The Effect of Providing Breakfast in Class on Student Performance

Scott A. Imberman, Michigan State University and NBER[1]

Adriana D. Kugler, Georgetown University and NBER

November 13, 2013

Abstract

Many schools have recently experimented with moving breakfast from the cafeteria to the classroom. We examine whether such a program increases achievement, grades and attendance rates. We exploit quasi-random timing of program implementation that allows for a difference-in-differences identification strategy. We find that providing breakfast in-class relative to the cafeteria raises math and reading achievement by 0.09 and 0.06 standard deviations, respectively. These effects are most pronounced for low performing, free-lunch eligible, Hispanic, and low BMI students. A lack of differences by exposure time and effects on grades suggest that these impacts are on test-taking performance rather than learning. At the same time, the results highlight the possibility that measured achievement may be biased downwards, and accountability penalties may be inappropriately applied, in schools where many students do not consume breakfast.

---

[1] Scott A. Imberman, Michigan State University, Department of Economics, Marshall-Adams Hall, 486 W Circle Dr Rm 110, East Lansing, MI 48824, imberman@msu.edu. Adriana D. Kugler, Georgetown University, Georgetown Public Policy Institute, Old North Building, Suite 311, 37th and O Streets, N.W., Washington, DC 20057, ak659@georgetown.edu. We would like to thank Katharine Abraham, Steven Craig, David Frisvold, Judy Hellerstein, Morris Kleiner, Aaron Sojourner, Diane Whitmore Schanzenbach, Dietrich Vollrath, employees of an anonymous large urban school district and participants at seminars at the Council of Economic Advisors the University of Minnesota, the APPAM Conference and the Stata Texas Empirical Microeconomics Conference for helpful comments and suggestions. All errors remain our own.

1. Introduction

Nutritionists have long been concerned that children in the United States do not consume enough breakfast. Indeed, it is estimated that between 12% and 35% of children in the U.S. skip breakfast (Gardner, 2008) despite the provision of free or low-cost breakfasts in school for low-income students through the School Breakfast Program (SBP) since 1975. Unfortunately, participation in the SBP is low. At most 60% of students eligible for free breakfast participate in the program (Dahl and Scholz, 2011). This could be due to "time/scheduling conflicts, [lack of] cafeteria space or the embarrassment associated with eating a free or reduced-price breakfast" (Cullen, 2010). Further, while the intent is for the SBP to increase breakfast consumption, it is possible that children with access to an SBP may eat less at home. Waehrer (2008) looks at time-diary data and finds that children in the School Breakfast Program actually consume less on weekdays than weekends suggesting that the program may reduce consumption, although it is not clear what the students would have eaten on weekdays in the absence of the program. Finally, even if students come from families that can afford to purchase breakfast and are thus ineligible for SBP, many children skip due to time constraints.

Due to the low take-up of free and reduced-price meals and clinical evidence that breakfast improves cognitive performance (Alaimo, Olson and Frongillo, 1999; Middleman, Emans, and Cox. 1996; Wesnes, Pincock, Richardson, Helm, and Hails, 2003), a number of school districts around the country have tried to reduce the time and effort costs of eating breakfast by moving the meals from cafeterias to the classrooms. School districts in Chicago, Dallas, Florida's Orange County, Houston, Little Rock, Maryland's Prince George's County, Memphis, New Mexico, New York City, and San Diego have all moved breakfast to classrooms. Providing breakfast in class avoids space and scheduling problems, reduces time constraints on students in the morning and eliminates the need for students who want to eat breakfast in school to arrive early. In addition, given that breakfast is offered to all students and not only those who come to the cafeteria in the morning, the stigmatization from getting a subsidized meal is plausibly eliminated. Hence, school districts hope that this leads to increased consumption of breakfast that in turn increases students' alertness and their capacity to learn. However, there are some potential disadvantages. First, it is possible that students will "double up" and eat breakfast both at home and at school, which could contribute to obesity. Second, there could be increased financial costs in providing breakfast in the classroom due to higher consumption and clean-up costs. Third, there are concerns that the time it takes to serve and eat the breakfast may reduce instruction time, though surveys of principals conducted by the district we study suggest the impact on instructional time is minimal.

In this paper, we assess the impact from the implementation of a free "in-class breakfast" (ICB) program in a large urban school district in the Southwest United States (LUSD-SW) on students' academic performance and attendance relative to providing free breakfast in the cafeteria.[2] LUSD first piloted the project in 33 schools and later expanded it to all elementary and middle schools starting on February 2nd, 2010 with the roll-out finishing in Fall 2010. A nice feature of the roll-out for the purposes of our empirical analysis is that the timing of implementation had little to do with school characteristics. While the roll-out was initially aimed to start in schools with a higher share of disadvantaged students, in practice the implementation did not work out this way. First, some schools had rollout dates changed to accommodate logistical necessities (i.e., having schools in the same areas start around the same time) or principals' requests. Second, and perhaps more importantly, 65% of elementary schools in

[2] Researchers seeking access to the data for replication should contact the authors, at which point we will identify the district for the requestors and provide instructions for how to submit a research proposal to the district's research department.

LUSD have economic disadvantage rates of 90% or higher.[3] As a consequence, during the first 11 weeks of the program rollout, there is remarkably little variation in terms of economic disadvantage rates and other characteristics of the schools across implementation dates. For example, the mean economic disadvantage rate in 2008-09 for schools adopting in week 1 is 93.5% while the mean in week 11 is 91.5%. This indicates that schools that implemented the program early differed little from later adopters as measured by observable characteristics.[4] More importantly given our difference-in-differences identification strategy, early and late adopters have virtually no differences in trends.

We test the validity of the difference-in-differences assumptions in three ways. First, we show that the timing of adoption is mostly uncorrelated with school characteristics and changes in those characteristics conditional on the school adopting in the first 11 weeks of implementation. Second, we estimate placebo tests in the spirit of Angrist and Krueger (1999) that estimate the difference-in-difference "impact" of adoption using only pre-ICB data. This checks for whether underlying trends may be influencing our results. These tests provide little evidence of any such trending. Third, we test whether there are difference-in-differences "impacts" on contemporaneous exogenous covariates and find very few significant effects.

Using schools that adopt during weeks 1 through 11, a time period that covers the testing days for the 5th grade state exam in week 9, we assess the impact of providing breakfast in class on achievement, grades and attendance. We find that achievement increased in schools that adopted ICB before testing compared to schools where ICB was adopted after testing. In

[3] A student is considered economically disadvantaged if she qualifies for free-lunch, reduced-price lunch or another Federal or state anti-poverty program.

[4] A small portion of schools in LUSD have relatively low disadvantage rates. These schools mostly began their programs after the 11th week of the roll-out. As a consequence schools that adopted in the 12th week and later differ substantially from those that adopted earlier. Hence, we only consider schools adopting during the first 11 weeks of the program in our analysis.

particular, the introduction of breakfast in the classroom increased test scores by 0.09 standard deviations in math and 0.06 in reading. Moreover, these effects were larger for students with low pre-program achievement, those who qualified for free lunch, Hispanics, children with limited English proficiency, and students with a low body-mass index (BMI). However, we find little evidence that impacts vary with exposure-time and we also find little evidence that ICB affected attendance or grades. Together, these findings suggest that the program likely helps students perform better on exams but does not appear to affect learning. This is indicative of a phenomenon similar to that found in Figlio and Winicki (2005) whereby schools increased calories in meals to improve test performance. Nonetheless, our evidence only provides suggestive support for this theory since, due to the short implementation window, we cannot rule-out longer-term effects of exposure time and the lack of grade impacts could reflect teachers adjusting their grading curves as students improve. Even short-term impacts, however, have important policy implications. The results imply that achievement scores in schools with low breakfast consumption may suffer from a downwards bias relative to high consumption schools. As a result, providing universal in-class breakfast may avoid situations where schools with large disadvantaged populations suffer accountability penalties and, as a result, receive fewer resources because their students have not received appropriate nutrition on testing days.

2. Previous Literature

There is an extensive literature on the link between nutrition and education in the developing country context. In general, the studies that are able to establish causal effects tend to find that good nutrition increases educational performance (Alderman, Behrman, Lavy and

Menon, 2001; Alderman, Hoddinott and Kinsey, 2006; Glewwe, Jacoby and King, 2001; Maluccio et al., 2009; Vermeersch and Kremer, 2004).

In contrast to studies in the developing world, most research on the effects of nutrition on learning in developed countries has been conducted by physicians and public health experts.[5] The most credible studies for developed countries have involved experimental trials which randomly assigned kids to receiving breakfast or no breakfast in a given week and the following week switched the assignment. The best of these include Pollitt, Leibel and Greenfield (1981) and Pollitt, Lewis, Garza and Shulman (1982) who find little impact of the change in diet on cognitive performance.

Most research on the impact of food programs in school focuses on how these affect non-academic outcomes such as nutrient intakes (Devaney and Fraker, 1989; Gleason and Suitor, 2003; Grainger, Senauer and Runge, 2007), food expenditures (Long, 1991), food insecurity (Bartfeld and Ahn, 2011; Gundersen, Kreider and Pepper, 2012**),** obesity (Anderson and Butcher. 2006; Hofferth and Curtin. 2005; Gundersen, Kreider and Pepper, 2012; Millimet, Tchernis and Hussein, 2010; Schanzenbach, 2009), and other health outcomes (Bernstein, McLaughlin, Crepinsek, and Daft, 2004; Bhattacharya, Currie and Haider, 2006). Other studies that consider the effects of these programs on academic outcomes in the U.S. use non-experimental data and for the most part do simple comparisons of schools participating in the School Breakfast and Lunch programs to non-participating schools. While some of these studies do not control for other school and student characteristics in the participating and non-participating schools, others attempt to control for observable differences. However, even controlling for observable differences may not be enough, since schools may self-select into participating in the program on

[5] See Rampersaud, Pereira, Girard, Adams and Metzl (2005) for a review.

the basis of unobservable characteristics (e.g., local wealth levels).[6] Likewise, other studies compare children eligible for free/reduced price meals to those not eligible, but these students differ on the basis of unobservable characteristics both at the school and student levels. One study by Meyers et al. (1989) finds that the Massachusetts school breakfast program is associated with higher test scores and lower levels of tardiness and absences but does not control for the selection described above. Dunifon and Kowaleski-Jones (2003), on the other hand, address potential selection into free/reduced price meal eligibility by comparing children within the same family one of whom attends a school with a school meal program while the other attends a school without a program. While there may still be the problem that parents send the children with greater nutritional needs to a school with a meal program while sending the other child to a school without a program, this study reports that the likelihood of split participation is not associated with improved child-specific factors (including health status or BMI).

Studies on the impact of school meal programs on student outcomes using natural experiments tend to find positive effects. Hinrichs (2010) exploits differences in eligibility rules across cohorts and states for free/reduced-price lunch. He finds that those who participated in the program as children experienced sizable and significant increases in educational attainment. In a recent paper, Frisvold (2012) exploits threshold variation across states and schools in the percent of free and reduced-price eligible students that mandate schools to adopt breakfast programs and finds positive effects on student achievement. Additionally, Leos-Urbel, Schwartz, Weinstein and Corcoran (2012) look at the implementation of universal free breakfast in New York City and find increased uptake and small positive effects on attendance for blacks and Asians. The exception to this pattern of positive results is Bernstein, McLaughlin, Crepinsek and Daft (2004)

[6] For example, Anderson and Butcher (2006) find that schools under financial pressure tend to adopt potentially unhealthy food policies.

who conducted randomized experiments in six school districts across the country that provide breakfast in some schools but not others. They find little evidence of improvements in terms of achievement, attendance or discipline.[7]

Our analysis is closest to Hinrichs (2010), Frisvold (2012) and Leos-Urbel, et al. (2012) in that we conduct a non-experimental analysis exploiting the differential timing in a change to a breakfast program in schools in a large urban school district in the Southwest United States to identify the effect of providing breakfast to all students in the classroom relative to providing breakfast to students in the cafeteria, and hence reducing the time and effort costs for students to consume breakfast, on student performance, including grades, test scores and attendance. To our knowledge, the only quasi-experimental study of in-class breakfast implementation is unpublished work by Dotter (2012). He examines a universal in-class breakfast program in San Diego and finds effects on test scores of 0.10 to 0.15 standard deviations but does not find impacts on student behavior. However, in schools that already offered free breakfast in the cafeteria – which is the case for all schools in LUSD - he finds no significant effect, in contrast to our findings of positive effects, despite finding increased participation in both these schools and those that did not previously provide universal breakfast. Given that many school districts are adopting these programs it is important to assess what impact, if any, they might have.

3. The In-Class Breakfast Program

[7] About 20 percent of the schools in Bernstein et al. (2004) provided breakfast in the classroom, but ultimately whether to provide breakfast in the classroom or the cafeteria was decided by the principal and, hence, that portion of the study was not randomized. As a result, their estimates on this treatment are potentially biased. Nonetheless, while they find greater take-up of breakfast when it is served in the classroom than when it is served to all students in the cafeteria, they find no significant impact of the in-classroom breakfast on achievement.

The LUSD in-class breakfast program provides free breakfast to all students in their classrooms at the beginning of the school-day. Prior to the introduction of the in-class breakfast program, all students were able to get a free breakfast in the cafeteria before the start of class.[8]

Thus, there are a few simultaneous changes as a result of the introduction of the in-class breakfast program. First, the venue of the breakfast was changed to the classroom, making it easier to get breakfast. Second, the timing of the breakfast was changed to the beginning of the school day, rather than before the start of class, potentially reducing instructional time. However, in interviews conducted by the school district, 93 percent of principals report that breakfast is administered in less than 15 minutes. Hence, it is unlikely that the ICB program reduces instructional time as the period of time during which breakfast is served is typically used by teachers and school officials to take attendance, provide announcements, collect homework, and plan the rest of the day, activities that can all be done concurrent with breakfast consumption. At the same time, it is possible that under the cafeteria-based system some students who wanted to eat breakfast could not do so without being late to class.

Thus, we interpret this movement of breakfast from the cafeteria to the classroom as a reduction in the cost to the student of consuming breakfast. First, the change in venue implies a reduction in time cost and an increase in convenience since the student does not need to arrive at school early or walk to the cafeteria. Second, although all students had access to cafeteria breakfast prior to ICB, some stigma effects from eating breakfast in the cafeteria may have remained. These reductions in costs likely led to an increase in calorie consumption on average. Unfortunately, we do not have the data to test directly for whether students consume more

[8] LUSD started providing free breakfast to all students, not just those eligible under the National School Breakfast Program, in 2006-07.

calories.[9] However, while it is feasible that some students may not change their behavior, it is likely that serving breakfast in the classroom will cause other students to consume more food overall. First of all, Bernstein et al. (2004) report that student take-up of breakfast at schools that provide free universal breakfast in the classroom is higher than in schools that provide free universal breakfast but serve it in the cafeteria. Second, a comparison of 33 pilot schools in the LUSD, which began providing in-class breakfast in 2008-09,[10] to non-pilot schools in the district found that while 80% of students in the former ate breakfast in school during 2008-09 only 41% of the latter did so.[11] Third, we know from six case studies of elementary schools conducted by the district before and after the 2009-10 implementation of the in-class breakfast program that take-up of breakfast increased from 33% to 83% in these schools. While these schools are more disadvantaged than the average school in the district, they are similar to schools in our sample as our identification strategy requires us to focus on very low-income schools. Further, one of the schools is similar to the average elementary school on observable characteristics. In this school uptake increased from 29% to 73%. All together this evidence suggests that uptake increased by between 40 and 50 percentage points, on average.

The caloric content of the breakfast served in LUSD hardly changed after the in-class breakfast program was introduced, so any change in calorie intake would come from increased breakfast consumption. Students are given an entrée that could be hot or cold (i.e. yogurt, chicken biscuit, pop-tart, mini pancakes, etc.) usually with a snack item (i.e. fruit, blueberry

[9] Food consumption data is collected by the food service department of the school district and is not linked to student data. Moreover, the food consumption information collected by the district's food department only includes breakfast and lunch consumed at the school and would not provide information on whether the students ate breakfast at home or the total calories consumed by the student during a day.

[10] We exclude these 33 pilot schools from all estimates of the impact of the in-class breakfast program as they were chosen on the basis of where food administrators thought the program could be best implemented.

[11] It is not obvious whether this is due to more students eating breakfast who weren't before, since some of the new in-school eaters may have been eating breakfast at home or the difference may reflect selection of schools into the pilot program. Nonetheless, given that principals entered the pilot voluntarily it is likely they did so in response to low in-school consumption, which would imply that this is an underestimate of the actual effect on take-up.

muffin, graham crackers), a juice and milk. On average a student could consume up to 534 calories from the breakfast. This is comparable to the meals offered in the cafeteria before ICB implementation where a student could consume up to 520 calories on average.[12] In addition, a comparison of available caloric information of breakfast served in the cafeteria and breakfast served in the classroom on exam days shows little difference in the menus.[13] Rather, it seems that it is increased consumption that changed after the introduction of the in-class breakfast program.

In 2009-10 LUSD started implementing the program in the non-pilot schools. All but a handful of elementary schools started in that year, while the rest of the elementary schools and secondary schools began ICB early in the 2010-11 school-year. Given this timing, we only assess elementary schools in this study. The initial intention was to implement the program in new schools each week starting with the schools with the highest rates of economically disadvantaged students and ending with the lowest. However, in practice the implementation did not occur this way. Adoption dates were modified for a number of logistical reasons such as principal requests for delays or to facilitate initial food deliveries. This combined with the fact that 65% of LUSD elementary schools had economic disadvantage rates above 90% made schools that adopted during the beginning of the 11 week period from February 2, 2010 to April 20, 201 remarkably similar on observable characteristics to those that adopted towards the end of the period. Hence, we argue that the implementation was quasi-random and we identify treatment effects using a difference-in-differences framework. That is, we assume that the timing of adoption during the first 11 weeks of the roll-out is uncorrelated with trends in unobserved school characteristics

[12] Authors' calculations from school menus and nutrition information.

[13] On April 6, 2010, the day of math testing, schools serving breakfast in the cafeteria served 4 items out of French toast sticks, cereal, Colby omelet, fruit and juice, while schools serving breakfast in-class served waffle sticks, animal crackers, orange juice and milk. On April 7, 2010, the day of reading testing, schools serving breakfast in the cafeteria served 4 out of the following items breakfast taco, cereal, skillet brown potatoes, fruit and juice, while schools serving breakfast in-class served mini pancakes, animal crackers, juice and milk.

conditional on observable student characteristics, observable school characteristics, and prior achievement.

Table 1 provides support for this assumption. In this table we provide some characteristics of elementary schools that adopted ICB at different times. New schools implemented the program every week from February 2, 2010 through September 21, 2010 with gaps during testing periods, spring break and summer break. This table shows that amongst schools that implemented the program from February 2, 2010 through April 20, 2010, the week of adoption is uncorrelated with many observable characteristic, including percent of students economically disadvantaged, black, Hispanic, with Limited English Proficiency, average teacher experience and tenure, student-teacher ratio, and attendance in the 2008-2009 school year.[14] Joint significance tests only show significant differences across weeks in per-pupil expenditure and reading scores. Tests comparing weeks 1 through 8 (schools that adopt prior to testing) to weeks 10 and 11 (post-testing adopters) also show few differences in characteristics between schools. A notable exception is economic disadvantage rates which are 3.1 percentage points lower in post-testing adopters. However, while statistically significant, this difference is economically small and we control for this and other time-varying school characteristics in our empirical analysis below.

More importantly for our difference-in-differences identification strategy, Panel B of Table 1 shows that the schools in our main sample where the program was introduced between February 2 and April 20, 2010 do not differ in terms of changes between 2006-07 and 2008-09 in any of the above mentioned characteristics. T-tests of differences between pre- and post-testing adopters show no significant differences nor do joint F-tests show significant differences across

[14] One concern may be that the district implemented the program first in schools at risk of sanctions from accountability regimes. However, none of the schools in our sample received the state's lowest accountability rating in the year prior to implementation and thus none were subject to sanctions.

the first 11 weeks. This suggests that initially the program was introduced in a close to random manner, at least conditional on fixed school characteristics. Later, we test this assumption further through estimates of impacts on exogenous covariates and placebo tests that look for impact estimates using only pre-implementation data.

4. Estimation Strategy

To implement our difference-in-differences strategy, we estimate the following regression to look at the effects of the ICB program on student achievement:

$$\mathrm{Y_{ijt}} = \alpha + \beta \mathrm{Post_t} \times \mathrm{ICB_j} + \gamma_1 \mathrm{Y_{ijt-1}} + \gamma_2 \mathrm{Y_{ijt-2}} + \psi_j + \tau_t + \mathbf{\Omega X_{ijt}} + \Gamma \mathrm{Z_{jt}} + \varepsilon_{ijt}. \quad (1)$$

where Y_{ijt} is student test scores, grades, or absenteeism for student i, in school j, at time t. X_{ijt} includes race, gender, and indicators for whether the student qualifies for free lunch, reduced price lunch or is otherwise economically disadvantaged, and grade fixed-effects. The regression also controls for school characteristics, Z_{jt}, such as the percent of students of each race/ethnicity in the school, economically disadvantaged, of limited English proficiency, in special education, in bilingual education, in each grade level, or referred to an alternative disciplinary program. Moreover, we include school fixed effects, ψ_j, to control for time-invariant unobservable characteristics of the schools, such as the quality of the teachers and principal, and we also include time fixed effect, τ_t, to control for time-varying factors that affect all schools. Finally, we include two lags of the dependent variable to capture persistence in achievement, grades and attendance and to help account for any pre-existing trends.[15]

[15] Andrabi, Das, Khwaja and Zajonc (2011) show that it is important to add lagged achievement to education production functions in order to account for persistence in achievement. When we estimate models with a single lag

This specification makes our analysis a "difference-in-differences" model where changes in outcomes before and after program implementation for earlier adopters are compared to changes in outcomes for schools that adopt late in the process. The difference-in-differences impact of the program is captured by the estimate for $Post_t \times ICB_j$ which is an interaction of a dummy for being in a period after the introduction of the program, $Post_t$, with an indicator of whether the school implemented the program prior to 5th grade testing in 2009-10, ICB_j (i.e., an indicator for whether the ICB was implemented in weeks 1 through 8). For test scores, 5th grade students took the state accountability exams in reading and math on April 6 and 7. Hence, for these students, we will estimate equation (1) by comparing schools where ICB started prior to April 6 to those where it started afterwards but before April 27.[16] Since it is unclear whether schools that implement during the week of April 6 provide the program to 5th grade students due to the testing, we drop all schools that adopt during this week (i.e., week 9) from our analyses.

The difference-in-differences framework described above only requires that trends for early adopters do not differ from trends for late adopters conditional on observable controls. Hence, we argue that the implementation is quasi-random in the sense that it is unrelated to underlying trends once we control for student characteristics, school characteristics, and two achievement lags. Below, we provide evidence that indicates the program implementation satisfies this assumption.

The difference-in-differences analysis can be extended to include the duration of exposure to the in-class breakfast program, $ICB\_Duration_{jt}$, or intensity of treatment as follows:

---

our estimates are slightly larger but we also find a slight increase in achievement in pre-test adopters relative to post-test in the year prior to implementation. Thus, we add a second lagged achievement score to the regressions to account for this trend.

[16] Since 3rd and 4th graders took the exam on the week of April 27th, we cannot estimate this model for them.

$$Y_{ijt} = \alpha + \beta_1 Post_t \times ICB_j + \beta_2 ICB_{Duration_{jt}} + \gamma_1 Y_{ijt-1}$$
$$+ \gamma_2 Y_{ijt-2} + \psi_j + \tau_t + \mathbf{\Omega X}_{it} + \Gamma Z_{jt} + \varepsilon_{ijt} \quad (2)$$

After controlling for student characteristics, school characteristics, and school fixed-effects we may expect students in schools that have participated longer in the ICB program to have improved nutrition and to have better achievement. If this is true then the estimate for β_2 should be positive and significant. On the other hand, it is possible any benefits accrue merely from a "day of testing" effect whereby the extra calories boost concentration on the exam but do little to improve general learning. In this case, we should see statistically insignificant estimates of β_2 that are close to zero. We will also provide estimates from a more flexible version of model (2) as follows

$$Y_{ijt} = \alpha + \sum_w \beta_w Post_t \times ICB\_Week_{wj} + \gamma Y_{ijt-1} + \delta Y_{ijt-2} + \psi_j + \tau_t + \mathbf{\Omega X}_{it} + \Gamma Z_{jt} + \varepsilon_{ijt} \quad (3)$$

where *w* is the week of implementation and ICB_Week_{wj} is an indicator for school j adopting during week *w*. This version of the model allows us to track the impact estimates from week to week as the program is implemented. Finally, since the availability of breakfast is unlikely to affect all students equally, and in particular is likely to have a bigger impact on low socio-economic status (SES) students, we provide analyses that split the sample by economic status, ethnicity, gender, LEP status and prior achievement, which will allow us to test whether the impact of the ICB program varies for different types of students. Further, we are able to test for differences in impacts by students' BMI for a subset of schools in 2008-09 and 2009-10.[17]

Since grades and attendance accrue continually, we use modified versions of equations (1) and (2) for these outcomes. Since there are four grading periods and six attendance periods during the school-year, in these cases we include grade level-period fixed-effects instead of year fixed-effects as we have both within-year and across-grade variation. This accounts for differences across grades in each time period as well as differences across time periods due to, for example, students becoming restless as the December holidays approach or becoming more likely to skip school as the school-year ends. We consider a school to be treated if it adopts ICB at any point during the grading/attendance period. Nonetheless, this may be a poor measure of exposure as a student who is exposed to ICB for the full period may be affected more than one who is exposed only for part of the period. Thus, our focus is on the duration model in equation (2). We also estimate the following model:

$$\mathrm{Y_{igjt}} = \alpha + \beta_1 \mathrm{FullyTreated_{jt}} + \beta_2\, \mathrm{PartiallyTreated_{jt}} +$$

$$\gamma_1 \mathrm{Y_{igjt-1}} + \gamma_2 \mathrm{Y_{igjt-2}} + \psi_\mathrm{j} + \tau_\mathrm{gt} + \boldsymbol{\Omega}\mathbf{X}_\mathrm{it} + \Gamma \mathrm{Z_{jt}} + \varepsilon_\mathrm{igjt} \quad (4)$$

where $\mathrm{FullyTreated_{jt}}$ is an indicator set equal to one if school j is treated for all weeks of period t while $\mathrm{PartiallyTreated_{jt}}$ equals one if the school was treated for some, but not all, weeks of period t. Both of these values are set to zero in any period prior to implementation.

[17] Although it would be informative to examine the impact of the in-class breakfast program on nutrition, we are not able to do this with the data we have. This is because while we have BMI data for 2009-10, most of the data were collected between January and April of 2010. Thus, at best, some students would have had only a few weeks of exposure to ICB when the data were collected. Further, the BMI data comes from height and weight information gathered as part of a physical examination conducted in some but not all schools. Unfortunately, more detailed nutrition information on calories consumed by students is not available.

We note however that a key limitation of our study is that we do not have access to data on the actual uptake of the breakfasts. Thus, we are limited to a reduced-form analysis based on the intention to treat rather than the treatment effects. Nonetheless, as mentioned above, the district's non-random pilot study found that uptake in pilot schools was double that of non-pilot schools and the case studies found that take-up increased 250% in more disadvantaged schools. Thus, this will allow us to generate back-of-the envelope calculations as to the effect of ICB on students who are induced to eat breakfast in school (e.g. effect of treatment on the treated).

5. Data Description

Our data comes from student records in a large urban school district in the Southwest U.S. (LUSD-SW). The district is one of the largest in the country with over 200,000 students. Given that the program implementation only overlapped with the testing for elementary students, we focus on students in grades 1 to 5. Testing data covers the 2002-03 through 2009-10 academic years, however we start our analysis with 2004-05 in order to allow for the inclusion of two achievement lags in our test-score regressions. For our other outcomes – grades and attendance – the data we have is more limited, with only 2008-09 and 2009-10 available for grades and 2009-10 for attendance rates.

Testing data comes from the state accountability exams in math and reading. These exams are "high stakes" in that the scores determine whether the students are permitted to advance to the next grade as well as the school's accountability rating and whether the school meets "Adequate Yearly Progress" under the No Child Left Behind Act of 2001. Students can take the exam multiple times until they pass. Unfortunately, we do not know whether a given exam score is from the first or a later administration. Hence, we use the student's minimum score in a subject in a given year as their achievement score under the assumption that, since students who fail tend to get extensive coaching for retakes, the lowest score is most likely from the student's first sitting. We then use these scores and standardize them within grade and year across the district.[18]

In addition to achievement, the data provides some other student outcomes. In particular, we assess the impact of the breakfast program on attendance rates within each six week attendance period in 2009-10 and the student's mean grade across all subjects in each nine week grading period in 2008-09 and 2009-10.[19] Finally, we have information on student demographics including race, gender, economic status, limited English proficiency, at-risk status, gifted status, and special education, along with BMI data for a subset of schools in 2008-09 and 2009-10.[20]

Table 2 provides summary statistics of students in 2009-10. We limit the sample to schools that started ICB between February 2 and April 27, 2010 excluding schools that adopt during the week of fifth grade testing (week 9) as it is unclear whether fifth grade students in these schools become treated before or after testing. We then separate our data into three samples for each of the outcome measures we assess – achievement, grades and attendance. As described above, we are limited to fifth grade students for achievement while our data covers grades 1 to 5 for attendance and grades. Nonetheless, the student characteristics are relatively similar across the samples. LUSD is a heavily minority district with 87% of students being Hispanic or black, but our subsample schools are more heavily minority as only 3% of students are not black or

---

[18] While it is more common to use scale scores in the standardization, the state changed the scaling procedure in 2009-10 from a horizontal to a vertical scaling regime making the scale scores in that year incomparable to prior years. Hence, we rely on raw scores for our standardizations.

[19] While it would be interesting to see the impact of the breakfast program on behavior, unfortunately the only measure of disciplinary incidents available to us – the number of in-school suspensions or more severe punishments a student receives – is too infrequent in elementary student populations to identify effects.

[20] A student is considered at-risk if he or she is low-achieving, has previously been retained, is pregnant or a parent, is LEP, has been placed in alternative education or juvenile detention, is on parole or probation, is homeless, or has previously dropped out of school.

Hispanic. This is not surprising, given that our subsample is limited to schools with high economic disadvantage rates, as is evidenced in the next row showing that 94% of students are disadvantaged. Further, a large majority of students are Hispanic rather than black. The schools also have high rates of limited English proficiency. The students in this sample are much more likely to be minority and to qualify for free/reduced price lunch, but they are also less likely to be classified as special education kids and more likely to be classified as gifted and talented.[21]

In total, we have 6,353 students and 84 schools in 2009-10 in the achievement sample, 37,309 students in 87 schools in the grades sample and 38,425 students in 87 schools in the attendance sample.[22] Our total estimation sample covers 2004-05 through 2009-10 for achievement, 2008-09 through 2009-10 for grades and 2009-10 only for attendance. They include approximately 30,700 math and 25,400 reading student-year observations for achievement regressions,[23] 188,700 student-grading period observations for grades regressions and 149,000 student-attendance period observations for attendance regressions.

6. Effects of In-Class Breakfast on Achievement, Grades and Absenteeism

6.1. Effects on Student Achievement

To see how the achievement in post-testing adopters compared to pre-testing adopters evolves over time, in Figure 1 we present estimates from the following regression model:

$$\mathrm{Y}_{\mathrm{ijt}} = \alpha + \sum_{t=2005-06}^{2009-10} \beta_t \mathrm{Year}_t \times \mathrm{ICB}_j + \gamma_1 \mathrm{Y}_{\mathrm{ijt-1}} +$$

[21] In 2010, the Digest of Education Statistics reports that on average 54.9% of students in the US are white, while only 17% are black and 21.5% are Hispanic. This source also reports that 44.6% of students qualify for free/reduced lunch, 13.2% are designated as requiring special education, and 6.7% are classified as gifted and talented at the national level.

[22] The difference in the number of schools in these samples reflects a handful of schools in LUSD that serve only grades K-3.

[23] For the math sample, 3,930 observations are in treated schools after implementation while 18,651 are in treated schools prior to implementation. For reading those counts are 3,937 and 14,657, respectively.

$$\gamma_2 Y_{ijt-2} + \psi_j + \tau_t + \mathbf{\Omega X}_{ijt} + \Gamma Z_{jt} + \varepsilon_{ijt}. \quad (5)$$

Each estimate of β is plotted along with its 95% confidence interval. 2004-05 is the left-out category. If the parallel trends assumption underling our difference-in-differences strategy is correct we should see no significant difference in achievement up through 2008-09. For math this is clearly the case. For reading, some of the estimates are significantly greater than zero, indicating an increase in achievement relative to 2004-05. However, from 2005-06 through 2008-09 the estimates do not significantly differ from each other and remain close. Thus, we see little to indicate a persistent trend after 2004-05, though there is a slight uptick in achievement in 2008-09. Later we will also show that our results are robust to limiting the sample to later years indicating that neither the increase from 2004-05 to 2005-06 in reading nor the slight increases in 2008-09 are substantial concerns. Given those results, we use all years in our analysis in order to improve precision. Looking at the change from 2008-09 to 2009-10 in the figure, we see an increase in achievement consistent with the main regression results we provide below.

Table 3 shows the results of regressions using equations (1) and (2) for test scores.[24] Panels A.I and B.I provide results from the basic difference-in-differences regressions for math and reading, respectively. Column (1) shows that, on average, the impact of ICB is to increase test scores by 0.09 standard deviations in math and 0.06 in reading (both significant at the 10% level). In panels A.II and B.II, we provide estimates that allow the impacts to vary by week of adoption. This specification is useful in determining whether the impacts are likely due to actual learning gains by students or if the breakfasts are simply increasing students' test-taking performance. If the former is true, then we would expect to see larger achievement impacts for students in early adopting schools than for late adopters. Nonetheless, the estimates in panels

[24] Standard errors are clustered by school. In Online Appendix Table 1, we repeat the analysis in Table 3 excluding school fixed effects and find similar results.

A.II and B.II suggest little difference by exposure to treatment. The point estimates on the weeks of exposure interactions with being in the post ICB period are insignificant and close to zero. In Figure 2, we provide graphs that show point estimates and 95% confidence intervals using equation (3) as the regression model. This figure shows whether any differences by exposure time can be discerned using a less restrictive model. There is little indication of variation by weeks of exposure. Although somewhat noisy, the week-by-week estimates appear to be centered a bit below 0.1 standard deviations in both subjects throughout the implementation period and show little evidence of trending. Thus, the estimates shown here along with those in panels A.II and B.II suggest that the impacts are due to improvements in exam performance but not necessarily from learning itself.[25] Later we provide evidence on course grades that corroborates this. Nonetheless, we caution that the implementation period is only two-months long. Further, a substantial portion of instruction during this period is focused on test preparation specifically. Hence, it is possible that there are learning effects, but they are only detectable over longer time periods.

In columns (2) through (8) of Table 3 we provide estimates that split the samples by the once lagged achievement levels for each student, first by whether the student is above or below the median achievement score and then by the student's achievement quintile. The results indicate that the achievement effects for math found in Column (1) are primarily coming from students who were low achievers prior to program implementation. For those students who score below the median in the previous year the effects size is an increase of 0.12 standard deviations. On the other hand, students who have above median prior achievement have a smaller and insignificant effect size of 0.06 standard deviations. Nonetheless, the below and above median

[25] Figlio and Winicki (2005) show that schools recognize the potential for extra consumption to improve achievement and, thus, increase calorie counts of in-school meals during testing weeks.

estimates do not statistically significantly differ from each other, so we take these results as suggestive rather than conclusive. For reading, the results are similar for low and high achievers. Similarly, Columns (4) through (8) provide estimates separated by prior achievement quintiles in which the point estimates for lower quintiles in math are generally higher than those for the upper quintiles. Finally, we also provide estimates that interact treatment status with exposure time for these models in panels A.II and B.II. As with the pooled estimates, there is little to indicate differences by week of adoption. Further, in Figure 3 we repeat the analysis shown in Figure 2 but split the samples by whether the students are above or below the median achievement level. This figure indicates that the impacts differ little by time of exposure regardless of the students' achievement levels.

One potential concern is that if the slight increase in reading in 2008 seen in Figure 1 is not natural variation, we might be over-estimating the impact. Hence, we also estimate the impacts limiting the sample to 2008 and 2009. Since we only have two years of data for this analysis we leave out school fixed effects. The estimate for math falls to 0.041 (standard error of 0.059). While this estimate is not statistically significant at conventional levels, it nonetheless remains positive. For reading, the estimate changes only slightly to 0.078 (0.046).

In Table 4, we provide results that examine whether there are heterogeneous effects of ICB on different groups of students. Columns (1) and (2) show no differences between boys and girls in the effects of the ICB on math and reading test scores. However, when we further split the sample by whether the students are high or low achievers in Columns (3) through (6) we find that the impacts on girls are heavily concentrated among low achievers. For boys, the results differ for math and reading exams. By contrast, the effects sizes for racial/ethnic groups clearly differ across groups. Columns (7)-(9) show that the ICB increased test scores for Hispanics by

0.12 and 0.09 of a standard deviation in math and reading but essentially had no impact on blacks.[26] For white students, there are too few observations for reasonable precision in the estimates.

This finding for blacks and Hispanics is interesting as it indicates that Hispanics were probably more likely to adjust their consumption patterns in response to the breakfast program than black students. Unfortunately, we can only speculate as to the reasons for this racial differential. One possibility is that black students in LUSD are less affected by stigma effects and hence were already eating in the cafeteria prior to program implementation. Another possibility is that LUSD black students are more likely to eat breakfast at home than Hispanic students. A third possibility is that being Hispanic is correlated with other risk factors for malnutrition at a higher rate than being black.

To explore this further, in Columns (10) and (11) we examine differences in economic status by free lunch and non-free-lunch eligibility.[27] This effectively separates the sample by those students from families with incomes below 130% of the Federal poverty line (eligible) and those above that income level (not eligible). While overall economic disadvantage rates for blacks and Hispanics in our sample are similar, 91% and 95%, respectively, eligibility rates for free lunch are 61% and 73% for blacks and Hispanics, respectively. Although they do not statistically significantly differ, the results suggest the ICB program has a bigger effect on math scores for those who are eligible. Nonetheless, in results not shown here, when we split the sample by both free-lunch status and race we still find the same racial gap between Hispanics and blacks regardless of free lunch status, so this does not appear to be an explanation for the gap.

[26] This difference is statistically significant at the 10% level for math and at the 5% level for reading.

[27] Unfortunately, since we have so few students in the sample who are not economically disadvantaged we cannot analyze differences along this dimension.

Turning to other demographic characteristics, Columns (12) and (13) show that, not surprisingly given the results for Hispanics, students with limited English proficiency also benefit more than non-LEP students.[28] Finally, in Columns (14) through (17) we look at whether impacts differ by body mass index. The BMI levels for each student come from height and weight taken during physical fitness tests at the end of the 2008-09 school-year. Unfortunately, the BMI data is only available for a subset of 5th grade schools.[29] Further, since we only have one pre- and one post-adoption year for this analysis we do not include school fixed-effects.

Since the relationship between BMI and obesity differ by age for children we classify the students into four categories based on the Centers for Disease Control's BMI-for-age values and the student's age in months. The four categories are low BMI (children are below the 25th percentile of weight for the CDC base year), medium weight (25th to 84th percentile), overweight (85 to 94th percentile) and obese (≥ 95th percentile). Note that the first two categories are not the same as those used by the CDC which are underweight (< 10th percentile) and healthy weight (10th to 84th percentile). We make this change to ensure we have sufficient power to detect effects since we have very few observations that would be classified as underweight.

The results indicate that in-class breakfast has a larger positive impact on children with low BMI. In particular, we find that math scores are significantly higher for these students with a point estimate of 0.24 standard deviations. Further, the low BMI estimate significantly differs from a pooled estimate for all other BMI categories. For all other weight categories the estimates are much smaller and statistically insignificant. For reading the results for low BMI are similar,

[28] We also investigate differences within sub-groups by high and low achievers. Unlike the gender results, there are only a few differences by achievement for the other estimates provided in Table 4. For whites, while the sample size is very small, we see a significant effect on low achievers at the 10% level. Further, reading impacts are bigger for high-income low achievers and low-income high achievers. These results are provided in Online Appendix Table 2.

[29] There is a small relationship between the likelihood of a school having BMI data available and being an early adopter. In particular, schools that adopt prior to week 10 are 8 percentage points more likely to have BMI data available than those that adopt in weeks 10 or 11. This relationship is significant at the 10% level.

but the point estimates for other categories are larger than those for math. One might think that this could also be a potential explanation for the racial gap if Hispanics are more likely to have low BMI. However, the opposite is, in fact, true. Low BMI rates in our sample are 8% for Hispanics and 11% for blacks. We also note that low BMI does not necessarily correlate with low nutrition in our sample. Indeed, in many cases, children who are malnourished tend to be obese as their malnourishment comes from low quality food rather than low quantity. Thus, we urge caution in interpreting these low BMI estimates as a proxy for malnutrition. Nonetheless, the larger impacts for free-lunch eligible students in math do provide some evidence that students at higher risk of malnutrition are affected more by the program.

In Tables 5 and 6 we provide two tests of the validity of our difference-in-differences identification strategy. First, in Table 5 we examine the possibility that schools that adopted prior to the 9th week had pre-existing trends. To test for these trends we conduct a placebo test where we estimate equations (1) and (2), including the same controls and fixed effects, on the sample prior to 2009-10 and label 2008-09 as the post-period.[30] If there are pre-existing trends then we should expect to see a significant "impact" on achievement for schools that adopt prior to testing in the year 2008-09 relative to the 2004-05 through 2007-08 period. The estimates in Table 5 show little to suggest the existence of pre-trends. In almost all cases – full sample, split by above/below median, and split by quintile – the point estimates on the *Post*Treated* and *Post*Exposure Time* variables are small and statistically insignificant. For the overall "impact" on treated schools before the introduction of the in-class breakfast program, the estimate for math is 0.001 with an se of 0.052 and the estimate for reading is 0.031 with an se of 0.042.[31]

[30] Estimates excluding controls and fixed effects are similar and provided in Online Appendix Table 3.
[31] We nonetheless note that the reading estimate is similar to that found for math in our main results when the sample is limited to 2008-09 and later.

Another concern is that if the timing of program implementation is related to changes in the characteristics of students in the adopting schools or if the program itself induced changes in the composition of the students who tested, our results could be biased. Hence, in Table 6 we provide estimates of the difference-in-differences "impacts" on observable characteristics. Note that these models do not include any controls except grade and year dummies.[32] Panel A of Table 6 shows that earlier adoption of the program had no statistically significant effects on students' gender, race, economic disadvantage, LEP status, at-risk status, gifted status, special education status, and most importantly mean lagged reading or math scores for students in 5^{th} grade.[33] Only gifted status is significant, and only at the 10% level. In Panel B, we show that if we use all students in grades 1 to 5 and there are no statistically significant effects.[34] We also note that aggregated school-level analyses show very similar results and are available upon request.[35]

It is instructive to note here that the main effects for being a school treated prior to week 10 do show some small but significant differences in student characteristics. In particular, schools that adopt in weeks 1 through 8 have 3 percentage points more economically disadvantaged students, achievement scores approximately one tenth of a standard deviation lower, lower gifted rates and higher at-risk and special education rates. This is the primary reason why we argue that the adoption timing is quasi-random rather than entirely random and, hence, we rely on a difference-in-differences strategy rather than a simple cross-sectional comparison. Nonetheless, the important take-away from this table is that there is little evidence

[32] Estimates with school fixed-effects are similar.

[33] Although student gender is fairly balanced across schools, the gender of those who test and are in the sample could change in response to access to breakfast, if, for example, some households give priority in access to food to boys over girls.

[34] For lagged achievement we are limited to grades 4 and 5 since testing begins in grade 3.

[35] We also find that there are no significant impacts on the likelihood of being in a given lagged achievement quintile.

that the changes in achievement found in Table 3 are correlated with contemporaneous changes in student characteristics.

In Table 7, we provide a set of specification checks for our baseline treatment effect estimates. In row (1), we estimate models with lagged achievement omitted. In row (2), we limit the sample only to 2007-08 and later years. In row (3) we provide estimates without school fixed effects. In all of these cases the estimates are qualitatively similar to those provided in Table 3. Lastly, in row (4) we provide exposure time estimates for students in 4th grade. Since the exam for 4th grade students occurs after week 11 we cannot estimate overall treatment effects. Nonetheless, we can use the variation in time of exposure to see if these estimates are consistent with our estimates for 5th grade students. Indeed, that is what we find, as there appears to be no relationship between time of exposure to ICB and achievement.

6.2. Effects on Absenteeism and Grades

Since advocates of moving breakfast to the classroom often argue that this kind of program helps to reduce tardiness and absenteeism we also look at attendance rates.[36] Unlike the testing regressions, in these analyses, along with the assessments of grades, we have access to data for grades 1 through 5 and, hence, we can see if any impacts arise for younger students. The attendance results are limited only to the 2009-10 school-year, since we do not have attendance rates by attendance period in prior years. Hence, we use differences in timing of implementation across attendance periods within 2009-10 – ICB was implemented during attendance periods 4, 5 and 6 – to identify treatment effects. To keep the estimates consistent with our achievement estimates we include lagged attendance rates for the prior two attendance periods in the models.

[36] Unfortunately, we do not have tardiness data.

Since the literature has not come to a consensus on the inclusion of lagged effects in non-achievement outcomes, we also estimated models excluding the lags and find similar results.

The results for attendance are provided in Table 8. We estimate three types of models. The first is a corollary to equation (1) where we include an indicator for whether ICB is in place at any point during period *t*. In Panel II, we modify the analysis to allow for separate estimates for being fully or partially treated as described in Section 4. Finally, in Panel III we estimate models based on equation (2) where the treatment effect is allowed to vary by weeks of exposure. In general, we find little evidence of impacts on absenteeism. The baseline treatment effect estimate is -0.06 with a standard error of 0.08. All other estimates are statistically insignificant whether we split the sample by prior achievement or grade level.[37]

Table 9 provides results for average course grades. Once again our data is more limited in years of coverage as the grades data is only available from 2008-09 through 2009-10. Nonetheless, these data provide us eight grading periods over the two years with ICB being implemented during the 3rd and 4th grading periods of 2009-10. As a result, we include the lagged grades from the prior two grading periods as controls, though once again our estimates are not sensitive to the exclusion of these lags. Using models that mirror those in Table 8 the results suggest there is little impact of the program on grades. In all three models, there are no statistically significant estimates overall, split by achievement level, or split by grade level.[38] One possible explanation for the lack of impact on grades despite the impact on achievement is that, since grades have a relative component, teachers may simply adjust their grades to the new and higher performance of the students. On the other hand, the lack of effects here are consistent with finding no exposure time gradient on achievement in that they likely reflect the program

[37] Estimates using student fixed effects instead of lagged dependent variables are similar and are provided in Online Appendix Table 4.
[38] We provide similar estimates using student fixed-effects instead of lags in Online Appendix Table 5.

impacting test performance but not overall learning given the relatively short period for which it had been in place by the end of the 2009-10 academic year. To examine whether the lack of an effect of grades is simply because teachers adjust their "curve", in results not shown we examine impacts on different quintiles. If teachers were adjusting their "curve" this would most likely show up at the lower end of the distribution rather than at the higher end where students have already reached their maximum grade. However, we do not find any differences in grades across quintiles, indicating that the lack of an overall effect on grades is unlikely coming from teachers adjusting their "curves" but rather due to a lack of an impact on learning.

7. Conclusion

Concerns about low take-up in school breakfast programs have led education officials in many school districts to provide free breakfast to all students in the classroom so that they do not need to get to school early to acquire breakfast from the cafeteria. These programs also have the potential to increase breakfast consumption over cafeteria-based programs as they reduce the potential for stigma associated with students going to the cafeteria for breakfast being identified by other students as low-income.

In this paper, we assess the impact of moving breakfast services from the cafeteria to the classroom on student achievement, attendance and grades. Since such a program reduces the time and effort costs and potential social costs to students from consuming breakfast such a program could affect student outcomes. We use data from a large urban school district in the Southwest United States (LUSD) that phased-in an in-class breakfast (ICB) program quasi-randomly over the course of two months in 2010. Since the phase-in period overlaps with 5th grade achievement testing, we are able to identify the impact of the program on math and reading achievement. Using a difference-in-differences strategy we find that providing free breakfast to all students in the classroom increases math and reading achievement by 0.09 and 0.06 standard deviations, respectively, relative to providing free breakfast in the cafeteria. These effects almost entirely come from Hispanic students, with black students showing little impact. Further, the effects are concentrated in students with low prior achievement and students with low body mass indices. We find no evidence of impacts on attendance or grades, however.

Since we cannot identify which students switch from not eating breakfast to eating breakfast, one should interpret this intention-to-treat effect as a lower-bound estimate of the actual treatment effect of consuming more food prior to school. To get an idea of the treatment effect, we can use results from a non-randomized pilot study by the school district which found that schools that implemented ICB had twice the consumption rate of schools that did not participate in the pilot. At face value, this indicates treatment effects are likely on the order of 0.12 standard deviations in reading and 0.18 SD in math. However, this estimate should be used with caution due to the non-randomness of the pilot. Alternatively, comparisons of take-up of classroom and cafeteria universal breakfast programs conducted by Bernstein et al. (2004) show that take-up of those served breakfast in the classroom is 2.4 times the take-up for those who get served in the cafeteria. Combining this statistic with our estimates implies treatment effects of 0.14 and 0.21 standards deviations in reading and math, respectively. Finally, we know that take-up after the in-class breakfast program in six "case studies" provided by the district was 2.5 times take-up prior to the program. In these schools, which were relatively more disadvantaged than the average school in the district, the treatment effects are estimated to be 0.16 SD in reading and 0.22 SD in math.

Moreover, cost information provided by the district for the case studies indicates that the cost of breakfast per student also goes down when breakfast is provided in the classroom rather than in the cafeteria. Food, labor, and supply costs per meal decreases from $1.76 per student to $1.32 per student after breakfast is moved from the cafeteria to the classrooms. Total, rather than per-meal, costs increase because of increased consumption – from 33% to 83% in the case study schools. While food and supply costs increase in line with the number of students served, the labor costs associated with serving breakfast increase by 36% which indicates that there are efficiencies of scale that can be gained from serving breakfast in the classroom.[39] Further, we note that the cost per hour of labor did not change. Rather the additional costs come from increased labor hours.

Using these figures we can calculate that a school would experience an increase in costs per-student of $0.51 per day. Assuming a 180 day school year this amounts to $91 per year. Thus, the school gains an average of 0.09 SD in math and 0.06 SD in reading for $91 per-student. These gains are, of course, not universal as only those who change consumption are likely to be affected. Thus, for these students the gains are between 0.18 and 0.22 SD in math and between 0.12 and 0.16 SD in reading. In either case, this is a large impact for such a small amount of spending – equal to 1.2% of the average elementary school operating expenditures in the district.

Nonetheless, one should also be cautious in interpretation. In particular, the results are likely due to impacts on test performance from higher calorie intake rather than actual impacts on learning. Figlio and Winicki (2005) establish that schools sometimes increase calories during testing days to generate these effects. We find some evidence that supports this. First, if the breakfast program increases learning we would expect to see schools that adopt earlier to have larger impacts than those that adopt later. Our results show little evidence of differences by

[39] Calculation is the average change in costs over all six schools for which we have case studies. Results are similar if we focus on the school most similar to the average LUSD elementary school.

adoption timing. Nonetheless, since the implementation period is short-term we cannot rule out that exposure time effects would emerge over longer time periods. Second, we look at impacts on course grades and find no evidence of any impact. While this could be due to teachers curving their grades to match the new, higher performance of students, this is also consistent with the achievement results being due to improved test performance rather than learning.

Regardless, this study does have important policy implications. Our results suggest that providing breakfast to students has a marked impact on achievement scores. Thus schools where breakfast consumption is high – which would tend to be wealthier schools – get higher test scores than low consumption schools – which tend to be poorer schools – even independent of learning. Since sanctions and rewards under accountability regimes are based on these tests, low consumption schools may be more likely to suffer the sanctions or miss out on the awards due to what essentially amounts to a potential statistical bias in the testing metric. Given the size of our estimates, such a testing error could potentially be quite large. Further research should assess to what extent malnutrition results in achievement testing mismeasuring actual learning.

References

Alderman, Harold, Jere R. Behrman, Victor Lavy and Rekha Menon. 2001. "Child Health and School Enrollment: A Longitudinal Analysis." *Journal of Human Resources,* 36(1): 185-205.

Alderman, Harold, John Hoddinott and Bill Kinsey. 2006. "Long Term Consequences of Early Childhood Malnutrition." *Oxford Economic Papers,* 58(3): 450-474.

Alaimo, Katherine, Christine Olson and Edward Frongillo. 1999. "Food Insufficiency and American School-Aged Children's Cognitive, Academic and Psychosocial Development," *Pediatrics,* 108(1): 44-53.

Anderson, Patricia and Kristin Butcher. 2006. "Reading, Writing, and Refreshments: Are School Finances Contributing to Children's Obesity?" *Journal of Human Resources,* 41: 467-494.

Andrabi, Tahir. Jishnu Das, Asim Khwaja and Tristan Zajonk. 2011. "Do Value Added Estimates Add Value? Accounting for Learning Dynamics," American Economic Journal: Applied Economics, 3(3): 29-54.

Angrist, Joshua and Alan Krueger. 1999. "Empirical Strategies in Labor Economics," *Handbook of Labor Economics, 3A*: 1277-1366.

Bartfield, Judith S. and Hong-Min Ahn. 2011. "The School Breakfast Program Strengthens Household Food Security among Low-Income Households with Elementary School Children." *Journal of Nutrition*, 141(3): 470-475.

Bernstein, Lawrence, Joan McLaughlin, Mary Kay Crepinsek, and Lyann Daft. 2004. "Evaluation of the School Breakfast Program Pilot Project: Final Report" Nutrition Assistance Program Report Series, No. CN-04-SBP. Alexandra, VA: US Department of Agriculture.

Bhattacharya, Jayanta, Janet Currie and Steven Haider. 2006. "Breakfast of Champions? The School Breakfast program and the Nutrition of Children and Families," *Journal of Human Resources*, 41(3): 445-466.

Cullen, Karen. 2010. "HISD Breakfast Program Aids Student Achievement." *Houston Chronicle*, February 17.

Dahl, Molly W. and John Karl Scholz. 2011. "The National School Lunch Program and School Breakfast Program: Evidence on Participation and Noncompliance." University of Wisconsin, mimeo.

Devaney, Barbara and Thomas Fraker. 1989. "The Dietary Impacts of the School Breakfast Program." *American Journal of Agricultural Economics* 71(4): 932 – 948.

Dunifon, Rachel and Lori Kowaleski-Jones. 2003. "The Influences of Participation in the National School Lunch Program and Food Insecurity on Child Well-Being," *Social Service Review*, 77(1): 72-92.

Dotter, Dallas. 2012. "Breakfast at the Desk: The Impact of Universal Breakfast Programs on Academic Performance," Mimeo.

Figlio, David and Joshua Winicki. 2005. "Food for Thought: the Effects of School Accountability Plans on School Nutrition," *Journal of Public Economics*, 89(2-3): 381-394.

Frisvold, David. 2012. "Nutrition and Cognitive Achievement: An Evaluation of the School Breakfast Program," Mimeo.

Gardner, Amanda. 2008. "Skip Breakfast, Pack on the Pounds," *The Washington Post*, March 3.

Gleason, Philip M. and Carol W. Suitor. 2003. "Eating at School: How the National School Lunch Program Affects Children's Diets." *American Journal of Agricultural Economics*, 85(4): 1047-1061.

Grainger, Corbett, Benjamin Senauer and C. Ford Runge. 2007. "Nutritional Improvements and Student Food Choices in a School Lunch Program." *Journal of Consumer Affairs*, 41(2): 265-284.

Gundersen, Craig, Brent Kreider and John Pepper. 2012. "The Impact of the National School Lunch Program on Child Health: A Nonparametric Bounds Analysis." *Journal of Econometrics*, 166(1): 79-91.

Gundersen, Craig, Brend Kreider and John Pepper. 2011. "The Economics of Food Insecurity in the United States." *Applied Economic Perspectives and Policy*, 33(3): 281-303.

Glewwe, Paul, Hanan Jacoby and Elizabeth King. 2001. "Early Childhood Nutrition and Academic Achievement: A Longitudinal Analysis," *Journal of Public Economics*, 81(3): 345-368.

Hinrichs, Peter. 2010. "The Effects of the National School Lunch Program on Education and Health," *Journal of Policy Analysis and Management*, 29(3): 479-505.

Hofferth, Sandra and Sally Curtin. 2005. "Poverty, Food Programs and Childhood Obesity," *Journal of Policy Analysis and Management*, 24: 703-726.

Leos-Urbel, Jacob, Amy Ellen Schwartz, Meryle Weinstein and Sean Corcoran. 2012. "Not Just For Poor Kids: The Impact of Universal Free School Breakfast on Meal Participation and Student Outcomes." New York University, mimeo.

Maluccio, John A., John Hoddinott, Jere R. Behrman, Reynaldo Martorell, Agnes R. Quisumbing and Aryeh D. Stein. 2009. "The Impact of Improving Nutrition During Early Childhood on Education among Guatamalan Adults." *Economic Journal* 119(537): 734-763.

Meyers, Alan, Amy Sampson, Michael Weitzman, Beatrice Rogers, and Herb Kayne. 1989. "School Breakfast Program and School Performance," *American Journal of Diseases of Children*, 143(10): 1234-1239.

Middleman, A.B., S.J. Emans, and J. Cox. 1996. "Nutritional Vit B12 Deficiency and Folate Deficiency in an Adolescent Patient Presenting with Anemia, Weight Loss, and Poor School Performance," *Journal of Adolescent Health* 19(1): 76-79.

Millimet, Daniel, Rusty Tchernis, and Muna Hussein. 2010. "School Nutrition Programs and the Incidence of Childhood Obesity," *Journal of Human Resources*, 45(3): 640-654.

Nord, Mark, Alisha Coleman-Jensen, Margaret Andrews and Steven Carlson. 2010. "Household Food Security in the United States, 2009." USDA Economic Research Report No. 108.

Pollitt, Ernesto, Rudolph L. Leibel and Daryl Greenfield. 1981. "Brief Fasting, Stress and Cognition in Children." *American Journal of Clinical Nutrition*, 34: 1526-1533.

Pollitt, Ernesto, Nita L. Lewis, Cutberto Garza and Robert J. Shulman. 1982. "Fasting and Cognitive Function." *Journal of Psychiatric Research* 17(2): 169-174.

Rampersaud, Gail C., Mark A. Pereira, Beverly L. Girard, Judi Adams, and Jordan D. Metzl. 2005. "Breakfast Habits, Nutritional Status, Body Weight and Academic Performance in Children and Adolescents." *Journal of the American Dietetic Association* 105(5): 743-760.

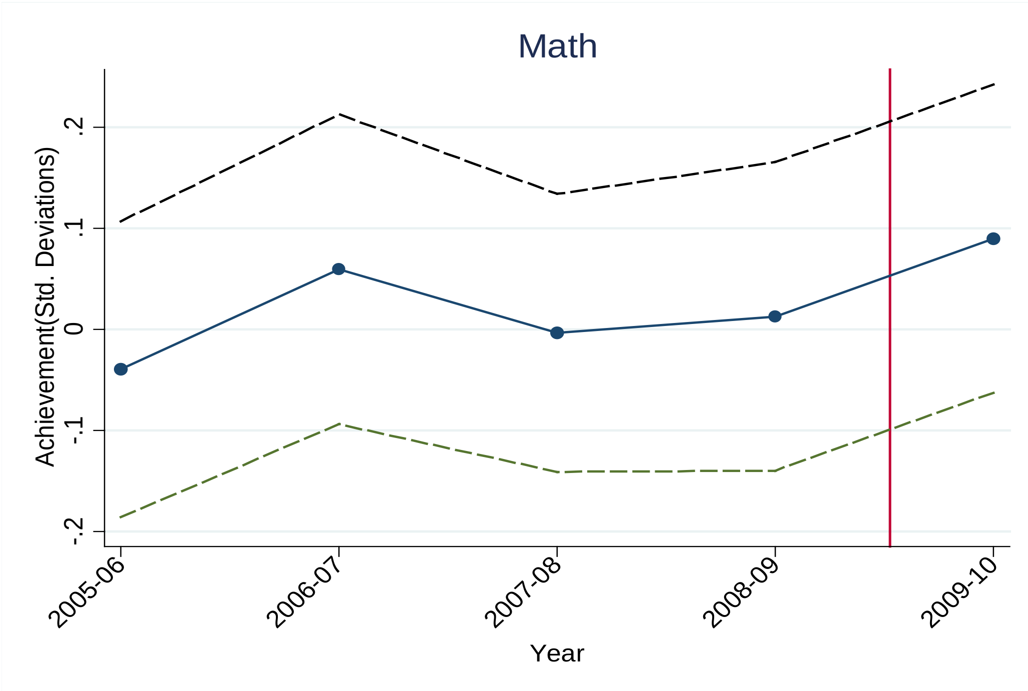
Schanzenbach, Diane Whitmore. 2009. "Do School Lunches Contribute to Childhood Obesity?" *Journal of Human Resources*, 44: 684-709.

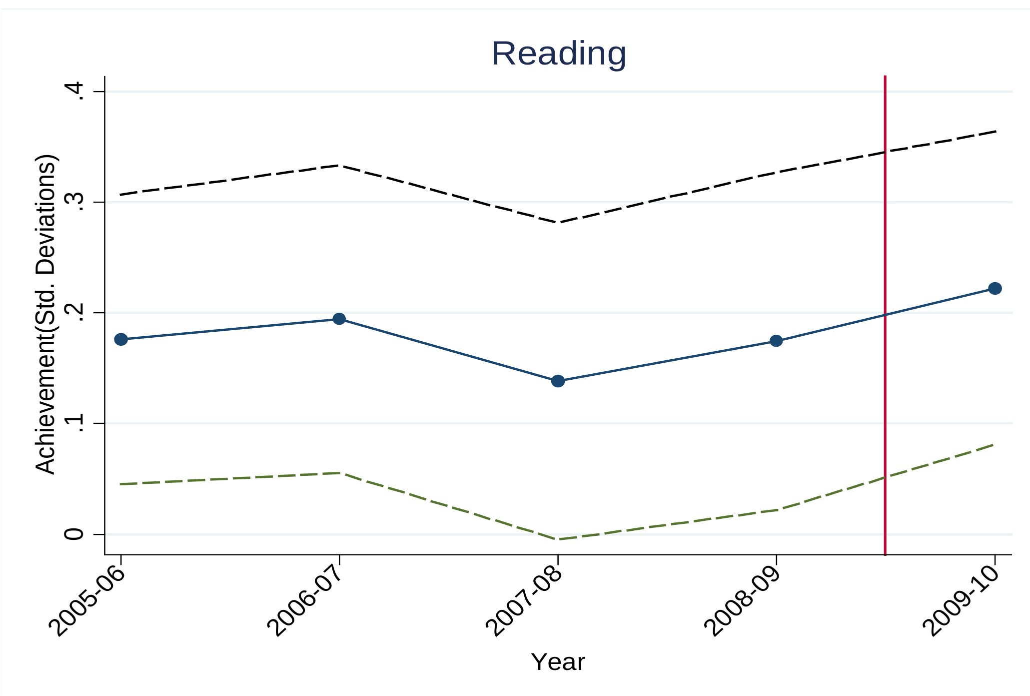
Vermeersch, Christel and Michael Kremer. 2004. "School Meals, Educational Achievement and School Competition: Evidence from a Randomized Evaluation," World Bank Policy Research Working Paper No. 3523.

Waehrer, Geetha M. 2008. "The School Breakfast Program and Breakfast Consumption." Institute for Research on Poverty Discussion Paper No. 1360-08.

Wesnes, Keith, Claire Pincock, David Richardson, Gareth Helm, and Simon Hails. 2003. "Breakfast Reduces Declines in Attention and Memory over the Morning in Schoolchildren," *Appetite*, 41: 329-331.

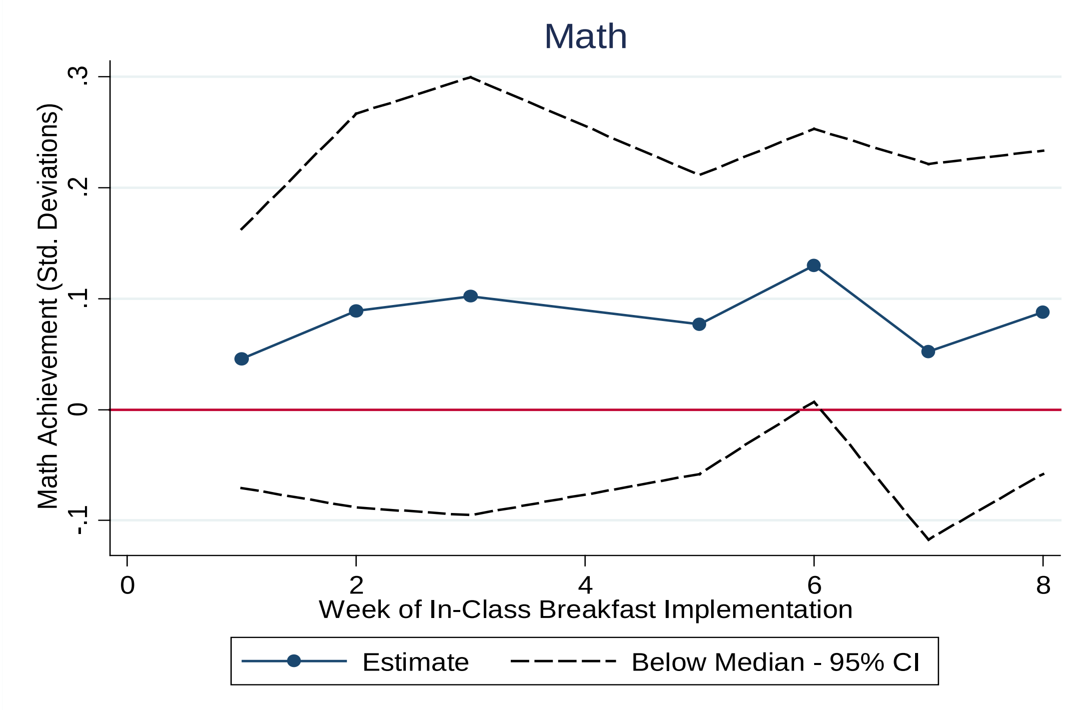
Figure 1: Estimated Pre- and Post-Treatment "Impacts"

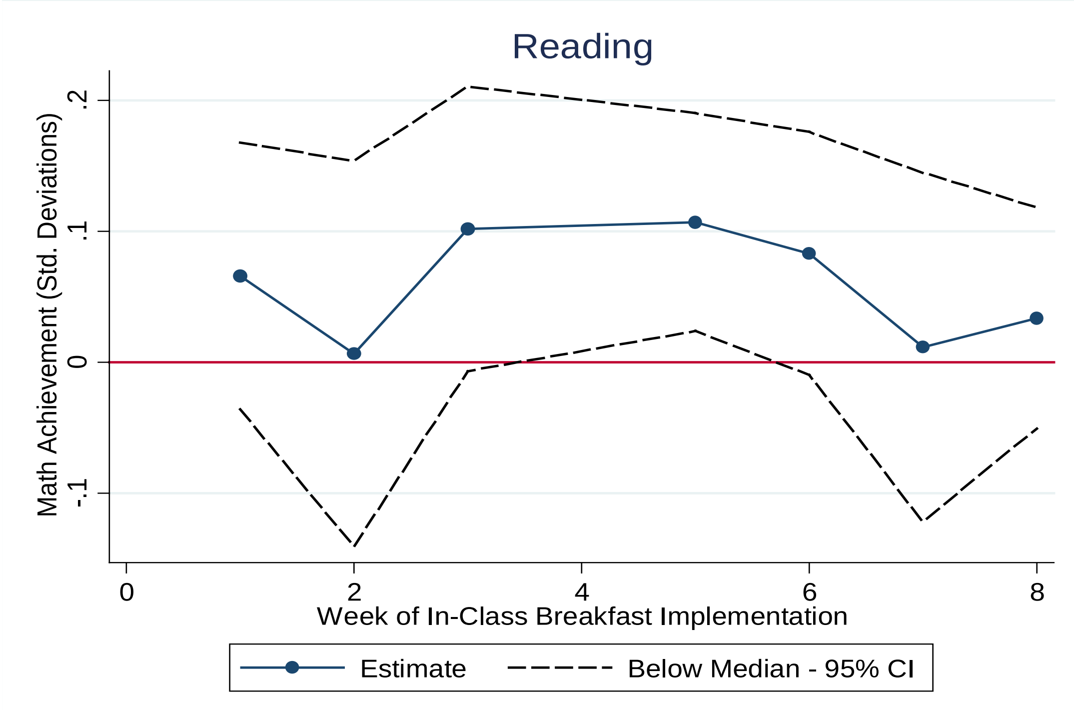




Figures show estimates from equation (5) in the text. The solid line shows point estimates with the dotted lines showing 95% confidence intevervals.

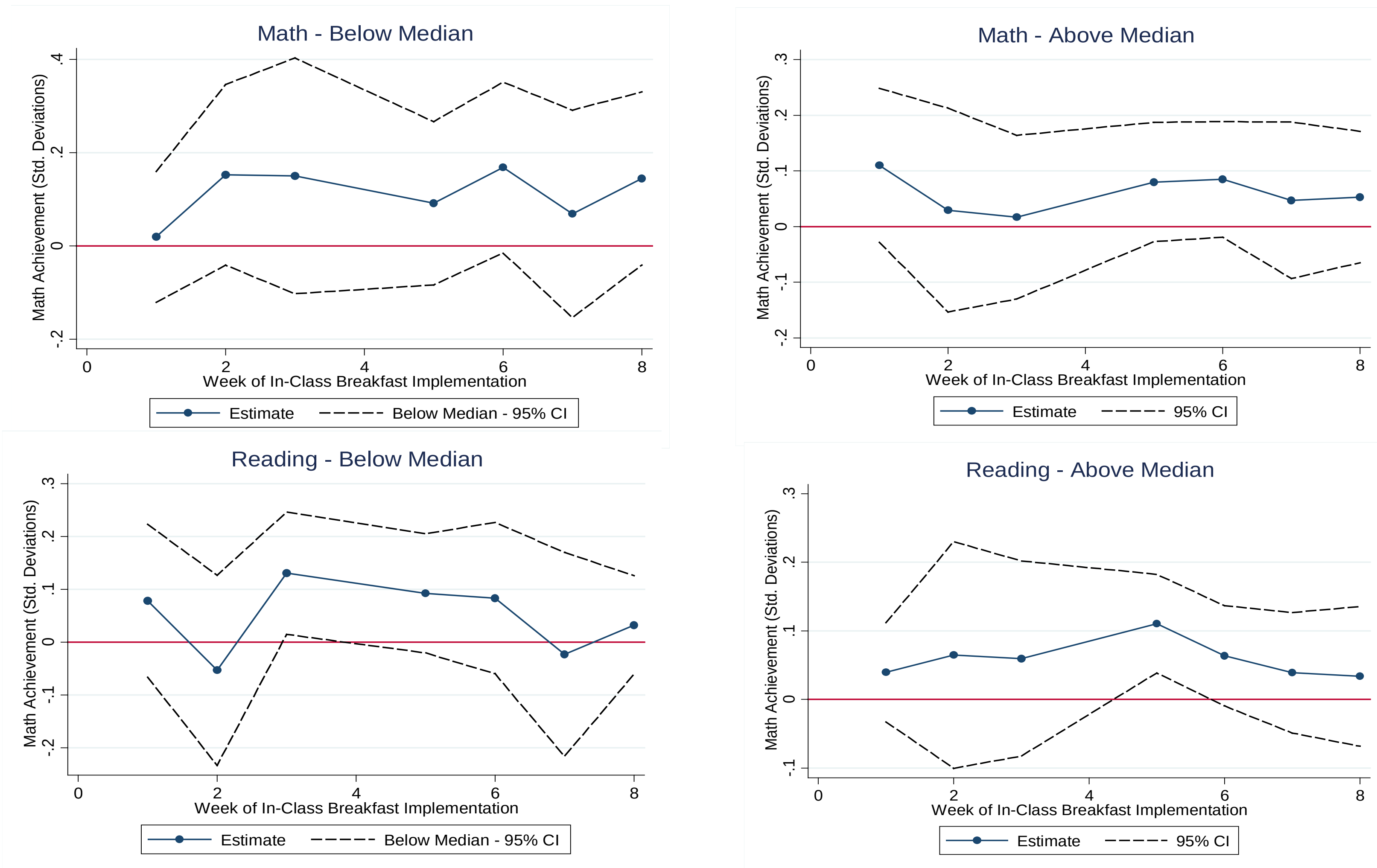
Figure 2: Impact of In-Class Breakfast on Achievement By Week of Implementation





Graphs show point estimates and confidence intervals from regressions of program impact on achievement where the impact estimates are allowed to vary by week of implementation.

Figure 3: Impact of In-Class Breakfast on Achievement
By Prior Achievement and Week of Implementation



Graphs show point estimates and confidence intervals from regressions of program impact on achievement where the impact estimates are allowed to vary by week of implementation. Samples are split for each exam by whether the student is above or below the median score on the 2008-09 achievement exam.

Table 1: Means of Elementary School Characteristics by Week of Implementation of In-Class Breakfast (Test Sample)

Week	Start Date	Important Events	# of Schools	% Econ Disadv	% Black	% Hisp	% LEP	Avg. Teacher Exp (years)	Student-Teacher Ratio	Attendance Rate	Per-Pupil Exp	Mean Math Achievement	Mean Reading Achievement
(Estimation sample includes only weeks 1 - 11)				(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						A. Levels in 2008-09							
1	2/2/2010		7	93.5	23.7	72.8	50.4	11.6	16.8	96.6	6913	-0.13	-0.19
2	2/9/2010		8	95.6	34.3	64.4	37.9	10.7	16.8	96.8	7367	-0.12	-0.09
3	2/16/2010	End of 4th Attendance Period	5	96.7	31.8	67.6	44.7	11.2	16.6	97.3	7003	0.17	0.13
5	3/2/2010		13	95.3	28.2	69.3	45.1	11.1	16.1	96.5	6615	-0.24***	-0.18***
6	3/9/2010	End of 3rd Grading Cycle	13	94.9	33.4	65.1	43.5	10.0	15.9	97.2	7635.9*	-0.09	-0.09
7	3/23/2010		10	94.7	34.3	63.9	40.3	12.4	16.0	96.8	7091	-0.07	-0.09
8	3/30/2010		9	94.0	32.9	65.7	43.3	12.3	14.8	97.2	7285	0.01	0.01
9	4/6/2010	5th Grade Testing, End of 5th Attendance Period	10	90.3	29.9	68.7	44.4	9.3***	16.7*	97.2	6588	0.15	0.14
10	4/13/2010		10	92.8	18.1	78.3	52.3	11.9***	15.8*	97.2	6933	-0.01	-0.16***
11	4/20/2010		9	91.5	27.1	66.1	50.3	11.8	16.8	97.0	6637	-0.03	-0.05
	4/27/2010	3rd & 4th Grade Testing											
Joint F-Test for Weeks 2 - 11 (F-statistics; Week 1 Omitted Categ				0.7	0.4	0.3	0.5	1.2	1.7	0.8	2.1**	1.6	2.0**
Estimate of Difference Between Weeks 10-11 and 1-8				-3.1**	-1.6	-3.0	5.1	0.5	0.8	0.0	-549*	0.05	0.05
(Standard Error)				(1.3)	(11.7)	(11.5)	(8.2)	(1.1)	(0.6)	(0.3)	(288)	(0.11)	(0.09)
13	5/4/2010		9	88.2**	15.6	78.1	48.4	12.6	16.6	97.4	6974.1**	0.11	0.06
14	5/11/2010		7	69.4***	41.3***	40.2***	23.7***	14.2	15.6***	96.5***	7096.6***	0.02	0.20
15	5/17/2010		7	58.4*	17.0***	51.6	30.8	11.1**	16.7***	97.1	6416	0.10	0.21
16	9/14/2010		11	24.5***	9.7*	26.9***	13.2***	13.8*	17.1	97.2	6310	0.44***	0.57***
17	9/21/2010		1										
					B. Changes From 2006-07 Through 2008-09								
1	2/2/2010		7	0.0	-1.4	0.9	4.2	0.7	-0.6	0.0	878	0.18	0.13
2	2/9/2010		8	2.5	-1.5	1.0	2.5	-0.5	-0.5	-0.1	1409**	-0.02	0.01
3	2/16/2010	End of 4th Attendance Period	5	0.1***	-1.0	1.0	4.0	0.8	-0.4	0.5**	1095	0.13	0.10
5	3/2/2010		13	0.9	-0.7	0.8	2.8	0.8	0.0	0.3	1116	-0.05	0.00
6	3/9/2010	End of 3rd Grading Cycle	13	0.4	0.0	0.2	1.7	0.7	0.0	0.4	1155	-0.06	-0.06
7	3/23/2010		10	1.8	-1.5	1.9	3.6	-0.2	-1.0*	0.3	930	0.12	0.10
8	3/30/2010		9	1.9	-4.0	5.0	6.1	0.0	-1.1	0.4	1081	0.12	0.12
9	4/6/2010	5th Grade Testing, End of 5th Attendance Period	10	-0.4	-1.0	1.6	2.4	-0.6	-0.1	0.2	846	0.07	0.06
10	4/13/2010		10	1.4	-0.3	0.2	3.3	0.7*	-0.5	0.3	942	0.06	-0.09*
11	4/20/2010		9	1.6	-1.4	1.4	2.5	0.2	-0.1	0.3	948	0.01	-0.01
Joint F-Test for Weeks 2 - 11 (F-statistics; Week 1 Omitted Categ				0.7	1.1	1.5	0.8	1.0	0.9	1.1	1.1	0.8	0.6
Estimate of Difference Between Weeks 10-11 and 1-8				0.4	-0.1	0.1	-0.8	-0.2	0.4	0.0	-132	-0.04	-0.04
(Standard Error)				(1.1)	(1.2)	(1.3)	(1.7)	(0.6)	(0.5)	(0.2)	(169)	(0.10)	(0.12)
13	5/4/2010		9	1.0***	-0.8	1.4	3.2	0.0	0.1	0.2	1224	0.10	0.05
14	5/11/2010		7	3.1***	-0.8	3.8*	3.1	1.0*	0.4	0.4	1037	-0.02	0.13
15	5/17/2010		7	-0.6	-0.700	0.2	2.2	0.0	0.0	0.1	910	-0.01	0.13
16	9/14/2010		11	-2.6	-2.0	0.8	1.6	0.8	0.0	0.0	1030	0.04	0.17
17	9/21/2010		1										

*, **, *** denote significant difference from prior week's schools at 10%, 5%, and 1% levels, respectively. Economic disadvantage refers to students who qualify for free meals, reduced-price meals, or other Federal or state low-income assistance programs.

Table 2: Summary Statistics in 2009-10

A. 5th Grade Tested Students Only (Test Sample)		B. All Students in Grades 1 - 5 (Grades Sample)		C. All Students in Grades 1 - 5 (Attendance Sample)	
Female	0.49 (0.50)	Female	0.48 (0.50)	Female	0.48 (0.50)
Black	0.23 (0.42)	Black	0.23 (0.42)	Black	0.23 (0.42)
White	0.02 (0.12)	White	0.01 (0.12)	White	0.02 (0.12)
Hispanic	0.74 (0.44)	Hispanic	0.74 (0.44)	Hispanic	0.73 (0.44)
Economically Disadvantaged	0.94 (0.24)	Economically Disadvantaged	0.94 (0.23)	Economically Disadvantaged	0.94 (0.23)
LEP	0.40 (0.49)	LEP	0.54 (0.50)	LEP	0.53 (0.50)
At Risk	0.67 (0.47)	At Risk	0.74 (0.44)	At Risk	0.74 (0.44)
Gifted	0.15 (0.35)	Gifted	0.13 (0.34)	Gifted	0.13 (0.34)
Special Ed	0.09 (0.29)	Special Ed	0.07 (0.26)	Special Ed	0.07 (0.26)
2008-09 Math†	-0.01 (0.94)	2008-09 Math†	-0.05 (0.97)	2008-09 Math†	-0.06 (0.98)
2008-09 Reading†	-0.06 (0.95)	2008-09 Reading†	-0.08 (0.97)	2008-09 Reading†	-0.08 (0.97)
2009-10 Math	-0.04 (1.01)	Mean Grade	83.1 (7.2)	Mean Absence Rate (%)	3.16 (5.22)
2009-10 Reading	-0.10 (1.01)				
Observations (Student)	*6353*	*Observations (Student-Time Period; 4 Periods)*	*145,203*	*Observations (Student-Time Period; 6 Periods)*	*230,550*
# of Students	*6,353*	# of Students	*37,309*	# of Students	*38,425*
# of Schools	*84*	# of Schools	*87*	# of Schools	*87*

† Prior year reading and math have 5908 observations in panel A, 50306 and 50377 observations, respectively for panel B and 77316 and 77424 observations, respectively for panel C. Scores only available for grades 4 - 5.

Standard deviations in parentheses

Table 3 - Effect of In-Class Breakfast on 5th Grade Achievement - Including School Fixed Effects and Controls

	Full Sample	By Above/Below Median Lagged Achievement		By Lagged Achievement Quintiles				
		Below	Above	Bottom	Second	Third	Fourth	Top
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Math								
I. Reduced-Form Treatment Effect								
Post*Treated	0.086*	0.115*	0.061	0.086	0.173***	0.023	0.119*	0.046
	(0.046)	(0.058)	(0.045)	(0.093)	(0.065)	(0.059)	(0.060)	(0.039)
Observations	30,771	15,557	15,214	5,055	7,011	6,977	6,455	5,273
II. Treatment Effect & Exposure Time								
Post*Treated	0.098	0.145	0.056	0.186	0.112	0.094	0.074	0.062
	(0.071)	(0.098)	(0.059)	(0.139)	(0.123)	(0.083)	(0.071)	(0.058)
Post*Exposure Time (Weeks)	-0.002	-0.006	0.001	-0.019	0.012	-0.014	0.009	-0.003
	(0.010)	(0.014)	(0.009)	(0.019)	(0.018)	(0.014)	(0.009)	(0.009)
Observations	30,771	15,557	15,214	5,055	7,011	6,977	6,455	5,273
B. Reading								
I. Reduced-Form Treatment Effect								
Post*Treated	0.062*	0.055	0.061**	0.055	0.066	0.072*	0.036	0.043
	(0.034)	(0.044)	(0.030)	(0.079)	(0.058)	(0.043)	(0.042)	(0.036)
Observations	25,445	14,065	11,380	5,227	5,982	5,617	4,886	3,733
II. Treatment Effect & Exposure Time								
Post*Treated	0.049	0.037	0.051	0.151	-0.000	0.150**	0.004	-0.041
	(0.053)	(0.074)	(0.047)	(0.123)	(0.090)	(0.066)	(0.069)	(0.061)
Post*Exposure Time (Weeks)	0.003	0.004	0.002	-0.019	0.013	-0.015	0.007	0.017
	(0.008)	(0.011)	(0.008)	(0.021)	(0.012)	(0.011)	(0.011)	(0.012)
Observations	25,445	14,065	11,380	5,227	5,982	5,617	4,886	3,733

Data covers the 2004-05 through 2009-10 academic years. Achievement scores are standardized within grade and year. Due to a change in the scaling procedure in 2009-10 we standarize using raw scores. "Treated" is an indicator for whether a school starts ICB prior to the testing week. Student level covariates include lagged achievement, twice lagged achievement, student's race/ethnicity, gender, and economic status along with year and grade level dummies. School level covariates include percent of students who are white, black, Hispanic, Native American, Asian, economically disadvantaged, LEP, in vocational education, in special education, gifted, in bilingual education, in each grade level, referred to an alternative disciplinary program, and school fixed-effects. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively. Standard errors clustered by school in parentheses.

Table 4: Effect of In-Class Breakfast on 5th Grade Achievement - By Student Characteristics

	Gender		Gender and Prior Achievement				Ethnicity		
	Male	Female	Male		Female		Black	Hispanic	White
			Below Median Lagged Achievement	Above Median Lagged Achievement	Below Median Lagged Achievement	Above Median Lagged Achievement			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Math									
Post*Treated	0.088*	0.087*	0.092	0.082	0.136**	0.045	-0.029	0.124***	0.150
	(0.051)	(0.047)	(0.064)	(0.051)	(0.066)	(0.050)	(0.066)	(0.047)	(0.327)
Observations	15,188	15,583	7,485	7,703	8,072	7,511	6,602	23,259	420
B. Reading									
Post*Treated	0.069	0.057*	0.040	0.080*	0.085*	0.033	-0.002	0.092**	0.120
	(0.046)	(0.030)	(0.059)	(0.041)	(0.051)	(0.036)	(0.066)	(0.037)	(0.232)
Observations	12,794	12,651	7,517	5,277	6,548	6,103	5,894	18,767	366

	Free Lunch		LEP Status		Body Mass Index†			
	Not Eligible	Eligible	Not Lep	LEP	Low Weight < 25 Percentile	Medium Weight 25 - 84 Percentile	Overweight 85 - 94 Percentile	Obese ≥ 95 Percentile
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
A. Math								
Post*Treated	0.008	0.132***	0.052	0.118*	0.244**	0.012	-0.048	0.051
	(0.056)	(0.050)	(0.051)	(0.060)	(0.108)	(0.074)	(0.110)	(0.084)
Observations	10,563	20,208	18,926	11,845	664	2,846	1,483	2,267
B. Reading								
Post*Treated	0.083**	0.046	0.056*	0.084	0.232*	0.050	0.114*	0.062
	(0.038)	(0.043)	(0.032)	(0.057)	(0.130)	(0.092)	(0.068)	(0.065)
Observations	9,290	16,155	15,935	9,510	683	2,905	1,511	2,331

† BMI data only includes a subset of schools in 2008-09 and 2009-10.

Data covers the 2003-04 through 2009-10 academic years. Achievement scores are standardized within grade and year. Due to a change in the scaling procedure in 2009-10 we standarize using raw scores. "Treated" is an indicator for whether a school starts ICB prior to the testing week. Student level covariates include lagged achievement, twice lagged achievement, student's race/ethnicity, gender, and economic status along with year and grade level dummies. School level covariates include percent of students who are white, black, Hispanic, Native American, Asian, economically disadvantaged, LEP, in vocational education, in special education, gifted, in bilingual education, in each grade level, referred to an alternative disciplinary program, and school fixed-effects. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively. Standard errors clustered by school in parentheses.

Table 5 : Achievement Placebo Test - Sample Limited to 2008-09 and Earlier and Set 2008-09 as "Post" Period
With Controls and School Fixed Effects

	Full Sample	By Above/Below Median Lagged Achievement		By Lagged Achievement Quintiles				
		Below	Above	Bottom	Second	Third	Fourth	Top
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Math				
I. Reduced-Form Treatment Effect								
Post*Treated	0.001	0.053	-0.002	0.123	-0.013	0.049	-0.038	-0.059
	(0.052)	(0.050)	(0.032)	(0.129)	(0.082)	(0.066)	(0.049)	(0.036)
Observations	25,365	12,732	12,633	4,051	5,793	5,794	5,355	4,372
II. Treatment Effect & Exposure Time								
Post*Treated	-0.042	-0.040	0.014	-0.008	-0.157	-0.023	-0.019	-0.006
	(0.079)	(0.089)	(0.048)	(0.194)	(0.136)	(0.097)	(0.086)	(0.052)
Post*Exposure Time (Weeks)	0.008	0.017	-0.003	0.023	0.026	0.013	-0.004	-0.010
	(0.012)	(0.012)	(0.007)	(0.031)	(0.020)	(0.016)	(0.012)	(0.008)
Observations	25,365	12,732	12,633	4,051	5,793	5,794	5,355	4,372
				B. Reading				
I. Reduced-Form Treatment Effect								
Post*Treated	0.031	0.009	-0.014	-0.020	0.134**	0.042	-0.018	0.007
	(0.042)	(0.042)	(0.033)	(0.102)	(0.062)	(0.052)	(0.037)	(0.046)
Observations	20,032	11,103	8,929	4,116	4,746	4,399	3,854	2,917
II. Treatment Effect & Exposure Time								
Post*Treated	0.043	0.052	0.040	0.053	0.042	0.013	0.071	0.091
	(0.068)	(0.073)	(0.055)	(0.154)	(0.112)	(0.080)	(0.069)	(0.067)
Post*Exposure Time (Weeks)	-0.002	-0.008	-0.010	-0.013	0.017	0.005	-0.017	-0.016
	(0.012)	(0.010)	(0.008)	(0.021)	(0.020)	(0.013)	(0.011)	(0.013)
Observations	20,032	11,103	8,929	4,116	4,746	4,399	3,854	2,917

Data covers the 2004-05 through 2007-08 academic years. Achievement scores are standardized within grade and year. Due to a change in the scaling procedure in 2009-10 we standarize using raw scores. The "Post" indicator is set equal to one in 2007-08. "Treated" is an indicator for whether a school starts ICB prior to the testing week. Schools treated in week 9 are dropped as this is the 5th grade testing week and some schools may have postponed the start of ICB for 5th grade students. Student level covariates include lagged achievement, twice lagged achievement, student's race/ethnicity, gender, and economic status along with year and grade level dummies. School level covariates include percent of students who are white, black, Hispanic, Native American, Asian, economically disadvantaged, LEP, in vocational education, in special education, gifted, in bilingual education, in each grade level, referred to an alternative disciplinary program, and school fixed-effects. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively. Standard errors clustered by school in parentheses.

Table 6: Tests of Impacts on Exogenous Covariates

	Female	Black	White	Hispanic	Economic Disadvantage	Free Lunch	Reduced-Price Lunch	LEP	At Risk	Gifted	Special Ed	Lagged Math	Lagged Reading
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(6)	(7)	(8)	(9)	(10)	(11)
A. Mean for 5th Grade Only													
Post*Treated	-0.003	-0.017	0.002	0.013	0.011	-0.017	0.002	0.013	0.011	-0.037*	-0.001	-0.021	-0.002
	(0.016)	(0.015)	(0.004)	(0.016)	(0.014)	(0.015)	(0.004)	(0.016)	(0.014)	(0.019)	(0.009)	(0.048)	(0.045)
Treated	-0.001	0.060	-0.006	-0.017	0.033***	0.060	-0.006	-0.017	0.033***	-0.039**	0.023**	-0.113**	-0.115***
	(0.007)	(0.069)	(0.005)	(0.068)	(0.009)	(0.069)	(0.005)	(0.068)	(0.009)	(0.016)	(0.010)	(0.048)	(0.040)
Observations	38,913	38,913	38,913	38,913	38,913	38,913	38,913	38,913	38,913	38,913	38,913	35,062	35,098
B. Mean for All Grades in School													
Post*Treated	0.005	-0.009	0.001	0.005	0.004	-0.009	0.001	0.005	0.004	-0.015	0.002	0.017	-0.004
	(0.009)	(0.011)	(0.002)	(0.011)	(0.007)	(0.011)	(0.002)	(0.011)	(0.007)	(0.013)	(0.005)	(0.047)	(0.039)
Treated	-0.003	0.066	-0.005	-0.021	0.033***	0.066	-0.005	-0.021	0.033***	-0.037***	0.012**	-0.118**	-0.107**
	(0.004)	(0.067)	(0.004)	(0.066)	(0.009)	(0.067)	(0.004)	(0.066)	(0.009)	(0.012)	(0.006)	(0.047)	(0.045)
Observations	201,206	201,206	201,206	201,206	201,206	201,206	201,206	201,206	201,206	201,206	201,206	75,859	75,899

Data covers the 2004-05 through 2009-10 academic years. For panel B, lagged achievement regressions are only possible for grades 4 and 5. Achievement scores are standardized within grade and year. Regressions include grade and year dummies as controls. "Treated" is an indicator for whether a school starts ICB prior to the testing week. Schools treated in week 9 are dropped as this is the 5th grade testing week and some schools may have postponed the start of ICB for 5th grade students. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively. Standard errors clustered by school in parentheses

Table 7 - Specification Checks

	Math	Reading
1) No Lagged Achievement		
Post*Treated	0.078	0.077*
	(0.060)	(0.044)
Observations	44,730	45,628
2) Limit to 2007-08 and Later		
Post*Treated	0.071	0.091**
	(0.052)	(0.039)
Observations	15,433	15,822
3) No School Fixed Effects		
Post*Treated	0.073	0.081**
	(0.047)	(0.035)
Observations	30,771	25,445
4) Exposure Time Effect on 4th Grade		
Post*Exposure Time (Weeks)	-0.004	0.008
	(0.007)	(0.006)
Observations	36,634	35,327

Data covers the 2004-05 through 2009-10 academic years. Achievement scores are standardized within grade and year. Due to a change in the scaling procedure in 2009-10 we standarize using raw scores. "Treated" is an indicator for whether a school starts ICB prior to the testing week. Student level covariates include student's race/ethnicity, gender, and economic status along with year and grade level dummies. School level covariates include lagged, achievement, twice-lagged acheivement percent of students who are white, black, Hispanic, Native American, Asian, economically disadvantaged, LEP, in vocational education, in special education, gifted, in bilingual education, in each grade level, referred to an alternative disciplinary program, and school fixed-effects. Fourth grade results only include one achievement lag. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively. Standard errors clustered by school in parentheses.

Table 8: Effect of In-Class Breakfast on Attendance Rate - Grades 1 to 5 with School Fixed-Effects

	Full Sample	By Above/Below Median Prior Year Math Achievement (4th & 5th Grade Only)		By Grade				
		Below	Above	1st	2nd	3rd	4th	5th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I. Any Treatment								
Treated	-0.060	-0.014	-0.039	-0.063	0.007	-0.107	0.011	-0.161
	(0.075)	(0.131)	(0.115)	(0.138)	(0.116)	(0.122)	(0.136)	(0.130)
II. Full or Partial Treatment								
Fully Treated	-0.079	-0.052	-0.125	0.012	0.021	-0.239	0.088	-0.330
	(0.156)	(0.257)	(0.196)	(0.289)	(0.212)	(0.180)	(0.239)	(0.242)
Partially Treated	-0.062	-0.018	-0.047	-0.056	0.008	-0.120	0.019	-0.180
	(0.080)	(0.138)	(0.119)	(0.145)	(0.119)	(0.124)	(0.138)	(0.138)
III. Treatment and Weeks of Exposure in Reporting Period								
Treated	-0.036	-0.145	0.116	-0.167	0.061	0.026	-0.095	0.002
	(0.101)	(0.161)	(0.146)	(0.187)	(0.163)	(0.181)	(0.174)	(0.181)
Weeks of Exposure	-0.008	0.043	-0.050	0.034	-0.017	-0.043	0.034	-0.054
	(0.027)	(0.041)	(0.038)	(0.056)	(0.047)	(0.038)	(0.042)	(0.048)
Observations	148,945	27,132	23,329	32,345	30,493	30,195	29,778	26,134

Data covers six reporting periods in 2009-10. Attendance rate is calculated as the number of days present divided by the number of days enrolled during a reporting period. In-class breakfast (ICB) phases in during periods 4, 5 and 6. "Treated" is an indicator set to one during any period when a student's school of record - defined by school attended in October - has at least one week of ICB. "Fully Treated" equals one if the school has all weeks in a period with ICB while "Partially Treated" equals one if the school has at least one but not all weeks in a period with ICB. "Weeks of Exposure" denote the number of weeks during a reporting period for which a student's school has ICB. Covariates include two lags of attendance in prior periods, student's race/ethnicity, gender, and economic status along with reporting period and grade level dummies. School level covariates are omitted as they are absorbed by school fixed effects. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. Standard errors clustered by school in parentheses.

Table 9: Effect of In-Class Breakfast on Grades - Grades 1 to 5 with School Fixed-Effects

	Full Sample	By Above/Below Median Prior Year Math Achievement (4th & 5th Grades Only)		By Grade				
		Below	Above	1st	2nd	3rd	4th	5th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I. Any Treatment								
Treated	-0.010	-0.065	0.020	0.013	0.053	-0.024	0.054	-0.137
	(0.035)	(0.072)	(0.059)	(0.017)	(0.081)	(0.094)	(0.086)	(0.103)
II. Full or Partial Treatment								
Fully Treated	-0.026	-0.123	0.034	0.025	0.062	-0.051	0.044	-0.189
	(0.058)	(0.114)	(0.097)	(0.028)	(0.126)	(0.141)	(0.140)	(0.169)
Partially Treated	-0.008	-0.057	0.018	0.013	0.052	-0.020	0.055	-0.129
	(0.032)	(0.067)	(0.054)	(0.017)	(0.075)	(0.088)	(0.080)	(0.096)
III. Treatment and Weeks of Exposure in Grading Period								
Treated	-0.031	-0.104	-0.017	0.012	0.030	-0.004	0.017	-0.191
	(0.038)	(0.082)	(0.066)	(0.017)	(0.082)	(0.090)	(0.100)	(0.124)
Weeks of Exposure	0.004	0.007	0.007	0.000	0.004	-0.004	0.007	0.010
	(0.003)	(0.006)	(0.005)	(0.001)	(0.007)	(0.008)	(0.008)	(0.009)
Observations	188,677	37,206	33,059	31,284	41,175	41,060	39,762	35,396

Data covers eight grading periods in 2008-09 and 2009-10. Grades are calculated as the mean numerical grade (on a scale of 50 to 100) over all courses. In-class breakfast (ICB) phases in during periods 7 and 8. "Treated" is an indicator set to one during any period when a student's school of record - defined by school attended in October - has at least one week of ICB. "Fully Treated" equals one if the school has all weeks in a period with ICB while "Partially Treated" equals one if the school has at least one but not all weeks in a period with ICB. "Weeks of Exposure" denote the number of weeks during a grading period for which a student's school has ICB. Covariates include two lags of grades in prior periods, student's race/ethnicity, gender, and economic status along with grading period and grade level dummies. School level covariates include percent of students who are white, black, Hispanic, Native American, Asian, economically disadvantaged, LEP, in vocational education, in special education, gifted, in bilingual education, in each grade level, referred to an alternative disciplinary program, and school fixed-effects. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. Standard errors