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The Effect of Universal Free School Meals on Child BMI

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The Effect of Universal Free School Meals on Child BMI[1]

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Abstract: We estimate the effect of universal free school meal access through the Community Eligibility Program (CEP) on child BMI. Through the CEP, schools with high percentages of students qualified for free or reduced-priced meals can offer free breakfast and lunch to all students. With administrative data from a large school district in Georgia, we use student-level BMI measures from the FitnessGram to compare within-student outcomes before and after CEP implementation across eligible and non-eligible schools. We find one year of CEP exposure increased expected BMI percentile by about 0.085 standard deviations, equivalent to a nearly 1.88-pound weight increase for a student of average height. We also find that the program led to a small increase in the likelihood of overweight and limited evidence of a small decrease in the likelihood of underweight. We do not find that the program increased student obesity risk. Examining the effects of CEP on child BMI by grade suggests that the overall effect is largely driven by students in middle schools, highlighting potential heterogeneity in the program's impact across grades. The findings of this paper are relevant for researchers and policymakers concerned with the effects of universal free school meals on student health.

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1. Introduction

During a typical academic year, children consume between one-third and one-half of their daily calories in school (Schanzenbach, 2009; Briefel et al., 2009). In 2019, the National School Lunch Program (NSLP) and the School Breakfast Program (SBP) provided lunch and breakfast to 30 million and 15 million students on average each day, respectively (FRAC 2021). Many students participating in the NSLP and SBP come from families with incomes at or below 185 percent of the federal poverty line; 22 million and 12.6 million in 2019, respectively (FRAC 2021). Both programs provide consistent access to food for millions of children from poor households during each school day. These programs do, however, come with a high cost. The total cost of meals served under the NSLP was $14.2 billion in 2018-19, making it the second most expensive government nutrition assistance program in the U.S. behind the Supplemental Nutrition Assistance Program (SNAP).[3]

While some students pay the higher "full price" for meals under the NSLP and SBP, students from low-income families can enroll in Free or Reduced-Price (FRP) meals dependent on their household income and participation in other government assistance programs. An alternative to the traditional system that does not rely on student-level eligibility, universal free school meal (UFSM) programs enroll a school's entire student body in free lunch and/or breakfast. Given the growing popularity of UFSM programs, there is increased concern over the quality of food served and their potential impacts on child health. The last nationwide policy targeting minimum nutrition content of school meals was the Healthy Hunger-Free Kids Act (HHFKA) of 2010. In addition to increasing minimum nutrition standards, the HHFKA introduced a new universal free school

[3] https://www.ers.usda.gov/topics/food-nutrition-assistance/child-nutrition-programs/national-school-lunch-program/

meal program available nationwide - the Community Eligibility Provision (CEP).[4] Under the CEP, schools with 40 percent or more of their students identified as eligible to receive FRP meals can adopt CEP. Through the CEP, schools serve universal free lunch and breakfast to their entire student body, regardless of individual students' previous eligibility or take-up of FRP meals. The CEP's primary goal is to address low FRP meal participation rates among previously eligible students and satisfying unmet need among ineligible students that would benefit from free meals. Low FRP meal participation under the traditional system may be due to program- and household-level barriers such as a lack of program awareness, application complexity, or the stigma attached to FRP meal participation. Before the start of the COVID-19 pandemic (school year 2019-20), 30,667 schools across the U.S. participated in CEP, offering free meals to 14.9 million students (Maurice et al., 2020).

A pivotal question surrounding school meals is their effect on student health. Evidence on the relationship between school meals and student Body Mass Index (BMI), a measure of weight for height, and related measures like obesity are mixed. Some studies find students receiving school lunches are more likely to have higher BMIs than their peers (Schanzenbach, 2009; Millimet et al., 2010), as does recent evidence suggesting breakfast in the classroom increases the risk of overweight, but not obesity (Abouk and Adams, 2022). Alternatively, other studies find no evidence of detrimental effects (Schanzenbach and Zaki, 2014; Corcoran et al. 2016). Schwartz et al. (2020) find that the NSLP improves weight outcomes for non-poor children receiving free meals through a UFSM program in New York City.

In this study we estimate the impact of attending a CEP school on student BMI and related outcomes. Like some other studies (Schwartz and Rothbart 2020; Davis 2020; Corcoran et al.

[4] Please see the Appendix for a glossary of acronyms.

2016), one strength of our study is access to student-level BMI data available for the same sample of students before and after CEP implementation. Additionally, while many existing studies use data from other parts of the U.S., we focus on students living in the Southeast, a region that may have unique underlying characteristics which could produce differential CEP impacts. Our data come from the FitnessGram, a mandatory physical fitness exam taken by students enrolled in physical education classes, and administrative student records from a large metro-Atlanta area school district covering seven years (2012-2018). These data let us compare students in schools that adopted CEP (treatment) to students in schools that did not (control) before and after the CEP's launch. Using a student-level fixed effects model, we estimate the within-student impact of attending a school that participates in the CEP on BMI, specifically student BMI percentile and BMI z-score, and the likelihood of underweight, overweight, and obesity.

We find that attending a CEP school increases child BMI by 2.65 percentile points, or 0.085 standard deviations, on average, normed within age and sex to a national BMI reference distribution of children in the U.S.[5] This change represents an average relative increase in body weight of about 1.88 pounds. We also estimate the program's effects on the probability of falling within the underweight, overweight, and obese BMI ranges. We find CEP school attendance leads to a 3.2 percentage point increase in the probability of being overweight, but no evidence to support an impact on the probability of obesity. We also find some evidence suggesting CEP school attendance may decrease the probability of underweight, representing a potentially beneficial impact of the program for underweight students. Finally, estimating the CEP's effect on student BMI percentile by student grade shows that our full-sample effect is largely driven by students in

[5] Reference categories for BMI for age (z-scores and percentiles) are taken from 2000 CDC Growth Reference charts. Therefore, student age-by-gender BMI calculations are not relative to peers in our data set, but to a nationally representative sample of children in the United States.

middle school grades with the greatest effects for students in 6^{th}-8^{th} grade. This finding highlights potential heterogeneity in the program's weight effects across K-12 grades which agrees with the findings of some previous studies (e.g., Rothbart et al. 2022). As highlighted by Rothbart et al. (2022), this result also aligns with Dwicaksono et al. (2018) who show that obesity rates among students in secondary grades are more sensitive than those of primary school students. We also show that our primary results are robust to several alternative sample restrictions and model specifications.

In summary, our results suggest that while CEP school attendance leads to an expected increase in student BMI and the probability of being overweight, the program did not lead to an expected increase in the likelihood of obesity and may improve outcomes for underweight students. Many existing studies of the CEP's impacts on student achievement, discipline, and attendance[6] have not estimated the program's impacts on child health, a potential channel through which CEP may affect these other outcomes. We contribute to the literature by providing evidence on how the CEP impacts child health and situate our results within existing CEP studies. Since many studies find that CEP leads to positive changes in other outcomes like student achievement and discipline, the increase in BMI and overweight we observe should be weighed against the beneficial effects of CEP adoption.

2. Background

2.1 Policy Details

The CEP was introduced to schools in the U.S. through 2010's HHFKA. Under the CEP, eligible schools can choose to provide free breakfast and lunch to their entire student body. School CEP

[6] Gordanier et al. (2020); Ruffini (2021); Gordon and Ruffini (2021)

eligibility is determined by an Identified Student Percentage (ISP). Each school's ISP represents the share of their students eligible for FRP school meals through participation in other government assistance programs like SNAP and Medicaid, or other special criteria like being homeless or from a migrant family.[7] Schools with ISPs of 40 percent or higher and districts with average ISPs at or above 40 percent are eligible for CEP.[8]

The CEP's primary goal is to address low FRP meal participation rates among eligible students. Eligible students may not enroll in FRP meals under the traditional system for a variety of reasons, including lack of information about their school meal options and difficulty navigating the FRP application process. There is also stigma attached to FRP participation, causing some children or parents not to enroll in FRP meals (Askelson et al., 2017). CEP adoption is meant to remove this stigma as all students are enrolled in free school meals, causing FRP participation to no longer be a valid signal of household income level.

After piloting the CEP in 11 states, the program was made available nationwide beginning in June 2014. The program has proven successful in terms of adoption. During the 2019-20 school year, 69 percent of all CEP eligible schools participated (Maurice et al., 2020). Early evidence suggests that CEP participation is correlated with higher levels of school meal participation, aligning with program goals (Harkness et al., 2015; Ruffini 2021). These findings also support previous evidence suggesting that UFSM programs increase school meal participation rates (Ribar and Haldeman, 2013; Leos-Urbel et al., 2013).

[7] For details regarding all "identified" eligibility criteria see: https://frac.org/wp-content/uploads/direct-cert-improves-low-income-school-meal-access.pdf. Students may qualify for FRP meals based on income eligibility even if they are not "identified" as eligible based on participation in other government assistance programs.

[8] Under district-wide enrollment, all schools in the district participate in the CEP, including schools that have ISPs below 40 percent. In our district, schools individual schools participated in the CEP rather than the district enrolling.

While the CEP take-up decision is complex, one potential reason why we do not see all eligible schools participate relates to federal meal reimbursement guidelines. Under the traditional school meal system, the United States Department of Agriculture (USDA) reimburses schools for each lunch and breakfast served at a predetermined rate. Free meals earn the highest reimbursement followed by reduced price meals and finally full price meals. All meals are provided to students for free under the CEP, but the USDA only reimburses a share of 1.6 times the school's ISP (1.6xISP) of meals at the free rate. The remaining share (100 – 1.6xISP) are reimbursed at the much lower full price rate.[9] For schools at the minimum ISP threshold for CEP eligibility of 40 percent, 64 percent of their meals served under CEP are reimbursed at the free rate while 36 percent of meals are reimbursed at the lower full price rate. This reimbursement rule may create a financial disincentive among the set of barely CEP eligible schools.

In Figure 1, we examine the relationship between CEP participation and ISP for our district. Figure 1 shows school ISP on the x-axis with schools' Free and Reduced-Price Lunch (FRL) participation rate on the y-axis for schools in our district separated by eventual CEP participation status and the first year of CEP participation for the set of ever-CEP schools. Most CEP schools in our sample first enrolled in 2016 with no schools dropping CEP in later years. As expected, all CEP schools in our district have an ISP at or above 40 percent. CEP eligible schools with higher ISPs are more likely to adopt the program relative to eligible schools with ISPs closer to 40 percent. This finding matches the assumed relationship between CEP participation and the reimbursement rule where CEP schools do not have 100 percent of their meals reimbursed at the free rate until an

[9] Average revenue earned by a school for each school meal served is the amount paid by a student for the meal (in the case of a full price or reduced-priced meal) plus the amount reimbursed by the USDA. Details on meal reimbursement rates are available at https://www.fns.usda.gov/school-meals/rates-reimbursement.

ISP of 62.5 percent or higher. Another interesting finding from Figure 1 is the relationship between ISP, FRL percent, and CEP participation. Even among the set of schools with ISPs above 62.5 percent, some schools with low FRL participation choose not to adopt CEP. A potential reason for this low take-up rate could be if school-level characteristics causing low FRL participation also decrease CEP enrollment probability. For example, these schools may face greater constraints in meal provision capacity, limiting their ability to feasibly offer UFSM. Regardless, while we do not observe all factors in the CEP participation decision, we find that many CEP schools in our district have both high FRL and high ISP.

By design, CEP adoption should increase the share of students receiving free meals and decrease the share receiving reduced-price meals. While we do not observe breakfast participation for our district, Figure 2 plots time trends of the share of students receiving free lunch, reduced-priced lunch, and full price lunch separated by eventual CEP participation status (never-CEP vs. ever-CEP). For never-CEP schools, reduced-price lunch enrollment is relatively stable across all years. There is a small decrease in the share of students on free lunch between 2015 and 2016, but the trend flattens after 2016. Alternatively, the share of students on free lunch at ever-CEP schools increases in 2016 and continues to increase until 2017 which was the last year new schools in our district adopted CEP. From 2017 onwards, the share of ever-CEP students on free lunch stabilizes around 100 percent as expected. At the same time, the share of students on reduced-price lunch trends towards 0 as students in CEP schools previously enrolled in reduced-price lunch transition to free lunch. As expected, the share of students paying full price for lunch at ever-CEP schools drops to nearly 0 percent during the post-CEP period. Appendix Table A2 shows the impact of

CEP school attendance on the likelihood of FRP lunch enrollment which provides a regression adjusted analog of Figure 2.[10]

2.2 Mechanisms: Ways the CEP may Influence Child Weight

The impact of any school meal policy on child weight primarily depends on two main factors: who is affected by the policy, and how the quantity and quality of meals consumed changes, if at all. One key comparison is among students bringing meals from home and students eating school meals. Studies assessing the quality of school meals relative to meals brought from home provide mixed evidence.[11] After the HHFKA's changes to minimum school meal nutrition standards, Smith (2017) finds that NSLP and SBP participation improves diet quality among nutritionally disadvantaged students. This effect varies considerably across the initial diet quality distribution, however, worsening diet quality for children in the distribution's healthy right tail. It is also important to note that meal nutrition quality varies across schools (Ralston et al., 2008; Anderson et al., 2018). Frisvold and Price (2019) show that while there is only a weak relationship between free, reduced-price, or paid lunch in terms of average calories. Free lunch students have access to school menus with an average Healthy Eating Index score roughly one-third of a standard deviation lower than students who pay for lunch. Our study compares students within a single district with one nutrition program office, so we do not expect significant differences

[10] As previously noted, we cannot observe if a student consumes school lunch or breakfast, meaning that we cannot determine if the BMI effects we estimate are driven by previously ineligible students getting free meals through CEP or the enrollment of previously free meal eligible students. We are, however, able to estimate if CEP participation leads to an increase in FRL enrollment for students in our district. Table A2 of the appendix shows that CEP school attendance increases the likelihood of FRL participation by more than 7 percentage points for both the full sample of students and for the subsample of students in our FitnessGram sample. We find an 11 percentage point increase in free school lunch enrollment from CEP attendance and a decrease in the likelihood of reduced-price lunch enrollment of 3.9 percentage points. This change matches our a priori expectations as students attending a CEP school who were previously enrolled in reduced-price lunch will be automatically switched to free lunch under the CEP. Additionally, non-FRL-eligible students and students who are eligible but not participating are automatically enrolled in free lunch under the CEP.

[11] See Artega and Heflin (2014), Cook et al. (2006), Eicher-Miller et al. (2009), Farris et al., 2014, Gundersen and Kreider (2009), Huang and Barnidge (2015), Huang et al. (2016), Kirkpatrick et al. (2010), Kuku et al. (2012)

in the quality of meals across schools serving the same grade levels.[12] Additionally, effects of school meals are not homogenous across lunch and breakfast, with school breakfast participation leading to an expected decrease in weight even in contexts where lunch participation leads to an increase in student weight (Millimet et al., 2010).[13] Since the CEP makes both lunch and breakfast free, it is unclear what the program's aggregate effect will be if the two meal types produce different weight effects.

The second factor likely determining UFSM effects is the type of students impacted. We identify three student groups most likely to see meal consumption and/or weight changes under CEP. The first is students who were ineligible for FRP meals prior to CEP adoption. Under the traditional system, these students could bring meals from home or pay full price for school meals. CEP adoption may cause students to switch from home meals to school meals if cost was a primary reason for bringing meals from home. For full-price students, CEP participation represents an increase in effective household incomes as they no longer pay for meals. Handbury and Moshary (2021) find that CEP adoption reduces grocery spending for households with children, representing a 10 percent decline in grocery sales at large local retail chains.

Additionally, some students eligible for FRP meals prior to the CEP did not participate. If this non-participation is driven by a preference for home meals over school meals, we might expect no change in weight after CEP adoption. The effective price of meals from home does not change post-CEP, though a reduction in stigma could cause students to participate in UFSM. For some families unsure of their FRP eligibility status or how to enroll, however, we may expect them

[12] In communication with the nutrition director of our district, we learned that all schools of the same type (elementary, middle, and high school) have the same daily menu. All schools in the district also receive food from the same distributor.

[13] This may partly be due to the differential impact of breakfast and lunch program on student dietary quality. See Bhattacharya et al. (2006), Frisvold (2015), Briefel et al. (2009), Gordon et al. (2007), Campbell et al. (2011), Fox et al. (2010).

to participate in free school meals once they are automatically enrolled under CEP. Lastly, children eligible for reduced-price, but not free, meals may start participating once meals become free for all, either through the reduction in stigma or meal costs.

For students that switch from home meals to school meals post-CEP, the program's impacts on weight depend on relative meal quality. If meals from home are higher quality than school meals, the CEP may lead to detrimental changes in BMI. Alternatively, if meals from home are lower quality, participation in school meals may produce beneficial weight effects.

If some families provide students with meals from home due to personal preference rather than cost, the CEP may not change their behaviors. If students continue consuming only meals from home under the CEP, their diet quality would remain unchanged leading to no expected change in body weight. Alternatively, it is possible that children may consume some combination of home meals and school meals post-CEP. The program's impact on body weight for these students is ambiguous depending on the relative quality of both meal types and what portions of both meals students consume. An extreme case would be a student consuming both meals entirely. Consuming both meals could produce substantial increases in a student's calorie consumption and expected body weight. In our data, we do not observe if students consumed school meals. Therefore, we estimate the CEP's overall effect across these various student groups.

As noted above, families may experience an income effect from CEP we cannot observe. Switching to UFSM effectively puts more money in the pockets of families who were previously purchasing school meals or bringing meals from home. While this money could be used any number of ways, we cannot rule out the case where families spend more on meals served at home and do not speculate on whether this change would increase a student's diet quality or food consumption. Weight effects from increased spending on food for home consumption are ambiguous, but potentially larger for low-income families that spend a greater share of their income on food. While it is possible that these families spend more money on non-school meals, Handbury and Moshary (2021) find that adoption of CEP in local schools causes households with children to reduce their grocery spending.

Existing studies evaluating the effects of school meal participation on child health provide mixed results. There is evidence that lunch participation increases weight and the likelihood of poor BMI outcomes like obesity (Schanzenbach, 2009; Millimet et al., 2010; Capogrossi and You, 2017). Alternatively, Hinrichs (2010) finds no evidence of a long run effect from school meals on BMI. Gundersen et al. (2012) show that school lunch participation significantly decreases childhood obesity rates, food insecurity, and poor health status. Abouk and Adams (2022) estimate effects of the Breakfast Before the Bell program on student weight, a program designed to increase breakfast participation by providing breakfast in the classroom. They find that the program increased the share of overweight students by 11.6 percent, but do not find effects for obesity, concluding that the BMI impacts affect students in the middle of the BMI distribution.

Given the limited time since the CEP's national introduction, we know less about the student health impacts of CEP compared to traditional meal programs. Davis (2020) uses student-level panel data from the ECLS-K, finding that CEP school attendance increases child BMI and the probability of being overweight/obese while decreasing the probability of healthy weight. Their estimates vary geographically, however, with some limited evidence of a beneficial effect for students in the Northeast. Rothbart et al. (2022) analyzes district level obesity rates and finds some evidence of a decrease in obesity rates for students in secondary grades. While our understanding of the CEP's effect on child health is relatively limited, studies of other UFSM programs find little effect on BMI and some evidence that the switch to UFSM improves weight outcomes among non-poor students (Schwartz et al., 2020). We position our results in context with the existing CEP literature below, but in general we find similar results to those of other studies.

3. Data

Data for this study come from a large Georgia school district in the metro-Atlanta area partnered with the Metro Atlanta Policy Lab for Education (MAPLE). The district's data include student demographic characteristics including race, age, grade level, gender, and FRL participation status. Data are available for all students in the district enrolled in grades K-12 from 2012 to 2018. Like many urban districts, there is considerable mobility across schools. We can observe students changing schools within our district, but we cannot follow students that leave the district. Therefore, our within-student empirical strategy relies on students we observe in the district during at least one pre- and one post-CEP school year.

For student body weight outcomes, we use data from FitnessGram examinations. Developed by the Cooper Institute, FitnessGram tests are used in schools across the U.S. to assess student health.[14] In Georgia, public-school students enrolled in a physical education (PE) class take mandatory FitnessGram tests at least once each year.[15] P.E. requirements vary by grade, implying that FitnessGram participation rates do as well. Between 2012 and 2018, roughly two-thirds of all students in our district took the FitnessGram at least once and over one-third took the FitnessGram more than once. Testing frequencies were higher for younger cohorts that entered the

[14] For details see https://www.cooperinstitute.org/vault/2440/web/files/662.pdf

[15] According to the Georgia Department of Education's 2017 Fitness Assessment Program Report, in the 2016-17 school year, 1.1 million students in Georgia (71.3 percent of the total student population) from 2,291 schools participated in the examination. While the test must be conducted at least once each year, schools may choose to administer the FitnessGram more frequently.

sample towards the start of our study period as they had more years of potential exposure.[16] Figure 3 shows the share of students taking FitnessGram each year separated by school CEP participation status and first year of CEP participation for ever-CEP schools. We find similar trends in FitnessGram share for ever-CEP and never-CEP schools, with a spike in 2018 for both groups.[17] After 2013, differences in the share of FitnessGram students across ever- and never-CEP schools are small and stable through 2017. Since 20 of the 27 ever-CEP schools in our district adopted CEP in 2016, the trend we observe is largely driven by the 2016 cohort of ever-CEP schools. To address potential FitnessGram participation selection concerns, we begin our analysis by examining the relationship between CEP adoption status and school- or student-level FitnessGram participation rates in Section 3.1 below. We find little evidence of systematic FitnessGram selection on observables that could bias our primary estimates.

In addition to tests of physical fitness, FitnessGram administrators directly measure each student's weight and height. We use weight and height to calculate student BMI, defined as weight in kilograms divided by height in meters squared, as well as BMI percentile and BMI z-score.[18] The CDC defines healthy (healthy weight) and unhealthy (underweight, overweight, and obese) BMI levels for children based on their BMI percentile among all children of a given age and sex.

[16] In Georgia during the 2016-17 school year 94.5 percent, 72.3 percent, and 63 percent of students in elementary, middle, and high school were enrolled in a PE class, respectively.

[17] In conversations with district administrators, we learned that the 2018 spike in participation was caused by hiring a new physical education coordinator. To gauge the sensitivity of our results to this change, we estimate our models without data from the 2018 school year and find that our results are robust. These results are available upon request.

[18] BMI z-score and percentile are calculated using the "zanthro" package in STATA. Unlike adults which have standardized interpretations of BMI, the relative interpretation of child BMI varies by age and gender. We generate BMI percentiles and z-scores based on each student's position in a nationally representative distribution of BMI by age and gender. Percentile scores are the cumulative area from negative infinity to the specific z-score value. For example, a z-score of -1.96 corresponds to a percentile ranking of 2.5. Similarly, 2.5 percent of all children in the distribution have a z-score larger than +1.96. Therefore, percentile rank increases more towards the middle of the distribution for the same increase in z-score. Our preferred specification is BMI percentile since it shows how far students' BMIs move within the national reference distribution. BMI z-scores show similar effects, but for BMI we believe the reference point provides a more concrete interpretation of our estimates.

Table 1 shows school-level summary statistics by school CEP and FitnessGram participation status.[19] Column 1 of Table 1 shows summary statistics for schools that did not report FitnessGram results during our study period. Columns 2 and 3 show summary statistics for schools with at least one period of FitnessGram participation separated by CEP participation status. On average, ever-CEP and never-CEP schools have a similar share of students taking FitnessGram. As expected, ever-CEP schools have higher a higher share of students that ever participate in free lunch pre-CEP at 91 percent compared to 37 percent for never-CEP schools. The share of students ever participating in reduced-price lunch pre-CEP is similar across both school types. Race/ethnicity is reported using non-mutually exclusive categories, specifically White, Black, and Hispanic. Because of small group sizes, we combine students reporting other races or ethnicities into an Other Race category. Ever-CEP schools serve much higher numbers of Black students than never-CEP schools at 84 percent and 34 percent, respectively. The share of Hispanic students is similar for both groups. Ever-CEP schools also have smaller shares of students in the Other Race category, most of whom are Asian. Ever-CEP schools are more likely to be elementary and middle schools than high schools, implying potential heterogeneity in the likelihood of CEP participation by school type. For enrollment size, ever-CEP schools serve fewer students than never-CEP schools with average enrollments of 836 and 1,052, respectively.

The average BMI z-score and BMI percentile of never-CEP schools are notably higher than the national reference distributions' means. Ever-CEP schools have higher average BMI z-scores and percentiles than never-CEP schools, but the unadjusted difference in means is statistically insignificant across both school types. On average, ever-CEP schools have higher shares of overweight and obese students and lower shares of normal weight and underweight students than

[19] For student-level summary statistics please see Table A1 of Appendix A.

never-CEP schools, but these differences are statistically insignificant. Finally, because ever-CEP schools tend to serve lower grades on average than never-CEP schools, we observe a statistically significant difference in average student height across both school types.

3.1 Trends in Student BMI

Figure 4 shows trends in BMI percentile across time for students in ever- and never-CEP schools. The set of ever-CEP schools is further separated by first year of CEP adoption. We find that students in ever-CEP schools have significantly higher BMIs on average. This difference supports our methodological approach which controls for student fixed effects and identifies effects using *within-student* changes in BMI outcomes caused by CEP school attendance. The overall trend for ever-CEP schools is driven by the 20 schools that adopted CEP in 2016 since the 2015 and 2017 CEP cohorts are small at 2 and 5 schools, respectively, producing higher variance across time.

Figure 5 plots the *difference* in mean BMI percentile across ever- and never-CEP schools during the pre- and post-CEP periods. Figure 5 also shows trends in the average BMI difference for ever- and never-CEP schools separated by year of CEP adoption to see if schools that adopted CEP in 2015, 2016, and 2017 have differential trends. Figure 5 suggests that differences in BMI percentile across ever- and never-CEP schools are relatively stable during the pre-CEP period for the full sample of ever-CEP schools and the sample of 2016 ever-CEP schools. This finding implies that the mean difference in BMI percentile for both school types saw little variation prior to CEP adoption. In the post-CEP period, the difference in BMI percentile averages increased for the full set of ever-CEP schools and 2016 ever-CEP schools, with the largest change occurring in 2017 before returning to a level only slightly above the pre-CEP period in 2018. The small number of schools adopting CEP in 2015 saw trends in the pre-CEP period similar to those of the full sample, but a larger increase during 2016 and 2017. Schools that first adopted CEP in 2017, however, show more variation in their trends during the pre-CEP period, but similar trends to the full ever-CEP sample during the post-CEP period. We examine the sensitivity of our results to excluding 2015 and 2017 ever-CEP schools in Section 5 and find similar effects.

To evaluate trends in BMI towards the upper end of the distribution, we show trends in schools' shares of overweight/obese students in Figure 6. Figure 6 shows trends for the full sample of ever-CEP schools and separate trends for ever-CEP schools by first year of CEP adoption. Trends in the share of overweight/obese students for never-CEP schools and the full sample of ever-CEP schools are largely similar pre-CEP. The difference closes somewhat in 2014 relative to 2013, but the change is relatively small. Post-CEP, we find that the share of overweight/obese for ever-CEP schools increased significantly from 2017 to 2018 compared to never-CEP schools. Like Figure 5, 2016 CEP schools have similar trends compared to the full sample of ever-CEP schools in Figure 6. Alternatively, 2015 CEP schools saw different trends in their share of overweight/obese students across the pre- and post-CEP periods compared to the full set of ever-CEP schools. The trends for 2017 ever-CEP schools more closely match those of the full ever-CEP sample, but they differed in 2014 and 2015. Figure A1 of Appendix A shows trends in the share of obese students by CEP participation status. We find the share of obese students is higher in ever-CEP schools than never-CEP schools, but both trends are relatively flat pre-CEP.

3.2 School Participation in the CEP and FitnessGram Reporting

The CEP was first introduced nationally during the 2014-15 school year (referred to as 2015 in our study). In 2015, only 2 of the 103 schools in our district adopted CEP. In 2016, CEP participation in our district increased to 22 schools.[20] In 2017, 5 schools newly adopted CEP. By the end of our sample period 27 schools adopted CEP, or roughly 25 percent of all schools in our district.

From 2012 to 2018, about 72 percent of schools in our district reported at least one FitnessGram test result. There is, however, variation in the share of students taking FitnessGram across schools in the same school year and in the average share of students in a school taking FitnessGram across years. We see the FitnessGram participation rate drop to its lowest point in 2017 with only 50 percent of schools reporting FitnessGram, followed by the highest participation rate in 2018 around 91 percent.

We test if variations in FitnessGram participation rates have any likely implications for our primary results in two ways. First, we model determinants of school-level FitnessGram reporting using the following model:

$$FG_{st} = \alpha_0 + \alpha_1 N_{st} + \alpha_2 FRL_{st} + \alpha_3 Female_{st} + \alpha_4 RaceEth_{st} + \alpha_5 CEP_{st} + \gamma MaxGrade_s + \delta MinGrade_s + \theta_t + \varepsilon_{st} \quad (1)$$

In equation (1), FG_{st} takes one of three forms: a binary indicator equal to 1 if school s administered FitnessGram to any students in year t and 0 otherwise; percentage of students in school s who took the FitnessGram in year t; and percentage of students in school s who took the FitnessGram in year t for the set of schools reporting any participation. We note that a small number of schools

[20] Data on school ISP and CEP eligibility were collected from the FRAC database. This information was not posted publicly for the 2016-17 and 17-18 school years at the time of data collection. Based on 2015-16 data for our district, 2 out of 12 schools with ISPs between 40-50 percent, 8 of 10 schools with ISPs between 50-69 percent, and 12 out of 13 schools with ISPs above 60 percent participated in the CEP.

reported very low numbers/shares of FitnessGram students which may represent reporting errors or trial runs of new reporting software. For our analyses, we eliminate observations from schools with fewer than 5 percent of students reporting FitnessGram in a given year.[21] N_{st} is the number of students enrolled in school s in year t measured in 100's of students. FRL_{st} is the proportion of students in school s participating in FRL during year $t. Female_{st}$ is the percentage of female students in school s in year t. $RaceEth_{st}$ is a set of variables measuring the percentage of students of different races/ethnicities in school s in year t. CEP_{st} is a binary indicator equal to 1 if school s had CEP in year t and 0 otherwise. $MaxGrade_s$ and $MinGrade_s$ are indicators for the minimum and maximum grade served by school s. θ_t represents a set of year fixed effects.

Table 2 shows the results of our school-level FitnessGram regressions. Column 1 shows estimates of the probability that a school reports any FitnessGram test results each year. We find that the number of students and share of students participating in FRL during a given year have positive and statistically significant impacts on the probability of reporting any FitnessGram. Alternatively, schools with higher percentages of Black students and higher percentages of Hispanic students are less likely to participate in FitnessGram. Important for our study, in Column 2 we do not find a significant effect of CEP participation on the likelihood of any FitnessGram participation at the school level. Furthermore, Column 3 of Table 2 shows that CEP *eligibility* does not have a statistically significant effect, implying that neither CEP participation nor eligibility is associated with schools' FitnessGram participation.

Columns 4 through 6 of Table 2 show results for our regression where the outcome is share of a school's students reporting FitnessGram each year. These results suggest that none of the

[21] This restriction has no noticeable effect on our results as relatively few schools were dropped from the sample. Results with these schools included are available upon request.

included school-level observable characteristics have a statistically significant effect on the share of a school's students reporting FitnessGram. Finally, Columns 7 through 9 of Table 2 show results of our regressions for the share of schools' students taking FitnessGram restricted to the sub-sample of schools reporting any FitnessGram participation. Like Columns 4 through 6, we find no statistically significant relationships between our school-level observable characteristics and the share of students taking FitnessGram among the sub-sample of schools reporting any FitnessGram.

3.3 Student-Level FitnessGram Participation

A potentially more relevant test for our study is if CEP adoption influences individual students' likelihoods of FitnessGram participation. To test this relationship, we estimate the following student-level regression:

$$FG_{igst} = \alpha + \beta CEP_{st} + \theta_{gt} + \psi_i + \varepsilon_{igst} \quad (2)$$

where FG_{igst} is a binary indicator equal to 1 if student i enrolled in grade g at school s takes the FitnessGram in year t and 0 otherwise. CEP_{st} is a binary indicator equal to 1 if school s had CEP in year t and 0 otherwise. θ_{gt} and ψ_i are grade-by-year and student-level fixed effects, respectively. In some specifications, we omit student fixed effects and include a set of time-invariant student-level covariates, X_i, along with school-by-year fixed effects. The coefficient on CEP_{st}, β, in equation (2) shows if CEP school attendance affects student-level likelihood of FitnessGram participation.

Estimates for equation (2) are shown in Table 3. Column 1 only includes CEP school attendance, if the student is enrolled in elementary school or middle school, and year fixed effects.

In Column 1, we find that CEP school attendance does not have a statistically significant effect on student likelihood of FitnessGram participation. Alternatively, students enrolled in elementary and middle school are significantly more likely to participate in the FitnessGram than high school students. This finding is expected as students in Georgia are more likely to take PE in lower grades which require them. Adding student-level covariates in Column 2 does not alter the relationship between CEP and student-level FitnessGram participation. Adding school and grade-by-year fixed effects in Column 3 produces similar results.

In Columns 4 and 5 of Table 3, we estimate our model with grade-by-year and student fixed effects to control for observable and unobservable time-invariant characteristics that may otherwise bias our estimates. Column 4 shows that CEP school attendance does not have a statistically significant effect on the likelihood of student FitnessGram participation. Additionally, Column 5 shows that the cumulative number of years a student attends a CEP school has no significant effect on their likelihood of FitnessGram participation. Finally, to determine if students with specific characteristics see differential effects of CEP school attendance on FitnessGram participation, we estimate our regression with CEP status and its interactions with ever FRL, Female, and Black in Column 6 of Table 3 along with student and grade-by-year fixed effects. These results suggest a small, marginally statistically significant relationship between CEP participation and FitnessGram participation for students who ever participate in FRL and female students. These estimates provide some evidence of a small difference in the likelihood of FitnessGram participation by CEP attendance for students with specific observable characteristics. In total, Table 3 suggests that student FitnessGram participation status may differ across certain student sub-groups, but it is largely independent of CEP school attendance.

4. Effects of CEP on Child BMI, Weight, and Height

We now turn to the primary analysis estimating the effect of CEP school attendance on our student weight outcomes of interest. Our model includes student fixed effects, relying on within-student changes over time and across students by CEP school attendance, net time-invariant observable and unobservable student-level characteristics. Most ever-CEP schools in our sample first adopted in 2016, with only 2 schools adopting in 2015 and 5 in 2017. With small numbers of schools adopting CEP in 2015 and 2017, we do not have the statistical power needed to exploit staggered adoption timing among ever-CEP schools, meaning we cannot feasibly apply some of the recent estimation techniques from the Difference-In-Differences literature for heterogeneous treatment timing (e.g., Callaway and Sant'Anna 2021, D'Haultfoeuille and de Chaisemartin 2022, Goodman-Bacon 2021). Key threats to identification in our case would arise from unobservable student-level factors that vary over time and are correlated with CEP attendance and changes in body weight. We discuss these threats in context with our suite of robustness checks in Section 5.

Our primary model is as follows:

$$Y_{igst} = \alpha + \delta CEP_{st} + \Gamma X_{it} + \theta_{gt} + \psi_i + \varepsilon_{igst} \tag{3}$$

where Y_{igst} is either continuous child BMI-Z score, BMI percentile, weight, height, or binary indicators of underweight, overweight/obese, overweight alone, and obese alone for student i, enrolled in grade g, in school s, in year t. X_{it} is a set of time-varying student level controls including student age used to account for variation in age within grade, an indicator for entering a new school to account for shocks potentially correlated with moving schools, and the number of years since the student's last observation in our data set. θ_{gt} are grade-by-year fixed effects and ψ_i represents student-level fixed effects. Finally, ε_{igst} is the model's idiosyncratic error term. The primary effect of interest in equation (3) is δ, which captures the expected change in each outcome caused by CEP school attendance.

We estimate equation 3 using three specifications for CEP exposure. First, CEP_{st} is a binary indicator equal to 1 if student i attended a CEP school in year t and 0 otherwise. Second, we use the cumulative number of years of CEP school attendance in year t. For example, if a student has 3 years of cumulative CEP school attendance by year t, the CEP variable takes a value of 3. The cumulative specification captures two important features of our data. First, a student may transfer into, or out of, a CEP school at various points during the sample period, noting that school transfer is accounted for in the vector X_{it}. For example, a CEP middle school student may transfer into a non-CEP high school, so while they were exposed to the CEP in earlier periods, that exposure ends after transferring schools.[22] Second, since we see school CEP status in all years but each student may not participate in the FitnessGram every year, we must account for previous CEP attendance between FitnessGram observations. For example, if a student attending a CEP school in 2016, 2017, and 2018 completed the FitnessGram in 2015 and again in 2018, their cumulative years of CEP attendance would be 3 in 2018, accounting for their exposure between FitnessGram observation years. This specification also allows the effect of CEP attendance to vary by each year of exposure. Finally, we estimate the cumulative exposure model using separate indicators for 1, 2, and 3 to 4 years of exposure, noting that only students attending a CEP school in 2015-2018 can have four years which comes from only two 2015 CEP schools.

Table 4 shows results from our primary model. Panel A provides estimates of δ from

[22] Note that no schools in our sample that adopted CEP stopped participation in later periods. Therefore, the only change in CEP school attendance comes from the change from the pre- to post-CEP adoption periods and student school migration.

equation (3) using our binary CEP attendance variable. Column 1 shows that attending a CEP school increases expected BMI percentile by 2.65 percentile points. Columns 2 and 3 provide estimates for CEP school attendance's effects on BMI z-score and student weight, respectively, showing that CEP school attendance leads to an increase in expected BMI of 0.085 standard deviations and an increase in weight of 1.88 pounds. For estimates using the log of weight in Column 4 to approximate percent changes in weight, we do not find a statistically significant effect of CEP school attendance which suggests that the 1.88 pound weight increase shown in Column 3 was driven by students with higher baseline weight. It is also important to note that changes in weight, whether measured in pounds or percent changes, are not age-gender normed like BMI percentile and z-score.

In Column 5 we test for an association between student height and CEP school attendance. We use the log of height to approximate percent changes since the relevant interpretation of changes in raw height vary by student age and gender. While changes in height could come from a change in nutrition or calorie consumption, we consider height as a quasi-placebo test in that large changes may reflect differential trends in height by CEP. The results shown in Column 5 provide no evidence of a statistically significant difference in height by CEP school attendance.

Finally, Columns 6, 7, 8, and 9 of Panel A show estimates for the effect of binary CEP school attendance on the likelihood of overweight or obese, overweight alone, obese alone, and underweight, respectively. In Column 6, we find that CEP school attendance leads to a statistically significant increase in the likelihood of being overweight/obese of 3.3 percentage points. Examining the effects separately by overweight and obese in Columns 7 and 8, respectively, we find that the effect of CEP attendance on the likelihood of overweight/obese in Column 6 is driven by changes in the likelihood of overweight rather than the likelihood of obesity. We do not find a statistically significant effect of CEP attendance on underweight likelihood, suggesting that students attending CEP schools are not more or less likely to fall in the unhealthy left tail of the BMI distribution using binary CEP attendance for the full sample.

Panel B shows estimates of equation (3) using cumulative years of CEP school attendance. Results are consistent with those of Panel A. Column 1 of Panel B shows that *each year* of CEP school attendance increases expected BMI percentile by 0.97 percentile points. Multiplying this effect by the mean of cumulative CEP school attendance, 2.34 years, represents a 2.3 percentile point increase in BMI which is only slightly smaller than the 2.6 percentile point change found using binary attendance. Effects on other outcomes are similar across both specifications, save for a very small and marginally statistically significant decrease in the log of height shown in Column 5 of Panel B.

Panel C shows estimates for equation (3) using separate indicators for 1, 2, and 3 to 4 years of cumulative CEP school attendance. This specification allows for differential effects of cumulative CEP exposure as opposed to the linear specification of cumulative attendance shown in Panel B. In general, the results of Panel C are in line with those of Panels A and B. Column 1 of Panel C suggests that a sizable portion of the total effect of CEP attendance on expected BMI percentile occurs during the first year (2.03 percentile points) and second year (2.91 percentile points) of CEP school attendance. The coefficient on 3 to 4 years of exposure is similar to the 2-year coefficient at 2.8 percentile points, implying that student weight may stabilize after two years of attendance with little additional change from the third and fourth years. We do, however, find that the effect on the probability of overweight or obese in Column 6 does not stabilize after the second year of attendance. Specifically, the likelihood of being obese/overweight increases by 5.2 percentage points compared to the 3.2 percentage point increase observed from the second year of attendance. This increasing effect suggests that longer periods of CEP school attendance may lead to an increasing risk of overweight/obese.

Separating effects by the likelihood of overweight alone and obesity alone in Columns 7 and 8, respectively, suggests that changes in the likelihood of overweight, rather than obesity, are driving the impact of CEP on overweight/obese in Column 6. This finding is in line with the results shown in Panels A and B. Finally, in Column 9 of Panel C, we show that different amounts of CEP school exposure have differential effects on the likelihood of underweight. We find that the first year of CEP school attendance leads to a marginally statistically significant decrease in underweight probability of 0.8 percentage points. The decrease from the second year of CEP school attendance is larger at 1.7 percentage points and statistically significant at the 5% level. We do not find a statistically significant effect on underweight for students with 3 to 4 years of attendance. Our results suggest that students attending CEP schools experience reductions in underweight likelihood from the first and second years of attendance, but the effect disappears as students' years of CEP exposure continues to increase.

Taken together, the estimates in Table 4 highlight a few key effects of CEP school attendance on student body weight. First, we find largely consistent effects across our various specifications. Our results suggest that CEP school attendance increases expected student BMI z-score and percentile, and the probability that a student's BMI falls within the overweight or obese range. The effect for overweight or obese is driven by changes in the likelihood of overweight rather than obesity, suggesting that while CEP attendance increases the likelihood of overweight, students are no more likely to be obese. We also find that the effect of CEP school attendance on underweight likelihood is statistically insignificant in most cases, save for a statistically significant decrease in underweight probability observed in the first and second years of CEP school attendance. Our findings suggest that CEP school attendance may produce beneficial BMI increases for students in the unhealthy lower tail of the BMI distribution, but the effect is not constant across years of cumulative attendance. Additionally, we find little evidence to suggest a significant effect on student height. While height may change following changes to a student's diet or calorie intake, finding large changes in height caused by CEP school attendance would give reason for concern due to the relatively short period of potential CEP exposure in our sample. In most cases, we find consistent effects for our BMI related outcomes across the various specifications of CEP exposure. Student BMI also increases notably in the first and second years of CEP attendance before stabilizing around the third and fourth year of attendance. These results imply that CEP may cause BMI increases which in turn drive students into a new BMI steady state rather than continuing to increase body weight with further exposure.

An outstanding question is whether the effect of CEP school attendance on child weight differs across grade level. We test this relationship by estimating our BMI percentile regression with grade and CEP school attendance interaction effects. We provide a graphical illustration of these differential grade effects in Figure 7 which suggests that students attending a CEP school in 6^{th}, 7^{th}, and 8^{th} grade are driving the effects for BMI percentile we observe in the full sample. Rothbart et al. (2022) also finds no statistically significant effects for students in primary grades, though contrary to our findings, they find a decrease in obesity risk from CEP. This finding also matches our prior beliefs as FitnessGram testing in our sample is highest in middle schools. Therefore, the differential effect of CEP attendance on BMI percentile for middle school students may be due to differences in effective sample size. We do note, however, that elementary schools have the highest CEP adoption rates. If students attending a CEP middle school also attend a CEP elementary school, the larger effect we observe may be the result of continued greater CEP exposure rather than inherent differences across school type. Regardless, our grade-specific estimates do highlight potential heterogeneity in the CEP's effects on student weight across K-12 grades.

5. Sensitivity Analyses and Robustness Checks

We use student fixed effects in our main analyses to control for time-invariant characteristics which may otherwise bias the estimated effect of CEP school attendance on student BMI outcomes. In this section, we begin by addressing concerns caused by potential sources of time-variant selection. We discuss the results and reasoning for these tests in detail in Appendix B.

Figure B1 of Appendix B shows the results of a school-level event study excluding 7 schools that adopted CEP in 2015 and 2017, restricting our sample to 2016 CEP schools only.[23] We find that while the standard errors are larger in our school-level event study, there is no evidence of a statistically significant pre-trend in BMI percentile prior to 2016. To see if the effects we observe are driven by differential pre-trends in FRP meal participation, we conduct a school-level event study for the set of 2016 CEP schools where the outcome is share of FRL students. These results, included in Figure B2 of Appendix B, show some evidence of a statistically significant difference in FRL share in 2012, but we do not find a statistically

[23] We rely on school-level event studies rather than student-level event studies for two reasons. The first is that treatment is not constant after first CEP school exposure at the student-level. For example, a student can exit their first CEP school and then "switch" to the untreated group in the post-treatment period of the event study. The second reason is that the treatment is, for all practical purposes, at the school level. Therefore, we believe the main concern is not that students who would eventually attend a CEP school were fundamentally different in terms of weight leading up to the adoption of the CEP, but rather that CEP schools would be exhibiting some differential trends during the pre-CEP period, for example increasing poverty levels or changing meal types (leading to changes in student BMI correlated with CEP adoption). We demonstrate that students were not moving selectively into or out of CEP schools in response to program adoption below. It is difficult to assume that students who attend CEP schools would have different trends in the case that the schools themselves did not. This relationship would only occur in the case that students who would eventually enroll in a CEP school had differential trends which our analyses do not support.

significant pre-trend in other pre-CEP years closer to first adoption. Testing if our results are driven by CEP eligibility rather than CEP adoption, we test for parallel trends in BMI percentile using an event study with the set of CEP eligible non-participating schools serving as a quasi "placebo" group. This approach provides insights into questions related to school-level CEP sorting among eligible schools. Figure B3 of Appendix B shows results from this CEP eligible non-participant event study. We find no evidence of statistically significant differences in student BMI percentile pre-trends across CEP eligible non-adopters and CEP ineligible schools.

We also test the sensitivity of our primary results to various modeling assumptions and sample restrictions in Table 5. For the sake of brevity, we use BMI percentile as the outcome of interest and binary indicator of CEP school attendance as the independent variable of interest in Table 5. For comparison purposes, Column 1 of Table 5 includes our original BMI percentile estimate using binary CEP attendance shown in Column 1 of Panel A in Table 4.

As discussed, most CEP schools in our district adopted CEP in 2016 while only 2 and 5 schools adopted CEP in 2015 and 2017, respectively. If schools that first participated in 2015 and 2017 are fundamentally different from the set 2016 CEP schools based on some unobservable characteristic(s), these differences could bias our main results. To examine this threat to identification, we estimate our BMI percentile regression without the set of 2015 and 2017 CEP schools, leaving only 2016 CEP schools in our sample. The results from this 2016 CEP only regression are shown in Column 2 of Table 5. Like our primary results, we find that CEP school attendance has a positive and statistically significant impact on BMI percentile. The magnitude of the estimates from our main specification in Column 1 and our 2016 only specification in Column 2 are similar, indicating that our main results are driven by schools that adopted CEP in 2016 rather than the small number of 2015 and 2017 CEP schools.

For the primary results, we restrict our sample to include students who have at least one FitnessGram observation in the pre- and post-CEP periods. While we can compare the outcomes for these students across the two periods, restricting our sample to students with more years of FitnessGram participation may influence our estimates if those students are fundamentally different than students who participate in only two years. To test this restriction, we estimate BMI percentile results using the sub-sample of students with four or more FitnessGram observations split across the pre- and post-CEP periods. The estimates for this analysis are shown in Column 3 of Table 5. We find that restricting our sample to students with four or more years of FitnessGram participation produces similar estimates to our main results.

While the primary analysis exploits the longitudinal nature of our data set to remove potential sources of unobserved heterogeneity at the student-level, there are likely differences in the characteristics of students who ever and never attend a CEP school. To compare students that are likely to attend more similar schools, we estimate our primary results using the set of students who attend a CEP school for at least one year during the post-CEP period. Results for the sub-sample of students who *ever* attend a CEP school are shown in Column 4 of Table 5. While the estimate with this restriction is smaller in magnitude than our primary results at 1.85 percentage points, the effect remains economically and statistically significant after excluding students who never attend a school that eventually adopts the CEP. It is also important to note that while this restriction reduces our sample size by roughly 75 percent, our estimate remains robust in terms of its direction and statistical significance.

Our primary estimates of CEP school attendance may capture several sources of variation. One concern is that some students may transfer into a CEP school for reasons correlated with BMI, for example a negative household shock causing the family to move from a non-CEP school to a new CEP school. While most variation in CEP exposure comes from students who were already attending the school and those who move to a new school as the result of normal grade progression (e.g., moving from an elementary school to a middle school), in Column 5 we estimate our model after dropping students newly exposed to CEP if their school in period *t* is not the same as their school in period *t-1.* While this restriction decreases the magnitude of our coefficient, it is qualitatively similar to our main estimate. This finding makes intuitive sense as most new exposures were driven by students who experienced a change in CEP status while attending the same school. Nevertheless, Column 5 does suggest that changes in a student's CEP attendance caused by transferring to a new CEP school may cause differential BMI percentile effects. While the source of this difference is unclear, it does provide direction for future research.

An important feature of our data set is that students can take the FitnessGram multiple times each year, providing us with multiple observations of BMI for some students in the same year. Our primary results utilize the mean of BMI for each student in a year averaged across multiple observations for students with more than one test result. Because students may take the FitnessGram early or late in the school year, however, CEP exposure duration may vary across testing periods. It is possible that only certain tests included in a student's mean BMI are driving our estimates and using a different approach to measure BMI for students with multiple observations would produce different results. To test for this possibility, we use the first FitnessGram observation for each student in a given year to estimate our BMI percentile results. Column 6 shows our estimate is similar in magnitude to our primary specification in Column 1, implying that our results are robust to the use of first BMI observation rather than mean BMI.

We also evaluate the robustness of our estimates to the inclusion of school fixed effects and school time trends to account for any unobservable time-invariant school specific characteristics and changes in school characteristics over time that may otherwise bias our treatment effects. Columns 7 and 8 show that while the magnitude decreases to 1.9 percentile points, the estimate is generally robust in terms of statistical significance and magnitude.

Finally, we examine the effect of including each school's ISP and CEP eligibility status in our BMI percentile regression. Schools with ISPs at or above 40 percent are CEP eligible. The decision to participate is voluntary, however, implying that some CEP eligible schools choose not to adopt CEP. Since our primary specification defines CEP school attendance as attending a school that participates in the program during the post-CEP period, it is possible that the effects we observe are driven by the characteristics of CEP eligible and ineligible schools instead of actual participation. We test for this threat to identification in three ways. First, we show the results of our BMI percentile regression after restricting our sample to students attending either CEP participating or eligible schools. This approach removes schools that are never eligible for the CEP from our analysis, identifying the effect of CEP school attendance on BMI percentile using variation in CEP school adoption timing and participation across both participating and non-participating eligible schools. These results are shown in Column 9 of Table 5. The estimated effect of CEP school attendance on BMI percentile for the eligible sub-sample of schools differs little from the primary estimate shown in Column 1.

We also show the results from our BMI percentile regression for the sub-sample of students who ever attend a CEP school or a CEP eligible school in Column 10 of Table 5. This restriction limits our sample to students who attend similar CEP eligible or participating schools at some point, removing students who only ever attend a school that is never eligible for the CEP. For this sub-sample of students, we find CEP school attendance leads to a statistically significant increase in BMI percentile very close in magnitude to our primary specification. Second, in

Column 11 of Table 5, we add an additional indicator to our full-sample regression equal to 1 if a school's ISP is greater than 40% (implying they are CEP eligible) and 0 otherwise. Adding this new indicator lets us determine if the effect of CEP school attendance on BMI percentile in our main regression is driven by school ISP rather than program adoption. Column 11 shows that the estimated effect of our ISP>40% indicator on BMI percentile is small and statistically insignificant. Additionally, Column 11 suggests that the effect of CEP school attendance on BMI percentile remains unchanged in magnitude and level of statistical significance compared to our primary estimate. Taken together, Columns 10 and 11 indicate that the effect of CEP school attendance on BMI percentile in our main regression is driven by changes in a student's CEP exposure rather than their attendance of a CEP eligible school. One potential reason for the positive relationship between ISP and the likelihood of CEP participation observed in Figure 1 stems from administrative cost savings following CEP adoption. Specifically, schools with higher ISPs also have more students enrolled in FRP meals. Processing FRP meal applications is done by the school, increasing administrative costs. If schools with higher FRP enrollment pre-CEP expect larger administrative cost savings under the CEP, they may be more likely to participate in the program than eligible schools with lower FRP meal enrollment rates.

One potential threat to identification we cannot directly test for stems from time-varying shocks correlated with CEP school adoption timing. To the best of our knowledge, however, there were no other programs introduced at the same time as CEP in our district that would have differentially impacted CEP and non-CEP schools, reducing these concerns.

6. Conclusion

Millions of children in the U.S. receive subsidized breakfast and lunch in schools. Evidence regarding the impact of school meals on child BMI is mixed, however, with results differing across meal type, delivery method, and program. Moreover, many existing studies on free school meals face common limitations such as estimating effects of interest only for students of specific ages or grades. In this paper, we estimate the impact of UFSM provision through the CEP in a large Georgia K-12 school district in the metro-Atlanta area. Using student-level height and weight data from the FitnessGram and administrative student/school records, we estimate a student fixed effects model exploiting variation before and after CEP adoption. On average, we find attending a CEP school increases student BMI by 2.6 percentile points, equivalent to a weight increase of about 1.88 pounds for a student of average height. The increase in BMI we observe may represent a detrimental or beneficial change in student health depending on initial BMI level. Examining the CEP's effects on specific weight category likelihoods, we find that CEP school attendance increases a student's expected probability of being overweight by 3.3 percentage points. We do not, however, find evidence of a similar increase in the probability of obesity. Furthermore, we find some evidence of a small decrease in the likelihood that a student falls into the underweight range of BMI. We find no evidence of impacts from the CEP in our quasi-placebo estimation for student height, suggesting that the BMI effects we observe are driven by changes in student weight rather than height. We also find no evidence of differential FitnessGram selection at the school or student levels by school CEP participation status or eligibility. Our results are robust to a set of robustness checks and alternative sample restrictions, but there is some evidence for the presence of unobservable difference across CEP and non-CEP schools which could produce bias in our primary estimates. Regardless, the magnitudes of our estimates are largely consistent and entirely consistent in their direction.

Our findings provide important information for policymakers and researchers concerned with the effect of UFSM on student health. In summary, our results suggest that CEP participation leads to an increase in student BMI and many of its derived outcomes, highlighting the need for careful consideration of the content and delivery of UFSM. One specific policy conversation our results speak to relates to school meal program changes which occurred during the COVID-19 pandemic. School closures during the pandemic were concerning as students lost access to subsidized meals provided at school. To buffer against the detrimental effects of losing school meals during the pandemic and to help households with children meet their nutritional needs, the federal government implemented several temporary policies. For example, starting in Fall 2020, free school meals were offered to all students attending public K-12 schools in the U.S. regardless of household income. This change was in effect through the end of June 2022, but there remains increased pressure from the public for policymakers to consider a permanent switch to UFSM through proposed legislation like the Universal School Meals Program Act of 2021 introduced by Congress in May of 2021.[24] Independent of federal policy, California and Maine became the first U.S. states to permanently offer UFSM to all students in July of 2021.[25] If the results of our study hold for schools outside of our district and the South Eastern U.S., we may expect future adoption of UFSM programs to increase student BMI with the potential for an increase in the number of students with BMIs in the overweight range and a decrease in the number of students with BMIs in the underweight range.

[24] https://www.washingtonpost.com/business/2021/04/20/usda-extends-universal-free-lunch/; https://www.congress.gov/bill/117th-congress/house-bill/3115/text

[25] https://www.washingtonpost.com/opinions/2021/08/13/universal-free-school-meals-moms-california-maine/

There are, however, several limitations of our study that future research should address. We use data from one large school district in Georgia. The CEP's impact on child BMI may differ in other locations depending on the quality of meals offered and various student/family characteristics and behaviors. Additionally, we cannot observe if a student consumes school meals in our district, and instead estimate the CEP's total effects. A promising avenue for future research is identifying the mechanisms driving changes in student weight observed in the growing CEP literature. Understanding if effects are caused by changes in the quantity of food, quality of food, reductions in stigma, or a combination of multiple factors is important for designing further iterations of the CEP and future UFSM policies.

Our results relate to those of previous studies which find beneficial or null effects of CEP on student weight outcomes. A notable example is a recent paper by Rothbart et al. (2022) which finds some evidence to support a decrease in the likelihood of obesity among students in secondary grades in New York City (NYC) schools. While we find that the CEP increases the likelihood of overweight, the program's effect on obesity is statistically insignificant in our district. We find some evidence of a beneficial effect from CEP school attendance on student weight outcomes through its negative impact on underweight probability in our non-linear effect specification. We believe that the most likely cause of the differences between our findings and those of studies like Rothbart et al. (2022) are differences in the underlying characteristics of schools, students, and meals served. As mentioned above, we do not observe meal quality or meal consumption in our district, making it difficult to identify the mechanisms driving our estimates. If the nutrition characteristics of school meals in our sample differ substantially from those of other schools and districts, those differences may account for the detrimental effect of CEP on the probability of overweight we observe. Identifying the source of these differences is crucial to understanding why the program's weight effects may prove beneficial under certain circumstances and detrimental under others. Furthermore, the increase in BMI we find does not speak to the potential short and long run benefits of UFSM found in existing studies. Other research finds evidence of beneficial UFSM impacts on student achievement and discipline (Gordanier et al. 2020; Ruffini 2021; Gordon and Ruffini 2021). Additionally, Lundborg et al. (2022) find that access to UFSM in Sweden, in the long run, lead to a 3 percent increase in lifetime incomes for students who received free meals during their entire primary school education. While UFSM have only recently become available to students across the U.S., evidence from other countries that have had similar programs for longer may provide much needed insights into the expected long run effects of UFSM on weight and non-weight related outcomes like income, education, and food security.

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Tables and Figures

Table 1: School level Summary Statistics by CEP and FitnessGram Participation Status

	Never FG (1)	Ever FG CEP schools (2)	Ever FG Non-CEP schools (3)	Difference (4)
Share of students who took FG in each year	-	0.34	0.39	-0.05
		(0.42)	(0.42)	(0.39)
Share of students ever participating in FG	0.36	0.73	0.80	-0.07
	(0.44)	(0.42)	(0.36)	(0.68)
Share of ever receiving free lunch	0.59	0.91	0.37	0.54***
	(0.37)	(0.28)	(0.38)	(9.05)
Share of students ever receiving reduced-price lunch	0.06	0.03	0.03	0.00
	(0.19)	(0.16)	(0.17)	(0.00)
Female	0.53	0.49	0.49	0.00
	(0.46)	(0.50)	(0.50)	(0.016)
White	0.22	0.16	0.55	-0.39***
	(0.35)	(0.28)	(0.41)	(5.33)
Black	0.74	0.84	0.34	0.50***
	(0.32)	(0.27)	(0.36)	(7.49)
Hispanic	0.06	0.15	0.15	0.006
	(0.23)	(0.27)	(0.31)	(0.08)
Other Race	0.07	0.01	0.15	-0.14***
	(0.16)	(0.10)	(0.29)	(3.43)
Elementary Schools	0.29	0.74	0.59	0.15***
	(0.12)	(0.00)	(0.01)	(121.19)
Middle Schools	0.41	0.15	0.22	-0.07***
	(0.15)	(0.00)	(0.01)	(44.04)
High Schools	0.30	0.11	0.20	-0.09***
	(0.03)	(0.00)	(0.00)	(108.7)
No. of students	394.9	835.61	1052.41	-216.81***
	(45.6)	(56.38)	(62.23)	(16.6)
BMI Z-score	-	0.76	0.47	0.28
		(1.09)	(1.05)	(1.16)
BMI Percentile	-	70.54	63.35	7.19
		(27.17)	(28.12)	(1.16)
Normal Weight	-	0.55	0.64	-0.09
		(0.50)	(0.48)	(0.80)

Overweight	-	0.23	0.20	0.03
		(0.42)	(0.40)	(0.36)
Obese	-	0.16	0.10	0.07
		(0.37)	(0.29)	(0.83)
Height (in)	-	55.82	58.00	-2.13**
		(4.53)	(4.16)	(2.13)
Weight (lbs)	-	93.02	98.31	-5.29
		(29.00)	(27.01)	(0.82)
No. of Schools	13	27	73	

Observations are at the school level, with student averages pooled over the full sample period (2012-2018). All CEP schools reported FG at some point during the sample period. Column 4 shows the difference between Columns 2 and 3, highlighting the difference and level of statistical significance of means across CEP and non-CEP schools in our sample. Standard Deviation in parentheses for Columns 1-3. T statistics in parentheses for Column 4 with level of statistical significance (* 0.10; ** 0.05; *** 0.01).

Table 2: School Level FitnessGram Participation Regression Results

	Any FG			Share taking FG			Share taking FG if FG>0		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
#Students/100	0.029***	0.029***	0.029***	0.006	0.006	0.006	-0.007	-0.007	-0.009
	(0.010)	(0.010)	(0.010)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
% FRL x 10	0.080***	0.079***	0.079***	0.01	0.012	0.012	-0.04	-0.034	-0.034
	(0.025)	(0.026)	(0.026)	(0.023)	(0.024)	(0.024)	(0.027)	(0.028)	(0.029)
% Female x 10	0.065	0.064	0.067	0.023	0.024	0.027	0.005	0.009	0.011
	(0.102)	(0.103)	(0.103)	(0.077)	(0.078)	(0.079)	(0.069)	(0.071)	(0.071)
% Black x 10	-0.078***	-0.077***	-0.079***	0.016	0.017	0.019	0.031	0.026	0.025
	(0.024)	(0.025)	(0.025)	(0.023)	(0.024)	(0.024)	(0.026)	(0.027)	(0.028)
% Hispanic x 10	-0.096***	-0.095***	-0.097***	-0.015	-0.017	-0.019	0.041	0.035	0.034
	(0.033)	(0.033)	(0.034)	(0.029)	(0.031)	(0.031)	(0.031)	(0.034)	(0.035)
% Other race x 10	-0.02	-0.019	-0.02	0.001	0.001	0.001	0.007	0.007	0.007
	(0.019)	(0.011)	(0.018)	(0.02)	(0.02)	(0.02)	(0.018)	(0.018)	(0.018)
CEP_{st}		0.011	0.032		-0.009	-0.012		-0.03	-0.017
		(0.065)	(0.068)		(0.044)	(0.047)		(0.05)	(0.051)
CEP eligible			0.047			0.046			0.031
			(0.052)			(0.039)			(0.04)
Mean Outcome	0.72	0.72		0.35	0.35		0.49	0.49	
R^2	0.33	0.33		0.38	0.38		0.44	0.44	
Obs. (schools x year)	728	728		728	728		524	524	

Notes: The outcome in Columns 1 and 2 is a binary indicator for whether the school administered the FitnessGram. The outcome in Columns 3-6 is the share of students in a school who participated in the FitnessGram. Student/100 is the number of students (in hundreds) enrolled in the school in year *t* , %FRL is the proportion of students in the school who were ever eligible for FRL in pre-CEP years (prior to 2015), and %Gender is the proportion of female students in the school. %Black, % Hispanic and % Other race measure the proportion of Black, Hispanic and Other race students in the school.[26] CEP indicates whether the school participated in the CEP in a given year. All estimations have indicators for year fixed effects and for the minimum and maximum grade offered in the school. The sample is all schools in the district for years 2012-2018. Observations are at the school-year level. All estimations control for fixed effects for year, the minimum grade in school and the maximum grade in school. Standard errors clustered by schools in parentheses (* 0.10; ** 0.05; *** 0.01)

[26] The following groups are included in Race: Black, Hispanic, Indian, Asian, Pacific Islander. White is the omitted group. Due to small sample sizes, we group Indian, Asian and Pacific Islanders into an “other race” category.

Table 3: Student Level FitnessGram Participation Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)
CEP	-0.016	0.004	-0.031	-0.009		0.007
	(0.037)	(0.039)	(0.039)	(0.037)		(0.051)
Student Cumulative CEP					-0.013	
					(0.020)	
Elementary	0.092**	0.092**				
	(0.031)	(0.032)				
Middle	0.384***	0.384***				
	(0.037)	(0.037)				
Ever FRL		-0.006	0.009*			
		(0.015)	(0.005)			
Female		-0.044**	-0.044***			
		(0.022)	(0.009)			
Black		-0.044*	0.001			
		(0.022)	(0.006)			
Hispanic		-0.008	0.003			
		(0.019)	(0.004)			
Other Race		-0.029	-0.040***			
		(0.025)	(0.007)			
Ever FRL x CEP						-0.041*
						(0.021)
Female x CEP						-0.016*
						(0.009)
Black x CEP						0.038
						(0.041)
Student FE				X	X	X
School FE			X			
Grade FE x Year FE			X	X	X	X
Year FE	X	X				
Obs. (student x year)	643,281	643,281	643,281	643,281	643,281	643,281

Notes: The outcome of Columns 1, 2, and 3 is whether a student *i* in year *t* had FitnessGram results. CEP indicates whether the student attended a school that participated in the CEP. Student Cumulative CEP is the cumulative number of years a student attends a CEP school. Ever FRL eligible in an indicator for whether the student was ever eligible for FRL in pre-CEP years (i.e., prior to 2015). Female, Black, Hispanic and Other race are indicators for whether the student was female, Black, Hispanic or of Other race, respectively. Columns 1 and 2 control for year fixed effects. Columns 3 and 4 control for school fixed effects and grade by year fixed effects. Column 5 and 6 control for individual student fixed effects in addition to grade by year fixed effects. The sample is all students with any FitnessGram observations in years 2012-2018. Standard errors in parentheses are clustered at the school level (* 0.10; ** 0.05; *** 0.01).

Table 4: Effect of CEP exposure on Student BMI Percentile, BMI Z-Score, Weight, Height and the Probability of Overweight/Obese

	(1) BMI (%)	(2) BMI (Z)	(3) Weight (lbs)	(4) log(Weight)	(5) log(Height)	(6) Overweight/ Obese	(7) Overweight	(8) Obese	(9) Under-weight
				Panel A: Binary Indicator for CEP Attendance					
CEP	2.647***	0.085***	1.881**	0.010	-0.004	0.033***	0.032***	0.001	-0.01
	(0.887)	(0.031)	(0.850)	(0.007)	(0.002)	(0.011)	(0.01)	(0.008)	(0.006)
				Panel B: Cumulative Years of CEP School Attendance					
Yrs. of CEP Exposure	0.971**	0.032**	0.968**	0.004	-0.002*	0.016***	0.012***	0.003	-0.003
	(0.418)	(0.016)	(0.396)	(0.003)	(0.001)	(0.006)	(0.004)	(0.004)	(0.003)
				Panel C : Non-linear Effect Estimation					
1 year of CEP	2.034**	0.065**	1.520**	0.009	-0.003	0.030**	0.028*	0.003	-0.008*
	(0.878)	(0.030)	(0.666)	(0.006)	(0.002)	(0.013)	(0.014)	(0.007)	(0.005)
2 years of CEP	2.908**	0.096**	2.041*	0.012	-0.004	0.032**	0.032***	0.001	-0.017**
	(1.111)	(0.041)	(1.169)	(0.009)	(0.003)	(0.015)	(0.011)	(0.012)	(0.007)
3 or 4 years of CEP	2.799**	0.097*	2.723**	0.012	-0.007*	0.052**	0.036***	0.016	-0.004
	(1.325)	(0.051)	(1.099)	(0.009)	(0.004)	(0.021)	(0.013)	(0.013)	(0.008)
Observations	188,233	188,233	188,233	188,233	188,233	188,233	188,233	188,233	188,233

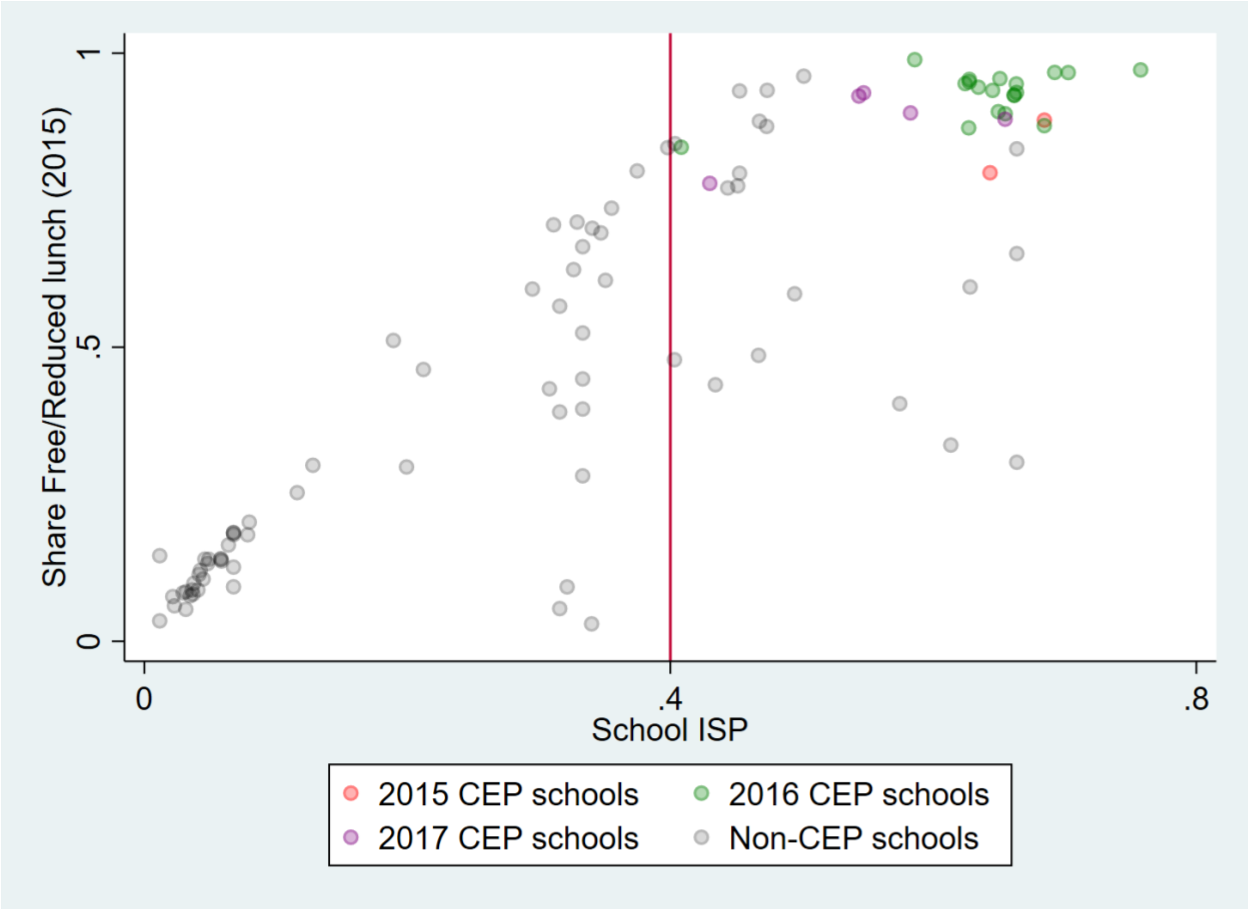
Note: Outcomes are BMI percentile in the national reference distribution, BMI Z-score in the national reference distribution, Weight in pounds, natural log of Weight, natural log of Height and indicators for being overweight/obese, overweight, obese, and underweight for Columns 1-9. Changes in the log of weight and the log of height are used to approximate percent changes in weight and height, respectively. CEP is a binary indicator for school participating in the CEP. Yrs of CEP Exposure is the number of years the student has attended a CEP school. 1 year of CEP, 2 years of CEP and 3 or 4 years of CEP are binary indicators for 1, 2 and 3 or 4 years of attending a CEP participating school, respectively. All estimations control for grade-by-year fixed effects, student fixed effects, student age, an indicator for entering a new school since last observation and the number of years since the last observation. Race, gender, and our measure of having ever received FRL are time-invariant and therefore differenced out with the inclusion of our student fixed effect. Standard errors in parentheses are clustered on individuals and schools. (* 0.10; ** 0.05; *** 0.01).

Table 5: Robustness Checks: Effect of CEP Attendance on Student BMI Percentile for Various Student Sub-Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Original Model	Excluding 2015 & 2017 CEP Adopters	4+ FG obs.	Students who ever attended a CEP school	Excluded if CEP is a new school	Using only the first BMI observation	School F.E Added	School Time Trend Added	Only CEP & ISP Schools	Ever CEP or ISP >40% school	CEP Eligibility as a covariate
CEP	2.647***	2.557**	2.394**	1.848**	1.896**	2.489***	1.913**	1.924**	2.411***	2.766***	2.676***
	(0.887)	(1.022)	(1.051)	(0.843)	(0.915)	(0.831)	(0.816)	(0.815)	(0.863)	(0.83)	(0.963)
ISP>40%											0.088
											(0.649)
Observations	188,233	179,463	80,894	46,306	183,810	188,233	188,235	188,235	69,499	90,084	188,233

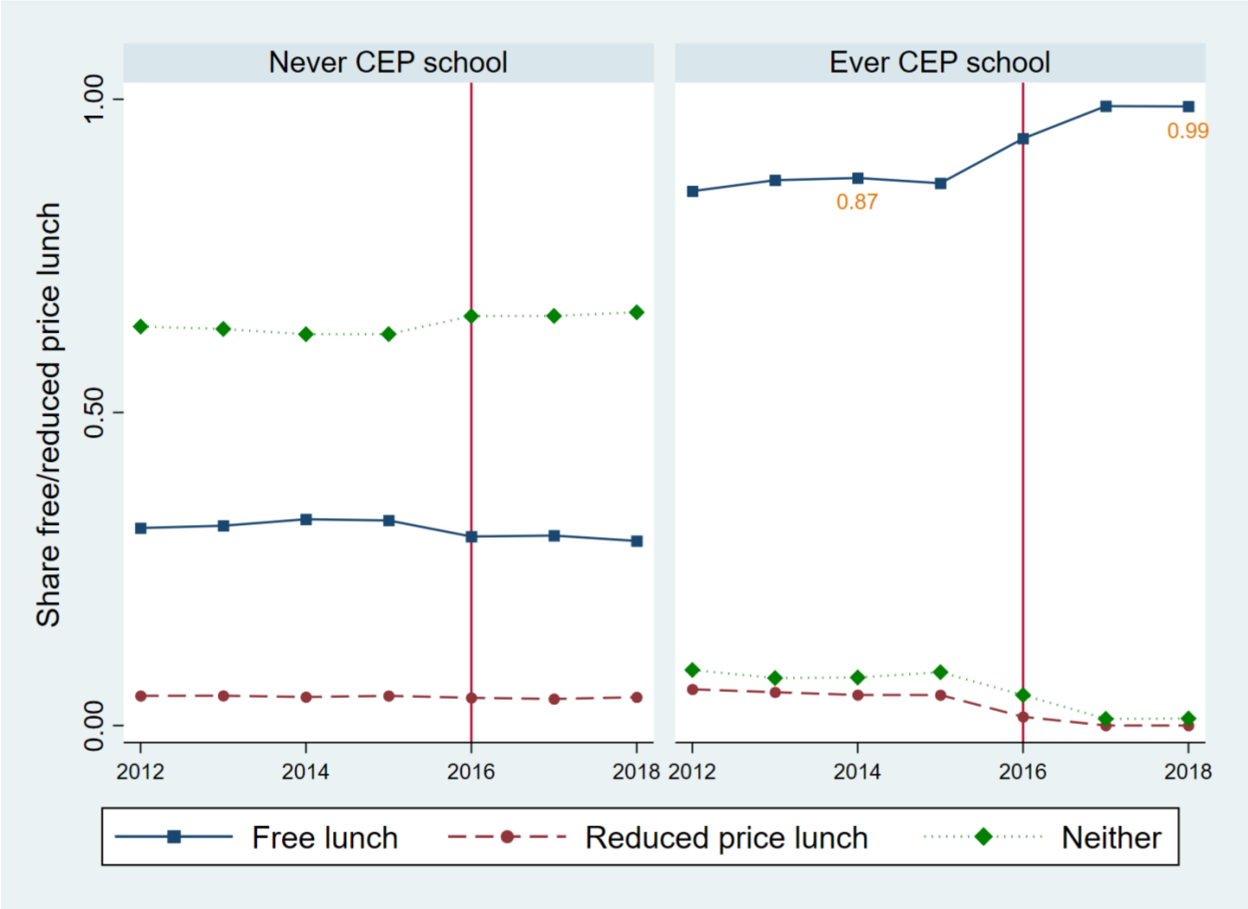
Note: Outcome is BMI-Z percentile. Columns are samples in each regression. Column 1 is full sample of students from Table 4. Columns 2 excludes students schools that adopted the CEP in 2015 or 2017. Columns 3 only uses set of students who ever attend an ever CEP school. Column 4 excludes students who join a school in the first year of the school's CEP participation. Column 5 restricts the estimation to the first FitnessGram observation for each student in a year. Column 6 restricts the sample to students with four or more FitnessGram observations. Column 7 controls for school fixed effects. Column 8 controls for a school time trend. CEP is a binary indicator for school's participation in the CEP. Sample in Column 9 is restricted to ever CEP schools. Sample in Column 10 is restricted to ever CEP schools and schools with ISP above 40% that did not participate in the CEP. Sample in Column 11 is the full analytical sample and the estimation includes an indicator for schools with ISP>40%. All estimations control for grade-by-year fixed effects, student fixed effects, an indicator for entering a new school since last observation and the number of years since the last observation. Race, gender, and our measure of having ever received FRL before 2015 are time-invariant and therefore differenced out with the inclusion of our student fixed effect. Standard errors in parentheses are clustered at the individual and school level. (* 0.10; ** 0.05; *** 0.01).

Figure 1: Free and Reduced-Price Eligibility Share by School Identified Student Percentage



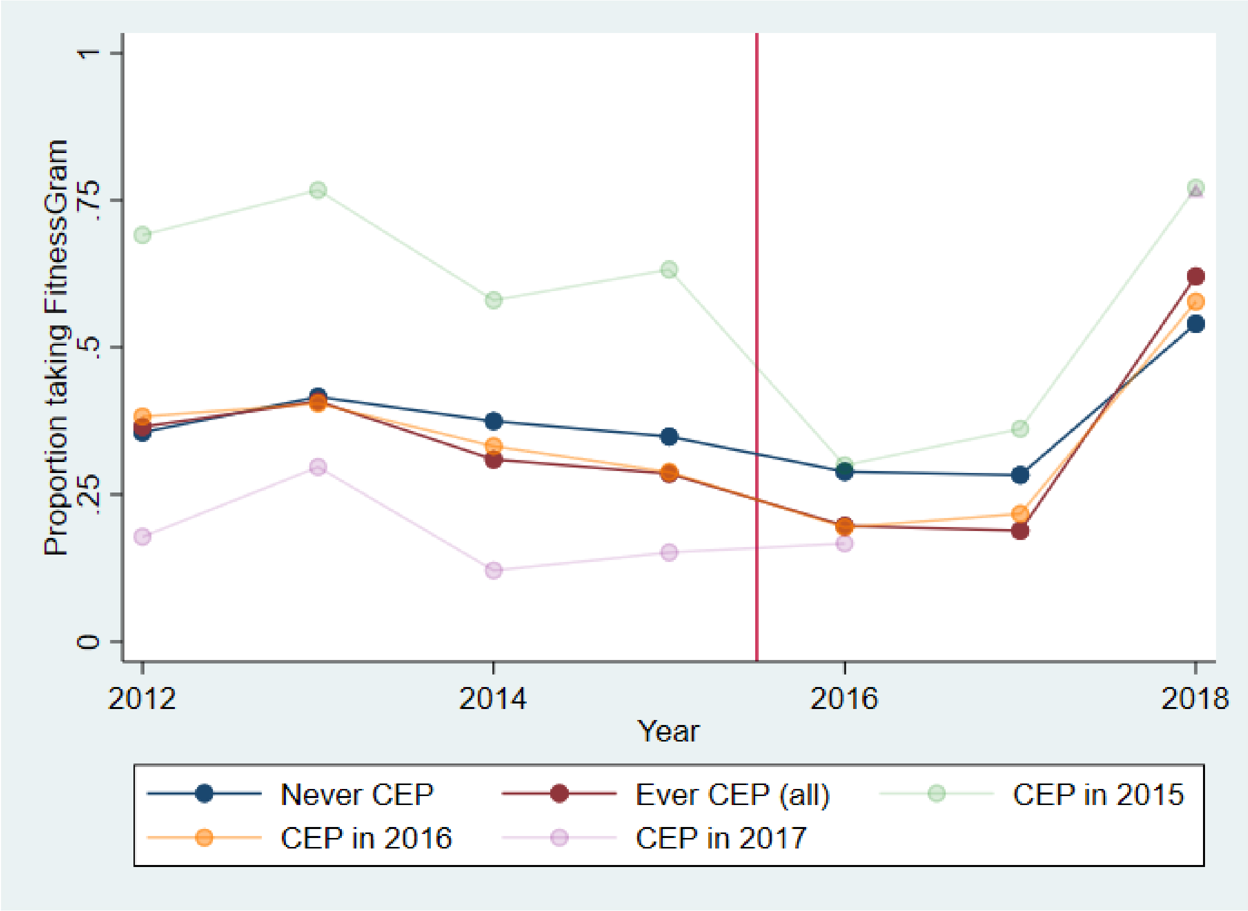
Notes: Sample is all schools in the study's district. Figure plots first year of CEP implementation by Identified Student Percentage (ISP) and share free/reduced lunch in 2015. The line at 0.4 indicates the school eligibility threshold for CEP where schools with ISP≥0.4 are eligible for the program.

Figure 2: Time Trends in Free and Reduced-Price Lunch Participation Share by CEP Participation Status



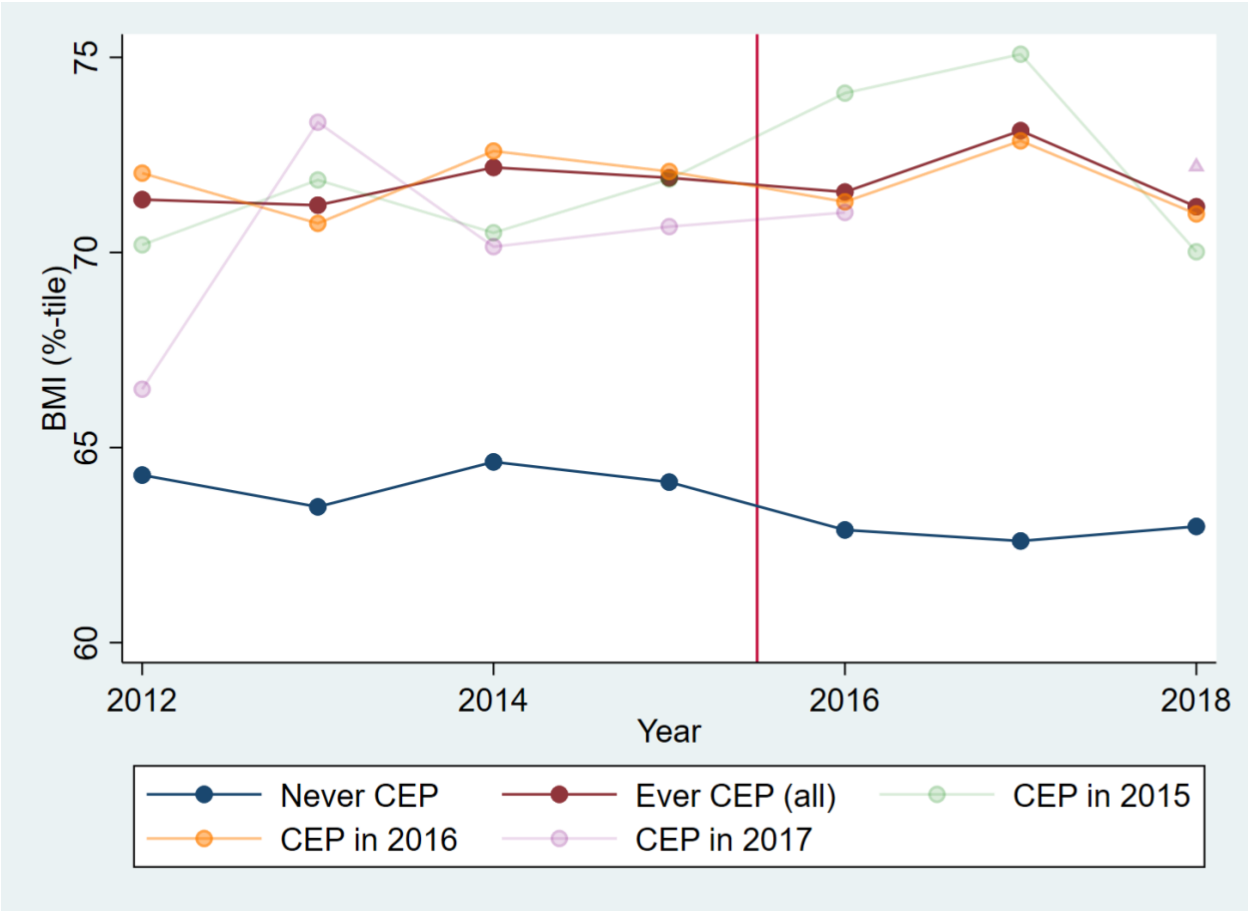
Notes: Figure plots share of students participating in free or reduced-price lunch in ever (right) and never (left) CEP adopting schools. Indication at 2016 indicates the year in which most schools in this district enrolled in CEP.

Figure 3: Time Trend in the Share of Schools' Students Taking the FitnessGram by CEP Participation Status



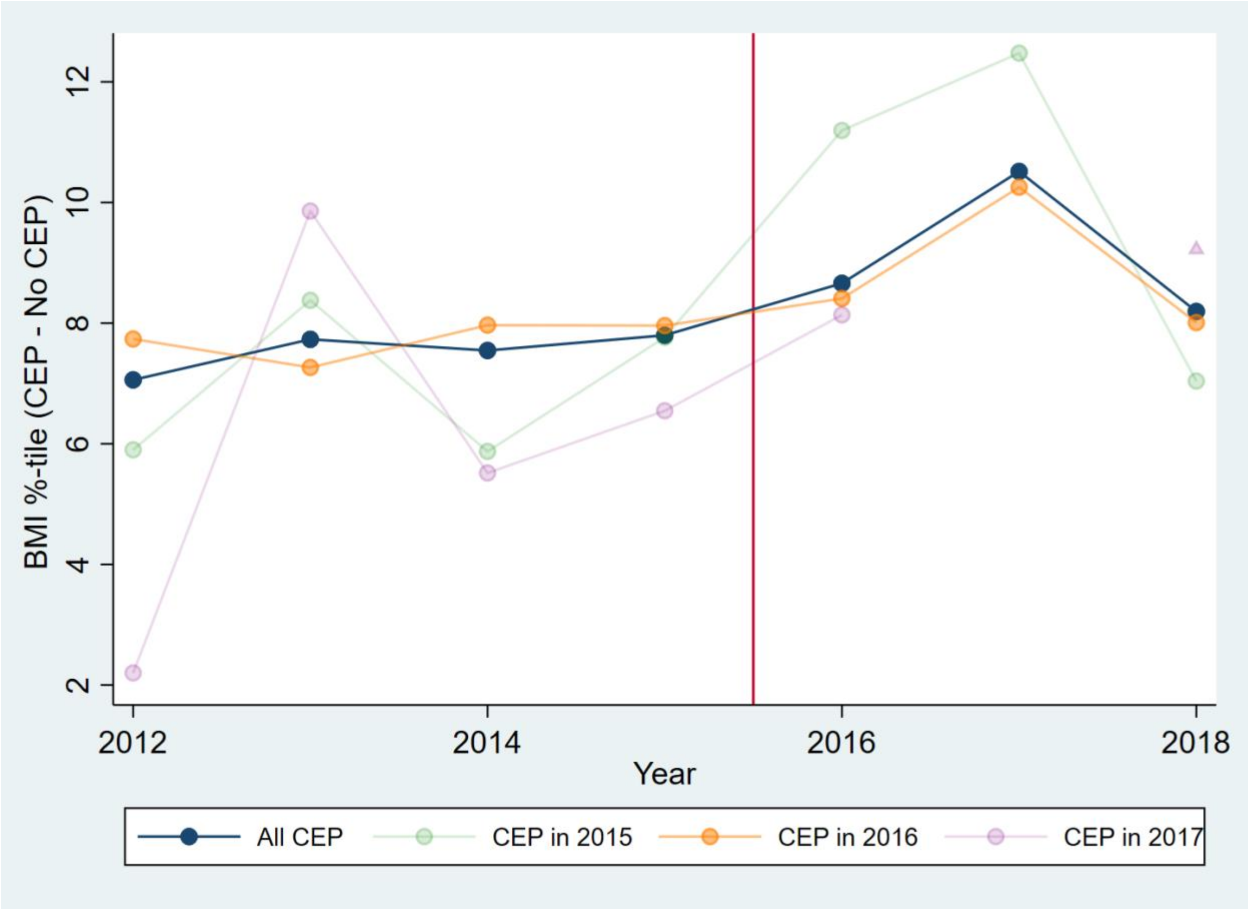
Notes: Figure plots the share of students taking FitnessGram over time by school CEP status and year the school first participated in the CEP for the set of ever-CEP schools.

Figure 4: Time Trend of Student BMI Percentile by CEP Participation Status



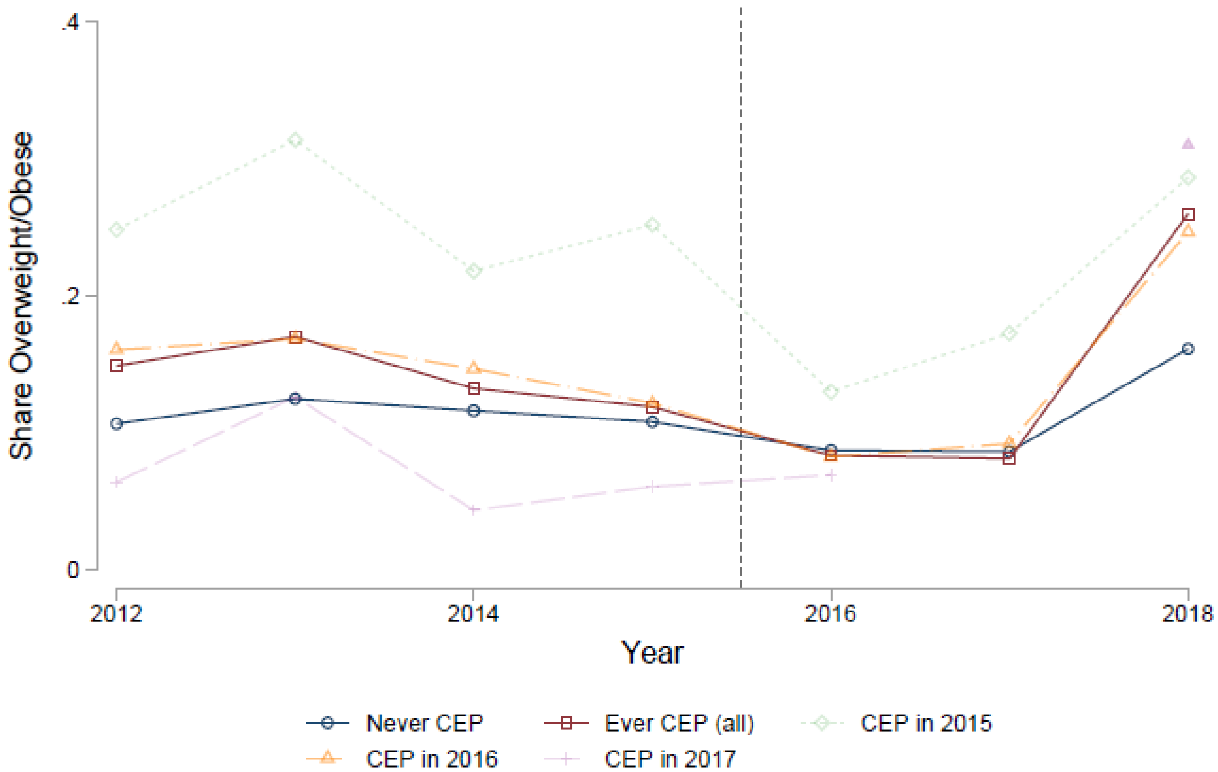
Notes: Figure plots mean BMI percentile over time for non-CEP schools (never CEP) and CEP adopting schools (ever CEP) by year the school first participated in the CEP.

Figure 5: Time Trend in the Average BMI Percentile Point Difference of Ever CEP Schools Relative to Never CEP Schools



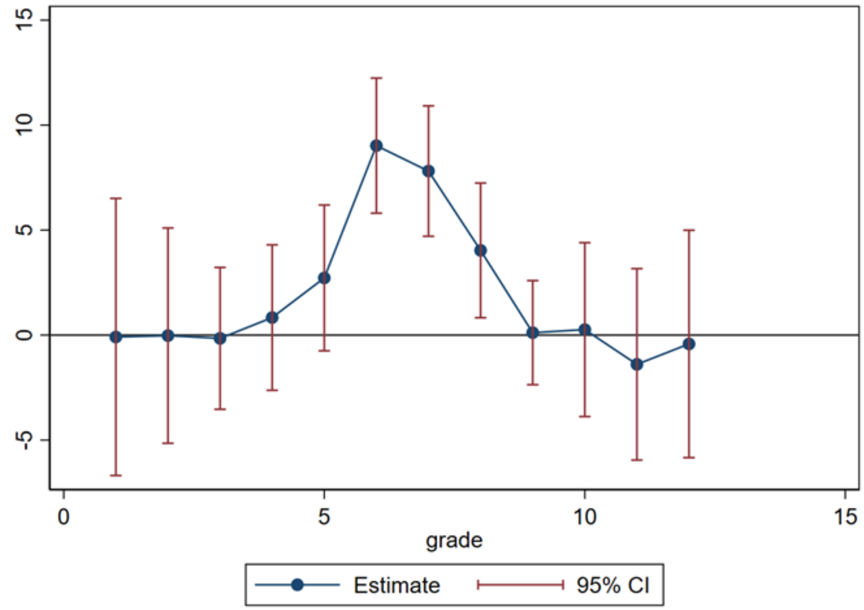
Notes: Figure plots mean difference in BMI percentile for CEP participating schools and non-CEP participating schools, by year the school first participated in the CEP.

Figure 6: Time Trend of Share of Overweight/Obese Students by CEP Participation Status



Notes: Figure plots mean difference in share of students overweight or obese in CEP schools and schools that never adopt CEP.

Figure 7: Heterogeneous Effects of CEP School Attendance on Student BMI Percentile by Grade



Notes: Estimate shows the event study estimates for grade level interacted with CEP school attendance.

Appendix A: Additional Tables and Figures

Table A1: Student-Level Summary Statistics by CEP Participation Status

	Not in Regression Sample		Regression Sample	
	Never FG	**1 FG Obs.**	**>1 FG, Never CEP**	**>1, Ever CEP**
# Fg Obs.	-	1.00	3.03	2.89
		(0.00)	(1.21)	(1.13)
Ever CEP	0.15	0.20	0.00	1.00
	(0.36)	(0.40)	(0.00)	(0.00)
Female	0.52	0.49	0.46	0.49
	(0.50)	(0.50)	(0.50)	(0.50)
Black	0.59	0.51	0.36	0.81
	(0.49)	(0.50)	(0.48)	(0.39)
White	0.33	0.41	0.54	0.20
	(0.47)	(0.49)	(0.50)	(0.40)
Hispanic	0.11	0.13	0.14	0.19
	(0.30)	(0.34)	(0.34)	(0.39)
Other	0.11	0.12	0.13	0.01
	(0.32)	(0.32)	(0.34)	(0.10)
Ever Eligible for Free Lunch	(0.50)	0.50	0.38	0.94
	(0.50)	(0.50)	(0.49)	(0.24)
Ever Eligible for RP Lunch	0.04	0.04	0.03	0.02
	(0.20)	(0.19)	(0.18)	(0.15)
Years observed	1.98	3.05	5.43	5.46
	(1.40)	(1.71)	(1.65)	(1.48)
Years CEP exposure	0.20	0.38	0.00	2.34
	(0.54)	(0.85)	(0.00)	(0.88)
BMI Z	-	0.54	0.49	0.82
		(1.08)	(0.98)	(0.94)
BMI %	-	65.1	64.04	72.03
		(28.5)	(26.4)	(23.3)
Underweight	-	0.06	0.06	0.04
		(0.23)	(0.19)	(0.14)
Normal weight	-	0.62	0.63	0.54
		(0.48)	(0.41)	(0.41)
Over weight	-	0.21	0.21	0.25
		(0.40)	(0.33)	(0.32)
Obese	-	0.11	0.10	0.17
		(0.32)	(0.26)	(0.33)
Height	-	58.3	60.43	57.3
		(7.70)	(5.94)	(6.17)
Weight	-	102.8	109.3	101.5
		(44.3)	(37.1)	(37.4)
Obs (students)	78,511	66,417	52,491	10,029

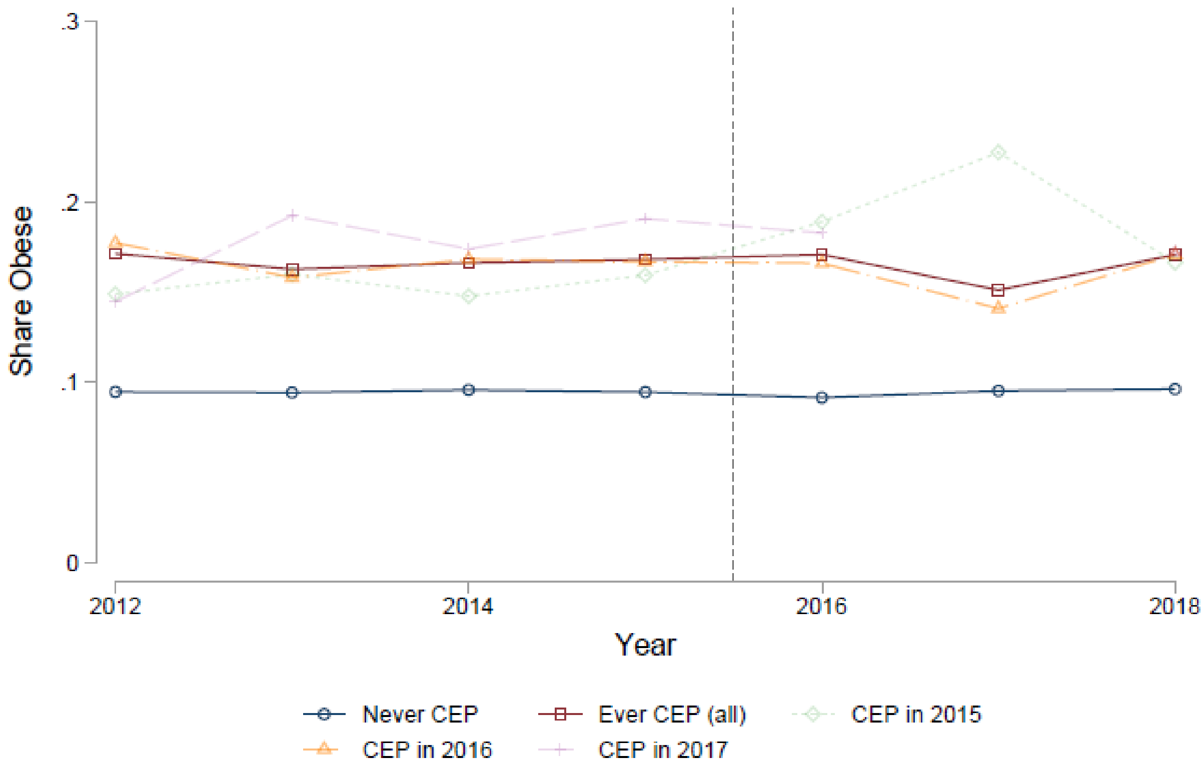
Data are at the student level (one observation for each student in the sample). Weight measurements are averaged over all student observations. FitnessGram measures are student averages. Race and ethnicity categories are not mutually exclusive. “RP” refers to “Reduced Price”.

Table A2: Effect of CEP Participation on Free and Reduced-Price Lunch Participation

	Free or Reduced-Price		Free	Reduced
	(1)	(2)	(3)	(4)
CEP	0.074***	0.072***	0.111***	-0.039***
	(0.011)	(0.011)	(0.014)	(0.004)
FG regression sample		x	x	x
All observations	x			
N	209,939	188,233	188,233	188,233

Sample in Column 1 includes all observations. Columns 2-4 use the FitnessGram regression sample. CEP is a binary indicator for school's CEP participation. All estimations control for age, an indicator for entering a new school since last observation, the number of years since last observation, year by grade fixed effects and student fixed effects. Standard errors are clustered on individuals and schools.

Figure A1: Time Trend of Share of Obese Students by CEP Participation Status



Notes: Figure plots mean difference in share of students obese in CEP schools and schools that never adopt CEP.

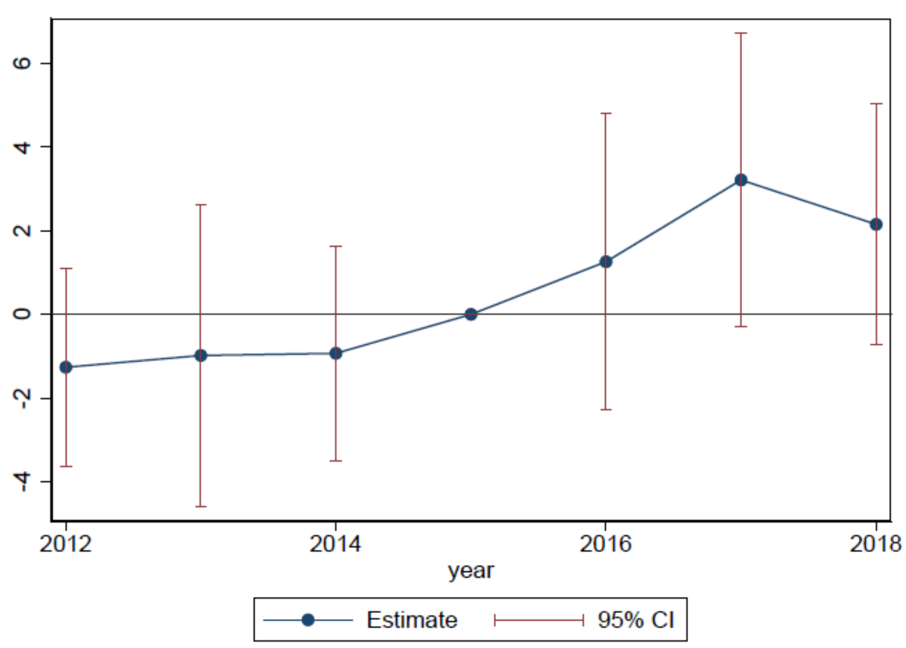
Appendix B: Robustness Checks

First, we establish parallel trends in the pre-CEP period. For sake of brevity, we focus on BMI percentile as our outcome of interest in all sensitivity analyses and robustness checks. Our estimate of δ from equation (3) would be biased if the BMI of students in CEP adopting schools was always increasing (or decreasing) prior to the adoption of CEP. To test this, we collapse our data to the school-year-level, dropping the 7 schools that adopted CEP in 2015 and 2017, relying instead on data from the majority of ever-CEP schools that adopted the program in 2016. This restriction provides a clean pre-trend which we evaluate in an event study framework. Figure 8 shows the event study estimates for schools' average BMI percentile weighted by the number of students who took the FitnessGram attending at each school in each year. While the standard errors of our estimates in Figure B1 are expectedly larger than those of our student-level regressions because we are examining the relationship at the school-level, the results show no evidence of a statistically significant pre-trend in BMI percentile for the set of 2016 CEP schools. Additionally, Figure 8 shows a notable increase in expected BMI percentile for CEP schools during the post-CEP period, but the coefficients are statistically insignificant, potentially the result of our reduced sample size.

We conduct a similar analysis for the share of each schools' students that participate in free, reduced-price, and free or reduced-price lunch separately, for the subsamples of 2016 CEP adopting schools and never CEP schools. In this case, we are concerned about differences in factors like rising or declining poverty rates in ever-CEP and never-CEP schools which may bias our estimates of CEP school attendance on BMI if those factors create differential trends in free and/or reduced-price lunch participation. For example, if we see a significant change in free lunch participation in ever-CEP schools prior to 2016, our main estimates may be capturing pre-existing changes in the share of students participating in free lunch rather than changes to participation caused by the program itself. Figure B2 shows the results of this lunch participation event study for the set of never-CEP schools and ever-CEP schools that adopted CEP in 2016. For the set of never-CEP schools in Figure B2, we find no notable differences in the share of students participating in free, reduced price, or either lunch type in the pre- or post-CEP periods. For the set of ever-CEP schools that adopted CEP in 2016, however, we see some evidence of a statistically significant difference in lunch participation in 2012, but the magnitude of these effects are quite small and not present in other pre-CEP period years. Following adoption of CEP in 2016, our results suggest that ever-CEP schools experienced substantial and statistically significant increases in the share of students receiving free school lunch, a significant decrease in the share of students enrolled in reduced price lunch, and a significant increase in the share of students receiving either free or reduced-price lunch. These findings match our expectations as all students are automatically switched to free school meals following adoption of the CEP. Taken together, the results of Figure B2 suggest that our primary results are likely not driven by differential trends in free and/or reduced-price lunch participation across ever- and never-CEP schools. Additionally, while the share of students participating in reduced-price lunch decreases in ever-CEP schools during the post-CEP period, the increase in free school lunch participation is large enough to raise the expected share of students who are enrolled in either free or reduced-price lunch.

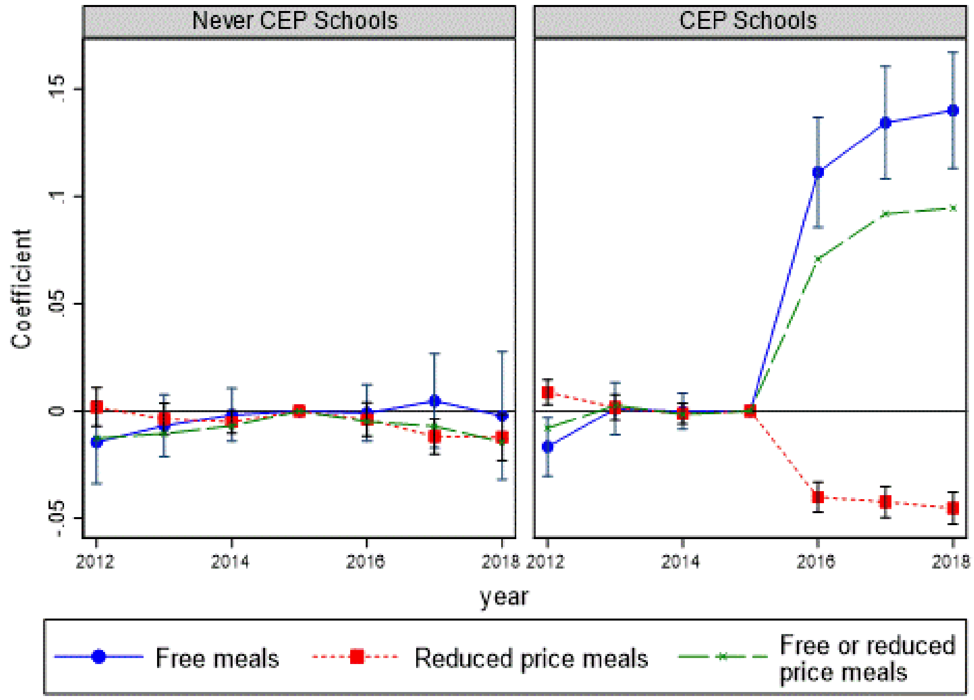
Next, we extend our test of the parallel trends assumption by separately considering CEP eligible schools with ISPs greater than or equal to 40 percent that did not choose to enroll in the CEP as a "placebo" comparison group along with the BMI percentile event study shown in Figure 8. While the set of CEP eligible non-participating schools is not a true placebo group, this approach provides insights into questions related to school-level sorting into the CEP. Since schools with ISPs below 40 percent are not CEP eligible, the only sorting into the program that can take place at the school level is among the set of CEP eligible schools. Figure B3 shows separate BMI percentile event study results for schools that adopted CEP in 2016 (the same results as those shown in Figure B1) and CEP eligible schools that did not adopt CEP. The results of Figure 10 suggest that the parallel trends assumption holds at the school-level for both groups regarding their mean BMI percentile. Furthermore, during the post-CEP period non-participating CEP eligible schools maintain a similar trend to the set of non-eligible schools. There is also no statistically significant difference in trends of BMI percentile during the post-CEP period for the set of eligible non-adopters as expected. These results support the validity of our parallel trends assumption among the set of CEP participating and CEP eligible non-participating schools which are included in our main regression. We also find no statistically significant differences in BMI percentile among the eligible non-adopting schools in the post period, implying that our primary results are not driven by spurious changes at the school level. We examine the impact of including the set of eligible CEP non-adopting schools in our analysis more in the following robustness checks.

Figure B1: School-Level Event Study Estimates for BMI Percentile



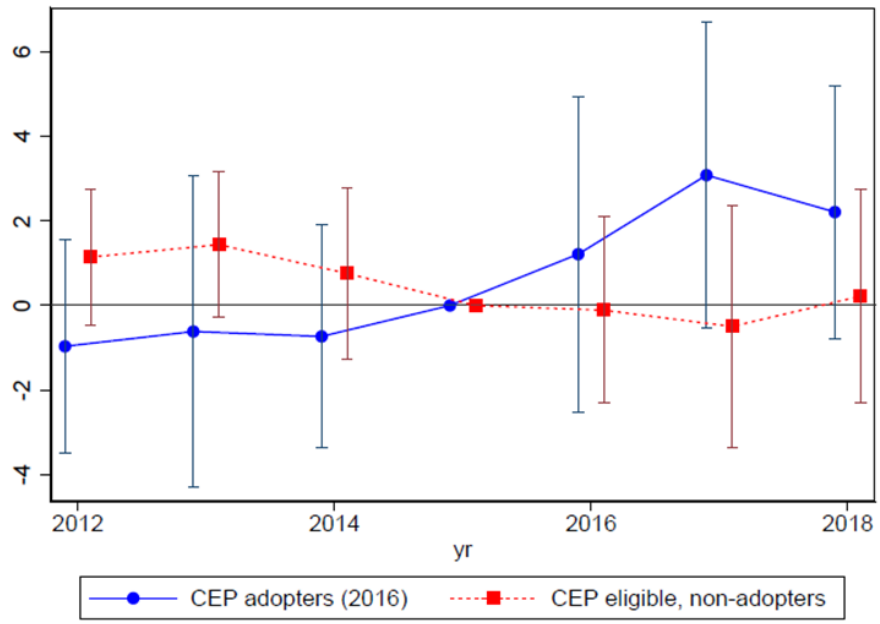
Notes: This figure shows results from an event study of BMI percentile regressed on the timing of school CEP adoption. The sample is restricted to only the set of schools that adopted CEP in 2016, omitting the 5 schools that adopted CEP in 2015 and 2017. The vertical red lines show 95 percent confidence interval bounds around each of the estimated coefficients shown by the blue dots. The regression is weighted by student population attending each school in each year.

Figure B2: School-Level Event Study Estimates for Share of Students Participating in Free and/or Reduced-Price Lunch



Notes: Figure plots event study estimates for share of students enrolled in free or reduced-price lunch in ever (right) and never (left) CEP adopting schools.

Figure B3: School-level Event Study Estimates for BMI Percentile for Schools that Adopted CEP in 2016 and CEP Eligible Non-Adopters



Notes: This figure shows the results from an event study of BMI percentile on the timing of school CEP adoption for two groups of schools. The first group, shown on the blue line, includes the set of schools that adopted CEP in 2016. The second group, shown on the red dotted line, is for a placebo group of schools that were eligible for the CEP but did not adopt the program in 2015, 2016, or 2017. The CEP adopter group is restricted to only the set of schools that adopted CEP in 2016, omitting the 5 schools that adopted CEP in 2015 and 2017. The vertical blue and red lines show the 95 percent confidence interval bounds of each model coefficient for the set of CEP adopting schools and the set of CEP eligible non-adopters, respectively. Regressions are weighted by student population attending each school in each year.

List of Acronyms

BMI	Body Mass Index
CEP	Community Eligibility Provision
FRL	Free and Reduced-Price Lunch
HHFKA	Healthy Hunger Free Kids Act
ISP	Identified Student Percentage
MAPLE	Metro-Atlanta Policy Lab for Education
NSLP	National School Lunch Program
PE	Physical Education
SBP	School Breakfast Program
SFP	School Feeding Programs
UFSM	Universal Free School Meals