When Should Children Start School?

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Abstract: Recent attention given to early childhood education highlights the importance of deciding the age at which children enter kindergarten. Although parents, schools, and state governments have chosen during recent decades to increase the average age at which children enter kindergarten in the United States, little is known about the consequences of these choices. By selecting a quasi-random sample of children from the ECLS-K data set, this paper is able to separately identify coefficients related to both absolute and relative entrance age effects. The identification strategy employed avoids the problems redshirting creates when using date of birth as an instrument for educational attainment. For children in the sample, estimated absolute age coefficients provide no evidence that increasing the average age at which children begin kindergarten increases achievement test scores. These results, while far from conclusive, suggest that increasing kindergarten entrance age need not be a top priority of early education policy.

Please note this is a VERY PRELIMINARY DRAFT.

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1 Introduction

Early childhood education is currently receiving a great deal of attention from researchers and policy-makers. Supported by empirical evidence such as the Perry Preschool Experiment and the Abecedarian Project (Currie (2001), Anderson (2008)), a consensus theory has emerged that educational investments made at early ages yield higher returns than similar ones made later in the life cycle (Cunha et al. (2005)). Policy has responded to the consensus among researchers. State-level funding has increased rapidly in recent years (Kirp (2007)), as has the number of children attending preschool programs (Magnuson et al. (2007)). At the federal level, President Obama has pledged to create a Presidential Early Learning Council to coordinate relevant policies (Dillon (2008)), and the American Recovery and Reinvestment Act included $5 billion for early childhood education programs (The White House (2009)).

The focus on children's pre-kindergarten education highlights the importance of another policy that has recently received attention: deciding the age at which children enter kindergarten. Children in the United States are eligible to enroll in kindergarten if they turn five before a specific date set by their state of residence, known as an entrance cutoff date. In recent decades many state governments have chosen to move their cutoff dates earlier in the year. For example, 22 states moved their cutoff dates to an earlier point in the year between 1975 and 2000 (Stipek (2002)).[1] Since an older entrance age is empirically associated with better performance in school, this change in policy could be seen as a response to legislation such as 1994's "Goals 2000: Educate America Act" and 2002's "No Child Left Behind Act of 2001," which give schools and state governments increasing incentives to raise children's test scores. However, high stakes testing is not the only cause of these policy changes, as the average kindergarten entrance age began to increase before the introduction of high stakes testing (Deming and Dynarski (2008)). This points to the importance of increasing concerns about the "readiness" of children for school (Graue and DiPierna (2000), Graue (1993)). Such concerns have led parents and schools to delay the enrollment of many children until the year after they are first eligible to enroll. The prevalence of this practice, known as redshirting, is 10% for all children, with increasing frequency for the relatively youngest in any cohort.[2]

The collective effect of these choices on the part of parents, schools, and state governments has been an increase in the average age at which children start kindergarten in the US during recent decades (Elder and Lubotsky (2008), Weil (2007)). However, despite the plethora of studies showing a positive correlation between entrance age and school performance (See Datar (2006) for an extensive citation of these papers.), the effects of delaying kindergarten enrollment are not well understood. This is due not only to the non-random assignment of entrance age, but also because the age at which children enter kindergarten affects outcomes in several ways. Entrance age determines both the years of schooling students must complete before becoming eligible to drop

[1] See Figure 1 in Elder and Lubotsky (2008) for a summary of the evolution of these laws during the past 40 years.

[2] These data will be discussed in depth in Section 3. In the ECLS-K data set 9.62% of first-time kindergarteners had waited a year before entering, while 11.77% of all children had either been redshirted or repeated kindergarten. For the youngest quarter of eligibility these percentages are 18.99% and 23.35%, respectively.

out of school (Angrist and Krueger (1991)) and the age at which they first enter the labor market (Deming and Dynarski (2008)). Furthermore, entrance age directly affects the classroom experience of each student in two ways. First, there is a relative age effect, as entrance age determines the relative age of one's classmates for a child born on any given date. Second, there is an absolute age effect, as entrance age determines both the age at which a child is exposed to educational inputs, as well as the amount of time a child spends at home before entering formal schooling.

The key contribution of this paper is to separately identify coefficients related to both the absolute and relative age effects of different entrance age policies. By carefully selecting a quasi-random sample, the identification strategy employed avoids the problems redshirting creates when using date of birth as an instrument for educational attainment. For children in the sample, estimated absolute age coefficients provide no evidence that increasing the average age at which children begin kindergarten increases achievement test scores. Examining subgroups of the sample, we find that an important factor for absolute age coefficients is the number of children's books at home, suggesting that a focus on home environments may be a fruitful approach for future early education research and policy.

The paper is organized as follows: Section 2 briefly discusses the related literature. Section 3 presents the simple econometric model used to identify both absolute and relative age coefficients. The selection of the quasi-random sample from the ECLS-K data set is described, along with its relative strengths and weaknesses, in Section 4. Section 5 presents the results and discusses their implications. Section 6 concludes.

2 Relevant Literature

Kindergarten cohorts have become progressively older since the late 1960s (Deming and Dynarski (2008), Stipek (2002))[3], yet the literature addressing entrance age effects on academic outcomes and their durability presents mixed findings.[4] Datar (2006) finds large effects of entrance age on test scores in Kindergarten and First grade for children in the ECLS-K data set. Cascio and Lewis (2006) find that an addition year spent in high school has quite large positive impacts on Armed Forces Qualifying Test (AFQT) scores in the NLSY79 data set. Using the international TIMSS data set, Bedard and Dhuey (2006) report findings on the reduced form, net effects of relative age across several countries. They find large net relative age effects on test scores at the 4th grade level. Although they find these effects persist into the 8th grade, they are considerably reduced by that time. Using Chilean data, McEwan and Shapiro (2008) find that delaying enrollment has significant positive effects on outcomes such as retention and standardized test scores.

[3]Researchers have not agreed upon whether these changes have been driven more by policy or more by individual choice. Deming and Dynarski (2008) find that a great deal of the change in grade-for-age over time is explained not by retention or promotion policies, but rather by entrance age policies. In contrast, Bedard and Dhuey (2006) find that of those behind in school, 41% are behind due to late entry and 59% are behind due to retention.

[4]In addition to the literature on the effects entrance age has on academic outcomes, there is also a literature documenting the important role of relative age in competitive sports (Muscha and Grondin (2001)) and leadership (Dhuey and Lipscomb (2008)).

Puhani and Weber (2005) and Fredriksson and Öckert (2005) examine data from Germany and Sweden and find that higher relative age is associated with higher academic attainment.

In contrast, Black et al. (2008) examine data from Norway and find little evidence of long-term entrance age effects on IQ or earnings. As well, Elder and Lubotsky (2008) find evidence that age-related differences in academic performance dissipate as children advance in school, attributing most of the initial differences to the accumulation of skills before children enter kindergarten. While Dobkin and Ferreira (2007) do find that relatively younger children are more likely to be held back, they also find that younger children have higher academic attainment. Dobkin and Ferreira (2007) also find that relative age has little if any effect on adult outcomes such as employment, wages, or home ownership.

Unfortunately, it is often difficult to interpret the estimates from this literature which uses date of birth as an instrument for educational attainment. As originally proposed in Angrist and Krueger (1991), this identification strategy uses date of birth and compulsory schooling laws to assign one group of students an extra year of education at age 17 or older. The early childhood literature analogously uses date of birth to assign different levels of schooling to children at the age of a standardized test (6, 7, or older). However, as discussed in detail in Aliprantis (2009), redshirting creates problems for this identification scheme due to what Heckman et al. (2006) term essential heterogeneity.[5] Consider treatment to be assigned a birth date just before the entrance cutoff date, and control to be assigned a birth date well before the entrance cutoff date. The intuition behind the problem is that redshirting allows the treatment group to receive different treatments. That is, while most children in the treatment group will be induced to receive more schooling at any age relative to the control group, those in the treatment group who redshirt will actually complete less schooling at any age relative to the control group.[6] This is in contradiction to the monotonicity assumption first proposed in Angrist and Imbens (1995) to identify heterogeneous treatment effects. As discussed in Aliprantis (2009), due to this violation of monotonicity, along with the prevalence and nature of redshirting, estimated parameters may be a weighted average of effects that are very different in interpretation from the LATE.[7] Furthermore, while one could assume a constant treatment effect to avoid this problem, the high correlation between redshirting status and demographic variables (See Dobkin and Ferreira (2007) or Aliprantis (2009).) suggests heterogeneous entrance age effects for those who do and do not redshirt.[8]

[5]This result has been shown independently in Barua and Lang (2009).

[6]Note that in addition to relative age used as an instrument for educational attainment given one's age, the same argument from Aliprantis (2009) applies to the use of relative age R as an instrument for age A given one's schooling g as in Bedard and Dhuey (2006). Specifically, assigning a lower relative age R_i will induce those of type L to have a greater age A_{gi} at any level of schooling, while it will induce those of type H to have a lower age A_{gi} at any level of schooling.

[7]Although redshirting considerations are important when using date of birth as an instrument for educational attainment with recent data from the US, more directly relevant to the time period studied in Angrist and Krueger (1991) (1930–1959) may be considerations related to the diffusion of public kindergarten in the US (Cascio (2009a), Deming and Dynarski (2008)).

[8]It should be noted that Dobkin and Ferreira (2007) also raise methodological issues in the analysis of adult outcomes when using date of birth as an instrument for educational attainment.

3 Identification Framework

3.1 Econometric Model

The central contribution of this paper is to identify coefficients related to both absolute and relative entrance age effects using a simple identification framework based on a quasi-random sampling scheme and a generalized least squares (GLS) model. The key insight of this identification scheme comes not from the econometric model, but rather from the identification of a quasi-random sample to be discussed in Section 4. Given this quasi-random sample, where S_j is an indicator for a child entering kindergarten in state j, RA_{ij} is child i's relative age, AA_j is absolute age as determined by the entrance cutoff age of state j (the decomposition of each child's age into Absolute and Relative Ages will be discussed in Section 4.2.), and X_{ij} is a vector of demographic characteristics, the basic model of outcome y of student i in state j is:

$$y_{ij}(a) = \alpha_j S_j + \beta RA_{ij} + \gamma AA_j + \delta X_{ij} + \epsilon_{ij} \tag{1}$$

where

$$\epsilon_{ij} \sim N(0, \sigma_j^2). \tag{2}$$

The coefficient β is identified by the variation in outcomes of children with different RA_{ij} across all states, while identification of the coefficient γ comes from the variation across states with different AA_j in the outcomes of children with the same RA_{ij}. Within our nonexperimental study design, identification of parameters rests on the quasi-random nature of the sample constructed in Section 4, which assumes that both date of birth and entrance cutoff date policies are randomly distributed. Given that such identifying assumptions are very difficult to verify (to be discussed in Section 4.4), the paper refrains from referring to β and γ as effects. Nevertheless, these parameters are best interpreted as reduced-form policy effects rather than production function parameters (Todd and Wolpin (2003)).

3.2 Multiple Inference Adjustments

Since this is an exploratory, inductive study where each outcome simply helps to paint a descriptive picture, overall judgment rests on no single outcome. As a result, it is not necessary in the strictest sense to control for the fact that multiple inferences are being made (Schochet (2008)). Rather, it is simply important to be mindful of the fact that the naïve (ie, assuming independence) standard errors presented in Tables 8 and 9, as well as in Figures 4–8, are much more liberal than those corrected to control either the familywise error rate (FWER – See Romano and Wolf (2005).), or the more lenient corrections developed to control the false discovery rate (FDR – See Benjamini et al. (2006).).[9]

[9] For discussions on the relative strengths, weaknesses, and applicability of the various approaches to solving the multiplicity problem see Anderson (2008), Benjamini and Hochberg (1995), and Schochet (2008).

4 Data

4.1 The ECLS-K

The Early Childhood Longitudinal Study, Kindergarten Class of 1998-99 (ECLS-K) is a nationally representative sample of 22,666 children enrolled in 1,277 schools who started kindergarten in the fall of 1998. Data was collected during the the fall and the spring of kindergarten (1998-99), the fall and spring of 1st grade (1999-2000), the spring of 3rd grade (2002), 5th grade (2004), and 8th grade[10] (2007) from the children, their parents/guardians, teachers, and school administrators. Note that since observations from only about 30% of the sample were taken in Round 3 (Fall of 1st grade), it will not be considered for the analysis.

4.2 Variables

Following the terminology in Bedard and Dhuey (2006), we refer to the relative age at which a child would be observed if they entered kindergarten when first eligible as *assigned relative age*, and the child's actual age relative to their school's cutoff date as *observed relative age*. Figure 1 shows this relative age measured in months. For example, consider a child who lives in a state where the entrance cutoff age is exactly 5 years old at the start of the school year. Then a child who is 5 years and 3 months old at the start of the school year when first eligible to enroll is in the relative age group M_4. If the child redshirts they will join M_{16}, and they will be in M_{-9} if they enter early. Note that only in group M_4 will the child's assigned relative age agree with their observed relative age.

In order to assign children in the ECLS-K to these relative age cohorts, the ECLS-K public data file was used to obtain data on respondents' exact birth date, as well as school-level entrance cutoff dates. All variables represented as calendar dates were first converted to a daily time line in which day 1 is January 1, 1990. After all time-related variables were first constructed using this time line, these daily variables were divided by 365 to create annual variables. The yearly variables were then multiplied by 12 in order to create variables measured in months. A child's relative age (RA) is constructed as the age (in months) at the cutoff date minus 60.

We also use the entrance cutoff date faced by each child to define their absolute age (AA), which is the number of months before January 1st the entrance cutoff date is set. We define absolute age in this way because then on January 1, 1999, each child's age (over 5) is simply the sum of their relative age RA and their absolute age AA. Figure 2 shows this decomposition for a child in our sample born on November 1st who resides in a state with entrance cutoff date set on September 1st. On January 1, 1999, this child will be 14 months over 5 years of age (ie, 74 months old). Of these 14 months, we will attribute 4 months to Absolute Age and 10 months to Relative Age. Figure 2 illustrates that keeping constant one's birthdate, moving the entrance cutoff to a later date will decrease one's Absolute Age while increasing one's Relative Age. We can also see that for a fixed

[10]Eighth grade will be the last round of data collection due to sample attrition.

cutoff date, having a later birthdate decreases one's Relative Age without affecting one's Absolute Age. Table 5 shows how entrance age policies, and as a result absolute age, were set by states.[11]

The outcome measures used in this analysis are Math and Reading IRT test scores.

4.3 Sample

Table 1 shows the distribution of observations in the ECLS-K in each relative age group when using school-level entrance cutoff dates, including children repeating Kindergarten. Table 2 shows the same data, but for the sample including only first-time kindergarteners. If we assume parents' decision rule for determining observed entry age does not change over time, cutoff dates stayed the same between 1997 and 1998[12], and that any seasonal patterns in number of births are repeated every year, then we may use Tables 1 and 2 to estimate the percentage of children in each relative age group who enter early, when first eligible, or after redshirting. These estimates are presented in Tables 1 and 2. Table 3 shows these estimates aggregated to the level of quarters.

The estimates presented in Table 2b guide the selection of a sample relatively free from selection bias, which is one of the main contributions of this paper. Note that 31% of children who turned 5 within one month of their school's cutoff date were either redshirted or held back a year. While this rate does decline as children become relatively older when first eligible, it is still 14% for children who turned 5 in the fourth month before their school's cutoff date. Also note that a negligible number of children enter early, except for 5% in the oldest assigned relative age group. Finally, turning our attention to children who turned five between 6 and 11 months before their school's entrance cutoff date, over 90% entered kindergarten when first eligible.

The sample used for the analysis in this paper is composed of those first-time kindergarteners in the relative age groups M_6 through M_{11}. These cohorts are selected because they are the least effected by selection bias. Furthermore, the sample is restricted to children living in states which set an entrance cutoff date between August 31st and January 1st. Children from states with Local Education Authority options (LEAs)[13], from the three states with entrance cutoff dates before August 31st[14], and from states with fewer than 20 respondents (at a a given round)[15] are omitted from our sample. The 27 states included in our sample are: Alabama, Arizona, California, Connecticut, Florida, Georgia, Illinois, Iowa, Kansas, Kentucky, Louisiana, Maine, Maryland, Michigan, Minnesota, Mississippi, New Mexico, North Carolina, Ohio, Oklahoma, Rhode Island, Tennessee, Texas, Utah, Virginia, Washington, and Wisconsin.

[11] Cutoff dates were obtained from Table 2 of the Appendix in Elder and Lubotsky (2008).

[12]

[13] Colorado, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, and Vermont.

[14] Alaska, Indiana, and Missouri.

[15] Arkansas, Delaware, DC, Hawaii, Idaho, Montana, Nebraska, Nevada, North Dakota, Oregon, South Carolina, South Dakota, West Virginia, and Wyoming.

4.4 Identifying Assumptions

The causal interpretation of regression coefficients as treatment effects is dependent on the given sample being random. Although this was the goal in choosing a sample of children for whom date of birth does not affect parents' redshirting decisions, there are several reasons that warrant a more tempered interpretation of the parameters reported here. First, there is evidence that date of birth is itself not random, but rather is the product of parents' choices, biological factors, or both. Seasonal birth patterns have been demonstrated to be related to maternal characteristics such as age, marital status, education, and birth order (Bobak and Gjonca (2001), Buckles and Hungerman (2008)), demographic characteristics such as income and race (Bound and Jaeger (2000)), and even tax schedules (Dickert-Conlin and Chandra (1999)). However, it is currently unclear how these socio-economic characteristics interact with biological factors (Rizzi and Dalla-Zuanna (2007)), temperature (Lam and Miron (1996)), and geographic location (Bobak and Gjonca (2001)) in determining seasonal birth patterns. Although this process has been formally modeled (Lam et al. (1994)), the parameters of such a model of seasonal birth patterns have yet to be identified. This, together with data limitations, leaves seasonality of births as a serious concern for our identification strategy.

Most studies of the effects of entrance age have dealt with concerns related to seasonal birth patterns by simply assuming that date of birth is exogenous. In support of this approach, Dickert-Conlin and Elder (2009) present compelling evidence that while birth date is clearly manipulated within short windows for nonmedical purposes, school entrance cutoff dates is not one such purpose. However, Dickert-Conlin and Elder (2009) focus on the manipulation of birth timing at the level of days or weeks through cesarean deliveries and the inducement of labor. Hence it is still possible that birth timing is planned around a less precise period such as quarter or season, leaving the identification strategy presented here open to the problems arising if parents make non-random choices about when to have children.

Tables 6 and 7 show data on birth date in the ECLS-K. This data is presented for the samples of the entire ECLS-K cross section, for only those in the ECLS-K who are first-time kindergarteners, and for those in the sample defined in Section 4.3. Table 6 shows the distributions of birth by month and quarter, demonstrating that the sample is composed primarily of children from the first and last quarter of the year. Furthermore, the results of F-tests on the null hypotheses of equal means for several characteristics are reported in Table 7. Apart from race, there does not appear to be strong evidence of birth seasonality in our sample. Thus it may be reasonable to follow convention and assume that date of birth is exogenous, while at the same time acknowledging that the current analysis will be greatly improved by further research into seasonal birth patterns.

Another issue for our identification framework is the variables of the ECLS-K used to construct relative age. In addition to the publicly available school-level cutoff date we have been using thus far, state-level entrance cutoff dates were constructed using residence information from the ECLS-K Base Year Restricted Use Geographic Identifier together with the state level entrance age policies reported in Table 2 in the Appendix of Elder and Lubotsky (2008). Figure 3a shows the discrepancy in our sample between relative age when it is constructed using these two different measures of entrance cutoff dates. Over all children, the discrepancy in relative age when using state-level versus school-level entrance cutoff dates is, respectively, more than 1, 2, and 3 months for 51%, 44%, and 26% of children. For the sample those percentages are 48%, 40%, and 22%. The similarity in these patterns between private and public schools, as shown in Figures 3b and 3c, would suggest that these differences are most likely not driven by parents moving between schools (See the discussion in McEwan and Shapiro (2008).), but rather by entrance cutoff dates being set at a local level even in states with a statewide policy. This evidence points to the need for further research into the process by which entrance age policy is set, and gives strong reason for caution against interpreting regression coefficients as causal parameters.

5 Results and Discussion

Monthly relative and absolute age coefficients (β and γ) are presented in Tables 8 and 9. In order to make these parameters interpretable, we divide them by the average monthly gain in mean test scores between tests of all children in the sample. That is, the spring of 1st grade parameters are divided by $\frac{\mu_1 - \mu_k}{12}$, while spring of 3rd grade and spring of 5th grade parameters are divided by $\frac{\mu_t - \mu_{t-1}}{24}$. These standardized parameters can be interpreted as the ratio of a coefficient to the average monthly gain in test scores, and are presented in Figures 4–8 along with 95% confidence intervals. Note that the covariates included in X in Equation 1 are dummies for gender, race, attending a private school, Socio-Economic Status (quintile level), mother's education level, and whether a foreign language is the child's home language.

5.1 Absolute Age Coefficients (γ)

We begin by examining the parameters of interest, the absolute age coefficients γ. Figure 4 shows the standardized coefficients for all children in the sample. There are only small changes between spring of 1st grade and fall of 5th grade, and these standardized coefficients are never large. No estimates are larger than 1, and nearly all are between 0 and 1. Figure 5 shows the standardized γ coefficients by gender. The reading absolute age coefficient of girls actually grows over time, so that by the spring of 5th grade it is over 1. In contrast, all of the other absolute age coefficients begin and end near 0. Figure 6 shows that like girls, the reading absolute age coefficients of white children grow over time. In contrast, the absolute age coefficients of black children are very negative. However, it is difficult to make strong inference from these data given the small sample size (Table 8).

Figure 7 displays large differences found in the absolute age coefficients of children with either few or many books at home in the fall of kindergarten. For reading test scores, those children with at least 100 books at home have large and growing absolute age coefficients. But for those children with less than 50 books at home, reading absolute age coefficients are not far from 0, and are slightly decreasing over time. Math test scores also shows an advantage to increasing absolute age for those children with 100 or more books relative to those children with fewer than 50 books at home. Standardized γ coefficients for those with high (SES $\in \{4, 5\}$) and low (SES $\in \{1, 2\}$) Socio-Economic Status are displayed in Figure 8. Although children from high SES families have higher absolute age coefficients than those from low SES families, it is interesting that this difference is not as large as the difference when children are divided into groups determined by the number of books at home. Again, like girls and white children, the reading absolute age coefficients of high SES children increase over time.

5.2 Relative Age Coefficients (β)

Figure 4 shows that for all children, the reading relative age coefficient β increases over time. However, these relative age coefficients are between 0 and 1, as are nearly all such coefficients. Figure 5 shows that in 5th grade relative age seems to be more important for girls than for boys in reading. However, neither boys' nor girls' relative age coefficients are much different than those of the overall sample. In contrast, one can see from Figure 6 that while math relative age coefficients do not vary much over race, reading relative age coefficients are quite different by race. Again, the small sample size (Table 8) makes these coefficients difficult to interpret. Figure 7 demonstrates that relative age coefficients, like absolute age coefficients, depend on the number of books a child has at home in the fall of kindergarten. The differences in relative age coefficients are much larger for reading test scores than math test scores. Finally, Figure 8 shows that math relative age coefficients do not vary much by SES level, although the reading relative age coefficient of high SES children does differ from that of low SES children by spring of 5th grade.

5.3 Discussion

Although these parameter estimates are not causal, and as discussed in Section 4.4 there are many reasons not to interpret them as such, they nevertheless begin to paint a descriptive picture. Over all children in our sample, the estimated absolute age coefficients provide no evidence that increasing the average age at which children begin kindergarten increases achievement test scores. In fact, the gain in test scores associated with time spent outside of school is less than the gain associated with time passing once one has begun school. If anything, this evidence is suggestive that children could benefit from entering school at earlier ages. It is also important to note that the parameter results only deal with achievement tests, and ignore other negative effects of increasing kindergarten entrance age such as increased child care costs (Cascio (2009b)), decreased mandatory schooling attainment (Angrist and Krueger (1991)), and foregone labor force participation (Deming and Dynarski (2008)). Finally, the importance of the number of children's books at home indicates that a focus on home environments may be a fruitful approach for future early education research and policy.

6 Conclusion

Early childhood education is currently receiving a great deal of attention from researchers and policy-makers. This focus on children's pre-kindergarten education highlights the importance of understanding how policies changing kindergarten entrance age affect outcomes. However, little is known about the consequences of parents, schools, and state governments having chosen during recent decades to increase the average age at which children enter kindergarten in the United States. In order to understand the relationship between kindergarten entrance age and achievement, this paper carefully selects a quasi-random sample of children, avoiding the problems redshirting creates when using date of birth as an instrument for educational attainment. This approach allows us to separately identify coefficients related to absolute and relative age effects on math and reading test scores. For children in the sample, estimated absolute age coefficients provide no evidence that increasing the average age at which children begin kindergarten increases achievement test scores. The importance of demographic characteristics such as the number of children's books at home suggests that a focus on home environments may be a fruitful approach for future early education research and policy.

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Figures

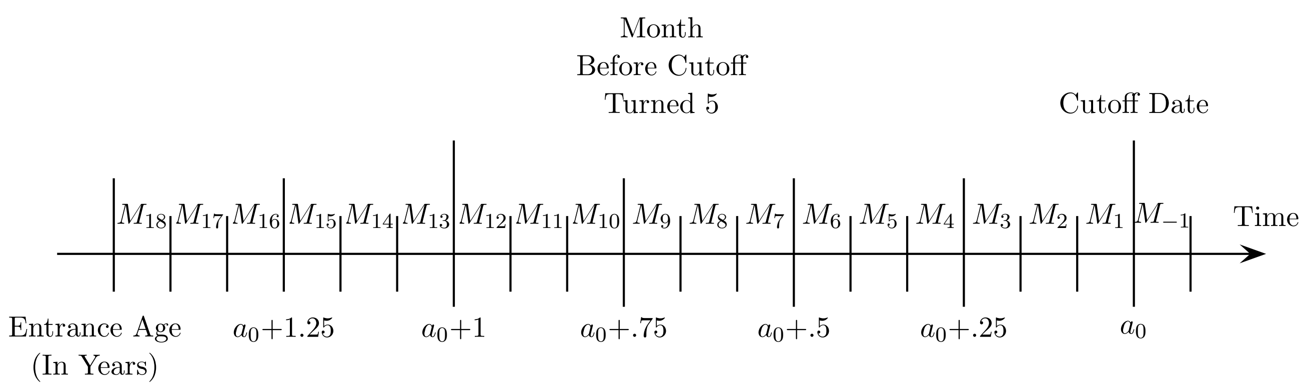


Figure 1

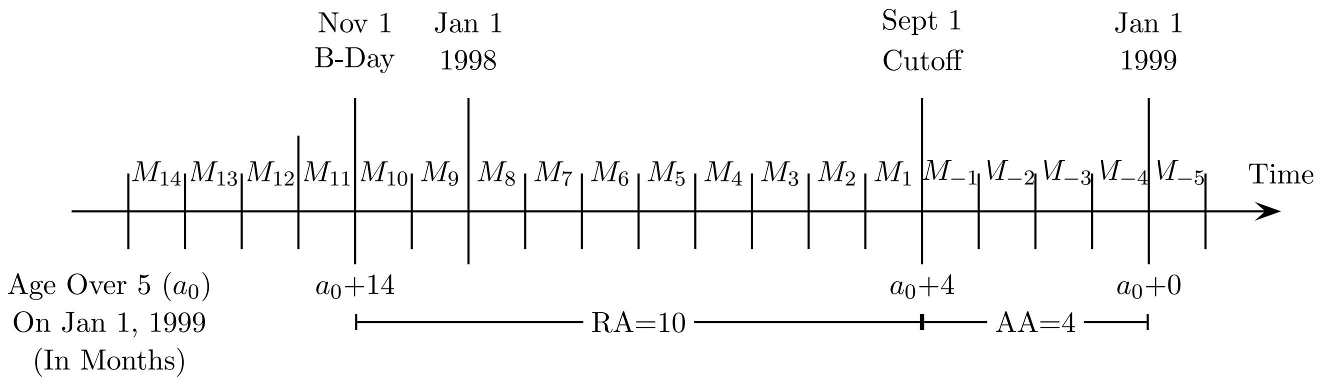
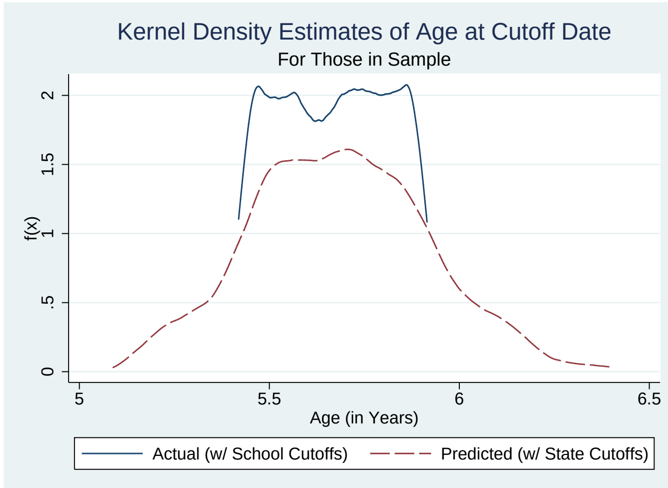
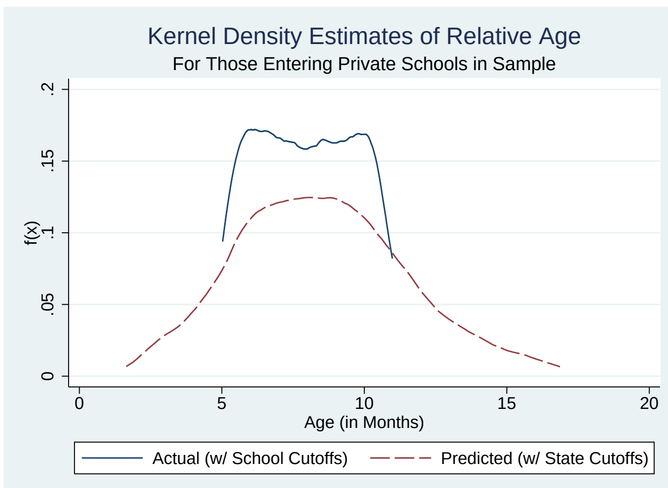


Figure 2: Age of a Hypothetical Child on January 1, 1999 as the Sum of Relative Age and Absolute Age



(a) Overall



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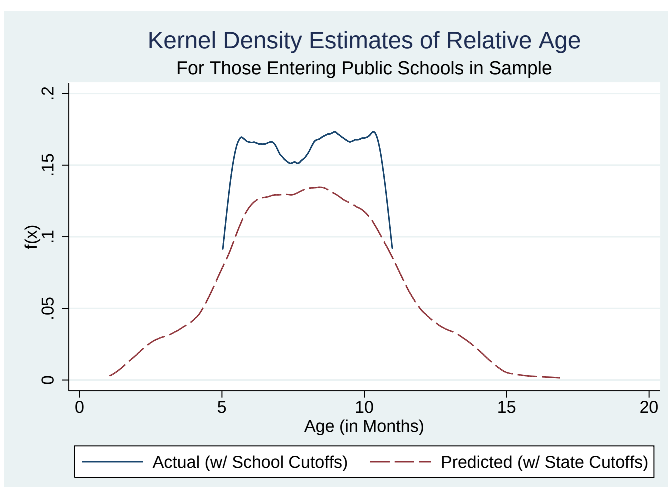
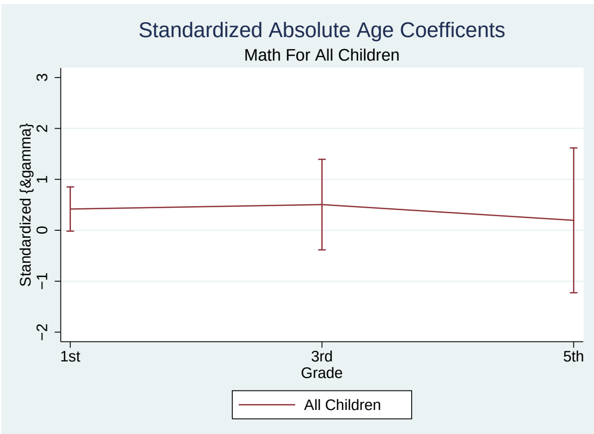
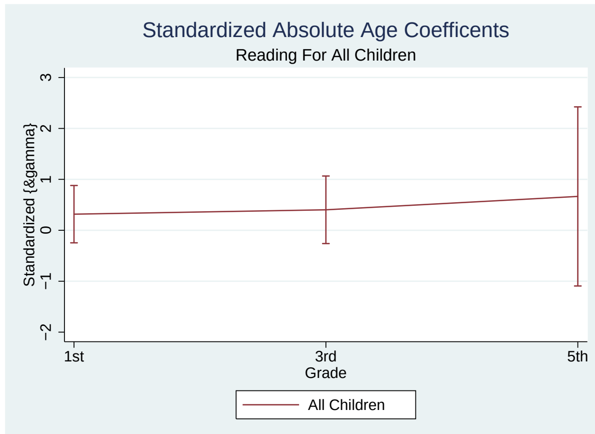


(c) Public

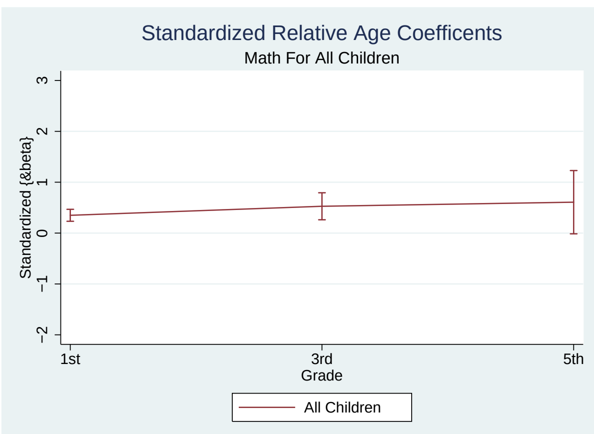
Figure 3: Sorting



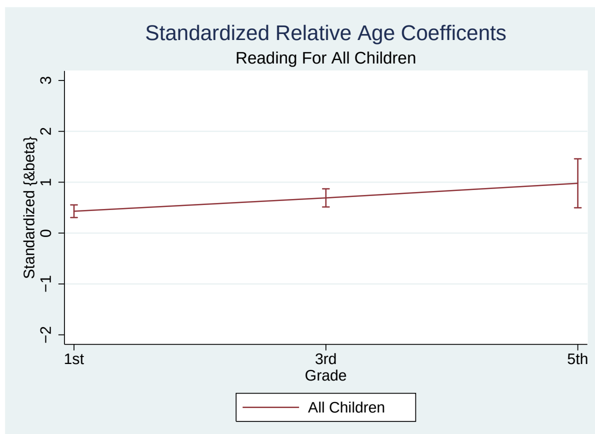
(a) Math Absolute Age Coefficient



(b) Reading Absolute Age Coefficient

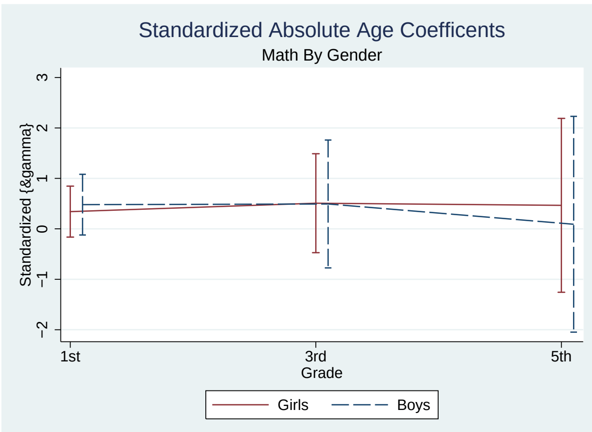


(c) Math Relative Age Coefficient

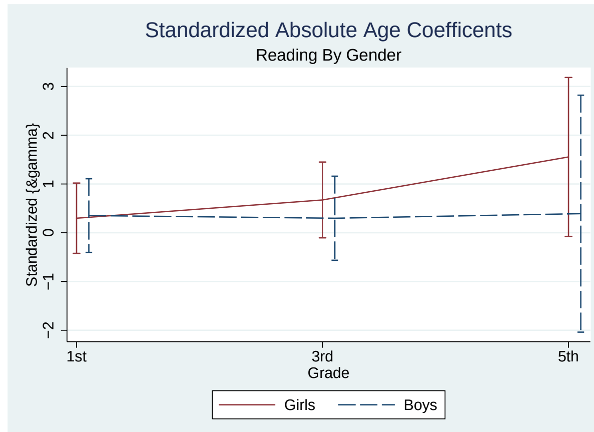


(d) Reading Relative Age Coefficient

Figure 4: Absolute and Relative Age Coefficients of All Children



(a) Math Absolute Age Coefficient



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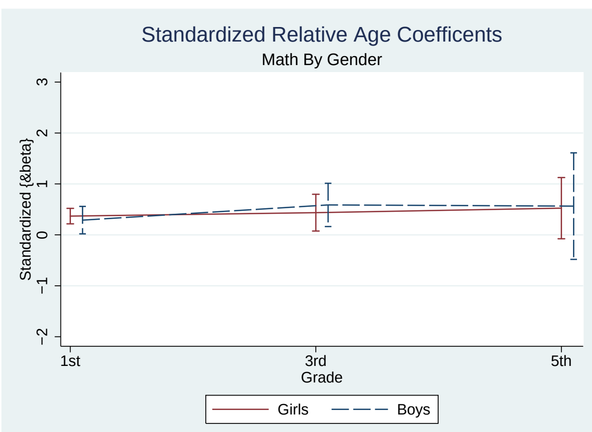
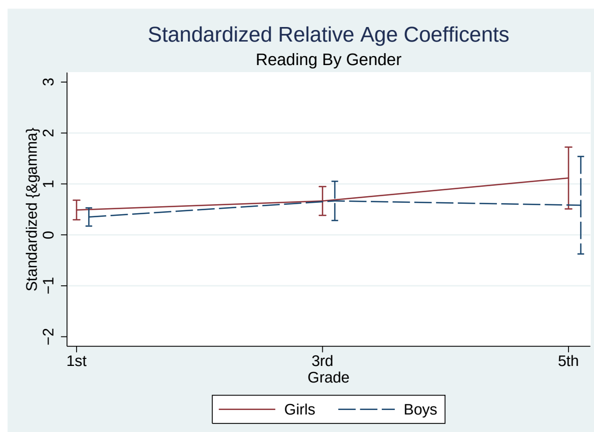
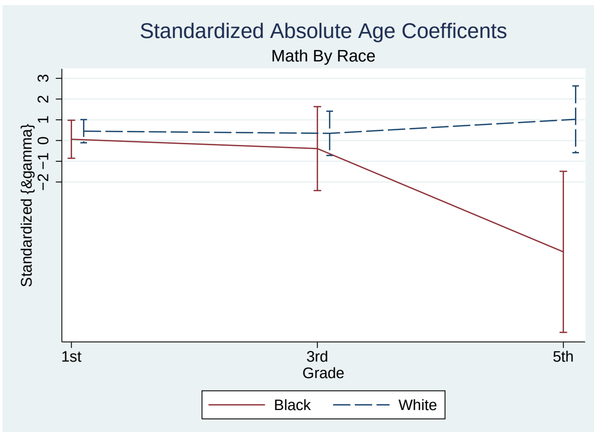


(c) Math Relative Age Coefficient

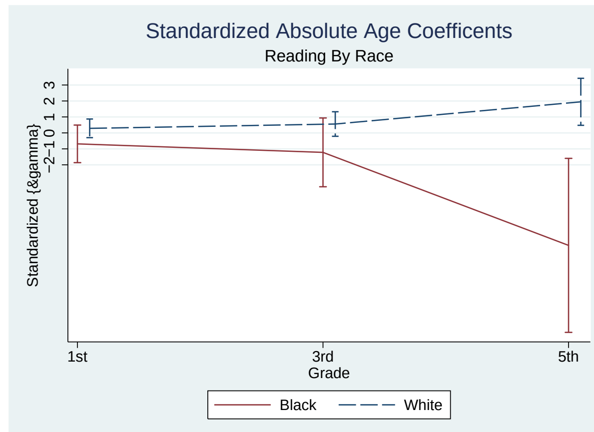


(d) Reading Relative Age Coefficient

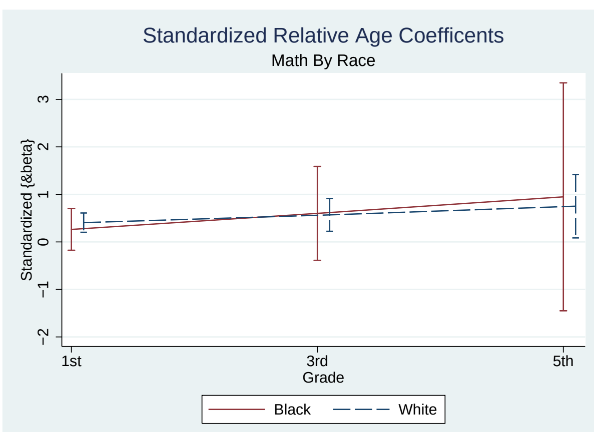
Figure 5: Absolute and Relative Age Coefficients By Gender



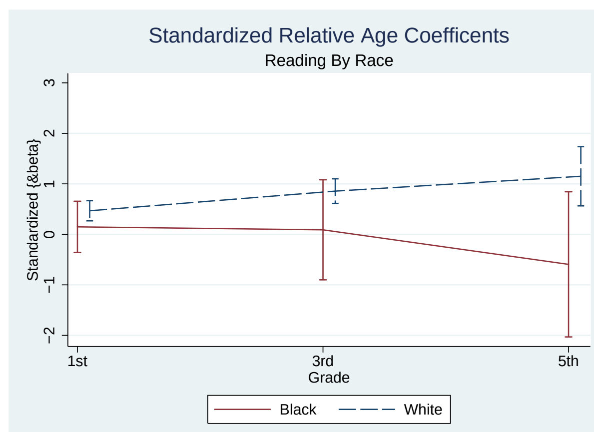
(a) Math Absolute Age Coefficient



(b) Reading Absolute Age Coefficient

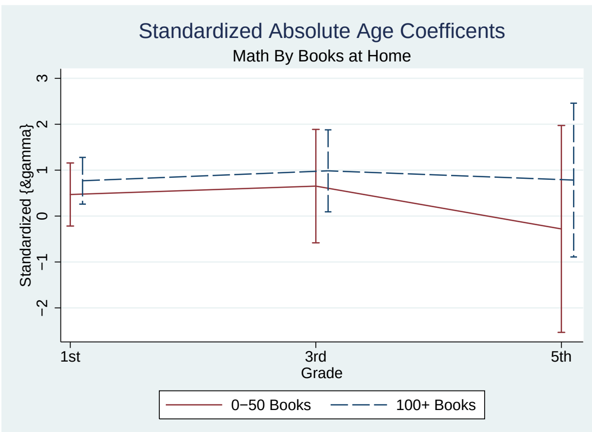


(c) Math Relative Age Coefficient



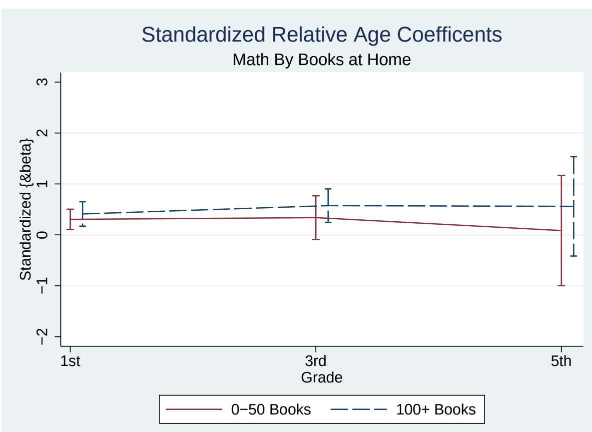
(d) Reading Relative Age Coefficient

Figure 6: Absolute and Relative Age Coefficients By Race

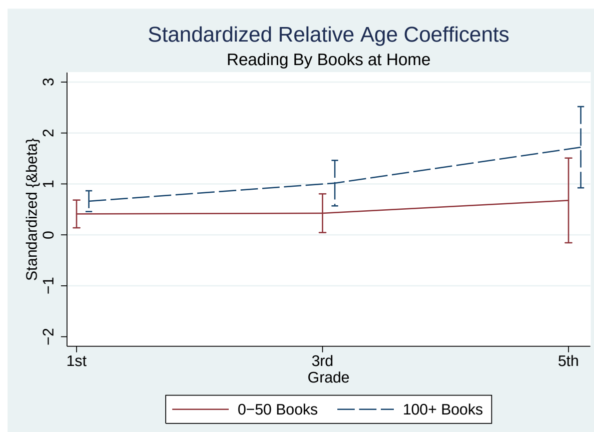


(a) Math Absolute Age Coefficient

(b) Reading Absolute Age Coefficient

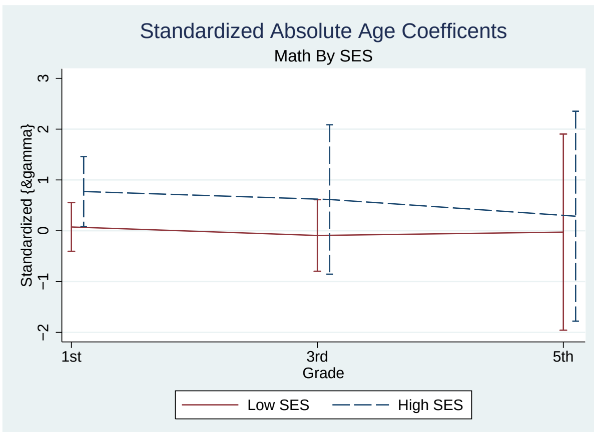


(c) Math Relative Age Coefficient

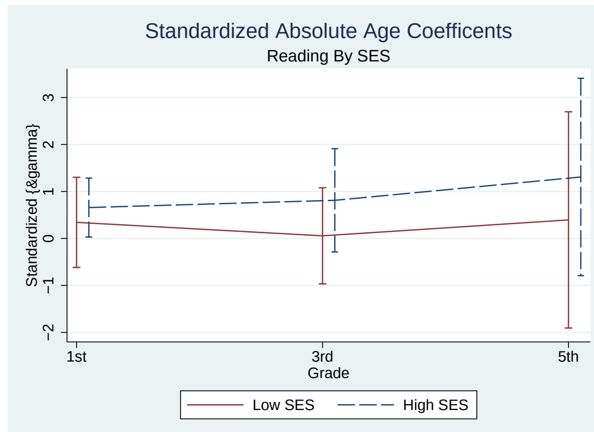


(d) Reading Relative Age Coefficient

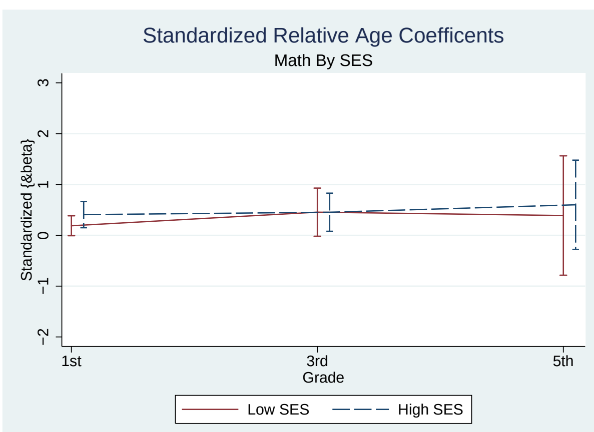
Figure 7: Absolute and Relative Age Coefficients By Number of Books at Home



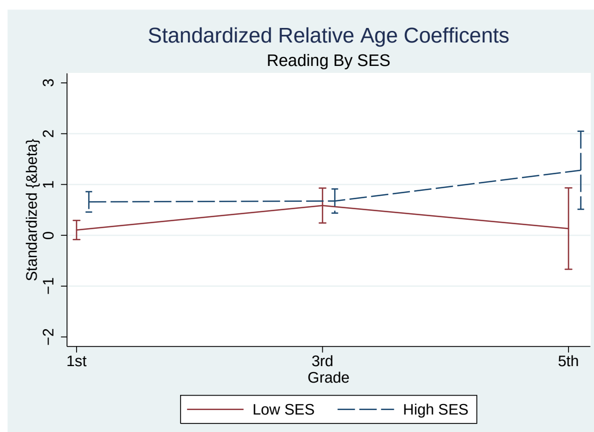
(a) Math Absolute Age Coefficient



(b) Reading Absolute Age Coefficient



(c) Math Relative Age Coefficient



(d) Reading Relative Age Coefficient

Figure 8: Absolute and Relative Age Coefficients By SES

Tables

Table 1: Cohorts of the ECLS-K (By Month)

	Cohort											
Cohort	M_{-1}	M_{-2}	M_{-3}	M_{-4}	M_{-5}	M_{-6}	M_{-7}	M_{-8}	M_{-9}	M_{-10}	M_{-11}	M_{-12}
n	57	15	9	6	4	3	7	5	1	3	1	0
Cohort	M_{12}	M_{11}	M_{10}	M_{9}	M_{8}	M_{7}	M_{6}	M_{5}	M_{4}	M_{3}	M_{2}	M_{1}
n	954	937	1,003	982	907	962	990	982	946	922	832	802
Cohort	M_{24}	M_{23}	M_{22}	M_{21}	M_{20}	M_{19}	M_{18}	M_{17}	M_{16}	M_{15}	M_{14}	M_{13}
n	38	55	60	69	65	82	83	125	155	196	223	361

(a) All Children

	Month Before Cutoff Turned 5											
Entering	12	11	10	9	8	7	6	5	4	3	2	1
Early (%)	5.4	1.5	0.8	0.6	0.4	0.3	0.6	0.4	0.1	0.3	0.1	0.0
On-Time (%)	90.9	93.0	93.6	92.9	92.9	91.9	91.7	88.3	85.8	82.2	78.8	69.0
Waiting (%)	3.6	5.5	5.6	6.5	6.7	7.8	7.7	11.2	14.1	17.5	21.1	31.0

(b) All Children

Table 2: Cohorts of the ECLS-K (By Month)

	Cohort											
Cohort	M_{-1}	M_{-2}	M_{-3}	M_{-4}	M_{-5}	M_{-6}	M_{-7}	M_{-8}	M_{-9}	M_{-10}	M_{-11}	M_{-12}
n	45	12	6	3	3	2	3	2	1	1	1	0
Cohort	M_{12}	M_{11}	M_{10}	M_{9}	M_{8}	M_{7}	M_{6}	M_{5}	M_{4}	M_{3}	M_{2}	M_{1}
n	790	780	857	838	790	855	854	857	842	798	738	698
Cohort	M_{24}	M_{23}	M_{22}	M_{21}	M_{20}	M_{19}	M_{18}	M_{17}	M_{16}	M_{15}	M_{14}	M_{13}
n	31	43	47	49	47	64	49	86	100	121	149	254

(a) First Time Kindergarteners Only

	Month Before Cutoff Turned 5											
Entering	12	11	10	9	8	7	6	5	4	3	2	1
Early (%)	5.2	1.4	0.7	0.3	0.4	0.2	0.3	0.2	0.1	0.1	0.1	0.0
On-Time (%)	91.2	93.4	94.2	94.2	94.0	92.8	94.3	90.7	89.3	86.7	83.1	73.3
Waiting (%)	3.6	5.1	5.2	5.5	5.6	6.9	5.4	9.1	10.6	13.2	16.8	26.7

(b) First Time Kindergarteners Only

Table 3: Cohorts of the ECLS-K (By Quarter)

	Quarter Before Cutoff Turned 5			
Quarter	4	3	2	1
Early (n)	81	13	13	4
On-Time (n)	2,894	2,851	2,918	2,556
Waiting (n)	153	216	363	780

(a) All Children

	Quarter Before Cutoff Turned 5			
Entering	4	3	2	1
Early (%)	2.59	0.42	0.39	0.12
On-Time (%)	92.52	92.56	88.59	76.53
Waiting (%)	4.89	7.01	11.02	23.35

(b) All Children

Table 4: Cohorts of the ECLS-K (By Quarter)

	Quarter Before Cutoff Turned 5			
Quarter	4	3	2	1
Early (n)	63	8	6	2
On-Time (n)	2,427	2,483	2,553	2,234
Waiting (n)	121	160	235	524

(a) First-Time Kindergarteners Only

	Quarter Before Cutoff Turned 5			
Entering	4	3	2	1
Early (%)	2.41	0.30	0.21	0.07
On-Time (%)	92.95	93.66	91.37	80.94
Waiting (%)	4.63	6.04	8.41	18.99

(b) First-Time Kindergarteners Only

Table 5: States and Entrance Cutoff Dates

States	% of Students Overall	% of Students Sample	Cutoff Date in 1998	Assigned AA_j in Months
22	44.95	48.15	Aug 31/Sept 1	4.0
1	0.00	0.00	Sept 10	3.67
2	4.09	3.45	Sept 15	3.5
6	14.93	16.93	Sept 30/Oct 1	3.0
3	5.20	5.45	Oct 15	2.5
2	24.74	21.38	Dec 1	1.0
5	6.09	4.64	Dec 31/Jan 1	0.0
41	13,701	3,597		

Table 6: The Distribution of Students

Sample:	All Children		First-Time K		Sample	
Month	n	%	n	%	n	%
January	1,371	6.45	1,321	6.42	507	14.10
February	1,357	6.39	1,319	6.41	536	14.90
March	1,503	7.07	1,468	7.14	594	16.51
April	1,408	6.63	1,360	6.61	284	7.90
May	1,508	7.10	1,469	7.14	156	4.34
June	1,526	7.18	1,478	7.18	154	4.28
July	1,530	7.20	1,446	7.03	61	1.70
August	1,587	7.47	1,502	7.30	17	0.47
September	1,526	7.18	1,457	7.08	47	1.31
October	1,463	6.89	1,394	6.78	343	9.54
November	1,345	6.33	1,279	6.22	430	11.95
December	1,428	6.72	1,384	6.73	468	13.01
n	21,247		20,572		3,597	

(a) By Month

Sample:	All Children		First-Time K		Sample	
Quarter	n	%	n	%	n	%
Q_1	4,231	19.91	4,108	19.97	1,637	45.51
Q_2	4,442	20.91	4,307	20.94	594	16.51
Q_3	4,643	21.85	4,405	21.41	125	3.48
Q_4	4,236	19.94	4,057	19.72	1,241	34.50
n	21,247		20,572		3,597	

(b) By Quarter

Table 7: F-Tests of Equality of Means of Characteristics by Quarter and Month

	Pr > F					
Sample:	All Children		First-Time K		Sample	
Characteristic	Quarter	Month	Quarter	Month	Quarter	Month
Race						
Black	0.28	0.06	0.39	0.11	0.00	0.00
White	0.36	0.40	0.41	0.42	0.00	0.00
Hispanic	0.32	0.74	0.35	0.73	0.00	0.00
Asian	0.51	0.85	0.64	0.83	0.00	0.00
Gender						
Female	0.90	0.67	0.97	0.61	0.21	0.23
Mom's Education Level						
Mom's HGC $<$ 12	0.33	0.77	0.39	0.86	0.94	0.53
Mom's HDR = HS Diploma	0.41	0.59	0.56	0.68	0.37	0.25
Mom's HDR $\geq$ BA	0.51	0.35	0.72	0.47	0.12	0.19
Home Characteristics						
# of Books at Home	0.04	0.16	0.13	0.24	0.86	0.31
Live with Father	0.69	0.38	0.56	0.55	0.27	0.46
Birth Characteristics						
Parents Married at Birth	0.05	0.27	0.07	0.32	0.71	0.85
Birth Weight	0.99	0.29	0.97	0.31	0.59	0.28
Socio-Economic Status						
SES=1	0.04	0.31	0.08	0.46	0.91	0.99
SES=2	0.83	0.62	0.66	0.41	0.14	0.15
SES=3	0.79	0.27	0.79	0.30	0.12	0.03
SES=4	0.87	0.23	0.78	0.24	0.42	0.04
SES=5	0.05	0.42	0.04	0.36	0.41	0.84

Table 8: Relative and Absolute Age Coefficients

	Math β	Math γ	Reading β	Reading γ	n Math	n Reading
Fall K	0.59**	0.68*	0.71**	0.71	2,611	2,479
	(0.10)	(0.27)	(0.10)	(0.45)		
Spring K	0.80**	0.93**	0.87**	0.72	2,777	2,677
	(0.10)	(0.21)	(0.18)	(0.50)		
Spring 1st	0.73**	0.87	1.12**	0.83	2,726	2,676
	(0.13)	(0.46)	(0.17)	(0.75)		
Spring 3rd	0.76**	0.72	1.33**	0.77	2,355	2,349
	(0.19)	(0.65)	(0.18)	(0.65)		
Spring 5th	0.52	0.27	0.83**	0.56	1,889	1,887
	(0.17)	(0.62)	(0.21)	(0.76)		

(a) All Children

	Girls						Boys					
	Math		Read		n		Math		Read		n	
	β	γ	β	γ	Math	Read	β	γ	β	γ	Math	Read
Fall K	0.58**	0.24	0.83**	0.18	1,273	1,214	0.58**	1.02**	0.57**	1.18	1,338	1,265
	(0.14)	(0.36)	(0.16)	(0.45)			(0.15)	(0.39)	(0.18)	(0.64)		
Spring K	0.77**	0.46	0.89**	0.41	1,342	1,298	0.81**	1.31**	0.85**	1.02	1,435	1,379
	(0.14)	(0.35)	(0.25)	(0.66)			(0.19)	(0.40)	(0.17)	(0.74)		
Spring 1st	0.77**	0.71	1.28**	0.78	1,317	1,295	0.60*	1.00	0.92**	0.92	1,409	1,381
	(0.16)	(0.54)	(0.26)	(0.96)			(0.29)	(0.64)	(0.24)	(1.01)		
Spring 3rd	0.62*	0.73	1.28**	1.29	1,156	1,154	0.84**	0.71	1.28**	0.57	1,199	1,195
	(0.26)	(0.72)	(0.28)	(0.76)			(0.31)	(0.93)	(0.38)	(0.85)		
Spring 5th	0.45	0.40	0.95**	1.32	926	925	0.48	0.08	0.49	0.33	2,611	962
	(0.27)	(0.75)	(0.26)	(0.70)			(0.46)	(0.94)	(0.41)	(1.05)		

(b) By Gender

	Black						White					
	Math		Read		n		Math		Read		n	
	β	γ	β	γ	Math	Read	β	γ	β	γ	Math	Read
Fall K	0.33	-0.25	0.73**	0.41	318	318	0.64**	0.46	0.70**	0.46	1,493	1,493
	(0.21)	(0.55)	(0.26)	(0.90)			(0.15)	(0.26)	(0.12)	(0.36)		
Spring K	0.56	-0.67	0.35	-0.86	339	340	0.88**	1.00**	0.97**	0.48	1,582	1,582
	(0.31)	(0.95)	(0.41)	(1.15)			(0.18)	(0.23)	(0.22)	(0.61)		
Spring 1st	0.55	0.13	0.39	-1.79	327	327	0.85**	0.94	1.22**	0.75	1,555	1,555
	(0.47)	(0.98)	(0.68)	(1.57)			(0.22)	(0.60)	(0.27)	(0.78)		
Spring 3rd	0.86	-0.56	0.17	-2.34	271	269	0.81**	0.50	1.65**	1.07	1,337	1,336
	(0.72)	(1.48)	(0.97)	(2.11)			(0.25)	(0.78)	(0.24)	(0.75)		
Spring 5th	0.81	-4.60**	-0.50	-5.97*	184	185	0.64*	0.88	0.97**	1.65**	1,073	1,071
	(1.05)	(1.70)	(0.62)	(2.36)			(0.29)	(0.70)	(0.25)	(0.64)		

(c) By Race

Table 9: Relative and Absolute Age Coefficients

	Math		Reading		n	
	β	γ	β	γ	Math	Reading
Fall K	0.59**	0.68*	0.71**	0.71	2,611	2,479
	(0.10)	(0.27)	(0.10)	(0.45)		
Spring K	0.80**	0.93**	0.87**	0.72	2,777	2,677
	(0.10)	(0.21)	(0.18)	(0.50)		
Spring 1st	0.73**	0.87	1.12**	0.83	2,726	2,676
	(0.13)	(0.46)	(0.17)	(0.75)		
Spring 3rd	0.76**	0.72	1.33**	0.77	2,355	2,349
	(0.19)	(0.65)	(0.18)	(0.65)		
Spring 5th	0.52	0.27	0.83**	0.56	1,889	1,887
	(0.17)	(0.62)	(0.21)	(0.76)		

(a) All Children

	0-50 Books						>100 Books					
	Math		Read		n		Math		Read		n	
	β	γ	β	γ	Math	Read	β	γ	β	γ	Math	Read
Fall K	0.36**	0.59	0.46**	0.11	902	788	0.77**	1.21**	1.05**	1.28**	945	946
	(0.07)	(0.51)	(0.08)	(0.67)			(0.15)	(1.21)	(0.14)	(0.41)		
Spring K	0.51**	0.72	0.78**	0.08	908	923	1.03**	1.47**	1.25**	1.16*	934	935
	(0.13)	(0.52)	(0.23)	(1.37)			(0.18)	(0.35)	(0.26)	(0.58)		
Spring 1st	0.64**	0.98	1.07**	0.52	897	900	0.86**	1.61**	1.73**	1.32	916	916
	(0.21)	(0.73)	(0.36)	(1.54)			(0.26)	(0.54)	(0.27)	(0.86)		
Spring 3rd	0.48	0.93	0.82*	0.13	749	742	0.82**	1.41*	1.95**	2.31**	805	807
	(0.31)	(0.90)	(0.37)	(1.09)			(0.24)	(0.65)	(0.44)	(0.88)		
Spring 5th	0.07	-0.24	0.57	-0.43	599	598	0.48	0.67	1.46**	2.30**	648	647
	(0.47)	(0.99)	(0.36)	(0.94)			(0.43)	(0.73)	(0.34)	(0.83)		

(b) By Books at Home

	Low SES						High SES					
	Math		Read		n		Math		Read		n	
	β	γ	β	γ	Math	Read	β	γ	β	γ	Math	Read
Fall K	0.16	0.24	0.29**	0.42	890	779	0.74**	1.12**	0.95**	1.19	1,206	1,200
	(0.12)	(0.28)	(0.11)	(0.44)			(0.14)	(0.36)	(0.15)	(0.61)		
Spring K	0.32**	0.71	0.27	0.66	962	871	1.02**	1.28**	1.32**	1.21*	1,269	1,266
	(0.12)	(0.37)	(0.18)	(0.70)			(0.20)	(0.31)	(0.29)	(0.55)		
Spring 1st	0.39	0.16	0.27	0.90	950	904	0.85**	1.62*	1.72**	1.72*	1,242	1,240
	(0.21)	(0.51)	(0.25)	(0.90)			(0.28)	(0.73)	(0.27)	(0.84)		
Spring 3rd	0.65	-0.13	1.13**	0.11	791	785	0.65*	0.88	1.30**	1.56	1,101	1,103
	(0.34)	(0.51)	(0.34)	(1.00)			(0.27)	(1.07)	(0.23)	(1.08)		
Spring 5th	0.33	-0.02	0.11	0.33	638	637	0.51	0.25	1.08**	1.11	887	886
	(0.51)	(0.84)	(0.35)	(1.00)			(0.38)	(0.90)	(0.33)	(0.91)		

(c) By SES