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ABSTRACT

In light of the recent rise in childhood obesity, the School Breakfast Program (SBP) and National School Lunch Program (NSLP) have received renewed attention. Using panel data on over 13,500 primary school students, we assess the relationship between SBP and NSLP participation and (relatively) long-run measures of child weight. After documenting a positive association between SBP participation and child weight, and no association between NSLP participation and child weight, we present evidence indicating positive selection into the SBP. Allowing for even modest positive selection is sufficient to alter the results, indicating that the SBP is a valuable tool in the current battle against childhood obesity, whereas the NSLP exacerbates the current epidemic.

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1 Introduction

As is quite evident from recent media reports, childhood obesity is deemed to have reached epidemic status. Data from the National Health and Nutrition Examination Survey (NHANES) I (1971–1974) and NHANES 2003–2004 indicate that the prevalence of overweight preschool-aged children, aged 2-5 years, increased from 5.0% to 13.9% over this time period.[1] Among school-aged children, the prevalence has risen from 4.0% to 18.8% for those aged 6-11; 6.1% to 17.4% for those aged 12-19 years.[2]

Given this backdrop, policymakers in the US have acted in a number of different directions, particularly within schools. Public Law 108-265 requires schools to have a local wellness program by the beginning of the 2006-2007 school year, which must address both nutritional and physical activity goals. The CDC started the KidsWalk-to-School Program to encourage communities to partner with parents and local public safety officials to enable students to safely walk or bicycle to school in groups accompanied by adults. Some schools have banned soda machines and vending machines containing unhealthy snacks, while others have taken aggressive measures to ensure the provision of nutritious meals.[3] Texas reinstated a physical education requirement, which had been previously removed in favor of more academic pursuits (Schanzenbach 2007). In November 2007, the US Department of Health and Human Services (HHS) launched the Childhood Overweight and Obesity Prevention Initiative.

Aside from these recent policy developments, two federal programs that have long been in existence have been met with renewed interest: the School Breakfast Program (SBP) and the National School Lunch Program (NSLP). Given the number of children affected and *potentially* affected by these programs, combined with the fact that the infrastructure for these programs is already in existence, it is the relationship between the SBP, NSLP, and child weight that we analyze here. Specifically, we have three main objectives. First, assess the relatively long-run relationship between participation in school nutrition programs and child weight using data collected *after* the most recent, large-scale reforms of the programs. Second, analyze the process by which children select into the SBP and NSLP. Finally, assess the impact of such selection on our ability to infer a causal relationship.

Understanding the relationship between school nutrition programs and child weight is clearly important. As the incidence of overweight children has increased, so too has our understanding of the negative consequences that result. First and foremost, overweight children are significantly more likely to become obese adults. Serdula et al. (1993) find that one-third of overweight preschool-aged children and one-half of

[1] Overweight is defined as an age- and gender-specific body mass index (BMI) greater than the 95^{th} percentile based on growth charts from the Center for Disease Control (CDC).

[2] See http://www.cdc.gov/nccdphp/dnpa/obesity/childhood/prevalence.htm.

[3] Andersen and Butcher (2006) find that the elasticity of BMI with respect to junk food exposure in schools is roughly 0.1.

overweight school-aged children become obese adults. Second, the adverse health effects of obesity include, among others, depression, sleep disorders, asthma, cardiovascular and pulmonary complications, and type II diabetes (Ebbling et al. 2002). In terms of economic costs, Finkelstein et al. (2003) report that medical spending attributed to obesity was close to $80 billion, or 9% of total medical expenditures, in the US in 1998. Bleich et al. (2007) provide a recent overview of the costs and consequences of adult obesity.

In this study, we utilize panel data on over 13,500 children during early primary school to examine the relatively *long-run* effect of participation in *both* the SBP and NSLP. Specifically, we analyze the relationship between child weight in the spring of third grade and program participation in spring of kindergarten. Analyzing the long-run impact allows us to capture dynamic effects of program participation such as nutritional habit formation and resource reallocation within households. In addition, after assessing the nature of selection into both programs, we examine the sensitivity of the estimated program effects to non-random selection, borrowing several methods from the program evaluation literature.

Our results are striking, yielding three salient findings. First, while SBP participation in kindergarten is *associated* with greater child weight in third grade and a greater change in child weight between kindergarten and third grade for many children, NSLP participation and child weight are *unrelated*. However, we find strong evidence of non-random selection into the SBP; children who gained more weight prior to kindergarten are more likely to participate. Consonant with Schanzenbach (2007), selection bias does not seem to be a concern when analyzing the NSLP. Finally, in nearly all cases, the positive associations between SBP participation and child weight are found to be extremely sensitive to non-random selection; even a *modest* amount of positive selection is sufficient to eliminate, if not reverse, the initial results for SBP. Moreover, allowing for modest positive selection into the SBP leads to a *detrimental* effect of NSLP participation on child weight; ignoring non-random selection into SBP biases the impact of the NSLP toward zero. Thus, admitting even modest positive selection into the SBP implies that the SBP is a *valuable* tool in the current battle against childhood obesity, whereas the NSLP *exacerbates* the current epidemic. The beneficial effect of the SBP, and the deleterious impact of the NSLP, strengthen the findings in Bhattacharya et al. (2006) and Schanzenbach (2007), respectively.

The remainder of the paper is organized as follows. Section 2 provides background information, both on the school nutrition programs themselves, as well as the previous literature. Section 3 presents a simple theoretical framework for thinking about school nutrition programs. Section 4 describes the empirical methodology and the data. Section 5 presents the results, while Section 6 concludes.

2 Background

2.1 Institutional Details

The NSLP was developed gradually, and made permanent by the National School Lunch Act in 1946. The program provides lunch to over 29 million children each school day, covering approximately 99,000 schools (95% of all public and private schools), with 17.5 million students receiving reduced price or free meals.[4] The SBP was established in 1966 by the Child Nutrition Act, and made permanent in 1975. During the 2005-2006 school year, the SBP provided breakfast to roughly 9.6 million children in 82,000 schools, with 7.7 million children receiving reduced price or free breakfasts (Cooper and Levin 2006).

As evidenced by these figures, the SBP is under-utilized relative to the NSLP. Roughly 83% of schools participating in the NSLP also participated in the SBP during the 2005-2006 school year, and roughly 45 students qualifying for free or reduced price meals participated in the SBP for every 100 students participating in the NSLP (Cooper and Levin 2006). That said, SBP participation is on the rise, having increased in all but three states from the 2004-2005 school year to the 2005-2006 school year.

The SBP and NSLP are similarly organized. Both programs are federally funded. Each is overseen by the Food and Nutrition Service (FNS) of the US Department of Agriculture (USDA), but administered by state education agencies. Schools deciding to participate in the programs must offer meals that meet federal nutritional requirements. In addition, students residing in households with family incomes at or below 130% of the federal poverty line are eligible for free meals, while those in households with family incomes between 130% and 185% of the federal poverty line are entitled to reduced price meals.[5] Eligible children apply directly to the school, with the same application covering both the SBP and NSLP. In addition, children from households that receive aid through food stamps, Temporary Assistance for Needy Families, or the Food Distribution Program on Indian Reservations are automatically eligible for free meals. All other students pay full price, though meals are subsidized by the federal government to a limited extent.

Schools establish their own prices for full price meals, but prices for reduced price meals are capped. Schools have flexibility with respect to the specific foods served, but are constrained by the fact they must operate their meal services as non-profit programs. In the 2005 fiscal year, the NSLP cost the federal government roughly $7 billion, while federal expenditures on the SBP in fiscal year 2006 totalled $2 billion.[6]

As stated above, reimbursement is conditional on the meals meeting federal nutritional requirements, established by Congress in 1995 under the "School Meals Initiative for Healthy Children" (SMI). SMI

[4] See http://www.fns.usda.gov/cnd/lunch/AboutLunch/NSLPFactSheet.pdf.

[5] For the period July 1, 2007, through June 30, 2008, 130% (185%) of the federal poverty line for a family of four is $26,845 ($38,203). The maximum price allowed for breakfast (lunch) to students qualifying for reduced price is $0.30 ($0.40).

[6] See http://www.frac.org/pdf/cnnslp.PDF and http://www.frac.org/pdf/cnsbp.PDF.

represented the largest reform of the programs since their inception (Lutz et al. 1999). For breakfast, this entails no more than 30% of the meal's calories be derived from fat, and less than 10% from saturated fat. Breakfasts also must provide one-fourth of the Recommended Dietary Allowance (RDA) for protein, calcium, iron, Vitamin A, Vitamin C, and contain an age-appropriate level of calories. For lunches, the same restrictions on fat apply. However, lunches must provide one-third of the RDA for protein, calcium, iron, Vitamin A, Vitamin C, and an age-appropriate level of calories calories. In addition, all meals are recommended to reduce levels of sodium and cholesterol, as well as increase the level of dietary fiber.

Enforcement of the SMI requirements is handled by requiring states to monitor local school food authorities through reviews conducted at least once every five years. In turn, the FNS monitors state compliance with this review requirement. The FNS has also begun to provide regional and local training to ensure adequate overview.

2.2 Literature Review

Given the size and cost of these programs, each has been studied to some extent over the decades. In the early 1990s, a series of studies were conducted utilizing the 1992 School Nutrition Dietary Assessment (SNDA-1) study. As part of the study, a random sample of school meals was analyzed, in addition to the diets of children. Gleason (1995) finds that SBP availability is not associated with a higher probability of eating breakfast. Moreover, the author finds that lunches provided under the NSLP derived an average of 38% of food energy from fat, exceeding guidelines. Burghardt et al. (1995) report that meals provided under the NSLP exceeded guidelines for total and saturated fat and sodium, whereas meals provided under the SBP exceeded guidelines for saturated fat and cholesterol. Gordon et al. (1995) use 24-hour dietary recall data and conclude that both SBP and NSLP participation are associated with higher intake of fat and saturated fat, but also some nutrients.

The results of the analyses using the SNDA-1 led to the 1995 SMI discussed above. While the SMI required schools to follow the nutrition guidelines by the 1996-1997 school year, some schools received a waiver until the 1998-1999 school year (Lutz et al. 1999). A second study, the SNDA-2, was conducted in 1998-1999. The evidence suggests some effect of the SMI on the nutritional content of meals, but school lunches in particular still have much room for improvement (Schanzenbach 2007).[7]

Since the SNDA-1 study, more recent analyses have focused greater attention on identifying the *causal* impact of SBP or NSLP participation on child health. Gleason and Suitor (2003) use two nonconsecutive days of 24-hour dietary recall data to obtain fixed effects estimates of NSLP participation. The authors find that NSLP participation increases intake of nutrients, but also increases intake of dietary fat. Hofferth

[7] See also http://www.iom.edu/Object.File/Master/31/064/Jay%20Hirschman.IOM%20Presentation.Oct%2026%202005.pdf.

and Curtin (2005) use data from the 1997 Child Development Supplement of the Panel Study for Income Dynamics (PSID) and find no effect of SBP participation on the probability of being overweight after controlling for NSLP participation. In addition, instrumental variables estimates – using public school attendance as the exclusion restriction – indicate no impact of NSLP participation. Bhattacharya et al. (2006) analyze the effects of SBP availability in the school on nutritional intake using NHANES III, which spans 1988-1994 (thus pre-dating the SMI). The authors employ a difference-in-differences strategy (comparing in-school versus out-of-school periods in schools participating and not participating in the SBP), concluding that SBP availability "has no effect on the total number of calories consumed or on the probability that a child eats breakfast, but it improves the nutritional quality of the diet substantially" (p. 447). Schanzenbach (2007) utilizes panel data methods, as well as a regression discontinuity (RD) approach that exploits the sharp income cut-off for eligibility for reduced-price meals, to assess the impact of the NSLP. Using data from the Early Childhood Longitudinal Study-Kindergarten Class of 1998-99 (ECLS-K), she finds that NSLP participation increases the probability of being obese due to the additional calories provided by school lunches. However, she finds little substantive difference between the RD estimates and those based on a panel data approach, suggesting little selection into the NSLP on the basis of unobservables that vary over time and across schools.

Finally, a few studies offer less direct evidence of the possible effects of the SBP and NSLP. For instance, Long (1991) assesses the crowding-out impact of SBP and NSLP benefits on total household food expenditures. The author finds that one dollar of NSLP (SBP) benefits displaces only $0.60 (none) of household food expenditures. Thus, both programs increase the *total value* of food consumed by the household. von Hippel et al. (2007) show that children are more at-risk of gaining weight during summer vacation than during the school-year. While this is potentially attributable to children's propensity to consume more food while at home, it could also be explained by the lack of access to school meal programs during the summer for non-summer school attendees.

We add to this literature in four important ways. First, we assess the *long-run* relationship between participation in *both* the SBP and NSLP program and children's weight. Second, we use data after the reforms enacted under the SMI should have been fully implemented. Most prior research (to our knowledge) assesses the contemporaneous relationship between SBP and/or NSLP participation and children's weight, typically focuses on only one of the programs (not both), and uses data from before the changes instituted under the SMI have been fully implemented. Third, we assess the nature of selection into both programs using data on birthweight and weight at the time of entry into kindergarten. Finally, we examine the sensitivity of the estimated program effects to non-random selection.

3 Theoretical Motivation

To provide some context for the empirical analysis, it is useful to think about the intrahousehold resource allocation effects of the SBP and NSLP. Figure 1 illustrates a very simple model. Households maximize utility, $U(c, f)$, where c is non-food consumption and f is food consumption subject to a standard budget constraint (as well as an implicit biological constraint restricting food consumption from falling below some threshold). In the figure, the solid budget constraint represents the initial budget constraint without school-provided nutrition programs. The corresponding optimal consumption bundle is labelled as point A. The dashed budget constraint incorporates the SBP and NSLP assuming children in the household receive an infra-marginal transfer of food for free at school. Thus, the programs lead to a kinked budget constraint, where the kink point lies to the left of point A given the assumption of an infra-marginal transfer (whereby the size of the transfer is less than food consumption without the program).

With an infra-marginal transfer, it is well known that the impact of the transfer is equivalent to pure income transfer. Since the transfer has only an income effect, the household will respond by increasing consumption of both c and f if both are normal goods. Point B illustrates this possible outcome. However, if the income elasticity of food consumption is zero, then the household may instead move to point C, in which case the household utilizes the savings from the transfer program purely to finance an increase in non-food consumption. This latter possibility is consistent with the findings in Bhattacharya et al. (2006) with respect to the SBP (as participation does not alter total caloric intake), but the former is consonant with the earlier findings in Long (1991) for both programs and Schanzenbach (2007) with respect to the NSLP.[8]

This model, while quite simple, illustrates two key points. First, participation in the SBP and NSLP may or may not increase food consumption. In the event that food consumption does increase, any health benefits of the SBP and NSLP require the nutritional gains from the food provided under these programs to more than compensate for the increase in overall food consumption if child health is to be improved. Second, participation in the SBP and NSLP provides an income benefit to households that allows households to increase their non-food consumption. Such an increase in consumption has theoretically ambiguous health consequences. For example, the effects on child's health would differ if the household uses the additional resources to buy a video game machine or to fund an extracurricular activity. Thus, in the end, the role of the SBP and NSLP in the childhood obesity epidemic – positive or negative – is an empirical question.

[8] Note, the distinction between the results in Bhattacharya et al. (2006) and Schanzenbach (2007) should not be interpreted as conflicting results across the two studies as the former (latter) focuses on the SBP (NSLP).

4 Empirics

4.1 Methodology

To assess the impact of school nutrition programs on child health, we utilize several estimators. To contrast the estimators in terms of the identification assumptions required, we utilize the potential outcomes framework often adopted in the program evaluation literature. However, here, we are simultaneously considering two treatments: SBP and NSLP participation.

To begin, let y_{1i} and y_{2i} denote child health if the child participates in the SBP only (denoted as $\widetilde{D}_{1i} = 1$) and NSLP only (denoted as $\widetilde{D}_{2i} = 1$), respectively. Let y_{3i} denote child health if the child participates in both programs (given by $\widetilde{D}_{3i} = 1$), and y_{0i} denote child health in the absence of either treatment (corresponding to $\widetilde{D}_{1i} = \widetilde{D}_{2i} = \widetilde{D}_{3i} = 0$). In this set-up, the effect of participating in the SBP only relative to the control of no participation in either program on the health of child i is given by $\tau_{1i} \equiv y_{1i} - y_{0i}$. Similarly, $\tau_{2i} \equiv y_{2i} - y_{0i}$ and $\tau_{3i} \equiv y_{3i} - y_{0i}$ measure the effect on the health of child i of participating in the NSLP only and of participating in both programs, respectively, relative to the control of no participation in either program. However, given the usual missing counterfactual problem, only $y_i = \widetilde{D}_{1i}y_{1i} + \widetilde{D}_{2i}y_{2i} + \widetilde{D}_{3i}y_{3i} + (1-\widetilde{D}_{1i})(1-\widetilde{D}_{2i})(1-\widetilde{D}_{3i})y_{0i}$ is observable.

To proceed, we specify a structural relationship for each potential outcome. Define

$$\begin{aligned} y_{0i} &= \mu_0(x_i) + u_{0i} \\ y_{1i} &= \mu_1(x_i) + u_{1i} \\ y_{2i} &= \mu_2(x_i) + u_{2i} \\ y_{3i} &= \mu_3(x_i) + u_{3i} \end{aligned} \tag{1}$$

where $\mathrm{E}[y_d|x_i] = \mu_d(x_i)$, $d = 0,1,2,3$, and x_i is a vector of observable attributes of child i (including an intercept). u_d captures the impact of unobservable attributes on child health when $D = d$, $d = 0,1,2,3$.

Assuming $\mu_d(x_i) = x_i\beta_d$, $d = 0,1,2,3$, and $\beta_0 = \beta_1 = \beta_2 = \beta_3$ except for the intercept terms, then one obtains the following regression model

$$y_i = x_i\beta_0 + \tau_1\widetilde{D}_{1i} + \tau_2\widetilde{D}_{2i} + \tau_3\widetilde{D}_{3i} + \left[u_{0i} + \widetilde{D}_{1i}(u_{1i}-u_{0i}) + \widetilde{D}_{2i}(u_{2i}-u_{0i}) + \widetilde{D}_{3i}(u_{3i}-u_{0i})\right] \tag{2}$$

where τ_d, $d = 1,2,3$, is the constant treatment effect. Furthermore, if one assumes that the participation in both programs is additive, such that $\tau_3 = \tau_1 + \tau_2$ and $u_{3i} = u_{2i} + u_{1i} - u_{0i}$ for all i, then (2) becomes

$$y_i = x_i\beta_0 + \tau_1 D_{1i} + \tau_2 D_{2i} + [u_{0i} + D_{1i}(u_{1i}-u_{0i}) + D_{2i}(u_{2i}-u_{0i})] \tag{3}$$

where $D_{1i} = 1$ for all SBP participants (zero otherwise) and $D_{2i} = 1$ for all NSLP participants (zero otherwise).[9] In other words, $D_{1i} = 1$ if $\widetilde{D}_{1i} = 1$ or $\widetilde{D}_{3i} = 1$, and $D_{2i} = 1$ if $\widetilde{D}_{2i} = 1$ or $\widetilde{D}_{3i} = 1$.

For OLS estimation of (3) to yield a consistent estimate of τ_1 and τ_2, participation in the SBP and NSLP must be independent, conditional on x, of unobservables that impact child health without participating, u_0, and unobserved, child-specific gains from participation in either program, $u_1 - u_0$ and $u_2 - u_0$.

In contrast, a consistent estimate of τ_1 and τ_2 may be obtained under two alternative sets of assumptions given the nature of the data (discussed below). First, given data prior to exposure to the SBP and NSLP, one may express child health in the pre-treatment period, $t-1$, and post-treatment period, t, by

$$y_{it} = x_i\beta_{0t} + \tau_1 D_{1it} + \tau_2 D_{2it} + [u_{0it} + D_{1it}(u_{1it} - u_{0it}) + D_{2it}(u_{2it} - u_{0it})] \quad (4)$$

$$y_{i,t-1} = x_i\beta_{0,t-1} + u_{0i,t-1} \quad (5)$$

where the child attributes, x, are time invariant, but the effects of these attributes are allowed to vary over time. First-differencing yields the following estimating equation

$$\Delta y_i = x_i\Delta\beta_0 + \tau_1 D_{1i} + \tau_2 D_{2i} + [\Delta u_{0i} + D_{1i}(u_{1it} - u_{0it}) + D_{2i}(u_{2it} - u_{0it})]\,. \quad (6)$$

where Δ indicates the change from the pre-treatment period. OLS estimation of (6) yields a consistent estimate of τ_1 and τ_2 if participation in the SBP or NSLP, conditional on x, is uncorrelated with *changes* over time in unobservables impacting child health under no participation in either program, Δu_0, in addition to the previous identification assumptions.

Second, given data on multiple students from the same school, (3) and (6) may each be augmented to include school fixed effects, following the strategy employed in Schanzenbach (2007). The benefit of including school fixed effects is that it accounts for potential non-random selection into schools based on the availability of school nutrition programs. This yields a consistent estimate of τ_1 and τ_2 if participation in the SBP or NSLP is uncorrelated with child-level deviations in u or Δu, as well as unobserved, child-specific gains from participation, from their respective school-level averages (conditional on deviations of x from its school-level average).

In addition to the preceding parametric estimators, we also estimate the average treatment effect (ATE) of each program using propensity score matching (PSM). Now quite commonplace in economics and other disciplines, it is well known that PSM estimation yields two potential benefits over regression methods

[9] This turns out to be a necessary restriction as there is not sufficient variation in the data to seperately identify the effect of SBP participation in isolation, τ_1, from SBP participation in conjunction with NSLP participation, τ_3. Thus, in our empirical analysis, we are predominantly identifying the impact of SBP participation using variation in outcomes from children participating in both programs relative to children participating only in the NSLP.

(Smith and Todd 2005). First, it is a semi-parametric estimator in that one does not need to specify a functional form for potential outcomes; $\mu_d(x_i)$ is left unspecified for all d. Second, issues of common support are explicitly addressed. Specifically, we estimate the ATE using only those observations for which the estimated propensity score (i.e., the probability of receiving the treatment given x) lies in the intersection of the supports for the treatment and control groups. In contrast, regression estimators – based on the entire sample – may extrapolate across observations with very different observable attributes. Aside from these two issues, PSM estimation relies on the same identification assumptions as detailed above.[10]

Finally, because all of the preceding estimators are susceptible to bias from selection on (at least some type of) unobservables, we borrow various strategies from the program evaluation literature to assess the sensitivity of our results to any remaining selection on unobservables. Specifically, for the parametric models, we apply the procedures developed in Altonji et al. (2005). For the PSM models, we assess the sensitivity of the results using Rosenbaum bounds (Rosenbaum 2002). We discuss these below.

4.2 Data

The data are obtained from the Early Childhood Longitudinal Study-Kindergarten Class of 1998-99 (ECLS-K). Collected by the US Department of Education, the ECLS-K follows a nationally representative cohort of children throughout the US from fall and spring kindergarten, fall and spring first grade, and spring third grade. The sample includes 17,565 children from 994 schools.

We measure participation in school nutrition programs at the earliest possible date, which is in spring kindergarten.[11] However, we measure the health status of each child either in spring third grade or as the change from fall kindergarten to spring third grade. Not only does the nature of the timing improve the likelihood that the assumptions required for consistent estimation are met, but it also implies that we are analyzing more of the long-run relationship between child health and participation in the two programs. The long-run impact may differ in magnitude from the contemporaneous effect due to the development of nutritional habits, leading to a cumulative effect. Alternatively, reallocation of resources within households in response to any change in child health that may result from program participation or due to the income effect of program participation may alter the direction and magnitude of the impact.[12]

[10] To implement the PSM estimator, we use kernel weighting with the Epanechnikov kernel and fixed bandwidth of 0.10. Standard errors are obtained using 100 repetitions. We perform the analysis twice, once using SBP participation as the treatment (i.e., D_1) and once using NSLP participation as the treatment (i.e., D_2).

[11] The relevant questions were not asked in the fall kindergarten wave.

[12] The long-run effect we seek to estimate also reflects, at least to some extent, the short-run impact as well; the correlation coefficients for program participation in spring kindergarten and spring third grade are 0.51 and 0.29 for the SBP and NSLP, respectively.

To measure child health, we utilize data on the age (in months) and gender of each child, as well as data on the weight and height of each child. The data allow us to construct seven measures of child health: body mass index (BMI) in levels or logs, growth rate in BMI from fall kindergarten to spring third grade, BMI percentile, change in BMI percentile from fall kindergarten to spring third grade, and indicators for overweight and obesity status, where percentiles are determined based on age- and gender-specific growth charts.[13] For the sake of expositional convenience, we define overweight (obese) as a BMI above the 85^{th} (95^{th}) percentile.

To control for parental and environmental factors, the following covariates are included in x: child's race (white, black, Hispanic, Asian, and other) and gender, child's birthweight, household income, mother's employment status, mother's education, number of children's books at home, mother's age at first birth, an indicator if the child's mother received WIC benefits during pregnancy, region, city type (urban, suburban, or rural), and the amount of food in the household. Finally, we also include higher order and interaction terms involving the continuous variables, as well as fall kindergarten measures of child health.[14]

Given the nature of our data, children with missing data for gender and race are dropped from our sample. Missing values for the remaining control variables are imputed and imputation dummies are added to the control set. However, particular care was needed to clean the data on child age, height, and weight. In terms of age, children with missing values in all waves are dropped, while missing ages in particular waves are imputed assuming all fall and all spring interviews were conducted during the same month each wave, and that spring interviews were conducted six months after fall interviews of the same school year. For height, we drop students with missing height in at least three waves, students with missing height in two waves but whose reported height falls at least once over time, and students whose reported height falls at least twice over time. For the remainder of students, we impute missing height or values of height that represented a decline from previously reported height by regressing 'valid' measures of height on age and imputing height. If the imputed value of height still represents a decline in height from previously reported height, the student is dropped. For weight, we begin by identifying suspicious values; those representing large declines or large gains in weight from the previous wave. Then, we drop students with missing weight in at least three waves, students with missing weight in two waves but whose reported weight falls by more than 15 pounds across two waves, and students with missing weight in at least one wave and a suspicious value in at least one other wave. For the remainder of students, we impute missing weight or suspicious values of weight by regressing 'valid' measures of weight on age and imputing weight. If the imputed value

[13] Percentiles are obtained using the -*zanthro*- command in Stata, which computes the age- and gender-specific percentiles based on pre-epidemic distributions summarized in the 2000 CDC growth charts.

[14] Except for maternal employment, all controls come from either the fall or spring kindergarten survey.

of weight is still deemed to be suspicious according to our criteria, the student is dropped. As a final check, we drop students if the resulting 'clean' data on age, height, and weight implied a change in BMI percentile of greater than 80 percentile points (in absolute value) from fall kindergarten to spring third grade.

The final sample contains 13,534 students, of which 5,423 participate in neither the SBP or NSLP, 2,826 participate in both, and 335 (4,950) participate in the SBP (NSLP) only. Table A1 in the appendix provides summary statistics. The average BMI during spring third grade is 18.4, up from 16.3 in fall kindergarten. The average growth rate in BMI over this time span is 11.2%, and the average increase in BMI percentile is 1.3 (from 61.0 to 62.3). Finally, while 11.4% (25.8%) of entering kindergarten children were obese (overweight), 17.1% (32.5%) of third grade students were obese (overweight). Also noteworthy, and particularly relevant for the contrasting the various estimators, is the fact that observable attributes of participants and non-participants in the school nutrition programs do differ, implying that issues of common support may be important. Specifically, participants in both the SBP and NSLP are more likely to be non-white, reside in the south, live in a poor household with a less educated mother, have fewer children's books in the home, and have a mother who was more likely to have given birth while a teenager.

5 Results

5.1 Baseline

The baseline results obtained using the full sample are presented in Table 1. The specification displayed in Column (1) includes all covariates mentioned in the previous section except terms involving the fall kindergarten health measures. Column (2) adds pre-treatment values of the dependent variable to the control set. Column (3) is identical to Column (2) except now we include school fixed effects. Finally, Column (4) presents the PSM results, utilizing the control set as in Column (2) to estimate the propensity scores (using probit models).[15]

While we do not wish to interpret the baseline results in a causal manner, several findings are noteworthy. First, SBP participation in kindergarten is associated with greater child weight in third grade, robust across all specifications and health measures (particularly in Panels I – V). Participation in the NSLP in kindergarten, on the other hand, has a statistically insignificant association with third grade child weight in all specifications. Second, conditioning on lagged child health in fall kindergarten in Column

[15] Millimet and Tchernis (2007) find that propensity score estimators perform better when over-specifying the propensity score equation. Thus, we follow specification (2) and include higher order and interaction terms involving all of the continuous variables. Moreover, note that because specification (2) includes the corresponding measure of child health from fall kindergarten, the exact propensity score model is specific to each outcome measure.

(2), or concentrating on the health measures that represent the change from fall kindergarten to spring third grade, reduces the coefficients on SBP by roughly one-third in Panels I – V. The decline indicates *positive selection* into the SBP: children who weigh more upon entry into kindergarten are more likely to participate. Third, inclusion of school fixed effects has no qualitative effect on the estimates in Panels I – V; there is an effect in Panels VI and VII, although this reflects the switch from a probit model to a linear probability model.[16] This is suggestive that non-random selection into the SBP is occurring at the student-level, not the school-level. We shall return to this point below. Finally, the PSM estimates in Column (4) are significantly larger in magnitude for SBP; the estimates for NSLP remain statistically insignificant. The increase ranges from 40% larger in Panel III (BMI growth rate) to over 100% larger in Panel VII (probability of being obese).

Table 2 relaxes the assumption implicit in (3) and (6) that school nutrition programs (and the control variables) have identical effects across children, but maintains the assumption of selection on observables and/or school-level unobservables. Since children entering kindergarten overweight or obese are the most likely targets of any policies designed to combat the recent rise in childhood obesity, we allow for heterogeneous effects by risk type: children entering kindergarten with a BMI below the 85^{th} percentile ('normal' weight) and students entering with a BMI between above the 85^{th} percentile ('overweight' or 'obese'). In the interest of brevity, we report results for only four health measures (the two based on changes between fall kindergarten and spring third grade, and the two binary measures). In addition, we only display estimates from the specifications used in Columns (1), (3), and (4) in Table 1.

The results indicate that the inferences drawn from the full sample in Table 1 are driven primarily by the sample of children entering kindergarten in the normal weight range. Only the PSM estimates of the SBP association are statistically significant in the overweight and obese sample, and then only in Panels I and IV. In addition, relative to the full sample results, the PSM estimates for the normal weight range sample in Column (3) are more similar to the regression estimates given in Column (2) in several instances. This is consonant with a greater similarity of the treatment and control groups in terms of observable attributes in this sub-sample. Coefficient estimates for NSLP remain statistically insignificant across all sub-samples and outcome measures.

In sum, the baseline results suggest a remarkably robust positive association between SBP participation and child weight in the relative long-run, with no corresponding detectable association between NSLP participation and child weight. In addition, the positive association is mainly attributable to children entering kindergarten in a healthy weight range. This positive association between SBP participation and

[16]Estimation of a linear probability model in Column (2) yields coefficient estimates of 0.019 (s.e. = 0.009) and -0.003 (0.008) for SBP and NSLP, respectively, in Panel VI; 0.012 (0.007) and 0.005 (0.006) in Panel VII.

child weight contrasts with the results in Bhattacharya et al. (2006) and Hofferth and Curtin (2005), but is consonant with the analysis in Long (1991). The lack of a relationship between NSLP participation and child weight also diverges from Schanzenbach (2007). However, before placing too much stock in these results, we need to assess the extent to which they are likely to represent a causal relationship. In the remainder of the paper, we investigate this issue.

5.2 Non-Random Selection into School Nutrition Programs

Pre-Program Health Outcomes The baseline results indicate two salient points. First, there does not appear to be any measurable selection bias at the school-level conditional on the covariates, given the similarity of the results including school fixed effects.[17] Second, conditioning on child weight in fall kindergarten eliminates about one-third of the positive association between SBP participation and child weight in third grade, indicating fairly strong positive selection on previous child weight in *levels*. If there is also positive selection on the basis of expected future *changes* in child weight, then the results thus far will not represent a causal relationship. We explore this possibility by examining selection into the SBP on the basis of weight growth prior to kindergarten.

To proceed, we follow the strategy employed in Schanzenbach (2007) and re-estimate our models using the change in weight from birth to kindergarten entry as the dependent variable (in both levels and growth rates). For comparison, we also use weight (in pounds) at kindergarten entry as the dependent variable (in both levels and logs). We report results for the full sample, as well as the sample partitioned by risk type.

The results are reported in Table 3. The specifications displayed are analogous to those used in Table 2, with the addition of child height measured during fall kindergarten (along with corresponding higher order and interaction terms) and the omission of child birthweight as a covariate in Panels II and IV.[18]

Viewing the results, four findings emerge. First, while the vast majority of the coefficients, on both SBP and NSLP, are positive, the only statistically significant coefficients are for SBP. Second, all of the statistically significant SBP coefficients – with the exception of one – occur in Panels II and IV, where the dependent variable reflects the change in weight from birth to kindergarten entry. Thus, there is

[17] A straightforward exercise using the NHANES data from Bhattacharya et al. (2006) also points to a lack of selection on the basis of school-level unobservables. Specifically, we re-estimate their difference-in-difference model with BMI as the outcome; we also used linear probability models with indicators for overweight and obese as the outcomes. We then assessed the correlation between the residuals from these equations and the residuals from a treatment equation (which, in their model, is not actual participation by the student, but rather SBP availability interacted with being in school) using the same covariates. The correlation is less than 0.03 in absolute value in all cases. Since the treatment in Bhattacharya et al. (2006) is a school-level measure of availability, this provides further evidence that the selection is not at the school level.

[18] We include controls for child height since the dependent variables are now based on measures of weight, rather than BMI.

(statistically) stronger evidence of positive selection into SBP on the basis of weight *trajectories* (as opposed to just the level of weight at the time of kindergarten entry). Interestingly, this implies that conditional on weight at the time of kindergarten entry (and the remaining covariates), children with lower birthweight – and therefore have gained more weight prior to kindergarten – are more likely to participate in the SBP. Third, this positive selection on trajectories applies to both sub-samples of children defined on the basis of risk type. Finally, inclusion of school fixed effects does not mitigate the evidence of positive selection into the SBP, and the PSM estimates of the SBP coefficients continue to be larger in magnitude in many cases.

Given that the positive selection into the SBP occurs at the child-level, it would be informative to know the precise factors accounting for such selection. Although this is beyond the scope of the current analysis, we put forth two possible explanations. First, parents of children who consume large breakfasts prior to kindergarten, leading to a steeper weight trajectory between birth and kindergarten entry, may be more likely to opt for school provided breakfasts. This preference may arise for two reasons: (i) school meals are available only in a fixed quantity, and (ii) schools meals, even purchased at full price, are subsidized to a limited extent. Second, children raised in home environments where parents devote less time to their children (e.g., due to demanding jobs or simply due to preferences over types of leisure) may be more likely to be on a steeper weight trajectory between birth and kindergarten entry. In addition, the time allocation choices of the parents may lead them to opt for more convenient school provided breakfasts. Since the ECLS-K does not contain data on the household environment prior to kindergarten, we cannot assess these hypotheses. However, future work utilizing the ECLS-Birth Cohort may shed some light.

These findings suggest that the associations presented in Tables 1 and 2 overstate the causal relationship between SBP participation and child weight. Equally important, however, not only does positive selection into the SBP bias the regression coefficients on SBP participation upward, it most likely biases the regression coefficients on NSLP participation downward given the positive covariance between SBP and NSLP participation. Thus, despite the lack of overwhelming evidence of any direct selection bias associated with NSLP participation, failure to address selection into the SBP biases the estimates of the NSLP effect.[19] To quantify exactly how sensitive the results are to selection into the SBP program, we turn to several methods proposed in the program evaluation literature useful for assessing sensitivity to selection on unobservables.

[19] For simplicity, consider the simple regression model $y = \alpha + x\beta + \varepsilon$, where x includes only SBP and NSLP participation dummies. The expectation of the OLS estimate, $\mathrm{E}\left[\widehat{\beta}\right]$, equals $\beta+(x'x)^{-1}x'\varepsilon$. Assuming $\mathrm{Cov}(SBP,\varepsilon) > 0$ and $\mathrm{Cov}(NSLP,\varepsilon) = 0$, conditional on the other element of x, and $\mathrm{Cov}(SBP, NSLP) > 0$, one can show that $\widehat{\beta}_{SBP}$ ($\widehat{\beta}_{NSLP}$) is biased up (down).

Bivariate Probit Model To assess the impact of positive selection into the SBP, we employ the bivariate probit model utilized in Altonji et al. (2005).[20] The model is given by

$$\begin{aligned} y_i &= \mathrm{I}(x_i\beta_0 + \tau_1 D_{1i} + \tau_2 D_{2i} + \varepsilon_i > 0) \\ D_{1i} &= \mathrm{I}(x_i\lambda_0 + \lambda_2 D_{2i} + \upsilon_i > 0) \end{aligned} \tag{7}$$

where $\mathrm{I}(\cdot)$ is the indicator function, $\varepsilon, \upsilon \sim N_2(0,0,1,1,\rho)$, y is a binary measure of child health (overweight or obesity status), and D_1 and D_2 represent SBP and NSLP participation, respectively, as in (3). The correlation coefficient, ρ, captures the correlation between unobservables that impact child weight and the likelihood of SBP participation; $\rho > 0$ implies positive selection on unobservables.

Given the bivariate normality assumption, the model is technically identified even absent an exclusion restriction. However, to assess the role of selection into the SBP without formally relying on the distributional assumption, Altonji et al. (2005) constrain ρ to different values and examine the estimates of the remaining parameters. Here, we set ρ to 0, 0.1, ..., 0.5, representing increasingly strong levels of positive selection on unobservables into the SBP. The results for the full sample using the same covariate sets as in Columns (1) and (2) in Table 1 are presented in Table 4. The results by risk type are relegated to the appendix, Table A2.[21]

The results are dramatic. First, across both specifications, both outcomes, and all data samples (the full sample and the sub-samples defined by risk type), the positive effect of SBP participation disappears when $\rho = 0.1$, and is negative and statistically significant in the full sample and sub-sample of children overweight or obese in kindergarten (Tables 4 and A2). When ρ increases to 0.2, the effect of SBP is also negative and statistically significant in the sub-sample of children entering kindergarten in the normal weight range (Table A2). Second, consistent with our earlier hypothesis that positive selection into the SBP biases the effect of NSLP participation downward, the coefficients on NSLP increase with ρ; in most cases, the positive coefficient on NSLP participation is statistically significant for $\rho = 0.2$ or 0.3. In the full sample (Table 4), the effect of NSLP is positive and statistically significant at conventional levels if $\rho = 0.2$.

In sum, the bivariate probit models indicate, first and foremost, that the positive associations documented earlier between SBP participation and child weight are *extremely* sensitive to selection on unobservables; even a modest amount of positive selection eliminates or even reverses the previous results. Equally important, allowing for positive selection into the SBP indicates that NSLP participation leads

[20] A similar strategy is used in Frisvold (2007) to assess the impact of Head Start participation on childhood obesity.

[21] In Table A2, we only present results using specification (1) since it is not possible to include indicators of overweight or obesity status upon kindergarten entry in Panel I.

to greater child weight. Thus, conditioning on SBP participation, but allowing for positive selection into the SBP, yields NSLP effects that are consistent with the contemporaneous relationship documented in Schanzenbach (2007) using alternative methodologies. Our findings are also consistent with findings from the SNDA-2 analysis of school meals conducted in 1998-1999. The SNDA-2 study found that the average percent of calories derived from fat (saturated fat) was 34% (12%), which still exceeds the requirements instituted under the SMI. Breakfasts, on average, met the SMI requirements, deriving 26% (9.8%) of calories from fat (saturated fat).[22] Moreover, a vast research touts the importance of eating breakfast in maintaining a healthy weight; skipping breakfast is associated with overall higher caloric intake (e.g., Morgan et al. 1986; Stauton and Keast 1989). On the other hand, the FNS found that even a dietitian could not select a low fat lunch provided by the NSLP in 10 − 35% of schools.

Prior to continuing, a few comments are warranted. First, while the Altonji et al. (2005) approach is informative, it does provide a different type of information than applied researchers are accustomed. Specifically, we are not arriving at point estimates of the effects of participation. While that should be the goal of future work, obtaining consistent point estimates of the effect of participation (as opposed to program availability, as in Bhattacharya et al. (2006)) requires a valid instrument. While the RD strategy pursued in Schanzenbach (2007) is promising, one might worry that the treatment effect being identified is only valid for students near the income thresholds used in the subsidy eligibility rules. Thus, the point estimates may not apply to a student chosen at random from the population. In light of this, we believe the preceding analysis to offer valuable insight: modest positive selection into the SBP implies a beneficial effect of participation on child health, and a bit more than modest positive selection implies an adverse effect of NSLP participation.

Second, while we do not know the true value of ρ (and, indeed, cannot know it absent a valid exclusion restriction or reliance on the bivariate normality assumption), a value around 0.1 does not seem unreasonable since potentially important factors, such as parental height and weight, family size, and measures of genetic endowments, are not included in the set of observables. Moreover, we did estimate the bivariate probit models without constraining ρ; thus, the models are identified solely from the parametric assumption. For the four models in Table 4, we obtain estimates between 0.17 and 0.43; between 0.16 and 0.68 in the four models in Table A2.[23] Finally, we exploited the identification strategy used in Schanzenbach (2007). Specifically, we used binary indicators for having a household income below 130% and 185% of the

[22] See also http://www.iom.edu/Object.File/Master/31/064/Jay%20Hirschman.IOM%20Presentation.Oct%2026%202005.pdf.

[23] The corresponding estimates for Table 4 are $\widehat{\rho} = 0.260$ and 0.175 for overweight status in specifications (1) and (2), respectively; for obesity status, $\widehat{\rho} = 0.433$ and 0.363. For Table A2, the estimates are $\widehat{\rho} = 0.164$ and 0.373 for overweight and obesity status, respectively, in Panel I; for Panel II, $\widehat{\rho} = 0.316$ and 0.678.

federal poverty line as exclusion restrictions; and, we included a fourth order polynomial for the ratio of household income to the poverty line in both the treatment and outcome equations. The estimates of ρ are quite similar, albeit the exclusion restrictions are only statistically significant at conventional levels in the sub-sample of children entering kindergarten in the normal weight range.[24]

Extent of Selection on Unobservables Altonji et al. (2005) offer an alternative method for assessing the role of unobservables, applicable to continuous outcomes as well. Intuitively, the idea is to assess how much selection on unobservables there must be, relative to the amount of selection on observables, to fully account for the positive association between SBP participation and child weight under the null hypothesis of no average treatment effect.

The (normalized) amount of selection on unobservables is formalized by the ratio

$$\frac{\mathrm{E}[\varepsilon|D_1 = 1] - \mathrm{E}[\varepsilon|D_1 = 0]}{\mathrm{Var}(\varepsilon)} \tag{8}$$

where D_1 denotes SBP participation as above and ε captures unobservables in the outcome equation (representing the full error term in (3) or (6)). Similarly, the (normalized) amount of selection on observables is formalized by the ratio

$$\frac{\mathrm{E}[x_o\widetilde{\beta}|D_1 = 1] - \mathrm{E}[x_o\widetilde{\beta}|D_1 = 0]}{\mathrm{Var}(x_o\widetilde{\beta})} \tag{9}$$

where x_o is the set of observable controls included in the outcome equation (representing both x and D_2 in (3) and (6)) and $\widetilde{\beta}$ is the corresponding parameter vector. The goal is to assess how large the selection on unobservables in (8) must be relative to the selection on observables in (9) to fully account for the positive association between SBP and child weight documented in Tables 1 and 2.

To begin, express actual SBP participation as

$$D_{1i} = x_{oi}\lambda + \upsilon_i \tag{10}$$

and substitute this into (3) or (6). Equation (3), for example, becomes

$$y_i = x_{oi}(\widetilde{\beta} + \tau_1\lambda) + \tau_1\upsilon_i + \varepsilon_i. \tag{11}$$

The probability limit of the OLS estimator of τ_1 in (11) is given by

$$\begin{aligned} \mathrm{plim}\,\widehat{\tau}_1 &= \tau_1 + \frac{\mathrm{Cov}(\upsilon, \varepsilon)}{\mathrm{Var}(\upsilon)} \\ &= \tau_1 + \frac{\mathrm{Var}(D_1)}{\mathrm{Var}(\upsilon)} \left\{\mathrm{E}[\varepsilon|D_1 = 1] - \mathrm{E}[\varepsilon|D_1 = 0]\right\}. \end{aligned} \tag{12}$$

[24] The corresponding estimates for Table 4 are $\widehat{\rho} = 0.255$ and 0.174 for overweight status in specifications (1) and (2), respectively; for obesity status, $\widehat{\rho} = 0.430$ and 0.405. For Table A2, the estimates are $\widehat{\rho} = 0.167$ and 0.436 for overweight and obesity status, respectively, in Panel I; for Panel II, $\widehat{\rho} = 0.344$ and 0.723.

Under the assumption that the degree of selection on observables – given by (9) – is equal to the degree of selection on unobservables – given by (8) – the bias term in (12) is

$$\frac{\mathrm{Cov}(\upsilon,\varepsilon)}{\mathrm{Var}(\upsilon)} = \frac{\mathrm{Var}(D_1)}{\mathrm{Var}(\upsilon)} \left\{ \frac{\mathrm{E}[x_o\widetilde{\beta}|D_1=1] - \mathrm{E}[x_o\widetilde{\beta}|D_1=0]}{\mathrm{Var}(x_o\widetilde{\beta})} \mathrm{Var}(\varepsilon) \right\}. \tag{13}$$

Under the null hypothesis that $\tau_1 = 0$, $\widetilde{\beta}$ can be consistently estimated from (11) using either OLS or a probit model and constraining τ_1 to be zero. Using the estimated $\widetilde{\beta}$ and variance of the residual (which is unity when (11) is estimated via probit), along with sample values of $\mathrm{Var}(D_1)$ and $\mathrm{Var}(\upsilon)$ yields an estimate of the asymptotic bias under equal degrees of selection on observables and unobservables.

Dividing the unconstrained estimate of τ_1 from (11) by (13) indicates how much larger the extent of selection on unobservables needs to be, relative to the extent of selection on observables, to entirely explain the treatment effect. If this ratio is small, the implication is that the treatment effect is highly sensitive to selection on unobservables. As discussed in Altonji et al. (2005), if one conceptualizes the set of variables included in x_o as a random draw of all factors affecting child weight (with the remaining factors being captured by ε) and no factor (observed or unobserved) plays too large of role in the determination of child weight, then the treatment effect should be interpreted as not robust if the ratio is less one.

The results for the full sample are shown in Table 5; the results disaggregated by risk type are relegated to the appendix, Table A3. For the full sample, and for the sub-samples based on risk type, the implied ratio is never greater than 0.5, rarely above 0.3, and often less than 0.1. Thus, if the (normalized) amount of selection on unobservables is even half the (normalized) amount of selection on observables, and often even ten percent, the positive effects of SBP participation are completely explained.

As in the bivariate probit model, this model does not yield point estimates of the treatment effect. Nonetheless, it provides very useful information; information which confirms the bivariate probit findings: even a modest amount of selection on unobservables is sufficient to explain the entire positive association between SBP participation and child weight.

Rosenbaum Bounds Our final method of assessing the role of selection on unobservables is to return to the PSM estimates reported in Tables 1 and 2 and utilize Rosenbaum bounds (Rosenbaum 2002). While there exist other methods of assessing the sensitivity of PSM estimates to selection on unobservables, Rosenbaum bounds are computationally attractive and also offer an intuitively appealing measure of the way in which unobservables enter the model (Ferraro et al. 2007).

In the interest of brevity, and because Rosenbaum bounds have become more widely used in econometric analyses of program evaluation, we do not provide the formal details. Instead, we simply note that the objective of the method is to obtain bounds on the significance level of a one-sided test for no treatment effect under different assumptions concerning the role of unobservables in the treatment selection process. Specifically, we report upper bounds on the p-value of the null of zero average treatment effect for different values of Γ, where Γ reflects the relative odds ratio of two observationally identical children receiving the treatment. Thus, Γ is one in a randomized experiment or in non-experimental data free of bias from selection on unobservables; higher values of Γ imply an increasingly important role of unobservables. For example, $\Gamma = 2$ implies that observationally identical children can differ in their relative odds of treatment by a factor of two.

The results for the full sample and by risk type are relegated to the appendix, Table A4. For the full sample, the positive effects of SBP participation in the full sample are sensitive to hidden bias if $\Gamma \geq 1.4$ for all outcomes except obesity status, and $\Gamma \geq 1.8$ for obesity status. Thus, if observationally identical children differ in their odds of participating in the SBP by even 40%, the program effect is sensitive to hidden bias. In the PSM literature, $\Gamma = 1.4$ is usually interpreted as 'small', implying that our PSM estimates of the average treatment effect of SBP participation is not free from hidden bias.

When splitting the sample by risk type, we find the effects of SBP to be sensitive to hidden bias if $\Gamma \geq 1.6$ for all outcomes for both sub-samples except when analyzing obesity status for children entering kindergarten overweight or obese (here, the estimate is sensitive to hidden bias if $\Gamma \geq 2$). Again, while Rosenbaum bounds do not yield point estimates of the treatment effects once hidden bias is taken into account, these findings are consistent with the prior results, indicating that relative modest positive selection is sufficient to account for the positive associations documented in Tables 1 and 2.

Summation The analysis contained herein yields a fairly consistent picture of the effects of school nutrition programs. First, SBP participation is likely related to unobservables correlated with trajectories for child weight (in addition to child weight in levels), whereas there is almost no evidence that NSLP participation is affected by selection on unobservables. Second, ignoring this selection biases estimates of the average treatment effect of SBP (NSLP) participation upward (downward) regardless of whether one examines measures of child weight in levels or changes. Finally, allowing for even modest positive selection into the SBP is sufficient to yield a negative (positive) causal affect of SBP (NSLP) participation on child weight. Thus, consonant with the results in Bhattacharya et al. (2006) and Schanzenbach (2007), we find that the SBP is not a contributing factor to the current obesity epidemic, and may actually constitute a valuable tool, but the NSLP is contributing to the current epidemic.

5.3 Final Robustness Checks

We perform two final robustness checks of our analysis. First, because the 335 responses indicating participation in the SBP, but not the NSLP, may reflect measurement error, or students sufficiently different from the remainder of the sample, we re-did the analysis omitting these observations. The results are unaffected and are available upon request.

Second, because some students attend full-day kindergarten and others only half-day programs, variation in participation in the two programs will, in part, reflect the type of school one attends. To circumvent this issue, we re-did the all of the preceding analysis measuring participation in the spring first grade. All other aspects of the analysis is unchanged. In particular, we still control for weight at kindergarten entry when incorporating pre-treatment outcomes, and we still assess selection into the programs by regressing weight at kindergarten entry or the change in weight from birth to kindergarten entry on the first grade participation decisions.

In sum, measuring participation in first grade matters, but does not change our fundamental conclusions. Specifically, we find even stronger evidence that NSLP is detrimental to child health, and we continue to find evidence that SBP is beneficial once positive selection into the SBP is addressed. Finally, we continue to find little evidence of non-random selection into the NSLP, particularly once we split the sample by risk type.[25]

6 Conclusion

Given the vast research on the importance of breakfast, as well as the nutritional requirements imposed on schools seeking reimbursement under the SBP and the NSLP, these programs are viewed by many as one potential component of any attempt to reverse the rise in childhood obesity. That said, empirical research on the impact of these programs on child weight subsequent to the required implementation of the reforms instituted under the School Meals Initiative for Healthy Children has been lacking. Using panel data on over 13,500 students from kindergarten through third grade, we assess the relatively long-run relationship between SBP and NSLP participation and child weight.

Our results are striking, and yield three primary conclusions. First, there is a strong, positive *association* between SBP participation in kindergarten and child weight in third grade as well as weight gain between kindergarten and third grade. There is no association between NSLP participation and child weight in third grade. However, we find evidence of positive selection into the SBP, particularly for children entering kindergarten in the normal weight range. Consonant with Schanzenbach (2007), selection bias does not

[25] The full set of results available at http://faculty.smu.edu/millimet/pdf/mth_AppendixB.pdf.

seem to be much of a concern when analyzing the NSLP. Finally, positive selection into the SBP of even modest magnitude is sufficient to overturn the initial findings: the *causal* relationship between SBP participation and child weight becomes negative and statistically meaningful. Moreover, in this case, the *causal* relationship between NSLP participation and child weight becomes positive. Thus, admitting even modest positive selection into the SBP implies that the SBP is a *valuable* tool in the current battle against childhood obesity, whereas the NSLP *exacerbates* the current epidemic.

These results complement the previous findings in Bhattacharya et al. (2006) and Schanzenbach (2007), confirming the positive (negative) effects of the SBP (NSLP) using data after the reforms of the late 1990s, employing alternative empirical methodologies, and examining more long-run measures of child health.

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Table A1. Summary Statistics

Variable	Full Sample Mean	Full Sample SD	Particpation in Neither Mean	Particpation in Neither SD	SBP Only Mean	SBP Only SD	NSLP Only Mean	NSLP Only SD	Participation in Both Mean	Participation in Both SD
SBP Participation (1 = Yes)	0.234	0.423	0	0	1	0	0	0	1	0
NSLP Participation (1 = Yes)	0.575	0.494	0	0	0	0	1	0	1	0
Third Grade Child Weight										
BMI	18.404	3.861	18.124	3.536	19.155	4.537	18.358	3.873	18.933	4.266
BMI Growth Rate	0.112	0.126	0.104	0.119	0.130	0.132	0.110	0.125	0.128	0.137
BMI percentile	62.326	30.105	60.966	29.867	65.363	30.300	61.686	30.409	65.697	29.739
Change in BMI Percentile	1.295	22.887	0.589	22.473	3.471	23.587	1.048	23.148	2.826	23.054
Overweight (1 = Yes)	0.325	0.468	0.304	0.460	0.397	0.490	0.320	0.466	0.365	0.481
Obese (1 = Yes)	0.171	0.377	0.150	0.357	0.248	0.432	0.172	0.377	0.204	0.403
Fall Kindergarten Child Weight										
BMI	16.265	2.142	16.168	1.977	16.600	2.667	16.259	2.179	16.423	2.295
BMI percentile	61.030	28.452	60.376	28.122	61.892	30.077	60.638	28.840	62.871	28.133
Overweight (1 = Yes)	0.258	0.438	0.244	0.430	0.293	0.456	0.258	0.437	0.282	0.450
Obese (1 = Yes)	0.114	0.318	0.103	0.304	0.185	0.389	0.114	0.318	0.125	0.331
Age (in months)	110.767	4.356	110.725	4.347	110.936	4.087	110.749	4.345	110.861	4.424
Gender (1 = boy)	0.507	0.500	0.511	0.500	0.522	0.500	0.494	0.500	0.523	0.500
White (1 = Yes)	0.579	0.494	0.721	0.449	0.591	0.492	0.587	0.492	0.291	0.454
Black (1 = Yes)	0.138	0.345	0.050	0.218	0.122	0.328	0.123	0.328	0.334	0.472
Hispanic (1 = Yes)	0.174	0.379	0.125	0.330	0.185	0.389	0.186	0.390	0.246	0.431
Asian (1 = Yes)	0.054	0.226	0.058	0.235	0.045	0.207	0.056	0.231	0.041	0.199
Child's Birthweight (ounces)	118.284	20.040	120.015	19.510	117.542	21.788	117.970	19.495	115.600	21.407
Child's Birthweight (1 = Missing)	0.121	0.326	0.098	0.297	0.143	0.351	0.117	0.322	0.167	0.373
Central City (1 = Yes)	0.395	0.489	0.356	0.479	0.310	0.463	0.425	0.494	0.428	0.495
Urban Fringe & Large Town (1 = Yes)	0.377	0.485	0.475	0.499	0.340	0.475	0.346	0.476	0.250	0.433
Northeast (1 = Yes)	0.182	0.386	0.265	0.441	0.334	0.472	0.134	0.340	0.089	0.285
Midwest (1 = Yes)	0.250	0.433	0.293	0.455	0.236	0.425	0.239	0.427	0.189	0.391
South (1 = Yes)	0.346	0.476	0.192	0.394	0.278	0.448	0.413	0.492	0.535	0.499
Mother's Age at First Birth ≤ 19 Years Old (1 = Yes)	0.227	0.419	0.141	0.348	0.290	0.454	0.208	0.406	0.418	0.493
Mother's Age at First Birth is 20-29 Years Old (1 = Yes)	0.522	0.500	0.566	0.496	0.507	0.501	0.544	0.498	0.398	0.490
Mother's Age at First Birth (1 = Missing)	0.104	0.305	0.085	0.279	0.143	0.351	0.102	0.303	0.139	0.346
WIC Benefits During Pregnancy (1 = Yes)	0.339	0.473	0.189	0.391	0.504	0.501	0.323	0.468	0.634	0.482
WIC Benefits During Pregnancy (1 = Missing)	0.112	0.315	0.095	0.293	0.134	0.342	0.113	0.316	0.141	0.348
Mother's Education = High School (1 = Yes)	0.198	0.398	0.172	0.377	0.278	0.448	0.197	0.398	0.239	0.426
Mother's Education = Some College(1 = Yes)	0.281	0.450	0.304	0.460	0.301	0.460	0.292	0.455	0.218	0.413
Mother's Education = Bachelor's Degree (1 = Yes)	0.144	0.351	0.198	0.398	0.057	0.232	0.152	0.359	0.038	0.192
Mother's Education = Advanced College Degree (1 = Yes)	0.084	0.277	0.125	0.330	0.027	0.162	0.078	0.268	0.023	0.151
Mother's Education (1 = Missing)	0.209	0.407	0.168	0.374	0.221	0.415	0.206	0.405	0.293	0.455

Notes: N = 13,534 (full sample); 5,423 (participation in neither); 335 (SBP only); 4,950 (NSLP only); 2,826 (SBP and NSLP). Data are from from the kindergarten wave of ECLS-K unless otherwise noted. Change in BMI percentile and BMI growth rate calculated using baseline data from fall kindergarten. Omitted category for race is 'other', city type is 'small town & rural', mother's age at first birth is greater than 29 years old, mother's employment is 'missing', mother's education is 'less than high school', and sufficient food is 'sometimes or often there is not enough to eat'.

Table A1 (cont.). Summary Statistics

Variable	Full Sample Mean	Full Sample SD	Particpation in Neither Mean	Particpation in Neither SD	SBP Only Mean	SBP Only SD	NSLP Only Mean	NSLP Only SD	Participation in Both Mean	Participation in Both SD
Household Income (dollars)	52150	32034	61774	33666	38744	23611	52855	31091	34036	21285
Mother Employed During 3rd Grade (1 = Yes)	0.572	0.495	0.613	0.487	0.513	0.501	0.594	0.491	0.462	0.499
Mother Employed During 3rd Grade (1 = No)	0.204	0.403	0.206	0.405	0.242	0.429	0.186	0.389	0.229	0.420
Sufficient Food of Type Desired in Household (1 = Yes)	0.847	0.360	0.901	0.299	0.758	0.429	0.859	0.348	0.733	0.442
Sufficient Food, but not of Type Desired in Household (1 = Yes)	0.138	0.345	0.093	0.290	0.209	0.407	0.130	0.337	0.231	0.422
Sufficient Food (1 = Missing)	0.001	0.028	0.000	0.014	0.000	0.000	0.001	0.032	0.002	0.042
Number of Children's Books in Household	74.930	57.030	91.101	58.567	67.774	56.005	74.002	54.846	46.369	45.065
Number of Children's Books in Household (1 = Missing)	0.097	0.296	0.085	0.279	0.134	0.342	0.097	0.296	0.117	0.321

Notes: N = 13,534 (full sample); 5,423 (participation in neither); 335 (SBP only); 4,950 (NSLP only); 2,826 (SBP and NSLP). Data are from from the kindergarten wave of ECLS-K unless otherwise noted. Change in BMI percentile and BMI growth rate calculated using baseline data from fall kindergarten. Omitted category for race is 'other', city type is 'small town & rural', mother's age at first birth is greater than 29 years old, mother's employment is 'missing', mother's education is 'less than high school', and sufficient food is 'sometimes or often there is not enough to eat'.

Table A2. Sensitivity Analysis: Bivariate Probit Results with Different Assumptions Concerning Correlation Among the Disturbances by Risk Type

	Correlation of the Disturbances					
	Specification (1)					
	ρ = 0	**ρ = 0.1**	**ρ = 0.2**	**ρ = 0.3**	**ρ = 0.4**	**ρ = 0.5**
I. Normal Weight Entering Kindergarten						
	A. Probability of Being Overweight					
School Breakfast	0.124*	-0.043	-0.210*	-0.377*	-0.543*	-0.709*
	(0.042)	(0.042)	(0.042)	(0.041)	(0.040)	(0.038)
School Lunch	-0.011	0.016	0.046	0.078†	0.112*	0.149*
	(0.035)	(0.035)	(0.035)	(0.035)	(0.034)	(0.034)
	B. Probability of Being Obese					
School Breakfast	0.155†	-0.013	-0.180*	-0.348*	-0.516*	-0.686*
	(0.061)	(0.060)	(0.059)	(0.058)	(0.057)	(0.055)
School Lunch	0.008	0.037	0.070	0.107†	0.147*	0.192*
	(0.053)	(0.053)	(0.052)	(0.052)	(0.051)	(0.050)
II. Obese or Overweight Entering Kindergarten						
	A. Probability of Being Overweight					
School Breakfast	-0.046	-0.213*	-0.381*	-0.548*	-0.717*	-0.887*
	(0.065)	(0.065)	(0.065)	(0.064)	(0.063)	(0.061)
School Lunch	-0.016	0.013	0.040	0.066	0.090	0.113†
	(0.057)	(0.057)	(0.057)	(0.056)	(0.056)	(0.056)
	B. Probability of Being Obese					
School Breakfast	0.032	-0.135†	-0.301*	-0.468*	-0.633*	-0.798*
	(0.058)	(0.058)	(0.057)	(0.057)	(0.055)	(0.053)
School Lunch	0.027	0.056	0.086‡	0.115†	0.145*	0.174*
	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.049)

NOTES: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors in parentheses. Specification (1) refers to control set used in Column (1) in Table 1.
See Tables 1, 4, and text for details.

Table A3. Sensitivity Analysis: Amount of Selection on Unobservables Relative to Selection on Observables Required to Attribute the Entire SBP Effect to Selection Bias by Risk Type

	Specification (1)			Specification (2)		
	Cov(ε,ν)÷ Var(ν)	**τ_1**	**Implied Ratio**	**Cov(ε,ν)÷ Var(ν)**	**τ_1**	**Implied Ratio**
I. Normal Weight Entering Kindergarten						
BMI: Levels	8.825	0.305	0.035	1.002	0.243	0.242
		(0.067)			(0.059)	
BMI: Logs	0.437	0.017	0.038	0.051	0.013	0.261
		(0.004)			(0.003)	
BMI: Growth Rates	0.363	0.013	0.035	0.204	0.013	0.065
		(0.003)			(0.003)	
Percentile BMI: Levels	76.532	3.077	0.040	7.903	2.198	0.278
		(0.798)			(0.668)	
Percentile BMI: Changes	68.900	1.608	0.023	11.741	2.195	0.187
		(0.716)			(0.668)	
Probability of Being Overweight	4.561	0.033	0.007			
		(0.011)				
Probability of Being Obese	2.633	0.016	0.006			
		(0.006)				
II. Obese or Overweight Entering Kindergarten						
BMI: Levels	14.461	0.272	0.019	0.837	0.177	0.211
		(0.189)			(0.131)	
BMI: Logs	0.561	0.010	0.017	0.037	0.006	0.170
		(0.008)			(0.006)	
BMI: Growth Rates	0.396	0.006	0.015	0.225	0.006	0.028
		(0.006)			(0.006)	
Percentile BMI: Levels	20.935	-0.081	-0.004	3.058	-0.179	-0.058
		(0.620)			(0.557)	
Percentile BMI: Changes	10.621	-0.147	-0.014	7.906	-0.179	-0.023
		(0.559)			(0.557)	
Probability of Being Overweight	0.640	-0.013	-0.021	0.640	-0.013	-0.021
		(0.019)			(0.019)	
Probability of Being Obese	2.531	0.012	0.005	0.395	0.007	0.017
		(0.023)			(0.020)	

NOTES: Standard errors in parentheses. Specifications (1) and (2) refer to control sets used in Table 1. See Tables 1 and 5 for details.

Table A4. Propensity Score Matching Sensitivity Analysis by Risk Type: Rosenbaum Bounds (SBP)

	Γ = 1	Γ = 1.2	Γ = 1.4	Γ = 1.6	Γ = 1.8	Γ = 2	Γ = 2.5	Γ = 3
I. Full Sample								
BMI: Levels	p = 0.000	p = 0.000	p = 0.411	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
BMI: Logs	p = 0.000	p = 0.000	p = 0.996	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
BMI: Growth Rates	p = 0.000	p = 0.000	p = 0.873	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Percentile BMI: Levels	p = 0.000	p = 0.989	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Percentile BMI: Changes	p = 0.000	p = 0.000	p = 0.718	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Prob. of Being Overweight	p = 0.071	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Prob. of Being Obese	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.828	p = 1.000	p = 1.000	p = 1.000
II. Normal Weight Entering Kindergarten								
BMI: Levels	p = 0.000	p = 0.000	p = 0.069	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
BMI: Logs	p = 0.000	p = 0.000	p = 0.840	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
BMI: Growth Rates	p = 0.000	p = 0.000	p = 0.000	p = 0.985	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Percentile BMI: Levels	p = 0.000	p = 0.487	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Percentile BMI: Changes	p = 0.000	p = 0.000	p = 0.610	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Prob. of Being Overweight	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.064	p = 1.000	p = 1.000
Prob. of Being Obese	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000
III. Obese or Overweight Entering Kindergarten								
BMI: Levels	p = 0.000	p = 0.000	p = 0.491	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
BMI: Logs	p = 0.000	p = 0.002	p = 0.826	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
BMI: Growth Rates	p = 0.000	p = 0.335	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Percentile BMI: Levels	p = 0.966	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Percentile BMI: Changes	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Prob. of Being Overweight	p = 0.527	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000	p = 1.000
Prob. of Being Obese	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.191	p = 1.000	p = 1.000

NOTES: Rosenbaum critical p-values for test of the null of zero average treatment effect. For controls included in the propensty score, see Table 1.

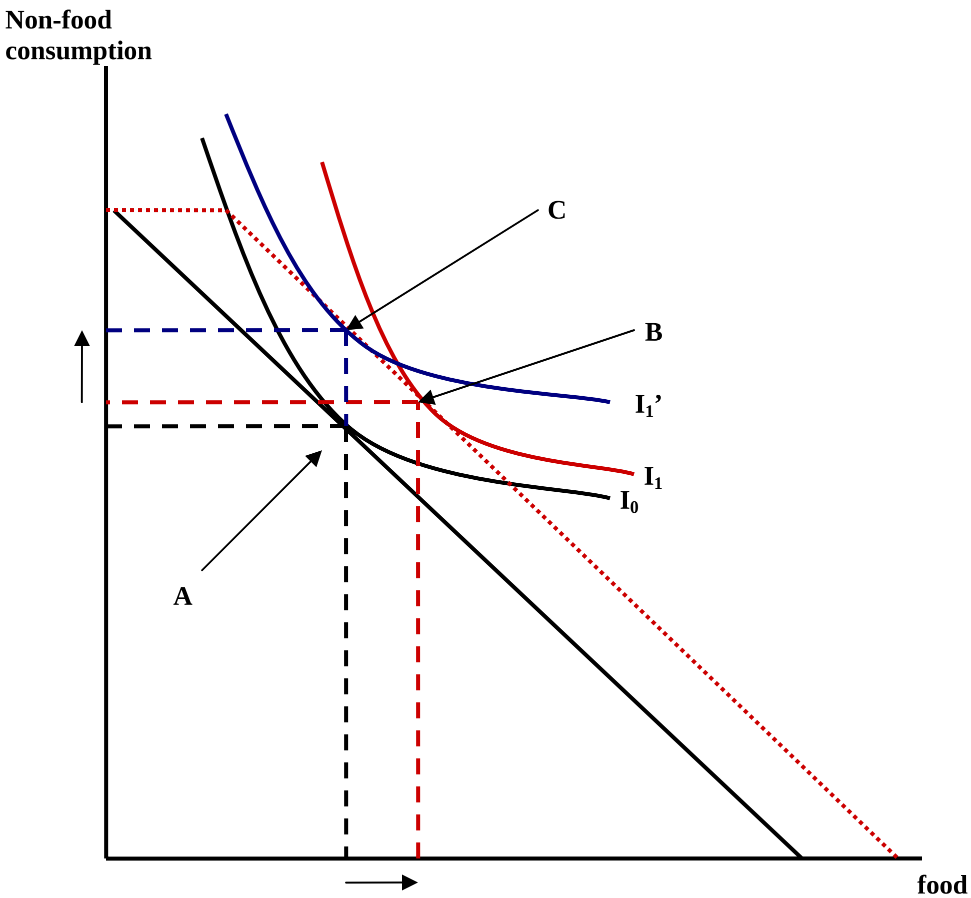

Figure 1. Theoretical Impact of Infra-Marginal Food Transfer Programs on Food and Non-Food Consumption.

NOTES: A – initial consumption point prior to food transfer program. B – final consumption point with food transfer program assuming food and non-food consumption are normal goods. C – final consumption point with food transfer program assuming non-food consumption is a normal good and the income elasticity of food consumption is zero.

Table 1. Full Sample Results

	OLS/Probit		School Fixed Effects	Propensity Score Matching
	(1)	(2)	(3)	(4)
I. BMI: Levels				
School	0.290*	0.209*	0.209*	0.353*
Breakfast	(0.092)	(0.056)	(0.063)	(0.120)
School	0.040	-0.004	-0.018	-0.022
Lunch	(0.075)	(0.046)	(0.062)	(0.096)
II. BMI: Logs				
School	0.014*	0.010*	0.010*	0.017*
Breakfast	(0.005)	(0.003)	(0.003)	(0.007)
School	0.002	-0.001	-0.002	-0.001
Lunch	(0.004)	(0.002)	(0.003)	(0.005)
III. BMI: Growth Rates				
School	0.010*	0.010*	0.010*	0.014*
Breakfast	(0.003)	(0.003)	(0.003)	(0.004)
School	0.000	-0.001	-0.002	0.000
Lunch	(0.002)	(0.002)	(0.003)	(0.003)
IV. Percentile BMI: Levels				
School	2.114*	1.478*	1.459†	2.178*
Breakfast	(0.714)	(0.510)	(0.575)	(1.023)
School	0.187	-0.258	-0.633	-0.023
Lunch	(0.582)	(0.415)	(0.560)	(0.729)
V. Percentile BMI: Changes				
School	1.009‡	1.475*	1.456†	2.462*
Breakfast	(0.548)	(0.510)	(0.575)	(0.826)
School	-0.350	-0.257	-0.633	-0.151
Lunch	(0.447)	(0.415)	(0.560)	(0.596)
VI. Probability of Being Overweight				
School	0.050	0.070†	0.017	0.031†
Breakfast	(0.032)	(0.036)	(0.011)	(0.016)
School	-0.004	-0.013	-0.007	-0.005
Lunch	(0.026)	(0.030)	(0.010)	(0.012)
VII. Probability of Being Obese				
School	0.055	0.064	0.017†	0.036*
Breakfast	(0.035)	(0.041)	(0.008)	(0.013)
School	0.015	0.032	0.001	-0.002
Lunch	(0.030)	(0.035)	(0.008)	(0.009)

NOTES: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors in parentheses. Marginal effects reported in Panels VI and VII. Additional controls in each model: (1) age, gender dummy, child's birthweight, 4 race dummies, 2 city type dummies, 3 region dummies, 3 dummies for mother's age at first birth, dummies for whether mother received WIC benefits during pregancy, 5 mother's education dummies, 2 dummies for mother's current employment status, household income, number of children's books in the household, 3 dummies for the amount of food in the household, quadratic and cubic terms of all continuous variables, and the complete set of pairwise interactions among the continuous variables. (2) previous control set plus the lagged dependent variable (from the fall kindergarten wave), quadratic and cubic terms of the lagged dependent variable (Panels I -- V only), and the complete set of pairwise interactions between the lagged dependent variable and the continuous variables included in the previous control set; (3) previous control set plus school fixed effects. Specification (3) in Panels VI and VII are estimated using a linear probability model. Column (4) reports separate propensity score matching estimates for school breakfast and school lunch using the variables from model (2) in the propensity score model (estimated via probit). Standard errors from 100 bootstrap repetitions. N = 13,534. See text for more details.

Table 2. Results: Children by Risk Type Entering Kindergarten

	Normal Weight Range			Overweight or Obese Entering Kindergarten		
	OLS/Probit (1)	School Fixed Effects (2)	Propensity Score Matching (3)	OLS/Probit (1)	School Fixed Effects (2)	Propensity Score Matching (3)
I. BMI: Growth Rates						
School	0.013*	0.014*	0.017*	0.006	0.001	0.012‡
Breakfast	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)
School	0.000	-0.004	0.001	0.000	0.000	-0.002
Lunch	(0.003)	(0.004)	(0.003)	(0.005)	(0.008)	(0.006)
II. Percentile BMI: Changes						
School	1.608†	2.096*	3.211*	-0.147	-0.482	0.227
Breakfast	(0.716)	(0.761)	(0.987)	(0.559)	(0.708)	(0.655)
School	-0.303	-0.800	0.042	-0.397	-0.589	-0.534
Lunch	(0.571)	(0.719)	(0.594)	(0.485)	(0.750)	(0.555)
III. Probability of Being Overweight						
School	0.124*	0.033*	0.032†	-0.046	-0.016	0.016
Breakfast	(0.042)	(0.012)	(0.015)	(0.065)	(0.024)	(0.021)
School	-0.011	-0.010	0.000	-0.016	-0.004	-0.012
Lunch	(0.035)	(0.012)	(0.010)	(0.057)	(0.025)	(0.018)
IV. Probability of Being Obese						
School	0.155†	0.022*	0.021†	0.032	0.011	0.101*
Breakfast	(0.061)	(0.007)	(0.009)	(0.058)	(0.025)	(0.026)
School	0.008	-0.004	0.001	0.027	0.003	-0.019
Lunch	(0.053)	(0.006)	(0.005)	(0.050)	(0.027)	(0.022)

NOTES: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors in parentheses. N = 10,039 (Normal) and 3,495 (Overweight or Obese). Specification (1) is identical to Specification (1) in Table 1; Specifications (2) and (3) are analagous to Specifications (3) and (4) in Table 1. Specification (2) in Panels III and IV is estimated using a linear probability model; in addition, these models exclude lagged values of the dependent variable in Model A since there is no variation by construction. See Table 1 for additional details.

Table 3. Selection into School Nutrition Programs

	Full Sample			Risk Type					
				Normal Weight Entering Kindergarten			Overweight or Obese Entering Kindergarten		
	OLS	School Fixed Effects	Propensity Score Matching	OLS	School Fixed Effects	Propensity Score Matching	OLS	School Fixed Effects	Propensity Score Matching
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
I. Weight (lbs.)									
School	0.038	0.053	0.371	0.069	0.065	-0.101	-0.089	0.098	1.034‡
Breakfast	(0.154)	(0.175)	(0.313)	(0.091)	(0.104)	(0.171)	(0.328)	(0.411)	(0.620)
School	0.134	0.147	0.003	0.056	0.014	0.108	0.242	-0.081	-0.298
Lunch	(0.125)	(0.170)	(0.210)	(0.073)	(0.098)	(0.137)	(0.284)	(0.437)	(0.526)
N		13534			10039			3495	
II. Weight (lbs.): Change in Levels									
School	0.067	0.084	0.402	0.098	0.096	-0.055	-0.074	0.103	1.166‡
Breakfast	(0.154)	(0.175)	(0.274)	(0.091)	(0.104)	(0.177)	(0.328)	(0.412)	(0.651)
School	0.137	0.151	0.011	0.058	0.022	0.110	0.196	-0.097	-0.339
Lunch	(0.125)	(0.171)	(0.180)	(0.073)	(0.099)	(0.105)	(0.284)	(0.437)	(0.486)
N		13534			10039			3495	
III. Weight (lbs.): Logs									
School	0.001	0.001	0.007	0.002	0.002	-0.002	-0.002	0.000	0.016
Breakfast	(0.003)	(0.003)	(0.006)	(0.002)	(0.002)	(0.004)	(0.005)	(0.007)	(0.012)
School	0.003	0.003	0.001	0.001	0.000	0.003	0.004	0.000	-0.004
Lunch	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.005)	(0.007)	(0.008)
N		13534			10039			3495	
IV. Weight (lbs.): Growth Rates									
School	0.011*	0.012*	0.021*	0.013*	0.014*	0.014	0.004	0.007	0.030‡
Breakfast	(0.004)	(0.004)	(0.009)	(0.004)	(0.004)	(0.009)	(0.007)	(0.008)	(0.016)
School	0.002	0.003	0.003	0.001	0.002	0.003	0.003	-0.001	-0.006
Lunch	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)	(0.006)	(0.006)	(0.009)	(0.011)
N		13534			10039			3495	

NOTES: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors in parentheses. Specification (1) is identical to Specification (1) in Table 1, except all terms involving child's birthweight are omitted in Panels II and IV; Specifications (2) and (3) are similarly analagous to Specifications (3) and (4) in Table 1. In addition, all regressions include controls for child's height in fall kindergarten (plus higher order and interaction terms). See Table 1 and text for details.

Table 4. Sensitivity Analysis: Bivariate Probit Results with Different Assumptions Concerning Correlation Among the Disturbances

	Correlation of the Disturbances											
	Specification (1)						Specification (2)					
	ρ = 0	ρ = 0.1	ρ = 0.2	ρ = 0.3	ρ = 0.4	ρ = 0.5	ρ = 0	ρ = 0.1	ρ = 0.2	ρ = 0.3	ρ = 0.4	ρ = 0.5
A. Probability of Being Overweight												
School	0.050	-0.117*	-0.284*	-0.449*	-0.614*	-0.778*	0.070†	-0.097*	-0.264*	-0.431*	-0.598*	-0.766*
Breakfast	(0.032)	(0.031)	(0.031)	(0.031)	(0.030)	(0.029)	(0.036)	(0.035)	(0.035)	(0.034)	(0.034)	(0.033)
School	-0.004	0.023	0.052†	0.082*	0.113*	0.145*	-0.013	0.015	0.044	0.074†	0.106*	0.139*
Lunch	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.030)	(0.030)	(0.029)	(0.029)	(0.029)	(0.029)
B. Probability of Being Obese												
School	0.055	-0.112*	-0.278*	-0.444*	-0.608*	-0.771*	0.064	-0.103†	-0.270*	-0.436*	-0.603*	-0.770*
Breakfast	(0.035)	(0.035)	(0.035)	(0.034)	(0.033)	(0.032)	(0.041)	(0.041)	(0.040)	(0.040)	(0.039)	(0.038)
School	0.015	0.044	0.075†	0.108*	0.144*	0.182*	0.032	0.061‡	0.092*	0.125*	0.160*	0.199*
Lunch	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)	(0.029)	(0.035)	(0.035)	(0.035)	(0.034)	(0.034)	(0.033)

NOTES: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors in parentheses. Specifications (1) and (2) refer to control sets used in Table 1. See Table 1 and text for details.

Table 5. Sensitivity Analysis: Amount of Selection on Unobservables Relative to Selection on Observables Required to Attribute the Entire SBP Effect to Selection Bias

	Specification (1)			Specification (2)		
	Cov(ε,ν)÷ Var(ν)	τ_1	Implied Ratio	Cov(ε,ν)÷ Var(ν)	τ_1	Implied Ratio
BMI: Levels	14.625	0.290	0.020	0.453	0.209	0.460
		(0.092)			(0.056)	
BMI: Logs	0.654	0.014	0.022	0.024	0.010	0.405
		(0.005)			(0.003)	
BMI: Growth Rates	0.490	0.010	0.021	0.175	0.010	0.057
		(0.003)			(0.003)	
Percentile BMI: Levels	77.583	2.114	0.027	4.465	1.478	0.331
		(0.714)			(0.510)	
Percentile BMI: Changes	57.727	1.009	0.017	7.718	1.475	0.191
		(0.548)			(0.510)	
Probability of Being Overweight	3.313	0.019	0.006	0.327	0.019	0.058
		(0.011)			(0.009)	
Probability of Being Obese	3.450	0.015	0.004	0.456	0.012	0.027
		(0.009)			(0.007)	

NOTES: Standard errors in parentheses. Specifications (1) and (2) refer to control sets used in Table 1, plus NSLP participation. Cov(ε,ν)/Var(ν) refers to the asymptotic bias of the unconstrained estimate under the assumption of equal (normalized) selection on observables and unobservables. τ_1 refers to the unconstrained estimate of the effect of SBP participation. The implied ratio is the latter divided by the former. See Table 1 and text for details.