeable effect relative to the 8.8 percent baseline retention rate. Similarly, being a year younger at entry raises the probability of being diagnosed with Attention Deficit/Hyperactivity disorder by 2.9 percentage points, which is also large relative to the 4.3 percent baseline diagnosis rate.

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 such as ADD/ADHD. For example, we estimate that moving a kindergarten cutoff from December 1 to September 1 increases ADD/ADHD diagnoses by approximately 25 percent of the baseline rate among children whose own entrance age is unaffected because these children are now younger relative to their classmates. 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.

Our estimates clearly indicate that children's reading and math abilities increase much more quickly once they begin kindergarten than they would have increased during the same time period if they delayed kindergarten entry. In the absence of a future policy that dramatically increases the accumulation of skills prior to kindergarten entry, increases in kindergarten entrance ages have the primary effect of delaying the rapid learning that children experience once they begin school, especially among those from low-income households.

II. The Origin of the Kindergarten Entrance Age Effect

We begin with a simple model of children's human capital accumulation to help understand why entrance age and academic performance may be related. Our model expresses human capital at age t, h_t, as a function of existing human capital, h_{t-1}, and new human capital acquired through parental investments and through schooling:

$$(1) \quad h_t = \beta h_{t-1} + I_t(Y) + \theta_t(S, EA).$$

$I_t(Y)$ are the per-year investment of parents in the human capital of their children, which is a function of parental resources, Y. Parental investments include any learning that takes place prior to kindergarten, including the choice to enroll a child in a preschool program. Following past empirical and theoretical literature, we assume that parental resources are positively associated with parental investment, so $\partial I_t(Y)/\partial Y > 0$. $\theta_t(S, EA)$ is the contribution of year S of schooling to human capital for a child who entered kindergarten at age EA. The return to schooling (at least for early grades) may be larger for children who are older at entry, so that $\partial\theta_t(S, EA)/\partial EA \geq 0$, which captures the idea of "kindergarten readiness."[6] θ_t also could depend on classmates' ages if, for example, lessons are geared to the average child. We study the effect of class average age, $\overline{EA}$, on achievement in Section VII. Without loss of generality, we assume initial investments and human capital are zero, $h_0 = I_0 = 0$. (To economize on notation, we have suppressed an individual subscript.) In this framework, skills depreciate at the rate of (1-β).

This model delivers three important predictions about the relationship between kindergarten entrance age, socioeconomic status, and school achievement: first, gaps in achievement between older and younger kindergarteners will be evident at the beginning of the school year. On the first day of kindergarten, children's human capital consists only of previous parental investments and is given by:

$$(2) \quad h_{EA} = \sum_{j=0}^{EA} \beta^j I_{EA-j}(Y)$$

Children who are older at the beginning of kindergarten will tend to be more skilled because they will have had more time to accumulate human capital during their preschool years. The youngest child in a typical kindergarten classroom tends to be about five years old, a full year younger than the oldest child in the classroom. This one year age difference represents a potentially large difference in prekindergarten learning. Formally, $\partial h_t/\partial EA|_{t=EA} = I_{EA-1}(Y)$, which captures the influence of additional parental investments prior to schooling on human capital at the beginning of kindergarten. One interpretation of this relationship is the entrance age effect among new kindergarten entrants captures the causal effect of a year of parental investments.

The model also predicts that the positive relationship between entrance age and achievement will be larger among children from high socioeconomic families, as long as parental investments are positively associated with parental resources ($\partial^2 h_t/\partial EA \partial Y|_{t=EA} > 0$). Because children in higher socioeconomic families learn

6. $\theta(S,EA)$ also captures the possible effect of ability tracking within schools. Older entrants may learn more in school, for example, if they are disproportionately tracked into classes with advanced students.

at a faster rate, the additional year of learning prior to kindergarten produces a larger difference in skills between older and younger children in rich families than in poorer families. Put differently, exogenous variation in entrance age potentially allows us to measure the differences between rich and poor families in the causal effect of an extra year of parental time and resources prior to kindergarten.

Once children begin school, differences in achievement between children with different entrance ages may result from differences in the return to schooling, differences in contemporaneous parental investments, and differences in prekindergarten skills. For example, human capital one year after kindergarten entry is given by:

$$(3)\quad h_{EA+1} = \beta h_{EA} + I_{EA+1}(Y) + \theta_{EA+1}(1, EA).$$

The first term, βh_{EA}, represents skills acquired prior to the start of kindergarten, the second term represents parental investments made during the first year of school, and the third term, $\theta_{EA+1}(1, EA)$, represents the contribution of the first year of schooling to children's human capital. Because all three of these terms are potentially correlated with a child's entrance age, estimates of differences in achievement between older and younger children in later grades by scholars from Bigelow (1934) to Bedard and Dhuey (2006) confound the ability to learn while in school $(\partial\theta_t(1, EA)/\partial EA|_{t=EA+1})$ with the differences in ability that existed prior to kindergarten.[7]

Finally, the model has important implications for the long-run impact of entrance age on skills and achievement. Human capital after k years of school attendance is given by

$$(4)\quad h_{EA+k} = \beta^k h_{EA} + \sum_{j=1}^{k} \beta^{k-j}\{I_{EA+j}(Y) + \theta_{EA+j}(j, EA)\}.$$

The effect of a one-year increase in kindergarten entrance age on human capital k years after school entry is then

$$(5)\quad \left.\frac{\partial h_t}{\partial EA}\right|_{t=EA+k} = \beta^k I_{EA-1}(Y) + \sum_{j=1}^{k} \beta^{k-j}\left\{\frac{\partial I_{EA+j}(Y)}{\partial EA} + \frac{\partial\theta_{EA+j}(j, EA)}{\partial EA}\right\}.$$

Ignoring parental investments after children begin schooling, kindergarten entrance age may have a lasting effect on human capital for two reasons: First, the effects of skills acquired prior to kindergarten entry by older entrants may fade away at a slow rate (if $\beta = 0.9$, 43 percent of differences in prekindergarten learning will still be noticeable after eight years of schooling). Entrance age effects also may persist if older entrants learn at a faster rate during some grades, so that

7. In an earlier version of this paper, we noted that test score differences by age within a grade reflect both the effect of kindergarten entrance age on scores and the effect of current age on scores. It is not generally possible to separate these effects since entrance age, current age, and accumulated schooling are perfectly collinear among ontrack children. The inability to separate "entrance age effects" from "age-at-test effects" at a point in time is essentially the same idea as our point above that test score differences confound prekindergarten learning with the ability to learn in school. Details are available upon request.

$\partial\theta_t(j, EA)/\partial EA|_{t=EA+j} > 0$. Our final test of whether the relationship between entrance age and achievement is primarily driven by prekindergarten learning or by heightened ability to learn in school is to test whether differences in performance between older and younger entrants expand from one year to the next, at least during the initial years of school.

III. Data

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. Sample statistics are given in Appendix Table A1.

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 1998–99 school year. Individuals were resampled 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 and 2004 (when most were in third grade and fifth grade, respectively). 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 nonmissing information on state of residence.

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.[8] 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 below and in the Appendix). We assign to each child the kindergarten cutoff in his or her state of residence in the fall of 1998, listed in Appendix Table A2. Some states do not have uniform state cutoffs (commonly known as "local education authority option" states), so we exclude children living in those states from our analysis. 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. Although private schools are not bound by the state kindergarten policies, we include children who attend private schools in our sample (and

8. 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).

compute their predicted entrance age using the public school cutoff) since the decision to attend a private school is plausibly related to the local public school's entrance policies.

Our central 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) and percentile test scores to facilitate comparability of scores across individuals and over time. The IRT 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 the test and on the student's past test performance. We compute each child's percentile rank among all children who took the same test in the same year (for example, the percentile among all reading tests taken in the spring of 2000, regardless of the child's grade). 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 three separate indicators: one for whether a child was diagnosed with any learning disability, one for whether a child was diagnosed with ADD or ADHD, and one for whether a child was diagnosed a disability other than ADD or ADHD. ECLS-K provides a host of questionnaire and longitudinal weights for each follow-up, but because our results are largely insensitive to the use of sample weights, we present unweighted estimates throughout.

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

NELS:88 is an NCES survey, which began in the spring of 1988; 1,032 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) and percentile 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. As in the ECLS-K, our results from NELS:88 are not sensitive to the use of sample weights, so we present unweighted estimates below.

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

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).

entrance age, but not actual entrance age, resulting in a decrease in the precision of IV results.[10] The consistency of the estimates is not affected. 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 entrance age is computed as the child's age on September 1 in the year he or she entered kindergarten. Again, we exclude from our analyses children living in states without a uniform kindergarten cutoff.

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

In this section, we discuss methods to identify the relationship between entrance age and children's outcomes. We begin with an intuitive description of our research design, then present the formal instrumental variables framework. The final subsection presents baseline estimates of the relationship between test scores and entrance age.

Variation in kindergarten entrance age potentially stems from three sources: First, the distribution of birthdates throughout the calendar year leads to variation in entrance age among children who begin kindergarten when first allowed by their state's entrance cutoff. Second, differences across states in kindergarten entrance cutoffs create variation in kindergarten entrance ages among children with the same birthday who live in different states. Finally, some children begin kindergarten earlier or later than proscribed by their state entrance cutoff. This can happen either because a child goes to a private school or because a parent petitions the local school for an exception to the state cutoff. Our research design estimates the relationship between entrance age and outcomes based only on the first two sources of variation, because these sources produce variation in entrance age that is arguably unrelated to other factors that influence children's outcomes (this identification assumption is discussed in more detail below). By contrast, parental decisions to delay or expedite their child's kindergarten entry are almost certainly related to other characteristics of parents and children. For example, children who begin kindergarten early are likely to be particularly skilled or gifted, while parents of children with developmental problems are likely to delay their children's enrollment.

A. The Reduced-Form Relationship between Entrance Age and Outcomes

Figure 2 shows the relationship in the ECLS-K between birth month, average actual entrance age, and average predicted entrance age in states with a September 1 cutoff. Recall that we define predicted entrance age as the children's entrance age if he or

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

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 IV estimates 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 percent of cases).

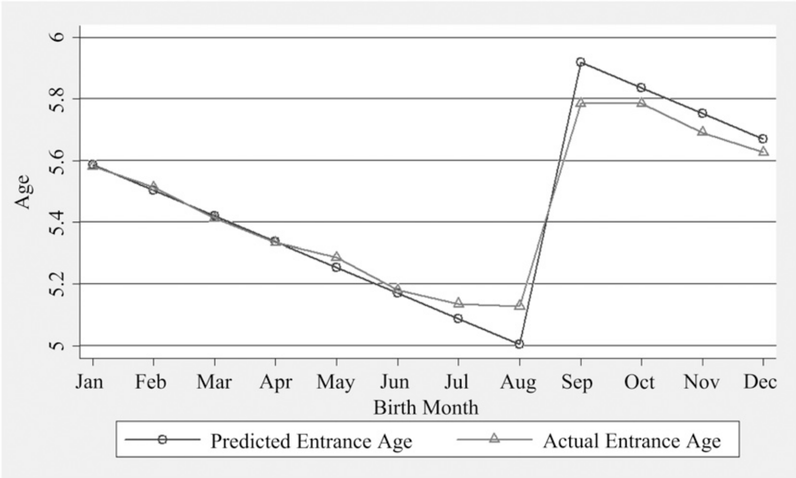

Figure 2
Average Predicted and Actual Entrance Ages by Birth Month in States with September 1 Cutoffs, ECLS-K

she started kindergarten in the year first allow by law.[12] Among children born before September 1, variation in birth month is associated with a month-for-month decrease in predicted 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 those with August birthdays and those with September birthdays. Noncompliance on either side of the entrance cutoff reduces the size of the discontinuity in average entrance age to less than 11 months, but the laws clearly exert a powerful influence on actual entrance decisions.

The discontinuity in entrance age is mirrored by a discontinuity in academic performance. Figure 3 shows the relationship in the ECLS-K between birth month and fall kindergarten math percentile scores in states with August 31 or September 1 cutoffs and states with December 1 or 2 cutoffs. In states with December 1 or 2 cutoffs, the oldest children in the class are born in December 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. Those with November birthdays score about 13.2 percentile points lower than their classmates with December birthdays, implying a one-year entrance age effect of 14.4 percentile points (= 13.2 / (11/12)). By

12. For example, a child born on October 1, 1993 would be four years and 11 months old (approximately 4.92 years 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 4.92. 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.

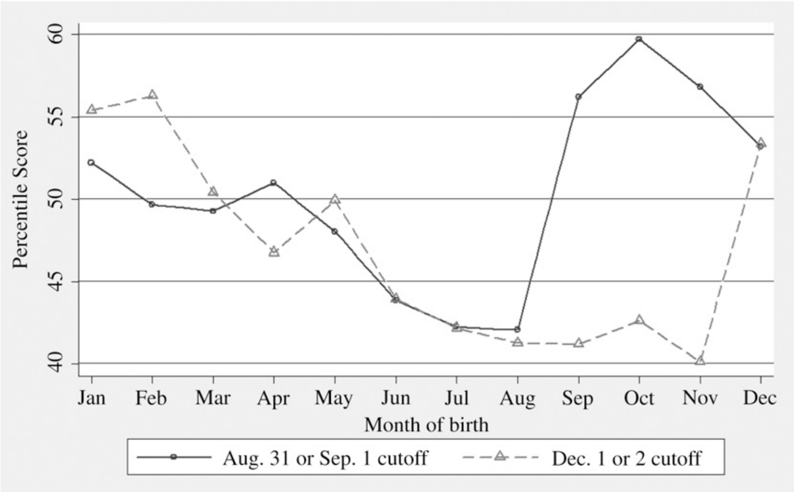

Figure 3
Fall Kindergarten Math Scores by Month of Birth, ECLS-K

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 percentile points. In unreported tabulations, we find a similar pattern with reading scores.

We also note that across-state variation in the entrance cutoff generates differences in the average entrance age within schools or classes. The average entrance age in states with an August 31 or September 1 cutoff is 5.45 years, while the average entrance age in states with a December 1 or 2 cutoff is 5.25 years. In Section VII, we focus on across-state differences in entrance cutoffs to identify separately the influence of a child's own entrance age on his or her outcomes from the influence of peers' average entrance age on outcomes.

Figures 4a and 4b combine within- and across-state variation in entrance age and show the reduced-form relationships between a child's birth month relative to statewide cutoffs and average reading and math percentile scores. Figure 4a shows data from the fall of 1998, when children first begin kindergarten. The oldest entrants, born after their state's cutoff but in the same month, have a value of "month of birth relative to cutoff" of zero and score at roughly the 57th percentile on the math test and the 56th percentile on the reading test.[13] The youngest entrants, born before their state's cutoff in either the same month or the previous

13. For expositional clarity, children who are born in months with mid-month cutoff dates (such as Nebraska, which has an October 15 cutoff) are assigned a value of "month of birth relative to cutoff" of zero if they are born later in the month than the cutoff and minus one if they are born earlier than the cutoff. This arbitrary numbering is not relevant to the estimates presented below.

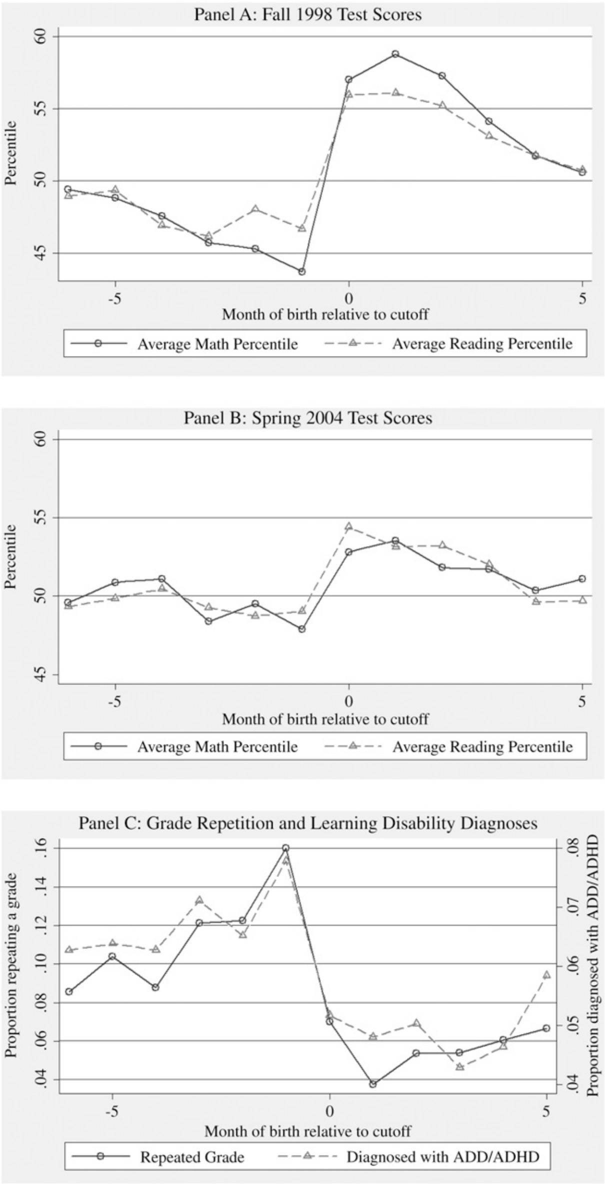

Figure 4
Average Outcomes by Birth Month Relative to Entrance Cutoff, ECLS-K

month, score at approximately the 44th percentile on math and 47th percentile on reading, on average. Apart from the sharp discontinuities among children born near the cutoff, we cannot reject that the relationship between test scores and birth month relative to cutoff is linear. The discontinuity in test scores around the entrance cutoff is noticeably smaller in the spring of 2004 (Figure 4b), when ontrack children are in fifth grade. The youngest entrants score at the 48th and 49th percentiles in math and reading, respectively, while the oldest entrants score at the 53rd and 54th percentiles.

Finally, Figure 4c illustrates the reduced-form relationship in ECLS-K between birth month relative to entrance cutoffs and the probability of being diagnosed with a learning disability or repeating a grade in school by grade three, respectively. As in the pattern for test scores, there is a strong relationship between birth month relative to the cutoff and these outcomes, with a large discontinuity between the oldest and youngest predicted kindergarten entrants. Specifically, the youngest children are diagnosed with learning disabilities at a nearly 50 percent higher rate than the oldest children (note that the figure includes two vertical axes). Among children born just before the cutoff date, the fraction who repeats a grade in school is over 16 percent, nearly triple the grade repetition rate of those born in the months after the cutoff.

Figures 2 through 4 illustrate the essence of the identification strategy we will pursue. If birthdays and kindergarten cutoff dates are exogenous with respect to test scores, grade retention, and learning disability diagnoses, variation in entrance age across predicted entrance ages can identify the causal effect of entrance age on these outcomes. Our main results below use both within-state and across-state sources of variation in entrance ages simultaneously.

B. Baseline Instrumental Variables Model

Our identification strategy uses a child's kindergarten entrance age if he or she began kindergarten when first allowed by state law as an arguably exogenous source of variation in his or her actual entrance age. The baseline model is given by the system

$$(6)\quad Y_i = \alpha EA_i + X_i\gamma + \varepsilon_i$$

$$(7)\quad EA_i = \beta PEA_i + X_i\delta + \nu_i$$

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 cognitive or noncognitive skills, and ν_i represents unobserved determinants of children's entrance age, which also may include a child's ability and maturity, as well as parental characteristics.[14] The coefficient α represents the average effect of entrance age on outcomes. OLS models of Equation 6 will deliver consistent estimates

14. Because 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 7 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.

of α if $\text{Cov}(\varepsilon_i, EA_i|X_i) = 0$, a condition which is not likely to be satisfied because parents choose whether to start a child in kindergarten on time, delay entry, or enter early based on the child's maturity and ability. α is identified in instrumental variables (IV) models if $\text{Cov}(\varepsilon_i, PEA_i|X_i) = 0$.

Our covariates include indicators for gender, race, ethnicity, 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-K, 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.

Consistent estimation of α relies on the exogeneity of both 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. The main results include Census region indicators, which control for regional variation in child ability. Our central findings are robust to the inclusion of state fixed effects, which forces identification to come from within-state variation in birthdates, and to limiting the estimation sample to a "regression discontinuity sample" of those born within one month of their state's entrance cutoff date. Although we do not include the school average entrance age in our baseline models in Section IVC. below, our results in Section VII indicate that the influence of a child's own entrance age on outcomes is not affected when we also include the school average entrance age in the model. The sensitivity of our results is further explored in the Appendix.[15, 16]

Finally, note that IV estimation of Equations 6 and 7 does not require full compliance with entry cutoffs, nor does it require that noncompliance be random. Full

15. Because 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. Following birth cohorts and following kindergarten entry cohorts will not necessarily produce similar estimates if birth year or entry year have independent effects on outcomes. Note also that our results for the NELS are conditional on being in eighth grade, while our ECLS-K results do not condition on children's grade level.

16. We do not pursue a more comprehensive regression discontinuity (RD) strategy in the spirit of Hahn, Todd, and van der Klaauw (2001); van der Klaauw (2002); and McCrary and Royer (2006) for three reasons. First, NELS:88 only reports a child's month of birth, rather than the exact date, so RD strategies based on exact date of birth are not possible in these data. More importantly, 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 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.

compliance would imply equality of entrance age and predicted entrance age, so OLS and IV would deliver identical results. Similarly, if noncompliance were random, OLS estimation of Equation 6 would be consistent and there would be no need for IV. In the conclusion, we discuss the implication of our findings for children who do not enter when first allowed by state law. As in all IV models, estimates of α identify local average treatment effects (LATE) among children whose actual entrance age is affected by their predicted entrance age.[17] The sensitivity analyses in the Appendix indicate that the point estimates are quite insensitive to using very different types of variation in entrance age, identifying different local average treatment effects. This provides some assurance that our findings are applicable to a wide set of children.

C. Baseline Estimates of the Relationship Between Entrance Age and Test Scores

Table 1 presents OLS and IV estimates of the effect of school entrance age on reading test scores from fall 1998, spring 1999, spring 2000, spring 2002, and spring 2004 from the ECLS-K, and from spring 1988 in NELS:88. For a child that follows the normal grade progression, the ECLS-K test dates correspond to the fall and spring of kindergarten, the spring of first grade, the spring of third grade, and the spring of fifth grade. All NELS:88 respondents were in eighth grade in spring 1988. Column 1 shows results from an OLS regression of reading test scores on entrance age without any additional covariates. An additional year of age at kindergarten entry is associated with a 3.79-point increase in fall kindergarten test scores, which is 14 percent of the average score of 27.5 and 38 percent of the standard deviation of scores. The associated standard error is 0.31, which is roughly 0.03 of the standard deviation.[18] Column 2 includes the full set of control variables. The OLS estimate is essentially unchanged, indicating that entrance age is largely uncorrelated with observable determinants of fall 1998 test scores. Columns 3 and 4 present IV estimates with and without control variables. Both of these estimates are larger than the corresponding OLS estimates, implying either that delayed entry is more common among students who would otherwise have low test scores or that early entry is more common among otherwise high-scoring students. The IV estimate with controls shows that being a year older at kindergarten entry increases average fall 1998 test scores by 5.28 points, corresponding to a 0.53 standard deviation effect. Finally, in Column 5 we express the reading test score as a percentile within the ECLS and NELS:88 samples (ranging from one to 100 with a mean of 50), with the IV estimate showing a 16.68 percentile point effect of one year of entrance age.

The relationship between entrance age and reading achievement widens somewhat between the fall and spring of kindergarten, but then steadily declines. An additional year of age at entry is associated with percentile point increases of 19.3 in spring 1999, 14.1 in spring 2000, 11.1 in spring 2002, 11.0 in spring 1998, and 6.2 in eighth grade. While the apparent gains during kindergarten may suggest some heightened ability to learn among older entrants, it is clear that this effect is quite small and short-lived. Note that in the ECLS-K, the raw IRT scores are measured on a common

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

18. All standard errors are robust to clustering at the school level.

Table 1
Estimates of the Effect of Kindergarten Entrance Age on Reading Test Scores

	Mean of IRT test score	Models of IRT test scores by estimation method				Test score percentile
Test date	S.D. N	OLS (1)	OLS (2)	IV (3)	IV (4)	IV (5)
ECLS-K						
Fall 1998	27.5	3.79	3.69	4.15	5.28	16.68
(Kindergarten)	10.0	(0.31)	(0.29)	(0.49)	(0.47)	(1.28)
	11,592	0.018	0.212	0.018	0.209	0.248
Spring 1999	38.9	5.07	5.05	6.20	8.17	19.33
(Kindergarten)	13.4	(0.40)	(0.39)	(0.64)	(0.62)	(1.33)
	11,975	0.018	0.192	0.017	0.187	0.211
Spring 2000	68.0	7.60	7.17	8.11	10.67	14.08
(First grade)	20.7	(0.59)	(0.55)	(0.95)	(0.89)	(1.22)
	12,046	0.017	0.219	0.017	0.216	0.213
Spring 2002	107.5	7.09	5.26	6.54	7.41	11.08
(Third grade)	20.2	(0.72)	(0.60)	(1.03)	(0.88)	(1.27)
	10,336	0.016	0.285	0.016	0.284	0.285
Spring 2004	139.4	7.44	5.64	6.69	8.38	10.59
(Fifth grade)	23.2	(0.86)	(0.73)	(1.27)	(1.09)	(1.33)
	8,210	0.013	0.286	0.013	0.284	0.280
NELS:88						
Spring 1988	50.2	−1.07	−0.34	2.33	2.27	6.21
(Eighth grade)	10.1	(0.19)	(0.15)	(0.50)	(0.50)	(1.40)
	16,213	0.000	0.228	0.000	0.217	0.215
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 robust to clustering at the school level. Covariates are described in the text. Grade levels in parentheses reflect the modal grade of students in each survey.

scale across survey periods, so test scores increase on average from 27.5 in fall 1998 to 139.4 in spring 2004 and become more dispersed over time. As a result, a given percentile point effect will correspond to a larger IRT score effect in later years than in fall 1998.[19]

19. Because there is a good deal of attrition in the ECLS-K sample, we have also run models restricting the sample to children who appear in the Spring 2004 survey and found no substantive differences from the results presented in Tables 1 and 2. We also found that entrance age effects tend to be slightly larger for boys than for girls, but in many cases, the differences are not statistically different from one another. These and other unreported results are available upon request.

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 1.0 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 (a 175 percent increase in income) and by 6.6 times as much as a one-year increase in mother's education.

Table 2 presents estimates of the effects of entrance age on math test scores. IV estimates in Model 5 indicate that an additional year of age at the time of kindergarten entry is associated with a 24.0 percentile point increase in initial math scores, a 9.0 percentile point increase in math scores in the spring of 2004, and a 3.8 percentile point increase in eighth grade. The initial effects are larger than those for reading scores but show the same pattern of decline from kindergarten through later grades, with the effect persisting until eighth grade. These estimates are similar in magnitude to Datar's (2006) findings in kindergarten and first grade and to Bedard and Dhuey's (2006) findings for third and eighth grade.[20]

In the IV models presented in Tables 1 and 2, the estimates are generally insensitive to the inclusion of a rich set of covariates. Although it is difficult to assess whether predicted entrance age is "as good as randomly assigned," the similarity of the point estimates in Columns 3 and 4 provides some reassurance about its validity as an instrumental variable. As mentioned above, in the Appendix we further 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 our baseline results are robust to these alternative specifications, suggesting that these baseline estimates identify a causal effect of entrance age on early educational outcomes.

Our examination of reading and math test scores thus far points to learning prior to kindergarten as the primary source of the relationship between test scores and entrance age. This relationship is strongest in kindergarten, with nearly all of the effect evident at the very beginning of the fall, before any real learning has taken place in school. Moreover, the performance of older and younger children converge relatively rapidly as children progress through school, which indicates that older children are not able to learn at a faster rate in school. The following section tests the final prediction from our model, that entrance age effects are largest among children from rich families.

20. Bedard and Dhuey (2006) use state entrance laws and children's birthdates to instrument for *current* age, rather than kindergarten entrance age, and report slightly larger effects in the NELS:88 than we do. The relatively high rate of grade repetition among the youngest kindergarten entrants reduces the gap in current age in later grades between young and old kindergarten entrants, and this will lead IV estimates of the effect of current age on test scores to be larger than corresponding effects of entrance age on test scores. Current age within a grade can only be manipulated by changing entrance age or grade progression, which is why we study these separately.

Table 2
Estimates of the Effect of Kindergarten Entrance Age on Math Test Scores

	Mean of IRT test score	Models of IRT test scores by estimation method				Test score percentile
	S.D.	OLS	OLS	IV	IV	IV
Test date	N	(1)	(2)	(3)	(4)	(5)
ECLS-K						
Fall 1998	21.5	5.90	5.07	6.62	7.41	24.03
(Kindergarten)	8.9	(0.29)	(0.27)	(0.44)	(0.42)	(1.17)
	12,313	0.056	0.288	0.056	0.281	0.302
Spring 1999	31.6	7.34	6.04	9.17	9.98	25.05
(Kindergarten)	11.5	(0.38)	(0.34)	(0.56)	(0.52)	(1.20)
	12,469	0.052	0.260	0.049	0.248	0.256
Spring 2000	54.6	8.81	7.00	9.72	10.34	18.44
(First grade)	16.0	(0.49)	(0.46)	(0.72)	(0.69)	(1.20)
	12,283	0.039	0.243	0.039	0.238	0.237
Spring 2002	84.6	6.85	5.03	6.43	7.27	11.54
(Third grade)	17.9	(0.58)	(0.52)	(0.86)	(0.74)	(1.20)
	10,411	0.019	0.259	0.019	0.258	0.258
Spring 2004	113.9	5.52	3.82	4.61	6.63	9.04
(Fifth grade)	21.3	(0.77)	(0.68)	(1.12)	(1.00)	(1.33)
	8,218	0.009	0.267	0.008	0.266	0.268
NELS:88						
Spring 1988	50.2	−0.92	−0.29	1.61	1.34	3.78
(Eighth grade)	10.1	(0.22)	(0.17)	(0.54)	(0.50)	(1.42)
	16,213	0.002	0.276	0.000	0.271	0.271
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 robust to clustering at the school level. Covariates are described in the text. Grade levels in parentheses reflect the modal grade of students in each survey.

V. Entrance Age, Achievement, and Family Background

A large body of research shows significant differences in early school performance across socioeconomic stratum and racial groups.[21] 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

21. See Duncan and Brooks-Gunn (1997); Mayer (1997); Carneiro and Heckman (2003); and Fryer and Levitt (2004) among many others.

children with higher levels of investment, children's prekindergarten experience will have a larger effect on test scores among rich children than among poor children. We test this prediction and find evidence that the entrance age effect is substantially larger among children from higher socioeconomic status families, implying that increases in the overall entrance age 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 6, such as gender, race and ethnicity, parental income and education, family structure, region, and urbanicity. 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 observable characteristics. A similar index is created for children in the NELS:88 based on their eighth grade reading test score.

Table 3 provides descriptive information about children, their families, childcare arrangements, and school performance across the four quartiles in ECLS-K, with Quartile 1 representing those with the lowest "family background index" and Quartile 4 representing the highest. Panel A shows that the 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 still 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 that the variables listed in Panel A are related to the family background index (with the exception of the grade repetition measure, these variables are used to construct the index), the correlations indicate that the index is strongly correlated with familiar measures of family background.

In Panel B of Table 3, 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," " three to six 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 lowest quartile report they read to their child every day, while 60.2 percent of parents in the highest 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

Table 3
Child and Household Characteristics by Family Background Quartile

Panel A: Socioeconomic Characteristics

	Fall kindergarten reading score	2nd grade in Spring, 2002	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: Reading and Related Activities

	Parent reads to child everyday	Parent talk to child about nature	Child reads to self everyday	Number of children's books in home	Child reads picture books
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

Panel C: Primary Childcare Arrangement prior to Kindergarten

			Non-Head Start formal care		
	Any formal care	Head Start	Preschool or Nursery School	Prekindergarten	Day Care
Quartile 1	0.576	0.334	0.137	0.099	0.078
Quartile 2	0.644	0.167	0.259	0.143	0.109
Quartile 3	0.734	0.057	0.398	0.177	0.117
Quartile 4	0.820	0.013	0.516	0.208	0.086
Overall	0.582	0.139	0.327	0.157	0.098

Note: Family background quartile is defined in the text. Unless noted, data in Panels A and B refer to characteristics measured in the fall of kindergarten. Panel C refers to childcare arrangements in the year prior to kindergarten. N=11,592
a. Median family income

library), and these children are also more likely to look at picture books every day. The fraction of children who 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.[22]

Finally, Panel C of Table 3 shows tabulations of children's primary source of childcare or schooling in the year prior to kindergarten. To varying degrees, formal childcare settings tend to develop young children's cognitive abilities and the first column of Panel C indicates there are noticeable socioeconomic disparities in formal childcare attendance: 57.6 percent of children in the poorest quartile attended some type of formal childcare, while 82.0 percent of children in the richest quartile did so. Data in the second column indicate that about a third of children in the poorest quartile participated in Head Start, accounting for nearly 60 percent of formal childcare arrangements among this group.[23] The steep gradient in formal childcare enrollment is primarily driven by sharp differences in preschool and prekindergarten enrollment. 51.6 percent of children in the richest quartile were enrolled in preschool or nursery school and an additional 20.8 percent were enrolled in prekindergarten. By contrast, only 23.6 percent of children in the poorest quartile were enrolled in either type of programs. The three panels in Table 3 indicate clear differences in resources and children's experiences across the family background spectrum. Next, we turn to direct evidence on differences in the rate of learning prior to kindergarten.

Table 4 explores the variation in the effect of entrance age across the four family background quartiles. Each entry in the table represents an IV estimate of α from models of test score percentiles. For both reading and math tests, entrance age effects rise with socioeconomic status. For example, being a year older at kindergarten entry raises average fall 1998 reading scores by 10.65 percentile points among students in the poorest quartile and by 23.66 percentile points in the richest quartile; these differences are statistically significant.[24] The remaining results in Panel A show large differences by quartile in reading score effects through fifth grade, and the results in Panel B show similar effects for math scores. Perhaps more importantly, the benefits of an additional year of entrance age "fade out" relatively quickly for the most disadvantaged children — the effect on reading score percentiles among the upper quartiles in fifth and eighth grade is larger than that for the lowest quartile in third grade. As late as fifth grade, the

22. Lubotsky (2001) and Todd and Wolpin (2006) use the National Longitudinal Survey of Youth-Children to show strong correlations between race or parental resources and a variety of parental behaviors that build children's skills among families of school-age children.

23. The four righthand columns of Panel C in Table 3 may add up to more than 100 percent because parents could report that their child was in both Head Start and one of the other arrangements. For simplicity, we combine children whose parents report they are in nursery school (1.6 percent of children) with those who report they are in preschool (31.2 percent of children).

24. The percentile estimates correspond to effects on IRT test scores of 2.8 points and 10.6 points in the lowest and highest quartiles, respectively. In general, the heterogeneity across quartiles appears stronger when tests are measured in IRT units rather than percentiles, but the variance of the IRT scores increases with grade level, making comparisons across grades more difficult. A version of Table 4 with IRT score results is available from the authors upon request.

Table 4
IV Estimates of Test Score Percentiles and Height, by Family Background Quartile

	Full sample	Family background quartile			
A. Reading tests		1	2	3	4
ECLS-K					
Fall 1998	16.68	10.65	15.80	18.79	23.66
(Kindergarten)	(1.28)	(2.18)	(2.47)	(2.54)	(2.89)
Spring 1999	19.33	16.32	19.40	21.92	22.87
(Kindergarten)	(1.33)	(2.29)	(2.70)	(2.53)	(2.94)
Spring 2000	14.08	11.60	13.17	16.97	16.29
(First grade)	(1.22)	(2.31)	(2.50)	(2.63)	(2.80)
Spring 2002	11.08	3.33	11.42	12.47	17.00
(Third grade)	(1.27)	(2.32)	(2.59)	(2.57)	(3.02)
Spring 2004	10.98	4.92	9.18	13.10	16.74
(Fifth grade)	(1.38)	(2.72)	(2.98)	(2.95)	(3.38)
NELS:88					
Spring 1988	6.21	2.24	6.76	9.33	6.01
(Eighth grade)	(1.40)	(2.01)	(2.83)	(2.74)	(3.20)
B. Math tests					
ECLS-K					
Fall 1998	24.03	20.09	22.97	25.96	28.98
(Kindergarten)	(1.17)	(2.25)	(2.40)	(2.38)	(2.78)
Spring 1999	25.05	22.73	22.30	27.19	28.44
(Kindergarten)	(1.20)	(2.25)	(2.44)	(2.42)	(2.94)
Spring 2000	18.44	15.48	17.20	19.94	21.77
(First grade)	(1.20)	(2.25)	(2.45)	(2.53)	(2.96)
Spring 2002	11.54	9.22	9.48	9.83	16.89
(Third grade)	(1.20)	(2.16)	(2.71)	(2.67)	(2.83)
Spring 2004	9.04	4.18	10.72	8.56	11.43
(Fifth grade)	(1.33)	(2.83)	(2.70)	(2.90)	(3.45)
NELS:88					
Spring 1988	3.78	1.93	3.81	6.24	2.11
(Eighth grade)	(1.43)	(2.13)	(2.82)	(2.79)	(2.85)
C. 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. Test scores are measured in percentile units. Standard errors are robust to clustering at the school level.
2) Average height is 44.7″ in fall 1998 and 53.1″ in spring 2002.

estimate for the top quartile is larger than the estimate for the bottom quartile for either fall or spring kindergarten. The patterns for math scores are similar but less dramatic.

The estimates in Table 4 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 for the association between children's entrance age and school performance is that age is strongly associated with physical maturity, which may prepare children for the physical and mental rigors of school. Panel C of Table 4 shows IV estimates of the association between a child's height (as one measure of maturity) 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. More importantly, the next four columns indicate that the relationship between entrance age and 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 and B is of such a large magnitude, it is unlikely that physical maturity is a driving force in any of the entrance age effects found above.[25]

VI. The Importance of Entrance Age in Disability Diagnoses and Grade Retention

As noted above, a number of prior studies have investigated the relationship between entrance age and test scores. Much less is known about how entrance age influences other child outcomes such as the diagnoses of learning disabilities like Attention Deficit/Hyperactivity Disorder (ADD/ADHD) and the successful progression from one grade to the next. Understanding the determinants of these outcomes is important for a number of reasons. Child mental health is among the most important facets of children's human capital, a point that the literature on cognitive development has only recently recognized. Currie and Stabile (2006) argue that children who exhibit symptoms of ADD/ADHD, the most common childhood mental health condition, accumulate skills in reading and math at a slower rate than children with common physical health problems, such as asthma. Diagnosis of ADD/ADHD requires a child to exhibit at least six symptoms by the age of seven and experience these symptoms in at least two settings, such as at home and at school. Teachers are therefore crucial in the process of identifying children who may be in need of professional care. There is also some debate about the accuracy with which child mental health conditions are diagnosed. Since classrooms contain

25. The models presented in Table 4 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 contrast, Puhani and Weber (2005) interview school headmasters and find that 21 out of 22 headmasters believe that 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 4.

children of varying ages, it is possible that teachers' perceptions of children who suffer from ADD/ADHD are clouded in part by differences in relative age and maturity.

Table 5 presents OLS and IV estimates of the effect of entrance age on various measures of learning disability diagnoses and the probability of repeating a grade in school, for both the ECLS-K and NELS:88 full samples and separately by the family background index described in the previous section. Each survey period, the NCES asks parents of ECLS-K 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 related learning disabilities, ADD/ADHD, and developmental delays. 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/ADHD diagnoses separately from diagnoses of all other learning disabilities. We present the results for the overall disability measure in the top row of Table 5. The baseline diagnosis rate is 8.8 percent, and the IV estimate in Column 4) indicates that being a year older at the time of kindergarten entry reduces the probability of diagnosis by 2.5 percentage points, which represents both the effect of being referred to a specialist and the effect of receiving a positive diagnosis. Note that the OLS and IV estimates are substantially different, implying that voluntary delayed entry is positively related to the latent propensity of diagnosis. The next two rows of the table show that ADD/ADHD diagnoses account for the entire entrance age-learning disability gradient, with an additional year of age at entry decreasing the probability of an ADD/ADHD diagnosis by 67 percent (= -0.029 / 0.043) relative to the baseline diagnosis rate. Disabilities other than ADD/ADHD have 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."[26] The results of Table 5 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/ADHD diagnoses.[27] This interpretation may confirm the notion that ADD/ADHD diagnoses are more subjective than diagnoses of mental retardation and learning disabilities such as dyslexia. Some 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 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.

The bottom rows of Table 5 present estimates of the effect of entrance age on the probability of repeating a grade in school. The ECLS-K grade repetition measure is equal to one for children who are in first or second grade in the spring 2002 interview, when ontrack children should be in third grade, and the NELS:88 measure is equal to one if a student reported having to repeat any grade before 1988. In both data sets, IV estimates show that children who enter at older ages are significantly less likely to

26. See, for example, Mick, Biederman, and Faraone (1996).

27. Goodman, Glendhill, and Ford (2003) survey the recent psychological literature and reach a similar conclusion using a sample of 10,438 British children aged five to 15 in 1999.

Table 5
The Effect of Kindergarten Entrance Age on Grade Retention and Learning Disabilities in the Full NELS:88 and ECLS-K Samples and by Family Background Quartile

Dependent Variable	Mean N	OLS (1)	OLS (2)	IV (3)	IV (4)	Family background quartile 1	2	3	4
ECLS-K									
Diagnosis of learning disability/ADD/ADHD/etc.	0.088	0.008	0.005	−0.026	−0.025	−0.038	−0.006	−0.053	−0.012
	12,860	(0.008)	(0.009)	(0.011)	(0.012)	(0.026)	(0.028)	(0.030)	(0.022)
						[−0.332]	[−0.056]	[−0.698]	[−0.190]
Diagnosis of ADD/ADHD	0.043	−0.004	−0.011	−0.021	−0.029	−0.040	−0.008	−0.042	−0.034
	12,860	(0.006)	(0.006)	(0.007)	(0.009)	(0.020)	−(0.049)	(0.021)	(0.015)
						[−0.808]	[−0.132]	[−0.974]	[−1.133]
Diagnosis of non-ADD/ADHD learning disability	0.045	0.012	0.014	−0.004	0.001	0.000	0.003	−0.009	0.018
	12,860	(0.005)	(0.005)	(0.007)	(0.008)	(0.016)	(0.018)	(0.017)	(0.015)
						[−0.004]	[0.077]	[−0.311]	[0.653]
In 1st or 2nd grade in Spring, 2002	0.088	−0.112	−0.112	−0.116	−0.131	−0.214	−0.135	−0.087	−0.120
	10,431	(0.010)	(0.011)	(0.013)	(0.015)	(0.038)	(0.029)	(0.026)	(0.026)
						[−1.232]	[−1.609]	[−1.808]	[−3.269]
NELS:88									
Retained in any grade K-8	0.214	−0.078	−0.092	−0.171	−0.155	−0.185	−0.187	−0.108	−0.112
	16,585	(0.011)	(0.011)	(0.019)	(0.022)	(0.046)	(0.045)	(0.039)	(0.032)
						[−0.497]	[−1.273]	[−0.734]	[−1.332]
Covariates?		No	Yes	No	Yes	Yes	Yes	Yes	Yes

Note: Entries include the coefficient and standard error for each model. Terms in [brackets] are the ratio of the coefficient to the probability of each outcome in each quartile. Standard errors are robust to clustering at the school level.
Covariates are described in the text.

repeat a grade. Our preferred estimates in Column 4 are -0.131 for ECLS-K and -0.155 in NELS:88, both of which are strikingly large relative to the sample probabilities of 0.088 and 0.214, respectively. As was the case for math and reading test scores, in the Appendix we find that the full-sample results of Table 5 are insensitive to alternative specifications such as a discrete version of a regression discontinuity design.

In the four rightmost columns of Table 5, we show separate estimates for each family background quartile. The baseline averages vary considerably across the quartiles for all five outcomes, 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. For example, an additional year of age at kindergarten entry lowers the probability of grade retention 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 is 12.0, which is 3.27 times their baseline retention rate of 3.7 percent. We find no pattern across quartiles for any of the learning disability diagnoses.

Although there appear to be differential effects on grade repetition across the four family background quartiles, this pattern does not shed much light on whether the association is due to learning before or after school entry. Unlike test scores, outcomes such as grade repetition and ADD/ADHD diagnoses confound skills learned prior to school entry and during kindergarten (and later grades) because they are not measured at a point in time immediately after kindergarten entry. Regardless of the reason, younger entrants are apparently more likely to suffer from shortcomings in skills or maturity by the end of kindergarten, and these deficits lead teachers and parents to suggest professional evaluation and grade repetition as remedies. In the following section, we pursue an additional strategy that will shed light on the mechanism underlying the ADD/ADHD and grade repetition effects.

VII. Peer Effects in Kindergarten Entrance Age – Is It Relative or Absolute Age That Matters?

We next investigate whether entrance age laws affect outcomes because they influence an individual child's age, because they influence the average age of a class (and hence a student's age relative to the class average), or both. There are several reasons why the average age of a class may influence student outcomes. First, an older class may have fewer disruptions or allow a teacher to focus on more advanced material.[28] 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.

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

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 entrance age gradients are solely due to the relative age mechanism, changes in entry cutoffs will simply change which children are the youngest in the class and which are the oldest, without any aggregate benefits for skill attainment. These policy changes would involve real costs, though, as some children would be forced to remain out of school an extra year.

To model the independent effect of classmates' average entrance age, we augment Equation 6 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

$$(8) \quad Y_{is} = \varphi_1 EA_{is} + \varphi_2 \overline{EA}_{s,-i} + X_{is}\gamma + \bar{X}_{s,-i}\theta + \varepsilon_{is}.$$

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 variation in absolute age at entry depends on entrance cutoffs and individual birthdays, although variation in school averages is generated by variation in average birth dates across schools and variation across schools in the entrance age cutoffs.[29] 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 8 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:

$$(9) \quad Y_{is} = \delta_1 EA_{is} + \delta_2 (EA_{is} - \overline{EA}_{s,-i}) + X_{is}\gamma + \overline{X}_{s,-i}\theta + \varepsilon_{is}$$

is equivalent to that in Model 8, 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 vice versa), or because teachers design curriculums to best teach the average child. Thus, we proceed with estimates of Equation 8 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.

Table 6 presents IV estimates of Equation 8 for math and reading test scores in ECLS-K and NELS:88. The above discussion implies that the class average entrance age will not be related to fall kindergarten test scores, but may affect later

29. 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 $\phi 2$ 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. Note that we cannot pursue the sort of sensitivity analyses in these models that we did for the models estimated earlier in the paper, but given the results of the Appendix and Appendix Table A3, we feel comfortable using both sources of variation to identify Model 8.

Table 6
The Effect of Individual and Class Average Kindergarten Entrance Age on Test Score Percentiles, ECLS-K and NELS:88

Reading Tests	Sample Size	IV Estimates		Math Tests	Sample Size	IV Estimates	
		Individual entrance age	School average age			Individual entrance age	School average age
ECLS-K				ECLS-K			
Fall 1998	11,576	17.08	5.28	Fall 1998	12295	24.73	0.98
		(1.25)	(3.36)			(1.17)	(2.78)
Spring 1999	11,957	18.66	9.33	Spring 1999	12451	24.33	7.47
		(1.22)	(3.84)			(1.16)	(3.09)
Spring 2000	12,032	13.66	5.55	Spring 2000	12269	17.95	5.59
		(1.15)	(3.58)			(1.14)	(3.19)
Spring 2002	10,323	10.87	3.01	Spring 2002	10398	10.97	5.71
		(1.28)	(2.99)			(1.20)	(3.34)
Spring 2004	8,199	10.55	4.67	Spring 2004	8207	9.52	−0.59
		(1.44)	(3.22)			(1.42)	(3.76)
NELS:88				NELS:88			
Spring 1988	16,209	5.72	3.11	Spring 1988	16206	3.47	2.77
(Eighth grade)		(1.41)	(3.83)	(Eighth grade)		(1.33)	(4.17)

Note: All models control for the individual covariates described in the text and school averages of those covariates. Standard errors (in parentheses) are robust to clustering at the school level.

outcomes. Although the standard errors are quite large, the estimates generally support this pattern: The coefficients on the class average entrance age in models of fall kindergarten math and reading test scores are 1.0 percentage points (with a standard error of 2.9) and 5.3 percentage points (with a standard error of 3.4), neither of which is statistically different from zero. Consistent with the idea that older classmates generate positive spillovers to younger children, the coefficients on the class average entrance age increase between fall and spring of kindergarten for both math and reading scores. The coefficients on the class average entrance age in the spring of kindergarten are 7.5 percentage points (with a standard error of 3.1) and 9.3 percentage points (with a standard error of 3.9) for math and reading tests. The effects decline after the spring of kindergarten, much like the effects of a child's own entrance age, and are generally about one-third to one-half of the size of the effect of a child's own entrance age, though they are generally estimated imprecisely.

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 17.1 point increase in Fall 1998 reading test percentiles and a 24.1 percentile point increase in math test scores, which are nearly identical to the estimates of 16.7 and 24.0 from Column 5 of Tables 1 and 2. Finally, our estimates of the impact of peers' entrance age and a child's own entrance age are not affected when we restrict the sample to children who were born between January and July and who live in states with entrance cutoffs after July 31. Among these children, variation in the predicted peer entrance age is only driven by difference in the entrance cutoff, while variation in the child's own entrance age is only driven by difference in birthdates.

Table 7 presents estimates of Equation 8 for the probability of being diagnosed with learning disabilities or repeating a grade in ECLS-K and NELS:88. In contrast to the results for test scores, there appears to be a modest *detrimental* peer effect on the probabilities of learning disability diagnoses and grade repetition. An increase in a class's average entrance 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 (that is, those born between December 1 and August 31, assuming full compliance with the law). While the estimates are somewhat noisy, and in the case of grade retention, statistically insignificant at conventional levels, they suggest that the beneficial effects older classmates exert on test scores do not extend to grade progression or diagnoses of learning disabilities. 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 5. Finally, note that we also report estimates of Equation 8 using height as an outcome variable as a basic specification check, finding no effect of the average age of peers on a child's own height in spring 2002.[30]

30. In unreported results, we find no evidence of an association between class average age and a host of other individual characteristics such as weight and family structure.

Table 7
The Effect of Individual and Class Average Kindergarten Entrance Age on Learning Disabilities, Grade Retention, and Height, ECLS-K and NELS:88

	Sample size	IV Estimates	
		Individual entrance age	School average age
ECLS-K			
Diagnosed with a learning disability / ADD / ADHD / etc.	12,840	−0.034	0.048
		(0.013)	(0.028)
In 1st or 2nd Grade in Spring 2002	12,377	−0.141	0.023
		(0.014)	(0.038)
Height in inches in Spring 2002	10,080	2.223	0.116
		(0.145)	(0.330)
NELS:88			
Retained in Any Grade K-8	16,579	−0.173	0.113
		(0.021)	(0.058)

Note: All models control for the individual covariates described in the text and school averages of those covariates. Standard errors (in parentheses) are robust to clustering at the school level.

In NELS:88, the positive (detrimental) effect of peers' average age on grade repetition is large and statistically significant. Assuming full compliance with state laws, a change in a state's kindergarten cutoff from December 1 to September 1 would increase the grade repetition rate by 2.8 percentage points (0.113 divided by four) 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). As with ADD/ADHD diagnoses, we interpret the positive effect of classmates' entrance age on the probability of being retained in grade as indicating that teachers use relative comparisons among children to aid in determining which children should be held back.

In summary, a child's age at entry into kindergarten and the average age of his classmates both appear to boost early achievement test scores, suggesting 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. This apparent use of relative standards by school officials may reinforce concerns that the evaluation and diagnosis of ADD/ADHD can be quite subjective. To the extent that learning disability diagnoses or grade repetition are undesirable educational outcomes, these findings also represent an unusual 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.

VIII. The Benefits of a Year of Prekindergarten Preparation Versus a Year Spent in Kindergarten

The results presented above establish that the relationship between entrance age and children's outcomes is largely caused by the lasting effects of skills acquired before kindergarten begins. The evidence is not consistent with the notion that delayed kindergarten entry increases the return to each year of schooling. Raising children's kindergarten entrance age, however, may still be an effective policy if children tend to learn more during an extra year at home than they would learn during a year of school. In this section, we highlight the tradeoff between a year spent at home versus a year spent in school.

In the ECLS-K, IRT test scores are comparable across grades and dates because all tests share a common scale. According to the estimates in Table 1, average IRT reading test scores increase from 38.9 to 68.0 between spring 1999 and spring 2000, a gain of 29.1 points; average math test scores increase by 23.0 points over the same period. Expressed as a fraction of the baseline standard deviation of test scores, these gains are qualitatively similar to those found in other data by Gormley and Gayer (2005) and Cascio and Lewis (2006). Importantly, these year-to-year gains from a year of schooling are considerably larger than any of the corresponding estimates of entrance age effects. Using the estimates from spring 1999, the ratio of the benefit of a year spent out of school to the within-school yearly increase in average test scores is 0.28 (= 8.17 / 29.1) for reading scores and 0.43 (= 9.98 / 23.0) for math scores. This implies that children's test scores increase much more quickly at young ages when they are in school than when they are not.

In Figures 5a and 5b, we provide additional evidence about the relative effectiveness of a year spent in school versus a year preparing for kindergarten by comparing test scores at age six years, six months among predicted early entrants and predicted older entrants. Figure 5a plots the empirical distribution of IRT reading scores among the 1,835 children in the ECLS-K who have predicted entrance ages between four years, ten months and five years; and among the 1,295 children who have predicted entrance ages between five years, ten months and six years. If there is full compliance with entrance age laws, the former group will be roughly the same age when they take the spring 2000 test (when ontrack children would be in first grade) as the latter group is when they take the spring 1999 test. To account for noncompliance, we replace the spring 2000 test score with the spring 1999 test score for children who should have been young kindergarten entrants but instead delayed enrollment. We similarly replace the Spring 1999 test score with the spring 2000 test score for children who should have been older entrants but instead entered early. This ensures that the average age at the time of the test is nearly identical in the two groups; the groups only differ by their predicted entrance age. The difference between the two distributions is striking: The children who are predicted to be younger entrants score much higher than the predicted older entrants do, even though they take the test at roughly the same age. The median of the predicted old entrants' test scores is 40.0 and is at roughly the 20th percentile of predicted younger entrants' scores, while the median of the predicted younger entrants' scores is 69.3, roughly the 85th percentile of the predicted old entrants' scores.

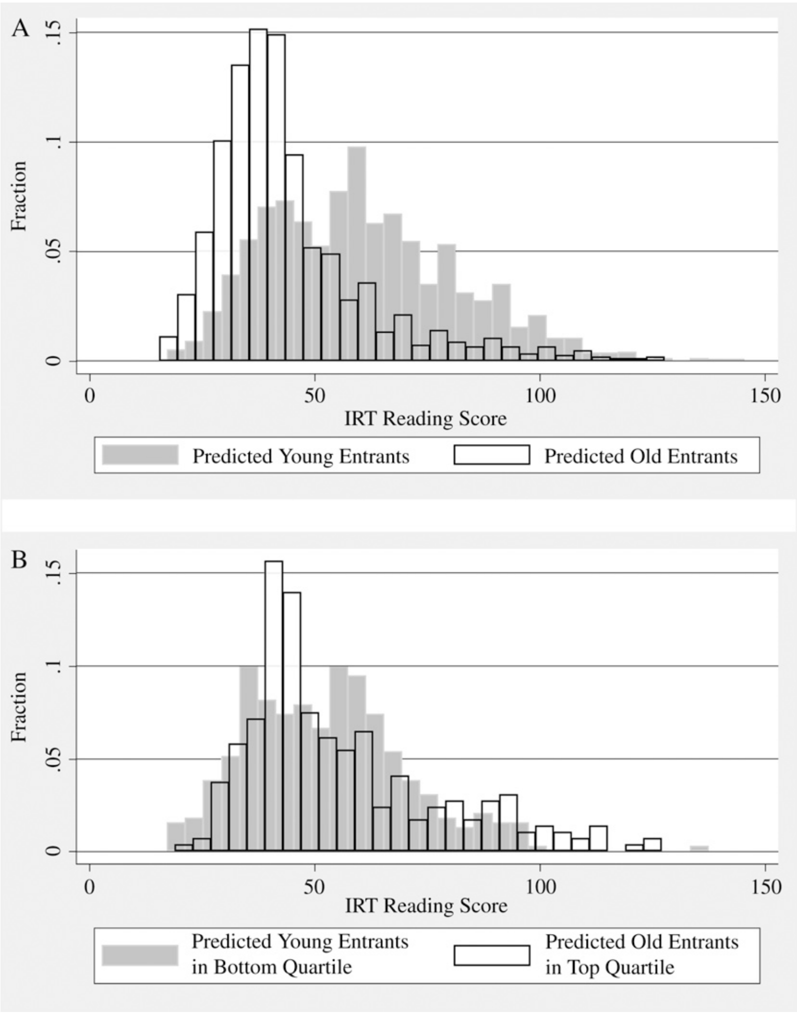

Figure 5

Distributions of Reading Scores at Age Six

Note: Predicted young entrants include children whose predicted entrance age is between four years and ten months. Predicted old entrants include children whose predicted entrance age is between five years and ten months. See text for additional details.

Figure 5b provides a framework for judging the magnitude of these differences by plotting the distribution of test scores among predicted early entrants from the poorest quartile of the family background index with the distribution of test scores of predicted older entrants from the richest quartile. The test scores of the two sets of children have qualitatively similar distributions; the mean among the poorest children is 3.2 points higher, or 20 percent of the standard deviation of scores among the rich group of children. That is, delaying kindergarten entry has the same negative effect on age-specific test scores as moving from the richest quartile to the poorest one.

The comparisons above imply that a policy designed to increase average entrance ages will raise average test scores at a given *grade* level but will substantially lower average test scores at a given *age* level, since children will have been in school fewer years at each age. While some parents may care about their child's rank within a grade, presumably the goal of policy is to raise test scores at a given age level.[31] If there are no long-term benefits of delayed kindergarten entry, the changes in the kindergarten cutoffs over the last three decades that increased average entrance ages have tended to reduce academic abilities among children of a given age, a result that likely runs counter to the goal of public educational policies.[32]

IX. Conclusions

The evidence presented in this paper indicates that the familiar positive relationship between achievement and the age at which children begin kindergarten is primarily driven by the skills older children acquired prior to kindergarten. 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, but sharply decline during the first few years of school enrollment. Among the most disadvantaged children, entrance age effects essentially disappear as early as fifth grade. We find no evidence to support the popular notion that older children learn at a faster rate, which corroborates other recent evidence that there are no long-term beneficial effects on earnings from entering kindergarten at an older age (Fredriksson and Öckert 2005; Dobkin and Ferreira 2006).

These initial differences in skills influence parents and teachers' decisions to intervene in dramatic ways: being a year older at the beginning of kindergarten decreases the probability of repeating kindergarten, first, or second 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, 75 percent of the baseline diagnosis rate.

31. In addition, as Stipek (2002) points out, some school administrators may be interested in raising achievement at each grade level to meet requirements of school accountability measures, even if that comes at the expense of slowing children's educational progress.

32. This is consistent with research by Michael (2003) who shows that five- to nine-year-old children in the United Kingdom tend to score higher than similarly aged American children on standardized reading and math tests. Michael attributes this difference to the fact that American children tend to start formal schooling at older ages.

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 a state's entrance 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 also must 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 will lower achievement at each age level. Such a change also will 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 IV estimates measure local average treatment effects of entrance age for children whose entrance age is influenced by state entry cutoffs. Since beginning kindergarten earlier or later than proscribed by law represents noncompliance with state kindergarten cutoffs, the existence of heterogeneous treatment effects implies that IV estimates may be 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, allows children to drop out of school having completed fewer years of schooling, and reduces future earnings of the child, because an extra year of preparation for kindergarten delays entry into the job market by one year. To the extent there are long-run benefits of being older at kindergarten entry, the benefits must be weighed against the costs to know whether, and for whom, delayed entry is worthwhile.

Appendix 1

Sensitivity Analysis

The identification strategy discussed in Section IV 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 IV estimates to the inclusion of controls suggests that predicted entrance age might be "as good as randomly assigned," 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 because states choose their kindergarten cutoff date taking into account the socioeconomic status of families in the state or the school performance of children compared with 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 A3 explores the robustness of our main results to different identification assumptions. For purposes of comparison, Column 1 of the table replicates the IV estimates from Tables 1, 2, and 5, using specifications that include all 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 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. The effect of entrance age on diagnoses of learning disabilities represents an exception, increasing from -0.025 to -0.009.

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. Since variation in the peer average entrance age is largely driven by across-state variation in the entrance cutoff, it is unlikely that these models suffer any omitted variables bias caused by the exclusion of mean peer entrance age from the model. 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 learning disability diagnoses 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.

Table A1
Means of Selected Characteristics

	Data source:	
	ECLS-K	NELS:88
Actual kindergarten entrance age (years)	5.40	5.33
Predicted kindergarten entrance age	5.37	5.28
Female	0.489	0.506
Hispanic	0.182	0.139
Black	0.146	0.126
Female headed household	0.190	0.160
Season of birth		
January–March	0.242	0.230
April–June	0.253	0.257
July–September	0.266	0.268
October–December	0.239	0.245
Region of residence		
Midwest	0.309	0.323
South	0.393	0.440
West	0.243	0.213
Northeast	0.055	0.024
Urbanicity		
City	0.414	0.332
Suburban	0.353	0.360
Rural	0.233	0.308
Mother's education (years)	13.4	13.2
Father's education	13.5	13.6
Household size (persons)	4.5	4.6
Family income	$52,128.65	$40,634.14
Sample size	12,328	16,213

Note: Means from the ECLS-K refer to characteristics at the baseline survey in 1998. Means from the NELS:88 refer to characteristics in eighth grade. All means are unweighted. The ECLS-K sample includes all children with valid reading or math test scores in the fall of kindergarten. The NELS:88 sample includes all children with valid reading or math scores in eighth grade. Both samples only include children who live in states with uniform kindergarten entrance cutoffs, as described in the text. Predicted kindergarten entrance age is the entrance age if the child entered when first allowed by state law. Means of mother and father's education refer only to parents who reside in the household. Family income in the ECLS-K is measured in 1998 dollars. Family income in the NELS:88 is measured in 1988 dollars.

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

Table A2
Kindergartern Cutoff Dates in the U.S.

	NELS:88 1978–1980	ECLS-K (1998)		NELS-88 (1978–1980)	ECLS-K (1998)
AL	Oct. 1	Sep. 1	NE	Oct. 15	Oct. 15
AK	Nov. 2	Aug. 15	NV	Sep. 30	Sep. 30
AZ	Jan. 1 (1978)	Sep. 1	NH	LEA	LEA
	Dec. 1 (1979)		NJ	LEA	LEA
	Nov. 1 (1980)		NM	Sep. 1	Sep. 1
AR	Oct. 1	Sep. 1	NY	LEA	LEA
CA	Dec. 2	Dec. 2	NC	Oct. 15	Oct. 15
CO	LEA	LEA	ND	Aug. 31	Aug. 31
CT	Jan. 1	Jan. 1	OH	Sep. 30	Sep. 30
DC	Dec. 31	Dec. 31	OK	Nov. 1 (1978-1979)	Sep. 1
DE	Dec. 31	Aug. 31		Sep. 1 (1980)	
FL	Jan. 1 (1978)	Sep. 1	OR	Nov. 15	Sep. 1
	Sep. 1 (1979–1980))		PA	LEA	LEA
GA	Sep. 1	Sep. 1	RI	Dec. 31	Dec. 31
HI	Dec. 31	Dec. 31	SC	Nov. 1	Sep. 1
ID	Oct. 15	Sep. 1	SD	Nov. 1 (1978)	Sep. 1
IL	Dec. 1	Sep. 1		Sep. 1 (1979–1980)	
IN	LEA	June 1	TN	Oct. 31	Sep. 30
IA	Sep. 15	Sep. 15	TX	Sep. 1	Sep. 1
KS	Sep. 1	Sep. 1	UT	LEA	Sep. 2
KY	Dec. 31 (1978)	Oct. 1	VT	Jan. 1	LEA
	Sep. 1 (1979)		VA	Dec. 31	Sep. 30
	Oct. 1 (1980)		WA	LEA	Aug. 31
LA	Dec. 31	Sep. 30	WV	Nov. 1	Aug. 31
ME	Oct. 15	Oct. 15	WI	Dec. 1 (1978)	Sep. 1
MD	Dec. 31	Dec.31		Nov. 1 (1979)	
MA	LEA	LEA		Oct. 1 (1980)	
MI	Dec. 1	Dec. 1	WY	Sep. 15	Sep. 15
MIN	Sep. 1	Sep. 1			
MS	Nov. 1 (1978)	Sep. 1			
	Oct. 1 (1979)				
	Sep. 1 (1980)				
MO	Oct. 1	Aug. 1			
MT	LEA (1978)	Sep. 10			
	Sep. 10 (1979–1980))				

Note: An entry of "LEA" refers to states that leave Kindergarten entrance age cutoff policies to local education authorities (typically school districts).

Table A3
Sensitivity of IV Results to controls for State of Residence, Birth Month, and Limiting the sample to Those Born within a Month of School Entry Cutoff Dates, Ecls-K

Dependent Variable:	Model			
	(1)	(2)	(3)	(4)
ECLS IRT Reading Scores				
Fall 1998	5.28	5.28	5.00	4.83
	(0.47)	(0.49)	(0.49)	(0.88)
Spring 1999	8.17	8.34	7.68	7.26
	(0.62)	(0.66)	(0.65)	(1.20)
Spring 2000	10.67	10.86	10.08	10.17
	(0.89)	(0.96)	(0.90)	(1.58)
Spring 2002	7.41	7.76	7.07	5.75
	(0.88)	(0.93)	(0.90)	(1.58)
Spring 2004	8.38	8.18	7.80	6.85
	(1.09)	(1.14)	(1.16)	(1.98)
ECLS IRT Math Scores				
Fall 1998	7.41	7.46	7.45	7.89
	(0.42)	(0.44)	(0.42)	(0.80)
Spring 1999	9.98	10.18	9.48	10.41
	(0.52)	(0.54)	(0.52)	(0.92)
Spring 2000	10.34	10.62	9.85	10.36
	(0.69)	(0.74)	(0.67)	(1.12)
Spring 2002	7.27	7.37	6.57	6.37
	(0.74)	(0.78)	(0.79)	(1.46)
Spring 2004	6.63	6.01	6.58	6.39
	(1.00)	(1.07)	(1.08)	(1.85)
"Discontinuity Sample"	No	No	No	Yes
Quarter of birth indicators	Yes	No	Yes	Yes
Month of birth indicators	No	Yes	No	No
Census region indicators	Yes	Yes	No	Yes
State indicators	No	No	Yes	No

Notes: The entries for each model are the coefficient with the standard error in parentheses. Standard errors are robust to clustering at the school level. Covariates are described in the text. Models with the "Discontinuity Sample" also include for the entrance cutoff date.
Sample sizes in column (4) are 1689, 1750, 1754, 1531, 1197, 1772, 1807, 1781, 1546, and 1997 for Fall 1998, Spring 1999, Spring 2000, Spring 2002, Spring 2004 reading scores and Fall 1998, Spring 1999, Spring 2000, Spring 2002, and Spring 2004 math score.

Table A3 (continued)

Dependent Variable:	Model			
	(1)	(2)	(3)	(4)
Diagnosis of learning disability/ ADD/ADHD/etc.				
	−0.025	−0.009	−0.035	−0.044
	(0.012)	(0.013)	(0.013)	(0.021)
NELS:88 Outcomes				
8th Grade Reading Score	2.27	2.39	2.07	2.53
	(0.50)	(0.54)	(0.51)	(0.77)
8th Grade Math Score	1.34	1.64	1.36	2.32
	(0.50)	(0.54)	(0.48)	(0.78)
Held back prior to 8th grade	−0.155	−0.154	−0.181	−0.152
	(0.022)	(0.024)	(0.021)	(0.035)
"Discontinuity Sample"	No	No	No	Yes
Quarter of birth indicators	Yes	No	Yes	Yes
Month of birth indicators	No	Yes	No	No
Census region indicators	Yes	Yes	No	Yes
State indicators	No	No	Yes	No

Notes: The entries for each model are the coefficient with the standard error in parentheses. Standard errors are robust to clustering at the school level. Covariates are described in the text. Models with the "Discontinuity Sample" also include indicators for the entrance cutoff date.
Sample sizes in column (4) are 1547 and 1863 for "In 1st or 2nd grade in Spring 2002", and "learning disability diagnosis", and 2553, 2546, and 2609 for 8th grade reading and math scores and "Held back prior to 8th grade", respectively.

long as children born close to the cutoff date are similar along unobservable dimensions. These models also include indicators for each cutoff date, thereby circumventing 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.[33] As with the models in Column 3 that include state fixed effects, estimates from these models minimize any potential bias caused by omitting the class average entrance age from the model.

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. As a result of using only two months of birth dates in each state, the estimation

33. Sorting around the cutoff is an important threat to the internal validity of regression discontinuity designs. Recent work, however, by McCrary and Royer (2006) on births in California and Texas, and by McEwan and Shapiro (2008) on births in Chile, suggests that sorting around school entry cutoffs does not systemically bias regression discontinuity estimates. Our own examination of the ECLS-K data indicates that there are no statistically significant differences in covariates between children born on either side of the cutoff.

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) so the 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 eight of the fifteen outcomes and larger for the remaining seven outcomes, suggesting that 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.[34]

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34. We have performed several additional robustness checks, including estimating all dichotomous outcome models as IV-probit models, stratifying the samples based on gender, eliminating children attending private schools from the analysis, and eliminating children born in states with December 31 or January 1 cutoffs. None of these alternative specifications or samples had a substantive impact on the results. We also estimated models separately by background quartile using the discontinuity sample, with results quite similar to those in Tables 4 and 5. Additional results are available upon request.

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