Redshirting, Compulsory Schooling Laws, and Educational Attainment

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Abstract: A wide literature uses date of birth as an instrument for educational attainment. Considering the assumptions of independence and monotonicity within the context of a latent index model, we find that this identification strategy produces biased estimates if parents choose to redshirt, or to delay their children's initial enrollment in kindergarten, based on treatment effect heterogeneity. An inspection of the ECLS-K data set indicates not only that such redshirting is common, but also that heterogeneity in the treatment effect of educational attainment is almost certainly a factor in parents' redshirting decisions. These findings have important implications for a broad assortment of parameter estimates in the literature.

Keywords: Instrumental Variable, Local Average Treatment Effect, Essential Heterogeneity, Independence, Monotonicity, Latent Index Model

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1 Introduction

A wide literature uses date of birth as an instrument for educational attainment. Considering the assumptions of independence and monotonicity within the context of a latent index model, we find that this identification strategy produces biased estimates if parents choose to redshirt, or to delay their children's initial enrollment in kindergarten, based on treatment effect heterogeneity. An inspection of the Early Childhood Longitudinal Study, Kindergarten Class of 1998-99 (ECLS-K) data set indicates not only that such redshirting is common, but also that heterogeneity in the treatment effect of educational attainment is almost certainly a factor in parents' redshirting decisions. These findings have important implications for a broad assortment of parameter estimates in the literature.

The paper is organized as follows: Section 2 defines the Local Average Treatment Effect (LATE) identified by the instrumental variable (IV) estimator within a standard treatment effect framework. Section 3 introduces the example of date of birth as an instrument for educational attainment, and then discusses how redshirting causes violations of the identifying assumptions presented in Section 2. Section 4 shows that the patterns of redshirting in the ECLS-K data set are inconsistent with the monotonicity assumption as discussed in Section 2. Finally, Section 5 discusses the implications of these issues for the interpretation of relevant parameter estimates in the literature.

2 Framework

We begin by defining the Local Average Treatment Effect (LATE) identified by the instrumental variable (IV) estimator within the standard treatment effect framework of Vytlacil (2002). Let $(\Omega, \mathcal{F}, P)$ be the probability space over which random variables are defined.[1] Let $(Y_0(\omega), Y_1(\omega))$ be the random variables associated with the potential outcomes in the untreated and treated states, respectively. $D(\omega)$ is a random variable associated with receipt of treatment, where

$$D(\omega) = \begin{cases} 1 & \text{if treatment is received;} \\ 0 & \text{if treatment is not received.} \end{cases}$$

The measured outcome variable $Y(\omega)$ is

$$Y(\omega) = D(\omega)Y_1(\omega) + (1 - D(\omega))Y_0(\omega).$$

Let $W(\omega)$ be the vector of observed covariates, which we partition into $W(\omega) = [X(\omega), Z(\omega)]$. Here X contains the covariates that directly affect potential outcomes (Y_0, Y_1) (as well as possibly affecting the treatment decision), and Z contains covariates that only affect the treatment decision D. We refer to Z as our instrument or excluded variable. Let $\mathcal{Z}$ denote the support of Z, and let

[1] Since Ω is the set of all possible outcomes and each $\omega \in \Omega$ is one realization, we may think of ω as being analogous to an i subscript.

z be a possible realization of Z. For each $z \in \mathcal{Z}$, let $D_z(\omega)$ be the counterfactual variable for $D(\omega)$ if $Z(\omega)$ had externally been set to z. We also define the propensity score

$$p(z) = Pr[D(\omega) = 1 | Z(\omega) = z].$$

2.1 Identifying Assumptions

Vytlacil (2002) next specifies the assumptions:

LATE Independence Assumption (L-1): *(i) For all $z \in \mathcal{Z}$, $Z(\omega) \perp\!\!\!\perp (Y_0(\omega), Y_1(\omega), D_z(\omega))$, and (ii) p(z) is a nontrivial function of z.*

LATE Monotonicity Assumption (L-2): *For all $(z, z') \in \mathcal{Z} \times \mathcal{Z}$, either $D_z(\omega) \geq D_{z'}(\omega)$ for all $\omega \in \Omega$, or $D_z(\omega) \leq D_{z'}(\omega)$ for all $\omega \in \Omega$.*

Angrist and Imbens (1995) show that under assumptions equivalent to (L-1) and (L-2), the linear IV estimand identifies the LATE, $E(Y_1 - Y_0 | D_1 = 1, D_0 = 0)$. The innovation of Angrist and Imbens (1995) is to allow for treatment effect heterogeneity by placing a nonparametric restriction, as summarized by Assumptions (L-1) and (L-2), on the process generating D as a function of Z. As further discussed in Imbens and Angrist (1994), the LATE is the average treatment effect for those individuals whose decision is influenced by the instrument, which can been seen as a weighted average of heterogeneous treatment effects.

3 Date of Birth and Educational Attainment: LATE Under Variable Treatment Intensity

Now let us consider the framework described in Section 2 under the application of date of birth as an instrument for educational attainment.[2] For the sake of exposition suppose that we are interested in test scores Y, where z is date of birth and $D_z(\omega)$ is educational attainment (measured in years of completed schooling) on the sixth birthday. Following the terminology in Bedard and Dhuey (2006), we refer to the age at which a child would be observed in school if they entered kindergarten when first eligible as their assigned age, and the child's actual age when observed in school to be their observed age.

[2] The Appendix replicates this analysis under the framework of Angrist and Imbens (1995).

3.1 Independence

We consider the instrument Z of assigned age[3], and we now deviate from the framework described in Section 2 and allow for treatment intensity to be continuous rather than binary:

$$D(\omega) \in [0, 1].$$

The measured outcome variable $Y(\omega)$ is

$$Y(\omega) = \beta D(\omega), \tag{1}$$

with β being the treatment effect. Furthermore, we denote the observed treatment intensity to be D, while we denote the assigned treatment intensity to be D^*. In our example of educational attainment the assumption of independence breaks down because treatment intensity depends on assigned age z. The key point is that externally changing z to z' would also change the assigned intensity of the treatment received, so that the counterfactual to $(z, D_z^*(\omega))$ is $(z', D_{z'}^*(\omega))$. Under independence the counterfactual to $(z, D_z^*(\omega))$ would be $(z', D_z^*(\omega))$.

For example, consider a state which sets a kindergarten entrance cutoff age of exactly five years on the first day of school. Figure 1 shows the dependence of D^* on z in this case. Consider $z = 0$, $z' = .5$, and $z'' = .75$. Then $D_z^*(\omega) = 1, D_{z'}^*(\omega) = 2/3$, and $D_{z''}^*(\omega) = 1/3$, so that $D_z(\omega) \in \{0, 1\}$, $D_{z'}(\omega) \in \{0, 2/3\}$, and $D_{z''}(\omega) \in \{0, \frac{1}{3}\}$. Thus the LATE Independence Assumption (L-1) in the framework of Vytlacil (2002) is violated.

Note that this violation of independence also violates the assumption in Theorem 2 of the regression discontinuity design in Hahn et al. (2001); specifically, that x_i is independent of β_i conditional on z_i near z_0.

3.2 Monotonicity and a Latent Index Model of Redshirting

We introduce what Heckman et al. (2006) term essential heterogeneity by assuming there are two types of children, $\tau \in \{H, L\}$. We write ω_H or ω_L to denote a realization of type H or L. Let $1\{\cdot\}$ be the indicator function, so that Equation 1 becomes

$$\begin{aligned} Y(\omega_\tau) &= \beta_\tau D(\omega_\tau) \\ &= \beta_H D(\omega_H) 1\{\tau = H\} + \beta_L D(\omega_L) 1\{\tau = L\}. \end{aligned}$$

Parents often choose to delay their children's entrance into kindergarten until a year after they are first eligible to enroll, a practice known as redshirting. Section 4 will discuss empirical evidence

[3] Note that the framework described here is the same for both the literature focusing on compulsory schooling laws and the early childhood literature. For example, Angrist and Krueger (1991) uses date of birth and compulsory education laws as a natural experiment to assign one group of students an extra year of education at age 17 or older. The early childhood literature such as Bedard and Dhuey (2006) uses date of birth to assign different levels of schooling to children at the age of a standardized test (6, 7, or older), which is analogous to the age set by a compulsory schooling law.

on redshirting, but for now we will simply assume a latent index model of redshirting, where the latent index is:

$$R^* = f(z, \tau) = \begin{cases} -1 & \text{if } \tau = H \\ 1\{z \in [0, .25]\} - 1\{z \in (.25, 1)\} & \text{if } \tau = L. \end{cases}$$

The observed decision is:

$$R = 1\{R^* \geq 0\},$$

where $R = 1$ indicates that a child has been redshirted. If $\beta_H \neq \beta_L$ then this latent index model is an example of sorting on the gain, which is a model of essential heterogeneity as defined in Heckman et al. (2006). Note that those of type H are never redshirted, while those of type L are redshirted only when they have an assigned relative age between 0 and 0.25.

Remembering that $D_z^*(\omega)$ is assigned educational attainment and $D_z(\omega)$ is observed educational attainment on the sixth birthday, we now have

$$D_z(\omega) = \max\left\{D_z^*(\omega) - R(\omega), 0\right\}. \tag{2}$$

This framework allows us to construct a counterexample to the LATE Monotonicity Assumption (L-2). Let $z = 0$ and $z' = 0.75$. Following Figure 1 and Equation 2, for those of type H, $D_z(\omega_H) - D_{z'}(\omega_H) = 1 - 1/3 = 2/3$, while for those of type L, $D_z(\omega_L) - D_{z'}(\omega_L) = 0 - 1/3 = -1/3$. Thus we have found $(z, z') \in \mathcal{Z} \times \mathcal{Z}$ such that $D_z(\omega) > D_{z'}(\omega)$ for some ω, but $D_z(\omega) < D_{z'}(\omega)$ for some other ω.[4]

Note, as mentioned in Heckman and Vytlacil (2005) and Heckman et al. (2006), that a another name for the monotonicity condition could be "uniformity," as it is a condition across people rather than a condition imposed on any particular individual. That is, the assumption is not that the instrument affects any particular person in any particular way, but rather that the instrument affects all individuals in the same way. It is also important to note that the problem discussed here are distinct from those discussed in Bound and Jaeger (2000), which assumes "the typical compulsory school attendance law requires students to start first grade in the autumn of the calendar year in which they turn 6," precluding the possibility of redshirting. Finally, the violation of monotonicity created by redshirting has been shown independently in Barua and Lang (2009).

4 Empirical Evidence

4.1 Data

We now use the Early Childhood Longitudinal Study, Kindergarten Class of 1998-99 (ECLS-K) data set to illustrate our example empirically. The 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

[4]Specifically, $D_z(\omega_H) > D_{z'}(\omega_H)$ and $D_z(\omega_L) < D_{z'}(\omega_L)$.

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 (2007) from the children, their parents/guardians, teachers, and school administrators.[5]

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 2 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.

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). Consider a sample of children 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, restrict the sample to children living in states which set an entrance cutoff date between August 31st and January 1st.[6] Figure 3a shows the discrepancy in this 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. The analysis in this paper proceeds using school-level entrance cutoff dates, but the large discrepancy

[5]Eighth grade will be the last round of data collection due to sample attrition.
[6]This sample is discussed in greater detail in Aliprantis (2009).

between state and school level data indicates a need to better understand how the selection and enforcement of entrance cutoff dates varies for schools within states.

4.3 Redshirting is Prevalent

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[7], 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.

Examining Tables 1 and 2, note that 27% of children who turned 5 within one month of their state's cutoff date were redshirted, and an additional 4% of such children were delayed in school. While this rate does decline as children become relatively older when first eligible, it is still 13% for children who turned 5 in the 4$^{\text{th}}$ month before their state's cutoff date, and it is 19% for those who turned 5 within three months of their state's cutoff date. Although they cannot speak to older data used in studies such as Angrist and Krueger (1991), these figures suggest that the scenario described in Section 3.2 has become widespread in recent years.

4.4 Redshirting is Correlated with Treatment Effect Heterogeneity

In the ECLS-K data set we observe four groups of children whose birth dates are within one month of their school's cutoff date, as shown in Figure 2:

Cohort M_{13} Children eligible in the fall of 1997 but who chose to wait until the fall of 1998 to enroll.

Cohort M_{12} Children ineligible in the fall of 1997 who waited until the fall 1998 to enroll.

Cohort M_1 Children eligible in the fall of 1998 who chose to enroll.

Cohort M_{-1} Children ineligible in the fall of 1998 but who chose to enroll anyway.

As shown in Table 2, very few parents would choose to delay their child when first eligible if they are in Cohort M_{12}. In contrast, cohort M_{13} is made up entirely of children whose parents chose to delay their enrollment when eligible in 1997, while M_1 is made up entirely of children whose parents chose to enroll them when eligible in 1998. This allows us to exploit the difference in the composition of groups M_{13} and M_1 to obtain information about those children whose parents would choose to delay their enrollment. Tables 5–10 show how these cohorts differ over characteristics correlated with educational outcomes. The children who delayed enrollment were disproportionately wealthy,

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white, male, English-speaking, and had better-educated parents. In addition to the methodological issues they raise in the analysis of adult outcomes when using date of birth as an instrument for educational attainment, Dobkin and Ferreira (2007) find similar evidence on redshirting patterns for children in California and Texas.

5 Implications for Relevant Literature

Angrist and Imbens (1995) discuss the problem of instruments affecting people differently. While the usual solution to this problem is to assume a constant treatment effect, the high correlation between redshirting status and demographic variables (Tables 5–10) suggests heterogeneous treatment effects for those who do and do not redshirt. Nevertheless, since redshirting causes violations of independence and monotonicity, any IV setup using birth date as an instrument for educational attainment must assume a constant treatment effect if redshirting is commonplace and nonrandom. This is relevant to the literature using the Angrist and Krueger (1991) identification strategy to estimate, among other quantities, the effect of schooling on AFQT scores (Neal and Johnson (1996), Cascio and Lewis (2006)), civic participation (Dee (2004), Milligan et al. (2004)), criminal activity (Lochner and Moretti (2004)), mortality (Lleras-Muney (2005)), happiness (Oreopoulos (2007)), and general health outcomes (Adams (2002)); the effect of maternal education on infant health (McCrary and Royer (2009)) and fertility decisions (Black et al. (2004)); the effect of parents' educational attainment on children's educational outcomes (Oreopoulos et al. (2006)); the magnitude of human capital externalities (Acemoglu and Angrist (2000)); and the effects of kindergarten entrance age on educational outcomes (Bedard and Dhuey (2006), Datar (2006), Elder and Lubotsky (2008) and McEwan and Shapiro (2008)).

5.1 Interpreting Parameter Estimates: Angrist and Krueger (1991)

We now consider a stylized version of the identification framework in Angrist and Krueger (1991). While interpretation depends on the exact identification framework used in each application, considering the original framework of Angrist and Krueger (1991) is a useful exercise to help illustrate the implications of Sections 2–4 for interpreting parameter estimates in the literature.

To begin our stylized version of Angrist and Krueger (1991) assume, for the sake of simplicity, that all children born in quarter 1 (Q_1) are born on February 15th, quarter 2 (Q_2) on May 15th, quarter 3 (Q_3) on August 15th, and quarter 4 (Q_4) on November 15th. Assume that school starts on August 15th and the cutoff date for all children to turn six is December 31st of the academic school year. Then Table 11 shows the pattern of ages at entry of first grade we will observe. Table 11 also shows the average age at enrollment for boys in the 1960 Census by quarter of birth (as reported in Angrist and Krueger (1991)), including the difference between the census average and the age predicted by our simple assumptions. Note that there is a large discrepancy in Q_4. This difference could be generated by schools with cutoff dates before January 1st, but it could also be evidence of significant redshirting in the fourth quarter.

Now assume that all children must attend school until they reach 16 years of age and that we are looking only at those children who would like to drop out once they turn 15 years old. Ignoring summer break, Table 12 shows the years of completed schooling at which students are eligible to drop out conditional on redshirting and quarter of birth. The spirit of the identification scheme used in Angrist and Krueger (1991) is that since laws apply to a student's age, and not their years of completed schooling, variation in students' birth dates means they face different laws regarding the years of schooling they must complete. In our example, students born in Q_4 must stay in school until they complete 10.25 years of schooling, while individuals born in Q_1 only have to complete 9.5 years of schooling before being eligible to drop out. Comparing these groups will tell us the treatment effect of interest (ie, the effect of attending an extra year of school against one's will).

Define our instrument as being

$$z = \begin{cases} 1 & \text{if born in } Q_4 \\ 0 & \text{if born in } Q_1. \end{cases}$$

Let S_1 be the years of completed schooling for those with $z = 1$, and S_0 the years of completed schooling for those with $z = 0$. Then if all children enroll when eligible and comply with the compulsory education law, we have from Table 12 that $S_1 = 10.25$ and $S_0 = 9.5$. Thus even with a heterogeneous response to treatment, we have an average of β_H and β_L:

$$\begin{aligned} E[Y_i|z=1] - E[Y_i|z=0] &= Pr(\tau = H)\left\{E[Y_i^H|S_1^H] - E[Y_i^H|S_0]\right\} \\ &\quad + Pr(\tau = L)\left\{E[Y_i^L|S_1^L] - E[Y_i^L|S_0]\right\} \\ &= Pr(\tau = H)\left\{E[Y_i^H|S_0 + 0.75] - E[Y_i^H|S_0]\right\} \\ &\quad + Pr(\tau = L)\left\{E[Y_i^L|S_0 + 0.75] - E[Y_i^L|S_0]\right\} \\ &= Pr(\tau = H)\beta_H + Pr(\tau = L)\beta_L. \end{aligned} \tag{3}$$

However, consider the situation under the latent index model described in Section 3.2. If redshirting is similar to the estimates from Section 4 for the data used in Angrist and Krueger (1991), Equation 3 becomes a weighted average of the effect of receiving more schooling for those of type H and the effect of receiving less schooling for those of type L:

$$\begin{aligned} E[Y_i|z=1] - E[Y_i|z=0] &= Pr(\tau = H)\left\{E[Y_i^H|S_0 + 0.75] - E[Y_i^H|S_0]\right\} \\ &\quad + Pr(\tau = L)\left\{E[Y_i^L|S_0 - 0.25] - E[Y_i^L|S_0]\right\} \\ &= 0.80\left\{E[Y_i^H|S_0 + 0.75] - E[Y_i^H|S_0]\right\} \\ &\quad + 0.20\left\{E[Y_i^L|S_0 - 0.25] - E[Y_i^L|S_0]\right\}. \end{aligned} \tag{4}$$

The estimates in Equations 3 and 4 have very different interpretations, showing the importance of accounting for essential heterogeneity when estimating and interpreting parameters of interest.

6 Appendix: Independence and Monotonicity in the Framework of Angrist and Imbens (1995)

Consider S_Z to be years of completed schooling by the sixth birthday, with Z assigned age. Let z be a person's true assigned age, and z' be a counterfactual assigned age. Again, if we consider $z = 0$ and $z' = .75$, then $S_z \in \{0, 1\}$, whereas $S_{z'} \in \{0, \frac{1}{3}\}$. A key assumption in Angrist and Imbens (1995) is that "S_Z is assumed to exist for each value of Z for each person, even though only one S_Z is observed" (p 434). While this assumption is correct, it illustrates the key problem with the identification scheme: S_Z changes as the value of Z changes. That is, changing to a counterfactual assigned age also changes the counterfactual treatment received.

The discussion of Monotonicity is similar. Let S_z be the number of years of school completed by a student conditional on their assigned relative age z. If $z = 1$ indicates having assigned relative age of 0 and $z' = 0$ indicates having an assigned relative age of 0.75, then $S_1^H > S_0^H$, but $S_1^L < S_0^L$. This contradicts the assumption of monotonicity.

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Figures

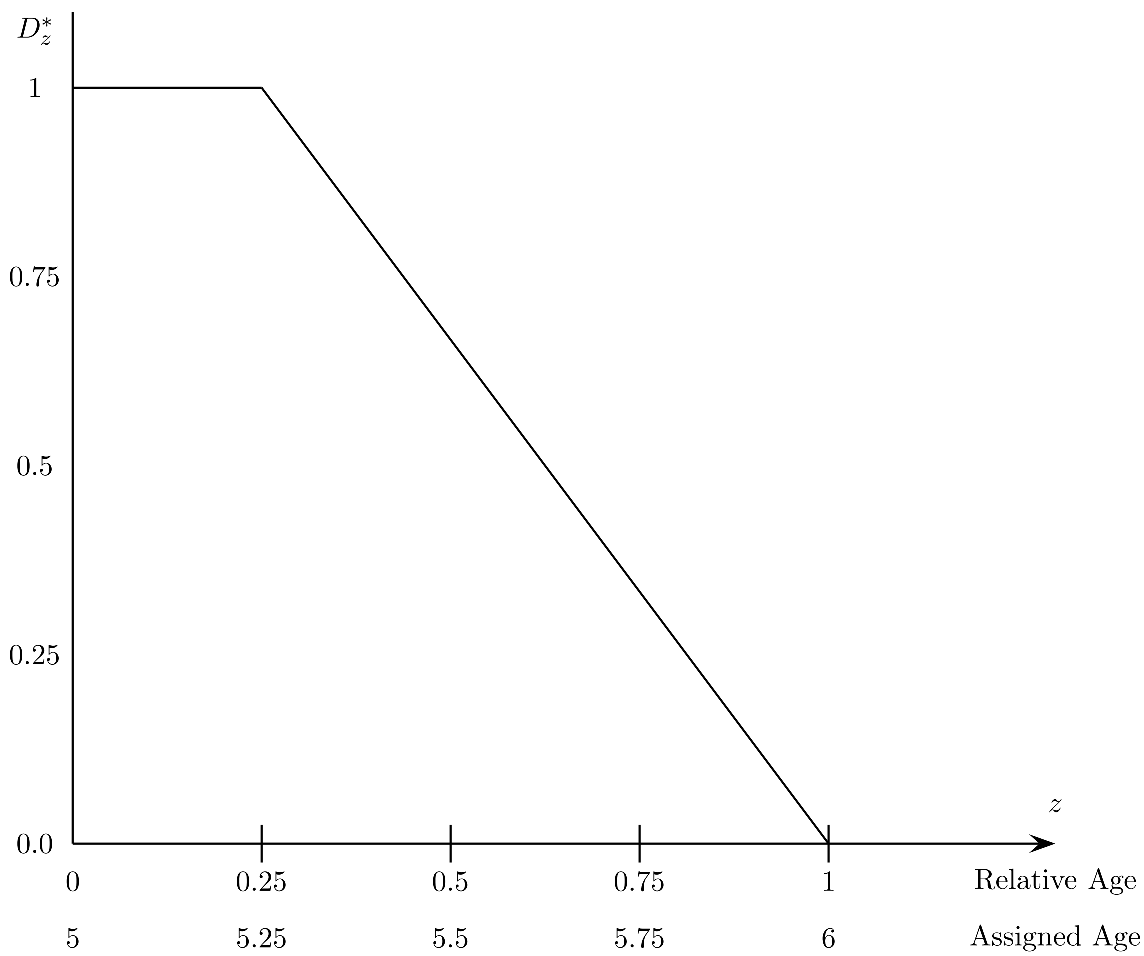
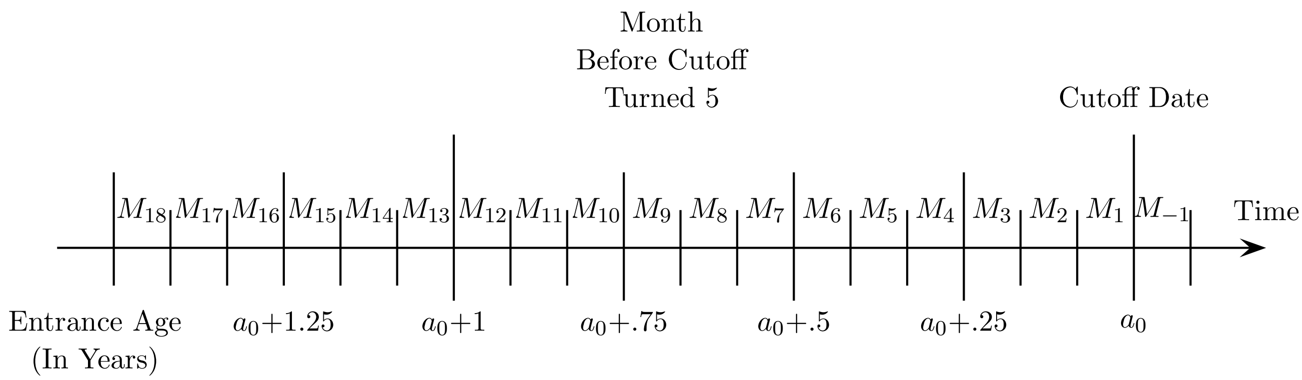
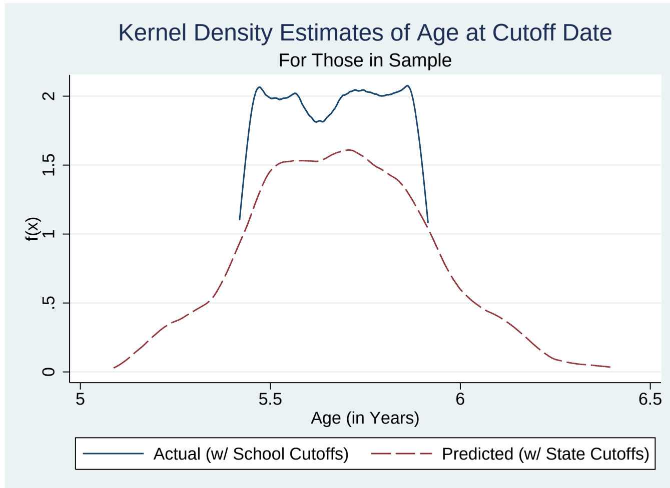


Figure 1: Assigned Treatment Intensity (D_z^*) as a Function of Birth Date (z)

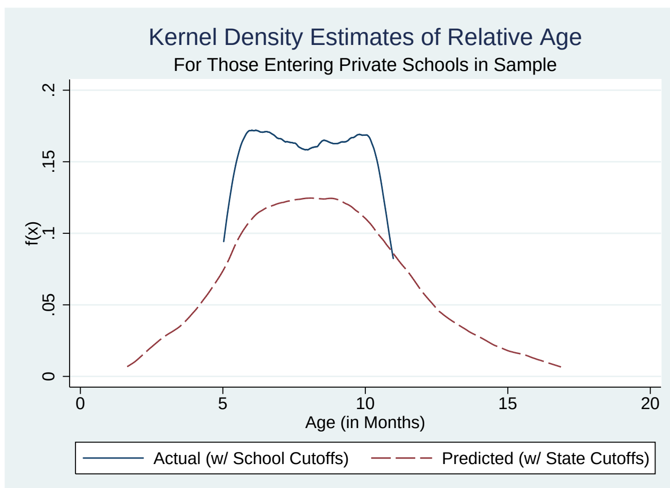


Month
Before Cutoff
Turned 5
Cutoff Date
M_{18} M_{17} M_{16} M_{15} M_{14} M_{13} M_{12} M_{11} M_{10} M_9 M_8 M_7 M_6 M_5 M_4 M_3 M_2 M_1 M_{-1}
Time
Entrance Age
(In Years)
a_0+1.25
a_0+1
a_0+.75
a_0+.5
a_0+.25
a_0

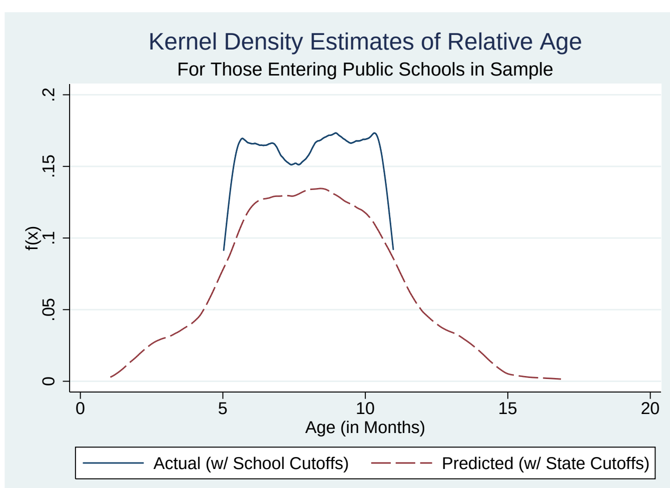
Figure 2



(a) Overall



(b) Private



(c) Public

Figure 3: Sorting

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: Descriptive Statistics by Cohort

Different Variables Summarized by Cohort						
Variable	Statistic	ECLS-K	M_{13}	M_{12}	M_1	M_{-1}
Income	Mean	52,946	61,098	52,456	48,531	61,912
	Std. Dev.	36,375	40,389	34,959	34,466	40,146
	n	12,326	241	702	619	57
# of Books	Mean	74.35	86.89	75.11	69.74	76.84
	Std. Dev.	59.87	60.64	57.57	57.45	65.18
	n	14,910	300	849	744	63
# of Children's Records, Tapes, or CD's	Mean	15.37	18.26	16.09	13.99	17.70
	Std. Dev.	17.86	20.33	18.81	17.42	17.68
	n	15,008	303	851	750	63
Age at entry to K (in months)	Mean	65.56	68.34	70.63	60.07	62.00
	Std. Dev.	4.26	5.53	2.91	1.67	5.25
	n	15,042	302	851	751	63
Mother's Age (At First Birth)	Mean	23.98	25.26	23.93	23.64	24.12
	Std. Dev.	5.44	5.74	5.58	5.37	5.49
	n	14,054	288	793	696	57
Cost Nonparental Childcare (Pre-K, in $)	Mean	43.82	51.43	41.50	39.74	59.38
	Std. Dev.	51.72	49.18	46.17	44.66	77.77
	n	11,465	223	653	559	54
Age at first Nonparental Care (in Months)	Mean	22.28	24.08	22.80	20.69	17.71
	Std. Dev.	19.59	20.95	20.72	18.24	17.71
	n	13,047	266	754	637	58
Hours Spent in Nonparental Care (Pre-K)	Mean	25.23	22.05	26.66	25.33	27.08
	Std. Dev.	21.73	19.79	23.31	22.15	20.32
	n	14,858	298	839	744	63

Table 6: WIC Benefits

Did the Child Receive any WIC Benefits as a Child (in %)					
	ECLS-K	M_{13}	M_{12}	M_1	M_{-1}
Yes	42.88	31.35	40.66	47.87	39.68
No	56.00	67.66	58.05	51.06	60.32
n	15,064	303	851	752	63

Table 7: Child's Language to Mother

Language to Mother in %					
Speaks Non-English Language	ECLS-K	M_{13}	M_{12}	M_1	M_{-1}
Never	79.53	93.07	80.85	79.26	77.78
Sometimes, Often, or Very Often	18.53	5.28	16.93	18.36	17.46
n	15,064	303	851	752	63

Table 8: Race

The Composition of Cohorts by Race (in %)					
Race	ECLS-K	M_{13}	M_{12}	M_1	M_{-1}
White, Non-Hispanic	56.31	75.29	60.95	53.94	63.01
Black or African-American, Non-Hispanic	14.20	9.30	13.18	13.87	12.33
Hispanic, Race Specified	8.52	3.49	7.51	6.58	1.37
Hispanic, Race Not Specified	8.91	5.52	7.71	11.40	4.11
Asian	6.35	2.03	5.68	7.17	15.07
Native Hawaiian or Other Pacific Islander	1.14	–	1.12	2.47	–
Native American or Native Alaskan	1.80	1.74	1.01	2.00	1.37
More than one race, Non-Hispanic	2.55	2.62	2.64	2.59	2.74
n	17,527	344	984	851	73

Table 9: Gender

The Composition of Cohorts by Gender (in %)					
Gender	ECLS-K	M_{13}	M_{12}	M_1	M_{-1}
Male	51.15	63.66	53.25	49.82	32.88
Female	48.78	36.34	46.75	50.18	67.12
n	17,554	344	986	851	73

Table 10: Parent's Highest Education Level

Parent's Highest Education Level by Cohort in %					
	ECLS-K	M_{13}	M_{12}	M_1	M_{-1}
8th Grade or Below	3.09	0.95	2.76	3.05	1.43
9th-12th Grade	6.55	3.15	6.62	6.62	4.29
High School Diploma/Equivalent	23.80	17.67	25.69	27.61	21.43
Voc/Tech Program	6.77	5.36	8.27	7.12	4.29
Some College	26.50	27.13	25.69	27.86	28.57
Bachelor's Degree	18.53	26.81	16.43	17.05	21.43
Grad/Professional School - No Degree	2.56	3.15	2.65	1.65	2.86
Master's Degree (MA, MS)	7.50	7.89	6.73	6.49	12.86
Doctorate or Professional Degree	4.71	7.89	5.18	2.54	2.86
n	15,961	317	907	786	70

Table 11: Entrance Age (in Years) by Quarter of Birth

Entrance Age (in Years)				
Entry	Q_1	Q_2	Q_3	Q_4
When Eligible	6.5	6.25	6	5.75
Redshirted	7.5	7.25	7	6.75
1960 Census Avg	6.45	6.28	6.08	6.07
'60 – Eligible	-0.05	0.03	-0.08	-0.32

Table 12: Years of Schooling Completed before Eligible to Drop Out by Quarter of Birth

Years of Schooling Completed				
Entry	Q_1	Q_2	Q_3	Q_4
When Eligible	9.5	9.75	10	10.25
Redshirted	8.5	8.75	9	9.25