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

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I. 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 schoolchildren by 2006 (Khera 2006), the Midday Meal Scheme (MDMS) 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 enrollment and attendance. Additionally, it was envisioned that the program would reduce undernourishment among schoolchildren.[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

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[1] The full text of the court orders in this regard is available at http://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 breakdown of caste barriers by children of different castes eating together.

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indeed improve the immediate nutritional intake of children (Jacoby 2002; Afridi 2010), 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 MDMS 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 capital outcomes in developing countries (e.g., Maccini and Yang 2009; see also the discussion in Strauss and Thomas [2007] and Ferreira and Schady [2009]). 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 a midday meal even during school vacations, which clearly reflects 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 article addresses these gaps in the existing literature. Using a recent longitudinal data set from the state of Andhra Pradesh (AP), we assess the impact of MDMS on the health status of children in primary schools. We aim to assess whether the 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 that 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 Young Lives in AP; details about the sample, and AP, are presented in Section III. The survey collected extensive information about children in two cohorts (born in 1994/95 and 2001/2, respectively) in 2002 and 2007. The MDMS school feeding program in India was introduced in the state in January 2003. In this study, we focus exclusively on the younger cohort of children. These children were born during the 18 months from January 2001 to June 2002; their average age was about 12 months in round 1 and about 5.5 years in round 2. At the time of the second round of the survey, children

[3] The Vermeersch and Kremer (2004) study focused on children of preschool age in Kenya. 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.

in our treatment group would have received the school meals for an average duration of 9 months.

The period between the two rounds of the survey coincided with severe 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, after the failure of the monsoon rains between July and September 2002. This drought was nationwide 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 AP, albeit to a smaller extent than in 2002.[5] Children in our sample, on average, would have been around 1 year of age at the onset of the 2002–3 drought and about 3 years of age at the start 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 use a nonlinearity in enrollment induced by a change in the calendar year of birth: this strongly affects the probability of enrollment, as it is used as a "rule of thumb" to determine the appropriate time for enrollment, but should not directly affect 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 data set, even though the treatment and comparison groups are only about 2 months apart in age on average, because data collection was carried out just as decisions on school enrollment were being made for the younger cohort children (who were between 4.5 and 6 years old in the second round); the nonlinearity 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 IV.

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 but that these negative effects are entirely compensated for by the MDMS. We present evidence that the negative effect of droughts (and the compensatory positive effect of the midday meals) is only present for droughts that had happened at least 18 months before the second round of

[4] The drought of 2002 was covered in great detail in the national and state media in 2002 and 2003. See, e.g., *Frontline* (2002), Kumar (2002), *Financial Express* (2003), *Hindu* (2003), and *Times of India* (2003). It was also reported in the international press, e.g., 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, e.g., *Financial Express* (2004) and Mukherjee and Chakraborty (2004).

data collection (i.e., no later than 2005); this, in our opinion, is a reflection of the particular severity of the 2002–3 drought. Since most children receiving the school meals would have enrolled in mid-2006, our results indicate that the major channel through which the compensatory effect of midday meals is realized is catch-up growth. We also show that the main results are robust to different identification and measurement strategies (Sec. V).

The magnitude of the effects of the MDMS program is very large: for boys age 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 SD, which roughly equals the distance between the 25th and 50th percentiles, and a weight loss of about 0.44 SD, which equals two-thirds of the same distance, in the World Health Organization 2007 growth charts (Onis et al. 2007); our results suggest that this entire gap is compensated for by the program.

While similarly large estimates of the negative impacts of shocks to the household on the anthropometric *z*-scores of children have previously been observed in the literature,[6] the large magnitude of the compensatory impacts of the MDMS 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-age, remediation for which is often not considered possible after the first 24–36 months of a child's life.

Catch-up growth in height-for-age has, however, been observed in large magnitudes in several countries. Our findings are broadly consistent with the medical literature on nutrition and supplementary feeding, which documents that while 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, e.g., Tanner 1981; Golden 1994; Coly et al. 2006). In motivating our empirical tests, we note that not only has catch-up growth in height-for-age been documented at these ages and later in childhood in different contexts such as the Philippines, Senegal, and Peru, but the magnitude of catch-up is frequently observed to be larger than the effect sizes we report in this article.

This article makes several new contributions to the literature. It is the only econometric evaluation, to our knowledge, of the effect of India's MDMS 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

[6] For example, Akresh, Verwimp, and Bundervoet (2011) document a fall of 0.86 SD in height-for-age for girls as a result of crop failure and a fall of 1.05 SD in height-for-age for children who suffered from exposure to the Rwandan civil war.

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 in which child malnutrition is very widespread;[7] 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.

II. The Midday Meal Scheme in India

The MDMS is among the most important initiatives 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 containing no less than 300 kilocalories and 8–12 grams of protein.[8]

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

AP started providing midday meals in January 2003 to children in all primary and upper primary public and private-aided schools.[9] As several studies document, this scheme was nearly universal from the very beginning. Drèze and Goyal (2003) report full implementation of the MDMS in 2003 in AP. 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.[10]

Much interest was generated in the performance of the MDMS after 2001, when the issue entered the mainstream political and media discourse in India.

[7] See Haddad (2011), e.g., 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.

[8] From the 2006–7 school year, which is the relevant school year for our study, the minimum nutritional standards were revised upward to at least 450 kilocalories 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.

[9] Private-aided schools are run under private management but receive government funding and support, have access to government schemes like the MDMS, and follow the same regulations, including those for pay and tenure, 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 AP, about 4% according to Mehta (2007).

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

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 noneconometric 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 completed during 2003–5 focusing on MDMS and reviews their major findings. In general, the surveys focused on the effect of the scheme on enrollment, attendance, and retention as well as aspects of infrastructure change, caste discrimination, and opinions of stakeholders (teachers and parents). The surveys were almost unanimous in documenting a rise in attendance rates as well as enrollment 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, noting large benefits in school participation especially for girls. Jayaraman and Simroth (2011) document a 13% increase in enrollment in response to the MDMS, identifying the effects from timing differences in the rollout 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 randomized 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%" (152). However, the question that we are interested in, namely, that of the longer-term impact of the MDMS on child health and its role as a safety net, has not been directly addressed by any previous study.

III. Data and Background

The data we use in this study were collected by the Young Lives study between September and December 2002 and between January and June 2007 in the state of Andhra Pradesh.[11] AP 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 subdistricts (*mandals*), which are the primary sampling units for the survey.

[11] About 94% of the interviews in round 2 were carried out between January and 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.

The surveys cover two cohorts: the first is composed of 2,011 children born between January 2001 and June 2002, and the second includes 1,008 children born between January 1994 and June 1995. In the second round conducted in 2007, 1,950 children of the younger cohort and 994 children of the older cohort were successfully resurveyed; attrition rates thus are low and do not pose a problem for the analysis.[12] In this article, for reasons of program identification, we focus exclusively on the younger cohort.[13]

The period between 2001 and 2007 saw severe drought in several parts of the state, especially during 2002–3. In these years, districts in our sample saw a severe shortfall 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 food grain production in AP was a quarter below the "normal" production in both of the main agricultural growing seasons in the state.[14] The drought was especially severe in the Rayalaseema and Telangana regions, which are particularly drought prone.

Figure 1 presents a timeline of the major events in our data and the age of the younger cohort children at the time. The children were born in an 18-month age range between January 2001 and June 2002; failure of the monsoon rains between July and September 2002 led to a severe drought; the first round of fieldwork was carried out between September and December 2002; the 2006/7 school year began in mid-June 2006, which marks the beginning of treatment for the program beneficiaries in our sample; and, finally, the second round of data collection was carried out between January and April 2007. The figure presents the age of the median child as well as the range of ages at these points.

In this article, we evaluate whether access to school-provided meals between mid-June 2006, when the children were between 4 and 5.5 years of age, and the time of the survey in 2007 had any direct impact on the anthropometric indicators of these children; further, we investigate whether participation in the program in 2006/7 compensated for negative health shocks caused by a severe

[12] For greater details about the representativeness of the Young Lives sample, as compared to the Demographic and Health Survey (National Family Health Survey, 1998–99) sample for AP, as well as details of attrition, please refer to Kumra (2008).

[13] 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. Ordinary least squares (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.

[14] These figures are based on data from the Directorate of Economics and Statistics, Government of Andhra Pradesh, and were retrieved from http://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.

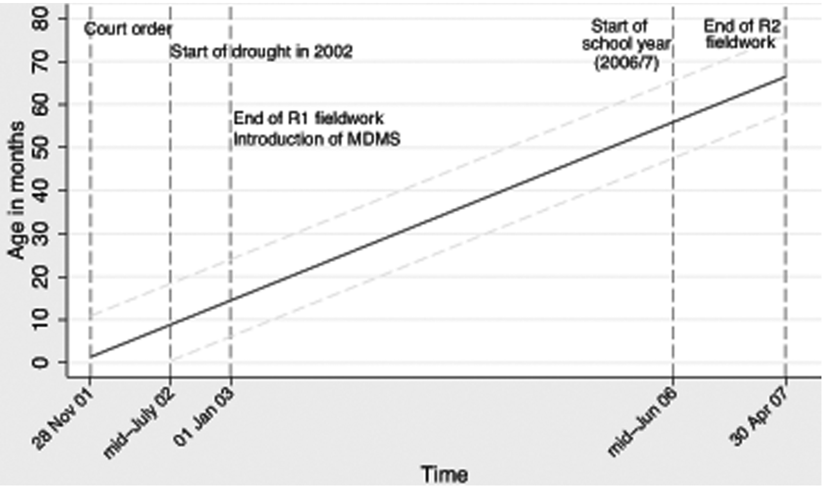

Figure 1. Timeline of events by age of children. Solid line shows age of median child; dotted lines show range in age. Color version available as an online enhancement.

drought in 2002–3 that had affected these children in infancy. The severe drought predates the school feeding by over 3 years, and thus any impacts that we find of the compensatory effects of school feeding must be operating through the channel of catch-up growth. 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 duration of about 9 months, with a minimum of 7 months and a maximum of 1 year. 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.

Our goal is to investigate whether a large nutritional intervention, the MDMS, could compensate for early nutritional deprivations caused by drought in the years before the intervention. The data set has several strengths for our purposes. First, it covers just the right period: the first round was in 2002 just before the program was implemented in AP, in January 2003, and the second round was in 2007, long enough for the scheme's teething problems to have been resolved. The period spanned a severe drought, making the data suitable for understanding the impact of midday meals in cushioning the impact of drought. Second, the longitudinal nature of the data helps greatly in dealing with problems in estimation and identifying impact. Third, the age of the younger cohort was exactly around the normal time of school enrollment; as we discuss later, 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.

IV. Framework and Methodology

Overview

Following Behrman and Deolalikar (1988), Senauer and Garcia (1991), and Behrman and Hoddinott (2005), we conceive of 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 production function of the form

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

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 recent 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 that 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 MDMS, 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 the Philippines. Following equation (1), we estimated the following two equations:

$$H_{i2} = \alpha + \beta_1 \times \text{MDMS}_{i2} + \beta_2 \times \text{Drought}_i + \beta_3 \times H_{i,1} + \beta_4 \times C_i + \varepsilon_i. \tag{2}$$

$$\begin{aligned} H_{i2} = \alpha + \beta_1 \times \text{MDMS}_{i2} + \beta_2 \times \text{Drought}_i + \beta_3 \\ \times \text{MDMS}_{i2} \times \text{Drought}_i + \beta_4 \times H_{i,1} + \beta_5 \times C_i + \varepsilon_i. \end{aligned} \tag{3}$$

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 a household wealth index.[15] All variables in C_i

[15] We will show that our conclusions are robust to using alternative measures of drought. We will also show later, using information on the timing of the drought, that our results are not driven by contemporaneous droughts but reflect catch-up growth in those children who had suffered health deterioration from drought in early childhood.

are from round 1 (2002). Equation (2) presents the average treatment effect for the sample. Equation (3) allows for an interaction effect between MDMS and drought that will permit us to explicitly investigate the role of the program as a safety net.

In considering the dynamic processes that determine health outcomes modeled in equations (2) and (3), it is important to keep in mind the timing of events and measurements described in Section III. One would probably expect that less than 1 year of exposure to a school meal program at age 4–5.5 years would have a larger influence on children's weight-for-age than on children's height-for-age, given that weight is usually more responsive to changes in nutrient intake in the short run.

However, predictions become more complicated when one considers the possibility for catch-up growth and how it may be related to previous nutritional deprivation. For a nutrition intervention at age 4–5.5 to reverse insults to nutrition caused by a major drought that started when the children were less than 2 years old, there must be biological evidence that catch-up growth under such circumstances is possible. Since height is a longer-term measure of health compared to weight, droughts that occurred some years ago may have a more pronounced impact on height than weight, providing more scope for compensating effects of a nutritional intervention. In contrast, recent or current droughts may have a larger impact on weight.

In fact, the nutritional literature presents a number of examples of such catch-up growth. While early growth deficits often persist into adulthood, it is not established that this is a biological necessity: as Golden (1994) hypothesizes, "the available data could be interpreted to show that a period of malnutrition in the first 2–3 years 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" (quoted in Boersma and Wit 1997, 649).

Furthermore, the magnitude of catch-up in the children who recover from previous health deprivations is frequently very large. Adair (1999), for example, 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 improvements in height-for-age *z*-scores of 1.14 SD. Similarly, Crookston et al. (2010), analyzing Young Lives data from Peru that compare children of exactly the same age in 2002 and 2006/7 as in our sample, 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 SD. Coly et al. (2006) document a similar magnitude of catch-up in Senegal, with large changes for those stunted at preschool: using World Health Organization norms, they find mean height-for-age *z*-score increases of 0.21, 0.90, and 1.79 SD for girls, and 0.31, 0.95, and 1.44 SD for boys, with no stunting, mild stunting, and marked stunting, respectively.

Another relevant result from the biological literature on catch-up growth is that it may be more difficult for children who suffer severe undernutrition in the first years of life. Adair (1999) documents in the Philippines that 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) note that catch-up seemed to be most likely for those who had higher height-for-age scores at initial assessment. We can also test whether, in our study sites in India, catch-up growth differs between children whose growth is stunted early in life and those whose early growth is not stunted.

Identification

Of the children in the younger cohort, who were between 4.5 and 6 years old in 2007, about 45% were in school by the second wave. Of these students, 79% were in public schools, and the rest were in private schools (including those run by nongovernmental organizations 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.) their child would be likely to join and the age at which they thought the child would be enrolled. The caregivers of over 95% of the children not yet enrolled report that they expect the child to be in school by age 6 years.[16]

In the data, only 1.47% of caregivers of the children enrolled in public schools (10 out of 682) report that their school does not provide a midday meal, thus confirming the widespread implementation of the program indicated by previous studies.[17] We therefore define the treatment group as all children currently attending public school.[18] Our results are not driven by the

[16] 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.

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

[18] The caregivers of about 98.5% of children in public schools report that the school provides the meals, indicating the 10 cases of reported nonavailability of food may either reflect temporary unavailability or the caregiver's lack of knowledge about whether the child receives the meal.

assumption that all children in public schools receive the meal; such an assumption should indeed bias our results toward finding a weaker impact of the program. 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 nonrandom program placement is the endogeneity of treatment (enrolling in a public school), especially via 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 MDMS in deciding whether and at what age to enroll 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 enroll 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 analysis, we use 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 soon; thus, our preferred specification compares only children currently in public schools to children who will go to public schools in the future.[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 and our (restricted) comparison group of children who will join public schools in the future. 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 our preferred (restricted) comparison group,

[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 AP because nearly all children in the state go to primary school. For instance, even in the first round of data collection (before the introduction of the MDMS), over 97% of the children in the older cohort, then age 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 enroll 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 enrollment 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.

TABLE 1
DESCRIPTIVE STATISTICS

Variable	Treatment Group	Comparison Group	Total
Male	.511	.506	.509
	(.5)	(.5)	(.5)
Urban	.058	.103***	.077
	(.233)	(.304)	(.267)
Drought	.34	.386*	.36
	(.474)	(.487)	(.48)
Wealth index (2002)	.327	.312	.32
	(.161)	(.16)	(.161)
Caregiver's education	1.87	2.429	2.185
	(3.02)	(5.667)	(4.708)
Household size	5.621	5.427	5.512
	2.435	2.424	2.43
Scheduled castes	.23	.211	.222
	(.421)	(.408)	(.416)
Scheduled tribes	.212	.164**	.191
	(.409)	(.371)	(.393)
Backward classes	.439	.515**	.472
	(.497)	(.5)	(.499)
Other castes	.119	.11	.115
	(.325)	(.313)	(.32)
Telangana region	.279	.375***	.321
	(.449)	(.485)	(.467)
Rayalaseema region	.344	.295*	.323
	(.475)	(.456)	(.468)
Coastal Andhra Pradesh	.377	.33*	.357
	(.485)	(.471)	(.479)
Height-for-age *z*-score (2002)	−1.351	−1.597***	−1.457
	(1.461)	(1.53)	(1.495)
Weight-for-age *z*-score (2002)	−1.621	−1.843***	−1.717
	(1.064)	(1.167)	(1.115)
Height-for-age *z*-score (2007)	−1.645	−2.1***	−1.844
	(.83)	(.96)	(.917)
Weight-for-age *z*-score (2007)	−1.879	−2.181***	−2.011
	(.854)	(.865)	(.872)
Age (years)	5.5	5.29***	5.41
	(.276)	(.324)	(.315)
N	695	536	1,231

Note. Means of variables by group; standard deviations in parentheses.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

rather than when we compare our treatment group to all children not currently receiving the school meals.[21]

Using this sample, our treatment and comparison groups are mainly differentiated by whether they have enrolled in school; as we discussed earlier, it is

[21] The comparison group of all children not currently enrolled in public schools (and thereby not receiving the meals) is presented in table A1.

plausible that this decision is endogenous and affected by the availability of the MDMS.[22] To address endogeneity problems caused by self-selection in enrollment, we adopt an IV approach. The requirement for an IV in this case is that it should be able to predict enrollment in school at the time of the survey, in this sample of children who are either in public schools already (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 nonlinearity in the relationship between age and enrollment induced by a change in the calendar year of birth; this nonlinearity affects the probability of enrollment 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 enrollment but not nutrition, at the same time controlling for the linear effects of age.[23] The intuition behind our use of this variable as an IV is straightforward. 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 enrollment between December 2001 and January 2002. That this nonlinearity is empirically important can be seen clearly in figure 2, which plots mean enrollment rates by month of birth. The proportion of children enrolled drops nearly in half from 56% of children born in December 2001 to 30% of those born in January 2002. Although there is noise in the month-to-month variation in enrollment rates, there is a sharply more negative relationship between birth month and enrollment rate in the months around the end of 2001.[24] This nonlinear-

[22] This concern ties in directly with the theoretical framework in which 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 enrollment as well, OLS estimates of the treatment effect would be biased. This concern prompts us to use an IV approach.

[23] Age was calculated on the basis of 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 2 months.

[24] That small differences in age are associated with large differences in enrollment, as in fig. 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 enrollment are being taken; at any other age outside this narrow window, we would expect to see no variation in enrollment induced by age differences of only 2–3 months. The lower rate of enrollment for children born between January and March 2001 seems puzzling but is explained by the fact that only 28 (~2%) children in the data set out of 1,231 children who are in or will join public schools were born in this period. Similarly, only eight children are born in June 2002,

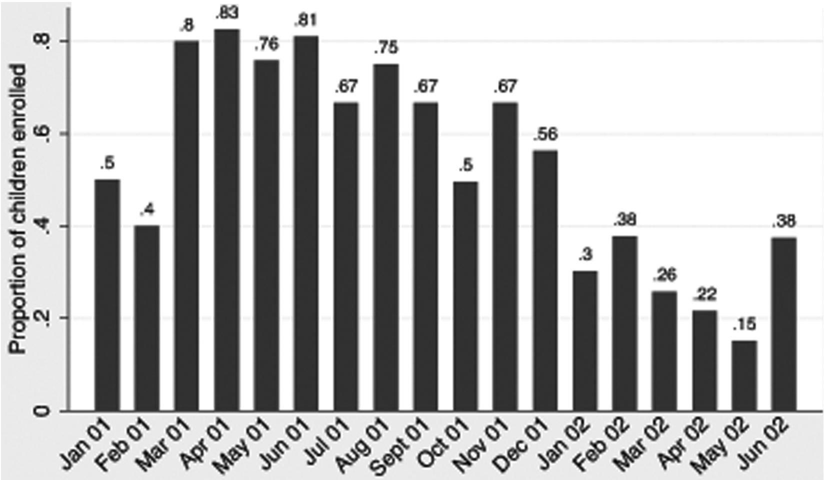

Figure 2. Proportion of children enrolled by month of birth. Sample restricted to children already in or planning to join public schools. Color version available as an online enhancement.

ity 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 enrollment.[25] When other months are chosen as the threshold point for changes in enrollment probability, they are much weaker and usually lack statistical significance, suggesting that the nonlinearity in the relationship between time of birth and enrollment 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 enrollment rates.

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

$$\mathrm{MDMS}_{i2} = \mu + \pi_1 \mathrm{Born2002} + \pi_2 \mathrm{Age} + \pi_3 Z + \varepsilon_i, \tag{4}$$

where Born2002 is an indicator variable for being born in 2002, equaling zero if the child was born in 2001; Age is age at the time of the survey measured in

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

[25] Five years is the prevailing norm for the age of enrollment into public schooling in AP. 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.

years with daily precision; and Z is a vector of exogenous variables including all exogenous covariates in the second-stage equation (eq. [3]), 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 that is used as an IV for the interaction term between MDMS and Drought.[26] The exclusion restriction on the IV would be violated if a change in the calendar year of birth had a nonlinear impact in this age range, not only on the probability of enrollment 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 nonenrolled groups are very close in mean age.

Furthermore, any general nonlinear impacts of age should not be confined to the impacts of the scheme on drought-affected children but on the group of beneficiaries as a whole. Our results, however, indicate that the entire benefit of the MDMS 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 2002–3 drought. Children born after year-end 2001 were younger when the drought began to create hardship in the second half of 2002 (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 be not only nonlinear but nonlinear around the specific threshold of 6 months.

A possible 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 enroll in private schools. Since these children did not have access to the MDMS, finding any nonlinear effects of age on nutritional outcomes would provide evidence against our identifying assumptions. As we describe in the next section, we do not find any evidence to this effect.

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

[26] 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.

is related to conditions during pregnancy and is very strongly correlated with a child's health in the first 18 months of life. The instruments are appealing because they are predetermined 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 a remaining correlation with unobserved household characteristics that affect current child health.[27]

V. Results

For descriptive purposes, we estimate the unconditional average treatment effect on the treated 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; 38.56% of households in rural areas in the restricted sample self-reported having been affected by drought between the two rounds.

Specifically, we estimated equations of the form

$$\Delta Y = \alpha + \beta_1 \mathrm{MDMS}_i + u. \tag{5}$$

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 0.23 SD for weight-for-age and 0.43 SD on height-for-age z-scores; there are no significant impacts on children who did not suffer from drought.

Next, we report the results of estimating (2), which captures the average impacts of treatment on the treated, not allowing for heterogeneous treatment effects. Then, we estimate (3) by adding an interaction between MDMS and an indicator variable for whether the child experienced drought in the previous 4 years. In both cases, we estimate the effect of MDMS both with and

[27] 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.

TABLE 2
UNCONDITIONAL TREATMENT EFFECT FROM OLS REGRESSIONS ON THE TREATMENT BINARY

	Full Public School Sample		Restricted to Drought Affected		Children Not Drought Affected	
Variable	Changes in Weight-for-Age (1)	Changes in Height-for-Age (2)	Changes in Weight-for-Age (3)	Changes in Height-for-Age (4)	Changes in Weight-for-Age (5)	Changes in Height-for-Age (6)
Midday meals	.074	.20**	.23***	.43**	−.028	.034
	(1.32)	(2.18)	(3.41)	(2.85)	(−.49)	(.39)
Constant	−.33***	−.48***	−.51***	−.95***	−.22***	−.19***
	(−5.81)	(−3.14)	(−9.19)	(−4.07)	(−3.63)	(−3.05)
Observations	1,215	1,193	436	422	779	771
R^2	.002	.006	.017	.024	.000	.000

Note. Robust *t*-statistics in parentheses.
** $p < .05$.
*** $p < .01$.

without the instrumentation of the MDMS dummy. 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).[28]

Results from estimating equation (2) are presented in table 3. Having suffered from drought between the two rounds has a significant negative impact on the weight-for-age and height-for-age z-scores in all specifications; the negative effect of drought on height-for-age is greater than on weight-for-age, which is consistent with the former being a longer-term measure of health and thus capturing the effect of past health deprivation. In all four regressions, the MDMS dummy has a positive impact; this effect is significant at the 1% level in both the uninstrumented and instrumented results for weight-for-age and for the uninstrumented results on height-for-age (but not when selection into treatment is accounted for). Standard errors in all regressions are clustered at the subdistrict level. Thus, according to the preferred IV specifications, the MDMS increased weight-for-age by 0.60 SD and increased height-for-age by 0.27 SD (not statistically significant), which is consistent with larger short-term impacts of changes in nutritional intake on weight.

[28] 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 and 12. First-stage results for the main specification (eq. [3]), which allows for heterogeneity of the effect of MDMS by drought incidence in the past, are presented in table A2.

TABLE 3
ESTIMATED IMPACT OF MIDDAY MEALS ON CHILDREN'S NUTRITION

	Weight-for-Age in 2006/7		Height-for-Age in 2006/7	
Variable	OLS (1)	IV[a] (2)	OLS (3)	IV[a] (4)
Midday meals	.15***	.60***	.22***	.27
	(3.56)	(3.07)	(2.95)	(.85)
Drought	−.11***	−.088**	−.22***	−.21***
	(−2.88)	(−2.05)	(−3.77)	(−3.57)
Age expressed in years	−.090	−.34***	.38***	.33
	(−1.10)	(−2.78)	(2.77)	(1.58)
Weight-for-age in R1	.61***	.57***		
	(7.52)	(7.30)		
Height-for-age in R1			.58***	.56***
			(5.24)	(5.77)
Constant	−.54	.53	−3.00***	−2.82***
	(−1.47)	(.93)	(−4.66)	(−2.83)
Observations	1,199	1,199	1,178	1,178
R^2	.378	.341	.220	.237
Kleibergen-Paap *F*-statistic	32.8	11.9	11.2	12.9
Hansen *J*-statistic *p*-value	.83	.40	.39	.30

Note. Robust *t*-statistics in parentheses. Standard errors are clustered at site level. Lagged anthropometric indicators are instrumented throughout, including in OLS (ordinary least squares) columns, using birth size and death of a household member during pregnancy as instruments. Base category = rural, female, other castes, coastal Andhra Pradesh, not drought affected. Coefficients on male, caste, urban and region dummies, caregiver's education, wealth index, and household size are not reported here. R1 = round 1.
[a] Instrument variable (IV) results correcting for self-selection using being born in 2002 as an instrument.
** $p < .05$.
*** $p < .01$.

These results, while documenting health deprivation as a result of the drought, do not provide any direct evidence of whether school meals are able to compensate for the effect of the past droughts in our sample. To answer this question, we next present the main estimation results based on estimation of equation (3), which allows for heterogeneous impacts of school meals on children whose households had suffered from drought between the two rounds. Table 4 presents the resulting estimates both with and without correcting for self-selection into the treatment.

As can be seen, having suffered from drought in the past 4 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 overidentification 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 compensatory effect of the MDMS is statistically significant across all selection-corrected estimates at the 5% level of significance. Results in this

TABLE 4

HETEROGENEOUS IMPACT OF MIDDAY MEALS ON DROUGHT-AFFECTED CHILDREN'S NUTRITION

	Weight-for-Age in 2006/7		Height-for-Age in 2006/7	
	OLS	IV[a]	OLS	IV[a]
Variable	(1)	(2)	(3)	(4)
Midday meals	.068*	.31	.15**	−.17
	(1.67)	(1.31)	(1.98)	(−.52)
Drought	−.23***	−.44***	−.33***	−.77***
	(−3.43)	(−4.00)	(−5.71)	(−3.86)
MDMS × drought	.21***	.62***	.19***	.98**
	(2.81)	(2.73)	(3.21)	(2.39)
Age expressed in years	−.091	−.32***	.38***	.38*
	(−1.12)	(−2.69)	(2.71)	(1.73)
Weight-for-age in R1	.61***	.58***		
	(7.62)	(7.89)		
Height-for-age in R1			.58***	.58***
			(5.25)	(5.70)
Constant	−.49	.55	−2.96***	−2.81***
	(−1.33)	(1.03)	(−4.54)	(−2.82)
Observations	1,199	1,199	1,178	1,178
R^2	.380	.339	.219	.180
Kleibergen-Paap *F*-statistic	31.7	8.00	11.4	8.15
Hansen *J*-statistic *p*-value	.71	.27	.36	.17

Note. Robust *t*-statistics in parentheses. Standard errors are clustered at site level. Lagged anthropometric indicators are instrumented throughout, including in OLS (ordinary least squares) columns, using birth size and death of a household member during pregnancy as instruments. Base category = rural, female, other castes, coastal Andhra Pradesh, not drought affected. Coefficients on male, caste, urban and region dummies, caregiver's education, wealth index, and household size are not reported here. MDMS = Midday Meal Scheme; R1 = round 1.

[a] Instrument variable (IV) results correcting for self-selection using being born in 2002 as an instrument.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

table suggest that the positive effect of the scheme on average, as documented in table 3, is concentrated among drought-affected children in the sample. For weight-for-age, the impact of the program on children not experiencing drought is positive but not statistically significant; for height-for-age the effect is negative and statistically insignificant. The heterogeneity in the effect of the school meals, thus, appears to be central to understanding the health benefits of the MDMS.

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 in which 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 that equals one if a household reports having suffered from drought in the past 4 years (i.e., between the two survey rounds) and zero otherwise. There could be systematic reporting bias in this variable that is correlated with time-varying unobservables that affect changes in nutrition. We do not think this likely to be a severe problem, 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 table A3 and display a very similar pattern of incidence of benefits from the MDMS.

To avoid self-reporting bias, we can also use reports of natural disasters from the community questionnaires collected at the same time as the household data. 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 4 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. Fifty out of 101 communities reported having been affected by drought in the past 4 years, of which 19 reported that the drought had happened in the last 13 months; all other communities reported the drought as having occurred at least 18 months ago.[29]

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 4 years instead of the self-reported drought measure, then using only a dummy variable for a drought in the last 13 months, and finally using only a dummy variable for a drought at least 18 months ago. Results from this analysis are presented in table 5. As can be seen, the effect of drought is negative (although not statistically significant for weight-for-age) in the first set of

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

TABLE 5

RESULTS USING COMMUNITY-LEVEL INFORMATION ON TIMING AND OCCURRENCE OF DROUGHT

	Drought Happened in the Last 4 Years				Drought Happened 18+ Months Ago				Drought Happened in Last 13 Months			
	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[a]	OLS	IV[a]	OLS	IV[a]	OLS	IV[a]	OLS	IV[a]	OLS	IV[a]
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Midday meals	.064	.41	.083	−.15	.11**	.45**	.14	.0035	.14***	.67***	.22***	.35
	(1.11)	(1.50)	(.87)	(−.45)	(2.46)	(2.25)	(1.59)	(.013)	(2.74)	(2.86)	(2.79)	(.86)
Drought	−.046	−.10	−.30***	−.55***	−.066	−.23*	−.25**	−.57**	.011	.15	.030	.11
	(−.69)	(−.71)	(−2.95)	(−2.74)	(−.77)	(−1.65)	(−2.14)	(−2.40)	(.094)	(.98)	(.23)	(.55)
MDMS × drought	.14**	.29	.21**	.64**	.083	.45*	.19*	.79**	.073	−.19	−.0088	−.14
	(2.15)	(1.33)	(2.14)	(2.12)	(1.19)	(1.86)	(1.65)	(2.04)	(.85)	(−.90)	(−.074)	(−.50)
Age expressed in years	−.085	−.33***	.41***	.37*	−.079	−.33***	.40***	.37	−.090	−.35***	.39***	.33
	(−1.02)	(−2.68)	(3.00)	(1.78)	(−.93)	(−2.74)	(2.88)	(1.62)	(−1.09)	(−2.67)	(2.93)	(1.47)
Weight-for-age in R1	.61***	.57***			.61***	.58***			.61***	.57***		
	(7.52)	(7.21)			(7.81)	(7.87)			(7.78)	(7.50)		

Height-for-age in R1			.58***	.56***			.57***	.57***			.58***	.57***
			(5.44)	(5.67)			(5.45)	(5.57)			(5.76)	(6.27)
Constant	−.54	.56	−3.05***	−2.75***	−.58	.56	−3.08***	−2.80**	−.55	.50	−3.12***	−2.89***
	(−1.42)	(.98)	(−4.74)	(−2.83)	(−1.52)	(1.00)	(−4.68)	(−2.52)	(−1.47)	(.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^2	.376	.342	.211	.220	.374	.318	.219	.194	.377	.328	.204	.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	.91	.46	.40	.31	.93	.56	.47	.53	.86	.49	.46	.38

Note. Robust *t*-statistics in parentheses. Standard errors are clustered at site level. Lagged anthropometric indicators are instrumented throughout, including in OLS (ordinary least squares) columns, using birth size and death of a household member during pregnancy as instruments. Base category = rural, female, other castes, coastal Andhra Pradesh, not drought affected. Coefficients on male, caste, urban and region dummies, caregiver's education, wealth index, and household size are not reported here. MDMS = Midday Meal Scheme; R1 = round 1.

[a] Instrument variable (IV) results correcting for self-selection using being born in 2002 as an instrument.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

results, which use a dummy variable for whether a drought had happened in the last 4 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 in which drought occurred in the last 13 months, and coefficients on neither drought nor its interaction with MDMS are significant. Finally, in the case in which 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. Thus, it appears that midday meals are compensating for the negative effects of the severe 2002–3 drought rather than the more recent, less severe drought. As noted previously, we believe this result is a reflection of the much greater severity of the 2002–3 drought than any other droughts in the study period and should not be taken as evidence that nutritional interventions have a greater role to play in addressing the effects of droughts only after some time has elapsed.

We also ran the analysis on a series of subsamples to better understand the pattern of nutritional benefits. In particular, we reran the analysis sequentially restricting the sample to rural children and by whether they were stunted/underweight in round 1 of the survey. We found that the results are driven entirely by the rural sample and by children who were not stunted/underweight in 2002.[30] This latter result, that the effect seems to be most conspicuous in the subsample of children who were not stunted/underweight in 2002, also agrees with the nutritional literature that finds that children stunted in the first years of life have more difficulty in achieving catch-up growth (Adair 1999; Crookston et al. 2010).

As described earlier in the discussion on identification (Sec. IV), one might be concerned that the positive interaction effect of drought and school meals (enrollment) is due to a nonlinear effect of the age of first exposure to drought. However, when we estimate our base specifications on the sample of children in rural areas who have enrolled in or plan to enroll 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 enrollment or estimate the direct effects of Born2002 interacted with Drought on child health outcomes after controlling linearly for Age and Age interacted with Drought.[31]

[30] Results are not presented here 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 the 10% level of significance and *F*-statistics also go down.

[31] 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 enroll in private schools typically would

While this provides suggestive evidence that our exclusion restriction is not violated by nonlinear effects of the age of exposure to drought, one might be still be concerned that parents of children enrolled in private school are wealthier than parents of children enrolled in public schools (and are thus able to smooth shocks better) and that the nonresult merely reflects this pattern. However, while households in the private schools subsample are wealthier on average, there is considerable overlap in the wealth index between households in the treatment group and the private school subsample in rural areas. Our nonresult 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 nonaffected households, even at the 10% level of significance, which reflects perhaps the severity of the 2002–3 drought.

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 2 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 sample children were nearly all less than 2 years of age when the drought 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 artifact of direct nonlinear 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 (see table A4). The results from the IV specifications are substantially similar when using the full sample.[32]

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spend a longer period (about 2 years) in preprimary kindergarten classes than children enrolling in government schools who make the transition from public preschools (*anganwadis*) to primary school earlier.

[32] 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 rigorously implemented in the public schooling sector than in private institutions that 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.

VI. Conclusion

Midday meals, as Afridi (2010) rigorously documents, 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, that official guidelines on the minimum nutritional content of school meals are not sensitive to the age of the child, and that these guidelines were revised substantially upward from the 2006–7 school year (whereas Afridi's data are 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.

These results also seem to make intuitive sense: children in drought-stricken areas see a decline in nutritional intake affecting their health negatively, but the MDMS 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 in which preschool nutritional programs, 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 article resonate with the opinion of Alderman and Bundy (2012), 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" (217). Taken in conjunction with findings from the literature reported in Alderman and Bundy (2012), 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 AP is one of the better performers among Indian states in service delivery generally, and in the MDMS in particular. The superior performance of the program in AP has been noted in both the academic literature (e.g., Drèze 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

[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 (2012), that needs to be considered explicitly in deriving policy implications from this study.

MDMS, unless the delivery mechanisms and political/administrative will can also be raised to similar levels.

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 AP, 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 seasonal variability in their food intake, 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. 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 policy makers working on health and education.

Appendix

TABLE A1
DESCRIPTIVE STATISTICS FOR TREATMENT GROUP, COMPARISON GROUP, AND ALL NONBENEFICIARIES

Variable	Treatment Group	Restricted Comparison Group	All Nonbeneficiaries	Total
Male	.511	.506	.551	.537
	(.5)	(.5)	(.498)	(.499)
Urban	.058	.103***	.347***	.244
	(.233)	(.304)	(.476)	(.43)
Drought	.34	.386*	.244***	.278
	(.474)	(.487)	(.43)	(.448)
Wealth index (2002)	.327	.312	.45***	.406
	(.161)	(.16)	(.21)	(.203)
Scheduled castes	.23	.211	.158**	.184
	(.421)	(.408)	(.365)	(.387)
Scheduled tribes	.212	.164**	.112***	.147
	(.409)	(.371)	(.315)	(.354)

TABLE A1 (*Continued*)

Variable	Treatment Group	Restricted Comparison Group	All Nonbeneficiaries	Total
Backward classes	.439	.515**	.479	.465
	(.497)	(.5)	(.5)	(.499)
Other castes	.119	.11	.252***	.205
	(.325)	(.313)	(.434)	(.404)
Telangana region	.279	.375***	.389***	.35
	(.449)	(.485)	(.488)	(.477)
Rayalaseema region	.344	.295*	.277***	.301
	(.475)	(.456)	(.448)	(.459)
Coastal Andhra Pradesh	.377	.33*	.334*	.349
	(.485)	(.471)	(.472)	(.477)
Height-for-age *z*-score (2002)	−1.351	−1.597***	−1.27	−1.298
	(1.461)	(1.53)	(1.487)	(1.478)
Weight-for-age *z*-score (2002)	−1.621	−1.843***	−1.504**	−1.546
	(1.064)	(1.167)	(1.158)	(1.127)
Height-for-age *z*-score (2007)	−1.645	−2.1***	−1.66	−1.655
	(.83)	(.96)	(1.068)	(.989)
Weight-for-age *z*-score (2007)	−1.879	−2.181***	−1.859	−1.866
	(.854)	(.865)	(.977)	(.935)
Age (years)	5.5	5.29***	5.343***	5.399
	(.276)	(.324)	(.334)	(.323)
N	695	536	1,255	1,950

Note. Means of variables by group; standard deviations in parentheses.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

TABLE A2
FIRST-STAGE RESULTS FOR ENDOGENOUS VARIABLES

			z-Score Round 1	
Variable	MDMS (1)	MDMS × Drought (2)	Weight-for-Age (3)	Height-for-Age (4)
Born in 2002	−.25***	.044	−.042	−.13
	(−4.67)	(1.68)	(−.26)	(−.65)
Born2002 × drought	−.047	−.42***	.088	.48*
	(−.76)	(−9.02)	(.74)	(1.96)
Perception of child's size at birth	−.018*	−.0049	−.29***	−.21***
	(−1.76)	(−.55)	(−7.70)	(−3.31)
Death/reduction of household members	−.19*	−.16*	−.39	−.35
	(−2.05)	(−1.82)	(−1.65)	(−1.63)
In last 4 years, has household suffered drought?	−.017	.68***	.052	.091
	(−.50)	(18.0)	(.79)	(.58)
Age expressed in years	.23**	.080	−.56**	−.72**
	(2.77)	(1.56)	(−2.54)	(−2.53)

TABLE A2 (*Continued*)

Variable	MDMS (1)	MDMS × Drought (2)	*z*-Score Round 1: Weight-for-Age (3)	*z*-Score Round 1: Height-for-Age (4)
Constant	−.53	−.40	2.75**	3.29**
	(−1.13)	(−1.44)	(2.20)	(2.16)
Observations	1,215	1,215	1,200	1,184
R^2	.174	.521	.134	.167

Note. Robust *t*-statistics in parentheses. Standard errors are clustered at site level. Base category = rural, female, other castes, coastal Andhra Pradesh, not drought affected. Coefficients on male, caste, urban and region dummies, caregiver's education, wealth index, and household size are not reported here due to space constraints. MDMS = Midday Meal Scheme.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

TABLE A3
RESULTS USING SITE-AVERAGED DROUGHT MEASURE

Variable	Weight-for-Age: OLS (1)	Weight-for-Age: IV[a] (2)	Height-for-Age: OLS (3)	Height-for-Age: IV[a] (4)
Midday meals	.057	.39	.090	−.18
	(1.29)	(1.61)	(1.01)	(−.52)
MDMS × drought (village average)	.24**	.52	.30**	1.00
	(2.00)	(1.59)	(2.01)	(1.57)
Drought (village average)	−.33***	−.40*	−.90***	−1.26***
	(−2.74)	(−1.94)	(−6.84)	(−4.05)
Age expressed in years	−.082	−.32***	.39***	.40*
	(−1.03)	(−2.62)	(2.93)	(1.84)
Weight-for-age in R1	.61***	.57***		
	(7.43)	(7.22)		
Height-for-age in R1			.57***	.56***
			(5.22)	(5.52)
Constant	−.53	.55	−2.98***	−2.85***
	(−1.48)	(.99)	(−5.09)	(−3.00)
Observations	1,199	1,199	1,178	1,178
R^2	.379	.345	.249	.248
Kleibergen-Paap *F*-statistic	33.5	8.05	11.1	8.47
Hansen *J*-statistic *p*-value	.72	.33	.21	.12

Note. Robust *t*-statistics in parentheses. Standard errors are clustered at site level. Lagged anthropometric indicators are instrumented throughout, including in OLS (ordinary least squares) columns, using birth size and death of a household member during pregnancy as instruments. Base category = rural, female, other castes, coastal Andhra Pradesh, not drought affected. Coefficients on male, caste, urban and region dummies, caregiver's education, wealth index, and household size are not reported here due to space constraints. MDMS = Midday Meal Scheme; R1 = round 1.
[a] Instrument variable (IV) results correcting for self-selection using being born in 2002 as an instrument.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

TABLE A4
ESTIMATES USING THE WHOLE SAMPLE

	Weight-for-Age in 2007		Height-for-Age in 2007	
	OLS	IV[a]	OLS	IV[a]
Variable	(1)	(2)	(3)	(4)
Midday meals	.063*	.70	.070	.10
	(1.78)	(1.51)	(1.39)	(.23)
Drought	−.16***	−.24	−.28***	−.57***
	(−4.38)	(−1.51)	(−4.15)	(−2.99)
MDMS × drought	.14***	.30	.14**	.80*
	(2.88)	(.80)	(2.32)	(1.70)
Age expressed in years	−.056	−.30**	.40***	.30*
	(−1.15)	(−1.97)	(4.79)	(1.81)
Wealth index	.34***	.53***	.39*	.43**
	(2.62)	(3.23)	(1.85)	(2.36)
Weight-for-age in R1	.65***	.62***		
	(11.0)	(10.1)		
Height-for-age in R1			.57***	.55***
			(6.96)	(7.25)
Constant	−.68**	.27	−3.09***	−2.57***
	(−2.24)	(.40)	(−6.87)	(−3.57)
Observations	1,900	1,900	1,874	1,874
R^2	.402	.309	.266	.248
Kleibergen-Paap F-statistic	50.7	5.11	24.3	4.36
Hansen J-statistic p-value	.86	.67	.25	.13

Note. Robust t-statistics in parentheses. Standard errors are clustered at site level. Lagged anthropometric indicators are instrumented throughout, including in OLS (ordinary least squares) columns, using birth size and death of a household member during pregnancy as instruments. Base category = rural, female, other castes, coastal Andhra Pradesh, not drought affected. Coefficients on male, caste, urban and region dummies, caregiver's education, wealth index, and household size are not reported here due to space constraints. MDMS = Midday Meal Scheme; R1 = round 1.

[a] Instrument variable (IV) results correcting for self-selection using being born in 2002 as an instrument.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

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