A Date With Destiny

Relative Age Effects in High School and Beyond

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Abstract

This paper examines the persistence of relative age effects. Using data on academic performance in high school, I investigate whether the well-documented benefits that students born earlier in the academic calendar receive in kindergarten and primary school persist into adolescence and adulthood. I conclude that relative age has a negligible effect on academic performance in high school and beyond, and potentially turns into an age penalty for older students. Notably, my results suggest that age has a sizable effect on decisions to dropout or go to college.

1 Introduction

This paper evaluates the impact of birth month on performance in high school and admission into college. A growing body of economics literature focuses on the effect of birth month on academic achievement (Navarro et al., 2015; Nam, 2014; Imchal et al., 2017). This effect is commonly referred to as the 'relative age effect.' Studies on relative age have been conducted worldwide using data on students from Korea, Japan, Israel, Spain, and states across the US (Benieto and Soria-Espín, 2020; Dhuey et al., 2017; Attar Cohen-Zadam, 2017; Kawaguchi, 2011; Navarro et al., 2015). Building on the existing literature, I investigate the effects of birth month on later life outcomes such as academic achievement during high school and admission into college. I find that older students—students who are born earlier in the academic calendar—perform no better in high school but are less likely to go to college. A more comprehensive explanation of the outcomes I consider is provided in the model section.

I offer a few important extensions to the existing literature. To my knowledge, I am the first to use a *national* US sample to evaluate the effects of relative age in high school and beyond. In the same vein, I am among the first to work across heterogeneous thresholds.[1] Additionally, I am among the first to connect the literature on relative age effects to the literature on mandatory schooling policies. Finally, I am the first to quantify omitted variables bias by comparing OLS and RDD estimates.

The remainder of the introduction offers institutional context. Section 2 reviews the existing literature. Sections 3 and 4 describe the theoretical foundation and empirical implementation of my model. Sections 5 and 6 describe the data and present results. Section 7 discusses the findings and concludes.

Most US states have an age criterion for kindergarten admission, with the majority

[1]Most previous work has been done in countries with a national kindergarten entry cutoff, or states within the US with a single cutoff. All work using a US national sample, like Elder and Lubotsky (2009) and Datar (2006), has not explicitly considered high school and college outcomes.

of cutoffs in early September. For example, many states require students to be five years old by September 1 of the year they enter kindergarten. Yet some states opt for cutoffs in October through December. For a complete list of state cutoffs please see Appendix A.1.

To visualize the relative age effect, consider students who entered kindergarten in 2000 in a state with a cutoff of September 1. Figure 1 below depicts the students' birthdates. Those born between September 2, 1994, and September 1, 1995, were enrolled in the correct class because they were between 5 and 6 on September 1, 2000. Students born on September 1, 1994 or before were enough to begin school on September 1, 1999. If they entered kindergarten in 2000, they were *redshirted* or held back a year.[2] If the student was born September 2, 1995 or later, they were not 5 by September 1, 2000. If they entered kindergarten in 2000 they must have been *anti-redshirted*—permitted by their school or district to enroll a year early.

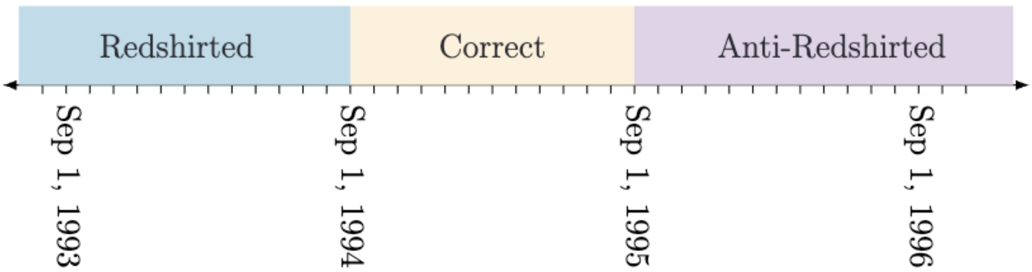

Figure 1: Timeline For 2000 Kindergarten Entry

Existing research shows that this maturity disparity at the kindergarten level results in an achievement gap between older and younger students but scholars dispute when the effect dissipates (Benieto and Soria-Espín, 2020; Dhuey et al., 2017; Attar

[2]Roughly 6 percent of students are redshirted every year. (Reeves, 2022) In appendix A.2 I find that less than 1 percent of students are anti-redshirted. Parents often redshirt their children so that they can be one of the oldest in their class. Consider a student born in August in a state with a cutoff of September 1. If not redshirted they are 11 months **younger** than the oldest non-redshirted student in their class. If redshirted, they are 13 months **older** than the oldest non-redshirted student in their class.

Cohen-Zada, 2017). While Elder and Lubotsky (2009) find that the effect dissipates after middle school, Benieto and Soria-Espín (2020) find that it persists into secondary school, and Kawaguchi (2011) concludes that it persists into adulthood. This paper investigates how long the effect persists for students in the United States.

2 Literature Review

Existing research identifies a relationship between relative age and academic performance. In what follows I first discuss the existing research on students in the US and then present international results. Datar (2006) uses a national sample of 1000 US kindergarten students during the 1998-1999 academic year and finds that redshirting increases kindergarten reading and math scores by 5.2 and 6 percentage points respectively. Using data from Florida, Dhuey et al. (2017) estimate the effect of relative age on state-wide tests. They estimate the effect of being born after the threshold against before or the effect of being 12 months older. They find that older students score, on average, 0.2 standard deviations above younger students on state math and reading tests administered in third through eighth grade. Elder and Lubotsky (2009), using a national US sample, find that relative age effects dissipate by 8th grade.

Similar to Dhuey et al. (2017), McEwan and Shapiro (2008) use a regression discontinuity design to study a national sample of Chilean students. They compare redshirted students against non-redshirted students, holding birth month fixed. They find that redshirting increases fourth and eighth-grade test scores by more than 0.3 standard deviations. McEwan and Shapiro also find that redshirting decreases the probability of being held back during elementary school.

McEwan and Shapiro are not the only international study in this area. Navarro et al. (2015) also use a national sample of 15,000 Chilean 8th graders and find a positive relative age effect on academic performance. Attar and Cohen-Zada (2017) use Israeli data to show that redshirting increases fifth-grade test scores in Hebrew by 0.29 standard deviations and in math by 0.16 standard deviations. They find that the effect in Hebrew diminishes by 8th grade while the effect in math doubles. Notably, Attar and Cohen-Zada (2017) measure how far birthdays fell from the cutoff and show that in Israel the cutoffs do not bind—students on both sides of the threshold often enroll in the wrong year. Their results suggest that any researchers studying relative age effects using a regression discontinuity method should employ a fuzzy regression discontinuity instead of a sharp regression discontinuity.

Nam (2014), using Korean data, finds a persistent relative age effect through middle school but not beyond. In contrast, Smith (2009) finds, from a sample of students in British Columbia, Canada, that older students perform better in numeracy, reading, and writing tests in grade 10. Furthermore, Kawaguchi (2011), using data on Japanese students, shows that greater relative age implies higher annual earnings for males during adulthood. The results of these studies may vary because the countries studied have differing education systems.[3]

Some of the literature on relative age effects also considers admissions into elite universities. For example, Beneito and Soria-Espin (2020), using data from the University of Valencia, find that relative age improves scores on college entrance exams for women. Similarily, Dhuey et al. (2017) find that relatively young Florida students are less likely to attend and complete college, particularly at selective institutions.

The Mechanism Behind The Relative Age Effect

Researchers propose two ways that relative age can impact performance on standardized tests. The first is *age at kindergarten entry*: students who enter kindergarten

[3]'Education system' is a broad term referring to syllabi, examinations, and pedagogical philosophies, among many other things. One component of a country's educational system that this paper discusses is the structure of mandatory schooling policies.

older are more cognitively developed, so the marginal productivity of instruction is higher for them. Therefore they perform better. The second is *age at testing*: students who are relatively older are more cognitively developed on the day of testing independent of schooling—thus they perform better.

Datar (2006) observes that greater relative age implies a steeper test score trajectory during the first two years of schooling. In contrast, Elder and Lubotsky (2009) suggest that the positive relationship between kindergarten entrance age and school achievement results from skill accumulation before kindergarten, rather than a greater ability to learn for older children. They find that the association between test scores and entrance age emerges during the initial months of kindergarten and then sharply declines in subsequent years. Carlsson et al. (2015) exploit random variation in testing dates for entry into the Swedish army and find that additional schooling improves performance on technical comprehension tests while additional age does not. Conversely, Carlsson et al. (2015) find that additional age improves performance on spatial and logic intelligence tests but additional schooling does. I do not focus on on the distinction between age at testing and age at school entry because both are relative age effects. But understanding the two hypothesized mechanisms is crucial because age at testing predicts that the relative age effect will persist into high school while the age at testing suggests that it won't because cognitive differences diminish. Despite the lack of emphasis put on this distinction in this paper, the surrounding discussion motivates the nuance around relative age effects and the reasons why this paper contributes meaningfully to the literature.[4]

[4]While this paper does not implement this technique, it is valuable to note that researchers can distinguish between the two relative age effects. Datar (2006) proposes a novel method for isolating the 'age at school entry' and 'age at testing' effects. Using panel data, the researcher constructs a measure for each individual's performance relative to the class average. Then the researcher measures an individual's change in performance over time. Using the difference between the average change among students who were relatively older at school entry and those who were relatively younger at school entry, the researcher can isolate the effect of relative age at school entry.

To visualize this, consider a cohort of n students: $\{s_1, ..., s_n\}$ who each take a standardized test in each year of high school. Let $t_{i,g}$ where $g \in \{10, 12\}$ represent individual s_i's score on the standardized test administered when they were in grade g. Then a researcher can compute $\bar{t_g}$ which

Family Income and Child's Sex

Elder and Lubotsky (2009) observe that relative age effects are greater for wealthier students at kindergarten entry. They hypothesize that wealthier students are more likely to have pre-kindergarten training. Wealthy families also redshirt their children more frequently than poor families (Datar, 2006; Elder and Lubotsky, 2009). Because redshirting children necessitates an additional year of heightened childcare costs, it may not be feasible for poorer families. Previous literature has focused on isolating relative age effects from the effect of wealth on student outcomes. But researchers do not universally agree that relative age effects are strongest for wealthy students. Smith (2009) finds that relative age effects are strongest for low-income students. After controlling for income, Datar (2006) finds that redshirting improves test scores, particularily in reading, for poor children, disabled children, and boys.[5]

Using data from the University of Valencia, Beneito and Soria-Espin (2020) discover that relative age positively influences university entrance examination scores for female students but not for their male classmates. They suggest that, while older students' cognitive advantages diminish over time, relatively older students maintain better academic self-confidence throughout their education.

Yet this effect may differ across sex. Beneito and Soria-Espin argue that women benefit more from the relative age effect because they are more likely to experience low self-confidence. The effect may also be stronger for women because girls develop cognitively faster than boys.[6] Smith (2009) observes that girls receive a greater

represents the average cohort score on the standardized test administered in grade g. From $\bar{t_g}$ and $t_{i,g}$ they can compute the quantile, $q_{i,g}$ of each student for each exam, relative to their cohort. Next, they can then calculate $c_i = q_{i,12} - q_{i,10}$ or the change in individual i's quantile over time. Because each student has the same relative age at testing for each test (for example, a student born in September will be 11 months older than a student born in August during each test), this method isolates the effect of relative age at kindergarten. The remaining effect is the age at testing effect.

[5]Schanzenbach (2016) studies children randomly assigned to different kindergarten classmates. She finds that having older classmates increases test scores up to eight years after kindergarten and increases the probability of taking college entrance exams. Elder and Lubotsky (2009) also find positive spillover effects from relative age.

[6]Then the relative age effect between a September- and May-born girl is stronger than a

relative age achievement boost through tenth grade. Conversely, McEwan and Shapiro (2007) find that the effect of relative age on test scores is more pronounced for boys.

Dropouts and Juvenile Delinquency

In the United States, students are required to attend school until they are a specified age. For example, in Minnesota students must attend school till they are 17. Consequently, relatively older students complete less schooling by the time compulsory attendance laws permit them to drop out. Angrist and Krueger (1991) use 1960 Census data to examine the connection between relative age and the likelihood of dropping out. They identify a positive relationship between the two. Furthermore, Dobkin and Fernando (2010), using data on students from Texas and California, reveal that lowering kindergarten entry age increases education attainment, but decreases academic performance while in school. Conversely, Kawaguchi (2011), studying Japanese students, finds a positive correlation between relative age and educational attainment. Kawaguchi argues that his results differ from those of Angrist and Krueger (1991) because Japanese school attendance laws are not age-dependent; therefore, relative age is not associated with the likelihood of dropping out.

Cook and Kang (2016), using data from North Carolina, discover that individuals born just after the cutoff date for enrolling in public kindergarten are more prone to high school dropout and felony commitment by age 19. McAdams (2016) generalizes these results by showing that a later school-starting age cutoff leads to lower rates of incarceration, even into adulthood. The results regarding the propensity to participate in crime align with Angrist and Krueger's conclusions about dropout rates. As Becker (1968) and Freeman (1999) posited, individuals who drop out are more likely to engage in criminal activities.

September- and May-born boy.

3 Model Foundations

This section establishes the foundation of the empirical analysis that follows. I first consider what relative age means in the context of this paper. I then formalize the economic model that underpins my empirical design.

Relative age effects are, as the name suggests, age effects—benefits received by those who are older. When labor economists consider age they are typically considering something that is correlated with age like experience or education. But relative age refers to age itself. Because cognitive abilities develop throughout childhood and adolescence, age improves performance on standardized tests by virtue of cognitive development. *Relative* age emphasizes that the comparison is between students in a single school cohort.

For example, a student born in September one year is considered relatively old while a student born in May two years prior is considered relatively young. Although the first student is younger than the second, the first is *relatively older* compared to their classmates and the second is *relatively younger* compared to theirs. In this paper, I consider the age of students who are all in the same ninth grade cohort. Because all students are in the same cohort, I can consider their age in months at school entry to measure the relative age effect.[7]

This paper ends with entry to college because variance in peer age grows considerably in college as students are exposed to peers who grew up in other countries with different cutoffs and different education systems. Then disentangling the effect of relative age from other sources of heterogeneity becomes more challenging and requires richer data.

[7]Because I only consider age at ninth grade entry all comparisons reflect nominal differences in age. For example, even when considering outcomes later in high school the coefficient on age should be read as the effect of being one month older, not **x** percent older. While a student that is one month older at high school entry will always remain one month older, the proportional difference in their ages will shrink over time,

Theoretical Foundations

Cunha and Heckman (2007, 2009) propose a model for skill accumulation that form the theoretical foundations for this paper. Let h denote parental characteristics like IQ and education and θ_0 a vector of abilities at birth. θ includes both cognitive abilities and non-cognitive abilities like patience, self-control, and temperament. Now let θ_t represent the vector of skills stocks in period t where t refers to the child's age in months. Cunha and Heckman model skill developments as

$$\theta_{t+1} = f_t(h, \theta_t, I_t)$$

where I_t is the quantity of investment into a child's skill vector in period t and f_t is a skills production function. Cunha and Heckman assume f_t is strictly increasing and strictly concave in I_t. That is,

$$\frac{\partial f_t}{\partial I_t} > 0 \qquad \text{and} \qquad \frac{\partial^2 f_t}{\partial I_t^2} < 0$$

The second condition guarantees an optimal quantity of investment in each period. Because the skill vector is defined recursively it can be rewritten as

$$\theta_{t+1} = f_t(h, \theta_0, I_1, I_2, ..., I_t)$$

Any investments made prior to birth are then accounted for by θ_0.

Cunha and Heckman additionally introduce two key properties of the model: dynamic complementarity and self-productivity. Dynamic complementarity states that the stock of skills acquired in period $t-1$ make investment in period t more productive. Formally,

$$\frac{\partial^2 f_t}{\partial \theta_t \, \partial I_t'} > 0.$$

Additionally, the self-productivity property asserts that a higher stock of skills in one period produces a higher stock of skills in the next period. Formally, self-productivity establishes that

$$\frac{\partial f_t}{\partial \theta_t} > 0$$

Jointly with dynamic complementarity, self-productivity explains why investments into poor young children have higher marginal productivities and investments into poor adolescent children have lower returns. Their lower stock of skills offer a lower dynamic complementarity effect.

I now explain relative age effects using this model. Consider two students, identical in every respect except for birth month. Let one student be born in September and another student be born in May of the following year so that they are enrolled in the same kindergarten cohort. Let T denote their age in months at kindergarten entry. Note that $T^{Sep} > T^{May}$. If the students are identical in all other respects including their initial endowments and their parents invest optimally in both of them, $|\theta_T^{Sep}| > |\theta_T^{May}|$ or the magnitude of the September-born student's skill vector is greater than the magnitude of the May-born student's skill vector at kindergarten entry. Additionally, if the two students are identical in all respects, $\phi_T^{Sep} > \phi_T^{May}$ where ϕ is an individual skill component of the overall skill vector, θ.[8]

In line with empirical results, Cunha and Heckman's model predicts that students who are born earlier in the academic calendar, and therefore have a longer runway before entering kindergarten, have a skill advantage over their peers. The size of the difference depends on the effect of dynamic complementarity. For all periods after T, the discrepancy in their skill vectors may persist because the quantity of investment into each child prevents their skill vectors from converging. Additionally, the magnitude of their skill difference may grow as a result of dynamic complementarity and

[8] As an example $\phi_{math\,aptitude}$, $\phi_{temperament}$ are components of θ.

self-productivity. It may also shrink as a result of a child investing into their own skillsets. Or it may shrink because teachers invest equally or invest more into weaker students. This paper evaluates which of these effects dominate for students in the United States.

Cunha and Heckman's model makes explicit the idea that investments early in life are quite important; they determine the optimal quantity of investment into skills later in life and, consequently, the stock of skills an individual will have in adulthood. Research in cognitive science identifies early adolescence, particularly between ages two and eleven, as a time of tremendous cognitive development (Craig and Bialystok, 2006; Schatz, 1992). Additionally, economists find that long-lasting ability gaps between individuals and across socioeconomic groups develop during this period (Cunha and Heckman 2009).

4 Empirical Design

A key concern among scholars is the potential endogeneity of birth dates with individuals' backgrounds. Buckles and Hungerman (2013) find children born in December through February are disproportionately born to teenage and unmarried mothers. Currie and Schwandt (2013) establish a correlation between birth month and birth weight and gestation length. They additionally find that birth weight and gestation length are important predictors of success later in life. In response prior studies such as Dhuey et al. (2017) employ a regression discontinuity design (RDD) to examine the relative age effect.

Because of the concerns articulated by Dhuey et al. (2017), Buckles and Hungerman (2013), and Currie and Schwandt (2013), a regression discontinuity design is preferred to a traditional least squares approach for its ability to convincingly eliminate omitted variable bias. These concerns are exacerbated by the omission of a variables measuring pre-kindergarten training which I do not have data on.[9] However, recent scholarship suggests that omitted variable biases may be smaller than initially believed (Dhuey et al., 2017). Consequently, the benefits of employing an RDD approach are unclear. The costs, however, are straightforward.

The number of observations preserved in an RDD is smaller compared to an ordinary least squares (OLS) design, as only the periods before an after the cutoff are considered. Furthermore the RDD measures the local average treatment effect, specifically being born before versus after the cutoff instead of one month older at any part in the year.[10] Additionally, I quibble with the implementation of a discontinuity design that other researchers have used. I discuss this point fully in appendix A.3. Given these trade-offs I estimate the relative age effect using least squares and logistic regressions. One additional advantage of this approach is that my estimates are robust to having the wrong kindergarten cutoffs because I use age in months instead of month of birth. To test for omitted variable bias I compare my least squares estimates against regression discontinuity estimates. RDD is used only to measure omitted variable bias.

Primary Model Specification

My primary model specification includes least squares and logistic regressions. In my analyses I consider three sets of outcome variables. The first set evaluates differences between relatively older and younger students early in high school. The second set examines differences late in high school. The final tests for differences in decisions to dropout, work, or go to college.

When studying differences between relatively older and younger students early in

[9] Additionally, RDD offers causal estimates. So, if the RDD estimates concur in direction with the primary model specification estimates, they support the idea that the least squares and logistic estimates are causal.

[10] The researcher can interpolate the effect of being one month older from this only if they can assume that the relative age effect is the same for any two months in the year. I discuss this further in the conclusion section.

high school I consider both grade 9 grade point average (GPA) and difficulty in grade 9 coursework. Grade 9 GPA measures students' overall academic performance in their first year of high school on a 0.0 to 4.0 scale. Difficulty in Grade 9 is a categorical variable which collects from parents information about whether their child had 'a lot of difficulty,' 'a little difficulty,' or 'no difficulty' in their grade 9 coursework. When examining differences late in high school, I consider cumulative high school GPA and performance on the SAT. Finally, I measure probability of attending college, probability of working after high school, probability of dropping out during high school, and average number of hours worked during grade 12.

The majority of analyses are conducted using a weighted least squares approach using student level weights. When considering difficulty in grade 9 coursework a weighted multinomial regression is implemented. Additionally, when measuring probability of working, going to college, working, and dropping out a traditional logistic regression (with weights) is chosen because the categories are not exclusive.[11] For example a student can simultaneously work and go to college. The following ordinary least squares model is estimated:

$$\mathrm{Y}_i = \alpha + \beta\,\mathrm{Age}_i + \gamma\,\mathbf{X_i}$$

where $\mathbf{X_i}$ is a vector of individual controls including log family income, parents' education, sex, race, and average age in state. Age is a numeric variable representing individuals' age in months. Average age in state allows for consideration of differences in the average age of students in each state. It is computed by averaging the ages of all students sampled in each state. One source of heterogeneity in average ages of students by state may be that students in wealthier, more urban states are more likely to be redshirted. Another source may be sampling of specific schools across the

[11] A number of extensions like GPA in specific subjects and admission into selective colleges are also considered. For college selectivity Barron's selectivity rating which a 3 point scale: 'not selective,' 'moderately selective,' 'very selective' is used. A multinomial regression is used to test college selectivity and a weighted least squares approach for all others

US. This term is included because logically having older classmates diminishes the relative age effect.

This paper builds on the results of Datar (2006) by more rigorously considering socioeconomic status. Datar (2006) employed a binary measure for socioeconomic status (categorizing individuals as either poor or not poor). In contrast, this paper uses a categorical variable with thirteen categories measuring family income in 2008. The lowest provided is $<$ \$15,000, the highest is $>$ \$235,000, and each category in between has a width of \$20,000. The categorical variable was converted to a numeric variable by assigning each observation the mean of its income category. Any family in the category $>$ \$235,000 a household income of \$235,000 because calculating the mean of the top category was not possible without an upper bound choosing \$235,000 offers the most conservative estimate of the effect of wealth.

Becker and Tomes (1958) suggest that family background, such as socio-economic status and parents' education, are predictive of investments into children's human capital. Additionally, Elder and Lubotsky (2009) find that pre-kindergarten training is an important predictor of academic performance later in life. Therefore both family income and parent education are included in the model. Parent education contains two categorical variables with highest education levels for mother and father ranging from less than high school diploma to a graduate degree. The bachelor's degree category is used as the control group. These variables are also used to implicitly control for pre-kindergarten training.

The existing literature as well suggests that both sex and race are predictors of individual outcomes like academic achievement (Elder and Lubotsky, 2009; Beneito and Soria-Espin, 2020). Sex is a dummy variable for male or female, and Race is a categorical variable for an individual's race, including multiracial backgrounds. Males and whites is used as the respective comparison groups.

Beneito and Soria-Espin (2020) suggest that women are more susceptible to the relative age effect. To test their claim I include an interaction term between sex and age. Additionally, to test whether older students get more attention, I test an Age^2 term. If older students get more attention, the relative age effect is a compound effect for the oldest students who receive both the effect of greater cognitive development and more attention from their teachers while students born in the middle of the year receive strictly the greater cognitive advantage effect. Therefore the squared term appropriately models older students getting more attention.[12]

Robustness Test

To test the robustness of my results I also test a regression discontinuity design and compare my results. In line with previous studies, I opt for a fuzzy regression discontinuity approach over a sharp regression discontinuity approach. When implementing the regression discontinuity, I treat birth month as an instrumental variable that 'randomly' assigns individuals to the 'relatively old' treatment group or the 'relatively young' control group. However, unlike in a sharp discontinuity approach, in a fuzzy discontinuity design I do not assume that the birthday cutoff assigns the treatment perfectly. For example, I do not assume that someone born a month before the threshold will be relatively young. They may be redshirted. Similarly, I do not assume that someone born after the threshold will be relatively old. They may be anti-redshirted.[13] Similar to a sharp RDD, I estimate the least squares regression:

$$\mathrm{Y}_i = \alpha + \beta_1 \,\text{Relatively Old}_i + \beta_2 \,(\text{Birth Month} - \text{Cutoff})_i + \gamma \,\mathbf{X_i}$$

where 'Relatively Old' is a dummy variable that takes the value of 1 if the individual

[12] I additionally test a series of model variations. The regression output for those can be found in appendix A.7

[13] Although I have year of birth and can therefore deduce who is either too old or too young to be in the cohort they are in, I do not wish to drop these observations because I want to compare my RDD estimates to my least squares estimates. In my least squares estimates I do not remove anyone who is too old or young (though I test whether my results are robust to this subsample). Therefore if I remove them in my RDD, I will be testing the RDD on a subsample of my sample for the OLS. For comparability I keep the sample the same between the two.

was born in a month after the threshold and 0 if they were born in a month before.[14] The variable (Birth Month - Cutoff) is a running variable the measures how far an individual's birth month lies from their state cutoff in months.[15] In Maine, Pennsylvania and Wyoming the cutoff do not lie at the end or beginning of a month. I use the month after the cutoff as their cutoff because this provides the most conservative estimate of the relative age effect.[16]

I also include two additional terms. The first is an interaction between sex and the running variable, sex:(Birth month - cutoff). The second is a squared running variable term, (Birth month - cutoff)^2. I include these terms term to mimic the least squares and logistic regressions as closely as I can. This helps make the estimate comparable. One final term I include is an interaction between Relatively Old and (Birth month - cutoff) which allows for different slopes on either side of the threshold. This allows as much flexibility to the RDD model as I can offer, while keeping the estimate of the relative age effect comparable to the primary model specification estimates.

The coefficient β_1 estimates the effect of being born just before versus just after the threshold—an almost 12 month age difference. But, because I do not assume that the treatment is perfectly assigned, I have to divide β_1 by how much being born after the cutoff increases the probability of being relatively old. I estimate that as:

[14] The month of the cutoff and the 5 months following are considered 'after' while the 6 months prior are considered 'before.'

[15] For example someone born in May in a state with a cutoff of September 1 will have a value of 4 while Someone born in October will have a value of 1. I acknowledge that day of birth is a better running variable but given my data constraints I use month of birth. Additionally, because I observe them in high school they may have moved between kindergarten and high school. This is not very concerning though because they are still 'relatively old' compared to their classmates in high school.

[16] By doing so I mis-assign some of their 'relatively old' students as 'relatively young.' This weakens the estimated effect of the treatment.

$$\Delta P(\textit{Relatively Old}) = P(\textit{Relatively Old} \mid \text{born after threshold}) - P(\textit{Relatively Old} \mid \text{born before threshold})$$
$$= [1 - P(anti - redshirted)] - P(redshirted)$$

Then I estimate the true effect of having a birth month before versus after the threshold as:

$$\frac{\partial\, Y_i}{\partial\, \textit{Relative Age}} = \frac{\beta_1}{\partial P(treatment)}$$

where $\frac{\partial\, Y_i}{\partial\, \textit{Relative Age}}$ is the true estimated effect of relative age on the outcome variable.[17] I compare this estimate to 12 times the coefficient on Age_i in my primary model specification to measure omitted variable bias.

Estimating Δ P

To estimate whether and how much being born before or after the threshold changes the probability of being relatively old I use data from the Minnesota Department of Education. For a discussion of this data, please refer to appendix A.2. Let t denote the year that a cohort of students begin kindergarten in Minnesota. Let A_t denote the set of students enrolled in, but not repeating, kindergarten in year t in Minnesota. Let $B_t \subseteq A_t$ be the set of students who are born more than 72 months before the birthday cutoff in year t. Let $C_t \subseteq A_t$ denote the set of students who are born less than 60 months before the birthday cutoff in year t. Note that $x \in B_t$ means that x was redshirted and $x \in C_t$ means that x was anti-redshirted. Figure 2

[17] Note that my analysis excludes the rare case that a student is born early in the academic calendar and is therefore relatively old, but their parents still choose to redshirt them (potentially due to emotional maturity concerns). It also excludes the converse: born late in the academic calendar and still anti-redshirted. This seems reasonable given the rates of redshirting or anti-redshirting. For further discussion on trends in redshirting and anti-redshirting, please reference Appendix A.2

demonstrates this.

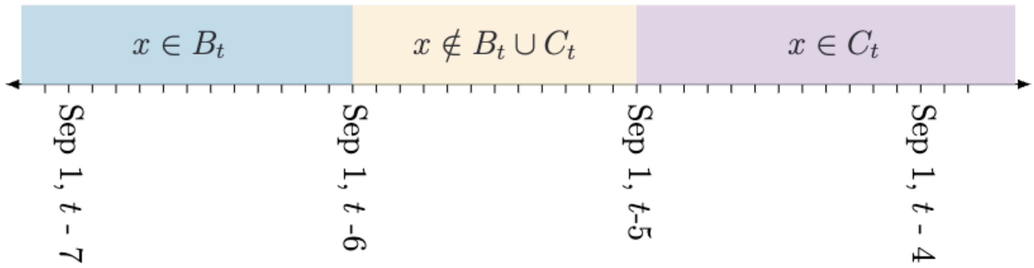

Figure 2: Timeline

Then $|A_t| - |B_t \cup C_t|$ is the number of students who are enrolled in kindergarten in year t and who were not redshirted or anti-redshirted. Additionally, B_{t+1} are students who enroll in year $t+1$ but should have enrolled in year t and C_{t-1} are students who enrolled in year $t-1$ but should have enrolled in year t. It follows that the total number of students who were born in the correct window to enroll in kindergarten in year t is:

$$\underbrace{|A_t| - |B_t \cup C_t|}_{\text{correctly enrolled}} + \underbrace{|B_{t+1}|}_{\text{redshirted}} + \underbrace{|C_{t-1}|}_{\text{anti-redshirted}} .$$

Therefore, I estimate the probability of being redshirted and anti-redshirted as:

$$P(redshirted) = \frac{|B_{t+1}|}{|A_t| - |B_t \cup C_t| + |B_{t+1} \cup C_{t-1}|}$$

$$P(anti - redshirted) = \frac{|C_{t-1}|}{|A_t| - |B_t \cup C_t| + |B_{t+1} \cup C_{t-1}|}$$

All together,

$$\Delta P(treatment) = [1 - \frac{|B_{t+1}|}{|A_t| - |B_t \cup C_t| + |B_{t+1} \cup C_{t-1}|}] - \frac{|C_{t-1}|}{|A_t| - |B_t \cup C_t| + |B_{t+1} \cup C_{t-1}|}$$

5 Data and Empirical Strategies

I use data from the High School Longitudinal Study of 2009 (HSLS: 09) which tracks a cohort of 23,503 ninth graders in 2009 from 944 high schools across the United States. The dataset follows them through high school and college and catalogues their academic performance through 2017 when they are 4 years out of college. In addition to the students, parents, counselors, and school administrators are also surveyed. Figure 3 shows the distribution of grade 9 entry ages for students in my sample.

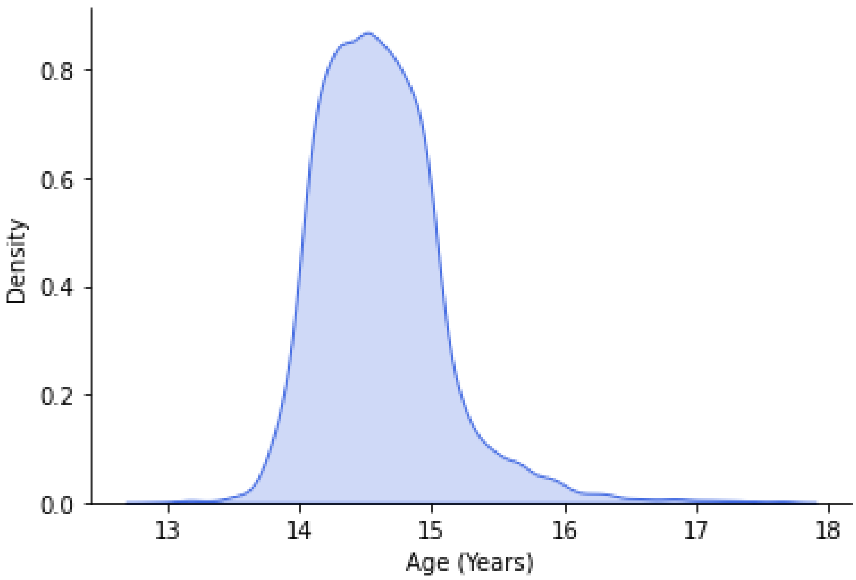

Figure 3: Distribution of Age at Grade 9 Entry

Note in figure 3 that the right tail is longer than the left tail. In line with the results I report in appendix A.2, redshirting is far more common than anti-redshirting. I suspect that students in the far right tail were both red-shirted and held back. As a robustness test I additionally estimate some regressions on a restricted sample of students who are all within 1 year of age.[18]

Scholars disagree about whether birth month is correlated with characteristics like nutrition, income, and race. While I cannot test for variables like gestation weight,

[18]This excludes any students who were redshirted, anti-redshirted, held back, skipped a grade. The regression output for these tests can be found in appendix A.6

I test for endogeneity between birth month and both race and poverty status in my dataset. No evidence of either appears. Appendix A.4 and A.5 contain figures with show patterns of birth months by poverty status and race.

The next page provides summary statistics for my data. The hispanic category combines two sub-categories: 'hispanic: no race specified' and 'hispanic: race specified.' Individual races are not specified for multiracial students. Therefore, they are omitted in my analysis.[19] The category 'trouble with coursework' refers to a survey asking parents how much trouble their child had with grade 9 coursework. The variables 'college,' 'high school dropout,' and 'work' refer to whether the student went on to college, worked after high school, or dropped out during high school. Finally, the variable 'hours worked in grade 12' measures on average how many hours per week students' worked during grade 12.

[19]The original data had categories for pacific islander and native american students but almost all observations where lost when dropping missing values so they were omitted before calculating summary statistics.

Summary Statistics:

Variable	Count	Percent
Family Income		
< \$15,000	621	5.3
\$15,000-\$35,000	1559	13.4
\$35,000-\$55,000	1790	15.3
\$55,000-\$75,000	1896	16.2
\$75,000-\$95,000	1490	12.8
\$95,000-\$115,000	1240	10.6
\$115,000-\$135,000	816	7.0
\$135,000-\$155,000	655	5.6
\$155,000-\$175,000	325	2.8
\$175,000-\$195,000	204	1.7
\$195,000-\$215,000	271	2.3
\$215,000-\$235,000	104	0.9
> \$235,000	700	6.0
Sex		
Female	5805	49.7
Male	5866	50.3
Race		
Asian	1089	9.3
Black	878	7.5
Hispanic	1977	16.9
White	7727	66.2
Mother's Education		
Less than HS	843	7.2
HS Diploma	4365	37.4
Associate's Degree	1778	15.2
Bachelor's Degree	2971	25.5
Master's Degree	1256	10.8
Graduate Degree	458	3.9
Father's Education		
Less than HS	1218	10.4
HS Diploma	4907	42
Associate's Degree	1404	12
Bachelor's Degree	2553	21.9
Master's Degree	951	8.1
Graduate Degree	638	5.5
School		
Private	2573	22
Public	9098	78
N	11670	

Variable	Count	Percent
Asian		
Female	552	50.7
Male	537	49.3
Public	873	80.2
Private	216	19.8
Black		
Male	448	51.0
Female	430	49.0
Public	680	77.4
Private	198	22.6
Hispanic		
Male	990	50.1
Female	987	49.9
Public	1611	81.5
Private	366	18.5
White		
Male	3891	50.4
Female	3836	49.6
Public	5934	76.8
Private	1793	23.2

Trouble With Coursework		
Lots of Trouble	8302	76.1
Some Trouble	2188	20.1
No Trouble	416	3.8
College		
Yes	6801	66.8
No	3376	33.2
High School Dropout		
Yes	700	6.0
No	10971	94.0
Work		
Yes	1028	10.1
No	9149	89.9

Variable	Mean	SD
Overall GPA	2.9	0.8
Grade 9 GPA	2.9	0.8
SAT	1034.5	202.1
Hours Worked in Grade 12	25.9	10.6

6 Results

Early High School Outcomes

Table 1 presents regression results evaluating the effect of relative age on early high school outcomes. The first two columns display the results for a multinomial regression examining the impact of age on the likelihood of facing difficulties in grade 9 coursework, while the last column assesses the effect of age on grade 9 GPA.

The findings indicate that age has a statistically insignificant and near 0 effect on the probability of encountering difficulty in grade 9 coursework. The results also reveal a statistically significant relationship between age and grade 9 GPA. An additional month of age is associated with a 0.03 GPA point decrease, equivalent to a 0.04 standard deviation reduction. Then, the impact of age on ninth-grade GPA is small but not negligible—for students born almost a year apart the model predicts a half a standard deviation difference in performance.

The effect of parental education on early high school outcomes is more pronounced even for students born just one month apart. Ceteris paribus, a student whose mother didn't complete high school is, on average, 91 percent more likely to have trouble with grade 9 coursework than a student whose mother has a bachelor's degree. Additionally, their child's GPA is, on average, 0.3 standard deviations lower. Ceteris paribus, a student whose father didn't complete high school is, on average, 58 percent more likely to find grade 9 coursework difficult compared to a student whose father has a bachelor's degree. They also perform, on average, 0.4 standard deviations worse. Higher maternal and paternal education substantially decreases the probability of a child encountering some difficulty, although the impact on lots of difficulty is more modest.[20] Family income is significantly negatively correlated with probability of experiencing academic difficulty and a positively correlated with high school GPA.

[20]However, estimates of the effect of maternal and paternal education lose statistical significance at higher levels of educational attainment, likely due to a limited number of observations.

	P(Some Difficulty)	**P(Lots of Difficulty)**	**Grade 9 GPA**
Age	0.01	0.02	-0.03***
	(0.02)	(0.03)	(0.01)
(Mother) Less than HS	0.26**	0.65***	-0.23***
	(0.13)	(0.24)	(0.04)
(Mother) HS Diploma	0.23***	0.40**	-0.28***
	(0.07)	(0.16)	(0.02)
(Mother) Associate's Degree	0.23***	0.41**	-0.21***
	(0.08)	(0.18)	(0.02)
(Mother) Master's Degree	0.00	0.29	0.05*
	(0.10)	(0.21)	(0.03)
(Mother) Graduate Degree	-0.23	0.27	0.27
	(0.17)	(0.35)	(0.05)
(Father) Less than HS	0.46***	0.29	-0.34***
	(0.11)	(0.22)	(0.03)
(Father) HS Diploma	0.26***	0.05	-0.16***
	(0.08)	(0.16)	(0.02)
(Father) Associate's Degree	0.11	0.12	-0.08***
	(0.10)	(0.19)	(0.03)
(Father) Master's Degree	0.06	-0.22	0.06*
	(0.11)	(0.25)	(0.03)
(Father) Graduate Degree	0.06**	-0.86**	0.06
	(0.14)	(0.41)	(0.04)
Female	-3.01***	-2.12	-0.51
	(1.43)	(2.63)	(0.42)
Asian	-0.79***	-0.78***	0.23***
	(0.11)	(0.25)	(0.04)
Black	-0.05	-0.05	-0.48***
	(0.09)	(0.19)	(0.03)
Hispanic	-0.13***	-0.12	-0.33***
	(0.07)	(0.14)	(0.02)
ln(Income)	-0.08***	-0.10*	0.07***
	(0.03)	(0.05)	(0.01)
Female:Age	0.04	0.10*	0.01
	(0.03)	(0.06)	(0.01)
Age^2	0.01	0.05*	0.01*
	(0.02)	(0.03)	(0.01)
Female:Age^2	-0.02	-0.10*	-0.01
	(0.03)	(0.06)	(0.01)
Average State Age	-0.02	-0.11**	0.01***
	(0.02)	(0.04)	(0.01)
Intercept	-2.39	4.81	2.85***
	(3.58)	(7.29)	(0.98)
R^2	0.05	0.05	0.25
N	10910	10910	10660

$* : p < .1$, $** : p < .05$, $*** : p < .01$

Table 1: Early High School Outcomes

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)

By high school entry (typically age 14), women outperform their male peers but are also more likely to report having difficulty.[21] Asian students have less trouble with coursework and, on average, perform 0.3 standard deviations better than their white peers. While the coefficients on black and hispanic are largely insignificant, they indicate that black and hispanic students are marginally less likely to report difficulty in coursework but perform notably worse—on average, 0.6 and 0.4 standard deviations below their white peers. Finally, the coefficients on Age^2 and the interaction between sex and age do not suggest that women have a greater relative age effect at the high school level or that older students receive more attention from teachers.[22]

The VIF test for multicollinearity indicates that average state age has a high degree of multicollinearity—a VIF of 28. Average state age is constructed from age so multicollinearity is expected. Despite the level of multicollinearity, the model continues to include average state age both because of its theoretical significance and because this paper is not particularily concerned with the coefficient on average state age—the model includes it only to be careful when estimating the relative age effect.

The public and private school categorical variable also shows signs of multicollinearity—a VIF of roughly 10. This is reasonable because attending private school is costly and thus requires greater family income. Additionally, attending public schools signals a high value for education which is reasonably correlated with parental education. Because the coefficient on public schools is extremely strong despite concerns about multicollinearity—which inflates standard errors—the empirical model is not modified.

Now I turn to the robustness test described in the empirical section. The complete regression discontinuity output can be found in appendix A.8. According to data from the Minnesota Department of Education, $\partial P = 0.92$, indicating that being born after the cutoff increases the probability of being relatively old by 92 percentage points.

The RDD output suggests that being 12 months older decreases GPA by 0.08 percentage points. The least squares estimate indicates that being 12 months older is associated with a 0.36-point decrease in GPA. Although these estimates deviate in magnitude, the direction of the effect remains consistent—indicating the the least squares estimates are causal but biased upward.[23]

[21]The coefficient on women having some difficulty is statistically significant while the coefficients on having lots of difficulty and their grade 9 GPA are insignificant.

[22]In accordance with the National Center for Education Statistics requirements, the observation counts in all tables have been rounded to the nearest 10.

[23]Fuzzy RDD estimates should be regarded as signals of omitted variable bias rather than precise estimates of the true effect. The R^2 of the least squares estimates is notably higher, and, as discussed in appendix A.3, discontinuity estimates may not accurately capture the true effect.

Late High School Outcomes

Table 2 presents regression results predicting overall performance in high school and performance on the SAT. Age, measured in months, has a negative impact on GPA. Specifically, an additional month of age decreases overall GPA and SAT performance by 0.04 standard deviations on average. Therefore students born in September perform, on average half a standard deviation worse than students born in August both in overall high school GPA and performance on the SAT.[24] Note that the effect of relative age on GPA is virtually identical between grade 9 GPA and overall GPA suggesting that the magnitude of the relative age effect does not change notably during high school.

Parental education is positively correlated with both overall GPA and SAT performance. Father's education predicts childrens' GPA slightly better than mother's educational. A father not having a high school diploma has a more detrimental impact on the child's high school GPA and a less pronounced effect on SAT scores compared to a mother not having a high school diploma. Additionally, income is positively correlated with both outcomes, but the effect is modest. The model indicates no significant interaction between sex and age or a non-linearity in the age effect.

[24] Additionally, an extra month of age is associated with a 0.2 percentage point average decrease in Math, Science, and English GPA. The regression output for these tests can be found in Appendix A.7.

	HS GPA	**SAT**
Age	-0.03***	-8.54***
	(0.00)	(1.81)
(Mother) Less than HS	-0.24***	-106.51***
	(0.03)	(14.40)
(Mother) HS Diploma	-0.25***	-51.97***
	(0.02)	(6.54)
(Mother) Associate's Degree	-0.19***	-42.13***
	(0.02)	(7.43)
(Mother) Master's Degree	0.07***	12.20
	(0.03)	(7.84)
(Mother) Graduate Degree	0.03	61.48***
	(0.04)	(14.89)
(Father) Less than HS	-0.35***	-93.14***
	(0.03)	(12.60)
(Father) HS Diploma	-0.16***	-58.61***
	(0.02)	(6.60)
(Father) Associate's Degree	-0.08***	-51.08***
	(0.02)	(8.13)
(Father) Master's Degree	0.04	32.03***
	(0.03)	(9.04)
(Father) Graduate Degree	0.08**	69.17***
	(0.04)	(11.99)
Female	-0.99***	-236.18
	(0.37)	(159.92)
Asian	0.14***	74.41***
	(0.03)	(10.72)
Black	-0.45***	-136.35***
	(0.02)	(8.73)
Hispanic	-0.27***	-30.96***
	(0.02)	(6.94)
ln(Income)	0.07***	15.51***
	(0.01)	(2.62)
Female:Age	0.01	4.55*
	(0.01)	(2.47)
Age^2	0.01*	2.79
	(0.00)	(1.71)
Female:Age^2	-0.00	-3.34
	(0.01)	(2.31)
Average State Age	0.00	3.14*
	(0.00)	(1.78)
Intercept	5.32***	1302.70***
	(0.88)	(324.57)
R^2	0.27	0.26
N	10990	5420

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

Table 2: Late High School Outcomes

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)

At age 14, women have GPAs on average almost one standard deviation above their male peers. Additionally, they perform, on average, 2.5 standard deviations better on the SAT—a dramatic effect. Black and Hispanic students, on average, perform about three-tenths and half a standard deviation worse than their white peers in overall high school outcomes. But the effect for black students is particularily strong on the SAT where they perform more a standard deviation worse then their white peers on average. Perhaps this reflects something about the structure of the exam or the nature of the questions. On average, Asian students outperform their white peers by about two-tenths of a standard deviation. These estimates are statically significant.

The findings related to high school GPA remain robust across various model variations and a restricted sample that excludes redshirted, anti-redshirted, or students held back a grade.[25] Additionally, the results for the ACT are in line with the results for the SAT. Please refer to appendix A.5 for more details.

The regression discontinuity estimates from Table **??** suggest that least squares estimates, despite offering a modest result for relative age, overstate the effect. Both the effect on overall GPA (0.10 vs 0.36) and SAT performance (22.8 vs 96) suggest that the least squares estimates are inflated. Still, both suggest that the effect is modestly negative, offering support for the least squares estimates being causal.

Outcomes Beyond Academic Performance

The preceding results suggest that the relative age effect on academic outcomes in high school diminishes to nearly zero and may become modestly negative, even when excluding students who are too old or too young to be correctly enrolled. To comprehend why the relative age effect may turn slightly negative, I draw on insights from Angrist and Krueger (1991) regarding the impact of mandatory schooling policies on dropout rates for older students.

Figure 6 presents the regression output for a model studying the effect of age on decisions to attend college, begin working after college, work during high school, or drop out during high school. This model incorporates interactions between sex and race, as well as race and public/private school. The first three columns depict three logistic regressions measuring the probability of going to college, working after high school, or dropping out. Because these groups are not exclusive, three logistic regressions are implemented instead of one multinomial.[26]

[25]For these results, please refer to appendix A.6.

[26]A student can drop out and then obtain their GED and still go to college or can simultaneously work and go to college.

Holding other factors constant, an additional month of age decreases the probability of attending college by roughly 3.5 percent on average. Additionally, an additional month of age increases the probability of working similarly. Consistent with Angrist and Krueger's findings, the results identify a positive relationship between age and the probability of dropping out—an increase of 7 percent on average. The regression also indicates that an additional month of age increases the number of hours worked by 0.03 standard deviations. Parental education is also positively correlated with the probability of attending college and negatively associated with the probability of working after college and dropping out.[27]

In general, women are more likely to go to college than men of the same race. White women are 40 percent more likely than white men.[28] While the coefficients on the female race interaction terms are insignificant, they suggest that minority women, in general, are less likely to go to college compared to white men. Women are also almost 40 percent less likely to work and not go to college and are only 2 percent less likely to dropout, though the coefficient for dropping out is insignificant. The coefficients on black and Hispanic are also insignificant. They suggest that black and hispanic students are less likely to work after high school or drop out compared to their white classmates.

[27] Statistical significance is lost at the upper levels of education

[28] The coefficients on Hispanic and Black women are not significant.

	P(College)	P(Work)	P(Dropout)	Hours Worked
Age	-0.04***	0.03***	0.08***	0.24***
	(0.00)	(0.01)	(0.01)	(0.03)
(Mother) Less than HS	-0.62***	0.74***	0.42**	2.14***
	(0.12)	(0.18)	(0.19)	(0.80)
(Mother) HS Diploma	-0.42***	0.83***	0.44***	1.11***
	(0.07)	(0.12)	(0.14)	(0.41)
(Mother) Associate's Degree	-0.28***	0.70***	0.35**	0.84*
	(0.07)	(0.14)	(0.16)	(0.47)
(Mother) Master's Degree	-0.02	-0.05	0.14	0.20
	(0.09)	(0.21)	(0.20)	(0.53)
(Mother) Graduate Degree	-0.13	0.03	0.35	0.63
	(0.13)	(0.36)	(0.32)	(1.00)
(Father) Less than HS	-0.49***	0.96***	0.55***	1.97***
	(0.10)	(0.16)	(0.18)	(0.68)
(Father) HS Diploma	-0.27***	0.88***	0.29**	1.68***
	(0.07)	(0.13)	(0.15)	(0.42)
(Father) Associate's Degree	-0.07	0.49***	0.30*	0.82
	(0.08)	(0.16)	(0.17)	(0.52)
(Father) Master's Degree	-0.05	-0.42	0.32	0.91
	(0.10)	(0.27)	(0.22)	(0.60)
(Father) Graduate Degree	-0.08	-0.38	-0.16	0.49
	(0.12)	(0.36)	(0.34)	(0.82)
Asian	-0.54***	0.14	-1.52	-4.27
	(0.19)	(0.63)	(1.04)	(3.76)
Black	-0.01	-1.07	-0.26	-2.71
	(0.21)	(0.74)	(0.62)	(2.78)
Hispanic	0.07	-0.14	0.48	-1.21
	(0.17)	(0.39)	(0.35)	(1.73)
Public	-0.52***	1.29***	0.87***	-0.46
	(0.07)	(0.17)	(0.19)	(0.52)
Female	0.34***	-0.53***	-0.01	-3.43***
	(0.05)	(0.08)	(0.10)	(0.33)
Asian:Public	0.78***	-1.33**	1.12	-3.12
	(0.19)	(0.66)	(1.04)	(3.72)
Black:Public	0.15	0.42	0.32	0.56
	(0.22)	(0.75)	(0.63)	(2.80)
Hispanic:Public	0.09	-0.17	-0.53	1.82
	(0.17)	(0.39)	(0.35)	(1.72)
Asian:Female	-0.35**	-1.07*	0.06	2.13
	(0.15)	(0.56)	(0.36)	(1.90)
Black:Female	-0.05	0.12	-0.60*	2.35*
	(0.17)	(0.30)	(0.32)	(1.21)
Hispanic:Female	-0.07	0.06	-0.08	-0.93
	(0.12)	(0.18)	(0.20)	(0.74)
ln(Income)	0.19***	-0.16***	-0.24***	-0.90***
	(0.02)	(0.04)	(0.04)	(0.16)
Average State Age	-0.00	0.09***	-0.05	0.43***
	(0.02)	(0.03)	(0.03)	(0.11)
Intercept	5.43*	-22.30***	-4.52	-77.38***
	(3.17)	(5.18)	(5.89)	(19.74)
R^2	0.07	0.14	0.11	0.11
N	10180	10180	11670	5050

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: College and Work Regressions

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)

Family income is positively correlated with the probability of going to college college and negatively correlated with the probability of only working after high school or dropping out. The effect of parental education on college attainment is concentrated at the lower rungs of education attainment. Parental education beyond a bachelor's does not significantly improve the probability of attending college. Parental education is negatively correlated with decisions to work after high school and decisions to dropout.[29] This may be a consequence of the small number of observations in this category.[30]

Students in public schools are roughly 40 percent less likely to attend college, more than three times more likely to work after high school, and more than twice as likely to drop out as their private school peers. While the direction of these effects aligns with expectations, the magnitudes are striking. These results underscore a significant divide between public and private schools.

Table A.8 provides the RDD output for the previous regression. Again, the RDD estimates lack significance but indicate that least squares estimates contain omitted variable bias. Despite this the direction of the effect coincides, (-0.1 vs -0.48 for P(College), 0.02 vs 0.36 for P(Work), 0.01 vs 0.96 for P(Dropout), and 1.42 vs 2.88 for hours worked).

7 Discussion and Conclusions

Academic Outcomes

Deciphering the true effect of the relative age effect requires reconciling the primary model specification results with the RDD results. The primary model specification estimates suggest than in high school the relative age effect may turn negative, with relatively older students performing worse than relatively young students. The RDD estimates suggest a smaller and near zero effect of relative age on nearly all outcomes considered. But the coefficient estimates lack statistical significance and the models themselves do not explain much of the variation in outcomes considered. Recognizing the evidence on omitted variable bias, I conservatively conclude that, when considering

[29] The coefficients on mother's master's or graduate degree are positive but statistically insignificant

[30] In appendix A.5 I run a multinomial regression on college selectivity using Barron's ratings. The results suggest that an additional month of age reduces the probability of admission to both moderately and very selective colleges by roughly 3 percent. These coefficients are not significant.

GPA, difficulty in coursework, or performance on the SAT relative age effects do not persist into high school for US students.

In line with Elder and Lubotsky who find that the relative age effect disappears by grade 8, this paper offers evidence that relative age effects do not persist into high school—and if they do they are modestly negative for older students. Notably, this result contrasts what Dhuey et al. (2017) found in Florida and suggests that their results do not generalize to the entire United States. Contrary to Kawaguchi (2011), Attar and Cohen-Zada, and Smith (2009), this paper finds that the relative age effect does not persist as long in the United States as it does abroad. While identifying what feature of the US education system leads it to dissipate more quickly is challenging, this paper finds compelling evidence that mandatory schooling policies tell part of the story.

Other scholars who have studied the relative age effect in primary school have suggested that intervention by the government and schools may be warranted to even the playing field for relatively younger students. Some scholars have even argued that, because of cognitive maturity differences during childhood, boys should start a year after girls (Reeves 2022). These findings suggest that intervention is not necessary—by early adulthood relative age effects wither. Consequently, in the long run redshirting does not offer strong benefits. Thus, this paper offers evidence that parents who redshirt children that are cognitively and emotionally ready to begin kindergarten strictly to give them a cognitive advantage ultimately do not succeeding in giving their child a leg up on their peers.

Effect on Dropouts

I offer greater weight to the primary model specification estimates when considering decisions about going to college or dropping out. When considering whether to go to college, even the RDD estimates suggest, in line with Angrist and Krueger (1991), that relative age has a significant impact. Additionally, when considering the decision to dropout, the primary model specification is strong enough—and the RDD estimate is weak enough—that I am less concerned by omitted variable bias concerns.

This paper offers evidence that the structure of US mandatory schooling policies may be harming student outcomes. I acknowledge that mandatory schooling policies are changing. When Angrist and Krueger (1991) wrote their paper, most of the United States required schooling till 16. Now most require schooling till 17 with a select few requiring schooling till 18. Furthermore some states have added contingencies like the student must be 16 years old or have completed 10th grade. This paper offers a positive outlook on these changes. But it finds that the older students are still more likely to dropout.

Eliminating age-based mandatory schooling policies entirely may result in students staying in school longer than desired by both them and taxpayers. However, implementing contingencies that allow students to drop out after reaching a specific age or education level can improve the graduation rate among older students. Crucially, that the age threshold in mandatory schooling policies must be sufficiently high to not incentivize them to dropout early. For example, I propose a policy mandating students to attend school until the age of 18 or the completion of 11th grade.

This paper resolves the apparent contradiction in the literature review regarding older students performing better yet having a higher likelihood of dropping out. My research, examining later outcomes than the related literature, indicates that older students do not outperform younger students in high school and may even perform worse, clarifying their increased likelihood of dropping out.

One plausible explanation for the lower college attendance rates among older students, consistent with my results, is their early entry into the workforce. In the US, students can start working limited hours at age 14 and full-time at age 16. Consequently, older students may commence working earlier in their education, preferring employment over schooling. Indeed, my findings show that older students work more during their senior year of high school, are more likely to dropout, and less likely to go to college.

Other Outcomes

I did not find evidence indicating a stronger relative age effect for either sex. The coefficients on the interaction between sex and age were statistically insignificant and close to zero. Likewise, my results do not support the notion that older students receive more attention from teachers, as the coefficient on the age squared term was similarly insignificant and close to zero.

In line with existing literature, my results highlight the substantial impact of family income and parental education on children's success in school and decisions to pursue higher education. I speculate that both family income and parental education levels signal the value placed on education.

I observe significant disparities between public and private school students. Consistent with Cook and Kang (2016), public school students are markedly more likely to dropout and less likely to attend college. While some of this difference may stem from varying levels of value for education among students and families in public and private schools, interventions to reduce dropout rates and increase college attendance at public schools may be necessary.

While, for the majority of my analyses, the coefficients on black and Hispanic were statistically insignificant, when significant, they indicated that black and Hispanic students perform worse than their white classmates, as measured by cumulative high school GPA and SAT performance. Notably, the effect for black students on the SAT was roughly twice as large as the effect in cumulative GPA (0.5 versus 1 standard deviation). Although this paper does not delve into the reasons behind the dramatic SAT effect for black students, it provides evidence that this is a question worthy of exploration by future scholars.

Limitations and Future Research

One significant limitation of my research was the absence of data on pre-kindergarten training. Although I utilized imperfect proxies—family income and parental education—future researchers should actively seek datasets with an explicit variable addressing pre-kindergarten training. Furthermore, my dataset lacked sufficient observations to adequately consider Pacific Islander and Native American students. Additionally, I had to exclude multiracial students altogether. A more comprehensive examination of the relative age effect should encompass these student groups.

Both prior scholars and I have operated under the assumption that the relative age effect remains consistent for any two consecutive months. While this assumption appears reasonable, given concerns about the endogeneity of birthdays, future researchers should rigorously test this assumption, potentially using a series of month dummy variables. In addition, my robustness test would have been significantly strengthened by data on day of birth which is a better running variable than month of birth. I hope other researchers find my argument for a least squares approach compelling and choose to implement the same robustness test, but I encourage them to utilize the day of birth as a running variable. Finally, future researchers should seek out data on income as a numeric variable rather than a categorical variable.

The study of relative age effects would benefit from longer and more extensive longitudinal datasets, enabling the tracking of the relative age effect on the same students from kindergarten through college. My analysis commenced with students in high school, overlooking cases where students, for example, moved from a state with an earlier cutoff to a state with a later cutoff. Researchers who want to continue studying relative age effects should move away from using national longitudinal studies and should consider using district wide administrative datasets. While gathering and cleaning administrative data is challenging, doing so will allow them to have far more observations and far richer data on students.

Conclusion

This paper evaluates whether relative age effects persist into high school. It concludes that by high school older students do not have an advantage over younger students in academic performance. But it conversely finds that older students choose to dropout more frequently than younger students and older students choose to go to college less frequently than younger students.

My research motivates the question of when relative age effects fall to 0. Future researchers should also consider studying late middle outcomes. While I, in line with Elder and Lubotsky (2009) has shown that older students do not out perform younger students in high school, I have not established when the relative age effect goes away. Future analyses should focus on building on the results of Dhuey et al. (2017) and Elder and Lubotsky (2009) to establish national consensus on when relative afe effects dissipate.

In conclusions, I believe my results firmly establish that cognitive advantages for older students dissipate by high school entry. Parents of students born around the kindergarten entry cutoffs should breathe a sigh of relief and recognize that ultimately the decision of whether to enroll their child in one school cohort or another appears inconsequential.

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A Appendix

A.1 Kindergarten Cutoffs by State

State	Cutoff	Avg Age (Years)	Count
Hawaii	5 by 7/31	14.28	7
North Dakota	5 by 7/31	14.71	27
Kentucky	5 by 7/31	14.74	178
Maine	5 by 10/15	14.80	16
South Dakota	5 by 9/1	15.01	27
Alabama	5 by 9/1	14.80	223
Alaska	5 by 9/1	14.65	44
Arizona	5 by 8/31	14.70	197
Arkansas	5 by 8/1	14.81	61
California	5 by 9/1	14.51	679
Colorado	5 by 10/1	14.64	189
Connecticut	5 by 1/1	14.54	65
Delaware	5 by 8/31	14.71	65
Florida	5 by 9/1	14.74	568
Georgia	5 by 9/1	14.85	572
Idaho	5 by 9/1	14.63	22
Illinois	5 by 9/1	14.70	511
Indiana	5 by 8/1	14.86	393
Iowa	5 by 9/15	14.71	75
Kansas	5 by 8/31	14.67	116
Louisiana	5 by 9/30	14.84	245
Maryland	5 by 9/1	14.47	192
Nevada	5 by 9/30	14.60	35
New Hampshire	Between 8/15 and 10/31	14.71	65
New Mexico	5 by 9/1	14.67	41
North Carolina	5 by 8/31	14.74	619
Ohio	Either 8/1 or 9/30	14.77	728
Oregon	5 by 9/1	14.55	101
Pennsylvania	Implicitly 1/25	14.68	663
Rhode Island	5 by 9/1	14.54	20
South Carolina	5 by 9/1	14.75	177
Tennessee	5 by 8/15	14.73	631
Texas	5 by 9/1	14.77	718
Utah	5 by 9/1	14.56	10
Vermont	Between 8/31 and 1/1	14.68	49
Washington	5 by 8/31	14.68	457
Wisconsin	5 by 9/1	14.64	152
Wyoming	5 by 9/15	14.74	27
District of Columbia	5 by 9/30		

A.2 Minnesota Department of Education Data

From the Minnesota Department of Education I gathered data on the number of students entering, but not repeating, kindergarten in the years 1998-2023. I also received data on the number of students who were less than 59 months or more than 72 months old at entry. Because this data is gathered at the kindergarten level, the only explanation for a student being in one of those groups is that they were redshirted or anti-redshirted.[31]. I then conduct the analyses I discuss in section 5. Here I motivate the use of data from Minnesota discuss trends in redshirting and anti-redshirting.

[31]Students cannot skip or repeat a grade before starting kindergarten

From this data I seek to estimate national rates of redshirting and anti-redshirting. Barring national data on this, I use data from Minnesota to proxy national rates. While this is an imperfect measure, I consider it an appropriate one. The proportion of Minnesota that is urbanized does not differ notably from national rates of urbanization (73 percent vs 83 percent). Additionally the GINI index for Minnesota and the US suggests that the level of income inequality in Minnesota closely models income inequality in the United States (0.456 vs 0.47). Then, in the factors that correlated with probability of redshirting, Minnesota closely models the United States.

Figure 4 depicts rates in redshirting and anti-redshirting over time. In line with previous results, this data suggests that redshirting is far more common than anti-redshirting. I speculate that redshirting that anti-redshirting is easier because a parent does not need special permission from the school to redshirt.

Notably, anti-redshirting fell dramatically in popularity in the early 2000's. Rates of redshirting remained fairly stable in popularity until the first year of the pandemic when it grew, likely as parents sought to protect their children from the virus, and fell subsequently.

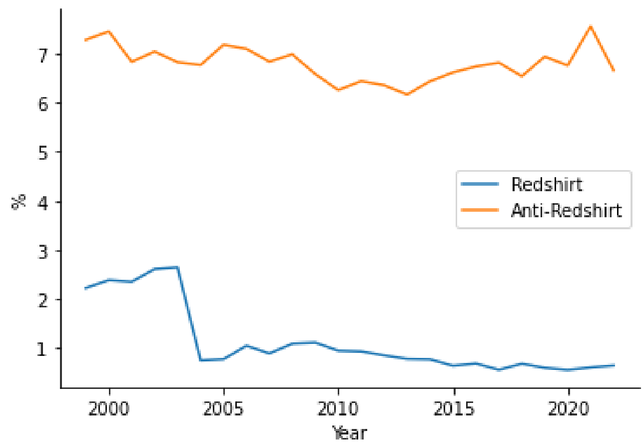

Figure 4: Rates of Redshirting and Anti-redshirting

A.3 My Concerns About A Discontinuity Design

Previous studies, like Dhuey et al. (2017), used a regression discontinuity design to explore the relative age effect. However, I argue that RDD might not be the most suitable method for accurately gauging this effect. The estimate generated by the discontinuity method lacks practical significance, representing a local average treatment effect over a twelve-month period. To infer a one-month effect, researchers need to assume a linear month-to-month relative age effect. While that is a largely reasonably assumption, the findings of this paper provide mixed evidence regarding the linearity of the relative age effect. While least squares estimates suggest no squared effect of relative age, the discontinuity estimates modestly indicate a non-linearity in the relative age effect (see the coefficient on (Birth Month - Age)^2 in appendix A.8).

Beyond concerns about the meaningfulness of RDD estimates, the relative age effect, in my view, does not meet the necessary conditions for a discontinuity design. One such condition is the continuous running variable—which I lack. I use the distance of birth months from the cutoff, a discrete variable with values between 0 and 6. Even studies with data on the day of birth use a discrete variable—albeit one that better approximates a continuous variable. Consequently, RDD estimates of the relative age effect, while less biased than least squares estimates, still exhibit some bias. Dhuey et al. (2017) employ a regression model as follows:

$$Y_i = \beta Sept_i + \gamma X_i + \epsilon_i$$

where $Sept_i$ is a dummy variable with a value of 1 for September-born children and 0 for August-born children. This design lacks a running variable altogether, estimating a level effect between months rather than a discrete change in treatment over a continuous variable.

While the distance from the cutoff in days approximates a continuous variable, its discrete nature introduces bias in RDD estimates. Despite acknowledging that all estimates are biased, I contend that the reduction in bias when estimating relative age effects through RDD may not be sufficient to justify the associated tradeoffs in statistical power and the ability to measure the effect globally. Consequently, I chose least squares as my primary model specification, using OLS only to assess relative omitted variable bias and to lend support to the causal nature of my estimates.

A.4 Endogeneity Between Birth Month and Poverty

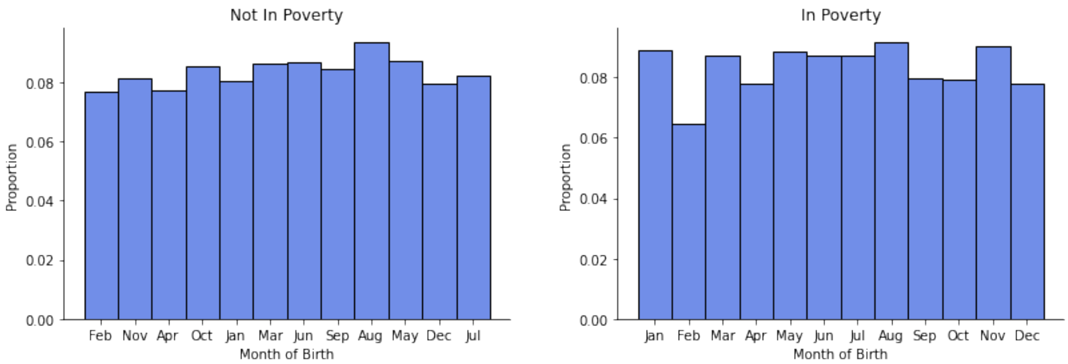

Figure 5: Month of Birth by Poverty Status

A.5 Endogeneity Among Birth Month and Race

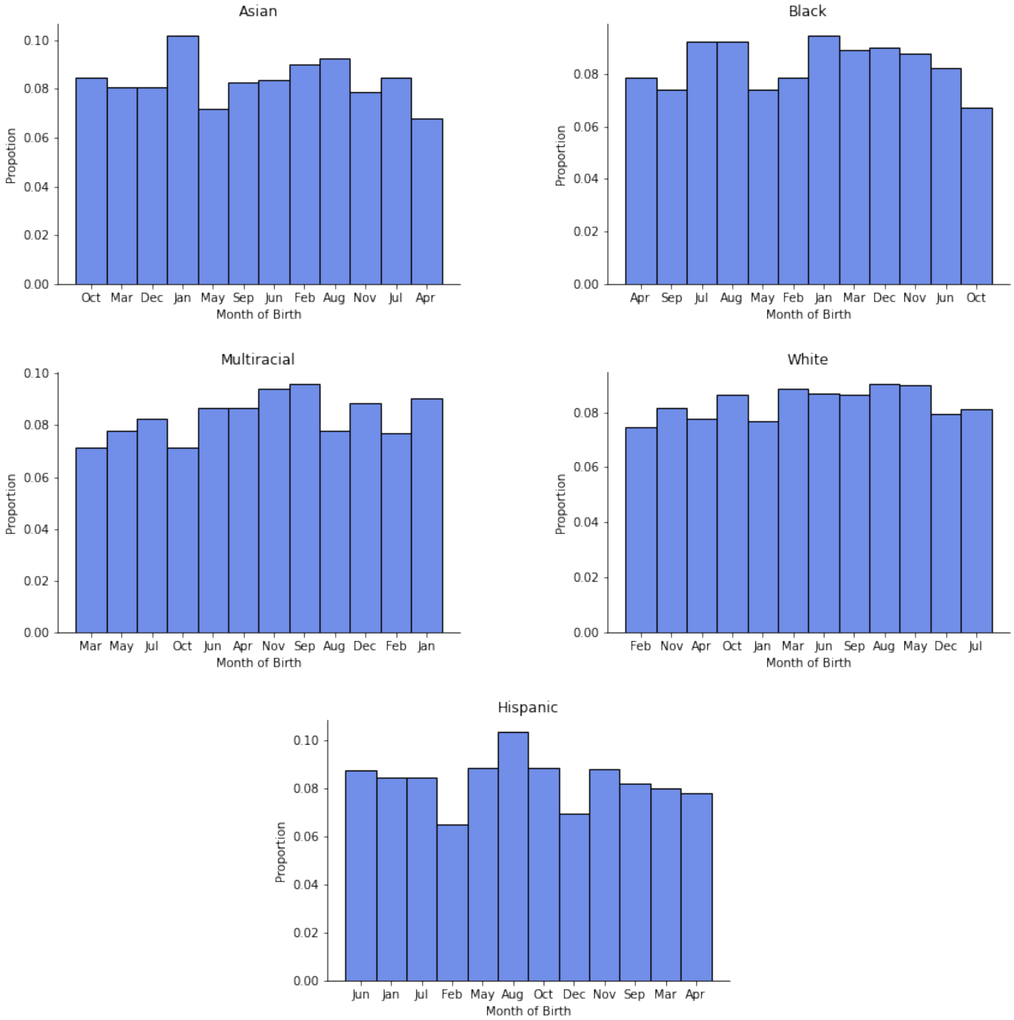

Figure 6: Month of Birth by Race

Regression Output

	ACT	Moderately Selective	Very Selective
Age	-0.20***	-0.03	-0.03
	(0.04)	(0.04)	(0.04)
(Mother) Less than HS	-2.69***	-0.63**	-0.53
	(0.36)	(0.31)	(0.35)
(Mother) HS Diploma	-1.34***	-0.45***	-0.74***
	(0.16)	(0.13)	(0.14)
(Mother) Associate's Degree	-1.07***	-0.60***	-0.95***
	(0.18)	(0.14)	(0.15)
(Mother) Master's Degree	0.27	-0.01	0.20
	(0.19)	(0.16)	(0.16)
(Mother) Graduate Degree	1.52***	0.20	0.97***
	(0.37)	(0.34)	(0.33)
(Father) Less than HS	-2.25***	-0.41*	-0.92***
	(0.31)	(0.24)	(0.28)
(Father) HS Diploma	-1.49***	-0.11	-0.54***
	(0.16)	(0.13)	(0.13)
(Father) Associate's Degree	-1.32***	-0.15	-0.70***
	(0.20)	(0.15)	(0.16)
(Father) Master's Degree	0.81***	0.06	0.31*
	(0.22)	(0.19)	(0.18)
(Father) Graduate Degree	1.74***	0.21	0.74***
	(0.30)	(0.26)	(0.25)
Female	-6.45	-0.86	-0.63
	(3.97)	(3.26)	(3.52)
Asian	1.85***	0.21	1.14***
	(0.27)	(0.19)	(0.19)
Black	-3.33***	-0.91***	-1.23***
	(0.22)	(0.14)	(0.17)
Hispanic	-0.75***	-0.31**	-0.14
	(0.17)	(0.14)	(0.15)
lnInc	0.36***	0.05	0.29***
	(0.06)	(0.05)	(0.05)
Female:Age	0.12*	0.04	0.09*
	(0.06)	(0.05)	(0.06)
Age^2	0.07	0.00	0.00
	(0.04)	(0.04)	(0.04)
Female:Age^2	-0.09	-0.04	-0.08*
	(0.06)	(0.05)	(0.06)
Average State Age	0.07	-0.07*	0.10*
	(0.04)	(0.04)	(0.04)
Intercept	29.76***	18.20***	20.09**
	(8.05)	(6.74)	(7.17)
R^2	0.26	0.09	0.09
N	5420	5230	5230

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

Table 4: Additional College Regressions

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)

A.6 Regression Output (Restricted Samples)

	GPA	SAT	P(College)	P(Dropout)
Age	-0.03***	-8.26***	-0.03***	0.08***
	(0.00)	(1.86)	(0.00)	(0.01)
(Mother) Less than HS	-0.23***	-108.20***	-0.61***	0.45**
	(0.03)	(14.62)	(0.12)	(0.20)
(Mother) HS Diploma	-0.24***	-50.01***	-0.43***	0.42***
	(0.02)	(6.60)	(0.07)	(0.14)
(Mother) Associate's Degree	-0.19***	-43.43***	-0.28***	0.34**
	(0.02)	(7.47)	(0.08)	(0.16)
(Mother) Master's Degree	0.08***	13.19*	-0.03	0.09
	(0.03)	(7.90)	(0.09)	(0.21)
(Mother) Graduate Degree	0.03	65.09***	-0.14	0.36
	(0.04)	(15.02)	(0.13)	(0.33)
Less than HS	-0.35***	-89.90***	-0.49***	0.55***
	(0.03)	(12.83)	(0.10)	(0.19)
(Father) HS Diploma	-0.16***	-56.57***	-0.27***	0.33**
	(0.02)	(6.63)	(0.07)	(0.15)
(Father) Associate's Degree	-0.08***	-50.83***	-0.06	0.32*
	(0.02)	(8.16)	(0.08)	(0.18)
Master's Degree	0.05	31.84***	-0.05	0.38*
	(0.03)	(9.14)	(0.10)	(0.22)
Graduate Degree	0.08**	68.41***	-0.10	-0.11
	(0.04)	(12.13)	(0.12)	(0.34)
Female	-0.31	-95.53	0.34***	-0.02
	(0.43)	(177.93)	(0.05)	(0.11)
Asian	0.15***	72.81***	-0.57***	-1.35
	(0.03)	(10.91)	(0.19)	(1.04)
Black	-0.43***	-129.13***	-0.04	-0.17
	(0.02)	(9.09)	(0.21)	(0.62)
Hispanic	-0.27***	-31.55***	0.08	0.33
	(0.02)	(7.00)	(0.17)	(0.37)
ln(Income)	0.07***	14.73***	0.19***	-0.25***
	(0.01)	(2.65)	(0.02)	(0.04)
Female:Age	0.01*	4.37*		
	(0.01)	(2.55)		
Age^2	0.01**	2.74		
	(0.00)	(1.71)		
Female:Age^2	-0.01	-3.95*		
	(0.01)	(2.33)		
Average State Age	0.00	2.30	-0.01	-0.04
	(0.01)	(1.80)	(0.02)	(0.03)
Public			-0.51***	0.87***
			(0.07)	(0.19)
Asian:Public			0.80***	1.04
			(0.20)	(1.04)
Black:Public			0.17	0.35
			(0.22)	(0.63)
Hispanic:Public			0.09	-0.40
			(0.17)	(0.37)
Asian:Female			-0.35**	0.03
			(0.16)	(0.36)
Black:Female			-0.03	-0.64*
			(0.17)	(0.34)
Hispanic:Female			-0.06	-0.00
			(0.12)	(0.20)
Intercept	4.88***	1421.99***	5.73*	-6.13
	(0.90)	(330.15)	(3.22)	(6.07)
R-squared	0.26	0.24	0.06	0.09
N	10750	5340	9970	11400

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Restricted Sample Regressions

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)

A.7 Regression Output

	Overall GPA	Overall GPA	Overall GPA	Math GPA	English GPA	Science GPA	Overall GPA (Restricted)
Age	-0.02***	-0.02***	-0.03***	-0.02***	-0.02***	-0.02***	-0.03***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
(Mother) Less than HS	-0.23***	-0.24***	-0.23***	-0.30***	-0.27***	-0.27***	-0.23***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)
(Mother) HS Diploma	-0.25***	-0.25***	-0.25***	-0.27***	-0.28***	-0.26***	-0.24***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
(Mother) Associate's Degree	-0.19***	-0.19***	-0.19***	-0.22***	-0.21***	-0.19***	-0.19***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
(Mother) Master's Degree	0.07***	0.07***	0.07***	0.05*	0.08***	0.07**	0.08***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
(Mother) Graduate Degree	0.03	0.04	0.03	0.10*	0.06	0.13**	0.03
	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)	(0.05)	(0.04)
(Father) Less than HS	-0.35***	-0.35***	-0.35***	-0.36***	-0.32***	-0.48***	-0.35***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)
(Father) HS Diploma	-0.16***	-0.16***	-0.16***	-0.20***	-0.16***	-0.23***	-0.16***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
(Father) Associate's Degree	-0.08***	-0.08***	-0.08***	-0.10***	-0.09***	-0.11***	-0.08***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
(Father) Master's Degree	0.04	0.04	0.04	0.07*	0.07**	0.02	0.05
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
(Father) Graduate Degree	0.08**	0.08**	0.08**	0.11**	0.12***	0.07	0.08**
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)
Asian	0.14***	0.14***	0.14***	0.16***	0.16***	0.13***	0.15***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)
Black	-0.45***	-0.45***	-0.45***	-0.53***	-0.45***	-0.48***	-0.43***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
Hispanic	-0.27***	-0.27***	-0.27***	-0.33***	-0.26***	-0.31***	-0.27***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Female	0.28***	-1.01***	0.28***	0.24***	0.38***	0.25***	-0.31
	(0.01)	(0.37)	(0.01)	(0.02)	(0.01)	(0.02)	(0.43)
ln(Income)	0.07***	0.07***	0.07***	0.05***	0.06***	0.07***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Average State Age	0.00	0.00	0.00	0.01**	-0.01		0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)		(0.01)
Female:Age		0.01***					0.01*
		(0.00)					(0.01)
Age^2			0.01**				0.01**
			(0.00)				(0.00)
Female:Age^2							-0.01
							(0.01)
Intercept	4.68***	5.27***	4.74***	2.45**	6.70***	4.84***	4.88***
	(0.87)	(0.88)	(0.87)	(1.07)	(0.99)	(0.28)	(0.90)
R^2	0.27	0.27	0.27	0.21	0.24	0.23	0.26
N	10990	10990	10990	10960	10970	10940	10750

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

Table 6: Model Variations

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)

A.8 Fuzzy Regression Discontinuity Output

	Overall GPA	**Grade 9 GPA**	**SAT**
Relatively Old	-0.09	-0.07	20.98
	(0.17)	(0.17)	(32.54)
(Mother) Less than HS	-0.36***	-0.40***	-185.64***
	(0.12)	(0.12)	(23.05)
(Mother) HS Diploma	-0.32***	-0.27***	-134.35***
	(0.07)	(0.07)	(13.81)
(Mother) Associate's Degree	-0.25***	-0.19**	-103.73***
	(0.08)	(0.08)	(15.72)
(Mother) Master's Degree	0.03	0.01	52.08***
	(0.10)	(0.09)	(18.30)
(Mother) Graduate Degree	-0.14	-0.19	-56.66*
	(0.17)	(0.16)	(31.95)
(Father) Associate's Degree	-0.04	-0.02	-41.29**
	(0.09)	(0.09)	(17.53)
(Father) HS Diploma	0.04	0.03	-74.37***
	(0.07)	(0.07)	(14.12)
(Father) Less than HS	-0.08	-0.03	-143.14***
	(0.11)	(0.11)	(21.38)
(Father) Master's Degree	0.32***	0.32***	47.06**
	(0.11)	(0.11)	(20.66)
(Father) Graduate Degree	0.23	0.25*	31.32
	(0.14)	(0.14)	(27.10)
Asian	0.28**	0.26**	106.88***
	(0.13)	(0.12)	(23.92)
Black	-0.80***	-0.76***	-102.60***
	(0.09)	(0.09)	(16.82)
Hispanic	-0.29***	-0.19***	-73.67***
	(0.07)	(0.07)	(12.79)
Female	0.43***	0.48***	84.80***
	(0.07)	(0.07)	(13.02)
Female:Relatively Old	-0.10	-0.15	-26.88
	(0.10)	(0.10)	(18.52)
(Birth Month - Cutoff)	0.21**	0.19**	-22.68
	(0.10)	(0.09)	(18.28)
Relatively Old:(Birth - Cutoff)	-0.24**	-0.20**	29.54
	(0.10)	(0.10)	(19.49)
(Birth - Cutoff)^2	0.03**	0.02*	-1.09
	(0.01)	(0.01)	(2.57)
Relatively Old:(Birth - Cutoff)^2	-0.03**	-0.02*	0.79
	(0.01)	(0.01)	(2.64)
ln(Income)	0.13***	0.14***	32.68***
	(0.03)	(0.03)	(4.92)
Average State Age	0.00	0.01	12.75***
	(0.02)	(0.02)	(3.46)
Intercept	0.10	-1.49	-2251.98***
	(3.27)	(3.19)	(621.65)
R^2	0.03	0.03	0.10
N	11670	11670	11670

Table 7: Fuzzy Regression Discontinuity Estimates

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)

	P(College)	P(Work)	P(Dropout)	Hours Worked
Relatively Old	-0.09	0.02	0.01	1.42
	(0.15)	(0.24)	(0.29)	(1.00)
(Mother) Associate's Degree	-0.27***	0.68***	0.33**	0.79*
	(0.07)	(0.14)	(0.16)	(0.47)
(Mother) HS Diploma	-0.43***	0.84***	0.48***	1.12***
	(0.06)	(0.12)	(0.14)	(0.41)
(Mother) Less than HS	-0.67***	0.79***	0.56***	2.78***
	(0.11)	(0.17)	(0.19)	(0.81)
(Mother) Master's Degree	-0.01	-0.07	0.09	0.19
	(0.09)	(0.21)	(0.20)	(0.53)
(Mother) Graduate Degree	-0.12	-0.00	0.28	0.43
	(0.13)	(0.36)	(0.32)	(1.01)
(Father) Associate's Degree	-0.08	0.51***	0.34**	1.01*
	(0.08)	(0.16)	(0.17)	(0.53)
(Father) HS Diploma	-0.29***	0.89***	0.35**	1.87***
	(0.07)	(0.13)	(0.14)	(0.42)
(Father) Less than HS	-0.53***	0.99***	0.67***	2.28***
	(0.10)	(0.16)	(0.18)	(0.69)
(Father) Master's Degree	-0.05	-0.41	0.32	0.78
	(0.10)	(0.27)	(0.22)	(0.61)
(Father) Graduate Degree	-0.08	-0.37	-0.15	0.46
	(0.12)	(0.36)	(0.34)	(0.83)
Asian	-0.50***	0.09	-1.51	-4.80
	(0.19)	(0.63)	(1.04)	(3.79)
Black	0.03	-1.12	-0.33	-3.14
	(0.21)	(0.74)	(0.62)	(2.80)
Hispanic	0.10	-0.17	0.42	-1.19
	(0.17)	(0.39)	(0.35)	(1.75)
Female	0.46***	-0.58***	-0.32**	-3.20***
	(0.07)	(0.11)	(0.13)	(0.44)
Public	-0.51***	1.28***	0.87***	-0.51
	(0.07)	(0.17)	(0.19)	(0.53)
Asian:Public	0.80***	-1.33**	1.00	-3.07
	(0.19)	(0.66)	(1.04)	(3.74)
Black:Public	0.10	0.48	0.50	1.05
	(0.22)	(0.75)	(0.63)	(2.82)
Hispanic:Public	0.07	-0.14	-0.47	1.57
	(0.17)	(0.39)	(0.35)	(1.74)
Asian:Female	-0.37**	-1.06*	0.09	2.57
	(0.15)	(0.56)	(0.35)	(1.92)
Black:Female	-0.05	0.13	-0.55*	2.29*
	(0.17)	(0.29)	(0.31)	(1.22)
Hispanic:Female	-0.07	0.06	-0.08	-0.70
	(0.12)	(0.18)	(0.19)	(0.75)
Female:Relatively Old	-0.16*	0.02	0.39**	-1.23**
	(0.09)	(0.14)	(0.16)	(0.57)
(Birth Month - Cutoff)	0.07	-0.18	-0.26	-1.14**
	(0.09)	(0.13)	(0.16)	(0.56)
Relatively Old:(Birth Month - Cutoff)	-0.02	0.17	0.26	1.40**
	(0.09)	(0.14)	(0.17)	(0.60)
(Birth Month - Cutoff)^2	0.01	-0.02	-0.04	-0.19**
	(0.01)	(0.02)	(0.02)	(0.08)
Relatively Old:(Birth Month - Cutoff)^2	-0.01	0.03	0.04*	0.18**
	(0.01)	(0.02)	(0.02)	(0.08)
ln(Income)	0.20***	-0.19***	-0.29***	-0.97***
	(0.02)	(0.03)	(0.04)	(0.16)
Average State Age	-0.03*	0.11***	0.03	0.65***
	(0.02)	(0.03)	(0.03)	(0.11)
Intercept	4.09	-21.61***	-5.14	-74.44***
	(3.16)	(5.18)	(5.82)	(20.00)
R-squared	0.06	0.14	0.07	0.10
N	10180	10180	11670	5050

Table 8: Fuzzy Regression Discontinuity Estimates

SOURCE: U.S. Department of Education, National Center for Education Statistics, High School Longitudinal Study of 2009 (HSLS:09)