

School Meals, Educational Achievement and School Competition: Evidence from a Randomized Evaluation

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Abstract

This paper examines the effects of subsidized school meals on school participation, educational achievement, and school finance in a developing country setting. We use data from a program that was implemented in 25 randomly chosen preschools in a pool of 50. Children's school participation was 30 percent higher in the treatment group than in the comparison group. The meals program led to higher curriculum test scores, but only in schools where the teacher was relatively experienced prior to the program. The school meals displaced teaching time and led to larger class sizes. Despite improved incentives, teacher absenteeism remained at a high level of 30 percent. Treatment schools raised their fees, and comparison schools close to treatment schools decreased their fees. Some of the price effects are due to a combination of capacity constraints and pupil transfers that would not happen if the school meals were offered in all schools. The intention-to-treat estimator of the effect of the randomized program incorporates those price effects, and therefore it should be considered a lower bound on the effect of generalized school meals. This insight on price effects generalizes to other randomized program evaluations.

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1 Introduction

High rates of child absenteeism from school are a serious impediment to economic growth in the developing world. In 2000, an estimated 88 million children were out of school, most of them in Southern Asia and Africa.¹ One of the measures that has been taken by governments in countries like India, Bangladesh, Brazil, Swaziland or Jamaica to encourage school attendance is the provision of government-subsidized school meals.² But despite the popularity and cost of school meals, there is little evidence on their impact on school participation and educational achievement. In this paper, I quantify the effects of subsidized school meals on these outcomes using data from a randomized school meals program in preschools in Western Kenya. The program was implemented in 25 preschools between 2000 and 2002, and provided a fully subsidized in-school breakfast on every school day to all pupils attending preschool. The 25 schools that received the school meals program were randomly chosen from a pool of 50 schools, which allows causal inferences to be drawn.

We first estimate the impact of the breakfast program on school participation using an intention-to-treat estimator. The intention-to-treat estimator assigns children to the treatment or comparison group before the start of the program. Presence in any school is counted and assigned to the original treatment status of the child. In the presence of transfers between schools, this estimator is appropriate because it avoids upward bias in the following way: children who transfer from a comparison school to a treatment school are not counted as new enrollments in the treatment school and not counted as absences in the comparison school. Similarly, children who transfer from outside the sample schools are not counted. Children in the treatment group participated in school 35.9 percent of the time, versus 27.4 percent in the comparison group. The difference is slightly less than one-third of the level in the comparison group. The program increased participation of both children who were previously enrolled (intensive margin) and children who would not have gone to school in absence of the program (extensive margin).

The second result of the paper is that the program improved learning, but only for children in schools where the teachers were more experienced at the onset of the program. The difference in test scores is approximately 0.4 of a standard deviation per standard deviation of teacher experience. The improvement in test scores for this group of children was in the area of curricular achievement. There was no impact on cognitive abilities. The program also benefited boys in terms of weight, but there was no effect on girls (height or weight) or on boys' height. Hence school meals affect learning in schools but the channel through which they do so is unlikely to be nutrition, otherwise

¹From the World Education Report (2000), Unesco.

²From the Global School Feeding Report (2002), World Food Program.

we would find improvements in cognitive abilities. What seems crucial is that the children who had better scores attended school more often and had a teacher with more experience.

The school meals program had a variety of other effects. The increased participation rates in treatment schools created crowding in the classroom, and the pupil-teacher ratio increased substantially. The higher pupil-teacher ratios were not offset by more days of teaching. Both in treatment and in comparison schools, teachers were absent 30 percent of the time. In fact, there is evidence that the total number of hours of teaching in the treatment schools decreased. The meal program displaced teaching time by approximately 15 percent, even though the meals were prepared by a cook who was specifically hired so the meals would not take too much time from the teacher. The program also had a spillover effect on the organization of the comparison schools. During the implementation of the program, over half of the comparison schools started implementing feeding programs of their own, which were not subsidized but funded by the parents. Since the meals were funded by the parents, it is not clear how this would affect the estimate of the effect of the program on school participation. Further data collection will allow us to determine whether this was due to geographical proximity to the treatment schools.

The program had important implications for the finances of the preschools and on competition between the schools, which in turn is important for the interpretation of the evaluation results. Kenyan preschools are not subsidized by the government, but raise most of their funds through school fees paid by the parents. After the start of the program, the effective price of a day in preschool was approximately 60 percent higher in treatment schools than in comparison schools. In addition, comparison schools located close to a treatment school significantly decreased their prices. The increase in payments in treatment schools was approximately 10 percent of the cost of the food, and most of the additional funds collected were paid to the teachers in the form of higher salaries. Some of the price changes that were observed in this program were related to the fact that not all schools in the area received the NGO aid, which resulted in competitive distortions in the preschool market. In particular, some children who had a choice of school transferred into the treatment schools and out of comparison schools, driving up the price in treatment schools and down the price in comparison schools close to the treatment schools. If school meals were offered globally, we would expect prices in treatment schools to be lower and prices in comparison schools to be higher than under the randomized program because there would be fewer transfers. Therefore the difference in school participation rates between the two types of schools in the randomized program is a lower bound on the difference in attendance rates between a situation with no school feeding and general school feeding.

The insight that competition between randomization units matters is a complement to the

literature on general equilibrium effects of interventions³ and the literature on the contamination of comparison groups in randomized evaluations.⁴ Heckman et al.(1998) write “when interventions have general equilibrium consequences, these [treatment] effects depend on who else is treated and the market interaction between the treated and the untreated.”⁵ While the literature stresses the importance of *including* the general equilibrium effects and externalities of an intervention into its effectiveness appraisal, we stress the importance of *excluding* effects that are specific to the randomization setup and would not carry through in a generalized intervention.

The findings in this paper also illustrate the trade-off between power and contamination/externalities in the design of randomized evaluations. On the one hand, the power of a randomized experiment in detecting a change in the dependent variable depends on the number of randomization units (e.g. 100 districts). Taking a smaller randomization unit for a given sample size (e.g. randomizing on the level of the school instead of on the level of the district, but including all schools in the 100 districts in the sample) increases the power of the experiment. However, smaller randomization units make it more likely that the comparison units will be contaminated.⁶ For example, Miguel and Kremer (2002) find that their estimates of the effects of deworming on school participation when treatment is randomized on the school level, are larger than in other studies where treatment is randomized within the schools. Even when treatment is randomized on the school level, they find that the difference estimate (between schools) is a lower bound because there are externalities from the treatment schools onto other schools. In the preschool meals program, the externalities do not run through a physical mechanism like in the case of the transmission of worms, but through economic competition. Of course, one could always randomize on a higher level, but cost considerations usually restrict this kind of expansion.

The next section briefly outlines the debates on the effects of school meals. Section 3 gives some background information on preschool education in Kenya and on the design of the program. Section 4 explains the identification strategy. Section 5 gives the econometric model and reports the results. Section 6 discusses price effects and their implications for the interpretation of the results. Section 7 discusses possible alternatives to school meals and concludes.

³See for example Heckman, Lochner and Taber (1998)

⁴See for example Miguel and Kremer (2001)

⁵The direction of the discrepancy very much depends on the particular setting of the intervention. For example, Heckman et al. (1998) simulate the effect of a job training program, incorporating general equilibrium effects on the returns to skills if the general training level increases, as well as distortion effects from the taxes that need to be raised to finance the program. They find that the general equilibrium impacts of tuition on college enrollment are smaller than the impacts found in microeconomic studies. By contrast, studies on for example, the impact of medical treatment, stress that the intention-to-treat estimator is a downward biased estimator of the impact of the intervention because it fails to take into account the positive externalities associated with the treatment.

⁶Randomization on smaller units also leads to ethical problems. For example, it would have been impossible to randomize school meals on the level of the family or on the level of the child.

2 Two debates on the effects of school meals

Proponents of school meals have argued that school meals can foster educational achievement, thereby increasing people's earnings potential and boosting economic growth. In answering the question of exactly how school meals can foster educational achievement, there have been two major lines of argument. The older argument is that school meals increase educational achievement by improving child nutrition. The second, more recent argument, is that school meals provide incentives for families to send their children to school.

The argument that school meals improve educational achievement through nutrition relies on two links: first, that school meals improve nutrition; and second, that improved nutrition leads to better educational achievement. Most evidence on these two links is non-experimental, though there are some experimental studies on the link between school meals and nutrition.⁷ Since child nutrition, child health and schooling reflect household preferences in human capital investments in the child, they might be correlated without any direct causal relationship between them. The evidence on the effect of school meals on nutrition is mixed. The counter-argument to the effectiveness of school meals at improving children's nutritional status is that families adjust to school meals by reducing resources allocated to children who benefit from the school meals, transferring them to other members of the households.⁸ A few economic studies have tried to infer causal relationships between childhood nutrition and educational achievement by using longitudinal data or various exogenous shocks to nutrition as identifiers.⁹ While these studies document a positive relation between the two factors, they primarily document the effects of early childhood nutrition, this is nutrition before a child reaches 18 months. Since school meals take place in schools, they do not reach children under the age of 18 months.

The more recent argument on school participation is a more promising motivation for funding for school meals. Casual observations from international organizations like the World Food Program suggest that subsidized school meals attract a high number of additional children to school.¹⁰ However, it is hard to draw causal inferences or estimate the magnitude of the effect from these observations. Schools that receive subsidies towards school meals tend to be chosen because they are disadvantaged, which makes it hard to compare them to schools that do not receive such subsidies. Similarly, schools that run feeding programs that are paid for by the pupils' parents will

⁷See for example Jacoby et al. (1996), Grantham-McGregor et al. (1997), Powell et al. (1998) for experimental evidence and Grantham-McGregor et al. (1998), Chandler and Walker (1995), DelRosso (1999) for non-experimental evidence.

⁸See for example Jacoby (2002), Long (1991) and Powell (1983).

⁹See for example Glewwe and Jacoby (1995), Glewwe, Jacoby and King (2001), Alderman et al. (1997) and Behrman and Lavy (1997).

¹⁰Global School Feeding Report (2002), WFP.

be intrinsically different from schools where the parents do not fund such a program.

3 Background and Program Design

In 1997, about 770,000 Kenyan children, 30 percent of the 4-6 age group, attended preschool centers.¹¹ About three quarters of the country's approximately 25,000 preschool centers are informal non-profit centers that are funded by the parents but located on the compound of a government-subsidized primary school¹². The remaining one quarter of Kenya's preschools are run by religious organizations, companies, NGOs and private for-profit providers. This paper will be concerned only with informal preschools in rural areas. The Kenyan Government developed a preschool curriculum and provides teacher training, but does not otherwise subsidize preschools. Parents pay fees that fund the teacher's salary, classroom materials and, in some cases, a feeding program. Funds for building a classroom are usually raised during a community fund-raiser.¹³

The amount of school fees paid by the parents depends on a negotiation process between the preschool teacher and the parents. Most schools do have an official preschool fee per term, but this official fee has little practical significance. In 2000, of all children that were found in the 50 program preschools at least once, only 3.5 percent had paid the official amount of fees due or more. Forty-six percent of children had not paid anything. Parents can enroll their child in preschool by recording the child's name in the teacher's book. After enrollment, fees are collected in a staggered way. A preschool child whose parents have not paid "enough" fees but who comes to school can be sent home by the teacher and be told to tell her parents to pay fees before coming back to school. As a result, there is a strong correlation between school participation and the amount paid by the parents, and parents pay on an approximate fee-for-service basis.¹⁴

Rural preschools usually have a low level of total funding, though this varies with the income level of the community and parents' valuation of preschool education. Equipment is minimal; preschool teachers have few teaching materials and classes are often held under a tree for lack of classrooms. Being a preschool teacher is a semi-volunteer position. A 1997 survey of 100 preschools

¹¹Approximate numbers calculated from data reported in the Welfare Measurement Survey III (1997), Kenya Central Bureau of Statistics.

¹²Total number of preschools is from Kenya Ministry of Education, Science and Technology (1999), data on organization are from Kipkorir and Njenga (1997).

¹³For a description of how community fund-raisers work, see Miguel and Gugerty (2002)

¹⁴For a given level of participation, the amount of fees collected varies substantially. Since preschool teachers are local women, they often know the families of the children attending their class. As a result, they have a pretty good idea of the economic background of the families, and it should be possible for them to price-discriminate between families. I will not discuss such price discrimination in this paper.

in the same area¹⁵ found that, on average, preschool teachers earned 45 dollars per year.¹⁶ Teachers supplement their income with work on their farms or elsewhere. Feeding programs in preschools are quite rare. Prior to the introduction of the feeding program evaluated in this paper, only 2 schools in the sample of 50 ran a snack program of their own.

Teachers usually have low levels of formal education and professional training, though they tend to be better educated than the average woman. The teachers in the 50 preschools involved in the school meals program had completed 10.6 years of formal education on average. All teachers in the 50 schools were able to read and write, while only 47 percent of adult women in the district at large were able to read.¹⁷ At the start of the school meals program, forty-two percent of the teachers had attended the training program organized by the Kenyan national institute for early childhood education. This training consists of five three-week residential sessions during the school holidays over a two-year period, with in-service training and follow-up during the school terms.¹⁸ The candidates are required to have completed primary school, but the training is subsidized and offered in various places throughout the country. Other possibilities for training are the short, 5-week training sessions organized by local governmental organizations. Admission criteria for these trainings are less strict. Both kinds of training offer instruction in early childhood development and health, classroom materials development and preschool center management. In addition to the government, various non-governmental organizations are involved in the promotion of preschool education and also offer training.

Classes run for only a few hours per day, usually in the morning between 8.30 am and 12.00 am. An average class has an enrollment of 85 pupils but only 35 pupils attend on a typical day. The average pupil-teacher ratio is 27. Absenteeism rates are very high. Enrolled children are absent over half of the time, while teachers are absent approximately 30 percent of the time. Both the range of classroom activities and the quality of the teacher vary a lot by school. In better preschools, the teacher is dedicated to her class and classroom activities include learning the alphabet and numbers, singing, dance, and sports. At the lower end of the quality spectrum, teachers are not very motivated or trained, and the preschool class is a place where children are provided with a relatively safe environment and supervision, but few activities that promote their development. Those schools offer little more than baby-sitting services.

We evaluate a school meals program that was implemented from 2000 to 2002 by a Dutch

¹⁵The 50 schools from the school meals program are included in this sample.

¹⁶The Kenyan GDP per capita was approx. 350 dollars in 1997.

¹⁷Kenya Central Bureau of Statistics, Welfare Monitoring Survey II (1994)

¹⁸The national institute is the National Center for Early Childhood Education (NACECE). The curriculum for the training was developed jointly by the Kenya Institute of Education, the Van Leer Foundation, and the Aga Khan Foundation. The training is part of a World Bank project.

NGO, Internationaal Christelijk Steunfonds (ICS), in Busia and Teso districts in Western Kenya. The districts count approximately 350 preschools. Busia and Teso districts are poor and have low educational achievement by Kenyan standards.¹⁹ In 1994, annual per capita income in those districts was only 48 percent of the national average. In 1997, 51 percent of people in the districts were living in “hard-core poverty”.²⁰ Malnutrition and hunger are highly prevalent in the districts. Nutrition indicators from 1994 show that 39 percent of children were stunted and 9 percent of children were wasted.²¹

ICS organized and funded the provision of a school breakfast in 25 schools, randomly chosen from a pool of 50 schools.(Figure 1) The breakfast consisted of a cup of porridge made from a protein-rich flour mixture, sugar, corn oil and water. One 500 ml serving has a nutrition value of 422 calories. Porridge is a common, culturally appropriate meal for both adults and children in Kenya. The variety served by ICS was sweeter, contained more fat, and was more nutritious than the average porridge served for breakfast in homes. ICS provided ingredients according to the number of children enrolled in the preschool class. Breakfast was prepared in the school on every school day by a cook under supervision of a parent representative. It was served to the preschool pupils, their teacher(s) and the parent representative. Primary school pupils and teachers were not included in the feeding.

The organization of the program was split between ICS, the schools, the cook and the parents. ICS provided the food, the transportation of the food to the school compound, the cooking utensils, hired a local woman as a cook in every school and paid her a salary. ICS field workers visited the schools at least once per term for monitoring purposes. The schools were responsible for providing a storage facility for the food and a proper cooking area, separate from the preschool classroom. Parents were responsible for supplying firewood for cooking the porridge and a cup for each child. In each school, a Feeding Committee was elected by the parents to oversee the daily implementation of the program and check the preschool teacher’s attendance register. All positions in the breakfast program except the cook’s were voluntary, though the cook, the parent representative and the teacher were allowed to eat porridge. ICS created a paid position for cooks to prevent the feeding from taking too much time from the teachers and to prevent girls from higher grades being called from their classes to prepare meals for the preschoolers.

¹⁹Data in this paragraph are from the Welfare Monitoring Survey II (1994) and III (1997), both published by the Kenya Central Bureau of Statistics.

²⁰The hard-core poor are defined as those who would not meet the minimum food calorie requirements even if they concentrated all their spending on food. In this case the rural hard-core poor are those whose total expenditure is less than KShs 927 (approx. 11.5 USD) per equivalent adult per month.

²¹Children are stunted (wasted) if their height (weight) Z-scores fall below two standard deviations from the median of the reference population.

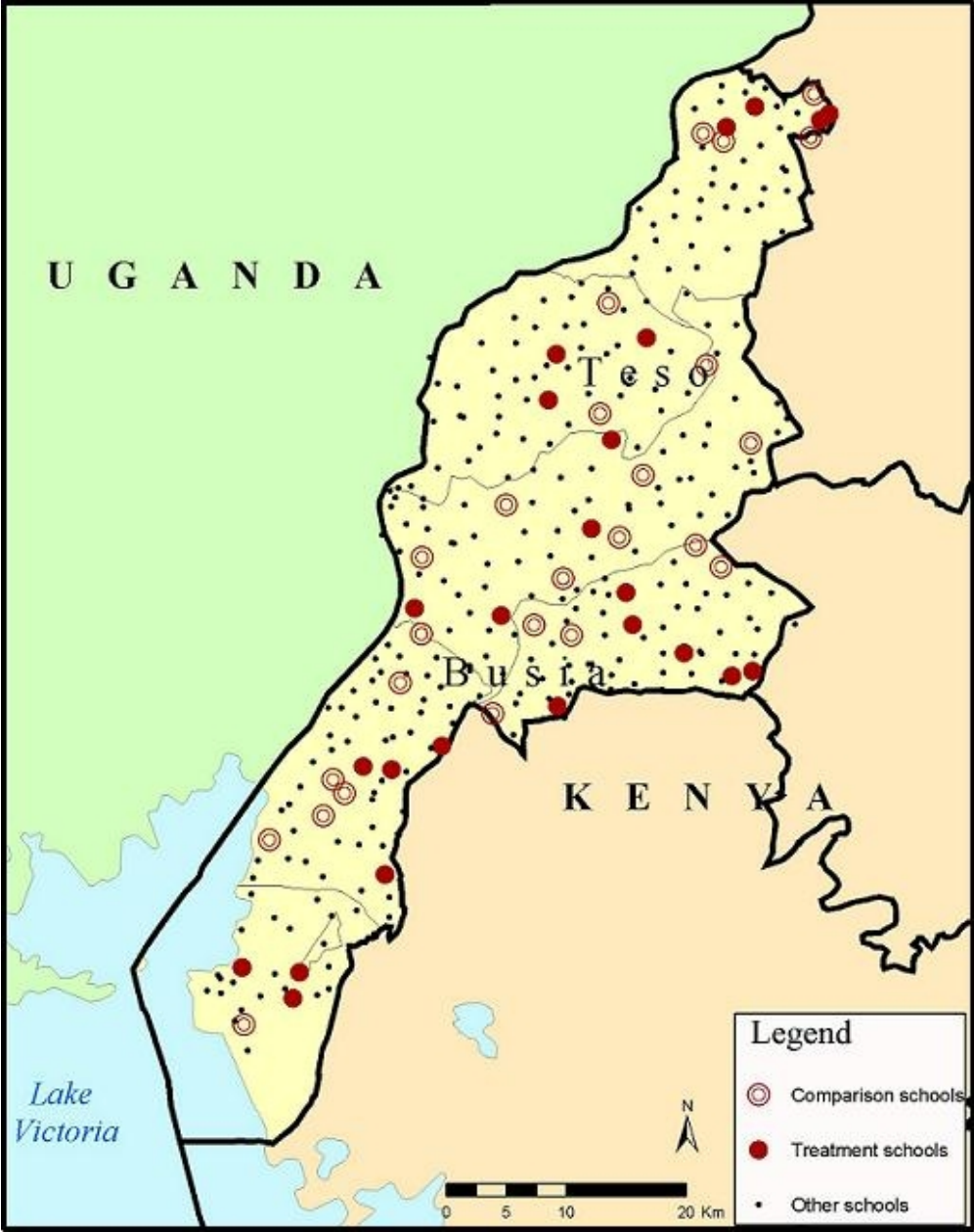


Figure 1: Location of the treatment and comparison schools

The 50 schools involved in the school meals program participated in an earlier preschool program run by the same NGO. The earlier program provided teacher incentive bonuses, teacher training and classroom teaching materials. The teacher incentive bonuses were meant to be a reward for teachers who came to school regularly. Throughout the school meals program, all 50 preschools continued to receive bonuses and classroom materials to ensure that the teachers would cooperate and keep records of the children who attended the preschool, their fee payment and official enrollment. From 1997 through 2000, the NGO also sponsored training of new teachers at the 5-week courses offered by the local government organization. So the level of training in these schools is relatively high. The internal validity of the results is not affected by the earlier program or by the provision of the additional inputs, because treatment and comparison schools received the same inputs. It is unlikely that the inputs seriously jeopardized the external validity of the program, because the schools and the districts where the program was implemented were relatively poor by Kenyan and East-African standards. In addition, the provision of these inputs had a very limited effect on the quality of teaching in the schools, and it did not affect school participation (cf. Chen and Kremer(2002)).

The timeline of the school meals program is described in Table 1. In January 2000, the 50 schools were randomly divided into a treatment group and a comparison group of 25 schools each. The schools were first stratified by geographic location and by their participation in other projects implemented by ICS. They were then grouped into pairs with similar characteristics. For each pair, a coin was flipped to decide which of the two schools would be the treatment school. The experiment was not blind. Both schools and NGO field staff were aware of the status of the schools. The intervention was explained to the school communities at parent-teacher meetings and the program inputs were easily observable. Prior to the start of the school meals program, treatment and comparison schools were similar on most dimensions (Table 2). They had similar levels of enrollment, preschool fees, infrastructure, classroom equipment and number of preschool teachers. In addition, preschool teachers in the treatment and comparison groups did not differ significantly in terms of quality, experience, education and professional training.

The amount of food delivered to each school depended on the number of children enrolled in the preschool. There are many reasons why the average quantity of food served to a child who attended school differed from the standard serving and also differed by school. There were differences in the ratio of official enrollment to school participation between schools, potential differences in graft rates, etc. In general, the quantity of food delivered to the schools per pupil present was larger than the quantity originally intended. On average, the NGO delivered 37 percent more than what was needed according to the recipe, and surpluses were larger in 2001 than in

Table 1: Preschool breakfast program timeline

Date	Activity
2000	
February-March	Baseline survey
January-March	School attendance check in preschools and primary grades 1-8
March-April	Parent-teacher meetings to announce the program in all schools
April	Start of the implementation of the breakfast program
May-November	School attendance checks in preschools (6 rounds)
2001	
January-February	School attendance check in preschools and primary grades 1-8
February-November	School attendance checks in preschools (7 rounds) and grade 1 (1 round)
November	Snack programs survey in comparison schools
2002	
January-March	School attendance checks in preschools, grades 1-2 (2 rounds)
February-March	Cognitive and attainment tests, anthropometric measurements
May-July	Elder siblings survey, socio-economic survey
	School attendance checks in preschools and primary grades 1-2 (2 rounds)
June-July	Parent-teacher meetings in 25 feeding schools, announcement of cost sharing
September-December	Introduction of cost sharing, attendance checks in preschools, grades 1-2

Table 2: Summary statistics

Variable	All	Treatm	Comp	t-stat	nobs
Pre-treatment					
<i>School-level variables</i>					
Enrollment, Primary grades 1-8 (1)	313.3	331.9	294.6	0.88	50
Preschool enrollment (1)	55.0	55.2	54.8	0.05	50
Preschool avg participation (1999)	33.2	33.6	32.8	0.16	50
No. of children in baseline	205.7	204.9	206.5	-0.08	50
Fee charged / pupil / term (2)	163.0	157.0	169.0	-0.44	49
Total fees collected, 1 year (3)	5530	5109	5952	-0.69	50
Ethno-linguistic fractionalization index (4)	0.24	0.23	0.25	-0.38	50
<i>Classroom equipment (as of term 1, 2000)</i>					
Structure Usable in Rain?	0.52	0.60	0.44	1.12	50
Blackboard	0.86	0.80	0.92	-1.22	50
Storybook(s) in Kiswahili	0.06	0.04	0.08	-0.59	50
Storybook(s) in local language	0.66	0.68	0.64	0.29	50
Alphabet chart	0.96	1.00	0.92	1.44	50
Number chart	0.96	1.00	0.92	1.44	50
<i>Teachers</i>					
Number of preschool teachers	1.48	1.40	1.56	-0.92	50
Quality of nursery teachers (5)	1.89	1.83	1.96	-0.64	50
Experience in years (6)	5.59	5.29	5.87	-0.56	74
Education in years (6)	10.55	10.31	10.77	-1.13	74
Any training or seminars (6)	0.68	0.66	0.69	-0.30	74
2-year ECD training (6)	0.34	0.31	0.36	-0.39	74
<i>Baseline children</i>					
Age (6)	3.74	3.76	3.72	0.49	9244
Female (6)	0.50	0.51	0.49	2.01	10220
Standard of sibling (6)	4.90	4.96	4.83	0.90	8114
Female head of household (6)	0.10	0.12	0.08	2.06	10284
Enrolled in preschool in February 2000 (6)	0.41	0.41	0.40	0.27	10284
Post-treatment					
Nr of partic. children who had breakfast at home	9.32	8.80	9.84	-0.69	521
Ratio of partic. children who had breakfast at home	0.22	0.17	0.26	-3.24	518
Non-salary expenditures in preschool (7)	217.3	357	185.6	0.78	50
Non-salary expend. as % of total fees collected (7)	3.38	5.75	1.32	1.59	43

Table 3: Summary statistics (Continued)

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- (1) Most recent as of 2000
 - (2) Official amount charged as of term 1, 2000
 - (3) Terms 2-3, 1999 and term 1, 2000
 - (4) Ethnic diversity is an index of ethno-linguistic fractionalization in all schools within 5 km of the school to whose baseline the child belongs. Following Miguel and Gugerty(2002), it is defined as $1 - \sum_i \pi_i^2$ where π_i is the proportion of ethno-linguistic group i .
 - (5) Assessment by ICS field staff during a 1999 visit, scale: 1= good, 2=average, 3=poor
 - (6) Standard errors are clustered at the school level
 - (7) Non-salary expenditures incl. construction, desks, blackboard, books, etc.
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2000. To the extent that the dummy for treatment that is used in the analysis does not capture the variation in serving size, the estimates will suffer from attenuation bias.

4 Identification strategy

The program’s randomized design is the most important source of identification. In the absence of transfers between schools, a pupil’s treatment status is not correlated with her observed or unobserved characteristics. However, the project might have attracted pupils who otherwise would have attended another school. Counting those pupils as new participants would bias the estimates upwards. To avoid this bias, we define a set of pupils whose composition is exogenous to the feeding program.

4.1 Baseline Survey

Randomized evaluations of programs in primary schools have commonly used a set pupils enrolled prior to the program as their sample (e.g. Miguel and Kremer (2001)). For several reasons, this approach is not feasible in preschools. First, the vast majority of preschool age children in Kenya never attend preschool, therefore we would pick up the effects of the program on a very select set of the population of preschool age children. Second, while we would pick up effects of the program on outcomes conditional on school enrollment, we would not capture any effects of the program on the decision to enroll in school.

The ideal sample for the evaluation of the preschool feeding program consists of all targeted children rather than of children who were enrolled before the start of the program. The targeted population of the intervention is the set of children between 4 and 6 who live within walking

distance of the school²² and whose parents would have chosen that particular school for their child if they had sent their child to school in absence of the program. Walking distance for preschool age children is approximately 4 km. Note that, since all enrolled children walk to school, they must belong to the set of targeted children. Characterizing the targeted population is difficult for two reasons. First, there is no household registry with records of where people live, hence there is no ready list of which children live within 4 km of the school. Second, even if there were such a list, one would still not know which school the parents would have chosen to send their children to, since most households live within 4 km of several schools.²³

This evaluation uses the following approach to characterize the targeted population. My basic assumption is that a household's choice of schools for its children is very correlated between the children. For example, we assume that a household's school choice for their 10-year old child is a good predictor of their school choice for their preschooler. If households have many children, if the probability that at least one child in the household goes to school is sufficiently large, and if all children are bound to go to school within 4 km of their home because they have to go to school on foot, then a baseline survey of all siblings age 4 to 6 of all children currently in the school will give a reasonable approximation of the targeted population.

ICS collected such a baseline survey prior to the program. The NGO first collected a list of all families that had at least one child in the preschool or in the associated primary school. Subsequently, the oldest child was interviewed and asked for the names and ages of her siblings²⁴ below the age of seven. The list of all families in a school, including the names of the parents, the names of the oldest child in school and the names and ages of all children below age 7 constitutes that school's baseline survey. To minimize the school's incentives to distort baseline information, all baseline surveys were collected prior to the announcement of the randomization results to the schools. In addition, the schools were informed that the baseline survey information would not be used in determining the amount of food they would receive in case they became a treatment school. Prior to the start of the program, the households and children in the baselines in treatment and comparison schools were similar on most dimensions (Table 2).

The results presented in this paper are internally valid for children belonging to the baseline.

²²Preschool and primary school children in rural Busia and Teso go to school on foot. Bicycles are used by teachers and secondary school children. Cars or busses are not used as a mode of transportation in this context.

²³The Kenyan system of financing school buildings and teachers provided strong incentives for building many schools. For a discussion of primary school finance, see Kremer, Moulin and Namunyu (2001). For a discussion of pupil sorting between schools, see Miguel and Gugerty (2002).

²⁴In the remainder of this paper, children living in the same compound will be called siblings, even though they are not necessarily biological brothers and sisters. During field work, the older children were asked for the names of all children "eating from the same kitchen", which is a common expression in Kenya and avoids the difficulties involved in identifying parents in polygamous households.

In addition, the results are likely to be also valid for children who had an older sibling in school but whom the older sibling forgot to list at the time of the interview, and to children who had a sibling in school but whose household was omitted during the enumeration. Those children share the most important characteristic of baseline children, which is that they were either in school themselves or that they had an older sibling in school. Therefore it is reasonable to assume that their elasticity of preschool attendance with respect to preschool conditions was similar to the elasticity for children in the baseline. Children who were not in school themselves and did not have an older sibling in school might have a different elasticity of preschool attendance, so the effects of the school meals program could have been different for them²⁵.

4.2 Measuring school participation

The first outcome of interest is school participation. In this paper, a child is defined to be participating in school on a given day if she is present in school on that day. School participation can quite different from official enrollment. For example, children might be enrolled in school but miss class because they are ill. Since we are interested in the amount of time children spend in school, participation is the right outcome to look at. If the program influences the probability that a child will show up in school given that she is enrolled, for example because it affects the probability that a child is ill, the change in enrollment will be a biased estimator of the change in school participation. In the Kenyan context, there is an additional reason why one should not trust enrollment data as indicators of school participation. As described in section 3, even when a child is enrolled in school, the teacher has a lot of discretion in deciding whether to keep a child in class when the parents have not paid “enough.” This makes enrollment a very noisy measure of school participation.

To circumvent these problems, this evaluation uses direct observations of school participation rather than measures of official enrollment. The NGO field staff visited the schools at least once per school term and observed and recorded pupil and teacher attendance, and conducted interviews with pupils and teachers. Attendance checks were done through name calls rather than headcounts, which allowed the field staff to match school participants to the baseline of the school. When the field staff found a child in class who was not yet on their list of existing participants for that school, they asked the child, the teacher and/or one of the child’s older siblings for the name of her parent, and tried to find the parent in the baseline.

²⁵The 1999 Kenya Census data will, when available, enable us to estimate the probability that a child does not have an older sibling in school and is not enrolled in school herself. This will allow us to assess how representative the baseline is of the general population.

School participation was observed repeatedly over 2000 and 2001. Prior to the implementation of the program, there was one round of school participation checks in all preschools²⁶. After the start of the program, there were five rounds of visits in 2000 and eight rounds in 2001. The NGO field staff visited the schools unannounced and in different order at each round of visits. Two of the 2001 rounds of visits included school participation checks in grade 1 of primary school. Though each round was meant to include all schools, in practice some schools were missed²⁷. The average number of actual attendance checks in preschool is 5.75 in 2000 and 6.12 in 2001, and the difference between treatment and comparison schools is not statistically significant²⁸.

We use an intention-to-treat estimator to estimate the effect of the program on school participation. We define the set of children that the program intends to treat as the baseline children, and we define the treatment as the availability of free breakfast at the school of a child's older sibling. As explained before, school participants were systematically matched to their school's baseline. Therefore if no school participant was matched to a particular baseline child, we assume that baseline child did not attend school. The school participation measure that we use does not make any distinction between length of absence from school or reason for absence. Children who are temporarily absent from school and children who permanently drop out of school are counted as absent. This makes sense because the line between prolonged absenteeism and definitive dropout is subjective. A child that has dropped out of preschool is very likely to return to school later on, either to the preschool or to primary school, so in that sense most absences are temporary. In addition, most reasons for absences, e.g. being sent home for fees, illness, lack of motivation, are potentially endogenous to the treatment. To avoid bias, we treat all absences as potentially endogenous to the program.

Matching school participants to the schools' baselines required considerable amounts of patience from field staff, teachers and children. If anything, matching was harder in treatment schools because class sizes were bigger. Matching errors occurring more often in treatment schools would tend to bias the school participation results downward. There were additional difficulties in matching school participants to the baseline. For example, Kenyan children frequently change names and families.²⁹ Therefore it is likely that a number of school participants were not matched

²⁶The pre-program school attendance visit was conducted concurrently with the baseline survey, though by different field teams. In some schools, the pre-program visit was conducted after the announcement of the program, though the actual program started at least one month later. Biases arising from this are likely to be downward.

²⁷Reasons for missed visits include a national strike, floods and NGO vehicle failures. In addition, sometimes visits were missed because a whole class or school had gone to inter-school games or meetings. Though such occurrences were school specific, they could not be avoided because all visits were unannounced. These schools were revisited later in the term if time permitted.

²⁸The treatment schools were visited one additional time at the beginning of each term, for the delivery of the food, but school attendance was not recorded during those visits.

²⁹It is common to have a specific childhood name, often associated with circumstances of birth. These names often

to the baseline, because they were listed in the baseline either under a different name or under a different family. This implies that matching was imperfect and that the total enrollment rates for preschools from this study are biased downward for baseline children. However, there is no reason to think that name changes affected schools disproportionately depending on their treatment status, so this problem is unlikely to affect the ITT estimator.

In 2000, 3176 out of 4243 (74.9 percent) school participants were matched to a child in the baseline. An additional 337 pupils (7.9 percent) were living in a baseline household but could not be found in the baseline. These children were either forgotten during the baseline enumeration, or they joined the household of the baseline parent after the start of the program. The children who were found in school but were matched neither to a baseline child nor to a baseline household (17.2 percent of those found in school) can be children from families that were “forgotten” during the baseline enumeration or children whose household did not belong to the school’s catchment area but transferred to the school. In 2001, the overlap between the baseline and the set of pupils found in school is smaller than in 2000. Just 2988 pupils out of 6333 (47 percent) school participants were matched to a child in the baseline, while an additional 974 (15.4 percent) were matched to a baseline household but not to a baseline child. The decrease in overlap is not especially worrying, since the unmatched children will include a new cohort of children who are the eldest in their families, as well as the 2000 cohort of eldest children, and the siblings of the 2000 cohort of oldest siblings.³⁰

4.3 Transfers of baseline children

In Busia and Teso districts, the potential for transfers between schools is high because of the high density of schools. A primary school has on average 7.9 other schools within 4 km, while the number for preschools is slightly higher. This makes it quite easy for children to switch between schools, even though most children go to school on foot. Other projects in the same region³¹ find a rate of transfers of 6 percent per year for with primary schools. In this section, we investigate the magnitude and implications of transfers of baseline children. Section 6 elaborates on the implications of transfers of children who do not belong to any baseline.

There are three classes of transfers of baseline children that might bias the results in this

get dropped as children grow up. Children also change names for religious reasons, if they or their parents become saved Christians. It is also quite common for children to transfer between households. Due to cultural practice and to the very high incidence of deaths due to HIV/AIDS among young adults in Western Kenya, a high proportion of children live with a guardian who is not their biological parent. The 1998 DHS survey found that approximately 10 percent of Kenyan children did not live with either of their biological parents.

³⁰We will investigate the effect of new cohorts moving into the schools when the Kenya census data are available.

³¹See for example Miguel and Kremer (2001) or Glewwe, Kremer, Moulin and Zitzewitz (2000)

study: transfers between program schools³², transfers between program schools and non-program schools, and hidden transfers. A hidden transfer is the enrollment of a baseline child in a school that is not the school to whose baseline she belongs. Transfers between schools are fairly easy to control for with the intention-to-treat estimator, because the data are sufficiently informative. We can distinguish between children who were absent because they had transferred and children who were just absent that day. During school participation checks, the field staff read all names of children previously found in the school, and the teacher (or the children who were present in the class) informed whether the child was absent, had dropped out or had transferred to another school. Similarly, the field staff asked where the children who were new to school came from, whether they came from home (i.e. whether they were first-time enrollers), or whether they came from another school.

The first class of transfers are transfers of baseline children between program schools. For those children, the data are complete in the sense that we observe school participation over the whole period. The intention to treat estimator assigns children to the treatment status of the school to whose baseline they belong, regardless of whether the school they actually attended was a treatment school or a comparison school, and regardless of whether the school they attended first was their baseline school or not.

The second class of transfers are transfers of baseline children from a non-program school to a program school or vice versa. Since the NGO did not collect information on school participation in non-program schools, there are no data on a baseline child's attendance prior to her transfer into the program school, or, if she transferred out of the program school, after her transfer. Baseline children who transferred in or out during a particular year were included in the sample for that year. We assume that their school participation rate prior to the transfer into the program school or after the transfer out of the program school was the same as their school participation rate in the time they spent in the program school.

Overall rates of transfer between schools are quite low. In 2000 and 2001, fewer than 3 percent of baseline children aged 4 to 6 transferred out of the program schools to any school (Table 4). In addition, children who transferred were similar on most dimensions to children who did not transfer (Table 5). Children who transferred were more likely to be enrolled prior to the program, but this is not surprising since a child can only transfer if she was previously enrolled. There are no significant differences between treatment and comparison school baselines in terms of how many children transferred or in terms of the observable characteristics of the baseline children who

³²The program schools are the 50 schools that participated in this project, whether they were treatment or comparison schools. All other schools are called non-program schools.

Table 4: Probability of transferring out, by treatment status

Year	(1)	(2)	(3)	(4)
	2000	dProbit regressions		2001
Treatment	-0.003	-0.004	0.014	0.011
	-0.005	-0.004	(0.007)**	(0.005)**
Girl		-0.002		0
		-0.004		-0.003
Female head		0.002		0.009
		-0.007		-0.006
Nr children <6yrs in household (2000)		-0.004		-0.005
		(0.002)**		(0.001)***
Age 5		0.014		0.036
		(0.007)**		(0.011)***
Age 6		0.014		0.049
		(0.006)**		(0.011)***
Age 7		0.016		
		-0.032		
Observations	4378	4354	4402	4373
Predicted value comparison	0.02	0.02	0.02	0.02
Predicted value treatment	0.02	0.02	0.03	0.03
Mean comparison	0.02	0.02	0.02	0.02
Mean treatment	0.02	0.02	0.03	0.03

Notes:

The dependent variable is a discrete variable that takes value 1 if the child transferred during the relevant period, and 0 otherwise. The sample consists of all children age 4-6 in 2000. For 2000, the sample is further restricted to children who were found in school in 2000. For 2001, the sample is restricted to children who were found in school in 2001 or in 2000 and who did not transfer in 2000. Standard errors are clustered at the school level. The regression model is probit with clustering of the standard errors at the school level. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Table 5: Child characteristics by transfer status

	Non-transfers			Transfers			Difference	
	Comp	Treat	All (1)	Comp	Treat	All (2)	(1)-(2)	t-value
Girl	0.49	0.50	0.49	0.44	0.53	0.49	0.00	0.01
Female headed household	0.07	0.13	0.10	0.11	0.17	0.14	0.05	1.80
Number of children under 6	2.66	2.48	2.57	2.30	2.17	2.22	-0.35	-4.59
Enrolled in February 2000	0.46	0.45	0.45	0.99	0.91	0.94	0.49	17.27
Age	4.52	4.53	4.52	5.28	5.10	5.18	0.65	11.03
Nobs	2899	2824	5723	106	151	297	6020	

Notes:

Table reports percentage of observations in each category. The sample is the set of baseline pupils found in school in 2000 and 2001. Transfers are pupils who transferred in or out of the program schools, in 2000 or 2001. Non-transfers are school participants who did not transfer in 2000 or 2001.

transferred. Hence it is unlikely that transfers between schools bias the estimation results when the estimation is confined to the sample of baseline children.

The third class of transfers is hidden transfers. A hidden transfer is the enrollment of a baseline child in a non-program school, when that child has never attended a program school. This child would be counted as absent in my school participation data. If the feeding program affected the rate at which baseline children enroll in a non-program school, the estimates will be biased. The bias in the estimate of the school participation effect will be upward if children in the baselines of treatment schools were less likely to enroll in a non-program school than children in the baselines of treatment schools. To try to get at this problem, retrospective data were collected on the whereabouts of baseline children. In 2002, 40 households from each school were sampled and the NGO staff tried to identify and interview one child from each household. The child was asked about the whereabouts of the household's children who were recorded in the baseline. This information was not yet available at the time of writing.

5 Econometric modeling and results

We estimate the impact of the program on the average school participation rate of a child, on the probability of finding a child in school at least once, on height and weight and on teacher absenteeism. In section 6, we estimate the impact of the program on the price of preschool.

5.1 Average school participation

In this section, we report estimates of the effect of the program on a child’s average school participation. Average school participation is defined as the number of times the NGO staff found a child in school divided by the number of observations of presence for that child over the relevant period, so it is a number between 0 (the child never attended school) and 1 (the child was found in school at every round of visit). First, we average school participation within each school baseline, and we regress the school baseline average on the treatment status of the school (Table 6). We find that average school participation of treatment children was 8.5 percentage points higher than that of comparison children. The average school participation rate in comparison schools was 0.27, which implies that the difference between the treatment and comparison groups was slightly less than one third of the rate in the comparison group. Children who were not in school prior to the program were 4.6 percentage points more likely to be in school if they belonged to the treatment group as opposed to the comparison group. The difference for children who were in school prior to the program was 11 percentage points.

Second, we run regressions on the individual child level, which allows us to introduce child- and school-level controls. We allow for random effects at the school baseline level. However, average participation is not normally distributed, even after controlling for observables. Over half of the children aged 4-6 never attend school over the course of a year (cf. section 5.2), implying that a non-negligible number of observations are censored at zero. In addition, approximately 10 percent of the sample is censored at 1. For these reasons, we estimate the effect of the program on school participation using a two-limit tobit model with random effects at the school level³³. For 2000, we estimate the model for the whole sample of children in the relevant age group, and separately for the children who were in school before the program and children who were not.

The two-limit tobit corresponds with the intuition behind preschool participation. Families do not need to send their children to school on a permanent basis. As explained in section 3, when parents pay a higher proportion of the official preschool fees, the probability that their child will be sent home because they have not paid enough decreases, increasing the proportion of times that the child will actually be present in school. Hence the observed level of participation in school can

³³ An alternative approach to using school participation averaged over a year and the 2-limit tobit model is to consider every round of participation checks as a separate observation. However, this introduces two-level, hierarchical unobserved effects. There is an unobserved random effect on the level of the child and an unobserved random effect on the level of the school baseline, and the two effects are nested. The data contain one pre-program observation on school participation which would in principle allow me to use fixed effects on the child level. However, measures of school participation that are based on a single observation are very noisy. Experimental computer programs allowing for hierarchical random effects in discrete response models (e.g. Bates and Pinheiro’s program, incorporated into S-plus version 7) do not allow for sufficient sample sizes for this project.

Table 6: Average school participation

Pre-program status of the sample	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	All	Pre-program	All	2000	Not enrolled	2000	Enrolled	2000	Enrolled	2000	All	2001	All	2001
Treatment	0.001 (0.029)	0.085 (0.022)***	0.046 (0.009)***	0.073 (0.036)**	0.111 (0.033)***	0.039 (0.024)	0.055 (0.024)**	0.287 (0.059)***	0.499 (0.024)***	0.317 (0.017)***	0.20	0.05	0.24	0.24
Constant	0.377 (0.021)***	0.274 (0.015)***	0.009 (0.007)	0.298 (0.026)***	0.499 (0.024)***	0.317 (0.017)***	0.287 (0.059)***	0.499 (0.024)***	0.499 (0.024)***	0.317 (0.017)***	0.20	0.05	0.24	0.24
Observations	49	50	49	49	49	50	50	49	49	50	50	50	50	50
R-squared	0.00	0.24	0.35	0.08	0.20	0.05	0.24	0.08	0.20	0.05	0.20	0.05	0.24	0.24

Notes:
 The sample consists of children who were in the baselines of the sample schools and were in the relevant age group (4-6 years) and of children who were found in the schools in a pre-intervention visit in early 2000. The sample is further restricted as specified in each column head. There were at most 6 visits in 2000, and at most 7 visits in 2001. The regression in column (7) includes geographical dummy variables for each of the 7 administrative divisions. Participation was calculated as follows: first for each child in the sample average participation was computed over the relevant visits. Then for each school, the participation rates of the children were averaged. One school did not have a pre-intervention visit in early 2000. The regression model is OLS. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

be either 0 (the child never goes to school) or 1 (the child always goes to school) or anything in the 0 to 1 interval (the child goes to school some of the time).

Consider the following model of a latent variable \widetilde{y}_{ij} and the observed school participation rate y_{ij} :

$$\begin{aligned}\widetilde{y}_{ij} &= x_{ij}a + T_i\beta + v_i + u_{ij} \\ &\text{with} \\ y_{ij} &= 1 \text{ if } \widetilde{y}_{ij} > 1 \\ y_{ij} &= 0 \text{ if } \widetilde{y}_{ij} < 0 \\ y_{ij} &= \widetilde{y}_{ij} \text{ if } 0 < \widetilde{y}_{ij} < 1 \\ u_{ij}|x_{ij}, T_i, v_j &\sim \text{Normal}(0, \sigma_u^2) \\ v_j|x_{ij}, T_i &\sim \text{Normal}(0, \sigma_v^2)\end{aligned}$$

where T_i is a dummy for the treatment status of school i , x_{ij} is a vector of pre-program characteristics of child j in the baseline of school i and v_j is a school level random effect. \widetilde{y}_{ij} can be interpreted as the scaled net benefit of the household from the child attending preschool, before payment of the school fees. Assume Y_H^* is the level of fees that is charged to households who want their child to attend all of the time, while Y_L^* is the level of fees paid by households who want their child to attend ε of the time (say, one day in the year). Now take a parent's willingness to pay \widetilde{Y}_{ij} and scale it to $\widetilde{y}_{ij} = \frac{\widetilde{Y}_{ij} - Y_L^*}{Y_H^*}$. Then if $\widetilde{y}_{ij} < 0$, parents' willingness to pay is below the minimum level accepted by the teacher, and the child will not attend school. If $\widetilde{y}_{ij} > 1$, parents' willingness to pay is above the maximum level demanded by the teacher and the child will attend school the whole time. Observations with $\widetilde{Y}_{ij} < Y_L^*$ and $\widetilde{Y}_{ij} > Y_H^*$ will be left-, resp. right-censored. We do not observe \widetilde{Y}_{ij} but the result of the household decision process, i.e. the proportion of time y_{ij} that the child is found in school. This result is bound between 0 and 1, even though \widetilde{y}_{ij} is not.

For 2000, we estimate the change in school participation for baseline children who were between ages 4 and 7. Average school participation of children from comparison school baselines was 21.9 percent, versus 29 percent for treatment school baselines. This difference is one third of the average participation rate of comparison baseline children, and the difference is statistically significant at the 5 percent level (Table 7), column 1). Girls have lower rates of participation than boys, but the difference is not statistically significant. There is no evidence that participation rates depend on the number of children under age 6 in the household. Children from female headed households tend to attend more often, and attendance rates increase significantly as the children get older.

Table 7: Child level school participation

Sample restriction	2000		2001	
	(1) All	(2) Not present pre-program	(3) Present pre-program	(4) All
Treatment	0.159 (0.068)	0.203 (0.098)	0.141 (0.040)	0.111 (0.071)
Present pre-program	0.815 (0.029)			
Present pre-program * Treatment	0.008 (0.039)			
Girl	-0.018 (0.022)	-0.050 (0.037)	-0.001 (0.026)	0.006 (0.041)
Female head of household	0.057 (0.028)	0.088 (0.046)	0.010 (0.040)	-0.097 (0.059)
No of children <age 6 in household	-0.003 (0.008)	-0.006 (0.014)	0.007 (0.010)	-0.005 (0.009)
Age 5	0.257 (0.033)	0.381 (0.052)	0.073 (0.031)	0.492 (0.053)
Age 6	0.346 (0.029)	0.558 (0.051)	0.085 (0.035)	0.802 (0.043)
Age 7	0.709 (0.083)	1.251 (0.180)	0.140 (0.061)	
Nobs	5110	3898	1213	4794
Number of panels	50	50	49	50
sigma v	0.595	0.830	0.360	0.795
sigma u	0.177	0.263	0.084	0.198
Predicted value if comparison	0.110	0.006	0.488	0.013
Predicted value if treatment	0.191	0.020	0.629	0.028
Mean if comparison	0.218	0.140	0.507	0.171
Mean if treatment	0.290	0.164	0.647	0.203

The sample is the same as in Table 4a. School participation is averaged by child. The estimation model is two-sided tobit with random effects at the school level. Predicted participation is at the mean of the dependent variables. Pre-program presence is based on one observation of school participation in term 1, 2000, prior to the implementation of the program. A constant was included in the regressions but the estimates are not reported. Standard errors are in parentheses.

While the program had a positive effect regardless of children’s prior attendance status, the effect does vary. Children who were not found in school in the first term of 2000 had average participation rates of 14 percent if they belonged to comparison school baselines and 16.4 percent if they belonged to treatment school baselines, which is a 17.4 percent difference from the level in the comparison group (Table 7, column 2). Rates for children who were found in school in the first term of 2000 were 50.7 percent and 64.7 percent respectively (Table 7, column 3). Both in terms of the percentage change from the comparison participation rate and in terms of absolute number of children, the effect was larger for children who were found in school before the program. Additional participation among treatment baseline children was due for two-thirds to higher participation rates among children who were found in school prior to the program, while the remaining one third was due to higher participation rates among children who were not found in school prior to the program.

The overall effect of the program was smaller in 2001. Children of the relevant age group participated in school 19.1 percent more often if they belonged to treatment school baselines. The estimates are noisier in 2001 than in 2000. There are several tentative reasons for this smaller effect. First, the coverage of the baseline is less extensive in 2001 than it is in 2000. The sample size of children aged between 4 and 6 decreases from 5110 to 4794. Second, the ages of the children as they are reported in the baseline are probably noisier for younger children, so the sample is less well defined. In addition, in 2001, an increasing number of comparison schools started organizing school meals on their own, with funding from the parents. Prior to the introduction of the breakfast program, only two schools, one treatment and one comparison, organized a school meal program on their own. By September 2001, 14 comparison schools out of 25 were organizing their own meal program. This phenomenon might have been the result of a mere time effect, or it might have been a spillover from the program itself that was contaminating the comparison schools.

It is possible that the implementation of the program in the treatment schools could have contaminated the comparison schools. Since the experiment was not blind, the comparison schools knew that the treatment schools were receiving the school meals. This potentially provided a signal that school meals were a worthwhile intervention, making it more likely that comparison schools would start a meal program on their own. Or there could have been a peer effect or increased competition. Possibly, schools close to the treatment schools learned from the program and incorporated that knowledge into their organization. Or they could have felt the competition from the treatment schools and started a school feeding program to try to compete better. With the data available at the time of writing, it is not possible to determine what the mechanism was. Data are currently being collected on non-subsidized school meals in all schools in the district. Combined with GPS data, these data will allow us to identify the effect of geographic proximity to

a treatment school.

There is some evidence that the difference estimator presented above might be biased because of imprecisions in the data. Ideally, for the intention to treat estimator, all children attending school would be matched to their respective baselines, whether they transfer or not. In practice, if a pupil in a particular school could not be matched to the baseline of that school, checks with the baseline of the nearby schools were not made in the field but in the office. This might have resulted in incomplete matching that could potentially bias the results. As a robustness check, we excluded from the sample the comparison schools located within 4 km of a treatment school, as well as the treatment schools located within 4 km of a comparison school and reran the regressions. The restrictions in the sample lead to imprecise estimates and we cannot reject that the treatment effect is different from zero. However, the treatment effect estimate is within one standard deviation of the original estimate.

5.2 Probability of finding a child in school

We have just shown that the feeding program had a significant effect on the average school participation rates of children in the treatment group, and that the effect was larger for children who were found in school prior to the implementation of the program. Next, we want to investigate to what extent the larger participation rates in treatment schools were due to a larger set of children attending at the same rates as in comparison schools, or to a similarly sized set of children attending at higher rates. To do this, we estimate the impact of the program on the probability that a child was found in school at least once per school year after the implementation of the program. This will give us an approximation of the set of children that attend school. Because the program implementation was randomized over schools, the difference in means between children from treatment school baselines and children from comparison school baselines will be an unbiased estimator of the average effect of the program on the probability of finding baseline children at least once over 2000 (resp. 2001).

First, we estimate the difference in means using a linear model with random effects at the school level. The model is

$$y_{ij} = \alpha + T_i\beta + v_i + u_{ij} \text{ with } v_i \stackrel{iid}{\sim} N(0, \sigma_v^2) \text{ and } u_{ij} \stackrel{iid}{\sim} N(0, \sigma_u^2)$$

where y_{ij} is 1 if child j in the baseline of school i , was found in school (any school) at least once in the relevant period, T_i is a dummy for the treatment status of school i , v_i is the school-level random effect, and u_{ij} is the error term for child j in the baseline of school i . We estimate this

relation for children aged 4 to 6 in 2000 and 2001. We do not pool observations on 2000 and 2001 because the relevant sample changes by school year. For 2000, we also run the regression separately for children who were in school prior to the program and for children who were not in school prior to the program. On average, 42.5 percent of children belonging to the baselines of comparison schools were found in school at least once, against 49.7 percent of children belonging to the baselines of treatment schools (Table 8). The difference is statistically significant at the 5 percent level. Probabilities for the subsamples of children who were enrolled in school prior to the implementation of the program were positive but not statistically different between treatment and comparison baselines. For children who were neither enrolled nor present before the program, the school meals raise the probability of being found in school from 2 to 9 percent, a statistically significant difference.

Second, we incorporate child-level controls x_{ij} into the regressions. We estimate a probit model with random effects at the school level. The assumptions of the model are:

$$\begin{aligned}
 P(y_{ij} = 1|x_{ij}, T_i, v_i) &= P(x_{ij}a + T_i\beta + v_i + u_{ij} > 0) \\
 u_{ij}|x_{ij}, T_i, v_i &\sim N(0, \sigma_u^2) \quad j = 1, \dots, N_i \\
 v_i|x_i, T_i &\sim N(0, \sigma_v^2) \\
 & y_{i1}, y_{i2}, \dots, y_{iN_i} \text{ are independent conditional on } (x_{ij}, T_i, v_i)
 \end{aligned}$$

where y_{ij} and T_i are as before and v_i is the school level random effect.

Depending on the number of controls included, the estimate of the proportion of the total variance contributed by the panel-level variance is between 0.06 and 0.14, and is statistically significantly different from zero in all cases (Table 8). The coefficient estimates on treatment are positive and statistically significant for the children who were not present prior to the program, both in 2000 and 2002. Girls are less likely to be found in school than boys, especially once they are old enough to go to primary school.

In section 5.1, we showed that school participation of baseline children was about one third higher in the treatment group, and that the difference was statistically significant at the 5 percent level. In this section, we find that the increases in school participation rates happened both on the intensive margin (children who went to school started attending more often) and on the extensive margin (new children starting to attend school).

Table 8: Probability of being found in school at least once

Sample	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	All	2000	Enrolled	Present	Enrolled	Not present	Enrolled	Not present	Enrolled	Not present	All	2001	All	2001
Treatment	0.072		0.015		0.066		0.072		0.264		0.038		0.119	
	(0.032)**		(0.020)		(0.057)		(0.016)***		(0.099)***		-0.027		(0.046)***	
Present pre-program									2.279				1.295	
									(0.098)***				(0.072)***	
Present pre-program									-0.053				-0.063	
* Treatment									(0.143)				(0.101)	
Girl									-0.039				-0.029	
									(0.047)				(0.041)	
Female head of household									0.16				-0.088	
									(0.080)**				(0.069)	
Nr of children <age 6 in household									-0.01				-0.005	
									(0.020)				(0.016)	
Age 5 in 2000									0.524				0.3	
									(0.058)***				(0.050)***	
Age 6 in 2000									0.764				0.377	
									(0.059)***				(0.051)***	
Constant	0.425		0.941		0.541		0.021				0.491			
	(0.022)***		(0.014)***		(0.041)***		(0.012)*				(0.019)***			
Model	RE		RE		RE		RE		RE		RE		RE	
Observations	4315		1132		1337		1750		4197		4445		4327	
Number of panels	50		49		49		49		49		50		49	
rho	0.04		0.04		0.12		0.03		0.07		0.02		0	
chi2 Ho: rho=0	5.27		0.61		1.36		20.02		64.74		2		0	

Notes:
The dependent variable is a discrete variable that takes value 1 if the child was found in school at least once in the post-intervention observation period, and 0 otherwise. The post-intervention period of observation in columns (1)-(5) is terms 2 and 3 of 2000 (at most 6 observations in total); for columns (6)-(7) it is terms 1, 2, and 3 of 2001 (at most 7 observations in total). The sample consists of all children with age between 4 and 6 at the time of the baseline (term 1, 2000). Pre-program presence is a dummy variable that takes value 1 if the child was found in school in term 1 of 2000 (1 observation). The regression model is linear random effects in columns (1)-(4) and random effects probit in column (5). Rho is the proportion of the total variance contributed by the panel-level variance component. Standard errors are reported in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

5.3 Cognitive abilities and learning

5.3.1 Tests and sampling procedure

We have just shown that the preschool feeding program significantly increased school participation. To evaluate whether treatment group children also learned more than comparison group children, cognitive and attainment tests were administered in early 2002, approximately two years after the introduction of the program. The tests were conducted in the schools and consisted of 10 minutes of oral curriculum testing, 10 minutes of oral cognitive testing and 40 minutes of written curriculum testing.³⁴ The oral test was conducted in the local language,³⁵ the written test was drafted in English but instructions and explanations were given orally in the local language and in Swahili. The tests were designed by a team of psychologists and extensively pretested locally.

The sample of pupils to be tested was drawn from the baseline, thus not from the pupils who were enrolled or present in school. The following method was used to sample the children who took the tests: The baseline was first divided by school and age. Only children who were 4, 5 or 6 in 2000 were retained. Within each school-age group, the children were randomly ordered into a sampling list using a random number generator. The first 10 children in each group formed the “core” sample of children to be tested. Thus each school had 30 core children to be tested. Core pupils who were absent on the day of the test were replaced by children further down on the sampling list until the test administrator had gathered 30 children to take the test. Replacement was done following the order of the list. Sampled baseline children who were not enrolled in school or who were enrolled but not present on the day of the test, had to be fetched home by an older sibling.

To minimize attrition, the tests were announced to the schools in advance. ICS sent a letter to each school specifying the test date and requesting that as many enrolled children as possible be present that day. The schools were also informed that all pupils taking the test would receive a doughnut, a pencil and an eraser, which probably provided a very strong incentive for children to show up in school that day.³⁶ Despite these efforts, attrition is high among the core sample children.

³⁴The oral cognitive test evaluated verbal fluency, ability at recognizing similarities between pictures, ability at Raven’s matrix, pattern construction skills, and memory using the digits forward technique. The written test consisted of two sections. The first section had 4 straight curriculum questions (writing the alphabet, writing numbers from 1 to 20, writing 5 dictated letters and writing 5 dictated numbers between 1 and 20). The second section comprised more complex questions where the child was asked to fill in missing letters in words, write down the name of an object represented in a picture, answer questions about the contents of a sentence and perform basic arithmetic operations.

³⁵The oral tests were translated into 5 local languages, Samia, Marachi, Khayo, Nyala and Ateso. For each language, at least one member of the NGO staff was trained in the administration of the test. The test administrators always conducted the oral test in their own mother tongue. Thus it is not possible to identify the effects of language and test administrator. One school did not participate in the tests because it was the only school where a particular language was spoken.

³⁶Pupils in Kenya do not seem to mind taking tests. In fact, there were a number of informal complaints from

Table 9: Attrition at the cognitive and curriculum tests

	(1) Core pupils tested out of 30 Oral test	(2) Core pupils tested out of 30 Written test	(3) Nr of pupils called to reach 30 pupils Oral test
Treatment	-0.39 (0.39)	-0.03 (0.03)	2.269 (0.35)
Constant	12.64 (17.86)**	12.28 (16.88)**	73.273 (15.56)**
Obs	49	49	46
R-squared	0.00	0.00	0.00

Notes:

The following method was used to sample the children who took the tests: The baseline was first divided by school and age. Only children who were 4, 5 or 6 in 2000 were retained. Within each school-age group, the children were randomly ordered into a sampling list using a random number generator. The first 10 children in each group form the "core" sample of children to be tested. Thus each school has 30 core children. The dependent variable in columns 1 and 2 is the number of core pupils who were actually tested. Core pupils who were absent on the day of the test were replaced by children further down on the sampling list, and replacement was done following the order of the list. The dependent variable in column (3) is the number of pupils in the sampling list who were called before the field staff had assembled a group of 30 pupils to take the test.

Three schools are excluded from regression (3), because the field officer did not apply the procedure for replacing absent core children correctly. One school did not participate in the tests at all, because no field staff was available to test the children in the local language. Standard errors in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

In practice, children who were not enrolled or happened to be absent on the day of the test were not fetched home and thus they were not tested. From the original core sample of 30 pupils per school, on average 12.6 were administered the oral test, which implies an average attrition rate of 58 percent (Table 9, column 1). The attrition rate for the written test was similar (Table 9, column 2). On average, a total of 75 names were called before the test administrator had gathered 30 children to take the test (Table 9, column 3).

There is no significant difference between treatment and comparison schools in either of the two measures of attrition. Several factors make this plausible. First, the children who were

parents who argued it was not fair that their child did not get tested while other children in the same class did get the test.

tested were on average 2 years older than the average preschool child, and two thirds of them were old enough to be in primary school. As shown in section 5.3.3, there is no evidence the feeding program affected primary school participation once the children were too old for preschool. Second, the incentives provided by the NGO to show up in school on the test day were sizable by local standards. So it is not implausible that a similar number of children showed up in treatment and comparison schools on the test day.

5.3.2 Test results

We regress the total test score and the scores on the separate test components on the treatment status, an indicator of the teacher’s experience and its interaction with the Treatment variable, an indicator for whether the preschool had a building with a roof, ethno-linguistic fractionalization, and child-level controls. We have several indicators for teacher quality: the teacher’s number of years of experience, number of years of formal education, an indicator of whether the teacher had taken a 2-year on the job training in early childhood education, and an indicator of whether the teacher had taken short courses in early childhood education. Since these variables are correlated and their inclusion in the test score regressions would inflate standard errors, we first extract the first and second principal factors using a rotated factor analysis. (Cf. 10) We then average the principal factors by school. The first principal component places relatively more weight on short seminars and experience, while the second factor places relatively more weight on formal education and the 2-year training. Many short seminars tend to be offered by outside organizations (like ICS) to existing teachers, and therefore the longer a teacher is in the school, the more likely it is that she could take part in one. Hence it seems natural to refer to the first factor as "Experience" and to the second factor as "Training". Ethno-linguistic fractionalization is calculated as $1 - \sum_i (\text{Proportion of ethno-linguistic group } i)^2$ for all children attending school within 5 km of the school. It is the probability that two people randomly drawn from the population are from distinct groups. (Miguel and Gugerty (2002)).

The school meals program increased curriculum test scores in the schools where the teacher was relatively experienced. The provision of school meals increased the test scores by approximately 0.38 of a standard deviation in schools with one standard deviation (0.82) higher than average experience levels. The oral curriculum test score for children in treatment schools where all teachers were trained prior to the program is 0.42 of a standard deviation higher than in comparison schools where none of the teachers were trained (Table 11). The effect of feeding on that group is significant at the 5 percent level. Pupils from the comparison baselines did not score better if the teacher in their school was experienced at the onset of the program. Scores on the oral cognitive test were not

Table 10: Principal factors analysis of teacher characteristics

Factor	Eigenvalue	
1	1.26	
2	0.3	
3	-0.17	
4	-0.26	

Rotated Factor Loadings (varimax)		
Variable	1	2
Formal education	-0.17	0.44
Short seminars	0.70	-0.12
Experience	0.67	-0.04
2-year on the job training	0.54	0.30

Scoring coefficients for the factors		
	"Experience"	"Education"
Formal education	-0.05	0.36
Short seminars	0.40	-0.15
Experience	0.34	-0.04
2-year on the job training	0.25	0.33

Table 11: Test results

	(1)	(2)	(3)	(4)	(5)	(6)
	Oral cognitive	Oral curriculum	Written curriculum	Oral cognitive	Oral curriculum	Written curriculum
Treatment	-0.03 (0.09)	0.07 (0.09)	0.02 (0.12)	-0.02 (0.09)	0.10 (0.08)	0.05 (0.11)
Experience (PF1)	0.15 (0.07)**	0.12 (0.07)*	0.25 (0.09)***	0.12 (0.09)	-0.08 (0.08)	0.01 (0.11)
Experience(PF1) *Treatment				0.08 (0.13)	0.47 (0.12)***	0.55 (0.16)***
Training (PF2)	-0.01 (0.09)	0.00 (0.10)	0.05 (0.13)	-0.01 (0.09)	-0.01 (0.08)	0.04 (0.12)
Classroom useable in the rain	0.07 (0.09)	-0.08 (0.09)	-0.11 (0.12)	0.06 (0.09)	-0.12 (0.08)	-0.16 (0.11)
Ethnic fractiona- lization	-0.20 (0.32)	-1.03 (0.34)***	-1.59 (0.46)***	-0.19 (0.33)	-0.96 (0.30)***	-1.50 (0.41)***
Girl	-0.15 (0.05)***	-0.13 (0.05)***	0.00 (0.05)	-0.15 (0.05)***	-0.13 (0.05)***	0.00 (0.04)
Female head of household	0.11 (0.09)	0.09 (0.09)	0.13 (0.08)	0.11 (0.09)	0.08 (0.09)	0.12 (0.08)
Nr children <6yrs in household	-0.03 (0.02)	0.00 (0.02)	-0.02 (0.02)	-0.03 (0.02)	0.00 (0.02)	-0.01 (0.02)
Age 7	0.60 (0.06)***	0.65 (0.06)***	0.60 (0.06)***	0.60 (0.06)***	0.66 (0.06)***	0.61 (0.06)***
Age 8	1.01 (0.06)***	1.14 (0.06)***	0.95 (0.06)***	1.01 (0.06)***	1.14 (0.06)***	0.95 (0.06)***
Constant	-0.42 (0.13)***	-0.34 (0.13)***	-0.10 (0.16)	-0.42 (0.13)***	-0.35 (0.12)***	-0.11 (0.15)
Observations	1350	1357	1326	1350	1357	1326
Number of schools	49	49	49	49	49	49
Sigma u	0.24	0.26	0.38	0.24	0.22	0.34
Sigma e	0.88	0.83	0.80	0.88	0.83	0.80
Rho	0.07	0.09	0.18	0.07	0.06	0.15
R2 overall	0.17	0.24	0.24	0.17	0.26	0.27
R2 between	0.12	0.31	0.44	0.13	0.50	0.57
R2 within	0.18	0.23	0.18	0.18	0.23	0.18

Notes:

The dependent variable is the normalized test score on the indicated test component. Experience (PC1) and Training (PC2) are the first and second principal component of a rotated principal components analysis of the following indicators: teacher's year of experience, years of formal education, an indicator of whether the teacher had taken a 2-year on the job training course in early childhood education, an indicator of whether the teacher had taken short courses in early childhood education. Ethnic fractionalization is defined in Table 1. The regression model is linear random effects at the school level. Standard errors are reported in parentheses.* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

different between the groups. That there is no effect on the cognitive part of the test is an indication that the program either did not improve children's nutritional status, or that the improvement in nutrition was insufficient to lead to better cognition (cf. section 5.4).

The test results show interesting relations between test scores and area and child characteristics. First, children in ethnically diverse areas score much worse on the tests than children in less diverse areas. An increase of one standard deviation in the ethnic fractionalization index is associated with a 0.19 decrease in the total test score. Miguel and Gugerty (2002) found that higher ethnic diversity leads to lower school funding in Kenya, so the lower scores in ethnically diverse schools could be due to lower levels of equipment. Second, girls score worse than boys, but this is only the case in the oral test (approximately 0.14 of a standard deviation), not in the written test. Since girls do not score worse on the curriculum tasks, it is unlikely that their lower score on the oral curriculum test compared to boys is due to lower cognitive ability. In addition, girls score worse than boys on the oral curriculum test but not on the written curriculum test. This strongly suggests that it is the oral aspect of the test that made girls score worse. Our interpretation of this is that girls are more shy than boys. Third, children from female headed households score better on the curriculum components but not on the cognitive components. Fourth, the size of the household is not correlated with test scores. Finally, test scores are positively correlated with age, which can be seen as an indicator of the overall power of the test to detect differences in cognition and abilities.

5.3.3 Discussion

The bottom line of the test results is that the school meals program improved test scores, but only in schools where the teacher was relatively experienced prior to the program. The findings confirm the intuition that it only makes sense to give incentives for children to go to school if they are offered an environment where they can actually learn something. Note that the coefficient on teacher experience is not an estimator of the causal effect of teacher experience on children's test scores. The amount of experience that the teachers had prior to the program is the result of a selection process that most probably depends on the characteristics of the teacher. For example, teachers who were more motivated and better at their job are likely to disproportionately chose to stay on teaching many years.

There are a number of other factors that are important in considering the effect of school meals on learning. First, the school meals displaced teaching time. The school meal was intended to be served before classes started, i.e. between 8 and 8.30 am. In practice, many schools served the meal later. The meal was served on time in less than one quarter of the observations. In 30

Table 12: Pupil-teacher ratios

	(1)	(2)	(3)	(4)
Schools	All	All	Treatment	Comparison
Treatment	7.86 (3.04)**	7.99 (2.71)***		
Pre-program PT-ratio		0.31 (0.11)**	0.37 (0.18)**	0.25 (0.14)*
Dummy for treatment school within 4km		-6.26 (2.86)**	-3.68 (5.18)	-8.34 (3.00)**
Number of schools within 4km		-0.57 (0.58)	-0.7 (1.23)	-0.65 (0.54)
Experience(PC1)		-0.44 (2.73)		
Experience(PC1)*Treatment		-1.9 (4.13)		
Constant	26.48 (2.15)***	25.26 (5.72)***	31.18 (10.72)***	28.25 (6.16)***
Obs	50	50	25	25
R-squared	0.12	0.4	0.26	0.44
Adj. R-squared	0.1	0.32	0.15	0.36
Maximum number of visits	10	10	10	10

Notes:

The dependent variable is the observed pupil-teacher ratio, averaged over all visits in terms 2 and 3 of 2000 and terms 1 through 3 of 2001. The pre-program pupil-teacher ratio is the average pupil-teacher ratio over terms 1 through 3 of 1999 and term 1 of 2000 (one observation for each term). Standard errors are in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1percent

percent of the cases, the porridge was served at 9.30 am or later. This is likely to be an optimistic estimate since the figures are based on the timing reported by the teacher rather than on actual observation. In addition, the schools reported that the feeding took half an hour on average, but casual observations suggest that this figure might be too low, especially in large schools. One half hour easily accounts for 15 to 20 percent of teaching time in preschools.

Second, the school meals program increased crowding in the classrooms. Even though several treatment schools hired additional preschool teachers, the observed number of pupils per teacher in treatment schools was 34, compared to 27 in comparison schools (Table 12). There were no reports that the increase in pupil-teacher ratio lead to disciplinary problems, though one can imagine that the larger class sizes did not foster learning. In fact, crowding in the classrooms

might have contributed to a common tendency in Kenyan schools: that teachers see their task as keeping order and discipline in their classrooms and instilling those principles in pupils rather than as transmitting knowledge. In addition, the increased class size might have been one of the reasons why the feeding program did not improve test performance in schools where the teachers were not much trained. Wößmann and West (2002) find that “the existence of class-size effects, and the lack thereof, in different school systems appears to be related to the relative quality of the teaching force.” If worse teachers were unable to cope with the large classes, preschool care in those schools might not have been better than home care. When children below six do not go to school, they often accompany their parents to their work in the fields or to the market. Grandmothers and other elderly members of the extended family often provide informal care to the children. A family member might provide children with better care, more dedication and intellectual stimulation than a poorly trained and poorly paid teacher with little equipment, few teaching materials and over 30 children in her classroom.

5.4 Anthropometric measures

Anthropometric measurements were taken of the children who took the cognitive and attainment tests. In practice, for 16 schools, the anthropometric measurements were taken on a different day than the tests. As a result, the anthropometric measurements suffer from double attrition: children who were in school on the day of the test but not on the day of the measurements have missing values for their height and weight. However, attrition as measured in the number of core children that were actually measured, did not differ significantly between treatment and comparison schools (Table 13, column 1).

There is evidence that the program affected boys’ weight (Table 13, columns 5 and 6), but there is no evidence that it affected the weight of girls. In addition, no effects were found on the height of either girls or boys. Since height is usually seen as a result of long-term nutritional status, this last is hardly surprising. The absence of improvement in girls’ weights suggests that families reallocate resources within the family, and do so differently for boys and girls. Alternatively, girls who eat breakfast might be more active during the day and burn more calories, resulting in no weight gain.³⁷ The available data do not allow to estimate whether any food was reallocated within the family, and if so, what the magnitude of the reallocation was. On average, 26 percent of participating children had eaten breakfast at home, while only 17 percent of treatment school children had (Table 2). However, in itself this is not evidence of reallocation of food since parents might have reallocated food between meals. In addition, this figure does not control for endogenous

³⁷This suggestion was made by Werner Kiene (WFP).

Table 13: Anthropometric measures

	(1) Attrition: core pupils measured (out of 30) OLS	(2) Attrition: Prob core pupil measured Probit	(3) Height for age Z-score OLS	(4) Height for age percentile OLS	(5) Weight for age Z-score OLS	(6) Weight for age percentile OLS
Treatment	-0.68 (1.16)	-0.16 (0.10)	0.21 (0.77)	3.37 (5.95)	0.79 (0.38)**	8.56 (3.62)**
Girl		-0.04 (0.11)	-0.18 (0.22)	-2.26 (2.39)	0.09 (0.07)	2.37 (1.65)
Girl * Treatment		0.18 (0.13)	-0.21 (0.50)	-0.39 (4.30)	-0.44 (0.23)*	-5.37 (2.66)**
Age 7		-0.91 (0.09)***	-1.34 (0.14)***	-24.30 (2.14)***	-1.11 (0.15)***	-22.16 (1.78)***
Age 8		-1.3 (0.10)***	-1.77 (0.16)***	-29.30 (2.00)***	-1.31 (0.13)***	-25.49 (1.64)***
Constant	10.84 (0.82)***	0.62 (0.07)***	0.04 (0.51)	40.18 (4.21)***	-0.70 (0.10)***	29.87 (2.39)***
Obs	50	2156	1184	1184	1184	1184
R-squared	0.01		0.06	0.11	0.09	0.13
F stat for Ho: Girl+ Girl*treatment=0			0.00	0.51	2.66	1.37

Notes:

The dependent variable in column 1 is the number of core pupils (out of 30) who were measured. For a description of the construction of the core sample, see Table 9. The dependent variable in column 2 is a discrete variable that takes on value 1 if a child was measured and 0 if a child was not, while the sample is the set of core pupils (30 pupils per school). In columns 3-6, Z-scores and percentiles are relative to a healthy US population and were computed using the anthro program (Centers for Disease Control and Prevention and World Health Organization). Ages in this table are ages in 2002. The regression model is OLS. Standard errors are clustered at the school level in columns 2-6. Robust standard errors are reported in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Table 14: Teacher attendance

	(1)	(2)	(3)	(4)	(5)
	Teacher	Teacher	Teacher	Teacher	At least
	present	present	present	present	one teacher
Year	2000-2002	2000	2001	2002	2000-2002
Treatment	0.01	0.02	0.01	-0.08	0.02
	(0.05)	(0.05)	(0.07)	(0.09)	(0.03)
Constant	0.72	0.80	0.65	0.80	0.81
	(0.03)***	(0.03)***	(0.05)***	(0.06)***	(0.02)***
Obs	61	69	63	57	50
R-squared	0	0	0	0.01	0.01
Max. nr. of visits	13	4	7	2	13

Notes:

The dependent variable regressions (1)-(4) is average teacher attendance over the relevant period. Absences for reasons of maternity leave, sickness leave and training are counted as absences. Absences due to definitive quits are counted as non-observations. The sample is restricted to teachers who were in post at the time of the baseline survey (Feb-Mar 2000). The standard errors are clustered at the school level. Robust standard errors are reported in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

selection into the schools, because it is based on participating children rather than on baseline children.

5.5 Teacher attendance

The school meals program did not increase school attendance of teachers, despite the fact that it provided relatively strong incentives to the teacher. First, the program substantially increased fee payments by the parents (cf. section 6.2). Second, the provision of porridge to the teacher was a non-negligible income transfer to the teacher: one portion of porridge cost a teacher's average daily salary. Third, by its very setup, the income transfer was totally dependent on the teacher attending school. Restricting the sample to teachers who were in post before the program started, we find that both in the treatment and in the comparison schools, teachers were absent approximately 30 percent of the time (Table 14, columns 1 to 4). In addition, there is no evidence of a change in the probability that at least one teacher was present in school, despite the fact that treatment schools hired new teachers to cope with the increasing number of pupils in the classes (Table 14, column 5).

This apparent inelasticity to the provision of incentives parallels findings from other stud-

ies in Kenya (e.g. Kremer et al. (2002) for primary school teachers, Chen and Kremer (2002) for preschool teachers). A possible explanation for this multiple evidence that teachers are little responsive to incentives is that, beyond a certain level, attendance starts to be very costly to the teacher due to factors like sickness or social obligations. For example, the Welfare Monitoring Survey (1994) finds that rural poor in rural Western Kenya missed an average of 5.9 days of work per month due to sickness. Over half of the respondents reported that they had fever or malaria in the preceding two weeks. In addition, people miss work for social reasons like funerals. For example, the parent representatives in the treatment schools were reported absent due to a funeral in 3 percent of the cases.

6 Price effects and the generalizability of the ITT estimate

External aid can have substantial implications for school finance, which in turn can influence outcomes like school participation. Assume an NGO enters a region and picks out a school for allocating aid to that school. *Ceteris paribus*, if the program has positive value to the parents, it will increase their valuation of school attendance in that school. If the market for schooling was competitive before the NGO started working with the school, the intervention can change competitive relations between the providers of education. A program that provides free school meals to the children attending school does not change the cost curve of supplying education, but it does change the demand for education in that school. For each unit of education provided, parents are willing to pay more, because their children receive a perk with each unit of education. By assigning pupils to their original treatment school rather than to the treatment status of their current school, the intention-to-treat estimator avoids upward biases that would arise from transfers into the treatment school

One problem with the ITT estimator is that it does not take into account the general equilibrium effects of the program on prices. As mentioned in section 3, school fees provide the bulk of funding in Kenya's rural preschools. ICS instructed the treatment preschools not to raise their fees as a response to the program. The data confirm that official fees in treatment schools were not higher than in the comparison schools after the start of the program (Table 15, column 1). However, since official fees were not binding prior to the program (cf. section 3), the treatment schools had the possibility of becoming more strict about fee payments of children who showed up in the preschools without transgressing the instructions, and the data strongly suggest that they did indeed become more strict. In addition, prices dropped in comparison schools located close to a treatment school. The important point is that some of these price effects are not likely to occur

Table 15: Fees per day of school participation

	(1)	(2)	(3)	(4)	(5)	(6)
	Official fee	Price of preschool per day of attendance		Comparison		Treatment
	All	All	All	All	All	Treatment
Sample schools	NA	Baseline	Non-baseline	All	All	all
Treatment	0.24 (0.67)	0.93 (0.34)***	1.30 (0.42)***	1.03 (0.30)***		
Dummy for treatment school <4km	0.34 (0.68)			-0.49 (0.29)*	-0.91 (0.46)**	-0.19 (0.30)
Treatment school <4km *Treatment	-1.00 (0.94)					
Number of schools <4km	0.09 (0.11)			0.02 (0.05)	0.12 (0.06)*	-0.07 (0.08)
Ethnic fractionalization	-0.85 (1.83)			-1.74 (1.06)	0.51 (1.55)	-2.78 (1.46)*
Additional teacher	-0.18 (0.40)			0.51 (0.24)**	0.64 (0.42)	0.53 (0.32)
Child in baseline				0.32 (0.18)*	0.46 (0.39)	0.25 (0.21)
Constant	2.30 (1.11)**	-0.29 (0.25)	-0.65 (0.35)*	-0.47 (0.59)	-2.02 (0.96)**	1.46 (0.83)*
Observations	49	2503	239	2742	1247	1495
R-squared	0.06					
Mean fee/day for Comp.	2.82	0.66	0.39	0.64	0.64	
Mean fee/day for Treatm.	2.62	1.06	1.00	1.06		1.06

Notes:
The dependent variable in column (1) is the official fee per term, divided by the number of school days per term. The dependent variable in columns (2) to (6) is fees paid in 2000 (terms 2 and 3), divided by observed participation over the same period and by the approximate number of school days. Observed participation is defined on a scale of 0 to 1. The sample only includes children who were found in school in 2000. Baseline children were assigned to their initial treatment status. Other children were assigned to the treatment status of the first school where they were found. Fees are in Kenyan Shillings (80 K.Sh.= 1 USD). The regression model in column (1) is OLS with clustering at the school level. The regression model in columns (2) to (6) is a one-sided tobit with clustering at the school level. Ethnic fractionalization is defined in Table 1. Robust standard errors are reported in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

if the feeding program were generalized to all schools in the area, which implies the ITT estimator of the effect of the randomized program would not be appropriate in the general setting.

6.1 Transfers, supply of schooling and prices

We develop a model that shows how the school meals program, capacity constraints and pupil transfers affect the ITT estimator through general equilibrium price effects. The model makes three assumptions. First, households choose optimum quantities of two goods, food and schooling.³⁸ Schooling is measured in hours. Second, households take the price of schooling, and the school capacity as given. Second, the equilibrium price and quantity of schooling in a school are determined by the interaction of the total demand³⁹ for schooling in that school and the supply curve in that school. Third, the quantity of learning that a child can receive in a school is limited by the maximum number of hours that can be spent in school and by the amount of learning that she can get from each hour in class. The amount of learning per hour of schooling depends on the pupil-resource ratio, on the characteristics of the school and on the characteristics of the child.

Case 1 : *Perfectly elastic supply curve of schooling*

First consider the case where the supply of schooling is perfectly elastic. Assume there are no transfers between schools. Consider the household's decision problem between food and learning. Then the provision of subsidized food in school rotates out the household's budget constraint, because an additional day in school now comes with an additional unit of food free of charge. This is equivalent to a fall in the relative price of schooling. As long as schooling is not a Giffen good, the family's demand for schooling will increase. Then the aggregate demand for schooling in the school will shift out, but since the supply curve is perfectly elastic, there is no additional general equilibrium effect on the relative prices of schooling and food, and the relation between the amount of schooling and the amount of learning is unchanged. In this case the difference between treatment and comparison schools in the number of children participating in class in an appropriate estimator of the effect of the program on school participation.

Now relax the assumption that there are no transfers. Prior to the randomized program, households choose the school and the quantity of schooling/learning that provide them with the highest utility. The cost per unit of learning (relative to food), the maximum amount of learning

³⁸I define "schooling" as the number of days spent in school, and "learning" as the amount of human capital the child gets from education. Learning depends on schooling, the quality of teaching and the pupil-resource ratio. The pupil-resource ratio involves all fixed costs in the school, eg. the pupil-teacher ratio if the supply of teachers is not perfectly elastic, the ratio of pupils per classroom, etc.

³⁹Total demand is aggregated over all families.

obtainable, and household preferences will together determine the pre-program choice of school and the amount of schooling demanded. The price of school per unit of learning differs by school because schools differ in quality, distance (and hence transportation costs), tribal affiliation, etc. When the feeding program is implemented in the treatment schools, the budget constraint conditional on choosing the treatment school rotates out, making it more likely that a household will choose the treatment school. Demand in treatment schools shifts out and demand in comparison schools shifts in, but there are no general equilibrium price effects because the supply curve is flat. The intention to treat estimator is an appropriate estimator because it avoids upward bias due to the transfers.

Case 2 : *Supply curve of schooling is upward sloping*

Now consider the case where the supply curve of schooling is upward sloping, for example because there are capacity constraints in the school. First assume that there are no transfers between schools and no hidden transfers, so that demand and supply remain unchanged in the comparison schools. For treatment school households, the budget constraint rotates out. In absence of general equilibrium effects, the demand for learning increases if learning is not a Giffen good. Since the household demands more learning, it also demands more schooling.

There are two general equilibrium effects. On the aggregate level, demand for schooling shifts out because the demand for schooling of every family increases. Hence, the market equilibrium price increases. So the price faced by every single consumer is higher than if the supply curve were perfectly elastic, which makes the budget curve rotate back in. In addition, the pupil-resource ratio in the school increases, which shifts in the family's constraint on the maximum amount of learning. In a general equilibrium, price and total quantity of schooling increase, but the effect on the amount of learning in a particular family can be both positive or negative depending on the family's preferences and budget constraint. If capacity constraints are similar in the randomized program and in a setting where school feeding is generalized, both the simple difference estimator and the ITT difference estimator of the impact of the randomized program are unbiased estimators of the impact of a generalized program.

Now relax the assumption that there are no transfers. There are three general equilibrium effects in this case. First, the aggregate demand in the treatment school shifts out because more households choose that school. Second, the demand for schooling in schools nearby the treatment schools shifts in. This leads to a higher price of schooling in treatment schools, and a lower price for schooling in the comparison schools. Third, as in the case without transfers, the pupil-resource ratio increases in the treatment school, leading to a decrease in the amount of learning per unit of learning. The household's budget constraint conditional on choosing the treatment school rotates

in and the maximum amount of learning decreases. The opposite happens to the budget curve conditional on choosing the comparison school: the pupil-resource ratio decreases and the amount of learning per unit of schooling increases. Hence the budget constraint rotates out, and the maximum amount of learning increases. This leads to more participation in comparison schools and less participation in treatment schools than there would have been in absence of the transfers or in the case of a perfectly elastic supply curve.

The intention-to-treat estimator computed from the data of the randomized evaluation incorporate price increases in treatment schools and price reductions in comparison schools that are due to the transfers. When school meals are implemented on a general scale, we would expect fewer transfers because the budget constraint of a household would rotate out regardless of which school it chooses. Therefore we would expect the prices in the general setting to be higher in comparison schools and lower in treatment schools than in the randomized evaluation. Hence the estimator of school participation in the randomized experiment is a lower bound of what would happen in a setting where school meals are implemented on a general scale. Note that we do not need baseline children to transfer at all to obtain these general equilibrium effects. It is sufficient that children who do not belong to the baseline but who would otherwise have attended, switch schools.

6.2 Empirical evidence on price effects

The empirical evidence from the school meals program supports the case of an upward sloping supply of schooling, though there are puzzles remaining. The upward sloping supply curve implies that the price of schooling in treatment schools will be higher than before the introduction of the program and that the price will be higher than in comparison schools. This is supported by the empirical evidence: the price per day of participation was 58 percent higher in treatment schools than in comparison schools (Table 15, column 4).⁴⁰ Children from treatment school baselines paid 57 percent more per day of school attendance than children from comparison school baselines. Non-baseline children paid 66 percent more in treatment schools than in comparison schools. The difference between the figures is statistically significant at the 1 percent level for both groups (Table 15, columns 2 and 3). This confirms the hypothesis that teachers became more stringent in fee collections. Most of the money went towards higher teacher salaries. Both in absolute amounts and

⁴⁰The price per day of school participation is defined as the total amount paid by the parents, divided by the observed average attendance of the child (on a scale of zero to one) and by the number of school days. Note that the price per unit of schooling is a function of the price per day and the amount of schooling per day. I show further in the paper that the school meals program crowded out teaching time. Therefore the difference in price per day of participation between treatment and comparison schools is a lower bound on the difference in price per unit of schooling.

as a percentage of the total amount of fees collected, treatment schools spent more on non-salary items like classroom materials and construction than the comparison schools, but the difference is not statistically significant. (Table 2) The total increase in payments was approximately one tenth of the cost of the food that was delivered to the schools.

On average, non-baseline children paid lower fees than baseline children (Table 15, columns 2 to 6). One explanation for this is that they may have belonged to families that were more marginal to school participation, possibly due to a lower socio-economic status. Non-baseline children were not enrolled prior to the program and did not have any siblings enrolled in the school. If the teacher operated price discrimination on the basis of socio-economic status, she would have charged a lower price to those children.

Case 2 predicts that the price of education in comparison schools where pupils could transfer to treatment schools should have been lower than the price in comparison schools were this was not possible. We assume that pupils could transfer from a comparison school to a treatment school if the two schools were less than 4 km apart. We find evidence that the price of a day in school in comparison schools that were located within 4 km of treatment school was indeed lower than the price of a day in school in comparison schools that were located more than 4 km from the closest treatment school (Table 15, column 5). In treatment schools, the price tended to be lower when there was at least one other treatment school nearby, though the effect is not statistically significant at conventional levels (Table 15, column 6). This is in line with the argument in case 2: having another treatment school nearby decreased the number of potential transfers, because children from other schools had several treatment schools to choose from if they wanted to transfer to a treatment school.

7 Concluding remarks

There are several lessons to be learned from the Kenyan preschool meals program. The first point is that the context in which school meals are implemented is very important. A program that increases school participation in an environment with low teaching quality is likely to fail to translate into better educational achievement. We find evidence of a very strong complementarity between teacher characteristics, i.e. the amount of experience, and school meals in improving test scores. School meals can hardly be complementary to teaching if there is little teaching going on, or if the teaching is of very poor quality.

This study also provides an insight on the generalizability of results from randomized evaluations. Randomized provision of inputs can provide schools, or other economic agents, with a

cost advantage or with a monopoly on a certain type of good, which could affect choices of inputs or pricing strategies. From the side of the consumer, school participation is not only a matter of choice between “going to school” and “not going to school”, but also a choice of which school to attend. As shown in this paper, a randomized intervention can affect the existing competitive relations between schools by changing the characteristics of the school that are valued by consumers. This affects prices and leads to general equilibrium effects. If the general equilibrium effects carry through when the intervention is carried out in all schools, then the evaluation results can be used for policy purposes. If not, then the estimates must be adjusted.

In his introduction to the Pratchi Education Report, Amartya Sen makes a case for introducing a system of mid-day meals in West-Bengal. Currently, West-Bengal has a system of take-home rations, in which pupils receive rations of uncooked rice conditional on attending school regularly. Sen argues that actual mid-day meals would be superior to take-home rations for three reasons: First, school meals would contribute to the nutrition of children and thus complement teaching. Second, they would enhance school attendance. Third, they would reduce abuse and corruption that arise in the dry rations system due to the fungibility of the distributed rations.

A priori, it is not clear what is the best way to provide food support to school children. In many practical implementations of food aid through schools, food is prepared and served at school. However, a number of programs have used a different approach, supplying food aid in the form of take-home rations. Under that setup, families usually receive a set amount of food conditional on their children attending school. The Bangladeshi Food For Education program is an example of this approach. A third approach is to give families cash transfers conditional on their children attending school, as in the Mexican Progresa program. The choice between cash transfers and school feeding is often a political choice. Funds from international donors are often in kind and donation agreements explicitly forbid reselling of the food. But even when donor contracts or other earmarking of funds allocates a budget to “school feeding”, one would ideally like to know the relative effectiveness of take-home rations and on-site school feeding. Unfortunately, there is little hard evidence on this.

On-site school meals potentially have drawbacks. First, they only reach the children going to school, excluding children who are too weak or too young to go to school. If parents reallocate food within the household, it might be more cost-effective to give food-aid in the form of take-home rations. Second, they might disrupt teaching and learning at schools. Class time might be substituted for meal time, leading to a decrease in time spent teaching. The results of this study are not very positive for the disruption effects of school meals. The breakfast program was designed so it would cause as little disruption as possible, with the NGO hiring and paying a cook, and planning

the meal so it would be served outside of normal teaching time. Despite these precautions, the program substantially displaced teaching time, which might partly account for why increases in participation failed to translate into better test scores in some schools.

There are, however, also objections to conditional food or money transfer schemes. In the Pratiche Report, Amartya Sen argues that the West-Bengal take-home program should be replaced by a program that provides real meals at school, partly because take-home rations are vulnerable to abuse and corruption. Conditional cash or food transfer programs stand or fall with the ability to monitor the conditionality of the transfers. As experienced in a cash transfer program to teachers in Western Kenya,⁴¹ local community members might not be in a good position to monitor the conditionality effectively. In effect, transfer programs that make local people responsible for monitoring, give those people the power to deny transfers to potential beneficiaries who do not abide by the rules of the transfer. At the same time, both parties live in the same community. The social pressure might be such that, in practice, it is impossible for them to deny the aid to families. As a result of these monitoring problems, conditional transfers are likely to be more effective at increasing official enrollment in schools than at increasing actual participation. School meals by contrast do not encounter this problem. By their very setup, feeding is conditional on actual participation in school rather than on some measure of official enrollment. In fact, if anything, the results from this paper indicate that the effect of the breakfast program on school participation was larger than its effect on school enrollment.⁴²

In future research, we hope to investigate the effects of school feeding in primary schools. My first objective is to compare the two major approaches in the implementation of school meals, i.e. take-home rations and on-site school meals in terms of impact and cost. Second, we want to investigate the interactions of school meals and other inputs in producing better educational outcomes. The preschool breakfast program points to an important interaction between teacher quality and rates of school participation, but it is likely that other aspects of school quality matter too.

⁴¹See Chen and Kremer (2002)

⁴²Here I take the probability of finding a child in school at least once per year as a proxy for enrollment.

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