

# School Meals as a Safety Net

## An Evaluation of the Midday Meal Scheme in India<sup>1</sup>

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### **Abstract**

Despite the popularity of school meals as interventions in education, little evidence exists on their effect on health outcomes. This study uses newly available longitudinal data from the state of Andhra Pradesh in India to estimate the impact of the introduction of a national midday meal program on anthropometric z scores of primary school students, and investigates whether the program had any impact on ameliorating the deterioration of health in young children due to the impact of a severe drought. Correcting for self-selection into the program using a non-linearity in the probability of enrolment, we find that the program acts as a safety net for children, cushioning them from negative nutritional factors; in particular, there are large and significant health gains for children whose families suffered from drought.

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## **Introduction**

In November 2001, in a landmark reform, the Supreme Court of India directed the Government of India to provide cooked Midday Meals in all government and government-aided primary schools “within six months”<sup>2</sup>. By 2003, most states started providing cooked meals in primary schools. Covering an estimated 120 million school children by 2006 (Khera 2006), the program now is the largest school-feeding program in the world<sup>3</sup>.

The program was premised on expectations of significant gains in schooling and nutritional outcomes. It was expected that school meals would provide a powerful incentive to increase school participation. Additionally, it was thought that the program would help in addressing problems of undernourishment among school children. And finally, it was expected that indirectly school-feeding would 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<sup>4</sup>.

The evidence, however, on the impact of the program on nutrition is rather thin. While there is evidence that school feeding in India does indeed improve the immediate nutritional intake of children (Afridi 2010), the effect of the program on nutritional outcomes has not been evaluated systematically.

The broader literature on school feeding in other countries also has failed to definitively answer this question. While Jacoby (2002) has shown convincingly that a similar program in the Philippines significantly increased the nutritional intake of children, there are few studies

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<sup>2</sup> The full text of the court orders in this regard are available at [www.righttofoodindia.org](http://www.righttofoodindia.org)

<sup>3</sup> The number of children covered under the Scheme is now certain to be larger as the Scheme has been extended to senior grades as well.

<sup>4</sup> Additionally the program was hypothesized to deliver other social benefits such as the break-down of caste barriers by children of different castes eating together.

documenting the effect of school feeding programs on outcome indicators of child nutrition, and those that are available find ambiguous effects (e.g. Kremer and Vermeersch, 2004<sup>5</sup>).

Furthermore, there has been no attempt, whether in the context of the Scheme in India or in the broader literature evaluating the outcomes of school feeding programs, to evaluate their role in coping with environmental shocks. As we show in our data, however, this role can be potentially very important in determining the distribution of impacts among program beneficiaries, especially since such shocks have been shown to have a large and enduring impact on future outcomes in developing countries (e.g. Maccini and Yang, 2009; see also the discussion in Strauss and Thomas, 2007). This omission in the literature is also surprising given that the role of school meals as a safety net has indeed been recognized by policy makers: in the Indian case, the Supreme Court ordered in 2004 that all children in drought-affected areas must be served the Midday Meals even during school vacations – an exercise clearly based on the recognition of this role of school feeding. An evaluation of this role is central to facilitate our understanding therefore of how large-scale school feeding schemes may affect a population in such contexts.

This paper addresses these gaps in the existing literature. Using a recent longitudinal dataset from the state of Andhra Pradesh (India), we assess the impact of the Midday Meals Scheme on the health status of children in primary schools. Further, we disaggregate these average impacts on the beneficiaries to understand the distributional pattern of benefits.

We analyze data from a longitudinal study of children in poverty collected by the Young Lives Project in Andhra Pradesh (A.P.). The survey collected extensive information about children in two cohorts (born in 1994/95 and 2001/02 respectively) in 2002 and 2006/07. The

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<sup>5</sup> The Kremer and Vermeersch study focused on children of preschool age in Kenya. However, the age of the children in their study (4-6 years) is almost identical with the age of the children in the Young Lives younger cohort, making their study useful for comparison purposes.

school feeding program, known as the Midday Meal Scheme in India (henceforth MDMS), was introduced in Andhra Pradesh in January 2003. The period between the two rounds of the survey coincided with severe and recurring drought in our study areas and marked a period of acute agrarian distress in several sites.

In this study we focus exclusively on the younger cohort of children. We use anthropometric z scores on two measures - weight-for-age and height-for-age - as the outcome variables to study the impact of the program on health and nutritional status. To correct for self selection into the program, we utilize a non-linearity in enrolment induced by a change in the calendar year of birth: this affects the probability of enrolment but should not directly impact nutrition when controlling for age in the regressions. We use an indicator variable for whether the child was born in 2002 as an instrumental variable (IV) for our treatment dummy variable. Our IV is informative in the dataset, even though the treatment and comparison groups are only about two months apart in age on average, because data collection was carried out just as decisions on school enrolment were being made for the younger cohort children (who were between 4 ½ and 6 years old in the second round); the non-linearity was a sufficiently strong predictor of whether children were in the treatment group at the time of the survey, even though we control for age differences separately.

This paper offers several new contributions. It is the only econometric evaluation, to our knowledge, of the effect of India's Midday Meals Scheme on the health outcomes of children; it contributes to the broader literature on school feeding as described above, including its unique focus on the impact of school feeding in coping with environmental shocks; and finally, it is one of the few evaluations of the impact of school feeding that corrects for self-selection and incorporates dynamic aspects of health determination.

We find large benefits for children whose households self report having suffered from drought between the two rounds; results from our preferred specification suggest drought exerts a substantial negative effect in both nutrition indicators but that this negative effect is entirely compensated for by the Midday Meals Scheme. We also show that the main results are robust to different identification and measurement strategies.

Our findings are broadly consistent with the medical literature on nutrition and supplementary feeding. In early childhood, serious damage can be done to the nutritional development of children with long-term implications for wider development, including cognitive skills. Nevertheless, as noted by several studies in that literature, whereas growth deficits persist into early adulthood if children remain in the same poor conditions, there is definitely potential for catch-up in height-for-age if circumstances improve for the better, such as through nutritional supplementation or migration when children are still young (see for example Tanner, 1981; Coly et al 2006; Golden, 1994). 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*”.

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

## **2. The Midday Meals Scheme in India**

The Midday Meals Scheme is perhaps the most important initiative by the Indian government in the area of education in recent years. Under the scheme, on every school day, all primary school students in public schools are provided with a cooked meal consisting of no less than 300 kcal and 8-12 grams of protein.

Although it was officially started in 1995, the Scheme remained unimplemented in most states until 2002. As noted previously, following a Supreme Court ruling in 2001, the Scheme was implemented across the country. As such it represents, at least in outreach, one of the most successful government interventions in recent years.

Andhra Pradesh started providing Midday Meals in January, 2003 to children in all primary and upper primary public and private-aided schools.<sup>6</sup> As several studies document, 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 surveys.

Much interest was generated in the performance of the Scheme after 2001, when the issue entered the mainstream political and media discourse in India. As a result, several field studies were carried out and reported over the next few years. Most studies of the program, with the exception of Afridi (2010, 2011), 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 focused 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

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<sup>6</sup> 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 including pay etc. as government schools. In practice, their quality and functioning is often indistinguishable from public schools (Kingdon, 1996). These form a very small part of the number of schools in Andhra Pradesh, about 4% according to Mehta (2007).

and parents). 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 (2011) confirms findings on attendance using a difference-in-differences estimator, finding large benefits in school participation especially for girls.

Afridi (2010) 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 question that we are interested in, namely that of longer term impact on child health, has 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 outcomes over a longer horizon and whether they help cushion children against a health deterioration caused by drought.

### **3. The Data**

The data we use in this study were collected by the Young Lives Project in 2002 and 2006-07 in the state of Andhra Pradesh. Andhra Pradesh is the fourth-largest state in India by area and had a population of over 84 million in 2011. It is divided into three regions – Coastal Andhra, Rayalaseema and Telangana – with distinct regional patterns in environment, soil and livelihood patterns. Administratively the state is divided into districts, which are further subdivided into *mandals*(sub-districts); *mandals* are the sentinel sites within our sample.

The surveys cover two cohorts: the first is comprised of 2011 children born between January 2001 and June 2002, and the second includes 1008 children born between January 1994 and

June 1995. In the second round conducted in 2006-07, 1950 children of the younger cohort and 994 children of the older cohort could be traced and resurveyed; attrition rates thus are low and therefore do not pose a problem for the analysis. In this paper, for reasons of program identification discussed below, we focus exclusively on the younger cohort<sup>7</sup>.

The period between 2001 and 2006 saw severe drought in several parts of the state in multiple years. Especially severe droughts affected Andhra Pradesh in 2002-03 and 2004-05. In these years, districts in our sample saw a severe shortfall in rain of up to 40% below normal rainfall; this has potentially devastating impacts on agricultural activity, much of which is primarily rain-fed. The droughts were especially severe in Rayalaseema and Telangana regions which are particularly drought-prone. The severity and spread of drought during this period in our sample is an important aspect to be incorporated in the analysis on evaluating the school meals program.

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 resolved and for outcomes to have been realized. The period also coincided with years of severe drought, making the data suitable for understanding the impact of Midday Meals in cushioning the impact of drought. Secondly, the longitudinal nature of the data helps greatly in dealing with problems in estimation and identifying impact. Thirdly, children in the younger cohort were aged between 4.5 and 6 years in Round 2 which is the age that questions

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<sup>7</sup> The only feasible comparison groups for the older cohort, who were about 8 years old in Round 1 and 12 years old in Round 2, are students in private schools or not enrolled, who are likely to differ in systematic ways from students in public schools, precluding a credible identification strategy. OLS regressions, similar to those implemented for the younger cohort, did not reveal any impact of the Midday Meals on nutrition outcomes for children in the older cohort.



of school choice are decided; as we later discuss, this is critical to our identification of program impact in this cohort. Finally, 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. Framework and Methodology

Following Senauer and Garcia (1991), Behrman and Deolalikar (1988) and Behrman and Hoddinott (2005), we visualize child health as entering directly into the welfare function of the household, reflecting the intrinsic value of child health to the household. Health is determined by a health production function of the form:

$$H_{it} = H (F_{it}, C_i, Z_{it}, H_{it-1}, U_{it}) \quad (1)$$

where  $H_{it}$  is the health of child  $i$  at time  $t$ ,  $F_{it}$  is the child's food consumption in period  $t$ ,  $C_i$  is a vector of time-invariant observable characteristics of the child, including determinants such as caste, gender, and parental education,  $Z_{it}$  is a vector of time-varying characteristics such as economic shocks,  $H_{it-1}$  is previous period health, and  $U_{it}$  is a vector of unobserved attributes of the child, parents, household, and community which affect the child's health status. The function allows for the possibility of interaction effects among its arguments.

Our focus here is not to estimate the structural parameters of the health production function but to evaluate the policy effect of MDMS on child malnutrition. We assume that access to the Midday Meal program, captured by the binary variable  $MDMS_{it}$ , results in a net increase in child food intake ( $F_{it}$ ), as found for example by Afridi (2010) in India and Jacoby(2002) in

Philippines. Following equation (1), we model health status as being determined by the following linear equation:

$$H_{it} = \alpha + \beta_1 \cdot MDMS_{it} + \beta_2 \cdot Z_{it} + \beta_3 \cdot MDMS_{it} \cdot Z_{it} + \beta_4 \cdot H_{i,t-1} + \beta_5 \cdot C_i + \varepsilon_{it} \quad (2)$$

Here, variables are as defined above. The specification allows for interactions between the treatment variable  $MDMS_{it}$  and time-varying characteristics  $Z_{it}$ . Following on from the specification above, our estimation equation is as follows:

$$H_{i2} = \alpha + \beta_1 \cdot MDMS_{i2} + \beta_2 \cdot Drought_i + \beta_3 \cdot MDMS_{i2} \cdot Drought_i + \beta_4 \cdot H_{i,1} + \beta_5 \cdot C_i + \varepsilon_i \quad (3)$$

Here, MDMS refers to the treatment dummy variable, Drought refers to self-reported drought having occurred between 2002 and 2006,  $H_{i,1}$  refers to first-period nutritional z score, and  $C$  is a vector of other controls, including dummy variables for different castes, being male, urban location as well as household size, caregiver's education and wealth index. All variables in  $C_i$  are from Round 1(2002).

### **Identification**

Of the children in the younger cohort, who range from 4½ - 6 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 were in private schools (including those run by NGOs and religious charities). Most of the children who are not yet in school in the second round would join formal schooling soon; the survey therefore also asked the caregivers what type of school (defined as public, private, religious etc.) would their child be likely to join and the age at which they thought

the child will be enrolled: the caregivers of over 95% of the children not yet enrolled report that they expect the child to be in school by the age of 6 years<sup>8</sup>.

In the data, only 1.47% of caregivers of the children enrolled in public schools (10 out of 682) reported that their school does not provide a midday meal, thus confirming the widespread implementation of the program indicated by previous studies<sup>9</sup>. We therefore define the treatment group as all children currently attending public school. As noted the caregivers of about 98.5% of children in public schools report that the school provides the meals, indicating the ten cases of reported non-availability of food may either reflect temporary unavailability or the caregiver's lack of knowledge about the whether the child receives the meal or not.

Our results are not driven by the assumption that all children in public schools receive the meal; such an assumption should indeed bias our results downwards, if at all, since non-recipients of the meals in public schools will drive the results downwards. The results are unchanged if we use the availability of the meals, as reported by the caregiver, to define the treatment group.

A major concern related to non-random program placement is the endogeneity of treatment (enrolling in a public school), especially through self-selection into the program. It is possible that self-selection into public schools is correlated with anticipated benefits of the program as reflected in changes in health or learning over time.

In our sample, children were too young to be enrolled in the first round but parents could have been influenced by the Midday Meals scheme in deciding whether and at what age to

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<sup>8</sup> The question of when the child is expected to join the school in the future elicited responses in completed years of age and not months.

<sup>9</sup> Caregivers of another 24 students (3.52%) report not receiving the midday meal because the child does not like the food.

enrol their children in public schools, which we observe in the second round. In fact, evidence from other studies suggests 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 (2006) and Afridi (2011)). Self-selection can take place through multiple 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 is likely to vary across regions. The first channel is unlikely to be very important in Andhra Pradesh because nearly all children in the state go to primary school. For instance, in the first round, over 97% of the children in the older cohort, then aged 8 years, were in school. We suspect the second channel also 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. It is the third channel that is most likely to be influential<sup>10</sup>.

In our analysis, we restrict the comparison group to children who are not currently enrolled but will be enrolled in a public school in the near future. This allows us to abstract from the endogeneity of the choice between private or public schooling. In Table 1 we present summary statistics across a range of measures for the treatment group, our restricted comparison group (children who will join public schools in the future), and all non-beneficiaries. There are significant differences in the mean of background variables between the treatment and the comparison groups; however these differences are frequently much smaller in magnitude and in statistical significance when using the restricted comparison

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<sup>10</sup> That this channel is influential in at least some cases has been documented in the qualitative data collected by Young Lives – some parents do enroll their children before the official age of enrolment just so that they can benefit from the Midday Meal.

group, comprising of only those children who are currently not in school but will join public schools later, rather than all non-beneficiary children. To the extent that the differences between the treatment and control groups is reduced similarly in relevant unobservables, we expect the bias caused by endogenous selection into the Scheme to also be lower; thus our preferred specification compares only children currently in public schools to children who will in the future go to public schools<sup>11</sup>.

To address endogeneity problems caused by self selection into the program, we exploit a non-linearity in the relationship between age and enrolment at this particular point of the children's educational trajectory where decisions around school enrolment were being made for the children in our sample.

The treated group is older on average than the comparison group in the sample, which is as we would expect; the mean difference is about two months. That enrolment differs so significantly, even though the associated age differences are very small, is a product of the specific point of their educational trajectory that the children are in i.e. at the very age that decisions about school enrolment are being taken; at any other ages outside this narrow window, we would expect to see no variation in enrolment induced by age differences of only 2-3 months.

Noting again that all children in our sample are born between January 2001 and June 2002, we create an indicator variable for being born after December 2001 and use this as an instrument that would predict enrolment but not nutrition, at the same time controlling for the linear effects of age (in years) based on the month and year of birth. Our instrumenting strategy outlined implies a first-stage equation of the form

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<sup>11</sup> We do, however, also report results including all children not currently enrolled in public schools, i.e. all children not yet enrolled and those enrolled in private schools, in the comparison group

$$MDMS_{i2} = \mu + \pi_1 Born2002 + \pi_2 Age + \pi_3 Z + \epsilon_i \quad (4)$$

Where *Born2002* is an indicator variable for being born in 2002, equalling zero if the child was born in 2001, *Age* is age at the time of the survey measured in years with daily precision, *Z* is a vector of exogenous variables including all exogenous covariates in the second-stage equation (Equation 3) and the instruments for first-period anthropometric z score (perceived size at birth and death of a household member during pregnancy) and an interaction term between *Born2002* and *Drought* variables which is used as an IV for the interaction term between *MDMS* and *Drought*<sup>12</sup>. As expected, results from the first-stage are strong and *Born2002* significantly predicts being in the treatment group, even controlling for all covariates in *Z* (including age which is controlled for in all specifications). These results are reported in Appendix 1<sup>13</sup>.

The intuition behind our use of this variable as an IV is straight-forward. Teachers in public school (and possibly parents) often use the calendar year of a child's birth to decide when he or she should enroll in school. Although the probability of being enrolled generally increases with age, such a rule of thumb would be expected to create a nonlinearity in the relationship between time of birth and enrolment between December 2001 and January 2002. That this non-linearity is empirically important can be seen clearly in Figure 1 which plots mean enrolment rates by month of birth. The proportion of children enrolled drops nearly in half from 56 percent in December 2001 to 30 percent in January 2002. Although there is noise in the month-to-month variation in enrolment rates, there is a sharply more negative relationship between birth month and enrolment rate in the months around the end of 2001<sup>14</sup>. This

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<sup>12</sup> Given the exogeneity of *Drought*, if *Born2002* is a valid IV for *MDMS*, then an interaction term of these two variables is a valid IV for the interaction term of *MDMS* and *Drought*.

<sup>13</sup> We report Kleibergen-Paap F-statistics in all the main estimation tables. They account for heteroskedasticity as well as the number of endogenous variables and excludable instruments. In most specifications on the restricted sample they are between 7-10.

<sup>14</sup> The lower rate of enrolment for children born between Jan-Mar 2001 seems puzzling but is explained by the fact that only 37 children in the dataset out of 1950 (<2%) were born in this period.

nonlinearity is consistent with the rule of thumb described based on calendar year of birth or could arise naturally around this time threshold due to social norms about the age of enrolment.<sup>15</sup> When other months are chosen as the threshold point for changes in enrolment probability, they are much weaker and usually lack statistical significance, suggesting that the nonlinearity in the relationship between time of birth and enrolment is specific to this time threshold.

Given the threshold nature of our instrument, our approach could be considered a regression discontinuity design; however the limited range of birth months and the significant month-to-month variation in enrolment rates precludes us from including higher order terms (beyond a linear control) for birth month as control variables. When we do so, the threshold variable lacks sufficient power to explain variation in enrolment rates. This is not surprising given that there is no strict enforcement of a rule that bases enrolment year on calendar year of birth.

The exclusion restriction on the IV would be violated if a change in the calendar year of birth had a non-linear impact in this age range, not only on the probability of enrolment but also on the changes in the anthropometric z scores. We do not however, have any reason to expect this to be the case: our anthropometric z scores are norm-referenced by age measured in days. The children in the enrolled and non-enrolled groups are very close in mean age.

Furthermore, any general non-linear impacts of age should not be confined to the impacts of the scheme on drought affected children but on the entire group of beneficiaries as a whole.

Our results however indicate that the entire benefit is concentrated on children whose households reported being drought affected. One possible effect of time of birth on child

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<sup>15</sup> 5 years is the prevailing norm for the age of enrolment into public schooling in Andhra Pradesh. For example even in the older cohort, 70% of the children who had joined public schools by Round 1 (2002) when they were 8 years old, entered formal schooling at 5 years of age.

health that could have affected children only in drought-affected areas is the age of exposure to the 2002-2003 drought. Children born after year-end 2001 were younger when the drought began to create hardship in the second half of 2002 (they were 0-6 months old in mid-2002) and so could have been more affected by the drought than the older children in the sample, who were 6-18 months old in mid-2002. However, for this to be a problem, the relationship between age of exposure and health impacts of the drought must not only be nonlinear but also must be nonlinear around the specific threshold of 6 months. When we run similar specifications on the sample of children who have enrolled in or plan to enrol in private schools, we do not find comparable effects of being born after December 2001 on health outcomes. This is true whether we estimate in IV regression with *Born2002* as an instrument for enrolment, or estimate the direct effects of *Born2002* interacted with *Drought* on child health outcomes after controlling linearly for *Age* and *Age* interacted with *Drought*. We take these results as suggestive evidence that our exclusion restrictions are valid. And even this were an issue, it is unlikely to explain the large magnitude of the treatment effects (discussed further below).

Incorporating the dynamic aspects of health determination is both desirable and essential but it exposes us to the problem of endogeneity of the lagged dependent variable. We instrument the lagged dependent variable (anthropometric score from Round 1) using the caregiver's perception of birth size and shocks during pregnancy (whether a household member had died). Birth size is related to conditions during pregnancy and is 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<sup>16</sup>.

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<sup>16</sup> 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. It is important to note that our results do not depend on the inclusion or instrumenting of the lagged dependent variable. The patterns around the impact of the drought, and the cushioning effect of the Midday Meals, are



## 5. Results

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 sample self-reported having been affected by drought between the two rounds.

Specifically we estimated equations of the form:

$$\Delta Y = \alpha + \beta_1 MDMS_i + u \quad (5)$$

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.

Table 2 presents the descriptive estimates of the unconditional impact estimated by the exercise above. These initial results indicate that the treatment had a significant impact on both measures for children who had suffered from drought but not for children who did not: these preliminary estimates imply a positive benefit of 0.22 s.d. for weight-for-age and 0.43 s.d. on height-for-age z scores; there are no significant impacts on children who did not suffer from drought.

Next, we present the main estimation results based on estimation of equation (4). We report first the results of our preferred specification which restricts the comparison group to children who plan to attend public schools but have not yet enrolled. Table 3 presents the results from

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similar in sign and statistical significance (although with much greater magnitudes) if we redefine our estimated equation using changes in z scores as the outcome variable and omit the lagged dependent variable from the regressors.

the OLS and IV estimates using weight-for-age and height-for-age z scores as dependent variables.

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. The over-identification tests for the IV regressions fail to reject the null of all included instruments being exogenous. 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 substantially. The compensatory effect of the Midday Meals is statistically significant across all selection-corrected estimates at the 5% level of significance.

The positive effect of the midday meals is larger for both health measures, across all specifications, than the negative impact of the drought, indicating that school meals more than compensate for the negative impact of the drought<sup>17</sup>. Following Supreme Court orders, 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 may be a plausible reason for why the benefits of the midday meals in drought-stricken areas outweigh the negative impact of the drought itself. The magnitude of this effect also could not be explained by greater (nonlinear) impacts of the 2002-2003 drought on younger children.<sup>18</sup>

One potential cause for concern in interpreting our estimates is that our drought measure is a self-reported binary variable which equals one if a household reports having suffered from

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<sup>17</sup> However, the overcompensation effect is not statistically significant as F-tests investigating whether the sum of coefficients of Drought and its interaction with MDMS is different from zero are not able to reject the null in most specifications. This pattern is also true of other ways of measuring drought where also the null cannot be rejected.

<sup>18</sup> One would not expect older children in drought-affected areas to have better age-adjusted health than younger children in areas not affected by drought, which would be necessary to produce program impacts that fully offset negative impacts of being exposed to drought.

drought in the past four years (i.e. between the two rounds) and zero otherwise. There could be systematic reporting bias in this variable that is correlated with time-varying unobservables which affect changes in nutrition. We do not think this likely to be a severe problem in our estimation given that the mean incidence of drought does not differ significantly at the 5% level between our treatment and comparison groups. Nonetheless, as a robustness check we reran our estimation using village-level averages of reported drought instead of self-reported drought directly; results from this exercise are shown in Appendix 2 and display a very close similarity in the pattern of incidence of benefits from the Scheme.

To avoid self-reporting bias, we can also use reports of natural disasters from the community questionnaire. A further advantage of using data from the community questionnaire is that, unlike the household questionnaire, we have information on the timing of droughts that affected the village in the last four years. This is important in order to assess whether the effect of the Midday Meals in cushioning the impact of drought is mostly contemporaneous (i.e. compensating for recent droughts) or whether it is compensating for health deterioration in the past (i.e. leading to catch-up growth). Context instruments were administered in each of the communities (villages or urban wards) from which the data are collected; these collected information from local key informants on the natural disasters that affected the community between rounds, including how long ago the disaster had taken place. 50 out of 101 communities reported having been affected by drought in the past four years, of which 19 reported the drought had happened in the last 13 months; all other communities reported the drought as having occurred at least 18 months ago<sup>19</sup>. We used this information to rerun our analysis in the following way: first using just the community-level variable for whether a drought had happened in the last four years instead of the self-reported drought measure, then only using a dummy variable for a drought in the last 13 months and finally only using a

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<sup>19</sup> Three communities reported drought twice in the intervening period. We used the more recent drought from that community in the estimation.

dummy variable for a drought at least 18 months ago. Results from this analysis are presented in Table 4. As can be seen, the effect of drought is negative (although not statistically significant for weight-for-age) in the first set of results which use a dummy variable for whether a drought had happened in the last four years and there is a significant positive impact for the Midday Meals across both measures of nutrition; this pattern breaks down entirely in the case where drought occurred in the last 13 months and coefficients on neither drought nor its interaction with Midday Meals are significant; finally, in the case where the drought happened at least 18 months ago, both the impact of the drought and the safety-net impact of the Midday meals are strongly significant and close in magnitude to our results using self-reported drought. Again, the preferred IV results find that the Midday Meals more than compensate for the negative effects of drought. We interpret this set of results as suggesting strongly that in our data Midday Meals are compensating for the negative effects of severe past droughts through catch-up growth, not contemporaneously preventing health deterioration due to any current droughts.

As a final robustness check, we report results using the full sample rather than just children attending or planning to attend public schools. Results are reported in Appendix 2. The results from the IV specifications are substantially similar when using the full sample.<sup>20</sup>

## **Discussion**

The magnitude of the effects of the MDMS program is very large: for boys aged 65 months (the mean age in our sample), for example, the preferred specifications using self-reported

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<sup>20</sup> Although results using the full sample exhibit the same patterns in the sign and magnitude of the coefficients, these are not always statistically significant. The insignificance of our results at times in the full sample is a product of the IV that we use. Norms around the age when children may be admitted to school are much more implemented in the public schooling sector than in private institutions which are more amenable to admitting students at younger ages as well. Thus our IV is much less informative in the full sample than it is in the restricted sample of children who are in or will later join public schools. This is borne out by weaker first-stage results and much lower F-statistics when using the full sample instead of the restricted sample.

drought suggest that drought creates a height loss which roughly equals the distance between the 25<sup>th</sup> and 50<sup>th</sup> percentiles, and a weight loss of about two-thirds of the same distance, in the WHO (2007) growth charts. The very large magnitude of the impacts of the Scheme may well appear surprising. However, it is worth recalling that the children are at a relatively young age; their susceptibility to negative nutritional shocks, and their responsiveness to supplementary feeding, are both higher than one would expect in later childhood. They have also passed through periods of severe nutritional stress and therefore perhaps the exhibition of catch-up growth is explicable.

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 safety net compensating for this previous health shock.

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 3 years of age. However, as noted in the introduction, the literature on nutrition contains several studies that refute this belief.

Finally, in discussing the wider applicability of these results, it should be noted that Andhra Pradesh is one of the better performers among Indian state in service delivery generally, and in the Midday Meals Scheme in particular. The superior performance of the Scheme in this state has been noted in both the academic literature (e.g. Dreze and Goyal,2003) and administrative reviews of the Scheme (Saxena, 2003). The findings may not generalize to

other states within India, especially to states such as Uttar Pradesh and Bihar noted as poor implementers of the Scheme, unless the delivery mechanisms and political/administrative will can also be raised to similar levels.

## **6. Conclusions**

The effect of school meals as a safety net can be of much importance. Much 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 academic literature but may be worth evaluating separately in future studies.

This omission in the academic literature regarding the role of school feeding in social protection is especially surprising given that the same is not true of related administrative and policy documents as noted earlier. Our findings indicate that the role of the safety net, at least for younger children, is very significant.

We believe that these results, combined with other evidence on the positive impact of school meals on school participation and daily nutrient intake, provide empirical support for the benefits of 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 more than 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. We conclude that school meals may well be one of the most potent school-based interventions available to policy makers in developing countries.

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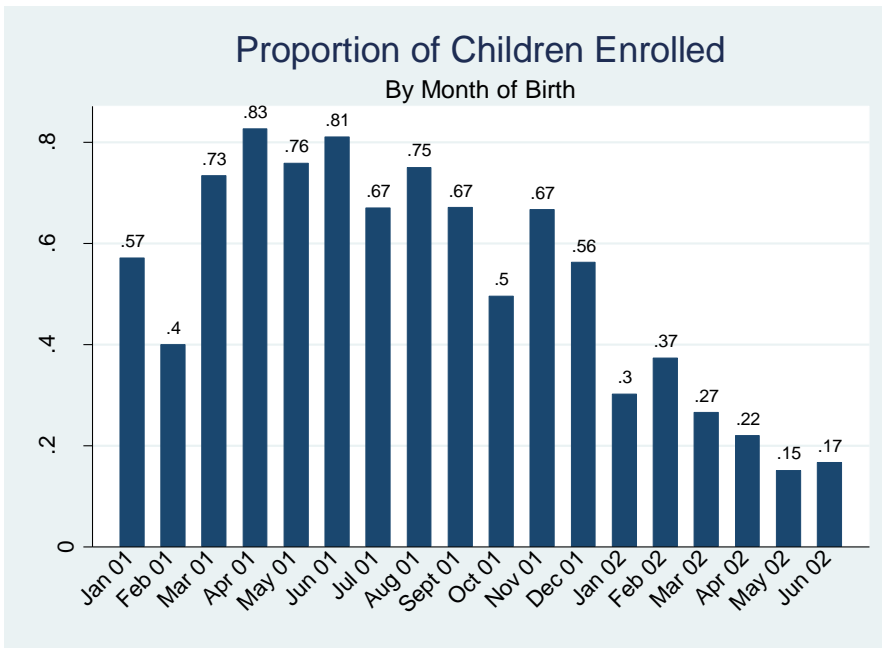
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Figure 1. Proportion of Children Enrolled by Month of Birth



**Table 1. Descriptive Statistics**

	Treatment Group	All non-beneficiaries	Restricted Comparison group	Total
Male	0.515	0.543	0.504	0.533
Urban	0.058	0.347***	0.103***	0.244
Drought	0.34	0.244***	0.386*	0.278
Wealth Index (Round 1)	0.254	0.385***	0.244	0.338
Scheduled Castes	0.24	0.149***	0.216	0.182
Scheduled Tribes	0.188	0.095***	0.142**	0.128
Backward Classes	0.455	0.492	0.524**	0.478
Other Castes	0.117	0.262***	0.116	0.21
Telangana region	0.279	0.389***	0.375***	0.35
Royalaseema region	0.344	0.277***	0.295*	0.301
Coastal Andhra Pradesh	0.377	0.334*	0.33*	0.349
Height-for-age z score (2002)	-1.351	-1.27	-1.597***	-1.298
Weight-for-age z score (2002)	-1.621	-1.504**	-1.843***	-1.546
Height-for-age z score (2007)	-1.645	-1.66	-2.1***	-1.655
Weight-for-age z score (2007)	-1.894	-1.859	-2.181***	-1.872
Age (in years)	5.501	5.343***	5.29***	5.399

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 2: ATT from OLS regressions on the treatment binary**

	Total Sample	Drought	Without Drought
ΔWeight-for-age	0.057 (1.05)	0.222*** (3.28)	-0.048 (-0.91)
ΔHeight-for-age	0.20** (2.18)	0.429** (2.85)	0.034 (0.39)

Standard errors were clustered at sentinel site level; t statistics in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3. Main Estimation Results on younger cohort nutrition**

VARIABLES	(1)	(2)	(3)	(4)
	Weight-for-age in 2006/7 OLS	IV	Height-for-age in 2006/7 OLS	IV
Midday Meals	0.038 (0.81)	0.19 (0.77)	0.14* (1.68)	-0.17 (-0.50)
MDMS x Drought	0.23*** (2.86)	0.57** (2.37)	0.19*** (3.12)	0.91** (2.27)
Drought	-0.24*** (-3.49)	-0.42*** (-3.50)	-0.33*** (-5.93)	-0.73*** (-3.74)
Age expressed in years	-0.044 (-0.47)	-0.21 (-1.40)	0.41*** (2.69)	0.42* (1.77)
Weight-for-age in R1	0.63*** (6.81)	0.60*** (6.76)		
Height-for-age in R1			0.59*** (5.00)	0.59*** (5.49)
Constant	-0.72* (-1.79)	0.051 (0.077)	-3.10*** (-4.49)	-2.99*** (-2.81)
Observations	1,188	1,188	1,172	1,172
R-squared	0.386	0.368	0.211	0.179
Kleibergen-Paap F-statistic	28.0	8.43	10.0	7.26
Hansen J-statistic p value	0.67	0.33	0.35	0.18

Robust z-statistics in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

N.B. (1) Standard errors are clustered at site level.

(2) Columns 2,4 (IV results) present results correcting for self-selection using being born in 2002 as an instrument

(3) Lagged anthropometric indicators are instrumented throughout, including in columns marked OLS, using birth size and death of a household member during pregnancy as instruments.

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

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

**Table 4: Results using Community-level Occurrence of drought and sensitivity to timing of drought**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	<b>Whether drought happened in the last 4 years</b>				<b>Whether drought happened in last 13 months</b>				<b>Whether drought happened at least 18 months ago</b>			
	Weight-for-age (2006/7)		Height-for-age (2006/7)		Weight-for-age (2006/7)		Height-for-age (2006/7)		Weight-for-age (2006/7)		Height-for-age (2006/7)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Midday Meals	0.011 (0.18)	0.17 (0.70)	0.076 (0.74)	-0.18 (-0.53)	0.11* (1.86)	0.49** (2.02)	0.21** (2.41)	0.32 (0.78)	0.077 (1.50)	0.27 (1.46)	0.13 (1.47)	-0.033 (-0.12)
MDMS x Drought	0.19*** (3.10)	0.41** (2.02)	0.21** (2.03)	0.65** (2.21)	0.092 (1.10)	-0.12 (-0.62)	-0.003 (-0.02)	-0.14 (-0.50)	0.12* (1.71)	0.51** (2.39)	0.18 (1.56)	0.78** (2.14)
Drought	-0.050 (-0.70)	-0.16 (-1.32)	-0.3*** (-2.91)	- (-2.81)	0.026 (0.21)	0.14 (0.93)	0.044 (0.34)	0.12 (0.67)	-0.089 (-1.01)	-0.27** (-2.32)	-0.26** (-2.17)	-0.58** (-2.53)
Age expressed in years	-0.039 (-0.41)	-0.22 (-1.45)	0.44*** (2.99)	0.41* (1.82)	-0.045 (-0.47)	-0.23 (-1.45)	0.42*** (2.93)	0.38 (1.58)	-0.029 (-0.31)	-0.22 (-1.46)	0.43*** (2.89)	0.41* (1.70)
Weight-for-age in R1	0.62*** (6.71)	0.59*** (6.29)			0.62*** (6.96)	0.59*** (6.54)			0.63*** (7.02)	0.60*** (6.84)		
Height-for-age in R1			0.59*** (5.15)	0.57*** (5.36)			0.59*** (5.47)	0.58*** (5.91)			0.58*** (5.17)	0.58*** (5.35)
Constant	-0.75* (-1.82)	0.080 (0.12)	- 3.19*** (-4.71)	- 2.91*** (-2.80)	-0.77* (-1.87)	-0.0010 (-0.002)	- 3.25*** (-4.60)	- 3.09*** (-2.74)	-0.82** (-1.99)	0.059 (0.087)	- 3.19*** (-4.69)	-2.98** (-2.55)
Observations	1,188	1,188	1,172	1,172	1,188	1,188	1,172	1,172	1,188	1,188	1,172	1,172
R-squared	0.383	0.371	0.204	0.213	0.385	0.364	0.198	0.210	0.380	0.347	0.216	0.189
Kleibergen-Paap F-statistic	31.2	6.91	10.3	8.20	35.0	6.35	11.6	9.81	33.5	9.22	11.4	9.14
Hansen J-statistic p value	0.88	0.55	0.38	0.31	0.82	0.57	0.45	0.39	0.91	0.68	0.44	0.52

Robust z-statistics in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

N.B. (1)Standard errors are clustered at site level.

(2)Columns 2,4,6,8,10,12 (IV results) present results correcting for self-selection using being born in 2002 as an instrument

(3) Lagged anthropometric indicators are instrumented throughout, including in columns marked OLS, using birth size and death of a household member during pregnancy as instruments.

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

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

### Appendix 1: First stage results for endogenous variables

VARIABLES	(1) MDMS	(2) MDMS x Drought	(3) Weight-for-age z score (R1)	(4) Height-for-age z score (R1)
Born in 2002	-0.15*** (-3.38)	0.028 (1.62)	-0.056 (-0.48)	-0.12 (-0.76)
Born2002 x Drought	-0.11* (-1.82)	-0.33*** (-6.81)	0.11 (1.03)	0.37 (1.70)
perception of child's size at birth	-0.011 (-1.09)	-0.0041 (-0.47)	-0.28*** (-9.62)	-0.21*** (-4.03)
Death of household member(s)	-0.12 (-1.67)	-0.088 (-1.48)	-0.36* (-1.99)	-0.38** (-2.83)
Drought	0.056 (1.57)	0.55*** (13.1)	-0.014 (-0.25)	0.041 (0.33)
Age expressed in years	0.14** (2.27)	0.054 (1.70)	-0.55*** (-3.43)	-0.79*** (-3.91)
Constant	-0.18 (-0.48)	-0.28 (-1.52)	2.37** (2.50)	3.35*** (3.07)
Observations	1,915	1,915	1,899	1,878
R-squared	0.214	0.426	0.163	0.162

Robust z-statistics in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

N.B. (1) Standard errors are clustered at site level.

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

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

## Appendix 2: Estimates using the whole sample

VARIABLES	(1) Weight-for-age in 2006/7 OLS	(2) IV	(3) Height-for-age in 2006/7 OLS	(4) IV
Midday Meals	0.037 (1.13)	0.59 (1.24)	0.066 (1.29)	0.10 (0.23)
MDMS x Drought	0.15*** (2.85)	0.28 (0.75)	0.13** (2.19)	0.74 (1.60)
in last 4yrs has hh suffered drought	-0.16*** (-4.57)	-0.24 (-1.50)	-0.28*** (-4.08)	-0.55*** (-2.86)
Age expressed in years	-0.023 (-0.40)	-0.24 (-1.46)	0.42*** (4.72)	0.31* (1.86)
Weight-for-age in R1	0.66*** (10.3)	0.63*** (9.64)		
Height-for-age in R1			0.57*** (6.82)	0.55*** (7.22)
Constant	-0.87*** (-2.81)	-0.023 (-0.033)	-3.14*** (-6.94)	-2.63*** (-3.57)
Observations	1,888	1,888	1,867	1,867
R-squared	0.408	0.341	0.269	0.256
Kleibergen-Paap F-statistic	48.0	5.29	23.4	4.24
Hansen J-statistic p value	0.89	0.69	0.25	0.13

Robust z-statistics in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

N.B. (1) Standard errors are clustered at site level.

(2) Columns 2,4 (IV results) present results correcting for self-selection using being born in 2002 as an instrument

(3) Lagged anthropometric indicators are instrumented throughout, including in columns marked OLS, using birth size and death of a household member during pregnancy as instruments.

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

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

### Appendix 3: Results using site-averaged drought measure

VARIABLES	(1) Weight-for-age in 2006/7 OLS	(2) Weight-for-age in 2006/7 IV	(3) Height-for-age in 2006/7 OLS	(4) Height-for-age in 2006/7 IV
Midday Meals	0.015 (0.33)	0.24 (1.01)	0.077 (0.82)	-0.19 (-0.53)
MDMS x Drought	0.29** (2.30)	0.52* (1.69)	0.31** (1.97)	0.95 (1.54)
Drought (Village-level average)	-0.36*** (-2.95)	-0.43** (-2.16)	-0.90*** (-7.12)	-1.24*** (-4.09)
Age expressed in years	-0.034 (-0.37)	-0.21 (-1.40)	0.42*** (2.90)	0.43* (1.84)
Weight-for-age in R1	0.62*** (6.67)	0.59*** (6.33)		
Height-for-age in R1			0.58*** (5.04)	0.57*** (5.34)
Constant	-0.75* (-1.91)	0.054 (0.080)	-3.11*** (-4.95)	-3.00*** (-2.90)
Observations	1,188	1,188	1,172	1,172
R-squared	0.385	0.374	0.240	0.242
Kleibergen-Paap F-statistic	29.8	8.41	9.84	7.45
Hansen J-statistic p value	0.67	0.38	0.21	0.13

Robust z-statistics in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

N.B. (1) Standard errors are clustered at site level.

(2) Columns 2,4 (IV results) present results correcting for self-selection using being born in 2002 as an instrument

(3) Lagged anthropometric indicators are instrumented throughout, including in columns marked OLS, using birth size and death of a household member during pregnancy as instruments.

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

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