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

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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-difference 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 0.059 standard deviations in math and 0.083 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 probability of obesity or overweight, or BMI. Results are robust to an array of alternative assumptions about sample and specifications.

JEL No. I24, I38, H52

Keywords: School Food, Academic Performance, Free Lunch, Childhood Obesity

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"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 & 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

I. 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 and costing $12 billion annually. A growing number of schools (and districts) have chosen to adopt *Universal Free Meals (UFM)* as an alternative, which provides free school lunch for all students, regardless of income. Advocates claim that UFM will reduce the stigma that limits participation, address food insecurity for needy students, improve student readiness to learn and reduce administrative burden. Skeptics argue that the benefits are insufficient to justify the additional cost and, indeed, UFM may be deleterious - increasing obesity, for example. The empirical research investigating these claims is, however, unfortunately thin, and there is no existing research providing credibly causal estimates of the impact of UFM per se. This paper begins to fill this gap, exploiting the conditionally random timing of the adoption of UFM by NYC public middle schools to estimate the effect of UFM on middle school student attendance and test scores as well as the impact of lunch participation on student outcomes. Additionally, we investigate potential unintended consequences for obesity and weight outcomes.

We use unique and 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. 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 also on the effect of school lunch *per se* on academic outcomes.

We focus on middle school students for three key reasons. First, middle school students are more likely to make autonomous decisions about lunch participation each day than elementary school students and are, therefore, more likely (than younger/elementary school students) 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, but is not as broad in elementary schools, which are typically smaller, resulting in limited statistical power to estimate impacts.[1]

[1] We exclude high school students from our analysis because students in grades 9 through 12 do not take a standardized ELA and a standardized math exam each year. High school students in New York State are required to

To preview our results, we find UFM increases academic performance by as much as 0.06 standard deviations in math and .08 in ELA for non-poor students, with smaller, statistically significant effects of .032 and .027 standard deviations in math and ELA for poor students.[2] 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 obesity, overweight or BMI. Results are robust to an array of alternative assumptions about sample 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.

II. 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 approximately $15 billion a year on NSLP and SBP ($11.6 billion for the NSLP in 2012 and $3

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pass one Comprehensive English and one Mathematics Regents Exam in order to graduate, but schools and students choose the grade in which students take those exams.

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

billion for the SBP in 2011; compared to about $75 billion annually on SNAP) and subsidize approximately 44 million meals a day (31.6 million for the NSLP in 2012 and 12.1 million for the SBP in 2011).[3]

According to the USDA, the NSLP program "improves nutrition and focuses on reducing childhood obesity" (US 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 and 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.[4]

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 income up to 135% of the Federal poverty line and at a *reduced* price to students with household income up to 185%.[5] That is, individual eligibility for subsidies through the national school meals programs is means-tested. Schools certify student eligibility using student-returned "lunch forms" or "direct certification."[6] Federal regulations also provide schools and districts with the option of applying to implement UFM.[7] UFM eliminates all fees charged to students to

[3] US Department of Agriculture, 2012, 2013.
[4] In addition, the NSLP provides an avenue for surplus food distribution, serving as a Federal farm subsidy.
[5] 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 like 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".
[6] 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, and/or SNAP benefits) to certify individual student eligibility for subsidies.
[7] 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

participate in the schools meals programs. That is, UFM makes 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 free or reduced price lunch eligible 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, and previous research has found significant stigma limiting 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, potentially, 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.[8] Established in 1980, Provision 2 reduces the burden of tracking meals served by student eligibility status to once every 4 years. During the base year, a school establishes reimbursement rates based on the percentage of meals served by student eligibility status (free, reduced, 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

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Federal reimbursement and the full cost of providing school meals. See US Department of Agriculture Food and Nutrition Service (2002).

[8] 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% of the student body be eligible for subsidies through "direct certification," verified through administrative records indicating student participation in SNAP and/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).

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

III. 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 socio-demographics. 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, Guilkey, Popkin & Wyckoff, 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 (Imberman & Kugler, 2014; Frisvold 2015; Hinrichs, 2010).[9] Others that target increasing

[9] 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 preprogram 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

nutritional and caloric content of meals also see improved test scores (Anderson, Gallagher & Ritchie, 2017; Figlio & Winicki, 2005).[10] 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, Schwartz, Weinstein & Corcoran, 2013; McEwan, 2013).[11] 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

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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.

[10] 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.

[11] 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 only some racial/ethnic subgroups. McEwan (2013) estimates the impact of providing higher calorie school meals in rural Chile, finding no impact on test scores.

for students with greater nutritional needs and no positive effects (and if anything negative) for students with lesser nutritional needs.

The evidence on the impact of UFM per se is even more limited. One notable exception is Kitchen et al. (2013), which examined a pilot universal free meals program in the United Kingdom (the UFSM program). Estimated effects were largely positive: most pupils (nearly 90%) 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 and, importantly, 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. Using school level data on participation, Leos-Urbel et al. (2013) 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, but find little evidence of an impact on academic outcomes (either test scores or attendance). Others find small-to-moderate positive effects of universal free breakfast programs on academic achievement (Crawford, Edwards, Farquharson, Greaves, Trevelyan, Wallace, & White, 2016).[12]

[12] 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. 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 is not the only consideration. Indeed, school meals might be unappealing due to poor preparation, stringent nutritional standards, taste or the stigma associated with meal participation (Glantz, Berg, Porcari, Sackoff, & Pazer, 1994; Gleason, 1995; Mirtcheva & Powell, 2009; Poppendieck, 2010). Instead, student participation reflects family resources and budget constraints, preferences over alternatives including brown bag lunch from home, purchased lunches from restaurants or stores outside school, vending machines in schools, and so on. Thus, school lunch participation may be un-responsive to price changes. Whether, and to what extent, UFM increases school meals participation is, in the end, an empirical question that we address in this paper.

This study uses new, richly detailed data on individual, daily participation and the timing of price changes (adoption of UFM) to estimate the impact on student academic outcomes. Further, 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.

IV. 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 1500 public schools annually. This includes over 200,000 students in the middle school grades, 6-8, and more than 500 schools serving them. Critically 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 of political, institutional and administrative factors, which makes the *precise timing* of UFM adoption by a particular school effectively random. 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 precise number of NYC schools that offer UFM has varied year-to-year since 2009, but the number of schools offering UFM has not grown or declined steadily over time (Figure 2). About half of schools participate in a UFM program for at least one year from 2010-2013. UFM use expanded in the 2010 and 2011 school years and contracted in 2012 and 2013.[13] UFM schools may return to standard procedures for counting meals and meal reimbursement at any point; alternatively, UFM schools may request a 4-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 (US 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.[14]

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

[14] Moreover, we found no documentation related to implementation of Provision 2 of the National School Lunch Act (from the USDA, the NYC Department of Education Office of School Food, or otherwise) that suggest priority is given to applicant schools 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 extent to which the characteristics of the school's student body in year t predict the future adoption of UFM in t+1 and by second examining the relationship between future UFM status in t+1 with current year student characteristics and outcomes in year t. Both sets of empirical results are consistent with the results of our review of the policy documentation and our meetings with those implementing the policy.

Why might some NYC schools adopt UFM and other 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" (US Department of Agriculture Food and Nutrition Service, 2002).[15] Thus, it is ultimately a choice made by school administrators in conjunction with the City school district and subject to Federal eligibility.

In addition to schools offering UFM under Provision 2, NYC Department of Education (NYCDOE) has taken steps to increase access to school meals citywide, including price and menu changes (Perlman, Nonas, Lindstrom, Choe-Castillo, McKie, Alberti, 2012). In 2004, the NYCDOE implemented universal free breakfast - eliminating the 25-cent price for full price students and the 5-cent price for reduced price students (Leos-Urbel et al. 2013). A decade later, in September 2014, the NYC DOE extended UFM status to all freestanding middle schools. Figure 1 shows citywide prices for full and reduced price meals in the period 2002-2015. As shown, citywide prices for school meals in non-UFM schools are stable in the 2010-2013 period. We focus our study on this stable period in an effort to isolate the effect of UFM from other price effects.

V. Data and Measures

Our analysis draws on a 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

[15] Moreover, informal conversations with administrators and advocates suggest that school leaders are sometimes concerned that offering UFM could also increase 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.

large subset of students. Again, we focus on 2010-2013, 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, and eligibility for free or reduced price lunch, participation in special education, attendance, and scores on ELA and math exams for grades 3-8, as well as student height and weight.[16] Student-level data also includes measures of participation in school lunch and school breakfast for students attending a school collecting such data.[17] Importantly, every student record includes a unique student identifier allowing us so 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.[18] Further, we restrict the sample to students with height and weight data, although we relax this constraint in robustness test described below.

Test scores are transformed into z-scores using grade and year specific means and variances, *zMath and zELA* for the math and English language arts exams, respectively. Weight outcomes include Body Mass Index (BMI), measured as z-scores (normalized by grade and 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

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[16] Beginning in 2006, NYC public schools has collected annual measures of height, weight, and physical fitness of almost every student as part of the Fitnessgram initiative. By 2012-13, the Fitnessgram covered roughly 875,000 students in 1,650 schools citywide.

[17] This new data is collected NYC Office of School Food (OSF) using an electronic Point of Service (POS) tracking system to record meal transactions with student ID and time stamps.

[18] 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 of the authors.

construct a time invariant measure of poverty, *Poor*, that takes a value of 1 if a student is identified as eligible for free or reduced priced lunch in any year between 2001-2013 and 0 otherwise. We use this measure to capture poverty, rather than the annual, student-level measure of eligibility for free or reduce price lunch typically used in education research, because UFM reduces the incentives for families (and schools) to verify individual eligibility. Thus, the annual poverty measure may be correlated with UFM treatment.[19] *Nonpoor* identifies students never observed as eligible for free or reduced price lunch during this period; that is, poor and non-poor are mutually exclusive.[20]

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, the number of students in each eligibility group, and total expenditures on school food. 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% eligible for free or reduced price lunch in at least one year between 2001 and 2013, and predominantly "minority" - only 15% are White. Hispanics represent almost 40%, with 28.5% Black and 17% Asian. Further, roughly one-sixth of all students are Foreign Born, more than half

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[19] Though, students who do not submit lunch forms are still certified through "direct certification," using matched NYC DOE student records and data indicating eligibility for SNAP, WIC, and other means-tested programs. Columns 3 and 4 of Table 1 show that we observe similar poverty rates for the subset of students exposed to UFM in each sample year (Always UFM) and those exposed in some years but not others (UFM Switchers).

[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% of NYC school aged children attend independent schools, which include both parochial schools and private schools. Instead, many of the students we term non-poor are of modest means, with family income exceeding 185% of the Federal poverty line by small margins. Again, due to cost of living differences, a substantial portion of non-poor students living in high-cost places like NYC have real (regionally adjusted) incomes that would have qualified them as free lunch eligible in a lower-cost of living district.

speak a language other than English at home, and almost ten percent qualify as Limited English Proficient. 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 6-8.[21] 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 the 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.[22] Of the 201,004 observations on 100,194 UFM Switchers, 71,971 are students who switch UFM status due to changing schools (ex. graduating from a school without UFM and entering a school with UFM).[23]

A second sample includes only observations with lunch and breakfast participation data - limiting our sample to students attending one of the schools using 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.[24] 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

[21] Regression models also include students in grades 3-5. The "Ever UFM" sample for grades 3-8 has 659,797 observations on 222,456 students attending 1,103 schools,

[22] Moreover, among Ever UFM students, differences between students currently exposed to UFM and those not currently exposed are not statistically significant.

[23] 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).

[24] The regression sample also includes 33,119 observations of 16,149 students in grades 3-5, including 80 elementary schools (typically enrolling fewer than 100 Ever UFM/POS students) and 36 schools serving both elementary and middle grades.

Table 1, the mean School Lunch Participation rate (*SLP*) is about 62.2% in Ever UFM middle schools and the mean School Breakfast Participation rate (*SBP*) is roughly 11.3%.[25]

VI. 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-difference specification of a model linking student outcomes to UFM status and time varying student variables:

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

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 probability *overweight*, *obese* and *underweight*). UFM_Middle_{igst} is the interaction between an indicator variable $Middle_{igst}$ (which takes a value of 1 if student i is in grades 6 through 8) and UFM_{st} (which takes a value of 1 if student i attends a UFM school in year t);[26] UFM_Elem_{igst} is the interaction between $Elem_{igst}$ (which takes a value of 1 if student i is in grades 3 through 5) and UFM_{st}.[27] $\mathbf{X}_{igst}$ is a vector of other student characteristics including those indicating LEP, and special education needs;[28] γ_{igst} is a grade-by-year fixed

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[25] 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.

[26] We omit $Middle_{ist}$ because it is perfectly collinear with the grade-by-year fixed effect

[27] 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 from authors.

[28] As a robustness check, we substitute school fixed effects in 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 does not speak English at home.

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.[29] 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 conditionally random and, particularly, uncorrelated with unobserved concurrent changes in policies, practices and characteristics of the school.[30] As noted earlier, institutional and practical considerations and exploratory empirical work suggest conditional randomness is both plausible and likely. We shows tests for the validity of the assumption empirically below.

Heterogeneity by Student Poverty Status

We then 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.[31] 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,

[29] See, for example, Cameron and Miller (2015).

[30] Estimated impacts in student fixed effects models are identified by within student changes in UFM status. We note that within student variation in UFM status most often results from changing schools (70% of UFM Switchers change UFM status at the time of changing schools).

[31] In these models, we add interactions between poverty status (poor / non-poor) and grade level (middle / elementary), so that impacts are estimated within these subgroups.

thus, only lunch prices change after UFM.[32] We use the same difference-in-differences strategy using the sample of POS students:

$$(2)\qquad SLP_{igst} = \beta_0 + \beta_1 UFM\_Middle_{igst} * Poor_i + \beta_2 UFM\_Middle_{igst} * NonPoor_i + \beta_3 UFM\_Elem_{igst} * Poor_i + \beta_4 UFM\_Elem_{igst} * NonPoor_i + \boldsymbol{X'_{igst}}\beta_5 + \gamma_{igst} + \delta_i + \varepsilon_{igst}$$

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 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 due, perhaps, to unobserved differences in income, motivation, or engagement between parents (or students) utilizing school lunch and those that do not or related to their reliance on lunch. Again, we estimate separate effects for poor and non-poor students:

$$(3)\qquad Y_{ist} = \beta_0 + \beta_1 SLP\_\widehat{M\imath ddle} * Poor_{igst} + \beta_2 SLP\_M\imath ddl\widehat{e * N}onpoor_{\imath gst} + \beta_1 SLP\_\widehat{Elem} * Poor_{igst} + \beta_2 SLP\_Ele\widehat{m * N}onpoor_{\imath gst} + \boldsymbol{X'_{igst}}\beta_5 + \gamma_{igst} + \delta_i + \varepsilon_{igst}$$

[32] 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.

To be clear, we use the four variables created by fully interacting UFM with poverty status and grade level as instruments for SLP, again, fully interacted with poverty status and grade level.[33] All other variables as defined previously. Thus, we estimate the effect of a one-percentage point increase in SLP on student outcomes. To give a better sense of magnitudes, we estimate the impact of increasing participation by about one lunch every 10 school days as $10^*\beta_1$ and $10^*\beta_2$, for the poor and non-poor respectively. We again cluster standard errors at the school level.[34]

VIII. 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 1-3 show results for the Ever UFM sample and columns 4-6 for the Ever UFM sample with POS (meal participation) data. As shown, among the EVER UFM students, UFM increases math and ELA scores for 6-8 grade students 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 4-6), 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.

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[33] 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 available upon request from the authors.

[34] 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.

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 1-3 are estimated using the Ever UFM sample and columns 4-6 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 statistically significant at conventional levels. As a result, we are unable to reject the hypothesis that impacts are same for the poor and non-poor in this sample.

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 (1/5 of a school day annually, on average) among non-poor students. Results for the more limited sample, however, are insignificant and becomes positive for the poor. None of the coefficients are large enough to be considered as substantively meaningful effects on attendance.

Taken together, 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). Alternatively, there is little evidence of a substantively meaningful effect on attendance for either group, and only one coefficient reaches the size of statistical significance using conventional levels.

These are substantively meaningful improvements in test scores - on average, roughly 10% to 12% 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 class rather than in the 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, which is typical for targeted educational interventions.

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.[35] That said, applying the Hill et al. parameters to our estimated coefficients suggests UFM would improve

[35] 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.

performance in math by as much as 7-10 weeks of learning and by as much as 6-9 weeks of learning in ELA for non-poor students. For poor students, effects are roughly 3-4 weeks of learning in math and ELA.[36]

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 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 the possibility of a 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 is a strong instrument for SLP; UFM status by grade level and poverty is highly predictive of SLP. [37] Table 5 shows our IV estimates of the impact of SLP on academic outcomes. To review, these models use UFM as an instrument for SLP, allowing us to estimate a causal effect of increasing participation in school lunch itself. The results indicate

[36] 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 an academic year, 36. We present the range of results across the three middle school grades rounded to the nearest week.

[37] 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*). F-tests of the first stage regressions are as follows: 44.50, 22.36, 16.06 and 22.97, respectively.

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% 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% of a standard deviation for non-poor students. (For ELA, these will be 7% and 6%, 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).[38] 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 estimating impacts on BMI, measured both as a z-score and with the natural logarithm, overweight, obese or underweight, nine of ten estimated coefficients have negative signs, but only one – a 2.5 percentage point decrease in the probability obese for non-poor students – is statistically significant. While other results are insignificant, point estimates for non-poor are larger than the poor. For example, coefficients in the BMI model presented in column 1 show 0.040 versus 0.003 standard

[38] 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.

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 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 a substantively meaningful; one additional school lunch every two school-weeks decreases probability that non-poor students are obese by one percentage point. As with reduced form estimates, point estimates are larger for the non-poor, but coefficients are insignificant.[39]

That said, a majority of height and weight assessments occur in the fall, meaning we capture very short run effects here. More important, 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.[40] 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.[41]

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[39] Results from school fixed effects models, which serve as a robustness check and are shown in Table A2 of the Appendix, also suggest that there is no effect of UFM on student weight outcomes. Tables A3 and A4 of the Appendix show that the first and second stage estimates from the two-stage least squares IV models with school FE are similar in magnitude as those shown in Tables 4, 5 and 7. Again, however, coefficients are more precisely estimate in models with student FE.

[40] Unlike ELA and Math exams, which occur toward the end of the spring semester.

[41] By construction, observations of students whom 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.

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 a one percentage point increase in SLP reduces BMI by 0.1 percent (column 2) and the probability 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. Thus, these results suggest SLP does not have deleterious effects on middle school student weight outcomes and may, instead, *improve* weight outcomes.

Our results differ markedly from Schanzenbach (2009), which 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 vs. 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 the school may be less healthy, that is, differences in access to and types of restaurant food alternatives or differences in out-lunch policies. 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; UFM and SLP have smaller effects on BMI and obesity, and that 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).

VIII. 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, γ_{igst}. While student fixed effects models (above) are identified by the variation in UFM exposure within student 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 school UFM status only. Again, standard errors are clustered at the school level.

Results from school fixed effects models are shown in Table A1 through Table A4 of the Appendix. Results are consistent with those derived from student fixed effects models, though coefficients are generally less precisely estimated. Similar point estimates in student and school fixed effects models offer credibility that both identification strategies produce unbiased estimates of the impact of UFM.

We test the robustness of the test score models to 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 and statistical significance for both Ever UFM and Ever UFM/POS students. These results are available up request of the authors.

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 do not provide any evidence of deleterious effects on weight. Again, results are available upon request of the authors.

Do Observables Predict UFM?

A key condition underlying a causal interpretation of our estimates is that the timing of the adoption of UFM is conditionally random. 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 2006-2013, we estimate a model linking student characteristics in t to UFM status in the following year.[42] We restrict the sample to Ever UFM *schools* for which UFM_{st}=0 in time t (that is, schools not offering UFM in t) and estimate:

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

where all variables are school level aggregates of student variables previously defined.[43] 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

[42] We also conduct this analysis focusing on just the 2010-2013 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.

[43] 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 characteristics, 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.

(conditionally random) at the school level. Here, 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 Appendix table Table A5. 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..[44]

We perform a similar exercise to assess the endogeneity of the phase out of UFM. Here we re-estimate model (4) restricting the sample to Ever UFM schools that do offer UFM in t, that is, (UFM_t=1). In this way, the model sheds light on the extent to which student characteristics in t predict UFM phase-out in the following year (t+1). As shown in column 2 of Table A5, the results suggest student characteristics do not, in fact, predict UFM phase out, providing support for the view that the 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 of UFM is predicted by observables, boosting confidence in the causal interpretation of our impact estimates.[45]

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 twelve student outcomes (test

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

[45] Across the two models with 16 coefficients each (32 coefficients in all), only three coefficients are statistically significant at the 10% level, which is what one would expect from random chance alone.

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 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 Appendix Table A6; Panel A shows results for the full sample and Panel B for the UFM=0 subset. The results are encouraging. None of the twenty-four estimated coefficients on UFM_{ist+1} is 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 ten of the twelve falsification models for the set of all 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.[46]

XI. 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 UFM may increase obesity and increase costs for schools. The dearth of evidence hampers decision-making. NYC DOE, 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.

[46] These results are available upon request of the authors.

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 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.

Findings for the non-poor suggest price matters for this group of students whose families have household income exceeding 185% of the Federal poverty line. Findings for the poor - who 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 lead 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 in 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 socio-economically 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, in turn, depend upon the school food environment, availability of fast food, or family resources. In another vein, impacts may depend upon school food policies such as open campus or "out-lunch" policies, or the characteristics of cafeterias and/or 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. Again, this is worthy of additional research.

We can also compare the assessed benefits of UFM to the costs of providing free meals to all students. In a 2017 policy brief, the NYC Independent Budget Office (IBO) suggests that expanding UFM to all NYC elementary school students under the CEP would increase school lunch costs by $13.5 million if there were no effect on SLP. It would cost an additional $5 million if UFM increased SLP by 10% (about the magnitude of the effect on SLP shown in this paper). For a school system the size of NYC (over 1 million children each year, over 400,000 of which are in elementary grades), this is a trivial increase in per pupil costs: less than $50 per pupil. Taken on its face, this is a small price to pay for up to a half a standard deviation increase in both math and ELA test scores. Thus, UFM appears to be an inexpensive and effective way to encourage middle school students' participation in school meals and improve academic achievement without deleterious effects on obesity.

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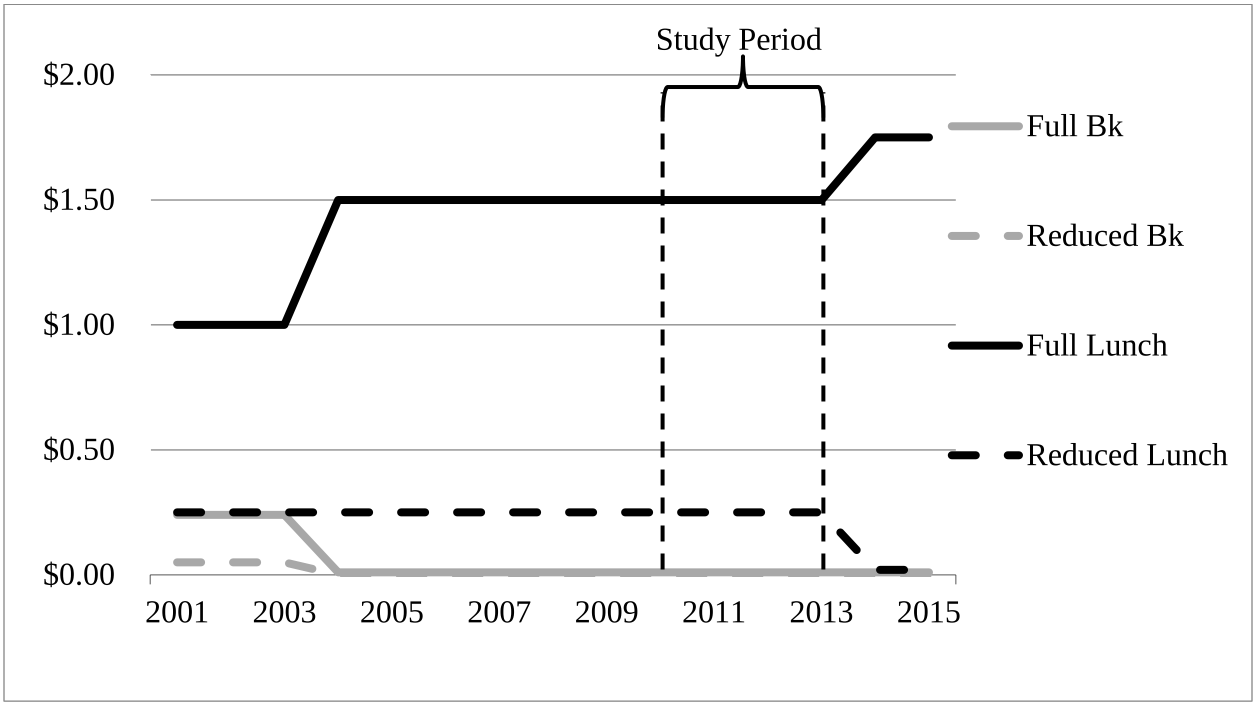
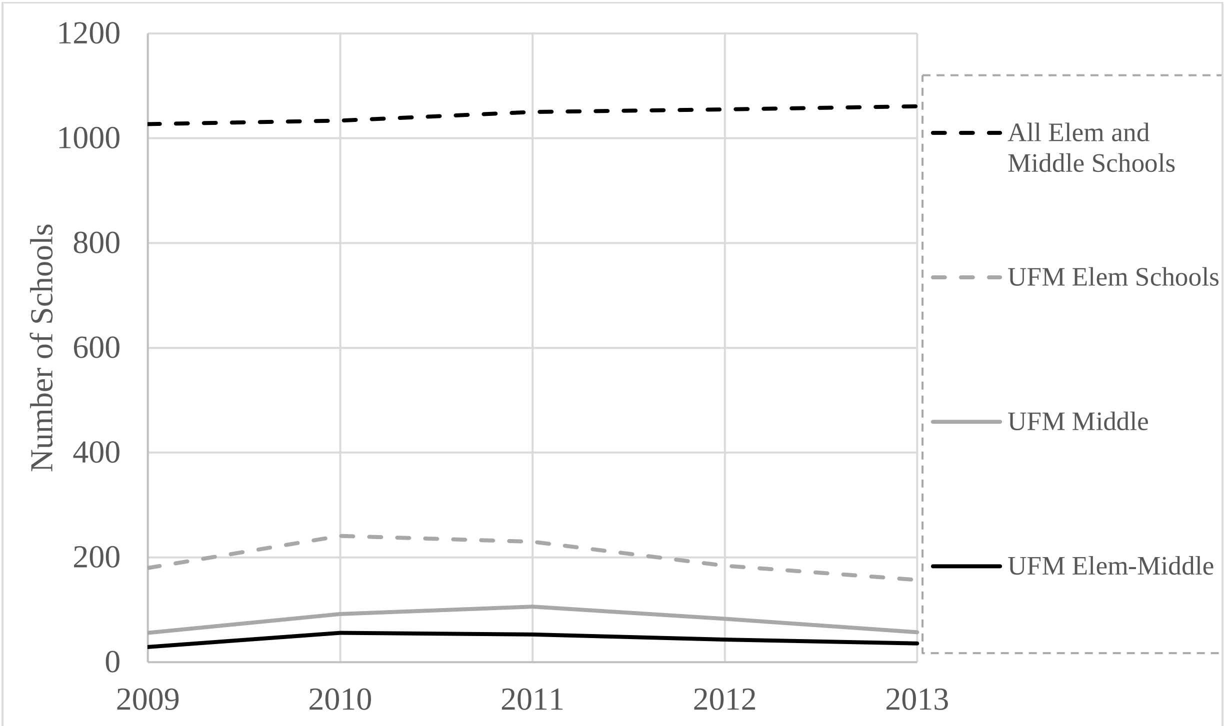
Tables and Figures

Figure 1. New York City Public School Meal Prices by Year, 2001-2015



Notes: Prices in NYC Public Schools for breakfast (BK) and lunch, academic years 2001-2015. Meals provided *free* of charge for students at or below 135% of the Federal poverty line, at a *reduced* price for those at or below 185% of the Federal poverty line and at *full* price for all other students. In AY 2015, UFM extended to all freestanding middle schools (6-8 and 5-8 schools only). Point of Service (POS) data is available for a subset of schools in academic years 2010-2013, 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. Number of UFM Schools by Year, 2009-2013



Notes: UFM schools provide free meals to all students, regardless of individual student eligibility. Elem indicates that a school serves 4th graders, Middle indicates that it serves 7th graders, and Elem-Middle indicates that a school serves both 4th and 7th graders. All Elem and Middle Schools reflects the sum of all UFM and non-UFM Elementary, Middle, and Elementary-Middle schools.

Table 1. Descriptive Statistics, Middle School Students ONLY, by UFM and POS status, 2010-2013

	(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 6^{th}-8^{th} grade students with at least two years of test scores and weight outcome data. Ever UFM students are either Always UFM or UFM Switchers.

Table 2. Estimated Impacts of UFM on Academic Outcomes, 2010-2013

	Ever UFM			Ever UFM/POS		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	zmath	zELA	Attd_rate	zmath	zELA	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
Constant	0.499***	0.286***	93.138***	1.018***	0.758***	92.731***
	(0.021)	(0.018)	(0.141)	(0.078)	(0.073)	(0.486)
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 in parentheses 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 2010-2013. Columns 4 through 6 include a subset of Ever UFM students with POS data. Samples include observations of 3rd-8th grade students with at least two years of test scores and weight outcome data. Results for students in grades 3-5 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.

Table 3. Estimated Impacts of UFM on Academic Outcomes by Poverty, 2010-2013

	Ever UFM			Ever UFM/POS		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	zmath	zELA	Attd Rate	zmath	zELA	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
Constant	-1.991***	-1.109***	101.654***	-1.532***	-1.061***	99.476***
	(0.075)	(0.056)	(0.682)	(0.160)	(0.127)	(1.064)
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 in parentheses 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 2010-2013. Columns 4 through 6 include a subset of Ever UFM students with POS data. Sample includes observations of 3rd-8th grade students with at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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 4. Estimated Impacts of UFM on School Meal Participation by Poverty, 2010-2013

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
Constant	73.454***	18.851***
	(3.447)	(3.457)
Observations	122,685	122,685
Students	47,887	47,887
Schools	233	233
R-squared	0.826	0.744

Notes: Robust standard errors in parentheses clustered by school (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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 5. Estimated Impacts of SLP on Academic Outcomes by Poverty, IV Model, 2010-2013

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 parentheses clustered by sequence of schools attended (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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-Poor-Middle, UFM-Non-Poor-Middle, UFM-Poor-Elementary and UFM-Non-Poor-Elementary) to address endogeneity of four regressors (SLP-Poor-Middle, SLP-Non-Poor-Middle, SLP-Poor-Elementary and SLP-Non-Poor-Elementary, respectively). F-tests of the first stage regressions 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, 2010-2013

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
Constant	0.010	3.012***	0.480***	0.211***	0.002
	(0.095)	(0.021)	(0.052)	(0.039)	(0.021)
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 in parentheses clustered by school (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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, 2010-2013

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 in parentheses clustered by sequence of schools attended (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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-Poor-Middle, UFM-Non-Poor-Middle, UFM-Poor-Elementary and UFM-Non-Poor-Elementary) to address endogeneity of four regressors (SLP-Poor-Middle, SLP-Non-Poor-Middle, SLP-Poor-Elementary and SLP-Non-Poor-Elementary, respectively). F-tests of the first stage regressions are as follows: 44.50, 22.36, 16.06 and 22.97, respectively. 1,283 singletons are dropped from this analysis.

Table 8. Estimated Impacts of SLP before Fitnessgram on Weight Outcomes, IV Model, 2010-2013

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 in parentheses clustered by sequence of schools attended (*p<.10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data, Fitnessgram dates after September, and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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-Poor-Middle, UFM-Non-Poor-Middle, UFM-Poor-Elementary and UFM-Non-Poor-Elementary) to address endogeneity of four regressors (SLP-Poor-Middle, SLP-Non-Poor-Middle, SLP-Poor-Elementary and SLP-Non-Poor-Elementary, respectively). F-tests of the first stage regressions are as follows: 30.36, 15.56, 9.90 and 20.89, respectively. 7,619 singletons are dropped from this analysis.

Appendix

Table A1. Robustness Check, School FE Models, Estimated Impacts of UFM on Academic Outcomes, 2010-2013

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
Constant	0.496***	0.457***	95.409***	-0.610***	-0.575***	87.589***
	(0.058)	(0.052)	(0.214)	(0.065)	(0.068)	(0.873)
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 in parentheses clustered by school (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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 A2. Robustness Check, School FE Models, Estimated Impacts of UFM on Weight Outcomes by Poverty, 2010-2013

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
Constant	-0.011	3.067***	0.413***	0.243***	0.028***
	(0.075)	(0.015)	(0.037)	(0.031)	(0.008)
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 in parentheses clustered by school (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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 A3. Robustness Check, School FE Models, Estimated Impacts of UFM on School Meal Participation by Poverty, 2010-2013

	(1)	(2)
VARIABLES	SLP	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
Constant	47.696***	5.954**
	(2.676)	(2.608)
Observations	122,685	122,685
Students	47,887	47,887
Schools	233	233
R-squared	0.374	0.225

Notes: Robust standard errors in parentheses clustered by school (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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, IV School FE Models, Estimated Impacts of SLP on Student Outcomes by Poverty, 2010-2013

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 in parentheses clustered by school (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. Results for students in grades 3-5 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-Poor-Middle, UFM-Non-Poor-Middle, UFM-Poor-Elementary and UFM-Non-Poor-Elementary) to address endogeneity of four regressors (SLP-Poor-Middle, SLP-Non-Poor-Middle, SLP-Poor-Elementary and SLP-Non-Poor-Elementary, respectively). F-tests pf the first stage regressions are as follows: 14.43, 14.10, 11.10 and 12.95, respectively. 7 singletons are dropped from this analysis.

Table A5. Regression Results, UFM Adoption and Phase out Models, AY 2006-2013

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 in parentheses 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 7th graders) that ever have UFM in academic years 2006-2013.

Table A6. Placebo Test: Estimated Effect UFM Next Year on Academic and Weight Outcomes This Year, 2010-2013

Panel A. Ever UFM and POS Students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	zmath	zELA	Att_rate	zBMI	ln(BMI)	Overwgt	Obese	Undrwgt	LEP	SPED	SLP	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
Constant	1.386***	0.950***	93.984***	2.958***	-0.005	0.402***	0.212***	0.105***	0.159***	0.233***	74.719***	16.490***
	(0.106)	(0.102)	(0.677)	(0.022)	(0.101)	(0.048)	(0.037)	(0.029)	(0.017)	(0.036)	(3.750)	(4.200)
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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
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
Constant	1.248***	1.245***	91.171***	0.157	2.980***	0.257	0.351	0.082	0.195**	0.131	68.424***	57.883
	(0.456)	(0.441)	(4.434)	(0.830)	(0.144)	(0.263)	(0.508)	(0.125)	(0.098)	(0.104)	(15.970)	(35.294)
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 in parentheses clustered by school (*p < .10. **p<.05. ***p<.01). Sample includes observations of Ever UFM students in 3rd-8th grade with POS data and at least two years of test scores and weight data from 2010-2013. All models include student fixed effects and grade-by-year fixed effects.