School Feeding Programs and the Nutrition of Siblings:

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 school feeding schemes on health outcomes for pre-school age children from low-income households in northern rural Burkina Faso. The two school feeding 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. A unique feature of this program is that data were collected for both children who were enrolled in school and those who were not, hence allowing a direct measure of the spillover effect on children who are too young to be enrolled. After the program ran for one academic year, we found the following impacts on children age 5 and under: take-home rations have increased weight-for-age by .34 standard deviations for boys and girls taken jointly, and by .57 standard deviations for boys taken separately. The school meals intervention has increased weight-for-age by .40 for boys. Neither program had significant impact on girls taken separately. We show that achieving the same gains through increased household expenditures would have required cash transfers much larger than the monetary value of the food transfers. This indicates that most of the gains are realized through intra-household food reallocation.

1. Introduction.

School feeding programs are popular transfer programs in both developed countries and low income settings. There is extensive evidence that these programs increase school enrolment or attendance in communities where schooling is not universal (Adelman et al., 2008). Their impact on nutrition is less clear, however, in part because the "window of opportunity" for nutrition closes long before class room education begins. This reflects the fact that malnutrition in utero or the first 24 months of life has irreversible lifetime consequences (Shrimpton et al. 2001).

There is also a concern that even when targeted to school aged children such school feeding programs have only a modest impact on this population since intrahousehold reallocation of resources can negate the targeting of food resources to students. However, if such a reallocation were to occur it may, in fact, increase the *overall* nutritional impact of the feeding program to the degree that the reallocation is targeted towards more vulnerable members of a household. Relatively few of the studies of school feeding, however, have included data on the younger siblings of the student population. For example, a recent comprehensive meta-analysis of medical and nutritional literature covering various dimensions of school feeding (Kristjansson et al., 2007) does not address the impact on siblings although it does find an impact on the weights of direct beneficiaries.

This study addresses the question of the impact of school feeding on the nutritional status of children not yet in school using a randomized design of a program in Burkina Faso, finding that two different types of food for education transfers lead to increased weight for age of these younger children. We use 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). There are two benefits to our research design. First, unlike many studies, we cover both children who are in an out of school. Second, and more importantly, our measure of nutritional status covers children who are too young to be enrolled (0 to 60 months). Hence, the design provides a direct measure of spillover effects.

2. Background on School Feeding and Nutrition

Food for education programs may be in the form of school meals and snacks or as take home rations (THR). The former are seldom targeted within a school (although in some program there is a fee which may vary by individuals). In partial contrast, THR can provide a transfer that is targeted to some students but not others. In neither case is the increment to the child's diet necessarily identical to the food transfer. Even in the case of meals consumed in the school meal or similar food supplement, the child may reduce his or her consumption of foods that would have been consumed in the absence of the school meal outside the school (Beaton and Ghassemi, 1982).[1] Similarly, the impact of THR on the child's food intake depends on intrahousehold allocation of the increased resources.

A common assumption is that the implicit additional income from the transfer is pooled (Becker, 1973) and that the within household allocation of food at the margin is the same as the shares of food allocated from other budget sources.[2] In the case of meals consumed at school, this sharing would come about from reallocation of food provided at home during other meals. This could partially offset the increment in school and, thus, achieve an indirect sharing of the meal or snack. However, using a random assignment of the dates of a 24 hour food recall survey dates Jacoby (2002) ascertained that school snacks in the Philippines were completely additional resources to the students in the program. That is, each additional calorie provided in school led to an identical increase to total calories consumed by the student during the day. However, unless the snack was unknown to the rest of the household, the full capture by the student is not compatible with most household allocation models. Even bargaining models are unlikely to produce a polar case with no sharing of resources; the absence of any reallocation to other household members is, more or less, a sharing rule of the nature of “what is yours is ours, what is mine is mine” implemented by a household dependent.

While the absence of any sharing is a puzzle, Jacoby’s empirical strategy is solid. Moreover, subsequent studies have used a similar methodology to replicate and expand upon Jacoby’s result. For example, Afridi (2008) looked at school meals rather than snacks in India. While the point estimates for the unit increase of total nutrient intake for each of five nutrients provided in the school meal program that was studied are less than one, these were often not significantly different from one and, thus, consistent with Jacoby’s results. In any case, they imply a larger than a plausible share of consumption of these children in their households. Ahmed (2004) used an individual fixed effect variant of Jacoby’s approach in Bangladesh and found, again, virtually a one to one increase in total calorie intakes from a snack provided in school.

Ahmed is one of the few researchers who measured the consumption of the other children in the household. He found that siblings of students in the program also increased their calorie consumption. Ahmed does not attempt to reconcile this with the 97% of calories in the snacks being consumed by the students although there are a few possible explanations. For example, if a small child has a few siblings in school, an occasional biscuit brought home or a modest reduction of food consumption after school by each individual can contribute a measurable increase in the resources for the younger child yet still lead to an increase in the student's consumption that is statistically indistinguishable from a one to one increment. Alternatively, or additionally, the increased consumption for the child at home can represent an attempt to achieve parity or fairness among siblings, even at the expense of other expenditures.

The current study investigates such an indirect impact on children who are not yet in school. We differ from Jacoby and similar studies, however, in that we do not measure consumption directly but rather we look at the increase in anthropometric status of children in two different randomized food for education programs. While an increase in food consumption is neither a necessary nor a sufficient condition for an improvement in anthropometric status, if such an improvement can be attributed to a school based intervention it would be a program benefit that is additional to any increases in attendance or learning that might also be achieved.

3. The Setting in Burkina Faso.

Program

School canteens which provide meals to the students attending school meals 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; female students who attend school on a regular basis receive a food ration (flour) that they can bring back home each month.

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 includes all new 46 new schools in the region which were first opened in the academic 2005-2006. As described in Figure 1, the experiment consisted of a random assignment of 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].

Two different programs were implemented: 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 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 (Figure 1 summarizes our experiment). It is apparent that the two interventions used different incentive structures. On the one hand, the school meal intervention gave students a relatively small of transfer each day they attended school (about 20 days a month). The daily food allocation was 162 gms of flour and 112 gms of sugar/oil/salt). On the other hand, the THR gave student a sizable transfer at the end of each month, conditional on 90 percent attendance. The school meals cost $41.46 per student while the take home ration was

$51.37. Both cost estimates are from the WFP office in Ouagadougou 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. The two interventions are likely to induce different behavioral responses, an issue we will return to when we discuss our results.

Attendance records were maintained by the school administration, according to the standard policies applied by the Ministry of Education. 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. The teachers oversaw the administration of the program in collaboration with a representative of the WFP. The 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.

Data

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), and a total of 1900 children aged between 0 and 60 months. We collected information on household backgrounds, household wealth, school participation for all children, and anthropometric data. The anthropometric status is standardized in Z scores for gender and age by subtracting the age specific median and dividing by the age specific standard deviation using current international (World Health Organization, 2009). 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.

As mentioned, 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. Hence, we have a direct measure of the spillover effects of the interventions on children who were not enrolled, and in particular on children aged between 0 and 60 months.

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.

The anthropometric data are consistent with severe food shortage, with average weight-for-age and height--for-age 2 standard deviations below the reference population[4,5]. 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 provides similar measures taken from the 2003 Demographic and Health Survey (Institut National de la Statistique et de la Démographie and 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 most 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 Model

Our primary interest is on reduced-form demand relations for child health outcome of which anthropometric is one dimension. Such health demand function can be expressed as dependent on food intake (itself a function of some exogenous variables such as prices), income, endowments and child and household characteristics. Such demand function can be derived from the constrained maximization of a unified household model in the tradition of Becker[6] or from an intra-household bargaining framework (e.g. Haddad, Hoddinott and Alderman, 1997). Thus child health outcome is defined other child food intake (N_i), child characteristics (X_i), household characteristics (X_h), and village level characteristics (X_v) such as health care infrastructure and the availability of other public goods relevant to the child production function.

$$h_{ih} = H(N_{ih}, X_{ih}, X_h, X_v) \quad (1)$$

If we adopt a linear approximation for H, we can estimate the child health outcome as follows:

$$h_{ihvt} = \beta_0 + \beta_1 N_{ihvt} + \beta_2 X_{ihvt} + \beta_3 X_{hvt} + \beta_4 X_{vt} + \varepsilon_{ivht} \quad (2)$$

We let child food intake depends on the household's food intake (F_{hvt}) in period *t* and the sharing rule δ_k (k=g, b) that governs the share of the household food intake which is allocated to each child. For simplicity, we assume that the sharing rule varies only by gender.

$$N_{ihvt,k} = \delta_k F_{hvt} \ , k=g, b \quad (3)$$

Substituting back (3) in (2), child health outcome can be written as:

$$h_{ihvt} = \beta_0 + \lambda_k F_{hvt} + \beta_2 X_{ihvt} + \beta_3 X_{hvt} + \beta_4 X_{vt} + \upsilon_{ivht} \quad (4)$$

Where $\lambda_k = \beta_1 \delta_k$

Together, equations 2, 3 and 4 offer a basis for understanding how the interventions operate. For pre-school age children who are not participating into the programs directly, the benefits of the program primarily come from increased *F*. However; the household can neutralize the program effect on a child by choosing a sharing rule after the intervention so that the child has the same food intake as before it. This is because the household food intake (*F*) in regression (4) results from the utility maximization, and is a function of the exogenous variables that govern food demand including income and prices. Since the interventions induce an exogenous variation in *F* we are able to identify the parameter of interest. While we do not observe prices our identification strategy relies on the random assignment of the villages to treatment and control groups. Thus, any variation in prices will not be correlated with the treatment variable and their omission will not bias the results. Note also, that the monthly allocation in both programs is less than an individual – never mind a household – generally consumes in a month. Thus the transfer is infra-marginal; both treatment and control groups are expected to face the same marginal price.

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 these children or their sibling took up 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.

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

$$h_{ihvt} = \beta_0 + \lambda_k T_v * Round2 + \beta_2 X_{ihvt} + \alpha_1 T_v + \alpha_2 Round2 + \upsilon_{ivht} \quad (5)$$

Where T_v is the treatment indicator, X_{ihvt} is a vector of child characteristics (e.g, age), and Round2 indicates the follow up survey. We estimate (5) controlling for village level fixed effects, and for boys and girls. Because the eligibility for one program is for both boys and girls and for the other it is for girls only, the appropriate control group differs for each program. Thus, the regressions are not pooled.

During the follow up round, we used electronic scales instead of mechanical ones that were used in the baseline. This improved measurement may introduce a spurious trend in our anthropometric data. We argue, however, that in a regression using both rounds of the survey, the trend will capture these types of variations due to improved scales during the follow up round. Moreover, we also present results using only the follow up round. These results are not influenced by the changes in the types of scales. Our empirical specification is then based on the following regression:

$$h_{ihvt} = \beta_0 + \lambda_k T_v + \beta_2 X_{ihvt\,v} + \upsilon_{ivht} \quad (6)$$

All variables are as defined previously. The impact of the program is given by λ_k. Our analysis then compares age cohorts rather than changes in individuals over a panel. We present results using the specification defined by equation 6 in the main text, but we include the DID estimation results using both the baseline and the follow up data that corresponds to the main results of the study in the appendix (Table A1)

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 former type of treatment contamination would lead, however, to an underestimation of the program effects because some households in the control villages would have access to the treatment. The latter impact is ambiguous; on the one hand the interventions would induce an increase of household resources per child in control villages but the if the foster children enter our household sample their effect would depend on whether they were more or less malnourished at baseline than were their counterparts. We will return to this issue when we discuss our empirical results, but we note that, since the villages included in the impact evaluation were only villages in which a *new* school was opened in the school year 2005-2006, control villages are typically not neighboring villages of treatment villages, making cross-over less likely. Moreover, as the survey indicates which child is fostered, we can check whether or results are affected by the inclusion or exclusion of these children.

5. Results

5.1. Program impact[7]

As indicated in table 4, neither form of school based transfers led to a significant increase in the weight for age of school age children. As mentioned above, these regressions are based on the intention to treat – that is, the dummy variable for program availability indicates that the village the child lived in received the intervention whether or not the child actually went to school. Thus, there is no need to accommodate endogenous choice. Eligibility for THR further requires that the household includes at least one girl of primary school age even if this girl is not in school. Despite the absence of a larger weight increase for the sample as a whole, the girls age 10-15 have better nutritional status on average as measured by weight-for-age z-scores.

Table 5 addresses the spillover effect on younger siblings. In contrast to the results for school age children the results for younger children show that *both* interventions lead to an increase in weight for age of boys between 5 and 60 months of age, but did not have any significant impact on girls of the same age group. While both of these increases are statistically different from their respective control group, they are not statistically different from each other. We exclude children under 6 months of age, who ideally should be reliant on breastfeeding, preferably exclusive breastfeeding. The results are essentially the same when we restrict the sample to children above 12 months and if we exclude children who are fostered. Moreover, the results are similar for dry rations when we run the model as a difference in difference model using village fixed effects. However, in the village fixed effects regression the school meal impact on boys is no longer significant (see Table A1 in the appendix).[8] The interventions did not have any significant impact on height-for-age[9].

As mentioned, for the regressions reported in table 5young children are deemed eligible for the take home rations if they live in a village randomly allocated to that intervention *and* if they have a sister who is of school going age (6 -12). They are deemed eligible for school meals if they live in a village randomly allocated to that intervention *and* if they have a brother or sister who is of school going age. In both cases eligibility is not based on actually attendance, and thus is not influenced by self selection.

Our analysis shows that the interventions had an impact on pre-school children (i.e. aged 6 to 60 months) who were not enrolled at the time of the survey, and who were not primarily targeted by the interventions. From equation (3), we can conjecture that the spillovers observed in table 5 could operate through two main channels. The first possible channel is an "income effect" which is reflected by an increased food intake by the household (increased F_{hvt}). The second channel is a "redistribution" effect, whereby the household modifies the food sharing parameters (δ). For instance, if the household realizes that boys are worse off than girls in terms of nutritional status, it could modify δ, to try to correct the initial imbalance[10].

5.2. Robustness Check

We use regressions using children who were not eligible for the program yet in the project sites to confirm that our results on the spillover to younger siblings are driven by the interventions and not by third factors common to the community. One concern is that in a rural area subject to frequent aggregate and individual income shocks, it is possible that a series of shocks unrelated to the interventions generate the pattern that we observe. Moreover, other interventions that target young children could have been in place, even if such interventions have not been found during our field surveys. If either the shocks or the distribution of interventions were not evenly distributed over the sample the results might be influenced by these factors. If this were the case we would expect the estimated program impact to be similar when using sample of children in the treatment sites that were ineligible since they are from households where there is no school age child in a school meals village, or no school age girl in a dry rations village. Barring substantial transfers across households, the interventions should not have affected these children.

In table 6, we show the results of regressions similar to those in table 5. However, the regressions in table 6 use a sample of non-eligible children. The results of the dry rations villages are in columns 1-6, and the results of the school meals villages are in columns 7-12. For all 12 regressions, the estimated program impact is not statistically different from zero, suggesting that the interventions did not have any discernable effect on ineligible children. The samples in table 6 are relatively small due to the filter employed. This contributes to a larger variance of the coefficients than in the estimations reported in table 5. However, point estimates in all the regressions reported are negative and this contrasts with those in table 5.

5.3. Comparing food and monetary transfers

Since our program impact estimate reflects both the income effect and the redistribution effect we can estimate the monetary transfers that would have been required to produce the observed effect in nutritional gains absent of redistribution to provide an approximation of the relative importance of each of these effects. In particular, we calculate how much income transfer is needed to increase child weight-for-age as much as the estimated program impact. We start by estimating to what extent child nutritional status responds to household income. We follow the common approach in the literature, and we presume that expenditures reflect a household's long run income potential

(Haddad et al., 2003). Therefore we estimate nutritional status as a function of logged household total expenditures as follows:

$$h_{ih} = \alpha_1 + \alpha_2 \ln(E_{ih}) + \alpha_2 X_i + \varepsilon_i \qquad (7)$$

Where h_i the outcome of interest, E_i is household total expenditures per adult and X is a set of other variables including the education of the child's mother and father. Since it is well documented that expenditures is subject to substantial measurement errors, we address measurement errors by instrumenting for total expenditures using the head count of household's livestock and fowls as instruments. Our rationale is that these variables are less subject to measurement errors and are good predictors of total expenditures. However, because livestock and fowls can be eaten, they may have a direct effect on nutritional status, thereby violating the exclusion restriction. We verified, however, that most livestock and fowls are not directly consumed, but rather sold[11].

The IV estimation results are reported in table 7 (the first stage estimation is reported in table A2 in the appendix). It can be inferred that girls' weight-for-age (columns 3 and 6) is more income elastic than boys' weight-for-age (columns 2 and 5). These results are consistent with other empirical studies in low income countries which found that investments in favored demographic groups are less price and income elastic than investments in less favored demographic groups. For example, Alderman and Gertler (1997) develop a theoretical model in which investments in human capital are more income and price elastic for less favored children. They showed empirically that the demand for medical care is more price and income elastic for girls than for boys in Pakistan. Using household survey from India, Rose (1999) finds that favorable rainfall shocks increase the probability that girls will survive more than they increase the probability that boys will survive in rural India.

We use the response of weight-for-age reported in table 6 to estimate the increase in total expenditures (in monetary terms) needed to achieve a gain in weight-for-age that is similar to the program impact. The results are reported in CFA francs in table 8. The coefficient of the logarithm of instrumented expenditures is within the range of those reported from household surveys as well as similar estimates from country level regressions (Haddad et al. 2003). At sample means and converting in US dollars, we find that $92 cash transfer per adult would be needed to realize gains in weight-for-age similar to the dry ration impact on boys and girls (.341), and $57 would be needed to realize a gain similar to the impact of school meals (.195). The food transfers expressed in monetary value were, however, much smaller: $4.70 and $7.15 per adult under the dry ration intervention and the school meals intervention, respectively (see table 8 for details).

Taken together, these results convey two main sets of information. First, in-kind transfers have a larger impact on child nutritional status than that would have been achieved under an equal size cash transfers. This would be consistent with the theoretical argument by Ross (1991) who 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[12]. Second, the program impacted boys predominantly, while the analysis of weight-for-age response to total expenditures showed that boys' nutritional status is not any more responsive to income than is girls' nutritional status. Thus, one can argue that if the interventions consisted of cash transfers and further that marginal allocation from cash transfers do not differ from the results in cross sectional regressions of total income the impact would have been as large or larger for girls.

There is however, one caveat to this interpretation. Households could change the sharing rule (δ) in way that allocates relatively more of the additional food to boys. This could be case if biological factors explain in part why girls fare better than boys in terms of nutrition (e.g. Wamani et al, 2007) and households are concerned about equity.

5.4. Cost-effectiveness

In table 9, we compare the program impacts to its costs. This cost-effectiveness analysis assesses how large the program effects are on a per-dollar basis. The cost estimates have been provided by WFP and include the food costs and the operation costs, and are expressed in US dollar[13]. The results are expressed in terms of additional gain in standard deviations of weigh-for-age per one US dollar. Focusing on boys (on whom the estimated impact is significant), the return from dry rations (.026) is twice as large as the return from school meals (.012). And these results are similar when we look only at children aged 12 to 60 months.

From this analysis, it appears that dry rations are more cost-effective than school meals if the objective is to improve the nutritional status of pre-school age children. School feeding interventions, however, may have other objectives (e.g. increase enrollment and retention) so that comparing take home rations and school meals requires looking at other outcomes as well.

6. Discussion

The increase in the weight of younger siblings of the children eligible for either school based transfer is relatively large. Indeed, it exceeds that which might be predicted on the basis of an income transfer alone. Using the parameters on instrumented expenditure in table 7 it would take a transfer equivalent to 98 percent of income per adult to achieve the same impact that dry rations have on boys aged 6 to 60 months (0.57 increase in the z-score). Reaching the impact of school meal on the same group of children (0.42 increase in z-score of weight-for-age) would require an increase in household expenditures by 69 percent. In comparison, over the 9 month period of the academic year, the THR per adult is equivalent to a 2 percent of average household expenditures per adult, and the school meal is equivalent to 5 percent of average household expenditures per adult. The estimated effect of household expenditures on weight-for-age in our sample is larger than the median observed in a dozen similar estimates from household data (Haddad, et al. 2003).

A relevant question is why the food transfer impact is much larger than the estimated impact from a same size cash transfer. It is plausible that food for education programs achieve an improvement on nutrition beyond the average impact of expenditures through a labeling effect by which a program encourages a reallocation of household resources (Kooreman, 2000). Such an increase of allocation towards food and nutrition beyond the pre-program marginal budgets has been observed for food stamps in the United States (Breunig and Dasgupta, 2005; Fraker, Martini, and Ohls, 1995) and for cash transfers in Ecuador (Paxson and Schady, 2008)[14]. The data do not allow us to distinguish the impact of the mode of transfer (in kind as opposed to cash) from the linkage to schools and, thus, to children. However, the fact that this impact on nutrition of young children is relatively large is clearly indicated.

While a 100% ‘flypaper effect’ (as Jacoby (2002) deems it) would imply no increase in overall consumption by the rest of the household it does not rule out a reallocation of this consumption. However, the fact that none of the school feeding programs had an impact on the nutritional status of school age children (table 4) seems to support the impact occurring within the household allocation of resources and not at the school itself.[15]

Nor would a concentration of additional resource on younger children be an undesirable outcome. Despite the concern with leakage of school meals in the literature it is questionable whether increasing the weight of school aged children who have previously been malnourished is desirable (Victoria et al., 2008); there is no doubt regarding such an objective for younger children.

7. Conclusion

In this paper, we have used a prospective randomized design to assess the impact of two school feeding schemes on health outcomes of pre-school age children from low income households 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. While the interventions were design to directly target school age children (who are enrolled), we focus on children who were too young to be enrolled at the time of the survey. Therefore, our study provides direct measures of spillovers effects within the household. 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 take-home rations have increased weight-for-age by 0.34 standard deviations for boys and girls under age 5 taken jointly and by 0.57 standard deviations for boys taken separately. The school meals intervention has increased weight-for-age by 0.40 for boys. Neither program had a significant impact on girls taken separately.

When we contrast those findings with the health outcome response to household total expenditures, we make two observations. First, consistent with previous studies, we show in our data that on average girls' health outcome is more responsive to household income than is boys' health outcome. The interventions, however, affected boys only. Second, to realize similar gains in health outcomes through increased household income would have required cash transfer about 9 times the value of the take home rations. It is apparent that in-kind transfers have a larger effect on child health outcome than general sources of income and are allocated differently than the household's other resources.

Our results show that the spillovers effects of school feeding interventions on young children are relatively large, as compared to the direct effects. This could be because in the short-run, the weight-for-age is more elastic for younger children than for school age children. While both interventions generated positive impacts on boys, we found that dry rations are most cost-effective than school meals: a dollar in dry rations and school meals earns a gain in weight-for-age of 0.026 and 0.012 standard deviations, respectively. While school feeding programs may not be the first best choice to address malnutrition for pre-school age children in a food insecure region, they do have a measurable impact. Failing to account these effects likely under-estimate the total benefits of school feeding programs, which include schooling and the indirect income support to target populations as well as the contribution to nutrition of younger children.

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Figure 1: Experimental Design

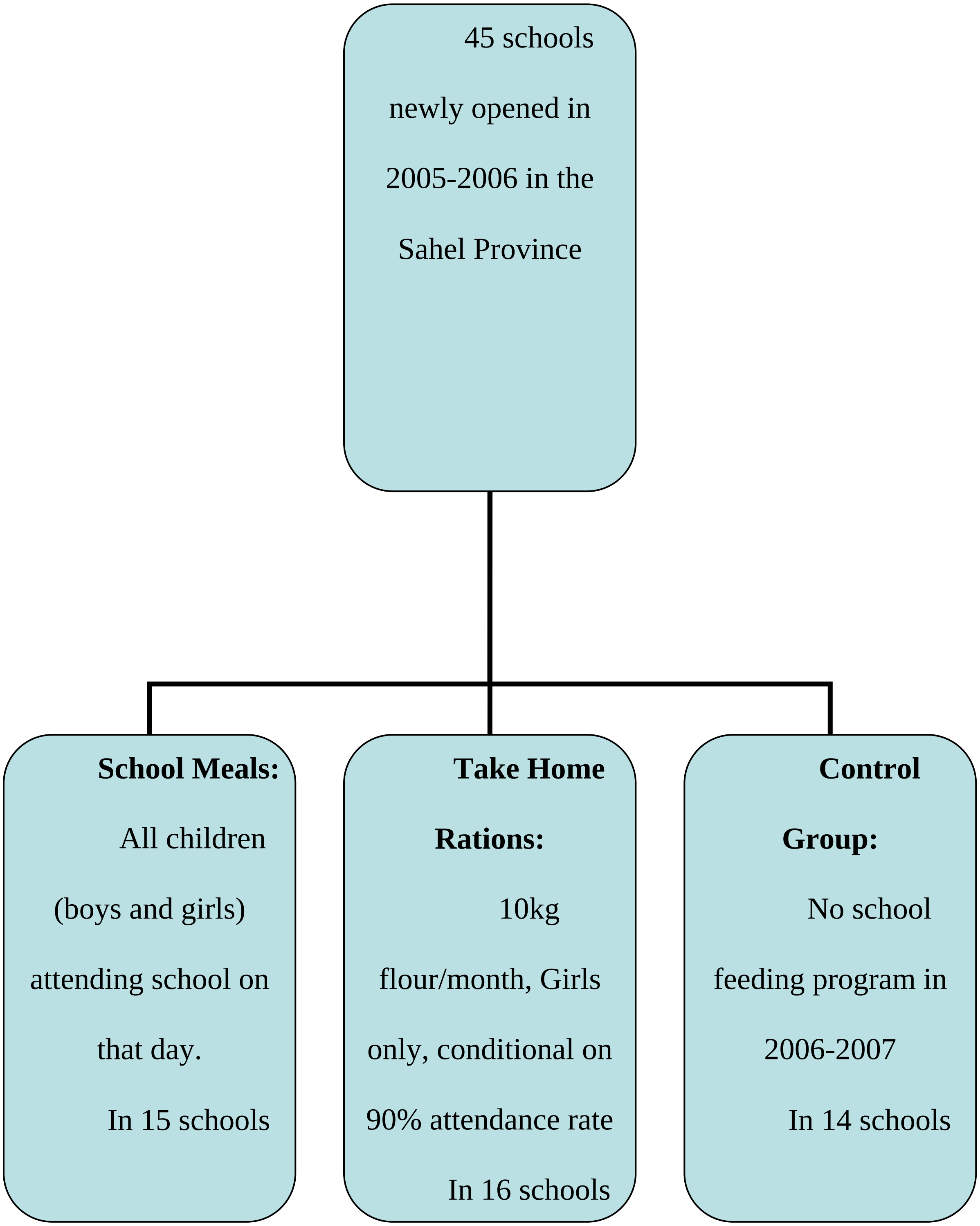

Table 1: Key Variables at Baseline

	(1)	(2)	(3)	(4)	(5)
Variable	Meals	THR	Control	Difference with control	
				Meals	THR
	Child level variables				
Enrolled	0.281	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	0.870	0.852	-0.003	0.018
	[0.017]	[0.019]	[0.018]	[0.025]	[0.026]
Child productive labor	0.650	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]
Cocking	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	0.360	-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	261.580	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	801.500	-82.563	-101.834
	[74.925]	[92.651]	[75.513]	[106.377]	[119.526]
Child is boy	0.495	0.520	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	0.060	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	0.040	0.070	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	0.460	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	0.020	0.021	0.007
	[0.021]	[0.010]	[0.006]	[0.021]	[0.011]
Head is of Noble descent	0.377	0.370	0.557	-0.180	-0.187*
	[0.084]	[0.071]	[0.075]	[0.113]	[0.103]
Head is of Captive descent	0.350	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	0.98	-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%
1 USD = +/- 500 CFA Francs.
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)

Baseline	(1) Weight-for-age (underweight)	(2) Weight-for-Height (wasted)	(3) Height-for-Age (stunted)
School meals	52.6	29.5	59.9
Take Home Rations	56.2	32.3	60.0
Control	55.3	31.6	61.7

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)

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

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

Table 4: Program Impact on school age children health, OLS on follow up

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	bmi-for-age (z-scores)						weight-for-age (z-scores)		
	All children			Children 6-10			Children 10-15		
	Boys&Girls	Boys	Girls	Boys&Girls	Boys	Girls	Boys&Girls	Boys	Girls
Dry Rations	0.061	0.024	0.106	-0.004	-0.047	0.036	0.204	0.116	0.247
	[0.119]	[0.145]	[0.112]	[0.124]	[0.184]	[0.106]	[0.197]	[0.208]	[0.217]
School meals	0.08	0.012	0.143	0.065	-0.008	0.135	0.122	0.071	0.159
	[0.117]	[0.124]	[0.126]	[0.124]	[0.157]	[0.125]	[0.200]	[0.197]	[0.223]
Girl	-0.015			-0.029			0.182		
	[0.040]			[0.060]			[0.063]***		
Constant	-1.026	-0.916	-1.147	-1.024	-0.965	-1.109	-2.496	-2.464	-2.328
	[0.138]***	[0.199]***	[0.125]***	[0.171]***	[0.230]***	[0.153]***	[0.142]***	[0.175]***	[0.158]***
Observations	3129	1474	1655	1970	921	1049	2053	966	1087
R-squared	0.06	0.06	0.06	0.04	0.04	0.04	0.08	0.08	0.07

Robust standard errors in brackets
* significant at 10%; ** significant at 5%, *** significant at 1%
Dependent variables are z-scores of body mass index (BMI) ????and weight-for-age.
Regressions also include controls for child age dummies and province dummies (not reported).

Table 5: Program Impact on preschool age children nutritional status, OLS on follow up (dependent variable is z-score of weight-for-age)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Children 6-60 months			Children 12-60 months			Children 6-60 months			Children 12-60 months		
	Boys&Girls	Boys	Girls	Boys&Girls	Boys	Girls	Boys&Girls	Boys	Girls	Boys&Girls	Boys	Girls
Dry Rations	0.341	0.570	0.070	0.321	0.567	0.044						
	[0.163]**	[0.191]***	[0.193]	[0.157]*	[0.184]***	[0.191]						
School meals							0.211	0.402	0.009	0.195	0.370	0.021
							[0.210]	[0.219]*	[0.240]	[0.212]	[0.218]*	[0.243]
Girl	0.177			0.147			0.137			0.119		
	[0.107]			[0.109]			[0.109]			[0.105]		
Constant	-2.264	-2.576	-1.609	-2.52	-2.972	-1.929	-2.445	-2.81	-1.885	-2.53	-3.107	-1.834
	[0.286]***	[0.391]***	[0.408]***	[0.215]***	[0.297]***	[0.252]***	[0.299]***	[0.350]***	[0.297]***	[0.304]***	[0.306]***	[0.319]***
Observations	720	385	335	678	360	318	1122	615	507	1045	572	473
R-squared	0.06	0.09	0.07	0.06	0.1	0.06	0.05	0.08	0.05	0.05	0.08	0.05

Robust standard errors in brackets. Regressions also include village fixed effects and child age categories, but not reported.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Program impact on non-eligible children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Children 6-60 months old			Children 12-60 months old			Children 6-60 months old			Children 12-60 months old		
	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls
Dry rations	-0.095	-0.074	-0.108	-0.088	-0.112	-0.058						
	[0.223]	[0.293]	[0.270]	[0.236]	[0.320]	[0.279]						
School meals							-0.396	-0.179	-0.682	-0.242	-0.156	-0.43
							[0.320]	[0.316]	[0.467]	[0.333]	[0.308]	[0.483]
Girl	0.418			0.370			0.52			0.566		
	[0.197]**			[0.204]*			[0.249]**			[0.239]**		
Constant	-1.798	-2.074	-1.207	-2.156	-2.060	-1.902	-2.001	-1.593	-1.756	-3.165	-2.831	-2.96
	[0.327]***	[0.417]***	[0.517]**	[0.335]***	[0.520]***	[0.455]***	[0.464]***	[0.507]***	[0.758]**	[0.505]***	[0.753]***	[0.679]***
Observations	487	241	246	435	220	215	239	111	128	211	101	110
R-squared	0.04	0.03	0.04	0.04	0.04	0.03	0.12	0.16	0.13	0.12	0.16	0.13

Robust standard errors in brackets. Regressions also include village fixed effects and child age categories, but not reported.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Child health response to household expenditures (dependent variable is z-score of weight-for-age)

	(1)	(2)	(3)	(4)	(5)	(6)
	6 to 60 months			12 to 60 months		
	Boys&Girls	Boys	Girls	Boys&Girls	Boys	Girls
lnExpCap	0.720	0.581	0.937	0.733	0.345	1.125
	[0.284]**	[0.384]	[0.431]**	[0.285]**	[0.353]	[0.460]**
Girl	0.113			0.089		
	[0.079]			[0.083]		
Constant	-9.984	-8.651	-11.759	-10.496	-6.463	-14.175
	[3.161]***	[4.358]**	[4.661]**	[3.144]***	[3.953]	[5.026]***
Observations	1579	828	751	1457	765	692

Standard errors in brackets. Regressions also child age, mother and father education levels, household characteristics, and village fixed effects.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Increase in expenditures required for the same impact as the interventions

	(1)	(2)	(3)	(4)	(5)	(6)
	Children 6-60 months			Children 12-60 months		
	Boys&Girls	Boys	Girls	Boys&Girls	Boys	Girls
(1)School meals	0.211	0.402	0.009	0.195	0.370	0.021
	[0.210]	[0.219]*	[0.240]	[0.212]	[0.218]*	[0.243]
(2) Dry rations	0.341	0.570	0.070	0.321	0.567	0.044
	[0.163]**	[0.191]***	[0.193]	[0.157]*	[0.184]***	[0.191]
Per adult expenditures increase required to achieve program impact						
(3) School meals	28486	67257	934	25859	104249	1814
(4) Dry rations	46037	95365	7262	42569	159754	3802
Per household expenditure increase required to achieve program impact						
(5) School meals	133886	316109	4388	121539	489970	8528
(6) Dry rations	216376	448214	34131	200073	750845	17868

In rows 3 and 4, the figure in each cell is the ratio of the estimated program impact to the corresponding marginal value of the regression of weight-for-age on expenditures (taken at the sample mean).

In rows 5 and 6, the corresponding figure is multiplied by the number of adults in the household.

Table 9: Cost-effectiveness analysis

	School meals	Dry Rations
Annual transfer per student (USD)*	41.46	51.37
Number of students per household	0.81	n.r.
Number of female students per household	n.r.	0.43
Annual program cost size per household	33.58	22.09
Annual program cost per adult (USD)	7.15	4.70
Program Impact (Children 6-60 months)		
Boys and Girls	0.211	**0.34**
Boys	**0.402**	**0.57**
Girls	0.009	0.07
Gains in weight-for-age per $ 1**		
Boys and Girls	0.0063	**0.015**
Boys	**0.0120**	**0.026**
Girls	0.0003	0.003
Program Impact (Children 12-60 months)		
Boys and Girls	0.195	**0.321**
Boys	**0.370**	**0.567**
Girls	0.021	0.044
Gains in weight-for-age per $ 1**		
Boys and Girls	0.0058	**0.015**
Boys	**0.0110**	**0.026**
Girls	0.0006	0.002

* Figures include food costs and operation costs. Figures are provided by WFP office in Ouagadougou
** Gains in standard deviations of weight-for-age
n.r. = not relevant.

Appendix

Table A1: Program Impact on pre-school age children nutritional status , OLS on two rounds (dependent variable is z-score of weight-for-age)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Children 6-60 months old			Children 12-60 months old			Children 6-60 months old			Children 12-60 months old		
	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls	Boys& Girls	Boys	Girls
Dry Rations	0.461	0.463	0.419	0.491	0.508	0.447						
	[0.181]**	[0.243]*	[0.270]	[0.182]***	[0.242]**	[0.273]						
School Meals							0.058	0.122	-0.028	0.078	0.131	0.006
							[0.148]	[0.195]	[0.228]	[0.149]	[0.194]	[0.230]
Round2	1.097	1.174	1.049	1.092	1.174	1.035	1.258	1.306	1.236	1.250	1.305	1.217
	[0.127]***	[0.164]***	[0.196]***	[0.127]***	[0.164]***	[0.197]***	[0.108]***	[0.138]***	[0.168]***	[0.108]***	[0.137]***	[0.169]***
Girl	0.162			0.156			0.105			0.101		
	[0.086]*			[0.087]*			[0.071]			[0.072]		
Constant	-2.907	-3.081	-2.448	-3.425	-3.432	-3.294	-3.000	-3.225	-2.690	-3.325	-3.480	-3.132
	[0.248]***	[0.336]***	[0.388]***	[0.197]***	[0.288]***	[0.250]***	[0.204]***	[0.284]***	[0.282]***	[0.172]***	[0.249]***	[0.225]***
Observations	1356	719	637	1305	688	617	1998	1074	924	1915	1029	886
R-squared	0.21	0.27	0.21	0.21	0.27	0.20	0.25	0.29	0.23	0.25	0.30	0.23

Robust standard errors in brackets. Regressions also include village fixed effects and child age categories, but not reported.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A2: First stage regression for IVE estimation (dependent variable is log expenditures per adult

	(1)	(2)	(3)
	Boys&Girls	Boys	Girls
gender==2	-0.004		
	[0.026]		
agemonth_12_17	0.037	-0.057	0.193
	[0.063]	[0.090]	[0.092]**
agemonth_18_23	-0.112	-0.197	-0.01
	[0.074]	[0.106]*	[0.106]
agemonth_24_29	0.005	-0.007	0.079
	[0.062]	[0.088]	[0.090]
agemonth_30_35	-0.078	-0.124	0.063
	[0.078]	[0.112]	[0.112]
agemonth_36_41	-0.066	-0.075	-0.006
	[0.057]	[0.080]	[0.083]
agemonth_42_47	0.108	0.078	0.21
	[0.081]	[0.116]	[0.118]*
agemonth_48_53	-0.047	-0.069	0.03
	[0.056]	[0.079]	[0.083]
agemonth_54_60	0.019	-0.007	0.124
	[0.055]	[0.077]	[0.081]
DadEd==1	0.05	0.105	0.124
	[0.100]	[0.145]	[0.144]
DadEd==2	0.23	0.209	0.273
	[0.043]***	[0.061]***	[0.063]***

MomEd==1	0.163	0.296	0.179
	[0.248]	[0.558]	[0.283]
MomEd==2	-0.157	-0.082	-0.204
	[0.085]*	[0.134]	[0.113]*
HeadMossi	0.073	0.055	0.117
	[0.059]	[0.081]	[0.090]
HeadPeulh	0.074	0.077	0.08
	[0.050]	[0.068]	[0.076]
HeadGourma	0.041	0.024	0.058
	[0.087]	[0.116]	[0.139]
HeadBlacksmith	0.042	-0.017	0.094
	[0.069]	[0.096]	[0.107]
HeadNoble	0.046	0.016	0.077
	[0.052]	[0.070]	[0.080]
HeadCaptive	0.047	0.097	-0.002
	[0.061]	[0.085]	[0.092]
HeadMuslim	0.167	-0.037	0.43
	[0.088]*	[0.120]	[0.134]***
Instruments			
Cattle	0.038	0.038	0.039
	[0.008]***	[0.011]***	[0.011]***
Sheep	0.03	0.038	0.025
	[0.006]***	[0.009]***	[0.009]***
Goat	0.011	0.012	0.008
	[0.003]***	[0.004]***	[0.004]*
Guinea fowls	0.026	0.015	0.042
	[0.009]***	[0.011]	[0.015]***

Chicken	0.025	0.03	0.024
	[0.006]***	[0.009]***	[0.008]***
Constant	10.834	11.043	10.481
	[0.113]***	[0.149]***	[0.178]***
Observations	1621	854	767
R-squared	0.13	0.13	0.15

standard errors in brackets. Regressions also include village fixed effects but not reported.
* significant at 10%; ** significant at 5%; *** significant at 1%

[1] While this is often referred to a leakage, it differs from leakage that occurs in either diversion of program resources at an administrative level or from errors in targeted in means based transfers. The leakage literature implicitly assumes that the target school child is more in need of the transfer than other family members, often presumed to be adults.

[2] For a review of intrahousehold allocation see Alderman, et al. 1995.

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

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

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

[6] For children between 0-60 months old, it is reasonable to assume that adults make consumption decisions. We also assume that mothers and fathers have the same preferences over child health.

[7] We present the program impact estimated at sample mean. Estimated program impacts across the distribution of the dependent variable, using quantile regressions are available from the authors on request.

[8] The fixed effects model would add additional controls for imperfect randomization. While, as indicated, the results on the program impacts are similar with the difference in difference model, there is an apparent strong temporal trend that likely reflects the chance in scales and, thus, is potentially misleading.

[9] Estimations results for height-for-age, and for weight-for-height are available from the authors.

[10] For pre-school age children in sub-Saharan Africa, the empirical evidence indicates that while malnutrition is widespread, girls are on average better off than boys. See Svedberg (1990) for an earlier analysis and Wamani et al (2007) for a more recent analysis using DHS data. Our estimations results for children who were not eligible for the program (table 8) are also consistent with this pattern.

[11] We found that about the value of livestock and fowls consumed by the household corresponds to 1.5 percent of its holding of livestock and fowls. In contrast, sales value corresponds to 27 percent of livestock and fowls. This would indicate that livestock and fowls holdings are more likely to influence nutritional status through expenditures rather than through own consumption of livestock products.

[12] In his review of the US food stamps, Fraker (1990) found that food stamps program leads to food consumption increase 2 to 10 times larger than what would have been expected if the benefits were in cash.

[13] At the time of the field work, the exchange rate was $1=CFA 500.

[14] In contrast to these previous findings, Hoynes and Schanzenbach (2009) find that households respond similarly to a dollar in cash income and to a dollar in food stamps. However, for households which are constrained—those desiring lower food expenditures than expected food stamps--, food stamps have a larger marginal effect on consumption than an equivalent cash-income.

[15] That in-kind transfers could lead to food reallocation within the household has been hypothesized by Currie and Ghavari (2008) in a broader context.