Does the Age that Children Start Kindergarten Matter?
Evidence of Long-Term Educational and Social Outcomes

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ABSTRACT

This study uses data from the National Education Longitudinal Survey to examine long-term effects of age at school entry on human capital accumulation. Recent studies suggest that individuals with summer birthdates, who are assumed to be younger at school entry, gain more education on average (Angrist & Krueger 1991; Mayer & Knutson 1999), while other research suggests that younger students perform worse on average in early elementary school (Stipek 2002). At present, little is known about the impact of age at school entry on education attainment as youths transition from high school into college and the labor market. This study focuses special attention on those students who enter kindergarten a year later than their peers. The results of this study suggest that delaying kindergarten does not create any long-term advantages for students, and that younger students may have a small advantage in human capital accumulation.

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KEYWORDS

economics of education, human capital

INTRODUCTION

Angrist & Krueger (1991) suggest that compulsory attendance laws allow students who start school older to dropout sooner, because they reach the legal age for dropping out before their younger peers. However, compulsory attendance laws only define the minimum age when students can leave school. The minimum age to begin school is typically just a guideline. In most states a child can enroll in kindergarten if she reaches her 5th birthday by the summer before the school year begins (Saluja, Scott-Little & Clifford 2000). Parents generally have the option to delay kindergarten an extra year.   Using historical U.S. Census data, Angrist & Krueger assume that individuals with summer births started school younger than individuals with winter births, but this is not necessarily true today. Parental preferences and school policies increasingly favor waiting an extra year for kindergarten – particularly for students with summer birthdates who would enter school as the youngest kindergarteners (Crosser 1998; Katz 2000; Stipek 2002). School entry is delayed in the hopes of improving academic or athletic opportunities for children (Katz 2000), but if Angrist & Krueger are correct, waiting an extra year may make students more likely to drop out of school thus reducing life time earnings.

Given the limitations of Angrist & Krueger's data, little is known about how age at school entry affects critical transitions that influence human capital accumulation. This study uses the National Education Longitudinal Survey (NELS) to identify age at kindergarten entry and test the effect of age at school entry on a number of academic, social, and labor market outcomes. First, we estimate the probability that a student will repeat a grade -- because grade retention also adds an extra year before graduation. Then, age at school entry, interacted with grade retention, is used to estimate of the probability of dropping out of school, having an out of wedlock birth, and attending college. We also test the effect of age at school entry on wages in early adulthood (approximately age 25). Finally, a semi-parametric matching procedure is used to compare outcomes for students with similar backgrounds who started kindergarten at different ages.

The results suggest that younger students are more likely to repeat a grade, which does have negative academic consequences. However, controlling for grade retention and interactions, starting school young does have modest advantages in terms of lifetime accumulation of human capital. The practice of delaying kindergarten does not appear to create any long-term advantages for students.

THEORETICAL BACKGROUND

The groundbreaking study by Angrist & Krueger suggests that compulsory education policies affect individuals differently based on season of birth (Angrist & Krueger 1991). Compulsory education laws differ by state, but typically children are able to enter kindergarten if they turn 5 by the summer before school begins (Saluja, Scott-Little et al. 2000). Under this structure, students with summer birthdates enter school at age 5, and students with winter birthdates enter school at age 5 ½. Minimum age requirements for dropping out of school imply that students with winter birthdates reach the dropout age having acquired approximately six months less time in school. Angrist & Krueger and Mayer & Knutson (1999) find that this difference translates to lower wages on average for individuals with winter birthdates. Based on these studies alone, it appears that starting school younger has lifelong advantages in terms of human capital accumulation.

In recent years, a competing approach to age at school entry has emerged from child development literature. Increasingly parents voluntarily keep children out of kindergarten until age 6 in order to gain an academic advantage. This practice has adopted the name “redshirting” after the older practice of holding freshman athletes back a year to gain physical strength (Katz 2000). The underlying assumption of redshirting is that an extra year of growth before kindergarten will make a child better able to learn, compete, and succeed in school (Ilg, Ames, Haines & Gillepspie 1978; Uphoff & Gilmore 1986; Elkind 1987). From the school’s perspective, primary school teachers may prefer older students who have more maturity and pre-school preparation. With rising concerns about school accountability and standardized testing as early as second grade, schools and school districts may see redshirting younger students as a low cost strategy to increase test scores (Karweit 1988; Meisels 1992; Crosser 1998). There is some evidence that redshirting is an unofficial policy for some school districts, where parents of incoming kindergarteners are pressured to wait an extra year to enroll children with summer birthdates (Shepard & Smith 1986). Other school districts have made the policy official by moving the cut-off date for kindergarten to exclude 5 year-olds with summer birthdates from kindergarten classes (Stipek 2002).

A conflicting perspective to this child development literature argues that it is beneficial for students to enter kindergarten at an early age. Students may gain cognitive skills just from being in a school environment, even if they are the youngest students in the class (Piaget 1970; Vygotsky 1978; Meisels 1992). Mayer & Knutson argue that the head start from early schooling can have lifelong benefits in terms of human capital accumulation (Mayer & Knutson 1999).

Today's schools also provide other valuable services – such as nutrition, psychological counseling, and remediation for learning disabilities – that can provide bigger benefits if interventions occur at an early age (Karweit 1988; May, Kundert, Nickoloff, Welch, Garrett & Brent 1994; May & Kundert 1997). Particularly if a child is in need of these services, school attendance may improve learning in the long-run.

Discussions of age at school entry typically focus on how student achievement and socialization is affected in early primary grades, but it is possible that greater differences emerge in high school. Older students reach puberty before their peers, and they may feel awkward about being bigger and more physically mature (Crosser 1998). Older female students have more years of fertility in school and therefore may be more likely to experience a teenage pregnancy. And as Angrist & Krueger point out, older students can drop out of school with fewer years of education. Alternately, younger student may suffer from being physically and emotionally immature (Uphoff & Gilmore 1986). Being young and small may preclude involvement in varsity sports and other activities. If academic advantages for older students in primary school continue into secondary school, younger students may be less competitive in high school and less likely to attend college.

A potentially intervening step in the process concerns whether students repeat a grade in elementary or secondary school. Like redshirting, grade retention implies that a student will have lower educational attainment when he reaches the minimum age for dropping out. Grade retention is widely believed to have negative consequences. Empirical studies show that students who repeat a grade are more likely to have future academic and behavioral problems and are more likely to drop out of school (Holmes 1989; Shepard & Smith 1990; Hauser 2000). The theoretical affect of age at school entry on retention is ambiguous. Younger students may be retained because they are perceived as immature or are not as academically able as their older peers (Shepard & Smith 1988). Older students may be retained because they are perceived as slow learners from the start. Therefore, estimation of long-term educational attainment should control for both grade retention and age at school entry.

PREVIOUS EVIDENCE

There are many studies that test the early effects of age at school entry on student achievement in a single school or school district. The results, which are not generalizable to the population as whole, are ambiguous. Generally, studies find that younger kindergarteners have an academic disadvantage (Carter 1956; Miller 1957; Green & Simmons 1962; Dickinson & Larson 1963; Hall 1963; Davis, Trimble & Vincent 1980; Langer, Kalk & Searls 1984; Shepard & Smith 1987; Sweetland & De Simone 1987; Cameron & Wilson 1990; Jones & Mandeville 1990; Bickel, Zigmond & Strayhorn 1991; Crosser 1991; McClelland, Morrison & Holmes 2000; Stipek & Byler 2001; Datar 2003). However, longer range studies show this gap shrinking in upper elementary school years (Miller 1957; Davis, Trimble et al. 1980; Langer, Kalk et al. 1984; Jones & Mandeville 1990; Bickel, Zigmond et al. 1991; Crosser 1991). A recent review of studies of age at school entry concludes that any achievement gap closes by the third grade (Stipek 2002).

Using census data, Angrist & Krueger (1991) find that men with summer birthdates have slightly higher earnings than men with winter birthdates, and this result is replicated by Mayer & Knutson (1999). These authors use season of birth as an instrumental variable to predict earnings. However, Bound & Jaeger (1996) argue that season of birth is not a valid instrument. Although the theoretical link is unclear, these authors cite a group of studies linking season of birth with variables that effect wages and education such as race, mental retardation, and mental illness.

Season of birth is most likely correlated with redshirting. Studies show that children with birthdates close to the kindergarten cut-off are more likely to be redshirted (Cosden, Zimmer & Tuss 1993; Brent, May & Kundert 1996; Stipek 2002). Redshirting is also correlated with individual and family characteristics that influence educational attainment. Overall, male students are most likely to be redshirted (Cosden, Zimmer et al. 1993; Brent, May et al. 1996). The effect of age and SES is unclear. There is some evidence that parents who redshirt are more likely to be white (Cosden, Zimmer et al. 1993; NCES 1997) and have higher income (Meisels 1992). High-income parents are more likely to have resources for an extra year of pre-school or home care before kindergarten. However, some education researchers are concerned that schools focused on test scores will recommend redshirting to parents of less advantaged or potentially lower achieving students who are less likely to access high quality preschool during the extra year before kindergarten (Shepard & Smith 1988; Stipek & Byler 2001; Stipek 2002). The issue of who is redshirted is an important empirical issue. If relatively advantaged students who would have performed well anyway are more likely to be redshirted, average outcomes will overstate the benefits of redshirting. If relatively disadvantaged students are more likely to be redshirted, average outcomes may understate the benefits of redshirting.

METHODOLOGY

A key concern in isolating the effect of age at school entry on long-term outcomes is that decisions about school entry may be correlated with unobservable family and child characteristics that also influence educational attainment. This is problematic if the same characteristics are associated with redshirting and educational outcomes. As Angrist & Krueger (1991) discuss, season of birth should be unrelated to factors that influence both outcomes and family background. If this assumption is correct, we can interpret an estimate of the effect of season of birth on student outcomes as strictly an age effect that is not contaminated by any correlation with unmeasured family background. Our first models make these assumptions.

First, we make a simple comparison of the younger and older group. These students were not redshirted, but differ in age simply because of their season of birth. This provides an assessment of whether there are long-term effects of being younger at school entry due strictly to season of birth. The models used are estimated with a standard probit regression if the long-term outcomes are dichotomous (e.g., high school completion, out-of-wedlock fertility, college attendance, etc.) and with Ordinary Least Squares (OLS) if the long-term outcomes are continuous (test scores, earnings, etc.)

Next, we estimate a second model to determine the effect of redshirting on long-term outcomes. Here, we compare children with summer birthdates who entered kindergarten at age five (the young group) and age six (the redshirted group). This provides an evaluation of the effects of being older in school due to delayed kindergarten entry.

Semi-Parametric Matching

The probit and OLS models are based on the assumption that the sample of students with summer birthdates that begin kindergarten at age 5 is not systematically different from the sample of students that begin kindergarten at age 6. This assumption is not likely to hold as judgments about redshirting are typically endogenous to a child's individual characteristics. There are two approaches in the literature to help overcome this selection problem. The first is to find an instrumental variable that is correlated with the likelihood of redshirting a student, but is not correlated with the outcome variable. In her study of the impact of age of kindergarten entry on early elementary achievement, Datar (2003) uses state laws concerning age at kindergarten entry as an instrumental variable that is endogenous to age at school entry but exogenous to achievement. This strategy did have a small impact on the results of the study, but it cannot be replicated with NELS data because we do not know what state the participants lived in during kindergarten. In addition, it is not clear that state cutoff dates are exogenous to achievement. States can purposely move up the cut-off date in an attempt to increase average elementary achievement (Stipek 2002).

An alternative approach is to deal with selection on observables by employing a matching method. While this methodology does not adjust directly for selection on unobservables, we are able to control for some of these issues by placing restrictions on the matches. We apply a variant of the method proposed by Rosenbaum and Rubin (1983) that imposes weaker assumptions on the functional form of the estimated equation. In particular, the functional form of the probit equation places restrictions on the estimation that may smooth over important differences between redshirted and young students. In this method, Rosenbaum and Rubin (1983) suggest using a propensity score to make matching feasible. The propensity score is a student's estimated probability of receiving the treatment – in this case being redshirted – given their observable characteristics. Rosenbaum and Rubin prove that matching on the propensity score provides as powerful a control as matching on all observable characteristics. This technique reduces the problem of matching on a large number of characteristics by matching on only one dimension – the propensity score for delayed kindergarten entry.[1]

In estimating the effect of delayed kindergarten entry, we permitted a single young student to match more than one redshirted student. This method minimizes the distance between treatments and their controls, but at the possible loss of some efficiency. Dehejia and Wahba (2002) find that in their sample this nearest-match algorithm performed better than algorithms that permit several "fairly near" controls to match a single treatment. In our study, only five percent of control observations were used in multiple matches.

We are able to improve on the efficiency of the matching estimator by matching within subgroups of the population to control for unobservable characteristics that may influence both redshirting and long-term outcomes. In the context of labor markets, Heckman, Ichimura, and Todd (1997) note the increased efficiency of matching within the same region, and Levine and

Painter (2003) show a similar increase in efficiency when estimating the effect of out-of-wedlock fertility. In the estimation presented below, we match by gender, by race, by urban vs. rural residency, and by two-parent vs. nontraditional families to capture some influences that are likely to be common across groups, but not an observable part of family background. To evaluate if redshirting is an effective way to prevent grade retention, we tested additional matches based on grade retention in kindergarten and grades 1-8, and the predicted probability of repeating a grade.

DATA

Unfortunately, there is no longitudinal data set, at present, which tracks students from kindergarten through young adulthood. Data for this study are drawn from the National Educational Longitudinal Study of 1988 (NELS), which tracks a cohort of eighth graders through high school and young adulthood. This dataset was selected because it follows a national sample of students through junior high, high school, college, and young adulthood – allowing for a rich selection of outcomes during this time-frame.

The NELS is sponsored by the National Center for Education Statistics (NCES) and carried out by the National Opinion Research Center. The data were drawn from a sample of 1,000 schools (800 public schools and 200 private schools, including parochial institutions). From this school sample, 25,000 eighth grade students were selected for the initial survey. Two follow-up surveys revisited the majority of the sampled students in 1990 and 1992, when the students were typically in tenth grade and twelfth grade. A third follow-up in 1994 included a randomized sample of 15,964 of the original student participants. A fourth follow-up was conducted in 2000 and included 15,273 students from the third follow-up. At the time of the fourth follow-up, the participants were approximately twenty-six years old and had been participating in the study for twelve years.

For each NELS participant, base year data were collected from students, parents, and schools to provide a rich variety of information about individual characteristics, school experiences, and family environments. Educational histories provide background data on school experiences leading up to eighth grade, such as repeating or skipping grades, and changing schools due to a move or other reason. School, parent, and student surveys from the first and second follow-ups provide rich data about student experiences during junior high and high school. However, other variables, such as family characteristics, are only available beginning in eighth grade. The problems associated with this data limitation are discussed later.

Long-term Outcomes

Our goal is to examine a small set of long-term outcomes that reflect students' progressive accumulation of human capital as they transition from adolescence to young adulthood. We selected eighth grade achievement test scores, having a child out of wedlock, dropping out of high school, entering college, and wages in 1999. These five outcomes represent important benchmarks towards labor market participation and address theoretical concerns about the pros and cons of redshirting.

Eighth grade achievement is measured as a composite score of reading and math tests administered to all NELS participants (see Levine and Painter (1999) for a full description of

NELS cognitive tests). The composite score is standardized with a mean equal to zero and standard deviation equal to one. Child out of wedlock is measured for females only, if a birth occurred during a student's high school years. Dropout is measured as not having a diploma or GED by age 20, and college enrollment is measured as entering a two- or four-year college by age 20. Wage data are available for the year 1999, when survey participants are approximately 25 years old. Wages were estimated for individuals who were working full-time and not enrolled in school, so the effects of graduate and professional school are partially excluded.

Additional variables were also tested based on theories of the influence of age at school entry on student outcomes to confirm the results on the main outcome variables. Additional academic outcomes include 10th and 12th grade test scores, college graduation, and repeating a grade in kindergarten or later elementary school. Social experiences at school were measured by student- and school-reported experiences. Survey items reported whether students felt put down by teachers and other students in 10th grade. Social interaction was also measured by variables representing participation in varsity sports and other school activities. Finally, behavior inside outside school was measured by whether the student was reported by teachers to have a behavioral problem in high school and whether the student was ever arrested through high school. Variables relating to in-school experiences are not available for students who dropped out of school, so some observations are lost for these additional outcome variables.

Independent Variables

We assume that educational outcomes are correlated with age at school entry, observable characteristics, and unobservable characteristics such as skills, motivation, and intelligence.

Compared with most past studies, this study employs far more detailed measures of family background and family involvement in education to control for observable characteristics. Family variables enable us to identify factors related to educational and social success that are also correlated with parent and school decisions about when a child enters kindergarten. Variables were selected because theories of redshirting indicate that they are correlated with redshirting and with long-term educational outcomes.

In their study of the effect of early school experience on dropout rates, Alexander, Entwisle, & Horsey (1997) provide a useful framework for organizing child and family variables that may affect redshirting and long-term outcomes. These authors examine how the original transition from home to school eventually influences the decision to drop out in terms of four factors: 1) socioeconomic status and background including income, race, and family structure; 2) family context including family stresses, parents' values, and parents' socialization; 3) personal resources including the child's attitudes and behaviors toward school; and 4) school experiences including achievement, tracking, and retention. Following this framework, we selected a rich set of control variables similar to those used by Alexander, Entwisle, and Horsey. Variables representing a student's attitude in kindergarten were not available in the NELS data, so the 'personal resources' category was excluded.[2]

Socioeconomic status and background variables describe the student's economic and family environment, which can influence readiness for kindergarten and future educational achievement. This set of variables includes occupational status (using Duncan's index), parental education, and family income. These variables were converted into z-scores with mean zero and standard deviation equal to one. When there were missing values for parental education because of a missing parent, these were given a z-score of 0 and categorical variables were included to note these important missing values.[3]

With limited information about student abilities, parents and teachers use demographic characteristics to make decisions about redshirting and grade retention (Hauser 2000; Stipek 2002). Socioeconomic variables include demographic characteristics: region, rural vs. urban/suburban, and a categorical variable for gender. Also included are categorical variables for whether the student, mother, and father were born in the United States. The students' position in a family is measured by the number siblings and whether the student is the oldest child.

Family structure can also influence redshirting and achievement. Ideally, we would have information about family structure at kindergarten when parents chose to enroll or delay kindergarten. This data is not available because the NELS began in eighth grade. We use eighth grade characteristics as the closest proxy for kindergarten characteristics. The six family structures were intact families, single parent families with either the biological mother present or the father present, step-families with either the biological mother or father present, and those families with no biological parent present. The other family structures can tell us something about whether a student experienced a divorce or remarriage, but not the number of such transitions. Finally, a categorical variable indicating whether the student was the child of a teenage mother was also included. Unfortunately, the dataset does not indicate whether the parents were married when the child was born.

Family context variables identify stresses, attitudes, and family socialization process that influence educational attainment (Alexander, Entwisle et al. 1997). From the parent questionnaire, indicators were obtained for whether the family was one of five religions and any of four levels of religious observance. These variables may proxy for the social capital available to the children (Coleman 1990). Two variables represent parents' involvement and expectations for the child's future. The parent involvement variable is equal to one if the parent belonged to a parent-teacher association or related organization or volunteered at school. The second variable is a measure of the parents' expectations for the child. It takes the value one if parents expected the student to obtain education levels beyond high school. To measure the learning environment at home, variables are also included that indicate whether the home had a library card, magazines, and many books. Finally, a variable for changing school during junior high or high school is added to represent the effect of discontinuity in education.

The effect of age at school entry is sometimes believed to be largely a result of its effect on student attitudes about school (Uphoff & Gilmore 1986). We included three variables – repeating kindergarten, repeating grades 1-8, and participating in an ESL program – to reflect school experiences that may positively or negatively affect student attitudes.[4] Research shows that repeating a grade has strong, negative effects of achievement and socialization (Holmes 1989; Hauser 2000). The effect of ESL is unknown, but the need for special language assistance may increase the likelihood of repeating a grade.

One drawback of the NELS data is that many background characteristics are only recorded from eighth grade forward. We are limited to using family histories from eighth grade as a proxy for family characteristics when children entered kindergarten. This influences variables representing socioeconomic status and background. Family income, parents' education and occupational status, location of residence, and family structure are all measured during 8th grade. Similarly, variables measuring parent expectations and participation in and support for children's education were measured during the eighth grade. For many families, especially those families that experience no change in family structure, family characteristics in 8th are likely a good proxy for the characteristics of households when the child entered kindergarten. In addition, we are able to identify certain experiences such as changing schools, repeating a grade, and participating in ESL programs from historical information gathered in the 1988 survey.

Determining Age at School Entry

The variable of interest in this study is age at school entry. This data was constructed by tracing students' lives backwards in order to identify the age a student began kindergarten. Month and year of birth give the students' age in eighth grade. Combined with data about the number of grade retentions, we can identify children by their season of birth and age at school entry.[5]

All students in the NELS were in eighth grade in 1988. We identified three groups of students for analysis. State cut-off dates for kindergarten vary, but most states require students to turn five between September and December of the year they enter kindergarten (Saluja, Scott-Little et al. 2000). Because of the variation across states, there is ambiguity about whether children born in autumn were held back due to redshirting or state cut-off dates. To avoid this problem, we limited our analysis to comparison of students with summer birthdates and winter birthdates. Research shows that of these students, children with summer birthdates are more likely to be redshirted (Brent, May et al. 1996).

The first group of students have summer birthdates and entered kindergarten soon after they turned five years old. For simplicity, we call label this group “young at school entry.”[6] The second group of students have winter birthdates and entered kindergarten when they were approximately five-and-a-half years old. This group is labeled “older at school entry.”[7] The third group of students has summer birthdates and started school one year late. These students are label “redshirted” to reflect that kindergarten entry was intentionally delayed.[8] Redshirted students entered school soon after they turned six years old. Figure 1 illustrates how survey participants were placed in the young, older, and redshirted groups based on season and year of birth and school experiences.

RESULTS

Family Characteristics

First, we examine the difference in characteristics between the young, older, and redshirted children. ‘Young’ children were approximately five years old, ‘older’ children were approximately five and one half years old, and ‘redshirted’ children were approximately six years old, when they entered kindergarten. By limiting the data set to summer and winter births, the number of observations is reduced. There are 2,535 observations in the ‘older’ group of winter births who started kindergarten at age 5 ½. There are a total of 3,021 observations with summer birthdates. Of the summer births, 2,794 are in the ‘young’ group and 227 were redshirted. This translates to a 9 percent redshirting rate for students with summer birthdates, which is similar to redshirting rates in other studies (Stipek 2002).

Table 1 shows the mean value of the independent variables for each group. The young and older groups have very similar observable characteristics. They have similar family structure, religiosity, family size, gender, and rates of native U.S. birth. Parent education and family income are slightly higher for the young children, but these differences are less than one standard error.

The redshirted group appears to be less advantaged than the young and older groups. These students are more likely to have nontraditional family structures and to have been born outside of the U.S. They are also more likely to be male and either Latino or Asian. Redshirted students also are less likely to have parents who expect them to attend college and who participate at school.

This comparison indicates that in the NELS, students with fewer advantages were delayed presumably because they were considered less likely to succeed if they were they youngest in the class. This differs from some studies of individual school districts where relatively privileged students were more likely to be redshirted (Stipek 2002). Students in the NELS may have been redshirted due to primarily to perceived academic disadvantages. Thus, in this study redshirted students may be more likely to have low educational attainment.

Long-term Outcomes

Table 2 tabulates the outcome variables for the young, older, and redshirted groups. With respect to long-term outcomes, young students have better outcomes on average than redshirted students. Young and older students are strikingly similar, with few significant differences in average outcomes. Redshirted students have slightly lower test scores, and are twice as likely to drop out as young students. Dropout is a particularly important issue for redshirted girls in this data set; one in five redshirted girls (not shown) ends up dropping out of school. Young and older students are more likely that redshirted students to attend college and graduate and earn higher salaries at age twenty-five. Most social outcomes show no statistically significant differences by age at school entry. We do observe that young students are the least likely to have behavioral problems in high school, and that redshirted students are the most likely to be arrested.

Young students are most likely to repeat kindergarten, presumably as a remedy for being immature or unready for kindergarten the first time. This supports previous studies showing an academic advantage for redshirted students in early grades. However, as in previous studies, this advantage does not persist. Redshirted students are more likely to repeat grades 1-8. This difference is most striking for redshirted girls (not shown), who are twice as likely to repeat as are girls in the young or older groups.

Regression Results

These observed differences in educational outcomes for students may be due to child and family characteristics rather than age at school entry. The regression models control for observable child and family characteristics. In Model 1, we compare students in the young and older groups, controlling for family background, family context, and school experiences. These students were not redshirted and differ only in season of birth. The variable *young* is a categorical variable for students in the young group. Because grade retention is thought to cause future academic and social problems, young students may suffer in the long-term due to the effects of retention rather than the effects of age differences. To control for this effect, we included repeating kindergarten and repeating grades 1-8 as independent variables in the regressions on long-term outcomes. In addition, interaction terms were included to capture the combined effect of being younger and repeating a grade. The results for key variables are summarized in Table 4, with the full estimation results in Appendix 1. The control variables have the expected effect on education and social success. Higher socioeconomic status and a stable family context are positively associated with achievement. Negative school experiences, represented by variables for repeating kindergarten and grades 1-8, have a negative effect on academic.

Controlling for grade retention, we found that young students have better opportunities to acquire human capital. The two groups were equally likely to drop out of school, but the interaction terms between repeating and being young show that young students who repeated a grade were significantly less likely to drop out than older students who repeated ($p<0.01$), which may be due to compulsory attendance laws. Consistent with this finding, young students were more likely to attend college ($p<0.05$) and have higher wages ($p<0.05$). Age at school entry did not have the same effect on of out-of-wedlock births. Despite six months less fertility in school, young and older students were equally likely to have an out-of-wedlock birth. Young and older students had similar eighth grade achievement.

The second regression model (Model 2) tests the effect of being redshirted on students with summer birthdates. Here we compare students in the young and redshirted groups. Both groups were born in summer, but the redshirted group delayed kindergarten entry until age six. Once again, the variable *young* reflects the effect of being in the youngest group. Overall, the results suggest no difference in outcomes between those that redshirt and those that begin kindergarten at age 5 across the five key outcomes (Table 4; Appendix 2). Controlling for child and family characteristics, young and redshirted students are equally likely to dropout, have a child out of wedlock, and attend college. Wages are greater for young students, but the difference is not statistically significant. Unlike estimates from Model 1, the variables that control for repeating a grade were not as consistently important.[9] The observed differences in average achievement for young and redshirted students disappear when we control for observable characteristics.

Additional outcomes

Examining additional outcomes, there is evidence some that young students actually gain an academic advantage in high school (Table 5). When compared to the *older* group, even though there was no significant effect of *young* on eighth grade test scores, young students had significantly higher test scores in tenth grade ($p<0.001$) and twelfth grade ($p<0.05$). Consistent with the result for college enrollment, young students were more likely to earn a bachelors degree. There is no evidence that summer birth students have a social disadvantage compared with older students. The *young* variable was not a significant predictor of any social outcome variable.

At the same time, young students were significantly more likely to repeat kindergarten ($p<0.001$) and grades 1-8 ($p<0.001$), indicating that being younger due to season of birth increases the likelihood of grade retention. This confirms findings from the literature that the younger students have lower achievement in elementary school.

When comparing young and redshirted students on these additional outcomes, the only significant effect is that tenth grade test scores are higher for younger students. The largest advantage for older students appears to be participation in varsity sports, but these results are not significant at the 10 percent level. As was the case when comparing the young students to the older ones, young students were significantly more likely to repeat kindergarten ($p<0.01$) and grades 1-8 ($p<0.1$) than redshirted students. Finally, young students were significantly more likely to earn a bachelors degree by age 26 than older students.

Semi-Parametric Matching

The regression results indicate that, controlling for family characteristics, the only advantage of redshirted students compared with young students comes from the decreased likelihood of repeating kindergarten and subsequent grades. We next turn to semi-parametric matching techniques to further compare young and redshirted students. This method controls for some of the selection bias introduced by voluntary redshirting by comparing students with similar predicted probabilities of being redshirted. The outcomes of young students were then compared with the outcomes of redshirted students to identify if there are any circumstances that create an advantage for redshirted students.

In the first column of Table 6, we matched students based on multiple criteria including gender, race, and urban/rural, in combination with a propensity score for redshirting. The results differ slightly with the regression results from Table 4. Young students have slightly higher test scores ($p<0.01$) and higher rates of attending college, but also a slightly higher probability of an out-of-wedlock birth. Dropout rates and college attendance rates are almost equal. Redshirted students earn slightly higher wages. Consistent with the regression results in Table 4 is the result that most of the differences are not statistically significant. Only the difference in test scores in statistically significant.

Redshirting may be used as a strategy to prevent grade repetition, and based on the regression results, this strategy may be effective. To further examine the outcome following grade retention, we matched redshirted students with similar students who were not redshirted but who repeated a grade. The results, displayed in Table 6 (Column 2), are mixed. The younger students who repeated a grade have significantly higher dropout rates ($p<0.10$) and are significantly less likely to attend college ($p<0.10$). These results suggest that redshirting is beneficial if students are certain to repeat a grade.

Since it is unlikely that parents and teachers will have perfect information about which students entering kindergarten will eventually repeat a grade, it is more realistic to examine outcomes for students based on the probability of repeating. Table 6, Column 3 displays the results of matching based on an estimated probability of repeating a grade. Here, the outcomes for young and redshirted students are very similar, with younger students having a slight advantage in test scores ($p<0.10$), but almost equal probability of dropping out and attending college. So if decisions about redshirting are based only on the predicted probability of repeating, it is unlikely that redshirting creates any long-term advantages. Finally, we matched young students who repeated and redshirted students who repeated to determine if repeating has greater negative effects on younger children. This match restricts the sample size to 42 matches, making it less likely that differences will be statistically significant. The results, displayed in Table 6, Column 4, suggest that younger students who repeat have higher test scores than redshirted students who repeat ($p<0.10$). Younger students who repeat also had higher rates of college attendance and higher wages, but these results are not statistically significant. Thus, given that we cannot determine in advance who will repeat a grade, it appears that students have better outcomes on average if they begin kindergarten at age 5.

Robustness checks

Because we discovered no significant differences between young and redshirted students, we conducted a series of robustness checks to determine if sampling affects the relationship between age at school entry and long-term outcomes. Because boys and girls mature at different rates, the effects of age at school entry may differ by gender. First, we tested the above results uses samples of only boys and only girls. In general, these results confirm the conclusion that the only long-term effect of age at school entry comes through the indirect effects of repeating a grade. There were no direct effects of age at school entry on long-term academic and social outcomes for boys or girls. We did find that young boys are more likely to repeat a grade than young girls. Thus, the negative effects of repeating a grade due to age at school entry are a greater concern for boys.

A second concern was that problems with the NELS dataset distort measurements of family background. Specifically, NELS data on family characteristics in 8th grade may not reflect family characteristics when children entered kindergarten. Family structure variables may have changed during the time between kindergarten and 8th grade. However, intact two-parent families did not undergo family structure changes during this time. Limiting the sample to only two-parent households, there are no significant effects of age at school entry on behavior problems, out-of-wedlock births, dropout, or college enrollment. There was also no effect of age at school entry on repeating grades 1-8. Also, young students appear to have an academic advantage in high school. Tests scores in 8th grade, 10th grade, and 12th grade were significantly higher for young students versus redshirted students.

As noted in Table 6, students who start kindergarten at age five and then repeat a grade perform worse than students who delayed kindergarten until age six. At the same time, students in this sample could have educational or social problems that led them to repeat a grade but are not observed in the data. Perhaps a better comparison is to measure the differences in long-term outcomes between those who start kindergarten at age five and repeat kindergarten and those that start kindergarten at age six. These two groups have similar ages in every grade after kindergarten and may be similar with respect to having a high probability of repeating kindergarten upon reaching age five. The only difference is that the first group attended kindergarten twice. We found that students who started kindergarten at age five and then repeated kindergarten at age six did as well or better than students who delayed kindergarten until age six. The students who repeated kindergarten were less likely to be arrested and more likely to attend college. Rates of behavioral problems and out-of-wedlock births were similar for both groups.

A reduced form model was also estimated to explore the isolated effect of summer versus winter birth, regardless of age at kindergarten entry. Ignoring year of birth, children with summer births are more likely to repeat a grade, but also more likely to attend college. There is no significant effect of summer birth on dropping out of school, behavior problems, or arrest. These effects are only apparent in the expanded model that includes the age at kindergarten entry.

DISCUSSION

This study addresses two competing theoretical issues relating to school policies about age at school entry. The first issue is that compulsory attendance laws are designed so that students who are older in kindergarten are potentially more likely to dropout of school or have an out-of-wedlock birth. The second issue is that increasingly rigorous elementary school curricula and accountability measures pressure schools and parents to delay kindergarten entry. If age at school entry influences accumulation of human capital in the long-term, determining the best age to start kindergarten is a concern for labor market policy, as well as school administrators and parents.

Our results suggest that, in the long-term, there are no benefits to a policy of intentionally delaying kindergarten entry. Although children who start kindergarten older are less likely to repeat a grade, the effects of repeating on dropout rates appear to be less severe for younger children. The matching results show that when students have similar probability of repeating a grade, the younger students perform as well as redshirted students. This indicates that the observed differences in outcomes for redshirted students are largely due to selection of students into the redshirted and non-redshirted groups. One caveat is that NELS participants appear to be redshirted due to potential academic disadvantages. The effects may be different in school districts where relatively privileged students are more likely to be redshirted.

Although being younger at school entry may raise achievement on average, parents should still be concerned with the higher risk of repeating a grade for younger kindergarteners. While parents can choose when their child should enter kindergarten, they have less control over grade retention decisions. Parents who elect to send a young child to kindergarten may regret the decision, if the school later elects to hold the child back because of his or her age. Therefore, the implication for parents is that they must consider school policies concerning both age at kindergarten and grade retention before making a decision about redshirting. If schools have a high propensity for retaining young students, parents may elect to redshirt as a prevention strategy. Alternately, parents who enter a young child in kindergarten could continue to advocate for their child regarding future retention decisions.

Despite past evidence that older students have an academic advantage in elementary school, our results suggest that in the long-run, there is no reason to expect delayed kindergarten entry to increase educational attainment in the long-run. Combined with the work of Angrist and Krueger (1991), this suggests that the most important effect of age at school entry may be that older students lose a year of participation in the workforce rather than that younger students are disadvantaged in early elementary years.

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[1] Dehejia & Wahba (2002) provide an important example of where matching techniques closely estimate the true treatment effects of a training program. Importantly, they find that the results from the matching method are closer to the actual experimental results than are the estimates predicted by a standard regression method.

[2] Alexander, Entwisle, and Horsey (1997) found student attitudes in kindergarten to be weak predictor of dropping out. Instead of controlling for early attitudes toward school, we examine whether early experiences in school (age at school entry) influences students attitudes toward school later in life.

[3] For father's education, this procedure is far from perfect. Most of these missing values are in female-headed households. Furthermore, it may be the case that these values are missing in precisely those families that are the most disadvantaged because of the least connection to the father. This will cause the coefficient on single parent to be biased upward. In addition, it is not clear in the NELS whether the value for a stepfamily is taken from the stepfather or the biological father. For these reasons, the analysis was replicated without the variable father's education, and the differences in the results were small and not statistically significant.

[4] Another potentially relevant school experience is attending preschool. However, the decision to send a child to preschool is similar to the decision about when to enroll a child in kindergarten. In addition, the NELS data does not provide specific information about preschool quality or quantity.

[5] Students who skipped a grade were excluded because there were too few of these students in the NELS sample to conduct a systematic analysis.

[6] "Young at school entry" students were born in June, July, or August, 1975 if they did not repeat a grade, and June, July, or August, 1974 if they did repeat a grade.

[7] "Older at school entry" students were born in December, 1973 or January or February, 1974 if they did not repeat a grade, and December, 1972 or January or February, 1973 if they did repeat a grade.

[8] "Redshirted" students were born in June, July, of August, 1974 if they did not repeat a grade, and June, July, or August, 1973 if they did repeat a grade.

[9] An interaction term for repeating kindergarten and entering kindergarten at age 5 was tested and omitted due to the fact that no girls were redshirted and repeated kindergarten.

Table 1: Mean Values of Family and Student Variables for Three Comparison Groups

	Young*		**Older****		**Redshirted*****	
	n=2794		n=2535		n=227	
Variable	**Mean**	**Std. Err.**	**Mean**	**Std. Err.**	**Mean**	**Std. Err.**
Group 1: SES Status & Background						
black	0.101	0.006	0.085	0.006	0.088	0.019
white	0.701	0.009	0.735	0.009	0.621	0.032
asian	0.065	0.005	0.057	0.005	0.128	0.022
hispanic	0.123	0.006	0.114	0.006	0.150	0.024
single mother	0.142	0.007	0.149	0.007	0.198	0.027
single father	0.003	0.001	0.004	0.001	0.009	0.006
stepmother	0.097	0.006	0.103	0.006	0.066	0.017
stepfather	0.024	0.003	0.016	0.002	0.018	0.009
other family structure	0.033	0.003	0.029	0.003	0.044	0.014
female	0.535	0.009	0.510	0.010	0.335	0.031
native born	0.826	0.007	0.818	0.008	0.634	0.032
mother foreign born	0.154	0.007	0.144	0.007	0.278	0.030
father foreign born	0.157	0.007	0.141	0.007	0.308	0.031
south	0.355	0.009	0.339	0.009	0.344	0.032
west	0.200	0.008	0.187	0.008	0.229	0.028
central	0.273	0.008	0.297	0.009	0.291	0.030
urban	0.241	0.008	0.243	0.009	0.273	0.030
rural	0.317	0.009	0.331	0.009	0.396	0.033
mother was teen parent	0.124	0.006	0.131	0.007	0.084	0.018
father's education	0.020	0.017	-0.033	0.018	-0.185	0.064
mother's eduction	0.024	0.019	-0.024	0.019	-0.328	0.070
father's occupation	0.007	0.017	-0.018	0.018	-0.088	0.057
father unemployed	0.058	0.004	0.062	0.005	0.101	0.020
mother's occupation	0.031	0.018	-0.022	0.019	-0.242	0.067
mother unemployed	0.297	0.009	0.290	0.009	0.308	0.031
oldest child	0.317	0.009	0.331	0.009	0.251	0.029
number of siblings	2.197	0.029	2.253	0.031	2.638	0.113
income in standard units	0.053	0.016	0.043	0.015	-0.340	0.074
Group 2: Family Context						
baptist	0.196	0.008	0.205	0.008	0.176	0.025
catholic	0.322	0.009	0.327	0.009	0.344	0.032
other religion	0.118	0.006	0.106	0.006	0.132	0.023
missing religion	0.036	0.004	0.036	0.004	0.053	0.015
number of siblings	2.198	0.029	2.253	0.031	2.639	0.113
not religious	0.027	0.003	0.026	0.003	0.048	0.014
very religious	0.416	0.009	0.390	0.010	0.322	0.031
religious	0.149	0.007	0.148	0.007	0.137	0.023
somewhat religious	0.167	0.007	0.166	0.007	0.141	0.023
books in household	0.890	0.006	0.877	0.007	0.824	0.025
magazines in household	0.749	0.008	0.743	0.009	0.634	0.032
library card	0.815	0.007	0.800	0.008	0.731	0.029
parents expect student to attend college	0.904	0.006	0.901	0.006	0.811	0.026
parents involved at school	0.543	0.009	0.544	0.010	0.454	0.033
changed school (other than normal transitions)+	0.224	0.008	0.247	0.009	0.273	0.030
Group 3: School Experiences						
repeated kindergarten	0.024	0.003	0.009	0.002	0.004	0.004
repeated grades 1-8	0.142	0.007	0.101	0.006	0.185	0.026
ESL	0.034	0.003	0.039	0.004	0.084	0.018

* students were age 5 at kindergarten entry
** students were age 5 1/2 at kindergarten entry
*** students were age 6 at kindergarten entry

+ missing values are imputed

Figure 1: Tracing Age at Kindergarten from NELS Data

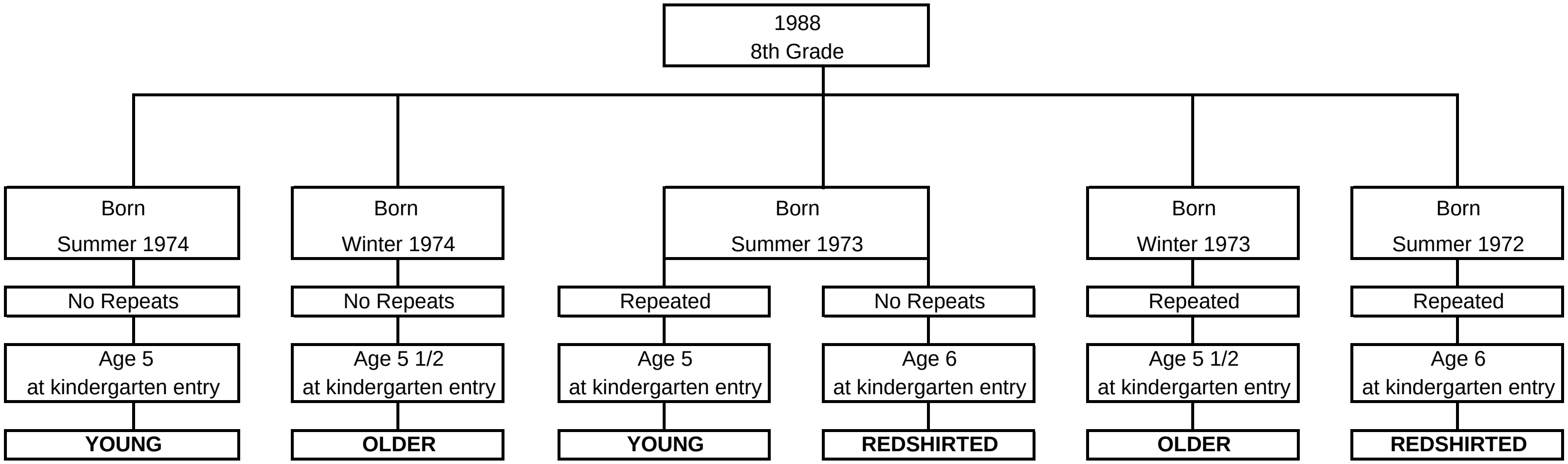
1988
8th Grade
Born
Summer 1974
No Repeats
Age 5
at kindergarten entry
YOUNG
Born
Winter 1974
No Repeats
Age 5 1/2
at kindergarten entry
OLDER
Born
Summer 1973
Repeated
Age 5
at kindergarten entry
YOUNG
No Repeats
Age 6
at kindergarten entry
REDSHIRTED
Born
Winter 1973
Repeated
Age 5 1/2
at kindergarten entry
OLDER
Born
Summer 1972
Repeated
Age 6
at kindergarten entry
REDSHIRTED

Table 2: Mean Values of Outcome Variables for Three Comparison Groups

	YOUNG* n=2794	**OLDER**** n=2535	**REDSHIRTED***** n=227
Variable	**Mean**	**Mean**	**Mean**
Key transitional outcomes			
8th grade achievement test	0.142 (0.016)	0.160 (0.018)	-0.056 (0.061)
behavior problems	0.111 (0.006)	0.117 (0.006)	0.154 (0.024)
child out of wedlock++	0.115 (0.008)	0.128 (0.009)	0.157 (0.042)
dropout	0.065 (0.005)	0.087 (0.006)	0.154 (0.024)
attended college	0.713 (0.009)	0.675 (0.009)	0.573 (0.033)
Additional outcomes			
Early School Experiences			
repeated kindergarten	0.024 (0.003)	0.009 (0.002)	0.004 (0.004)
repeated grades 1-8	0.142 (0.007)	0.101 (0.006)	0.185 (0.026)
High School - Academic			
10th grade achievement test+	0.006 (0.018)	-0.038 (0.019)	-0.139 (0.079)
12th grade achievement test+	-0.048 (0.019)	-0.081 (0.020)	0.041 (0.085)
High School - Social			
felt put down by teachers	0.160 (0.008)	0.149 (0.008)	0.182 (0.031)
felt put down by students	0.207 (0.009)	0.185 (0.009)	0.226 (0.033)
varsity sports	0.490 (0.009)	0.500 (0.010)	0.493 (0.033)
school activities	0.688 (0.009)	0.675 (0.009)	0.634 (0.032)
ever arrested	0.126 (0.006)	0.139 (0.007)	0.207 (0.027)
Post-Secondary			
earned a bachelors degree	0.555 (0.011)	0.528 (0.012)	0.517 (0.045)
income in 2000+++	$29,591 (594)	$28,607 (562)	$26,914 (1,216)

* students were age 5 at kindergarten entry
** students were age 5 1/2 at kindergarten entry
*** students were age 6 at kindergarten entry

+students who dropped out did not take the achievement test
++ females only
+++if working and not in school

Table 3: Model 1 Results (Older vs. Young Students)

	8th grade test		**child out of wedlock**		**dropout**		**college**		**wages**	
Variables	**effect size+**	**std. error**	**effect size++**	**std. error**	**effect size++**	**std. error**	**effect size++**	**std. error**	**effect size++**	**std. error**
young	-0.030	0.024	-0.012	0.011	-0.002	0.004	0.038	0.014 ***	1403.930	673.932 **
repeated kindergarten	-0.370	0.165 **	0.089	0.109	0.048	0.043 *	-0.113	0.093	-2011.167	1703.212
young*repeat k	0.236	0.194	-0.009	0.070	-0.007	0.015	-0.006	0.100	-476.866	2102.039
repeated grades 1-8	-0.242	0.059 ***	0.052	0.033 *	0.033	0.011 ***	-0.140	0.037 ***	4812.656	4569.199
young*repeat 1-8	0.116	0.073	-0.008	0.027	-0.013	0.004 ***	-0.009	0.041	-10225.060	5377.779 *
R-Squared+++	0.10		0.21		0.42		0.23		0.10	
number of obs.	4483		2788		5329		5329		4145	

* p<.1 ** p<.05 *** p<.01

+ marginal effect estimated by OLS
++ marginal effect (df/dx) estimated by probit with robust standard errors
+++ for probit estimations, reported r-squared is the pseudo r-squared statistic

Table 4: Model 2 Results (Redshirted vs. Young Students)

	8th grade test		child out of wedlock		dropout		college		wages	
Variables	effect size+	std. error	effect size++	std. error	effect size++	std. error	effect size++	std. error	effect size++	std. error
young	0.0921	0.0643	0.016	0.023	-0.008	0.010	0.039	0.038	1559.627	1898.658
repeated kindergarten	0.1552	0.1799	0.095	0.151	0.027	0.028	-0.118	0.068 *	-5247.854	2912.166 *
repeated grades 1-8	-0.302	0.1499 **	-0.020	0.055	0.017	0.022	-0.144	0.102	276.250	4420.560
young*repeat1-8	0.1689	0.1547	0.050	0.100	-0.008	0.011	0.000	0.090	-2474.823	4571.584
R-Squared+++	0.10		0.26		0.40		0.25		0.09	
number of obs.	2537		1572		3021		3021		2345	

*p<.1 **p<.05 ***p<.01

+ marginal effect estimated by OLS

++ marginal effect (df/dx) estimated by probit with robust standard errors

+++ for probit estimations, reported r-squared is the pseudo r-squared statistic

Table 5. Summary of Marginal Effects for Additional Outcomes

	Young vs. Older			Young vs. Redshirted		
Dependent Variable	**df/dx**	**std. error**		**df/dx**	**std. error**	
Early School Experiences						
Repeat kindergarten	0.011	0.002	***	0.013	0.003	***
Repeat grades 1-8	0.049	0.008	***	0.033	0.017	*
High School - Academic						
10th grade achievement test	0.085	0.027	***	0.172	0.075	**
12th grade achievement test	0.064	0.029	**	0.042	0.08	
High School - Social						
feel put down by teachers	0.009	0.012		-0.043	0.037	
feel put down by students	0.021	0.013		0	0.037	
varsity sports	-0.017	0.015		-0.063	0.04	
school activities	0.012	0.014		-0.016	0.036	
behavior problems at school	0.002	0.009		-0.01	0.023	
ever arrested	0.005	0.008		-0.01	0.02	
Post-Secondary						
earned a bachelors degree	0.035	0.019	*	-0.028	0.055	

* p<.1 ** p<.05 *** p<.01

Table 6: Results of Semi-Parametric Matching (Young vs. Redshirted Students)

	Redshirted student matched with not-redshirted student				Matching a redshirted student with not-redshirted student who repeated grades K-8				Matching based on similar probability of repeating a grade				Matching a redshirted student who repeated redshirted student who repeated		
	N=216 pairs				N=205 pairs				N= 125 pairs				N=42 pairs		
Variable	YOUNG	REDSHIRTED	Difference = (young-redshirted)	N	YOUNG	REDSHIRTED	Diff	N	YOUNG	REDSHIRTED	Diff	N	YOUNG	REDSHIRTED	Diff
	mean value	mean value			mean value	mean value			mean value	mean value			mean value	mean value	
8th grade achievement test	-0.078	-0.304	0.226 **	216	-0.302	-0.316	0.014	205	0.105	-0.138	0.243 *	125	-0.329	-0.878	0.549 *
dropout	0.153	0.144	0.009	216	0.205	0.146	0.059 *	205	0.072	0.072	0.000	125	0.357	0.357	0.000
child out of wedlock++	0.214	0.143	0.071	70	0.246	0.145	0.101	69	0.079	0.105	-0.026	38	0.364	0.182	0.182
attended college	0.597	0.583	0.014	216	0.493	0.571	-0.078 *	205	0.704	0.672	0.032	125	0.310	0.238	0.072
income in 2000+++	$26,029	$26,204	-$175	114	$27,429	$23,783	$3,646 *	100	$27,844	$27,177	$667	64	$23,948	$21,737	$2,211

Note: All matches are based on probability of being redshirted, urban, race, two-parent households, and gender
++ females only
+++ income is reported when both observations in a matched pair are working full-time and not in school

* p<.1 ** p<.05 *** p<.01

Appendix 1: Model 1 Results (Older vs. Young Students)

	8th grade test		dropout		behavior		child out of wedlock		college	
Variables	effect size+	std. error	effect size++	std. error	effect size++	std. error	effect size++	std. error	effect size++	std. error
young	-0.030	0.024	-0.002	0.004	0.002	0.009	-0.012	0.011	0.038	0.014 ***
Group 1: SES Status & Background										
black	-0.227	0.046 ***	0.019	0.008 ***	0.046	0.018 ***	0.117	0.028 ***	0.007	0.024
white (omitted)										
asian	0.085	0.058	-0.017	0.004 ***	-0.010	0.020	-0.053	0.018 **	0.119	0.034 ***
hispanic	-0.101	0.045 **	-0.007	0.005	0.011	0.016	0.040	0.024 *	0.031	0.025
single mother	0.019	0.037	0.017	0.007 ***	0.046	0.015 ***	0.065	0.020 ***	-0.024	0.021
single father (omitted to to multicolinearity)										
stepmother	-0.019	0.041	0.005	0.006	0.036	0.016 **	0.045	0.021 **	-0.021	0.023
stepfather	-0.085	0.083	0.021	0.017	0.093	0.040 ***	-0.042	0.022	-0.017	0.048
other family structure	-0.103	0.069	0.007	0.009	0.057	0.028 **	0.051	0.034 *	-0.012	0.039
female	0.017	0.023	0.002	0.003	-0.072	0.008 ***			0.037	0.013 ***
native born	-0.008	0.038	-0.211	0.021 ***	-0.049	0.015 ***	-0.116	0.023 ***	0.200	0.025 ***
mother foreign born	-0.009	0.051	-0.016	0.005 **	0.003	0.019	-0.026	0.020	0.102	0.027 ***
father foreign born	0.064	0.050	-0.002	0.008	-0.025	0.016	-0.021	0.020	0.075	0.026 ***
south	-0.077	0.035 **	0.000	0.005	-0.006	0.013	-0.040	0.015 **	-0.001	0.021
west	-0.046	0.039	0.005	0.006	0.064	0.017 ***	0.002	0.018	-0.016	0.024
central	-0.105	0.034 ***	0.000	0.005	0.013	0.013	0.007	0.017	-0.039	0.021
urban	0.010	0.029	-0.004	0.004	0.004	0.010	0.007	0.014	0.002	0.018
rural	-0.034	0.028	-0.006	0.004	-0.014	0.009	0.001	0.012	-0.029	0.016 *
mother was teen parent	-0.006	0.038	0.000	0.004	0.032	0.015 **	0.043	0.018 ***	-0.042	0.022 **
father's education	0.075	0.018 ***	-0.004	0.003	0.000	0.007	-0.016	0.008 *	0.058	0.011 ***
mother's education	0.049	0.016 ***	-0.004	0.002	0.004	0.006	-0.004	0.008	0.035	0.010 ***
father's occupation	0.038	0.016 **	-0.007	0.002 ***	0.012	0.006 **	-0.006	0.007	0.030	0.009 ***
father unemployed	0.017	0.051	0.000	0.006	-0.016	0.016	0.012	0.021	0.088	0.024 ***
mother's occupation	0.003	0.014	-0.001	0.002	0.006	0.005	-0.010	0.006 *	0.017	0.008 **
mother unemployed	0.021	0.026	0.001	0.004	-0.013	0.009	0.001	0.011	0.016	0.015
oldest child	0.005	0.025	-0.001	0.004	-0.033	0.008 ***	-0.005	0.011	0.032	0.015 **
number of siblings	-0.002	0.008	0.002	0.001 *	0.001	0.003	0.011	0.003 ***	-0.015	0.005 ***
Group 2: Family Context										
baptist	-0.062	0.035 *	0.012	0.006 **	0.009	0.013	0.021	0.017	-0.018	0.020
catholic	-0.070	0.030 **	-0.005	0.005	-0.005	0.011	-0.011	0.014	0.033	0.018 *
other religion	-0.015	0.042	0.001	0.006	-0.027	0.012 **	0.024	0.021	-0.034	0.026
missing religion	0.080	0.066	-0.007	0.006	-0.058	0.013 ***	-0.005	0.026	0.025	0.034
not religious	-0.110	0.074	0.010	0.013	0.023	0.027	0.052	0.041	-0.046	0.044
very religious	0.000	0.030	-0.022	0.004 ***	-0.023	0.010 **	-0.028	0.013 **	0.095	0.016 ***
religious	-0.023	0.037	-0.007	0.004	-0.013	0.012	-0.011	0.015	0.073	0.018 ***
somewhat religious	-0.053	0.036	-0.012	0.003 ***	-0.016	0.011	-0.004	0.015	0.054	0.019 ***
books in household	0.032	0.039	-0.007	0.005	0.011	0.012	-0.001	0.015	0.010	0.021
magazines in household	0.058	0.029 **	-0.001	0.004	0.014	0.009	-0.026	0.013 **	0.030	0.017 *
library card	0.073	0.030 **	-0.014	0.005 ***	-0.041	0.012 ***	-0.035	0.016 **	0.046	0.018 ***
income in standard units	0.050	0.019 ***	-0.001	0.002	-0.010	0.007	-0.007	0.007	0.060	0.012 ***
parents expect student to attend college	0.160	0.042 ***	-0.029	0.008 ***	-0.056	0.017 ***	-0.004	0.016	0.252	0.028 ***
parents involved at school	0.011	0.024	-0.003	0.003	-0.002	0.008	-0.007	0.011	0.047	0.014 ***
changed schools (other than normal transitions)****	-0.121	0.037 ***	0.023	0.007 ***	0.047	0.014 ***	0.039	0.017 **	-0.061	0.022 ***
imputed changed schools****	0.067	0.048	-0.012	0.004 ***	-0.036	0.012 ***	-0.020	0.016	0.026	0.025
Group 3: School Experience										
repeated kindergarten	-0.370	0.165 **	0.048	0.043 *	-0.013	0.052	0.089	0.109	-0.113	0.093
young*repeat kindergarten	0.236	0.194	-0.007	0.015	0.017	0.076	-0.009	0.070	-0.006	0.100
repeated grades 1-8	-0.242	0.059 ***	0.033	0.011 ***	0.107	0.026 ***	0.052	0.033 *	-0.140	0.037 ***
young*repeat 1-8	0.116	0.073	-0.013	0.004 ***	-0.039	0.015 **	-0.008	0.027	-0.009	0.041
ESL student	0.090	0.061	-0.004	0.007	0.028	0.025	-0.029	0.020	0.032	0.035
contant (for OLS only)	0.048	0.080								
Log liklihood function (for probit only)			-826.315		-1694.377		-812.143		-2524.217	
R-Squared+++	0.10		0.42		.10		0.21		0.23	
number of obs.	4483		5329		5329		2788		5329	

* p<.1 ** p<.05 *** p<.01

**** missing values are imputed, dummy variable is included to represent that value was imputed

+ marginal effect estimated by OLS

++ marginal effect (df/dx) estimated by probit with robust standard errors

+++ for probit estimations, reported r-squared is the pseudo r-squared statistic

Appendix 2: Model 2 Results (Redshirted vs. Young Students)

	8th grade test		behavior		child out of wedlock		dropout		college	
Variables	effect size+	std. error	effect size++	std. error	effect size++	std. error	effect size++	std. error	effect size++	std. error
young	0.0921	0.0643	-0.010	0.023	0.016	0.023	-0.008	0.010	0.039	0.038
Group 1: SES Status & Background										
black	-0.191	0.0575 *	0.033	0.022	-0.060	0.014 *	0.004	0.008	0.036	0.030
white (omitted)										
asian	0.1228	0.0735	-0.035	0.022	0.022	0.026	-0.017	0.005 *	0.125	0.039 ***
hispanic	-0.081	0.0575	0.002	0.020	-0.001	0.007	-0.003	0.007	0.029	0.032
single mother	0.0325	0.0483	0.041	0.020 **	0.071	0.026 ***	0.015	0.009 **	-0.022	0.028
single father	0.2466	0.302	0.045	0.095	omitted		0.054	0.056	-0.018	0.134
stepmother	0.0082	0.0555	0.033	0.022	0.034	0.024 *	0.004	0.008	-0.013	0.031
stepfather	-0.093	0.1	0.123	0.054 ***	-0.049	0.017	0.015	0.017	-0.078	0.063
other family structure	-0.094	0.0883	0.089	0.039 ***	0.041	0.038	0.019	0.015	-0.109	0.056 **
female	0.0163	0.03	-0.074	0.011 ***	-0.119	0.029 ***	0.006	0.004	0.041	0.018 **
native born	0.0173	0.0491	-0.048	0.020 ***	-0.044	0.019 *	-0.182	0.024 ***	0.178	0.032 ***
mother foreign born	-0.023	0.0687	0.044	0.031	-0.018	0.024	-0.015	0.007	0.082	0.037 **
father foreign born	0.0775	0.0649	-0.041	0.020 *	0.001	0.014	-0.009	0.009	0.104	0.033 ***
south	-0.04	0.0459	-0.021	0.016	0.005	0.021	-0.002	0.006	0.027	0.027
west	-0.08	0.0503	0.048	0.022 **	0.003	0.019	0.008	0.008	0.027	0.029
central	-0.052	0.0458 **	0.002	0.017	-0.009	0.015	-0.003	0.006	-0.025	0.027
urban	-0.089	0.0386 *	0.015	0.014	0.010	0.015	-0.009	0.005 *	0.017	0.023
rural	-0.063	0.0362	-0.010	0.013	0.044	0.024 **	-0.008	0.004 *	-0.003	0.021
mother was teen parent	-0.063	0.0517 ***	0.066	0.022 ***	-0.012	0.009	0.008	0.007	-0.002	0.028
father's education	0.0763	0.0236 **	0.002	0.009	0.006	0.009	-0.001	0.004	0.072	0.015 ***
mother's education	0.0497	0.0214	0.005	0.008	-0.015	0.009 *	-0.003	0.003	0.020	0.013
father's occupation	0.023	0.0208	0.001	0.007	0.035	0.030	-0.006	0.003 *	0.033	0.012 ***
father unemployed	-0.035	0.0659	-0.023	0.020	0.024	0.022	0.010	0.010	0.061	0.032 *
mother's occupation	0.0062	0.0185	-0.001	0.007	0.008	0.013	-0.004	0.003 *	0.035	0.010 ***
mother unemployed	0.0094	0.0339 ***	-0.012	0.012	0.081	0.031 ***	-0.008	0.004 *	0.010	0.020
oldest child	0.0119	0.0332	-0.028	0.011 **	-0.022	0.018	-0.005	0.005	0.031	0.020
number of siblings	0.0084	0.0105	0.002	0.004	-0.022	0.020	0.002	0.001 *	-0.021	0.006 ***
income in standard units	0.033	0.0235	-0.015	0.008 *	-0.022	0.022	-0.003	0.003	0.054	0.014 ***
Group 2: Family Context										
baptist	-0.05	0.0462 **	0.006	0.017	0.004	0.019	0.009	0.008	-0.016	0.027
catholic	-0.082	0.0393	-0.009	0.014	0.024	0.027	-0.010	0.006	0.024	0.023
other religion	0.0192	0.0533	-0.040	0.015 **	0.048	0.043	-0.007	0.006	-0.054	0.035
missing religion	0.0918	0.0858	-0.057	0.017 **	0.012	0.038	-0.009	0.007	0.014	0.046
not religious	-0.067	0.0941	0.051	0.039	-0.025	0.016 *	0.004	0.013	-0.042	0.057
very religious	0.0238	0.0391	-0.007	0.013	-0.022	0.015	-0.017	0.005 ***	0.089	0.022 ***
religious	-0.007	0.0485	-0.013	0.016	-0.015	0.015	-0.005	0.005	0.083	0.023 ***
somewhat religious	-0.028	0.0468	0.003	0.016	0.011	0.004 ***	-0.012	0.004 **	0.038	0.025
books in household	0.0435	0.052 *	0.004	0.016	-0.036	0.016 **	-0.004	0.007	0.038	0.030
magazines in household	0.0689	0.038 ***	0.005	0.013	-0.037	0.018 **	-0.006	0.005	0.053	0.022 **
library card	0.1033	0.0395	-0.035	0.015 **	-0.013	0.007 *	-0.014	0.007 ***	0.050	0.023 **
parents expect student to attend college	0.132	0.0534	-0.066	0.023 ***	-0.003	0.012	-0.036	0.012 ***	0.225	0.036 ***
parents involved at school	0.0042	0.0318 ***	0.024	0.011 **	0.045	0.021 **	0.003	0.004	0.032	0.018 *
changed schools (other than normal transitions)****	-0.138	0.0485	0.044	0.018 ***	-0.029	0.015	0.029	0.010 ***	-0.065	0.031 **
imputed changed schools****	0.0691	0.0625	-0.044	0.015 **			-0.016	0.004 ***	0.060	0.032 *
Group 3: School Experience										
repeated kindergarten	0.1552	0.1799 **	-0.035	0.039	0.095	0.151	0.027	0.028	-0.118	0.068 *
repeated grades 1-8	-0.302	0.1499	0.039	0.051	-0.020	0.055	0.017	0.022	-0.144	0.102
young*repeat1-8	0.1689	0.1547	0.003	0.045	0.050	0.100	-0.008	0.011	0.000	0.090
ESL student	0.1203	0.0793 **	0.018	0.031	-0.003	0.018	0.000	0.011	0.055	0.041
contant (for OLS only)	-0.139	0.1187								
Log liklihood function (for probit only)			-958.490		-419.191		-464.667		-1380.576	
R-Squared+++	0.10		0.11		0.26		0.40		0.25	
number of obs.	2537		3021		1572		3021		3021	

*p<.1 **p<.05 ***p<.01

**** missing values are imputed, dummy variable is included to represent that value was imputed

+ marginal effect estimated by OLS
++ marginal effect (df/dx) estimated by probit with robust standard errors
+++ for probit estimations, reported r-squared is the pseudo r-squared statistic