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

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Abstract

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 tool to improve educational outcomes, particularly in areas where school participation is initially low. Compared to other programs, such as conditional cash transfers and scholarships, school meals may provide a stronger incentive to attend school because children must be in school in order to receive the rations, and have the potential to improve nutritional and general health status as well. In this paper, we find that the Cambodia FFE, that was implemented in six Cambodian regions between 1999 and 2003, increased enrollment, school attendance and completed education. We also ask who benefited the most, and how cost-effective such a program is compared to other types of interventions.

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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 go to school to receive the rations. Moreover, the provision of food can contribute to alleviate short-term hunger during the school-day and 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 102 million beneficiaries in 78 countries in 2008. This study is an evaluation of the impact of WFP's Food for education program in Cambodia, that was implemented in primary schools (grade 1 to 6) in six Cambodian regions between 1999 and 2003. Beyond the average impact of the program, we investigate also who benefits most. Finally we tentatively assess how cost-effective such a program is compared to other types of interventions.

The program was phased-in across six provinces (of 24 in total) between 1999 and 2003, allowing us to examine three different forms of FFE programs: i) in-school breakfast, ii) in-school breakfast together with take-home ration provided to families of poor girls in grade 4 to 6, and iii) the "full package" consisting of in-school breakfast, poor girls' take-home rations and deworming medicine to all participating schools. The identification of the effect is based on a difference-in-difference strategy exploiting the variation in the exposure to the program both across time (before and after) and across geographical location (treated and non-treated schools or communes).

We find that the impact of the program on enrollment varied according to the type of FFE program. School enrollment always increased during the first year of treatment, for any type of program, and this effect is largest from the full package program. The enrollment continued to increase at a somewhat slower pace in the following years, with the only exception of the 2000 treatment group, where enrollment increased less than in the control schools in the second year of treatment. This may point to supply constraints (the schools reach full capacity after the enrollment increase of the first year). An alternative interpretation is that, because of a general growth trend involving control schools as well, the treatment only affects the timing of enrollment growth in treated schools.

Turning to the second set of results based on the individual level data, our intention-to-treat estimates reveal that children in primary school age who live in a commune with at least one treated school (regardless of what type of treatment) have on average 1.8 months longer education and 10 percentage points higher probability of being in school. We find that the probability to be in school in 2003/04 is highest for the group of children who started treatment the year before. For the same group of children we do not find any strong evidence that they also stayed in school longer compared to a control group: at the point when we observe them, they did complete, on average, the same grade. On the contrary, children who started treatment three years before (in 2000/2001) are not comparatively more likely to be in school in 2003/04 but, at the point when we observe them, have completed a higher grade than the control group. An intuitive explanation is that a longer duration of treatment (at least three years) is needed to keep the children in school for additional school-years. Alternatively, it might be the case that we do not see any effect on highest completed grade for the 2001 and 2002 treatment groups because we observe them too soon after the treatment. Looking at the heterogenous effects, we find that the program had a stronger impact on the highest grade achieved by girls, children of low educated fathers and children from middle income families and on the probability to be in school for children of low educated parents.

The contribution of our paper is threefold. First, we are able to make comparison between three different types of FFE scheemes, which has only been done in a few studies before. The evidence on the central policy question of cost-effectiveness of such programs is even more rare. We compare each type of FFE program to alternative programs and find that the full package scheme yielded the highest impact on enrollment per dollar spent. One plausible explanation, consistent with previous evidence[1], is that this is due to the deworming treatment, konwn to be very effective at attracting children in school and at the same time extremely cheap. Moreover, most studies focused on enrollment as an outcome. Given our rich set of data, we are also able to investigate different measures of participation and attendance and say something about the class size effect. This is our second contribution[2]. Third, 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. Although the school fee for primary school is completely subsidized in Cambodia, there is evidence that other cost burdens still dissuade the poorest families from sending their children to school. Policy interventions directly targeting the poor have been shown to be the most effective means of increasing participation rates in developing countries[3] From a policy perspective, if the major objective is to increase the short-term enrollment then our findings are encouraging but if the objective is to make children stay longer in school as well as to improve their learning, then more efforts are needed on the supply side (teachers and classrooms).

The reminder 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 the descriptive statistics. We present the quantitative results in section 5, and a cost-benefit analysis in section 6. Section 7 concludes.

[1]Miguel and Kremer (2004) find that the cost per additional year of school participation is only 3.50 USD which is very cost-effective compared to other programs.

[2]Only one robust study looked into similar issues and found that school meals for preschool children displaced teaching time and led to larger class sizes (Vermeersch et al. (2005)). However this study is confined to pre-school children.

[3]SeeGlewwe 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.

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. By comparing potential future benefits of education to current costs parents decide how much to invest in the education of each child. There are two types of education costs, the direct costs (school fees, supplies, books, uniforms, and travel to school) and the indirect costs, for example 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. By subsidizing these schooling costs through FFE programs, greater investment in education may be achieved.

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. Moreover, take-home rations work 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 are food rations given to the household conditional on a child's enrollment in school and a minimum level of attendance. Take-home rations focus 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[4] as well as enhance school attendance. They 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[5]. The major objectives 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

[4]Because the meals are served before the school-day, the child learns more effectively, undistracted by short-term hunger and more able to focus.

[5]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.

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 these hypotheses are being 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 preschools, and found that school participation was 30 percent greater in the 25 Kenyan preschools 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.

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 producing the stated education and nutrition objectives and that other cost-effective mechanism exists. Very few studies investigate the cost-effectiveness of FFE programs and the types of school feeding program that are most effective. There are also very few studies that look at the differential impacts of FFE on children by age and gender, and compare the impact on both enrollment and school attendance. There are a number of reasons why these two 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, maybe due to incomplete school records. Taking both these measures into account would give policy-makers a broader picture of the program's impact.

Given our rich set of data, we are able to investigate different measures of participation and perform a deeper analysis about the effect of the program beyond enrollment. Moreover, the program studied in this paper takes, in the different waves, three different forms: i) on site meals, ii) on site meals and take-home rations, and iii)the "full package", i.e. on site meals and take-home rations together with a deworming program, which allows us to make comparisons.

3 Background

After decades of political unrest, Cambodia has during the last decade 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 550 USD in 2007 and about 35 percent of the total population lives below the poverty line.[6]. Agriculture, mainly rice production and small-scale subsistence agriculture, is still the main economic activity for a majority of households. In primary education, enrollment is still far from being universal although the government is committed 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[7], the net enrollment rate for primary education was 89 percent in 2007, while the primary dropout rate was 46 percent.

The recent global economic crisis threatens to have a considerable negative impact on poverty reduction and education outcomes. In 2008, the domestic price of rice doubled compared to the previous year while meat and fish prices went up by 30-60 percent, whereby many children were withdrawn from school[8]. The children had to

[6]See Cambodia Demographic and Health Survey, DHS, 2005.

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

[8]See "Safety nets in Cambodia. Concept note and inventory"; CARD, WFP and WB (2009).

join the workforce in order to complement the reduced household incomes. Moreover, the FFE program, running since 2000, was cut due to the soaring global prices, increasing the cost of schooling for families[9]. Past instances of similar real income shocks in combination with increases in commodity prices have shown to constitute a significant risk to educational outcomes for the poor. For example, the 1997 economic crisis in Indonesia led to a doubling of the children out of school[10], while droughts in Sub-Saharan Africa have been associated with declines in both schooling and child nutrition[11]. The global food, fuel, and financial crises have therefore created a new role for FFE programs as a potential safety net and as a social support measure that helps keep children in school.

3.1 The Cambodian FFE

The Cambodian FFE program started in 1999-2000 as a pilot project in Takeo province[12] with only 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[13]. 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

[9]Source: WFP Food Security Atlas for Cambodia.

[10]See Frankenberg et al. (1999)

[11]Schady (2008).

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

[13]The broad goal of this operation is 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 chronicle cycle of debt in rural areas.

meals, apart from WFP's food provision, were born by the community.

In 2001-2002 the program continued in cluster schools[14] where additional inputs from the World Bank-supported Education Quality Improvement Project (EQIP) within the MoEYS together with other primary education, health, and community support programs were available. This expansion was undertaken 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 being piloted this year as an incentive to keep these girls in school: girls in that ages are in fact more vulnerable to dropout. 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: in collaboration with the Ministry of Health, WHO and UNICEF, WFP provided deworming medicine to students and infection prevention training for all teachers and students.

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

3.1.1 The selection criteria

The selection of schools in the pilot phase was based on the Cambodia Vulnerability Analysis and Mapping (VAM), which is a WFP technical tool used worldwide to assess and analyze food security in order to target interventions. The analysis and

[14]This definition refers to a particular administrative structure, in which different school levels are clustered under a common administration.

Table 1: WFP School Feeding Coverage 1999-2003

	1999-2000 Pilot	**2000-2001**	**2001-2002**	**2002-2003**
PROVINCE (Partners)	Takeo (EQIP)	Takeo (EQIP) Kampot (EQIP) Kg Cham (KAPE)	Takeo (EQIP) Kampot (EQIP) Kg Cham (KAPE) Kg Speu (UNICEF) Prey Veng (UNICEF)	Takeo (EQIP) Kampot (EQIP) Kg Cham (KAPE) Kg Speu (UNICEF) Prey Veng (UNICEF) Kg Thom (UNICEF)
SCHOOLS	64/320	201/593	403/1,078	565/1,122
PUPILS	37,500	125,000	291,593	317,053
TYPE OF FFE	On-site	On-site	On-site Take-home	On-site Take-home Deworming

Note: The number of treated schools and pupils reported in this table are according to the ex-ante planning by the implementing institutions and may differ from the actual numbers that we observe in the data.

mapping involve taking measures of human vulnerability[15] across the various geographic areas of the country, and creating maps to present the information visually. In general, two composite indexes are used for school feeding programs: an index of basic education need (that looks at primary and lower secondary school aged children) and an index of adult education need (that looks at the adult population aged over 15). The communes with the lowest values for these composite indexes have the highest levels of education need, and hence should be given the highest priority for intervention[16]. However, given the that the targeting demanded a significant amount of staff time and attention and that the criterias and procedures were changed almost annually, the implementer were recommended by their evaluation team to put less emphasis on commune targeting. These criteria were only supposed to work as broad guidelines and not function as the sole basis of selection. In fact, after the pilot

[15]Vulnerability is defined as anything that increases the likelihood of a person suffering disadvantage or deprivation of any kind.

[16]For methodological details of the vulnerability analysis and mapping exercise, we refer the reader to the project technical reports, published by the RGC and WFP in 2002 and 2003.

year, the selection of schools was based on school clusters under the EQIP project, plus the formal submission and commitment by the schools themselves to prepare all cooking and storage facilities.[17]

As discussed later, the rule of prioritizing the most vulnerable schools was not followed. However, we found that the schools selected for treatment were sistematically different in terms of lower repetition rates. The fact that treatment was given to better performing schools in this sense, and the self-selection connected to the formal submission and commitment to prepare the food might cause biases in our estimates: selection bias might imply that we overestimate the effect of treatment, while mean reversion might lead to underestimate it. We discuss further these potential biases 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[18]. The main 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[19]. The data can be perfectly merged with the information on treatment status that has the same school identification number. We have access to an additional EMIS panel (same source as the main one but lacking the school identification numbers), that covers 5,250 schools between years 1997 and 2002. Information on treatment status is here merged based on the location name (province, district, commune and village)

[17]Source: WFP (2000), "Mid-term evaluation of PRRP Cambodia 6038.00", WFP/EB.2/2000/3/6; WFP (2000), "Full Report of the Evaluation of CAMBODIA PRRO 6038 - Food Aid for Recovery and Rehabilitation", Rome.

[18]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, parents associations, etc.

[19]3089 schools in our 6 provinces of interest.

and school name. The merging may not be exact due to alternative spellings of location and school names, so we use the file for robustness checks.

Individual level data are taken from two waves of the Cambodia Socio-Economic Survey (CSES 1999 and CSES 2004), large-scale nationally-representative household surveys collected by the National Institute of Statistics[20]. Using this dataset, we can analyze two more outcomes: the highest educational achievement, which is based on the survey question "'What is the highest level ..[NAME].. successfully completed?"' and the probability to be in school in a given year which is based on the following survey question "'Is ..[NAME].. currently in the school system?"'. The former is an indicator of whether the child actually completed the full school year. Although it measures the lenght or the quantity of education in a long-run perspective, it also says something about the quality of education, because it implies that the children did not just attend school for the sake of the free food, but also completed the full school year. Given the huge influx of enrolled children due to the FFE program, if the schools adjust their resources (teachers and classrooms) accordingly to the increased number of children in school then countervailing effects from crowded classrooms negatively affecting teaching quality and learning are less likely to happen. Instead of a short-term enrollment and a high pupil turnover, we would rather observe an actual increase in the highest grade achieved for, in particular, the most vulnerable children that would otherwise have dropped out. The latter outcome is an indicator of enrollment that, in contrast to the school data, should be less subject to the overreporting problem, since it is self-reported by the household. Another difference is that it might not only capture the enrollment but rather the attendance since a child might have incomplete school records and be unable to enroll but still attend school. Unfortunately, there is no information on which specific school the individuals are attending. Based on the school data we are able to see that there is only slightly

[20]CSES 1999 covers 6000 household and was carried out from January to August 1999. CSES 2004 covers 15000 households and spans from November 2003 to January 2005. Besides the socio-economic background variables (consumption, age, sex, income, etc.), this dataset contains more detailed information about schooling at the individual level: attendance and highest grade completed, literacy, 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).

more than one school in each commune, and hence, the commune level would be the closest one to the treatment assignment level. We merge the information on treatment status at the commune level and therefore adopt an intention-to-treat approach.

The Cambodian Demographic and Health Survey (DHS) from 1998 is used to check the pre-treatment summary statistics at the village level. The DHS is a nationally representative survey with a sample size of 5000 households.

4.2 Descriptive statistics

Table 2 reports the pre-treatment summary statistics from the main school panel and the DHS, showing differences in enrollment, repetition rates, school and village characteristics between treated and non-treated units in 1998. Selection bias might be a concern, due to the VAM criterias followed in prioritizing schools for treatment, as detailed in the previous section. However, the treated schools do not seem to be generally worse-off before the treatment: they have slightly lower repetition rates, if anything, and they are less likely to be defined as disadvantaged by the MoEYS, and more likely to have a parents' association. The average class size is not significantly different. Only the student/teacher proportion is slightly worse in treated schools. By and large, though, the data do not reveal that particularly bad performing schools were selected into the program. The village level data from the DHS 1998 show that the treatment and control villages did not differ significantly in terms of educational outcomes for the adult population either.

In order to control for potential unobservable confounding factors, we use school fixed effects. However, we cannot control for potential confounding factors that change over time. For example, it could be the case that being less often defined as disadvantaged and having more parents' associations gives these schools better prospects in terms of future perfomance. Table 2 reveals however that these differences, though significant, are very small.

Table 2: Pre-treatment summary statistics

	Control	Treatment	Diff.	P-value	Obs.
			SCHOOL LEVEL		
Enrollment					
Grade 1	126.9	124.8	-2.168	0.625	2236
Grade 2	86.1	85.4	-0.616	0.856	2236
Grade 3	67.7	66.6	-1.160	0.700	2236
Grade 4	50.5	49.1	-1.352	0.596	2236
Grade 5	38.4	36.7	-1.743	0.420	2236
Grade 6	27.8	25.7	-2.105	0.220	2236
New intakes	72.3	71.1	-1.138	0.672	2236
Girls, grade 1	59.5	58.4	-1.117	0.601	2236
Girls, grade 2	39.4	39.3	-0.163	0.918	2236
Girls, grade 3	30.9	30.3	-0.674	0.638	2236
Girls, grade 4	22.8	22.1	-0.790	0.510	2236
Girls, grade 5	16.9	15.8	-1.095	0.280	2236
Girls, grade 6	11.6	10.1	-1.430	0.064	2236
Total	397.7	388.6	-9.144	0.567	2236
Girls, total	181.4	176.2	-5.270	0.476	2236
Girls /Boys	0.458	0.458	0	0.873	2236
Repetition rate					
Grade 1	0.40	0.39	-0.011	0.262	2236
Grade 2	0.25	0.22	-0.022	0.004	2083
Grade 3	0.19	0.16	-0.013	0.093	1834
Grade 4	0.12	0.09	-0.021	0.002	1620
Grade 5	0.07	0.06	-0.009	0.154	1485
Grade 6	0.04	0.03	-0.010	0.059	1344
Total	0.23	0.22	-0.017	0.007	2236
School characterstics					
Frac. disadvantaged	0.11	0.08	-0.032	0.041	2236
Frac. w parents assoiation	0.67	0.74	0.065	0.005	2389
Income p /c	26149	50780	25857	0.245	2389
Teachers /100 stud	2.23	2.13	-0.106	0.021	2236
Av. class size	54.6	55.5	1.17	0.326	2402
			VILLAGE LEVEL		
Frac. w primary edu.	0.49	0.48	-0.007	0.892	102
Education level, 15-24	4.7	4.0	-0.770	0.113	102
Literacy rate	0.67	0.57	-0.1	0.164	102

Source: DHS and EMIS

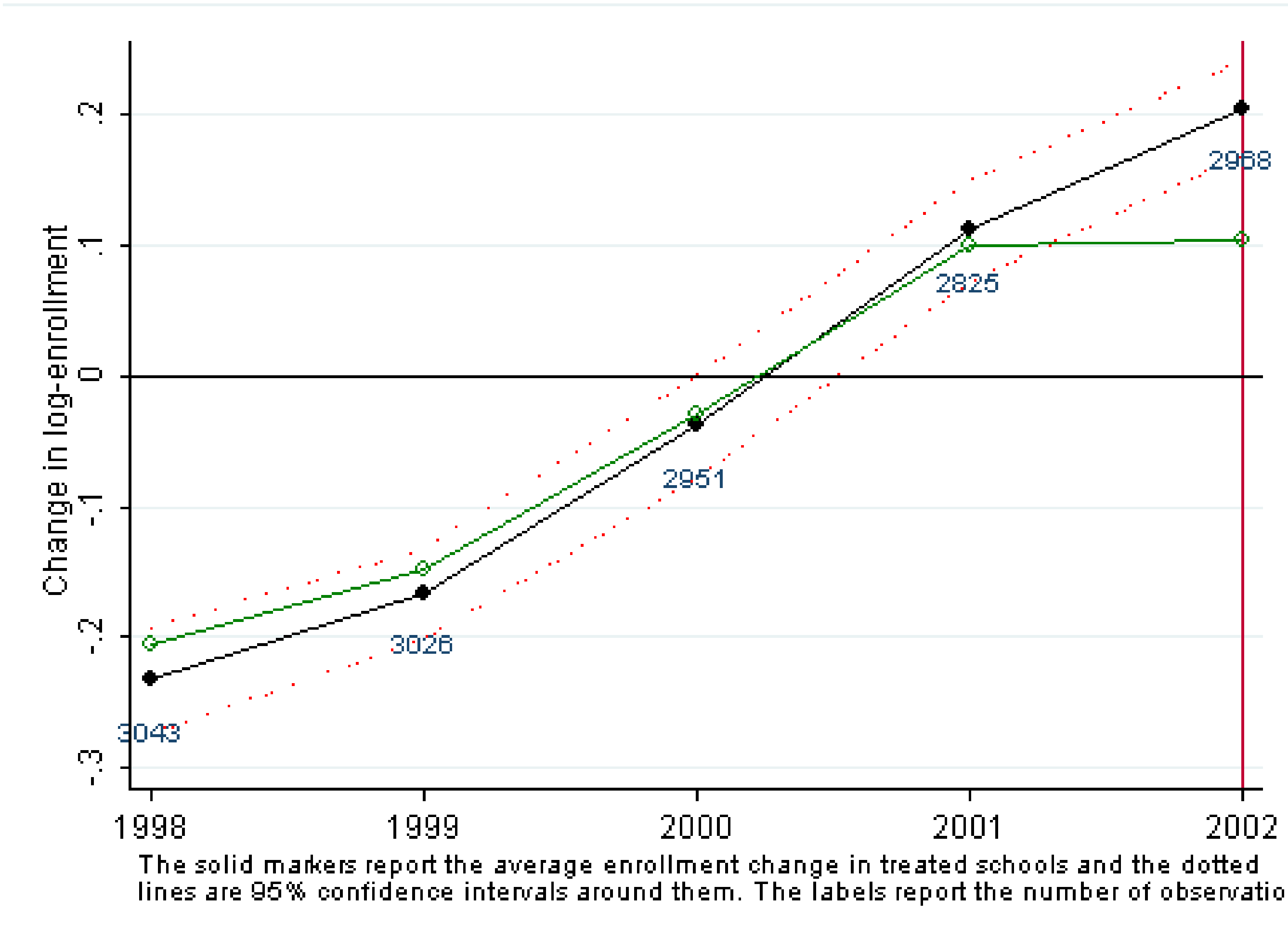

Figure 1: Enrollment trends before the treatment in treated and control schools

To further alleviate the concerns about selection, we look at enrollment trends before and after the treatment, in figure 1. The figure shows the enrollment trends plotted over time, pooling together all the treatment and control groups. For each year after 1998, the schools treated in that year are dropped from the plot, so that all the schools are observed exclusively before receiving the treatment, except for 2002. The series, purged of school fixed effects, are clearly parallel, and diverge only in the year of treatment.

Enrollment rates alone, as mentioned above, might not give a clear picture of the success of a policy. First of all, increased enrollment not matched by increased resources, like teachers, classrooms, etc., might even lead to negative outcomes when it comes to school quality and learning. Moreover, the short term availability of food in school might simply result in likewise short term enrollment and high turn-over in pupils, rather than an actual increase in their total education achievement. This point can be addressed studying the household data. Figure 2 reports the average highest completed grade for each of the birth cohorts that were in primary school age between 1999 and 2002 in treated and non-treated communes[21] These children, aged 8 to 15 in 2004 when the survey was conducted, are compared with children aged 8 to 15 in 1999, at the time of the previous survey. The figure shows in the upper panel that the highest grade achieved increased in general between the two survey waves. However, while the highest grade is always lower in treated communes compared to non-treated communes in 1999, in 2004 this pattern is often reversed. In other words, education achievement increased *comparatively more* in treated communes. The lower panel shows how the distribution of highest completed grade has changed between the two points in time, revealing a drastic reduction in the number of zeroes. In other words, the proportion of children that do not have any education at all went down, and once again this effect is stronger in the treated communes. These patterns are very similar when we investigate the subsample of girls (results not shown).

[21] We namely take here an intention to treat approach. The oldest children that potentially received the treatment were 12 (and officially enrolled in 6th grade if they had started school at the official entry age of 6) in 1999 and the youngest were 6 (1st graders) in 2002.

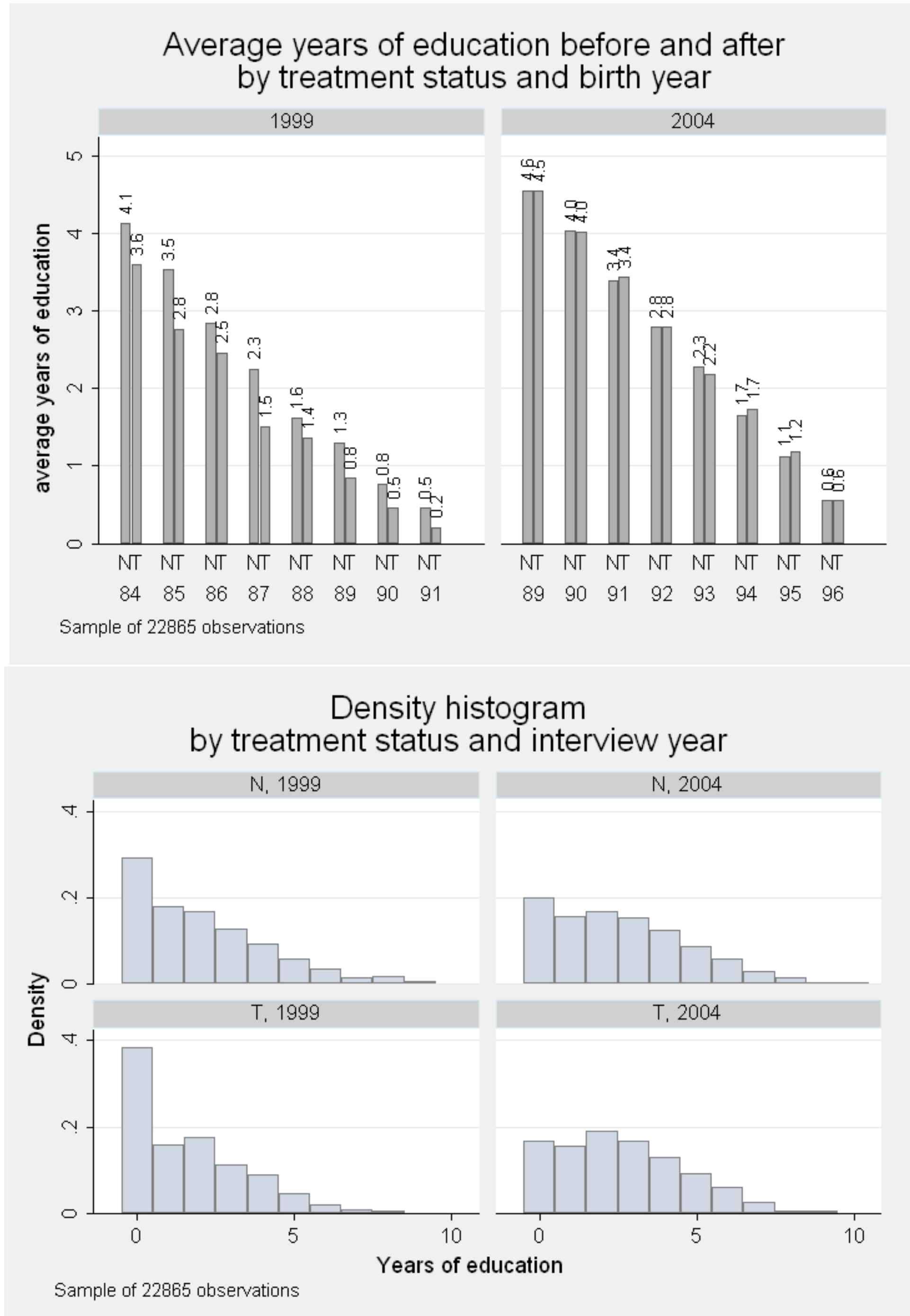

Figure 2: Distribution of highest grade completed

4.3 Specifications

The identification of the effect 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 analyse the effect on enrollment for each year of treatment $g = (2000, 2001, 2002)$ using the following specification:

$$Enr_{itg} = \alpha + \beta * Aft_g + \gamma * Tr_g * Aft_g + \sum_{k=1999}^{g} \gamma_k Tr_g * Aft_g * TrGr_k + \mu_i + \sigma_{itg} \quad (1)$$

where the subscript i denotes the school and t the year in which enrollment is observed. The dependent variable is the natural logarithm of the total enrollment, in order to smooth a dependent variable that can otherwise take some rather extreme values. In a given year, among all the treated schools (Tr) there will be 2 to 4 sets of schools that differ in terms of when they started receiving the treatment, i.e. which treatment group they belong to (TrGr99, TrGr00, TrGr01 or TrGr02[22]). As we want to observe the effect of treatment over time, we allow the estimate to have a separate intercept and slope for treated schools that differ in their length of treatment (Tr*Aft*TrGr). $g - k + 1$ indicates hence the number of years of treatment. Besides total enrollment (in logs, to take into account school size), we also look at enrollment by gender and grade[23]. A simple difference-in-difference specification, with treatment group dummies instead of the school fixed effects, is also reported in table 4.

For the individual data, the ideal would be to use commune fixed effects to account for unobservable characteristics at the commune level, which is the closest to the treatment assignment level. But since most of the communes appear only in one of the surveys, making a comparison within commune over time impossible, we

[22] To be more precise, the TrGr99 is defined as a group of treatment units (either schools or communes) that received treatment for the first time during school year 1999/2000, and so on.

[23] Estimations by grade are not shown. The main patterns are summarized in the result section. Tables can be requested to the autors.

use a fixed effect specification at the district level. Most of the districts, in fact, are represented in both the surveys. The following is estimated:

$$Educ_{idt} = a + b * Aft + c * Aft * Tr + m_d + e_{idt} \tag{2}$$

where i, d and t index respectively individual respondent, district and year. Since the sample we are using contains children of different age, all the specifications include age dummies to account for any age-related differeces in education. Moreover, given that the CSES 2004 survey was running over two school years (November 2003 to January 2005), we will observe children of the same age but born in different cohorts, according to when exactly they were interviewed. We therefore include birth year dummies taking the value one for childen born in a given year and observed in the CSES 2004. The outcome variables here are the highest grade achieved and the probability to be in school, in 2004 versus 1999. We further use the same specification with additional interaction terms for the per capita income quintile (proxied by per capita consumption), gender and parents' education level. A simple difference-in-difference specification, with treatment group dummies instead of the district fixed effects is also reported.

4.3.1 Selection bias

As mentioned earlier, according to the selection rule during the pilot phase, communes with the highest education needs were prioritized for the intervention. After the pilot phase, it was decided that schools with formal submission and commitment to prepare cooking and storage were more likely to be given the intervention. We test whether the rule was actually followed by running a simple regression at the school level. The dependent variable is the treatment status indicator and a set of selection variables are tested as determinants: a dummy for whether the school is defined as disadvantaged, a dummy for having a parents' association, the total primary enrollment, the poverty rate in the commune of the school, the repetition rate at primary level. The regression is run for both year 1997 and 1998, i.e. before the intervention. We find that (results not shown) the only factor significantly determinig the treatment status before the intervention is the repetition rate: a negative coefficient implies that schools with lower repetition rates were prioritized for receiving the treatment. Hence, the rule of prioritizing the most vulnerable schools was not followed. But the fact that treatment was given to better performing schools in terms of repetition rates, and the self-selection connected to the formal submission and commitment to prepare the food might cause biases in our estimates: selection bias might imply that we overestimate the effect of treatment, while mean reversion might lead to underestimate it. To deal with a potential mean reversion problem, we use an additional specification, where we interact the average repetition rate for 1997 and 1998 with the after-treatment indicator variable. The results are very similar suggesting that the bias is relatively small.

5 Results

We start with a placebo-like test: table 3 presents the effect of the treatment *before* the treatment, in other words, the change in enrollment between 1997 and 1998, comparing the various treatment groups to the respective control group. For this purpose, we use the additional EMIS panel for which we have data from 1997 as well. We already ruled out that schools receiving treatment were ex-ante different in the levels of enrollment. If they had been ex-ante different in terms of their *rate* of increase in enrollment, then 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 parallell trend assumption holds for our identification.

5.1 Effects on enrollment

Table 4 presents the results in a simple difference-in-difference setting for each treatment year, including only schools receiving treatment for the first in that year[24].

[24]If we instead look (results not shown) at all the schools treated each year without considering that schools belong to different treatment groups, we do no see any effect of treatment, except in

Table 3: Placebo test - effect on enrollment between 1997 and 1998

	(1)	(2)	(3)	(4)	(5)
Treatment group	1999	2000	2001	2002	All
TreatXAfter	0.0241	0.00937	-0.00234	-0.00127	-0.00127
	(0.0185)	(0.0122)	(0.0108)	(0.0100)	(0.0100)
R^2	0.350	0.005	0.006	0.002	0.009
Schools	2236	2236	293	1212	1871
Observations	4443	4451	578	2409	3725

Note: The dependent variable is the natural logaritm of enrollment. The coefficients compare enrollment in 1998 with 1997. The control groups include all the non-treated schools within the same provinces for each treatment group. Standard errors clustered at the school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The table shows means of (log) enrollment by year (comparing enrollment in the year of treatment with enrollment in the year before) and treatment status. The control groups, one for each treatment group, include all the never-treated schools within the same provinces. We see significant increases in enrollment with respect to the control group and the relative increases are quite similar across the treatment groups, with the exception of the 2002 "full package" group. These group-level difference-in-difference estimations are however very noisy, because schools can be very different in terms of size, location, income, infrastructures and many other fixed characteristics.

Given the panel structure of the data, we are able to observe the same schools after each year of treatment. Table 5 reveals that the effect on enrollment is in fact always positive and significant in the first year of treatment, even controlling for the school fixed effects, and slightly decreases over time. From column (3)), we learn that the increase in enrollment due to the FFE program after the first year of treatment is 5.8% with only on-site feeding (treatment group 1999 and 2000), 5.2% when the take-home rations were also provided (treatment group 2001), and almost 19% with the full-package including deworming (treatment group 2002).

The impact on enrollment for each single treatment group can be followed over time by summing the coefficients corresponding to the interaction terms in equation

1999. This happens because schools with different treatment length are pooled together, while the effect is not constant over the length of treatment, the enrollment increase being smaller for the schools in their second, third or fourth year of treatment.

1. For example, in the 2000 treatment group, enrollment increases by 5.82% during the first year, then by 5.22 - 8.34 = -3.12% in the second year, and finally by 18.8 - 11.3 = 7.5% in the third year. Only in this particular case, the 2000 treatment group observed in 2001/02 (their second year of treatment), we observe a negative effect (although it cannot be distinguished from zero), which means that enrollment in the treatment schools increases less than in the control schools. However, also in the other groups the enrollment increases clearly slow down in the following years as compared to the first year of treatment. One possible interpretation for the fading out of the effect is that all the eligible children that were still out of school and are sensitive to the program (i.e., they live in households for which the program is enough to shift the balance of costs and benefits of school towards the benefit side), are already attracted to school during the first year of treatment. Another 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 55 in 1999 and 70 in 2003. Similarly, there is one teacher for 57 pupils on average in 1999 and one for 62 in 2002. Yet another interpretation could be that the quality of learning goes down as an effect of the increase in enrollment immediately after the introduction of free meals, and this might crowd out some students over the following years. Finally, we have to acknowledge the strong general increasing trend in enrollment, clearly visible in Figure 1, that seems to interest the whole country in this period. It might well be possible that the presence of the school meal program only has an effect in anticipating this growth in enrollment in the treated schools, but the control schools follow suit anyway.

The analysis by grades and gender, not reported, shows that the bulk of the effect comes from grades 4-6, and from girls. The enrollment increases are particularly big for girls, in absolute sense and as compared to boys, in 2001 and 2002, which we interpret as a potential effect of the take-home rations.[25] However, there are positive effects also for boys in these years, which might suggest that the rule of targeting exclusively poor girls with take-hom rations was not stricltly followed.

[25] 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.

Table 4: Simple difference-in-differences after 1 year of treatment, school level data

	Non-treated	Treated	Difference
	Treatment group 1999		
Before	5.930	6.140	0.210
	(0.059)	(0.072)	(0.093)
After	5.982	6.24	0.258
	(0.048)	(0.065)	(0.079)
Diff-in-diff			**0.041***
			(0.019)
n			345
	Treatment group 2000		
Before	5.692	5.937	0.244
	(0.028)	(0.061)	(0.068)
After	5.833	6.121	0.288
	(0.028)	(0.051)	(0.058)
Diff-in-diff			**0.043***
			(0.024)
n			1239
	Treatment group 2001		
Before	5.732	5.780	0.048
	(0.022)	(0.056)	(0.060)
After	6.024	6.111	0.094
	(0.019)	(0.047)	(0.051)
Diff-in-diff			**0.046***
			(0.025)
n			1815
	Treatment group 2002		
Before	5.624	5.281	-0.343
	(0.022)	(0.080)	(0.083)
After	5.939	5.764	-0-.175
	(0.019)	(0.066)	(0.068)
Diff-in-diff			**0.167*****
			(0.036)
n			2014

Note: The dependent variable is the natural logaritm of enrollment. The control groups include all the non-treated schools within the same provinces for each treatment group. Robust standard errors clustered at the school level in parentheses. Statistic significance is displayed only for the difference-in-difference term: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect on enrollment, by type and length of treatment

	Diff-in-diff	Fixed effects	Fixed effects
	All treated schools observed in 2003/04		
TreatXAfter	0.146***	0.036*	
	(0.024)	(0.019)	
	Treated schools observed in 2000/01		
TreatXAfter	0.0311	0.0511***	0.0582**
	(0.0201)	(0.0182)	(0.0236)
Treat00XAfterXTrGroup99			-0.0223
			(0.0265)
R^2	0.023	0.164	0.164
Schools	1302	1302	1302
Observations	2555	2555	2555
	Treated schools observed in 2001/02		
TreatXAfter	-0.000808	0.00728	0.0522**
	(0.0179)	(0.0172)	(0.0249)
TreatXAfterXTrGroup00			-0.0834**
			(0.0333)
TreatXAfterXTrGroup99			-0.0212
			(0.0317)
R^2	0.039	0.425	0.427
Schools	2010	2010	2010
Observations	3957	3957	3957
	Treated schools observed in 2002/03		
TreatXAfter	0.0303	0.0544***	0.188***
	(0.0191)	(0.0182)	(0.0359)
TreatXAfterXTrGroup01			-0.131***
			(0.0432)
TreatXAfterXTrGroup00			-0.113***
			(0.0351)
TreatXAfterXTrGroup99			-0.00339
			(0.0348)
R^2	0.038	0.397	0.406
Schools	2402	2402	2402
Observations	4715	4715	4715

Note: The dependent variable is the natural logaritm of enrollment. The control group include all non-treated schools within the same provinces for each treatment group. Columns (2) and (3) include school fixed effects. Column (3) allows for a separate intercept and slope for the schools depending on which treatment group they belong to. Robust standard errors clustered at the school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Effect on highest grade and probability of being in school

We follow a similar approach for the household data, and start by reporting, in table 6, the simple difference-in-difference for the highest grade achieved and the probability of being in school for all the children that, based on their birth year, were supposed to be in school during at least one treatment year. Children interviewed in 2004 in one of the treated communes are compared with children in non-treated communes, and then with the corresponding cohorts of children interviewed in 1999, before the treatment started. Since there are no data prior to 1999, communes treated in 1999 are excluded from the sample. The first treatment year in this part of the analysis is hence 2000.

Table 6: Simple difference-in-difference, individual level data

	Non-treated	Treated	Difference
	Highest grade completed in year 2004/2005		
Before	2.1	1.6	-0.44
	(0.057)	(0.118)	(0.13)
After	2.6	2.65	0.051
	(0.045)	(0.099)	(0.108)
Diff-in-diff			**0.491*** **
			(0.152)
	Probability of being in school in year 2004/2005		
Before	0.76	0.69	-0.069
	(0.011)	(0.032)	(0.033)
After	0.85	0.89	0.036
	(0.006)	(0.012)	(0.013)
Diff-in-diff			**0.106** **
			(0.034)

Note: The dependent variable is the highest grade completed in the first panel and the probability for being in school in year 2004/2005 in the second panel. The control group includes all the children in the same age cohorts interviewed in non-treated communes. Robust standard errors clustered at the commune level in parentheses. Statistic significance is displayed only for the difference-in-difference term: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7 shows the OLS estimations including both age and birth year dummies (column 1 and 3). In order to reduce the noise in the data, district fixed effects are

Table 7: Average treatment effect, individual level data

	(1)	(2)	(3)	(4)
	Highest grade		Probability	
	OLS	FE	OLS	FE
Treat	-0.487***	-0.130	-0.0693**	-0.0686*
	(0.121)	(0.154)	(0.0341)	(0.0373)
After	0.516***	0.319***	0.0973***	0.182***
	(0.0652)	(0.0494)	(0.0142)	(0.0190)
TreatXAfter	0.510***	0.271*	0.106***	0.0995***
	(0.144)	(0.141)	(0.0339)	(0.0334)
R^2	0.383	0.493	0.056	0.148
Districts	168	168	168	168
Communes	852	852	852	852
Observations	22499	22499	22497	22497

Note: The dependent variable is the highest grade completed in columns 1-2 and the probability for being in school in year 2004/2005 in columns 3-4. The control group includes all the children in the same age cohorts interviewed in non-treated districts. All the regressions include age and birth year fixed effects. Column (2) and (4) include districts fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

added (column 2 and 4) which makes the treatment estimates smaller in size. The district fixed effect estimates imply almost 2 months longer education (0.27 years more) relative to the before-treatment mean of 1.8[26]. The same specification is used for the probability of being in school.

The fixed effect estimates show that this probability increases 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 2004 (69%), implies a 14% increase due to the program. These effects are averages of all the treated communes in a given year and do not take into account the length of treatment.

In table 8, we want to investigate whether the progam effect differs with the length of treatment. Starting with the highest grade achieved, only children treated

[26]This figure is so low because it is an average for all children aged 7-15, including those with zero education. The mean excluding the zeroes, i.e. the mean completed education for those that have been to school at some point, is 2.8.

in 2000 have significantly higher achievements compared to the control children when we observe them in the 2004 survey. No effect is found for the treatment groups 2001 and 2002 (communes receiving treatment for 2 and 1 year, respectively, when observed in the 2004 survey). Column (4) to (6) reveal, though, that these children are significantly more likely to be in school in 2004 than the control children. One intuitive explanation is that the duration of the program is important: children that receive food in school for three years (started in 2000/01), stay in school longer than they would have otherwise (however not until 2004, since their probability in column (4) is not significantly higher). For those that receive food for shorter durations, the treatment does not make a difference. In the same spirit, we do not observe any increase in the number of those who complete the full primary school, because probably three years are not enough to make a difference for this decision[27]. An alternative interpretation is that many of these children that are more likely to be in school in 2004 are repeaters: in their case, the potential extra years of school attendance would not show up in the completed grade. We also look at the effect by birth year but do not find any particular pattern in this respect.

Tables 9 and 10 present the heterogeneous effects between groups, without considering the which treatment group they belong to. Girls and children of fathers with lower education, groups that we would expect to be disadvantaged in terms of schooling, have achieved a comparatively higher grade in 2004, while children of parents with lower education are more likely to be in school in 2004.

[27] This results are not shown. Notice that the FFE went on until 2008. It is possible hence that such longer term effects will be visible in later data.

Table 8: Effect by treatment group 2000, 2001 and 2002

	(1)	(2)	(3)	(4)	(5)	(6)
	Highest grade			Probability of being in school		
Treatment group	2000	2001	2002	2000	2001	2002
Treat	-0.380	0.260	-0.231	-0.0149	-0.0296	-0.293**
	(0.252)	(0.218)	(0.400)	(0.0448)	(0.0478)	(0.145)
After	0.663***	0.880***	0.292***	0.116***	0.119***	0.168***
	(0.0605)	(0.0547)	(0.0452)	(0.0158)	(0.0430)	(0.0194)
TreatXAfter	0.450*	0.162	0.230	0.0445	0.0739*	0.342**
	(0.263)	(0.189)	(0.391)	(0.0444)	(0.0430)	(0.136)
R^2	0.396	0.427	0.445	0.129	0.134	0.173
Districts	165	167	167	165	167	167
Communes	801	806	789	801	806	789
Observations	16350	16188	15537	16348	16187	15536

Note: The dependent variable is the highest grade completed in columns 1-3 and the probability for being in school in year 2004/2005 in columns 4-6. The control group includes all the children in the same age cohorts interviewed in non-treated districts. All the regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Heterogeneous effects on highest grade achieved

	(1)	(2)	(3)	(4)	(5)	(6)
	Girls	Boys	Low mother's educ	High mother's educ	Low father's educ	High father's educ
Treat	-0.0943	-0.136	-0.0224	-0.485*	-0.124	0.140
	(0.169)	(0.168)	(0.169)	(0.249)	(0.154)	(0.238)
After	0.369***	0.881***	0.287***	0.378***	0.765***	0.833***
	(0.0609)	(0.0748)	(0.0523)	(0.0891)	(0.0601)	(0.105)
TreatXAfter	0.277*	0.240	0.260	0.333	0.355**	-0.0206
	(0.160)	(0.151)	(0.159)	(0.246)	(0.139)	(0.241)
R^2	0.504	0.492	0.489	0.517	0.465	0.584
Districts	168	168	168	164	168	154
Communes	850	849	850	799	846	743
Observations	10919	11580	16017	6482	15790	6709

Note: The dependent variable is the years of education in year 2004/2005. The treatment group consists of all children interviewed in any of the treated communes regardless of when they were treated or what type of treatment. The control group includes all the children in the same age cohorts interviewed in non-treated districts. All the regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Heterogeneous effects on probability of being in school

	(1) Girls	(2) Boys	(3) Low mother's educ	(4) High mother's educ	(5) Low father's educ	(6) High father's educ
Treat	-0.0707*	-0.0688	-0.0628	-0.0739	-0.0423	-0.0647
	(0.0415)	(0.0458)	(0.0414)	(0.0594)	(0.0413)	(0.0463)
After	0.182***	0.107***	0.190***	0.173***	0.148***	0.0406***
	(0.0256)	(0.0146)	(0.0221)	(0.0291)	(0.0161)	(0.0144)
TreatXAfter	0.101**	0.0992**	0.110***	0.0522	0.0976***	0.0578
	(0.0394)	(0.0386)	(0.0358)	(0.0602)	(0.0372)	(0.0445)
R^2	0.161	0.154	0.157	0.162	0.157	0.126
Districts	168	168	168	164	168	154
Communes	850	849	850	799	846	743
Observations	10919	11578	16015	6482	15788	6709

Note: The dependent variable is the probability for being in school in year 2004/2005. The treatment group consists of all children interviewed in any of the treated communes regardless of when they were treated or what type of treatment. The control group includes all the children in the same age cohorts interviewed in non-treated districts. All the regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 11 we look at the last year of treatment, to investigate further on the effect of take-home rations. If we separate between girls and boys in that year, we find that the effect in terms of higher probability of being in school is indeed stronger and larger in size for girls. Once again, this does not add to the total duration of their education.[28] This might once again suggest that the rule of targeting girls was not strictly followed.

Table 11: Effect by birth year and gender for treatment group 2002

	(1)	(2)	(3)	(4)
	Highest grade		Probability	
	Girls	Boys	Girls	Boys
[1em] TreatXAfterXByr1991	0.436	0.0279	0.452***	0.293**
	(0.492)	(0.513)	(0.171)	(0.149)
TreatXAfterXByr1992	-0.0633	-0.127	0.371**	0.314*
	(0.581)	(0.457)	(0.160)	(0.172)
TreatXAfterXByr1993	-0.155	-0.140	0.369*	0.215
	(0.694)	(0.522)	(0.189)	(0.170)
TreatXAfterXByr1994	0.515	0.104	0.413***	0.307*
	(0.551)	(0.535)	(0.148)	(0.185)
TreatXAfterXByr1995	0.629	0.0638	0.327*	0.187
	(0.597)	(0.407)	(0.191)	(0.174)
TreatXAfterXByr1996	0.753	0.0971	0.491***	0.140
	(0.500)	(0.459)	(0.176)	(0.228)
R^2	0.459	0.439	0.181	0.181
Districts	168	168	168	168
Communes	847	846	847	846
Observations	8099	8696	8099	8695

Note: The dependent variable is the highest grade completed in columns (1) and (2) and the probability of being in school in 2004 in columns (3) and (4). All the regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[28] Although not significant, the estimates are larger for girls than for boys even in this case.

Although parents' education level can be considered as a proxy for income, Table 12 looks in more detail at the effects of the program along the income distribution.[29] The probability of being in school in 2004 is highest for the poorest quintile [30] and is decreasing with higher income, which indicates that the program is indeed a pro-poor intervention. The effect on completed education in column (1) has instead an inverted-U shape, displaying larger effects for the middle quintiles. The very poorest and the richest households in the sample increase their total education less or not at all. One interpretation is that, in these households, the cost-benefit balance of completing the full year is not affected by the school meals. The rich children, generally having a better educational prospects, complete their education anyway with or without the free food. For the poorest households, it could be that they i) are only in school during the free breakfast but not attending the classes (maybe need to help with family business), ii) have incomplete school records making it impossible to enroll and officially complete the full year, or iii) they are more sensitive to countervailing effects from crowded classrooms.

To investigate whether the program attracted older cohorts of children, we look at birth cohorts that, according to official age limits, should have been too old to be in school during the treatment years. Table 13 presents the effect of the treatment in terms of both highest grade achieved and probability of being in school for three cohorts of older children, aged between 12-14, 12-17 and 18-20. None of these cohorts are more likely to be in school in 2004 as a consequence of the treatment; on the other hand, for the younger ones a very strong positive effect can be observed in terms of highest grade achieved. Although not (differentially more) enrolled in year 2004, when they are 16 and older, these children went to school longer than children of the same age in the non-treated communes, which implies that they have been enrolled in primary school during the program although they were then already over 12.

[29] The per capita income is here proxied by per capita consumption.

[30] The quintiles are computed with respect to the general population. The analysis using quintiles computed within the sample are not reported but showes almost identical estimates.

Table 12: Effects by income quintiles

	(1) Highest grade	(2) Probability of being in school
Treat	-0.153	-0.0744**
	(0.158)	(0.0375)
After	0.217***	0.169***
	(0.0495)	(0.0188)
TreatXAfterXQ1	0.104	0.134***
	(0.163)	(0.0366)
TreatXAfterXQ2	0.359**	0.112***
	(0.169)	(0.0373)
TreatXAfterXQ3	0.440***	0.0876**
	(0.165)	(0.0396)
TreatXAfterXQ4	0.240	0.0687*
	(0.210)	(0.0370)
TreatXAfterXQ5	0.0722	0.0734*
	(0.213)	(0.0377)
R^2	0.519	0.161
Districts	168	168
Communes	852	852
Observations	22499	22497

Note: The dependent variable is the highest grade completed in column 1 and the probability for being in school in year 2004/2005 in column 2. The control group includes all the children in the same age cohorts interviewed in non-treated districts. All the regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Effect on older cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
		Highest grade			Probability of being in school	
	Aged 12-14	Aged 12-17	Aged 18-20	Aged 12-14	Aged 12-17	Aged 18-20
Treat	-0.824**	-0.967**	0.0480	-0.00778	0.00932	-0.00464
	(0.325)	(0.387)	(0.508)	(0.0539)	(0.0448)	(0.0191)
After	0.767***	0.444***	0.599***	0.103***	0.00984	0.00842
	(0.113)	(0.160)	(0.201)	(0.0188)	(0.0167)	(0.0124)
TreatXAfter	0.894***	0.535	0.274	0.00335	0.00610	0.0168
	(0.303)	(0.378)	(0.467)	(0.0523)	(0.0449)	(0.0215)
R^2	0.245	0.251	0.252	0.145	0.117	0.074
Districts	168	168	166	168	168	166
Communes	843	842	813	843	842	813
Observations	7812	6609	5606	7811	6603	5604

Note: The dependent variable is the highest grade completed in columns 1-3 and the probability for being in school in year 2004/2005 in columns 4-6. The control group includes all the children in the same age cohorts interviewed in non-treated districts. All the regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 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. The average cost of running FFE programs at WFP in 2001 was 19 cents per child per day, including everything from the values of all food commodities provided by WFP, to the cost of transportation and monitoring, to internationally- and locally-recruited staff[31]. In 2005, the average cost was 7.9 cents per child per day, or 15.79 USD per child per year[32]. In general, the take-home rations are more expensive, around 30 USD per child per year, due to transport costs and differences in food bundles[33]. 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[34]).

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[35]. Table 14 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 both is 10 USD per child[36]. Assuming that it is sufficient with one tablet of Praziquatel 600 mg and one tablet of Mebendazole 500 mg to treat a child per 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.

[31] Source: WFP Brief "WFP Global School Feeding Campaign - Into School, Out of Hunger", 2001.

[32] We are using the world's average of 200 school days per year for cross-country comparisons.

[33] See Adelman et al. (2008).

[34] See "Global School Feeding Report 2006", WFP.

[35] See WFP, 2004, "Protracted Relief and recovery operation Cambodia 10305.0", Jan 2004, for food costs and FASONLINE WFP for detailed resource allocation in 2001/2002.

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

Table 14: Program costs, 2001-2002

Resource allocation 2000/2001		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		
	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 15: Cost effectiveness of the different interventions

Treatment	(1) On site	(2) On site + take-home	(3) On site + take-home + deworming	(4) Scholarships to poor girls in 6th grade	(5) Meals to pre-school in Kenya
Year	2000-01	2001-02	2002-03	2004-06	2000-01
Budgeted n. of children	125,000	291,593	317,053	2,765	2,750
Total cost (USD)	1,012,000	2,956,753	3,284,669	124,425	127,875
Actual n. of children in the treatment group before treatment	167,230	227,550	291,930	2,765	2,750
Program impact	5.82%	5.22%	18.8%	30%	8.5%
Additional children in school due to the treatment	9,733	11,878	54,883	830	234
Cost per additional child in school (USD)	104	249	60	150	546

Note: The total cost is computed multiplying the average cost per pupil from Table 14 by the number of children in the remaining years. The budgeted number of children 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 can differ from the ex-ante estimated number. The program impact is our own estimate, from Table 5 column (4). The figures in column (4) and (5) are our elaborations respectively from Filmer and Schady (2008) and Vermeersch et al. (2005).

To assess the cost-effectiveness, in Table 15 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 5. We find that the on-site feeding is quite cost-effective, while distributing take-home rations is relatively expensive, as expected. 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, we also report in the table the cost of a conditional cash transfer program, the Japan Fund for Poverty Reduction (JFPR) scholarship program in Cambodia. This scholarship program, started in 2004, awarded poor girls who were completing sixth grade a scholarship of 45 USD[37]. The program increased the enrollment and attendance of recipients at program schools by about 30 percentage point, 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.

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 autors do not report explicitly the costs of the program, so the figures in the last column of Table 15 are our own elaborations based on data reported in their paper.

At 60 USD per child in school, the FFE looks still quite expensive compared to the programs overviewed in Miguel and Kremer (2004), for example. Their estimate of the cost of a deworming intervention is hard to beat, at 3.5 USD per child in school. If the objective is purely that of attracting more children in school, that is. However, if 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.

[37] See Filmer and Schady (2008).

7 Conclusions

This study provides an insight on the impact of three types of school feeding program 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. Enrollment continued to increase at a slower pace after the first year, hinting to potential resource constraints. If the program attracts more children, but the school resources remain fix, this might lead to deterioration in the student-teacher ratios and class size, which in turn might impair learning. Also, children who were already attending school may suffer negative peer effects from lower ability children joining school. Beyond enrollment, the intervention increased also the probability of being in school after one or two years. But in most cases it did not lead to higher education achievements which might again suggest a negative countervailing class size effect.

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 by Acemoglu (2010) for a case very similar to the instance we are looking at. The authors show that a simple model of the relation between cost of schooling and investments in education, and the relative reduced-form estimations, will not be informative about counterfactuals involving large-scale interventions in the presence of constraints on individual choice. One such constraint can be given for example by school size, which we suspect to be present in this case. In fact, we see the large first-year impacts fade out when the intervention is expanded over time. This consideration, together with the fact that the intervention is a natural experiment and as such it lacks the full strength of randomization, should lead to interpret our estimates with caution.

An alternative explanation is however that the time horizon after the program's implementation is too short to find any effect on the total duration of education. The FFE program seems to have attracted also many overaged children, who boosted the school enrollment figures especially in the 4th to 6th grade (extensive margin), but for some reasons (perhaps due to their incomplete school records) did not stay in school to increase their total completed education (intensive margin). It will be possible to learn more about this outcome by looking at later data, which have become recently available.

Keeping in mind 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. However, it seems to have reached the intended aim of increasing schooling outcomes for girls.

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 had also long-term effects on wages and employment. These are open questions for future research.

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