# The Impact of Food for Education Programs on School Participation in Northern Uganda*

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**Abstract**

There is a general consensus that Food for Education (FFE) programs increase primary school participation. Although this view is widely held, there is limited causal evidence to support it, making it difficult to anticipate the size of expected impacts. Moreover, little is known about how the design of FFE programs affects schooling outcomes. This paper presents evidence of the impacts of alternative methods of FFE delivery on schooling in Northern Uganda using a prospective, randomized controlled evaluation conducted from 2005-2007. We compare the impacts of the World Food Programme's in-school feeding program (SFP) with an experimental take-home rations (THR) program conditional on school attendance to examine how outcomes are affected by the timing of meals and the placement of incentives with children versus parents. Results show that the in-school meals program increased enrollment for those children who were not enrolled at baseline, but who had reached the recommended age of school entry. For many outcomes we cannot reject that the THR impact is equivalent to that of the SFP. Both programs had large impacts on school attendance, with impacts varying by grade and gender. Moreover, both the SFP and THR programs reduced grade repetition, with larger impacts coming from the SFP program. The SFP program also reduced girls' age at entry to primary school. Neither program affected progression to secondary school. In fact, children in grades 6 and 7 in SFP schools in 2005 were significantly more likely to remain in primary school in 2007, suggesting that school meals induce hungry children to delay completing primary school.

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# 1 Introduction

The Millennium Development Goal of Universal Primary Education is one that has attracted considerable interest, in part because it can be addressed, at least nominally, directly through education policy. Many developing country governments have moved to eliminate primary school fees and institute a policy of Universal Primary Education in the past decade. Although these policies have increased officially documented enrollment rates, progress in improving primary school attendance has been limited. This pattern has led donors and governments to consider complementary programs to further improve primary school participation. In this context, food for education programs have received renewed attention.

Food for education programs are generally considered to be effective at increasing school participation. A large body of research supports this view, though estimates of the size of the effect differ considerably by context. Important factors affecting the magnitude of the impacts include initial attendance rates, school quality, and the food transfer size (Ahmed, 2004; Vermeersch and Kremer, 2004; Jacoby et al., 1996; Powell et al., 1998). Moreover, the evidence from many studies is limited because the effect is measured only for children already in school, or else the research design is not causal, or the sample is not representative of school-age children (Adelman et al., 2008b). As a result, it can be difficult for policy makers to anticipate the size of the increase in school enrollment and attendance that will occur in response to a new FFE program.

The ambiguity regarding FFE affects on school participation weakens support for new food for education initiatives because these programs can be relatively expensive to operate. At a cost of $28-$63 USD per child per year (Bundy et al., 2009), their cost almost rivals the cost of education itself in some developing countries. If raising school participation is the only goal of a FFE program (though it rarely is), research suggests that other programs, such as deworming, free school uniforms, parent-teacher partnerships, and programs improving

teacher incentives may be more cost-effective (Miguel and Kremer, 2004; Tan et al., 1999). The impacts of these programs on school participation may not be as large as from a FFE program, but these alternatives are significantly cheaper to operate. However, when food for education programs provide nutritious food to undernourished pupils, they can reduce short-term hunger and help improve pupils' learning and cognitive development (Adelman et al., 2008b). In order to understand the full impacts of FFE programs, it is first necessary to obtain reliable estimates of their impacts on school participation.

This study presents rigorous evidence of the impact of two food for education programs operated by the World Food Programme on primary school participation in Northern Uganda. Using a prospective, cluster randomized, controlled field experiment carried out from 2005-2007, we obtain causal estimates of the impact of the programs on measures of primary school enrollment, school attendance, age at school entry, grade promotion, and progression to secondary school for a random sample of school-age children living in the service area of the schools. The household sample for this study is drawn from clusters identified by the boundaries of Internally Displaced People's (IDP) camps in Pader and Lira districts in Northern Uganda. These IDP camps represent well-defined service areas for the primary schools they contain.

The two food for education programs of interest are an in-school meals program and a take-home rations program conditional on school attendance. This further expands the contribution of this research by enabling an investigation into how differences between these two delivery methods of food for education programs determine their impacts on school participation. Moreover, to our knowledge, there are only two studies that investigate the impacts of take-home rations programs. Using a similar evaluation design to the one used here, Kazianga et al. (2008) find a 6 percent increase in girls' enrollment from a take-home rations program in Burkina Faso. It is important to note that in their study the baseline enrollment is extremely low, at approximately 27 percent for all school-age children. Ahmed

and del Ninno (2002) provide evidence of the impact of a THR program provided to poor households in rural Bangladesh. They show that the program had fairly significant impacts on school participation, including an eight percent increase in primary school enrollment and a 12 percent increase in school attendance recorded during unannounced attendance visits. In an earlier study of the same program, Ravallion and Wodon (2000) found that the THR increased school participation (whether "currently attending") by 19 percentage points for boys and 18 percentage points for girls on average.

By directly comparing SFP and THR programs operating in the same context in Northern Uganda, this study helps to explain which components of FFE programs are most vital to improving school participation. These program components, such as, the timing and location of the meals and control over the transfer by other household members, have rarely been systematically altered in order to study their contribution to FFE objectives. Take-home rations conditional on school attendance provide an informative counterfactual to an SFP program by substituting for meals provided at school and during the school day with a monthly dry food ration provided at home. This comparison allows the investigation of whether providing meals at school to hungry children is uniquely effective at attracting them to school day after day.

In particular, this study considers how the difference in the timing of meals inherent in the two modalities affects their impacts. Although it is possible for children receiving take-home rations to bring food with them to school or, in some cases, return home for lunch, it is more difficult and costly for the pupil and the household than receiving a meal at school. In practice, THR beneficiaries rarely supplied their own lunch in Northern Uganda. As a result, children receiving take-home rations had systematically different timing of access to the food during the day than their counterparts in SFP schools.

Moreover, the monthly rations from the THR program were under the control of the beneficiary child's caregivers. These caregivers were free to use the food as they saw fit,

including redistributing it to other household members or selling it. One effect of this difference in modality is that under a THR program, parents rather than school children have the incentive for the child to attend school. The attendance of pupils enrolled in THR beneficiary schools was monitored and rations could be terminated if the child did not attend at least 85 percent of school days. Moreover, it is easier to redistribute some of the food to other household members with take-home rations. This further dilutes the child's incentive to attend school and reduces the physiological and nutrition benefits that make attending easier through reduced morbidity and improved attention span.

We find positive impacts of both FFE programs on school participation measures. The results show nuanced positive impacts of the in-school meals program on primary school enrollment when we restrict the analysis to children who were not enrolled before the introduction of the FFE programs. Moreover, based on the results from unannounced attendance data, we find significant positive impacts of both in-school meals and take-home rations on morning and afternoon attendance. The results also show a weakly significant impact of both FFE programs on age at entry to primary school and a reduction in grade repetition from the SFP program for boys, but the SFP impact is not statistically different from the THR program. Finally, we find no impact of either program on progression to secondary school. However, children in grades 6 or 7 in school feeding program schools in 2005 were significantly more likely to remain in primary school as of 2007. This suggests that school meals may have the unintended effect of increasing the time taken to complete primary school.

The remainder of the paper is organized as follows. Section 2 describes how SFP and THR programs impact school participation. Section 3 describes the empirical strategy, outlining the details of the randomization of the two programs in order to identify the main program effects. Section 4 describes the study setting in Northern Uganda, the details of the design and operation of the school feeding programs, and the evaluation study data. Section 5

provides the main empirical results and section 6 concludes.

## 2 The Conceptual Framework

Food for education programs, such as SFP and THR programs, improve school participation by decreasing the net cost of sending children to school. For a given school year, parents decide to enroll their children in school if the expected effect on the child's future earnings exceeds the net cost of having the child enrolled for that year. This cost includes the direct cost of school enrollment, including school fees, uniforms, and school supplies. The other cost component is the opportunity cost of schooling, including the loss of the child's income, agricultural labour for the household, and household labour. The net cost of school participation is the sum of these direct costs and the opportunity costs minus any direct benefit from school participation, including transfers from a FFE program. The decision regarding a child's school attendance is similar. Parents will send a child to school in a given week or on a given day if the expected benefits to having the child at home working, caring for siblings, or recovering from an illness is outweighed by the cost of lost time learning, missed school meals, or the potential to lose the next month of take-home rations.

The factors that determine these school participation decisions are often different for boys and girls and vary with child age, particularly in terms of the child's expected contribution to household income, farm labour, caring for siblings and sick relatives, and other household chores, such as, fetching water or firewood. Children also have different susceptibility to infection, a major cause of missed school days in developing countries. Furthermore, the quality of the school affects the participation decision by changing the expected benefits of schooling in terms of learning and future earnings. Parents are often unmotivated to send children to school if they believe the education is of little value. Factors affecting school quality include teacher training, ability, and attendance, the physical school infrastructure,

the availability of school supplies, such as, textbooks and chalkboards, and the pupil/teacher ratio.

The effect of the two FFE modalities on the school participation decision may also differ as a result of differences between modalities in the timing of meals and control over the transfers. If providing meals during the school day has important effects on a child's school performance, an in-school meals program may have a larger effect on school attendance than take-home rations. Alternatively, if the timing of meals is not crucial and children are able to smooth the benefits to school performance of the additional food consumption over a 24-hour period, then the effect of SFP and THR programs on school attendance will not differ in this regard.

Differences between SFP and THR programs in control over the food transfers by the child or a caregiver may have substantial effects on school participation decisions. These effects derive from differences in the strength of incentives to attend school and from the amount of additional food the school-age child receives. Meals served at school provide a direct incentive to a child to attend, particularly if that child is hungry and the food is substantial. In this setting, the role of other factors that typically affect school attendance, such as school quality, may be muted because the child is motivated to attend by the meals and may place less emphasis on the learning environment, either intrinsically or due to hunger. In a THR program, the food ration is generally controlled by the child's parent or caregiver, who receives the incentive to send the child to school. The caregiver will weigh the benefit of the ration, as additional income or as a source of nutrition for the school-age child or a younger sibling, for example, against the opportunity cost of schooling for the child in deciding whether to send the child to school. In this sense, differences between the child and the caregiver in the perceived importance of the food from the FFE program may affect the relative strength of incentives for attendance in the SFP and THR modalities.

These program-based differences in control over the food transfers also affect which house-

hold members receive the additional nutrition provided by the program. Though the beneficiary child in an in-school meals program may consume all of the food transfer at school, the child may receive less food at home at other meals on those days.[1] In a THR program, this potential redistribution of program resources to other household members may be greater because even the transfer itself can be redistributed. This potential diversion of the nutritional benefits of the FFE transfers through THR may further weaken their effects on child attendance.

Another important difference in the way that SFP and THR programs can affect learning, and so school participation, is through the interruption of learning activities in SFP schools while meals are being prepared and served. Organizing and conducting school meals can be time consuming and disruptive, particularly in large schools or with programs that provide more than one meal during the school day.

The differences in FFE modalities can also affect other schooling outcomes, including child age at school entry and education attainment, as measured by grade promotion and progression to secondary school. The effect of both SFP and THR programs on a child's age at entry to primary school is similar to the general effects on school enrollment and attendance, increasing the benefits of school enrollment for younger children through an income effect. The effect of these alternative FFE modalities on grade promotion and progression to secondary school operates through their effects on improved attendance, school performance, and reduced morbidity. A child is more likely to successfully complete a grade and continue to the next if the child has attended school regularly and performed well on exams. Thus, many of the factors that determine how SFP and THR programs affect school participation are also relevant to these programs' effects on grade promotion and progression to secondary school. However, school performance is an important determinant of these promotion out-

[1] Jacoby (2002) referred to the share of the food transfer that 'sticks' to the child as the 'intrahousehold flypaper effect'. He showed that children in an SFP program in the Philippines received a substantial fraction of the additional food provided by the SFP transfers.

comes, so differences in the effects of SFP and THR programs on a pupil's concentration in the classroom or food-based biological changes that affect cognitive development may be important. Many of these differences in effects derive from differences in the timing of meals and from the fraction of the food transfer that the child receives under the two delivery methods.

There are two common differences in SFP and THR modalities that often induce different impacts on education outcomes that are not present in the program analyzed in this study but should nonetheless be mentioned briefly. They are the size of transfer provided and the way the programs are targeted within schools. Most THR programs run by the World Food Programme, for example, are targeted within schools to groups with low education outcomes, particularly to girls in some countries or to extremely poor households. THR programs are more easily targeted than in-school meals because they are less public and require only monthly transfers that attract less attention and fewer claims of unfairness or exclusion. On the other hand, most in-school meals programs in developing countries are targeted at the school level, with all children at program schools receiving meals. The ease of targeting children within schools is a strength of the THR modality that can boost its cost effectiveness, particularly regarding school participation. Typically, a large share of food transfers in school feeding programs goes to children who were already enrolled with very high attendance rates.

The other common difference in the two programs is the amount and type of food provided under each modality. In-school meals often include milk products or other nutrient-dense foods while take-home rations typically include cereals and oils, which may or may not be fortified. In-school meals programs often provide more food and food of higher nutritional quality than take-home rations. The benefits of the programs may differ for this reason alone. Therefore, the programs in Northern Uganda were designed so that a child who attended school every day and received in-school meals received the same amount and quality of food

as a pupil who attended the minimum required days in at a THR school.

# 3 The Empirical Strategy

## 3.1 The Identification Strategy

The evaluation uses an experimental, randomized, prospective design. This makes it possible to control for pre-program child and household characteristics and to observe changes in outcome variables during the interventions. The experimental design was achieved by randomly assigning similarly-eligible IDP camps, which serve as the service area for primary schools in most cases, to the treatment groups (SFP, THR, and control).

The random assignment of IDP camps into treatment groups makes it possible to place a causal interpretation on estimated impacts (Heckman and Smith, 1995) by eliminating potential bias from program targeting or lobbying, although bias from sampling error or from household selection effects may still exist. Sampling error can be checked by testing for the equality of mean outcomes in the baseline sample. Gilligan et al. (2006) present such tests on the 2005 baseline survey data for various outcomes and household characteristics, including household demographics, education, child anthropometry, morbidity, and iron status. Most tests fail to reject the equality of means of these variables between the treatment groups, though small significant differences were found for some measures of school attendance and learning. A summary of these results are reported in Table 1.

If the randomization is effective and sampling error is not of concern, the impact of the program on outcome, Y, can be measured by the average difference in outcomes between the treatment group, T, and the comparison group, C, after implementation as in equation 1,

$$\Delta^{SD} = E[Y_1^T - Y_1^C] \tag{1}$$

Table 1: Comparison of Baseline Characteristics

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Variable | SFP | THR | Control | SFP=Control | THR=Control | SFP=THR |
| **Individual Outcomes (Age 6-13)** | | | | | | |
| Enrollment | | | | | | |
| Net | 0.866 | 0.861 | 0.806 | 0.122 | 0.188 | 0.820 |
| | (0.013) | (0.020) | (0.035) | | | |
| Gross | 0.887 | 0.883 | 0.836 | 0.119 | 0.191 | 0.832 |
| | (0.010) | (0.018) | (0.030) | | | |
| Attendance (self-reported) | 0.950 | 0.905 | 0.862 | 0.026** | 0.388 | 0.217 |
| | (0.009) | (0.034) | (0.037) | | | |
| Test Scores | | | | | | |
| Lower Primary Numeracy | 42.613 | 44.145 | 50.022 | 0.095* | 0.246 | 0.749 |
| | (2.790) | (3.701) | (3.137) | | | |
| Upper Primary Numeracy | 44.435 | 36.813 | 32.077 | 0.070* | 0.573 | 0.274 |
| | (3.410) | (5.674) | (5.286) | | | |
| Lower Primary Literacy | 26.306 | 31.889 | 28.902 | 0.589 | 0.671 | 0.487 |
| | (3.699) | (6.195) | (2.658) | | | |
| Upper Primary Literacy | 11.880 | 10.000 | 14.154 | 0.686 | 0.536 | 0.692 |
| | (2.426) | (3.969) | (4.941) | | | |
| Age at Entry | 7.086 | 6.997 | 7.126 | 0.786 | 0.362 | 0.434 |
| | (0.083) | (0.076) | (0.117) | | | |
| Grades Repeated | 0.500 | 0.549 | 0.444 | 0.454 | 0.147 | 0.406 |
| | (0.043) | (0.039) | (0.059) | | | |
| **Individual Characteristics (Age 6-13)** | | | | | | |
| Age | 9.381 | 9.233 | 9.269 | 0.407 | 0.809 | 0.182 |
| | (0.065) | (0.086) | (0.117) | | | |
| Orphan Status | 0.140 | 0.104 | 0.097 | 0.148 | 0.827 | 0.269 |
| | (0.022) | (0.023) | (0.021) | | | |
| Class level | 2.668 | 2.563 | 2.550 | 0.356 | 0.914 | 0.259 |
| | (0.063) | (0.064) | (0.102) | | | |
| **Household Characteristics** | | | | | | |
| Household Size | 6.662 | 6.442 | 6.606 | 0.796 | 0.338 | 0.186 |
| | (0.145) | (0.067) | (0.155) | | | |
| Literacy Status | | | | | | |
| Mother | 0.333 | 0.345 | 0.370 | 0.665 | 0.770 | 0.864 |
| | (0.049) | (0.047) | (0.066) | | | |
| Father | 0.833 | 0.768 | 0.780 | 0.257 | 0.838 | 0.219 |
| | (0.024) | (0.045) | (0.039) | | | |
| Agricultural Land | 7.955 | 9.415 | 8.710 | 0.634 | 0.723 | 0.468 |
| (acres) | (1.040) | (1.534) | (1.032) | | | |

Notes: Columns (1)-(3) report the means of the variables of interest by treatment status. Columns (4)-(6) report p-values for a t-test of the equality of means across treatment groups. Linearized standard errors are reported in parentheses. Standard errors are stratified at the district level and clustered at the camp level. Attendance is self-reported attendance in the previous 7 days as a proportion of days the school was open. An orphan is defined as a child whose biological parents are no longer alive. Literacy status is a dummy variable for whether the individual is literate. Agricultural land is the number of acres the household owned prior to being displaced.

where the subscript 1 refers to the period after program implementation. This is sometimes referred to as a 'single difference' (SD) estimator of program impact, since it compares only post-program outcomes. If the presence of sampling error leads to differences in outcomes by treatment group before the program (period 0), unbiased impacts can be calculated using a treatment group 'difference-in-difference' (DID) impact estimate. This is calculated as the average 'before-and-after' change in the outcome for individuals in an intervention group minus the comparable average change in the outcome for the control group (or alternative treatment group) as in equation 2.

$$\Delta^{DID} = E[(Y_1^T - Y_0^T) - (Y_1^C - Y_0^C)] \tag{2}$$

In the impact estimates constructed here, a child's treatment status is determined by age and by the treatment assignment of the IDP camp in which they resided at baseline. This measure of program impact, an 'intent-to-treat' impact estimate, represents the effect of offering access to the program, rather than the effect of participation in the program (Burtless, 1995). The effect of participation in a program is harder to measure because program managers can usually control access to the program, but once the program is available, households control the decision to participate.

## 3.2 The Econometric Specification

We denote access to the SFP program by $T_1$ and $T_2$ represents access to the THR program. The single difference impact of the programs in equation 1 can be estimated as

$$Y_{ic1} = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \epsilon_{ic} \tag{3}$$

where $Y_{ic1}$ is the outcome of interest of child $i$ in IDP camp $c$ in period 1, after the implementation of the programs. $T_1$ is a dummy variable representing access to the SFP treatment and takes a value of 1 if the child resides in an IDP camp randomized into the SFP treatment, and zero otherwise. Similarly, $T_2$ is a dummy variable representing access to the THR treatment. $\epsilon_{ic}$ is the unobserved child and camp specific error term. Because the randomization and sampling was stratified by district, these error terms are independent across districts. We allow for correlation in the error structure within IDP camp by clustering at the IDP camp level.

If the randomization was effective, leading to no differences in mean outcomes before the programs, estimating equation 3 on outcomes measured after the programs have been implemented provides a well-identified estimate of the impact of access to the SFP program, $\beta_1$, and of access to the THR program, $\beta_2$.

If pre-program data on outcomes are available, and particularly if sampling error results in differences in these outcomes before the programs, DID estimates in equation 2 can be obtained by estimating

$$Y_{ict} = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \beta_3 R_1 + \beta_4 T_1 R_1 + \beta_5 T_2 R_1 + \epsilon_{ict} \tag{4}$$

where $R_1$ is a dummy variable that takes on the value of 1 if the observation is from period 1, and zero otherwise. t indexes the time period with 0 representing the baseline and 1 representing the post-implementation period.

In equation 4, $\beta_4$ is the DID estimate of the impact of access to the SFP program on the change in the outcome before and after the program began and $\beta_5$ is the DID estimate of the impact of access to the THR program on the change in the outcome. Conditional impact estimates can be obtained by adding a vector X of individual-, household-, school-, and camp-level control variables to equation 4. Controlling for the effect of pre-program

characteristics in the analysis can improve the precision of the impact estimates.

# 4 The Setting

## 4.1 Sample Description

The randomized food for education experiment analyzed in this paper was conducted in Northern Uganda during a time of conflict and displacement. The data were collected in two rounds; the first prior to the implementation of the programs from October-December 2005 and the resurvey from March-April 2007. During this time period, Northern Uganda was the site of a rebel group insurgency that forced the displacement of the rural population into camps. The sample was drawn from displaced households in two districts, Lira and Pader, in Northern Uganda. Due to the threat of attack by the Lord's Resistance Army (LRA), the rebel group active in the region, camp residents were confined to camp boundaries except for brief periods during the day, leaving the majority of the population without access to their ancestral homes and land. With few sources of income, camp residents relied on food aid for survival. The World Food Programme (WFP) provided monthly food rations to each household living in the IDP camps. In 2005, households living in camps in Pader district received monthly 'general food distribution' (GFD) rations of grain, oil, beans, and fortified corn-soya-blend tailored to meet 75 percent of the household's food needs, by household size. In Lira district, GFD rations were meant to meet 50 percent of household food needs in 2005, indicating that WFP believed Lira camp residents generally had better access to other income sources. The general food rations were subsequently reduced by WFP in 2006 as the security situation in the region improved. The composition of the general food rations and the FFE rations are similar. Therefore, the food provided by the interventions is an exact substitute for what is typically served in the home. This suggests that under both treatments, the FFE ration increased the amount of food available to the household, but not

its composition.

Pader and Lira districts were selected for this evaluation study because they were WFP's desired locations for the expansion of school feeding in Uganda. The FFE programs were introduced only in IDP camps in these districts because living conditions were generally considered to be worse inside the camps than in towns and WFP had limited funding for the FFE expansion. We argue that this is a very relevant setting in which to analyze the impacts of food for education programs. In 2004, 50 percent of all World Food Programme school feeding beneficiaries were part of an emergency response (World Food Programme, 2007)[2]. The World Food Programme also notes that "[t]he scant documentation and limited existing body of knowledge about school feeding projects in emergency contexts does not indeed correspond to the significant number of SF projects carried out in WFP Emergency or Protracted Relief and Recovery Operations" (World Food Programme, 2007, p. 4).

Primary school in Uganda consists of grades 1 through 7, with pre-school being extremely rare, especially in rural Uganda. In 2002, Uganda adopted Universal Primary Education (UPE)[3] which, in theory, abolished all primary school fees. In practice, some schools do require additional payments from pupils for such things as parent-teacher association fees, textbooks, and uniforms. Primary education is present in IDP camps through learning centers; agglomerations of pupils and staff from schools displaced from their villages. In some instances, the classes of the original schools are preserved within the learning center, while in others, pupils from different displaced schools are intermingled in classes. In 2005, all IDP camps in this study contained at least one learning center; some camps contained two or three learning centers. For the evaluation, randomization of the treatment arms was done at the IDP camp level, so that all school-age children in a single camp had access to the

[2]WFP defines emergency contexts to be "natural, man-made, slow or sudden onset, and with different population groups; refugees, Internally Displaced Persons (IDPs), host communities." (World Food Programme, 2007, p. 2)

[3]Uganda first adopted UPE in 1996 but for a maximum of four children per family. In 2002, it was extended to all children of primary school age.

same treatment. Virtually all pupils in a learning center reside in that same camp, so the camps represent the service area of the learning centers except in those few camps containing more than one learning center.

Therefore, IDP camps were chosen as the sampling unit for the randomization of the FFE programs and camp census data were used to draw the household sample. Camps were sampled from a list of priority camps in these two districts identified by WFP because of poor food security and living conditions and due to the severity of the effects of the rebel insurgency there. Out of 54 priority camps representing most of the IDP camps in the two districts, a district-stratified sample of 31 camps was randomly drawn for the study. The number of camps selected was determined by WFP's budget for the FFE expansion in the coming school year. These 31 IDP camps were then randomly assigned into the three intervention groups (SFP, THR, and control), with 11 camps allocated to SFP and 10 each to the THR and control groups. After the randomization, two camps, one each from the THR and control groups, were reassigned to the SFP group because of their proximity to other camps receiving SFP. WFP felt that if these camps were not reassigned, their students would migrate to the SFP program camps to gain access to the school meals, contaminating the control group.

The baseline survey was conducted in November-December 2005, before the introduction of the FFE programs in early 2006. Households with children aged 6-17[4] were randomly sampled. Random household sampling was stratified by block[5] with the fraction of the camp sample drawn from each block proportional to that block's share of households with children aged 6-17.

The resurvey in 2007 aimed to locate those households sampled in 2005 and to resurvey them. The household tracking for the resurvey was complicated by the resettlement of

[4]The upper end of the 6-17 year age range is high for primary-school age, but, in 2005, a large share of children age 14-17 attended primary school in Northern Uganda, so they were included in the sample.

[5]The IDP camps were organized into blocks for organizational purposes.

households out of IDP camps that began in Lira in April 2006 and several months later in Pader. Peace talks between the Government of Uganda and the LRA officially began in July 2006 leading to relative security in the region. As a result, the government began to resettle the camps. In Lira, nearly all households in the sample had returned home by March 2007. In Pader, households did not return home, but in some cases resettled into smaller, less populated resettlement camps located closer to their homes. These new camps were a step towards the complete return home and provided many households with daily access to their land. By the time of the resurvey in April 2007, 70 percent of households in the sample had changed location since the baseline survey in November 2005. Despite this extraordinary degree of movement, 81 percent of baseline households were located and re-interviewed in the second survey round. In the analysis that follows, we test for effects of this attrition on the estimated impacts of the programs.

The data collection included several survey instruments. They consisted of a detailed household questionnaire, health data, a camp questionnaire, a school/learning center questionnaire, price lists, learning achievement tests, and unannounced attendance measures. During the 2006 school year after the interventions had begun, the survey team conducted unannounced attendance visits in four rounds of visits to the IDP camps. Not every school was visited in each round, but each school received an unannounced attendance visit at least once. This unannounced attendance data is available only after the start of the FFE programs in March 2006. Therefore, a single difference estimation strategy is used when analyzing data from the unannounced attendance visits. Baseline controls are included in the estimation to capture observable baseline differences.

## 4.2 The Food for Education Programs

The two food for education programs evaluated in this paper were managed and funded by the World Food Programme. The in-school meals program provided a free fortified mid-

morning snack and lunch to all students enrolled in schools operating the program. The snack consisted of a porridge made from micronutrient fortified corn-soya-blend (CSB), sugar, and water. The lunch consisted mainly of beans and either hot posho (maize meal) or rice. The lunch also included vegetable oil and salt. Together, the meals provide 1049 kcals of energy, 32.6 gm of protein, and 24.9 gm of fat. The daily transfer met two thirds of a child's daily vitamin and mineral requirements, including 99 percent of iron requirements. Households with children in the program were required to contribute firewood for cooking and a fee of approximately US$0.10 per month toward the pay of the cooks, although children were usually not penalized for failure to pay this fee.

The dry rations provided in the take-home rations (THR) program were equal in size and composition to the food received by in-school meals beneficiaries. This facilitates the comparison of the impacts of both modalities of school feeding provision. The THR rations were provided to beneficiary households once per month. THR beneficiary households received a THR ration for each primary-school age child that was enrolled and attended school at least 85 percent of school days in the previous month.

The relocation of households, beginning in 2006, lead to some disruption of the SFP and THR programs, particularly in Lira district. The programs were restarted in most Pader satellite camps after an interruption of only a few weeks, as the schools relocated to these new camps. In Lira district, the programs began again in relocated schools after an interruption of a couple of months on average.

# 5 Results

The analysis of the impact of the SFP and THR programs on school participation focuses on primary school enrollment, attendance, age at entry, grade repetition, and progression to secondary school. Table 2 lists the outcome variables examined, the variable definition, the

data source from which the variable was constructed, and the sample mean of the outcome prior to the implementation of either program.

## 5.1 Enrollment

Impacts on enrollment are examined using two standard measures of school enrollment, gross primary school enrollment and net primary school enrollment. Gross enrollment is the proportion of all children enrolled in primary school to the number of 6-13 year olds in the service area of the primary school. This proportion can be greater than one because delayed or early school entry, gaps in schooling, and grade repetition leave many children enrolled in primary school beyond age 13; the expected age of primary school completion. Net enrollment is the proportion of 6-13 year old children enrolled in primary school to the number of 6-13 year olds in the service area of the school.

The implementation of Universal Primary Education in Uganda in 2002 abolished all overt primary school fees, and established a school funding formula based on the number of enrolled students. With this policy in place, it is unsurprising that baseline enrollment levels in the sample are high. The baseline net enrollment rate in the sample is 84.9 percent, while the gross enrollment rate is 88.0 percent.

The estimated difference-in-difference impacts of the SFP and THR programs on net and gross enrollment are presented in Table 3. We find no impacts of either program on either measure of enrollment. Furthermore, no impacts were found on net enrollment of 6-9 year olds (column 3), who may have been more likely to respond to the enrollment incentives provided by the FFE programs. Table 4 presents results from child fixed effects estimation, which controls for unobserved child level effects. Controlling for child fixed effects does not meaningfully change the results on net and gross enrollment. In Table 5, we restrict the sample to children who were not enrolled in primary school in the baseline, and present single-difference estimates of the impact on enrollment in the resurvey. As

Table 2: School Participation Outcome Variables

| Outcome Variable | Definition | Data Source | Baseline Mean |
|---|---|---|---|
| Gross enrollment | Ratio of children of any age enrolled in primary school to primary school age children (aged 6-13) | Household survey | 0.880 (0.011) |
| Net enrollment | Ratio of children aged 6-13 enrolled in primary school to all children aged 6-13 | Household survey | 0.849 (0.013) |
| Net daily attendance (morning and afternoon) | 1 if child was found in school, 0 if child was absent or not enrolled (children aged 6-17 in the baseline) | Unannounced attendance visits | N/A |
| Net attendance in past 7 days (conditional on enrollment) | Share of school days in the past 7 days that an enrolled 6-13 year old child attended school | Household survey | 0.914 (0.016) |
| Age at entry to primary school | Age (in completed years) when child first enrolled in primary school | Household survey | 7.068 (0.053) |
| Grade repetition | The number of grades repeated (2005-2007) | Household survey | N/A |
| Progression to secondary | Whether a child in grade 6 in 2005 enrolled in secondary in 2007 | Household survey | N/A |
| Remaining in primary | Whether a child in grade 6 or 7 in 2005 remained enrolled in primary in 2007 | Household survey | N/A |

Notes: Linearized standard errors in parentheses are stratified at the district level and clustered at the camp level.

demonstrated in Table 1, there are no significant differences in baseline enrollment across treatment groups. We restrict the sample to children age 6-13 (column 1) who, according to Ugandan Government recommendations, should have been enrolled at baseline. We also present estimates on children age 6-9 at baseline (column 2) to determine whether impacts on enrollment are larger on this younger sample that has likely never enrolled in school. We find a significant positive impact of the SFP program on enrollment in 2007 of children age 6-13 who were not enrolled in 2005. The estimates indicate that providing meals at school lead to a 9 percentage point increase in the probability that a child not enrolled at baseline would enroll in primary school by 2007. The impact of SFP on children age 6-9 who were not enrolled at baseline is also significant but is not much larger than impacts on 6-13 year olds. Estimated impacts of the THR program on enrollment in 2007 conditional on not being enrolled in 2005 are smaller than the SFP impacts and are not significant. However, we cannot reject that the SFP and THR programs had the same impact on enrollment for both age groups.

## 5.2 Attendance

The most readily available sources of data on school attendance include school records and self-reports gathered during household interviews. However, data from both of these sources are likely to overstate attendance rates. Teachers may feel that poor student attendance reflects badly on their ability to motivate students, and so may overstate attendance. Parents may be embarrassed to report that a child is not regularly attending school, may be concerned that the data would be reported to school officials, or may be unaware that the child is not at school during school hours. These sources of bias are likely to mute impact estimated impact of FFE programs on school attendance based on these data sources.

To overcome this problem, we conducted unannounced attendance visits at primary schools in the sample at four intervals during the first year of the FFE programs. Be-

Table 3: FFE Impacts on Enrollment, 2005-2007

| | (1) Net Enrollment 6-13 | (2) Gross Enrollment 6-13 | (3) Net Enrollment 6-9 |
|---|---|---|---|
| SFP*R2 | -.045 | -.041 | -.025 |
| | (.041) | (.033) | (.058) |
| THR*R2 | -.022 | -.023 | .007 |
| | (.038) | (.031) | (.060) |
| R2 | .079** | .070** | .078* |
| | (.035) | (.029) | (.047) |
| SFP | .060 | .056* | .067 |
| | (.037) | (.029) | (.051) |
| THR | .055 | .050 | .054 |
| | (.039) | (.032) | (.058) |
| Age | .033*** | .018*** | .101*** |
| | (.003) | (.002) | (.011) |
| Female | -.017 | -.015 | -.008 |
| | (.015) | (.012) | (.018) |
| Pader | .030* | .025** | .045* |
| | (.015) | (.012) | (.026) |
| Constant | .489*** | .651*** | -.033 |
| | (.056) | (.045) | (.115) |
| No. of Observations | 3134 | 4018 | 1609 |
| $R^2$ | .07 | .061 | .103 |
| p-value SFP=THR | 0.347 | 0.375 | 0.525 |

Notes: Impact estimates are difference-in-difference intent-to-treat effects. R2 is a dummy variable indicating the observation is from the resurvey. Standard errors are in parentheses and are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4: FFE Impacts on Enrollment with Child Fixed Effects, 2005-2007

| | (1) Net Enrollment 6-13 | (2) Gross Enrollment 6-13 | (3) Net Enrollment 6-9 |
|---|---|---|---|
| SFP*R2 | -.031 | -.039 | -.050 |
| | (.041) | (.034) | (.071) |
| THR*R2 | -.015 | -.028 | -.039 |
| | (.040) | (.034) | (.073) |
| R2 | .129*** | .110*** | .256*** |
| | (.035) | (.030) | (.059) |
| Constant | .812*** | .854*** | .685*** |
| | (.009) | (.007) | (.017) |
| No. of Observations | 3717 | 4768 | 1902 |
| $R^2$ | .077 | .055 | .182 |
| p-value SFP=THR | 0.583 | 0.633 | 0.857 |

Notes: Impact estimates are based on a difference-in-difference intent-to-treat model with child fixed effects. R2 is a dummy variable indicating the observation is from the resurvey. Standard errors are in parentheses and are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: FFE Impacts on Enrollment in 2007, Conditional on Not Being Enrolled at Baseline

| | (1) Enrollment 6-13 | (2) Enrollment 6-9 |
|---|---|---|
| SFP | .089** | .094** |
| | (.035) | (.040) |
| THR | .059 | .054 |
| | (.060) | (.058) |
| Age | -.021 | .021 |
| | (.023) | (.023) |
| Female | -.093** | -.071* |
| | (.036) | (.038) |
| Pader | .063 | .051 |
| | (.039) | (.042) |
| Constant | 1.012*** | .686*** |
| | (.189) | (.209) |
| No. of Observations | 183 | 156 |
| $R^2$ | .054 | .043 |
| p-value SFP=THR | 0.593 | 0.474 |

Notes: Impact estimates are single-difference intent-to-treat effects. Standard errors are in parentheses and are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%.

cause attendance data collected during these surprise visits were based on observation and school officials and households were not informed of the date of these visits, the attendance estimates should be free of bias from self-reporting or anticipation effects. The attendance variable is defined as 1 if the child attended school that day and 0 if the child did not attend school, regardless of whether the child was enrolled.[6] These data were collected in both the morning and in the afternoon, as is customary in Ugandan primary schools, for all children age 6-17 at baseline who could be identified. Children in grades 1 and 2 in Uganda do not attend school in the afternoon, so they are not included in the afternoon attendance visits. Not all of the schools were visited during each unannounced attendance data collection round, so some but not all children have multiple observations in these data. Attendance may vary systematically through the school year due to periodic demand for work on farms, for example, so we control for the month of the attendance visit in all attendance estimates based on the unannounced attendance data. We also control for the child's age and gender and the district of residence. We first report estimates of the impact of the FFE programs on school attendance based on the unannounced attendance visit data, and then discuss results based on self-reported attendance rates from the household survey, which are clearly inflated.

Table 6 presents results of the impact of the FFE programs on attendance taken in the morning during unannounced attendance visits. We find no significant impact of either program on average morning attendance of children age 6-13. However, there are positive and statistically significant impacts of both the SFP and THR programs on the morning attendance of older children, aged 10-17, ranging from 8-12 percentage points. In table 7 we

[6]It is possible that a child attending school on the day of an unannounced attendance visit could be mistakenly recorded as absent because the child could not be found. Although enumerators made great efforts to find the students at school, the learning centers in sample IDP camps were relatively large. Average enrollment in these learning centers in 2004 was 3,293 students, but we cannot reject equality of learning center enrollment across treatment arms. As a result, attendance may be somewhat understated from these measures, but we do not expect the difficulty in locating students to be correlated with the treatment. Estimated differences in impact across treatment groups should be unbiased.

divide the sample by gender and report results for girls in columns (1)-(5) and for boys in columns (6)-(10). We find that the in-school meals program had a positive impact on the morning attendance of girls of all ages and that the THR program had a significant positive impact on the attendance of boys aged 10-17. We suspect that girls have a larger attendance response to the SFP meals in part because girls' attendance rates are lower overall, so they have a higher capacity to respond to treatment. Again, for both girls and boys, the impacts of the two programs are not significantly different from one another except for the impact on boys aged 10-17, where the THR program performs significantly better than the SFP program.

Tables 8 and 9 present the same results as Tables 6 and 7 but for afternoon attendance. We find positive impacts of access to in-school meals on all age groups in table 8 and positive impacts of access to take-home rations on 6-9 year olds and 10-17 olds, but not 10-13 olds. For children age 6-17, the SFP and THR transfers increase afternoon school attendance by 14.6 and 14.1 percentage points, respectively. When the sample is separated by gender in Table 9, we find positive impacts of both programs on girls aged 6-9 and positive impacts of the THR program on 10-17 year old boys. It is important to note that the 6-9 year old girls with afternoon attendance information are in grades 3 and above.

These results show considerable impacts of the SFP and THR programs on school attendance. Based on the results from unannounced attendance data, we find significant positive impacts of both in-school meals and take-home rations on morning and afternoon attendance. For most age group and gender categories, we cannot reject the equality of impacts of both programs. This comparison of impacts across modalities suggests that differences in the timing of meals between the two programs is not the major contributor to the impacts of access to the programs on attendance. Similarly, differences in the placement of attendance incentives, either with the child or the caregiver, does not appear to play a significant role in the impact of the programs on average attendance. However, in order to further investigate

Table 6: Impact of FFE on Morning School Attendance

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 6-13 | 6-17 | 6-9 | 10-13 | 10-17 |
| SFP | .072 | .085* | .090 | .058 | .084* |
| | (.047) | (.050) | (.056) | (.047) | (.051) |
| THR | .063 | .091* | .055 | .075 | .118** |
| | (.047) | (.051) | (.058) | (.050) | (.056) |
| Age | .015*** | -.0002 | .025 | .031** | -.014** |
| | (.004) | (.003) | (.016) | (.014) | (.007) |
| Female | -.039** | -.040** | -.016 | -.060*** | -.059*** |
| | (.017) | (.016) | (.025) | (.022) | (.019) |
| Pader | .163*** | .159*** | .193*** | .131*** | .134*** |
| | (.041) | (.041) | (.054) | (.037) | (.039) |
| May | -.163*** | -.116*** | -.124* | -.201*** | -.107** |
| | (.043) | (.039) | (.067) | (.053) | (.054) |
| June | -.120** | -.123** | -.208*** | -.050 | -.074 |
| | (.060) | (.054) | (.067) | (.062) | (.052) |
| July | -.166** | -.173* | -.294*** | -.051 | -.094 |
| | (.083) | (.089) | (.113) | (.055) | (.069) |
| November | -.083 | -.078 | -.184*** | .0002 | -.015 |
| | (.059) | (.052) | (.067) | (.056) | (.044) |
| December | .051 | .073 | -.073 | .171** | .182*** |
| | (.072) | (.070) | (.086) | (.069) | (.068) |
| Constant | .601*** | .716*** | .591*** | .373** | .854*** |
| | (.067) | (.053) | (.164) | (.168) | (.087) |
| No. of Observations | 2266 | 2782 | 1177 | 1089 | 1605 |
| $R^2$ | .052 | .044 | .057 | .061 | .051 |
| p-value SFP=THR | 0.818 | 0.885 | 0.537 | 0.661 | 0.366 |

Notes: Standard errors are in parentheses and are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%. May through December are dummy variables for the month of the unannounced attendance visit. The omitted month is April.

Table 7: Impact of FFE on Morning School Attendance by Gender

| | Female | | | | | Male | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 6-13 | 6-17 | 6-9 | 10-13 | 10-17 | 6-13 | 6-17 | 6-9 | 10-13 | 10-17 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| SFP | .111** | .108* | .115* | .131** | .114** | .038 | .067 | .067 | .019 | .068 |
| | (.053) | (.055) | (.066) | (.062) | (.057) | (.047) | (.051) | (.052) | (.052) | (.057) |
| THR | .070 | .080 | .061 | .096 | .102 | .064 | .105* | .045 | .085 | .145** |
| | (.054) | (.054) | (.077) | (.062) | (.062) | (.049) | (.056) | (.063) | (.052) | (.059) |
| Age | .017** | .002 | .041* | .067*** | -.003 | .014*** | -.002 | .008 | .003 | -.023*** |
| | (.007) | (.005) | (.025) | (.022) | (.010) | (.005) | (.004) | (.016) | (.012) | (.008) |
| Pader | .176*** | .179*** | .187*** | .161*** | .175*** | .154*** | .143*** | .201*** | .110** | .102** |
| | (.043) | (.043) | (.063) | (.039) | (.038) | (.045) | (.045) | (.055) | (.048) | (.048) |
| May | -.215*** | -.134*** | -.125 | -.302*** | -.130* | -.101 | -.094 | -.120 | -.105 | -.087 |
| | (.060) | (.052) | (.117) | (.100) | (.078) | (.064) | (.057) | (.081) | (.086) | (.077) |
| June | -.092 | -.096 | -.154* | -.051 | -.068 | -.145** | -.147** | -.266*** | -.055 | -.085 |
| | (.076) | (.066) | (.083) | (.082) | (.060) | (.060) | (.065) | (.083) | (.069) | (.081) |
| July | -.180* | -.220* | -.300** | -.076 | -.177 | -.162* | -.141 | -.304** | -.040 | -.037 |
| | (.098) | (.124) | (.133) | (.065) | (.109) | (.095) | (.089) | (.125) | (.077) | (.082) |
| November | -.034 | -.032 | -.135* | .055 | .034 | -.131** | -.120* | -.235*** | -.056 | -.059 |
| | (.067) | (.058) | (.070) | (.066) | (.050) | (.066) | (.065) | (.081) | (.070) | (.073) |
| December | .148* | .156* | .046 | .248*** | .253*** | -.052 | -.007 | -.235** | .115 | .138 |
| | (.082) | (.081) | (.095) | (.086) | (.082) | (.074) | (.075) | (.093) | (.082) | (.089) |
| Constant | .489*** | .613*** | .397** | -.176 | .605*** | .664*** | .771*** | .777*** | .745*** | 1.003*** |
| | (.094) | (.069) | (.198) | (.254) | (.118) | (.068) | (.055) | (.156) | (.154) | (.101) |
| No. of Observations | 1108 | 1316 | 603 | 505 | 713 | 1158 | 1466 | 574 | 584 | 892 |
| $R^2$ | .068 | .06 | .067 | .115 | .07 | .041 | .035 | .054 | .033 | .049 |
| p-value SFP=THR | 0.322 | 0.492 | 0.471 | 0.289 | 0.766 | 0.595 | 0.391 | 0.709 | 0.181 | 0.080* |

Notes: Standard errors are in parentheses and are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%. May through December are dummy variables for the month of the unannounced attendance visit. The omitted month is April.

Table 8: Impact of FFE on Afternoon School Attendance

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 6-13 | 6-17 | 6-9 | 10-13 | 10-17 |
| SFP | .122** | .146*** | .252*** | .064* | .114*** |
| | (.050) | (.048) | (.076) | (.037) | (.040) |
| THR | .093* | .141*** | .205** | .051 | .120*** |
| | (.049) | (.049) | (.084) | (.045) | (.043) |
| Age | .035*** | .008 | .067** | .025 | -.014** |
| | (.008) | (.005) | (.028) | (.017) | (.007) |
| Female | .006 | -.010 | .0009 | .015 | -.011 |
| | (.024) | (.020) | (.043) | (.031) | (.026) |
| Pader | .089** | .086** | .158** | .057 | .059* |
| | (.042) | (.040) | (.076) | (.035) | (.032) |
| May | -.210** | -.136 | -.185 | -.240*** | -.160** |
| | (.095) | (.089) | (.135) | (.072) | (.068) |
| June | -.018 | -.042 | -.008 | -.034 | -.067 |
| | (.095) | (.088) | (.144) | (.073) | (.063) |
| July | -.030 | -.019 | .008 | -.065 | -.043 |
| | (.101) | (.098) | (.189) | (.075) | (.072) |
| November | -.002 | -.006 | -.011 | -.019 | -.018 |
| | (.096) | (.088) | (.149) | (.064) | (.058) |
| December | .155 | .189* | .363** | .047 | .142* |
| | (.112) | (.105) | (.173) | (.076) | (.075) |
| Constant | .200 | .453*** | -.170 | .391* | .791*** |
| | (.138) | (.103) | (.301) | (.204) | (.089) |
| No. of Observations | 1359 | 1855 | 424 | 935 | 1431 |
| $R^2$ | .049 | .03 | .096 | .021 | .026 |
| p-value SFP=THR | 0.493 | 0.886 | 0.509 | 0.755 | 0.835 |

Notes: Standard errors are in parentheses and are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%. May through December are dummy variables for the month of the unannounced attendance visit. The omitted month is April.

Table 9: Impact of FFE on Afternoon School Attendance by Gender

| | Female | | | | | Male | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 6-13 | 6-17 | 6-9 | 10-13 | 10-17 | 6-13 | 6-17 | 6-9 | 10-13 | 10-17 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| SFP | .179** | .180** | .354*** | .062 | .098 | .082 | .124*** | .113 | .076 | .126*** |
| | (.076) | (.074) | (.094) | (.071) | (.069) | (.050) | (.039) | (.081) | (.050) | (.035) |
| THR | .097 | .132* | .242** | -.002 | .057 | .096** | .149*** | .135 | .107* | .164*** |
| | (.080) | (.077) | (.104) | (.078) | (.074) | (.048) | (.044) | (.098) | (.055) | (.045) |
| Age | .043*** | .009 | .069** | .044 | -.019* | .027*** | .006 | .072* | .008 | -.012 |
| | (.010) | (.007) | (.030) | (.028) | (.011) | (.008) | (.007) | (.040) | (.014) | (.009) |
| Pader | .129*** | .137*** | .180** | .094** | .113*** | .062 | .050 | .134 | .025 | .021 |
| | (.048) | (.052) | (.090) | (.042) | (.042) | (.046) | (.039) | (.082) | (.044) | (.036) |
| May | -.354*** | -.219* | -.285** | -.384*** | -.227** | -.037 | -.039 | -.044 | -.097 | -.097 |
| | (.134) | (.123) | (.143) | (.124) | (.113) | (.097) | (.092) | (.137) | (.085) | (.081) |
| June | -.048 | -.069 | -.186 | .001 | -.046 | .006 | -.022 | .178 | -.078 | -.095 |
| | (.120) | (.100) | (.148) | (.102) | (.078) | (.095) | (.106) | (.154) | (.082) | (.100) |
| July | .002 | -.026 | .011 | -.008 | -.048 | -.057 | -.013 | .043 | -.133 | -.054 |
| | (.110) | (.123) | (.233) | (.083) | (.109) | (.118) | (.114) | (.178) | (.113) | (.107) |
| November | .018 | .007 | -.080 | .032 | .018 | -.022 | -.018 | .050 | -.081 | -.060 |
| | (.106) | (.094) | (.150) | (.083) | (.073) | (.110) | (.111) | (.168) | (.091) | (.099) |
| December | .181 | .226* | .275 | .043 | .194* | .141 | .165 | .507*** | .038 | .106 |
| | (.136) | (.127) | (.175) | (.118) | (.113) | (.118) | (.118) | (.192) | (.099) | (.106) |
| Constant | .079 | .393*** | -.177 | .158 | .825*** | .320** | .497*** | -.180 | .622*** | .797*** |
| | (.165) | (.123) | (.308) | (.359) | (.157) | (.144) | (.117) | (.392) | (.175) | (.119) |
| No. of Observations | 619 | 821 | 207 | 412 | 614 | 740 | 1034 | 217 | 523 | 817 |
| $R^2$ | .099 | .057 | .157 | .065 | .045 | .025 | .018 | .076 | .011 | .025 |
| p-value SFP=THR | 0.069* | 0.328 | 0.194 | 0.132 | 0.312 | 0.769 | 0.543 | 0.805 | 0.539 | 0.348 |

Notes: Standard errors are in parentheses and are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%. May through December are dummy variables for the month of the unannounced attendance visit. The omitted month is April.

the way that the placement of attendance incentives with the child or the child's caregiver affects attendance outcomes, we estimated the models for impact on morning and afternoon attendance from unannounced attendance visits controlling for school quality variables collected during these attendance visits. The measures of school quality collected include whether the teacher is a female, the log of class size, and whether the teacher is present. We expect these measures of school quality to be more important to parents' or caregivers' decisions about whether the send a child to school than to the child, either because the child is less informed about the returns to a quality education or because the motivation to receive meals at school overwhelms the importance of school quality in the child's attendance decision. Table 10 presents the results of these attendance models, controlling for school quality. As expected, the measures of school quality have no effect on attendance decisions of households in SFP camps, but have a significant effect on attendance of households in THR camps. These results confirm that the placement of attendance incentives with children or parents matters in the attendance decision and is an important feature of school feeding program design. It is important to note that the results of Table 10 are consistent with another explanation. SFP transfers are effectively conditioned on school attendance based on the provision of the meals at schools. If THR transfers are effectively unconditioned, then school quality measures would be relevant to the attendance decisions of households receiving THR transfers but their effects would be muted for households receiving SFP transfers. We think this alternative explanation is somewhat less likely because WFP monitored attendance in THR schools.

We also examined the impact of the FFE programs on school attendance using self-reported attendance data from the household survey questionnaires. In these data, we measure attendance as the number of days a child attended school in the last 7 days in proportion to the number of days the school was open (usually 5). Self-reported attendance rates were much higher than attendance rates observed in unannounced attendance

Table 10: Impact of FFE on School Attendance, Conditional on School Quality

| | Morning | | | Afternoon | | |
|---|---|---|---|---|---|---|
| | All | SPF Alone | THR Alone | All | SFP Alone | THR Alone |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SFP | .043 | .048 | | .093** | .094** | |
| | (.035) | (.033) | | (.043) | (.039) | |
| THR | .035 | | .033 | .088* | | .104** |
| | (.041) | | (.040) | (.047) | | (.042) |
| Child age in years | .000 | .004 | .001 | .004 | .005 | .005 |
| | (.004) | (.004) | (.006) | (.005) | (.005) | (.008) |
| Female teacher | -.062* | -.054 | -.101** | -.008 | .094 | -.023 |
| | (.034) | (.033) | (.045) | (.061) | (.065) | (.052) |
| Log of class size | .058* | .038 | .111** | .046 | .018 | .122 |
| | (.030) | (.033) | (.052) | (.042) | (.044) | (.076) |
| Teacher is present | .121*** | .121** | .175** | .103*** | .052* | .182*** |
| | (.042) | (.055) | (.062) | (.037) | (.030) | (.050) |
| Constant | .409** | .526** | .035 | .350 | .564** | -.199 |
| | (.155) | (.186) | (.222) | (.232) | (.224) | (.384) |
| Observations | 2523 | 1715 | 1372 | 1441 | 1036 | 714 |
| F stat on school quality variables | | 3.09 | 9.61*** | | 1.59 | 4.89** |

Notes: Omitted grade is 3. Omitted month of attendance visits is November. Grade and month dummy variables not shown. Standard errors in parenthesis are clustered at the camp level. * significant at 10%, ** significant at 5%, *** significant at 1%.

visits. In the second household survey round, average self-reported attendance over the past week was 95.3 percent, while average morning attendance was only 74.2 percent during the unannounced attendance visits.[7] We found no significant impacts of either program on self-reported attendance rates.

## 5.3 Age at Entry to Primary School

The FFE programs have the potential to reduce age at entry to primary school but, in the short-run, may attract older children to school who, without the program, would not have entered primary school at all. Therefore, the expected impact of the programs after two years is ambiguous. In Uganda, the recommended age for beginning primary school is six years old but the average age at entry in our sample in the baseline is over seven years old. There are no significant differences in the age at entry across treatment groups in the baseline as reported in Table 1. In Table 11, we present single-difference estimates of

[7]As noted in table 1, there was significant sampling error in the baseline self-reported attendance rates from the household survey between the SFP and control groups. Given the maximum attendance possible is 100 percent and the high baseline self-reported means, there is little room for improvements in attendance rates, especially in the SFP group.

Table 11: Age at Entry to Primary School, 2007

| | (1) |
|---|---|
| | SD |
| | 6-13 |
| SFP | -.150* |
| | (.090) |
| THR | -.204** |
| | (.097) |
| Female | .108 |
| | (.096) |
| Pader | -.336*** |
| | (.085) |
| Constant | 6.907*** |
| | (.084) |
| No. of Observations | 622 |
| $R^2$ | .03 |
| p-value SFP=THR | 0.610 |

Notes: Impact estimates are single-difference intent-to-treat effects.
R2 is a dummy variable indicating the observation is from the resurvey.
Standard errors are in parentheses and are clustered at the camp level.
* significant at 10%, ** significant at 5%, *** significant at 1%.

the impact of the FFE programs on age at entry to primary school for children who began primary school after the baseline survey. We find a significant impact of both programs on age at entry to primary school, reducing the age of entry by about two standard deviations.

## 5.4 Grade Repetition

Grade repetition is quite common in the sample, with 44 percent of children enrolled in primary school at baseline having repeated at least one class. FFE programs should reduce grade repetition if they improve learning. Results regarding learning are presented in (Adelman et al., 2008a). There are no differences in the number of classes repeated in the baseline across treatment groups as reported in Table 1. Results are reported in Table 12. Column (1) reports treatment group difference-in-difference estimates, and columns (2) and (3) present these estimates by gender. The results show that the in-school meals program

Table 12: Impact of FFE on Grade Repetition

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | DID | | Child Fixed Effects | | |
| | All | Girls | Boys | All | Girls | Boys |
| | 6-13 | 6-13 | 6-13 | 6-13 | 6-13 | 6-13 |
| SFP*R2 | -.115 | -.020 | -.212** | -.151** | -.080 | -.213** |
| | (.077) | (.081) | (.103) | (.072) | (.109) | (.094) |
| THR*R2 | -.099 | -.081 | -.119 | -.108 | -.134 | -.094 |
| | (.079) | (.093) | (.100) | (.081) | (.104) | (.117) |
| R2 | .047 | .014 | .084 | .200*** | .188** | .213*** |
| | (.058) | (.057) | (.077) | (.063) | (.091) | (.052) |
| SFP | .045 | -.027 | .118 | | | |
| | (.065) | (.073) | (.091) | | | |
| THR | .112* | .072 | .156* | | | |
| | (.064) | (.079) | (.091) | | | |
| Female | .026 | | | | | |
| | (.026) | | | | | |
| Age | .094*** | .089*** | .099*** | | | |
| | (.006) | (.008) | (.009) | | | |
| Pader | .008 | -.012 | .027 | | | |
| | (.027) | (.038) | (.037) | | | |
| Constant | -.469*** | -.349*** | -.570*** | .413*** | .414*** | .412*** |
| | (.061) | (.089) | (.095) | (.018) | (.025) | (.031) |
| No. of Observations | 2691 | 1341 | 1350 | 3196 | 1607 | 1589 |
| $R^2$ | .086 | .077 | .096 | .015 | .016 | .018 |
| p-value SFP=THR | 0.828 | 0.525 | 0.327 | 0.490 | 0.500 | 0.368 |

Notes: R2 is a dummy variable indicating the observation is from the resurvey.
Standard errors are in parentheses and are clustered at the camp level.
* significant at 10%, ** significant at 5%, *** significant at 1%.

lead to a significant decrease in the number of classes repeated for boys. Columns (4)-(6) present child fixed effects for all children, and again by gender. The results are consistent with the treatment group difference-in-difference estimates. However, there is no significant difference in the impacts of the two treatments.

## 5.5 Progression to Secondary School

Conceptually, the FFE programs' impacts on progression to secondary school are ambiguous. The programs may have improved learning and progression within primary school and, therefore, increased progression to secondary school. Alternatively, the programs may have enticed pupils to remain in primary school in order to continue to receive the transfers,

rather than progressing to secondary school or dropping out. Both of these possibilities are examined below.

We investigate the impact of the FFE programs on progression to secondary as well as on an alternative measure, remaining in primary school. The latter measure considers whether children in the late grades at the start of the FFE programs remained in primary school longer in order to continue to receive the interventions. The sample of interest is pupils in grades 6 and 7 during the baseline survey. Those in grade 7 were expected to complete primary school shortly after the survey and before the introduction of the interventions. Those in grade 6 would receive an intervention or be assigned to the control group the following year. If they did not repeat the class, they would have been expected to complete primary school and begin secondary school prior to the resurvey. These two samples allow us to study the impact of the interventions on the progression to secondary school and on the completion of primary school.

Many of the pupils in grades 6 and 7 in the 2005 baseline survey repeated a grade and had not progressed to secondary by 2007. Of the grade 6 pupils in the baseline who had not progressed to secondary school by the time of the resurvey, 19 percent remained in grade 6, 37 percent were attending grade 7, and 33 percent were no longer attending school. Seventy percent of pupils who did not progress to secondary school were in grade 6 at the baseline, while the remaining 30 percent had been in grade 7.

Results are reported in Table 13. Results on progression to secondary school are presented in column (1). As expected the coefficients on the interventions interacted with grade 7 are insignificant because these individuals were effectively untreated when they completed primary school at the end of 2005. In addition, we find no impact of the programs on pupils in grade 6 in the baseline. Therefore, we investigate whether in fact the programs are resulting in individuals remaining in primary school. These results are presented in column (2). We find an increase in the probability of remaining in primary school with the introduction of

Table 13: FFE Impacts on Progression to Secondary School

| | (1) | (2) |
|---|---|---|
| | Progression to Secondary | Remaining in Primary |
| P6*SFP | -.010 | .178** |
| | (.081) | (.083) |
| P6*THR | .019 | .040 |
| | (.081) | (.094) |
| P7*SFP | .080 | .017 |
| | (.146) | (.133) |
| P7*THR | .063 | -.128 |
| | (.144) | (.149) |
| P6 | -.222* | .140 |
| | (.119) | (.151) |
| Age | -.002 | -.015 |
| | (.007) | (.011) |
| Female | -.070 | -.036 |
| | (.051) | (.070) |
| Pader | .125** | -.073 |
| | (.051) | (.061) |
| Constant | .337* | .619** |
| | (.193) | (.257) |
| No. of Observations | 214 | 214 |
| $R^2$ | .137 | .108 |
| p-value SFP=THR | 0.613 | 0.096* |

Notes: Standard errors are in parentheses and are clustered at the camp level.
* significant at 10%, ** significant at 5%, *** significant at 1%.

in-school meals. Although these children had not progressed to secondary school, they are more likely to remain in primary school than to have dropped out as a result of the SFP transfers. This suggests the possibility that in-school meals may have the unintended effect of increasing the completion time of primary school, at least in the short term. If this effect is at work, it could be removed by offering a similar in-school meals program in secondary schools.

# 6 Conclusion

FFE programs are generally acknowledged to increase primary school participation. However, the size of these effects varies by context and the number of rigorous evaluations of this topic is relatively few. This paper presents new evidence on the impact of two different methods of food for education delivery on school participation in Northern Uganda using a prospective, randomized controlled evaluation design. Moreover, this study compares the impacts of the World Food Programme's in-school meals program with an experimental take-home rations program conditional on school attendance. Differences between these two modalities in the timing of meals and in the control over the food could lead to differences in impacts.

The results show positive impacts of the in-school meals program on primary school enrollment when we restrict the analysis to children who were not enrolled before the introduction of the FFE programs. Though only the impact of the SFP program is statistically significant, we do not find significant differences between the impacts of the two programs. Moreover, based on the results from unannounced attendance data, we find significant positive impacts of both in-school meals and take-home rations on morning and afternoon attendance. The results also show a weakly significant impact of both FFE programs on reducing age at entry. Furthermore, we find a reduction in grade repetition from the SFP program for boys, but the SFP impact is not statistically different from the THR program. Finally, we find no impact of either program on progression to secondary school. However, children in grades 6 in school feeding program schools in 2005 were significantly more likely to remain in primary school as of 2007. This suggests that school meals may have the unintended effect of increasing the time taken to complete primary school.

These results lend considerable support to the potential for FFE programs to achieve their primary goal of increasing school participation. In general, both the SFP and THR

program performed similarly well. These results suggest that food for education programs remain an effective strategy for attracting children to school.

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