Let Them Eat Lunch: The Impact of Universal Free Meals on Student Performance

Amy Ellen Schwartz
Michah W. Rothbart

Abstract

This paper investigates the impact of extending free school lunch to all students, regardless of income, on academic performance in New York City middle schools. Using a difference-in-differences design and unique longitudinal, student-level data, we derive credibly causal estimates of the impacts of "Universal Free Meals" (UFM) on test scores in English Language Arts (ELA) and mathematics, and participation in school lunch. We find UFM increases academic performance by as much as 0.083 standard deviations in math and 0.059 in ELA for non-poor students, with smaller, statistically significant effects of 0.032 and 0.027 standard deviations in math and ELA for poor students. Further, UFM increases participation in school lunch by roughly 11.0 percentage points for non-poor students and 5.4 percentage points for poor students. We then investigate the academic effects of school lunch participation per se, *using UFM as an instrumental variable. Results indicate that increases in school lunch participation improve academic performance for both poor and non-poor students; an additional lunch every two weeks increases test scores by roughly 0.08 standard deviations in math and 0.07 standard deviations in ELA. Finally, we explore potential unintended consequences for student weight outcomes, finding no evidence that UFM increases the probability that students are obese or overweight. We also find no evidence of increases in average body mass index (BMI). Instead, we find some evidence that participation in school lunch improves weight outcomes for non-poor students. Results are robust to an array of alternative specifications and assumptions about the sample.*

"Only one-third of public school students eligible for free or reduced-price lunch take part in the program. What is stopping them? Stigma."
■ David Sandman, President and CEO, New York State of Health Foundation

"What they're offering people is a full stomach and an empty soul."
■ Rep. Paul Ryan, 54th Speaker of the U.S. House of Representatives

"Free meals should go only to those students who are eligible for free meals, and reduced-price meals should go only to students eligible for reduced-price meals. Other students should be eligible for neither. This obvious and commonsense point has been lost."
■ Daren Bakst and Rachel Sheffield, Heritage Foundation

Journal of Policy Analysis and Management, Vol. 0, No. 0, 1–29 (2019)

Published by Wiley Periodicals, Inc. View this article online at wileyonlinelibrary.com/journal/pam
DOI:10.1002/pam.22175

INTRODUCTION

The National School Lunch Program (NSLP) is the second largest nutrition assistance program in the U.S., subsidizing over 30 million meals each school day at a Federal cost of $14 billion annually (U.S. Department of Agriculture Food and Nutrition Service, 2018). Traditionally, NSLP provides free or reduced-price meals for eligible low-income students. A growing number of schools (and districts) have adopted "Universal Free Meals" (UFM) providing free lunch and breakfast for all students, regardless of income. Advocates hope UFM will reduce the stigma that limits participation, address food insecurity, improve student readiness to learn, and reduce administrative burden. Skeptics worry about possible deleterious effects on weight either because of excess consumption or because the school lunch is less healthy (higher calorie) than the alternative. In a different vein, some worry about the administrative costs and potential budget pressures. Unfortunately, there is a dearth of credibly causal estimates of the impact of UFM on student outcomes, particularly important as UFM spreads across the country.[1] This paper begins to fill this gap, exploiting the plausibly exogenous timing of the adoption of UFM by New York City (NYC) public middle schools to estimate the impact of UFM on student outcomes, including participation in school meals, performance on standardized tests (English Language Arts [ELA] and Mathematics), attendance, and a set of weight outcomes (obesity, overweight, BMI).

Specifically, we use detailed student-level data to estimate the impact of UFM on attendance and test scores, using a difference-in-differences design with student (or school) fixed effects and a range of student and school control variables. We then estimate the impact of UFM on participation in school lunch using unique student-level data on lunch transactions in schools with Point of Service (POS) tracking systems. This is the first use of such data that we are aware of and a significant improvement over survey data typically used in previous research.[2] We then investigate the academic effects of school lunch participation *per se*, using UFM as an instrumental variable. Finally, we explore unintended consequences for student obesity. Thus, our analyses provide reduced form estimates of the impact of UFM on academic outcomes and school meal participation—particularly relevant for policymakers—and on the effect of school lunch *per se* on academic outcomes. The findings of this study will inform policymakers weighing the benefits and potential unintended consequences of expanding UFM.

We focus on middle school students for three reasons. First, middle school students are more likely than elementary school students to make autonomous decisions about lunch participation each day and are, therefore, more likely to be price sensitive. Second, NYC subsequently expanded UFM to all middle schools as part of a broader effort to address the difficulties of middle school-aged children (and then to all public schools in the 2017/2018 academic year). Third, POS data coverage is sufficiently broad in middle schools to allow us to estimate the impact on school lunch participation and consequences for student outcomes.[3]

[1] The Federal government made expansion of UFM a priority in the reauthorization of the NSLP in 2010 (Healthy Hunger Free Kids Act of 2010), creating a new UFM program designed to encourage more schools and districts to participate (e.g., the CEP, "Community Eligibility Provision").

[2] Virtually all previous policy research uses parent responses to surveys or aggregate school data to measure participation in the school meals programs. We are able to measure individual, daily participation, allowing us to assess participation rates prior to measurement of outcomes, such as participation before measurement of height and weight (used to calculate BMI).

[3] POS coverage is more limited in elementary schools (which are typically smaller and where stigma is likely to be less problematic) yielding smaller sample sizes and limited statistical power for impact

To preview our results, we find UFM increases academic performance by as much as 0.083 standard deviations in math and 0.059 in ELA for non-poor students, with smaller, statistically significant effects of 0.032 and 0.027 standard deviations in math and ELA for poor students.[4] Further, UFM increases participation in school lunch by roughly 11.0 percentage points for non-poor students and 5.4 percentage points for poor students. Instrumental variable results suggest increases in school lunch participation improve academic performance for both poor and non-poor students; an additional lunch every two weeks increases test scores by roughly 0.08 standard deviations in math and 0.07 standard deviations in ELA. Finally, we find no evidence that UFM increases the probability students are obese or overweight. We also find no evidence of increases in average BMI. Results are robust to an array of alternative samples and specifications. Implications for policymakers considering expanding (or contracting) universal free meals programs are clear: UFM can be an effective tool to improve student outcomes.

BACKGROUND ON NATIONAL SCHOOL MEALS PROGRAMS AND UNIVERSAL FREE MEALS

The national school meals programs (NSLP and the School Breakfast Program [SBP]) provide free and low-cost meals to tens of millions of children each day, in over 100,000 schools and childcare centers nationwide. The NSLP is the second largest food and nutrition assistance program in the United States, trailing only SNAP (Supplemental Nutrition Assistance Program). Together, the Federal government spends almost $18 billion a year on NSLP and SBP (about $14 billion for the NSLP and $4 billion for the SBP in 2017, compared to about $75 billion on SNAP) and the programs subsidize approximately 44 million meals a day (31.6 million for the NSLP in 2012 and 12.1 million for the SBP in 2011).[5]

According to the U.S. Department of Agriculture (USDA), the NSLP program "improves nutrition and focuses on reducing childhood obesity" (U.S. Department of Agriculture Office of Communications, 2010). Nutritious school meals may also serve a supportive function for education by providing food to ensure that children are not distracted by hunger during class (Bogden, Brizius, & Walker, 2012). That is, the programs serve multiple roles, including working to reduce child hunger and food insecurity, improving student health and well-being and, perhaps, getting kids ready to learn.[6]

Established by the National School Lunch Act of 1946, the NSLP subsidizes low cost or free lunches for over 30 million children every school day. Traditionally, in public schools, lunch and breakfast are provided *free* to students with household incomes up to 130 percent of the Federal poverty line and at a *reduced* price to

estimation. Unfortunately, students in grades 9 through 12 do not take a standardized ELA and a standardized math exam each year. Instead, New York State High school students must pass one Comprehensive English and one Mathematics Regents Exam to graduate, but schools and students choose the grade in which those exams are taken.

[4] In this paper, we define the poor as those individually certified as eligible for free or reduced-price lunch in any year in our data, including certification through returned lunch forms or direct certified participation in other means-test programs like SNAP. We define non-poor students as those never certified as eligible for free or reduced-price lunch in any year, which would include a small set of students with low family income who are not certified as eligible for free or reduced-price lunch in any year.

[5] U.S. Department of Agriculture, 2012, 2013; U.S. Department of Agriculture Food and Nutrition Service, 2018.

[6] In addition, the NSLP provides an avenue for surplus food distribution, serving as a Federal farm subsidy.

students with household incomes up to 185 percent.[7] Individual eligibility for subsidies through the national school meals programs is means-tested. Schools certify student eligibility using student-returned "lunch forms" or through "direct certification."[8] Federal regulations also provide schools and districts with the option of applying to implement UFM.[9] UFM eliminates all fees charged to students who participate in the school meals programs, making school lunch and breakfast free to all students regardless of their income.

UFM may increase meals participation through two key mechanisms. First, since UFM decreases the price of school meals for those students who are not eligible for free or reduced-price lunch to zero, it may increase participation through a *substitution* effect. Second, UFM may reduce *stigma* of participating in school lunch, which can, in principle, affect all students. Previous research has found significant stigma limits participation among poor students in traditional public schools (Sandman, 2016). UFM may reduce stigma both by eliminating the differences in form (or amount) of payment often visible in cafeteria transactions and, perhaps, by increasing the utilization rates of non-poor students.

Federal regulations allow schools and districts to implement UFM under Provision 2 of the National School Lunch Act (42 USC 1759a), subject to approval from a state agency.[10] Established in 1980, Provision 2 reduces the burden of tracking meals served, because Provision 2 schools only need to track the individual eligibility of meals participants once every four years. During the base year, a school establishes a reimbursement rate based on the percentage of meals served to students eligible for free, reduced-price, or full-price meals. Reimbursement rates in subsequent years are then determined by base year percentages, such that schools only have to count the total number of meals served per day and not track student eligibility. Following the four-year cycle, a state agency may approve continuation for another four years if the school provides evidence that student income levels have not risen substantially. By law, the school is responsible for the difference, but—to date—NYC has picked up this cost. Failure to comply with the regulations of Provision 2 or other components of the school meals programs puts schools at risk of losing Federal reimbursements for school meals. In this paper, we exploit variation in timing of the adoption of

[7] The thresholds for free and reduced-price meals rely on Federal poverty lines, which are not adjusted by region. Due to cost of living differences across the country, a substantial portion of non-poor students living in high-cost places such as NYC have real (regionally adjusted) incomes that would qualify them as free lunch-eligible in a lower-cost district. Thus, many of NYC's students not eligible for free or reduced-price meals are, in a sense, "near poor."

[8] Direct certification is a process of using municipal records on student participation in other means-tested poverty programs (such as eligibility for Temporary Assistance for Needy Families [TANF] or SNAP benefits) to certify individual student eligibility for subsidies.

[9] In order to continue receiving Federal reimbursements for meals served under UFM programs, schools must receive approval from their respective state agencies. Schools or districts must then pay the difference between Federal reimbursement and the full cost of providing school meals. See U.S. Department of Agriculture Food and Nutrition Service (2002).

[10] Since 1980, schools where at least 80 percent of enrolled children are eligible for free or reduced-price meals can also implement UFM under Provision 1. Since 1995, schools can also offer UFM under Provision 3, which sets reimbursement levels based on the average number of meals served by eligibility group in the most recent year in which the school tracked individual lunch utilization (rather than the average percentages by eligibility group, the method used under Provision 2). Under Provision 3, reimbursements are adjusted for inflation and enrollment, but not for changes in the number of meals served. Finally, since 2010, schools or districts can offer UFM under the Community Eligibility Provision (CEP) of the Child Nutrition Reauthorization Healthy, Hunger-Free Kids Act of 2010 (HHFKA), which requires 40 percent of the student body be eligible for subsidies through direct certification, verified through administrative records indicating student participation in SNAP or TANF. Under the CEP, reimbursement rates are based on the share of the school with direct certification (set as the free meal reimbursement rate times the share of students with direct certification, and then multiplied by a factor of 1.6).

school UFM status under Provision 2 of the National School Lunch Act, focusing on students ever exposed to UFM.

LITERATURE

There is limited and mixed evidence on the effects of school lunch (or breakfast) on academic achievement. In part, this reflects the scarcity of data with direct measures of individual school meal participation and of data linking participation to student performance and sociodemographics. Moreover, disentangling the effect of school lunch *per se* from the effects of poverty (or low income) on academic outcomes is complicated by the direct relationship between school lunch and poverty that arises from the use of household income to determine eligibility for school meals subsidies.

Perhaps not surprisingly, students who pay lower prices are more likely to participate in meals programs regularly (Akin et al., 1983). A handful of studies examine the effects of policies aimed at expanding access to school meals and find positive effects on education and achievement (Frisvold, 2015; Hinrichs, 2010; Imberman & Kugler, 2014).[11] Others that target increasing nutritional and caloric content of meals also see improved test scores (Anderson, Gallagher, & Ritchie, 2017; Figlio & Winicki, 2005).[12] Other studies find little effect of policies aimed at expanding access to school meals and increasing caloric content on test scores and mixed evidence on attendance (Leos-Urbel et al., 2013; McEwan, 2013).[13] The effects may differ across studies for a number of reasons, including measurement error in parent surveys, omitted variables correlated with poverty and achievement, or differences in the policies and populations studied. Finally, heterogeneity in impacts may arise due to differences in the school or neighborhood context—depending, for example, on the cost and availability of alternative meal options.

Turning to the effects of school lunch on student health, two key studies find that participation increases childhood obesity (Millimet, Husain, & Tchernis, 2010; Schanzenbach, 2009). Others find evidence that expanding the availability of and eligibility for school meals improves health outcomes (Bhattacharya, Currie, & Haider, 2006; Gundersen, Kreider, & Pepper, 2012). Still others find policies that increased access to school meals have no effect on obesity (Corcoran, Elbel, & Schwartz, 2016). Finally, Smith (2017) finds heterogeneity in the impact of school food on diet quality, with improvements for students with greater nutritional needs and no positive effects (and, if anything, negative) for students with lesser nutritional needs.

[11] Imberman and Kugler (2014) study the impact of a free in-class breakfast program in a large urban school district, finding improved achievement, particularly among schools with high shares of students with low pre-program achievement and who qualified for free lunch. Frisvold (2015) studies the impact of policies that mandate school participation in the School Breakfast Program among schools with high shares of students eligible for free meals, finding the availability of the SBP increases National Assessment of Educational Progress (NAEP) test scores. Hinrichs (2010) exploits changes to the NSLP reimbursement funding formulas that make reimbursement more generous, arguing that these funding changes may increase meals participation either by increasing the number of participating schools or by lower lunch prices. Hinrichs (2010) finds more generous reimbursement formulas increase number of years of completed education.

[12] Anderson et al. (2017) study the effect of schools in California contracting with healthier lunch vendors, finding student test scores increase, particularly among students eligible for free or reduced-price lunches. Figlio and Winicki (2005) study school menus during testing periods, finding school districts in Virginia facing potential sanctions increase the caloric content of meals at a greater rate than others during the week of exams and providing some suggestive evidence this practice improves test scores.

[13] Leos-Urbel et al. (2013) examine the impact of universal free breakfast in NYC, finding increases in breakfast participation, but no detectible effect on test scores and only small attendance gains for some racial/ethnic subgroups. McEwan (2013) estimates the impact of providing higher calorie school meals in rural Chile, finding no impact on test scores.

The evidence on the impact of UFM *per se* is even more limited. One notable exception, Kitchen et al. (2013), examined a pilot universal free meals program in the United Kingdom (the UFSM program). Estimated effects were largely positive; nearly 90 percent of pupils took up free school meals, increasing participation both among those students who would otherwise be eligible for meal subsidies and among those otherwise ineligible. Further, UFSM shifted consumption from foods associated with packed lunches to those associated with hot meals. It also improved academic attainment, especially among poor students and those with lower prior attainment. The UFSM pilot program, however, did not have significant effects on attendance, parent reports of student behavior or BMI, obesity, and other health outcomes.

A related literature has focused on the effects of universal free breakfast. Previous research that is most similar in context and setting is Leos-Urbel et al. (2013), which focuses on the effects of NYC's universal free breakfast program. Using school-level data on participation, the authors find NYC's implementation of universal free breakfast increased participation for all students, with the largest increases among those who would not have been eligible for free meals otherwise. However, they find little evidence of an impact on academic outcomes (either test scores or attendance). We build on that work, which uses only school-level data for a two-year sample period, creating concerns about power and minimal detectible effects that are noted by the authors. First, we exploit student-level data on participation and student outcomes to increase sample size and precision. Second, we use a four-year panel period, further increasing power and also allowing us to more rigorously test the assumptions of difference-in-differences models, including the parallel trends assumption. Third, we examine the consequences of UFM programs, instead of the effects of only universal breakfast, which may have very different effects since more students participate in lunch than breakfast. Fourth, we are the first large-scale study to our knowledge to use direct measures of individual student participation in school meals as opposed to school-level measures or parent responses to surveys.

Others also find small-to-moderate positive effects of universal free breakfast programs on academic achievement (Crawford et al., 2016), but are also not able to explore individual participation. Again, there is reason to believe school breakfast is different than school lunch. Student participation in the SBP is much lower than the NSLP (Bartfeld & Kim, 2010). Impacts of UFM programs may vary with levels of utilization, perhaps with larger effects at low levels of participation (or larger effects for one type of meal versus the other). Moreover, impacts may depend upon which students are induced to increase their participation and the relevant alternatives to school meals (which may differ between lunch and breakfast).

It is worth noting that price of school meals is not the only consideration for students deciding whether or not to participate. School meals might be unappealing due to poor preparation, stringent nutritional standards, taste or the stigma associated with meal participation (Glantz et al., 1994; Gleason, 1995; Mirtcheva & Powell, 2009; Poppendieck, 2010). Student participation reflects family resources and budget constraints, preferences over alternatives including brown bag lunches from home, purchased lunches from restaurants or stores outside school, vending machines in schools, and so on. Thus, school lunch participation may be unresponsive to price changes. Whether, and to what extent, UFM increases school meals participation is an empirical question that we address in this paper.

This study uses new, richly detailed data on individual-level, daily participation and the timing of price changes (adoption of UFM) to estimate the impact on student academic outcomes. The richness of the data allows us to identify the impact of the policy change on participation, the effects on student outcomes, and heterogeneity in these effects across student subgroups. This is the first study to our knowledge that estimates the effects of school meals participation on academic achievement and obesity using direct, individual student measures of school meals participation.

UFM IN NYC

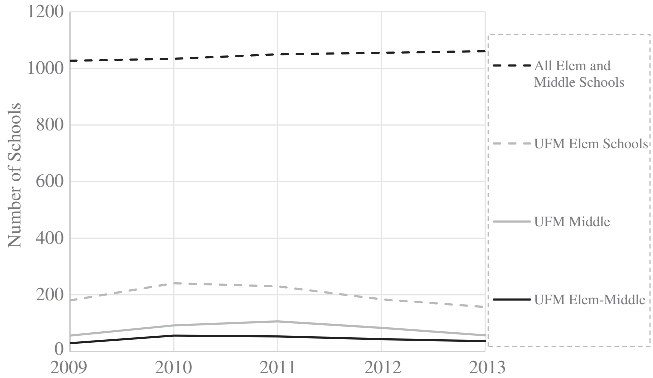
NYC provides a unique opportunity to study UFM. The largest school district in the country, NYC public schools enroll over 1.1 million students in more than 1,500 public schools annually. This includes over 200,000 students in the middle school grades, 6 through 8, and more than 500 schools serving them. Critical to this study, more than 400 NYC public schools implemented UFM under Provision 2, and schools across the city operate under a set of common regulations and procedures—including standardized menus across the district—reducing the potential for bias due to changes in the nutritional value of school meals (or other factors) concurrent with the adoption of UFM.

While schools in NYC may differ in their preferences for UFM, whether and when those preferences translate into adoption of UFM reflects the interplay of myriad political, institutional, and administrative factors, which makes the *precise timing* of UFM adoption by a particular school plausibly exogenous. In particular, the application process for Provision 2 in NYC involves a number of administrative steps that vary idiosyncratically in time to approval. NYC schools are free to apply for UFM when they choose, but the order of approval is not solely based on timing of initial application or even application completion. According to the NYC Department of Education (NYCDOE) Office of School Food, it can take more than a year for a school to gain approval, during which any number of items could delay the process. These items include, for example, increased staff workloads, staff turnover, budget considerations, changes in student composition, and a variety of other institutional factors. NYCDOE Office of School Food officials suggest that the approval process is not strategic or targeted, and both adoption and approval are not a result of political considerations. Similarly, it would be difficult for families to choose UFM schools due to the ambiguity of timing of approval or the processes to get approval. Moreover, it is unlikely that—among all school characteristics—a family chooses a school based on UFM status. As a result of these idiosyncratic processes, it is fair to consider UFM status within each NYC public school (or changes in exposure to UFM within student, over time) as plausibly exogenous. Thus, our NYC setting offers the opportunity to gain insight into the efficacy of UFM and school lunch programs in practice, on a large scale and in a large urban school district.

The number of NYC schools that offer UFM has varied year to year since 2009 but has not grown or declined steadily over time (Figure 1). About half of schools participated in a UFM program for at least one year from 2010 to 2013. UFM use expanded in the 2010 and 2011 school years and contracted in 2012 and 2013.[14] UFM schools may return to standard procedures for counting meals and meal reimbursement at any point; alternatively, UFM schools may request a four-year extension of the program at the end of their UFM cycle as long as there are only "negligible" changes in the share of students with direct certification (U.S. Department of Agriculture Food and Nutrition Service, 2002). While the number of schools offering UFM under Provision 2 varied over time, the eligibility criteria did not.[15]

[14] In part, this may be related to availability of American Recovery and Reinvestment Act (ARRA) funds. The number of UFM schools in NYC expanded considerably again in the years following our sample period, including adoption in all NYC public schools for the 2017/2018 academic year.

[15] Moreover, we found no documentation related to implementation of Provision 2 of the National School Lunch Act (from the USDA, the NYCDOE, or otherwise) that suggest priority is given to applicant schools



Notes: UFM schools provide free meals to all students, regardless of individual student eligibility. "Elem" indicates that a school serves fourth graders, "Middle" indicates that it serves seventh graders, and "Elem-Middle" indicates that a school serves both fourth and seventh graders. All Elem and Middle Schools reflects the sum of all UFM and non-UFM Elementary, Middle, and Elementary-Middle schools.

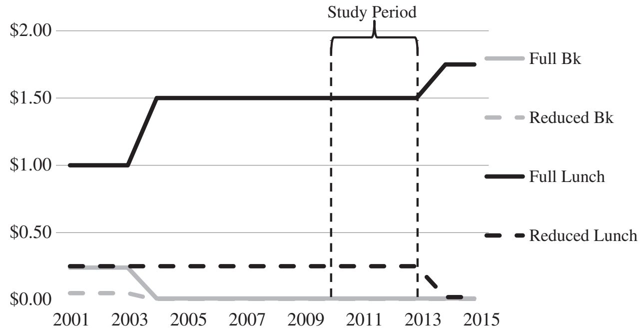
Figure 1. Number of UFM Schools by Year, 2009 to 2013.

Why might some NYC schools adopt UFM and others not? According to the USDA, "a school considering Provision 2 must evaluate whether the savings in administrative costs associated with reducing application burdens and simplifying meal counting and claiming procedures under Provision 2 offset the costs of providing meals to all children at no charge" (U.S. Department of Agriculture Food and Nutrition Service, 2002).[16] Thus, it is ultimately a choice made by school administrators in conjunction with the City school district and subject to Federal eligibility. As noted previously, NYCDOE covers gaps in the direct costs of food services for Provision 2 schools in NYC. Still, even with NYC covering much of the financial costs of UFM, there are substantial administrative costs to applying for UFM and getting reimbursed from NYC. As such, schools will only do this if the perceived benefits outweigh the transaction costs, one reason we limit the sample to ever UFM students.

In addition to schools offering UFM under Provision 2, NYCDOE has taken steps to increase access to school meals citywide, including price and menu changes (Perlman et al., 2012). In 2004, the NYCDOE implemented universal free breakfast—eliminating the 25-cent price for full-price students and the five-cent price for reduced-price students (Leos-Urbel et al., 2013). A decade later, in September 2014, the NYCDOE extended UFM status to all freestanding middle schools. Figure 2 shows citywide prices for full- and reduced-price meals in the period 2002 to 2015.

that serve more (or less) disadvantaged student populations, have better (or worse) history of academic performance, higher (or lower) school lunch and breakfast participation rates, or other selection criteria that might confound model estimates. Multiple meetings with the NYCDOE Office of School Food suggest that they also have not established informal criteria based on these sorts of considerations. We test these claims empirically by first testing the extent to which the characteristics of the school's student body in year t predict the future adoption of UFM in $t+1$ and, second, by examining the relationship between future UFM status in $t+1$ with current student characteristics and outcomes. The empirical results are consistent with the results of our review of the policy documentation and our meetings with those implementing the policy.

[16] Moreover, informal conversations with administrators and advocates suggest that school leaders are sometimes concerned that offering UFM could also increase the risk of losing funding for other Federal and state aid programs that rely on measures of students individually eligible for free and reduced-price meals.



Notes: Prices in NYC Public Schools for breakfast (BK) and lunch, academic years 2001 to 2015. Meals provided *free* of charge for students at or below 130 percent of the Federal poverty line, at a *reduced* price for those at or below 185 percent of the Federal poverty line and at *full* price for all other students. In Academic Year (AY) 2015, UFM extended to all freestanding middle schools (grades 6 to 8 and 5 to 8 schools only). Point of Service (POS) data are available for a subset of schools in academic years 2010 to 2013, which is our study period. Breakfast is free for all students in our study period. UFM only affects lunch prices during this period.

Figure 2. New York City Public School Meal Prices by Year, 2001 to 2015.

As shown, citywide prices for school meals in non-UFM schools are stable in the 2010 to 2013 period. We focus our study on this stable period in an effort to isolate the effect of UFM from other price effects.

DATA AND MEASURES

Our analysis draws on rich longitudinal student- and school-level data, for all NYC public elementary and middle school students and student-transaction-level data on meal participation for a large subset of students. We focus on 2010 to 2013, to take advantage of data availability and the stability of meal prices in NYC.

Student data include sociodemographic characteristics such as gender, race/ethnicity, primary language spoken at home, English proficiency, birth country, certified eligibility for free or reduced-price lunch, participation in special education, attendance, scores on ELA and math exams for grades 3 through 8, and student height and weight.[17] Student-level data also include measures of participation in school lunch and school breakfast for students attending a school collecting such data.[18] Importantly, every student record includes a unique student identifier allowing us to follow individual students over time. We exclude students in full-time special education settings and those with less than two years of test score data to facilitate the estimation of the student fixed effects models, described in greater detail below.[19] Further, we restrict the sample to students with height and weight data, although we relax this constraint in the robustness test described below.

[17] Beginning in 2006, NYC public schools have collected annual measures of height, weight, and physical fitness of almost every student as part of the Fitnessgram initiative. By 2012/2013, the Fitnessgram covered roughly 875,000 students in 1,650 schools citywide.

[18] These new data are collected by NYC Office of School Food using an electronic Point-of-Service (POS) tracking system to record meal transactions with student ID and time stamps.

[19] The use of student fixed effects means that students with only a single observation will not contribute to the estimation, and the impact of UFM will be identified by the UFM Switchers. As a robustness check to our test score results, we re-estimate our models without excluding students missing weight data; results from these models are substantively unchanged and are available upon request from the authors.

We transform test scores into z-scores using grade-by-year specific means and variances, *zMath and zELA* for math and English language arts, respectively. Weight outcomes include Body Mass Index (BMI), measured as z-scores (normalized by grade-year), *zBMI*, or as a natural logarithm, *ln(BMI)*, and indicator variables for *overweight*, *obese*, and *underweight*, which we create using age- and sex-specific growth charts from the Centers for Disease Control and Prevention.

Finally, we construct a time-invariant measure of poverty, *Poor*, that takes a value of one if a student is certified as eligible for free or reduced-priced lunch in any year between 2001 and 2013, and zero otherwise. *Poor* is a more inclusive measure of economic disadvantage than contemporaneous certified eligibility and is protective against potential under-reporting of individual eligibility for school meals subsidies among UFM students. *Nonpoor* identifies students never observed as certified eligible for free or reduced-price lunch during this period; that is, *Poor* and *Nonpoor* are mutually exclusive.[20] Schools certify student eligibility for free or reduced-priced lunch using either submitted free and reduced-price meal applications or a system known as direct certification. During our sample period, NYC became heavily reliant on direct certification, which uses municipal records on household participation in other means-tested poverty programs (such as participation in Temporary Assistance for Needy Families [TANF] or SNAP) to certify individual student eligibility for free lunch.[21] Still, out of an abundance of caution, we use the time-invariant indicator, *Poor* (instead of an annual measure of certified eligibility for free or reduced-price lunch typical in education research) to differentiate effects between those that are "never" and "ever" certified eligible individually (2001 to 2013).[22,23] As a result of our efforts, about 90 percent of *Poor* students in our sample are certified eligible each year they attend a non-UFM school (90 percent of observations in 2010, 87 percent in 2011, 84 percent in 2012, 89 percent in 2013), and the share of students who are *Poor* is statistically indistinguishable between "Always UFM" students and "UFM Switchers" (Table 1).

Annual school data include indicators for UFM status, enrollment, grades served, mean student characteristics, test scores and attendance rates, the number of breakfasts and lunches served, and the number of students in each eligibility

[20] It is important to note that non-poor students in NYC public schools are not typically well-to-do. Although many NYC residents are quite rich by national standards, many of the richest eschew public schooling for their children. Indeed, more than 15 percent of NYC school-aged children attend parochial or private schools. Instead, many of the "non-poor" students are of modest means, with family income barely exceeding 185 percent of the federal poverty line. The federal poverty line is not regionally adjusted for cost-of-living differences. The high cost of living in NYC means that a substantial portion of NYC non-poor students live in poverty once accounting for regional differences. For most of these students, a move to a lower cost-of-living district and a concomitant reduction in family income to adjust for the lower cost of living would give them an income low enough that they would become eligible for free lunch.

[21] NYC began utilizing direct certification in 2006, in part because it reduces reliance on school lunch forms, reducing paperwork for schools and families, and reducing the likelihood that UFM—or other policies—will affect measured poverty rates used in administering other programs, such as Title 1. To be clear, direct certification misses poor students from households not enrolled in other means-tested programs but who might be eligible for free or reduced-price lunch, including foreign-born students who may be unaware of their eligibility or the "near poor." Our results are robust to restricting the sample to native-born students, and are available from the authors upon request.

[22] We test the sensitivity of our results to an alternative poverty measure, "Poor2," which takes a value of one if the student is certified eligible for free or reduced-price lunch in a year in which they are not exposed to UFM, and zero otherwise. The sample is slightly smaller, but results are substantively unchanged. Results are available from the authors upon request.

[23] Note that Michelmore and Dynarski (2017) find that Michigan students who were ever certified eligible for free or reduced-price lunch ("*Poor*") perform substantially worse on academic achievement tests than those who never are ("*Nonpoor*"), even when assessing the "effects" of future certified eligibility.

Table 1. Descriptive statistics, middle school students only, by UFM and POS status, 2010 to 2013.

	(1) All	(2) Ever UFM	(3) Always UFM	(4) UFM Switchers	(5) Ever UFM/POS
Percentage: Female	50.3	50.5	50.1	50.7	50.0
White	15.0	12.1	10.9	12.7	13.7
Asian	17.4	19.5	18.0	20.3	19.7
Black	28.1	25.8	25.3	26.1	26.5
Hispanic	39.5	42.6	45.6	40.8	40.1
Poor	90.0	92.4	91.9	92.7	90.7
Foreign Born	16.5	17.5	19.5	16.4	17.7
No English at Home	57.0	52.1	50.5	53.1	54.7
LEP	9.6	10.0	11.2	9.3	8.8
Special Ed.	11.6	11.2	11.2	11.2	10.3
Mean: SLP	N/A	N/A	N/A	N/A	62.2
SBP	N/A	N/A	N/A	N/A	11.3
N	645,204	318,637	117,633	201,004	89,566

Notes: Sample includes observations of students in grades 6 to 8 with at least two years of test scores and weight outcome data. Ever UFM students are either Always UFM or UFM Switchers.

group. Table 1 presents descriptive statistics for key variables, beginning with the sample of "All" general education middle school students (with two or more years of test scores and weight data) to provide context. As shown, NYC students are predominantly poor, with 90.0 percent eligible for free or reduced-price lunch in at least one year between 2001 and 2013, and predominantly minority—only 15.0 percent are White. Hispanics represent almost 40 percent, with 28.5 percent Black and 17.4 percent Asian. Further, roughly one-sixth of all students are foreign born, more than half speak a language other than English at home, and almost 10 percent qualify as Limited English Proficient (LEP). Finally, 11.6 percent of these students qualify for part-time special education services.

We restrict our regression sample to students who attended a UFM school at some point between 2010 and 2013. This "Ever UFM" sample includes 318,637 observations of 155,496 students in grades 6 to 8.[24] Ever UFM students are, unsurprisingly, different from "All" students, which includes students never attending a UFM school. As shown in Table 1, column 2, Ever UFM students are disproportionately Asian, Hispanic, Poor and foreign born, compared to All students. Among Ever UFM, students exposed to UFM every year (Always UFM, column 3) and those exposed to UFM in some years but not others (UFM Switchers, column 4) are quite similar.[25] Of the 201,004 observations on 100,194 UFM Switchers, 71,971 are students who switch UFM status due to changing schools (e.g., graduating from a school without UFM and entering a school with UFM).[26]

A second sample includes only observations with lunch and breakfast participation data—limiting our sample to students attending one of the schools using

[24] Regression models also include students in grades 3 to 5. The "Ever UFM" sample for grades 3 to 8 has 659,797 observations on 222,456 students attending 1,103 schools.
[25] Moreover, among Ever UFM students, differences between students currently exposed to UFM and those not currently exposed are not statistically significant.
[26] 37,829 students switch UFM status due to changes in school UFM policy, of which 9,982 switch status at least twice (once by switching schools and once due to attending a school that switches status).

an electronic Point of Service (POS) system to track meal transactions. Our "Ever UFM/POS" sample is, then, a subset of the Ever UFM sample and, for middle school grades, includes 89,566 observations of 39,229 students in 153 middle schools.[27] We measure school lunch participation (SLP) as the number of lunch transactions divided by the number of school days in the year. School breakfast participation (SBP) is defined similarly. As shown in column 5 of Table 1, the mean school lunch participation rate (SLP) is 62.2 percent in Ever UFM middle schools and the mean school breakfast participation rate (SBP) is 11.3 percent.[28]

EMPIRICAL STRATEGY

Baseline Model

We exploit changes in student exposure to UFM over time in order to estimate the impact of UFM on academic achievement, school lunch participation, and weight outcomes. To do so, we estimate a student fixed effects, difference-in-differences specification of a model linking student outcomes to UFM status and time varying student variables:

$$Y_{igst} = \beta_0 + \beta_1 UFM\_Middle_{igst} + \beta_2 UFM\_Elem_{igst} + \boldsymbol{X'}_{\boldsymbol{igst}}\beta_3 + \gamma_{gt} + \delta_i + \varepsilon_{igst} \qquad (1)$$

where Y_{igst} is a vector of variables reflecting outcomes for student i, in grade g, attending school s, in year t, including test scores (*zMath* and *zELA*), attendance, and weight (*zBMI*, *ln(BMI)* and indicators for *overweight*, *obese*, and *underweight*). UFM_Middle_{igst} is the interaction between an indicator variable $Middle_{igst}$ (which takes a value of one if student i is in grades 6 through 8) and UFM_{st} (which takes a value of one if student i attends a UFM school in year t).[29] UFM_Elem_{igst} is the interaction between $Elem_{igst}$ (which takes a value of one if student i is in grades 3 through 5) and UFM_{st}.[30] $\boldsymbol{X_{igst}}$ is a vector of other student characteristics including those indicating LEP, and special education needs;[31] γ_{gt} is a grade-by-year fixed effect and δ_i is a student fixed effect. We cluster standard errors by school because UFM is a school-level intervention and students are clustered in schools.[32] Our coefficient of interest is β_1, which captures the impact of UFM on student outcomes in middle school. Notice that equation (1) can also be viewed as the reduced-form equation in an instrumental variables model linking academic outcomes to participation in school lunch.

Here, estimated coefficients will capture causal effects if the precise timing of the exposure of the student to UFM is exogenous, including if the timing within student is uncorrelated with unobserved concurrent changes in policies, practices,

[27] The regression sample also includes 33,119 observations of 16,149 students in grades 3 to 5, including 80 elementary schools (typically enrolling fewer than 100 Ever UFM/POS students) and 36 schools serving both elementary and middle grades.
[28] We do not observe plate waste, etc., or any other direct measure of consumption. Future work examining the effect of UFM on consumption *per se* would be warranted.
[29] We omit $Middle_{ist}$ because it is perfectly collinear with the grade-by-year fixed effect.
[30] Again, regressions include students in elementary grades to increase precision of our middle school estimates, but regression results for *UFM_Elem* are suppressed. Elementary school regression results are largely insignificant, reflecting smaller sample size due to limited POS coverage in elementary schools. Results are available upon request.
[31] As a robustness check, we substitute school fixed effects for student fixed effects. In these alternative specifications, the vector X_{ist} also includes variables indicating if a student is black, Hispanic, Asian, female, poor, foreign born, and speaks a language other than English at home.
[32] See, for example, Cameron and Miller (2015).

and characteristics of the school.[33] As noted earlier, institutional and practical considerations and exploratory empirical work suggest conditional exogeneity is both plausible and likely. We show results from empirical tests of the validity of the assumption below.

Heterogeneity by Student Poverty Status

We explore heterogeneity in impacts by student poverty status by estimating separate coefficients for poor and non-poor students. We do so by fully interacting model variables with *Poor* and *Nonpoor*, respectively.[34] Again, we focus on the impacts for middle school students identified by within-student changes in access to UFM over time.

Impact on School Meals Participation

We then turn to impacts on SLP, which is the key mechanism for UFM impacts on academic outcomes. Importantly, breakfast is already free for all NYC students at the beginning of the study and, thus, only lunch prices change after UFM.[35] We use the same difference-in-differences strategy using the sample of POS students:

$$\begin{aligned} SLP_{igst} = {} & \beta_0 + \beta_1 UFM\_Middle_{igst} * Poor_i + \beta_2 UFM\_Middle_{igst} * NonPoor_i \\ & + \beta_3 UFM\_Elem_{igst} * Poor_i + \beta_4 UFM\_Elem_{igst} * NonPoor_i \\ & + \boldsymbol{X'}_{\boldsymbol{igst}} \beta_5 + \gamma_{gt} + \delta_i + \varepsilon_{igst} \end{aligned} \tag{2}$$

where SLP_{igst} captures SLP for student i, in grade g, in school s, in year t, and all other variables as defined previously. We estimate impacts on SLP (and SBP) by student poverty status and grade level, as described above.

Notice that equation (2) can also be viewed as the first-stage model of a two-stage least squares regression model linking academic outcomes to participation in school lunch. If UFM does, indeed, affect participation in school lunch, then it may be an effective instrumental variable for school lunch participation in subsequent models.

IV Models: The Effects of School Lunch Participation

We then estimate the effect of *SLP* on student outcomes, using UFM as an instrument to address potential endogeneity of *SLP* due to unobserved time-varying differences in student characteristics such as income, motivation, or engagement between those

[33] Estimated impacts from student fixed effects models are identified by within student changes in UFM status. We note that within student variation in UFM status largely reflects changing schools in the Ever UFM sample (69 percent of UFM Switchers change UFM status upon changing schools, 40 percent switch due to their schools changing UFM status, and 9 percent experience both). In the Ever UFM-POS sample, however, we observe substantially fewer students switching schools because observing a switch requires that students move from one POS school to another. Thus, in the Ever UFM-POS sample, we observe only 11 percent of the students switching schools and the within-student variation in UFM status is driven by schools adopting (or giving up) UFM. (Only 24 percent of UFM Switchers change UFM status at the time of changing schools.) The estimated impact of UFM for students switching schools and schools switching policies are not statistically different from one another and the pooled estimates shown in this paper. Additional estimates are available upon request.

[34] These models include interactions between poverty status (poor / non-poor) and grade level (middle / elementary).

[35] In addition to estimating impacts of UFM on lunch participation, we show the estimated effect of UFM on breakfast participation using the same model. Null results for SBP are necessary for the identifying assumptions of the IV model outlined below.

who utilize school lunch and those who do not. Again, we estimate separate effects for poor and non-poor students:

$$\begin{aligned} Y_{ist} = {} & \beta_0 + \beta_1 SLP\_\widehat{Middle} * Poor_{igst} + \beta_2 SLP\_\widehat{Middle * Nonpoor}_{igst} \\ & + \beta_1 SLP\_\widehat{Elem} * Poor_{igst} + \beta_2 SLP\_\widehat{Elem * Nonpoor}_{igst} \\ & + \boldsymbol{X'}_{\boldsymbol{igst}}\beta_5 + \gamma_{gt} + \delta_i + \varepsilon_{igst}. \end{aligned} \tag{3}$$

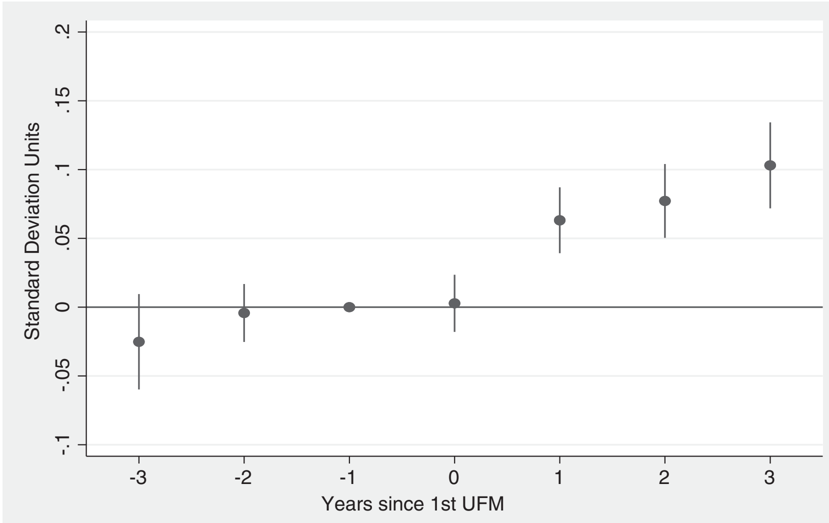
To be clear, we use the four variables created by fully interacting UFM with poverty status and grade level as instruments for *SLP* (also fully interacted with poverty status and grade level).[36] All other variables are as defined previously. Thus, we estimate the effect of a one percentage point increase in *SLP* on student outcomes. We again cluster standard errors at the school level.[37]

Parallel Trend Test

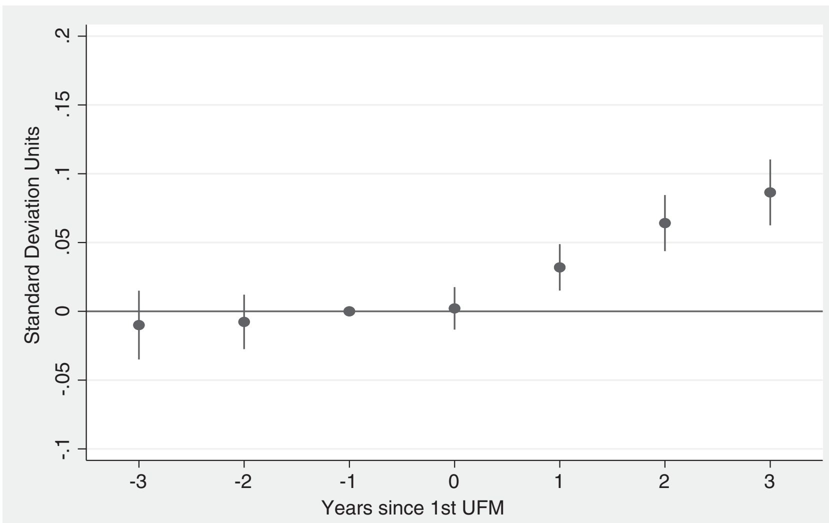
We assess the parallel trend assumption using an event study framework to test whether the pre-treatment trend is not statistically different from zero. While we do not have sufficient data on SLP and weight outcomes to meaningfully explore trends prior to first UFM exposure, we are able to do so for ELA and math test scores.[38] We extend our Ever UFM panel to include data for the same students in the prior three academic years. (That is, we add data on student characteristics, test scores, and UFM status for 2007 to 2009.) We then measure the difference between time *t* (the current academic year) and the first year a student is exposed to UFM (using enrollment records and school UFM status going back to 2004), and construct a vector of indicator variables, *UFM Year*, to capture the years before and after first UFM exposure. We then estimate the relationship between ELA (and Math) scores and the vector of *UFM Year* variables, omitting the year just prior to first UFM exposure as the reference year.[39] The results, in Figure 3, provide little evidence of changes in test scores prior to UFM treatment. The coefficients in the years before UFM are not statistically different from one another or from zero. None of the results suggest substantive changes to test scores prior to UFM exposure, thus supporting a causal interpretation of our impact estimates.[40]

[36] Including the impact of UFM on SLP for poor middle, non-poor middle, poor elementary, and non-poor elementary students. Again, elementary coefficients are suppressed but are available upon request from the authors.
[37] In the IV fixed effects model, we cluster by the sequence of schools attended, instead of the school attended in each year, so that students switching schools are assigned to a single cluster representing the pair of schools attended and not two different clusters, one for each school attended.
[38] We test whether SLP and weight outcomes are related to UFM adoption and phase-out at the *school level* below and find no evidence of such a relationship. Results are shown in the Appendix. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[39] We use the following model: $Y_{igst} = \beta_0 + \boldsymbol{UFM\_Year'}_{\boldsymbol{igst}}\beta_1 + \boldsymbol{X'}_{\boldsymbol{igst}}\beta_2 + \alpha_g + \zeta_t + \delta_i + \varepsilon_{igst}$, where variables are as previously defined and α_g and ζ_t, are grade and academic year fixed effects, respectively. By construction, all of these students are exposed to UFM at some point in later grades. As a result, in a model using student fixed effects, we are unable to identify grade-by-year fixed effects in years prior to the main analytic sample period. In order to pick up the nonparametric UFM-year effects, we can only include grade and year fixed effects instead of the interactions of the two. Our difference-in-differences results are robust to including grade and year fixed effects in lieu of grade-by-year fixed effects.
[40] We observe few test scores for students four or more years before or after first UFM exposure, because students only take exams in six grades (grades 3 to 8) and we only observe seven years of data (2007 to 2013). Thus, observations four or more years away from first UFM treatment are more likely to reflect outcomes for students who have been retained. We report the coefficients for "4+" years prior to and after UFM exposure in Table A1, which also provides no evidence of trends prior to first UFM exposure.

Panel A: zMath



Panel B: zELA



Notes: Figures display point estimates and 95 percent confidence intervals derived from an event study of Ever UFM students in 3rd to 8th grade in years 2007 to 2013. Models control for student limited English proficiency, special education status, and student grade and year fixed effects. Zero (0) is the year of first UFM exposure. Negative 1 (−1) is the reference category. Results for four or more (4+) years before or after first UFM treatment are suppressed. Numerical estimates are reported in Table A1. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Figure 3. Event Study Depicting Effect of First UFM Exposure on Test Scores, 2007 to 2013.

Table 2. Estimated impacts of UFM on academic outcomes, 2010 to 2013.

	Ever UFM			Ever UFM/POS		
VARIABLES	(1) zMath	(2) zELA	(3) Attd_rate	(4) zMath	(5) zELA	(6) Attd_rate
UFM Middle	0.036***	0.030***	−0.038	0.049	0.043**	0.127
	(0.014)	(0.011)	(0.070)	(0.031)	(0.018)	(0.165)
Student Char.	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y	Y
Observations	659,797	659,797	659,797	122,685	122,685	122,685
Students	222,456	222,456	222,456	47,887	47,887	47,887
Schools	1,103	1,103	1,103	233	233	233
R-squared	0.837	0.806	0.812	0.866	0.832	0.828

Notes: Robust standard errors are in parentheses and are clustered by school (*p < .10; **p<.05; ***p<.01). Columns 1 through 3 include students who attend a UFM school at least one year from 2010 to 2013. Columns 4 through 6 include a subset of Ever UFM students with POS data. Samples include observations of students in third to eighth grade with at least two years of test scores and weight outcome data. Results for students in grades 3 through 5 are suppressed. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services, student fixed effects, and grade-by-year fixed effects.

RESULTS

Impacts on Test Scores

Table 2 shows the impact estimates from our preferred model with student and grade-by-year fixed effects as well as controls for time-varying student characteristics. Columns 1 to 3 show results for the Ever UFM sample and columns 4 to 6 for the Ever UFM sample with POS (meal participation) data. As shown, among the EVER UFM students, UFM increases math and ELA scores for students in grades 6 to 8 by 0.036 and 0.030 standard deviations, respectively. We find no significant effects, however, on attendance rates. Coefficients estimated using the Ever UFM-POS sample (in columns 4 to 6), are somewhat larger in magnitude, although standard errors increase, and only ELA results are statistically significant. Again, we find no effect on attendance. In sum, our core results indicate UFM significantly increases math and ELA test scores by between 0.030 and 0.043 standard deviations with no effect on attendance.

Our next analyses, shown in Table 3, allow the impact of UFM to differ for poor and non-poor students. As before, models reported in columns 1 to 3 are estimated using the Ever UFM sample and columns 4 to 6 using the Ever UFM with POS sample, and we only show the middle school results. UFM increases student performance for both poor and non-poor students in both ELA and math. The Ever UFM sample results for poor students are quite similar to the findings for the overall sample—coefficients on math and ELA are 0.032 and 0.027, respectively—but effects for non-poor students are more than double. UFM increases math and ELA scores by 0.083 and 0.059 standard deviations, respectively. Furthermore, the differences in impacts between poor and non-poor students are significantly different.

Again, restricting the sample to students with meal participation (POS) data yield substantively similar results. Point estimates for the poor are a bit larger (0.048 and 0.042 for math and ELA, respectively), for the non-poor a bit smaller (0.061 and 0.055), and standard errors larger, leaving only the ELA coefficient for the poor

Table 3. Estimated impacts of UFM on academic outcomes by poverty, 2010 to 2013.

	Ever UFM			Ever UFM/POS		
VARIABLES	(1) zMath	(2) zELA	(3) Attd Rate	(4) zMath	(5) zELA	(6) Attd Rate
UFM Middle						
Poor	0.032**	0.027**	−0.029	0.048	0.042**	0.153
	(0.014)	(0.011)	(0.073)	(0.029)	(0.018)	(0.173)
Non-Poor	0.083***	0.059***	−0.124*	0.061	0.055	−0.067
	(0.025)	(0.021)	(0.071)	(0.063)	(0.041)	(0.166)
Student Char.	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y	Y
Observations	659,797	659,797	659,797	122,685	122,685	122,685
Students	222,456	222,456	222,456	47,887	47,887	47,887
Schools	1,103	1,103	1,103	233	233	233
R-squared	0.837	0.806	0.812	0.866	0.832	0.829

Notes: Robust standard errors are in parentheses and are clustered by school (*p < .10; **p<.05; ***p<.01). Columns 1 through 3 include students who attend a UFM school at least one year from 2010 to 2013. Columns 4 through 6 include a subset of Ever UFM students with POS data. Sample includes observations of students in third to eighth grade with at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 through 5 are suppressed. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services, student fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status.

statistically significant at conventional levels. As a result, we are unable to reject the hypothesis that impacts are the same for the poor and non-poor in this sample.[41,42]

Here, attendance results are a bit more mixed. In the Ever UFM models, estimates indicate a small negative effect on attendance, a statistically significant 0.124 percentage point decrease (one-fifth of a school day annually, on average) among non-poor students. Coefficients for the more limited sample, however, are insignificant and becomes positive for the poor. None are large enough to be considered substantively meaningful.

In summary, Table 3 provides evidence of positive effects of UFM on academic performance for both groups, with larger effects for the non-poor (0.05 to 0.08 standard deviations)—for whom UFM changed the price of lunch—than the poor (0.03 to 0.05 standard deviations). These are substantively meaningful improvements in test scores—on average, roughly 10 to 12 percent of the black-white test score gap in this sample, with even bigger effects for non-poor students. Our results for non-poor students are similar in magnitude to studies of related interventions. Imberman and Kugler (2014) find that providing free breakfast in classroom rather than in the

[41] We test the sensitivity of our results to an alternative poverty measure, "Poor2," which takes a value of one if the student is certified eligible for free or reduced-price lunch in a year in which they are not exposed to UFM, and zero otherwise. The sample is slightly smaller, but results are substantively unchanged. Results are available from the authors upon request.

[42] We test the sensitivity of our results to possible endogeneity due to students switching schools by excluding all students who ever change schools during our sample period. Results are robust and not statistically different from the point estimates in our preferred model—with the exception of the effect of UFM on SLP, which is somewhat larger—and standard errors are somewhat larger with the reduced sample.

cafeteria raises math and reading test scores by 0.09 and 0.06 standard deviations, respectively. Similarly, Frisvold (2015) finds that universal free breakfast increases math and reading test scores by 0.08 and 0.05 standard deviations, respectively. Further, in the education policy literature, a common rule of thumb is that a 0.1 standard deviation improvement in test scores in one subject is small, but indicative of a successful intervention (Bloom et al., 2006). Our UFM effect on test scores is nearly that large, particularly for non-poor students. Moreover, UFM improves performance in *two subjects*—math and ELA—rather than one, while targeted educational interventions typically improve performance in only one subject.

Finally, to give a sense of the potential magnitude of the effects, we draw on Hill et al. (2008) to translate our effect sizes into a "weeks of learning" metric. To be clear, these "back of the envelope" estimates are best viewed as crude and their accuracy depends upon whether, and to what extent, the properties of the NYC context (e.g., tests) match those of the national context of the Hill study.[43] That said, applying the Hill et al. parameters to our estimated coefficients suggests UFM would improve performance in math by as much as seven to 10 weeks of learning and by as much as six to nine weeks of learning in ELA for non-poor students. For poor students, effects are roughly three to four weeks of learning in math and ELA.[44]

Impact on School Meals Participation

Table 4 presents the results of the models estimating the impact of UFM on SLP. These are, perforce, estimated using the sample of students with POS data only. As shown in column 1, we estimate that attending a UFM school increases school lunch participation for both poor (5.395 percentage points) and non-poor students (10.974), relatively large compared to their lunch participation rate of 63.96 and 45.55, respectively.

Column 2 shows our estimates of the impact of UFM on school breakfast participation, investigating a possible substitution effect. We find no effect of UFM on SBP. Coefficients are negative but small compared to the standard error and not approaching significance at conventional levels.

IV Results: The Effects of School Lunch Participation

As shown in column 1 of Table 4, our SLP models suggest UFM may be a useful instrument for SLP; UFM status by grade level and poverty is highly predictive of SLP.[45] Table 5 shows our IV estimates of the impact of SLP on academic outcomes. As described earlier, these models use UFM as an instrument for SLP, which

[43] Assumptions include: (1) mean academic growth for middle school students in NYC is the same as the national average; (2) a standard deviation in test scores for two pooled grades and nationally normed as in Hill et al. (2008) reflects the same variation in achievement as a standard deviation in test scores normed within one grade in NYC; (3) test scores reflect learning during the 36 weeks of the school year not differential learning loss over the summer.

[44] Mechanically, we divide the point estimates of the impact of UFM presented in Table 3 by the mean annual gain in effect sizes presented in Hill et al. (2008) between the springs of grades 5 and 6, between 6 and 7, and between 7 and 8 (mean annual effect size gains for math are 0.41, 0.30, and 0.32, respectively, and for reading/ELA are 0.32, 0.23, and 0.26, respectively). We then multiply the quotient by the number of school weeks in an academic year, 36. We present the range of results across the three middle school grades rounded to the nearest week.

[45] Specifically, we use four instruments (*UFM_Middle*Poor*, *UFM_Middle*Nonpoor*, *UFM_Elem*Poor*, *UFM_Elem*Nonpoor*) to address potential endogeneity of four regressors (*SLP_Middle*Poor*, *SLP_Middle*Nonpoor*, *SLP_Elem*Poor*, *SLP_Elem*Nonpoor*). The joint significance (F-statistics) of the excluded instruments (i.e., *UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) are 44.50, 22.36, 16.06, and 22.97 in the first stage models for *SLP_Middle*Poor,*

Table 4. Estimated impacts of UFM on school meal participation by poverty, 2010 to 2013.

VARIABLES	(1) SLP	(2) SBP
UFM Middle		
Poor	5.395***	−1.956
	(1.389)	(1.566)
Non-Poor	10.975**	−1.104
	(4.389)	(3.196)
Student Char.	Y	Y
Student FE	Y	Y
Grade*Year FE	Y	Y
Observations	122,685	122,685
Students	47,887	47,887
Schools	233	233
R-squared	0.826	0.744

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grade with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 through 5 are suppressed. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services, student fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status.

is potentially endogenous, allowing us to estimate a causal effect of increasing participation in school lunch itself. The results indicate participation in school lunch increases performance on both ELA and math for both poor and non-poor middle school students. More specifically, a one percentage point increase in SLP increases math scores by 0.008 standard deviations for poor students and 0.006 for the non-poor. ELA results are similar at 0.007 and 0.006 for poor and non-poor, respectively. To give a sense of the magnitudes, these suggest math scores will rise by 8 percent of a standard deviation if school lunch participation increases by one lunch every two weeks (about a 10 percentage-point increase) for poor students and by 6 percent of a standard deviation for non-poor students. (For ELA, these will be 7 percent and 6 percent, respectively.) Thus, while UFM has a larger effect for non-poor than poor students, the IV estimates suggest that increasing SLP improves test scores for both types of students at about the same rate (and, perhaps, at a greater rate for poor students).[46] Finally, we find no effect of SLP on attendance, as before.

Unintended Consequences for Student Weight Outcomes

Lastly, we explore possible unintended consequences for student weight outcomes. Reduced form estimates of the impact of UFM on student BMI and other weight outcomes, shown in Table 6, provide no evidence of deleterious effects on weight outcomes for poor or non-poor students in middle school. Out of five models

*SLP_Middle*Nonpoor, SLP_Elem*Poor, SLP_Elem*Nonpoor*, respectively. First stage results are available from the authors upon request.

Table 5. Estimated impacts of SLP on academic outcomes by poverty, IV Model, 2010 to 2013.

VARIABLES	(1) zMath	(2) zELA	(3) Attd_rate
SLP Middle			
Poor	0.008**	0.007***	0.026
	(0.003)	(0.002)	(0.019)
Non-Poor	0.006**	0.006***	0.007
	(0.003)	(0.002)	(0.011)
Student Char.	Y	Y	Y
Student FE	Y	Y	Y
Grade*Year FE	Y	Y	Y
Observations	121,402	121,402	121,402
Students	46,604	46,604	46,604
Clusters	2,465	2,465	2,465

Notes: Robust standard errors in are parentheses and clustered by sequence of schools attended (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 through 5 are suppressed. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services, student fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status. We instrument for student SLP using UFM status, differentiating between student grade level and poverty status. We use four instruments (*UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) to address endogeneity of four regressors (*SLP_Middle*Poor, SLP_Middle*Nonpoor, SLP_Elem*Poor, SLP_Elem*Nonpoor*, respectively). F-statistics of the excluded instruments in the first stage regressions (i.e., the joint significance of *UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) are as follows: 44.50, 22.36, 16.06, and 22.97, respectively. 1,283 singletons are dropped from this analysis.

Table 6. Estimated impacts of UFM on weight outcomes by poverty, 2010 to 2013.

VARIABLES	(1) zBMI	(2) ln(BMI)	(3) Overwgt	(4) Obese	(5) Undrwgt
UFM Middle					
Poor	−0.003	−0.000	−0.001	−0.003	−0.000
	(0.016)	(0.003)	(0.008)	(0.006)	(0.003)
Non-Poor	−0.040	−0.010	−0.004	−0.025***	0.009
	(0.031)	(0.008)	(0.012)	(0.009)	(0.011)
Student Char.	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y
Observations	122,685	122,685	122,685	122,685	122,685
Students	47,887	47,887	47,887	47,887	47,887
Schools	233	233	233	233	233
R-squared	0.904	0.904	0.820	0.819	0.639

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 to 5 are suppressed. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services, student fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status.

Table 7. Estimated impacts of SLP on weight outcomes by poverty, IV Model, 2010 to 2013.

VARIABLES	(1) zBMI	(2) ln(BMI)	(3) Overwgt	(4) Obese	(5) Undrwgt
SLP Middle					
Poor	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)
Non-Poor	−0.002	−0.001	−0.000	−0.001**	0.001
	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)
Student Char.	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y
Observations	121,402	121,402	121,402	121,402	121,402
Students	46,604	46,604	46,604	46,604	46,604
Clusters	2,465	2,465	2,465	2,465	2,465

Notes: Robust standard errors are in parentheses and clustered by sequence of schools attended (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 to 5 are suppressed. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services, student fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status. We instrument for student SLP using UFM status, differentiating between student grade level and poverty status. We use four instruments (*UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) to address endogeneity of four regressors (*SLP_Middle*Poor, SLP_Middle*Nonpoor, SLP_Elem*Poor, SLP_Elem*Nonpoor*, respectively). F-statistics of the excluded instruments in the first stage regressions (i.e., the joint significance of *UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) are as follows: 44.50, 22.36, 16.06, and 22.97, respectively. 1,283 singletons are dropped from this analysis.

estimating impacts on BMI, measured both as a z-score and with the natural logarithm, overweight, obese, or underweight, nine of 10 estimated coefficients have negative signs, but only one—a 2.5 percentage point decrease in the probability of being obese for non-poor students—is statistically significant. While other results are insignificant, point estimates for non-poor are larger than for poor. For example, coefficients in the BMI model presented in column 1 show 0.040 versus 0.003 standard deviation reductions (and column 2 show 0.010 percent vs. 0.000 percent reductions) for non-poor versus poor students, respectively.

Estimates from the IV models (shown in Table 7) are, again, largely negative and insignificant. Only one coefficient is statistically significant (column 4): A one percentage point increase in SLP decreases the probability that a non-poor student is obese by 0.1 percentage point. This is substantively meaningful; one additional school lunch every two school weeks decreases the probability that non-poor students are obese by one percentage point. Coefficients are larger but insignificant for the non-poor.[47]

[46] To be clear, while the point estimates are different for the poor and non-poor, the statistical significance of the differences between these varies. Confidence intervals overlap in some cases. Future work will investigate the sensitivity of these differences and their statistical significance.

[47] Results from school fixed effects models, which serve as a robustness check, also suggest that there is no effect of UFM on student weight outcomes (Table A3). Tables A4 and A5 show that the first and second stage estimates from the two-stage least squares IV models with school FE are similar in magnitude to those shown in Tables 4, 5, and 7. Again, however, coefficients are more precisely estimated in models with student FE. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 8. Estimated impacts of SLP before Fitnessgram date on weight outcomes, IV Model, 2010 to 2013.

VARIABLES	(1) zBMI	(2) ln(BMI)	(3) Overwgt	(4) Obese	(5) Undrwgt
SLP-Middle					
Poor	−0.004	−0.001	−0.001	−0.001	0.000
	(0.004)	(0.001)	(0.002)	(0.002)	(0.001)
Non-Poor	−0.005	−0.001*	−0.001	−0.002*	0.001
	(0.003)	(0.001)	(0.002)	(0.001)	(0.001)
Student Char.	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y
Observations	86,257	86,257	86,257	86,257	86,257
Students	34,616	34,616	34,616	34,616	34,616
School Paths	2,047	2,047	2,047	2,047	2,047

Notes: Robust standard errors are in parentheses and clustered by sequence of schools attended (*p<.10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data, Fitnessgram dates after September, and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 to 5 are suppressed. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services, student fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status. We instrument for student SLP using UFM status, differentiating between student grade level and poverty status. We use four instruments (*UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) to address endogeneity of four regressors (*SLP_Middle*Poor, SLP_Middle*Nonpoor, SLP_Elem*Poor, SLP_Elem*Nonpoor*, respectively). F-statistics of the excluded instruments in the first stage regressions (i.e., the joint significance of *UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) are as follows: 30.36, 15.56, 9.90, and 20.89, respectively. 7,619 singletons are dropped from this analysis.

That said, a majority of height and weight assessments occur in the fall, meaning we capture very short-run effects here. More importantly, perhaps, is that the SLP measure used above captures participation over the entire academic year, much of which will be after the Fitnessgram assessment, suggesting measurement error and potential attenuation bias.[48] To address this, we use the daily meal participation data and student-specific Fitnessgram dates (month/year) to construct a measure of SLP in the month(s) *prior to* the Fitnessgram assessment. As an example, our “pre-Fitnessgram SLP” will capture September and October participation for students with November height and weight measurements.[49]

Estimates from models with this more accurate measurement of pre-Fitnessgram SLP, shown in Table 8, suggest participation in school lunch *improves* weight outcomes for middle school students, particularly the non-poor. Coefficients are larger in magnitude and more precisely estimated than those estimated using annual SLP in Table 7 (consistent with reducing measurement error). Specifically, we find that

[48] Unlike ELA and Math exams, which occur toward the end of the spring semester.
[49] By construction, observations of students who have Fitnessgram assessments in September (28,793 observations) are excluded from this analysis since there are no academic months before September. In additional sensitivity analyses, we include both the months prior to and including the month of Fitnessgram assessment in construction of the “pre-Fitnessgram SLP” variable. Thus, for this example, we measure SLP using September, October, and November participation for students who have their heights and weights measured in November. In these sensitivity analyses, observations with September Fitnessgram assessments are included. Results are consistent with those shown here and are available upon request.

a one percentage point increase in SLP reduces BMI by 0.1 percent (column 2) and the probability of being obese by 0.2 percentage points (column 4) for the non-poor. As before, estimates suggest SLP decreases student weight as well, though the differences between the poor and non-poor are smaller in these preferred estimates. Taken together, then, our results suggest SLP does not have deleterious effects on weight outcomes and provide some support for the hypothesis that SLP improves weight outcomes.

Our results differ markedly from Schanzenbach's (2009), who finds poor students participating in school lunch are more likely to be obese. There are many possible explanations for the difference. We note three here. First, we study middle school students while Schanzenbach (2009) focuses on early elementary grades. Differences in our findings may reflect differences in metabolic processes for these ages or, perhaps more convincingly, differences in reliance on school lunch versus alternatives (brown bag or fast food) between middle and elementary school students. Second, the quality of NYC school meals may have been better than the average school in the ECLS-K data used by Schanzenbach. (NYCDOE suggests that they made substantial efforts to improve school lunch menu quality.) Third, the NYC food environment outside school may be less healthy; that is, there may be differences in access to and types of alternative lunches. We leave the exploration of these possibilities for future work.

To summarize, we find UFM induces a large increase in meal take-up rate (measured as SLP); that SLP increases both for poor students who would otherwise be eligible for meal subsidies and for non-poor students who would be ineligible in the absence of UFM; that UFM improves academic achievement, but has no significant effects on attendance; that UFM and SLP have smaller effects on BMI and obesity, and that the evidence there suggests somewhat improved weight outcomes. All of these findings mirror the results of the pilot UFSM program evaluation in the United Kingdom (Kitchen et al., 2013).

PROBING THE RESULTS

Robustness Checks

We explore the robustness of our results first by estimating models using school fixed effects, μ_s, instead of student fixed effects, δ_i. While student fixed effects models (above) are identified by the variation in UFM exposure within students over time—due both to students switching into/out of schools with UFM and schools adopting/removing UFM—school fixed effects models rely on the changes in schools' UFM status only. Again, standard errors are clustered at the school level.

Results from school fixed effects models are shown in Tables A2 through A5.[50] Results are consistent with the student fixed effects results, though coefficients are generally less precisely estimated. Thus, our results are robust to alternative identification assumptions, encouraging confidence in the causal interpretation of our impact estimates.

We test the robustness of the test score models by expanding the student sample to include those students previously excluded because of missing height and weight data. The new sample includes Ever UFM students with at least two years of test score data, even if they have missing height or weight data. Test score results are robust to this more inclusive sample. Results are similar in direction, magnitude,

[50] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

and statistical significance for both Ever UFM and Ever UFM/POS students. These results are available upon request.

In addition, we explore potential heterogeneity of results for weight outcomes by gender and do not find substantively different results for boys and girls. Mirroring results in Tables 6 and 7, coefficients are substantively small and provide no evidence of deleterious effects on weight. Again, results are available upon request.

Do Observables Predict UFM?

A key condition underlying a causal interpretation of our estimates is that the timing of the adoption of UFM is plausibly exogenous. We explore the plausibility of this assumption by examining the extent to which the characteristics of the *school's* student body in year *t* predict the adoption of UFM in *t+1*. To be specific, using *school level* data for 2006 to 2013, we estimate a model linking student characteristics in *t* to UFM status in the following year.[51] We restrict the sample to Ever UFM *schools* for which UFM_{st} takes a value of zero in time *t* (that is, schools not offering UFM in *t*) and estimate:

$$UFM_{st+1} = \beta_0 + \boldsymbol{X}'_{\boldsymbol{st}}\beta_1 + \gamma_t + \mu_s + \varepsilon_{st} \tag{4}$$

where all variables are school-level aggregates of student variables previously defined.[52] The coefficients, β_1, capture the extent to which student characteristics predict UFM status in the following year, and provide suggestive evidence on the plausibility of the assumption that UFM adoption is exogenous at the school level. Significant coefficients would suggest selection bias (or endogeneity problems); insignificant coefficients suggest a causal interpretation may be warranted.

We show school level results in column 1 of Table A6. We find little evidence that school characteristics in *t* predict UFM adoption in *t+1*, providing support for the hypothesis that the precise timing of UFM adoption can be treated as exogenous to the school and bolstering the case for a causal interpretation of the results.[53]

We perform a similar exercise to assess the endogeneity of the phase-out of UFM. Here we re-estimate equation (4) restricting the sample to Ever UFM schools that do offer UFM in *t*, (that is, UFM_{st} takes a value of one). We then estimate the probability of *Removal*, which takes a value of one if the school drops UFM, and zero otherwise. In this way, the model sheds light on the extent to which school characteristics in *t* predict UFM phase-out in the following year (*t+1*). As shown in column 2 of Table A6, the results suggest school characteristics do not, in fact, predict UFM phase-out, providing support for the view that the timing of phase-out is exogenous to the school and bolstering, again, the case for a causal interpretation of the results. Taken together, these analyses show neither adoption nor phase-out

[51] We also conduct this analysis focusing on just the 2010 to 2013 sample period, also finding statistically insignificant results, but with much larger standard errors due to the limited sample size for school-level analyses. We find the null results from the longer panel even more convincing due to the imprecision of the estimates from school-level models using the shorter panel period. Results from the short panel are available upon request.

[52] We aggregate variables as follows: SLP and SBP are mean school-level participation rates; obese, overweight, underweight, female, Asian, Hispanic, Black, immigrant, LEP, SPED are shares of students with each characteristic, and Math – 3, Math – 4, ELA – 3, and ELA – 4 are shares of students with achievement levels 3 or 4 on the statewide math and ELA exams, respectively.

[53] Not a single coefficient among the model's 16 variables is statistically significant at the 10 percent level.

of UFM is predicted by observables, boosting confidence in the causal interpretation of our impact estimates.[54]

We then repeat the above tests using the student POS data. Table A7 shows that we also find little evidence that new UFM exposure or loss of UFM is predicted by current year student characteristics, including participation in school breakfast or lunch, weight, test scores, or program participation. Thus, neither new exposure nor loss of UFM is predicted by observables, again boosting confidence in the causal interpretation of our impact estimates.

Falsification Tests

In a set of falsification tests, we examine the sensitivity of our results to substituting future UFM status for current UFM status to test whether our estimates reflect changes in outcomes that predate UFM and precipitating the adoption of UFM, rather than the impact of the policy itself. In particular, we examine the relationship between future UFM status (UFM_{ist+1}) and 12 student outcomes (test scores, attendance, weight, SLP, SBP, etc.) in year t. First, we use the full sample of Ever UFM POS students. Second, we use only the subset of students without UFM in year t (that is, $UFM_{ist} = 0$). The intuition is straightforward. If the link between UFM and student outcomes is causal, future UFM should have no impact on current outcomes and coefficients will be insignificant. In contrast, significant coefficients on UFM in these models would suggest our impact estimates might be biased by selection into or out of UFM.

Results are shown in Table A8. Panel A shows results for the full sample and panel B for the $UFM_{ist} = 0$ subset. The results are encouraging. None of the 24 estimated coefficients on UFM_{ist+1} are statistically significant. That is, there is no evidence that future UFM predicts current outcomes, bolstering confidence in the causal interpretation of our results.

Finally, we estimate 10 of the 12 falsification models for the full set of Ever UFM students—relaxing the requirement that the student has POS data. Since some of these students will not have SLP and SBP data, we only estimate the other models. Again, all coefficients on future UFM are insignificant.[55]

CONCLUSIONS

Advocates argue UFM will deliver a variety of benefits: reduce stigma that limits participation, reduce food insecurity for needy students, improve student readiness to learn, and reduce administrative burden. Critics are skeptical, charging that UFM may increase obesity and costs for schools. The dearth of evidence hampers decisionmaking. NYCDOE, for example, after much debate, extended UFM to all public school students and would benefit from better understanding of the consequences such a policy is likely to have on students' academic achievement and weight. This paper begins to fill that gap, providing credibly causal estimates of the impacts of UFM on student academic and weight outcomes. Moreover, our unique data allow us to exploit adoption of UFM to contribute new credibly causal evidence on the effect of school lunch on student outcomes. In particular, we use longitudinal, student-level data on participation in school breakfast and lunch for a large sample of students to investigate the effect of UFM on SLP and, subsequently, the impact of

[54] Across the two models with 16 coefficients each (32 coefficients in all), only three coefficients are statistically significant at the 10 percent level, which is what one would expect from random chance alone.
[55] These results are available from the authors upon request.

SLP on student outcomes. This work informs the national debate over the benefits of school meals programs.

Our findings point to a positive effect of UFM on the test scores of middle school students—both poor and non-poor—with the largest increases for non-poor students. We find UFM increases participation in school lunch, and the increases in participation induced by UFM improve student performance on both ELA and math exams—again, for both poor and non-poor. These results are more conclusive than previous work by Leos-Urbel et al. (2013), who studied the effects of a universal breakfast program in NYC using school-level data over a two-year sample period. We improve on this work, lengthening the sample period, increasing the sample size, and using measures of student-level participation. Further, we find larger increases in participation for lunch (from UFM) than Leos-Urbel et al. found for breakfast, perhaps explaining larger reduced form effects on student outcomes. The effects of SLP itself also may differ from the effects of SBP for a number of reasons. First, it could be that school lunches provide more nutrition than school breakfasts, because USDA requirements for the calories provided in school breakfasts are about two-thirds of that required for lunches;[56] there is evidence that increased caloric content can improve academic outcomes (Anderson, Gallagher, & Ritchie, 2017; Figlio & Winicki, 2005). Alternatives to school breakfast may also differ from alternatives to school lunch, such as eating at home versus bringing a meal from home. In addition, offering meals during the school day may differ from meals offered before the bell (Corcoran, Elbel, & Schwartz, 2016). Taken together, in addition to improving upon previous research on free breakfast programs, our findings suggest that future research should continue to explore differences in the impact of UFM programs by meal type, differentiating between breakfast and lunch.

Findings for the non-poor suggest price matters for this group of students whose families have household incomes exceeding 185 percent of the Federal poverty line. Further, the positive impacts on test scores for this group suggest that even students who are not certified eligible for free or reduced-price meals may face budget or nutritional constraints that limit academic performance (at least in high-cost cities like NYC). Findings for the poor—who largely would experience no direct change in price—suggest that stigma plays a role in participation decisions as well. As for unintended consequences, we see no evidence that the reduction in the price of school lunch leads to a decrease in participation in school breakfast due, perhaps, to a substitution effect. (Breakfast was already free in NYC public schools.)

Finally, we find no evidence that UFM or school lunch participation itself increases student weight, or the incidence of obesity, overweight, or even underweight. Instead, the preponderance of negative, but largely insignificant, coefficients on obesity, overweight, and BMI models suggest possible improvements in obesity and weight outcomes due to UFM and SLP. Indeed, our preferred IV models, which focus on the impact of SLP prior to assessment of weight and height, suggest SLP may reduce obesity and BMI. Further research is needed to identify the contexts and conditions under which UFM and school meals affect student health, and to explore heterogeneity across socioeconomically and demographically different students. We are particularly interested in heterogeneity of impacts across subgroups defined by race/ethnicity, immigrant status, and urbanicity and food environment around the school. It is possible, for example, that the impact depends upon the alternatives to school meals, which may depend upon the school food environment, availability of fast food, or family resources. Impacts may also depend on school food policies

[56] According to the USDA Food and Nutrition Service (2012), school breakfasts must provide 350 to 500 calories in elementary school grades and 400 to 550 calories in middle school grades. School lunches must have 550 to 650 calories for elementary and 600 to 700 calories for middle (USDA FNS, 2012).

such as open campus or "out-lunch" policies, or the characteristics of cafeterias and menus. We plan to return to these questions in future research, exploiting additional years of data as UFM expands, and collecting new data on the neighborhood food environment, school food policies, etc.

We note that our paper focuses on the impacts of UFM policies adopted under Provision 2. These UFM policies affect the prices paid by students the same as district-wide UFM policies. That said, it is plausible that a district-wide UFM policy may have a different effect than the school-wide UFM policies offered under Provision 2. This is also worthy of future research.

We can also compare the assessed benefits of UFM to the costs of providing free meals to all students. The NYC Independent Budget Office (2017) estimates that expanding UFM to all 400,000 NYC elementary school students would increase school lunch costs by $13.5 million if there were no effect on SLP. If SLP increased by 10 percent (about the magnitude of our estimated effects on SLP), it would cost an additional $5 million. This amounts to roughly $50 per student per year. If costs for middle school students are similar, then UFM is a bargain. Indeed, UFM promises to be extraordinarily cost effective—generating increases in math and ELA test scores up to a tenth of a standard deviation for about $50 per pupil. Bottom line, the evidence from NYC suggests UFM is an inexpensive and effective way to improve academic achievement among urban school children. Perhaps, contrary to Bakst and Sheffield (2016), UFM might turn out to be an "obvious and commonsense" investment. District and school leaders nationwide might do well to consider adopting the UFM program.

AMY ELLEN SCHWARTZ is the Daniel Patrick Moynihan Chair in Public Affairs and Professor of Economics and Public Administration and International Affairs at The Maxwell School of Syracuse University, 426 Eggers Hall, Syracuse, NY 13244 (e-mail: amyschwartz@syr.edu).

MICHAH W. ROTHBART is an Assistant Professor of Public Administration and International Affairs at The Maxwell School of Syracuse University, 426 Eggers Hall, Syracuse, NY 13244 (e-mail: mwrothba@syr.edu).

ACKNOWLEDGMENTS

We gratefully acknowledge funding from the National Institutes of Health (R01 grant: 5R01DK097347-02) and the Tufts/University of Connecticut Research Innovation and Development Grants in Economics Center (Tufts/UConn RIDGE Center), underwritten by the U.S. Department of Agriculture. We thank the NYC Department of Education for providing data and for their support, especially the Office of School Food, Dennis Barrett, Stephen O'Brien, and Armando Taddei. For data support, advice, and suggestions, we also thank Meryle Weinstein, Emily Gutierrez, Zac McDermott, Siddhartha Aneja, Michele Leardo, Sarah Cordes, Joanna Bailey, and Brian Elbel. We thank seminar participants at the University of Connecticut, the University of Virginia, the University of Hong Kong, the Andrew Young School at Georgia State University, the Maxwell School at Syracuse University, Federal Reserve Bank of Cleveland, Federal Reserve Bank of New York, and participants at the APPAM, AEFP, and NTA conferences for useful comments on previous drafts. The opinions expressed are those of the authors and do not represent views of the National Institutes of Health, the U.S. Department of Agriculture, or the NYC Department of Education.

REFERENCES

Akin, J., Guilkey, D., Popkin, B., & Wyckoff, J. (1983). The demand for school lunches: An analysis of individual participation in the School Lunch Program. The Journal of Human Resources, 18, 213–230.

American Recovery and Reinvestment Act of 2009 (ARRA). (2009). Public Law 111–5.

Anderson, M. L., Gallagher, J., & Ritchie, E. R. (2017). School lunch quality and academic performance. NBER Working Paper No. 23218. Cambridge, MA: National Bureau of Economic Research.

Bakst, D., & Sheffield, R. (2016, September 6). Op-Ed; Do the wealthy really need free school meals? The Hill. Available at http://thehill.com/blogs/pundits-blog/education/294551-do-the-wealthy-really-need-free-school-meals.

Bartfeld, J., & Kim, M. (2010). Participation in the School Breakfast Program: New evidence from the ECLS-K. Social Service Review, 84, 541–562.

Bhattacharya, J., Currie, J., & Haider S. (2006). Breakfast of champions? The School Breakfast Program and the nutrition of children and families. Journal of Human Resources, 41, 445–466.

Bloom, H., Hill, C., Black, A. R., & Lipsey, M. (2006). Effect sizes in education research: What they are, what they mean, and why they're important. Institute of Education Sciences, 2006 Research Conference. Washington, DC.

Bogden, J. F., Brizius, M., & Walker, E. M. (2012). Chapter E. Policies to Promote Healthy Eating in National Association of State Boards of Education. Fit, Healthy, and Ready to Learn (Chapter E). Arlington, VA: National Association of State Boards of Education.

Cameron, A. C., & Miller, D. L. (2015). A Practitioner's Guide to Cluster-Robust Inference. The Journal of Human Resources, 50, 317–372.

Corcoran, S. P., Elbel, B., & Schwartz, A. E. (2016). The effect of breakfast in the classroom on obesity and academic performance: Evidence from New York City. Journal of Policy Analysis and Management, 35, 509–532.

Crawford, C., Edwards, A., Farquharson, C., Greaves, E., Trevelyan, G., Wallace, E., & White, C. (2016). Magic Breakfast: Evaluation report and executive summary. Education Endowment Foundation. Available at https://educationendowmentfoundation.org.uk/public/files/Projects/Evaluation_Reports/EEF_Project_Report_Magic_Breakfast.pdf.

Figlio, D. N., & Winicki, J. (2005). Food for thought: The effects of school accountability plans on school nutrition. Journal of Public Economics, 89, 381–394.

Frisvold, D. (2015). Nutrition and cognitive achievement: An evaluation of the School Breakfast Program. Journal of Public Economics, 124, 91–104.

Glantz, F. B., Berg, R., Porcari, D., Sackoff, E., & Pazer, S. (1994). School lunch eligible non-participants. U.S. Department of Agriculture, Food and Consumer Service, Office of Analysis and Evaluation.

Gleason, P. M. (1995). Participation in the National School Lunch Program and the School Breakfast Program. American Journal of Clinical Nutrition, 61, 213S–220S.

Gundersen, C., Kreider, B., & Pepper, J. (2012). The impact of the National School Lunch Program on child health: A nonparametric bounds analysis. Journal of Econometrics, 166, 79–91.

Healthy, Hunger-Free Kids Act of 2010. (2010). Public Law 111–296.

Hill, C. J., Bloom, H. S., Black, A. R., & Lipsey, M. W. (2008). Empirical benchmarks for interpreting effect sizes in research. Child Development Perspectives, 2, 172–177.

Hinrichs, P. (2010). The effects of the national school lunch program on education and health. Journal of Policy Analysis and Management, 29, 479–505.

Imberman, S. A., & Kugler, A. D. (2014). The effect of providing breakfast in class on student performance. Journal of Policy Analysis and Management, 33, 669–699.

Kitchen, S., Tanner, E., Brown, V., Payne, C., Crawford, C., Dearden, L., Greaves, E., & Purdon, S. (2013). Evaluation of the Free School Meals pilot: Impact report. National Centre for Social Science Research and Department for Education, Ref: DFE-RR227. Accessed November 17, 2013 from https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/184047/DFE-RR227.pdf.

Leos-Urbel, J., Schwartz, A. E., Weinstein, M., & Corcoran, S. (2013). Not just for poor kids: The impact of universal free school breakfast on meal participation and student outcomes. Economics of Education Review, 36, 88–107.

McEwan, P. J. (2013). The impact of Chile's school feeding program on education outcomes. Economics of Education Review, 32, 112–139.

Michelmore, K., & Dynarski, S. (2017). The gap within the gap: Using longitudinal data to understand income differences in educational outcomes. AERA Open. Available at https://doi.org/10.1177/2332858417692958.

Millimet, D. L., Husain, M., & Tchernis, R. (2010). School nutrition programs and the incidence of childhood obesity. Journal of Human Resources, 45, 640–654.

Mirtcheva, D. M., & Powell, L. M. (2009). Participation in the national school lunch program: Importance of school-level and neighborhood contextual factors. Journal of School Health, 79, 485–494.

New York City Independent Budget Office. (2017). If no student pays: Cost to provide free lunch for all New York City's elementary school students. New York, NY. Available at http://www.ibo.nyc.ny.us/iboreports/if-no-student-pays-cost-to-provide-free-lunch-for-all-of-new-york-citys-elementary-school-students.html.

Perlman, S. E., Nonas, C., Lindstrom, L. L., Choe-Castillo, J., McKie, H., & Alberti, P. M. (2012). A menu for health: Changes to New York City school food, 2001 to 2011. Journal of School Health, 82, 484–491.

Poppendieck, J. (2010). Free for all: Fixing school food in America. Berkeley, CA: University of California Press.

Richard B. Russell National School Lunch Act. (1946). 79 P.L. 396, 60 Stat. 230.

Ryan, P. (2014, March 6). On Principles & Values. Speech presented at the Conservative Political Action Conference. Washington, DC.

Sandman, D. (2016, April 22). Op-Ed; Such a thing as a free lunch—for all school kids. The Huffington Post. Available at http://www.huffingtonpost.com/david-sandman/.

Schanzenbach, D. (2009). Do school lunches contribute to childhood obesity? Journal of Human Resources, 44, 684–709.

Smith, T. A. (2017). Do school food programs improve child dietary quality? American Journal of Agricultural Economics, 99, 339–356.

U.S. Department of Agriculture, Food and Nutrition Service. (2002). Provision 2 Guidance: National School Lunch and School Breakfast Programs. Washington, DC. Available at https://www.fns.usda.gov/sites/default/files/prov2guidance.pdf.

U.S. Department of Agriculture, Food and Nutrition Service. (2012). Nutritional standards in the National School Lunch and School Breakfast Programs; Final Rule. Federal Register, 77(17), 7 CFR Parts 210 and 220.

U.S. Department of Agriculture, Food and Nutrition Service. (2018). Federal cost of school food programs. Washington, DC. Available at https://fns-prod.azureedge.net/sites/default/files/pd/cncost.pdf.

U.S. Department of Agriculture, Office of Communications. (2010). Healthy, Hunger-Free Kids Act of 2010 Fact Sheet. Release No. 0098.14 Washington, DC. Available at https://www.usda.gov/media/press-releases/2014/05/20/fact-sheet-healthy-hunger-free-kids-act-school-meals-implementation.

U.S. Department of Agriculture. (2012). SBP Fact Sheet. Washington, DC. Available at https://www.fns.usda.gov/sites/default/files/sbp/SBPfactsheet.pdf.

U.S. Department of Agriculture. (2013). NSLP Fact Sheet. Washington, DC. Available at https://www.fns.usda.gov/sites/default/files/NSLPFactSheet.pdf.

APPENDIX

Table A1. Estimated impacts of UFM on academic outcomes, event study models, 2007 to 2013.

	(1) zMath	(2) zELA
UFM Year		
−4 or more	−0.002	−0.016
	(0.036)	(0.023)
−3	−0.025	−0.010
	(0.018)	(0.013)
−2	−0.004	−0.008
	(0.011)	(0.010)
−1	–	–
0	0.003	0.002
	(0.011)	(0.008)
+1	0.063***	0.032***
	(0.012)	(0.009)
+2	0.077***	0.064***
	(0.014)	(0.010)
+3	0.103***	0.086***
	(0.016)	(0.012)
+4 or more	0.112***	0.097***
	(0.019)	(0.015)
Student Char.	Y	Y
Student FE	Y	Y
Grade FE	Y	Y
Year FE	Y	Y
Observations	934,625	923,309
Students	222,481	222,481
R-squared	0.78	0.75

Notes: Robust standard errors are in parentheses and clustered by school (*p<.10; **p <.05; ***p<.01). Samples include Ever UFM students in third to eighth grades in years 2007 to 2013. All models control for a vector of student characteristics including indicators for limited English proficiency and special education services and student, grade, and year fixed effects. Zero (0) is the year of first UFM exposure. Negative 1 (−1) is the reference category.

Table A2. Robustness check, school FE models, estimated impacts of UFM on academic outcomes, 2010 to 2013.

VARIABLES	(1) zMath	(2) zELA	(3) Attd_rate	(4) zMath	(5) zELA	(6) Attd_rate
UFM Middle	0.046	0.029*	−0.007			
	(0.033)	(0.016)	(0.150)			
UFM Middle, Poor				0.045	0.031*	0.000
				(0.032)	(0.017)	(0.156)
UFM Middle, Non-Poor				0.047	0.011	−0.037
				(0.053)	(0.035)	(0.177)
Student Char.	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y	Y
Observations	122,685	122,685	122,685	122,685	122,685	122,685
Students	47,887	47,887	47,887	47,887	47,887	47,887
Schools	233	233	233	233	233	233
R-squared	0.343	0.327	0.124	0.344	0.327	0.124

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 to 5 are suppressed. All models control for a vector of student characteristics including indicators for student gender, race/ethnicity, birth outside the U.S., poverty status, and participation in limited English proficiency and special education programs, school fixed effects, and grade-by-year fixed effects.

Table A3. Robustness check, school FE models, estimated impacts of UFM on weight outcomes by poverty, 2010 to 2013.

VARIABLES	(1) zBMI	(2) ln(BMI)	(3) Overwgt	(4) Obese	(5) Undrwgt
UFM Middle					
Poor	0.000	−0.000	−0.000	−0.001	0.001
	(0.016)	(0.003)	(0.008)	(0.005)	(0.003)
Non-Poor	−0.014	−0.002	−0.007	−0.011	0.005
	(0.029)	(0.007)	(0.015)	(0.010)	(0.006)
Student Char.	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y
Observations	122,685	122,685	122,685	122,685	122,685
Students	47,887	47,887	47,887	47,887	47,887
Schools	226	226	226	226	226
R-squared	0.048	0.103	0.035	0.035	0.017

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 to 5 are suppressed. All models control for a vector of student characteristics including indicators for student gender, race/ethnicity, birth outside the U.S., poverty, and participation in limited English proficiency and special education programs, school fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status.

Table A4. Robustness check, school FE models, estimated impacts of UFM on school meal participation by poverty, 2010 to 2013.

VARIABLES	(1) SLP	(2) SBP
UFM Middle		
Poor	6.334***	−2.073*
	(1.185)	(1.178)
Non-Poor	15.908***	1.302
	(3.364)	(2.309)
Student Char.	Y	Y
School FE	Y	Y
Grade*Year FE	Y	Y
Observations	122,685	122,685
Students	47,887	47,887
Schools	233	233
R-squared	0.374	0.225

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 to 5 are suppressed. All models control for a vector of student characteristics including indicators for student gender, race/ethnicity, birth outside the U.S., poverty, and participation in limited English proficiency and special education programs, school fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status.

Table A5. Robustness check, IV school FE models, estimated impacts of SLP on student outcomes by poverty, 2010 to 2013.

VARIABLES	(1) zMath	(2) zELA	(3) Attd_rate	(4) zBMI	(5) ln(BMI)	(6) Overwgt	(7) Obese	(8) Undrwgt
SLP Middle								
Poor	0.006	0.005*	0.000	0.001	0.000	0.000	0.000	−0.000
	(0.004)	(0.003)	(0.023)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
Non-Poor	0.003	0.002	−0.002	−0.000	0.000	−0.000	−0.000	0.000
	(0.003)	(0.002)	(0.012)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)
Student Char.	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade*Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	122,678	122,678	122,678	122,678	122,678	122,678	122,678	122,678
Students	47,880	47,880	47,880	47,880	47,880	47,880	47,880	47,880
Schools	226	226	226	226	226	226	226	226

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. Results for students in grades 3 to 5 are suppressed. All models control for a vector of student characteristics including indicators for student gender, race/ethnicity, birth outside the U.S., poverty, and participation in limited English proficiency and special education programs, school fixed effects, grade-by-year fixed effects, and interactions between student grade level and poverty status. We instrument for student SLP using UFM status, differentiating between student grade level and poverty status. We use four instruments (*UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) to address endogeneity of four regressors (*SLP_Middle*Poor, SLP_Middle*Nonpoor, SLP_Elem*Poor, SLP_Elem*Nonpoor*, respectively). F-statistics of the excluded instruments in the first stage regressions (i.e., *UFM_Middle*Poor, UFM_Middle*Nonpoor, UFM_Elem*Poor, UFM_Elem*Nonpoor*) are as follows: 14.43, 14.10, 11.10, and 12.95, respectively. Seven singletons are dropped from this analysis.

Table A6. Regression results, UFM adoption and removal models, AY 2006 to 2013.

VARIABLES	(1) UFM Adoption	(2) UFM Removal
Share:		
SLP	0.003	0.005**
	(0.002)	(0.003)
SBP	0.000	0.001
	(0.002)	(0.003)
Obese	−1.020	1.435
	(0.893)	(1.097)
Overweight	0.877	−1.223
	(0.709)	(0.949)
Underweight	0.916	−0.092
	(0.936)	(1.087)
Math – 4	0.184	0.437
	(0.340)	(0.573)
Math – 3	−0.410	−0.150
	(0.298)	(0.459)
ELA – 4	0.072	3.767***
	(0.473)	(1.121)
ELA – 3	−0.198	0.033
	(0.375)	(0.566)
Female	−0.252	0.668
	(0.722)	(1.089)
Asian	−0.097	0.111
	(0.738)	(1.720)
Black	0.723	0.602
	(0.684)	(1.514)
Hispanic	0.041	2.495*
	(0.748)	(1.360)
Immigrant	0.255	−0.860
	(0.491)	(0.626)
LEP	0.715	0.281
	(0.688)	(1.086)
SPED	−0.035	−0.419
	(0.570)	(0.907)
School FE	Y	Y
Year FE	Y	Y
Observations	753	619
R-squared	0.631	0.568

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Column 1 includes schools that do not have UFM in the current year. Column 2 includes schools that have UFM in the current year. Samples include middle schools (schools that serve seventh graders) that ever have UFM in academic years 2006 to 2013.

Table A7. Regression results, new UFM exposure and loss of UFM next year, AY 2010 to 2013.

	(1)	(2)	(3)	(4)
	Ever UFM/POS		Ever UFM/POS Change UFM in Same School	
VARIABLES	New UFM Exposure	Loss of UFM	New UFM Exposure	Loss of UFM
Share:				
SLP	−0.000	−0.001	0.001	−0.001
	(0.002)	(0.001)	(0.003)	(0.001)
SBP	0.001	−0.001	0.001	−0.001
	(0.004)	(0.002)	(0.003)	(0.002)
Obese	−0.033	0.001	0.003	0.007
	(0.111)	(0.029)	(0.145)	(0.031)
Overweight	−0.023	−0.016	0.028	−0.003
	(0.093)	(0.034)	(0.088)	(0.039)
Underweight	−0.066	−0.002	−0.070	−0.017
	(0.162)	(0.037)	(0.158)	(0.036)
zMath	−0.082	−0.006	−0.029	−0.012
	(0.083)	(0.025)	(0.064)	(0.025)
zELA	0.051	−0.012	0.040	−0.012
	(0.066)	(0.018)	(0.051)	(0.018)
LEP	−0.170	0.153*	−0.265	0.161*
	(0.295)	(0.086)	(0.274)	(0.093)
SPED	0.204	−0.035	0.044	−0.039
	(0.433)	(0.054)	(0.364)	(0.057)
Student FE	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	9,203	64,406	8,168	62,292
Students	7,852	43,790	7,169	42,786
R-squared	0.921	0.818	0.949	0.820

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013 (observations in 2013 are excluded because UFM status is not observed for the following year). Columns 1 and 3 include students not exposed to UFM in the current year. Columns 2 and 4 include students exposed to UFM in the current year. Columns 3 and 4 include a subset of POS students who do not change schools in the observation year. Reported variables are jointly insignificant for all columns; F-statistics = 0.35, 0.66, 0.29, and 0.75 for columns 1 through 4, respectively.

Table A8. Placebo test: Estimated effect UFM next year on academic and weight outcomes this year, 2010 to 2013.

Panel A. Ever UFM and POS Students												
VARIABLES	(1) zMath	(2) zELA	(3) Att_rate	(4) zBMI	(5) ln(BMI)	(6) Overwgt	(7) Obese	(8) Undrwgt	(9) LEP	(10) SPED	(11) SLP	(12) SBP
UFM_next	0.033	0.000	−0.085	0.003	0.013	0.011	−0.004	−0.000	−0.008	0.000	−0.028	−0.602
	(0.025)	(0.023)	(0.156)	(0.007)	(0.032)	(0.018)	(0.010)	(0.005)	(0.005)	(0.002)	(1.617)	(1.704)
Student FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Grd*Yr FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	99,108	99,108	99,108	99,108	99,108	99,108	99,108	99,108	99,108	99,108	99,108	99,108
Students	47,827	47,827	47,827	47,827	47,827	47,827	47,827	47,827	47,827	47,827	47,827	47,827
Schools	228	228	228	228	228	228	228	228	228	228	228	228
R-squared	0.882	0.847	0.859	0.915	0.915	0.845	0.845	0.690	0.953	0.961	0.852	0.799
Panel B. Ever UFM and POS Students Not Offered UFM in Current Year												
UFM_next	−0.215	−0.078	1.424	−0.018	−0.102	−0.089	−0.085	−0.050	0.013	0.043	−4.432	1.13
	(0.263)	(0.195)	(1.755)	(0.036)	(0.179)	(0.100)	(0.085)	(0.048)	(0.082)	(0.040)	(6.412)	(33.095)
Student FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Grd*Yr FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	12,258	12,258	12,258	12,258	12,258	12,258	12,258	12,258	12,258	12,258	12,258	12,258
Students	10,497	10,497	10,497	10,497	10,497	10,497	10,497	10,497	10,497	10,497	10,497	10,497
Schools	173	173	173	173	173	173	173	173	173	173	173	173
R-squared	0.972	0.964	0.959	0.981	0.982	0.962	0.957	0.913	0.985	0.989	0.961	0.935

Notes: Robust standard errors are in parentheses and clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in third to eighth grades with POS data and at least two years of test scores and weight data from 2010 to 2013. All models include student fixed effects and grade-by-year fixed effects.