# Designing effective transfers: Lessons from India's school meal program

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**Abstract**

We estimate attendance gains for primary school children from a cost-neutral change in the design of India's school meal program. Municipal schools in the capital region of Delhi switched from packaged food to cooked meals in 2003, with no change in payments to meal providers. Using the staggered implementation of this transition and child-level panel data, we estimate a 3 percentage point rise in average monthly attendance, with large effects for early grades. We also find girls are more responsive to the cooked meals, but since they attend morning schools while boys attend afternoon shifts, this may simply reflect benefits from better timed meals. Our study illustrates how better designed transfers can improve outcomes within tightly constrained budgets.

**Keywords:** school meals, attendance, program design

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# 1 Introduction

Of the three and a half million children in the world who have access to school meals, one-third are in India (World Food Programme, 2013). The Indian mid-day meal program aims to improve student enrollment and attendance while simultaneously improving child health through better nutrition. It is remarkable for its overall size and its low cost per child. The state allocates meal providers free foodgrains and supports the cost of converting them into hot cooked meals. The current cooking cost is about 6 U.S. cents per meal, or 12 dollars for a 200-day school year. These costs have remained roughly constant in real terms over the last two decades.[1] To put these figures in perspective, the cost of running the Food for Education (FFE) program in Bangladesh was 25 USD in 1996, and school meals administered by the World Food Program in 2005 cost 15.79 USD on average (Ahmed and Del Ninno, 2002; Adelman et al., 2008).[2]

The Indian program, formally known as the Nutritional Support for Primary Education Program, was first launched in 1995. The federal government committed to providing each of the Indian states free transfers of grains and sharing the costs of converting them into cooked school meals. As with many well-intentioned development schemes in India, implementation of the 1995 program was slow and most states provided either dry rations for students to take home or biscuits and other snacks. In November 2001, the courts intervened. In a landmark judgement, the Supreme Court of India ordered all states to complete the transition to on-site meals.[3]

States started to comply to the Supreme Court order in 2003. Municipal schools in the capital region of Delhi transitioned in two phases during the 2003-2004 academic year. The authorities responsible for implementation shifted the existing allocations from packaged snacks to cooked meals. The providers prepared and transported the meals to schools for distribution during recess. Thus, from the perspective of the state-budget, the transition had little effect on the per unit cost of the program. This paper studies the effects of this switch to cooked meals on the monthly attendance of primary school children by combining variation in the date of introduction of the cooked meals with child-level panel data on attendance.

[1] The history of the program can be found at http://mdm.nic.in. Official circulars on the website and with the Ministry of Human Resource Development document changes in the nominal cost of the meal over time.

[2] Costs are higher even in countries that are much poorer than India. For example, in a well-studied program in Burkina Faso they were about 40 USD per student per year (Kazianga et al., 2012).

[3] The order was passed on November 28, 2001 in response to a writ petition filed by the People's Union for Civil Liberties.

The Indian mid-day meal program is widely acknowledged as a salient educational and health intervention in India. There have been several evaluations of the program in different parts of the country. Afridi (2010, 2011) finds significant positive effects on child nutrition and school attendance in a set of villages in central India. Singh et al. (2014) use longitudinal data on children from the state of Andhra Pradesh in South India and find that cooked school meals are able to insulate children from household income shocks during drought years. Jayaraman and Simroth (2015) use enrollment data for the Indian states between 2003 and 2004 and find higher enrollment rates for children in school-ready age groups. These findings support earlier descriptive studies (Khera, 2006, 2013; Dreze and Goyal, 2003).

We see our main contribution to this literature as identifying the effects of a switch from packaged snacks to freshly cooked meals on a *given* set of children. We are able to do this by obtaining attendance data on each child in our sample both before and after the switch and combining it with the precise dates at which the meal was introduced in each school. This is valuable because of controversies surrounding the program and recurring demands from particular quarters to replace the cooked meal program with pre-packaged foods. The Ministry of Women and Child Development has, from time to time, advocated a move towards fortified packaged meals, and the state of Maharashtra has recently decided to move from fresh cooked meals to fortified dehydrated packaged meals for children between the ages of 3 and 6 in government run creches.[4]

By carefully tracking children and their schools, we are able to estimate effects on the *intensive margin*. In general, transfers that raise both enrollment and attendance could result in lower average attendance rates if newly enrolled children have a lower propensity to attend school. We also focus on attendance while most studies of school meals in India have used enrollment as a measure of participation. While enrollment rates have been steadily rising, regular attendance continues to be a serious problem (Government of India, 2014; Educational Consultants India Limited, 2009).

Public primary schools in Delhi are spread across twelve zones. The data for our study are assembled from attendance and administrative records for about 1500 students in 19 randomly sampled schools in one of these zones. The schools in our sample are at most 15 kilometers apart and administered by the same nodal authority, the Municipal Corporation of Delhi (MCD). We restrict attention to this geography within the vast expanse of Delhi to limit the otherwise wide

[4]Maneka Gandhi, the current Minister proposed this on World Hunger day in 2017, and Renuka Chowdhury her predecessor proposed this in 2008. For an analysis of the recent change in Maharashtra, see Ramani (2017).

variation we are likely to encounter in unobservable school, child and household characteristics. Although we track attendance by child and can control for unobservable child characteristics that affect the level of attendance, restricting ourselves to a single zone makes it more likely that attendance trends for our sample of schools are also similar.

The school year for all public schools in Delhi starts in April and ends in March. For each student in our sample, we use attendance registers to compute student-level monthly attendance for the period April 2002- April 2004. Schools transitioned to cooked meals in two phases. We adopt a difference-in-differences strategy to compare attendance rates of students in schools that transitioned early to those that transitioned later. By using multiple observations on the same student over our study period, we are able to control for unobservable individual heterogeneity that affects the level of attendance rates. We are able to verify that treatment and control schools would have behaved similarly in the absence of the program by comparing attendance in the treatment and control school groups in the year prior to the initiation of the school meals. Both sets of schools have very similar intra-year attendance patterns before the intervention.

We find the cooked meals resulted in a 3 percentage point increase in the average monthly attendance rates. This effect varies considerably by grade and school shift. The largest effects are for young children and for schools that operate in the morning as opposed to afternoon shifts. We observe no effects on Grade 5 children while those in Grade 2 experience a 6 percentage point increase in attendance. These results suggest that cooked meals made attending school more attractive for young children. Section 1 contains a theoretical framework which can explain these different grade-wise results.

The impact we estimate is the combined effect of meal content, timing and frequency. Our coefficient estimates have to be interpreted appropriately. For example, the boys schools in our sample operate in the afternoon shift, in the same building occupied by a girls school in the morning. This makes it difficult to disentangle gender effects from those related to meal timing. Also, our data are administrative and collected retrospectively, so we do not have measures of food intakes or detailed household characteristics during the program transition. We do not know whether food, under either the ready-to-eat program or after the switch to cooked meals, was actually consumed by a particular child. In this sense we estimate the effects of changes in the design of a program, rather than the effects of actual treatment on particular children.

We conjecture that two main features of the new program caused the attendance gains. First, the more perishable and less portable cooked meals increased the regularity with which students were fed. A study conducted by the Nutrition Foundation of India around the time of our research reported that, on average, packaged snacks were distributed for only 50 days during the school year, although the national policy mandated 200 days (Sharma et al., 2006). In contrast, we find that cooked school meals were served for 150-200 days per school year during our study period. The shift away from packaged food with a long shelf-life may have reduced leakages in the delivery system and improved accountability. Second, a hot cooked meal may be perceived as nutritionally balanced and more satisfying than the snacks that were distributed under the previous regime. Many households in our sample are among the poorest in Delhi and parents work as day laborers or are self-employed in the informal sector.[5] The convenience of a meal at school may also have saved them the time and expense involved in feeding young children at home.

As with all studies using administrative data, there may be questions about the reliability of attendance data. School staff may have reasons to inflate attendance rates, especially in response to the meal plan. In their study of a school transfer program in the Indian city of Mumbai, Linden and Shastry (2012) find that teachers manipulated attendance records to benefit some students. Since we seek to identify the effects of a switch in a meal program that occurred well before the start of our study, we were not able to cross-check our data by using direct head counts and cannot fully eliminate this concern. We are somewhat heartened by the difference we observe across grades. If there were incentives to manipulate attendance, we might perhaps have seen them for children of all ages, whereas we see effects only for lower grades. We also note that in a nationwide study that compares attendance registers with head-counts from unannounced visits to schools, the two rates are roughly the same in urban Delhi (Educational Consultants India Limited, 2009), although differences are large in some other states. Our empirical strategy may not be viable in those contexts. One important difference between the cooked meal program we study and the take-home rations studied by Linden and Shastry (2012) is that in their program children were required to meet an attendance threshold in order to receive benefits. This may create incentives for teachers to move children from just below the threshold to above it. Nevertheless, we acknowledge the possibility of some reporting error in our attendance rates.

Our paper also contributes to a larger literature on how transfers to poor families act as an

[5] Admissions registers for each school list the occupation of the father. Although these data are not complete enough for us to use systematically, we do see a preponderance of casual laborers and informal sector workers.

instrument to improve the performance of school children. A variety of programs have been implemented across the world, including cash transfers, scholarships, free textbooks and school meals. Many of these initiatives have improved participation rates, especially for girls and young children (Glewwe and Kremer, 2006; Schultz, 2004). School meals in particular, have also been studied in many countries. For example, Alderman et al. (2012) compare take-home rations and school feeding programs of equivalent value in a region affected by conflict and food insecurity in Uganda and find large effects on attendance. Kazianga et al. (2012) compare the relative effectiveness of school meals and take-home rations in Burkina Faso and find little impact on absenteeism. Ravallion and Wodon (2000) and Ahmed (2004) find that the take-home rations program in Bangladesh resulted in higher school enrollment and daily attendance by students.

The rest of the paper is organized as follows: Section 2 provides a simple theoretical framework which forms the basis of our empirical approach and also illustrates why the impact of school meals may differ by school, country context, and the sample of households being considered. Section 3 discusses the data and estimation strategy. The results are presented in section 4 and section 5 concludes.

## 2 The Attendance Decision

An enrolled child attends school on a particular day based on the expected benefits and costs of attendance. We are agnostic about how these are aggregated within families and the precise role parents have in the attendance decision. We assume only that families trade-off benefits and costs of school participation which could depend on factors such as the opportunity cost of parental time, the need for the child's labor at home, and the quality of school instruction. Transfers of food have effects at the margin on attendance decisions that may be largely determined by these considerations.

For a child indexed by $i$, we denote the average cost of attendance by $\mu_i$. A child may face higher or lower costs on a particular day for idiosyncratic reasons. We capture these by the random variable $\epsilon$. Since all systematic factors are included in the mean $\mu_i$, we assume the distribution of $\epsilon$ is identical across students for each month. It is denoted by $F(.)$, has zero mean and density $f$.

For a student $i$ on a day $d$ of a given month, the cost of attending school is

$$c_{id} = \mu_i + \epsilon_d$$

and benefits per day are $b_i$. A child will attend if $c_{id} < b_i$ , so the monthly attendance rate is

$$A_i = F(b_i - \mu_i)$$

A transfer that increases benefits by $t$ will increase the attendance rate by

$$\Delta A_i = F(b_i + t - \mu_i) - F(b_i - \mu_i). \tag{1}$$

We see from equation (1) that the change in attendance in response to a transfer depends on child characteristics as captured by the levels of $b_i$ and $\mu_i$.

Figure 1 uses an example to illustrate why attendance gains may be non-monotonic in the average cost $\mu_i$. There are three different types of students with average costs of 5, 5.5 and 9 respectively and $\epsilon$ is normally distributed with unit variance. Benefits and transfers for all types are the same, with $b = 6.6$, $b + t = 7$. The change in attendance rates in response to a transfer is directly related to the density $f(\epsilon)$ over the interval $[b_i, b_i + t]$. Baseline attendance for the lowest cost student is 94.5 per cent and this changes by 3.2 percentage points (the sum of the solid and striped areas). The change in attendance is greatest (86 to 93 percent) for the student with intermediate costs (the sum of all three shaded areas). For the student with the highest cost, attendance hardly changes (the solid area).

Varying benefits by child would further complicate this figure. The purpose of the example is to show that unless $f$ is monotonic, attendance gains are not systematically related to the level of $\mu_i$ and to baseline attendance. Program effects depend on a possibly large set of factors that influence the costs and benefits of attendance. These might include grade level, gender, age and unobservable school and student characteristics. The next section estimates their importance for the cooked meal program.

# 3 Data and Empirical Strategy

## 3.1 Data

Our data come from a sample of public primary schools run by the Municipal Corporation of Delhi (MCD). The MCD administers 80% of all primary schools in Delhi and 90% of primary school students in the state system are in these schools (Mahajan and Goyal, 2005). There are about 1800 MCD schools divided into 12 zones. We restricted our study to a single zone (the central zone) to ensure that our sampled schools served similar households. Table A1 in the Appendix uses available census data to show that the demographic characteristics of this zone were similar to Delhi as a whole.

Students in the MCD schools first started receiving packets of biscuits and other snacks in 1997. These were received infrequently, even though the program stipulated daily distribution.[6] Following the Supreme Court order on cooked meals in 2001, the MCD invited applications from potential meal providers. This was done in two phases in 2003. The first set of contracts for 410 schools were signed in July. The program was extended in September and all schools were covered by April 2004. This resulted in exogenous staggering of the transition from ready-to-eat to cooked meals across schools. We use these differences in the timing of the cooked meals to study the impact of the new meal program on attendance.[7]

When we began our data collection in January 2008, there were three different meal providers in the central zone. The MCD administration provided us with lists of the schools served by them and we selected a random sample of 8 schools from each list, giving us with a total sample of 24 schools. Although these were not the original providers with whom contracts were signed in 2003, by stratifying our sample in this way we covered most areas in the central school zone. We gathered administrative data on attendance and meal content for two school years, 2002 and 2003. We use the 2003 data (before and after the transition to cooked meals) to estimate program effects and the 2002 data to test whether control and treatment schools had different attendance trends in the pre-program period.

From each school we collected data on meal frequency and content, child-attendance and school

[6]Biscuits were the most common snack, followed by rice and wheat puffs.

[7]This information was obtained from MCD administrative records.

infrastructure. Each school maintains a school meal register with a daily entry for the menu offered and the number of students present. These data are used by the MCD to compensate meal providers, who receive payments based on average daily attendance. From this register we obtained the exact date each school transitioned to cooked meals. We do not have retrospective data on the actual consumption of meals and therefore estimate the impact of meal availability. At the time of our data collection in 2008, we did measure uptake and found that it was near universal. In a random sample of 571 students in our sampled schools, 94.2% had consumed the cooked school meal on the day of our school visit.

Attendance rates are our primary dependent variable. We use monthly averages of daily attendance for each student in our sample collected from classroom registers for 2002 and 2003. The digitization of these attendance data was the most time-intensive part of our data collection process.[8] Some of the larger schools have more than one section per grade level. For these schools we followed a single, randomly chosen, section. Students seldom change their section as long as they are in the same school so we were able to construct a student level panel data set from these registers.

The school year starts on April 1st and has 200 work days. After joining their new classes in April, students get a summer vacation of about six weeks from the middle of May to early July. July has some late enrollments and also high absenteeism of already enrolled students because many families delay returning to Delhi after the summer. By September most students are attending regularly. Annual exams are in March and attendance is again sporadic and dependent on the schedule of exams. Our focus is on the months of April and September since April is the start of the school year and September neatly divides the school sample into two roughly equal sets of schools, one having made the transition to cooked meals and the other still using packaged snacks and acting as a control.

Gathering retrospective attendance data was complicated by the fact that schools are required to maintain attendance records for only 5 years and we started our study in 2008. We were provided with at least some attendance registers in 22 of the 24 schools. For 19 of these, we have some records for both the 2002 and 2003 schools years and for a smaller set of 12 schools,

[8]Data on aggregate attendance by grade are also available in a mid-day meal register since these are used to determine payments to providers. These are not however adequate for our purposes, since they do not record child-level data and would not allow us to control for student fixed-effects. The use of classroom registers has the additional advantage of recording daily attendance for each student and are more difficult to manipulate than aggregate head-counts.

attendance records for 2003 are complete for all grades. Our estimates by grade are restricted to students in these 12 schools.[9]

Among the 19 schools, cooked meals began in 9 schools by early August 2003. We call these Phase 1 schools. The rest continued to receive packaged snacks and transitioned to the meal program in October and November 2003. We call these Phase 2 schools. Children in Phase 1 school form our treatment group and we compare changes in their attendance between April and September 2003 with those in the Phase 2 schools. The attendance rates for grade 1 are less reliable than for the other grades because we are not certain of the date at which newly enrolled students actually began school. We therefore do not use grade 1 data. Figure 2 marks our schools on a zonal map with their treatment status and lists the neighborhoods in which they are located.

In addition to meal-timing and child attendance, we collected information on school infrastructure from a retrospective survey with school principals. Table 1 presents descriptive statistics for the 19 schools in our sample by treatment status. The top half of the table is based on census data for 2001, the census year closest to the meal transition. We use literacy rates and the fraction of disadvantaged castes for each of the census areas (called wards) in which a school was located and average these for schools in the treatment and control groups.[10] The bottom half of the table is based on our own administrative and survey data. On average, the areas in which the two types of schools are located are similar and their observable school characteristics are also comparable. Admittedly, we observe a limited set of characteristics which is why our empirical models control for child-level fixed effects.

## 3.2 Empirical strategy

We use student-level monthly attendance for the months of April and September 2003 to estimate the following model:

[9]The few observable school level characteristics we have are similar for the schools for which we obtained data and the 5 schools for which school records were missing.

[10]We do not report the fraction of Scheduled Tribes, the other officially recognized disadvantaged group, since there are hardly any such households either in the wards in which the schools are located or in the schools themselves.

$$A_{ijm} = \alpha_0 + \alpha_1 * Sept + \alpha_2 Treat_j * Sept + \mu_i + \epsilon_{ijm}. \quad (2)$$

$A_{ijm}$ is the attendance rate for child $i$ for month $m$ in school $j$, *Sept* is an indicator for the month of September and $Treat$ is an indicator for Phase 1 schools which constitute our treatment group that transitioned to cooked meals in July and August 2003. The child fixed-effect $\mu_i$ captures time-invariant factors that affect attendance rates of child $i$ and $\epsilon_{ijm}$ is an idiosyncratic error term. The difference-in-difference (DID) estimate of the impact of the cooked meal program is given by $\alpha_2$. With April as the base month, this is the additional increase in attendance for the April-September period for treatment relative to control schools.[11]

Although the phased implementation of the program is plausibly exogenous, there may be unobservable, time-varying school characteristics that influenced both the introduction of the meals and attendance rates. To address this concern, we estimate (2) using attendance rates in 2002 for equivalent grades in our sample of 19 schools. This provides us with a more stringent test of our program effect - if we find treatment schools did better in 2002, before the program was initiated, it is likely that some of the program effect we estimate in 2003 is driven by different attendance trends in the treatment and control sample. As we see in the next section, this was not the case.

Since our empirical model includes student fixed-effects, it can only be estimated on the sample of children enrolled in both April and September. In this sense, our estimates measure program impact at the intensive margin. We restrict ourselves to this margin because many of the children attending municipal schools are from families of laborers and informal sector workers who either do not have permanent homes in Delhi or move between schools and neighborhoods. There are therefore frequent changes in the composition of the student body whose effects on aggregate attendance are hard to predict given the minimal information we have on student backgrounds. Reported standard errors for all our estimates are clustered at the student level. As treatment is at the school-level, we also report Moulton-effect corrected standard errors for intra-cluster correlation at school level.

[11] There are two reasons for restricting our analysis to April and September 2003. First, while we collected attendance data for all months of 2003, these were available for only four months of 2002- April, July, September and February. We can therefore test for parallel trends in 2002 using only these months. Second, we exclude July because it was a month of program transition and February because all schools had switched to cooked meals by November 2003.

As discussed above, we estimate average treatment effects using the school-grade panel of 19 schools for which we have attendance data in both 2002 and 2003. Keeping equivalent grades of the same schools for the two years gives us a sample of 1591 children for 2003 and 1564 for 2002. For this school sample, we also construct a panel of 827 students who were present in 2002, before the program began, and remained in the same school through 2003.[12] Our first set of results are based on estimating equation (2) for these sets of students. We also estimate equation (2) separately for girls and boys. We are however limited in our ability to identify gender effects because our school sample has only two co-educational schools. For the rest, the girls' schools are in the first half of the day while the boys attend afternoon shifts. This confounds gender effects with those of meal timing.

For the subsample of 12 schools with complete grade-wise attendance data in 2003, we are able to estimate and compare program effects by grade, keeping school characteristics unchanged. As already mentioned, we restrict ourselves to grades 2-5 because we do not have accurate admission dates for grade 1. Table A2 summarizes the samples used in our analysis.

# 4 Results

## 4.1 Average attendance gains

Table 2 shows average attendance rates and changes for schools in the treatment and control groups for the years 2002 and 2003. Prior to the introduction of cooked meals, in April 2002, the average attendance rate for the treatment schools was slightly lower but attendance in both treatment and control schools increased by 0.06 between April and September 2002. In contrast, between April and September 2003, attendance gains in control schools were similar to the previous year (0.07) while treatment schools had a much larger increase (0.11). The additional gain of 4 percentage points made by the treatment schools in 2003 suggests a sizable impact of the cooked meal program.

Table 3 presents our main estimates of program impact. The first column has coefficient esti-

[12]Each student, at the time of admission, is identified by a unique enrollment number. We use this to match attendance records in 2002 and 2003. We lose 3 schools for which we could not obtain the attendance records of the relevant grades in 2002.

mates of (2) for the program year, 2003. Column 2 presents estimates from the same model for 2002, the year prior to the program. Since attendance varies by grade, we restrict our sample in both cases to include only those grades in our 19 schools for which we have attendance data for both 2002 and 2003. We have two observations per student because we use attendance data for the two months of April and September for each student. From Column 1 we see that the new program resulted in an estimated increase in student attendance of 2.7 percentage points. From Column 2, we see that the coefficient on $Treat \times Sept$ is close to zero (though imprecisely estimated), suggesting that attendance trends in treatment schools were similar to those in control schools in the pre-program period.

Column 3 has estimates from equation (2) for the smaller set of 827 students who were enrolled in both years and for whom we therefore have data from April 2002 through September 2003. The estimate of 3.1 percentage points is slightly higher than the estimate in Column 1, possibly indicating that students from families with stable enrollment were more responsive to the program. Once again, we do not see similar effects for 2002 (Column 4) suggesting that the changes in 2003 are indeed attributable to the cooked meals.

## 4.2 Grade, gender and timing

To examine differential effects by grade, we estimate equation (2) for the sample of 12 schools with complete grade-wise attendance data for 2003. Results are in Table 4. In spite of the smaller school sample, we have 1680 students because we are not conditioning on the availability of data for the same grade in 2002. The average treatment effect for this sample is 3 percentage points, close to our previous estimates. The highest effect of 5.8 percentage points is for second grade.

Table 5 compares estimates by gender. We find a positive effect of the meals only on girls (Columns 1 and 2). This gender effect, however, is confounded with the effect of meal timing in our sample. Most MCD schools run in two shifts - morning and afternoon. Schools for girls and co-educational schools both operate in the morning and end between noon and 1 p.m. while many boys' schools operate in the afternoon, from 1 p.m to 6 p.m., in the same buildings. Columns 3 and 4 show meal effects for morning schools. In the last column we restrict the sample to schools that operate in the morning and interact the treatment indicator with gender for students in these schools. We find no systematic difference in the impact for boys and girls. These coefficients are however difficult to estimate precisely given our small sample of boys in morning schools. We have a total sample of 303 boys, of which 116 are in the treated schools. Our results on gender may therefore simply reflect better-timed meals. It is possible that a meal served in the late afternoon, around 3.00 p.m., is less attractive than one served in the middle of the morning, around 10.30 a.m. Since morning classes begin early in the day, students may not be able to eat a full meal at home before coming to school, unlike students in the afternoon shifts.[13]

## 4.3 Robustness checks

All our estimates report standard errors clustered at the student level while our treatment is at the school level. Given the small number of schools in our sample, clustering standard errors at that level could bias estimates. We address this by reporting the Moulton-factor adjusted standard errors for clustering at school level in Table 6. The Moulton adjustment is a parametric correction of OLS standard errors by accounting for intra-cluster correlation (Angrist and Pischke, 2009). Column 1 in Table 6 recaps the results from Table 3 and 4 reported earlier. These are the treatment estimates with student-fixed effects. To account for school-level heterogeniety, we run equation (2) with school, instead of student, fixed effects. Column 2 shows the same results but with school-fixed effects. The estimated treatment effect remains unchanged but the standard errors increase slightly. School-fixed effects will not account for intra-cluster correlation within schools and we adjust for this by making the Moulton correction in this specification. In column 3, we report the Moulton-adjusted standard errors. Our estimates remain significant after including school-fixed effects and correcting for intra cluster correlation.[14]

## 4.4 Potential Mechanisms

All our results point to improved rates of school participation, particularly for younger children, following the transition to cooked meals in public primary schools. There are several reasons why cooked meals may have raised the level of transfers and better linked them to school participation. Prior to cooked meals, biscuits and other snacks were typically procured by

[13] The school breakfast program in the United States was instituted to ensure children were fed before the start of the school day.

[14] In a similar exercise, we adjusted for school intra-cluster correlation in the student-fixed model and find no difference in Moulton adjusted standard error and standard errors clustered at the student level.

the school teachers for several weeks at a time, using funds released by the MCD. Delays in receiving funds and difficulties in finding staff to procure the snacks would often lead to interrupted distribution. The cooked meals shifted the responsibility of provision away from schools to specialized providers. Also, since meals are prepared daily, their supply can better match attendance. They are also less portable and easier to monitor, leading to fewer leakages through corruption. For some schools in our sample for which we have information on the frequency of distribution around the time of the transition, the percentage of school days with no distribution was 50 percent lower under the cooked meal program. This is consistent with data from the Nutrition Foundation of India (Sharma et al., 2006) and other qualitative studies (Khera, 2006) of the mid-day meal program.

We obtained detailed data on costs of the program to the government under both regimes and these figures are presented in Table 7. The new meals came with clearly specified nutritional norms of 300 calories and 8-12 grams of protein per meal. The grain content of the meal was provided by the state and this was increased from 75 to 100 grams of wheat per meal to meet the new norms. Since this was transferred from grain warehouses of the government and not purchased at market prices, the associated increase in cost was marginal.[15] Under the cooked meal program providers were reimbursed at the rate of Rs. 2.35 per child per day for expenditures incurred on meal ingredients other than grains. These include vegetables, spices, fuel, transportation and wages paid for meal preparation and distribution. This was similar to the per child per day cost under the ready-to-eat regime. In fact, during 2002-04 the real cost of the program was unchanged as indicated by Table 7.[16] With comparable per unit program expenditures, the targeting and nutritional content of the meals improved.

The reason for larger effects among younger children could be that the meal provided forms a larger proportion of their required daily calorie intake (Afridi, 2011). The attendance decision of these children could also be more sensitive to transfers for other reasons. For example, a child in grade 5 may care much more about school quality than small transfers of food, whereas a child entering school may be attracted by a meal. In terms of our framework in Section 1, the density of the cost distribution is higher for young children at the point at which the daily benefits from attending equal daily costs. The absence of detailed student and household level information during the transition prevents us from a quantitative attribution of the overall effect to these factors. Nevertheless, our results show that school programs that are more effective

[15] The cost increase was approximately Rs. 0.11, or less than 5% per child per day.

[16] We have not included in our cost calculation teacher salaries, which are set according to government norms. To the best of our knowledge, teacher salaries did not change between 2002 and 2003.

in delivering the transfer and are better aligned with the needs of the recipient are likely to provide greater benefits relative to costs.

# 5 Conclusion

This paper examines whether fiscally neutral changes in the design of a school feeding program can have substantial effects on student participation rates. Public primary schools in Delhi transitioned from an on-site program of providing packaged snacks such as biscuits to serving cooked meals to all enrolled students in the 2003-04 academic year. This transition was rolled out in two phases over a period of five months in schools managed by the Municipal Corporation of Delhi (MCD). We use this staggered implementation of the program across MCD schools to identify transition impacts, comparing the attendance rates of students of grades 2 to 5 in schools that transitioned to cooked meals first to those that transitioned later in the year. We find that changes in meals that make them more nutritious, easier to monitor, and timed to better match students' eating schedules can be a cheap and effective way of raising primary school attendance rates, especially for lower grades.

Our results align with the existing literature on the effects of school meals on student participation rates. However, we are better able to look at the effects of design because the transition to cooked meals was not accompanied by significant cost changes. These findings are especially relevant in the Indian context, where the transition to cooked meals and then to their improved and regulated quality is yet to be completed in most parts of the country.[17] Moreover, we find that the impact of the meals is substantial even through initial attendance rates were high relative to the national average of 68% (Educational Consultants India Limited, 2009). Therefore, our estimates may well be a lower bound on the potential impact of this type of programmatic change on school participation.

Our research shows that greater attention to the design and delivery of school transfer programs can yield high returns. A recent study on another major social insurance program in India, the National Employment Guarantee Scheme, has found that the use of technology through biometric smartcards reduced leakages and improved delivery (Muralidharan et al., 2017). We

[17] Shukla (2014) finds that protein and calorie standards ten years were not met by most meals, even ten years after the cooked meals were introduced. Continued improvements in the meals may lead to further gains in school participation.

find analogous improvements in the context of school meals.

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Figure 1: The Effects of School Transfers on Attendance.

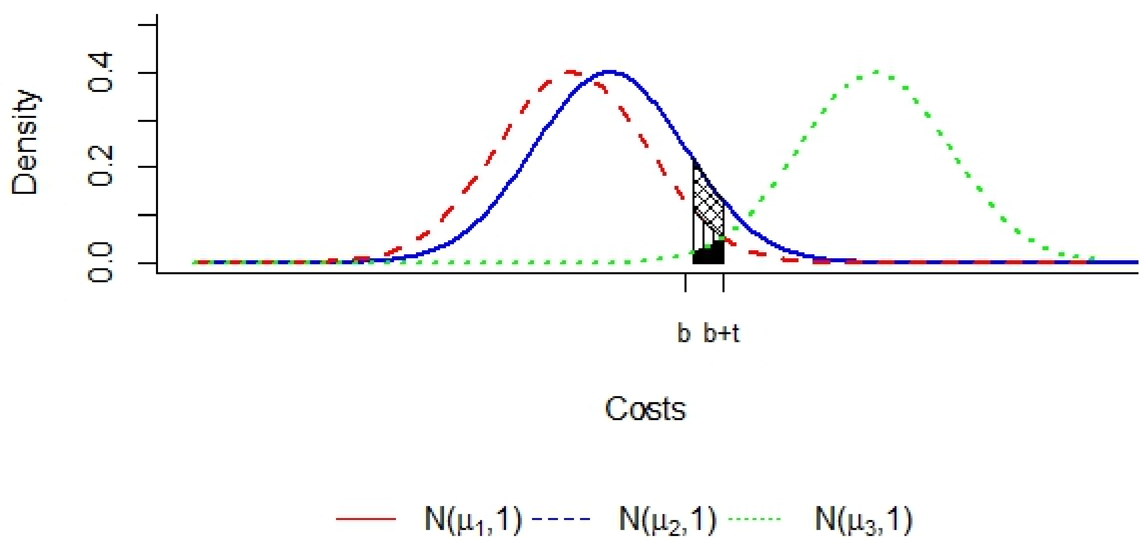
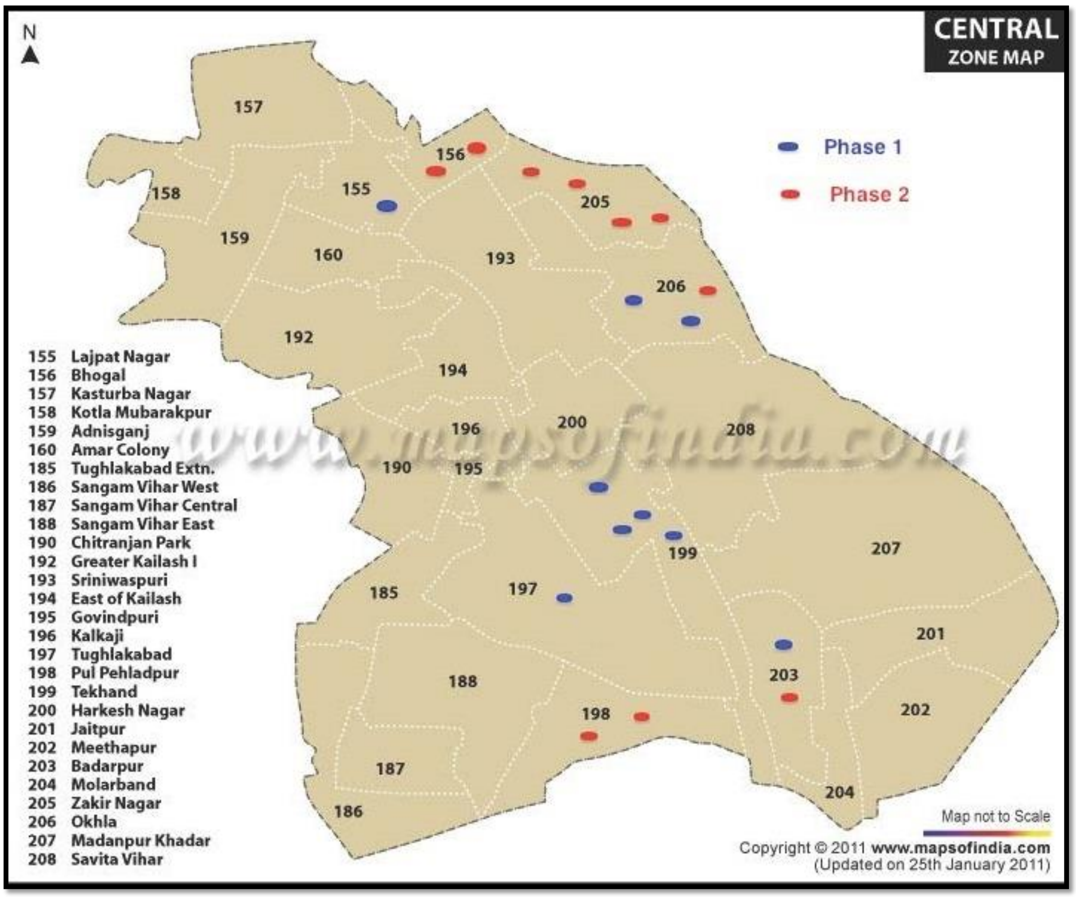


Figure 2: Sampled Schools and their Neighborhoods.




Neighborhoods are demarcated by lines and listed in the legend. Phase 1 schools are those that transitioned early to the cooked meal program and form our treatment group. Phase 2 schools transitioned later and form the control group.

Table 1: Descriptive Statistics by Treatment Status.

| | All | Control | Treat | Difference |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4)=(3)-(2) |
| *Neighbourhoods* | | | | |
| Literacy rate | 0.79 | 0.81 | 0.77 | -0.04 |
| | (0.014) | (0.007) | (0.027) | (0.027) |
| Male literacy rate | 0.86 | 0.87 | 0.85 | -0.02 |
| | (0.008) | (0.003) | (0.016) | (0.016) |
| Female literacy rate | 0.69 | 0.72 | 0.66 | -0.06 |
| | (0.025) | (0.016) | (0.048) | (0.049) |
| Proportion of scheduled castes | 0.13 | 0.12 | 0.14 | 0.02 |
| | (0.018) | (0.022) | (0.030) | (0.037) |
| *Schools* | | | | |
| Attendance in April 2003 | 0.79 | 0.81 | 0.78 | -0.03 |
| | (0.018) | (0.027) | (0.026) | (0.037) |
| Proportion of girls in April 2003 | 0.62 | 0.50 | 0.76 | 0.26 |
| | (0.107) | (0.166) | (0.126) | (0.212) |
| Distance from provider in 2003 | 4.57 | 4.44 | 4.71 | 0.27 |
| | (0.904) | (1.225) | (1.413) | (1.861) |
| School infrastructure score | 4.83 | 4.78 | 4.89 | -0.11 |
| | (0.121) | (0.222) | (0.111) | (0.248) |
| Number of schools | 19 | 10 | 9 | |

*Notes*: The top panel of the table is based on data from the Census of India, 2001. Schools are matched to the census wards in which they are located. Attendance and gender composition is for the sample of 19 schools and the grades described in the text. Grade 1 is excluded. The school infrastructure score is the number of facilities present out of the following 5 facilities in April 2003 - library, computer room, playground, drinking water and toilets. School infrastructure data are missing for one control school. The proportion of Schedules Tribes is close to zero and not reported.

Standard errors are in parentheses.

None of the differences between treatment and control schools are statistically significant at conventional levels.

Table 2: Average Attendance Levels and Changes between April and September.

| | Control (1) | Treatment (2) | Difference (3)=(2)-(1) |
|---|---|---|---|
| (A) Δ 2002 | 0.06 | 0.06 | 0 |
| | (0.008) | (0.010) | (0.013) |
| Mean attendance in April 2002 | 0.81 | 0.79 | |
| | (0.073) | (0.087) | |
| (B) Δ 2003 | 0.07 | 0.11 | 0.04*** |
| | (0.008) | (0.009) | (0.012) |
| Mean attendance in April 2003 | 0.80 | 0.78 | |
| | (0.086) | (0.063) | |
| Difference (B)-(A) | 0.01 | 0.05*** | 0.04*** |
| | (0.011) | (0.014) | (0.018) |

*Notes*: The sample consists of grades in 19 schools (10 control and 9 treatment) for which attendance data were available for 2002 and 2003. Attendance rates are calculated by averaging over all enrolled students in grades 2-5 in the relevant month.

Standard errors in parentheses.

*p<0.1; **p<0.05; ***p<0.01 .

Table 3: Average Program Effects.

| Dependent Variable: Attendance in year | 2003 (1) | 2002 (2) | 2003 (3) | 2002 (4) |
|---|---|---|---|---|
| Treat × Sept | 0.027** | 0.006 | 0.031* | 0.007 |
| | (0.012) | (0.011) | (0.016) | (0.015) |
| Sept | 0.073*** | 0.047*** | 0.081*** | 0.066*** |
| | (0.008) | (0.007) | (0.012) | (0.009) |
| Constant | 0.816*** | 0.844*** | 0.815*** | 0.845*** |
| | (0.003) | (0.003) | (0.004) | (0.004) |
| Student fixed effects | X | X | X | X |
| No. of schools | 19 | 19 | 16 | 16 |
| No. of students | 1591 | 1564 | 827 | 827 |
| $R^2$ | 0.121 | 0.047 | 0.149 | 0.094 |
| Observations | 3182 | 3128 | 1654 | 1654 |

*Notes*: Columns 1 and 2 are based on the sample of those grades in the 19 schools for which attendance records were available for 2002 and 2003. Column 1 shows estimates for 2003 and column 2 for 2002. Columns 3 and 4 are based on the panel of 827 students who were present from April 2002 to September 2003. These 827 students are from 16 of the 19 schools.

Standard errors clustered at the student-level in parentheses.

*p<0.1; **p<0.05; ***p<0.01 .

Table 4: Program Effects by Grade.

| Variables | Grades | | | | |
|---|---|---|---|---|---|
| | **2 to 5** | **2** | **3** | **4** | **5** |
| | (1) | (2) | (3) | (4) | (5) |
| Treat x Sep | 0.030*** | 0.058** | 0.023 | 0.041* | 0.003 |
| | (0.011) | (0.023) | (0.020) | (0.022) | (0.024) |
| Sep | 0.059*** | 0.023 | 0.053*** | 0.043** | 0.112*** |
| | (0.008) | (0.015) | (0.014) | (0.017) | (0.018) |
| Constant | 0.835*** | 0.832*** | 0.838*** | 0.844*** | 0.822*** |
| | (0.003) | (0.006) | (0.005) | (0.005) | (0.006) |
| Student fixed effects | X | X | X | X | X |
| No. of schools | 12 | 12 | 12 | 12 | 12 |
| No. of students | 1680 | 404 | 448 | 427 | 401 |
| $R^2$ | 0.097 | 0.062 | 0.090 | 0.085 | 0.181 |
| Observations | 3360 | 808 | 896 | 854 | 802 |

Notes: These estimates are based on the sub-sample of 12 schools for which we obtained attendance records of 2003 for all grades. Standard errors clustered at student-level in parentheses. Significance levels *** 1% ** 5% * 10%.

Table 5: Program Effects by Gender and School Shift.

| Dependent Variable: Attendance by | Gender | | School shift | | |
|---|---|---|---|---|---|
| | Boys | Girls | Afternoon | Morning | Morning |
| | (1) | (2) | (3) | (4) | (5) |
| Treat × Sep | -0.007 | 0.020* | -0.008 | 0.030*** | 0.020* |
| | (0.019) | (0.011) | (0.025) | (0.011) | (0.011) |
| Sep | 0.077*** | 0.078*** | 0.111*** | 0.059*** | 0.078*** |
| | (0.011) | (0.008) | (0.012) | (0.008) | (0.008) |
| Boy ×Treat ×Sep | | | | | 0.033 |
| | | | | | (0.031) |
| Boy × Sep | | | | | -0.092*** |
| | | | | | (0.020) |
| Constant | 0.823*** | 0.816*** | 0.785*** | 0.830*** | 0.830*** |
| | (0.004) | (0.003) | (0.005) | (0.003) | (0.003) |
| Student fixed effects | X | X | X | X | X |
| No. of schools | 8 | 14 | 5 | 14 | 14 |
| No. of students | 848 | 1405 | 545 | 1708 | 1708 |
| $R^2$ | 0.078 | 0.145 | 0.159 | 0.104 | 0.119 |
| Observations | 1696 | 2810 | 1090 | 3416 | 3416 |

*Notes*: All grades of the 19 schools for which attendance records of 2003 were available are included. Standard errors clustered at student-level in parentheses.

$^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01

Table 6: Robustness checks.

| | (1) | (2) | (3) |
|---|---|---|---|
| *Panel 1: Repeated cross-section school and grade* | | | |
| Treat x Sep | 0.027** | 0.027** | 0.027** |
| | (0.0117) | (0.0126) | (0.0125) |
| No. of students | 1591 | 1591 | 1591 |
| No. of schools | 19 | 19 | 19 |
| *Panel 2: Student panel* | | | |
| Treat x Sep | 0.031* | 0.031* | 0.031* |
| | (0.0163) | (0.0168) | (0.0168) |
| No. of students | 827 | 827 | 827 |
| No. of schools | 16 | 16 | 16 |
| *Panel 3: Grade sample* | | | |
| Treat x Sep | 0.030*** | 0.029*** | 0.029*** |
| | (0.011) | (0.0114) | (0.012) |
| No. of students | 1680 | 1680 | 1680 |
| No. of schools | 12 | 12 | 12 |
| FE | Student | School | School |
| Standard error | Het. Robust | Het. Robust | Moulton adjusted |

Notes: Panel 1- repeated cross-section of school-grade in 2003 and 2002; Panel 2 - panel of students who were enrolled from April 2002 to September 2003; Panel 3 - sample of schools with complete grade attendance records in 2003. Significance levels *** 1% ** 5% * 10%.

Table 7: Costs of the Cooked Meals (Indian Rupees per day).

| | Costs | Ready-to-eat Snacks 2002-03 | Cooked Meals 2003-04 |
|---|---|---|---|
| | *Federal Costs* | | |
| 1 | Payment to FCI for food grains | 0.31 | 0.41 |
| 2 | Transportation subsidy to provider | 0.0378 | 0.05 |
| | *Delhi Government Costs* | | |
| 3 | Cost of cooking (paid to service providers) | 2 | 2 |
| | Total nominal cost (1+2+3) | 2.3478 | 2.46 |
| | Total real cost (WPI ) | 2.3478 | 2.3327 |
| | Total real cost (CPI) | 2.3478 | 2.3736 |

*Source*: Data obtained via official communications from the Ministry of Human Resource Development (via Letter No- D-186 DDE/MDM(HQ)/11); wholesale price index (WPI) and consumer price index (CPI) for Delhi obtained from the Annual Report of the Planning Commission of Delhi.

*Notes*: FCI - Food Corporation of India. Transportation subsidy to provider per child per day= Transport subsidy per gram x food grain allocation per child per day.

## Appendix

Table A1: Demographic Characteristics for the Sampled Zone and Delhi.

| | Delhi (Urban) | MCD Central Zone |
|---|---|---|
| Total literacy | 0.82 | 0.82 |
| Male literacy | 0.87 | 0.88 |
| Female literacy | 0.75 | 0.74 |
| Scheduled caste | 0.17 | 0.16 |
| *Occupation of main worker* | | |
| Cultivators | 0.002 | 0.002 |
| Agricultural labor | 0.001 | 0.002 |
| Household industry | 0.03 | 0.02 |
| Others | 0.97 | 0.98 |

*Notes*: Data aggregated from ward level statistics from Census, 2001. Main workers are defined as those household members who spend the most time working for income. Household industry is defined as small scale businesses run by members of the household within its precincts. 'Others' primarily includes salaried workers and those engaged in services sector .

Table A2: Samples used in our analysis.

| | Sample | Number of schools | Number of students | Description |
|---|---|---|---|---|
| 1a. | School-grade panel | 19 | 1591 | Schools with attendance data for the same grades in both 2002 and 2003 |
| 1b. | Student panel | 16 | 827 | Students observed in both 2002 and 2003 |
| 2. | School-grade subsample | 12 | 1680 | Schools with attendance data for all grades in 2003 |