Educational and Child Labor Impacts of Two Food for Education Schemes: Evidence from a Randomized Trial in Rural Burkina Faso*

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Abstract

This paper uses a prospective randomized trial to assess the impact of two food for education schemes on education and child labor outcomes for children from low-income households in northern rural Burkina Faso. The two food for education programs under consideration are, on the one hand, school meals where students are provided with lunch each school day, and, on the other hand, take home rations which provide girls with 10 kg of cereal flour each month, conditional on 90 percent attendance rate. After the program ran for one academic year, both programs increased enrollment by 3 to 5 percentage points. The scores on mathematics improved for girls in both school meals and take-home rations villages. Conditional on enrollment, the interventions caused attendance to decrease, but this was mainly driven by lower attendance among new enrollees. The interventions also led to adjustment in child labor, with children (especially girls) with access to food for education programs, in particular the take home rations, shifting away from on farm labor and off-farm productive tasks which possibly are more incompatible with school hours.

1. Introduction

While universal primary school attendance is a stated goal by many governments and the millennium development goals (MDG), enrollment rates remain 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 food for education 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. While there is a similar literature on food for education programs (FFEs) seldom do these studies 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 food for education schemes in the same environment.

In general, three objectives can be directly associated with food for education programs (e.g. Adelman et al., 2007; Levinger, 1986). First, FFEs can motivate parents to enroll their children and see that they attend school regularly. Second, FFEs can improve the nutritional status of school age children over time, and alleviate short-term hunger in malnourished or otherwise well-nourished schoolchildren. Third, FFEs 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, FFEs could also improve the nutritional status of household members who are not in school, especially when FFEs entail take home rations. Overall, FFEs are appealing because if properly designed and implemented they lead to increased number of children being enrolled with better academic performances. In addition to these impacts on education, FFEs can also serve as a scalable safety net to support the consumption of low income families (Bundy et al, 2009).

The two forms of FFEs that we consider consist of school meals and take home rations (THR). Under a 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 FFEs has its specific merits. Meals served at schools go directly to the students who are supposed to benefit from the program. However, parents could react by reallocating food in the household away from these children. Food received by the household under THR is more likely to be shared by other household members, possibly reaching children who may be in as much or greater 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 depends both on the policy objectives as well as on the evidence base. The latter empirical question is addressed by this paper.

The paper uses a randomized experiment to assess the relationships between FFEs on the one hand, and enrollment, attendance, academic performance, and cognitive development on the other hand. The focus of this study is the Sahel region of northern Burkina Faso in West Africa. Northern Burkina Faso is an appropriate context to evaluate the impact of food for education programs for two main reasons. First, the region has some the world’s lowest primary school participation. On average only 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 by rigorously evaluating the impact of two alternative food for education 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.

We find that both school meals and THR increase enrollment for boys and girls by about

4 to 6 percentage points. We find a small increase in scores of mathematics for girls and boys in the full sample, but the estimated impacts are statistically for girls who are enrolled in school.

The impact on attendance is unexpected since students who were exposed to the interventions have lower attendance on average. We show that the interventions caused attendance to decrease in households that are low in child labor supply while attendance did not change for households which have a relatively large child labor supply. This may indicate that labor constraints matter. Further, we find suggestive evidence that attendance (conditional on enrollment) mainly decreased for children who would not have enrolled absent of the interventions. Even with the somewhat greater risk of absenteeism this group, nevertheless, clearly had more schooling that if they had not enrolled at all. The reduced attendance is likely linked to the weak impacts on learning outcomes that we observed. The interventions also led to adjustment in child labor, with children (especially girls) with access to food for education programs, in particular the take home rations, shifting from on farm labor and off-farm productive labor, which possibly are more incompatible with school hours.

The paper is organized as follows. We provide a brief review on food for education programs 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 Food for Education Programs

FFEs seek to induce a change in household behavior, with the goal of improving educational and nutritional outcomes (Grantham-McGregor, Chang, and Walker, 1998). The rationale is that by subsidizing schooling costs, FFEs can induce parents to invest more in their children's education than they would have in the absence of the program (e.g. Adelman, Gilligan, and Lehrer, 2007). Additionally, FFEs can make investments in education more efficient.

FFEs 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 programs since the 19th century in the US), 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). FFEs 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, FFEs are similar to conditional cash transfers which have been the focus of a growing body of empirical research.

The benefits of FFEs are, arguably, very large (Adelman, Gilligan, and Lehrer, 2007, Adelman et al., 2008; Ahmed, 2004). First, nutritional and health statuses 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 impacts their cognitive functions and reduces 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, FFEs can have a far reaching impact on children nutritional and health status and how they perform in school. Criticisms of FFEs primarily revolve around their costs as well as whether the resources can achieve more educational or nutritional outcomes if allocated to other programs.

Few studies, however, have the details to assess some of the potentially offsetting adverse impacts of FFEs 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 FFEs. 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 or the peer effects of new students can influence the learning environment for all the students (Ahmed, and Arends-Kuenning, 2006). 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 FFEs are reported to have produced mixed results on academic performances at best. Grantham-McGregor, Chang, and Walker (1998) show that in Jamaica, learning outcomes deteriorated in poorly organized schools following the introduction of a school breakfast program. Ahmed and del Ninno (2002) find that take home rations were effective in increasing enrollment and attendance in Bangladesh, but academic performance measured by standardized tests was lower than in schools that did not benefit from 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 FFEs 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, FFEs would not improve academic performances (e.g. General Accounting Office, 2002).

In an earlier similar work, Ravallion and Wodon (2000) used household participation in food for education (FFE) program in Bangladesh to identify the effect of child labor on school participation. The authors found that food for education increased schooling by far more than it reduced child labor. The current study differs, however, from Ravallion and Wodon in three ways. First, ours is an evaluation of two different food for education interventions, unlike Ravallion and Wodon. Second, because we have more detailed information on the type of labor, and thus allows us to investigate the shifts across of types of labor. Third, in addition to school participation, we are also able to investigate attendance and learning outcomes.

3. Program Descriptions and Research Design

School canteens which provide meals to the students attending school were first introduced in Burkina Faso by the Catholic Relief Service/Cathwell (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 targeted only 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 food for education 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: 16 schools to school meals, 16 schools to take home rations and 14 schools to a control group. The randomization took place 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[2].

The data from three villages were not properly collected, and the data were miscoded in one village. We have dropped these villages, two villages in the control and one village in each treatment arm. Therefore, the analysis uses 15 villages in each treatment arm, and 12 villages in the control, i.e. 42 villages in total. We verify in table A1 (in the appendix) that our randomization would be balanced if we had used the full set of villages. Furthermore, the mean values of the key variables reported in table 1 are similar to those shown in table A1, indicating that excluding the four villages does not introduce selection issues. We summarize the experimental design in figure 1.

Different programs were implemented in the two intervention groups: school meals and THR. Under the school meals intervention, lunch was served each school day. The only requirement to have access to the meal is that the pupil be present. Both boys and girls were

[2] The trial was originally scheduled to last two years but the implementers were reluctant to continue the random assignment and the control group into the second year.

eligible for the school meals intervention. The THR stipulated that each month, each girl would receive 10 kg of cereal flour, conditional on a 90 percent attendance rate. Attendance records were maintained by the school administration, according to the standard policies applied by the Ministry of Education.

The school meals cost $41.46 per student per year while the take home ration was $51.37. Both cost estimates are from the WFP office in Ouagadougou (the national capital city) and are inclusive of transport and other operational costs. The value to the household, however, may differ from the program costs since it is based on what the household might have to pay to purchase the equivalent food and services locally.

In both cases, WFP has developed a quarterly delivery schedule, and the food staples were stored within the school. In keeping with existing local policy, boys were not eligible for the THR program. The teachers 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 4236 school age children (i.e. aged between 6 and 15).[3] There are about 1493 students in the school meals villages, 1498 in the THR villages and 1245 in the control villages.

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. The field work differs from many foods for education evaluation studies, not only in its randomized assignment of

[3] Adelman et al. (2008) claim that few studies of food for education use a population based sample rather than a school based one. This limits the ability to understand reasons for non-enrollment as well as any spillover effects.

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 where 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, 28 percent of children in the school meals villages, 24 percent in the take home villages and 24 percent in the control villages were enrolled in schools[4]. In both treatment and control villages, only a small fraction (about 17 percent) of those children who are enrolled has 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 got less than half the answers correct. The anthropometric data point to severe nutritional problems. Weight-for-age and height-for-age are two standard deviations below the reference population[5], corroborating our observation that the region may be facing substantial food shortages.

Before proceeding with the analysis, we explore attrition within our sample. Even if the treatment and control groups are similar at baseline, changes in the composition of the groups between baseline and follow-up could result in biased estimated program impact. Table 2 shows

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

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

that attrition rates were low, at about 3 percent level. Such attrition rates are within the range of attrition rates from developing countries (e.g. Alderman et al, 2001). Moreover, we show in the last two columns that attrition rates were not significantly different between treatment and control villages. Therefore, we argue that our results are not influenced by attrition.

The math and the cognitive tests required meeting the child face-to-face unlike enrollment status, attendance and child labor that could be reported by parents. Hence a child who was reported as not having attrited could have still failed to take the math and cognitive tests if the enumerators could not meet with him or her face to face. Moreover, a child who was present may still have declined to take the tests. We analyze this type of selective non responses in table 3. Columns 1-3 look at whether a child who is listed on the household roster has been survey for enrollment, child labor and attendance. Our estimates indicate that the probability of child being surveyed did not differ across treatment and control groups.

In columns 4-7 we examine whether the likelihood of taking the math and the cognitive tests differed across treatment and control groups. The regression analysis indicates that the likelihood of taking both tests also did not differ across the two treatments arms and the control groups. Since non- response is not correlated with program placement, the available samples should give an unbiased estimate of the program impacts.

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, Glennerster and Kremer, 2008). Our identification strategy relies on the random assignment of the villages to treatment and control groups. Because of this random assignment the estimated program impact has a causal 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) effect, 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 limited 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.

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

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

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

Therefore, the AIT is given by

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

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.

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 T_i * F + \beta_3 F + \beta_4 X_{it} + \mu_{it} \qquad (4)$$

Where y_i is the outcome of interest measured at the child level, T_i is the treatment indicator, Xi 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[6].

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 crossover would lead to an underestimation of the impact. However, children from control villages attending school in treatment villages are not likely to be a concern, since treatment and control villages were not contiguous, and sending children from control to treatment villages would have required those children to walk long distances. We also verified the impact of the interventions on fostering behaviors, the second type of cross over. The results (available upon request) indicated that intervention villages ended up taking in more children and sending fewer out with the net take being higher in school meals villages. This would suggest that if anything, the intervention impact that we report is biased downward if fostered children were not previously enrolled. We address the potential bias that fostering could introduce by presenting the program impact on enrollment with and without including fostered children.

Eligibility into the interventions differed for boys between the treatment arms. In particular, boys and girls of school age are eligible in the school meals villages. In the THR villages, school age girls are eligible, but boys eligible only if they have school age sisters. Nevertheless, we treat all boys in these villages as eligible. Nevertheless, we treat all boys in these villages as eligible.

[6] In each round, we consider children aged 6 to 15 years. Hence, in the second round we include children who were 5 years old at baseline (i.e. who would not have started school), and we exclude children who were 15 at baseline and should have passed elementary school.

5 Estimation Results and Discussion

5.1. Enrollment

Table 4 reports the program impact on enrollment status, i.e. whether a child is registered in school at the time of the survey. The sample covers children aged 6 to 15 years at the time of the survey. The results in columns 1 to 3 include fostered children, while these children are excluded from the estimation in columns 4-6. All regressions control for age dummies and village fixed effects, and standard errors are clustered at the village level[7]. The last row shows the p-values of rejecting a null hypothesis that the two programs have equal impacts on enrollment

The program impacts on children in villages which were randomly selected to receive school meals are shown in the first row. These children were on average 4 percentage points more likely to be enrolled (column 1). The point estimate is larger on boys (4.7 percentage points—column 2) than on girls (3.5 percentage points as shown in column 3), but the two coefficients are not statistically different at the five percent level since the 95 percent confidence interval overlap. The program impacts on children from THR villages are shown in the second row. The overall impact is 4.8 percentage (column 1), significant at the one percent level. Enrollment increased by 4.5 percentage points for boys (column 2), and girls by 5.3 percentage points for girls (column 3). The point estimates are significantly different than zero at five percent and the one percent levels, respectively.

The THR intervention targeted girls, but we find that that boys have as large a response to the intervention as girls (the point estimates are 0.45 and 0.53, respectively (in fact the 95 percent level confidence interval indicates that the point estimates for boys and girls are statistically similar). Moreover, a boy in the treatment villages with THR has as good a chance of increasing

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[7] Our results are robust to alternative specifications which include other controls, specifically household and mother education levels, household size, and household assets.

enrollment with no sister as with one. It would be rather cost effective if one could, in various settings, provide a benefit only to girls yet get increased enrollment across the board. Our findings are are consistent with previous studies. For example, Kim, Alderman and Orazem (1999) found that a program that targeted girls in Quetta, Pakistan induced parents to invest in boys as well. In Burkina Faso, a girl-friendly schools intervention increased enrollment for boys as well (although the impact on girls was larger) relative to schools which were not in the program (Kazianga, Levy, Linden and Sloan, 2010). In Busia, Kenya, Kremer, Miguel and Thornton (2009) found significant effects of a girl scholarships scheme on male classmates who were not eligible for the scholarships. The authors suggested that these positive externalities could be due to higher teacher attendance and positive peer effects among students.

In columns 4-6, we repeat the same regressions, with fostered children excluded[8]. The magnitude of the program impact increases slightly, but the results remain qualitatively the same. This would suggest that either more children are fostered in control villages or that treatment villages are getting non-enrolled foster children. Regardless of the explanation, it is apparent that including fostered children does not drive the results and make for a project impact that would be lost if the interventions were scaled up.

Compared to other school feeding programs, this program thus appears to be quite effective in increasing enrollment, at least in the short run. For example, Afridi (2007) finds that India's national meal program led to attendance and small enrollment increases among girls. He (2009) finds that a Sri Lankan national program led to increased enrollments by 5.9 percent.

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. There are two main reasons one would expect a positive impact of FFEs on learning outcomes. First, if more children enroll because of the interventions, then one would expect the average learning outcome to increase over the full sample. Second, for children who are enrolled,

[8] Fostered children are non-biological children who were not living in the household at baseline.

the interventions may improve learning outcomes through regular attendance. Moreover, by reducing short term hunger the interventions would increase children's 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 possible to find some variation among the population (Yamauchi, 2008).

The results are reported in table5 for children between aged 6 to 15 years[9]. In columns 1-3, we show the estimation results for all children, regardless of enrollment status. In columns 4-6, we focus on the subsample of children who were enrolled at the time of the survey. As shown in column 1, the proportion of correct answers increased by 9.6 % for children who were exposed to the school meals intervention (first row) and by 8.4 percent for children from the THR villages. The point estimates are statistically significant at the one and five percent levels, and are not statistically different from each other Program impacts on boys and girls are shown in columns 2 and 3, respectively. Boys' math test scores increased by 7.9 percent in school meals villages (significant at the 10 percent level) and by 7.4 percent in THR villages (not statistically significant). In contrast, girls scores test increased by 11.3 in school meals villages (significant at the one percent level) and by 9.4 percent in THR villages (significant at the five percent level). Overall, the intervention had similar positive impacts on math outcomes, and the he point estimates are larger for girls than boys.

If children who enroll become better at completing these basic math questions, then the positive impact on learning outcome could be explained by the increased enrollment that we have shown above. These would corroborate recent findings from rural Burkina Faso which show that substantial increased enrollment caused by upgraded school infrastructures and environment resulted in improved learning outcomes (Kazianga et al, 2010).

Estimated impacts on children who were enrolled at the time of the survey are shown in

[9] Giving the math tests required that enumerators meet face to face with children. When after three attempts the enumerator could not meet the child in person, the test was not administered. Therefore, the test sample size is about 14 percent smaller for the test outcomes. We verified that there is no significant difference in the results on enrollment for this sample. These are available from the lead author.

columns 4-6. Although point estimates are positive in column 4, none is significant, implying for children who were enrolled at the time of the survey, the interventions did not have any discernible effects on boys and girls pooled together[10]. In column 5, we show that there is no significant effect on boys. Only the point estimate of girls shown in column 6 is significant..

5.3. Cognitive Development

During the follow up survey children between 5 and 15 were administered the Raven's progressive matrices tests and revised Wechsler Intelligence Scale for Children (WISC). The Raven's CPM is a measure of fluid intelligence or problem solving ability. The test asks candidates to identify the missing segment required to complete a larger pattern, and thus does not depend heavily on verbal skills, making it relatively "culture free" (Borghans, Duckworth, Heckman, and Weel 2008). The WISC digit span is a measure of short term working memory and ability to concentrate. The test assesses the number of digits a child can retain and recall correctly. The test has both a forward and backward component (Weschler 1974). For both the WISC and Raven scores, we use adjusted z-score to control for age effects.

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 (an assumption consistent with the data in table 1). The estimations results are presented in tables 6 for the Raven's CPM tests. In columns 1-3, we report the results for the full sample. In column 4-6, we show the results for children who were enrolled at the time of the survey. The results indicate that, looking at AIT, 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

[10] This could be explained if the interventions induce parents to enroll children that they would have not enrolled otherwise. For instance, negative impacts on learning outcomes have been reported from Northern Uganda by Adelman et al. (2008). Likewise Grantham-McGregor, Chang, and Walker (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 a feeding program was likely to have a negative impact in schools which were not so well organized.

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[11].

5.4. Attendance

The survey collected information on the number days a child was absent from school in May, i.e. the month preceding the survey. We convert this information into the number of days present out of a total of 20 school days (Ahmad, 2004). Estimates of the interventions' impact on attendance are shown in table 7. In columns 1-3, we show the estimates for all school age children, regardless of enrollment status. In this case, we are reporting the program combined impact on enrollment and attendance. In columns 4-6, the sample is restricted to children who were enrolled in school at the time of the surveys. The results reported in columns 1-3 indicate that compared to the control group, attendance increased by 0.7 day for school age boys and girls who resided in school meals villages (first row), and the point estimate is significant at the five percent level. When taken separately, boys' attendance shown in column 2 increased by 0.9 day (significant at the 5 percent level). In contrast, the point estimate of girls' attendance reported in column 2 (0.5 day) is smaller and not statistically different from zero at the 10 percent level. The THR intervention impacts on attendance are reported in the second row of columns 1-3 for the full sample. The point estimates indicate that attendance increased by about 0.9 day for boys and girls taken together (column 1), and by about a day for boys (columns 2) and by 0.8 day for girls (column 3). Both point estimates are significant at the five percent level. While this relatively large effect on boys' attendance for an intervention that targeted girls is puzzling, it provides more suggestive evidence that the THR intervention did not crowd out boys. As mentioned the positive impact of THR on boys is also consistent with other findings in rural Burkina Faso (Kazianga et al, 2010), Pakistan (Kim et al, 1999) and Kenya (Kremer et, 2009), for example.

In columns 4-6, the sample is restricted to children who were enrolled at the time of the survey. The point estimate in column 4 indicates that students who were receiving school meals missed on average 0.85 (significant at the 5 percent level) day more than the students in the control

[11] We did not find any significant effects using the WISC tests either. For the sake of space, we do not report these results, but they are available from the authors upon request.

group. When the estimation is disaggregated for boys (column 5) and girls (column 6), it is apparent that attendance decreases for girls (-1.2 day, and significant at the one percent level). For boys, the point estimate is negative (-0.57) but is not statistically different from zero. The estimates for the THR are shown in the second row of columns 4 to 6. Attendance decreased by 1 day for girls (column 6, significant at the one percent level). In contrast, there were no statistically detectable changes for boys and girls taken together, neither for boys as shown in columns 4 and 5, respectively. As indicated by the p-values testing for different impacts across programs, both school meals and THR have similar impact on attendance, except in column 5 (boys currently enrolled) where the difference is statistically significant, indicating that for boys attendance decreased more in school meals villages than in THR villages.

Taken at face value, these results suggest the program had no impact on enrolled boys' attendance and led to relatively lower attendance among enrolled girls. There are two possible explanations for this result with somewhat different welfare implications. First, it is possible that the negative impact in attendance among enrolled girls results entirely from lower attendance by girls who would not have enrolled absent of the intervention. For these girls the alternative would have been zero attendance. Thus, overall, no child attends less because of the interventions. Hence (in terms of attendance) the interventions would be unambiguously welfare improving. Alternatively, the interventions could have lowered attendance among children who would have enrolled in the absence of the interventions. If this were to happen, the interventions would be causing these children to receive less schooling. We elaborate on these two possible explanations below.

One can imagine a first scenario where the lower attendance is explained by a household's strategic behavior in the absence of labor markets. For farm households, school investment decisions are associated with decisions regarding time allocation of various household members. It is reasonable to assume that the opportunity costs of schooling increases as the wages for child labor increase. In the absence of a functioning labor market, wages of child labor are endogenous and are given by the marginal product of child labor (e.g. Singh, Squire and Strauss, 1986). In particular, with thin labor market, the marginal product of child labor decreases with household size. Hence when households are provided with incentives for schooling, decisions on children's allocation of time may vary according to the contribution of child labor in the household production function (e.g. Orazem and King, 2008).

Consider two identical households A and B where A is the treated and B is the control. Each household has two school age girls. In the absence of the program one girl is enrolled and the other one 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 and Fayed, 1998; Ray, 2000,) 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 off 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.

In a second scenario, suppose the introduction of the school feeding scheme induces some households who would not have even enrolled their children (given some fixed cost of enrollment) to do so. These households send their children to school just enough to meet the THR threshold in that treatment arm, and otherwise in the school meals treatment they choose attendance days to equate the marginal values and costs (including foregone labor) of schooling. Now, because previously enrolled children only miss an average of around 1 day of schooling per month at baseline, it is possible for these new children to bring down the average attendance even while satisfying the THR threshold. Under this model, attendance decreased only for children who would not have enrolled without the interventions.

We can use the number of school age children (6-15) to empirically test these scenarios. We consider the presence of other (i.e. other than the student) children between 6 and 15 years old in the household at the time of the survey, independently of whether or not those other children 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 (ATT) on absenteeism for each group.

The estimation results are reported in table 8. Columns -3 show the results for households with no other school age child than the student. This case is special since it rules out substitution effects across children. The interventions have an unambiguous negative effect on attendance. In villages which received school meals, boys and girls taken together missed on average 1.8 day as shown in column 1 (significant at the one percent level). When taken separately, boys missed 1.3 day (column 2) and girls missed 2.1 days (column 3). The point estimates are significant at the one percent level in each case. The estimated intervention effect shown in the second row column (1) indicates that in the THR villages, boys and girls with no school age sibling missed 1.5 day (significant at the one percent level). Boys missed on average 1.2 days (column 2) and girls missed on average 1.8 days (column 3). The point estimates are significant at the one percent level in each case.

We repeat the same regressions in columns 4-6 for students with one to three school age siblings and in columns 7-9 for students with four or more school age siblings. Across all specifications in columns 4-6, the point estimates although still negative are smaller in magnitude (less than 0.5 day), and statistically not different from zero at the ten percent level. For students with four siblings or more, girls in school meals villages missed -0.4 day (column 9), while attendance increased by 1.2 days for boys in THR villages (column 8). Despite the discrepancy in column 9 (first row) (where the estimated impact on girls' attendance is negative and significant at the five percent), the overall pattern of the estimates suggest that enrolled children who were exposed to the interventions were less likely to miss school if they had one or more school age sibling.

The evidence presented in Table 8 seems to partly distinguish the two mechanisms. If attendance is reduced because households 'game' the FFE schemes by enrolling extra children and cutting down on the attendance of each child, in effect allowing for easier sharing of the absent days (that is, days taken off for labor) among all students, then there should not be a negative effect on attendance for children with no siblings. Instead, the largest negative effects are found for children with no other sibling. This is consistent with a model in which the negative attendance effect is driven by selection into schooling of households with lower value for education.

In table 9, we run separate regressions for new enrollees and other enrollees. The sub-sample of old enrollees (columns 1-3) consist of children who were already attending school before the interventions started, hence we are certain that enrollment were not induced by the interventions. The sub-sample of new enrollees (columns 4-6) consist of children would have enrolled absent of the interventions and those who enrolled because of the interventions. The evidence shows that the negative impacts on attendance are confined to the sub-sample of newly enrolled children. These results, while only suggestive, support the scenario in which attendance (conditional on enrollment) is lower for children who would not have enrolled absent of the program.

5.5 Child labor

In Table 10 we summarize the average intent to treat (AIT) impact of the interventions on child labor. The survey asked whether a child participated in a specific task during the last week the school was open. We regrouped the different tasks in three categories[12]: any form of child labor including all possible tasks (panel A); productive work including farm work, non-farm work (which is not household chores: e.g., keeping a small shop) and livestock herding (panel B), and domestic work including fetching water, fetching fire wood, tending for younger siblings and household chores (panel C)[13]. Columns 1-3 show the estimation results for the full sample and columns 4-6 show the results for the subsample of children who were enrolled at the time of the survey.

The results shown in columns 1-3 of panel A indicate that the interventions did not have large effects on child labor participation, with the exception of boys in school meals villages (column 2).. Noticeably, boys from school meals villages (first row) are 6.4 percent more likely to engage in any task than boys from the control villages. In contrast, labor participation decreased by 6.4 percent (second row) for boys in the THR villages. The point

[12] Results for each activity separately are presented in the working paper version of this article (Kazianga, de Walque and Alderman, 2009).

[13] The full regressions are available upon request.

estimates are significant at the 10 percent level, and the null hypothesis that the estimates are equal is rejected at the one percent level. We observe a similar pattern when the sample is restricted to children who are currently enrolled (columns 4-6). Labor participation increased for boys from school meals villages (column 5, first row) while it declined for boys from THR villages (column 5, second row). In contrast, neither intervention has a statistically significant effect on girls' labor participation (column 6).

In panel B and C, we consider child participation to productive tasks and to household chores, respectively. Child participation to productive labor decreased in intervention villages (panel B), although the point estimates are not significant for the school meals villages. In THR villages, children who are enrolled at the time of the survey participate less than children from the control villages. Boys and girls taken together are 21 percent less likely to engage in productive labor (column 4), and the point estimate is significant at the one percent level. This pattern holds when boys (column 5) and girls (column 6) are considered separately. A priori there is no reason why THR would reduce boys' participation to child labor. It is plausible, however, that the income effect from the food transfers is large enough to reduce child labor (e.g. Edmonds, 2005, 2008).

Most of the estimates in panel C are not statistically different from zero. This indicates that the interventions did not change child participation in household chores significantly, except for girls in the THR villages (column 3, second row) whose participation in household chores increased by 7.6 percent (significant at the 10 percent level). The point estimates are, however, consistently positive for girls and negative for boys, providing very weak evidence that girls and boys participation to household chores moved in opposite directions. Such pattern would be consistent in a context where labor is divided along gender lines, and household chores are perceived as women's responsibilities.

Overall, the evidence is suggestive that the interventions did not eliminate child labor, but instead altered the allocation of child labor (especially in THR villages). In particular, children participated less in productive activities which the children may not be able to combine with school activities.

6. Conclusion

In this paper, we have used a prospective randomized design to assess the impact of two food for education schemes on educational and child labor outcomes of children from low income household in northern rural Burkina Faso. We considered two programs: school meals which provide lunch in school, and take home rations which provide girls with 10 kg of cereal flour each month, conditional on 90 percent attendance rate. Because we can rely on a baseline and on a follow up surveys, we were able to use difference in difference regressions to estimate the impact of the programs. 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 the school meal program increased both boys and girls' enrollment by 5 and 4 percentage points, respectively. The THR, targeted only to girls, also increased enrollment of girls by more than 5 percentage points. Interestingly, the THR intervention also increased enrollment among boys by 4.5 percentage points. These gains in percentage correspond to 19 percent and 28 percent increase over girls' initial enrollment (18 percent) in school meals and in THR villages respectively. For boys in, the five percentage points correspond to 20 percent increase over initial enrollment (25 percent) in school meals villages, and to 21 percent increase over initial enrollment in THR villages (24 percent).

There is no evidence that indicates one modality of transfer dominates the other. The scores on mathematics improved for girls in both school meals and take-home rations villages. Attendance, unconditional on enrollment increased. Attendance conditional on enrollment (i.e. among children who were enrolled at the time of the survey), however, decreased. We argue that this reflects heterogeneity among the program beneficiaries: the increased enrollment could be accompanied by higher absenteeism rates if there is no an active labor market and some households are labor constrained and/or child labor is complementary to adult labor. We show that the interventions caused absenteeism to increase in household who are low in child labor supply while absenteeism decreased for households which have a relatively large child labor supply. We also find suggestive evidence that attendance (conditional on enrollment) decreased for children who would not have enrolled absent of the interventions. Even with the somewhat greater risk of absenteeism this group, nevertheless, clearly had more schooling that if they had not enrolled at all. The interventions, in particular the take home rations modified 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.

Overall, our results show that food for education programs in this specific context of agricultural households without an active labor market can increase enrollment, but are not always as successful in improving attendance and academic performance among enrolled children. This calls for more investigation of the circumstances under which food for education 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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Figure 1: Experimental design

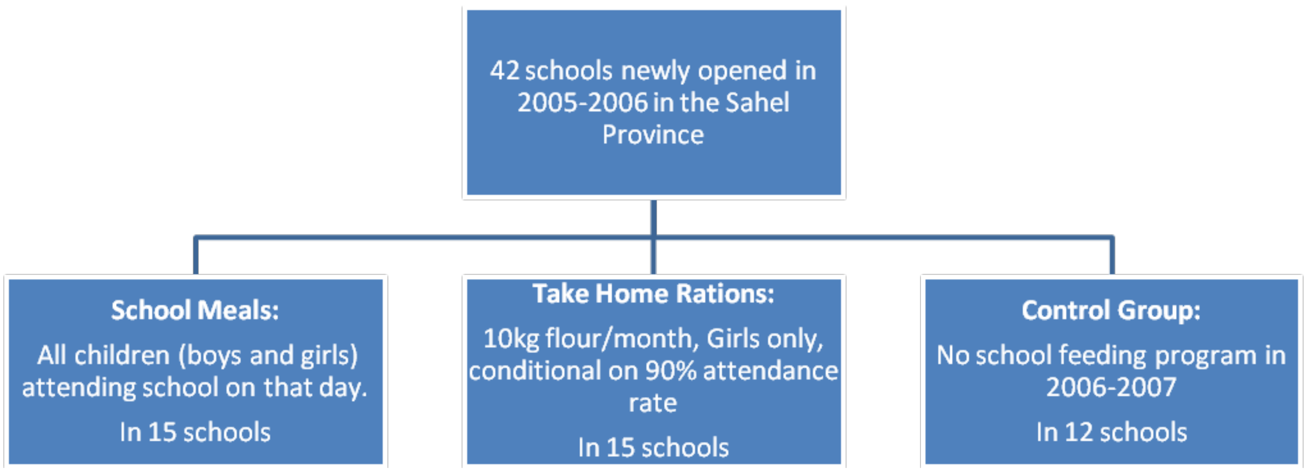

Table 1: Key variables at baseline

	(1)	(2)	(3)	(4)	(5)
Variable	Meals	THR	Control	Difference with control	
				Meals	THR
		Child level variables			
Enrolled	0.270***	0.232***	0.251***	0.018	-0.020
	[0.035]	[0.031]	[0.028]	[0.044]	[0.041]
Math answers	0.414***	0.434***	0.413***	0.002	0.021
	[0.030]	[0.036]	[0.038]	[0.049]	[0.053]
Math time-adjusted	259.222***	251.292***	264.161***	-4.939	-12.868
	[9.369]	[9.817]	[9.367]	[13.248]	[13.568]
Days missed in May	0.522***	0.635***	1.342***	-0.820*	-0.707
	[0.188]	[0.175]	[0.405]	[0.446]	[0.441]
Child labor (any)	0.854***	0.870***	0.859***	-0.004	0.011
	[0.017]	[0.019]	[0.019]	[0.026]	[0.027]
Child productive labor	0.671***	0.637***	0.608***	0.063	0.029
	[0.028]	[0.031]	[0.037]	[0.046]	[0.048]
Farm labor	0.618***	0.595***	0.578***	0.041	0.018
	[0.030]	[0.032]	[0.042]	[0.051]	[0.052]
Non farm labor	0.290***	0.236***	0.159***	0.131**	0.077
	[0.042]	[0.055]	[0.034]	[0.054]	[0.064]
Household chores	0.642***	0.656***	0.692***	-0.051	-0.036
	[0.024]	[0.033]	[0.031]	[0.039]	[0.045]
Cooking	0.336***	0.315***	0.342***	-0.006	-0.027
	[0.021]	[0.023]	[0.023]	[0.031]	[0.032]
Fetch water	0.470***	0.493***	0.538***	-0.068	-0.045
	[0.029]	[0.042]	[0.042]	[0.051]	[0.059]
Fetch wood	0.359***	0.396***	0.348***	0.012	0.048
	[0.024]	[0.032]	[0.040]	[0.046]	[0.051]
Tend youngsters	0.249***	0.198***	0.186***	0.063	0.012
	[0.027]	[0.024]	[0.032]	[0.042]	[0.040]
Other hh chores	0.395***	0.388***	0.415***	-0.020	-0.027
	[0.016]	[0.026]	[0.027]	[0.031]	[0.037]
weight (kg)	23.064***	23.397***	22.707***	0.357	0.690
	[0.726]	[0.706]	[0.682]	[0.996]	[0.982]
height (cm)	125.473***	125.538***	124.736***	0.737	0.802
	[1.082]	[1.317]	[1.524]	[1.869]	[2.014]
Body mass index	14.365***	14.569***	14.356***	0.009	0.214
	[0.287]	[0.192]	[0.212]	[0.357]	[0.286]

Weight-for-age (6-60 months)	-2.216***	-2.522***	-2.323***	0.106	-0.200
	[0.179]	[0.159]	[0.178]	[0.252]	[0.239]
Height-for-age (6-60 months)	-2.383***	-2.085***	-2.318***	-0.066	0.233
	[0.156]	[0.111]	[0.157]	[0.222]	[0.193]
Weight-for-height (6-60 months)	-0.779***	-1.128***	-0.907***	0.128	-0.222
	[0.148]	[0.107]	[0.166]	[0.223]	[0.198]
Registration fee	293.578***	548.495***	297.122***	-3.544	251.373
	[84.077]	[153.750]	[88.441]	[122.028]	[177.372]
Educ expenditures	2530.260***	3044.398***	2556.115***	-25.855	488.283
	[323.377]	[471.534]	[313.446]	[450.356]	[566.209]
PAFees	716.667***	711.371***	832.194***	-115.528	-120.823
	[83.938]	[93.370]	[79.484]	[115.599]	[122.620]
Child is boy	0.496***	0.520***	0.508***	-0.012	0.012
	[0.008]	[0.017]	[0.012]	[0.014]	[0.021]
age	9.775***	9.790***	9.842***	-0.067	-0.052
	[0.074]	[0.080]	[0.084]	[0.112]	[0.116]
Father has some formal ed.	0.015***	0.024**	0.021***	-0.006	0.003
	[0.005]	[0.012]	[0.006]	[0.008]	[0.013]
Father has some Koran ed.	0.142***	0.164***	0.201**	-0.058	-0.037
	[0.035]	[0.040]	[0.085]	[0.092]	[0.094]
Mother has some formal ed.	0.004	0.011	0.012***	-0.008	-0.001
	[0.003]	[0.007]	[0.004]	[0.005]	[0.008]
Mother has some Koran ed.	0.015*	0.060***	0.107	-0.092	-0.048
	[0.008]	[0.020]	[0.076]	[0.076]	[0.078]
Maternal orphan	0.025***	0.029***	0.022***	0.003	0.007
	[0.006]	[0.010]	[0.006]	[0.009]	[0.012]
Paternal orphan	0.039***	0.070***	0.060***	-0.021	0.009
	[0.009]	[0.010]	[0.017]	[0.020]	[0.020]
	Household level variables				
Head age	43.076***	45.669***	45.089***	-2.013	0.580
	[1.103]	[1.191]	[1.348]	[1.742]	[1.799]
Head is male	0.977***	0.978***	0.978***	-0.001	0.000
	[0.005]	[0.007]	[0.007]	[0.009]	[0.010]
Head is Mossi	0.138**	0.094**	0.113**	0.025	-0.018
	[0.067]	[0.045]	[0.053]	[0.085]	[0.069]
Head is Fulani	0.413***	0.460***	0.418***	-0.005	0.043
	[0.092]	[0.101]	[0.114]	[0.147]	[0.152]
Head is of Blacksmith descent	0.044**	0.027***	0.019***	0.025	0.008
	[0.022]	[0.010]	[0.005]	[0.022]	[0.011]
Head is of Noble descent	0.400***	0.370***	0.557***	-0.157	-0.187*
	[0.086]	[0.071]	[0.088]	[0.123]	[0.113]
Head is of Captive descent	0.340***	0.391***	0.162***	0.178	0.230**

	[0.093]	[0.085]	[0.062]	[0.112]	[0.105]
Head is Muslim	0.965***	0.978***	0.987***	-0.022	-0.008
	[0.019]	[0.012]	[0.008]	[0.021]	[0.015]
Hh. asset value (1000 CFA)	70.981***	92.805***	79.774***	-8.793	13.031
	[12.630]	[20.061]	[6.904]	[14.394]	[21.215]
Sample size	1493	1498	1245		

Robust standard errors in brackets.
* Significant at 10%; ** significant at 5%, *** significant at 1%
1 USD = +/- 500 CFA Francs.
Mossi and Fulani are two ethnic groups from the region. Blacksmith, Noble or Captive descents are castes used to categorize households within these ethnic groups.

Table 2: Proportion of school age surveyed at baseline who were not found at follow up

All sample	Meals	THR	Control	Difference with control	
				Meals	THR
		Boys and girls			
0.028	0.018	0.038	0.029	-0.011	0.009
[0.004]***	[0.006]***	[0.006]***	[0.008]***	[0.009]	[0.010]
		Boys			
0.018	0.014	0.026	0.015	-0.001	0.011
[0.004]***	[0.005]***	[0.006]***	[0.007]***	[0.008]	[0.009]
		Girls			
0.038	0.023	0.052	0.043	-0.020	0.009
[0.006]***	[0.008]***	[0.012]**	[0.011]***	[0.014]	[0.017]

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

Table 3: Analysis of attrition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: 1 if						
	surveyed at:			answered math questions at:			Answered
	baseline	follow up	baseline & follow up	baseline	follow up	baseline & follow up	Cognitive Tests
School meals	-0.015	-0.040	-0.055	0.010	-0.035	-0.026	-0.041
	[0.017]	[0.028]	[0.037]	[0.020]	[0.029]	[0.031]	[0.052]
THR	-0.005	-0.011	-0.016	-0.024	-0.007	-0.032	-0.063
	[0.010]	[0.012]	[0.017]	[0.021]	[0.014]	[0.022]	[0.049]
Constant	0.956***	0.987***	0.943***	0.749***	0.978***	0.729***	0.764***
	[0.007]	[0.007]	[0.012]	[0.016]	[0.011]	[0.018]	[0.042]
Observations	4236	4236	4236	4236	4236	4236	3551
R-squared	0.001	0.016	0.009	0.003	0.013	0.004	0.019

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; ** significant at 5%, *** significant at 1% Dependent variable is whether the child is enrolled in school at the time of the survey.
Regressions also control for child age dummies, and village fixed effects (not reported).

Table 4: Program Impact on enrollment of children 6-15 years old

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Excluding fostered		
	Boys & Girls	Boys	Girls	Boys & Girls	Boys	Girls
School Meals	0.039**	0.047**	0.035**	0.046**	0.056**	0.039**
	[0.017]	[0.021]	[0.017]	[0.018]	[0.021]	[0.019]
THR	0.048***	0.045**	0.053***	0.052***	0.050**	0.054***
	[0.016]	[0.021]	[0.016]	[0.016]	[0.022]	[0.017]
Round2	0.006	-0.000	0.012	0.002	-0.005	0.009
	[0.011]	[0.014]	[0.011]	[0.011]	[0.015]	[0.013]
Girl	-0.072***			-0.073***		
	[0.011]			[0.011]		
Constant	0.137***	0.123***	0.081***	0.139***	0.123***	0.083***
	[0.017]	[0.020]	[0.017]	[0.017]	[0.020]	[0.017]
Observations	8,753	4,438	4,315	8,536	4,358	4,178
R-squared	0.102	0.098	0.123	0.103	0.098	0.123
Meals=THR	0.637	0.908	0.318	0.749	0.801	0.395

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; ** significant at 5%, *** significant at 1% Dependent variable is whether the child is enrolled in school at the time of the survey.
Regressions also control for child age dummies, and village fixed effects (not reported).
The last row of the table shows the p-values from an F-test testing the null hypothesis that estimated impacts of school meals and of THR are equal

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Currently enrolled		
VARIABLES	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls
Meals*Follow up	0.096***	0.079*	0.113***	0.038	-0.011	0.113**
	[0.036]	[0.042]	[0.036]	[0.036]	[0.046]	[0.048]
THR*Follow up	0.084**	0.073	0.094**	0.055	0.010	0.124**
	[0.041]	[0.047]	[0.041]	[0.036]	[0.045]	[0.051]
Follow up	-0.108***	-0.111***	-0.105***	-0.006	0.023	-0.054
	[0.033]	[0.039]	[0.032]	[0.024]	[0.035]	[0.037]
Girl	-0.010			0.001		
	[0.008]			[0.013]		
Constant	0.102***	0.096***	0.098***	0.329***	0.318***	0.356***
	[0.014]	[0.015]	[0.015]	[0.033]	[0.040]	[0.043]
Observations	7615	3861	3754	1864	1073	791
R-squared	0.441	0.449	0.446	0.267	0.304	0.258
Meals=THR	0.656	0.839	0.546	0.668	0.612	0.810

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; **significant at 5%, *** significant at 1%
The dependent variable in columns is the proportion of correct answers (ranges from 0 to 1).
Regressions also control for child age and village fixed effects (not reported).
The number of observations in columns 1-6 differs from the number of observations in columns 1-6 of table 3 because all test data are missing for some children. Enrollment status was reported by the parents, but administering the test required that enumerators meet with the children face to face. We verified that excluding children who did not take the math tests does not affect our enrollment results.
The last row of the table shows the p-values from an F-test testing the null hypothesis that estimated impacts of school meals and of THR are equal

Table 6: Program Impact on Cognitive Abilities: Raven Tests

	(1) Boys& Girls	(2) Boys	(3) Girls	(4) Boys& Girls	(5) Boys	(6) Girls
Meals	0.095	0.016	0.170	0.099	0.028	0.205
	[0.110]	[0.107]	[0.120]	[0.112]	[0.125]	[0.134]
THR	0.101	0.010	0.186	0.030	-0.081	0.170
	[0.106]	[0.102]	[0.123]	[0.113]	[0.117]	[0.141]
Girl	-0.023			0.062		
	[0.034]			[0.053]		
Constant	0.042	0.163	-0.105	0.154	0.241	0.072
	[0.121]	[0.113]	[0.148]	[0.166]	[0.200]	[0.231]
Observations	3,489	1,775	1,714	979	558	421
R-squared	0.009	0.018	0.015	0.018	0.037	0.018
Meals=THR	0.944	0.914	0.832	0.490	0.304	0.794

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; ** significant at 5%, *** significant at 1%
Dependent variable is z-score of number of correct answers to Raven colored progressive matrices.
Regressions also control for child age and province fixed effects. Village fixed effects are not included since the dependent variable is measured only during the follow up.
Note that we use only observations from the follow up round.
The last row of the table shows the p-values from an F-test testing the null hypothesis that estimated impacts of school meals and of THR are equal

Table 7: Program Impact on Attendance

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Children who are currently enrolled		
VARIABLES	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls
Meals*Follow up	0.694*	0.910**	0.504	-0.853**	-0.569	-1.116***
	[0.372]	[0.417]	[0.399]	[0.380]	[0.452]	[0.332]
THR*Follow up	0.908***	1.044**	0.811**	-0.483	0.010	-1.086***
	[0.334]	[0.405]	[0.377]	[0.360]	[0.430]	[0.337]
Follow up	0.262	0.040	0.459	0.981***	0.784**	1.149***
	[0.252]	[0.278]	[0.307]	[0.328]	[0.382]	[0.312]
Girl	-1.346***			0.057		
	[0.219]			[0.088]		
Constant	2.491***	2.177***	1.519***	18.908***	18.554***	19.581***
	[0.348]	[0.400]	[0.342]	[0.359]	[0.543]	[0.162]
Observations	8,414	4,230	4,161	2,062	1,187	873
R-squared	0.108	0.105	0.129	0.095	0.111	0.166
Meals=DR	0.501	0.679	0.360	0.109	0.054	0.839

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; ** significant at 5%, *** significant at 1% Dependent variable number of days missed in May.
Regressions also control for child age and village fixed effects (not reported).
The last row of the table shows the p-values from an F-test testing the null hypothesis that estimated impacts of school meals and of THR are equal

Table 8: Attendance (conditional on enrollment) and number of other children in household

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	No other siblings			1 to 3 siblings			4 siblings or more		
VARIABLES	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls
Meals*Follow up	-1.794***	-1.275***	-2.138***	-0.297	-0.442	-0.146	-0.163	0.052	-0.442*
	[0.468]	[0.447]	[0.654]	[0.502]	[0.742]	[0.407]	[0.280]	[0.443]	[0.239]
THR*Follow up	-1.494***	-1.193***	-1.832***	-0.283	-0.047	-0.357	0.658	1.196**	-0.353
	[0.455]	[0.425]	[0.646]	[0.435]	[0.626]	[0.478]	[0.451]	[0.592]	[0.446]
Follow up	1.841***	1.753***	1.857***	0.770**	0.857	0.522	0.084	-0.032	0.241
	[0.437]	[0.389]	[0.640]	[0.382]	[0.586]	[0.384]	[0.124]	[0.250]	[0.148]
Girl	-0.067			0.054			0.020		
	[0.134]			[0.182]			[0.103]		
Constant	18.917***	18.951***	19.012***	18.779***	18.384***	19.684***	18.945***	18.392***	19.964***
	[0.323]	[0.463]	[0.555]	[0.566]	[0.693]	[0.234]	[0.650]	[1.156]	[0.188]
Observations	599	316	283	877	510	366	587	363	223
R-squared	0.258	0.307	0.321	0.103	0.159	0.258	0.128	0.165	0.212
Meals=THR	0.206	0.816	0.249	0.972	0.467	0.473	0.129	0.133	0.809

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; ** significant at 5%, *** significant at 1% Dependent variable number of days missed in May.
Regressions also control for child age and village fixed effects (not reported).

The last row of the table shows the p-values from an F-test testing the null hypothesis that estimated impacts of school meals and of THR are equal

Table 9: Attendance conditional on new enrollments

	(1)	(2)	(3)	(4)	(5)	(6)
	Old enrollees			New Enrollees		
	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls
Meals*Follow up	-0.083	0.552	-0.899	-1.190***	-1.399***	-0.738*
	[0.612]	[0.777]	[0.639]	[0.390]	[0.447]	[0.418]
THR*Follow up	0.467	1.126**	-0.681	-1.085***	-1.233***	-0.934**
	[0.521]	[0.451]	[0.621]	[0.366]	[0.391]	[0.450]
Follow up	0.154	-0.222	0.801	1.275***	1.598***	0.822*
	[0.448]	[0.356]	[0.564]	[0.360]	[0.391]	[0.413]
Girl	-0.111			0.140		
	[0.100]			[0.133]		
Constant	19.469***	19.019***	19.769***	18.892***	18.675***	19.472***
	[0.282]	[0.338]	[0.346]	[0.407]	[0.612]	[0.226]
Observations	1,012	582	430	1,046	604	442
R-squared	0.111	0.143	0.200	0.186	0.223	0.233
Meals=THR	0.273	0.441	0.557	0.504	0.492	0.166

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; ** significant at 5%, *** significant at 1% Dependent variable number of days missed in May.
Regressions also control for child age and village fixed effects (not reported).

The last row of the table shows the p-values from an F-test testing the null hypothesis that estimated impacts of school meals and of THR are equal

Table 9: Program Impact on Child Participation in Child Labor

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Children currently enrolled		
	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls
Panel A: child labor (any task)						
Meals*Follow up	0.042	0.064*	0.018	0.059*	0.092**	0.014
	[0.030]	[0.036]	[0.031]	[0.034]	[0.040]	[0.039]
THR.*Follow up	-0.020	-0.064*	0.018	-0.066*	-0.107**	0.005
	[0.030]	[0.034]	[0.032]	[0.035]	[0.042]	[0.051]
THR=Meals	0.021	0.000	0.995	0.000	0.000	0.860
Panel B: child labor (farm and productive off-farm tasks)						
Meals*Follow up	-0.013	-0.020	-0.011	-0.052	-0.073	0.008
	[0.043]	[0.044]	[0.061]	[0.046]	[0.050]	[0.085]
THR*Follow up	-0.102**	-0.091**	-0.115**	-0.209***	-0.232***	-0.146*
	[0.039]	[0.044]	[0.052]	[0.051]	[0.057]	[0.075]
	0.057	0.060	0.089	0.003	0.001	0.068
Panel C: child labor (household chores)						
Meals*Follow up	-0.004	-0.021	0.017	0.040	0.015	0.045
	[0.038]	[0.054]	[0.042]	[0.057]	[0.078]	[0.059]
THR.*Follow up	0.002	-0.066	0.076*	0.001	-0.063	0.091
	[0.044]	[0.061]	[0.042]	[0.060]	[0.084]	[0.069]
THR=Meals	0.855	0.440	0.0507	0.432	0.318	0.316

Robust standard errors in brackets, clustered at the village level.
* Significant at 10%; ** significant at 5%, *** significant at 1%
Dependent variable is whether child participated in any task of child labor the week before the survey when schools were open.
Regressions also control for child age and village fixed effects (not reported).

The last row of each panel shows the p-values from an F-test testing the null hypothesis that estimated impacts of school meals and of THR are equal

Table A1: Key variables at baseline using 46 villages

	(1)	(2)	(3)	(4)	(5)
Variable	Meals (16 villages)	THR (16 villages)	Control (14 villages)	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	0.419***	0.434***	0.400***	0.018	0.033
	[0.028]	[0.036]	[0.034]	[0.044]	[0.050]
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]
Cooking	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 youngsters	0.237	0.198	0.186	0.052	0.012
	[0.028]	[0.024]	[0.031]	[0.042]	[0.039]
Other h. 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 Fees	261.58	543.478	319.667	-58.086	223.812
(CFA Francs)	[79.570]	[154.603]	[85.403]	[116.727]	[176.623]
Education Expenses	2351.689	3012.625	2556.167	-204.477	456.459
(CFA Francs)	[334.717]	[476.950]	[291.944]	[444.147]	[559.207]
PTA Fees	718.937	699.666	801.5	-82.563	-101.834
(CFA Francs)	[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 CFA)	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% ; 1 USD = +/- 500 CFA Francs.
Mossi and Fulani are two ethnic groups from the region. Blacksmith, Noble or Captive descents are castes used to categorize households within these ethnic groups