Educational and Health Impact of Two School Feeding Schemes: Evidence from a Randomized Trial in Rural Burkina Faso*

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Abstract

We use a prospective randomized trial to assess the impact of two school feeding schemes on educational and health outcomes of children from low income household in northern rural Burkina Faso. The school feeding programs under consideration are school meals where students are provided with lunch each school day, and take home rations which provide students with 10 kg of cereal flour each month, conditional on 90 percent attendance rate. After the program ran for one academic year, we found that both school feeding schemes increased girls' enrollment by 6 percent. While we did not observe any significant impact on raw scores on mathematics, we observed that the time-adjusted scores on mathematics improved slightly for girls. An unexpected lower average attendance was observed. We argue that this reflects the absence of an active labor market and the fact that households are labor constrained and/or child labor is complementary to adult labor. We show that the interventions caused attendance to decrease in household who are low in child labor supply while attendance improved for households which have a relatively large child labor supply, consistent with the labor constraints. This, in turn, explains the mixed impacts on learning outcomes that we observed. Finally, for younger siblings of beneficiaries, aged between 12 and 60 months who were not in school, take home rations have increased weight-for-age by .38 standard deviations and weight-for-height by .33 standard deviations. In contrast, school meals did not have any significant impact on the nutrition of younger children.

1 Introduction

While universal primary school attendance is a stated goal by many governments and the millennium development goals (MDG) , enrollment rates continue to be low in many developing countries (e.g. UNESCO, 2007). To foster enrollment, many governments have eliminated primary school fees, as well as established programs such as school feeding food programs (see Levinger, 1986, for an early review) or conditional cash transfers more recently (see Schultz, 2004, for an analysis of the Progresa program in Mexico) to increase the demand for schooling. There exists a large body of empirical evidence which documents the effectiveness of conditional cash transfers to increase investment in education in different settings. In contrast school feeding programs (SFPs) have received relatively less attention in recent economic literature (see Adelman *et al.*, 2007, for a review). Significantly, seldom do the studies of SFPs assess the relative impact of different modalities of interventions; the current study addresses this gap in the literature by providing a rigorous evaluation of alternative school feeding schemes in the same environment.

In general, three objectives can be directly associated with school feeding programs (e.g. Adelman *et al.*, 2007; Levinger, 1986). First, SFPs can motivate parents to enroll their children and see that they attend school regularly. Second, SFPs can improve the nutritional status of school age children over time, and alleviate short-term hunger in malnourished or otherwise well-nourished schoolchildren. Third, SFPs can improve cognitive functions and academic performance via reduced absenteeism and increased attention and concentration due to improved nutritional status and reduced short-term hunger. Indirectly, by increasing the amount of food available to the household, SFPs could improve the nutritional status of household members who are not in school, especially when SFPs entail take home rations. Overall, SFPs are appealing because if properly designed and implemented they lead to increased number of children being enrolled with better academic performances.

The two forms of SFPs that we consider consist of school meals and take home rations (THR). Under school meals program breakfast and/or lunch (possibly fortified with micronutrients) is served at the school every school day. Under THR a student receives a certain amount of food staples each period conditional on maintaining a specified attendance rate during that period. Each scheme of SPFs has its specific merits. We are certain that meals served at schools reach the students who are supposed to benefit from the program. However, parents could react by reallocating food in the household away from these children who are already benefiting from the program. Food received by the household under THR is more likely to be shared by other household members, hence reaching children who may be in need of additional food. Because the nutritional benefits would be diluted within the household, the impact on academic performance is likely to be lower for THR than with a school meals program. Which program should be preferred is both an issue of policy objectives as well as the evidence base. The latter empirical question is addressed by this paper.

The paper uses a randomized experiment to assess the hypothesized relationships between SFPs on the one hand, and enrollment, academic performance, cognitive development and preschool children nutritional status on the other hand. The focus of this study is the Sahel region of northern Burkina Faso in West Africa. Northern Burkina is an appropriate context to evaluate the impact of school feeding program for two main reasons. First, the region has some the world lowest primary school participation. On average 20 percent of school age children (6 to 16 years old) attend school, based on recent national surveys (e.g. Institut National de la Statistique et de la Démographie & ORC Macro, 2004). Therefore there exists a large scope for increasing enrollment. Second, income levels are very low and severe food shortages are frequent (Zoungrana *et al.*, 1999). Hence, the value of the food offered should be a sufficient incentive to attract children to school.

Our analysis adds to the literature on education in low income countries in two ways. First, it rigorously evaluates the impact of two alternative school feeding schemes within the same context. The use of a randomized experiment has the advantage of avoiding the issue of site selection that may have limited the causal interpretation of many previous studies. Hence the paper provides new insights on how a range of educational outcomes including enrollment, attendance and academic performances respond to two related types of interventions. Second, in addition to educational outcomes, the paper also explores the impact of SFPs on the nutritional status of school age children as well as of younger children who are not enrolled in school. While previous studies have looked at the nutritional impact of school age children, none has taken into account the potential spillovers effects to younger children. This possibility would imply additional long-term benefit of SFPs which may have been previously under-estimated.

We find that both school meals and THR increase new enrollment for girls by about 5 to 6 percent. The interventions also led to adjustment in child labor, with children (especially girls) with access to SFPs shifting from on farm labor and off-farm productive labor to more domestic tasks, possibly those that are more compatible with school hours. We find a small increase in time-adjusted scores of mathematics for girls, but not a significant impact on raw scores of mathematics. We do not find an impact on other measures of cognitive development.

The impact on attendance is unexpected since students who were exposed to the interventions have lower attendance on average. We argue that the increased enrollment could be accompanied by lower attendance rates if there is no active labor market and households are labor constrained and/or child labor is complementary to adult labor. We show that the interventions caused attendance to decrease in household who are low in child labor supply while attendance improved for households which have a relatively large child labor supply, which is an indication that labor constraints matter. This in turn explains the mixed impacts on learning outcomes that we observed.

Regarding nutrition, for children between 6 and 60 months who were not in school, take home rations have increased weight-for-height by .33 standard deviations and weight-for-age by .38 standard deviations. Overall, both SFPs improved enrollment with take home rations having positive spillovers onto younger children.

The paper is organized as follows. We provide a brief review on school feeding program in section 2. We discuss our program design and the data collection in section 3 and our empirical strategy in section 4. We present the estimation results in section 5 while section 6 concludes.

2 Overview of School Feeding Programs

SFPs seek to induce a change in household behavior, with the goal of improving educational and nutritional outcomes (Grantham-McGregor *et al.*, 1998). The driving rationale is that by subsidizing schooling costs, school SFPs can induce parents to invest more in their children education than they would have absent of the program (e.g. Adelman *et al.*, 2007). Additionally, SFPs can make investments in education more efficient.

SFPs are not new. School meals in particular have been used for a long time in developed countries (see Dwyer, 1995, for an analysis of school feeding program since the 19^{th} century in the USA), and their introduction in low income countries dates back to at least three decades (Levinger, 1986). In contrast, take home rations are a more recent intervention that has been recently promoted by the World Food Program (WFP). SFPs can be perceived as conditional in-kind transfers. School meals are conditioned in the sense that a child must be enrolled and attend school regularly to receive a transfer. THR can be made conditional on enrollment and attendance. From this perspective, SPFs are similar to conditional cash transfers which have been the focus of a growing body of empirical research.

The benefits of SPFs are, arguably, very large (Adelman *et al.*, 2007, 2008; Ahmed, 2004). First, nutritional and health status have powerful influences on a child's learning and on how well a child performs in school. In particular, poor nutrition among school-age children impact their cognitive functions and reduce their ability to participate in learning experiences in the classroom. Second, malnourished or unhealthy children are likely to attend school irregularly leading to poor academic performances. Third, it has been shown in the nutrition literature that even short-term hunger (common in children who do not eat before going to school), can have severe adverse effects on learning and academic performances in general. Simply stated, children who are hungry have more difficulties concentrating and performing complex tasks, even if otherwise well nourished. Overall, beyond getting parents to enroll their children, SFPs can have a far reaching impact on children nutritional and health status and how they perform in school.

Nevertheless, few studies address some of the potentially adverse impacts of SFPs on academic performances. Intuitively, the positive impacts on academic performances would require that the learning environment remains constant or improves when enrollment increases. One can, however, anticipate several changes in the learning environment following the introduction of SFPs. First, if teachers allocate some of their time to administering the programs, the actual teaching time could decrease. Second, classrooms could become overcrowded since enrollment is likely to increase. In this case the teacher may become less efficient. Schools may find themselves lacking other inputs (e.g. books, notebooks) which could effectively reduce academic performances. Moreover, the additional incentives of the program will bring in students whose parents previously assessed the benefits of schooling as lower than the costs; at the margin, these students can be expected to be less able to gain from schooling.

There are instances where SFPs are reported to have produced mixed results on academic performances at best. Grantham-McGregor *et al.* (1998) show than in Jamaica, learning outcomes deteriorated in less well organized schools following the introduction of a school breakfast program. Ahmed & Ninno (2002) find that take home rations were effective in increasing enrollment and attendance in Bangladesh, but academic performance measured by standardized tests were lower compared to schools who did not benefit from the program. The authors note, however, that the students' poor performance can not be attributed to the program because non-eligible students in the program schools scored as well as students in schools whose schools were not part of the program. Adelman *et al.* (2008) find that the literacy scores decreased for some segments of their sample which were receiving the take home rations, but do not elaborate on the mechanisms. Reviewing 22 studies, Levinger (1986) concludes that SFPs do indeed increase enrollment, but the impact on academic performances is mixed and depends on the local conditions. Overall, the conventional wisdom is that unless other factors such as adequately trained teachers, other learning materials and adequate physical facilities are present, SFPs would not improve academic performances (e.g. General Accounting Office, 2002).

In the economic literature a long standing debate is whether in-kind transfers can be as efficient as cash transfers (e.g. Gahvari & Mattos, 2007). Ross (1991) shows that in-kind transfers (in the absence of a resale market) may be more effective than cash transfers at raising the welfare of all household members when parents put insufficient weight on children welfare than society would have preferred. In the case under consideration, SPFs would be more effective than an equivalent amount of cash transfers if one thinks that parents would have put less weight than the society wanted on the nutritional status of their children.

Equivalent values of school meals and THR may have different distributional impacts. For example, precise targeting to an individual with school meals is possible because the child who is enrolled and attends school is the only one receiving the meal. This targeting, however, assumes that households do not neutralize the benefits of the school meals to the eligible children by modifying food reallocation within the household (Jacoby, 2002). Analyzing a school feeding program in the Philippines, Jacoby (2002) finds virtually no intra-household reallocation of calories in response to the feeding programs on average, but reallocation was more pronounced in poorer households of the sample. Jacoby (2002) explains his finding by arguing that either the specific feeding program (snacks) is not substitutable with household food, or it is the reflection of a "labeling effect" as argued by Kooreman (2000) using Dutch data[1].

In environments with low enrollment rates, perfectly targeting children who are enrolled might not be the best policy objective if one wants to reach most of the malnourished children. First, nutritionists generally believe that the window of opportunity for nutrition is during gestation and the first two years of life (Bhutta *et al.*, 2008). Hence making sure that the food distributed reaches younger children may be important by its own right. Second, parents may selectively enroll the healthiest and the best fed of their children (e.g. Alderman *et al.*, 2009; Glewwe & Jacoby, 1995). If so, it may be desirable to use THR to reach children who are not enrolled since they are likely

[1]Using Dutch data, Kooreman (2000) shows that the marginal propensity to consume child clothing out of exogenous child benefits is larger than the marginal propensity to child clothing out of income from other sources. He then argues that households act this way because the benefits have been "labeled child benefits".

to be the ones who need the intervention the most[2].

3 Program Description and Research Design

School canteens which provide meals to the students attending school meals were first introduced in Burkina Faso by the Catholic Relief Service (a non-governmental organization) in the mid 1970's in the aftermath of severe famine spells which affected the Sahel region of West Africa. Dry take home rations are a more recent intervention, also initiated in Burkina Faso by the Catholic Relief Service/Cathwell; students who attend school on a regular basis receive a food ration (flour) that they can bring back home each month. Take home rations are generally targeted to girls.

Starting from the 2005-2006 school year, after a reorganization of the operational zones of the different actors, the World Food Program (WFP) assumed responsibility for all school nutrition programs (canteens and take home rations) in the Sahel region. Our study covers the region served by the WFP, and all new 46 new schools which were first opened in the academic 2005-2006. The experiment consisted in randomly assigning these schools to three groups (school canteens, take home rations and control group) after a baseline survey in June 2006. The program was implemented in the following academic year (i.e. 2006-2007) and a follow up survey was fielded in June 2007 at the end of that academic year[3].

Lunch was served each school day in the school with the canteens. The only requirement to have access to the meal is that the pupil be present. The THR stipulated that each month, each girl would receive 10 kg of cereal flour, conditional on 90 percent of attendance rate. In both cases, WFP has developed a quarterly delivery schedule, and the food staples were stored within the school. In keeping with local policy, boys were not eligible for the THR program[4]. The teachers

[2]In Ghana, Glewwe & Jacoby (1995) find that malnourished children start school later and complete fewer years of school as compared to better nourished children. Therefore holding age constant, at any given time malnourished children are more likely to be out of school than well nourished children.

[3]The trial was originally schedule to last two years but the implementers were reluctant to continue the random assignment into the second year.

[4]This followed from local WFP policy.

oversaw the administration of the program in collaboration with a representative of WFP. WFP has not reported any issues of concern with the program administration. However, because we did not run random checks on the program administration we cannot completely rule out problems that the WFP itself would not have known about.

We surveyed a random sample of 48 households around each school, making a total of 2208 households, having a total of about 4140 school age children (i.e. aged between 6 and 15). We collected information on household backgrounds, household wealth, school participation for all children, and anthropometric data. In both rounds of the survey, all school age children were asked to solve simple mathematical operations (addition, subtraction, multiplication and division). In the follow up round we administered formal cognitive tests, including the Raven's Colored Progressive Matrices tests and forward and backward digit span tests in order to measure the program impact on cognitive development and short term memory. In addition hemoglobin levels were taken for all children younger than 16 and all women of reproductive age (between 15 and 49) in the follow up round. The field work differs from many school feeding evaluation studies, not only in its randomized assignment of treatments, but also in that it surveyed children not in school.

We summarize our key variables at baseline in table 1. The first three columns report the averages for the villages with school meals, take home rations and for the control villages. The last two columns (4 and 5) report the tests whether these variables are statistically different across treatment and control groups. We consider child level variables, which include educational and health outcomes as well as socioeconomic characteristics, and household level variables which include the household head socioeconomic characteristics and household wealth.

It is apparent that prior to treatment, the groups were similar on most variables including enrollment, child health and nutritional status, household and socioeconomic characteristics. Out of the 86 differences reported in columns 4 and 5, there are 5 instances were the estimated differences are statistically significant. Overall, we conclude that the random assignment of villages to treatment and control groups was reasonably successful.

These summary statistics also show that these villages are characterized by low enrollment rate and poor child health. At the time of the baseline survey, 29 percent of children in the school meals villages, 26 percent in the take home villages and 28 percent in the control villages were enrolled in schools[5]. In both treatment and control villages, only a small fraction (about 17 percent) of those children who are enrolled have access to all the required books. If this is any indication of the learning environment, one could conjecture that other school inputs are constraining as well. On the math tests, children scored less than 2 points which correspond to getting half the answers correct. The anthropometric data point to severe food shortage, with weight-for-age and height-for-age 2 standard deviations below the reference population[6][7]. The figures in table 2 (top panel) indicate that prior to the treatment, more than half of children were underweight or stunted, and about one third were wasted. Table 3 provide similar measures taken from the 2003 Demographic and Health Survey (Institut National de la Statistique et de la Démographie & ORC Macro, 2004) which is the most recent available national survey at the time of our study. It can be seen that child malnutrition is widespread, and the northern region (which includes our study area) is worse off than the other regions. Together, these figures indicate that these households are facing severe constraints on nutrition and one could expect significant gains from the program.

4 Empirical Strategy

The study uses an experimental, prospective randomized design in which villages are randomly assigned to treatment and control groups and data are collected before the interventions are rolled out and after the interventions have been implemented (Burges, 1995; Duflo *et al.*, 2008). Our identification strategy relies entirely on the random assignment of the villages to treatment and control groups. Because of this random assignment the estimated program impact has a causal

[5]As previously noted, these differences are not statistically significant as shown in columns 4 and 5.

[6]We use the World Health Organization Child Growth Standards Package (WHO Multicentre Growth Reference Study Group, 2006).

[7]As previously noted, these differences are not statistically significant as shown in columns 4 and 5.

interpretation. Our main assumption is that the outcomes of interest would have remained identical across these groups if the program has not been implemented. Therefore differences observed across the groups in the follow up survey can be attributed to the program.

Because the program was offered at the village level, we estimate the average intent to treat (AIT), that is, the impact of the program, on average, for all children in a given age range within a village whether or not all children in the village were receiving the treatment. This estimate measures the average program impact on eligible individuals (i.e. the impact of the intervention instead of the impact of the treatment), and is relevant for two reasons. First, since in practice policy makers have no influence on program participation, AIT is relevant for policy analysis. Second, AIT provides a lower bound for average treatment on the treated (ATT) under the assumption that the program impact on non participants in treatment groups is lower than its effect on compliers.

To estimate the AIT we use children in the control villages as the counterfactual group, with the assumption that control villages are not impacted by the program. We measure the program effect as the difference between the potential outcome (y_{1i}) for children in a treated village ($T_i = 1$) in the presence of the treatment and the potential outcome (y_{0i}) for children in a treated villages in the absence of the treatment.

$$AIT = E\left(y_{1i}|T_i = 1\right) - E\left(y_{0i}|T_i = 1\right) \tag{1}$$

However, since we do not observe the potential outcome for children in a treated village in the absence of the treatment, (y_{0i}), we use children in control villages ($T_i = 0$) as the counterfactual. We assume that the potential outcome for children in a treated village in the absence of the treatment would be the same as the potential outcome for children in the absence of the treatment in control villages, or

$$E\left(y_{1i}|T_i = 1\right) = E\left(y_{0i}|T_i = 0\right) \tag{2}$$

Therefore, the AIT is given by

$$AIT = E\left(y_{1i}|T_i = 1\right) - E\left(y_{0i}|T_i = 0\right) \tag{3}$$

Given that we have both a baseline and a follow up surveys, we use a difference-in-differences (DID) specification to estimate the program impact.

$$y_{ivt} = \beta_0 + \beta_1 T_d + \beta_2 T_d * F + \beta_3 F + \beta_4 X_{it} + \mu_{ivt} \tag{4}$$

where y_i is the outcome of interest measured at the child level, T_i is the treatment indicator, X_i is a vector of child characteristics (e.g. gender, age), and F indicates the follow up survey. The impact of the program is given by β_2. The analysis then compares age cohorts rather than changes in individuals over a panel.

Our identification strategy could be weakened if control communities are indirectly affected by the program. For example, there could be cross over in which households in control villages have their children attend school in treatment villages so that they gain access to the program. Also households in the program villages could chose to foster in children from villages without the programs. The first type of cross over would lead to an underestimation of the impact while the latter could bias the measured effects upwards.

5 Estimation Results and Discussion

5.1 Enrollment

Table 4 reports the program impact on new enrollments. By new enrollments, we mean children who are entering school for the first time. The first three columns show the estimation results for children aged between 6 and 15 years. New enrollment increased by 6.2% overall and by 5.6% for girls in villages which were randomly selected to receive THR. New enrollment for girls increased by

5% in villages that received school meals. The main insight from this table is that both interventions were successful in increasing new enrollment. However, the impact is confined to younger children. This may reflect the fact that the program only ran for one academic year and older children may not be able to register. It is worth noting also that there is no evidence that THR targeting girls crowds out boys registration.

5.2 Learning outcomes

To assess how the interventions impacted learning outcomes, four simple arithmetic questions were asked to each child between 5 and 15. The enumerator recorded whether the answer was right and the time the child took to answer the question. There are two main reasons one would expect a positive impact on learning outcomes. First, if more children enroll because of the interventions, then one would expect the average learning outcome to increase (assuming that schooling has a positive impact on learning). Second for children who are enrolled, the interventions may improve learning outcomes through regular attendance and by reducing short term hunger the interventions would increase children ability to focus while in the classroom (e. g. Pollit, 1995). Our approach relies on the observation that even with simple questions on math skills, it is still plausible to find some discrimination among the population (Yamauchi, 2008).

The maximum time allotted was 120 seconds, and in the data we recoded all incorrect answers and non-response to 125 seconds. The results are reported in tables 5 and 6 for all children between age 6 and 12. In table 6, we report the time each child took to answer. Overall there was a 22 seconds gain for girls in school meals villages. From columns 6 and 9, it can be seen that these gains in time are confined to older girls in school meals villages (13 to 15 years old), as these girls took 54 seconds less than the control group to complete the exercise. We do not, however, see any significant impact on boys.

A potentially interesting question is why the effects for older boys in the school meals villages in table 5 are negative and statistically significant. At this point, we do not have an explanation.

However, similar negative impacts on learning outcomes have been encountered in Northern Uganda by Adelman *et al.* (2008). Likewise Grantham-McGregor *et al.* (1998) have identified a negative impact of school breakfast on learning outcomes in Jamaica, that they have associated with school level organization. In particular, they have remarked that feeding program was likely to have a negative impact in schools which were less well organized.

5.3 Cognitive Development

During the follow up survey children between 5 and 15 were administered the WISC and Raven's progressive matrices tests, and a digital span test. The WISC and Raven tests were originally developed by Raven in 1936. The test asks candidates to identify the missing segment required to complete a larger pattern. The digit span test assesses the number of digits a child can retain and recall. Hence the forward digit span test can be seen as a measure of short term or working memory. Since these tests were not administered during the baseline, we rely on the differences between treatment and control villages in the follow up assuming that the differences at the baseline would have been minimal. The estimations results are presented in tables 7 for the WISC and Raven's and in table 8 for the digit span tests. In each case the outcome variable is the child's total score. The results indicate that the interventions did not produce any discernible impact on these outcomes. In general cognitive abilities are more likely to be influenced by interventions which target children less than two years old when the brain is still forming (Bhutta *et al.*, 2008). Therefore, it is not necessarily surprising that these interventions which target school age children do not produce any discernible impact.

5.4 Attendance

Attendance is measured by the number of school days missed in May by each student as reported by the survey respondent. Hence, a negative coefficient indicates that children exposed to the program missed fewer days in May whereas a positive coefficient indicates that children exposed to the program missed more days in May. We show the estimation results in table 9. The sample is restricted to children who were enrolled in school at the time of the surveys. The results indicate that compared to the control group, students who were receiving school meals missed on average .7 (significant) days more and students who were receiving the take home rations missed .4 (not significant) days more, out of an estimated 20 school days in May. These results are driven by the program impact on girls. From column (2), it is apparent that the school meals program did not affect boy's attendance significantly. Column (3) reveals that compared to the control group, girls who received school meals or the THR missed 1 day more.

Taken at face value, these results suggest the program had no impact on boys' attendance and leads to relatively lower attendance among girls. One can imagine a scenario where increased enrollment due to SFPs is accompanied by lower attendance. This explanation is related to household strategic behavior. Consider two identical households A and B where A is the treated and B is the control. Each household has two school age girls. Absent of the program one girl is enrolled and the other is in charge of the household chores. With the program, household A enrolls both girls. Without an active labor market and if the household is labor constrained or child labor is complement to adult labor (Edmonds, 2008; Diamond & Fayed, 1998; Ray, 2000, e.g.) it is plausible that at least one girl in household A will miss school sometimes whereas the only girl who is enrolled in household B does not have to miss school. Specifically, as long as each girl in household A attends enough school to qualify for the THR, household A is still better of in terms of welfare. The same reasoning can be made for the school meals, although in this case there is not a cut off level of attendance for benefits. When registration fees fall below a certain threshold, it is optimal for the household to register all her children and have them attend school only those times when the household values the school meals more than the child labor. Either way, attendance *conditional on enrollment* is likely to be lower with the program than without the program.

Overall it is plausible that with the program children who would have stayed out of school are enrolled but some of them attend less regularly. We can use the number of school age children (6-

15) to empirically test this conjecture. We consider the presence of other (other than the student) children between 6 and 15 years old in the household at the time of the survey whether or not they were enrolled themselves. The number of children between 6-15 years old can be considered as given in the short term, and could not influenced by the program during the relatively short period it has been in place. To assess the effects of household labor supply, we construct three groups of households based on the number of children in the household and estimate the program impact on attendance for each group. The estimation results are reported in table 10. Columns 1-3 show the results for households with no or at most one child in addition to the student (column 1), those with two or three children in addition to the student (column 2), and those with four or more children in addition to the student (column 3), respectively. We repeat the same regressions in columns 4-6 restricting the sample to younger students (i.e. 6-12 years old). Consistent with our conjecture, for households with at most one child in addition to the students (columns 1 and 4), the intervention has lowered attendance for more than a full day compared to the control group. There is no significant effect for households with 2 or 3 children (columns 2 and 4). For households with 4 or more children in addition to the student (columns 3 and 6), THR has lead to a reduction of the number of days missed (increased attendance) by about 1.4 day for all students (column 3) and by about 1.7 days for younger students (column 6).

5.5 Child labor

Tables 11 to 16 look at the interventions impact on child labor. The survey asked whether a child participated into a specific task during the last week the school was open. Girls living in THR villages were .09% less likely to participate in farm labor or in productive non-farm labor (tables 11 and 12). The impact is in general larger for girls ,and is confined to girls who are 6 to 12 years old. These girls were also less likely to tend for their younger siblings (table 15), an activity that the social organization usually bestows onto young girls. These results are consistent with the observation that the take home rations targeted girls exclusively. However,girls 6-12 who benefited from the program, especially in the THR villages, were likely to be more involved in domestic activities such as fetching water, fetching woods and other household chores, as can be seen in tables 13, 14 and 16.

Overall, the interventions did not eliminate child labor, but instead altered the allocation of child labor (especially among girls) away from productive activities and more toward domestic activities which the children may be more able to combine with school activities.

5.6 Nutritional Status

Table 17 shows the program impact on nutritional status of children less than 5 years old. We report the results for children between 6 and 60 months old and for children between 12 and 60 months old. For THR villages, a child is defined as being in a treatment household if there is at least one school age girl (6-12 years old) in the household whether or not she actually attended[8]. In villages which received take home rations, weight-for-age increased by .36 standard deviations (significant at the 5% level) and weight-for-height increased by .29 standards deviations (although not statistically significant) compared to the controls. In the last three columns we restrict the sample to children between 12 and 60 months old. It can be seen that the program impact increases both in magnitude and in statistical significance: .38 standard deviations increase for weight-for-age (significant at the 5% level) and .33 increase in for weight-for-height (significant at the 10% level). In contrast school meals did not have any discernable impact. Furthermore, there is no significant impact on height-for-age. This latter result is expected since height-for-age is more a long run measure of child nutritional status which cannot be influenced in a relatively short period of time and which reflects breast feeding and weaning practices as much as household food availability.

Unless parents reallocate food within the household away from children who have access to school meals and towards children who are not in school, one would not expect school meals to have any impact on children who are not in school, especially if parents do not reallocate food

[8] Since THR targeted girls only, a household without a school age girl would not have been eligible.

away from the children who have access to school meals as observed by (Jacoby, 2002). On the other hand the significant impact of THR on younger children who are not in school warrants two observations. First, food that comes from THR is not used exclusively for the child who has access to the program but is accessible to other household members as well. Hence, while school meals target precisely the child who is in school, THR have positive spillovers have for younger children. Second, for a relatively marginal food transfer to have such a strong effect in a relatively short period of time, it must be the case that these households were severely food constrained.

In table 18 we report the program impact on the nutritional status of school age children. We use body mass index for age (bmi) and weight-for-age, both expressed in z-scores based on the reference tables provided by Onis *et al.* (2007)[9]. It can be seen that the program did not have any significant impact on bmi, although there is a marginal gains in weight-for-age for 6-10 years old (column 7).

Table 19 shows hemoglobin levels taken during the second round of the survey. Assuming that hemoglobin levels at baseline would not have been different across treatment and control groups once age and gender were adequately controlled for, in keeping with the randomization our estimated program impact can be interpreted as causal.

The first three columns show the estimation results for children 6 to 15 years old. Column one shows the results for the entire subsample of this age range and columns two and three show the results for boys and girls separately. In columns 4-6, we repeat the analysis for children aged between 6 and 12 years. Column 7 shows the results for women of reproductive age, i.e. between 16 and 49 years. It is apparent that the program did not have any significant impact on hemoglobin levels. Nevertheless, it can be seen that the program impact is generally larger under THR, and the standard errors are smaller in relative terms. Moreover, comparing columns 1-3 to columns 5-6, it can be seen that the impact on younger children is relatively larger. This is consistent with our previous finding that the interventions raised enrollment of younger children the most. Furthermore,

[9]In the reference tables reported by Onis *et al.* (2007), bmi-for-age is defined over the age range of 6 to 19 years of age, and weight-for-age is defined over 6 to 10 years of age.

while THR have relatively a larger impact, it is surprising that the impact is almost confined to younger boys as can be seen from comparing columns 5-6. This could be an indication that food redistribution within the household tends to favor boys than girls. However, these interpretations must be taken with caution since estimations are not statistically significant.

6 Conclusion

In this paper, we have used a prospective randomized design to assess the impact of two school feeding schemes on educational and health outcomes of children from low income household in northern rural Burkina Faso. We considered the school meals which provide lunch in school, and take home rations which provide students with 10 kg of cereal flour each month, conditional on 90 percent of attendance rate. Because with can rely a baseline and on a follow up surveys, we were able to use difference in difference regressions to estimate the impact of the program. Moreover, because we have a randomized experiment, we can interpret the estimated impact as causal.

After the program ran for one academic year, we found that both school feeding schemes increased girls' enrollment by 6 percent. For younger siblings of beneficiaries, aged between 12 and 60 months who were not in school, take home rations have increased weight-for-age by .38 standard deviations and weight-for-height by .33 standard deviations. We do not find, however, a significant impact on academic performances and the average impact on attendance is counter-intuitive in the sense that the children who were exposed to the interventions have lower attendance. We argue that the increased enrollment could be accompanied by lower attendance rates if there is no an active labor market and households are labor constrained and/or child labor is complementary to adult labor. We show that the interventions caused attendance to decrease in household who are low in child labor supply while attendance improved for households which have a relatively large child labor supply. This in turn explains the counter-intuitive impact on learning outcome that we observed.

Overall, our results show that school feeding programs in this specific context (of agricultural households without an active labor market) can increase enrollment, but may fail to improve attendance and academic performance for a larger number of children. This calls for more investigation of the circumstances under which school feeding programs could increase enrollment and improve academic performances, and a more direct comparison of this type of conditional “in kind” transfers with conditional cash transfers.

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Tables

Table 1: Key Variables at Baseline

		(1)	(2)	(3)	(4)	(5)
Variable		Meals	THR	Control	Difference with control	
					Meals	THR
	Child level variables					
Enrolled		0.281	0.241	0.243	0.039	-0.001
		[0.033]	[0.032]	[0.025]	[0.042]	[0.041]
Math answers		1.823	1.923	1.818	0.005	0.106
		[0.113]	[0.143]	[0.132]	[0.173]	[0.194]
Math time-adjusted		180.528	171.661	183.433	-2.905	-11.772
		[6.070]	[6.328]	[6.913]	[9.199]	[9.372]
Days missed in May		0.513	0.713	1.276	-0.763*	-0.563
		[0.165]	[0.189]	[0.382]	[0.416]	[0.426]
Child labor (any)		0.848	0.87	0.852	-0.003	0.018
		[0.017]	[0.019]	[0.018]	[0.025]	[0.026]
Child productive labor		0.65	0.637	0.603	0.047	0.034
		[0.033]	[0.031]	[0.034]	[0.047]	[0.046]
	Farm labor	0.585	0.595	0.574	0.011	0.021
		[0.042]	[0.032]	[0.039]	[0.057]	[0.050]
	Non farm labor	0.292	0.236	0.163	0.129**	0.073
		[0.039]	[0.055]	[0.032]	[0.050]	[0.063]
Household chores		0.643	0.656	0.686	-0.043	-0.030
		[0.022]	[0.033]	[0.029]	[0.037]	[0.044]

	Coocking	0.334	0.315	0.344	-0.009	-0.028
		[0.020]	[0.023]	[0.024]	[0.031]	[0.033]
	Fetch water	0.467	0.493	0.527	-0.059	-0.034
		[0.027]	[0.041]	[0.039]	[0.048]	[0.057]
	Fetch wood	0.359	0.396	0.36	-0.001	0.035
		[0.022]	[0.032]	[0.036]	[0.043]	[0.048]
	Tend yongsters	0.237	0.198	0.186	0.052	0.012
		[0.028]	[0.024]	[0.031]	[0.042]	[0.039]
	Other hh chores	0.391	0.388	0.413	-0.022	-0.025
		[0.015]	[0.026]	[0.025]	[0.029]	[0.036]
weight (kg)		23.135	23.397	22.747	0.388	0.650
		[0.682]	[0.706]	[0.631]	[0.929]	[0.947]
height (cm)		125.627	125.542	124.941	0.686	0.601
		[1.020]	[1.315]	[1.362]	[1.702]	[1.893]
Body mass index		14.378	14.569	14.331	0.047	0.238
		[0.269]	[0.192]	[0.201]	[0.336]	[0.278]
Weight-for-age (6-60 months)		-2.202	-2.521	-2.394	0.192	-0.126
		[0.172]	[0.159]	[0.178]	[0.248]	[0.238]
Height-for-age (6-60 months)		-2.351	-2.086	-2.317	-0.034	0.231
		[0.152]	[0.111]	[0.146]	[0.211]	[0.184]
Weight-for-height (6-60 months)		-0.786	-1.125	-0.903	0.117	-0.222
		[0.143]	[0.108]	[0.156]	[0.212]	[0.190]
Registration fee		261.58	543.478	319.667	-58.086	223.812
		[79.570]	[154.603]	[85.403]	[116.727]	[176.623]
Educ expenditures		2351.689	3012.625	2556.167	-204.477	456.459
		[334.717]	[476.950]	[291.944]	[444.147]	[559.207]

PAFees	718.937	699.666	801.5	-82.563	-101.834
	[74.925]	[92.651]	[75.513]	[106.377]	[119.526]
Child is boy	0.495	0.52	0.504	-0.010	0.016
	[0.008]	[0.017]	[0.012]	[0.014]	[0.021]
age	9.783	9.793	9.837	-0.054	-0.044
	[0.069]	[0.081]	[0.076]	[0.103]	[0.111]
Father has some formal ed.	0.014	0.024	0.026	-0.012	-0.002
	[0.005]	[0.012]	[0.008]	[0.009]	[0.014]
Father has some Koran ed.	0.169	0.164	0.202	-0.034	-0.038
	[0.041]	[0.040]	[0.077]	[0.087]	[0.086]
Mother has some formal ed.	0.004	0.011	0.011	-0.007	0.000
	[0.003]	[0.007]	[0.004]	[0.005]	[0.008]
Mother has some Koran ed.	0.029	0.06	0.107	-0.078	-0.048
	[0.016]	[0.020]	[0.068]	[0.070]	[0.071]
Maternal orphan	0.032	0.029	0.024	0.008	0.005
	[0.009]	[0.010]	[0.006]	[0.011]	[0.012]
Paternal orphan	0.04	0.07	0.055	-0.015	0.015
	[0.009]	[0.010]	[0.016]	[0.018]	[0.019]
	Household level variables				
Head age	42.881	45.669	45.629	-2.748*	0.040
	[1.052]	[1.190]	[1.223]	[1.613]	[1.707]
Head is male	0.976	0.978	0.978	-0.002	0.000
	[0.005]	[0.007]	[0.006]	[0.008]	[0.010]
Head is Mossi	0.129	0.094	0.101	0.028	-0.007
	[0.064]	[0.045]	[0.048]	[0.079]	[0.066]

Head is Fulani	0.389	0.46	0.411	-0.023	0.049
	[0.090]	[0.101]	[0.102]	[0.136]	[0.144]
Head is of Blacksmith descent	0.041	0.027	0.02	0.021	0.007
	[0.021]	[0.010]	[0.006]	[0.021]	[0.011]
Head is of Noble descent	0.377	0.37	0.557	-0.180	-0.187*
	[0.084]	[0.071]	[0.075]	[0.113]	[0.103]
Head is of Captive descent	0.35	0.391	0.193	0.157	0.198*
	[0.088]	[0.085]	[0.058]	[0.105]	[0.103]
Head is Muslim	0.967	0.978	0.987	-0.020	-0.009
	[0.018]	[0.012]	[0.007]	[0.019]	[0.014]
Household asset value (1000 CF)	66.522	92.109	78.966	-12.443	13.143
	[12.659]	[19.772]	[6.027]	[14.020]	[20.670]

Robust standard errors in brackets.

* significant at 10%; ** significant at 5%, *** significant at 1%

Mossi and Fulani are two ethnic groups from the region.

Blacksmith, Noble or Captive descent are castes used to categorize households within these ethnic groups.

Table 2: Percentage of children two standard deviation below the median (z-scores: children between 6 and 60 months old)

	Weight-for-age	Weight-for-Height	Height-for-Age
Baseline			
School meals	52.6	29.5	59.9
Take Home Rations	56.2	32.3	60.0
Control	55.3	31.6	61.7
Follow up			
School meals	42.8	20.6	58.9
Take Home Rations	42.8	19.5	53.3
Control	42.7	23.8	55.6

Table 3: Percentage of children two standard deviation below the median in rural Burkina Faso, 2003 (z-scores: children between 0 and 60 months old)

	(1)	(2)	(3)	(4)
	Height-for-age	Weight-for-height	Weight-for-age	N
Region				
Ouagadougou (area)	16.4	12.4	17.9	486
North	41.7	19.4	41.8	1587
East	47.2	18.7	38.4	2147
West	35.7	19.3	37.6	2328
Central/South	35.1	19.2	38.4	1722
Total Rural	41.4	19.7	40.3	7166

Source: ORC Macro, 2008. MEASURE DHS STATcompiler. http://www.measuredhs.com, August 1 2008.

Table 4: Program Impact on New Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	0.017	-0.015	0.049	0.012	-0.034	0.064	0.025	0.059	-0.005
	[0.019]	[0.028]	[0.025]**	[0.023]	[0.034]	[0.031]**	[0.025]	[0.042]	[0.028]
THR*Followup	0.062	na	0.056	0.065	na	0.065	0.049	na	0.029
	[0.021]***	na	[0.025]**	[0.026]**	na	[0.031]**	[0.023]**	na	[0.023]
Follow up	-0.153	-0.16	-0.147	-0.178	-0.178	-0.179	-0.066	-0.092	-0.042
	[0.014]***	[0.021]***	[0.019]***	[0.017]***	[0.025]***	[0.023]***	[0.020]***	[0.035]***	[0.021]**
Girl	-0.041			-0.045			-0.037		
	[0.009]***			[0.011]***			[0.013]***		
Constant	0.177	0.173	0.137	0.191	0.186	0.145	0.069	0.045	0.057
	[0.014]***	[0.022]***	[0.016]***	[0.016]***	[0.025]***	[0.017]***	[0.014]***	[0.028]	[0.018]***
Observations	6587	2537	4050	5097	1967	3130	1490	570	920
R-squared	0.12	0.14	0.11	0.11	0.15	0.09	0.1	0.13	0.14

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is whether the child started school in survey year.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 5: Program Impact on Learning Outcomes: Answers to Math Questions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	-0.038	-0.152	0.087	0.028	-0.077	0.124	-0.300	-0.505	-0.131
	[0.070]	[0.099]	[0.100]	[0.079]	[0.111]	[0.112]	[0.153]*	[0.215]**	[0.228]
Rations*Followup	-0.078	na	-0.042	-0.084	na	-0.063	-0.054	na	0.044
	[0.092]	na	[0.105]	[0.099]	na	[0.113]	[0.244]	na	[0.276]
Follow up	0.044	0.076	0.005	0.040	0.061	0.018	0.073	0.106	-0.034
	[0.049]	[0.071]	[0.070]	[0.055]	[0.079]	[0.078]	[0.109]	[0.157]	[0.161]
Girl	-0.050			-0.045			-0.098		
	[0.034]			[0.038]			[0.073]		
Constant	0.554	0.496	0.545	0.533	0.474	0.533	2.933	3.053	2.802
	[0.052]***	[0.080]***	[0.063]***	[0.056]***	[0.085]***	[0.065]***	[0.120]***	[0.173]***	[0.156]***
Observations	5548	2302	3246	4467	1826	2641	1081	476	605
R-squared	0.39	0.42	0.39	0.34	0.36	0.34	0.16	0.18	0.22

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is number of correct answers (varies from 0 to 4).

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 6: Program Impact on Learning Outcomes: Time Taken to Answers Math Questions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	-8.253	4.854	-21.635	-8.524	-2.432	-12.985	-12.374	34.066	-53.817
	[7.066]	[9.997]	[10.015]**	[7.526]	[10.633]	[10.690]	[18.976]	[27.154]	[27.803]*
Rations*Followup	-4.669	na	-8.263	-0.610	na	0.833	-16.602	na	-39.108
	[9.480]	na	[10.716]	[9.798]	na	[11.109]	[30.521]	na	[34.490]
Follow up	6.616	2.491	10.545	2.237	3.294	0.823	22.276	2.091	45.857
	[4.893]	[6.924]	[6.925]	[5.201]	[7.361]	[7.336]	[12.982]*	[18.671]	[19.069]**
Girl	5.290	0.000	0.000	2.144	0.000	0.000	19.735	0.000	0.000
	[3.352]	[0.000]	[0.000]	[3.588]	[0.000]	[0.000]	[8.749]**	[0.000]	[0.000]
Constant	453.636	457.561	455.342	458.440	460.099	458.853	250.082	248.805	268.538
	[4.922]***	[7.280]***	[5.932]***	[5.097]***	[7.590]***	[5.985]***	[14.178]***	[19.849]***	[18.464]***
Observations	5624	2323	3301	4541	1847	2694	1083	476	607
R-squared	0.31	0.34	0.3	0.26	0.28	0.26	0.15	0.16	0.2

Robust standard errors in brackets
* significant at 10%; ** significant at 5%, *** significant at 1% Dependent variable is time taken to give a correct answer in second. Maximum time allowed is 120 seconds for each of the 4 questions. Wrong answer and no-answer are given 125 second.
Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 7: Program Impact on Cognitive Abilities: WISC%Raven Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals	0.222	-0.083	0.512	0.134	0.04	0.257	0.561	-0.807	1.242
	[1.066]	[1.062]	[1.137]	[1.095]	[1.067]	[1.160]	[1.201]	[1.253]	[1.423]
THR	-0.132	na	-0.045	-0.2	na	-0.179	0.086	na	0.478
	[1.222]	na	[1.233]	[1.241]	na	[1.258]	[1.455]	na	[1.525]
Girl	-0.046			-0.041			-0.13		
	[0.217]			[0.239]			[0.518]		
Constant	7.065	6.341	7.458	6.831	5.937	7.317	12.293	12.007	11.885
	[1.544]***	[2.079]***	[1.545]***	[1.594]***	[2.092]***	[1.582]***	[1.844]***	[2.659]***	[2.020]***
Observations	2651	1057	1594	2078	841	1237	573	216	357
R-squared	0.43	0.49	0.4	0.42	0.49	0.39	0.41	0.45	0.41

Robust standard errors in brackets
* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is number of correct answers of to WISC and Raven colored progressive matrices .
Regressions also control for child age, orphanhood, parents education, ethnicity, religion and household assets (not reported). Village fixed effects are not included since the dependent variable is measured only during the followup.

Table 8: Program Impact on Cognitive Abilities: Forward and Backward Digit Spans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals	0.112	0.327	0.011	0.068	0.377	-0.094	0.369	0.149	0.457
	[1.103]	[1.221]	[1.087]	[1.191]	[1.288]	[1.159]	[1.004]	[1.282]	[1.062]
THR	-0.005	na	-0.104	-0.213	na	-0.351	0.836	na	1.011
	[0.948]	na	[0.977]	[1.026]	na	[1.054]	[0.949]	na	[0.965]
Girl	-0.142			-0.168			-0.178		
	[0.236]			[0.284]			[0.488]		
Constant	8.489	7.969	8.73	8.399	7.458	8.854	12.859	13.098	12.189
	[1.885]***	[2.819]***	[1.676]***	[1.969]***	[2.917]**	[1.721]***	[2.021]***	[3.382]***	[1.962]***
Observations	2679	1076	1603	2095	853	1242	584	223	361
R-squared	0.42	0.47	0.4	0.43	0.48	0.41	0.36	0.43	0.35

Robust standard errors in brackets
* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is number of correct answers of to backward and forward digital spans.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion and household assets (not reported). Village fixed effects are not included since the dependent variable is measured only during the followup.

Table 9: Program Impact on School Attendance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	0.659	0.26	1.012	0.706	0.345	0.982	0.098	-0.65	-0.701
	[0.280]**	[0.399]	[0.353]***	[0.302]**	[0.442]	[0.366]***	[0.699]	[1.243]	[0.880]
THR*Followup	0.369	na	1.014	0.459	na	1.042	-0.377	na	-0.603
	[0.287]	na	[0.389]***	[0.310]	na	[0.406]**	[0.870]	na	[1.456]
Follow up	-0.987	-0.787	-1.107	-1.073	-0.892	-1.123	-0.298	0.151	0.823
	[0.225]***	[0.285]***	[0.338]***	[0.249]***	[0.334]***	[0.355]***	[0.354]	[0.559]	[0.748]
Girl	-0.057			-0.098			0.756		
	[0.097]			[0.104]			[0.381]**		
Constant	1.168	1.536	0.541	1.218	1.547	0.576	-0.112	-0.011	0.336
	[0.332]***	[0.505]***	[0.225]**	[0.335]***	[0.514]***	[0.233]**	[0.497]	[0.763]	[0.477]
Observations	1935	1116	819	1738	973	765	197	143	54
R-squared	0.11	0.15	0.18	0.11	0.15	0.19	0.22	0.27	0.94

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable number of days missed in May.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 10: School Attendance and number of children

	(1)	(2)	(3)	(4)	(5)	(6)
	All students			Students aged 6-12		
	Number of children other than student					
	0-1	2-3	4 or more	0-1	2-3	4 or more
Meals*Followup	1.478	0.189	-0.272	1.512	0.221	-0.235
	[0.423]***	[0.534]	[0.406]	[0.449]***	[0.565]	[0.397]
THR*Followup	1.245	0.268	-1.375	1.363	0.437	-1.653
	[0.404]***	[0.428]	[0.697]**	[0.434]***	[0.454]	[0.764]**
Followup	-1.652	-0.91	0.175	-1.788	-1.052	0.208
	[0.387]***	[0.386]**	[0.245]	[0.421]***	[0.417]**	[0.293]
Girl	0.023	0.02	-0.063	-0.047	-0.028	-0.075
	[0.164]	[0.179]	[0.134]	[0.178]	[0.191]	[0.150]
Constant	1.001	1.262	1.431	1.12	1.368	1.467
	[0.380]***	[0.485]***	[0.732]*	[0.376]***	[0.493]***	[0.723]**
Observations	568	830	537	511	747	480
R-squared	0.29	0.12	0.19	0.32	0.13	0.22

Robust standard errors in brackets
* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable number of days missed in May.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 11: Program Impact on Child Participation in Farm Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	0.024	0.01	0.033	0.04	0.014	0.06	-0.034	0.01	-0.111
	[0.025]	[0.032]	[0.037]	[0.029]	[0.038]	[0.042]	[0.045]	[0.046]	[0.080]
THR*Followup	-0.045	na	-0.089	-0.05	na	-0.089	-0.044	na	-0.118
	[0.032]	na	[0.037]**	[0.037]	na	[0.042]**	[0.068]	na	[0.084]
Followup	0.087	0.047	0.13	0.075	0.041	0.114	0.133	0.071	0.21
	[0.017]***	[0.023]**	[0.026]***	[0.020]***	[0.027]	[0.029]***	[0.032]***	[0.032]**	[0.056]***
Girl	-0.178			-0.176			-0.188		
	[0.012]***			[0.014]***			[0.022]***		
Constant	0.334	0.247	0.227	0.336	0.261	0.223	0.827	0.816	0.657
	[0.022]***	[0.030]***	[0.027]***	[0.023]***	[0.033]***	[0.028]***	[0.033]***	[0.038]***	[0.045]***
Observations	6047	2462	3585	4757	1931	2826	1290	531	759
R-squared	0.26	0.29	0.24	0.25	0.27	0.23	0.23	0.19	0.21

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is whether child participated in any farm labor the week before the survey.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 12: Program Impact on Child Participation in Non Farm Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	-0.006	-0.022	0.007	-0.006	-0.021	0.003	0.009	-0.025	0.044
	[0.023]	[0.033]	[0.033]	[0.026]	[0.037]	[0.036]	[0.054]	[0.077]	[0.079]
THR*Followup	-0.074		-0.090	-0.082		-0.103	-0.043		-0.031
	[0.025]***		[0.030]***	[0.028]***		[0.033]***	[0.060]		[0.071]
Followup	0.073	0.059	0.088	0.083	0.065	0.104	0.030	0.034	0.013
	[0.015]***	[0.022]***	[0.022]***	[0.017]***	[0.024]***	[0.024]***	[0.035]	[0.052]	[0.049]
Girl	-0.028			-0.028			-0.030		
	[0.011]**			[0.013]**			[0.026]		
Constant	0.126	0.112	0.109	0.128	0.111	0.115	0.327	0.369	0.258
	[0.018]***	[0.027]***	[0.020]***	[0.019]***	[0.028]***	[0.021]***	[0.035]***	[0.050]***	[0.041]***
Observations	6047	2462	3585	4757	1931	2826	1290	531	759
R-squared	0.23	0.22	0.24	0.22	0.21	0.24	0.27	0.3	0.3

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is whether child participated in any non farm labor the week before the survey.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 13: Program Impact on Child Participation in Fetching Water

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	0.049	0.017	0.084	0.045	0.000	0.102	0.071	0.077	0.073
	[0.026]*	[0.036]	[0.036]**	[0.030]	[0.040]	[0.042]**	[0.054]	[0.079]	[0.068]
THR*Followup	0.055	0.000	0.096	0.058	0.000	0.112	0.048	0.000	0.075
	[0.031]*	[0.000]	[0.036]***	[0.036]	[0.000]	[0.042]***	[0.056]	[0.000]	[0.062]
Followup	-0.031	0.004	-0.066	-0.043	0.001	-0.089	0.002	0.026	-0.025
	[0.019]	[0.026]	[0.025]***	[0.021]**	[0.029]	[0.030]***	[0.037]	[0.056]	[0.046]
Girl	0.330	0.000	0.000	0.311	0.000	0.000	0.403	0.000	0.000
	[0.013]***	[0.000]	[0.000]	[0.014]***	[0.000]	[0.000]	[0.026]***	[0.000]	[0.000]
Constant	0.075	0.160	0.347	0.082	0.158	0.342	0.445	0.435	0.865
	[0.020]***	[0.028]***	[0.027]***	[0.022]***	[0.029]***	[0.028]***	[0.037]***	[0.061]***	[0.036]***
Observations	6047	2462	3585	4757	1931	2826	1290	531	759
R-squared	0.24	0.19	0.20	0.22	0.17	0.19	0.30	0.28	0.14

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is whether child has fetched water the week before the survey.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 14: Program Impact on Child Participation in Fetching Wood

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	0.064	0.108	0.011	0.081	0.107	0.054	0.009	0.11	-0.13
	[0.026]**	[0.033]***	[0.039]	[0.029]***	[0.037]***	[0.043]	[0.060]	[0.079]	[0.091]
THR*Followup	0.056		0.035	0.079		0.072	-0.026		-0.083
	[0.033]*		[0.039]	[0.037]**		[0.043]*	[0.073]		[0.087]
Followup	-0.077	-0.1	-0.05	-0.096	-0.114	-0.081	-0.021	-0.05	0.034
	[0.018]***	[0.024]***	[0.027]*	[0.020]***	[0.026]***	[0.030]***	[0.041]	[0.055]	[0.062]
Girl	0.204			0.181			0.292		
	[0.013]***			[0.014]***			[0.029]***		
Constant	0.048	0.113	0.193	0.06	0.114	0.188	0.322	0.332	0.619
	[0.019]**	[0.025]***	[0.025]***	[0.020]***	[0.026]***	[0.027]***	[0.040]***	[0.056]***	[0.049]***
Observations	6047	2462	3585	4757	1931	2826	1290	531	759
R-squared	0.17	0.11	0.16	0.16	0.11	0.17	0.19	0.20	0.14

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is whether child has fetched wood the week before the survey.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 15: Program Impact on Child Participation in Tending for Younger Siblings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	-0.041	0.004	-0.084	-0.055	-0.015	-0.081	-0.001	0.070	-0.110
	[0.023]*	[0.025]	[0.037]**	[0.026]**	[0.030]	[0.042]*	[0.046]	[0.044]	[0.085]
THR*Followup	-0.006	na	0.009	-0.015	na	-0.006	0.022	na	0.051
	[0.030]	na	[0.036]	[0.035]	na	[0.041]	[0.061]	na	[0.077]
Followup	0.001	0.011	-0.012	0.023	0.024	0.018	-0.078	-0.029	-0.121
	[0.015]	[0.018]	[0.025]	[0.018]	[0.022]	[0.028]	[0.030]**	[0.031]	[0.054]**
Girl	0.181			0.172			0.214		
	[0.011]***			[0.013]***			[0.023]***		
Constant	0.165	0.182	0.335	0.169	0.175	0.336	0.114	0.136	0.302
	[0.019]***	[0.024]***	[0.027]***	[0.020]***	[0.025]***	[0.028]***	[0.030]***	[0.031]***	[0.046]***
Observations	6047	2462	3585	4757	1931	2826	1290	531	759
R-squared	0.13	0.12	0.11	0.13	0.11	0.12	0.20	0.26	0.19

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is whether has tended youngsters the week before the survey.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 16: Program Impact on Child Participation in Other Household Chores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	6-15 years old			6-12 years old			13-15 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
School Meals	0.027	-0.002	0.044	0.028	-0.009	0.056	0.008	0.017	0.012
	[0.022]	[0.024]	[0.036]	[0.026]	[0.027]	[0.042]	[0.042]	[0.052]	[0.066]
Dry Rations	0.065	na	0.116	0.055	na	0.113	0.090	na	0.110
	[0.030]**	na	[0.036]***	[0.035]	na	[0.042]***	[0.057]	na	[0.068]
Followup	-0.051	-0.005	-0.092	-0.051	0.007	-0.100	-0.054	-0.042	-0.071
	[0.016]***	[0.018]	[0.026]***	[0.019]***	[0.021]	[0.030]***	[0.030]*	[0.039]	[0.047]
Girl	0.574			0.528			0.740		
	[0.011]***			[0.013]***			[0.021]***		
Constant	-0.066	0.114	0.373	-0.049	0.106	0.362	0.147	0.176	0.869
	[0.020]***	[0.020]***	[0.028]***	[0.021]**	[0.021]***	[0.030]***	[0.031]***	[0.043]***	[0.036]***
Observations	6047	2462	3585	4757	1931	2826	1290	531	759
R-squared	0.40	0.09	0.17	0.37	0.10	0.17	0.55	0.10	0.09

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variable is whether child participated in other household chores the week before the survey.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 17: Program Impact on children (6 to 60 months old) nutritional status

	(1)	(2)	(3)	(4)	(5)	(6)
	Children 6-60 months old			Children 12-60 months old		
	Weight for Age	Weight-for-Height	Height-for-Age	Weight for Age	Weight-for-Height	Height-for-Age
Meals*Followup	-0.219	0.005	-0.19	-0.172	0.062	-0.135
	[0.229]	[0.157]	[0.180]	[0.132]	[0.160]	[0.186]
THR*Followup	0.355	0.291	-0.212	0.376	0.333	-0.189
	[0.153]**	[0.181]	[0.209]	[0.157]**	[0.187]*	[0.216]
Follow up	0.547	0.161	0.298	0.516	0.166	0.201
	[0.093]***	[0.112]	[0.133]**	[0.094]***	[0.114]	[0.137]
Girl	0.138	0.121	0.116	0.148	0.109	0.123
	[0.056]**	[0.065]*	[0.075]	[0.058]**	[0.067]	[0.079]
Constant	-1.65	-0.098	-0.792	-2.077	-0.437	-0.749
	[0.277]***	[0.337]	[0.358]**	[0.525]***	[0.625]	[0.758]
Observations	3422	3085	3079	3200	2890	2876
R-squared	0.15	0.08	0.08	0.16	0.09	0.08

Robust standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variables are z-scores of weight-for-age, weight-for-height and height-for-age.

Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).

Table 18: Program Impact on nutritional status of school age children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	bmi-for-age (z-scores)						weigt-for-age (z-scores)		
	6-15 years old			6-12 years old			6-10 years old		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Meals*Followup	0.022	0.003	-0.024	0.082	0.166	-0.011	0.225	0.235	0.1
	[0.088]	[0.135]	[0.121]	[0.118]	[0.189]	[0.167]	[0.112]**	[0.172]	[0.160]
THR*Followup	-0.124	na	-0.106	-0.152	na	-0.172	0.153	0.047	0.087
	[0.110]	na	[0.123]	[0.153]	na	[0.174]	[0.121]	[0.202]	[0.165]
Follow up	0.276	0.341	0.247	0.273	0.286	0.295	0.03	-0.041	0.11
	[0.062]***	[0.096]***	[0.086]***	[0.084]***	[0.135]**	[0.122]**	[0.080]	[0.123]	[0.115]
Girl	0.075			0.102			0.135		
	[0.049]			[0.071]			[0.057]**		
Constant	-1.161	-1.169	-1.153	-1.197	-1.158	-1.124	-2.308	-2.328	-2.232
	[0.064]***	[0.098]***	[0.080]***	[0.075]***	[0.108]***	[0.087]***	[0.062]***	[0.087]***	[0.085]***
Observations	5290.000	2073.000	3217.000	3269.000	1277.000	1992.000	3884	1800	2084
R-squared	0.520	0.610	0.610	0.590	0.660	0.670	0.63	0.71	0.72

Standard errors in brackets

* significant at 10%; ** significant at 5%, *** significant at 1%

Dependent variables are z-scores of bmi (columns 1-6) and z-scores of weight for age (columns 7-9).

Regressions also control for child age, gender and household fixed effects (not reported).

Table 19: Program Impact on hemoglobin levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Children aged 6-15			Children aged 6-12			Women
	All	Boys	Girls	All	Boys	Girls	16-49
Meals	0.081	0.138	0.036	0.118	0.249	0.006	-0.057
	[0.173]	[0.175]	[0.189]	[0.172]	[0.176]	[0.189]	[0.151]
THR	0.069	na	0.055	0.133	na	0.089	-0.008
	[0.192]	na	[0.203]	[0.196]	na	[0.208]	[0.129]
Girl	0.003			-0.022			
	[0.050]			[0.058]			
Constant	10.703	10.832	10.600	10.729	10.794	10.646	11.082
	[0.242]***	[0.256]***	[0.234]***	[0.239]***	[0.251]***	[0.241]***	[0.267]***
Observations	2518.000	1018.000	1500.000	2032.000	831.000	1201.000	2211.000
R-squared	0.09	0.11	0.09	0.10	0.13	0.10	0.03

Robust standard errors in brackets
* significant at 10%; ** significant at 5%, *** significant at 1%
Dependent variable is measured level of hemoglobin in ml per liter.
Regressions also control for child age, orphanhood, parents education, ethnicity, religion, household assets and village fixed effects (not reported).