Disabled or Young?
Relative Age and Special Education Diagnoses in Schools

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Abstract

This study extends recent findings of a relationship between the relative age of students among their peers and their probability of disability classification. Using three nationally representative surveys spanning 1988-2004 and grades K-10, we find that an additional month of relative age decreases the likelihood of receiving special education services by 2-5 percent. Relative age effects are strong for learning disabilities but not for other disabilities. We measure them for boys starting in kindergarten but not for girls until 3rd grade. We also measure them for white and Hispanic students but not for black students or differentially by socioeconomic quartiles. Results are consistent with the interpretation that disability assessments do not screen for the possibility that relatively young students are over-referred for evaluation. Lastly, we present suggestive evidence that math achievement gains due to disability classification may differentially benefit relatively young students.

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1. Introduction

Students with disabilities represented about 13.7 percent of the public school enrollment in the United States by 2005-06, with about half diagnosed with learning disabilities.[1] All students with disabilities are entitled by law to a free and appropriate public education, which can be considerably more costly than educating students not classified with special needs. Spending on students with disabilities has been estimated to be 90 percent higher than for other students, on average (Chambers, Parrish, and Harr, 2004)[2]. Special education spending also has grown faster than regular education spending since the 1980s, representing a larger share of district budgets (Lankford and Wyckoff, 1995; Parrish, 2001).

A recent study by Elder and Lubotsky (2009) finds compelling evidence that school officials may use relative standards in classifying children as having a disability. Their results indicate that children who start school at older biological ages are less likely to be classified with Attention Deficit Disorder (ADD) or Attention Deficit Hyperactivity Disorder (ADHD) by fifth grade.[3] The effects are large; starting school a year older decreases the likelihood of diagnosis with one of these conditions by 67 percent. Conditional on students' individual entry ages, the probability of diagnosis appears to rise with the average starting age of their school peers.

The findings in Elder and Lubotsky (2009) regarding ADD and ADHD are part of a larger study of the relationship between school starting age and academic achievement that also examines test score and grade repetition outcomes, and interactions with socioeconomic status. The purpose of our study is to expand the research begun by Elder and Lubotsky on disability patterns with respect to school starting age by considering a wider range of outcomes, data sources, and grades. Specifically, we disaggregate disabilities by type, further investigate interactions with demographic characteristics, and examine disability evaluation and diagnosis processes separately. Although our main data source— the *Early Childhood Longitudinal Study, Kindergarten Class of 1998-99* (*ECLS*) —is the same, we find consistent results across two other national samples as well.[4] Each of these analyses helps to provide a fuller picture of the role of school starting age in special education classification decisions. Moreover, all of our results

[1] See https://www.ideadata.org, Table B1

[2] Duncombe and Yinger (2005) detail methods to estimate the extra costs of educating disadvantaged students.

[3] Goodman, Gledhill, and Ford (2003) find a similar negative relationship between relative age and child psychiatric disorders in the United Kingdom.

[4] The other data sources are the *National Education Longitudinal Study* (*NELS)* and the *Education Longitudinal Study (ELS)*.

include school effects, meaning that inferences pertain to age-based differences in classification patterns within individual schools. The final section of our analysis considers the question of whether special education enrollment may help to narrow the test score gap that has been shown to exist between students starting school at younger and older ages.

Most studies that investigate possible implications of different school starting ages utilize the fact that many states have a uniform cutoff date that determines when a child is old enough to begin formal schooling. If the cutoff date is September 1, a child must be five years old by September 1 to enter kindergarten at the beginning of that school year. The distribution of birthdates throughout the year relative to this cutoff creates a range of ages at school entry. A child born in August and entering school in a state with a September 1 cutoff will start kindergarten at approximately 60 months old as the youngest in her cohort. A child born in September, in contrast, must wait an additional year to enroll and consequently will become the relatively oldest in her cohort.

We find that an additional month of age relative to the cutoff date is associated with a 2 to 5 percent reduction in the probability of receiving special education services, depending on the sample. Unless the incidence of disabilities across students relates systematically to their month of birth in relation to a state legislature's choice of cutoff date, our findings support Elder and Lubotsky's conclusion of an apparent relative standard for identifying childhood disabilities. Specifically, parents and schools may use special education classification in part to target supplemental services to students whose disability may simply be relative youth. To the extent they do, we are unaware of research on whether it is the most cost effective approach to increasing educational outcomes. The fiscal implications are important for schools because children with disabilities have a legal entitlement to free, appropriate services once classified.

Our focus on a determinant of classification decisions within school cohorts separates this study from most economic research on special education, which largely concentrates on how special education enrollment responds to fiscal and accountability systems, student peer effects, and program effectiveness. For instance, a consensus is emerging that institutional incentives do affect special education enrollment rates.[5] The evidence on the peer effects of having disabled peers in the classroom points to small effects, although it is unresolved whether they are positive

[5] For example, see Cullen (2003), Cullen and Reback (2006), Dhuey and Lipscomb (2010), Figlio and Getzler (2002), Jacob (2005), Kwak (2008), and Mahitivanichcha and Parrish (2005).

or negative (Fletcher, 2010; Friesen and Krauth, 2008; Hanushek, Kain and Rivkin, 2002). Finally, Cohen (2007) and Hanushek, Kain, and Rivkin (2002) find that special education services do boost student achievement but neither study can speak to the cost effectiveness of these programs.

This study analyzes three recent nationally representative surveys of school-aged cohorts that span 1988 through 2004 and encompass kindergarten through 10th grade. Across the different samples and survey years, we find consistent evidence that relatively older students are less likely to be evaluated for a possible disability and less likely to be diagnosed with one. The strongest evidence of relative age effects is for learning problems. In contrast, relative age effects in categories like hearing problems and orthopedic problems are statistically insignificant and numerically small. These results are consistent with the notion that identifying learning disabilities is a more subjective process. In fact, this subjectivity may give rise to relative age effects within the special education system.

The analysis next focuses on the evaluation and diagnosis of learning problems to better understand whether relative age effects differ by gender, race/ethnicity, and socioeconomic status. We find larger effects for boys than for girls. In fact, we cannot reject that boys are entirely responsible for the overall effect up until 3rd grade. In later grades, however, measurable relative age effects emerge for girls as well. Across race/ethnic groups, the relative age effect is strongest among white students. There is some evidence of a relative age effect among Hispanic students in some years but no evidence that it exists among black students. Although insufficient statistical power is a possible explanation, we do find statistically larger effects for white students than black students in some cases, suggesting that relative age effects for black students are small, if they exist at all. Consistent with Elder and Lubotsky (2009), we find no differences by socioeconomic quartiles.

Our ability to track disability evaluations and diagnoses separately through the 5th grade allows us to examine the likelihood that students who are evaluated for a possible disability are ultimately diagnosed with one. On average, relatively younger students are more likely to be evaluated but just as likely to be diagnosed once evaluated. We find some differences in this relationship by gender, but no difference by race/ethnicity or socioeconomic factors. These results point to age-based differences in classification rates emerging at the referral stage, with little evidence to support diagnostic assessments fully screening out children who are over-referred based on their relative youth.

Lastly, we find marginally significant evidence of a positive relationship between disability classification and standardized test score gains in math using a model with student fixed effects. Math score gains are largest for the relatively youngest students, suggesting that special education programs may help reduce achievement gaps between children that enter school at older and younger ages. By comparison, math scores for the relatively oldest children appear to fall following disability identification. These findings are not based on a causal research design but they suggest that there may be an important interaction between the academic benefits of special education programs and the age that students enter kindergarten.

Our results contribute to a growing economic literature regarding relative age effects. Many studies demonstrate that children that are relatively older than their classmates at school entry are more likely to benefit in terms of a wide range of important outcomes. For example, relatively older students score higher on standardized achievement tests (Bedard and Dhuey, 2006; Datar, 2006; Elder and Lubotsky, 2009; Puhani and Weber, 2007; Smith, 2009), enroll in college more frequently (Bedard and Dhuey, 2006), are more likely to become high school leaders (Dhuey and Lipscomb, 2008), and earn higher adult wages (Fredriksson and Öckert, 2006). However, not all studies conclude that there are lasting long-term benefits to starting school at older ages (Dobkin and Ferreira, 2010; Elder and Lubotsky, 2009; Fertig and Kluve, 2005).

Currently no definitive answer exists as to how age exactly affects outcomes because relatively older children are also biologically older and take the standardized exams at an older age. A common explanation is the inherent difficulty in distinguishing between maturity and ability when children are young and beginning formal schooling. This difficulty may lead some relatively younger students to be placed in a lower stream or track (Allen and Barnsley, 1993). However, Elder and Lubotsky (2009) conclude that observed relative age effects are the outcome of differences in early educational experiences prior to formal schooling. In other words, the biological age difference is more important than the relative age difference. Additional recent studies try to separate the effect of entering school at an older age with the effect of being relatively older than ones classmates[6] or try to separate the effect of entering school at an older age with the effect of taking the exam at an older age.[7] In this study, we only examine the total effect, which we refer to as the relative age effect.

2. Background

The Individuals with Disabilities Education Act (IDEA) has protected the right of students with disabilities in the United States to a free, appropriate public education since 1975. Prior to IDEA, public school officials in many states could refuse to enroll and serve students that they deemed uneducable. IDEA instituted a general framework for making eligibility decisions, developing Individualized Education Programs (IEPs) for students, and protecting the rights of families under the law.

While federal rules govern the process that identifies student disabilities, parents and local school officials jointly make the decisions that ultimately determine placement. The identification process begins with a referral for an evaluation by either a parent or a school employee. The school's psychologist, physician, or educational diagnostician then selects and administers an appropriate assessment. If a diagnosis is made, an IEP is developed. Parents may then approve the IEP, after which services commence, or appeal the outcome of the evaluation process.

Specific learning disabilities (SLDs) constitute half of diagnoses nationwide.[8] Special services for learning disabilities aim to treat specific deficiencies in the learning process (Lyon, 1996). A SLD is identified under IDEA when it is determined that a child does not achieve commensurate with his or her age and intellectual ability level.[9] Gaps in achievement cannot be the primary result of a different factor, such as another disability or limited English proficiency. There is no universally accepted test or standard to identify SLDs. Traditionally, districts have

[6] See Fredriksson and Öckert (2006), Elder and Lubotsky (2009), Cascio and Schanzenbach (2007), and Kawaguchi (2006).
[7] See Black, Devereux, and Salvanes (2008), Crawford, Dearden, and Meghir (2007) and Smith (2008).
[8] Speech impairments account for approximately twenty percent of special education enrollment. In contrast, only ten percent is for physical disabilities like orthopedic impairment, blindness, and deafness (Hanushek, Kain, and Rivkin, 2002).
[9] The IDEA definition of a specific learning disability is a disorder in "one or more of the basic psychological processes involved in understanding or in using language (spoken or written)." Categories of SLDs include oral expression, listening comprehension, written expression, basic reading skills, reading fluency skills, reading comprehension, mathematics calculation, and mathematics problem solving.

tried to measure discrepancies between IQ and achievement. Lyon (1996) suggests that the lack of a precise definition regarding what constitutes a discrepancy has led to variation in diagnoses across schools and districts.[10] Due to our inclusion of school effects, any variation in how definitions are applied across schools and districts cannot explain the patterns of evaluations and diagnoses that we observe within school cohorts.

3. Empirical Framework

Most specifications in this study estimate the effect of a child's age relative to her classmates on the probability of disability evaluation or diagnosis. The ideal regression equation is

$$D_{is} = \alpha_1 + \alpha_2 A_{is} + X_{is}\lambda + S_s\phi + \varepsilon_{is}, \qquad (1)$$

where *i* denotes an individual, *s* denotes a school, and ε is the usual error term. The outcome D is an indicator for a disability referral or diagnosis (see Section 4 for more details on outcome measures). The variable of interest A is an individual's age in months on September 1 of a given school year. The vector X controls for gender, race/ethnicity, mobility[11] and quartiles of socioeconomic status. Chaikind and Corman (1991) and Corman and Chaikind (1998) find a link between birth weight and childhood disabilities. Therefore, we include a control for birth weight in ounces in our analysis up through 5th grade to address this potential confounding factor.[12] Finally, S is a vector of school fixed effects. Standard errors are clustered at the school level.

The causal interpretation of α_2 rests on the assumption that $E(A_{is}\varepsilon_{is} \mid X_{is}, S_s) = 0$. There is little reason to believe that this assumption holds due to the prevalence of nonrandom delayed entry into primary school. In addition, the direction of potential bias is unclear. For example, wealthier parents may delay their children's entrance into school and have them screened for disabilities more often, introducing an upward bias. However, Bedard and Dhuey (2006) find that children from higher socioeconomic backgrounds are slightly more likely to be born in the

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[10] The 2004 reauthorization of IDEA, which became effective after the survey years in this study, updated the identification criteria for SLDs. The major changes permit states to prohibit the use of IQ-discrepancy models and require that states allow districts to use the results of scientific, research-based interventions.
[11] The mobility variable depends on the sample. It is either the number of times a child moves schools (*NELS*, *ELS*) or whether a move occurs since the last sample wave (*ECLS*) for reasons other than grade promotion.
[12] This variable is unavailable for 8th and 10th grades. However, our estimates from *ECLS* are not significantly different if this control is omitted.

summer months, making them among the youngest students according to most state cutoffs. This may lead to downward bias in α_2 if these students also have a lower prevalence of disabilities on account of unobserved differences in health care quality or standard of living typically associated with higher socioeconomic status.

We overcome this difficulty through an instrumental variables strategy used by Bedard and Dhuey (2006). The strategy uses a child's birth month relative to her school's entry cutoff date, known as a child's assigned relative age, as an exogenous determinant of her actual age. The first stage equation is

$$A_{is} = \beta_1 + \beta_2 R_{is} + X_{is}\beta + S_s\theta + v_{is} \,, \tag{2}$$

where R is a child's assigned relative age. The assigned relative age measure used in this study is the linear distance in months between a child's date of birth and the state-specified cutoff date for kindergarten entrance.[13] For example, a child is assigned a relative age of $R = 0$ if she is born in the last eligible month before the cutoff and $R = 11$ if she is born in the first eligible month after the cutoff. More specifically, if the cutoff date is October 1, children born in September are assigned $R = 0$ and children born in October are assigned $R = 11$.[14] The IV estimator is the local average treatment effect among students whose actual entrance age is influenced by their assigned relative age. Generally, this group includes those students who enter and progress through school on time.[15]

Two conditions must hold for the instrumental variables strategy to be consistent. The first requirement is that the $Cov(A_{is}, R_{is} \mid X_{is}, S_s)$ is sufficiently large. Actual age and assigned relative age are highly correlated in the data because most children start school as soon as they are eligible. The first stage F-statistics from IV specifications of equation 2 range from 91-2108. In other words, assigned relative age is a strong determinant of a student's actual age. The second requirement is that the $Cov(R_{is}, \varepsilon_{is} \mid X_{is}, S_s) = 0$. The second non-testable condition requires that children born at different times of the year cannot have higher or lower levels of inherent disabilities. Several studies in psychology and medicine do find systematic differences

[13] These cutoff dates were collected from state statutes and corresponding historical session laws. See Bedard and Dhuey (2009) for a complete list of cutoff dates.
[14] Results are similar if a nonlinear measure such as relative quarter of birth is used instead. Results are available from authors upon request.
[15] The local average treatment effect also includes children that either delayed entry into school and then skipped a grade or accelerated entry into school and were held back a year. In practice, this is a very small fraction of children.

in identified disability and mental illness rates in people born at different times of the year.[16] However, the variation in cutoff dates across states reduces the probability that our assigned relative age measure proxies for season of birth effects. In addition, we can control for season of birth effects directly through alternative specifications that include quarter of birth along with the measure of relative age.[17] The results are similar with the extra controls, making us more confident that we are not confounding season of birth and relative age (see Bound and Jaeger, 2000).

Specifications in Sections 5.1 utilize the reduced form from the two stage least squares model as well. Reduced form estimates apply to all students whether or not they are making normal progress through school. In particular, the reduced form equation is

$$D_{is} = \delta_1 + \delta_2 R_{is} + X_{is}\delta + S_s\varphi + w_{is}, \qquad (3)$$

where the coefficient of interest is δ_2. The reduced form is informative because relatively younger children have a higher rate of retention (Bedard and Dhuey, 2006). If relative age is both a predictor of retention and correlated with special education outcomes, then the reduced form relative age effects should be smaller than the IV estimates because the assigned relative age cannot predict the age of children observed ahead or behind their expected grade. We will explore this issue in more detail in Section 5.1.

Lastly, Section 5.5 also uses a reduced form approach but this time in the context of student fixed effects models of test score gains and grade repetition that are described by Equation 4.

$$O_{it} = \kappa_1 + \kappa_2 D_{it} + \kappa_2 D_{it} * R_i + X_{it}\kappa + T_t + I_i + \zeta_{it}, \qquad (4)$$

The outcome variable, O, is the math or reading standardized test scores for student *i* in year *t* or an indicator for whether student *i* had repeated a grade by time *t*. In these models *D* enters on the right-hand side as in Hanushek, Kain, and Rivkin (2002) to describe the relationship between disability classification (e.g. D_{it} switches from 0 to 1) and changes in achievement. We also include an interaction with the relative age variable to see how any academic benefits associated with disability classification vary with the expected school entry age. The main effect of relative age, along with several other student demographic controls, is time invariant. Standard errors are clustered at the individual level.

[16] See Barak et al (1995), Livingston, Adam, and Bracha (1994), and Mortenson et al (1999).
[17] These results are available from the authors upon request.

4. Data and Descriptive Statistics

This study uses data from three nationally representative samples that encompass six different grade levels: kindergarten, 1st, 3rd, 5th, 8th and 10th grade. The use of multiple samples with the same or similar questions regarding childhood disabilities facilitates a comparison over different samples and across grade levels. The first sample is from the *Early Childhood Longitudinal Study, Kindergarten Class of 1998-99* (*ECLS*), which surveyed kindergarteners in the fall 1998. From that base sample, we drop students who live in states that do not have a uniform kindergarten entry cutoff date, have missing birth date information, or missing values for the dependent variables. We then follow the remaining students who are observed in the 1st grade (spring 2000) survey, the 3rd grade (spring 2002) survey, and the 5th grade (spring 2004) survey. Therefore, we use a balanced panel of children from kindergarten through 5th grade from the *ECLS* survey.

The next sample comes from the *National Education Longitudinal Study* (*NELS*), a survey of eighth graders in 1988. The final sample comes from the *Education Longitudinal Study* (*ELS*), a survey of tenth graders in 2002. We use the base year samples from each of these two latter surveys and assign cutoff dates to students based on their year of birth and their base year state of residence.[18] From those samples, we drop students who live in states that do not have a uniform kindergarten entry cutoff date, have missing birth date information, or missing values for the dependent variables.

Table 1 provides the summary statistics and sample sizes for the special education variables used in the analysis.[19] The first outcome measure, "ever a handicap program recipient," is based on parent-reported data about whether their child ever received therapy services or took part in a program for children with disabilities. The mean value of this variable varies from 6.9 percent to 14.4 percent in *ECLS* and 19.6 percent for 8th graders in *NELS.* From 1st to 3rd grade, 4.2 percentage points or 38 percent more children started receiving services for a special education problem. The mean value for the sample outcome measure is 7.6 percent for *ELS* 10th graders because the question applies only to high school years. The next outcome measure asks

[18] It is likely that some students are assigned the incorrect state cutoff date because we do not have information regarding state of residence at school entry. However, this will likely cause random noise in our estimation because it is very unlikely migration decisions are based on school entry cut off dates.

[19] Disability evaluations and diagnoses can happen prior to kindergarten. We attribute these as occurring during kindergarten in our data. We cannot track disabilities that are identified prior to kindergarten if students are not reported as being disabled in kindergarten.

parents whether a qualified professional has ever evaluated their child for a disability.[20] Among *ECLS* kindergarteners, 13.6 percent had been evaluated for a disability.[21] This percentage increased to 29.2 percent by 5th grade. The next outcome measure asks whether a qualified profession has ever diagnosed their child with a disability. The diagnosis rate among *ECLS* respondents is 9.2 percent in kindergarten and 21.5 percent by 5th grade.[22]

In addition to general information on special education participation, we have information regarding specific diagnoses in *ECLS* and *NELS*. In *ECLS*, we know whether a child has been evaluated for or diagnosed with any one of six different categories: learning problems, speech problems, visual handicaps, hearing problems, emotional problems, and mental retardation.[23] In kindergarten, 4.9 percent of children have been evaluated for a learning problem. This percentage increases to 18.6 percent by 5th grade. Much larger percentages exist for ever being evaluated for a visual or hearing problem because such evaluations are standard for all students in many school districts. The diagnosis rate for learning problems in the *ECLS* is 2.6 percent in kindergarten and 13.2 percent by 5th grade. Similarly, 10.8 percent have been diagnosed with speech problems by 5th grade. As well, about 30.2 percent of children in *ECLS* have a vision problem by 5th grade. This includes children who use eyeglasses or contact lenses. In contrast, 1.6 percent of children have hearing problems and 4.8 percent have been diagnosed with emotional problems. Emotional problems include panic disorder, separation anxiety disorder, agoraphobia, social phobia, obsessive-compulsive disorder, generalized anxiety disorder, and bipolar disorder. Finally, only 0.3 percent have mental retardation by the 5th grade.

NELS also contains information regarding specific diagnoses. The largest categories are specific learning disabilities and speech problems, at 7.3 and 7.0 percent, respectively. In addition, 1.8 percent of students in 8th grade have ever received services for hearing impairment while only 0.3 percent of students have ever received services due to deafness. The last three

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[20] Professionals include doctors, pediatricians, nurses or nurse practitioners, optometrists, ophthalmologists, psychologists, psychiatrists, psychiatric social workers, and speech pathologists. The definition does not include teachers.

[21] This excludes evaluation for hearing and vision because it is standard in many school districts for all children to be evaluated for both hearing and vision problems and is not based on suspicion that the child has a disability.

[22] We do not include children diagnosed with vision problems only as having a disability in this sample because the *ECLS* definition of a vision problem includes any need for prescription eyewear.

[23] The disability categories in *ECLS* do not always correspond with the federal disability categories. In particular, we define learning problems as the combination of disabilities identified by *ECLS* as learning, activity, and behavior problems.

rows in Table 1 include the mean value for the assigned relative age measure, the mean value of age in months and number of observations for each wave of each survey.

In Appendix Table 1, we explore the possibility of nonrandom attrition in the *ECLS* since we use a balanced panel design. The top portion of the table examines the dependent variable, ever evaluated for a disability, and the bottom portion exams the dependent variable, ever diagnosed with a disability. Part A examines the different attrition rates between children being evaluated and children not being evaluated for the relatively youngest quarter of children. Columns 1-3 calculate the proportion of children who were evaluated that are still in the sample in the next wave and columns 4-6 calculate the proportion of children who were not evaluated that are still in the sample in the next wave. For example, the 84 percent listed in column 1, row 1 indicates the percentage of the relatively youngest children who were evaluated by fall of their kindergarten year that remain in the sample in spring of 1st grade. This percentage should be compared to the 85 percent listed in column 4, row 1, which is the percentage of children not evaluated by fall of kindergarten remaining by spring of the 1st grade year. Columns 7-9 list the test statistic for the test of difference in proportions between the children who were not evaluated and the children who were. We find no statistically significant differences in the attrition rate between the two samples for the relatively youngest quarter.

Part B is similar to part A except that the sample includes only the relatively oldest quarter. We find some evidence of nonrandom attrition in spring ’04, the year most *ECLS* students are in 5th grade. More children who are evaluated are lost from the sample than children who are not evaluated. We find no evidence, however, that attrition is based on relative age. Part C lists the test statistics that compare the difference between the proportion of children who are relatively young and still in the sample to the proportion of children who are relatively old and still in the sample. The statistics are each insignificant. The results in the bottom panel, examining attrition for children who were diagnosed with a disability, are similar to the top panel, which examines attrition for children who were evaluated.

These results suggest the potential for bias due to the nonrandom attrition in spring ‘04. Due to these concerns, we estimated all our specifications with a balanced panel including only kindergarten through 3rd grade to allow for the largest span of grades that do not suffer from nonrandom attrition.[24] The estimates using this modified panel are similar in magnitude and significance to the analysis presented in this research.

In addition, one may be concerned with sampling issues because the baseline data for *NELS* and *ELS* may not be representative. About five percent of *NELS* 8th graders were excluded from participation by their schools (Ingles and Quinn, 1996). Of the excluded, 66 percent were classified as ineligible due to mental disabilities, 6 percent were excluded due to a physical disability and 8 percent were excluded and classified as "disability unknown" (Ingles and Quinn, 1996).[25] Due to these restrictions, the base year sample may not be representative of all 8th graders or representative of all 8th graders with a disability. However, the children who have disabilities that are affected by their relative age, which should be the less severe disabilities, are most likely not the same students who are being excluded from the base year sample. Readers should take caution in interpreting our *NELS* results as being representative of all 8th graders. In contrast to *NELS*, all students attending schools surveyed by *ELS* were deemed eligible for participation despite their disability status and base year contextual data was collected for all students (Ingles et. al., 2006). Nevertheless, students with severe disabilities and who are serviced at special schools may still be underrepresented in the surveys. Therefore, this study may not speak to the effects of relative age on children with those types of disabilities.

5. Results

5.1 The Effect of Relative Age on Special Education Participation

Table 2 reports α_2 from the instrumental variables specification that accounts for the endogeneity of a student's actual age. The specification found in Panel A of Table 2 includes school level fixed effects. Therefore, the identification comes from within school differences in disability rates by relative age. Row 1 contains estimates for the dependent variable "ever a handicap program recipient." The first point estimate indicates that a one-month age advantage at school entry decreases the predicted probability of being a handicap program recipient by 1st grade by 0.28 percentage points. The point estimate is -0.22 for 3rd grade and -0.39 for 5th grade. This implies that an additional month of age decreases the probability of receiving special

[24] Using a non-balanced panel we obtain estimates that are not substantially different than using the balanced panel. These results are available from the authors upon request.

[25] In contrast, less than 1 percent of the *ECLS* base year was excluded due to disability status.

education services by 2-4 percent. Comparable point estimates in the *NELS* 8th grade sample and the *ELS* 10th grade sample both imply 5 percent decreases.

The next outcome measures address whether *ECLS* respondents have ever been evaluated or diagnosed with a disability. The results show that age is an important predictor of both variables in most survey years. For instance, the point estimate in the 5th grade evaluation equation implies a 3 percent decrease for each month of age. To put this into perspective, if a child is the relatively oldest in her 5th grade class, she is approximately 33 percent less likely to be evaluated for a disability than is her relatively youngest classmate. Similarly, the 5th grade point estimate of -0.63 percentage points in the ever diagnosed equation translates to a 2.9 percent decrease for every month of relative age.

Similar to the relative age literature on standardized testing, the mechanism causing relative age effects in special education lacks a definitive answer. It may be the inherent difficulty in distinguishing between maturity and ability when children are very young. Relatively older students are also biologically older and yet they are held to the same academic standards as their younger classmates. We would expect differential classification rates by age if educators recommend special education to students who achieve at lower levels.

Due to the fact that our panel includes four different grade levels, we are able to examine how the relationship between special education classification and relative age evolves over time. Interestingly, despite the increase in coefficient size, the percent effect stays relatively constant from kindergarten to 5th grade. Overall, these results point to a substantial effect of being relatively older in terms of receiving special education services, being evaluated for a disability, and for being diagnosed with a disability.

The school fixed effects capture much of the variation in identification rates. In Panel B, we replace them with state fixed effects and then perform the same analysis. This latter specification allows for between school variation along with within school variation. The results are very similar to Panel A, suggesting that the analysis is relatively insensitive to the level of fixed effects. Our preferred specification includes school level fixed effects because the decision to evaluate and diagnose students is performed at the school level.

In the bottom portion of the table, we report δ_2 from the reduced form specification from equation 3. The reduced form specification does not eliminate the contribution of students who are not in their appropriate grade for their age. As expected, we find that these estimates are smaller than the IV estimates but have similar patterns of significance.

5.2 The Effect of Relative Age on Special Education Services Received by Diagnosis

The estimates presented in Table 2 indicate that relative age is a strong predictor of special needs placements. This section analyzes these results in more detail by focusing on specific disabilities. Table 3 disaggregates disabilities by type and corroborates the findings in Elder and Lubotsky (2009) of a strong relationship between expected starting age and learning problems.[26] Given the potential for subjectivity during the placement process as well as the frequency of these diagnoses, this is not altogether surprising. The top panel shows the instrumental variable coefficient from separate regressions run for each disability type. In the top panel, the dependent variable is whether or not the student has ever been evaluated for a disability. The only statistically significant point estimates are for learning problems. The point estimates range from -0.34 to -1.05. Despite the difference in magnitude between the point estimates, the percent reduction for being one month older ranges from 5.6-7.1 percent.

The bottom panel explores the relationship between relative age and the likelihood of disability diagnosis by type. Again, the most robust findings are for learning problems. We find a statistically significant estimate for 1st, 3rd, 5th, and 8th grade for learning problems. Each month of relative age decreases the probability of a learning problem diagnosis in the *ECLS* by 0.29 to 0.66 percentage points, or 4.8-5.2 percent. The point estimate in the *NELS* data is similar in magnitude as the *ECLS* data but the percent effect is larger, approximately 8.4 percent. We attribute the difference in effect sizes to a narrower definition of learning problems in *NELS*.

Other than learning problem diagnoses, there is one additional point estimate that is statistically significant. Each month of relative age decreases the probability of a speech problem in 8th grade by 0.57 percentage points. We find no effect of relative age on speech problems in *ECLS*. This may be caused by the difference in timing of the surveys or by differences in questions in each survey. For instance, the *ECLS* survey, which was conducted ten years later than the *NELS* survey, asked the parent if the child had ever been evaluated/diagnosed by a professional in response to his/her ability to communicate. By contrast, *NELS* parents were

[26] Much of the relationship between expected school entry age and the probability of a learning problem reflects patterns of ADD/ADHD diagnoses. Elder and Lubotsky (2009) find this as well.

asked directly, "in their opinion, did their eighth grader have a speech problem?" The estimates for the other categories of disability types are not statistically significant.

5.3 Differences by Sex, Race/Ethnicity, and Socioeconomic Status

The previous results show strong relative age effects with regard to the evaluation and diagnosis of learning problems. An important unanswered question is whether relative age affects all children equally. For instance, are girls and boys affected similarly? Are white children affected more than black children? Or, are socioeconomically-advantaged students affected less than socioeconomically-disadvantaged students? The next three tables explore possible interactions between relative age and different demographic characteristics on the probability of being evaluated and diagnosed with a learning problem.

We estimate the following equation using the two stage least squares procedure described in Section 3,

$$L_{is} = \alpha_1 + \alpha_2 A_{is} + A_{is} * C_{is}\gamma + C_{is}\delta + X_{is}\alpha + S_s\phi + \varepsilon_{is}, \quad (5)$$

where C is a vector of demographic indicators that depend on the specification. Specifically, C is alternatively an indicator for female, indicators for black and Hispanic, or indicators for socioeconomic quartiles. $A_{is} * C_{is}$ is the interaction term between age and one set of the demographic indicators. Our identification strategy uses R_{is} and $A_{is} \times R_{is}$ as instruments for A_{is} and $A_{is} \times C_{is}$. The coefficient of interest is γ, which is the average differential effect of being relatively older on students within a particular demographic characteristic.

Table 4 examines gender differences, reporting the IV coefficient for both age (α_2) and the interaction of age and female (γ) from Equation 5. In this framework, α_2 is the effect of age for boys and $\alpha_2 + \gamma$ is the effect of age for girls. Estimates are reported separately for learning problem evaluations (upper panel) and diagnoses (lower panel).

The results for both dependent variables show that relative age effects are stronger for boys than for girls through 1st grade. In fact, we cannot reject a zero effect for girls. For example, an additional month of age decreases the male evaluation rate in kindergarten by 0.62 percentage points or 9.5 percent. However, the evaluation rate for females decreases only 0.1 percentage points. The F test of the null hypothesis that relative age effects are non-existent for girls fails to reject at the five percent level.

In the 3rd grade sample, we find evidence that relative age affects the probability of a learning problem evaluation for both boys and girls in this year. In other words, while relative age eventually predicts disability outcomes for all students, in the early grades it matters almost exclusively for boys.

The bottom panel uses "ever been diagnosed with a learning problem" as the dependent variable. A similar pattern as the evaluated dependent variable emerges. We find a statistically larger effect for males up through 5th grade at the 10 percent level or better. The overall effect for females is statistically insignificant and numerically close to zero through 3rd grade but it becomes significant in 5th and 8th grade, meaning that relative age eventually affects both genders. As a percentage of their baseline rates in the *ECLS*, the relative age effect for boys shrinks toward zero (i.e. -8.5 percent in kindergarten to -5.3 percent in 5th grade) while for girls it widens away from zero (i.e. 2.9 percent in kindergarten to -4.6 percent in 5th grade).

One possible explanation for these findings is that a one-year age gap in earlier grades leads to a more pronounced maturity difference for boys than for girls, making learning problems more readily identifiable. For example, classroom disruption, which is often associated with lack of maturity, is a primary cause of referrals for boys (Anderson, 1997), potentially indicating how the age gap could affect boys more than girls in early grades. In addition, Anderson (1997) reviews the literature and finds a large gender bias in the special education referral process. She finds that teacher referrals are often affected by the gender of the student referred and that these referrals are influenced by classroom behavior. Vogel (1990) suggests that in order for a girl to be diagnosed with a learning disability, she must be older and more severely impaired than her male counterparts.

We next explore the possibility that relative age affects children differently by race/ethnicity. In this case, the sample is limited to children who are white, black, or Hispanic.[27] Table 5 shows the results from regressions that interact age with indicators for black and Hispanic. The main effect of age is statistically significant at the five percent level in almost all grades for both dependent variables, indicating that relative age decreases the predicted probabilities for white students. The magnitudes imply a 6.1-9.1 percent effect for evaluations and 6.2-10.4 percent effect for diagnoses. Table 5 shows evidence that the relative age effect tends to be close to zero for black students. Consistent with this evidence is that we never reject

[27] Students who are classified as "other" race are excluded from this analysis.

the hypothesis of a non-existent age effect for these students. For Hispanic students, we find a statistically significant effect for ever being evaluated in 1st through 5th grade and in 5th grade for ever being diagnosed.

Table 6 displays the results from specifications that interact age with indicators for socioeconomic quartiles. The quartiles are constructed by using the socioeconomic status measure included in the surveys. The point estimates on the interaction terms are relative to quartile 1, the most socioeconomically disadvantaged students. Overall, we find little evidence of differences by socioeconomic status quartiles.

5.4 Is it the Referral Process or the Assessment Process?

The process of diagnosing disabilities is twofold. First, a parent or educator refers a child for evaluation if a disability is suspected. Then, the results of the evaluation indicate whether a diagnosis should be made. Our ability to track evaluations and diagnoses separately in the *ECLS* enables us to examine the role of relative age in both steps of the process, shedding light on why relatively young children have a higher probability of diagnosis. Is it higher simply because they are more likely to be evaluated? Or are relatively young children also systematically more likely to be diagnosed given that they are evaluated?

The first row of Table 7 examines the probability of diagnosis among the sample of children that were evaluated for a disability. The results are statistically insignificant, indicating that disabilities are diagnosed in the same percentage of relatively young and old evaluated children. One possible explanation is that the incidence of childhood disabilities is truly higher for relatively young students. While we cannot discount this possibility outright, it seems unlikely given that the variation in relative age across individuals is due to state-specified cutoff dates and the distribution of birthdates throughout the year.

An alternative scenario is that relatively younger and older students are equally likely to be disabled yet vary in their rates of diagnosis. This scenario is consistent with research cited by Cullen (2003) suggesting that examiners may at times search for tests that support the initial reason for a child's referral. In other words, disability assessments do not appear to screen for age. The best way to see this is to think about age as an imperfect indicator of disability status. In this case, one might expect a higher percentage of relatively young students to be evaluated when they are not actually disabled. If disability assessments screened for age, then we would expect a statistically positive point estimate because the assessments would lead to diagnoses in a higher percentage of relatively older evaluated students.

The next panel of Table 7 includes an interaction between age and an indicator for female and shows important differences between boys and girls in the diagnosis process. The main effect is numerically negative and sometimes marginally significant, providing some evidence that relatively younger boys may be diagnosed at higher rates among those evaluated. For females, the interaction effect is positive, but the F statistic for the joint test of significance is not statistically significant in any grade.

The next two panels run the same analysis but include interactions for race/ethnicity and socioeconomic status. Overall, there seems to be no differential effects of being diagnosed given an evaluation for either race/ethnicity or socioeconomic status.

5.5 Relative Age, Disability Status, and Academic Outcomes

Many studies find that children who are younger at school entry are more likely to score lower on standardized achievement tests and are more likely to fail a grade.[28] Special education programs may help mitigate the effect of this age-based gap in achievement. We explore this issue in the *ECLS* by adapting a student fixed effects model used by Hanushek, Kain, and Rivkin (2002) to study academic gains associated with special education programs. Hanushek, Kain, and Rivkin use year-to-year variation in student outcomes among those who transition into and out of special education programs to conclude that a year of participation improves math scores by about 0.1 standard deviations. While the design is not causal, it allows for better inferences of program effectiveness than cross sectional analyses by isolating all time invariant factors.[29]

We study three academic outcomes using a similar student fixed effects design on the *ECLS* balanced panel. The outcomes are math and reading scores (e.g. normalized IRT scores with a mean 50 and a standard deviation 10) and an indicator for any grade repetition by each survey period. We use the "ever diagnosed" with a disability indicator from Table 2 as an explanatory variable. The indicator takes the value one when a child is first identified with a disability and in subsequent waves. We also include an interaction between the "ever diagnosed"

[28] See Bedard and Dhuey (2006); Datar (2006); Elder and Lubotsky (2009); Puhani and Weber (2007); Smith (2009); Crawford, Dearden, and Meghir (2007).
[29] Hanushek, Kain, and Rivkin (2002) describe several potential confounds that inhibit causal inference of program effectiveness. These include the potential for simultaneous skill reduction and disability classification, and the potential for classification to follow abnormally low prior-year achievement.

variable and relative age to see whether any academic benefits associated with special education programs vary for students of different starting ages. The main effect of relative age is time invariant, and therefore drops out of the equation.

The first column of Table 8 suggests a marginally significant relationship between disability classification and a 0.85 percentage point gain in math scores for the relatively youngest students. The point estimate implies an effect size of about 0.085, which is comparable to the Hanushek, Kain, and Rivkin (2002) study. The interaction effect with relative age is negative, however, indicating that math gains associated with disability classification are larger for relatively younger students. In fact, disability classification may contribute negatively to math scores among relatively older students. By contrast, we find only statistically insignificant findings for reading scores (column 2). Because the analysis is not based on a causal design, we cannot unilaterally discount to possible role of other omitted time-varying factors. At the minimum, however, we believe the results for math invite further study on the issue of whether special education programs hold the potential to help relatively young students reduce achievement gaps with students who enter at older ages.

The last column of Table 8 uses an indicator for ever repeating a grade as the dependent variable. These variables are interesting to examine together because both are potential interventions for students who fail to meet expected grade-level progress. For some students, special education placement may be an appropriate alternative to grade retention (Burkam et. al. 2007). A special education student's IEP may also prescribe participation in grade-level curriculum, reducing the potential use of retention (Beebe-Frankenberger et. al., 2004). In other cases, schools may turn to both retention and special education for students who they perceive not to have responded to just one of the two. We find in Table 8 a strong positive association between repeating a grade and disability identification.[30] The probability of grade repetition does not appear to vary, however, for students expected to begin kindergarten at different ages.

6. Conclusion

Because most diagnoses are made during the first years of formal schooling, the problem of distinguishing between a specific gap in achievement due to relative immaturity rather than

[30] The magnitude of the association, 0.09, is larger than the panel-mean value of the dependent variable. The probability of grade repetition grows considerably from 3 percent to 11 percent over the four survey years.

relative inability is especially acute. The relative age literature presents evidence that a few months of additional within-cohort age can substantially influence outcomes in education. This study is no exception. Our evidence from three national surveys is consistent with past research (Elder and Lubotsky, 2009) and indicates that relative age is a powerful predictor of special needs placements. At the same time, we add to the existing research by exploring in greater detail differences in effect sizes by disability type and for individuals with different demographic characteristics. Finally, we are the first study to explore how relative age affects students at both the referral and the assessment stage of the diagnosing process.

Our findings suggest that educators and parents use special education classification in some cases as a supplemental service program that targets additional resources to some younger students. If special education is used in this way, then its ability to effectively boost student achievement takes on an even greater role in policy debates. Much of what we know about special education effectiveness comes from a small number of studies like Hanushek, Kain, and Rivkin (2002) and Cohen (2007). These studies find that special education increases math test scores, particularly among students with learning and speech problems. We extend these findings by suggesting that the benefits of disability classification on math achievement may be largest for those students who start school at young ages. As the research continues to progress in this area, policymakers should have an even fuller understanding of the extent to which classifying a disproportionate fraction of relatively young students as disabled has in terms of equalizing educational outcomes for all students.

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Table 1 - Summary Statistics for the ECLS, NELS, and ELS samples

	ECLS				NELS	ELS
	Fall '98 Kindergarten	Spring '00 1st Grade	Spring '02 3rd Grade	Spring '04 5th grade	Spring '88 8th grade	Spring '02 10th grade
Special education participation						
Ever a handicap program recipient (only H.S. for ELS)		6.9	11.1	14.4	19.6	7.6
Ever evaluated for a disability (excl. hearing/vision in ECLS)	13.6	17.5	23.4	29.2		
Ever diagnosed as disabled (excl. hearing/vision in ECLS)	9.2	12.3	16.8	21.5		
Ever evaluated for a disability						
Learning/Activity/Behavior problem	4.9	8.8	13.3	18.6		
Speech problem	11.1	12.5	13.3	14.2		
Visual handicap	73.1	74.7	77.5	80.8		
Hearing problem	72.2	72.3	72.4	72.5		
Emotional problem			4.2	7.6		
Student ever diagnosed with (Rec'd services for NELS)						
Learning/Activity/Behavior problem (Learning disability)	2.6	5.6	9.0	13.2	7.3	
Speech problem	7.9	9.1	10.0	10.8	7.0	
Visual handicap	3.7	8.6	18.7	30.2	1.2	
Hearing problem	1.0	1.2	1.4	1.6	1.8	
Mental retardation	0.2	0.2	0.3	0.3	0.02	
Emotional problem			2.5	4.8	3.3	
Deafness					0.3	
Orthopedic problem					1.1	
Other physical disability					0.9	
Other health problem					2.5	
Other						
Assigned Relative Age (in months)	5.4	5.4	5.4	5.4	5.3	5.4
Entry Age (in months)	65.7	83.7	107.7	131.7	163.1	187.0
Observations	8,120	8,120	8,120	8,120	16,870	12,140

Note: Estimates are population weighted. Sample sizes are rounded to the nearest ten to comply with restricted data access requirements.

Table 2 - Program Participation IV Regression Results

	ECLS				NELS	ELS
	Fall '98 Kindergarten	Spring '00 1st Grade	Spring '02 3rd Grade	Spring '04 5th grade	Spring '88 8th grade	Spring '02 10th grade
Instrumental Variables						
Panel A - School Fixed Effects		6.9	11.1	14.4	19.6	7.6
Ever a handicap program recipient (only H.S. for ELS)		**-0.28**	-0.22	**-0.39**	**-0.99**	**-0.38**
		(0.13)	(0.15)	(0.16)	(0.24)	(0.19)
Ever evaluated for a disability*	**-0.35**	**-0.48**	**-0.62**	**-0.87**		
	(0.17)	(0.18)	(0.19)	(0.22)		
Ever diagnosed as disabled*	*-0.27*	**-0.32**	**-0.48**	**-0.63**		
	(0.15)	(0.16)	(0.18)	(0.19)		
Panel B - State Fixed Effects						
Ever a handicap program recipient (only H.S. for ELS)		**-0.29**	**-0.25**	**-0.38**	**-0.99**	**-0.42**
		(0.09)	(0.10)	(0.11)	(0.25)	(0.15)
Ever evaluated for a disability*	*-0.26*	**-0.40**	**-0.49**	**-0.74**		
	(0.14)	(0.14)	(0.14)	(0.16)		
Ever diagnosed as disabled*	**-0.25**	**-0.33**	**-0.47**	**-0.62**		
	(0.12)	(0.13)	(0.13)	(0.12)		
Reduced Form						
Panel C - School Fixed Effects						
Ever a handicap program recipient (only H.S. for ELS)		**-0.21**	-0.16	**-0.29**	**-0.42**	*-0.17*
		(0.10)	(0.11)	(0.13)	(0.10)	(0.09)
Ever evaluated for a disability*	**-0.26**	**-0.36**	**-0.46**	**-0.65**		
	(0.13)	(0.14)	(0.15)	(0.17)		
Ever diagnosed as disabled*	*-0.20*	**-0.24**	**-0.35**	**-0.47**		
	(0.11)	(0.12)	(0.14)	(0.15)		
Panel D - State Fixed Effects						
Ever a handicap program recipient (only H.S. for ELS)		**-0.22**	**-0.18**	**-0.28**	**-0.43**	**-0.19**
		(0.07)	(0.07)	(0.08)	(0.12)	(0.07)
Ever evaluated for a disability*	*-0.19*	**-0.29**	**-0.36**	**-0.55**		
	(0.10)	(0.10)	(0.11)	(0.12)		
Ever diagnosed as disabled*	**-0.18**	**-0.25**	**-0.34**	**-0.46**		
	(0.09)	(0.10)	(0.10)	(0.10)		

Note: Standard errors are clustered by base year school ID. Estimates are population weighted. First stage F-statistics range from 91-2108. Additional controls include indicators for black, Hispanic, other race, quartiles of socioeconomic status, mobility, and kindergarten school or state ID. ECLS specifications include an additional control for birthweight.
*excluding hearing and vision

Table 3 - IV Regression Results by Disability Category

	ECLS				NELS
	Fall '98 Kindergarten	Spring '00 1st Grade	Spring '02 3rd Grade	Spring '04 5th grade	Spring '88 8th grade
Ever evaluated for a disability					
Learning problem	**-0.34**	**-0.54**	**-0.77**	**-1.05**	
	(0.11)	(0.14)	(0.15)	(0.18)	
Speech problem	-0.23	-0.21	-0.15	-0.20	
	(0.16)	(0.16)	(0.17)	(0.18)	
Visual handicap	0.23	0.14	0.21	0.17	
	(0.21)	(0.21)	(0.21)	(0.20)	
Hearing problem	0.02	0.01	0.003	-0.02	
	(0.23)	(0.23)	(0.23)	(0.23)	
Emotional problem			*-0.16*	-0.15	
			(0.10)	(0.13)	
Student ever diagnosed with (Rec'd services for NELS)					
Learning problem (Learning disability - NELS)	-0.11	**-0.29**	**-0.43**	**-0.66**	**-0.61**
	(0.08)	(0.12)	(0.14)	(0.16)	(0.15)
Speech problem	-0.21	-0.19	-0.14	-0.13	**-0.57**
	(0.13)	(0.14)	(0.14)	(0.15)	(0.16)
Visual handicap	0.11	0.03	0.18	0.12	-0.003
	(0.09)	(0.13)	(0.20)	(0.22)	(0.06)
Hearing problem	-0.02	-0.04	-0.06	-0.07	0.03
	(0.04)	(0.04)	(0.05)	(0.05)	(0.08)
Mental retardation	-0.03	-0.03	-0.04	*-0.05*	0.003
	(0.02)	(0.02)	(0.03)	(0.03)	(0.003)
Emotional problem			-0.07	-0.06	-0.03
			(0.07)	(0.09)	(0.11)
Deafness					*-0.05*
					(0.03)
Orthopedic problem					0.01
					(0.07)
Other physical disability					-0.09
					(0.06)
Other health problem					0.05
					(0.09)

Note: Standard errors are clustered by base year school ID. Estimates are population weighted. First stage F-statistics range from 439-2108. Additional controls include indicators for black, Hispanic, other race, quartiles of socioeconomic status, mobility, and kindergarten school ID. ECLS specifications include an additional control for birthweight.

Table 4 - Learning Problem IV Regression Results, Male/Female Differences

	ECLS				NELS
	Fall '98 Kindergarten	Spring '00 1st Grade	Spring '02 3rd Grade	Spring '04 5th grade	Spring '88 8th grade
Ever evaluated for a learning problem					
Age	**-0.62**	**-0.90**	**-1.17**	**-1.50**	
	(0.18)	(0.24)	(0.27)	(0.32)	
Female * Age	**0.52**	**0.69**	**0.76**	**0.84**	
	(0.21)	(0.27)	(0.33)	(0.38)	
F statistic: Main Effect + Female Interaction = 0	0.66	2.32	**6.13**	**10.77**	
Mean Values					
Female	3.2	5.6	8.8	12.6	
Male	6.5	11.8	17.5	24.5	
Ever diagnosed with a learning problem					
Age	**-0.28**	**-0.54**	**-0.68**	**-0.95**	**-0.83**
	(0.13)	(0.21)	(0.25)	(0.29)	(0.26)
Female * Age	**0.34**	**0.47**	*0.48*	*0.55*	0.39
	(0.16)	(0.23)	(0.28)	(0.33)	(0.31)
F statistic: Main Effect + Female Interaction = 0	0.33	0.29	1.92	**6.01**	**6.21**
Mean Values					
Female	1.8	3.5	5.7	8.7	5.7
Male	3.3	7.6	12.2	17.6	8.8

Note: Standard errors are clustered by kindergarten school ID. Estimates are population weighted. First stage F-statistics range from 179-1188. Additional controls include indicators for black, Hispanic, other race, quartiles of socioeconomic status, mobility, and kindergarten school ID. ECLS specifications include an additional control for birthweight.

Table 5 - Learning Problem IV Regression Results, Race/Ethnicity Differences

	ECLS				NELS
	Fall '98 Kindergarten	Spring '00 1st Grade	Spring '02 3rd Grade	Spring '04 5th grade	Spring '88 8th grade
Ever evaluated for a learning problem					
Age	**-0.47**	**-0.63**	**-1.10**	**-1.24**	
	(0.17)	(0.21)	(0.25)	(0.29)	
Black * Age	*0.55*	0.61	**1.00**	0.56	
	(0.32)	(0.39)	(0.47)	(0.56)	
Hispanic * Age	0.17	-0.11	0.43	0.15	
	(0.24)	(0.32)	(0.37)	(0.42)	
F statistic: Main Effect + Black Interaction = 0	0.10	0.01	0.07	2.32	
F statistic: Main Effect + Hispanic Interaction = 0	2.41	**8.61**	**6.30**	**13.62**	
Mean Values					
Black	6.6	9.7	14.5	19.6	
Hispanic	3.2	6.7	9.1	14.1	
White	5.2	9.4	14.5	20.3	
Ever diagnosed with a learning problem					
Age	*-0.22*	**-0.42**	**-0.80**	**-0.95**	**-0.86**
	(0.12)	(0.18)	(0.23)	(0.26)	(0.21)
Black * Age	0.39	0.54	**1.03**	**0.94**	*0.65*
	(0.25)	(0.35)	(0.40)	(0.44)	(0.38)
Hispanic * Age	0.19	0.10	0.51	0.13	**0.84**
	(0.19)	(0.27)	(0.33)	(0.37)	(0.35)
F statistic: Main Effect + Black Interaction = 0	0.65	0.16	0.51	0.00	0.43
F statistic: Main Effect + Hispanic Interaction = 0	0.04	2.52	1.58	**10.23**	0.01
Mean Values					
Black	3.4	5.3	8.1	11.9	4.5
Hispanic	1.7	4.1	5.4	9.2	4.9
White	2.9	6.2	10.7	15.4	8.3

Note: Standard errors are clustered by kindergarten school ID. Estimates are population weighted. First stage F-statistics range from 26-968. Additional controls include indicators for black, Hispanic, other race, quartiles of socioeconomic status, mobility, and kindergarten school ID. ECLS specifications include an additional control for birthweight.

Table 6 - Learning Problem IV Regression Results, High/Low SES Differences

	ECLS				NELS
	Fall '98 Kindergarten	Spring '00 1st Grade	Spring '02 3rd Grade	Spring '04 5th grade	Spring '88 8th grade
Ever evaluated for a learning problem					
Age	-0.29	-0.41	-0.43	**-0.89**	
	(0.23)	(0.27)	(0.29)	(0.33)	
Quartile 4 (Richest) * Age	0.20	0.28	0.51	0.44	
	(0.33)	(0.41)	(0.48)	(0.54)	
Quartile 3 * Age	-0.07	-0.29	-0.53	-0.11	
	(0.31)	(0.38)	(0.47)	(0.54)	
Quartile 2 * Age	-0.26	-0.38	**-1.08**	*-0.78*	
	(0.30)	(0.37)	(0.44)	(0.48)	
F statistic: Main Effect + Quartile 4 = 0	0.15	0.16	0.04	1.05	
F statistic: Main Effect + Quartile 3 = 0	*3.63*	**7.64**	**8.53**	**6.73**	
F statistic: Main Effect + Quartile 2 = 0	**7.86**	**8.95**	**20.85**	**20.98**	
Mean Values					
Quartile 4	3.8	6.8	10.7	15.5	
Quartile 3	4.0	7.6	12.1	17.7	
Quartile 2	4.9	9.2	14.5	19.5	
Quartile 1	6.6	11.2	15.4	21.6	
Ever diagnosed with a learning problem					
Age	-0.11	-0.22	-0.21	**-0.61**	**-0.73**
	(0.17)	(0.22)	(0.25)	(0.27)	(0.33)
Quartile 4 (Richest) * Age	0.11	0.10	0.41	0.43	-0.10
	(0.24)	(0.34)	(0.42)	(0.47)	(0.45)
Quartile 3 * Age	0.04	-0.22	-0.51	-0.02	-0.08
	(0.22)	(0.29)	(0.37)	(0.44)	(0.44)
Quartile 2 * Age	-0.11	-0.11	-0.59	-0.43	0.55
	(0.21)	(0.30)	(0.36)	(0.42)	(0.42)
F statistic: Main Effect + Quartile 4 = 0	0.00	0.18	0.32	0.20	**6.79**
F statistic: Main Effect + Quartile 3 = 0	0.18	**4.64**	**6.73**	*3.46*	**7.78**
F statistic: Main Effect + Quartile 2 = 0	*2.80*	2.34	**8.57**	**10.14**	0.40
Mean Values					
Quartile 4	2.2	4.6	7.6	11.6	6.0
Quartile 3	2.4	5.0	8.8	13.4	6.7
Quartile 2	2.4	5.8	9.8	13.5	8.2
Quartile 1	3.3	6.7	9.7	14.2	8.1

Note: Standard errors are clustered by kindergarten school ID. Estimates are population weighted. First stage F-statistics range from 42-733. Additional controls include indicators for black, Hispanic, other race, quartiles of socioeconomic status, mobility, and kindergarten school ID. ECLS specifications include an additional control for birthweight.

Table 7 - Probability of Diagnosis given an Evaluation, Main Effect and Demographic Interaction - ECLS

	ECLS			
	Fall '98 Kindergarten	Spring '00 1st Grade	Spring '02 3rd Grade	Spring '04 5th grade
*Ever Diagnosed Given an Evaluation**				
Age	-0.92	-0.36	-0.35	-0.43
	(1.01)	(0.86)	(0.60)	(0.48)
Mean Values of the Dependent Variables	67.6	70.1	71.6	73.6
*Ever Diagnosed Given an Evaluation w/ Female Interaction**				
Age	*-2.19*	-1.42	-1.09	*-1.14*
	(1.20)	(1.11)	(0.75)	(0.64)
Female * Age	**3.71**	*2.96*	*1.98*	**1.86**
	(1.71)	(1.59)	(1.11)	(0.92)
F statistic: Main Effect + Female Interaction = 0	1.18	1.69	1.07	1.12
Mean Values of the Dependent Variables				
Female	68.5	69.5	69.4	70.9
Male	67.1	70.4	72.9	75.2
*Ever Diagnosed Given an Evaluation w/ Race Interactions**				
Age	-0.40	-0.17	-0.59	-0.92
	(1.30)	(1.11)	(0.73)	(0.63)
Black * Age	2.10	1.29	0.54	**2.96**
	(2.76)	(2.22)	(1.63)	(1.44)
Hispanic * Age	-4.20	-2.50	-0.15	-0.88
	(2.78)	(2.56)	(1.77)	(1.33)
F statistic: Main Effect + Black Interaction = 0	0.49	0.35	0.00	*2.59*
F statistic: Main Effect + Hispanic Interaction = 0	**3.91**	1.43	0.22	2.45
Mean Values of the Dependent Variables				
White	72.0	73.7	75.8	76.8
Black	61.9	63.6	65.2	68.7
Hispanic	58.4	62.2	62.3	68.1
*Ever Diagnosed Given an Evaluation w/ SES Interactions**				
Age	-2.30	-1.32	-1.18	-0.57
	(1.74)	(1.44)	(1.22)	(0.89)
Quartile 4 (Richest) * Age	-0.77	-0.52	0.05	-0.57
	(3.18)	(2.72)	(2.14)	(1.51)
Quartile 3 * Age	3.04	0.90	0.76	-0.07
	(2.66)	(2.14)	(1.69)	(1.27)
Quartile 2 * Age	2.88	2.87	1.99	0.81
	(2.77)	(2.21)	(1.66)	(1.27)
F statistic: Main Effect + Quartile 4 = 0	1.23	0.60	0.43	0.80
F statistic: Main Effect + Quartile 3 = 0	0.13	0.06	0.13	0.52
F statistic: Main Effect + Quartile 2 = 0	0.09	0.97	0.64	0.07
Mean Values of the Dependent Variables				
Quartile 4	75.5	75.8	74.9	78.8
Quartile 3	69.9	70.3	73.9	74.4
Quartile 2	64.6	72.4	72.4	74.2
Quartile 1	62.6	63.5	66.2	68.2

Note: Standard errors are clustered by base year school ID. Estimates are population weighted. First stage F-statistics range from 7 to 547. The Q4 interaction terms for kindergarten and first grade are the only terms with F-statistics below 10. Additional controls include indicators for black, Hispanic, other race, quartiles of socioeconomic status, mobility, and kindergarten school ID. ECLS specifications include an additional control for birthweight. *excluding hearing and vision

Table 8 - Reduced Form Student Fixed Effects Regressions for Test Scores and Grade Repetition, ECLS-K

	Math score	Reading score	Ever repeat a grade
Ever diagnosed with a disability	*0.85*	-0.48	**0.09**
	(0.49)	(0.59)	(0.02)
Ever diagnosed with a disability * relative age in months	*-0.15*	0.02	-0.003
	(0.08)	(0.09)	(0.004)
Mean Value	51.36	51.36	0.07

Note: Math and reading scores are normalized IRT scores with mean 50 and standard deviation 10. The ECLS disability control is the "ever diagnosed" variable used in Table 2. Standard errors are clustered by student ID. Estimates are population weighted. Additional controls include an indicator for mobility and indicators for each survey year (i.e. fall kindergarten, spring first grade, spring third grade, and spring fifth grade). Indicators for gender, race-ethnicity, quartiles of socioeconomic status at kindergarten entry, kindergarten school ID, birthweight, and the main effect of relative age in months are time invariant.

Appendix Table 1 - Attrition Analysis for *ECLS*

	Evaluated by:			Not evaluated as of:			Tests for different proportions		
	Fall '98	Spring '00	Spring '02	Fall '98	Spring '00	Spring '02	Fall '98	Spring '00	Spring '02
Percentage in ECLS as of:	K	1st Grade	3rd Grade	K	1st Grade	3rd Grade	K	1st Grade	3rd Grade
A. Relative quarter 1 (relatively youngest children)									
Spring '00	84%			85%			-0.43		
Spring '02	73%	85%		74%	88%		-0.75	-1.44	
Spring '04	55%	66%	75%	57%	68%	77%	-0.84	-0.97	-1.08
B. Relative quarter 4 (relatively oldest children)									
Spring '00	85%			85%			0.17		
Spring '02	74%	86%		75%	88%		-0.32	-0.87	
Spring '04	52%	63%	73%	59%	69%	79%	**-2.74**	**-2.71**	**-2.83**
C. Tests for different proportions between relative quarters 1 and 4									
Spring '00	-0.49			-0.11					
Spring '02	-0.39	-0.40		-0.26	-0.09				
Spring '04	1.06	0.91	0.85	-1.22	-1.15	-1.15			

	Diagnosed by:			Not diagnosed as of:			Tests for different proportions		
	Fall '98	Spring '00	Spring '02	Fall '98	Spring '00	Spring '02	Fall '98	Spring '00	Spring '02
Percentage in ECLS as of:	K	1st Grade	3rd Grade	K	1st Grade	3rd Grade	K	1st Grade	3rd Grade
D. Relative quarter 1 (relatively youngest children)									
Spring '00	83%			85%			-0.94		
Spring '02	74%	85%		74%	88%		-0.27	-1.30	
Spring '04	56%	64%	75%	57%	68%	77%	-0.46	-1.30	-1.15
E. Relative quarter 4 (relatively oldest children)									
Spring '00	86%			85%			0.29		
Spring '02	75%	87%		75%	88%		0.09	-0.56	
Spring '04	52%	61%	72%	58%	69%	78%	**-2.12**	**-2.80**	**-3.01**
F. Tests for different proportions between relative quarters 1 and 4									
Spring '00	-0.87			-0.01					
Spring '02	-0.35	-0.51		-0.31	-0.09				
Spring '04	0.97	0.86	1.01	-1.11	-1.03	-1.12			