

The Impact of Alternative Food for Education Programs on School Participation and Education Attainment in Northern Uganda*

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Abstract:

There is consensus that Food for Education (FFE) programs increase primary school participation and education attainment. However, this view relies on limited causal evidence, making it difficult to anticipate the size of expected impacts. Moreover, little is known about how the design of FFE programs affects outcomes. This paper presents evidence on the impact of alternative FFE programs on schooling in Northern Uganda using a prospective, randomized controlled evaluation conducted from 2005-2007. We compare the impact of World Food Program'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 neither FFE program had an impact on primary school enrollment rates, which were already high following the introduction of free Universal Primary Education in 2002. School attendance (morning and afternoon) was measured through unannounced attendance visits to overcome bias in attendance data collected from respondents or school registers. Both programs had large impacts on school attendance, with impacts varying by grade and gender. The SFP program increased boys' morning attendance rates in grades 1-2 by 13 percentage points and increased average afternoon attendance by 9.3 percentage points. THR increased boys' afternoon attendance by 9-12 percentage points. In grades 6-7, THR had significantly larger impacts than SFP, increasing average attendance by 17-18 percentage points, and girls' morning attendance by 30 percentage points. Both SFP and THR reduce grade repetition, but SFP impacts are larger. SFP also reduced girls' age at entry. Neither program affected progression to secondary school. However, children in grades 6-7 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

Though their effectiveness is debated, the Millennium Development Goals have led to new policies and sometimes to the commitment of additional resources to address basic problems of economic development. The MDG on Universal Primary Education is one that has attracted considerable attention 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 this has increased enrollment rates on the books, progress in improving primary school attendance has been limited. This pattern has led donors and governments to consider complementary programs to boost primary school participation. In this context, school feeding programs (SFPs) have received renewed attention.

School meals 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 importantly by contextual factors such as initial attendance rates, school quality, and transfer size (Ahmed, 2004; Vermeersch and Kremer, 2004; Jacoby, Cueto, and Pollitt, 1996; Powell et al., 1998). Also, the evidence from many studies is limited because the effect is measured only for children already in school, the research design is not causal, or the sample is not representative of school age children (Adelman, Gilligan and Lehrer, 2008). 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 SFP program.

This ambiguity about how much school feeding increases school participation weakens support for new SFP initiatives because these programs can be fairly expensive to operate. At a cost of \$20-\$35 per child per year, they begin to rival the investment in education itself in some developing countries. If raising school participation is the only goal, research suggests that other programs such as deworming, free school uniforms, parent-teacher partnerships and other programs that increase teacher incentives may be more cost-effective (Miguel and Kremer, 2004; Evans, Kremer and Ngatia, 2008; Tan, Lane and Lassibille, 1999). The impact of these programs on school participation may not be as large as from an SFP, but these alternatives are cheaper to operate. However, when school feeding programs provide nutritious food to undernourished students, they can reduce short-term hunger and help improve students' learning

and cognitive development (Adelman et al., 2008). Most other programs designed to improve school participation, including deworming alone, cannot claim such broad impacts. In order to understand the full impact of SFPs, it is first necessary to obtain additional evidence and more reliable estimates on their impact on school participation.

This study presents rigorous evidence of the impact of a school feeding program operated by the World Food Programme (WFP) on primary school participation and education attainment 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 school feeding program 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 were formed between 1997 and 2003 in response to security problems brought on by a rebel insurgency led by the Lords' Resistance Army (LRA). Most of the rural population in these two districts was living in the IDP camps at the time of the baseline survey in 2005. These IDP camps represent well-defined service areas for the primary schools they contain.

As part of this study, WFP also operated a take-home rations (THR) program conditional on school attendance in a subset of randomly selected schools. This expands the contribution of this research by enabling an investigation into how differences between these two modalities of food-for-education (FFE) programs determine their impacts on school participation and education attainment. Ahmed and del Ninno (2002) provide evidence of the impact of a similar 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. Because the program was targeted to poor households in selected communities, it is difficult to determine whether the magnitude of these effects would have been different under a more common in-school feeding program.

By directly comparing SFP and THR programs operated in the same context in Northern Uganda, this study should help to explain which components of FFE programs are most vital to improving school participation and education attainment. These important program components,

including 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.² Take-home rations conditional on school attendance provide an informative counterfactual to an SFP by substituting for meals provided at school and during the school day with a monthly dry food ration provided at home. This comparison will allow us to determine whether providing meals at school to hungry children is uniquely effective in attracting them to school day after day.

In particular, this study considers how this difference in the timing of meals inherent in the two modalities affects their impacts. Although it is possible for children receiving THR to bring food with them to school or, in some cases, return home for lunch, it is more difficult and costly than receiving a meal at school. In practice, THR beneficiaries rarely supplied their own lunch. As a result, children receiving THR had systematically different timing of access to the food during the day than their counterparts in SFP schools. Also, 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 THR parents rather than school children have the incentive for the child to attend school. Attendance was monitored and rations could be terminated if the child did not attend at least 80 percent of school days. Also, it is easier to give some of the food to other household members in THR. 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 an improved attention span.

The remainder of this paper is organized as follows. Section 2 explores the alternative mechanisms through which SFP and THR programs affect school participation and education attainment. Section 3 describes the study design. Section 4 describes the FFE programs and the data. The results are presented in Section 5 and Section 6 concludes.

² For this study, the composition and quantity of the SFP and THR rations were kept the same in order to focus attention on differences in these program modalities, rather than differences in type or size of transfers, or “dose” effects.

2. How FFE Programs Effect School Participation and Educational Attainment

FFE programs, such as the SFP and THR programs in Northern Uganda, improve school participation by decreasing the net cost of sending children to school. The economic rationale for FFE programs is that they have a positive income effect on the decision to send a child to school. Parents decide to enroll their children in school if the expected effect on the child's future income exceeds the net cost of having the child enrolled this school year. This net cost includes the direct cost (e.g., school fees, uniforms and school supplies) plus the opportunity cost of schooling (e.g., loss of the child's income, labor on farm, and labor time at home) minus any direct benefit from school participation (e.g, transfers from an FFE program). The decision regarding a child's school attendance is similar. Parent's will send a child to school in a given week or day if the 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 months take-home ration.

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 labor, care-giving 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. The quality of the school also affects the participation decision by changing the expected benefits of schooling in terms of learning and future income. 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 quality of school physical and sanitation infrastructure; availability of school supplies such as textbooks and chalkboards; classroom crowding and the quality of child's peers.

The effect of the two FFE modalities on the school participation decision also differs in terms of 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, a school feeding program may have a larger effect on school attendance than take-home rations. Alternatively, if the nutritional effect of the food transfers on school performance comes in terms of overall nutrition, so that children

are able to smooth the benefits of the additional food consumption over a 24-hour period, then the effect of SFP and THR on school attendance will not differ in this regard.

The way that control over the food transfers from SFP or THR programs by the child or parent affects school participation decisions might be substantial. 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 in an SFP provide a direct incentive to a child to attend school, particularly if that child is hungry and the food is enjoyable and substantial. This suggests that school quality may have less of an effect on school attendance in SFP schools than THR schools because the child is motivated to attend by the meals, regardless of the effect on learning. In a THR program, the food ration is controlled by the child's parent or caregiver, who receives the direct incentive to send the child to school. If the transfer is important to the parent as additional income or as a source of nutrition for the school-age child or a younger sibling, for example, the parent will send the child to school. In this sense, differences in how the composition of the food from the FFE program, in terms of quality, quantity, taste and nutrition, are viewed by the child and the parent 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 household members receive the additional nutrition provided by the program. Though the beneficiary child in a school feeding program consumes all of the food transfer at school, parents may rationally respond by redistributing some food away from that child to other household members at other meals. Jacoby (2002) termed the amount of the food transfer that "sticks" to the child in terms of increased food consumption as the intrahousehold "flypaper effect". Though researchers and managers of school feeding programs have often expressed concern that the flypaper effect may be small, Jacoby showed that children in an SFP in the Philippines received a substantial fraction of additional food provided by the SFP transfers. In a THR program, the potential for small flypaper effects is even greater because it is easier for parents to redistribute the food to other household members. This redistribution may be well justified because the benefits of the additional nutrition to a child under age two or to a sick family member may be much greater than to the school-age child. There is currently no evidence on the relative size of this flypaper effect between SFP and THR programs.

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

These 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 each modality on a child's age of entering school is similar to the general effects on school enrollment and attendance, after accounting for the child's young age. The effect of these alternative FFE modalities on education attainment 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 grade 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 education attainment. However, school performance is has a strong influence on grade promotion, so differences in the effect of SFP and THR transfers on a student'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.

Two other common differences in SFP and THR modalities include the size of transfer provided and the way the programs are targeted within schools. Most THR programs run by WFP, for example, are provided as complementary programs to in-school meals to boost program impact on groups with low education outcomes, particularly girls in some countries or extremely poor households. In these instances, the food provided in the ration is often limited to a tin of cooking oil or a small amount of grain, since the beneficiary child is already receiving food transfers at school. However, in order to study relative impacts of the SFP and THR programs driven by the other differences in these modalities, the THR program in Northern Uganda provided an identical food ration in quantity and composition to that provided under the SFP. The only differences in amount of ration received by beneficiaries under either program should be due to differences in implementation, not design. There are few examples of comparable, large-scale take-home food ration programs conditional on school attendance in

developing countries. One such THR program was the one in Bangladesh studied by Ahmed and del Ninno (2002). Another similar modality to the THR program operated in Northern Uganda is the Conditional Cash Transfer (CCT) programs popularized in Latin America and now being piloted in many parts of Africa and Asia.³ The main difference between the CCT programs and this THR program is that transfers are provided in cash rather than food. If the food provided is *inframarginal* (equal or less in quantity to what the child would have consumed in the absence of the program) and functioning markets for the food transfers exist, the THR and CCT program modalities should have comparable effects.⁴

THR programs are also more easily targeted than SFPs to poorer households or to children less likely to attend school 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 needy 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 THR program conducted in Northern Uganda did not target children within schools in order to make the program as comparable to the SFP as possible.

3. Empirical Strategy

The Identification Strategy

The evaluation of the impact of the FFE modalities relies on an experimental, randomized, prospective design. A prospective study collects data before the interventions begin and after a period of implementation. This makes it possible to control for pre-program child and household characteristics and to observe changes in outcome variables during the interventions. The

³ For evidence of the impact of CCT programs on school participation and educational attainment, see Schultz (2004) for Mexico, Maluccio and Flores (2004) for Nicaragua, Gilligan and Ahmed (2007) for Turkey.

⁴ This suggests that the comparison between SFP and THR modalities conducted here could provide evidence on the relative impacts of the SFP program to a CCT providing transfers of comparable value. This comparison is somewhat limited in the Northern Uganda setting because markets in the IDP camps were fairly thin, so that cash transfers may have had different, unanticipated effects than the food transfers provided through take-home rations.

experimental design was achieved by randomly assigning each IDP camp in the sample to one of three intervention or “treatment” groups: SFP, THR or control. In nearly all of the IDP camps, the camp boundary serves as the catchment area for the primary school learning center operating in the camp.

The random assignment of IDP camps into treatment groups makes it possible to place a causal interpretation on estimated impacts. The intuition is that if access to the program is random within a group of similarly eligible IDP camps, beneficiary or treatment status cannot be correlated with the outcomes. As a result, any observed differences in average outcomes over time between the treatment groups and the control group must be a result of the program. When access to the program is not random, measures of program impact based on a comparison of mean outcomes between program beneficiaries and a nonexperimental comparison group may be biased due to selection effects.⁵ Selection effects are caused by characteristics of the IDP camps or households that are correlated with the outcomes of interest and with the probability of receiving the intervention. Typically there are two causes of selection effects: (i) targeting of the program to communities based on factors affecting the outcome, and (ii) actions by the community or the household that affect participation in the program, either through lobbying the government or organization providing the treatment, or through the household’s decision to participate.

Random assignment of IDP camps to the interventions eliminates potential bias from program targeting or lobbying, but bias from sampling error or from household selection effects may still exist. Sampling error arises when, by chance, there are differences in mean preprogram outcomes or relevant household characteristics between the treatment and control group after the randomization.⁶ In a large sample of IDP camps sampling error would be small, but in moderate sized samples some sampling error may exist. This can be checked by testing for equality of mean outcomes in the baseline sample.

If the randomization is effective and sampling error is not a 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,

⁵ Heckman and Smith (2005) and Heckman, Ichimura and Todd (1997) describe how randomizing program access eliminates selection bias and identifies causal impacts of the program.

⁶ This is equivalent to flipping a coin ten times and getting eight “heads.” The expectation is for an equal probability of heads and tails, but this is not always achieved in finite samples.

$$(1) \quad \Delta^{SD} = E[Y_1^T - Y_1^C],$$

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-differences” (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),

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

DID estimates of the relative difference in impact between the SFP and THR treatments can be constructed in a similar manner, replacing T with SFP and C with THR in equation (2). That is, the difference in the average change in outcomes between SFP and THR camps provides an unbiased estimate of their relative impact.

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 she resides. This measure of program impact 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 control access to the program (unless people are willing to migrate to gain access), but once the program is available households control the decision to participate. In the evaluation literature, measures of the impact of access to a program are referred to as ‘intent to treat’ impact estimates, while measures of the impact of participation are referred to as the average impact of the ‘treatment on the treated.’ Intent to treat measures of program impact are typically lower than measures of the impact of the treatment on the treated because impacts are reduced whenever a potential beneficiary decides not to participate.

In some cases, it is appropriate in impact analysis to control for other factors that may affect program impact even in randomized experiments. One such case arises when other exogenous or independent events, such as economic shocks, occur during the program with different frequency or intensity across the treatment groups. Failure to control for such events in the analysis would lead to misleading attribution of program impact. A second case arises when there are systematic differences in household preprogram characteristics that may affect program outcomes, even if there is no difference in average preprogram outcomes themselves. In this case, controlling for the effect of these preprogram characteristics in the analysis may be justified and can improve the precision of the impact estimates. In these cases, impacts can be estimated conditional on a vector of pretreatment characteristics or contemporaneous shocks, X ,

$$(3) \quad \Delta^{DID|X} = E[(Y_1^T - Y_0^T) - (Y_1^C - Y_0^C) | X].$$

Econometric Specification

Regression analysis was used to estimate the impact of the SFP and THR programs. This is a convenient way to estimate differences in mean outcomes, to test for statistical significance, and to control for other factors when necessary. Let T_1 represent access to the SFP program and T_2 represent access to the THR program. The single difference impact of the programs in (1) can be estimated as

$$(4) \quad Y_{ic} = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \varepsilon_{ic},$$

where

Y_{ic} is the outcome for the i th child in camp c

$T_1 = 1$ if the child resides in a camp assigned to the SFP program, 0 otherwise

$T_2 = 1$ if the child resides in a camp assigned to the THR program, 0 otherwise

ε_{ic} is the unobserved child and camp specific error term.

If the randomization was effective, leading to no difference in mean outcomes before the programs, estimating (4) on outcomes measured after the programs have been implemented

provides a well-identified estimate of the impact of the SFP program in β_1 and of the THR program in β_2 .

If preprogram data on outcomes are available, and particularly if sampling error results in differences in these outcomes before the programs, DID estimates in (2) can be obtained by estimating

$$(5) \quad Y_{ict} = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \beta_3 R_2 + \beta_4 T_1 R_2 + \beta_5 T_2 R_2 + \varepsilon_{ict},$$

where

R_2 indicates the second survey round, conducted after program implementation

Y_{ict} is the outcome for the i th child in camp c in period t

ε_{ict} is the unobserved child-, camp-, and period-specific error term.

Here β_4 is the DID estimate of the impact of the SFP program on the change in the outcome before and after the program began and β_5 is the DID estimate of the impact of the THR program on the change in the outcome. Conditional impact estimates such as those in (3) can be obtained by adding a term for X in equation (5).

4. The Survey Data and the FFE Programs in Northern Uganda

Description of the Northern Uganda Sample

The sample was drawn from households living in IDP camps in Pader and Lira districts in Northern Uganda. In response to the conflict in the North, over 80 percent of the population moved to IDP camps starting in 1997, though most households entered a camp from 2002-2003. Due to the threat of attack by the LRA, camp residents were confined to the 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 rely on food aid for survival. WFP provides monthly food rations to each household living in the camps. In 2005, households living in camps in Pader district received monthly “general food distribution” (GFD)

rations of grain, oil, beans, and fortified corn soy 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 IDP camps are densely populated and living conditions in the camps are poor. Sanitation, water and other infrastructure are provided by donors, but these facilities were sometimes inadequate and water shortages were common.

Pader and Lira districts were selected for this evaluation study because they were WFP's desired locations for the next 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.

Consultations with WFP district staff from Lira and Pader indicated that most IDP camps contained only one "learning center" at the primary school level and that nearly all students in the learning center would be residents of the camp.⁷ Therefore, camps were chosen as the sampling unit for the randomization of the FFE programs and recent camp census data was 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 SFP getting 11 camps and THR and control each getting 10. 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 were

⁷ These learning centers are agglomerations of schools that existed outside the IDP camps before the local population was displaced and required to resettle in IDP camps. These schools generally operated jointly in one location in the IDP camp. The learning centers were very large, averaging more than 4300 enrolled students in 2005.

randomly sampled.⁸ The IDP camps were organized into blocks for organizational purposes. Blocks are small neighborhoods identified by footpaths, roads or natural boundaries. Random household sampling was stratified by block 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 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 baseline households had changed location since the baseline in November 2005. Despite this extraordinary degree of mobility, 81 percent of baseline households were located and re-interviewed in the second survey round.

Description of the FFE Interventions

All students who enrolled in primary school in 2006 were eligible to receive the FFE program for that camp. The FFE programs are managed and funded by the World Food Programme. The in-school meals program provides a free fortified mid-morning snack and lunch to all students enrolled in schools operating the program. The snack consists of a porridge made from micronutrient fortified corn-soy-blend (CSB), sugar, and water. The lunch consists mainly of beans and either hot *posho* (maize meal) or rice. The lunch also includes vegetable oil and salt. The combined meals provide roughly 1049 kcals of energy, 32.6 gm protein, and 24.9 gm fat at a cost of US\$0.17 per child per school day. The ration also meets two thirds of the child's daily vitamin and mineral requirements, including 99 percent of iron requirements.

⁸ The upper end of the 6-17 year age range is high for primary-school age, but a large share of children age 14-17 attend primary school, so they were included in the sample. However, many children in this older age range migrate outside the camps, so the share of the sample with children only in this older age range is very small. In order to avoid the selection problems that come with this migration, most of the analysis is restricted to children age 6-13, a more typical age range for primary school.

In order to qualify for the in-school meals program, schools were required to meet infrastructure requirements, including the presence of cooking facilities, latrines, and a basic hand washing facility. Families with children in the program were required to contribute firewood for cooking and a fee of about US\$0.12 per month toward the pay of the cooks. According to the World Food Programme, there is no limit to the number of primary-school-age children from a household that can receive school-based feeding.

The rations provided in the take-home rations (THR) program are equal in size and composition to the food received by in-school meals beneficiaries. These rations are provided to THR beneficiary households once per month. THR beneficiary households receive a THR ration for each primary-school age child that is enrolled and attends school at least 80 percent of the days in the previous month.

The composition of the SFP meals and take-home rations is very similar to that of the monthly GFD rations provided to residents of all IDP camps. Therefore, the food provided by the interventions is a very close substitute for what is typically available in the home. This suggests that under both treatments, the FFE ration increases the amount of food available to the household, but not the type.

Survey Data and Baseline Characteristics

The survey instruments used in the two rounds of household data collection in 2005 and 2007 included a detailed household questionnaire, a health questionnaire, a school questionnaire, and a camp questionnaire. 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.

The outcome variables examined include net and gross enrollment, net attendance, age at primary school entry, grade repetition, and progression to secondary school. Table 1 lists the outcome variables examined, provides a definition, and lists the data source from which the variable was constructed. Data on enrollment, self-reported attendance, age at entry, grade promotion and progression to secondary school come from the household questionnaire. Each of these variables is available from both the baseline and the resurvey, allowing for a difference-in-difference estimation strategy. Unannounced attendance visit data was collected on several visits

to each school. A visit involved attendance being taken in the morning and then again in the afternoon on the same day. This is the common format for taking attendance in Ugandan primary schools. These data are 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.

Even with a randomized treatment assignment, differences in the distribution of baseline outcome and control variables can arise in moderate sized samples due to sampling error. The use of a difference-in-difference estimator of program impacts should account for pre-treatment differences in the distribution of outcome variables. In some of the estimates, baseline characteristics are also included as regressors. If the randomization was effective, these controls should not be significant, but in moderate sized samples, they may be significant and will improve the precision of the impact estimates.

Baseline treatment group means of selected child, household and school characteristics are presented in Table 2.⁹ There is evidence of differences across the treatment groups for some of the outcome variables. The most important difference in distributions identified are significantly higher mean baseline self-reported attendance rates for the past 7 days in SFP camps and significantly higher baseline enrollment in both SFP and THR camps. We found few significant differences in means of control variables across the treatment groups, including household characteristics, school characteristics or the primary school aged child's characteristics prior to the introduction of the interventions.

5. Results

The analysis of the impact of the SFP and THR programs on school participation focuses on primary school enrollment, attendance, and age at entry. We then present results for measures of educational attainment, including grade repetition and progression to secondary school. Table 1

⁹ For further details regarding the randomization and its success, see Gilligan, Adelman & Lehrer (2006).

lists all outcome variables examined, the variable definition, and the data source from which the variable was constructed.

Enrollment

Impacts on enrollment are examined using two standard measures of school enrollment, gross primary school enrollment and net primary school enrollment. Gross enrollment (see Table 1) is the proportion of all children enrolled in primary school to the number of 6-13 year olds enrolled in primary school. This proportion can be greater than one because delayed start of schooling, 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 enrolled in primary school. We also extend our analysis to focus on younger children, who were more likely to have been affected by the introduction of the programs.

The implementation of the Universal Primary Education (UPE) policy in Uganda in 2002 abolished all overt primary school fees, and established a formula for funded schools based on the number of enrolled students. With this policy in place, it is unsurprising that baseline enrollment levels in the sample were high. The baseline net enrollment rate in the sample was 84.9 percent, while the gross enrollment rate was 88.0 percent. Table 2 shows that the control group had a significantly lower baseline gross and net enrollment rates compared to the school feeding and take-home rations groups. Therefore, difference-in-difference estimates that account for these differences in baseline enrollment are presented below.

The estimated difference-in-difference impact of the SFP and THR programs on net and gross enrollment is presented in Table 3. We find no impacts of either program on net or gross 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 in the estimates. Controlling for child fixed effects does not meaningfully change the results on net and gross enrollment of 6-13 year olds or on net enrollment of 6-9 year olds.¹⁰ However, we also examine the impact of the programs on enrollment of children aged 6-13 and aged 6-9 who were not enrolled in primary school in the

¹⁰ We also measure impacts by gender for these age groups and found no differences (results not presented).

baseline. Here, we find that the SFP program had a weakly significant impact on enrollment for these children who had not previously enrolled in school. The estimates indicate that providing meals at school lead to a 12 percent increase in the probability that a child who was 6-9 years old at the baseline would enroll in school by the time of the resurvey in 2007. The point estimate of the impact of the THR program on this group was half as large and was not significant, though a test showed that the difference in the estimated impacts between SFP and THR were not significant.

FFE programs lead to greater investments in education primarily by subsidizing schooling costs, though the SFP and THR modalities differ by whether they provide incentives for enrollment to the school-age child or to the parents. These results suggest that the education subsidy provided by the FFE programs were not large enough to change average enrollment rates of school age children, who were already enrolling at fairly high rates as a result of the UPE policy. However, focusing on younger children who had not enrolled in school at the baseline showed that the SFP program may have played a significant role in bringing these children into school. Because the estimated impact of SFP was not significantly different than THR for this measure, we cannot conclude whether providing incentives for enrollment directly to the child by providing meals at school was an important factor contributing to this result.

Attendance Overview

The survey data allow us to construct several measures of primary school attendance on which to examine the impacts of the FFE programs. Unannounced attendance visits were conducted at four intervals during the first year of the program and self-reported attendance data was collected in the household survey for all children enrolled in primary school. The attendance variable from the unannounced visits 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 (see Table 1). Children who were not identified during the attendance visit were not included in these estimates. These data were collected in both the morning and in the afternoon for all children age 6-17 at baseline who could be identified.¹¹ Children in grades 1 and 2 do not attend school in the afternoon, so they are not included in the afternoon attendance visits. Not all schools were visited during each unannounced attendance visit, so some but not all children have multiple observations in these

¹¹ It is customary in Ugandan primary schools to record both morning and afternoon attendance.

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 visit data. From the self-reported attendance data in the two rounds of the household survey, 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).

We believe the attendance measures based on the unannounced attendance visits provide the strongest measure of primary school attendance. Respondents to the household survey may be inclined to overstate their child's attendance if they think the data may be reported to school officials. As a result, self-reported attendance rates were much higher than attendance rates observed in unannounced attendance visits. In the second household survey round, average attendance over the past week was 95.3 percent, while average morning attendance was only 74.4 percent during the unannounced attendance visits. This tendency for respondents to overstate attendance leads to bunching of the distribution of attendance in the data, which makes it more difficult to identify differences in attendance by treatment group. Also, there was substantial sampling error in the baseline self-reported attendance rates from the household survey. Table 2 shows that average baseline attendance in SFP schools was 94.9 percent, which was 8.5 percentage points higher than in the control group. One plausible explanation for this pattern is that households in camps that were about to begin receiving school meals became aware of this and believed that they needed to demonstrate high attendance to be eligible for the program. The awareness could have been created if WFP began preparing the camps for the school meals program before the end of the 2005 school year. Camps in the program needed to have kitchens, latrines and water sources in place by the start of the program in early 2006 and some time was needed to prepare this infrastructure. It is unclear whether SFP beneficiary households would respond to this awareness by either overstating attendance in November 2005 or actually increasing attendance at this time just before the program. However, there is considerable evidence of these kinds of "anticipation effects" before the start of social programs in many other contexts.

Even if this difference in self-reported baseline attendance is not due to anticipation effects, the observed means leave very little room for improvement in attendance rates in the SFP group. In difference-in-differences estimates, this pattern also makes it difficult to find a larger

change in the SFP group than in the control group. We undertake an analysis to determine whether this baseline difference is due to sampling bias and not anticipation effects, and discuss how the impact estimates might be affected.

Attendance from Unannounced Visits

Table 5 presents results on the impact of the FFE programs on attendance taken in the morning during unannounced attendance visits. We find no significant average impact of either program on morning attendance in any of the specifications considered. In addition to the unconditioned mean impact estimates presented in column 1, we present results from a child random effects estimator in column 2. In column 3, the estimates control for the child's age and the effect of several school variables including whether the child's teacher is a female, the log of class size and whether the teacher is present on the day of the attendance visit. The results show that when the teacher is in attendance, the probability that the child attends school that morning increases 12 percentage points. This may not be surprising, but with teacher absenteeism a significant problem in the IDP camps, it highlights the cost of their absence on student performance. Columns 4-8 present estimates separately for female and male children, with and without controls. A surprising finding is that female students who have a female teacher are significantly less likely to attend school, but the same effect is not found for male students. It is not possible to know what mechanism or form of bias lies behind this result. One explanation is that female teachers are harder on female students, making the student less motivated to attend school. An alternative explanation is that female teachers are more likely to forgive an absence by a female student than by a male student.

Table 6 presents FFE impacts on morning attendance by grade level. SFP has a significant effect on male student attendance in grades 1-2, increasing the probability that boys in these grades attend school by 13 percentage points. THR has a significant effect on attendance of for all students in grades 6-7. This effect is driven by a larger impact on girls' attendance. The size of the effect of the THR program at this age range is large, at 18 percentage points on average, and nearly 30 percentage points for girls.¹² Tests show that the impact of THR on

¹² These estimates are based on standard errors clustered at baseline IDP camp. With the relatively small number of clusters in the sample, this leads to fairly conservative hypothesis tests. Another meaningful level of clustering includes clusters formed at the level of the IDP camp interacted with grade in school. The data show that, for morning attendance, the intraclass coefficient on IDP camps is 0.06, while that for IDP camp by grade in school is

morning attendance of children in grades 6-7 is also significantly larger than the estimated impact of SFP.

For afternoon attendance, Table 7 shows that the school meals program has a significant impact (at the 5 percent level with controls; at the 10 percent level without controls). In the specification with controls for school quality, the estimated effect of the SFP program is a 9.3 percentage point increase in the probability that a child attends school. Both the SFP and THR programs have significant impacts on afternoon attendance for boys. Also, we cannot reject that the size of the impacts are identical for the two programs in any specification. However, differentiating by grade level, Table 8 shows that impacts on afternoon attendance are largest for THR given to students in grades 6-7, and these impacts are significantly larger than the impact of the SFP program for all children in grades 6-7 and for girls in those grades.¹³

These results show considerable impacts of the SFP and THR programs on school attendance. An encouraging result is that the SFP has a significant impact on afternoon attendance of 9.3 percentage points in the preferred specification in column (3) of Table 7, and that we cannot reject that THR has an impact of equal size. Other results show that impacts of each program vary by morning and afternoon and by grade and gender of the student. It is difficult to develop a good understanding for why SFP should have larger impacts on boys' attendance in the morning in grades 1-2, but THR has larger impacts in grades 6-7 in the morning and afternoon. Nonetheless, the large effect of the THR program on attendance of children in the late grades of primary school is encouraging and suggests that this could ultimately lead to improved rates of progression to secondary.

These are the first estimates comparing impacts of the two programs on school participation that show a significant difference in impacts between SFP and THR. The results indicate that THR is significantly more effective than SFP at improving morning and afternoon school attendance among children in the upper grades of primary school (grades 6 and 7). Though this is a somewhat narrow result on children in the upper grades, it suggests that, at this age and grade level, providing food at home rather than at school does not weaken incentives for attendance. It also indicates that if more food is redistributed away from the school age child in

0.10; there greater correlation within clusters defined by IDP camp and class. Using this level of clustering, the estimated impact of SFP on morning attendance of females in grades 3-5 is significant at the 5 percent level.

¹³ The impact of SFP on girls in grades 3-5 shown in column 2 of Table 9 is significant at the 5 percent level when clustering at the camp-grade level.

the THR program than in the SFP program, this difference in flypaper effects is not large enough to adversely affect the child's attendance. On the contrary, the results suggest that providing meals at home rather than at school is more effective at improving school attendance for children in the upper grades.

Self-Reported Attendance

The results on self-reported attendance are not as encouraging, though we believe these estimates are affected by sampling error, recall bias, and possibly anticipation effects. Estimates reported in Table 9 indicate that school meals lead to a significant *reduction* in the change from 2005 to 2007 in the number of reported days a child attended school in the last week. This negative effect of the SFP program is found on average for school-age children (age 6-13), for younger children (age 6-9), and weakly for girls. The results show no impact of the THR program on any measures of self-reported attendance.

We believe the large and significant difference in baseline self-reported attendance between the SFP and control groups, a result of sampling error or anticipation effects, is responsible for these negative impact results. Although difference-in-differences estimates account for baseline differences in outcomes, anticipation effects could lead to measurement error in baseline estimated means that would lead to bias even in difference-in-differences estimated impacts. If sampling error alone were responsible for the baseline difference in mean attendance between SFP and control groups, there is a statistical procedure available to attempt to correct the estimates for this source of bias. Wooldridge (2005) presents a method called inverse probability weighting that can be used to correct for such baseline differences in outcomes. The method relies on estimating a model of the selection probability, the probability that a child is more likely to report high attendance. The inverse of the estimated propensity scores from this model are then used as weights in the model that generates the impact estimates. We attempted to apply this approach to the Northern Ugandan data. We found that the difference in baseline mean attendance rates between SFP and control households was reduced, but that this did not entirely remove the significant negative estimated effects of the school meals program. This suggests that anticipation effects may also have played a role in contributing to biased self-reported attendance estimates.

Age at Entry

The FFE programs have the potential to reduce age at entry to primary school. In Uganda, the recommended age for beginning primary school is six years¹⁴ but average age at entry in our sample in the baseline is 8 years old. There are no significant differences in the age at entry across treatment groups in the baseline as reported in Table 2. There are significant baseline differences across gender, with girls starting school at a significantly younger age than boys. Girls' average age at entry at baseline is 7.54 years compared to 8.41 for boys. Therefore, in columns 3 and 4 of Table 10, we report results by gender. Age at entry decreased by one year in the full sample between the baseline and the resurvey, making the average age at entry into primary school 7 years. The average age fell in both treatment groups and in the control group, and for both boys and girls. The average age at entry for boys fell to less than 7 years, below that of girls.

Column 1 of Table 10 reports difference-in-difference impacts of the mean age at entry by treatment group. There is no statistically significant impact of either FFE program on age at entry. Next, we focus solely on those children who began primary school after the baseline survey. In column 2, we present single difference estimates of children who were not yet enrolled in primary school in the baseline. These estimates control for district of residence and the gender of the child. We find no statistically significant impact of either program but these estimates are very close to the treatment group DID estimates and are close to significance. Finally, columns 3 and 4 report results for boys and girls, respectively. Here, we find a weakly significant impact of school meals on age at entry to primary school for girls, reducing the age of entry by more than two standard deviations. Though this effect is only significant for school meals, we do not find a significant difference in impact between school meals and take-home rations for either girls or boys.

Grade Repetition

Grade repetition is quite common in the sample, with 58 percent of children enrolled in primary school at baseline having repeated at least one class. There are no differences in the number of classes repeated in the baseline across treatment groups as reported in Table 2. Results are

¹⁴ The New Vision, Kampala, 10 June 2008.

reported in Table 11. Column 1 reports treatment group difference-in-difference estimates. The results show that both FFE programs lead to a weakly significant decrease in the number of classes repeated. Under this specification, there is no statistically significant difference in the size of the impacts of the two programs. Column 2 presents child fixed effects estimates that control for any omitted child specific variables that could bias the estimates. Once we control for child characteristics using child fixed effects, only school meals significantly reduce grade repetition. Moreover, the estimated impact of SFP is now significantly larger than that of THR.

Progression to Secondary

The FFE programs' impacts on progression to secondary school are *a priori* ambiguous. The programs may have enticed pupils to remain in primary school instead of continuing to secondary school in order to continue to receive the programs. Alternatively, the programs may have improved learning and progression within primary school and, therefore, improved progression to secondary school. 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 may have remained in primary school longer in order to continue to receive the interventions. The sample of interest is those individuals 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 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 completion of primary school.

Many of the students 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, 28 percent remained in grade 6, 48 percent were attending grade 7 and 24 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 12. The impacts of each program are shown separately for children in grade 6 and children in grade 7 in the baseline. Results on progression to secondary 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 from the baseline in grade 6. 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 in-school meals, though the effect is weakly significant. 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. Conclusions

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

The results show no average impact of either FFE program on primary school enrollment, in part because enrollment rates were already high as a result of the elimination of school fees and provision of incentives to primary schools under the Universal Primary Education policy. However, we found that the SFP program had a weakly significant impact on enrollment of 6-9 year old children who were not enrolled in primary school during the 2005 baseline survey. The SFP program also led to a 9.3 percentage point increase in afternoon attendance at primary schools in the sample, based on results from unannounced attendance

visits conducted as part of this study. This effect included an even larger impact of SFP on attendance of children in grades 1 and 2. However, at the upper grades (6 and 7) the THR program led to significant increases in both morning and afternoon attendance and these effects were very large, averaging 17-18 percent in both the morning and afternoon, and including a 30 percent increase in attendance for girls in the morning.

The estimates show no average impact of either program on age at primary school entry, but there is a weakly significant impact of school meals on age at entry for girls, reducing the age of entry by more than two standard deviations. Grade repetition is common in Ugandan primary schools, and is one indicator of poor school performance. We find evidence that both the SFP and THR programs reduce grade repetition. In our preferred estimates controlling for child fixed effects, the SFP leads to a significantly larger reduction in grade repetition than 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 needed to complete primary school.

These results lend considerable support to the potential for FFE programs to achieve their primary goals of increasing school participation and attainment. In general, the in-school feeding program performed somewhat better than the THR program at increasing school attendance, and reducing age at entry for girls and decreasing grade repetition. One exception is that the THR program was more effective at bringing kids to school in the upper grades of 6 and 7. These results suggest that providing meals at school remains an effective strategy for bringing children, especially girls, into school, and that shifting the transfers to dry rations provided at home may have somewhat weaker effects on schooling. It is also encouraging that these programs worked so effectively even in the difficult setting of IDP camps.

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Table 1: Definitions of Outcome Variables

<i>Outcome Variable</i>	<i>Definition</i>	<i>Data Source</i>
Gross enrollment	Proportion of children of any age enrolled in primary school to primary school age children (ages 6-13)	Household survey
Net enrollment	Proportion of children aged 6-13 enrolled in primary school to all children aged 6-13	Household survey
Net daily attendance, morning and afternoon	1 if child was found in school, 0 if child was absent or unenrolled (unidentified children not included), for children age 6-17 in the baseline sample	Unannounced attendance visits
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
Age at entry to primary school	Age (in completed years) when child first enrolled in primary school	Household survey
Grade repetition	The number of grades repeated from 2005-07	Household survey
Progression to secondary	Whether the child in grade 6 in 2005 enrolled in secondary in 2007	Household survey
Remaining in primary	Whether a child in grade 6 or 7 in 2005 remained enrolled in primary in 2007	Household survey

Table 2: Baseline Child and Household Characteristics by Treatment Group, 2005

	<i>SFP</i>	<i>THR</i>	<i>Control</i>	<i>SFP-Control</i>	<i>THR-Control</i>
<i>Outcome Variables</i>					
Gross Enrollment	0.893	0.890	0.848	0.045** (0.020)	0.042* (0.022)
Net Enrollment	0.866	0.861	0.807	0.059** (0.025)	0.054** (0.027)
Net Enrollment, ages 6-9	0.803	0.793	0.741	0.062 (0.039)	0.052 (0.042)
Self-Reported Net Attendance, Past 7 Days	0.949	0.905	0.864	0.085** (0.037)	0.041 (0.049)
Age at Entry to Primary School	8.096	8.060	8.068	0.036 (0.218)	-0.008 (0.244)
Number of Grades Repeated	0.675	0.713	0.648	0.028 (0.060)	0.065 (0.057)
<i>Household Characteristics</i>					
Household Size	7.337	7.194	6.863	0.474* (0.244)	0.331 (0.211)
Father's Schooling	6.604	6.152	6.214	0.390 (0.580)	-0.062 (0.555)
Mother's Schooling	4.547	5.247	4.884	-0.337 (0.629)	0.363 (1.264)
<i>Primary-School-Aged Child Characteristics</i>					
Age	9.386	9.233	9.264	0.122 (0.131)	-0.031 (0.142)
Female	0.476	0.512	0.517	-0.041 (0.025)	-0.005 (0.027)
Grade	2.672	2.563	2.542	0.130 (0.126)	0.021 (0.121)
<i>School Characteristics</i>					
School Size	2108.3	2131.8	1554.0	554.3 (631.2)	577.8 (803.5)
Head Teacher's Experience	10.77	5.02	9.53	1.24 (5.16)	-4.52 (4.88)
Pupils per Classroom	144.83	124.25	88.00	56.84 (40.74)	36.25 (36.25)
Pupils per Teacher	94.19	56.48	67.26	26.93 (29.05)	-10.78 (25.45)

Notes: Standard errors in parentheses are robust to clustering at the baseline IDP camp.

* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 3: FFE Impacts on Enrollment, Treatment Group DID, 2005-2007

	Net Enrollment Age 6-13 (1)	Gross Enrollment Age 6-13 (2)	Net Enrollment Age 6-9 (3)
School meals	-0.0046 (0.042)	-0.035 (0.033)	-0.027 (0.058)
Take-home rations	-0.022 (0.039)	-0.015 (0.031)	0.017 (0.057)
Observations	3134	4020	1609
R-squared	0.011	0.009	0.013
Test equality of impacts (p-value)			
H ₀ : SFP = THR	0.348	0.349	0.331

Notes: Estimates are treatment group DID estimates of impact measured as the difference in the change in mean enrollment between the identified treatment group and the control group. Standard errors in parentheses are robust to clustering at the baseline IDP camp. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

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

	Net Enrollment Age 6-13 (1)	Gross Enrollment Age 6-13 (2)	Net Enrollment Age 6-9 (3)	Not Enrolled at Baseline	
				Net Enrollment Age 6-13 (4)	Net Enrollment Age 6-9 (5)
School meals	-0.032 (0.029)	-0.040* (0.023)	-0.048 (0.056)	0.094 (0.061)	0.124* (0.069)
Take-home rations	-0.014 (0.030)	-0.027 (0.024)	-0.035 (0.058)	0.066 (0.065)	0.061 (0.072)
Observations	3134	4020	1609	967	672
R-squared	0.007	0.006	0.007	0.510	0.460
Test equality of impacts (p-value)					
H ₀ : SFP = THR	0.498	0.557	0.806	0.643	0.374

Notes: Estimates are DID estimates controlling for child fixed effects. Standard errors in parentheses are robust to clustering at the baseline IDP camp. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 5: Impact of FFE on School Attendance in the Morning, Unannounced Attendance Visits

	Average	Child Random Effects	With controls	Females	Males	Females with controls	Males with controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School meals	0.042 (0.048)	0.042* (0.024)	0.043 (0.035)	0.052 (0.059)	0.036 (0.043)	0.055 (0.037)	0.038 (0.034)
Take-home rations	0.026 (0.053)	0.029 (0.025)	0.035 (0.041)	0.022 (0.069)	0.062 (0.055)	0.031 (0.051)	0.069 (0.046)
Grade 2			-0.025 (0.054)			0.029 (0.050)	-0.075 (0.082)
Grade 3			0.033 (0.053)			0.058 (0.063)	0.040 (0.057)
Grade 4			0.073 (0.056)			0.086 (0.068)	0.081 (0.066)
Grade 5			0.073 (0.067)			0.084 (0.082)	0.048 (0.072)
Grade 6			0.046 (0.074)			0.008 (0.090)	0.046 (0.081)
Grade 7			0.149 (0.091)			0.127 (0.124)	0.152 (0.091)
Female teacher			-0.062* (0.034)			-0.105*** (0.038)	0.023 (0.034)
Child age in years			0.000 (0.004)			0.007 (0.008)	-0.005 (0.004)
Log of class size			0.058* (0.030)			0.053 (0.033)	0.050 (0.036)
Whether teacher is present			0.121*** (0.042)			0.162*** (0.053)	0.063 (0.052)
Month: April	0.001 (0.054)	0.037 (0.033)	-0.080* (0.047)	-0.072 (0.054)	0.051 (0.062)	-0.137*** (0.042)	-0.037 (0.060)
Month: June	-0.061* (0.033)	-0.071*** (0.016)	-0.121*** (0.037)	-0.093** (0.036)	-0.048 (0.036)	-0.165*** (0.041)	-0.097** (0.040)
Month: July	-0.031 (0.059)	-0.031 (0.029)	-0.118* (0.062)	-0.140** (0.068)	-0.009 (0.079)	-0.224*** (0.066)	-0.100 (0.071)
Constant	0.750*** (0.038)	0.744*** (0.020)	0.409** (0.155)	0.750*** (0.047)	0.756*** (0.034)	0.341* (0.180)	0.552*** (0.189)
Observations	2833	2791	2523	1295	1436	1162	1273
N individuals		1555					
R-squared	0.006		0.040	0.015	0.008	0.067	0.036

Notes: Omitted grade is 1. Omitted month of attendance visits is November. Standard errors in parentheses robust to clustering at baseline IDP camp level. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Impact of FFE on School Attendance in the Morning by Grade Level, Unannounced Attendance Visits

	By Grade Groupings	Females by Grade Groupings	Males by Grade Groupings
	(1)	(2)	(3)
School meals	0.045	-0.032	0.135**
in grade 1 or 2	(0.063)	(0.077)	(0.062)
Take-home rations	-0.026	-0.100	0.083
in grade 1 or 2	(0.080)	(0.098)	(0.084)
School meals	0.018	0.110	-0.045
in grade 3, 4 or 5	(0.053)	(0.074)	(0.053)
Take-home rations	0.007	0.080	0.024
in grade 3, 4 or 5	(0.068)	(0.087)	(0.065)
School meals	0.025	0.053	0.029
in grade 6 or 7	(0.072)	(0.102)	(0.073)
Take-home rations	0.183**	0.292***	0.129
in grade 6 or 7	(0.074)	(0.089)	(0.087)
Grade 3, 4 or 5	0.054	-0.074	0.172***
	(0.040)	(0.057)	(0.058)
Grade 6 or 7	0.013	-0.110	0.113
	(0.081)	(0.100)	(0.095)
Month: April	-0.014	-0.086	0.031
	(0.056)	(0.056)	(0.065)
Month: June	-0.064*	-0.100**	-0.051
	(0.034)	(0.039)	(0.035)
Month: July	-0.029	-0.132*	-0.017
	(0.063)	(0.067)	(0.079)
Constant	0.741***	0.816***	0.664***
	(0.053)	(0.059)	(0.058)
Observations	2795	1272	1423
R-squared	0.018	0.035	0.022
Test equality of impacts			
H ₀ : SFP=THR, grades 1-2	0.071	0.068	0.052
	(0.078)	(0.097)	(0.074)
H ₀ : SFP=THR, grades 3-5	0.011	0.030	-0.069
	(0.070)	(0.080)	(0.071)
H ₀ : SFP=THR, grades 6-7	-0.158***	-0.239***	-0.101
	(0.041)	(0.055)	(0.065)

Notes: Omitted control group grade indicator is for grades 1-2. Omitted month of attendance visits is November. Standard errors in parentheses robust to clustering at baseline IDP camp level.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Impact of FFE on School Attendance in the Afternoon, Unannounced Attendance Visits

	Average	Child Random Effects	With controls	Females	Males	Females with controls	Males with controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School meals	0.084* (0.047)	0.078** (0.032)	0.093** (0.043)	0.109 (0.072)	0.071* (0.039)	0.096 (0.074)	0.093*** (0.032)
Take-home rations	0.063 (0.046)	0.050 (0.035)	0.088* (0.047)	0.093 (0.074)	0.087** (0.038)	0.112 (0.070)	0.123*** (0.041)
Grade 4			0.021 (0.037)			0.015 (0.057)	0.031 (0.044)
Grade 5			0.018 (0.049)			0.006 (0.061)	-0.014 (0.055)
Grade 6			0.015 (0.059)			-0.054 (0.084)	0.002 (0.062)
Grade 7			0.207** (0.082)			0.184 (0.115)	0.198** (0.087)
Female teacher			-0.008 (0.061)			0.078 (0.060)	0.055 (0.046)
Child age in years			0.004 (0.005)			0.009 (0.008)	0.000 (0.007)
Log of class size			0.046 (0.042)			0.054 (0.044)	0.008 (0.054)
Whether teacher is present			0.103*** (0.037)			0.022 (0.039)	0.116*** (0.040)
Month: April	0.016 (0.071)	0.005 (0.043)	-0.046 (0.067)	-0.032 (0.067)	0.026 (0.107)	-0.142** (0.065)	-0.011 (0.089)
Month: June	-0.105** (0.043)	-0.120*** (0.024)	-0.132** (0.050)	-0.160*** (0.050)	-0.081* (0.045)	-0.214*** (0.060)	-0.098** (0.048)
Month: July	-0.006 (0.054)	-0.016 (0.036)	-0.053 (0.045)	-0.033 (0.053)	-0.029 (0.063)	-0.108* (0.055)	-0.059 (0.057)
Constant	0.671*** (0.036)	0.669*** (0.028)	0.350 (0.232)	0.679*** (0.062)	0.670*** (0.027)	0.392 (0.289)	0.530* (0.261)
Observations	1711	1685	1441	719	927	600	782
N individuals		997					
R-squared	0.016		0.040	0.032	0.013	0.068	0.040
Test equality of impacts							
H ₀ : SFP=THR	0.021 (0.041)	0.028 (0.029)	0.005 (0.036)	0.016 (0.049)	-0.016 (0.040)	-0.017 (0.037)	-0.030 (0.042)

Notes: Omitted grade is 3. Omitted month of attendance visits is November. Standard errors in parentheses robust to clustering at baseline IDP camp level. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Impact of FFE on School Attendance in the Afternoon by Grade Level, Unannounced Attendance Visits

	By Grade Groupings	Female by Grade Groupings	Male by Grade Groupings
	(1)	(2)	(3)
School meals	0.084	0.130	0.049
in grade 3, 4 or 5	(0.053)	(0.089)	(0.039)
Take-home rations	0.030	0.092	0.057
in grade 3, 4 or 5	(0.060)	(0.096)	(0.040)
School meals	0.051	0.009	0.094
in grade 6 or 7	(0.070)	(0.110)	(0.076)
Take-home rations	0.175**	0.178	0.168*
in grade 6 or 7	(0.080)	(0.107)	(0.095)
Grade 6 or 7	0.036	0.073	0.008
	(0.079)	(0.133)	(0.076)
Month: April	-0.009	-0.054	0.001
	(0.066)	(0.068)	(0.102)
Month: June	-0.115**	-0.179***	-0.086*
	(0.042)	(0.051)	(0.043)
Month: July	-0.006	-0.031	-0.035
	(0.056)	(0.060)	(0.062)
Constant	0.680***	0.689***	0.682***
	(0.041)	(0.074)	(0.030)
Observations	1679	699	915
R-squared	0.027	0.049	0.020
Test equality of impacts			
H ₀ : SFP=THR, grades 3-5	0.054	0.038	-0.008
	(0.057)	(0.074)	(0.049)
H ₀ : SFP=THR, grades 6-7	-0.124**	-0.169***	-0.074
	(0.052)	(0.060)	(0.064)

Notes: Omitted control group grade indicator is for grades 3-5. Omitted month of attendance visits is November. Standard errors in parentheses robust to clustering at baseline IDP camp level.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: DID Estimates of Impact of FFE on Self-Reported Net Attendance in Last 7 Days

	Age 6-13	Age 6-9	Age 10-13	Females Age 6-13	Males Age 6-13
	(1)	(2)	(3)	(4)	(5)
School meals	-0.060** (0.028)	-0.079** (0.036)	-0.042 (0.032)	-0.050* (0.026)	-0.070 (0.044)
Take-home rations	-0.036 (0.039)	-0.040 (0.050)	-0.032 (0.043)	-0.048 (0.040)	-0.023 (0.050)
Observations	2625	1252	1373	1291	1333
R-squared	0.027	0.037	0.019	0.032	0.028
Test equality of impacts (p-value)					
H ₀ : SFP = THR	0.434	0.352	0.738	0.958	0.157

Notes: Standard errors in parentheses robust to clustering at baseline IDP camp level.
 * significant at 10%; ** significant at 5%; *** significant at 1%

Table 10: Age at Entry to Primary School

	Average Impact (1)	Single Difference (2)	Single Difference Females (3)	Single Difference Males (4)
School meals	-0.166 (0.231)	-0.163 (0.101)	-0.245* (0.140)	-0.082 (0.153)
Take-home rations	-0.109 (0.240)	-0.155 (0.096)	-0.149 (0.126)	-0.156 (0.165)
Observations	2951	811	397	414
R-squared	0.095	0.023	0.029	0.017
Test equality of impacts (p-value)				
H ₀ : SFP = THR	0.820	0.942	0.420	0.715

Notes: Standard errors in parentheses are robust to clustering at the baseline IDP camp. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 11: Grade Repetition

	Number of Grades Repeated Treatment Group DID (1)	Number of Grades Repeated Child Fixed Effects (2)
School meals	-0.116* (0.068)	-0.120** (0.059)
Take-home rations	-0.148* (0.088)	-0.004 (0.062)
Observations	4125	2530
R-squared	0.002	0.310
Test equality of impacts (p-value)		
H ₀ : SFP = THR	0.709	0.032

Notes: Standard errors in parentheses are robust to clustering at the baseline IDP camp. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 12: FFE Impacts on Progression to Secondary School

	Progression to Secondary (1)	Remaining in Primary (2)
School meals * grade 7	0.055 (0.166)	0.076 (0.132)
Take-home rations * grade 7	0.057 (0.140)	-0.096 (0.144)
School meals * grade 6	-0.013 (0.083)	0.165* (0.086)
Take-home rations * grade 6	0.047 (0.093)	0.027 (0.107)
Grade 6	-0.266** (0.113)	0.210 (0.145)
Constant	0.381*** (0.110)	0.273** (0.110)
Observations	194	214
R-squared	0.12	0.09

Notes: Standard errors in parentheses are robust to clustering at the baseline IDP camp. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.