Very Preliminary
Comments Welcome

Is Being in School Better?
Using School Starting Age to Identify the Impact of Schools on Children's Obesity

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Patricia M. Anderson
Dartmouth College and NBER

Kristin F. Butcher
Wellesley College

Elizabeth Cascio
Dartmouth College and NBER

and

Diane Whitmore Schanzenbach
U. of Chicago Harris School of Public Policy

Abstract

In this paper, we use school starting age cutoff dates to compare weight outcomes for similar age children with one versus two years of school exposure. As is the case with academic outcomes, school exposure is related to unobserved determinants of weight outcomes. If one does not account for this endogeneity, it appears that an additional year of school exposure results in a greater BMI and a higher probability of being overweight or obese. When actual exposure is instrumented with expected exposure based on the school starting dates, the significant positive effects disappear, and most point estimates become negative, but are only occasionally significant. In the case of the lower socioeconomic status children, however, the point estimates remain positive, albeit are never significant. When controlling flexibly for direct effects of age using a regression discontinuity approach, the sign of the point estimates is similar to those found in the IV estimates. Overall, though, we detect no significant effect of school exposure on weight outcomes.

I. Introduction

In recent decades, there has been a stark increase in childhood obesity, with rates tripling from 5% in the early 1970s to 15% by the early 2000s. This increase in childhood obesity raises many concerns. For example, Type II diabetes is occurring at younger ages. In fact, we can no longer refer to juvenile diabetes and adult-onset diabetes, but instead use the terms Type I and Type II. While there are many known complications of diabetes, it is less clear what the impact will from contracting diabetes at a young age. In addition to health concerns, overweight children have been found to have lower quality-of-life scores, and there is some evidence that they may have worse academic outcomes (look up this cite). These concerns about childhood obesity have led to many studies, but to this point there is no “smoking gun” that clearly explains a majority of the increase. Given that children spend a large amount of time in school, many studies have focused on the school environment.

For example, Schanzenbach (2005) finds that children who regularly eat the school lunch (as opposed to bringing a lunch from home) are about 2 percentage points more likely to be obese. Similarly, Anderson and Butcher (2006) find that a 10 percentage point increase in the likelihood of being exposed to junk food in school results in a 1 percent increase in the average student’s BMI. While these studies are consistent with school attendance being deleterious for one’s health, it is important to realize that they do not necessarily imply that this is the case. Rather, they both indicate that some school environments are worse than others – that is, that schools with higher quality lunches and less junk food would produce leaner children. It may still be the case, though, that being in school is better than being out of school. Von Hippel et. al. (2007) try to more directly address this question, by comparing weight gain in the summer to weight gain during the school year. They conclude that the rate of weight gain is faster during the summer, although there are many caveats to their findings, including issues of seasonality.

A straightforward way to think about the impact of being in school versus not being in school takes advantage of school starting age cutoffs, an approach that has previously been used to estimate the effect of educational attainment on test scores (Gormley and Gayer, 2005; Cascio and Lewis, 2006), adult well-being (Dobkin and Ferreira, 2007), and birth outcomes (McCrary and Royer, 2006). Consider a state that requires that a child reach age 5 by September 1 in order to start kindergarten that fall. A child born on August 31 will start kindergarten and be in school for the next year, while one born on September 2 will stay out of school for the year. The only difference between these similar age children is being born one day before or after an arbitrary cutoff. Ideally, we would want to compare these children at the end of the year where one is in school and the other is not. However, since the slightly older child will always have one additional year of school exposure for a given age, later comparisons should also be informative. Much later comparisons, however, may begin to conflate the positive effect of education on health with a pure school exposure effect (get a cite for this). Zhang (2007) uses school starting age laws in combination with the NLSY97 to determine that teenage girls with more education are less likely to be overweight, a finding she attributes to the possibility that education promotes healthier eating habits.

In this study, we will use data from the Early Childhood Longitudinal Study – Kindergarten cohort of 1998 (ECLS-K) to compare children who have one versus two years of school exposure due to school starting age cutoffs. We will first use a basic regression approach, where we instrument for actual exposure using expected exposure based on the school starting cutoff. We then use a regression discontinuity approach based around the cutoffs. We find no strong evidence that an additional year of schooling has either positive or negative effects on weight outcomes, although there are small indications that there may be differences across subgroups. The paper continues first with a description of our data and empirical strategy. Section III then presents results from a basic regression approach, while Section IV discusses the regression discontinuity results. Section V concludes.

II. Strategy and Data

The fact that most states set a date by which children should be five-years-old in order to start kindergarten implies that some six-year-olds will be in first grade, while others will be in kindergarten. Consider, for example, a state with a September 1 cutoff. A child born on September 2 will miss the cutoff and will be six at the beginning of kindergarten the following year (a late starter). By comparison, a child born just a day earlier who was allowed to start kindergarten (the early starter) will now be in first grade. Thus, we have two children of a similar age, one of whom (the early starter) has been exposed to school for an additional year. If, in fact, being in school is good for weight outcomes, as implied by von Hippel et. al. (2007) and Zhang (2007), then the early starters should have a healthier weight for their age than the late starters. If, instead, the early starters have a less healthy weight for their age than the late starters, then we can conclude that not only are some school environments worse than others (as in Anderson and Butcher (2006) and Schanzenbach (2006)), but that any school can be bad for weight outcomes.

We use data from the Early Childhood Longitudinal Study – Kindergarten Cohort of 1998 (ECLS-K) to investigate the impact of earlier school starting on children’s weight. All of the children in the ECLS-K were in kindergarten in the fall of 1998, thus it is important to be clear on how we are able to use these data to implement our strategy. The key aspect of the data is that the children are weighed and measured near the end of both kindergarten and first grade. Thus, comparing measurements from early starters in first grade to those from late starters in kindergarten will successfully compare similar aged children with different school exposures. Table 1 illustrates how we process the first two years of the ECLS-K panel into our final data set. For simplicity, the table assumes that all states use a September 1 cutoff date. In fact, while this is the modal cutoff, there are 14 different cutoff dates employed by the states and we use the cutoff appropriate for each student's state of residence.[1]

The first column of Table 1 shows the birth months and years of children in our sample. For brevity, the oldest and youngest percent are excluded. The next two columns give the age of a child born on the first of the given month in that year on May 1 of kindergarten and first grade. While the children are not all assessed on exactly May 1 of each year, the median date is close to May 1, so we will use it as our assumed assessment date. Now we define the early starters to be those in the six months before the cutoff date. In this example, this implies children born between March 1993 and August 1993. Similarly, the late starters are those in the six months after last year's cutoff date, which are children born between September 1992 and February 1993. This effectively takes a full twelve months of birthdates, from September 1992 to August 1993, and splits them into early and late starters. For these school entry law "compliers" (highlighted in the first column), the late starters are between 6 years 3 months and 6 years 8 months in kindergarten, while the early starters are between 6 years 9 months and 7 years 2months in first grade. Those born just around September 1 are almost the exact same age, while those born further from the cutoff are increasingly differently aged.

[1] We use the 1998 cutoff dates specified in Datar (2004). Note that 38 percent of the children live in states using a September 1 cutoff and 70 percent are subject to cutoffs in August, September or October.

In addition to compliers, however, there are a range of children who either started this year, despite having missed the cutoff, or did not start last year, despite having made the cutoff. These non-compliers are also considered to be either early starters (born after September 1, 1993) or late starters (born before September 1, 1992) and we observe them in first grade and kindergarten, respectively. Note that every child is in our analysis sample once, and for a given state children are only observed in a one year range of ages. Thus, for the September 1 cutoff of the example, this age range is 6 years 3 months to 7 years 2 months.[2] Table 2 provides some descriptive statistics for the full sample, and broken down by observations from kindergarten and first grade.

III. Basic Regression Approach

Regression analysis provides a simple approach to analyzing the impact of early school starting on weight outcomes using these data. With appropriate age controls, we can compare otherwise similar children, one of whom has an additional year of schooling due having a birth date that just makes the state cutoff date.[3] Growth charts provided by the Centers for Disease Control make clear that growth varies with not just age, but with gender, and in a manner that is quite non-linear. Thus, all of our regression models will control for a gender dummy, a cubic in age, and the interactions. Additionally, since cutoffs vary at the state level, but there may be unobserved determinants of weight outcomes that also vary at the state level, we include state fixed effects. Finally, to control for observable background characteristics, we include a set of race dummies and indicators for mother's level of education (less than high school, high school

[2] In reality, because there are multiple starting cutoff dates, the analysis sample has a range of ages from about 5 years 7 months to 7 years 11 months.
[3] Note that this is only strictly true for compliers. The presence of non-compliers results in school exposure being endogenous, a complication that we will address below.

graduate, greater than high school). In each case, the comparison group is made up of those with missing information.

We have a range of variables to choose from to evaluate weight outcomes. Since weight varies predictably with height, the most common measure of weight-for-height is the Body Mass Index (BMI) which is calculated as weight in kilograms over height in meters squared. We choose to use the natural log of BMI, since percentage changes in BMI are more naturally interpreted than level changes. For a child of a given height, a percentage change in BMI can simply be thought of as a percentage change in weight. While using ln(BMI) provides a picture of the impact of school exposure on average weight, in many cases we are most interested in the extremes of the distribution. While for adults the categories of underweight, overweight and obese are based on simple BMI cutoffs, for children the corresponding cutoffs are based on the gender-age growth charts.[4] Since these charts are based on a fixed population from the past, the 5^{th}, 85^{th}, and 95^{th} percentile cutoffs can be used to define underweight, overweight and obese. As can be seen in Table 2, in our sample just 3 percent are underweight, while 11.5 percent are obese and 26 percent are overweight or obese.

The complication with a simple OLS regression of a weight outcome on school exposure comes from the presence of non-compliers. If everyone started or stayed out of kindergarten solely due to the statutory starting date cutoff, than the only variation in exposure would be due to birthdates being randomly before or after these cutoffs. Since that is not the case, we also have variation in exposure due to parental decisions on whether to hold back a child or push for an early start. Presumably, these parental decisions are based on unobserved aspects of the child's development. In the literature focused on educational outcomes, the concern is that

[4] The adult cutoffs are under 18.5, above 25 and above 30, respectively. Note that the terminology used for children is actually underweight, at-risk of overweight and overweight, but for simplicity we will use the adult terminology.

children with more aptitude will be early starters while those with delays will be late starters.[5] It is not as clear that parents will be making school starting decisions based on weight. Nonetheless, we can use instrumental variables to allow for this possibility. Given each child's birthdate and the state's school starting cutoff, we can calculate what grade a complier would have reached in May of 1999 and 2000. This expected exposure can then be used as instrument for the observed exposure.

A. Overall Sample Results

Table 3 presents the results of both the OLS (top panel) and IV (bottom panel) regressions described above. Looking at the first column, we see that the simple OLS regression implies that children exposed to an additional year of school are 1 percent heavier. Based on the next three columns, we can see that this weight gain is at the top of the distribution. The probability of being obese is significantly higher by 2.4 percentage points, while the probability of being either overweight or obese is significantly higher by 2.5 percentage points. At the same time, the probability of being underweight is insignificantly lower by 0.5 percentage points. Thus, the increase in weight observed in column (1) appears to be associated almost entirely with increases in the fraction of children who are obese.

Looking at the bottom panel of the table, however, we see a very different story. The estimated impacts of school exposure on ln(BMI), obese and overweight are all negative, while that on underweight is positive. In each case, the effect is not significantly different from zero. The point estimate of the effect on ln(BMI) is quite small, implying a reduction in weight of 0.7 percent. It is worth noting that very large effects of either sign can be ruled out, as the

[5] Children who enter kindergarten after they are first eligible by law do appear to be negatively selected on test performance. For example, in the ECLS, ordinary least squares estimates of the relationship between elementary school test performance and age at school entry are significantly smaller than their two-stage least squares counterparts, where expected age at school entry (given birthday and school entry cutoff) is used as an instrument (Elder and Lubotsky, 2007).

confidence interval ranges from a positive impact of 0.5 percent to a negative 1.9 percent. Comparing the top and bottom panels of Table 3 leads to the conclusion that school exposure is indeed endogenous. The implication is that the children held out of school are smaller than those pushed to start earlier. Given that premature children are likely to be both smaller and developmentally delayed, this result may be related to the standard academic outcome findings. Additionally, anecdotes of parents holding back children to gain physical (not just social or academic) maturity are common, although one might think of physical maturity as being better captured by height than by BMI.[6]

Columns (5) and (6) of Table 3 provide a check on both our method and our theory about the late starters. Looking at height and ln(birthweight), the OLS results again imply significantly positive effects of school exposure. In these cases it seems clear that a causal interpretation is impossible. However, the results are very supportive of a reverse-causality story in which parents hold back their smaller children. It is reassuring to see, then, that the plausibly causal IV estimates in the lower panel imply that there is no significant effect of school exposure. While not shown in the table, we also estimated these same OLS and IV models using math test scores as the outcome, lest one worry that our method is incapable of obtaining significant estimates. In this case, the IV estimates find that an additional year of school significantly increases math scores by about 0.7 standard deviations, down from the almost 0.8 standard deviations implied by the OLS regression. The implication here is that while parents pushed children with the most aptitude to start early, and held back those with the least aptitude, there is a causal impact of schooling on math scores.[7]

B. Subgroup Results

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[6] Typically, these stories revolve around boys. We will look at boys and girls separately, below.

[7] Note that this based on the same scaled score math test that was given to students in both grades. If the math scores are standardized within grade, the younger starters perform worse than older starters, as expected.

As indicated above, anecdotes suggest that boys may be held back due to physical maturity issues more often than girls. In such a case, the OLS estimates would be expected to be more biased for boys. Thus, Tables 4a and 4b repeat the models from Table 3, but separately for boys (4a) and girls (4b). Looking first at the boys, we see fairly similar OLS results to those for the full sample, although the significance levels are lower due to the smaller samples. Interestingly, though, it does appear that the OLS models for boys suffer from much more bias. Instrumenting for exposure results in significantly negative effects of an extra year of schooling on both ln(BMI) and the probability of being obese. Boys exposed to an additional year of school are estimated to be about 2 percent lighter and almost 5 percentage points less likely to be obese. Reassuringly, there are still no significant effects on height or ln(birthweight) in the IV models. Turning to Table 4b, we again see marginally significant positive effects in the biased OLS models, but as was the case for the full sample, there are no significant effects in the IV models.

The anecdotes about holding children back from school tend to focus not only on boys, but on better-off parents. Starting public school earlier can save parents a year of daycare payments. Thus, holding back a child is unlikely to be undertaken lightly by a family facing a tight budget. In addition to there likely being differences in school starting behavior by socioeconomic status, there are also likely to be differences in the out-of-school environment. Thus, our next set of tables looks at out basic OLS and IV models separately for children whose mothers are high school dropouts, high school graduates and have more than a high school education. An additional set of tables compares white children to black and Hispanic children.

Tables 5a, 5b and 5c present the results by maternal education. Looking first at the OLS estimates in the top panels, we see that it is only for the high-school-dropout mothers (5a) that we see the positive effect of schooling on weight. While the effects become insignificant in the IV models, they all remain positive. For high-school-graduate mothers (5b), while none of the OLS estimates are significant, there is a marginally significant, negative effect on the probability of being obese in the IV model. While relatively imprecise, the point estimate is fairly large, implying that an additional year of school reduces the probability of being obese by almost 5 percentage points. Finally, for mothers with more than a high school degree (5c), there are no significant impacts on their children's weight.

Turning to Tables 6a and 6b, we see some differences across racial/ethnic groups. For whites (6a) we see only a marginally significant positive effect on ln(BMI) in the biased OLS model, but a significantly negative impact in the IV model. For the blacks and Hispanics (6b), the OLS effect is significantly positive, and the IV point estimate is identical, but insignificant. The other weight outcomes are similar, in that for whites the point estimates switch from positive to negative in the OLS versus IV models, while for blacks they remain positive in both models. It is important to note, however, that the IV impacts for blacks are not significant, and for whites the negative impact on obesity is only marginally significant. The point estimate, however, is relatively large, implying that an extra year of school reduces the probability that a white child is obese by 4 percentage points.

While imprecise, there are a few things we can learn from these socioeconomic subgroup analyses. First, it does appear that that the OLS estimates are more biased (relative to the IV estimates) for whites than for blacks and Hispanics, which is supportive of the idea that lower socioeconomic status families are less likely to hold back their children. Given the small sample sizes, it is not as clear whether estimates for the more educated mothers are more biased than those for the less educated mothers. Second, the significantly negative impacts estimated for whites and high-school-graduate mothers raise an issue about the potential mechanism. Recall that we are just comparing two years of schooling to one year, making it unlikely that we can attribute any positive effects of schooling to learning important things about nutrition. It would seem more reasonable to think of the effects as working through the school environment versus the out-of-school environment. Given that lower socioeconomic status individuals are more likely to be overweight, one's first instinct is to assume that the out-of-school environment for these groups would be worse, and thus that school might have a negative effect on obesity. We find essentially the opposite, however. It is the better-off groups for whom school appears to have a positive effect in terms of reducing obesity. One possible explanation lies in the role of Head Start. Previous research has indicated that Head Start has a large effect in reducing obesity among blacks (Frisvold, 2006). If the alternative to school for more blacks is being in Head Start, we may see a positive impact. At the same time, if white children were more likely to be at home or in part-time nursery schools, the additional structure provided by full-time schooling may have a beneficial effect.

A final, alternative explanation is that, in fact, our IV estimates are not able to isolate the causal impact of an additional year of schooling. The significant positive effect for ln(birthweight) estimated for blacks and the marginally significant negative effect estimated for whites point to this possibility. Clearly, schooling exposure cannot causally affect birth weight. Thus, there is the concern that our instrument, expected exposure calculated based on birth date and the state starting date cutoff, is somehow correlated with the unobserved determinants of birth weight. Given this concern, we also try an alternative approach, implementing a regression discontinuity design. The key difference between this approach and that used above is the flexibility with which we will allow age to have a direct impact on weight outcomes.

IV. Regression Discontinuity Approach

Since for children born approximately a year apart (i.e. right around the cutoff date), we can observe their weight outcomes at approximately the same age but year apart in school, we can also approach the question using a regression discontinuity design. In order to implement this regression discontinuity design, we need to rearrange the data in our analysis sample around the cutoff. That is, we want to calculate a "centered age" that is zero for someone born exactly on the cutoff day (i.e. who turns five just in time to start kindergarten), is 1 for someone born the day before the cutoff, and -1 for someone born the day after. Table 7 illustrates how we use our analysis sample to calculate this centered age, using the example of a September 1 cutoff. Everyone with a positive centered age is in the "after" group for the discontinuity, while those with a negative centered age are in the "before" group. Note that if everyone was a complier, we would have a perfect discontinuity. Everyone in the after group would be early starters and observed in first grade, while everyone in the before group would be late starters and observed in kindergarten. The presence of non-compliers makes the discontinuity imperfect, such that there is a group of late starters in the after group and some early starters in the before group. Nonetheless, as seen in Figure 1, we do have a clear discontinuity in school exposure.

A. The Discontinuity in Figures

Before using a regression framework to estimate the impact of school exposure, it is worthwhile to first take a look at the discontinuities in figures. Figure 2 presents the weight measures we used in the basic regression analysis – ln(BMI), obesity, overweight and underweight. Note that in each panel, the gridlines are set to illustrate likely significance in the regression models, as they are spaced at intervals equal to approximately 2 standard errors from those regressions. The figures present a locally weighted regression of the outcome on centered age, computed separately before and after the discontinuity. The dots represent the raw data collapsed from the day to the week level.

Looking first at ln(BMI), we see some evidence of a negative impact of school exposure, but it appears unlikely to be significant. Thus, this figure seems very similar to the basic regression finding for ln(BMI), where the IV estimate for the overall sample was negative, but insignificant. The panel for overweight appears to similarly indicate a very small negative impact, but those for obese and underweight reveal no impact whatsoever.

Figure 3 focuses on several non-weight related outcomes. As was the case before, we do not expect to see a significant effect on either ln(birthweight) or height, since school exposure can clearly not have a causal impact on these outcomes. While there is slight evidence of a negative discontinuity in the ln(birthweight) panel, it appears insignificant. In the height panel, there is no sign of even the smallest discontinuity. The other two panels of Figure 3 focus on non-weight outcomes where we do expect to see a clear discontinuity. In the first of these two panels, we present math test scores standardized using both the kindergarten and first grade samples. .Here, we see that an extra year of school exposure results in higher scores. Reassuringly, the reduced-form effect on test scores of an extra year of school exposure is also similar to that found in previous research (Gormley and Gayer, 2005; Cascio and Lewis, 2006). In the second panel, we standardize the math scale score within, instead of across, grades. Here, we also see the standard result that students who are younger for grade perform worse. Once again, the within-grade age gradient is very similar to that observed in previous studies using the ECLS (Elder and Lubotsky, 2007) and in other data for the United States (Cascio and Schanzenbach, 2007).

B. The Regression Discontinuity

The key to estimating the regression discontinuity is providing for a flexible function in centered age. To that end, we control for five powers of centered age (in days), interacted with dummy for being after the discontinuity (i.e. having a positive centered age). In addition, a female dummy is included and interacted with all of the powers of centered age and their interactions. We take two approaches to estimating the impact of school exposure. The first is simply a reduced form model, where we regress our outcomes on a dummy for being after the discontinuity, along with the flexible centered age controls just described. Since, as discussed above, not everyone with a centered age after the discontinuity is actually exposed to an extra year of school, we also estimate an IV version of the model. In this case, observed school exposure is instrumented with being after the discontinuity.

Table 8 presents a set of estimates for the same outcome variables used in Table 3. The results are as expected, given the figures. As was the case with the basic regression IV models, there is no significant effect of an extra year of school on ln(BMI), obesity, overweight or underweight. Additionally, there are no significant effects on our control outcomes, height and ln(birthweight). Again, while not shown there are, indeed, significant effects on academic outcomes, where an additional year of school increases performance on a math test by 0.485 standard deviations in the reduced form and 0.786 in the IV model.

C. Regression Discontinuity Results for Subgroups

As with the basic regressions, we can investigate differences across subgroups using the regression discontinuity approach. Tables 9a and 9b repeat the regression discontinuity models for males and females, respectively. As was the case in the basic regression IV models, we see a negative point estimate implying that an additional year of school reduces boys' BMI and the probability that boys are obese. In this case, however, the estimates are not significantly different from zero. The estimates for females are similarly insignificant, although this was also the case in the basic regression IV models. Overall, then, the preponderance of the evidence is that the impacts of school exposure on weight outcomes do not differ by gender, with neither group seeing an effect that is significantly different from zero.

Tables 10a, 10b, and 10c, provide results for subgroups defined by mother's educational attainment. Looking first at children whose mother's are high school dropouts (10a), we see that while the point estimates have the same sign as in the basic regressions (implying that additional school exposure increases weight), some are unexpectedly large and significant. The results are somewhat unexpected both due to the smaller and insignificant basic regression results, and due to the preliminary pictures. Figure 4 illustrates the marginally significant obesity effect and significant overweight effect for the high school dropout subgroup, along with the insignificant overweight effects for the other education subgroups. Looking at the data, it's not really clear that only the high school drop out group would have a significant overweight effect. Recall that the gridlines are set to approximate two standard errors from the full sample regression, which are about half the size of the standard errors for a small subgroup such as maternal high school drop outs. Thus, based on these figures, it is surprising that the regression models find such large and significant effects.

Turning to Tables 11a and 11b, which provides a different crude cut on socioeconomic status, we see no significant effects for the black and Hispanic subgroup. As was the case with the low education mothers, the estimated impacts on the probability of obesity and of overweight are positive, but in this case neither is even marginally significant. Overall, then, while there is some evidence for school having a deleterious effect on weight outcomes for lower socioeconomic status children, the statistical significance of the result remains questionable. Interestingly, von Hippel et. al. (2007) find that out-of-school weight gain is largest for black and Hispanic children, which is in contrast to these point estimates. It is important to remember that while using data from the ECLS-K, these authors employ a very different strategy which compares weight gain during the summer between kindergarten and first grade with weight gain during those school years for a subset of the data. We are implicitly comparing spending the year before kindergarten out of school with starting earlier and thus being in first grade after a year.

V. Conclusions

Public health policymakers have tended to focus on schools as an important battleground in the fight against childhood obesity, feeling that the current school environment may be a contributing factor to the increase in childhood obesity. While studies have found that eating school lunches and being exposed to junk food in schools may result in weight gain (Schanzenbach (2005), Anderson and Butcher (2006)), these studies only show that some school environments are worse than others. They do not necessarily imply that the school environment in general is worse than the non-school environment. In fact, other studies indicate that summer is worse than the school year for weight gain (von Hippel et. al. (2007)) and that teen girls with more education are less likely to be overweight. In this paper, we use school starting age cutoff dates to compare weight outcomes for similar age children with one versus two years of school exposure.

As is the case with academic outcomes, school exposure is related to unobserved determinants of weight outcomes. If one does not account for this endogeneity, it appears that an additional year of school exposure results in a greater BMI and a higher probability of being overweight or obese. It appears that parents are likely to hold back a child who meets the age cutoff, but who is relatively small. The bias in OLS due to this parental behavior appears to be larger for boys and higher socioeconomic status children. When actual exposure is instrumented with expected exposure based on the school starting dates, the significant positive effects disappear, and most point estimates become negative, but are only occasionally significant. In the case of the lower socioeconomic status children, however, the point estimates remain positive, albeit are never significant.

A regression discontinuity is an alternative approach to dealing with the endogeneity of school exposure. As was the case with the IV estimates, there is no evidence of a significant effect of school exposure for the overall sample, although point estimates for ln(BMI) and the probability of being overweight are negative. Also as in the IV estimates, point estimates for the lower socioeconomic status children are positive, but generally not significant. Some of our point estimates are consistent with the findings of von Hippel et. al. (2007) and Zhang (2007), but the subgroup findings are generally in contrast. While our point estimates for blacks and Hispanics indicate school exposure has a positive effect on weight, von Hippel et. al. found the largest out of school weight gain for this group. Similarly, while Zhang found a beneficial effect for girls, several of our point estimates for girls imply a deleterious effect of school exposure. Overall, though, we are unable to find a significant effect of school exposure on weight outcomes.

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Figure 1: School Starting Dates Create a Clear Discontinuity in School Exposure

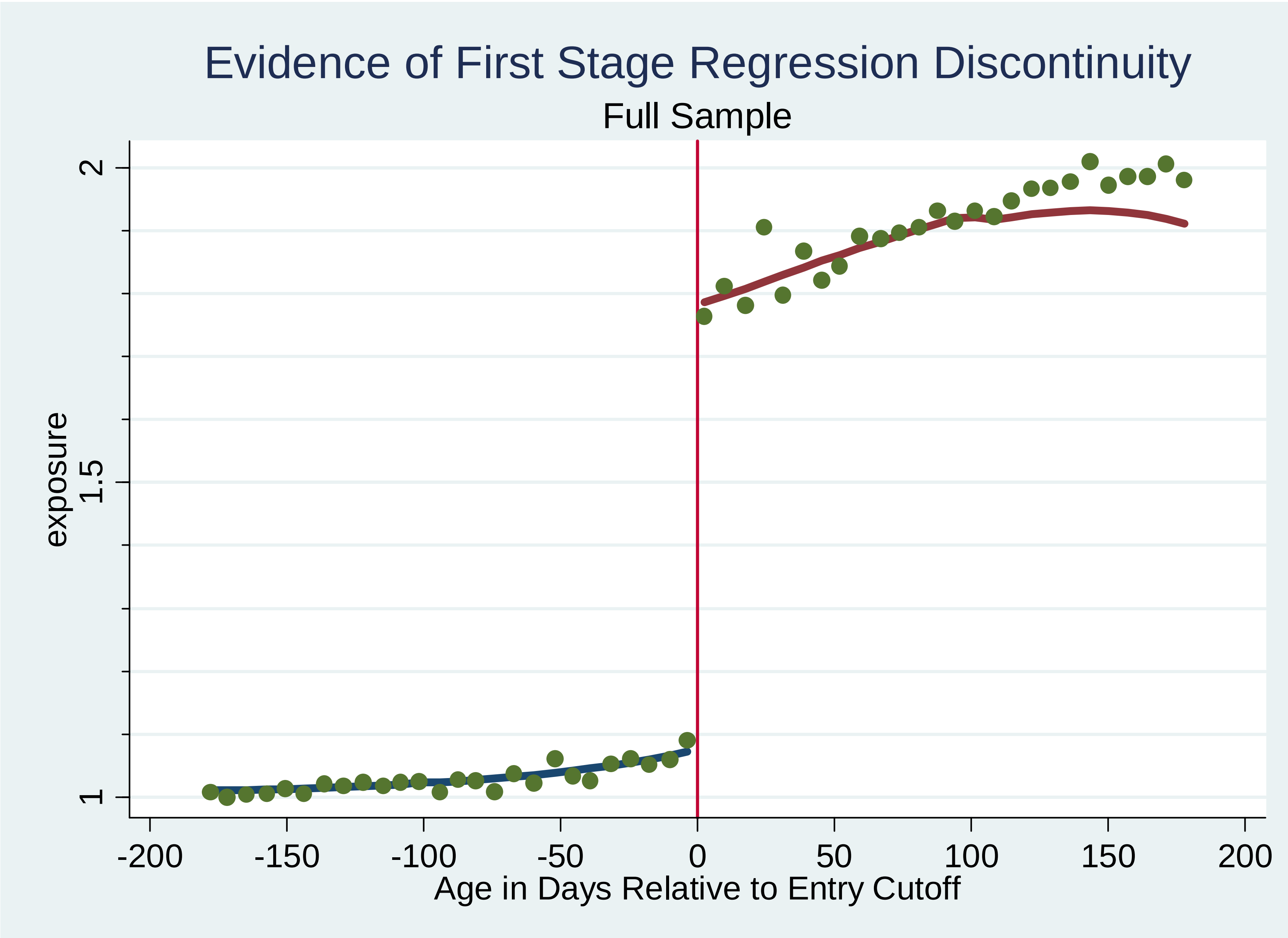

Figure 2: Effects of School Exposure on Various Weight Measures

Figure 3: Effect of School Exposure on Various Non-Weight Measures

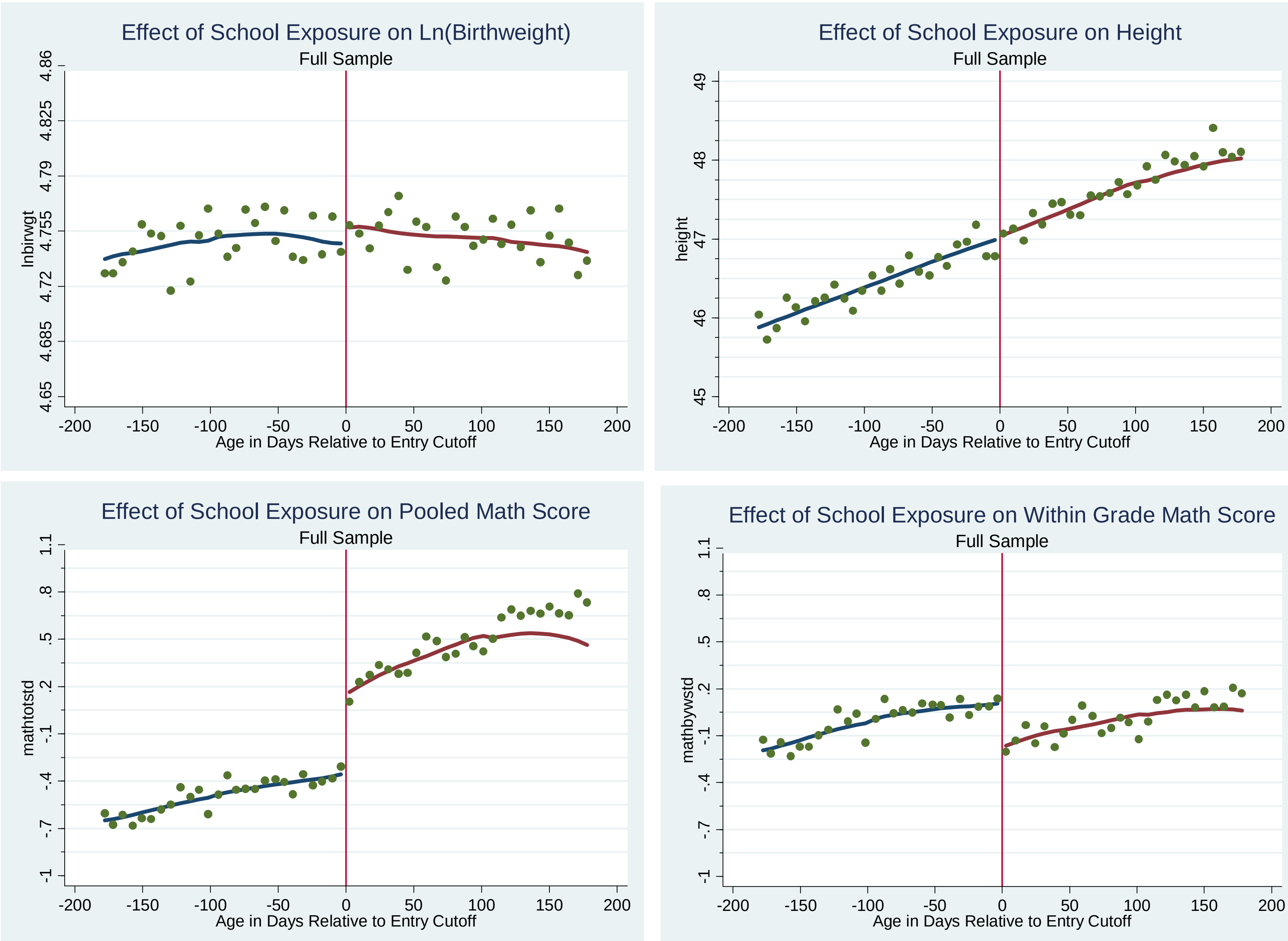

Figure 4: Illustrating Two Significant Subgroup RD Regressions, Along with Insignificant Comparison Subgroups

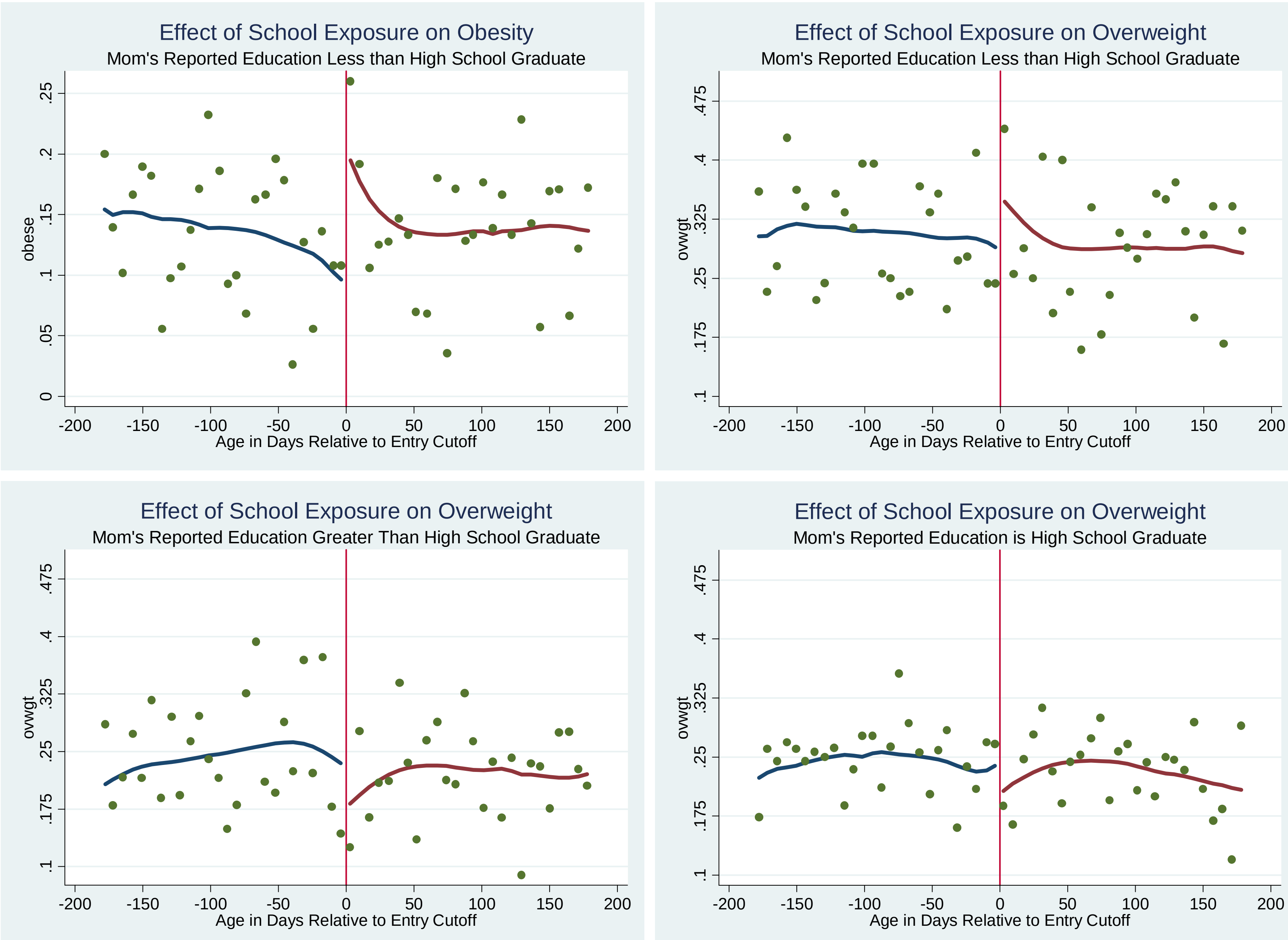

Table 1
Illustrating the Creation of the Analysis Sample,
Assuming a September 1 School Start Cutoff

Birthdate	**Age on May 1 (K)**	**Age on May 1 (1st)**	
March 92	7 years 2 months	8 years 2 months	
April 92	7 years 1 months	8 years 1 months	
May 92	7 years 0 months	8 years 0 months	
June 92	6 years 11 months	7 years 11 months	
July 92	6 years 10 months	7 years 10 months	
August 92	6 years 9 months	7 years 9 months	
September 92	6 years 8 months	7 years 8 months	Compare to Aug 93
October 92	6 years 7 months	7 years 7 months	
November 92	6 years 6 months	7 years 6 months	
December 92	6 years 5 months	7 years 5 months	
January 93	6 years 4 months	7 years 4 months	
February 93	6 years 3 months	7 years 3 months	
March 93	6 years 2 months	7 years 2 months	
April 93	6 years 1 months	7 years 1 months	
May 93	6 years 0 months	7 years 0 months	
June 93	5 years 11 months	6 years 11 months	
July 93	5 years 10 months	6 years 10 months	
August 93	5 years 9 months	6 years 9 months	Compare to Sep 92
September 93	5 years 8 months	6 years 8 months	
October 93	5 years 7 months	6 years 7 months	
November 93	5 years 6 months	6 years 6 months	

Notes: 98% of observations in our sample have birthdates in the above range. Complier birthdates are shaded in yellow. Those compliers reaching age 5 in the six months before the cutoff are considered to be early starters, and we look at their assessment in first grade. For the even younger non-compliers, we also use first grade. All of these early starters are shaded in pink. Those compliers reaching age 5 in the six months after the previous-year cutoff are considered to be late starters, and we look at their assessment in kindergarten. For the even older non-compliers, we also use kindergarten. All of these late starters are shaded in blue.

Table 2
Descriptive Statistics

Variable	(1) Full Sample	(2) Observed in Kindergarten	(3) Observed in First Grade
Ln(BMI)	2.7958	2.7901	2.803
	(.1255)	(.1219)	(.1296)
Obese	.1148	.1096	.1216
	(.3189)	(.3124)	(.3269)
Overweight	.2601	.2558	.2656
	(.4387)	(.4363)	(.4417)
Underweight	.0288	.0314	.0254
	(.1672)	(.1745)	(.1572)
Height	47.0709	46.5462	47.7463
	(2.281)	(2.2001)	(2.2044)
Ln(Birthweight)	4.7499	4.7475	4.7531
	(.2163)	(.2218)	(.2089)
Black	.1394	.1348	.1452
	(.3464)	(.3416)	(.3524)
Hispanic	.1747	.1702	.1805
	(.3797)	(.3758)	(.3846)
Asian	.0643	.0624	.0667
	(.2452)	(.2418)	(.2495)
Other race	.0582	.0551	.0622
	(.2341)	(.2281)	(.2415)
Mom < HS grad	.1771	.1745	.1805
	(.3818)	(.3796)	(.3846)
Mom HS grad	.3102	.3108	.3094
	(.4626)	(.4629)	(.4623)
Mom > HS grad	.2068	.2076	.2057
	(.405)	(.4056)	(.4042)
Age in months	79.2601	76.9593	82.2217
	(3.8663)	(3.1892)	(2.3479)
Exposure	1.4752	1.0599	2.0097
	(.5079)	(.2374)	(.0982)
Observations	13165	7409	5756

Note: Standard deviations in parentheses

Table 3
The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.010**	0.024**	0.025*	-0.005	0.263**	0.017*
	(0.003)	(0.009)	(0.012)	(0.004)	(0.063)	(0.007)
Observations	13165	13165	13165	13165	13165	12283
R-squared	0.03	0.01	0.02	0.01	0.14	0.04
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.007	-0.017	-0.019	0.002	0.050	-0.001
	(0.006)	(0.015)	(0.021)	(0.008)	(0.104)	(0.011)
Observations	13165	13165	13165	13165	13165	12283
R-squared	0.02	0.01	0.02	0.01	0.14	0.03

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 4a
The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach
Males

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.008+	0.022+	0.017	-0.004	0.293**	0.025**
	(0.005)	(0.012)	(0.016)	(0.006)	(0.085)	(0.009)
Observations	6787	6787	6787	6787	6787	6310
R-squared	0.03	0.02	0.02	0.02	0.13	0.03
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.019*	-0.047*	-0.035	0.014	0.043	0.006
	(0.009)	(0.023)	(0.031)	(0.013)	(0.153)	(0.016)
Observations	6787	6787	6787	6787	6787	6310
R-squared	0.02	0.01	0.02	0.01	0.12	0.03

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 4b
The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach
Females

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.013*	0.023+	0.034+	-0.006	0.217*	0.008
	(0.006)	(0.013)	(0.018)	(0.006)	(0.097)	(0.010)
Observations	6378	6378	6378	6378	6378	5973
R-squared	0.03	0.01	0.02	0.02	0.15	0.03
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.003	0.009	-0.004	-0.009	0.056	-0.008
	(0.009)	(0.021)	(0.028)	(0.010)	(0.145)	(0.014)
Observations	6378	6378	6378	6378	6378	5973
R-squared	0.03	0.01	0.02	0.02	0.15	0.03

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 5a
The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach
Mother's Reported Education Less Than High School Graduate

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.025**	0.086**	0.083*	0.001	0.278+	0.005
	(0.010)	(0.027)	(0.033)	(0.012)	(0.163)	(0.015)
Observations	2332	2332	2332	2332	2332	2287
R-squared	0.04	0.03	0.03	0.04	0.16	0.04
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.002	0.043	0.007	0.006	-0.125	0.011
	(0.015)	(0.041)	(0.051)	(0.017)	(0.245)	(0.021)
Observations	2332	2332	2332	2332	2332	2287
R-squared	0.04	0.03	0.03	0.04	0.16	0.04

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 5b
The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach
Mother's Reported Education is High School Graduate

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.007	0.014	0.008	0.005	0.294**	0.018+
	(0.006)	(0.013)	(0.019)	(0.008)	(0.106)	(0.010)
Observations	4084	4084	4084	4084	4084	4037
R-squared	0.02	0.01	0.02	0.02	0.15	0.04
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.005	-0.048+	-0.010	0.009	0.080	-0.015
	(0.011)	(0.028)	(0.039)	(0.014)	(0.203)	(0.018)
Observations	4084	4084	4084	4084	4084	4037
R-squared	0.02	0.01	0.02	0.02	0.15	0.04

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 5c

The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach

Mother's Reported Education Greater than High School Graduate

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.002	-0.022	0.014	-0.021*	0.179	0.014
	(0.008)	(0.017)	(0.027)	(0.011)	(0.123)	(0.013)
Observations	2722	2722	2722	2722	2722	2686
R-squared	0.04	0.03	0.02	0.03	0.13	0.06
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.020	-0.036	-0.038	-0.010	0.008	-0.037
	(0.015)	(0.035)	(0.053)	(0.020)	(0.250)	(0.025)
Observations	2722	2722	2722	2722	2722	2686
R-squared	0.03	0.03	0.02	0.02	0.13	0.06

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 6a
The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach
Blacks and Hispanics

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.017*	0.039*	0.039	-0.005	0.288*	0.025+
	(0.007)	(0.019)	(0.023)	(0.008)	(0.123)	(0.015)
Observations	4135	4135	4135	4135	4135	3748
R-squared	0.02	0.02	0.02	0.02	0.15	0.05
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.017	0.030	0.052	0.010	0.186	0.036*
	(0.011)	(0.029)	(0.036)	(0.012)	(0.180)	(0.018)
Observations	4135	4135	4135	4135	4135	3748
R-squared	0.02	0.02	0.02	0.02	0.15	0.05

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 6b
The Impact of Years of School Exposure on Child Outcomes: Basic Regression Approach
Whites

OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.008+	0.017	0.019	-0.007	0.228**	0.019*
	(0.004)	(0.011)	(0.016)	(0.005)	(0.076)	(0.008)
Observations	7401	7401	7401	7401	7401	7117
R-squared	0.02	0.01	0.01	0.01	0.13	0.02
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.018*	-0.042+	-0.049	-0.007	-0.149	-0.026+
	(0.009)	(0.022)	(0.030)	(0.012)	(0.145)	(0.015)
Observations	7401	7401	7401	7401	7401	7117
R-squared	0.02	0.01	0.01	0.01	0.13	0.02

Notes: All models include controls for race, gender and mother's education, as well as a cubic in age (measured in days) that varies with gender. In the IV models, exposure is instrumented with the exposure expected based on birth date and school starting laws. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 7
Arrangement of Data for the Regression Discontinuity Approach,
Assuming a September 1 School Start Cutoff

Centered Age	**-6**	**-5**	**-4**	**-3**	**-2**	**-1**	**0**	**1**	**2**	**3**	**4**	**5**
Birthdate (compliers)	Feb 93	Jan 93	Dec 92	Nov 92	Oct 92	Sep 92	Aug 93	Jul 93	Jun 93	May 93	Apr 93	Mar 93
Age on May 1	6–3	6–4	6–5	6–6	6–7	6–8	6–9	6-10	6-11	7–0	7–1	7–2
Birthdate (non-compliers)				Nov 93	Oct 93	Sep 93	Aug 92	Jul 92	Jun 92	May 92	Apr 92	Mar 92
Age on May 1				6–6	6–7	6–8	6–9	6-10	6-11	7–0	7–1	7–2

Notes: This table is meant to be illustrative of the regression discontinuity at 0, where children on either side of the discontinuity are approximately the same age, but are a year apart in school. Centered age is actually measured in days, not months, see text for details. Complier birthdates are shaded in yellow. Those compliers reaching age 5 in the six months before the cutoff are considered to be early starters, and we look at their assessment in first grade. For the even younger non-compliers, we also use first grade. All of these early starters are shaded in pink. Those compliers reaching age 5 in the six months after the previous-year cutoff are considered to be late starters, and we look at their assessment in kindergarten. For the even older non-compliers, we also use kindergarten. All of these late starters are shaded in blue. 98% of observations in our sample have birthdates in the above range.

Table 8
The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	-0.010	0.001	-0.019	0.004	0.252	0.004
	(0.010)	(0.026)	(0.036)	(0.013)	(0.177)	(0.018)
Observations	13165	13165	13165	13165	13165	12283
R-squared	0.01	0.01	0.01	0.01	0.13	0.02
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.016	0.002	-0.031	0.006	0.406	0.007
	(0.016)	(0.042)	(0.059)	(0.022)	(0.286)	(0.030)
Observations	13165	13165	13165	13165	13165	12283
R-squared	0.01	0.01	0.01	0.01	0.13	0.02

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 9a

The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach

Males

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	-0.015	-0.024	-0.001	0.006	0.384	0.019
	(0.013)	(0.036)	(0.047)	(0.019)	(0.234)	(0.025)
Observations	6787	6787	6787	6787	6787	6310
R-squared	0.01	0.01	0.01	0.02	0.12	0.02
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.026	-0.040	-0.002	0.010	0.650	0.033
	(0.022)	(0.060)	(0.079)	(0.032)	(0.396)	(0.043)
Observations	6787	6787	6787	6787	6787	6310
R-squared	0.01	0.01	0.01	0.01	0.12	0.02

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 9b

The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach

Females

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	-0.004	0.030	-0.035	0.001	0.121	-0.012
	(0.016)	(0.038)	(0.056)	(0.018)	(0.274)	(0.027)
Observations	6378	6378	6378	6378	6378	5973
R-squared	0.02	0.01	0.01	0.01	0.13	0.01
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.005	0.046	-0.054	0.001	0.196	-0.018
	(0.025)	(0.060)	(0.087)	(0.028)	(0.426)	(0.041)
Observations	6378	6378	6378	6378	6378	5973
R-squared	0.02	0.01	0.01	0.01	0.13	0.01

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 10a

The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach

Mother's Reported Education Less Than High School Graduate

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	0.043	0.135	0.208*	0.004	-0.069	0.010
	(0.028)	(0.084)	(0.095)	(0.024)	(0.493)	(0.039)
Observations	2332	2332	2332	2332	2332	2287
R-squared	0.04	0.03	0.03	0.04	0.15	0.04
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.076	0.241+	0.372*	0.007	-0.123	0.017
	(0.050)	(0.145)	(0.167)	(0.044)	(0.884)	(0.072)
Observations	2332	2332	2332	2332	2332	2287
R-squared	0.04	0.03	0.02	0.04	0.15	0.04

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 10b

The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach

Mother's Reported Education is High School Graduate

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	-0.019	-0.032	-0.085	-0.006	0.184	0.001
	(0.018)	(0.044)	(0.065)	(0.025)	(0.299)	(0.028)
Observations	4084	4084	4084	4084	4084	4037
R-squared	0.02	0.02	0.02	0.02	0.14	0.03
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.036	-0.062	-0.165	-0.011	0.356	0.005
	(0.035)	(0.084)	(0.126)	(0.049)	(0.582)	(0.055)
Observations	4084	4084	4084	4084	4084	4037
R-squared	0.01	0.01	0.01	0.02	0.14	0.03

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 10c

The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach

Mother's Reported Education Greater than High School Graduate

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	-0.025	-0.006	0.069	-0.008	0.299	-0.008
	(0.023)	(0.056)	(0.086)	(0.035)	(0.405)	(0.045)
Observations	2722	2722	2722	2722	2722	2686
R-squared	0.03	0.02	0.02	0.03	0.13	0.04
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.047	-0.012	0.134	-0.014	0.573	-0.016
	(0.044)	(0.107)	(0.166)	(0.067)	(0.785)	(0.086)
Observations	2722	2722	2722	2722	2722	2686
R-squared	0.02	0.02	0.02	0.03	0.13	0.04

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 11a
The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach
Blacks and Hispanics

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	0.013	0.074	0.041	0.025	-0.003	0.046
	(0.022)	(0.058)	(0.077)	(0.021)	(0.366)	(0.041)
Observations	4135	4135	4135	4135	4135	3748
R-squared	0.02	0.02	0.02	0.02	0.13	0.04
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	0.019	0.111	0.062	0.038	-0.005	0.069
	(0.033)	(0.086)	(0.115)	(0.032)	(0.552)	(0.061)
Observations	4135	4135	4135	4135	4135	3748
R-squared	0.02	0.01	0.02	0.01	0.13	0.04

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%.

Table 11b
The Impact of Years of School Exposure on Child Outcomes: Regression Discontinuity Approach
Whites

Reduced Form	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
After	-0.011	-0.027	-0.012	-0.018	0.229	-0.008
	(0.013)	(0.034)	(0.047)	(0.017)	(0.231)	(0.023)
Observations	7401	7401	7401	7401	7401	7117
R-squared	0.02	0.01	0.01	0.01	0.13	0.02
IV	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(BMI)	Obese	Overweight	Underweight	Height	Ln(Birthweight)
Exposure	-0.017	-0.040	0.003	-0.021	0.320	-0.011
	(0.024)	(0.061)	(0.085)	(0.032)	(0.413)	(0.047)
Observations	7401	7401	7401	7401	7401	7117
R-squared	0.01	0.01	0.01	0.01	0.13	0.02

Notes: All models include five powers of centered age (measured in days), plus interactions of these with indicators for gender, being after the discontinuity, and both. In the IV models, exposure is instrumented with an indicator for being after the discontinuity. Robust standard errors in parentheses, with significance levels indicated as + significant at 10%; * significant at 5%; ** significant at 1%