

School Policies and Children's Obesity

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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. 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, though, many studies have focused on the school environment. For example, Schanzenbach (forthcoming) 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. Recently, surveys indicate that since the passage of school accountability in the form of No Child Left Behind (NCLB), schools have increased the time spent on math and language arts (the tested subjects) at the expense of other subjects. In particular, time for physical education and recess is being cut, reducing activity levels for students. Below, we investigate the impact of NCLB on children’s obesity, and find that schools close to the annual thresholds in one year have a higher rate of overweight and obesity in the following year.

Each of these school policy studies is consistent with school attendance being deleterious for one’s health, but do not necessarily imply that it is. Rather, they indicate that some school environments are worse than others – that is, that schools with higher quality lunches, less junk food and less accountability pressure would likely produce leaner children. It may still be the case 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. We approach the question of whether school attendance affects children's obesity in a straightforward manner, by taking advantage of school starting age cutoffs. A given 7-year-old whose birthday is just before the cutoff will have finished two years of school, while one whose birthday is just after the cutoff will have had to wait to start, and now just finishing the first year.

In the next section, we discuss our findings on the impact of NCLB, followed by our analysis of the overall effect of school exposure. We then evaluate the plausibility of the estimates of the effects of school policies on children's obesity through a series of simulations. The final section concludes.

II. Accountability Pressures

In 2002, the Federal No Child Left Behind (NCLB) legislation was passed, requiring states to define and implement stringent accountability standards and prescribing increasing penalties for schools that fail to meet their state's standard. To investigate whether these new accountability pressures inadvertently impacted children's weight outcomes, we created a unique data set for Arkansas.¹ These data combine school-level rates of "obesity" and "overweight" for children in all schools in Arkansas with data from the Arkansas Department of Education on standardized test pass rates for all schools, by grade and subgroup.² The standardized test pass rates are those used for

¹ In 2003, Arkansas passed legislation requiring BMI report cards for children and at the school level, making the state ideal for this task.

² Obesity is defined as having a body mass index (BMI) greater than the 95th percentile of a distribution of age- and sex-specific BMIs from a baseline population from the 1970s. Overweight is defined analogously, with BMI greater than the 85th percentile. The official Arkansas documentation follows CDC convention and labels these thresholds differently as "overweight" and "at risk of overweight," respectively. We will use the more common terms "obese" and "overweight" instead.

determining whether a school is making adequate yearly progress (AYP) under NCLB. Our working premise is that NCLB-induced behavioral changes are likely to be greatest among schools that are close to the AYP thresholds. That is, schools easily meeting AYP standards are unlikely to feel the need to change their behaviors in the face of NCLB. Similarly, schools very far from making the standards may feel pressure, but will be less likely to think that a small change such as a reduction in recess time will be useful in addressing their deficiencies. Thus, we expect that schools with test scores just above and just below the AYP threshold in year t-1 are the most likely to make changes that might result in more overweight and obese students by year t. By comparing these schools to those far away from the thresholds, we can determine if NCLB is having an unintended impact on children's health.

Accountability under NCLB is mainly implemented by each state setting a passing score on a standardized test and creating a schedule showing what fraction of students must pass each test in each year to be considered meeting Adequate Yearly Progress (AYP). The passing rate increases over time until 100 percent of students are required to meet the standard by 2014. A school's AYP designation is determined not only by the average passing rate of its students overall. In addition, the passing rate of all designated sub-groups that have an enrollment of 40 or more students must meet the goal. Student sub-groups are defined by race (for whites, African Americans, Hispanics, etc.), and as low socio-economic status, English language learners, migrants, and students with disabilities. If any one of the student subgroups fails to attain AYP, then the entire school is designated as failing to meet AYP.³

Since a school will fail to make AYP if any subgroup fails to meet the passing threshold, we are especially interested in the worst-performing subgroup. For each school-grade year, for each

³ We refer to these as "failing" schools, though the official nomenclature is that these schools are in "School Improvement Status."

test, for each subgroup with an acceptable group size, we standardize the passing rates around the AYP threshold. For example, for the 4th grade math test, the initial passing threshold is 40 percent. If a subgroup had a 45 percent passing rate, their standardized rate for 4th grade math in the initial year is 5. Similarly if a subgroup had a passing rate of 30, their standardized rate for 4th grade math in the initial year is -10. Thus, positive standardized rates represent meeting AYP, while negative ones represent failure to meet AYP. Since AYP is determined at the school-level, not grade-level, we then aggregate the data to the school-year level. We use the worst performing grade overall and for each subgroup to be representative of the school. We then choose the worst performing subgroup upon which to base our assessment of AYP performance. We also maintain the overall math and literacy rates, as they reflect more generally on the school's academic performance. Additionally, based on the Common Core of Data, we calculate the percentage of the school's students who are nonwhite and the percent that are poor to control for observable demographics.

An important concern in determining if NCLB has an effect on child health is the fact that the types of schools doing better or worse in terms of AYP are likely to be very different in terms of the characteristics of their students and neighborhoods. At the same time, we have very limited access to background characteristics. However, while it is likely that very poor performing schools are more likely to have students with low socioeconomic (SES) characteristics that are positively correlated with overweight in the US, the opposite is most likely true of very high performing schools. By comparing the “middle-performing” schools that are close to the AYP thresholds to the pooled extreme groups, we are less likely to simply be estimating a low SES effect.⁴ We define close to the AYP threshold as being 5 percentage points above or below the threshold.⁵ While schools may have some idea that they are going to be close to making or missing the AYP threshold

⁴ In fact, in the raw data being “close” to the threshold is negatively related to the percent of the students who are poor and the percent that are nonwhite.

⁵ We have also experimented with 3 and 8 points above and below with qualitatively similar results.

and change behaviors contemporaneously, we will nonetheless estimate current rates of overweight or obese based on the previous year's test results to ensure that the school has had time to react to being close to the AYP threshold.

Table 1 presents our estimates. In the first column of the top panel, we see that compared to schools that were well above or below the AYP threshold in the previous year, schools that were within 5 points of that threshold have a student overweight and obese rate that is three-quarters of one percentage point higher. While, this effect is quite small, it is statistically significantly different from zero. Theoretically, these close schools should be changing behaviors in a manner that neither those well below the AYP threshold nor well above it will do. Thus, we further investigate whether the positive effect seen in the first column is driven entirely by a comparison with one end or the other. The second column, then, includes an indicator for being well below the AYP threshold, so that the close indicator is now in comparison only to those well above the threshold. Alternatively, the third column includes an indicator for being well above the AYP threshold, so that the close indicator is in comparison only to those well below the threshold.⁶ First, it is important that in both of these columns the point estimate for being close to the AYP threshold is positive. That said, it is clear that more of the significant overall effect is being driven by the comparison with the higher group. Compared to schools scoring well above the AYP thresholds, close schools' student overweight rate is more than a full percentage point higher. When compared to schools scoring well below the AYP thresholds, though, close schools' student overweight rate is only about one half of one percentage point higher. Additionally, only the former comparison is significantly statistically different from zero.

⁶ Obviously, the point estimates in this third column could be obtained from the estimates in the second column. We estimate this model in order to present the standard errors of all the coefficients.

It seems reasonable to conclude that “marginal” schools under NCLB, that is, those who realistically expect to be close to just making AYP, are those making changes that are adversely affecting their students’ health. The impact is very small, increasing obesity rates by about 2 percent.⁷ Note however, that this number represents a one-year impact. The second panel of Table 1 further investigates the timing of the effect, estimating models with additional lags.⁸ The first column is simply that from the main model from the top panel, translated into a long run impact. In the second column we see that the additional lag is significant, and slightly bigger than the original model. At the same time, the first lag also remains significant and only slightly smaller than before. The resulting long run impact is significant and almost twice the size of the original model, implying an increase in the rate of overweight and obesity of 1.4 percentage points (or a 3.7 percent increase on the base of 38). The final column adds one more lag, although this now reduces the sample by a full year’s worth of data. With three lags included, the significance of each lag drops, but all remain significant at the 10 percent level and the long run impact is very significant, at 1.687. That said, this effect is not significantly different from that using two lags, and the price in terms of data reduction is steep. Thus, it seems safe to conclude that the long run impact of being a marginal school under NCLB is about twice that of the short run impact, and is in the range of a 1.5 percentage point (4 percent) increase in the share of students who are overweight or obese. Even if being in a marginal school is worse than attending a non-marginal school, being in school may not be worse than not being in school. In the next section we directly compare same-age children who have been exposed to one or two years of school to investigate this broader question.

⁷ For overweight and obesity, a .753 increase on a base of 38.24 is a 1.97% increase, while for obesity only, a .465 increase on a base of 21.04 is a 2.21% increase.

⁸ Note that because we have data on test scores for more years than we have data on obesity rates, adding additional lags does not appreciably reduce our sample until a lag for year t-3 is included.

III. Using School Starting Age to Identify the Impact of Schools on Children's Obesity

A straightforward way to think about the impact of being in school versus not being in school is to take 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). Additionally, 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.

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 earlier starters in first grade to those from later starters in kindergarten will successfully compare similar aged children with different school exposures. 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 a year apart in school, we can 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 2 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 earlier starters and observed in first grade, while everyone in the before group would be later 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.

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 a 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 to appropriately scale the effects.

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 $\ln(\text{BMI})$ 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.⁹ Since these charts are based on a fixed population from the past, the 5th, 85th, and 95th percentile cutoffs can be used to define underweight, overweight and obese.

Table 3 presents a set of estimates for these outcome variables. There is no significant effect of an extra year of school on $\ln(\text{BMI})$, obesity, overweight or underweight. Additionally, there are no significant effects on a set of control outcomes, height and $\ln(\text{birthweight})$. 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. Thus, it is not the case that our methodology is simply incapable of finding any effects of school exposure.

We also investigate differences across subgroups, with Tables 4a and 4b repeating the regression discontinuity models for males and females, respectively. In Table 4a, 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.¹⁰ The estimates for females are similarly insignificant. 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 5a, 5b, and 5c, provide results for subgroups defined by mother's educational attainment. Looking first at children whose mother's are high school dropouts (5a), we see that the point estimates imply that additional school exposure increases weight, and some are unexpectedly large and significant. Below we will turn to

⁹ 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.

¹⁰ Note that qualitatively similar, but statistically significant results are found in a more straightforward regression approach (see Anderson, Butcher, Cascio and Schanzenbach, 2008), but some significant results are also found for the control outcome of birth weight in these models.

evaluating if these point estimates can be considered at all plausible. For now, turning to Tables 6a and 6b, which provide 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 plausibility of the only significant 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 another year.

IV. Simulating School Policy Impacts

In order to evaluate what size impacts should be considered reasonable, we simulate several potential school policy changes. Recognizing that the only way to affect a child's weight is for there to be an imbalance between calories taken in and calories burned, we start with a model of the basal metabolic rate (BMR), the resting energy expenditure. We chose to use equations from Schofield (1995), which were found by Wong et. al. (1996) to be similar on average to measuring BMR by indirect calorimetry, although McDuffie et. al. (2004) found that at the individual-level race can also be an important component of BMR. The Schofield equations are based solely on observed height and weight, but with separate equations for younger and older children.

(1) $BMR = 17.0 * (\text{weight in kg}) + 1.6 * (\text{height in cm}) + 371$ – for ages 3 to 10 years

(2) $BMR = 8.4 * (\text{weight in kg}) + 4.7 * (\text{height in cm}) + 200$ – for ages 10 to 18 years

These equations (taken from Wong et. al., 1996) represent the number calories burned by a child of a given age, height and weight if they did nothing but exist.

Activity also burns calories, at a rate that depends on the amount of time spent in the activity, the level of intensity, and current weight. Activity levels are measured as metabolic equivalent (MET) intensities, which are multiples of BMR. Thus we can define calories burned (CB) quite simply.

(3) $CB = (\text{hours spent in activity}) * (\text{weight in kg}) * MET$

A small number of calories are also burned via the thermic effect of food, which is estimated to be about 10 percent of the calories contained in that food. Thus, if we assume an individual is currently in balance, the amount of calories taken in should be equal to $1.1 * (BMR + CB)$.

To carry out our simulations, we take a sample of children age 6 to 16 years old from the 1999-2004 panels of the National Health and Nutrition Examination Survey (NHANES). Using the measured age, height and weight, we can calculate BMR for each child. According to CEP (2007), elementary schools devoted about 4 hours per week to PE and recess. We calculate CB from these activities by assigning an MET of 2.8, equivalent to a light level of activity.¹¹ Assuming all else equal and that the children are currently in balance, we can calculate current caloric intake for each child. All calculations are adjusted to represent calories per week. To simulate the impact of school policies, we will simply change the implied calories in (school lunch, junk food) or implied calories out (PE/recess) and calculate the implied weekly caloric imbalance (all else equal). We then adjust the weight for the fact that about 7500 excess calories increases weight by one kilogram and repeat

¹¹ This is the MET assigned to such activities as “standing – playing with children – light.” We also investigate “walk/run – playing with children – moderate,” which has an MET of 4.0.

the caloric imbalance calculation until the simulation has been carried out for an entire school year (36 weeks).

The simplest calculation is to simulate the impact of eating school lunch. As reported in Schanzenbach (forthcoming), eating school lunch versus brown bagging implies eating an extra 60 calories per day. Our NHANES sample starts with an obesity rate of 0.198, and an overweight/obese rate of 0.364, which increase to 0.219 and 0.407 by the end of the school year when we add 60 calories per school day. This two percentage point increase in the obesity rate is exactly in the range found by Schanzenbach. A simulation of junk food availability is a bit less straightforward, since it is unclear how much the average child eats. Thus, in Table 7, we start by just simulating a range of additional calories per week. Note that 300 is equivalent to eating school lunch instead of brown bagging, while 750 is close to having a non-diet soda in school each day and 1250 is similar to eating a standard vending-machine-size bag of a snack such as Doritos each day. Note that in all cases we are assuming no other changes in behavior, which may lead to overestimates of the impact if diet or exercise patterns actually change in a compensatory manner.

Table 7 presents not only the impact on the percentage of sample children who are overweight/obese and obese, but also on the average BMI and $\ln(\text{BMI})$ for the sample. The key point to take away from this simple simulation of additional in-school calories is that it is very easy to see large impacts on children's weight outcomes. All it takes is buying (and eating) a bag of chips from the vending machine each day to increase average BMI by about 10 percent and result in almost 60 percent of children being overweight. The fact that we are not yet seeing rates of overweight this high is likely an indication that the average child does not add 1250 *net* calories per week at school, despite how easy it would be to do. Even the typical child who eats from the

vending machine frequently is probably using the chips as a substitute for other foods, rather than as an addition, or as a snack supplement on an unusually active day.

Despite the ease of weight gain from school-provided foods, recall that in general our findings for school exposure were of no impacts on weight, albeit with negative point estimates overall. One possibility is that the structure inherent in going to school reduces the amount of snacking during the day. Thus, the next panel simulates a reduction in weekly calories. We consider fairly small changes, the biggest of which is equivalent to not having two chocolate chip cookies each day. Again, though, even small changes have noticeable impacts. In fact, the smallest change (cutting out one cookie per week), has an estimated $\ln(\text{BMI})$ impact of about the same size as the point estimate in Table 3, although our simulation for the effect on percent overweight is not quite as large as our point estimate. Recall, though, that these estimates were not statistically significantly different from zero. In fact, given the standard errors in Table 3, we would just barely be able to detect a significant negative effect of the size implied by the biggest of these changes, and then only for some of the outcomes.

Thinking again about a specific school policy, NCLB, we simulate a reduction in PE/recess of 1.5 hours, which is the amount reported by CEP (2007) as the decrease since passage of NCLB. Thus, we simulate a reduction of that size, from the baseline of 4 hours to just 2.5 hours of light activity per week. A change of this size is more than sufficient to produce an increase in fraction of overweight/obese of the size we found for schools close to the AYP threshold in Arkansas. Recall that rates were estimated to increase by 1 to 2 percentage points, while this reduction in activity increases the rate by just over 2.5 percentage points. The next lines in Table 7 show that we can get the estimated effect for Arkansas from smaller declines in PE/recess time.

Again thinking more broadly about exposure to school, it is possible that even with no changes in PE/recess the structured time of school reduces activity levels for some children relative to not being in school. According to time use surveys in the Panel Study of Income Dynamics (PSID), on a weekend day children age 6-8 spend about 3 hours in the three time uses that I classify as active – play, sports, and outdoor activities. On a weekday, the time spent in these activities is just 1.5 hours. Note that the main use of time on weekdays is being in school, so any activity in school is not included separately. Suppose that in the absence of school, every day would be like a weekend, meaning children would be active for 21 hours per week. Instead, they get 6 hours of activity on weekends, 7.5 hours on weekdays, and 4 hours in school (PE/recess). Thus, the next line simulates a change from 21 weekly hours of activity to 17.5 weekly hours. The result is a fairly large increase in child overweight of about 6 percentage points.

Finally, to better reflect the overall impact of school exposure, we combine this change in activity from school attendance with changes in nutrition that may occur. The first combinations reflect offsetting changes. That is, the structure of school reduces activity, but also reduces snacking opportunities. Combining an activity reduction with just a 500 reduction in calories leaves the fraction overweight exactly the same, with essentially no changes in the other measures either. Increasing the calorie reduction reduces weight in the sample. With a 1500 calorie reduction, the obesity rate is down 4 percentage points, similar to the point estimate for males in Table 4a. Finally, we simulate reinforcing effects – the activity reduction combined with an increase in calories consumed. Recall that in Table 5a there were some very large estimates of school exposure for children of low education mothers. Interestingly, it is possible to simulate quite large impacts on the fraction overweight simply by combining reduced activity with a daily bag of chips. This impact of 0.288 on overweight is still quite a bit smaller than the point estimate of 0.372 found in

Table 5a, although given the standard errors, we cannot reject that they are the same. In fact, the standard errors are large enough that we cannot reject that they are the same as the simulations of much smaller changes.

Overall, there are several lessons to be learned from these simulations. First, seemingly small changes in diet and exercise can have fairly substantial effects on the overweight status of the school-aged population. Second, the existing empirical estimates of school policy impacts generally seem quite reasonable given these simulations. Finally, relatively small changes in eating behaviors seem to have larger effects than potentially more time-consuming changes in activity levels.¹²

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. Past studies have found that eating school lunches and being exposed to junk food in schools may result in weight gain (Schanzenbach (forthcoming), Anderson and Butcher (2006)), while this study has shown that NCLB may also have increased rates of overweight. However, 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

¹² Note that changing the activity level from light to moderate (i.e 2.8 MET to 4.0 MET) implies bigger impacts for exercise (e.g. reducing PE/recess to 2.5 hours would result in an overweight rate of 0.405 instead of the reported 0.391 if the activity was more vigorous. However, given that the estimates for Arkansas are even smaller, only the light activity results are reports.

education are less likely to be overweight. Thus, 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. A regression discontinuity is an ideal approach to dealing with the endogeneity of school exposure. We find no real evidence of a significant effect of school exposure for the overall sample, although point estimates for $\ln(\text{BMI})$ and the probability of being overweight are negative. Also, point estimates for the lower socioeconomic status children are positive, but generally not significant. Finally, using a model of basal metabolic rate, metabolic equivalent intensities of activity and the fact that an excess of 7500 calories adds a kilogram of weight, we simulate the potential effect of a range of school policies. Quite small changes in policy can result in noticeable changes in the weight distribution of children. Additionally, the existing empirical estimates are generally very much in the range expected based on these simulations.

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Table 1
Effect of a School Being “Close” to Meeting AYP on Student Weight Status

	Pct of Students Who Are Obese or Overweight	Pct of Students Who Are Obese or Overweight	Pct of Students Who Are Obese or Overweight
W/in 5 points of AYP threshold last year (t-1)	0.753 (0.278)	1.024 (0.323)	0.450 (0.363)
Below 5 points of AYP threshold last year (t-1)	--	0.573 (0.405)	--
Above 5 points of AYP threshold last year (t-1)	--	--	-0.573 (0.405)
Observations	2661	2661	2661
R-Square	0.2976	0.2984	0.2984
	Pct of Students Who Are Obese or Overweight	Pct of Students Who Are Obese or Overweight	Pct of Students Who Are Obese or Overweight
W/in 5 points of AYP threshold last year (t-1)	0.753 (0.278)	0.635 (0.273)	0.531 (0.318)
W/in 5 points of AYP threshold year t-2	--	0.786 (0.291)	0.569 (0.323)
W/in 5 points of AYP threshold year t-3	--	--	0.586 (0.301)
Long Run Effect	0.753 (0.278)	1.421 (0.486)	1.687 (0.682)
Observations	2661	2512	1815
R-Square	0.2976	0.3098	0.3129

Notes: Standard errors (in parentheses) are corrected for within school correlation. All models include a time trend and for year t-1: four powers of the overall literacy rate relative to AYP, four powers of the overall math rate relative to AYP, four powers of the percent of students who are nonwhite, and four powers of the percent of students who are poor.

Table 2
Arrangement of Data for the Regression Discontinuity Approach,
Assuming a September 1 School Start Cutoff

<i>Centered Age</i>	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5
<i>Birthdate (compliers)</i>	Feb 93	Jan 93	Dec 92	Nov 92	Oct 92	Sep 92	Aug 93	Jul 93	Jun 93	May 93	Apr 93	Mar 93
<i>Age on May 1</i>	6-3	6-4	6-5	6-6	6-7	6-8	6-9	6-10	6-11	7-0	7-1	7-2
<i>Birthdate (non-compliers)</i>				Nov 93	Oct 93	Sep 93	Aug 92	Jul 92	Jun 92	May 92	Apr 92	Mar 92
<i>Age on May 1</i>				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 3
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 (0.010)	0.001 (0.026)	-0.019 (0.036)	0.004 (0.013)	0.252 (0.177)	0.004 (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 (0.016)	0.002 (0.042)	-0.031 (0.059)	0.006 (0.022)	0.406 (0.286)	0.007 (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 4a
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 (0.013)	-0.024 (0.036)	-0.001 (0.047)	0.006 (0.019)	0.384 (0.234)	0.019 (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 (0.022)	-0.040 (0.060)	-0.002 (0.079)	0.010 (0.032)	0.650 (0.396)	0.033 (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 4b
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 (0.016)	0.030 (0.038)	-0.035 (0.056)	0.001 (0.018)	0.121 (0.274)	-0.012 (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 (0.025)	0.046 (0.060)	-0.054 (0.087)	0.001 (0.028)	0.196 (0.426)	-0.018 (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 5a
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 (0.028)	0.135 (0.084)	0.208* (0.095)	0.004 (0.024)	-0.069 (0.493)	0.010 (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 (0.050)	0.241+ (0.145)	0.372* (0.167)	0.007 (0.044)	-0.123 (0.884)	0.017 (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 5b
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 (0.018)	-0.032 (0.044)	-0.085 (0.065)	-0.006 (0.025)	0.184 (0.299)	0.001 (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 (0.035)	-0.062 (0.084)	-0.165 (0.126)	-0.011 (0.049)	0.356 (0.582)	0.005 (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 5c
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 (0.023)	-0.006 (0.056)	0.069 (0.086)	-0.008 (0.035)	0.299 (0.405)	-0.008 (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 (0.044)	-0.012 (0.107)	0.134 (0.166)	-0.014 (0.067)	0.573 (0.785)	-0.016 (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 6a
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 (0.022)	0.074 (0.058)	0.041 (0.077)	0.025 (0.021)	-0.003 (0.366)	0.046 (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 (0.033)	0.111 (0.086)	0.062 (0.115)	0.038 (0.032)	-0.005 (0.552)	0.069 (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 6b
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 (0.013)	-0.027 (0.034)	-0.012 (0.047)	-0.018 (0.017)	0.229 (0.231)	-0.008 (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 (0.024)	-0.040 (0.061)	0.003 (0.085)	-0.021 (0.032)	0.320 (0.413)	-0.011 (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%

Table 7
Simulations of the Impacts of School Policy Changes

Model	Description	Overweight Rate	Obesity Rate	BMI	Ln(BMI)
A	Baseline	0.364	0.198	21.241	3.026
Additional Calories in/week					
B	100	0.377	0.206	21.412	3.034
C	300 (school lunch each day)	0.407	0.219	21.755	3.052
D	500	0.442	0.238	22.097	3.068
E	750 (soda each day)	0.487	0.261	22.526	3.089
F	1250 (bag of chips each day)	0.594	0.318	23.382	3.129
Reduction in Calories in week (no snacks)					
G	150 (no cookie in week)	0.347	0.188	20.984	3.012
H	500	0.308	0.168	20.384	2.981
I	750 (no cookie each day)	0.283	0.154	19.956	2.957
J	1500 (2 less cookies each day)	0.222	0.122	18.671	2.883
Reduction in PE/Recess per week					
K	4 Hours Weekly PE/Recess Reduced to 2.5 hours (light activity)	0.391	0.216	21.610	3.043
L	4 Hours Weekly PE/Recess Reduced to 3 hours (light activity)	0.383	0.210	21.492	3.037
M	4 Hours Weekly PE/Recess Reduced to 3.5 hours (light activity)	0.373	0.204	21.366	3.032
Reduction in Activity due to school structure					
N	4 Hours Weekly PE/Recess plus 13.5 Hours play/outdoors/sports, vs. 21 Hours play/outdoors/sports	0.426	0.233	22.041	3.062
Reduction in Activity & Change in Calories					
H and N	Reduce Activity & 500 kcal less	0.364	0.202	21.256	3.023
I and N	Reduce Activity & 750 kcal less	0.339	0.188	20.863	3.002
J and N	Reduce Activity & 1500 kcal less	0.272	0.152	19.684	2.938
C and N	Reduce Activity & 300 kcal more	0.471	0.259	22.513	3.085
F and N	Reduce Activity & 1250 kcal more	0.652	0.358	24.006	3.154

Notes: Simulation based on a sample of 7562 children age 6 to 16 in the 1999 – 2004 panels of the National Health and Nutrition Examination Survey (NHANES).