9-2011

Is Being in School Better? The Impact of School on Children's BMI when Starting Age is Endogenous

Patricia M. Anderson

Kristin F. Butcher
Wellesley College, kbutcher@wellesley.edu

Elizabeth U. Cascio

Diane Whitmore Schanzenbach

Recommended Citation
The Journal of Health Economics, vol. 30, no. 5, September 2011, pp.977-986.

Is Being in School Better?
The Impact of School on Children's BMI When Starting Age is Endogenous

May 2011

Patricia M. Anderson
Dartmouth College and NBER

Kristin F. Butcher
Wellesley College and NBER

Elizabeth U. Cascio
Dartmouth College and NBER

and

Diane Whitmore Schanzenbach
Northwestern University and NBER

Acknowledgements: Financial support from the USDA through the RIDGE Grant Program and the Robert Wood Johnson Foundation's Healthy Eating Research Program (Award #57922) is gratefully acknowledged. We also thank Kosali Simon, Tom DeLeire and seminar participants at Indiana University, the University of Florida, CIE-ITAM, and the American Economic Association Annual Meetings for helpful comments.

Abstract

In this paper, we investigate the impact of attending school on body weight and obesity using a regression-discontinuity design. As is the case with academic outcomes, school exposure is related to unobserved determinants of weight outcomes because some families choose to have their child start school late (or early). 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 we compare the weight outcomes of similar age children with one versus two years of school exposure due to regulations on school starting age, the significant positive effects disappear, and most point estimates become negative, but insignificant. However, additional school exposure appears to improve weight outcomes of children for whom the transition to elementary school represents a more dramatic change in environment (those who spent less time in childcare prior to kindergarten).

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. 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 (see Taras and Potts-Datema, 2005 for a review of this literature).

These concerns about childhood obesity have led to much research. Given that children spend a large amount of time in school, many studies have focused on the school environment. For example, Schanzenbach (2009) concludes that regularly eating school lunch (as opposed to bringing a lunch from home) increases obesity rates by about 2 percentage points. 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, schools with higher quality lunches and less junk food would produce leaner children than those with lower quality lunches and more junk food. 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 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.[1]

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 (Cahan and Davis, 1987; Cahan and Cohen, 1989; Gormley and Gayer, 2005; Cascio and Lewis, 2006; Luyten 2006), adult well-being (Dobkin and Ferreira, 2010), and birth outcomes (McCrary and Royer, 2011). 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 at the age of five years and one day and be in school for the next year, while one born on September 2 will have to wait until the following year to begin kindergarten and will be aged 5 years and 364 days on entry. The only difference between these similar-age children is being born one day before or after September 1. Ideally, we would 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 any positive effect of education on health with a pure school exposure effect.[2] Zhang (2007), for example, 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 estimate the impact of early elementary school on children's body weight. In particular, we use data from the Early Childhood Longitudinal Study – Kindergarten cohort of 1998 (ECLS-K) to compare the weights of children who have completed first grade to

[1] In addition, Frisvold and Lumeng (2011) find that lengthening the Head Start day decreases obesity.
[2] See Cutler and Lleras-Muney (2010) for a recent discussion of the education gradient in health.

those of same age children who have completed kindergarten only.[3] We first show that a simple ordinary least squares (OLS) approach is misleading: children completing only kindergarten by a given age are on average lighter, consistent with parents holding back children who are small for their age. We then present alternative estimates that exploit the sharp difference in predicted school exposure among children whose birthdays are near school entry cutoff dates, described above. Using this regression-discontinuity (RD) approach, we find no strong evidence that an additional year of schooling has either positive or negative effects on weight outcomes in the full sample. However, the RD estimates suggest that school attendance improves the weight outcomes of children for whom the transition to elementary school represents a more dramatic change in environment, such as those who spent little time in childcare prior to entering kindergarten. There is also some indication that some aspects of the school environment mediate effects of school exposure on child weight.

II. Empirical Approach

Most states set a date by which children should be five years old in order to start kindergarten. As a result, within a state some children who are six years old 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 be six at the beginning of kindergarten the following year (an "older starter"). By comparison, a child born just a day earlier who was allowed to start kindergarten (a "younger starter") will be in first grade when she is six. Thus, we have two children with almost identical ages – who would arguably have the same weight outcomes on average in the absence of differences in school exposure – one of whom (the

[3] Most children in the second wave of the ECLS-K are in first grade, but some are in kindergarten again. Throughout the paper, we say that they are in first grade for ease of exposition. In practice, the second year of kindergarten constitutes an additional year of school exposure, so the interpretation of our findings is not affected.

younger starter) has been exposed to school for an additional year. If, in fact, being in school improves weight outcomes, as implied by von Hippel et al. (2007), then the younger starters should have a healthier weight for their age than the older starters. If, instead, the younger starters have a less healthy weight for their age than the older starters, then we can conclude that not only are some school environments worse than others (as in Anderson and Butcher, 2006, and Schanzenbach, 2009), but that any school can be bad for weight outcomes.

If all children complied with the school starting age dates set by the states, a simple regression of weight outcomes on years of school exposure, controlling flexibly for age, would replicate the thought experiment described above. One would indeed be comparing the six year old born September 2 and now in kindergarten with the six year old born August 31 and now in first grade. However, many schools do not strictly enforce the state cutoffs, and will allow the September 2 child to go ahead and start if the family requests it. More importantly, the entry cutoffs, even if strictly enforced, are only eligibility cutoffs in many states. Thus, while a child turning age five on August 31 is eligible to start kindergarten, there may be no requirement that he start. Typically, school attendance requirements only apply to an older age (as part of truancy laws). For example, a state with a September 1 cutoff makes a child who is five on September 1 eligible for kindergarten, but the child may not be required to be in school until she is six (or even seven if the state does not require kindergarten) on September 1.[4]

How many years a child has been in school by a given age is therefore determined not just by state legislation, but also by parents' decisions. Anecdotally, physical size is an important factor for parents in the decision to delay school entry. Indeed, the term "redshirting" – now commonly used to describe delayed school entry – has its origins in college athletes

[4] See http://www.fcps.edu/start/kindergarten.htm for an example of just such a rule.

delaying their eligibility for collegiate competition to buy time to get bigger and stronger.[5] Even if physical size is not taken into consideration, cognitive development and parental perceptions of academic readiness may be positively correlated with physical development. Either way, if children with lower body weight are considered less "ready" for kindergarten and held back by parents, a simple regression of weight on years of school exposure, controlling for age, will make it appear that school exposure increases weight. To avoid this bias, we will exploit the variation in school exposure arising from school entry laws described above.

More formally, consider the following model for child *i*:

$$(1) \qquad Y_i = f\left(age_i;\alpha\right) + \beta\ in\ 1st_i + \varepsilon_i$$

where *Y* is a measure of body weight, $f\left(\cdot\right)$ is a continuous, flexible polynomial in age at measurement in days, *age* (with parameter vector α), and *in 1st* is an indicator variable set to one if the child is in first grade at this age and zero if she is in kindergarten.[6] We expect that this model, when estimated via OLS, will be biased upward by the endogeneity of the school starting decision.

To address this bias, we instrument for *in 1st* in model (1) with a dummy set to one if the child *should* be in first grade given her age – that is, if she has a birthday that falls either on or in the six months prior to the school entry cutoff in her state of residence. The first stage model is given by:

$$(2) \qquad in\ 1st_i = f\left(age_i;\gamma\right) + \pi\ \ predicted\ in\ 1st_i + v_i\,,$$

where *predicted in 1st* is an indicator set to one if $age_i \geq age_0$, where age_0 is the age at measurement for the youngest complier with the state's school entry law, whose birthday is the

[5] The "red shirts" are apparently the red jerseys worn by these players in scrimmages.
[6] All children in the ECLS-K are weighed and measured at the end of both kindergarten and first grade. As described in the next section, we choose the spring kindergarten or spring first grade measurements for a given child to ensure continuity in age at measurement through the age of the youngest complier with a state's school entry law.

school entry cutoff date, or "threshold." Equation (2) is thus a regression-discontinuity (RD) model which flexibly controls for the running variable – age – and which incorporates a sharp break in the "treatment" – predicted school exposure based on the child's birthday relative to the starting age threshold. If all children complied with the school starting age rule, then π, the coefficient on the indicator for a child having a birthday on or before the threshold, would be equal to 1. We expect that most but not all children predicted to be in first grade will be observed in first grade, or that $0 < \pi < 1$. If there is an effect of school exposure on weight, we should also expect to see a sharp break in weight outcomes for children observed at ages close to the threshold.

The instrumental variables (IV) estimate of β, the effect of school exposure on weight outcomes, is simply a scaled version of the reduced-form RD estimates for weight outcomes. The reduced-form estimates show whether there is a break in weight outcomes at the age threshold; the IV estimates take into account the fact that not all children who are born on or before the threshold comply with starting age rules and have more exposure to school. The IV estimate thus scales the break in weight outcomes at the threshold by the break in school exposure at the threshold (π).

This "RD/IV" approach will identify the causal effect of school exposure as long as unobserved determinants of weight outcomes are continuous through the age threshold. If other determinants of weight outcomes, besides school exposure, are discontinuous at the threshold, then this approach is not valid. We provide some evidence below in support of the assumption that other determinants do not vary at the threshold.

III. Data

Our estimation sample is drawn from the Early Childhood Longitudinal Study – Kindergarten Cohort of 1998 (ECLS-K). If the data had been collected on, say, a single birth cohort regardless of the grade attended by the child, it would be relatively straightforward to implement our RD/IV approach. In such a case, for example, we would be able to compare a child born on August 31, 1992 to one born on September 2, 1992. If both children complied with the September 1 school entry-age cutoff rule, and we weighed them both when they were exactly 6 years and 8 months old (approximately May 1, 1999), then the August-born child would be in first grade at the time of measurement and the September-born child would be in kindergarten. Both children have approximately the same age, but the August-born child has (essentially exogenously) one additional year of schooling. We would then attribute any difference in weight between the children to the impact of the additional year of school.[7]

In practice, though, constructing the comparison is more complicated because the ECLS-K follows only a single *grade*-based cohort. All of the children in the survey were in kindergarten in the fall of 1998. The key feature of the data that allows us to construct the RD is that the children are weighed and measured near the end of both kindergarten and first grade. Thus, comparing measurements from "younger starters" in first grade to those from "older starters" in kindergarten will successfully compare similar-aged children with different exposure to school.

[7] We have also estimated our models using data from the National Longitudinal Survey of Youth 1979 Mother-Child matched file, which has this structure and allows us to observe multiple birth cohorts in different grades. The working paper version of this paper (Anderson et al., 2011) provides details on the sample and data construction, and Appendix Table 2 summarizes our RD/IV findings for five to six year olds (Panel I), and five to ten year olds (Panel II). The results show no statistically significant relationships. The NLSY has a number of drawbacks as a data set to examine the exogenous effect of changes in school exposure on children's weight. The sample restricted to 5-6 year-olds is too small to allow for precise estimates. The 5-10 year old sample is larger, but as mentioned earlier, the effect of school exposure on weight outcomes may change in later grades since there is likely to be an effect of education as children learn about healthful choices in school (as in Zhang, 2007), which may be different from the effect of exposure at very early grades. More generally, the NLSY may be unsuited for this exercise as it includes many cohorts of children and both the "alternate environment" prior to school and the school environment are changing a great deal over this period.

To see this, we continue the example described in the paragraph above, which assumes compliance with a September 1 entry-age cutoff. In order to be in the 1998-99 kindergarten cohort, a September-born child should be born in 1992, while an August-born child should be born in 1993. When the kindergartners are weighed and measured on May 1, 1999 after nearly 1 year in school, the September-born child is approximately 6 years 8 months old, while the child born on August 31 is almost a year younger (almost 5 years 8 months old). When they are weighed in first grade at the end of their second year in school, the September- and August-born children are approximately aged 7 years 8 months, and 6 years 8 months, respectively. Therefore, in order to compare the two children at approximately the same age but (exogenously) different years of school exposure, we would compare the September-born child measured in kindergarten with the August-born child measured in first grade, when each is about 6 years 8 months old.

Table 1 lists the age distribution at the point in time that the students are weighed and measured in both kindergarten and first grade. The data are shaded to illustrate how we choose one measurement per child from the spring kindergarten and spring first grade waves of the ECLS-K panel to create our final cross-sectional data set. For simplicity, the table and exposition below use a September 1 cutoff date. While this is the modal cutoff, there are in fact 14 different cutoff dates employed by the states, and we use the cutoff appropriate for each student's state of residence.[8] In this example, we use the measurement taken when the student's age is between 6 years 3 months and 7 years 2 months. Among entry-age compliers, this amounts to using the kindergarten measurement for those born September – February (the "older starters"), and the first grade measurement for those born March – August (the "younger starters"). Since we measure age relative to the cutoff in days, the September and August

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

students will be only a few days different in age at the time of observation. More generally, we use a 12-month range of age centered on the age at the end of kindergarten of the youngest complier with a state's school entry cutoff, or threshold.

There are two types of non-compliers in the ECLS-K kindergarten cohort. First are students who would have entered kindergarten in the year prior to the ECLS-K cohort, but delayed entry, so that instead of entering school when they had recently turned five years old, they delayed until they were six. In the case of the September 1 cutoff illustrated in Table 1, these students were born between March and August of 1992 and range between ages 6 years 9 months and 7 years 2 months when they are measured in kindergarten. For these students, we use the measurement taken in kindergarten. The second type of non-compliance comes from students who miss the cutoff date but nonetheless start school early, before they reach age five. In the example, these students were born between September 1993 and February 1994, and when they are measured in first grade are between 6 years 3 months and 6 years 8 months old.

Table 2 illustrates how the sample is arranged to provide one observation per child that is measured when the child fits into the desired age range, and is assigned an age relative to the state cutoff for the purposes of implementing the RD/IV empirical strategy. Note that we limit our sample to the two years of birth dates implied by Table 2.[9] That is, all individuals are born in either the 6 months before/after the current year cutoff (the right side of the top panel/left side of the bottom panel), or the 6 months before/after the previous year cutoff (the right side of the bottom panel/left side of the top panel).[10]

---

[9] This uses 97% of the sample, excluding mainly those who delayed starting school by over a year.
[10] We are thus missing from our sample a very small group of students who deviate from the normal patterns. Continuing with the example using the September 1 cutoff birth date, to obtain a fully representative sample of children born in August, we would want to observe three groups of students: those that start on time, those that start a year late (the "red-shirters"), and the extremely small group (1 student in the ECLS-K) that starts a year early even though that would make them extremely young relative to their grade cohort. These groups are born in August 1993, August 1992, and August 1994, respectively. We cannot include any students born in August 1994 because we do

If all children complied with the state laws, we would have a sharp discontinuity, and would simply compare the left side of the top panel of Table 2 with the right side. In reality, the presence of non-compliers makes the discontinuity a fuzzy one. First, there are some younger starters (the left-side of the bottom panel) who miss the cutoff, but are nonetheless exposed to an extra year of school. Far more common are the "red-shirts" or older starters (the right-side of the bottom panel) who make the cutoff, but are not exposed to the extra year of school. The actual relationship between age at measurement (relative to cutoff) and school exposure at the time of measurement is shown in Figure 1. The dots on the figure give the share of children observed at the end of first grade by age measured in 7-day bins. Observations to the right of the cutoff (represented by a vertical line at x=0) are predicted to be in first grade. While the discontinuity is clear, its magnitude is less than 1, diminished by children who delay school entry or start early.

The maintained assumption for identification in an RD/IV approach is that there is no other variable that changes discretely at the threshold. Our sample selection method has ensured that calendar age increases smoothly with age relative to the school entry cutoff. As a result, the children on either side of the entry cutoff are just days apart in age at measurement, but have been exposed to sharply different amounts of school.[11] Adding further credibility to our identification strategy, there are also no sharp differences at the threshold along any other background characteristic (figures available upon request).

---

not observe a weight measurement for them during the proper age range (between 6 years 3 months and 7 years 2 months). We have the reverse problem with September-born students. While we observe those who start on time (born 1992) and those who start early (born 1993), we are forced to omit the 6 students who delay school entry an extra year even though they would have been among the oldest students in their cohort if they complied with the entry cutoff (born 1991). To the extent these missing cases are inherently heavier/lighter than average, a small bias towards OLS will result.

[11] We assume in our analysis that all children were weighed and measured on May 1, 1999, so that age at measurement is perfectly collinear with exact day of birth. However, the ECLS-K only reports age in months at the time of measurement. We show in the working paper version of this paper (Anderson et al., 2011) that reported age at measurement in months is smooth through the threshold.

IV. Results

A. Full Sample Models

Table 3 presents the main results of our analysis.[12] In each of the models in this table, we control for a cubic in *age* (relative to the age of the youngest complier, age_0) interacted with *predicted in 1st*. Only state dummies and this flexible polynomial in age are included in column (1). All subsequent columns add a vector of controls that includes dummies for race and gender, household income, and mother's characteristics.[13] The weight outcomes vary by column and are indicated in the table and described in more detail below. All models use standard errors that are robust to heteroskedasticity and within-school correlation in the error terms.[14]

Panel I presents the simple OLS estimate of model (1). Columns (1) and (2) estimate the relationship to the natural log of BMI. Controlling for additional demographics makes very little difference, and in both cases, greater school exposure is positively and statistically significantly correlated with body weight. Columns (3) and (4) estimate the relationship to whether a child is overweight or obese, respectively. These outcomes are defined as whether the child's BMI is above the gender-and-age specific cutoffs published by the Centers for Disease Control (2000). The same pattern holds true in columns (3) and (4), where the probability of being overweight or obese is significantly higher with more school exposure. In column (5), the relationship with the probability of being underweight is negative, but not significant at conventional levels.

As noted above, though, to the extent that parental decisions to comply with the starting date cutoff are influenced by body weight or factors correlated with it, then these OLS estimates are likely to be upward biased. Thus, the most appropriate interpretation of this top panel is *not*

---

[12] Appendix Table 1 provides summary statistics on the key variables for our estimation sample.
[13] Mother's characteristics include age at first birth, marital status and whether she was employed between the child's birth and kindergarten entry.
[14] Clustering standard errors on centered age (in days) yields similar inferences.

that school exposure causes children to gain weight. Rather, consistent with what one might have expected, it appears that the determination of school "readiness" that parents make either includes body weight explicitly, or includes things that are highly correlated with it, since the younger starters are significantly heavier than the older starters when observed at the same age.

Panel II substitutes predicted school exposure for actual school exposure, meaning that we replace *in 1st* with *predicted in 1st*, the dummy variable for being predicted to be in first grade given birth date relative to the school entry cutoff date. Thus, this panel implements the reduced-form version of our RD/IV model. The corresponding first-stage regression estimates, relating *in 1st* to *predicted in 1st*, are presented in Panel III. The first stage relationship is very strong. As was clear in Figure 1, however, the presence of non-compliers means the prediction is not perfect, giving us a coefficient of 0.64 instead of the 1.0 that would be obtained with perfect compliance.

Returning to the second panel, the effect of removing the endogeneity of the school starting decision is clear: school exposure now appears negatively related to ln(BMI), as shown in columns (1) and (2). However, the estimated coefficients on *predicted in 1st* are not statistically significant. While the impact on the probability of being overweight also becomes negative, the estimated effect on the probability of being obese remains positive, albeit smaller in magnitude than in the first panel, and not statistically significant. Interestingly, the negative effect in column (3) for the probability of being overweight comes closer to being significant than any of the other columns, but the p-value is still just 0.143.[15] Figure 2 illustrates this result, plotting the mean of overweight by age in 7-day bins (as in Figure 1) as well as predicted fits

[15] Note that this p-value is for a two-sided test, if one were to feel that a one-sided alternative were more appropriate, the estimated effect would be marginally significant.

from the RD specification.[16] Finally, as was the case with the OLS model in the top panel, there is no discernible effect of school exposure on the probability of being underweight, although in this RD model the point estimate is very close to zero and the p-value is very high.

The final panel presents the IV estimates that correspond to these reduced-form and first-stage RD estimates. Recall that the IV estimates scale up the reduced-form coefficient on *predicted in 1st* in the weight regressions by the corresponding coefficient in the first-stage model of school exposure, so as to represent the effects of one full additional year of school.[17] While we are unable to reject that the estimated effects are equal to zero, in all cases except for columns (4) and (5), we can reject that the IV estimates are the same as the OLS estimates. For example, in column (2) the IV point estimate suggests that an additional year of school *reduces* BMI by 1.7 percent, and this effect is significantly different from the OLS point estimate that suggests that an additional year of school *increases* BMI by 1.5 percent.[18] Given the pattern of OLS versus IV estimates, it is clear that there is a positive bias in the OLS estimates. The IV estimates suggest that increased school exposure is not likely to (on net) increase body weight. There is also not statistically strong evidence that it decreases body weight.

In a related paper, Von Hippel et al. (2007) estimate the monthly increase in BMI across kindergarten and first grade and find that growth during the summer months is 2.3 to 3.8 times the growth during the school year. Using the point estimates reported in Table 1 of that paper, we can predict the implied BMI difference for students who have been exposed to one year of school instead of two. We assume the school year covers 9 months of the year, and that BMI growth for

[16] The jaggedness of the fits reflects small changes in the average values of covariates across the bins.
[17] Thus, looking at column (3), for example, -0.046 / 0.637 ≈ -0.072.
[18] One way to benchmark the effect sizes is to compare the coefficients to the unconditional difference in ln(BMI) between whites and blacks which is 0.017. The black-white gap in overweight and obesity is 0.055 and 0.028, respectively. An extra year in school has an impact large enough to close the black-white gap in BMI and overweight, and to reduce the gap in obesity by about half.

a child who is not enrolled in school follows the summer growth trajectory over that year.[19] By these calculations, the implied difference in ln(BMI) for a 1-year difference in school would be -0.023. This is very close to (and not statistically different from) our IV estimate of -0.017 in column (2) of Table 3.

Table 4 repeats the same exercise as in Table 3, but with a set of alternate outcomes. The purpose here is two-fold. First, as a falsification exercise, we look at two physical outcomes that should not be affected by an additional year of school exposure. Column (1) looks at the natural log of height, while column (2) looks at the natural log of birth weight.[20] The positive and significant coefficients in these columns in Panel I are just further evidence that the OLS models are biased due to parents basing school starting decisions on unobservable characteristics of the child that are correlated with physical attributes.[21] By focusing on the school starting date cutoffs, though, the spurious significantly positive effect is removed. For height, the IV point estimate is essentially zero, while for birth weight the positive estimate is cut to half of the OLS coefficient, and is not statistically significantly different from zero (Panel II).

Second, column (3) documents that the IV approach used here finds strong impacts of school exposure on student test scores, as the literature suggests should be the case. The purpose here is to demonstrate that our research design is powerful enough to detect an effect of school exposure on an outcome where it is clearly expected. The dependent variable is the score on a

---

[19] In particular, Von Hippel et al. (2007) report that the average BMI at the beginning of kindergarten is 16.205. During the school year children gain 0.020 (0.033) BMI points per month during kindergarten (first grade), and during the summer they gain 0.076 BMI points per month. Note that all children are expected to have increasing BMI over this age range (see Centers for Disease Control, 2000). A child who was exposed to two 9-month school years and one 3-month summer is predicted to have a 16.91 BMI at the end of first grade. A child who was instead only exposed to one 9-month school year and the rest of the time grew at the summer rate is predicted to have a BMI of 17.297. The log difference between these predicted values is -0.023.

[20] Technically, school exposure could affect height if education increases nutritional quality sufficiently, but this is unlikely to be the case in the United States when comparing kindergarteners to first graders.

[21] Obviously, parents may actually be making decisions based on height (e.g. not wanting the child to be the smallest in class) or on birth weight (e.g. basing decisions for premature children based on age since conception). The key is that the results in this panel cannot represent causal estimates, and the same is true of the OLS estimates in Table 3.

math test, standardized across both grades to have mean zero and standard deviation one; the expectation is that a child with two years of school has presumably learned more than one with just a single year of school and thus should score higher on the test. Comparing the top panel with the bottom, we first again see the positive bias in the OLS estimates. Since parents are more likely to hold back a child who is less prepared academically, school exposure will appear to have a more positive impact on academic outcomes than it actually does. While the IV estimates are less positive, they are very precisely estimated and significantly different from both zero and the OLS estimates.

B. Subgroup Analyses

At this point, it appears that there is no deleterious effect of school exposure on children's weight outcomes. If anything, the point estimates imply a reduction in BMI and in the probability of being overweight. However, one limitation of our analysis thus far is that it may be difficult to detect an effect of school exposure on weight if, for many children, school does little to change the everyday environment with regard to eating and exercise. The impact of school exposure on weight may therefore be related to the intensity of the school treatment or to the contrast between school and pre-school activities.

To explore this, we estimate the fully specified IV models of Table 3 for subsamples of our data that might be differentially affected by school exposure. Table 5 presents the results. We first divide the sample by whether the child was attending childcare for more or fewer than 25 hours per week in the year prior to kindergarten.[22] Children who previously spent more time in childcare might experience less of a shock to their daily routine from entering school than those who were primarily at home, and as a result the impact of school attendance for them

[22] The 25-hour cutoff is somewhat arbitrary, but is close to the median amount of 22 hours per week. Note that time in childcare prior to kindergarten is not exogenous. As a result, differences in the effects of school exposure across subsamples could be affected by other differences across these groups.

might be smaller. We find evidence in support of this hypothesis in Panel I. For children who experienced little childcare prior to kindergarten, an additional year of schooling reduces BMI by almost 4 percent (with a marginally significant p-value of 0.099). School also appears to reduce the likelihood that these children are overweight or obese, but only the former is statistically significantly different from zero. On the other hand, among students who experienced 25 or more hours per week of childcare, the estimated coefficients are opposite signed though statistically insignificant.

Pushing these results further, we try to isolate the students for whom the change from pre-school to school was largest. We concentrate on the group that experienced little childcare prior to kindergarten then attended full-day kindergarten, and find some evidence that the impact of schooling is stronger for them. As shown in Panel II, an additional year of schooling reduces BMI by a statistically significant 6 percent for this group. Impacts on overweight and obesity are similar, but have higher p-values.[23] The results for students who either attended 25 or more hours per week of childcare or a half-day kindergarten (or both) are smaller and less precisely estimated.

The results in Table 5 are thus broadly consistent with the idea that going from a relatively unstructured home environment to a structured environment might reduce opportunities for snacking and/or increase a child's amount of physical activity. As shown in Anderson, Butcher and Schanzenbach (2010), cutting the equivalent of one cookie per school day (about 300 calories per week) out of a child's diet over the course of a school year can reduce BMI by over 2 percent. Thus, a reduction in snacking upon going to school certainly has the potential to reduce body weight. This interpretation is consistent with Cutler, Glaeser and

---

[23] Note that Frisvold and Lumeng (2011) find that spending a year in full-day Head Start versus half-day Head Start reduces the probability of obesity by four percentage points. While not significantly different from zero, we get a similar size effect of an extra year of schooling for this group.

Shapiro (2003), who find that much of the recent increase in adult BMI can be attributed to increased snacking opportunities during the day. Further, school does (usually) provide recess and physical education. For some children, providing such an opportunity for physical activity may actually increase their daily exercise.

In auxiliary analyses, we also explored whether the impact of school exposure on body weight varied across observed characteristics of the school environment. For example, using the same dataset employed here, Schanzenbach (2009) finds that consumption of school lunch increases obesity among young children. When we divide the sample into those who report eating the school lunch and those who report not eating it, the IV point estimates are much more negative for the sample of those not eating school lunches. We also split the sample by whether the child's school had adequate cafeteria, gymnasium or playground facilities, but found no consistent pattern in the results.

V. Conclusions

Public health policymakers have focused 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 (e.g. Haskins et al., 2006). While studies have found that eating school lunches and being exposed to junk food in schools may result in weight gain (Schanzenbach, 2009; 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 spending a full day in Head Start is better than spending just half a day (Frisvold and Lumeng, 2011).

In this paper, we use a regression-discontinuity design that to compare weight outcomes of similar age children with one versus two years of school exposure due to regulations on school starting age. Researchers have long recognized that the endogeneity of the school-starting decision biases estimates of the effect of schooling on academic outcomes, as parents will take into account a child's academic readiness (unobserved to the researcher) in making that decision. As is the case with academic outcomes, we find that school exposure is also related to unobserved determinants of child weight. 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 predicted school exposure in a regression-discontinuity framework, the significant positive effects disappear. However, while the point estimates generally become negative and are significantly different from the OLS results, they are generally not significantly different from zero.

In order for weight outcomes to be affected by exposure to school, it must be that the school environment is different, in opportunities to either consume or expend energy, from the alternative environment. It is not clear from intuition whether, on average, school environments or alternative environments would lead to better weight outcomes for children. Schools may provide fewer opportunities for expending energy than being at home in the backyard, but many children are not at home prior to starting school, and even if they are at home, they may be inside consuming snacks and screen time. Our overall results suggest that, on average, the transition to school does not herald a large change in a child's opportunities to consume and expend energy. However, an additional year of school exposure significantly lowers BMI for children who experienced low levels of non-parental care before starting full-day kindergarten. For those children who likely experience the biggest change in the structure of their day when they enter elementary school, school thus appears to improve weight outcomes.

These findings have important implications for policies aimed at reducing childhood obesity. First, simple comparisons that show that more school exposure is harmful for children's weight are incorrect, biased due to the fact that parents' decisions to start children in school are correlated with their physical stature. Second, our findings show that school exposure, per se, seems unlikely to cause weight gain, and may even be beneficial for some children. There is nothing inherent in what schools need to do to educate children – like getting them to sit still and pay attention – that necessarily leads them to gain weight. These results, coupled with the research that shows that some school environments are better than others, suggest that policies that work to improve nutrition and exercise in schools are potentially powerful policy levers to improve children's health.

References

Anderson PM, Butcher KF. Reading, writing and refreshments: Do school finances contribute to childhood obesity? Journal of Human Resources 2006; 41; 467-494.

Anderson PM, Butcher KF, Cascio EU, Schanzenbach DW. Is being in school better? The impacts of school on children's BMI when starting age is endogenous. National Bureau of Economic Research Working Paper #16673; 2011.

Anderson PM, Butcher KF, Schanzenbach DW 2010. School policies and children's obesity. In: Slotje D, Tchernis R. (Eds), Current Issues in Health Economics. Bingley, UK: Emerald Group Publishing Limited; 2010.

Cahan S, Davis D. A between-grade-levels approach to the investigation of the absolute effects of schooling on achievement. American Educational Research Journal 1987; 24; 1-12.

Cahan S, Cohen N. Age versus schooling effects on intelligence development. Child Development 1989; 60; 1239-49.

Cascio EU, Lewis EG. Schooling and the armed forces qualifying test: Evidence from school-entry laws. Journal of Human Resources 2006; 41; 294-318.

Centers for Disease Control and Prevention, National Center for Health Statistics. CDC growth charts: United States. http://www.cdc.gov/growthcharts/. May 30, 2000.

Cutler DM, Glaeser EL, Shapiro JM. Why have Americans become more obese? Journal of Economic Perspectives 2003; 17; 93-118.

Cutler DM, Lleras-Muney A. Understanding differences in health behaviors by education. Journal of Health Economics 2010; 29; 1-28.

Datar A. Does delaying kindergarten entrance give children a head start? Economics of Education Review 2004; 25; 43-62.

Dobkin C, Ferreira F. Do school entry laws affect educational attainment and labor market outcomes? Economics of Education Review 2010; 29; 40-54.

Frisvold D, Lumeng JC. Expanding exposure: Can increasing the daily duration of Head Start reduce childhood obesity? Journal of Human Resources 2011; 46; 373-402.

Gormley WT, Gayer T. Promoting school readiness in Oklahoma: An evaluation of Tulsa's pre-K program. Journal of Human Resources 2005; 40; 533-558.

Haskins R, Paxson C, Donahue E. Policy brief: Fighting obesity in the public schools. The Future of Children 2006.

Luyten H. An empirical assessment of the absolute effect of schooling: Regression-discontinuity applied to TIMSS-95. Oxford Review of Education 2006; 32; 397-429.

McCrary J, Royer H. The effect of female education on fertility and infant health: Evidence from school entry laws using exact date of birth. American Economic Review 2011; 101; 158-95.

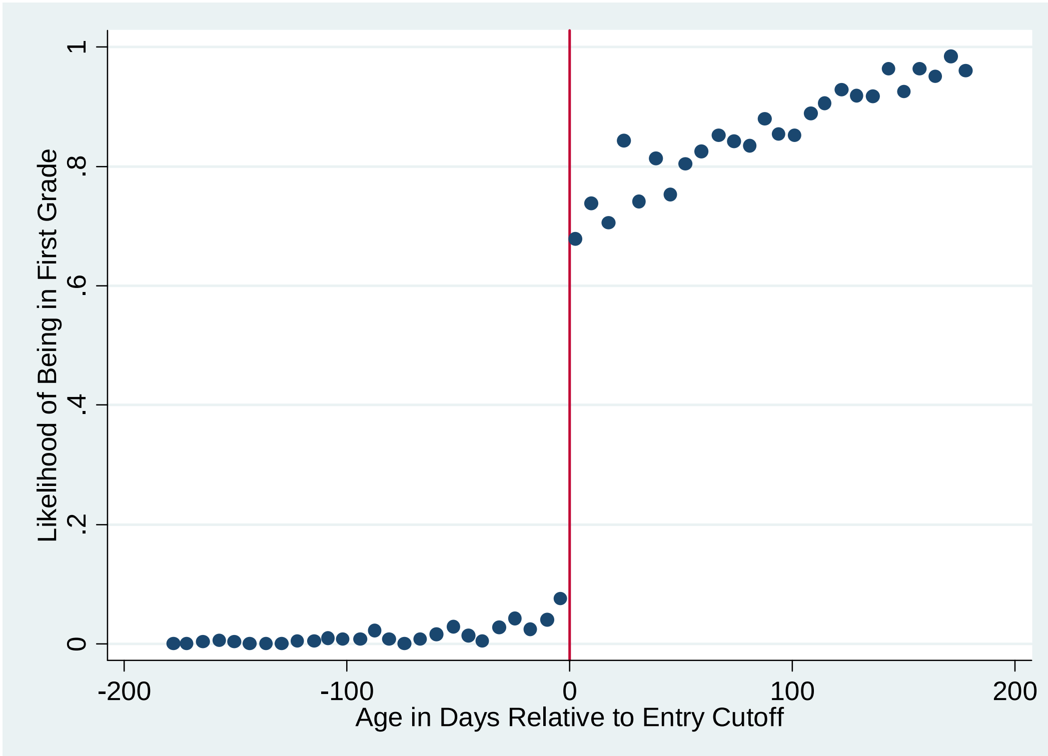
Schanzenbach DW. Do school lunches contribute to childhood obesity? Journal of Human Resources 2009; 44; 684 – 709.

Taras H, Potts-Datema W. Obesity and student performance at school. The Journal of School Health 2005; 75; 291-295.

Von Hippel PT, Powell B, Downey DB, Rowland, NJ. The effect of school on overweight in childhood: Gain in body mass index during the school year and during summer vacation. American Journal of Public Health 2007; 97; 696-702.

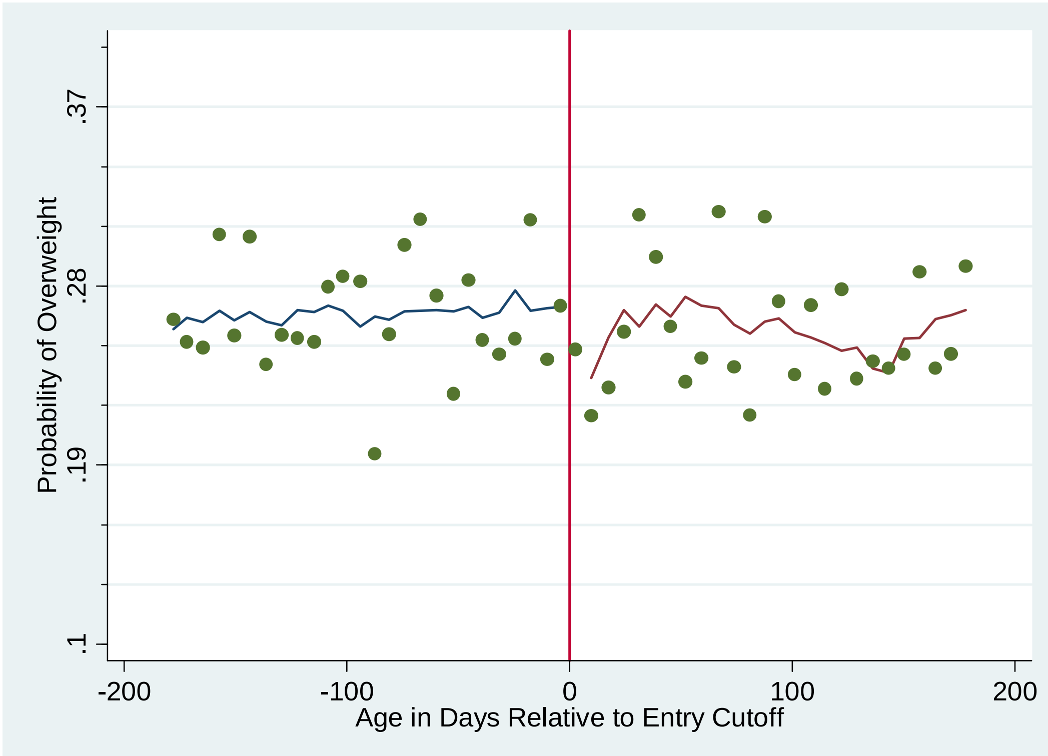
Zhang N. Does early school entry prevent youth obesity? 2007. Available at SSRN: http://ssrn.com/abstract=992235

Figure 1: The Regression Discontinuity in Actual Grade (ECLS-K)



Note: Each dot represents the average probability of being observed in first grade by age measured in 7-day bins. Observations to the right of the cutoff (represented by a vertical line at x=0) are predicted to be in first grade. Gridlines are spaced at approximately one standard error for the estimated model.

Figure 2: Estimated Regression Discontinuity Model for Probability of Overweight



Note: Each dot represents the average probability of overweight by age measured in 7-day bins. Observations to the right of the cutoff (represented by a vertical line at x=0) are predicted to be in first grade. The regression lines fit a 3rd order polynomial in relative age in days, separately estimated on each side of the cutoff. Gridlines are spaced at approximately one standard error for the estimated model.

Table 1
Illustrating the Creation of the Analysis Sample for ECLS-K,
Assuming a September 1 School Start Cutoff

Birthdate	**Age on May 1 (K)**	**Age on May 1 (1^{st})**	
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	
December 93	5 years 5 months	6 years 5 months	
January 94	5 years 4 months	6 years 4 months	
February 94	5 years 3 months	6 years 3 months	

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

Table 2
Arrangement of Data for the Regression Discontinuity Approach (ECLS-K),
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
Grade in which observed	k	k	k	k	k	k	1	1	1	1	1	1
Birthdate (non-compliers)	Feb 94	Jan 94	Dec 93	Nov 93	Oct 93	Sep 93	Aug 92	Jul 92	Jun 92	May 92	Apr 92	Mar 92
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
Grade in which observed	1	1	1	1	1	1	k	k	k	k	k	K

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 green. Those compliers reaching age 5 in the six months before the cutoff are considered to be younger starters, and we look at their assessment in first grade. For the even younger non-compliers, we also use first grade. All of these younger starters are shaded in yellow. Those compliers reaching age 5 in the six months after the previous-year cutoff are considered to be older starters, and we look at their assessment in kindergarten. For the even older non-compliers, we also use kindergarten. All of these older starters are shaded in blue.

Table 3
Estimated Effects of School Exposure on Body Weight

Panel I: OLS Regression					
	(1)	(2)	(3)	(4)	(5)
	ln(BMI)	ln(BMI)	Overwt.	Obese	Underwt.
In 1st Grade	0.018	0.015	0.041	0.033	-0.007
	(0.004)	(0.004)	(0.014)	(0.010)	(0.006)
p-value	0.000	0.000	0.003	0.001	0.202
Observations	12,754	12,754	12,754	12,754	12,754
R-squared	0.014	0.028	0.018	0.016	0.013
Panel II: Reduced-Form Regression					
	(1)	(2)	(3)	(4)	(5)
	ln(BMI)	ln(BMI)	Overwt.	Obese	Underwt.
Predicted to be in 1st Grade	-0.010	-0.011	-0.046	0.009	-0.001
	(0.009)	(0.009)	(0.031)	(0.023)	(0.012)
p-value	0.281	0.221	0.143	0.687	0.918
Observations	12,754	12,754	12,754	12,754	12,754
R-squared	0.013	0.027	0.018	0.015	0.013
Panel III: First Stage Regression					
	(1)	(2)	(3)	(4)	(5)
	In 1st Gr.	In 1st Gr.	In 1st Gr.	In 1st Gr.	In 1st Gr.
Predicted to be in 1st Grade	0.635	0.637	0.637	0.637	0.637
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
p-value	0.000	0.000	0.000	0.000	0.000
Observations	12,754	12,754	12,754	12,754	12,754
R-squared	0.739	0.743	0.743	0.743	0.743
Panel IV: Instrumental Variables Regression					
	(1)	(2)	(3)	(4)	(5)
	ln(BMI)	ln(BMI)	Overwt.	Obese	Underwt.
In 1st Grade	-0.016	-0.017	-0.072	0.015	-0.002
	(0.014)	(0.014)	(0.049)	(0.037)	(0.018)
p-value	0.282	0.222	0.144	0.687	0.918
Observations	12,754	12,754	12,754	12,754	12,754
R-squared	0.01	0.024	0.014	0.016	0.013
Mean dependent var.	2.796	2.796	0.261	0.115	0.028

Notes: Standard errors (in parenthesis) are adjusted for heterogeneity and within school correlation. BMI is calculated as weight in kilograms divided by height in meters squared. Overweight and obesity status are coded as whether a child's BMI is above the relevant cutoff, which varies by gender and age in months. "In 1st Grade" is a dummy variable equal to 1 if we use the individual's observation from the first grade interview and zero otherwise. "Predicted to be in 1st Grade" is a dummy variable equal to 1 if the child was born on or before the birthday cutoff for beginning kindergarten in his/her state in 1998. The instrumental variables regressions in Panel IV use "Predicted to be in 1st Grade" as an instrument for "In 1st Grade." Column 1 controls for a cubic in age in days relative to the state specific school starting age cutoff, an interaction of the age variables with "Predicted to be in 1st Grade," and state fixed effects. Column (2) adds family income and dummy variables for race/ethnicity and sex, and the following maternal characteristics (including dummies for whether they are missing): dummies for education level (dropout, high school graduate, greater than high school), age at first birth, a dummy for marital status, and a dummy for whether she was employed prior to the child entering kindergarten. The right hand side variables are the same in Columns (2)-(5).

Table 4
Estimated Effects of School Exposure on Alternative Outcomes

	(1)	(2)	(3)
Panel I: OLS Regression			
	ln(Height)	ln(Birth Weight)	Math Score
In 1st Grade	0.010	0.030	1.011
	(0.002)	(0.008)	(0.028)
p-value	0.000	0.000	0.000
Observations	12,754	11,914	12,754
R-squared	0.146	0.041	0.452
Panel II: Instrumental Variables Regression	(1)	(2)	(3)
	ln(Height)	ln(Birth Weight)	Math Score
In 1st Grade	0.000	0.015	0.776
	(0.005)	(0.023)	(0.084)
p-value	0.940	0.517	0.000
Observations	12,754	11,914	12,754
R-squared	0.144	0.041	0.443
Mean dependent var.	3.85	4.75	0.006

Notes: Standard errors (in parenthesis) are adjusted for heterogeneity and within school correlation. Height is measured in inches. Birth weight is measured in ounces. Math scores are standardized across grades with a mean zero and a standard deviation of one. The instrumental variables regressions in Panel II use "Predicted to be in 1st Grade" as an instrument for "In 1st Grade," where "Predicted to be in 1st Grade" is a dummy variable equal to 1 if the child was born on or before the birthday cutoff for beginning kindergarten in his/her state in 1998. Each specification includes the full set of controls, used in columns (2) to (5) of Table 3. See notes to Table 3 for details.

Table 5
Estimated Effects of School Exposure on Body Weight Across Different Groups: Instrumental Variables

Panel I: Childcare Before Kindergarten

	<25 hours per week			>=25 hours per week		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(BMI)	Overwt.	Obese	ln(BMI)	Overwt.	Obese
In 1st Grade	-0.037	-0.141	-0.044	0.013	0.048	0.089
	(0.022)	(0.078)	(0.057)	(0.022)	(0.080)	(0.060)
p-value	0.099	0.072	0.435	0.557	0.550	0.135
Observations	5,720	5,720	5,720	5,189	5,189	5,189
Mean dependent var.	2.791	0.250	0.108	2.800	0.272	0.118

Panel II: Difference between Pre-School and School Environment

	<25 hours per week childcare and full-day kindergarten			>=25 hours per week childcare or half-day kindergarten		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(BMI)	Overwt.	Obese	ln(BMI)	Overwt.	Obese
In 1st Grade	-0.061	-0.136	-0.063	-0.003	-0.051	0.041
	(0.027)	(0.095)	(0.061)	(0.017)	(0.057)	(0.044)
p-value	0.024	0.152	0.301	0.852	0.370	0.351
Observations	2,888	2,888	2,888	9,866	9,866	9,866
Mean dependent var.	2.786	0.234	0.099	2.798	0.269	0.120

Notes: Standard errors (in parenthesis) are adjusted for heterogeneity and within school correlation. Each column and panel presents estimates from instrumental variables regressions that use "Predicted to be in 1st Grade" as an instrument for "In 1st Grade," where "Predicted to be in 1st Grade" is a dummy variable equal to 1 if the child was born on or before the birthday cutoff for beginning kindergarten in his/her state in 1998. Panel I reports results separately by whether a child attended on average more or fewer than 25 hours per week of childcare in the year prior to school entry. Columns (1) – (3) of Panel II are limited to the sample of children who attended less than 25 hours per week of childcare prior to kindergarten and then attended full-day kindergarten. The sample used in columns (4) – (6) of Panel II includes students who attended 25 hours or more of childcare prior to kindergarten or were enrolled in half-day kindergarten. See notes to Table 3 for description of outcome and control variables.

Appendix Table 1
Summary Statistics – ECLS-K

Variables	**Mean**	**Std. Dev.**
Ln(BMI)	2.796	0.125
Obese	0.115	0.319
Overweight	0.261	0.439
Underweight	0.028	0.165
Math Score (Standardized Across Grades)	-0.001	0.993
ln(birthweight)	4.75	0.216
ln(height)	3.85	0.048
African American	0.14	0.347
Hispanic	0.175	0.38
Other Race	0.058	0.233
Female	0.487	0.5
Mother H.S. Drop Out	0.178	0.383
Mother H.S. Graduate	0.311	0.463
Mother’s Education Missing	0.265	0.442
Mother's Marital Status	0.550	0.498
Mother's Marital Status Missing	0.416	0.493
Mother's Age at First Birth	19.32	10.53
Mother's Age Missing	0.189	0.392
Mother Employed Before Child Entered Kindergarten	0.687	0.464
Mother's Employment Missing	0.076	0.264
Family Income Prior to Kindergarten	49861	53534
Income Missing	0.033	0.179
In 1st Grade	0.434	0.496
Predicted to be in 1st Grade	0.5	0.5
Age in Days Relative to the Cutoff / 100	0.003	1.03
Observations	12762	

Notes: The sample includes children with weight and height data available at an age within a 12-month range centered on the age at the end of kindergarten of the youngest complier with a state’s school entry cutoff. See Tables 1 and 2 and the text for further details.

Appendix Table 2
Estimated Effects of School Exposure on Body Weight for
NLSY Children: Instrumental Variables

	Panel I: Age 5-6 Only		
	(1)	(2)	(3)
	Ln(BMI)	Obese	Overweight
Grade level	0.030	0.118	0.134
	(0.043)	(0.091)	(0.114)
p-value	0.482	0.196	0.237
R-squared	0.073	0.031	0.038
	First-stage for Grade Level		
Birth date “makes” cutoff	0.424	0.424	0.424
	(0.063)	(0.063)	(0.063)
Observations	5,536	5,536	5,536
	Panel II: Ages 5-10		
	(1)	(2)	(3)
	Ln(BMI)	Obese	Overweight
Grade level	0.016	0.083	0.079
	(0.038)	(0.071)	(0.085)
p-value	0.672	0.243	0.354
R-squared	0.179	0.046	0.064
	First-stage for Grade Level		
Birth date “makes” cutoff	0.446	0.446	0.446
	(0.052)	(0.052)	(0.052)
Observations	15,929	15,929	15,929

Notes: Standard errors (in parenthesis) are adjusted for heterogeneity and within family correlation. “Grade level” is the grade reported, converted to expected grade in the fall of a given year. “Birthdate ‘makes’ cutoff” means the individual was born on or before the date by which the individual has to turn 5 in order to be eligible to start kindergarten in a given cohort in a given state. All columns include controls for a cubic in days from the individuals’ birth days to the school starting age cutoff for the individuals’ states of residence when they were 5 years old. Interactions between these age variables and a dummy variable indicating the individual “made the cutoff” are also included. Ten year dummies are included. Twenty-one dummy variables are included for each cutoff date used by states. Additional controls are included for race/ethnicity, sex, mothers’ characteristics: African American, Hispanic Female, mother H.S. drop out, Mother completed H.S., Mother has some college, Mother graduated college (additional education is the omitted category), mother is missing education information, mother’s Armed Forces Qualifying Test (AFQT) score, Mother’s AFQT information is missing, Mother’s ln(BMI), Mother’s BMI information is missing, mother’s age at the child’s birth. OLS estimates are not significantly different from IV, but are available upon request.