The Impact of a Food For Education Program on Schooling in Cambodia

Maria Cheung* Maria Perrotta†

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Abstract

This study is an evaluation of the impact of a Food for Education program implemented in primary schools (grade 1 to 6) in six Cambodian provinces between 1999 and 2003. We find that school enrollment increased to varying degree in relation to different designs of the intervention. We also investigate the effect of the program in terms of completed education and probability of having ever been in school, following up the affected cohorts in a 2009 survey. With an estimated cost of 85 USD per additional child in school, the program can be considered very cost-effective within a comparable class of interventions.

Keywords: Food for Education, program evaluation, Cambodia

JEL: I25, I38, F35

*Department of Economics, Stockholm University, SE-106 91 Stockholm, Sweden. Email: maria.cheung@ne.su.se

†SITE, Stockholm School of Economics, P.O. Box 6501, SE-113 83 Stockholm, Sweden. Tel: +46 (0)8 7369690; fax: +46 (0)8 316422. Email: maria.perrotta@hhs.se. We are grateful to the World Food Programme in Cambodia for providing the data. The authors thank Jakob Svensson, David Strömberg, Andreas Madestam, Martin Berlin, Erik Lindqvist, Olof Johansson-Stenman and all participants at IIES and Department of Economics seminars at Stockholm University for valuable comments. Editorial assistance by Christina Lönnblad is gratefully acknowledged.

1 Introduction

There is today a wealth of programs and policies generally designed to achieve the two Millennium Development Goals (MDGs) of universal primary education and reduced gender disparities in education. Food for education (FFE) programs, which consist of meals served in school and in some cases take-home rations and deworming programs conditional on school attendance, are considered a powerful means to this aim, particularly in areas where school participation is initially low. Compared to other programs, such as conditional cash transfers (CCT) and scholarships, school meals may provide a stronger incentive to attend school because children must physically be in school to receive the rations. Moreover, the provision of food contribute to alleviate short-term hunger during the school-day and may so improve learning and cognitive outcomes for undernourished children. The largest international implementer of these programs in the developing world is the World Food Programme (WFP) with 22.4 million beneficiaries in 62 countries in 2010. This study is an evaluation of the impact of WFP's Food for education program that was implemented in primary schools in Cambodia.

The program was phased-in across six provinces (of 24 in total) between 1999 and 2003, and different components were introduced over time, from only on-site meals to a complementary take-home ratio for poor girls in grade 4 to 6, to a "full package" including also a deworming program. This allows us to make within-school comparisons over time and examine three different forms of FFE programs.

We find that the impact of the program on enrollment varied according to the type of FFE program. School enrollment increased between 5 and 14%, but the effect was largest from the "full package" program including on-site feeding, take-home rations and deworming. We perform a series of robustness checks on these results, including placebo tests, regressions on sub-populations, alternative estimation methods and alternative datasets.

The contribution of this paper is threefold. First, we are able to make comparisons between three different types of FFE schemes, which has only been done in a few studies before. Empirical evidence on the central policy question about the cost-effectiveness of such programs is even less frequently provided. We compare not only the different types of FFE program but also alternative demand-side programs previously studied in the literature[1] and find that the full package scheme yielded the highest impact on enrollment per dollar spent.

Second, our study also links to a broader debate about alternative schemes aimed at reducing the cost, including the opportunity cost, of education for poor families. Policy interventions directly targeting the poor have been shown to be the most effective means of increasing participation rates in developing countries.[2] Our findings, coming from a country where the fee for primary school is completely subsidized, show that other cost burdens still dissuade the poorest families from sending their children to school, but are at the same time encouraging in terms of indicating one solution to this problem.

Finally, most studies focused on enrollment as an outcome. Given our rich data, we are also able to investigate different measures of participation and attendance. There are a number of reasons why these outcomes may differ. In some cases enrollment numbers cannot be trusted, because the schools might have incentives to boost them in order to receive more funds. On the other hand, it is also possible for a child to attend school without being enrolled, perhaps due to incomplete school records. Looking at self-reported attendance, we are able to complement the picture painted by enrollment numbers. This is our third contribution.

The remainder of the paper is organized as follows. The next section reviews the FFE programs in general and previous studies. Section 3 presents some general background and the details of the Cambodian FFE. Section 4 describes the data and the methods used, as well as providing descriptive statistics. We present the quantitative results in Section 5, and a cost-benefit analysis in Section 6. Section 8

[1]We do not consider pure deworming programs in the same category, because they do not act on the household budget constraint with respect to the decision on schooling. Within a broader class of programs, deworming is still the most cost-effective way of attracting children to school. FFE and other demand-side interventions, like scholarships and CCT, offer, however, additional benefits for the household.

[2]See Glewwe and Olinto (2004), Schultz et al. (2004), Attanasio et al. (2006), Todd and Wolpin (2006), Barrera-Osorio et al. (2008) on cash incentives; Kremer et al. (2003) on free uniforms and free textbooks and Miguel and Kremer (2004) on deworming programs.

concludes.

2 FFE programs in general and previous studies

The objective of FFE programs is to promote households' investments in the human capital of their children. According to standard economic theory, parents decide how much to invest in the education of each child by comparing potential future benefits of education to current costs. There are two types of education costs. The direct costs are for example school fees, supplies, books, uniforms, and travel to school. The indirect costs relate to the opportunity cost of the child's time: instead of being in school, the child could be caring for other family members, working on a family farm or business, or working outside the household to provide additional income. The idea behind demand-side incentives, including FFE, is to reduce these schooling costs so that greater investment in education may be achieved for every given level of benefit, however distant in the future, uncertain and imprecisely estimated these are.

Standard neoclassical theory also implies that the best way to achieve this outcome would be through (unconditional) cash transfers. However, our societies have chosen not to let this decision to the pure market outcome, which is evident in the fact that most countries today have legislated in favor of compulsory primary education. Notwithstanding the legal prescription and often the financial incentives (in Cambodia, for example, tuition fees are completely subsidized), other frictions beyond budget constraints still keep many children out of school. Policy instruments such as FFE, or CCT for that matter, are built to overcome these frictions.

FFE programs generally take two forms: in-school meals and take-home rations. Compared to other demand-side incentive programs (conditional cash transfers and scholarship programs), school meals provide a stronger incentive to attend school because the child has to be in school in order to receive the meal[3].Take-home rations, food rations given to the household conditional on a child's enrollment in school

[3]In principle, the school records needed to collect the subsidy in a CCT scheme may be falsified.

and a minimum level of attendance, work instead as a complementary cash transfer, compensating the household for the foregone income that would be generated by the child if not in school. Take-home rations focus hence relatively more on improving food security at the household level (Pollitt, 1995). Sen (2002) argues that in-school meals are superior to take-home rations since the former contribute to the nutrition of children and thus complement teaching as well as enhance school attendance: because the meals are served before the school-day, the child learns more effectively, undistracted by short-term hunger and more able to focus. School meals might also reduce abuse and corruption that arise in a dry ration system due to the fungibility of the distributed rations. On the other hand, school meals may also disrupt teaching and learning if class time is substituted for meal time (see Vermeersch et al. (2005)). Breakfast programs designed to cause as little disruption as possible (served outside the normal teaching time) may therefore be the best policy choice. Besides the details, the major objectives of both in-school meals and take-home rations are however the same: to increase food consumption and improve education outcomes and nutritional status of the children. Many of the FFE interventions also offer other components, related to education, nutrition, or health including deworming programs.

The broad range of contexts in which FFE interventions have been implemented has led to increasing awareness of the potential benefits of FFE for different outcomes including education, nutritional status, social equity and agricultural development. Given the growing popularity of such interventions across the developing world, and the resources targeted towards them, it is important that they are rigorously evaluated.

The literature on the impacts of FFE programs is very big, and almost unanimous in suggesting that these programs have considerable impacts on primary school participation (Jacoby et al., 1996; Ahmed, 2004; Ahmed and Del Ninno, 2002), in particular for girls (Kazianga et al., 2009; Afridi, 2009). School feeding coupled with take-home rations seems to have a greater impact on girls' enrollment compared with that of boys (Gelli et al., 2007; Kazianga et al., 2009). The empirical investigations based on experimental or quasi-experimental designs providing causal evidence is relatively scant. Vermeersch et al. (2005) conducted a randomized evaluation of the impact of school meals on participation in Kenyan pre-schools, and found that school participation was 30 percent greater in the 25 Kenyan pre-schools where a free breakfast was introduced than in the 25 comparison schools. In schools where the teacher was relatively well trained prior to the program, the meals program led to higher test scores (0.4 of a standard deviation) on academic tests. Although more studies with a solid identification strategy are needed, it is fair to say that the impact of FFEs seems to be by-and-large positive.

Despite these potential benefits, there is an ongoing debate among donors and policy-makers on the point that these programs are an expensive method for achieving the stated education and nutrition objectives and that other cost-effective mechanisms exist. Only few studies investigate the cost-effectiveness of FFE programs and the types of school feeding program that are most effective. And not many either compare the impact on both school and individual records. Taking both these measures into account would give policy-makers a more complete picture of the program's impact.

3 Background

After decades of political unrest, during the last decade Cambodia experienced political stability and high rates of sustained economic growth, at nearly 9 percent on average. Despite the progress, Cambodia remains one of the least developed countries in East Asia. Its GNI per capita was estimated at approximately 650 USD in 2009 and about 33 percent of the total population lives below the 1-dollar-a-day poverty line, 77% below 2 dollars a day (Cambodia Socio Economic Survey, 2007). In primary education, enrollment is still far from being universal although the government showed strong committment towards this goal. Most children enroll in primary school but a large share complete only two or three grades. Based on figures from the national school census,[4] the net enrollment rate for primary education was 89

[4] Education Management Information System (EMIS) maintained by the Ministry of Education, Youth and Sports (MoEYS).

percent in 2007, while the primary dropout rate was 46 percent.

The Cambodian FFE program started in 1999-2000 as a pilot project in Takeo province[5] with only in-school feeding and was phased in during the following three years. It was first undertaken by the WFP and the World Bank jointly with the Ministry of Education, Youth and Sports (MoEYS) as a part of a larger WFP Relief and Recovery Operation.[6] The year after, the school feeding program was running in Takeo, Kampot and Kampong Cham provinces. Children were provided with one meal per day (breakfast) before school which contained the standard WFP ration of rice, canned fish, vitamin A fortified vegetable oil, and iodized salt in order to meet the minimal daily nutritional needs of students. The participating schools were required to provide fresh vegetables, water and fuel for the preparation of the WFP-supplied commodities. Parents and community members who volunteered to prepare the hot meal received a dry ration of rice for their help. The costs of providing the meals, apart from WFP's food provision, were born by the community.

In 2001-2002 the program was expanded in cooperation with a local NGO, Kampuchean Action for Primary education (KAPE), and UNICEF to include 407 schools and about 291,593 students in five provinces, Kampot, Kampong Cham, Kampong Speu and Prey Veng. In addition, take-home rations for families of 16,000 girls in grades 4 to 6 was piloted this year as an incentive to keep these girls in school. Girls in that age are in fact more vulnerable to dropout since their productivity in the home gains value.[7] The program experienced a further expansion in 2002-2003 to include an additional province (Kampong Thom) and to introduce a deworming program to all participating schools. The previously treated schools also received the additional treatment.

In addition to providing school meals during the day, WFP operations also helped establish complementary health and sanitation activities to improve the overall edu-

[5]See a map of Cambodian provinces in the appendix.

[6]The broad goal of this operation was to sustain food security among chronically hungry poor, along with the promotion of re-emerging social cohesion and support system. Some of these activities include food for work which is a food-based safety net program to the chronically and transient poor, school feeding to primary schools, and rice-banks to counter the chronic cycle of debt in rural areas.

[7]Source: UNICEF.

cational environment. These activities include the identification of safe drinking water and improvements in basic health, hygiene and sanitation practices for students at school and at home. HIV/AIDS prevention education was also a fundamental part of the educational package.

The phase-in structure of the program is summarized in Table 1.

Table 1: WFP School Feeding Coverage 1999-2003

School year	1999-2000	2000-2001	2001-2002	2002-2003
Type of program	On site	On site	On site + take-home	On site + take-home + deworming
Province	Takeo	Takeo Kampot Kg Cham	Takeo Kampot Kg Cham Kg Speu Prey Veng	Takeo Kampot Kg Cham Kg Speu Prey Veng Kg Thom
Schools	64/323	201/593	403/1,078	565/1,122
Pupils	37,500	125,000	291,593	317,053

We want to stress at this point that the Cambodia FFE was not a randomized intervention. The schools were selected based on administrative criteria, which might correlate with other characteristics potentially influencing enrollment. Moreover the fact that local communities were asked to help preparing the food andprovide the fresh vegetables to complement the meals might also induce self-selection. We discuss the potential biases connected with the non-random selection and our approach to deal with them later in the paper.

4 Data and methods

4.1 Data

The data used in this paper come from multiple sources. School level data are drawn from the Education Management Information System (EMIS) maintained by the MoEYS. The EMIS includes information on enrollment and repetition rates broken down by grade and gender; teaching staff age, education, experience and gender; and various school characteristics such as number of classrooms and other facilities, as well as school location, income, and more, for the universe of Cambodian schools. The panel we base our analysis on spans the whole length of the program, from 1998 to 2003, covering 8,443 schools from all the 24 provinces, of which 3,089 in our six provinces of interest. The data can be perfectly merged with the information on treatment status provided by the WFP, which has the same school identification number.

Individual level data are taken from two waves of the Cambodia Socio-Economic Survey (CSES 1999 and CSES 2009), large-scale nationally-representative household surveys collected by the National Institute of Statistics. CSES 1999 covers 6,000 household and was carried out from January to August 1999. Besides socio-economic background variables (consumption, age, sex, income, etc.), this dataset contains more detailed information about schooling at the individual level: literacy, attendance and completed years of schooling, but also reasons for not attending, as well as total costs (including school fees, text books, other school supplies, allowances for children studying away from home, transport costs, even gifts to teachers). Using this dataset, we can analyze two more outcomes: the highest educational achievement and the probability of having attended school for 15- to 21-year old respondents in 2009, i.e. the cohorts that were in primary school when the program started.

Unfortunately, there is no information about which school the individuals attended. Based on the school data we are able to see that there is only slightly more than one school on average in each commune, and hence the commune level is the closest we can get to the treatment assignment level. We merge the information on treatment status at the commune level and therefore adopt an intention-to-treat approach: we assume that all the children living in a commune with a treated school actually received the treatment.

4.2 Descriptive statistics

Table 2 reports the pre-treatment summary statistics, showing differences in enrollment, repetition rates and other school characteristics between treated and non-treated schools in 1999. Selection bias might be a concern, due to the non-random selection for treatment, as detailed in the previous section. However, the treated schools do not seem to be generally worse-off before the treatment. As revealed in Table 2, they have both more rooms and higher enrollment, resulting in not significantly different class size. Howerver, they also have slightly higher repetition rates and over-age enrollment, and less personnel per student, both teaching and non-teaching. Total income and land area of the school are not significantly different. By and large, hence, the data reveal that schools selected into the program are different under some potentially important respects.

We use school fixed effects in order to control for potential unobservable confounding factors that are time-invariant. However, our identifcation strategy is based on the assumption of parallel *trends*, i.e. that schools in the treated and control group would display the same *growth* of enrollment over time, if not for the treatment. The school fixed effects cannot account for potential confounding factors that change over time and hence affect the trend in enrollment. For example, it could be the case that the pre-treatment higher repetition rates and smaller staff are an indication that these schools have worse prospects in terms of future performance. Table 2 reveals, however, that these differences, though statistically significant, are small. Moreover, the relatively bad performance in terms of enrollment growth would go against us finding positive effects of the treatment.

To give a visual idea about the parallel trend assumption, in Figure 1 we plot enrollment trends for the treatment and control schools in the three groups. In the first and third panels, the 2000 and 2002 treatment groups, the series are clearly parallel, and diverge only in the year of treatment. Things are less clear for the 2001 group, were the trends seem to diverge already before treatment. The regression analysis can clarify the patterns by including controls for the baseline characteristics interacted with time dummies.

Table 2: Pre-treatment characteristics, simple means

	Treat=0	Treat=1	Difference
Rooms	7.344	9.469	2.126
p-value			0.000
Enrollment	369.615	409.709	40.094
p-value			0.007
Enrollment girls	162.518	187.318	24.800
p-value			0.000
Class size	57.401	59.411	2.010
p-value			0.102
Repeaters	19.081	22.545	3.464
p-value			0.000
Over-aged	8.282	9.696	1.414
p-value			0.027
Teaching staff	0.027	0.019	-0.008
p-value			0.000
Non-teaching staff	0.003	0.002	-0.002
p-value			0.000
Income	0.820	1.878	1.058
p-value			0.294
Area	43.017	41.776	-1.241
p-value			0.834

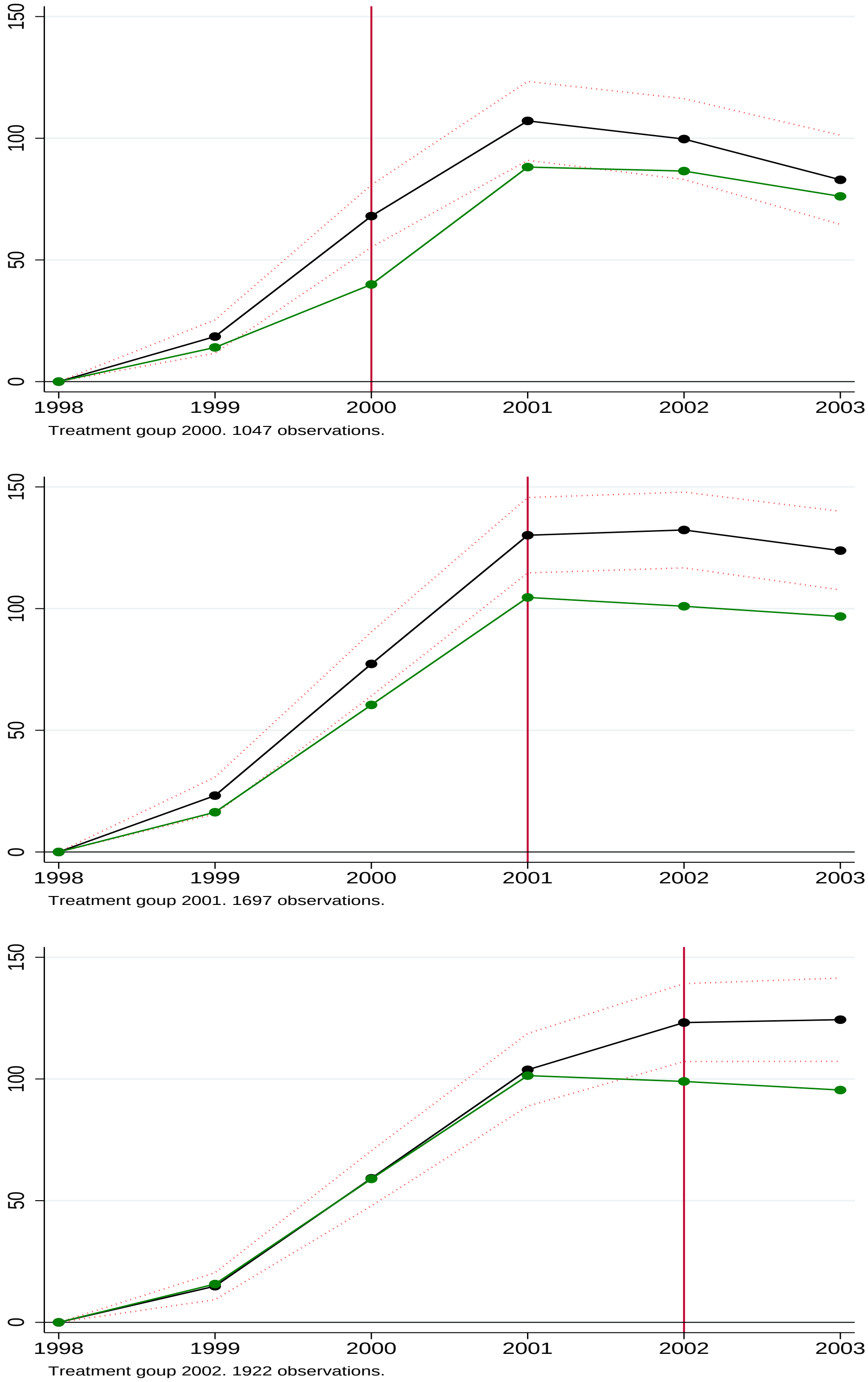

Figure 1: Average enrollment in the treated schools (1998=0) in black, control schools within the same provinces in green. The red dotted lines are 90% confidence intervals.

4.3 Specification

The identification is based on a difference-in-difference strategy which allows us to control for time invariant unobservables that are correlated with program placement and participation. For the school data, we use a fixed effect specification at the school level, looking separately at each treatment group. Given the panel structure, we can analyze the effect on enrollment for each year of treatment and, as a placebo, in the years before the treatment, using the following specification:

$$Enr_{igt} = \alpha + \beta * TG_g * Yr_g + \sum_{k<g} \beta_k TG_g * Yr_k + \sum_{h>g} \beta_h TG_g * Yr_h + \nu_t + \mu_i + \sigma_{it} \quad (1)$$

where the subscript i denotes the school, $g = (2000, 2001, 2002)$[8] the treatment group and t the year in which enrollment is observed. The dependent variable is the natural logarithm of total enrollment. The coefficient β is the average effect of treatment. Besides total enrollment, we also look at enrollment by gender.

5 Results

Table 3 presents the results of the fixed-effect regression for each treatment year, including controls for all the baseline school characteristics that proved to be significantly different between treatment and control schools, interacted with year dummies: enrollment, girls enrollment, number of classrooms, teaching and non-teaching staff, repetition rate and over-age enrollment. The table reveals that the effect on enrollment is positive and significant in the first year of treatment for the 2000 and 2002 groups, while it takes one year lag for the 2001 group. The increase in enrollment due to the FFE program is about 5% with on-site feeding and on-site plus take-home rations (treatment groups 2000 and 2001), and almost 14% with the full-package including deworming (treatment group 2002).

Given the panel structure of the data, the impact on enrollment for each single

[8] We exclude the schools from the 1999 pilot.

treatment group can be followed over time. For schools in the 2000 treatment group, enrollment does not increases further after the first year of treatment. In 2003, it increases *less* compared to control schools (the coefficient is negative). The optimistic interpretation for the fading out of the effect is that all the eligible children that were still out of school, and belonging to households sensitive to the program (i.e., households for which the program is enough to shift the balance of costs and benefits of schooling towards the benefit side), are already attracted to school during the first year of treatment. An alternative possibility is that the schools reach full capacity after the first year's increase, and cannot enroll more children during the following years. In fact, the average class size in the treated schools in our data is 58 in 1999 and 69 in 2003. Similarly, there is one teacher for 52 pupils on average in 1999 and one for 58 in 2002. In the other two groups, the effect continues, with about the same size, even in 2003.

Table 3: Baseline results: enrollment trends over time

	(1) 2000 group	(2) 2001 group	(3) 2002 group
TG*(year==1999)	-0.00653	0.00648	0.00988
	(0.0154)	(0.0128)	(0.0169)
TG*(year==2000)	0.0490*	0.0232	0.0327
	(0.0229)	(0.0215)	(0.0264)
TG*(year==2001)	-0.0177	0.0337	0.0572
	(0.0245)	(0.0223)	(0.0307)
TG*(year==2002)	-0.0376	0.0486*	0.132***
	(0.0249)	(0.0241)	(0.0318)
TG*(year==2003)	-0.0604*	0.0442	0.139***
	(0.0258)	(0.0256)	(0.0350)
R^2	0.363	0.406	0.410
Schools	1053	1706	1934
Observations	6308	10223	11588

Note: The dependent variable is the log-enrollment. All regressions include school and year fixed effects. Baseline school characteristics interacted with year effects: class size, number of classrooms, total enrollment, girl enrollment, teaching and non-teaching staff, repetition rate, over-age enrollment. Robust standard errors clustered at the school level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 4: Girls' enrollment over time

	(1) 2000 group	(2) 2001 group	(3) 2001 group	(4) 2002 group	(5) 2002 group
TG*(year==1999)	-0.00561	0.00707	0.0412	-0.00738	-0.0361
	(0.0165)	(0.0142)	(0.0318)	(0.0202)	(0.0314)
TG*(year==2000)	0.0590*	0.0223	0.0272	0.0222	-0.0914
	(0.0242)	(0.0248)	(0.0338)	(0.0292)	(0.0480)
TG*(year==2001)	-0.0195	0.0231	0.00868	0.0405	-0.0787
	(0.0251)	(0.0253)	(0.0402)	(0.0335)	(0.0507)
TG*(year==2002)	-0.0453	0.0418	0.0667	0.117***	-0.0616
	(0.0251)	(0.0272)	(0.0418)	(0.0346)	(0.0485)
TG*(year==2003)	-0.0755**	0.0326	0.0886*	0.122***	0.0222
	(0.0281)	(0.0285)	(0.0407)	(0.0369)	(0.0533)
R^2	0.361	0.401	0.486	0.398	0.487
Schools	1053	1706	1535	1934	1663
Observations	6308	10223	8567	11588	8991

Note: The dependent variable is the log-enrollment of girls. All regressions include school and year fixed effects. Baseline school characteristics interacted with year effects: class size, number of classrooms, total enrollment, girl enrollment, teaching and non-teaching staff, repetition rate, over-age enrollment. Robust standard errors clustered at the school level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The placebo coefficients are all zero as expected, i.e. there is no effect of treatment *before* treatment. We could not rule out that schools receiving treatment were ex-ante different in the *levels* of enrollment, however this would not be a problem for the fixed-effect specification. More problematic would be the case if they had been ex-ante different in terms of the *rate of increase* in enrollment, because this would invalidate the parallel trends assumption. If this was the case, though, we would expect some positive coefficients in these placebo regressions. But we see that the placebo treatment has no effect on any of the treatment groups, indicating that the parallel trend assumption holds for our identification.

In Table 4, we look at girls enrollment. In columns (3) and (5) we report a third difference for the enrollment of girls in 4th to 6th grade relative to lower grades. Starting as a pilot in 2001 and expanded in 2002, families of girls in the grades 4 to 6 were provided take-home rations, as girls in these grades are most vulnerable to dropout. The differential effect on older girls is only visible in the 2001 group, and even in this case with one year lag. For the other groups, the effects are similar to Table 3.

6 Robustness

6.1 Non-treated schools in treated districts

In order to better understand the effect of our three treatments, we start by looking at what happens to non-treated schools located in the same districts as treated ones. Table 5 shows that schools located within districts treated in 2000 see losses in enrollment as big as the treatment effect observed in treated schools (Table 3). This might indicate that students move from non-treated to treated schools within the same district. If this is the case, then we might conclude that the treatment with only in-school meals had no net effect on enrollment.

No similar effect is observed instead for the other two groups, indicating that the addition of take-home rations and especially deworming make the treatment more effective.

6.2 Coarsened exact matching

Given that, as observed earlier, the treated schools are significantly different under some important respect compared to the average school within the province, the choice of control group might affect the size of the effect we observe. In this section, we adopt a matching strategy, to better balance treatment and control schools on observables. Table 6 shows the balancing of the covariates used above (number of classrooms, baseline enrollment, repetition rates and over-age enrollment, teaching and non-teaching staff, location, type and income of the school) to serve as base for a coarsened exact matching (CEM). The first column, reports the difference in means between treated and non-treated schools within the matched sample. The remaining columns in the table report the difference in the empirical quantiles of the distributions of the two groups for the 0th (min), 25th, 50th, 75th, and 100th (max)

Table 5: Enrollment in non-treated schools within treated districts

	(1) 2000 group	(2) 2001 group	(3) 2002 group
TG*(year==1999)	-0.0251	0.00706	0.00441
	(0.0133)	(0.00955)	(0.0213)
TG*(year==2000)	-0.0591**	-0.0183	0.0484
	(0.0199)	(0.0158)	(0.0325)
TG*(year==2001)	-0.0662**	-0.0135	0.0360
	(0.0231)	(0.0174)	(0.0374)
TG*(year==2002)	-0.0784**	-0.0164	0.0705
	(0.0269)	(0.0199)	(0.0385)
TG*(year==2003)	-0.0887**	-0.0270	0.0525
	(0.0283)	(0.0211)	(0.0440)
R^2	0.366	0.394	0.392
Schools	922	1517	1779
Observations	5522	9089	10659

Note: The dependent variable is the log-enrollment. All regressions include school and year fixed effects. Baseline school characteristics interacted with year effects: class size, number of classrooms, total enrollment, girl enrollment, teaching and non-teaching staff, repetition rate, over-age enrollment. Robust standard errors clustered at the school level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

percentiles for each variable. In each group defined by this procedure the covariates are fairly balanced and the assignment to treatment can be considered random.

Table 6: Balancing of covariates

	mean	min	25%	50%	75%	max
Classrooms	-0.03687	0	0	0	0	0
Bas. Enrollment	-2.6545	16	1	-3	4	0
Bas. Enrollment (girls)	-1.695	7	7	-1	1	-53
Teaching staff p/stud	-0.00066	0	-0.00087	-0.00114	0.00045	-0.0024
Non-teaching staff p/stud	-8.20E-05	0	0	0	-0.00021	-0.00139
Repetition rate	-0.24002	0	-0.46695	-0.24509	-0.50125	-5
Overage enrollment	-0.38264	0	0	-1.2539	-1.2369	0
School income	-2.40E-15	0	0	0	0	0
Location	0	1	0	0	0	0
School type	0	1	0	0	0	0

Tables 7 and 8 report the estimates using CEM. The effects are very similar to the baseline results.

6.3 Individual level data

In Table 9 we analyze individual level data. If the increases in enrollment are significant in economic and not only in statistical terms, this should be reflected in individual outcomes as well. Using data from the Cambodia socio-economic surveys, we look at the average of the highest grade completed and the probability of having ever attended school for all the children that, based on their birth year, were of primary-school age during the treatment years. Respondents aged 15 to 21 in 2009 are compared across treated and non-treated communes, and then with the corresponding cohorts interviewed in 1999, before the treatment started. These effects are averages of all the treated communes in a given year and do not take into account the length of treatment, including children that received as much as three and as

Table 7: Coarsened exact matching

	(1) 2000 group	(2) 2001 group	(3) 2002 group
TG*(year==1999)	0.000453	0.000101	-0.00566
	(0.0155)	(0.0138)	(0.0183)
TG*(year==2000)	0.0608*	0.0216	0.0312
	(0.0236)	(0.0240)	(0.0295)
TG*(year==2001)	0.00696	0.0586*	0.0630
	(0.0262)	(0.0249)	(0.0335)
TG*(year==2002)	-0.00117	0.0807**	0.136***
	(0.0278)	(0.0271)	(0.0359)
TG*(year==2003)	-0.0224	0.0800**	0.136***
	(0.0290)	(0.0286)	(0.0390)
R^2	0.378	0.446	0.449
Schools	604	1010	1124
Observations	3620	6055	6737

Note: The dependent variable is the enrollment level. All regressions include school and year fixed effects. Robust standard errors clustered at the school level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8: Coarsened exact matching, girls

	(1) 2000 group	(2) 2001 group	(3) 2002 group
TG*(year==1999)	0.00307	0.0101	-0.0111
	(0.0174)	(0.0161)	(0.0221)
TG*(year==2000)	0.0666**	0.0220	0.0223
	(0.0250)	(0.0279)	(0.0331)
TG*(year==2001)	0.00523	0.0558*	0.0569
	(0.0268)	(0.0284)	(0.0368)
TG*(year==2002)	-0.00511	0.0782*	0.134***
	(0.0280)	(0.0312)	(0.0388)
TG*(year==2003)	-0.0295	0.0831*	0.135**
	(0.0316)	(0.0330)	(0.0415)
R^2	0.390	0.439	0.435
Schools	604	1010	1124
Observations	3620	6055	6737

Note: The dependent variable is the enrollment level for girls. All regressions include school and year fixed effects. Robust standard errors clustered at the school level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

little as one year of treatment.[9]

The commune fixed effect estimates imply almost one year (0.7) longer education, 11% of the before-treatment mean of 6.14. The same specification is used for the probability of having ever been in school. The fixed effect estimates show that this probability increased by about 10 percentage points more for the children in treated communes compared to children in non-treated communes, which, relative to the mean in 1999 (85%), implies a 12% increase due to the program. Only the latter effect is robust to collapsing the data at the commune level (columns (2) and (4)).

Table 9: Effect on highest grade and probability of being in school

	(1)	(2)	(3)	(4)
	Grade completed		Probability of attending	
year==2009	2.019***	1.972***	0.0757***	0.0824***
	(0.130)	(0.117)	(0.00996)	(0.0115)
(year==2009)*treat	0.721*	0.538	0.102**	0.0875*
	(0.349)	(0.333)	(0.0355)	(0.0353)
R^2	0.356	0.579	0.228	0.223
Observations	13139	1064	14667	1073

Note: The dependent variable is the highest completed grade in columns (1) and (2) and a dummy for having ever been in school in columns (3) and (4). The data are collapsed at the commune level in columns (2) and (4). All regressions include commune and year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

7 Cost-benefit analysis

A central policy question is whether FFE programs yield higher impact per dollar spent than alternative programs. There are, however, very few studies on the cost per outcome for school feeding programs. We elaborate on cost information from different sources to try and answer this question.

[9]The effects were not significantly different across the three groups, and pooling all the observations together increases efficiency.

Using the food allocation from the 2001/2002 school year multiplied with the 2004 average cost per metric tons for each food item, we are able to calculate a rough measure of the total value of the Cambodia FFE program for that year.[10] Table 10 shows that the average cost for on-site breakfast is around 8 USD per child per year; take-home rations cost 37 USD per girl, so the average cost for breakfast in school plus take-home to poor girls is 10 USD per child.[11] The per child cost of deworming in 2005 was around 22 cents per treatment against both Soil-Transmitted Helminth (STH, one tablet of Mebendazole 500 mg costs approximately 2 cents) and Schistosomiasis (one tablet of Praziquatel 600 mg costs 20 cents, see WFP (2006)). Since the normal dosage is one tablet of Praziquatel and one tablet of Mebendazole, assuming that each child gets the infection at most once a year, the total cost for a full package intervention that includes on-site meals, take-home rations for poor girls and deworming is around 10.36 USD per child per year.

To assess the cost-effectiveness, in Table 11 we use the program cost with the 2004 food values divided by the number of additional children enrolled due to treatment. The latter is computed using our fixed effect estimates from Table 3. We find that the on-site feeding is quite cost-effective, while distributing take-home rations is relatively expensive, given the limited impact. However, adding the deworming intervention is a way to make the full package much more cost-effective, due to the fact that this complete package attracts many more pupils, while the deworming medications are extremely cheap.

For comparison, the cost of a conditional cash transfer program, the Japan Fund for Poverty Reduction (JFPR) scholarship program in Cambodia (Filmer and Schady, 2008), is reported in the table. This scholarship program, started in 2004, awarded poor girls who were completing sixth grade a scholarship of 45 USD. The program increased the enrollment and attendance of recipients at program schools by about 30%, hence the cost per additional child in school was 150 USD. With the exception of year 2001/2002, the FFE intervention was hence more cost-effective.

[10] See WFP (2006) for food costs and FASONLINE WFP for detailed resource allocation in 2001/2002.

[11] Bear in mind that these costs are only food costs and do not include indirect costs such as transport costs, staff costs, etc.

Table 10: Program costs, 2001-2002

Resource allocation 2001/2002		Quantity (mt)	2004 value (USD)	No of Pupils	Cost per pupil (USD)
Rice	on site	3,470	697,478		
	take home	2,038	409,630		
	both	5,508	1,107,108		
Vegetable oil	on site	255	201,958		
	take home	236	186,423		
	both	491	388,381		
Canned fish	on site	663	1,453,296		
	take home	-	-		
	both	663	1,453,296		
Salt	on site	99	7,920		
	take home	-	-		
	both	99	7,920		
Total	on site	4,487	2,360,652	291,593	8.10
	take home	2,274	596,053	16,000	37.25
	both	6,762	2,956,705	291,593	10.14
Deworming (2002)	Mebendazole (against STH)		5,831	291,593	
	Praziquatel (against Schistosomiasis)		58,318	291,593	
	both		64,150		0.22
Full package incl. deworming					10.36

Note: The average cost per metric tons of rice is 201 USD, vegetable oil is 791 USD, canned fish is 2192 USD, iodized salt is 80 USD, source: "Protracted Relief and recovery operation - Cambodia 10305.0", January 2004. The quantity of resources is based on the allocation plan for the school year 2001/2002, source: FASONLINE WFP.

Table 11: Cost effectiveness of the different interventions

	(1)	(2)	(3)	(4)	(5)
Treatment	On site	On site + take-home	On site + take-home + deworming	Scholarships to poor girls in 6th grade	Meals to pre-school in Kenya
Year	2000-01	2001-02	2002-03	2004-06	2000-01
Budgeted n. of pupils	125,000	291,593	317,053	2,765	2,750
Total cost (USDth)	1,012	2,956	3,284	124	127
Actual n. of pupils in the treatment group before treatment	167,230	227,550	291,930	2,765	2,750
Program impact	4.9%	4.8%	13.2%	30%	8.5%
Additional pupils in school due to the treatment	8,194	11,059	38,535	830	234
Cost per additional pupil in school (USD)	123	267	85	150	546

Note: The total cost is computed multiplying the average cost per pupil from Table 10 by the number of children in the remaining years. The budgeted number is from the program documentation, and is WFP's own estimate. The actual number of enrolled children is what we observe in the data, and it can differ from the ex-ante estimated number. The program impact is our own estimate, from Table 3). The figures in columns (4) and (5) are our elaborations from Filmer and Schady (2008) and Vermeersch et al. (2005) respectively.

The Cambodia FFE was also more cost effective than the Kenya pre-school FFE program studied in Vermeersch et al. (2005), discussed in section 2. The authors do not report explicitly the costs of the program, so the figures in the last column of Table 11 are our own elaborations based on data reported in their paper.

At 85 USD per child in school, the FFE still looks quite expensive compared to the programs overviewed in Miguel and Kremer (2004), for example. Their estimate of the cost of a deworming intervention, at 3.5 USD per child in school, is hard to beat, if the objective is purely that of attracting more children in school. But since FFEs also contribute to the nutrition and general health status of children, especially the poor and malnutrient ones, then this comparison is not really fair. We did not look at these outcomes in the present paper, but this is certainly a very important area of inquiry for future studies.

8 Conclusions

This study provides an insight on the impact of three types of school feeding programs on enrollment, education achievements and the probability to be in school. We show that the FFE program boosted the school enrollment in the short run for all the three types of treatment: on-site feeding, take-home rations and full package including deworming. Beyond enrollment, the intervention increased also the probability of having ever attended school for youths in 2009, who were of primary-school age during the treatment years, compared to previous cohorts. But it did not lead to higher education achievements for the same group of youths. This might be interpreted as the result of a countervailing class size effect. If the program attracts more children, but the school resources remain fix, this might lead to deterioration in the student-teacher ratios and size of classes, which in turn might fail to lead to a longer stay in school. Also, children who were already attending school may suffer negative peer effects from lower ability children joining school because of the program.

This calls to mind the critique frequently raised against treatment evaluations, namely that partial equilibrium estimates that ignore responses from general equilibrium and political economy sources are to be taken with caution. The argument is clearly spelled out in a recent contribution, Acemoglu (2010), for a model case very similar to the program that we are studying here. The authors show that a simple model of the relation between cost of schooling and investments in education, and the relative reduced-form estimates, will not be informative about counterfactuals involving large-scale interventions in the presence of constraints on individual choice. One such constraint, which we suspect to be present in this case, might be fixed school resources. This consideration, together with the fact that the intervention is not randomized and the possible issues with selection, should lead to interpret our estimates with caution.

Stressing once more that the impact estimates have to be taken with caution, and also that we used approximative figures on costs, we tried anyway to say something about the cost-effectiveness of this program, and make it comparable to other types of interventions. A rough measure of the cost-effectiveness reveals that school feeding alone is a very cost-effective intervention, in a set of comparable programs, but adding to it deworming medicines, very cheap and extremely effective, makes the full-package scheme even better. Take-home rations proved instead to be a very expensive intervention when put in relation to the average benefit.

The impact on nutrition and general health of this program remains to be investigated. Moreover, given the (weak) impact observed on educational outcomes, it is possible that the program also had long-term effects on wages and employment for the individuals involved. These are open questions for future research.

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Appendix

Samrong
ODDAR MEANCHEY
BANTEAY MEANCHEY
Sisophon
SIEM REAP
Siem Reap
Tbeng Meanchey
PREAH VIHEAR
STUNG TRENG
Stung Treng
Banlung
RATANAKIRI
Battambang
PAILIN
Pailin
BATTAMBANG
Kampong Thom
KAMPONG THOM
MONDOLKIRI
Sen Monorom
Pursat
KRATIE
Kratie
Kampong Chhnang
PURSAT
KAMPONG CHHNANG
KAMPONG CHAM
Kampong Cham
Koh Kong
KOH KONG
KAMPONG SPEU
Kampong Speu
Phnom Penh
Prey Veng
Takhmau
KANDAL
PREY VENG
Svay Rieng
SVAY RIENG
Takeo
TAKEO
KAMPOT
SIHANOUKVILLE
Sihanoukville
Kampot
KEP
Kep