YOUNG LIVES STUDENT PAPER

Do School Meals Work? Treatment Evaluation of the Midday Meal Scheme in India

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Young Lives
An International Study of Childhood Poverty

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All errors are my own. I would appreciate comments and feedback at abhijeetsingh1@gmail.com

Abstract

Despite the popularity of school meals as interventions in education, their effect on learning and health outcomes is not clear. This study uses newly available longitudinal data from the state of Andhra Pradesh in India to estimate these effects in a non-experimental setting. Further, it aims at disaggregating the average program impacts to see if some groups benefited more than the others i.e. whether heterogeneity in program impacts was present. We use changes in WHO anthropometric z scores as the outcome variables to evaluate impact on nutrition, and scores on the Peabody Picture Vocabulary Test (PPVT) to assess the impact on learning skills. The approach taken employs several different estimation strategies to correct for econometric issues in estimation; our preferred estimate for estimating health gains adopts an Instrumental Variables (IV) approach and for learning gains we use a difference-in-means estimator with propensity score matching methods.

We find the scheme delivers non-trivial gains in both nutrition and learning which are highly significant. The program acts as a security net for children, cushioning them from negative nutritional factors; in particular, among younger children, there are large and significant gains for children who suffered from the impact of drought. In cognitive skills, we find that school meals boost PPVT scores by over 0.6 s.d. Evidence presented here, combined with previous findings of gains in school participation in other studies, reaffirms the effectiveness of school meal programs in developing countries.

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Introduction

There is a long history of feeding programs in schools around the world. These programs are premised on expectations of significant gains in schooling and nutritional outcomes; in developing country contexts particularly, school meals are thought to exert powerful incentives that increase school participation. Additionally they are thought to help in addressing problems of undernourishment among school children through nutritional supplementation. It is also expected that indirectly these programs will lead to improved levels of learning through various channels: by boosting attendance, by reducing 'classroom hunger' and thus improving concentration, and by improving the children's overall levels of nutrition (and thereby productivity).

The evidence, however, of the impact of school feeding programs on several of these outcomes is rather thin. While there is evidence that school feeding does indeed improve the immediate nutritional intake of children (Jacoby, 2002;Afridi 2005) and school participation rates (e.g. Afridi, 2007;Dreze and Goyal2003), the effect of these programs on learning, cognitive skills or longer term nutritional status is not clear. For example, Kremer and Vermeersch (2004) find some evidence of improved learning but only in schools with experienced teachers while Adelman *et al* (2008) find an impact on test scores but only for children between 11-14 years and not for younger children. The effect on long term nutrition is even more of a mystery: there are few studies documenting the effect of school feeding programs on indicators of child nutrition, and those that are available find ambiguous effects (e.g. Kremer and Vermeersch, 2004). Given the growing popularity of similar interventions in schools across the world, and the resources being devoted to them, it is important that these hypotheses are subjected to greater scrutiny and rigorous evaluations.

Furthermore, it is not enough to know the *average* impact of the program on recipients: we need to know who benefits most from these programs. Do the benefits vary by the gender, caste or initial nutritional status of the recipient? Does the program help mitigate the effect of negative nutritional shocks on children? Clearly, the distributional impacts of these programs, as of any other, may also be of great interest.

This paper attempts addressing the gaps in our knowledge identified above. Using a recent longitudinal dataset from India, we attempt to assess the impact of a nationally mandated school meal program on the nutritional status and cognitive skills of children in primary and upper primary schools. Further, we try disaggregating these average impacts on the beneficiaries to understand the distributional pattern of these benefits.

The data we use come from a longitudinal study of children in poverty collected by the Young Lives Project in the state of Andhra Pradesh (A.P.) in India. The survey collected extensive information about children in two cohorts (born in 1994/95 and 2001/02 respectively) in 2002 and 2006/07. The school feeding program, known as the Midday Meal Scheme in India (henceforth MDMS), was introduced in Andhra Pradesh in January 2003.

We use anthropometric z scores as the outcome variables to determine the impact of the program on health and nutritional status. For the younger cohort we use z scores on two measures - weight-for-age and height-for-age[1]. For the older cohort we use height-for-age and BMI-for-age as the use of weight-for-age is only recommended by the WHO up to the age of 10 years; all children in this cohort were aged over 10 years in 2007.

Different anthropometric measures correspond to different biological states. Deficit in the weight-for-age measure corresponds to the biological state of being *underweight*. According to Svedberg (2000) –*"Children with a low weight-for-age thus comprise both those who are chronically and those who are acutely deprived in terms of nutrition and/or healthcare."* Deficit in the height-for-age measure corresponds to linear growth retardation i.e. the inability to reach the genetic potential in terms of height. This is supposed to be a longer term measure of deprivation than weight-for-age which is more sensitive to short-term or seasonal variations in food availability. Height, and by extension height-for-age, is also said to have a strong relationship with mental function and mortality (Gopalan, 1992). BMI-for-age is a measure of the Body Mass Index normalized by age as z scores with reference to an international population.

This paper offers several significant contributions: it contributes to the broader literature on school feeding and nutritional supplementation addressing the gaps that were identified earlier in the discussion; it is the only econometric evaluation, to our knowledge, of the effect of the Midday Meals Scheme in India on the health and learning outcomes of children; and finally, it is one of the few attempts at using non-experimental data in evaluating the impact of

[1] A third anthropometric score – BMI-for-age – is also computable for the younger cohort. It is not reported in this paper since its suitability for children till the age of 5 is not clear. We did run the same analysis on it however yielding no consistent patterns. Results are available on request. Analysis on weight-for-height was not possible because the new WHO standards only allow weight-for-height z scores to be computed till 60 months of age, a threshold that had already been crossed by much of the younger cohort.

school feeding that successfully tries to correct for self-selection and incorporates the dynamic aspects of health determination.

Briefly, the results do not indicate a general benefit in health for all program beneficiaries. They do however suggest large benefits for children in drought-stricken areas; results from our preferred specification suggest gains of about 1 standard deviation in both weight-for-age and height-for-age z scores, which more than compensates for significant negative impact of the drought on health.

With regard to cognitive skills, the results suggest an average improvement of over 0.6 standard deviations in the PPVT raw scores which is a significant improvement.

The rest of the paper is structured as follows: the next section gives a brief introduction to the Midday Meal Scheme; Section 3 outlines our conceptual framework; Section 4 reviews the data, the estimation strategy and the specification of the analysis and presents the results of the analysis and Section 5 concludes.

2. The Program

Midday Meals are the most important of all government initiatives in education in recent years. Under the scheme, on every school day, all students in primary classes in public schools are to be provided a cooked meal consisting of no less than 300 kcal and 8-12 grams of protein.

Though officially started in 1995, the National Midday Meal Scheme remained unimplemented in most states till 2002. Following a Supreme Court ruling in November 2001, most states started providing school meals by 2003. As such it represents, at least in outreach, one of the most successful government interventions in recent years, having now become universal across the country.

Andhra Pradesh, the State covered in the Young Lives surveys, started providing Midday Meals in January, 2003 to children in all primary and upper primary public and private aided schools. As several studies indicate, this Scheme was near universal from the very beginning. Dreze and Goyal (2003) report full implementation of the Scheme in 2003 in A.P. In later years, Thorat and Lee (2005) and Pratham (2007) report that over 98% of government schools in the State were serving a Midday Meal on the day of their school survey.

In India, much interest was generated in the performance of the Midday Meal Scheme (MDMS) after 2001, when the issue entered mainstream political and media discourse. As a result, several field studies were carried out and reported over the next few years. Most studies of the program in India, with the exception of Afridi (2005, 2007), were non-econometric in nature and looked at descriptive statistics based on school records.

Khera (2006) is the best review article of these surveys; it lists nine surveys done in the period 2003-2005 focusing on MDMS and reviews their major findings. In general the surveys focussed on the effect of the scheme on enrolment, attendance and retention as well as aspects of infrastructure change, caste discrimination and opinions of stake-holders (teachers and parents) about the scheme. The surveys were almost unanimous in documenting a rise in attendance rates as well as enrolment rates especially benefiting girls and in one study children from the scheduled castes. Afridi (2007) confirms these findings using a difference-in-differences estimator, finding large benefits in school participation especially for girls.

Afridi (2005) is the only paper that looks at the nutritional impact of the program in India. Using a 24-hour recall of food intake in a randomised evaluation in Madhya Pradesh she found that *"daily nutrient intake of program participants increases by 49% to 100% of the transfers. For as low a cost as 3 cents per child, the program reduces daily protein deficiency of participants by 100% and calorie deficiency by almost 30%."*

The questions that we are interested in, namely those of long term impacts on child health and learning ability, have not been dealt with satisfactorily in the literature. We know that Midday Meals help increase child school participation and daily calorific intake on school days, but we are clueless about how it then impacts their health and learning outcomes over a longer horizon. It is this gap in our knowledge that this paper wishes to address, at least in the Indian context.

3. Conceptual Framework

Following Senauer and Garcia (1991), and Behrman and Hoddinott (2005), we visualize child health as entering into the welfare maximization problem of the household as an argument, thus reflecting the intrinsic value of child health to the household. we assume welfare to be positively related to nutrition i.e. welfare increases as health improves.

We think of health being determined through a health production function of the form:

$$H_i = H(F_i, C_i, D_i, G_i, U_i) \qquad -(1)$$

where H_i is the health of the child as indicated by anthropometric z scores , F_i is that child's food consumption, C_i is a vector of the child's observable characteristics, such as caste, gender and health status in the previous period , D_i is a vector of the observed personal characteristics of the child's parents, such as their occupation and education, G_i is a vector of household characteristics, such as household location and wealth, and U_i is a vector of unobserved attributes of the child, parents, household, and community which affect the child's health status, for example, the child's genetic endowment, the parental preferences and unobserved environmental factors like the disease environment and community sanitation. we assume that the Midday Meal program results in a net increase in child food intake, as found for example by Jacoby(2002); this increase is given by ΔF. In this case the change in health, ΔH will be determined as under:

$$\Delta H = f(\Delta F, \Delta G, C_i) \qquad -(2)$$

This means that changes in the health status are driven by changes in food intake, changes in household characteristics (such as an economic shock affecting the household)[2] and the vector of child attributes(C). The vector C_i is included in the function to allow for heterogeneous program impact based on child characteristics.

The vector C_i includes the child's health status from the previous period. We include lagged health status as part of the function since the evolution of health is most likely best modelled as a dynamic process where a person's health this period is strongly influenced by his/her health in the previous period All of this implies that the evolution of nutrition and health depends in part on the *lagged values* of nutrition/health from the previous period.

The strategy for estimating health benefits is derived from the above conceptual framework. we model health status being determined as under:

$$Y = \alpha + \beta_1.MDMS_{it} + \beta_2.X_i.t + \beta_3.Z_{it} + \beta_4.MDMS_{it}.Z_{it} + \gamma.t + \mu_i + \varepsilon_{it}$$

Where Y is the child's anthropometric score, MDMS is a binary variable denoting treatment status, X is a vector of time-invariant attributes that includes a child's caste and gender, parental education, initial wealth and household location (urban/rural)[3], Z is a vector of child-specific time-varying attributes like shocks, t is a 'time dummy' equalling 1 for Round 2 and μ is

[2] Representing nutritional shocks as ΔG here, we are also interested in seeing whether $\partial H/\partial F.\partial G = 0$ – if not, then clearly there is heterogeneity in impact for children affected by negative shocks.

[3] Because we do not have consumption expenditure data for Round 1, we use a wealth index to account for households' economic prosperity.

the unobserved fixed effect. At this stage, we do not include lagged value of the dependent variable for ease of analysis.

Differencing equation (3) gives us the following:

$$\Delta Y = \beta_1 \Delta MDMS_i + \beta_2 . X_i + \beta_3 . \Delta Z_i + \beta_4 . \Delta MDMS . \Delta Z_i + \gamma + \Delta \varepsilon_i \quad \text{- (4)}$$

Here we have rid ourselves of the unobserved fixed effects and accounted for trend effects of several time-invariant characteristics. Because the panel has only two rounds between which the program was introduced MDMS equals ΔMDMS. This specification thus is essentially a first-differenced equation incorporating time trends for some determinants to allow for time-varying heterogeneity. Time varying heterogeneity will exist because the effect of different determinants (*even time-invariant*) on health varies by the age of the child i.e. time-invariant determinants may have trend effects here. For example, boys and girls may have different trends in height gain across different ages.

Finally, in order to convert this equation above into a dynamic specification, we simply add the lagged health status; this gives us the following specification –

$$\Delta Y = \beta_1 \Delta MDMS_i + \beta_2 . X_i + \beta_3 . \Delta Z_i + \beta_4 . \Delta MDMS . \Delta Z_i + \beta_5 Y_{t-1} + \gamma + \Delta \varepsilon_i \quad \text{-(5)}$$

Equations (4) and (5) are the key specifications for estimation of nutritional benefits in this paper.

In the succeeding sections, we discuss briefly the data, definitions of the treatment and control groups, problems of endogeneity in the analysis and our attempts to solve them, and finally, present the results of the analysis.

4. Empirical Section

4.1 The Data

The data we use in this study were collected by the Young Lives Project in 2002 and 2007 in the State of Andhra Pradesh. The surveys cover two cohorts: the first of 2011 children born between January 2001 and June 2002, and the second of 1008 children born between January 1994 and June 1995. In the second round (2006/7), 1950 children of the younger cohort and 994 children of the older cohort could be traced and resurveyed; attrition rates are low and therefore do not pose a problem for analysis.

The dataset has several strengths for our purposes. Firstly, it covers just the right period: the first round was in mid-2002 just before the program was implemented in A.P. in January 2003, and the second round was in 2007, long enough for the teething problems to have been sorted and for outcomes to have been realized. Secondly, since the survey is longitudinal, the data are much more suitable for modelling dynamics than repeated cross-sections. Also, one can use data from the first round to get a richer and more nuanced pattern in which beneficiaries gain from the scheme. Thirdly, no other baseline surveys for the Indian scheme exist, to our knowledge, from which we can obtain a better estimate; this in itself makes the data very important.

4.2 Defining treatment and control groups

Regrettably, reliable data on program availability is not available for the older cohort. To define our treatment and control group in this case, we make two assumptions: first, that all children in public schools get the meal (which is a reasonable assumption given previous studies) and second, that no children in private schools get a school meal. The latter assumption will be violated in the case of the private aided schools which are covered under the scheme but as they form a little less than 4% of total number of schools in the state, it is not likely to be a major violation[4]. Thus we define the treatment group as those children who were in public schools in both rounds of the survey i.e. in 2002 and 2007, and the control group as those who were *not* in public schools in *either* round. We ignore children who migrated from private to public schools or vice versa because information is unavailable on when they migrated and cannot therefore assess how long they received the treatment[5]. Also, migration of children from private to public schools may indicate self-selection into the program, thus making it prudent to exclude these children from the analysis. It is important to note that even if *either* of these assumptions is violated, they would tend to bias our results downwards and that therefore our results are not driven by these assumptions.

For the younger cohort, data on program availability is available. Of the children in the younger cohort, who range from 5- 6 ½ years old in 2007, about 45% were in school by the second wave. Of these students, about 79% were in public schools and the rest in private schools (including those run by NGOs and religious charities). Only 1.47% of caregivers of the children enrolled in public schools currently (10 out of 682) reported that their school does not provide a midday meal, thus confirming the widespread implementation of the program indicated by

[4] Private aided schools are run under private management but receive government funding and support, have access to government schemes like the Midday Meal Scheme, and follow the same regulations as government schools. In practice, their quality and functioning is indistinguishable from public schools (Kingdon, 2001).

[5] A total of 80 students migrated from public to private schools, and 67 from private to public, in the cohort.

previous studies[6]. For the sake of consistency, we define our treatment and control groups analogously to the older cohort – the treatment group includes all children who have begun formal schooling in a public school and the control group includes everyone else (those in private schools and those not yet enrolled).

To check the robustness of our results we also used alternative definitions of the treatment and control groups. For the older cohort, we also used a specification where we defined the treatment group as all children enrolled in public schools in the first round; we do this to check that excluding children who migrated did not create any implicit selection issues. For the younger cohort, instead of using self-reported availability of the program to define the treatment group, analogously to the older cohort, we defined the treatment group as all children in public school and the control group as all children not in public school. In neither cohort do the results change if we use the alternative specifications.

Tables 1 and 2 present the summary statistics of different characteristics of the two cohorts by treatment and control group. As even a cursory glance at the tables for the older cohort will indicate, children in public schools differ markedly from those in private schools; they are significantly more likely to be poorer, from traditionally lower castes, living in rural areas and to initially have had poorer health.

A similar pattern is noticeable also in the younger cohort; the differences are however less stark because the control group does not consist solely or chiefly of children in private schools but also those children who will join a public school but have not started schooling yet. This marked heterogeneity between the treatment and control groups, which may extend also to unobserved differences, is the central problem we face in this evaluation; in the following sections these problems and possible solutions shall be discussed in detail.

[6] Caregivers of another 24 students (3.52%) report not receiving the midday meal because the child does not like the food.

Table1. Descriptive Statistics: Older Cohort

	Total Sample	Control Group	Treatment Group	t statistic of difference
Gender, wealth and household location				
Male	48.61	56.32	46.18	2.34**
Wealth Index (mean)	0.346	0.571	0.270	20.75***
Urban	25.82	71.26	11.45	19.32***
Telangana Region	35	38	27	3.75***
Rayalseema Region	29	27	35	-1.39
Coastal A.P.	35	33	38	-2.43**
Caste				
Scheduled Caste	19.61	06.32	23.82	-5.15***
Scheduled Tribe	10.08	06.32	11.27	-1.89*
Other Backward Class	50.0	44.83	51.64	-1.57
Other Castes	20.02	41.95	13.09	8.70***
Nutrition				
Underweight(2002)	48	34	53	4.57***
Stunted(2002)	33	17	39	-5.60***
Deficit in BMI-A(2002)	26	23	29	-1.33
Δ Height-for-age (mean)	-0.12913	-0.13457	-0.11801	-0.39
Δ BMI-for-age (mean)	-0.08159	0.067135	-0.12799	2.17**
Observations	734	184	550	

N.B. Statistics are expressed as percentages unless otherwise stated

Table 2. Descriptive Statistics: Younger Cohort

	Total Sample	Control Group	Treatment Group	t-statistic of difference
Gender, wealth and household location				
Male	53.2	54.1	51.6	1.049
Wealth Index (mean)	.306	.346	.231	14.99***
Urban	24.4	34.5	5.6	13.05***
Telangana Region	34.87	38.80	27.57	4.99***
Rayalseema Region	29.69	27.6	33.58	-2.75***
Coastal A.P.	34.87	33.44	38.70	2.32**
Caste				
Scheduled Caste	18.1	15.0	24.0	-4.98***
Scheduled Tribe	12.8	9.5	19.0	-5.95***
Other Backward Class	47.8	49.2	45.3	1.645*
Other Castes	21.0	26.0	11.7	7.49***
Nutrition and age				
Underweight(2002)	32.9	32	35.1	-1.47
Stunted(2002)	31	29.7	34	-1.93
Δ weight-for-age (mean)	-.316	-.347	-.258	-2.04**
Δ height-for-age (mean)	-.306	-.341	-.24	-1.592
Age (mean, in years)	5.40	5.34	5.50	-10.6***
Observations	1950	1268	682	

4.3 Problems of endogeneity

Self Selection

A major concern related to non-random program placement is that of *self-selection* into the program. In particular, we know that the Midday Meal Scheme exerts powerful incentives on public school enrolment; it is entirely possible that many children in these schools may have enrolled earlier or moved from private schools attracted by the Midday Meal Scheme. In that case, our program placement is endogenous and our estimation biased. If the self-selection into treatment is based on observed characteristics that we control for, such as wealth or caste, the treatment effect can be correctly estimated but if it is on the basis of unobserved characteristics, then we need other ways to correct the potential endogeneity.

We attempt to tackle this issue differently in the two cohorts. Most children in the older cohort were already in school in the first round. As noted in the previous section, the treatment group for this cohort is defined as those children who were in a public school in both periods. Thus the choice of school for the children in question had already been decided in the first round *before the scheme was introduced*. Thus we probably do not face a problem of self-selection for these children. As described above, the estimation is not driven by the exclusion of students migrating between public and private schools.

For the younger cohort, self-selection is much more critical. We know that the availability of Midday meals has a strong influence on whether parents send children to public school and at what age(see for example Khera 2005;Afridi 2007); ironically, the success of Midday meals in boosting enrolment also ensures that we face a self-selection issue in an estimation of its other benefits.

Being in the treatment group is essentially the same as *already* going to a public school. Thus self-selection can take place through the following mechanisms: attracted by the introduction of the Midday Meals parents can i) decide to send their children to a public school rather than no school at all or ii) to a public school *instead of* a private school, or iii) they can decide to enrol their child in a public school at a younger age than they otherwise might have in order to benefit from the program. The relative importance of these channels of self-selection varies widely across states. In states where enrolment rate is low, such as Rajasthan, the first channel is likely to be very important. However, in the case of Andhra Pradesh, it is unlikely to be important as most children in A.P. go to school; even in the first round, over 97% of the children in the older cohort, then aged 8 years, were in school. The latter two channels are important sources of endogeneity we need to address. Even among these, we suspect the second channel is not too important as the program is likely to be an incentive only for poorer households, and children from these households, especially in rural areas, would typically enrol in a public school anyway. The third channel is however still possibly very influential.

To deal with the endogeneity created by the selection issue, we adopt an instrumental variable (IV) approach.

Whether a child is enrolled in school (public or private) or not is determined strongly by his/her age; older children are much more likely to be enrolled than younger children. The children in the younger cohort were born between January 2001 and June 2002; there is thus an 18 month range in their age, which influences the likelihood of them being in school and receiving a Midday meal. We use the age of the child in years as an IV for the treatment variable. As expected, there is a strong first-stage relationship between the IV and the MDMS variable; children born in later quarters are much less likely to be in school.

Correlation with the endogenous regressor (here MDMS) is however only one of the requisite conditions for a valid IV; the other is that the exclusion restriction must be maintained i.e. the instrument must not affect the dependent variable at all apart from its effect through the endogenous regressor. The latter condition is likely to be violated by the instrument that we have proposed, namely, the age of the child. The age of a child in our sample is determined by the quarter of his/her birth. This, in turn, is strongly correlated to the season of birth and thereby the environmental conditions during pregnancy and early childhood. In particular, especially in a tropical country like India, the disease environment varies much by season. Also, the quarter of birth is also strongly correlated to food availability for the mother which often depends on the agricultural season. Thus the quarter of birth is a major determinant of child health in the first year and these effects are likely to persist into future health status as well. In Sub-Saharan Africa Artadi (2005) finds – *"..variation in weather and nutrition causes children born in certain months to be up to three percentage points more likely to die."*; similar effects are likely to be present in India as well.

This correlation might make age in years an invalid IV but does not in this case. The argument is as follows: the quarter of birth (and thereby age) should directly only affect initial period health, any further effect on health should only arise from the *persistence* of the initial effect. Apart from its effect on initial health which may persist into later childhood, it is unlikely that quarter of birth would exert an independent influence on child nutrition at age 5 or 6. If indeed the season of birth only affects future health through the health in the first period, as we have here explicitly assumed, in any *static* model of health determination (as in equation 3) the age will be a valid IV by itself.

More convincingly we can use age as an IV in a dynamic specification because we also control for the health status in the first round through a lagged anthropometric z score. Any effects the quarter of birth may directly exert should already be present in the first period health status and its further effect on health should entirely be captured in the effect exerted by the lagged health measure. In that case, the IV will be valid.

In the above specification, we instrument MDMS with age in years. We also instrument the interaction of drought with the treatment using interactions of the age with the Drought as an IV. Since the incidence of drought is exogenous, as long as age in years satisfies the exclusion restriction, interaction of drought with the age in years will satisfy the exclusion restrictions as well. This exogeneity, coupled with strong first-stage relationships, makes these interactions valid instruments[7].

Endogeneity of lagged health status

Incorporating the dynamic aspects of health determination is both desirable and essential but exposes us to a further problem: potential endogeneity of the lagged dependent variable. Moreover, unlike self-selection into treatment, this source of endogeneity affects both cohorts, not just the younger one. Since we have only two rounds in our panel we do not have recourse to familiar GMM methods that are standard in trying to deal with this problem. The best option before us is to find other valid IVs to instrument the lagged variable with; fortunately for us, the data are rich enough for us to find these. In the younger cohort we instrumented the lagged dependent variable (anthropometric score from Round 1) using the caregiver's perception of birth size. Birth size is related to conditions during pregnancy and is

[7] Due to constraints of space, we have not reproduced results from first-stage regressions in the paper. we have reported the Cragg-Donald F statistic and (where applicable) the p-value of the Sargan over-identification test with the main IV results.

very strongly correlated with a child's health in the first 18 months of his/her life. Moreover, it can reasonably be taken to be exogenous[8].

In the older cohort, we instrument the lagged variable with negative shocks that occurred shortly *before* the first round of the survey. The survey had collected data on a number of shocks affecting the household such as natural disaster, job loss in the household, death or illness in the household, crop failure or theft, livestock theft or death etc. These shocks should exhibit adequately strong first-stage relationships with the initial z score. It is also plausible to argue that a shock that occurred five years ago will only affect future health at age 12 or 13 only through the initial health status; this satisfies the exclusion restriction.

4.3 Econometric specification

In this section we present the econometric specification(s) adopted in the analysis; these are closely based on the specifications presented above in Section 3. Similar specifications were used for both cohorts but there are differences in instrumenting strategy (due to self-selection in the younger, but not the older, cohort).

As a descriptive measure, we estimated the unconditional average treatment effect on the treated (ATT)by a simple OLS regression of the change in the z score on treatment. We ran the regression on the full sample, and also separately for children who had suffered from drought, and children who had not. Drought is the major economic shock in this region; 35.83 % of households in rural areas in the younger cohort, and 36.11% in the older cohort, self-reported having been affected by drought between the two rounds.

Specifically we estimated equations of the form –

$$\Delta Y = \alpha + \beta_1 MDMS_i + u \qquad \text{- (3)}$$

Here *Y* is the health measure and *MDMS* the treatment binary. This merely shows the difference between the average changes in Y between the two groups. It is only intended as a first look at the data and ignores the econometric problems discussed in the previous section.

To allow for the interaction effects specifically, and to disaggregate the benefits by various child characteristics, we move back to a regression framework. Specifically, we estimate equations of the following form –

[8] Birth weight might have been a better IV but was impracticable in this case. Birth weight was only available for about half the sample as many of the children were born at home and without medical attention.

$$\Delta Y = \alpha + \beta_1 MDMS + \beta_2 X + \beta_3 Drought + \beta_4 MDMS.drought + u \qquad -(4)$$

Here *Y* is the health measure i.e. the anthropometric z score in the second round, *MDMS* is a treatment dummy variable, *X* is a vector of child and household attributes (sex, caste, urban residence, household wealth, caregiver's education, household size, region), and *Drought* is a dummy variable for whether the household reported being affected by drought in the four years between the two rounds. The treatment variable was interacted with all characteristics in the vector X, and with drought. This equation does not incorporate dynamics yet and is still a static model even though we are looking at changes in the z score.

In both cohorts the above equation is estimated using OLS. In the younger cohort, we instrument MDMS and its interaction with drought with age (in years) and its interaction with drought respectively.

From the above static specification, we moved to a dynamic specification. The equations we estimated incorporating dynamics were of the form:

$$\Delta Y = \alpha + \beta_1 MDMS + \beta_2 X + \beta_3 Drought + \beta_4 MDMS.drought + \beta_5 Y_{t-1} + u \qquad - (5)$$

This is similar to the previous specification but for the incorporation of the lagged value of the health measure (Y_{t-1})[9]. we first run this as an OLS regression. Again, we need to correct for self-selection for which we instrument in exactly the same manner as above. As noted earlier, the exclusion restrictions for the IV are even more convincing in this case. Further, to deal with endogeneity that may be caused by a lagged dependent variable, we instrument the lagged variable and its interaction with the treatment with shocks from before the first period.

4.2.2 Learning Effects

The surveys incorporated in both rounds a reading and writing test(although obviously the younger cohort were not administered those in 2002). In addition, the children in the older cohort were administered the Raven's test in 2002 and both cohorts were administered the Peabody Picture Vocabulary Test(PPVT) in the second round.

The PPVT is an individually administered achievement test of receptive vocabulary that measures the listening comprehension of spoken words for individuals aged 2 ½ through 90+ years. The test is designed to serve two purposes: (1) an achievement test of receptive

[9] Incorporating the lagged dependent variable effectively makes the above a growth specification.

(hearing) vocabulary attainment; and (2) a screening test for verbal ability(AGS,1997). The test, originally designed for testing individuals in English, was adapted into local languages.

To assess the impact of midday meals on learning and cognitive skills we will focus exclusively on the older cohort; this is because, of the children in the younger cohort, about 55% are not in schools yet and we would not know whether any effects we may pick up come from the fact of being in school(and thereby getting the meal) or because of the meal itself. Comparisons between public and private school children will also not be of any avail as we would merely be catching differences in the quality of schooling between the two sectors. For the older cohort, however, we can condition for Round 1 proficiency in reading and writing, as well as the Raven's test scores from last period, in addition to various parental and household characteristics, and thereby be reasonably assured we are comparing like with like.

Since we do not have the PPVT scores for 2002, we cannot run our differenced regression specification for PPVT scores. Thus we again resort to propensity score matching to estimate the ATT on PPVT scores.

4.3 Results

4.3.1 Results on Nutrition

In this section we present the results of the analysis of the gains in nutrition arising from the Midday Meal Scheme. we present the results separately for both cohorts.

Younger Cohort

Table 3 presents the estimates of the unconditional ATT estimated by regressing changes in the z score on the treatment variable using OLS which were calculated for the whole sample, and separately for children who suffered (did not suffer) from drought in the past four years.

Table 3: ATT from OLS regressions on the treatment binary : Younger Cohort

	Total Sample	Drought	Without Drought
ΔWeight-for-age	**.089**** (2.05)	**.169**** (2.22)	.069 (1.3)
ΔHeight-for-age	.10 (1.59)	**.264**** (2.05)	.089 (1.25)

t statistics in parentheses

These initial results indicate that the treatment did indeed have a significant impact on the weight-for-age in this cohort. The ATT is .089 standard deviations for weight-for-age and .10 for height-for age (although the latter figure is not significant). More interestingly, the ATT differs greatly in magnitude between children afflicted by drought and other children; specifically, the positive impact on the drought-afflicted is much greater than on other children. Whereas those not affected by drought gain an additional .069 standard deviations in weight-for-age, those who were affected by drought gain .169 standard deviations. The point estimates of the height-for-age regressions also exhibit the same pattern. For both variables, the ATT is statistically significant only for the children who suffered from drought and not others.

A more detailed (and reliable) picture emerges from the expanded regressions which account for differences in various child and household characteristics. Table 4 presents the results from the OLS and IV estimates of the gains in weight-for-age and height-for-age in the younger cohort, obtained from both static and dynamic specifications.

As can be seen, having suffered from drought in the past four years has a significant negative impact on both height-for-age and weight-for-age across all specifications. The negative impact of drought is compensated for by school-feeding in all specifications as well (although the effect is not significant in the OLS regressions). Correcting for self-selection, the estimates of both the negative impact of the drought and the effect of school-feeding on drought-affected children rise drastically. Rather surprisingly, the positive effect of the midday meals is much larger for both health measures, across all specifications, than the negative impact of the drought; this indicates that school meals more than compensate for the negative impact of the drought. More worryingly, this implies that if you are a program beneficiary, improvements in health are greater if you are in a drought-stricken area than otherwise.

A possible explanation for this lies in a specific policy addition to the basic structure of the Midday Meals Scheme by the A.P. State government. The A.P. government announced that school meals were to be provided to all school children in drought stricken areas even in the summer vacations. We do not know from the data how well this policy was carried out but if it was indeed implemented fully then children in drought stricken areas would have received the school meals for an additional month-and-a-half compared to children in other areas in the years they were affected by drought. This is a plausible reason for the benefits of the midday meals in drought-stricken areas outweighing the negative impact of the drought itself. Here, it is worth recalling that though they are consistent, IV estimates are still biased and perhaps that may explain the apparent paradox.

Table 4. Regressions on nutrition: Younger cohort

LABELS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Changes in weight-for-age				Changes in height-for-age			
	Static Regressions		Dynamic regressions		Static Regressions		Dynamic regressions	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
MDMS	0.0169	-0.361	0.0572	-0.665***	0.0858	1.472***	0.159***	0.622**
	(0.30)	(-1.30)	(1.26)	(-2.85)	(1.07)	(3.23)	(3.24)	(2.19)
Drought	**-0.144****	**-0.713*****	**-0.145*****	**-0.587*****	**-0.299*****	**-1.051*****	**-0.222*****	**-0.634*****
	(-2.14)	(-3.95)	(-2.72)	(-3.81)	(-3.16)	(-3.53)	(-3.83)	(-3.36)
MDMS x drought	0.139	**1.452*****	0.114	**1.151*****	0.179	**1.829*****	0.112	**0.967****
	(1.42)	(3.62)	(1.47)	(3.36)	(1.31)	(2.75)	(1.33)	(2.34)
Male	0.129***	0.134***	0.0330	0.0596	0.0446	0.0705	-0.077**	-0.0369
	(3.10)	(3.09)	(1.00)	(1.53)	(0.76)	(0.97)	(-2.15)	(-0.81)
Urban	-0.0280	-0.128	-0.0154	-0.202**	0.0697	0.386**	0.0976*	0.188*
	(-0.41)	(-1.30)	(-0.28)	(-2.46)	(0.72)	(2.39)	(1.65)	(1.96)
Scheduled Castes	0.139*	0.149*	-0.00915	0.121*	0.0862	-0.179	-0.102*	-0.153**
	(1.95)	(1.84)	(-0.16)	(1.77)	(0.86)	(-1.33)	(-1.67)	(-1.98)
Scheduled Tribes	0.199**	0.169*	0.000957	0.115	0.135	-0.118	-0.0817	-0.110
	(2.36)	(1.86)	(0.014)	(1.49)	(1.14)	(-0.78)	(-1.12)	(-1.26)
Other Backward Classes	0.0312	0.0275	-0.0817*	-0.0260	-0.0347	-0.114	-0.138***	-0.142**
	(0.55)	(0.46)	(-1.79)	(-0.50)	(-0.43)	(-1.13)	(-2.80)	(-2.45)
Wealth Index Round 1	0.0206	-0.0722	0.461***	0.183	-0.0475	0.207	0.657***	0.597***
	(0.13)	(-0.41)	(3.62)	(1.17)	(-0.21)	(0.71)	(4.77)	(3.34)
Weight-for-age z-score			-0.506***	-0.347***				
			(-33.3)	(-5.20)				
Length/height-for-age z-score							-0.679***	-0.513***
							(-56.2)	(-5.98)
Constant	-0.442***	-0.252	-1.206***	-0.617***	-0.156	-0.834***	-1.288***	-1.263***
	(-4.36)	(-1.47)	(-14.4)	(-3.77)	(-1.10)	(-2.97)	(-14.4)	(-7.08)
Observations	1920	1920	1920	1894	1905	1905	1905	1880
R-squared	0.020	-0.075	0.380	0.257	0.081	-0.414	0.656	0.537

t statistics in parentheses;*** p<0.01, ** p<0.05, * p<0.1

N.B. (1)Base category: rural, female, Other Castes, Coastal A.P., not drought-affected

(2)Coefficients on male, urban, region dummies, caregiver's education and household size are not reported here due to space constraints.

(3) All IV equations are exactly identified.

The negative coefficients on the lagged health measures indicate that there is some mean reversion in the sample which may be interpreted as partial 'catch-up' between rounds. Reassuringly, our results on drought are not significantly altered by including the lagged terms.

Older Cohort

Table 5 presents, as in the younger cohort, the estimates of the unconditional ATT estimated by regressing changes in the z score on the treatment variable using OLS calculated for the whole sample, and separately for children who suffered, or did not suffer, from drought in the past four years.

Table 5: ATT from OLS regressions on the treatment binary : Older Cohort

	Total Sample	Drought	Without drought
ΔHeight-for-age	.02 (0.58)	.207 (1.11)	-0.013 (-0.23)
ΔBMI-for-age	-.1951 (-2.85)	-.206 (1.12)	-0.94 (1.47)

Unlike the younger cohort, there is no significant pattern in the ATT in the height-for-age. The changes in both measures, the height-for-age and the BMI-for-age, are more positive for those children who suffered from drought than those who did not.

The results from the expanded regression analysis are presented in Table 6. Most noticeable is the fact that Midday meals seem to exert no significant effect on nutrition. This is true for the base category as also those who have suffered from drought. In this respect, results from the older cohort differ drastically from those in the younger cohort. Another noticeable factor is that our instrumenting strategy has not been successful in this cohort; the very low values (<1) of the Cragg-Donald F statistic for the IV results indicates a severe weak-instrument problem, making interpretation of the IV results hazardous. Nonetheless, because no pattern emerges even from the static regressions, we may reasonably conclude that the absence of any significant effect of the midday meals is not a problem arising out of inadequate instrumentation, but rather reflects patterns present in the data. Finally, it is also worth noticing that even at this age there is some evidence of partial catch up although the catch-up is much less than in the younger cohort.

Table 6. Regressions on nutrition: Older cohort

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes in height-for-age			Changes in BMI-for-age		
	Static	Dynamic		Static	Dynamic	
	OLS	OLS	IV	OLS	OLS	IV
MDMS	0.134	0.0200	0.160	-0.0266	-0.0486	0.00856
	(1.37)	(0.21)	(1.02)	(-0.20)	(-0.39)	(0.052)
Incidence of drought	-0.0952	-0.120	-0.0897	-0.217	-0.197	-0.248
	(-0.49)	(-0.64)	(-0.45)	(-0.83)	(-0.79)	(-0.78)
MDMS x Drought	0.156	0.128	0.162	-0.106	-0.0729	-0.159
	(0.75)	(0.64)	(0.76)	(-0.38)	(-0.27)	(-0.46)
Scheduled Caste	-0.0929	-0.104	-0.0903	0.388***	0.420***	0.338**
	(-0.97)	(-1.14)	(-0.92)	(3.02)	(3.41)	(1.99)
Scheduled Tribe	-0.249**	-0.285**	-0.241*	0.141	0.221	0.0118
	(-2.08)	(-2.50)	(-1.89)	(0.88)	(1.44)	(0.045)
Other Backward Classes	-0.137*	-0.141*	-0.137*	0.178*	0.157	0.213
	(-1.76)	(-1.90)	(-1.73)	(1.71)	(1.57)	(1.58)
wealth index	0.114	0.142	0.108	0.274	0.358	0.140
	(0.56)	(0.73)	(0.52)	(1.01)	(1.37)	(0.37)
Initial height-for-age z-score		-0.237***	0.0536			
		(-8.66)	(0.21)			
Initial BMI-for-age z-score					-0.294***	0.468
					(-7.92)	(0.71)
Constant	-0.0149	-0.251	0.0384	-0.0397	-0.404*	0.540
	(-0.086)	(-1.50)	(0.13)	(-0.17)	(-1.77)	(0.63)
Observations	719	719	719	719	719	719
R-squared	0.03	0.12	-0.02	0.07	0.14	-0.37
Cragg-Donald F statistic	-	-	0.947	-	-	.347
Sargan test p-value	-	-	.198	-	-	.598

4.3.2 Results on Learning

We estimated the impact on learning by employing propensity score matching methods using PPVT scores of the older cohort in 2007 as the outcome variable, conditioning on a range of child and household characteristics including the scores on the Raven's test in 2002 and whether the child could then read or write fluently.

As can be seen from Table 7, there was a significant impact of the midday meals on the test scores; the ATT ranges between 25.785 to 27.258 points as estimated by the nearest neighbourhood and kernel matching methods respectively. This effect is large in absolute terms ranging between 0.63 and 0.676 standard deviations; the mean and standard deviation of the scores are 135.44 and 40.3 respectively. This increase in the verbal ability and cognitive skills could have come from any of the following channels: through increased attendance, or through better nutrition which leads to a greater long term productivity, or simply a rise in short-term concentration due to the elimination of 'classroom hunger'. Our data do not allow us to decompose this impact further to see the relative contribution of these channels; however, given that we do not see any evidence of medium or long term health improvements arising from the scheme in older children, it is likely that the effects operate through an attendance channel rather than a health channel.

Table 7. Results on learning from Propensity Score Matching

	Nearest Neighbourhood	**Kernel matching**
ATT	25.785	27.258
t stat	1.960	2.278
No. treated	525	525
No. Control	50	83

There is however one major caveat that we must specify before fully accepting this result. By definition, our treatment group consists of children who were in public schools in both rounds. If, in the intervening period between the two rounds, any reforms affecting the quality of teaching or learning had taken place in public schools(apart from introduction of school meals) then we may well be capturing the effects of that reform and not school meals and our results would be upwardly biased(assuming the reforms were good). Unfortunately for our analysis, this possibility cannot be ruled out. Various government schemes operate simultaneously in public schools to try and raise the quality of education and it is possible they drive the results, not Midday Meals[10].

We are not able to offer any definitive conclusions on the effect of midday meals on learning outcomes. As pointed above, this is mainly a product of the inadequacy of our data for this purpose. It would be worthwhile to collect more suitable data for such analysis. In the meanwhile we believe the positive effects on PPVT scores shown by our ATT estimation are highly suggestive of such learning gains; how strongly this translates into performance in standardized reading, writing or math tests is not clear and bears further investigation.

4.3.3 Discussion

Having presented a range of results using a variety of specifications, it is time that we tried to consolidate the results and examine any conclusions we may have arrived at.

The first, and most obvious, conclusion is that contamination of the treatment and control groups arising from non-randomized placement, especially in the younger cohort, makes identification and isolation of the program impact very difficult. This is responsible for the underestimation of the benefits in the younger cohort by OLS. To the extent possible, however, we have tried to deal with this problem. This is less so a problem in the older cohort since all the children kept in our sample had already chosen schools before the introduction of the program; excluding children who migrated between public and private schools might have created a sample-selection issue but since alternative specifications incorporating the migrants do not change any results, we believe that is not the case here.

Two results from our analysis, we believe, can be accepted as genuine proof of the program's benefits: results on drought in the younger cohort, and on learning in the older cohort.

The results on drought, indicating that drought had a negative impact on health but that this was counteracted by the Midday Meals are, as we have seen, robust to a variety of specifications and estimation methods. They also seem to make intuitive sense; children in drought-stricken areas see a decline in nutritional intake impacting their health negatively, but the Midday Meals Scheme in these situations acts as a security net compensating for this decline in food availability.

[10] In particular, the Sarva Shikhsa Abhiyan has in recent years made significant investments in public primary schools in India.

It may appear surprising at first glance, that drought and school feeding has a significant effect not only on weight-for-age but also on height for-for-age. Height-for-age is a measure of longer term or chronic deprivation; moreover it is often believed that height-for-age is hard to influence after 2 years of age. However, the literature on nutrition contains several studies that refute this belief: Adair(1999), for instance, finds from a longitudinal survey of over 2000 children in the Philippines that there is *"a large potential for catch-up growth in children into the preadolescent years"* . Other scholars have also concluded that, whereas growth deficits persist into early adulthood if children remain in the same poor conditions, there is definitely potential for catch-up if circumstances improve for the better such as through nutritional supplementation or migration(see for example Tanner, 1981; Martorell, 1994; Coly et al2006; Golden,1994). The case for catch-up growth arising from nutritional supplementation is, in fact, made very strongly in the nutrition literature and has been buttressed by several empirical and clinical studies; our own study further advances this case.

The results of learning stand, we believe, because we are not faced with selection problems as vexing as in the younger cohort and we have been able to control for most other relevant factors as far as possible. That does not eliminate the possibility of bias in the results as that can still arise from any unobserved but relevant determinants. In particular, the possibility of other changes in schools could have biased results. Nonetheless, based on the perceived state of public schooling in India it is reasonable, we believe, to attribute most of the learning improvement to the Midday Meals. This is, of course, a point that clearly deserves much more detailed investigation using data more suitable for the purpose.

Finally, it may appear a little counter-intuitive that children in the younger cohort show some health benefits from the scheme after merely a year of treatment whereas the children in the older cohort do not even after four years of receiving the treatment. Moreover, even drought does not seem to exert a significant impact on them. This can be explained by one or more of the following factors; since the meal is of the same nutritional intake for all students regardless of age, it represents a smaller increment to the daily diet of older children; health in later childhood is less vulnerable to shocks and relatedly, as has been noted in the health literature, the responsiveness of child health to supplementary feeding declines with every passing year after infancy.

Midday meals, therefore, are important for older children mainly on grounds of school participation and learning but for younger children are also valuable as a safety net. This contrast in results between the younger and older cohorts underlines a fundamental principle often neglected in policy discussions: the impact of any external factor, including programs like school meals, on children will very much vary depending on the age of the child and ignoring this may lead to an incomplete or even erroneous understanding of a program's benefits.

5. Conclusions

Whereas there have been several intractable problems in estimation, we believe we have been able to reasonably deal with most of them. Despite the best efforts, not all possible problems have been ruled out. That said, we do have some findings of interest.

The effect of school meals as a security net can be of much importance if upheld. Most of India's population depends on agriculture for their livelihood; agricultural shocks, of which droughts are the most prominent example in many parts of India including Andhra Pradesh , lead to a decline in household food availability and a worsening of child nutrition and health. The pernicious impact of this childhood nutritional deprivation on an individual's health and nutritional status may persist into adulthood, and is likely to affect their ability to function fully in daily life. If school meals can cushion children from these shocks and reduce the variability in intra-seasonal food intake of children, it may be of great importance for their future biological development. This effect of school meals has not, to our knowledge, been studied or highlighted at all in the literature but may be worth evaluating separately in future studies.

The results on learning are also very encouraging. It is certainly not enough to get children to school, it is necessary that they learn in the time they are there. The results of our estimation are hopeful even though they are only suggestive. We must remember that even if our caveat *is* violated, and indeed the improvements in cognitive learning reported above are a product of other reforms in the public education sector, midday meals still must be given credit for getting children to school more often to benefit from them.

Finally, we believe that these results, combined with other evidence on the positive impact of school meals on school participation and daily nutrient intake, provide a strong justification for the program in India. With regard to the Indian context, this is one of the few attempts at a rigorous evaluation of a scheme that covers 120 million children nationally and as such its findings should be of obvious interest to administrators and educational policy makers. It does also underscore the need for better and more extensive evaluations that can inform us of the precise worth of this scheme and others like it. As things stand school meals may well be one of the most potent school-based interventions available to policy makers in the developing world.

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