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Stefan Dercon, Albert Park and Abhijeet Singh

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Stefan Dercon, Oxford University and CEPR
Albert Park, Hong Kong University of Science and Technology
Abhijeet Singh, Oxford University

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Centre for Economic Policy Research
77 Bastwick Street, London EC1V 3PZ, UK
Tel: (44 20) 7183 8801, Fax: (44 20) 7183 8820
Email: cepr@cepr.org, Website: www.cepr.org

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ABSTRACT

School Meals as a Safety Net: An Evaluation of the Midday Meal Scheme in India*

Despite the popularity of school meals, 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 ameliorated the deterioration of health in young children caused by a severe drought. Correcting for self-selection into the program using a non-linearity in how age affects the probability of enrolment, we find that the program acted as a safety net for children, providing large and significant health gains for children whose families suffered from drought.

JEL Classification: I12, J13, O12 and O15
Keywords: health, india, nutrition and school meals

Stefan Dercon
Department of International Development
Oxford University
3 Mansfield Road
Oxford, OX1 3TB
UK

Email: stefan.dercon@economics.ox.ac.uk

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Albert Park
Hong Kong University of Science and Technology
Room 3335A, Academic Bldg
Clear Water Bay, Kowloon
HONG KONG

Email: albertpark@ust.hk

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Abhijeet Singh
Nuffield College, New Road,
Oxford

Email:
abhijeet.singh@nuffield.ox.ac.uk

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www.cepr.org/pubs/new-dps/dplist.asp?authorid=175689

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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"[1]. By 2003, most states had 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.

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 for school enrolment and attendance. Additionally, it was envisioned that the program would reduce undernourishment among school children[2].

The evidence, however, on the impact of the program on nutrition is rather thin. While there is evidence that school feeding in India and elsewhere does indeed improve the immediate nutritional intake of children (Afridi 2010, Jacoby 2002), there are few studies documenting the effect of school feeding programs on outcome indicators of child nutrition, and those that are available find ambiguous effects (e.g. Vermeersch and Kremer, 2004[3]).

Furthermore, there has been no attempt, whether in the context of the Scheme in India or in the broader literature on school feeding programs, to evaluate their role in coping with large negative income 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 human

[1] The full text of the court orders in this regard are available at www.righttofoodindia.org
[2] It was also 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. And finally, the program was hypothesized to deliver other social benefits such as the break-down of caste barriers by children of different castes eating together.
[3] The Vermeersch and Kremer (2004) 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 our sample, making their study useful for comparison purposes.

capital 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 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 therefore central to understanding the benefits of large-scale school feeding schemes.

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. We aim to assess whether the Midday Meals program ameliorates the negative impact of weather shocks (drought) on children's' health. Further, we aim to understand whether school meals only compensate for shocks which happen contemporaneously with the program or whether they mitigate the nutritional impacts of shocks experienced earlier in childhood through catch-up growth.

We analyze data from a longitudinal study of children in poverty collected by the Young Lives Project in the state of Andhra Pradesh; details about the sample, and the state of Andhra Pradesh (A.P.), are presented in Section 3. The survey collected extensive information about children in two cohorts (born in 1994/95 and 2001/02 respectively) in 2002 and 2007. The school feeding program, known as the Midday Meal Scheme in India (henceforth MDMS), was introduced in Andhra Pradesh in January 2003. In this study we focus exclusively on the younger cohort of children. These children were born during an 18-month period from January 2001 and June 2002; their average age was about 12 months in Round 1 and about five-and-a-half years in Round 2. At the time of the second round of the survey, children in our treatment group would have received the school meals for an average of 9 months.

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 many villages. There was a very severe drought in 2002-3, following the failure of the monsoon rains between July-September 2002. This drought was nation-wide in impact and was the worst since at least 1987 and possibly much longer[4]. Monsoon rainfall was also deficient in 2004 in India, and specifically in Andhra Pradesh, albeit to a smaller extent than in 2002[5]. Children in our sample on average would have been around one year of age at the time of the 2002-3 drought and about 3 years of age at the time of the 2004-5 drought.

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 strongly affects the probability of enrolment, as it is used as a 'rule of thumb' to determine the appropriate time for 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. Details of our identification strategy are presented in Section 4.

[4] The drought of 2002 was covered in great detail in the national and state media in 2002 and 2003. See, for example, Frontline (2002), Kumar (2002), Financial Express (2003), The Hindu (2003) and The Times of India (2003). It was also reported in the international press, for example, the New Scientist (Tata, 2002). The Agriculture Secretary at the time was reported to have called the 2002 drought the worst in 120 years (Financial Express, 2003).

[5] See, for example, Financial Express (2004) and Mukherjee and Chakraborty (2004).

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 on both nutrition indicators (about -0.4 s.d. in weight-for-age z scores and -0.7 s.d. in height-for-age z scores) but that these negative effects are entirely compensated for by the Midday Meals Scheme. We present evidence that this effect is entirely driven by droughts that had happened at least 18 months before the second round of data collection, i.e. no later than 2005, and that thus this compensatory effect of the Midday meals arises from remedial 'catch-up growth' since most children receiving the school meals would have enrolled in mid-2006. We also show that the main results are robust to different identification and measurement strategies (Section 5).

As we discuss in greater detail in Section 6, our findings are broadly consistent with the medical literature on nutrition and supplementary feeding. Whereas height deficits caused due to external shocks are commonly believed to be hard to compensate for after the first 24-36 months of a child's life, 'catch-up growth' has in fact been documented in several datasets from developing countries such as the Phillipines, Peru and Senegal even up until the age of 12 years; average magnitudes of improvement in height-for-age, where such catch-up is observed, are large across all these studies. Whereas growth deficits persist into early adulthood if children remain in poor conditions, there is 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).

This paper makes several new contributions to the literature. 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 negative income shocks; it is, to our knowledge, one of the few papers that evaluates a plausible policy measure to facilitate catch-up growth in a context where child malnutrition is very widespread[6]; 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.

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[7].

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 interventions by the Indian government 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.[8] As several studies document, this Scheme was nearly 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

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[6] See Haddad (2011), for instance, detailing the high rates of undernutrition in India, and the necessity for policy interventions to combat this; 40% of Indian children were stunted in 2005-6 according to the National Family Health Survey Round 3.
[7] From the 2006-7 school year, which is the relevant school year for our study, the minimum nutritional standards were revised upwards to at least 450 kcal and 12 grams of protein. The school meals in our study therefore, may have represented an even bigger nutritional increment to the diet of beneficiaries than estimated in previous studies.
[8] 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).

Pratham (2007) report that over 98% of government schools in the state were serving a Midday Meal on the day of their school surveys[9].

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 over the next few years. Most studies of the program, with the exception of Afridi (2010, 2011) and Jayaraman and Simroth (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 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. Jayaraman and Simroth (2011) document a 13% increase in enrolment in response to the Scheme identifying the effects from timing differences in the roll-out of the Scheme across different Indian states.

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

[9] See Jayaraman and Simroth (2011) for a detailed discussion of the Scheme and its implementation across different Indian states.

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%."

However, the question that we are interested in, namely that of the longer term impact of the Scheme on child health and its role as a safety net, has not been directly addressed by any previous study.

3. The Data

The data we use in this study were collected by the Young Lives Project between September-December 2002 and between January-June 2007[10] 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 sub-divided 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 2007, 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[11]. In this paper, for reasons of program identification discussed below, we focus exclusively on the younger cohort[12].

[10] About 94% of the interviews in Round 2 were carried out between January-April 2007; children interviewed after this period were often those who had migrated outside the original Young Lives communities and thus needed to be interviewed separately from the rest of the sample.

[11] For greater details about the representativeness of the Young Lives sample, as compared to the DHS sample for Andhra Pradesh, as well as details of attrition, please refer to Kumra (2008).

[12] 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

As noted in the introduction, the period between 2001 and 2007 saw severe drought in several parts of the state, especially in 2002-3. In these years, districts in our sample saw a severe shortfall in rain of up to 40% below normal rainfall. This had a devastating impact on agricultural activity, much of which is primarily rain-fed; in 2002-3, the total foodgrain production in Andhra Pradesh was a quarter below the 'normal' production in both of the main agricultural growing seasons in the state[13]. The drought was especially severe in Rayalaseema and Telangana regions which are particularly drought-prone.

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 Scheme's teething problems to have been resolved and for outcomes to have been realized. The period also spanned a period 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 around the normal time of school enrolment; as we later discuss, this is critical to our identification of program impact. 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 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

implemented for the younger cohort, did not reveal any impact of the Midday Meals on nutrition outcomes for children in the older cohort.

[13] These figures are based on data from the Directorate of Economics and Statistics, Government of Andhra Pradesh and were retrieved from www.indiastat.com in January 2012.
Although rainfall was deficient in 2004 also, the effect on agricultural production was much smaller at around 2% below normal.

the household, reflecting the intrinsic value of child health to the household. Health is determined by a health production function of the form:

(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 Scheme, 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:

(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 equation (2), our estimation equation is as follows:

(3)

Here, MDMS refers to the treatment dummy variable, Drought refers to self-reported drought having occurred between 2002 and 2006[14], $H_{i,1}$ 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 a household 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 of children not yet in school 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[15].

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[16]. We therefore

[14] We will also show that our conclusions are robust to using alternative measures of drought.
[15] 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.
[16] Caregivers of another 24 students (3.52%) report not receiving the midday meal because the child does not like the food.

define the treatment group as all children currently attending public school[17]. 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.

At the time of the second round of the survey in 2007, children in our treatment group would have received the meal for an average of about 9 months, with a minimum of about 7 months and a maximum of about one year[18]. Children in the treatment group were aged between 48 and 65 months, with an average age of about 57 months, at the start of the 2006/7 school year.

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.

Parents could have been influenced by the Midday Meals scheme in deciding whether and at what age to enrol their children in public schools. 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[19]. In our

[17] 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.

[18] These durations are calculated as the period of time between the start of the school year in mid-June 2006 and the date of the interview.

[19] 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.

analysis, we utilize the information on the type of school that children will join in the near future to restrict the comparison group to children who are not currently enrolled but will be enrolled in a public school in the near future; thus our preferred specification compares only children currently in public schools to children who will in the future go to public schools[20]. 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 group, comprising only those children who are currently not in school but will join public schools later.

Using this sample, our treatment and comparison groups are mainly differentiated by whether they have enrolled in school; as we discussed earlier, it is plausible that this decision is endogenous and affected by the availability of the Scheme[21]. To address endogeneity problems caused by self-selection in enrolment, we adopt an instrumental variable approach.

The requirement for an IV in this case is that it should be able to predict enrolment in school at the time of the survey, in this sample of children who are either in public schools already

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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. 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.

[20] 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

[21] This concern ties in directly with the theoretical framework where we posited that unobservable factors at the household and child level might directly affect nutrition; if these unobservable factors (e.g. parental concern) directly affect the probability of enrolment as well, OLS estimates of the treatment effect would be biased. This concern prompts us to use an instrumental variable approach.

(treatment group) or will join public schools in the future (comparison group), but not otherwise be an independent determinant of nutrition. To this end, we exploit a non-linearity in the relationship between age and enrolment induced by a change in the calendar year of birth; this non-linearity affects the probability of enrolment at this particular point of the children's educational trajectory, but is not expected to be associated with the nutritional outcomes of children once we separately control for age.

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[22].

The intuition behind our use of this variable as an IV is straight-forward. Parents and teachers 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 of children born in December 2001 to 30 percent of those born 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[23].

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[22] Age was calculated based on the difference in days between the date of interview and the date of birth. 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 small differences in age are associated with large differences in enrolment, as in Figure 1, 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.

[23] 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. Similarly, only 6 children

This 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.[24] 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 can be considered a regression discontinuity design in which we control for the running variable (age) with a linear term and use the discontinuity as an instrument for the treatment of interest. The linear control for age is reasonable given the limited range of birth months; when we try including higher order terms for age, the threshold variable lacks sufficient power to explain variation in enrolment rates.

Our instrumenting strategy outlined implies a first-stage equation of the form

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

are born in June 2002 which has a higher enrolment rate than the preceding months. Results are not sensitive to the exclusion of children born in these months.

[24] 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.

between *MDMS* and *Drought*[25]. 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. Additionally, 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 of the Scheme is concentrated on children whose households reported being drought affected. One possible effect of time of birth on child 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.

Another way to test whether the effects we find are due to nonlinear effects of age on nutrition when exposed to drought is to test for threshold effects of age on nutritional outcomes for the sample of children enrolled in or planning to enrol in private schools. Since these children did not have access to the Midday Meals Scheme, finding any nonlinear effects of age on nutritional outcomes would provide evidence against our identifying assumptions.

[25] 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*.

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. The instruments are appealing because they are pre-determined when the lagged health measurement is taken and so should be less correlated with current outcomes than the more recent lagged measurement; however, we cannot rule out remaining correlation with unobserved household characteristics that affect current child health.[26]

5. Results

For descriptive purposes, 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:

(5)

[26] 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 similar in sign and statistical significance (although with even 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.

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 0.23 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 the OLS and IV estimates using weight-for-age and height-for-age z scores as dependent variables. 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[27].

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. However, the negative impact of drought is compensated for by school-feeding in all specifications. The over-identification tests for the IV regressions fail to reject the null of all 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

[27] 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.

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. 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.

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 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; results from this exercise are shown in Appendix 3 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 questionnaires. 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[28].

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 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. 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.

[28] Three communities reported drought twice in the intervening period. We used the more recent drought from that community in the estimation.

We also ran the analysis on a series of sub-samples to better understand the pattern of nutritional benefits. In particular, we re-ran the analysis sequentially restricting the sample to rural children and by whether they were stunted/underweight in R1 or not. We found that the results are driven entirely by the rural sample, and that they are driven entirely by children who were *not* stunted/underweight in 2002[29]. These results are as we would expect. As described earlier, one might be concerned that the positive interaction effect of drought and school meals (enrolment) is due to a nonlinear effect of the age of drought exposure. However, when we estimate our base specifications on the sample of children in rural areas who have enrolled in or plan to enrol in private schools, we do not find any effects of being born after December 2001 on health outcomes of drought affected children. 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*[30]. One might be concerned that parents of children enrolled in private school are wealthier than parents of children enrolled in public schools, accounting for the lack of drought impacts that are nonlinear with age. However, while households in the private schools sub-sample are wealthier on average, there is considerable overlap in the wealth index between households in the treatment group and the private school sub-sample in rural areas. Our non-result holds even when we restrict the sample to rural households with wealth levels similar to our treatment group. We also note that within rural areas the baseline wealth index did not differ significantly between the drought-affected households and the non-affected households even at the 10% level of significance, which reflects perhaps the severity of the 2002-3 drought.

[29] Results not presented in the interests of space. It should be noted that as a result of severely restricting sample sizes, occasionally the statistical significance of the results declines to 10% level of significance and F-statistics also go down.

[30] Our instrument is very weak when applied only to the sample of children who are already in or will soon attend private schools; this is because children about to enrol in private schools typically would spend a longer period (about two years) in pre-primary Kindergarten classes than children enrolling in government schools who make the transition from public preschools (anganwadis) to primary school earlier.

Finally, in the light of a large literature documenting the long-term impacts of environmental shocks in early childhood (e.g. Almond 2006; the literature surveyed in Almond and Currie, 2011) , it seems implausible to us that the nonlinear effects of age of exposure to the drought could be such that older children (who were only two months older than the control sample) experienced no negative effects from the 2002-3 drought while children in the control sample experienced large negative effects of drought on nutrition. The older children were nearly all less than two years of age when the famine hit. This is, however, precisely the claim that would have to be put forth if we are to believe that the pattern of Midday Meals compensating for the negative impacts of drought is, in fact, an artefact of direct non-linear impacts of age on nutrition.

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.[31]

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 drought suggest that drought creates a height loss of about 0.77 s.d. which roughly equals the distance between the 25^{th} and 50^{th} percentiles, and a weight loss of about 0.44 s.d. which equals two-thirds of the same distance, in the WHO (2007) growth charts; our results suggest that this entire gap is compensated for by the Midday Meals Scheme Similarly large estimates

[31] 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.

of the negative impacts of shocks to the household on the anthropometric z-scores of children have previously been observed in the literature: for example, Akresh, Verwimp and Bundervoet (2011) document a fall of 0.86 s.d. in height-for-age for girls in as a result of crop failure and a fall of 1.05 s.d. in height-for-age for children who suffered from exposure to the Rwandan civil war. However, the large magnitude of the impacts of the Scheme may appear surprising. At first glance, it may also appear surprising that drought and school feeding has a significant effect not only on weight-for-age but also on height for-for-age.

As noted in the Introduction, however, catch-up growth in height-for-age has been observed in large magnitudes in several countries. Adair(1999) looks at catch-up growth between 2 and 12 years using the Cebu panel study from the Philippines and finds that almost a third of the children stunted at 2 years of age experienced catch-up growth by the time they were 8.5 years old. Those who did experience catch-up growth had mean increments in height-for-age z scores of 1.14 s.d. Coly et.al.(2006) document similar magnitude of catch-up in Senegal with especially large changes for those stunted at preschool: using WHO norms, they find mean height-for-age z score increases of 0.21, 0.90,and 1.79 s.d. for girls , and 0.31, 0.95, and 1.44 s.d. for boys, with no stunting, mild stunting, and marked stunting, respectively. Crookston et. al. (2010), analyzing Young Lives data from Peru, which compares children of exactly the same age in 2002 and 2006/7 also document catch-up for a large proportion of the children who were stunted in Round 1; for those children, in whom catch-up growth is observed, the magnitude of catch-up is an average of 1.13 s.d.[32]

While it is true that early growth deficits generally tend to persist into adulthood, it is not established that this is a biological necessity: as Golden et. al. (1994) hypothesize: *"the available data could be interpreted to show that a period of malnutrition in the first 2–3 yr*

[32] They further document that there is no gap in cognitive achievement at 5 years of age between children who recovered from stunting through catch-up growth and children who weren't stunted in either round.

*irrevocably changes the child so that he is 'locked into' a lower growth trajectory with a lower potential for future growth. The alternative hypothesis is that full catch-up growth is possible. However, this is not observed in practice because the correct conditions are not satisfied because in most populations environment and diet do not change." (*as quoted in Boersma and Wit, 1997*)*.

As Afridi (2010) rigorously documents, midday meals represent a substantial increase in nutritional intake for children. Given that children in her study (with a mean age of 8.5 years) were about 3-4 years older on average than our treatment group at time of first enrolment, that official guidelines on the minimum nutritional content of school meals are not sensitive to age of the child, and that these guidelines were revised substantially upwards from the 2006-7 school year (whereas Afridi's data is from 2004), it is plausible in our opinion that the nutritional increment from the school meals program could be substantially greater even in comparison to the large increments that are documented in her study.

Combined with the vulnerability of the children due to a severe drought in early childhood, and an extensive literature documenting that these negative effects can be quantitatively very large, we view the large cushioning impacts of the Midday Meals as plausible. Our result, documented in the last section that the effect seems to be most conspicuous in the sub-sample of children who were not stunted/underweight in 2002 also agrees with the literature presented above; as Adair (1999) documents in the Philippines, the likelihood of recovery from stunting is lowest for those who were stunted in the first year of their birth; similarly Crookston et. al. (2010) also note that catch-up seemed to be most likely for those who had higher height-for-age scores at initial assessment.

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, at least for young children just entering school. In a context where preschool nutritional programmes, most notably the Integrated Child Development Scheme, face major weakness in delivery and have not been able to universalize access such a role is important[33].

Our findings in this paper resonate with the opinion of Alderman and Bundy (2011) who conclude in a recent review article that it is quite likely that "Food For Education is a plausible candidate for social protection on par with Conditional Cash Transfer Programmes". Taken in conjunction with findings from the literature reported in Alderman and Bundy (2011), our results could be taken to support a broader program of individually targeted food transfers to young children in areas suffering from various shocks.

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

[33] Note that our results do not imply that preschool feeding would not be as effective or perhaps even more effective since it targets children at younger ages. This is an important point, stressed in relation to school feeding in India and elsewhere by both Haddad (2011) and Alderman and Bundy (2011), which needs to be considered explicitly in deriving policy implications from this study.

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.

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Figure 1. Proportion of Children Enrolled by Month of Birth

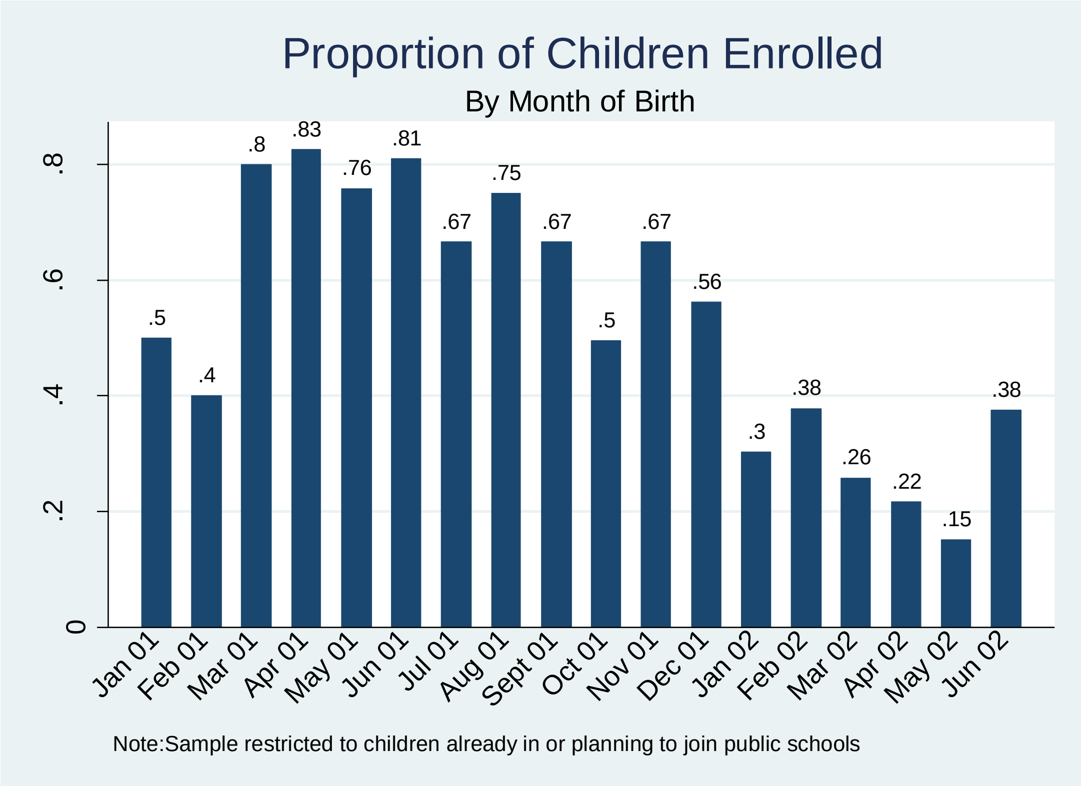

Table 1. Descriptive Statistics

VARIABLES	Treatment Group	Restricted Comparison Group	All non-beneficiaries	Total
Male	0.511 (0.5)	0.506 (0.5)	0.551 (0.498)	0.537 (0.499)
Urban	0.058 (0.233)	0.103*** (0.304)	0.347*** (0.476)	0.244 (0.43)
Drought	0.34 (0.474)	0.386* (0.487)	0.244*** (0.43)	0.278 (0.448)
Wealth index (2002)	0.327 (0.161)	0.312 (0.16)	0.45*** (0.21)	0.406 (0.203)
Scheduled Castes	0.23 (0.421)	0.211 (0.408)	0.158** (0.365)	0.184 (0.387)
Scheduled Tribes	0.212 (0.409)	0.164** (0.371)	0.112*** (0.315)	0.147 (0.354)
Backward Classes	0.439 (0.497)	0.515** (0.5)	0.479 (0.5)	0.465 (0.499)
Other Castes	0.119 (0.325)	0.11 (0.313)	0.252*** (0.434)	0.205 (0.404)
Telangana region	0.279 (0.449)	0.375*** (0.485)	0.389*** (0.488)	0.35 (0.477)
Rayalaseema region	0.344 (0.475)	0.295* (0.456)	0.277*** (0.448)	0.301 (0.459)
Coastal Andhra Pradesh	0.377 (0.485)	0.33* (0.471)	0.334* (0.472)	0.349 (0.477)
Height-for-age z score (2002)	-1.351 (1.461)	-1.597*** (1.53)	-1.27 (1.487)	-1.298 (1.478)
Weight-for-age z-score (2002)	-1.621 (1.064)	-1.843*** (1.167)	-1.504** (1.158)	-1.546 (1.127)
Height-for-age z-score(2007)	-1.645 (0.83)	-2.1*** (0.96)	-1.66 (1.068)	-1.655 (0.989)
Weight-for-age z-score (2007)	-1.879 (0.854)	-2.181*** (0.865)	-1.859 (0.977)	-1.866 (0.935)
Age (in years)	5.5 (0.276)	5.29*** (0.324)	5.343*** (0.334)	5.399 (0.323)
N	695	536	1255	1950

Note: This table presents means of variables by group; standard deviations in parentheses.

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

Table 2: ATT from OLS regressions on the treatment binary

	Full public school sample		Restricted to drought-affected		Children not drought-affected	
VARIABLES	(1) Changes in weight-for-age	(2) Changes in height-for-age	(3) Changes in weight-for-age	(4) Changes in height-for-age	(5) Changes in weight-for-age	(6) Changes in height-for-age
Midday Meals	0.074	0.20**	0.23***	0.43**	-0.028	0.034
	(1.32)	(2.18)	(3.41)	(2.85)	(-0.49)	(0.39)
Constant	-0.33***	-0.48***	-0.51***	-0.95***	-0.22***	-0.19***
	(-5.81)	(-3.14)	(-9.19)	(-4.07)	(-3.63)	(-3.05)
Observations	1,215	1,193	436	422	779	771
R-squared	0.002	0.006	0.017	0.024	0.000	0.000

Robust t-statistics in parentheses

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

Table 3. Estimated Impact of Midday Meals on children's nutrition

	(1)	(2)	(3)	(4)
	Weight-for-age in 2006/7		Height-for-age in 2006/7	
VARIABLES	OLS	IV	OLS	IV
Midday Meals	0.068*	0.31	0.15**	-0.17
	(1.67)	(1.31)	(1.98)	(-0.52)
Drought	-0.23***	-0.44***	-0.33***	-0.77***
	(-3.43)	(-4.00)	(-5.71)	(-3.86)
Age expressed in years	-0.091	-0.32***	0.38***	0.38*
	(-1.12)	(-2.69)	(2.71)	(1.73)
MDMS x Drought	0.21***	0.62***	0.19***	0.98**
	(2.81)	(2.73)	(3.21)	(2.39)
Weight-for-age in R1	0.61***	0.58***		
	(7.62)	(7.89)		
Height-for-age in R1			0.58***	0.58***
			(5.25)	(5.70)
Constant	-0.49	0.55	-2.96***	-2.81***
	(-1.33)	(1.03)	(-4.54)	(-2.82)
Observations	1,199	1,199	1,178	1,178
R-squared	0.380	0.339	0.219	0.180
Kleibergen-Paap F-statistic	31.7	8.00	11.4	8.15
Hansen J-statistic p value	0.71	0.27	0.36	0.17

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.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Drought happened in the last 4 years				**Drought happened 18+ months ago**				**Drought happened in last 13 months**			
VARIABLES	Weight-for-age (2007)		Height-for-age (2007)		Weight-for-age (2007)		Height-for-age (2007)		Weight-for-age (2007)		Height-for-age (2007)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Midday Meals	0.064	0.41	0.083	-0.15	0.11**	0.45**	0.14	0.0035	0.14***	0.67***	0.22***	0.35
	(1.11)	(1.50)	(0.87)	(-0.45)	(2.46)	(2.25)	(1.59)	(0.013)	(2.74)	(2.86)	(2.79)	(0.86)
Drought	-0.046	-0.10	-0.30***	-0.55***	-0.066	-0.23*	-0.25**	-0.57**	0.011	0.15	0.030	0.11
	(-0.69)	(-0.71)	(-2.95)	(-2.74)	(-0.77)	(-1.65)	(-2.14)	(-2.40)	(0.094)	(0.98)	(0.23)	(0.55)
Age expressed in years	-0.085	-0.33***	0.41***	0.37*	-0.079	-0.33***	0.40***	0.37	-0.090	-0.35***	0.39***	0.33
	(-1.02)	(-2.68)	(3.00)	(1.78)	(-0.93)	(-2.74)	(2.88)	(1.62)	(-1.09)	(-2.67)	(2.93)	(1.47)
MDMS x Drought	0.14**	0.29	0.21**	0.64**	0.083	0.45*	0.19*	0.79**	0.073	-0.19	-0.0088	-0.14
	(2.15)	(1.33)	(2.14)	(2.12)	(1.19)	(1.86)	(1.65)	(2.04)	(0.85)	(-0.90)	(-0.074)	(-0.50)
Weight-for-age in R1	0.61***	0.57***			0.61***	0.58***			0.61***	0.57***		
	(7.52)	(7.21)			(7.81)	(7.87)			(7.78)	(7.50)		
Height-for-age in R1			0.58***	0.56***			0.57***	0.57***			0.58***	0.57***
			(5.44)	(5.67)			(5.45)	(5.57)			(5.76)	(6.27)
Constant	-0.54	0.56	-3.05***	-2.75***	-0.58	0.56	-3.08***	-2.80**	-0.55	0.50	-3.12***	-2.89***
	(-1.42)	(0.98)	(-4.74)	(-2.83)	(-1.52)	(1.00)	(-4.68)	(-2.52)	(-1.47)	(0.84)	(-4.59)	(-2.71)
Observations	1,199	1,199	1,178	1,178	1,199	1,199	1,178	1,178	1,199	1,199	1,178	1,178
R-squared	0.376	0.342	0.211	0.220	0.374	0.318	0.219	0.194	0.377	0.328	0.204	0.216
Kleibergen-Paap F-statistic	35.1	7.13	11.6	10.0	37.5	8.86	13.0	11.0	39.4	6.05	13.1	12.2
Hansen J-statistic p value	0.91	0.46	0.40	0.31	0.93	0.56	0.47	0.53	0.86	0.49	0.46	0.38

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.

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	-0.25***	0.044	-0.042	-0.13
	(-4.67)	(1.68)	(-0.26)	(-0.65)
Born2002 x Drought	-0.047	-0.42***	0.088	0.48*
	(-0.76)	(-9.02)	(0.74)	(1.96)
perception of child's size at birth	-0.018*	-0.0049	-0.29***	-0.21***
	(-1.76)	(-0.55)	(-7.70)	(-3.31)
death/reduction of household members	-0.19*	-0.16*	-0.39	-0.35
	(-2.05)	(-1.82)	(-1.65)	(-1.63)
in last 4yrs has hh suffered drought	-0.017	0.68***	0.052	0.091
	(-0.50)	(18.0)	(0.79)	(0.58)
Age expressed in years	0.23**	0.080	-0.56**	-0.72**
	(2.77)	(1.56)	(-2.54)	(-2.53)
Constant	-0.53	-0.40	2.75**	3.29**
	(-1.13)	(-1.44)	(2.20)	(2.16)
Observations	1,215	1,215	1,200	1,184
R-squared	0.174	0.521	0.134	0.167

Robust t-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 2007 OLS	(2) IV	(3) Height-for-age in 2007 OLS	(4) IV
Midday Meals	0.063*	0.70	0.070	0.10
	(1.78)	(1.51)	(1.39)	(0.23)
in last 4yrs has hh suffered drought	-0.16***	-0.24	-0.28***	-0.57***
	(-4.38)	(-1.51)	(-4.15)	(-2.99)
MDMS x Drought	0.14***	0.30	0.14**	0.80*
	(2.88)	(0.80)	(2.32)	(1.70)
Age expressed in years	-0.056	-0.30**	0.40***	0.30*
	(-1.15)	(-1.97)	(4.79)	(1.81)
wealth index	0.34***	0.53***	0.39*	0.43**
	(2.62)	(3.23)	(1.85)	(2.36)
Weight-for-age in R1	0.65***	0.62***		
	(11.0)	(10.1)		
Height-for-age in R1			0.57***	0.55***
			(6.96)	(7.25)
Constant	-0.68**	0.27	-3.09***	-2.57***
	(-2.24)	(0.40)	(-6.87)	(-3.57)
Observations	1,900	1,900	1,874	1,874
R-squared	0.402	0.309	0.266	0.248
Kleibergen-Paap F-statistic	50.7	5.11	24.3	4.36
Hansen J-statistic p value	0.86	0.67	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.

Appendix 3: Results using site-averaged drought measure

VARIABLES	(1) Weight-for-age in 2007 OLS	(2) IV	(3) Height-for-age in 2007 OLS	(4) IV
Midday Meals	0.068*	0.31	0.15**	-0.17
	(1.67)	(1.31)	(1.98)	(-0.52)
MDMS x Drought (Village-level average)	0.21***	0.62***	0.19***	0.98**
	(2.81)	(2.73)	(3.21)	(2.39)
Drought (Village-level average)	-0.23***	-0.44***	-0.33***	-0.77***
	(-3.43)	(-4.00)	(-5.71)	(-3.86)
Age expressed in years	-0.091	-0.32***	0.38***	0.38*
	(-1.12)	(-2.69)	(2.71)	(1.73)
Weight-for-age in R1	0.61***	0.58***		
	(7.62)	(7.89)		
Height-for-age in R1			0.58***	0.58***
			(5.25)	(5.70)
Constant	-0.49	0.55	-2.96***	-2.81***
	(-1.33)	(1.03)	(-4.54)	(-2.82)
Observations	1,199	1,199	1,178	1,178
R-squared	0.380	0.339	0.219	0.180
Kleibergen-Paap F-statistic	31.7	8.00	11.4	8.15
Hansen J-statistic p value	0.71	0.27	0.36	0.17

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.