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RE-EVALUATING CONDITIONAL CASH TRANSFERS: IS THE OPORTUNIDADES PRIMARY SCHOOL STIPEND NECESSARY?

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Primary school enrolment and completion rates are almost universal in rural Mexico. Not surprisingly, estimates of impact suggest marginal effects of OPORTUNIDADES on primary school enrolment, which raises concerns about inefficiencies in the current transfer scheme. While removing the primary school component would involve substantial budget savings, equity and distributional concerns may argue against this alternative. In this paper, we investigate whether the primary school transfer generates positive externalities in the household. Specifically, we exploit the randomized nature of the data and baseline household structure to isolate the impact of the primary school transfer from the overall average treatment effect. Preliminary findings suggest no direct effects of the primary school grant on other outcomes, namely child health, household consumption and secondary school enrolment.

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1. Introduction

Conditional Cash Transfer (CCT) programs are demand-oriented anti-poverty measures that transfer monetary resources to poor targeted households conditional on household investment in their children's education, health and nutrition. In 1997, Mexico launched the first large scale CCT program, the Programa de Educación, Salud y Alimentación – PROGRESA, renamed OPORTUNIDADES under the Fox Administration. Similar programs have since been implemented in Brazil (Bolsa Escola), Colombia (Familias en Acción), Honduras (Programa de Asignación Familiar), Jamaica (Program of Advancement through Health and Education), and Nicaragua (Red de Protección Social).The success of this first generation of programs in increasing enrolment rates, improving preventive health care and raising household consumption, has motivated governments worldwide to undertake similar interventions. A second generation of CCT programs is currently being designed or already operating in other Latin American countries (Argentina, Ecuador, Peru), the Middle-East (West Bank and Gaza) and Turkey, South-East Asia (Cambodia) and Africa (South-Africa, Mozambique).[1]

Both the expansion of CCT programs to different environments and their large scope – population and budget wise – have motivated the quest for greater cost-efficiency in increasing their desired impacts on human capital formation. Successful scale up of CCT programs involves many challenges including a better knowledge of: (i) the general equilibrium effects of these interventions (for example, what the indirect or unintended effects are on the targeted population group or others); (ii) heterogeneity in their impacts as opposed to focusing on average treatment effects (if and why different subpopulation groups are affected); (iii) the political economy related to their implementation (what changes do local or country institutions need to undertake to accommodate this type of measures); and (iii) the mechanisms through which changes in behaviour and outcomes operate (which outcome measures are affected by the different components of the intervention package and why).

Deep understanding of these matters requires moving away from a black box approach to program evaluation towards an approach that specifically elaborates on implementation and operation issues; processes; and on the analysis of heterogeneity. One can also undertake a more 'structural' evaluation approach that models individual and household behaviour such that it is possible to anticipate (or simulate) responses to changes in incentives in a systematic and reliable fashion. In either case, the ultimate purpose should be to foster the discussion on how to (re-)design the scheme of incentives and the implementation of CCT interventions in order to maximize their cost-efficiency.

[1] See Fiszbein and Schady (2009) for a recent survey on Conditional Cash Transfer programs.

In this paper, we use the framework and data provided by OPORTUNIDADES to look at heterogeneous program impacts across households eligible to receive different program benefits. All OPORTUNIDADES beneficiaries receive a health package, which includes a nutritional grant and preventive health visits for all household members. In addition, households with children in school age receive an educational fellowship for each child enrolled in school between the third grade of primary and the third grade (last) of secondary school. The main purpose of this study is to isolate the impact of the conditional primary school stipend from the overall effect of the program on a series of outcomes, thus contributing to the understanding of the relative efficiency of each program subcomponent and to the debate on how to improve the program's cost-effectiveness overall.

As much of the relevant literature on the program puts forward, the OPORTUNIDADES package has had very small effects on primary school enrolment, of about one percentage point increase (Schultz 2004, Behrman et al 2001). This finding may not be too surprising given that primary school enrolment and completion rates are already very high in rural Mexico – around 94 and 96 percent, respectively. However, it does raise concerns about an inefficiency of the current transfer scheme associated to the fact that the program is paying people for what they do anyway. As de Janvry and Sadoulet (2005) note, the primary school incentive may only alter the behaviour of the 4 percent of the children that are not completing primary school.

Different proposals to improve the efficiency of the program over the current design come to mind. For example, one could consider transferring the primary school grant – directed to primary school aged children enrolled in school – to all households unconditionally, as part of the nutritional grant. This would at least reduce part of the targeting and monitoring costs. Or alternatively, one could replace the current transfer scheme with one aimed at maximizing gains by offering the most benefits to the groups where behavioural effects are more important – namely, secondary school aged children. The new scheme could re-direct all resources associated to the primary school scholarships to children in the transition to secondary school – as suggested in Schultz (2004), de Janvry and Sadoulet (2005) and Skoufias (2005) – or to all teenagers in secondary school age – as proposed in Attanasio et al (2007). If budget-neutral, such changes in the transfer scheme would entail significant gains in program.[2] A final option would be to remove the primary school component of the program altogether. This alternative would clearly involve

[2] Note that in the absence of liquidity constraints – which is unlikely for these poor rural households (see Gertler et al 2008) – any balanced budget change in the transfer scheme would be roughly equivalent to a redistribution over time of resources to the beneficiary families and would not have any effects.

substantial savings, of about one fourth of the total program budget, at a minor cost (loss of 1 percent increase in primary school enrolment).[3]

If the only goal of the program was to induce school enrolment, then it is obvious that abolishing the primary school grants or decreasing them to pay for larger secondary school scholarships are preferred policy options. But OPORTUNIDADES was conceived as a general anti-poverty measure disguised as a schooling subsidy. Therefore, equity and distributional concerns may argue against these alternatives.

Given that almost all primary school aged children in beneficiary households are already in school – i.e. the conditionality is not binding – the 'extra' primary school grant may be serving other purposes. For example, beneficiary households eligible to receive the primary school stipend may use this 'extra' income to buy more and better food and hence improve the health and nutritional status of younger children. Or by relaxing liquidity constraints, the 'extra' income may facilitate school enrolment of secondary school age teenagers, whose labour income is no longer needed in the household. If the grant that is labelled as 'primary school grant' does generate changes in household behaviour such that other outcomes are positively affected, then its existence might be justified in spite of the low impacts it has on primary school enrolment.

To try to address this issue, in this paper, we exploit the randomized nature of the evaluation sample and baseline household structure and conditions to examine whether the conditional-on-attendance primary school transfer generates positive externalities in the household. Specifically, test for the existence of heterogeneous treatment effects between beneficiary households with children eligible to receive the primary school stipend and beneficiary households without, on the following outcomes: younger children health and morbidity, household consumption and share of expenses, household investments, and older children secondary school enrolment.

Our estimates suggest little effects of the primary school grant on other outcomes. Overall, the program impacts are not substantially different between treatment households eligible and non-eligible to receive the primary school grant. The only exceptions are on household investment on production animals, the share of children clothing and newborns morbidity. Arguably, the effect of the primary school grant on children clothing may be explained by a non-separability argument – i.e. children need appropriate clothes and shoes to go to school. Results on child morbidity are inconclusive given low sample sizes and the poor quality of the data. Note also that this type of exercise does not fully answer the question that we

[3] See Section 3 for further details.

pose above. For example, we do not know what the nutritional status of young children with older siblings in primary school would have been in the absence of the primary school grant. However, the exercise we perform does provide circumstantial evidence on the issue at hand.

The remainder of the paper is organized as follows. In the next section, we review the program design and structure of incentives. In Section 3, we summarize the evidence in the OPORTUNIDADES literature on the benefits and costs of the conditional primary school transfer. In section 4, we describe the experimental evaluation data and discuss the empirical strategy we use to isolate (identify) the impact of the conditional primary school stipend from the overall average treatment effect. Section 5 is organized in different subsections, each of which presents results on a different set of outcomes. Finally, Section 6 discusses the main findings and concludes.

2. The Rural OPORTUNIDADES Program

The Mexican Government established OPORTUNIDADES in 1997 to alleviate short- and long-term poverty by giving parents financial incentives to invest in the human capital of their children. Over its first three years, the program extended benefits to almost all eligible families in rural areas. Starting in 2001, it expanded to urban areas and now covers around 5 million families all over Mexico.[4]

In rural areas, OPORTUNIDADES determined household eligibility in two stages. First, underserved communities were identified based on the proportion of households living in poverty as defined in the 1995 population census. Second, low-income households within these communities were chosen using a proxy means test (Skoufias et al 2001). Pre-intervention data to construct the index was collected on all households in eligible communities through the Survey of Household Socioeconomic Characteristics (*Encuesta Socioeconómica de Hogares*, ENCASEH). This classification scheme designated 52 percent of households in selected communities as eligible for benefits.[5]

All eligible households living in treatment localities were offered OPORTUNIDADES and over 90 percent enrolled. Once enrolled, households received benefits for a three-year period with the possibility of being recertified if all household members obtained the prescribed preventive medical care, children attended school and mothers participated in educational talks ("pláticas") on health, hygiene and nutrition.

[4] www.oportunidades.gob.mx

[5] In 1998, just before the start of the program, a set of slightly wealthier households was included as eligible in a process called "densification" (Skoufias et al 2001). However, many of these households did not receive benefits when the program began because of administrative delays and other operational difficulties (Hoddinott and Skoufias 2004). To avoid attributing treatment effects to untreated households and/or introducing any selection bias – being a "densified" household is not random – we drop "densified" households from our sample.

Verification of the conditionalities was done through medical providers at public clinics, who certified that households actually completed the required health care visits. A similar procedure in schools was followed for the cash transfer associated with school attendance. About 1 percent of households were denied the cash transfer for non-compliance.

The cash transfers represent over 20 percent of total household income, and are given directly to the household mother. They come bimonthly in two forms. The first is a nutritional grant intended for families to spend on more and better nutrition. It is complemented with regular health checkups for all household members, and with nutritional supplements and immunization directed to 0 to 2 year olds and to pregnant and lactating women. The second are educational scholarships given to each child younger than 18 and enrolled in school between the third grade of primary school and the third grade (last) of secondary school.

The educational stipend increases with the child's grade to offset the greater opportunity costs of schooling for older children, who are more likely to engage in household production or market work.[6] It also rises substantially after graduation from primary school and is higher for girls than boys during secondary school, where traditionally, girls have lower enrolment rates. The educational grant is received conditional on children attending a minimum of 85 percent of school days and on not repeating more than twice a grade. Beneficiary children also receive money for school supplies once or twice a year. Total transfers for any given household are capped at a pre-determined upper limit. Table 1 details transfer amounts in October 1997 prices.

3. Primary School Scholarships: Existing Evidence on Benefits and Costs

It is generally agreed in the literature on OPORTUNIDADES that the program impacts on primary school enrolment and continuation rates are very small. Schultz (2004) estimates a modest – albeit significant – 0.92 percentage points increase in girls' primary school enrolment averaged over three post-intervention data rounds, from October 1998 to November 1999. For boys, the increase in primary school enrolment is of about 0.80 percentage points. Given a 94 percent enrolment rate at baseline, these effects imply less than a one percent increase.

Behrman et al (2005) report some moderate gains in grade progression for primary school children using a Markov schooling transition model. The authors estimate 8 percent higher grade progression and lower

[6] Schultz (2004) estimates that the transfer amount for a girl enrolled in ninth grade amounts to roughly two-thirds of what a child this age earns in these rural communities if working full time (or 44 percent of the typical male wage).

repetition rates for treatment children enrolled in grades 3 and 4 (at ages 8 and 9, respectively). Similar effects are observed for children enrolled in grade 5 (at age 10).

In terms of educational attainment, Schultz (2004) estimates the resulting accumulated effect of increased enrolment of 0.72 additional years for girls and 0.64 years for boys. This is computed summing the change in enrolment at each grade level – from the first grade of primary to the third (last) grade of secondary school – induced by the program for the average treatment child.[7] Given that the effects on secondary school enrolment are of 9.2 (6.2) percentage points for girls (boys), or a 14 (8) percent increase over a baseline enrolment rates of 67 (73) percent, the contribution of increased primary school enrolment to the overall gains in schooling is only marginal. Indeed, prior to the intervention, the average youth aged 18 already completed slightly more than primary school (6.2 years of schooling) and primary school completion rates were of 96 percent, on average (de Janvry and Sadoulet 2005).

In fact, the critical dip in enrolment rates amongst the poor in rural Mexico appears to be in the transition to secondary school, when the conditional (on primary school completion) enrolment rate declines to 58 percent. Enrolment rates go back up to over 90 percent during the last two years of secondary school and fall again to 63 percent during the first year of senior secondary school for those qualified to enter (Schultz 2004). The program aims to improve transition to secondary school rates by increasing secondary educational grants by about 50 percent over the primary school stipend (see Table 1). As a result of these efforts, the largest differences in mean enrolment between treatment and control groups – of about 11 percentage points – are observed for children transiting to secondary school (Schultz 2004). This impact is disproportionately concentrated amongst girls, who receive a larger transfer amount than boys during secondary school and who tend to drop out of school more during the transition.

By year 2000, OPORTUNIDADES had already achieved full coverage of marginal rural municipalities reaching 2.6 million families. The overall program budget for this year was about 9.6 billion pesos (or US$ 100 million), which represented 0.2 percent of Mexican GDP. Coady (2000) estimates that the educational transfers and school materials accounted for 42 percent of this total budget, of which 23.4 percent (US$ 23.4 million) were for primary scholarships (including materials).[8] This represents 56.1 percent of the program budget for education in 2000.

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[7] Using Markov transition matrices, Behrman et al (2005) estimate a 0.68 increase in total education years.

[8] Authors' own calculations using data in http://www.oportunidades.gob.mx/indicadores_gestion/main.html

The educational and nutritional benefits (monetary or in-kind) to beneficiary families account for the larger chunk of the program budget. However, Caldés et al (2005) estimate that, over the first four years of implementation, 10.6 cents of each dollar transferred to beneficiaries were absorbed by administrative costs. The largest program cost items are the targeting of beneficiaries (43 percent) and the delivery of benefits (22 percent). Conditionality and monitoring – i.e. ensuring that households meet their responsibilities – represents 18 percent of the total program costs and is the third more costly activity. Program design and planning, internal and external evaluation activities, amongst others, account for the remaining 17 percent of the program administrative costs.

Note that some of these costs, such as targeting, can be treated as fixed as they are associated with program start-up activities. This implies that their relative importance declines with the expansion of the program to new beneficiaries. On the other hand, some other cost items increase with every new beneficiary incorporated. For example, the share of conditionality and monitoring activities increased from 8 percent in 1997 to 24 percent in 2000 (Caldés et al 2005). The authors estimate that for each dollar transferred, 2 cents were spent on monitoring education and health activities between 1997 and 2000 on average, or equivalently 1.8 percent of the program budget.

Even if we are unable to unravel what fraction is directly related to monitoring the conditionality attached to the primary school scholarships alone, these figures imply that about one fourth of the total program budget is absorbed the primary school costs – i.e. transfer payments plus related administrative costs. It is thus crucial to ensure that there is an adequate return to these activities. However, the little program gains on primary school enrolment reported above do suggest that the current transfer scheme may be unnecessarily expensive.

4. Empirical Strategy

The purpose of our exercise is to examine whether the primary school subsidy has an impact on outcomes other than primary school enrolment. In particular, we compare the average impact of the program between beneficiary households with and without children eligible to receive the primary school scholarships on the following outcomes: household consumption and share of expenses, household investments in productive activities, school enrolment of secondary school aged children, and young children morbidity, health and nutritional status.

4.1. Experimental Evaluation Sample and Data Sources

We benefit from the fact that the Mexican Government was committed to a rigorous evaluation of the impact of OPORTUNIDADES using a controlled-randomized evaluation design. Given budgetary and logistical constraints, the Government could not enrol all eligible families in the country simultaneously and decided to phase in the enrolment of entire communities over time. As part of this process, the Government randomly chose 320 treatment and 186 control communities in seven states for a total of 506 experimental communities in rural areas. Behrman and Todd (1999) statistically assess the validity of the randomization.

Eligible households in treatment communities began receiving benefits in April of 1998; whereas eligible households in control communities were not incorporated until the end of November 1999. In order to minimize anticipation effects, households in control communities were not informed that they would receive OPORTUNIDADES benefits until two months before incorporation. Attanasio et al (2005) test for the existence of anticipation effects amongst control households and find no evidence.

The data we analyze were gathered through two baseline surveys collected in October 1997 (ENCASEH) and March 1998, and the three follow-up surveys (*Encuesta de Evaluación de los Hogares Rurales*, or ENCEL) that were administered before the phase in of control communities in October 1998, May 1999, and November 1999.[9] These data were collected on approximately 24,000 eligible and ineligible households in the 506 communities of the experimental evaluation sample. As noted above, we restrict our sample of analysis to eligible households as classified according to the original classification scheme. In this sample, a straightforward comparison between treatments and controls is possible to obtain 'intention to treat' (ITT) estimates of the program impact on outcomes.

Because neither the ENCASEH nor the ENCEL surveys included basic anthropometric data, we use a third data source for the analysis of child nutrition and morbidity. These data were collected separately by Mexico's National Institute of Public Health (*Instituto Nacional de Salud Pública* or INSP) and consist of a longitudinal rotating child-based sample that partially overlapped with the ENCEL surveys. The data include weight, height, haemoglobin levels in blood, amongst other measurements, for children younger than five (see Rivera et al 2000 for further details). As before, we use data from the two rounds gathered in August/September 1998 and in October/December 1999, before the phase-in of control households.

[9] Three more evaluation surveys were collected in May 2000, November 2000 and November 2003; after the incorporation of control communities to the program by the end of 1999.

However and despite the original sample design, the INSP data was not perfectly balanced by the randomization procedure nor was it a random draw of the ENCEL sample. For example, nutrition data was collected in the state of Morelos, which is not part of the evaluation sample. Other errors in entering individual and locality identifiers make it impossible to link all observations in the INSP data to their mothers or to the household they belong to in the ENCEL sample. This results in a working dataset with substantially less observations than in the original INSP dataset. Moreover, baseline comparisons show that children in the control group have on average a better nutritional status than children in the treatment group (see Behrman and Hoddinott 2001 for further details). The authors also report more significant differences in individual and household characteristics that would be expected by chance. It will be important to bear in mind these caveats when interpreting estimates obtained on this sample.

4.2. Specification and Identification

We rely on household demographic composition to generate sufficient variation to isolate the average impact of the primary school subsidy from the total – i.e. 'program package' – average treatment effect on outcomes. As the program benefit allocation rules in Table 1 put forward, household demographics determine the amount and type of transfers treatment households receive, conditional on household members meeting the relevant requirements. Our empirical approach compares the average program impact on selected outcomes between treatment households with children that could a priori be enrolled in grades 3 to 6 of primary school – i.e. households that qualify to receive the primary school stipend – and treatment households that do not have children in such situation – i.e. households that do not qualify to receive the primary school stipend. More formally, for each outcome of interest Y_{ijt} , we estimate the following reduced form:

$$Y_{ijt} = \alpha_o + \alpha_1 T_{jt} + \alpha_2 D_{jt} + \alpha_3 T_{jt} D_{jt} + \alpha_4 NbD_{jt} + \sum_t \alpha_{5t} WAVE_t + \sum_k \beta_k X_{ijkt} + \nu_j + \xi_{ijt}$$

(1)

where T_{jt} is an indicator of whether household *j* – where individual *i* lives – is in the treatment group at time *t*. D_{jt} is an indicator variable that takes on the value of 1 if there are children in the household *potentially* enrolled in grades 3 to 6 of primary school, and NbD_{jt} represents their number. $WAVE_t$ are wave dummies for each post intervention period; and X_{ijkt} is a vector of current individual characteristics, baseline household demographic structure, and other baseline household and community characteristics.

Whenever available, we include the baseline (October 1997 or March 1998) value of the dependent variable as an additional regressor.[10]

We estimate equation (1) at the individual (i) or at the household (j) level depending on the nature of the dependent variable. If estimated at the household level, v_j is modelled as a household random effect. If at the individual level – and sample sizes permitting – we randomly sample one individual in the household to be included in the analysis and model v_j as an individual random effect.[11] This term allows for serial correlation between individuals (or households) observed repeatedly across the longitudinal survey. ξ_{ijt} is an idiosyncratic disturbance. We cluster standard errors at the community level because of the community clustered sampling and randomization.

Our estimate of interest is the parameter on the interaction $T_{jt}D_{jt}$, $\hat{\alpha}_3$. The test $\hat{\alpha}_3 \neq 0$ (or $\hat{\alpha}_3 > 0$ conditional on $\hat{\alpha}_1 \geq 0$) is equivalent to the test of the treatment effect on outcome Y_{ijt} being significantly different (or larger) for treatment households that receive the primary school stipend – i.e. have children potentially enrolled in grades 3 to 6 of primary school – than for those that do not. Treatment households with children that have completed primary school grades 2 to 5 receive a primary school scholarship per each child effectively enrolled in (and attending) the next primary school grade. The primary school grant(s) is(are) received on top of the nutritional grant, which is received by all treatment households irrespective of their demographic composition. Moreover, if there are children that could potentially be enrolled in secondary school – i.e. children with grade 6 of primary to grade 2 of secondary school completed – the household may additionally receive a secondary school scholarship per each child actually enrolled and attending school. As noted, households in the control group did not receive benefits during the period considered.

We proxy children *actually* enrolled in school with children *potentially* enrolled in order to correct for the possible endogeneity in the household's decision to send their children to primary school and thus benefit from the primary school scholarships. To identify children *potentially* enrolled in primary school, we take the child school enrolment status and grade enrolled at baseline (1997) and predict her school grade at t assuming no grade repetition or drop out. As expected, the number of children potentially enrolled in

[10] The specific covariates included in each outcome regression are listed in the corresponding table footnote. Missing values of these covariates have been replaced with the sample mean and the replacements have been accounted for with dummy variables.
[11] For individual level outcomes, results are robust to including all household members falling in the relevant age category and modeling v_j as a household random effect.

primary school overestimates the number of children effectively enrolled even if the distributions of both variables are very similar. The simple correlation amongst them is 0.76. If we control for time effects and baseline covariates, the number of potential children in primary school explains 0.57 percent of the variation in the number of children effectively enrolled.

In this setting, unbiased identification of $\hat{\alpha}_3$ is then guaranteed by randomization. The random allocation of treatment at the community level implies that the treatment status is orthogonal to the error term, conditional on observables, $T_{jt} \perp \xi_{ijt} \mid (X_{ijt}, D_{jt})$. Thus, $E(\xi_{ijt} \mid X_{ijt}, D_{jt}) = 0$. Because the number of children *potentially* enrolled in primary school is predicted applying the program's benefit allocation rules to baseline household demographic composition and child enrolment status, D_{jt} (and hence NbD_{jt}) is orthogonal to the treatment status by construction $(T_{jt} \perp D_{jt})$. Table 2 shows that household demographic structure is indeed balanced between treatment and control groups not only prior but also during the intervention. Then, by probability laws, the interaction term of interest – namely the marginal impact of the primary school scholarship for households with children *potentially* enrolled in primary school – is also orthogonal to the error term: $T_{jt} D_{jt} \perp \xi_{ijt} \mid (X_{ijt}, D_{ijt})$ and so $E(\xi_{ijt} \mid (T_{jt} D_{jt}, X_{ijt}, D_{ijt}) = 0$.

The interpretation of $\hat{\alpha}_3$ will depend on how the reference group is defined, which will in turn depend on how the estimation sample is defined. Indeed and for each dependent variable, we will consider a different estimation sample in order to have $\hat{\alpha}_3$ identify the impact of the primary school subsidy net of the impact of any other component of the OPORTUNIDADES benefit package. In some situations for example, we will restrict the estimation sample to households with no children that could potentially be enrolled in secondary school ("primary school only" sample) in order to avoid confounding the impact of the secondary school stipend with that of the primary school transfer in households with both children of primary and secondary school age ("primary school" sample). As a matter of fact, about 35 percent of the households in the evaluation sample have children in primary and secondary school age. This may introduce sample selection bias if differently composed households have different probabilities of receiving treatment and/or are systematically different in terms of their observable characteristics.

Table 3 tests the difference in mean pre-intervention characteristics for treatment and control households with different demographic structures. Out of the 48 variables tested at baseline across different household groups, we only find 3 almost statistically significant (significant at the 10 percent) differences. Given the fact that all different subsamples are well balanced, we are confident that treatment and control groups are comparable in terms of their observable (and unobservable) characteristics. These results along with those in Table 2 dismiss the potential for sample selection bias. Nonetheless, it will be crucial to keep in mind which sample we are working with in the discussion of results.

For each dependent variable and estimation sub-sample, we first estimate the average treatment effect of the program package. This estimate corresponds to the coefficient on T_{jt}, $\hat{\alpha}_1$, when $D_{jt} = 0$ for all *j*. The average program impact for most of the outcomes considered has been reported in many OPORTUNIDADES evaluation papers (see Skoufias 2005 for a review of the main evaluation results). As noted, our focus here is on introducing a new dimension in the analysis specific to the hypothesis we want to test – i.e. whether the primary school grant alone has an effect on outcomes other than primary school enrolment. Nonetheless, we report the average program effect on our estimation sub-sample for two reasons: first, to set the benchmark against which to compare our results; and second, to show that the average program impact on this sample is substantially (qualitatively) similar to that reported elsewhere.

For each outcome, we estimate equation (1) on the balanced sample of individuals or households with non-missing information on the dependent variable across the three post-intervention survey rounds considered. The next section provides more details on the data, the sample of analysis and the estimation procedure employed for each outcome variable considered, before discussing results.

5. Effects of the Primary School Subsidy on Other Outcomes: Data and Results

5.1. Household Consumption and Share of Expenditures

We begin by comparing the differential program impact between households with and without children eligible to receive the primary school grant on household expenditure. We consider total per capita adult equivalent expenditures and the share of different types of nondurables expenditures: food, adults' clothing, children's clothing and health.[12] Data on expenditure and consumption on many commodities were not recorded before the implementation of the program and hence we cannot perform an exogeneity test nor control for baseline levels in the regressions. In addition, data on consumption domestically produced (home production), which we include in our measure of consumption, was only recorded in two of the three evaluation rounds: October 1998 and May 1999. The questionnaire asks for expenditures on food consumption items during the last week, on services and transport items over the last month and on

[12] Adult equivalent household size is defined as the number of children aged 12 or younger times 0.5 plus the number of family members older than 12.

clothing over the last six months. To compute shares, we convert all the figures to monthly expenditures. All values are expressed in November 1997 prices (baseline).

Table 4 presents Least Squares household level random effects estimates on household monthly consumption (per capita adult equivalent). In the first column (Model 1), we report an estimate of the average impact of the program, $\hat{\alpha}_1$, on the entire (and balanced) sample of households. Model 2 adds baseline household demographics and characteristics as additional controls.[13] In Model 3, we interact treatment with and indicator of the presence of children potentially enrolled in grades 3 to 6 of primary school in the household and control for the number. Note that in these households there may also be children potentially enrolled in secondary school age. Hence, the coefficient on the interaction, $\hat{\alpha}_3$, 'identifies' the marginal program impact on all households receiving the primary school grant – irrespective of whether they also receive secondary school scholarships – on per capita consumption. In this case, the reference group is constituted by control households with no children potentially enrolled between grades 3 and 6 of primary school.

In contrast, the interaction term in Model 4 takes on the value of 1 for treatment households with children potentially enrolled in grades 3 to 6 of primary school *and* no children potentially enrolled in secondary education. This means that the approximately 25 percent of households with children potentially enrolled in any grade between the third grade of primary and the third grade of secondary school are now included in the reference group (if they live in a control household) or under the 'Treatment =1' label (if they live in a treatment household). The purpose of this specification is to net out the contribution of the secondary school grant to the estimated impact of the primary school grant on consumption reported in Model 3. Finally, the specifications in Models 5 to 7 are analogous to those in Models 1 to 3 but the estimation sample is now restricted to households with no secondary school aged children.

The reported coefficients in Models 1 and 2 show that the monetary transfers from the program increase monthly per capita (adult equivalent) consumption by 21 to 23 pesos on average, or a 11 to 12 percent increase. The size of this effect is similar to that reported in previous research on OPORTUNIDADES: Hoddinott et al (2000) and Gertler et al (2008) estimate 13 to 14 percent increases in monthly per capita consumption amongst treatment households, respectively. Models 5 and 6 show similar average treatment effects amongst the restricted sample of households with no children potentially enrolled in secondary school. Moreover, the average treatment effect is not significantly different for households with primary

[13] See the table notes for the complete list of covariates.

school aged children (Model 3) nor for households with children in primary school age but not in secondary school age (Models 4 and 7). Note that, $\hat{\alpha}_3$, the coefficient on the interaction of interest – treatment interacted with presence of children potentially enrolled in the eligible primary school grades in the household – takes values between 0.15 and 2.70 pesos and is never statistically different from zero. This explains why the estimated coefficient on the treatment dummy, $\hat{\alpha}_1$, stays rather constant or decreases only marginally across specifications. These results suggest no differential effects on consumption amongst households eligible to receive the primary school stipend, implying that the conditional on attendance primary school transfer has no direct impact on household consumption.

On average, food consumption represents 75 percent of overall expenditures in these poor rural households (Attanasio and Lechene 2002). While the program has not altered the budget share of food expenditures (results available upon request), results in Table 5 show that there has been a reallocation of the food share from less to more nutritious (and expensive) food. In treatment households, there is a 7 percent decrease in the share of cereal expenditures over total food expenditures (Models C) and an 11 and 14 percent increase in the share of vegetables (Models A) and meat expenditures (Models B).[14]

Consistent with the estimates on monthly consumption in Table 4, the size of the average program effect on food shares is rather constant across specifications (Models 1 to 7). Moreover, we find no significantly different impact for households eligible to receive the primary school transfer. The only exception is the almost significant increase (at the 10 percent level) in the share of meat (Table 5, Model 3B). However, this effect disappears as we narrow the scope of the coefficient to the effect of the primary school transfer alone, as opposed to that of the primary and secondary school transfers (Table 5, Models 4B and 7B). Overall, these findings suggest that all households alter their food shares in a similar fashion in response to the program incentives, regardless of the specific nature of these incentives.

Next, we present results on the budget share (over total household consumption including home production) of the three nondurable items where we found a significant average treatment effect: children's clothing, adults' clothing and health expenditures. As shown in Table 6 and consistent with Hoddinott et al (2000), OPORTUNIDADES increases household expenditures in children and adult clothing (Models A and B) and reduces the budget share of health expenditures (Model C). The later result may be a consequence of the increased health services that the program provides and/or the improved caloric intake and in consequence improved health status of beneficiary household members.

[14] Hoddinott et al (2000) and Hoddinott and Skoufias (2004) also find that the program impact on increased calorie intake is concentrated amongst these same food categories: fruits and vegetables, and animal products.

The coefficient on the interaction of interest, $\hat{\alpha}_3$, is now positive and significant for the children's clothing equation. This result is relevant for two reasons: first, it suggests that treatment households use the primary school transfer to buy shoes and clothes for children in school age; and second, it validates the identification strategy we use. Regarding the share of adult clothing and health expenditures, $\hat{\alpha}_3$ has the opposite sign to the main treatment effect and is almost significant (significant at the 10 percent) in some specifications. This indicates that the average treatment effect observed is not fuelled through the primary school stipend.

5.2. Investment in Productive Assets and Activities

Gertler et al (2008) argue that OPORTUNIDADES can raise long term living standards permanently amongst beneficiary households through boosting investment in micro-enterprise and agricultural activities (farm animals and land for agricultural production). On average, these investments generate an estimated return of 15 percent. The question we pose here is whether the investment observed in productive activities is financed by the primary school scholarships as the conditionality attached to these – i.e. primary school attendance – is already binding. For this purpose, we compare the impact of the program between beneficiary households with and without children eligible to receive the primary school grant on household investment in draft and production animals and micro-enterprises.

Following Gertler et al (2008), we define draft animals to be those traditionally used for plowing and/or transportation. These include donkeys, mules, horses and oxen. Production animals are those whose meat and/or by-products (milk, cheese, eggs, etc) are sold or consumed. These include goats and sheep, cows, chickens, hens and turkeys, pigs and rabbits. Data on animal ownership was collected at baseline and on each evaluation round, and refers to asset holdings during the 12 months preceding the interview. We include baseline ownership of draft and production animals as an additional control in all regressions.[15]

The indicator variable micro-enterprise takes on the value of 1 if any household member had engaged in a "self-motivated" non-agricultural activity during the month before the interview. Activities considered are sewing clothes, making food for sale, carpentry and construction, sale of non-food items such as handcrafts, repair of artefacts or machinery, domestic service and other activities done on your own. No

[15] Information on the number of draft and production animals that the household owns is also available. Results on the continuous outcome (number of animals owned) are qualitatively similar to those on animal ownership and are available upon request.

information on micro-enterprise activity was collected at baseline. For both animal ownership and micro-enterprise, we estimate a Least Squares model with random effects at the household level.

Estimates in Table 7 show similar average treatment effects on the probability of owning draft animals (Models A), production animals (Models B) and of engaging in micro-entrepreneurial activities (Models C) on our estimation sub-samples to those reported in Gertler et al (2008). However and in contrast with what observed in the consumption and food share regressions, we find that program impact on animal ownership varies with household composition and hence with the type of benefits the household receives.

On the one hand, draft animal ownership increases in households without children potentially enrolled in school – i.e. households that receive the nutritional stipend alone – but not in households with children potentially enrolled in grades 3 to 6 of primary school (see Models 3A and 7A in Table 7). On the other hand, the observed increase in production animal holdings seems to be at least partly driven by the primary school transfer. When we include the interaction treatment status with presence of children potentially enrolled in grades 3 to 6 of primary school in the household, the coefficient on the treatment dummy is no longer significant even if it remains positive. The coefficient on the interaction, however, is positive in all specifications (Models 2B, 3B and 7B in Table 7). For the restricted sub-sample of households without secondary school aged children, this coefficient is significant and similar in magnitude to the estimated treatment effect (compare Models 7B and 2B in Table 7).

As shown in Models C, the increase in micro-enterprise participation due to the program is not significantly different across households eligible to receive different benefit types.

5.3. Secondary School Enrolment

As noted earlier, secondary school enrolment rates were, on average, 67 percent for girls and 73 percent for boys prior to the intervention, indicating that many teenagers in these rural communities drop out of the educational system upon primary school completion. By increasing the opportunity cost of secondary school, and more so for girls (see Table 1), OPORTUNIDADES successfully reversed this tendency. Schultz (2004) estimates an average increase in secondary school enrolment of 14 percent for girls and 8 percent increase for boys over a year an a half of benefits, suggesting that households could not afford to send their children to secondary school due to liquidity constraints.

If so, one may expect that treatment households with children in primary and secondary school age use the 'extra' primary school scholarship to overcome binding liquidity constraints and thus facilitate secondary school enrolment. For example, the primary school grant may help pay tuition fees and other secondary school related costs. Or alternatively, they may compensate for the contribution teenagers make to household income (through wages or though household production), etc. As a result, we should observe larger program impacts on secondary school enrolment in households that benefit from both the secondary *and* the primary school grant, besides the 'universal' nutritional grant. In this subsection, we empirically investigate this issue.[16]

We report findings in Table 8, which has a very similar structure to the Tables discussed in the previous subsections. The first set of coefficients in Models A presents results on the probability of enrolment in grades 1 to 3 of secondary school. In this case, we work with the balanced sample of teenagers aged 12 to 16 that have completed primary school but not secondary school at any point in time over the three post-intervention rounds considered (October 1998, May 1999 and November 1999). In households with more than one teenager with these characteristics, we randomly select one individual to include in the sample. We then model the specific component of the error term as an individual (teenager) specific random effect.

Consistent with the findings in Schultz (2004), our estimates show that secondary school enrolment increases amongst teenagers in treatment households (Models 1A and 2A). However, we find no evidence of heterogeneous impacts by type of benefits received. The parameter estimate of interest, $\hat{\alpha}_3$, is negative and significant at the 10 percent (Model 3A), suggesting no differential effect on secondary school enrolment for teenagers in households with children eligible to receive the primary school grant. In Models 4A to 6A, we further restrict the estimation sample to teenagers living in households with no children younger than seven and find similar results.

The second set of coefficients in Models B report estimates of impact on the probability of secondary school enrolment for students in the transition from primary to secondary education. We now work with the pooled sub-sample of teenagers aged 12 to 16 with primary school completed but never enrolled in secondary school at any time in October 1998, May 1999 or November 1999; and estimate a household level random effects model. Results are very similar to those described above.

5.4. Child Health

[16] We have also used cross-sectional data on time use to explore whether the allocation of teenagers' time between school and work activities (paid and unpaid) is differently altered in households eligible and non-eligible to receive the primary school stipend, and found no evidence (results available upon request).

OPORTUNIDADES promotes investment in early childhood health through nutrition supplements, health checkups, child immunization, growth monitoring and the "pláticas" (health, hygiene and educational talks). Moreover, the nutritional stipend is intended for families to invest in improved consumption. As reported in Section 5.1, treatment households increase their food share of meat and vegetables, and reduce their relative consumption of cereals and grain. Similarly, Hoddinott and Skoufias (2004) report increases in food availability both in terms of quantity (calories) and quality (richer in protein and micronutrients) amongst beneficiary households. In this section, we examine whether the primary school grant has a 'direct' impact on child morbidity and on nutritional outcomes.

5.4.1. Maternal Reports on Child Morbidity

We first look at the probability that a mother reports that her child experienced an illness in the 4 weeks prior to the interview. This measure of child morbidity was collected in three evaluation surveys – October 1998, May 1999 and November 1999 – as part of the main evaluation survey (ENCEL). In addition, we take March 1998 information as baseline and include it in the regression to control for individual pre-intervention morbidity and maternal reporting bias.[17] Results on the number of days the mother reports her child is sick – a measure of the duration of illness – are very similar to the ones discussed below on the occurrence of illness, and are available upon request.

Following Gertler (2004), we estimate the model separately for children aged 0 to 35 months at baseline – hence 24 to 59 months during the evaluation period – and for babies born once the intervention was already in place. The newborn sample is restricted to children older than one month that first appear in the data in the May 1999 survey round. If in the treatment group, these children have also been exposed to prenatal care benefits for at least 6 or 7 months, besides being exposed to all other OPORTUNIDADES benefits (nutritional supplements, vaccination, health care, etc).[18]

Table 9 presents results on the incidence of illness for children 0 to 35 months (Models A) at baseline and for newborns (Models B). As in previous tables, the first column (Models 1) reports the estimate of the average impact of the program, $\hat{\alpha}_1$, for the balanced sample of children in the age category of interest – less than 3 years at baseline or newborn by May 1999 – living in households with children in primary school age or younger. As before, if there is more than one child in the household in the relevant age group we randomly select one child (per household) and estimate an individual random effects model. We condition on the incidence of sickness in March 1998 (for children 0 to 35 months at baseline only) and

[17] See Gertler and Boyce (2001) for a discussion on self-reporting biases in the data and ways to address them.
[18] Recall that most treatment households were phased-in between March/April 1998 and October/November 1998.

on the number of children in the household in the age category of analysis. Models 2 additionally control for individual and baseline household demographics and characteristics. As in previous tables, in Models 3 we interact treatment with an indicator of the presence of children potentially enrolled in grades 3 to 6 of primary school in the household (regardless of the presence of children in secondary school age) and control for their number. To net out the contribution of the secondary school grant in $\hat{\alpha}_3$ for the approximately 16 percent of households with children both in primary and secondary school age, the interaction term in Model 4 takes on the value of 1 for treatment households with children potentially enrolled in grades 3 to 6 of primary school *and* no children potentially enrolled in secondary education. The specifications in Models 5 to 7 are analogous to those in Models 1 to 3 but on the restricted sample of households with no secondary school aged children.

The first set of results in Table 9 suggests that children younger than 3 at baseline living in treatment households are less likely to be sick than similar children in control households. This evidence is robust across specifications (Models 1A to 7A) and is consistent with Gertler and Boyce (2001) and Gertler (2004). The treatment dummy remains negative and significant after controlling for the presence of children potentially enrolled in primary school in treatment households (Models 3A, 4A and 7A). In fact, we observe that the reductions in young children morbidity are smaller for households eligible for primary school scholarships than for non-eligible households. Even if these differences are not significant, they suggest that the estimated reduction in morbidity for 0 to 3 year olds is not a 'direct consequence' of the primary school transfer. Arguably, these children may also be more exposed to the virus and bacteria their primary school aged siblings bring home from school.

The evidence on the newborn sample is less straightforward. As the second set of results in Table 9 shows, and consistent with previous evidence, the average treatment effect on occurrence of illness for newborns is negative and significant (Models 1B, 2B, 5B and 6B). However, once we disaggregate the effect of the primary school grant from that of the overall OPORTUNIDADES package – i.e. once we control for the presence of children potentially enrolled in grades 3 to 6 of primary school in treatment households – the coefficient on the treatment dummy is no longer significant, even if it remains negative. Moreover, the coefficient on the interaction term is negative, which suggests similar reductions in newborn morbidity both in households eligible and non-eligible to receive the primary school scholarships. Alternatively, and given the (relatively) small sample sizes, the coefficient on the treatment dummy in Models 3B, 4B and 7B may no longer be significant due to a lack of power.

Next we look at two different measures of child morbidity as reported in the INSP nutrition data, for the first time in the May 1999 follow up round. A first indicator of morbidity measures the occurrence of diarrhea over the two weeks prior to the interview for children younger than five. A second measurement takes on the value of one if the child has had a respiratory problem or infection during the two weeks before the interview. The following respiratory problems are included: cold, flu, throat infection, bronchitis and pneumonia. In both cases, the information is based on maternal reports on the number of days the child has had diarrhea or a respiratory incidence. Results at the intensive margin (duration of incidence) are qualitatively very similar to the ones reported below on incidence and are available upon request.

For this part of the analysis, we work with all children with non-missing information on the dependent variable in May 1999. We estimate linear probability models separately for children in different age groups: 12 to 23 months, 24 to 35 months and 36 to 59 months, at the time of the survey. We construct age in months using information on when the interview was conducted and date of birth, which we consider a more accurate measure than age as reported in the survey.[19] As usual, for households with more than one child in each age group, we randomly select one child (per household) to be included in the analysis. Results are presented in Tables 10 and 11 for diarrhea and respiratory infections respectively. The structure of the table is similar to that discussed above. Each block of results presents estimates on a different age group. For each group, the first three columns report estimates on the sample of children living in households with children in primary school age or younger, irrespective of whether there are also children in secondary school. In the remaining two columns, we restrict the analysis to the sample of households with no children potentially enrolled in primary *and* in secondary school. Due to space restrictions, we report results on the specifications that include controls for individual, maternal, household and community characteristics (listed in the tables' footnotes).

Following Behrman and Hoddinott (2001, 2005), we redefine treatment such that the treatment dummy no longer represents the household (or locality) treatment status but rather whether each eligible child in treatment households has effectively received the nutritional supplements. In other words, treatment is now conditional on actual intake of the nutritional supplement, which is the component of the OPORTUNIDADES benefit package that is more likely to affect the health status of young children. We will refer to these estimates as '*treatment on the treated*' (TOT) estimates. Behrman and Hoddinott (2001, 2005) report substantial mismatches between treatment status (intended treatment) and actual supplements intake (actual treatment). Indeed, anecdotal evidence suggests substantial contamination

[19] The distributions of age reported and constructed (using date of interview and date of birth) are quite similar.

across household members and from treatment to control children, partly because staff at the health centres distributed supplements amongst children at their own discretion and on the basis of need (as opposed to on the basis of the child's treatment status).

Consistent with the results on morbidity above using the ENCEL data, the TOT estimates in Table 10 show that children aged 12 to 35 in treatment households and that received supplementation are less likely to suffer from diarrhea than similar children in control households. This effect is particularly strong for the younger cohort, namely children 12 to 23 months. Moreover, for these children, the interaction term of interest is positive and highly significant, suggesting that they have a larger probability of having diarrhea if they live in treatment households with children (siblings or other relatives) in primary school age. A potential explanation for this result may be in that these older children bring home viruses and other infections they caught in school and transmit them to their very young (and vulnerable) siblings. In any case, this evidence indicates that the reduction in child morbidity (i.e. occurrence of diarrhea) for children between one and two years of age is not a 'direct consequence' of the primary school transfer. Estimates in Models 2B, 3B and 8B in Table 10 show that treatment children ages 24 to 35 months that received the supplements, experienced qualitatively similar reductions in the occurrence of diarrhea both in households with and without children potentially enrolled in primary school. Even if the negative coefficient in Table 10 is not significantly different from zero, these results suggest that we cannot rule out that the primary school stipend plays a role in reducing diarrhea for children 24 to 35. Results in Table 11 show no significant program impact on reducing the occurrence of respiratory problems for children in any of the different age groups considered.

A note of concern on the interpretation of these results is in order for two reasons. First, the small sample sizes we are working with; and second, the lack of longitudinal data. The latter implies that we cannot control for individual specific unobserved heterogeneity, including systematic biases in the maternal reports and the possibility that past health states may affect current health. This problem is even more important given that the lack of balance between treatment and control groups in the INSP, as discussed in Section 4.1.

5.4.2. Child Nutritional Status: Blood Test and Anthropometric Measures

In this final section, we focus on objective health outcomes based on height, weight and hemoglobin measurements available for children in the INSP sample. Specifically, we look at anemia as a measure of micro-nutrient deficiency; and stunting and underweight as indicators of long and short term under-nutrition, respectively.[20] Following Rivera et al (2004), we define anemia as serum hemoglobin concentrations lower than 11g/dL. For children living in altitudes above 1000 meters, the cutoff value is adjusted according to the adjustment developed for Mexican populations in Ruiz-Argüelles and Llorente-Peters (1981).[21] Stunting and underweight are defined, respectively, as height for age and weight for age z-scores two standard deviations below the international WHO reference group by age and sex.[22] Rivera et al (2004) provide specific details on how the measurements were collected and processed.

As for the morbidity measures described above, we estimate linear probability models separately for children in different age groups – 12 to 23 months, 24 to 35 months and 36 to 59 months – according to our constructed age measurement (using date of interview and date of birth). As usual, for households with more than one child in each age group, we randomly select one child (per household) to be included in the analysis. Information on haemoglobin levels is available only in May 1999, and hence we use this cross-section to run a linear probability model on the probability of the child being anaemic. Height and weight measurements were recorded both at the 'baseline' round of the INSP data (August/September 1998) and at follow up (May 1999). Following Behrman and Hoddinott (2201, 2005), we use both data rounds to construct a balanced panel of children with non-missing information in either wave and estimate a fixed effect model, thus controlling for individual unobserved heterogeneity.[23] In both cases, the relevant treatment variable is treatment status and intake of supplements; this is to say, actual treatment (TOT).

Estimates for anaemia, stunting and underweight are presented in Tables 12, 13 and 14, respectively. The structure of each table is analogous to those showing results for diarrhea (Table 10) and respiratory infections (Table 11) and discussed above. First of all, we notice that the impacts of the program on these outcomes – arguably amongst the most important as they concern children that are young enough so that their outcomes will be extremely persistent – are not very strong and not very robust. TOT estimates of the effect of OPORTUNIDADES on anemia show significant reductions in the probability of being

[20] We do not consider wasting (weight-for-height two standard deviations below the reference group median) given the very low prevalence rates in the OPORTUNIDADES rural data (3 to 4 percent).
[21] Following Villalpando et al (2004), we drop outliers for hemoglobin at values below 4.5 gr/dL or over 18.5 gr/dL.
[22] See WHO 2006 Child Growth Standards at http://www.who.int/childgrowth/standards/en/. Observations with outlier values for age, height or weight, as flagged by the Stata software igrouwup_stata, available at http://www.who.int/childgrowth/software/en/, have been dropped from the sample. To our knowledge, previous evaluation studies on OPORTUNIDADES have used an older version of the WHO standards. The main improvement of the new standards is that they are based on children from widely different ethnic backgrounds and cultural settings (Brazil, Ghana, India, Norway, Oman and the USA). As noted by the publishers, the new standards are likely to result in larger stunting rates. For underweight, weights are likely to increase substantially from 0 to 6 months and to decrease thereafter (see http://www.who.int/childgrowth/faqs/change_estimate/en/index.html).
[23] We thank Jere Behrman and John Hoddinott for sharing their data with us.

anemic for children ages 36 to 59, but not for other groups. This effect is not significantly different for children that take the supplementation and live in treatment households where there also are children potentially enrolled in primary school (Table 12). Models C estimates in Table 13 show a similar pattern on the probability of low height for age (stunting) for children in this same age group. However, the TOT parameter estimate is only significant when we work with children in the restricted sample of households with no children potentially enrolled in primary and no children potentially enrolled in secondary school (Models 4C and 5C). We observe significant effects for children ages 24 to 35 in treatment households with and without older children eligible to receive the primary school stipend. However, while the effect is negative and significant for children 24 to 35 months in treatment households with no older children potentially enrolled in primary school; it is positive (!) and significant for children 24 to 35 months in households eligible to receive the primary school stipend. One explanation is that in these households, older (primary school aged) children are eating part of the supplements directed to their younger siblings. Results in Table 14 show no significant program impact on underweight for children in any of the different age groups considered.

6. Conclusions

During the last decade, many governments worldwide have undertaken CCT programs similar to and inspired by OPORTUNIDADES to fight the intergenerational transmission of poverty. These programs can be large and expensive, which motivates the quest for greater efficiency in increasing their impacts on human capital formation. In the specific case of OPORTUNIDADES, it is now well established in the literature around the program's evaluation that the package of benefits the program provides has had marginal effects on primary school enrolment, of about one percentage point increase (Schultz 2004, Behrman et al 2001). Because primary school enrolment and completion rates are almost universal in rural Mexico, this finding is not too striking. However, it does raise concerns about an inefficiency of the current transfer scheme associated to the fact that by offering the primary school grants conditional on primary school enrolment and attendance, the program is paying people to do what they would do anyway.

Previous research has considered alternative transfer schemes to improve the program cost-effectiveness. For example, de Janvry and Sadoulet (2005) propose re-directing all resources associated to the primary school transfer to children in the transition from primary to secondary school. Similarly, Attanasio et al (2007) show that a budget-neutral switch of the primary school grant from primary to secondary school age would have a greater impact in school attainment via stimulating secondary school enrolment. A more extreme option, that would involve substantial budget savings, would involve removing the primary school component of the program altogether. However, equity and distributional concerns may argue against this alternative. OPORTUNIDADES was conceived as a general anti-poverty measure disguised as a schooling subsidy rather than as a policy purely designed to induce children into school participation. The primary school grant may have other effects, for example, on health and cognitive as well as physical development of other household members, through improved consumption or other.

In this paper, we have investigated whether the primary school transfer generates positive externalities in the household. All OPORTUNIDADES beneficiaries receive a health package, which includes a nutritional grant and preventive health visits for all households members. In addition, households with children in school age receive an educational fellowship for each child enrolled in school between the third grade of primary and the third grade (last) of secondary school. We have exploited the randomized nature of the data and baseline household structure and conditions to isolate the impact of the primary school transfer from the overall average treatment effect. Specifically, we have analyzed the following outcomes: children health and morbidity, household consumption and share of expenses, household investments, and secondary school enrolment. The purpose of this research is to contribute to the understanding of the relative efficiency of each program subcomponent and to the debate on how to improve the program's cost-effectiveness overall.

Our very preliminary findings suggest that there are little effects of the primary school grant on other outcomes. Overall, the program impacts are not substantially different between treatment households eligible to receive the primary school grant and non-eligible treatment households. The only exceptions are on household investment on production animals, the share of children clothing and very young children morbidity. Arguably, the effect of the primary school grant on children clothing may be explained by a non-separability argument – i.e. children need appropriate clothes and shoes to go to school. It is also important to bear in mind that our findings on child morbidity are inconclusive given low sample sizes and the poor quality of the data. Hence, at this stage, we acknowledge that further research on this issue is still needed to be able to provide compelling evidence on the real impacts and relative effectiveness of the primary school stipend.

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8. Appendix: Tables

Table 1: OPORTUNIDADES Monthly Transfer Amounts at Baseline (Oct 1997)

Transfer Component	Level	Grade	Boys	Girls
Education Stipend	Primary School	3rd year	60	60
		4th year	70	70
		5th year	90	90
		6th year	120	120
	Junior High School	1st year	175	185
		2nd year	185	205
		3rd year	195	225
School Supplies Stipend	Primary, 1rst payment		80	80
	Primary, 2nd payment		40	40
	Junior High School		150	150
Nutritional Stipend (per family)			90	
Transfer Cap I (per family)[1]			550	

Source: OPORTUNIDADES (www.oportunidades.gob.mx). Transfer amounts adjusted for inflation every semester according to the Consumer Price Index published by the Bank of Mexico.

[1]Transfer Cap I is the maximum transfer amount awarded for basic education (primary and secondary) and nutrition

Table 2: Balance across Households with Different Demographic Structures (Baseline and Evaluation Period)

	Treatment Group			Control Group			
	N	Mean	SD	N	Mean	SD	t-stat
Baseline (October 1997)							
Households with Preschool Children Only =1	6124	38.766	0.487	3695	38.809	0.487	-0.026
Households with Children in Primary =1	6124	56.662	0.496	3695	56.536	0.496	0.083
Number of Children in Primary	3470	1.617	0.753	2089	1.601	0.739	0.602
Households with Children in Secondary =1	6124	22.175	0.415	3695	22.246	0.416	-0.043
Number of Children in Secondary	1358	1.261	0.492	822	1.240	0.478	0.970
Households with Children in Primary Only =1	6124	39.059	0.488	3695	38.945	0.488	0.086
Households with Preschoolers and Children in Primary =1	6124	95.428	0.209	3695	95.345	0.211	0.137
Households with Preschoolers and Children in Primary Only =1	6124	77.825	0.415	3695	77.754	0.416	0.043
Evaluation Years (Oct 1998, May 1999, Nov 1999							
Households with Preschool Children Only =1	18372	31.287	0.464	11085	30.690	0.461	0.437
Households with Children in Primary =1	18372	61.909	0.486	11085	62.246	0.485	-0.256
Number of Children in Primary	11374	1.740	0.818	6900	1.708	0.830	1.230
Households with Children in Secondary =1	18372	34.291	0.475	11085	34.452	0.475	-0.109
Number of Children in Secondary	6300	1.281	0.502	3819	1.269	0.503	0.746
Households with Children in Primary Only =1	18372	34.422	0.475	11085	34.858	0.477	-0.414
Households with Preschoolers and Children in Primary =1	18372	93.196	0.252	11085	92.936	0.256	0.463
Households with Preschoolers and Children in Primary Only =1	18372	65.709	0.475	11085	65.548	0.475	0.109

Notes: Balanced panel of households. T-stat of differences in means computed clustering SE at the community level. Mean of dichotomous variables expressed in percentages. "Preschool Children Only" refers to households with no children potentially enrolled in primary nor secondary school. Note that this group also includes older households with no children at all. "Children in Primary" refers to households with children potentially enrolled in primary school irrespective of whether there are also children potentially enrolled in secondary school. "Children in Primary Only" excludes those households with children simultaneously enrolled in primary and in secondary school. Number of children in primary and secondary are computed using baseline household demographic information and chidlren enrolment status (potential variables).

Table 3: Test of Equality of Means for Explanatory Variables at Baseline by Household Composition

	Treatment Group			Control Group			
	N	**Mean**	**SD**	**N**	**Mean**	**SD**	**t-stat**
All Households							
Age Household Head	6124	42.193	14.012	3695	42.735	14.511	-1.204
Head's Education (Years)	4245	4.077	2.300	2547	3.920	2.181	1.536
Indigenous Head =1	6124	42.309	0.494	3695	44.817	0.497	-0.441
Household Size	6070	5.961	2.366	3671	5.982	2.363	-0.269
Household Crowding Index	6070	0.306	0.214	3671	0.302	0.228	0.473
Home Ownership =1	6076	94.388	0.230	3672	93.192	0.252	1.428
Dirt Floor (Household) =1	6124	72.812	0.445	3695	75.589	0.430	-1.056
Bathroom (Household) =1	6124	54.654	0.498	3695	54.425	0.498	0.068
Production Animal Ownership (Household) =1	6124	82.397	0.381	3695	83.843	0.368	-0.791
Land Owned (Household) (Ha)	3727	2.691	2.776	2158	2.923	2.790	-1.390
Mobile Health Center in the Community =1	6124	71.604	0.451	3695	73.857	0.439	-0.420
Distance to Secondary School (Km)	4781	3.149	1.871	2854	3.264	2.371	-0.356
Households with Preschool Children "Only" [1]							
Age Household Head	2374	41.191	17.664	1434	41.462	18.197	-0.343
Head's Education (Years)	1561	4.400	2.369	906	4.237	2.303	1.164
Indigenous Head =1	2374	42.628	0.495	1434	44.282	0.497	-0.272
Household Size	2336	4.424	1.858	1416	4.479	1.885	-0.613
Household Crowding Index	2336	0.376	0.271	1416	0.368	0.277	0.534
Home Ownership =1	2339	91.962	0.272	1417	89.555	0.306	1.902+
Dirt Floor (Household) =1	2374	77.043	0.421	1434	79.777	0.402	-1.019
Bathroom (Household) =1	2374	46.251	0.499	1434	44.421	0.497	0.485
Production Animal Ownership (Household) =1	2374	76.369	0.425	1434	76.499	0.424	-0.056
Land Owned (Household) (Ha)	1259	2.433	2.499	711	2.725	2.582	-1.431
Mobile Health Center in the Community =1	2374	72.325	0.447	1434	73.013	0.444	-0.112
Distance to Secondary School (Km)	1927	3.261	2.033	1139	3.561	2.785	-0.621
Households with Children in Primary [2]							
Age Household Head	3470	42.394	10.911	2089	43.064	11.262	-1.875+
Head's Education (Years)	2496	3.900	2.230	1516	3.762	2.086	1.193
Indigenous Head =1	3470	41.844	0.493	2089	45.045	0.498	-0.551
Household Size	3456	7.016	2.119	2085	7.015	2.110	0.021
Household Crowding Index	3456	0.256	0.147	2085	0.256	0.179	-0.040
Home Ownership =1	3459	95.808	0.200	2085	95.252	0.213	0.667
Dirt Floor (Household) =1	3470	70.173	0.458	2089	73.097	0.444	-1.002
Bathroom (Household) =1	3470	59.654	0.491	2089	60.507	0.489	-0.259
Production Animal Ownership (Household) =1	3470	86.167	0.345	2089	88.320	0.321	-1.185
Land Owned (Household) (Ha)	2273	2.813	2.904	1337	3.020	2.906	-1.137
Mobile Health Center in the Community =1	3470	71.182	0.453	2089	74.150	0.438	-0.557
Distance to Secondary School (Km)	2650	3.101	1.769	1608	3.086	2.057	0.062
Households with Children in Primary "Only" [3]							
Age Household Head	2392	41.809	11.434	1439	42.591	11.749	-1.781+
Head's Education (Years)	1680	3.877	2.267	1016	3.715	2.108	1.257
Indigenous Head =1	2392	40.468	0.491	1439	44.614	0.497	-0.708
Household Size	2379	6.812	2.153	1435	6.815	2.114	-0.026
Household Crowding Index	2379	0.255	0.148	1435	0.255	0.192	-0.048
Home Ownership =1	2382	95.130	0.215	1435	94.913	0.220	0.218
Dirt Floor (Household) =1	2392	71.948	0.449	1439	73.801	0.440	-0.617
Bathroom (Household) =1	2392	55.477	0.497	1439	56.011	0.497	-0.154
Production Animal Ownership (Household) =1	2392	84.574	0.361	1439	87.352	0.333	-1.485
Land Owned (Household) (Ha)	1539	2.731	2.803	908	3.028	2.914	-1.564
Mobile Health Center in the Community =1	2392	71.614	0.451	1439	74.010	0.439	-0.443
Distance to Secondary School (Km)	1908	3.215	1.869	1145	3.229	2.192	-0.049

Notes: +significant at 10%. Balanced panel of households. T-stat of differences in means computed clustering SE at the community level. "Preschool Children Only" refers to households with no children potentially enrolled in primary nor secondary school. Note that this group also includes older households with no children at all. "Children in Primary" refers to households with children potentially enrolled in primary school irrespective of whether there are also children potentially enrolled in secondary school. "Children in Primary Only" excludes those households with children simultaneously enrolled in primary and in secondary school.

Table 4: Total vs. Primary Grant Impacts on Household Monthly Expenditures

	Monthly Expenditures Per Capita Adult Equivalent						
	Mod 1	Mod 2	Mod 3	Mod 4	Mod 5	Mod 6	Mod 7
Treatment =1	22.734**	20.824**	19.303**	20.599**	20.633**	19.095**	17.878**
	(4.688)	(3.466)	(4.353)	(3.581)	(5.038)	(3.938)	(4.567)
Treatment * Children in Primary =1			2.293				
			(3.292)				
Treatment * Children in Primary Only =1				0.146			2.254
				(3.165)			(3.808)
Children in Primary = 1			2.699				
			(3.206)				
Children in Primary Only =1				-0.378			5.255
				(2.744)			(4.129)
Number Children in Primary			4.715**				
			(1.139)				
Number Children in Primary Only				6.194**			5.562**
				(1.085)			(1.623)
Covariates	N	Y	Y	Y	N	Y	Y
Wave Dummies (98O, 99M)	Y	Y	Y	Y	Y	Y	Y
Number of Observations	20794	20794	20794	20794	14302	14302	14302
Number of Households	10397	10397	10397	10397	7151	7151	7151
Proportion Children in Primary			0.603	0.354			0.513
Proportion Treat Hhs w/ Children in Primary			0.379	0.223			0.322
Mean Dependent Variable	183.885	183.885	183.885	183.885	190.235	190.235	190.235

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. LS regressions with RE at the household level. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. When specified, the following controls are included: head's age, age squared, education and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7 and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). Outliers trimmed at the top 0.5% of the dependent variable distribution.

Table 5: Total vs. Primary Grant Impacts on Share of Household Food Expenditures

	Share of Fruits and Vegetables							Share of Meat							Share of Cereal and Grain						
	Mod 1A	Mod 2A	Mod 3A	Mod 4A	Mod 5A	Mod 6A	Mod 7A	Mod 1B	Mod 2B	Mod 3B	Mod 4B	Mod 5B	Mod 6B	Mod 7B	Mod 1C	Mod 2C	Mod 3C	Mod 4C	Mod 5C	Mod 6C	Mod 7C
Treatment =1	0.014**	0.013**	0.015**	0.013**	0.014**	0.013**	0.014**	0.022**	0.021**	0.017**	0.021**	0.019**	0.019**	0.017**	-0.022*	-0.024**	-0.025**	-0.022**	-0.024**	-0.026**	-0.027**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
Treatment * Children in Primary =1			-0.004+							0.007+							0.002				
			(0.002)							(0.004)							(0.007)				
Treatment * Children in Primary Only =1				-0.001			-0.002				-0.000			0.005				-0.004			0.001
				(0.002)			(0.003)				(0.004)			(0.005)				(0.007)			(0.008)
Children in Primary = 1			0.001							-0.001							0.003				
			(0.002)							(0.004)							(0.006)				
Children in Primary Only =1				-0.001			-0.000				0.003			0.003				0.007			-0.001
				(0.002)			(0.003)				(0.003)			(0.005)				(0.005)			(0.007)
Number Children in Primary			0.001							-0.001							0.001				
			(0.001)							(0.002)							(0.002)				
Number Children in Primary Only				0.000			0.001				-0.001			-0.001				0.001			0.001
				(0.001)			(0.001)				(0.001)			(0.002)				(0.002)			(0.003)
Covariates	N	Y	Y	Y	N	Y	Y	N	Y	Y	Y	N	Y	Y	N	Y	Y	Y	N	Y	Y
Wave Dummies (98O, 99M)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	20708	20708	20708	20708	14250	14250	14250	20708	20708	20708	20708	14242	14242	14242	20714	20714	20714	20714	14266	14266	14266
Number of Households	10354	10354	10354	10354	7125	7125	7125	10354	10354	10354	10354	7121	7121	7121	10357	10357	10357	10357	7133	7133	7133
Proportion Children in Primary			0.603	0.355			0.515			0.603	0.355			0.514							
Proportion Treat Hhs w/ Children in Primary			0.380	0.223			0.323			0.380	0.224			0.324							
Mean Dependent Variable	0.123	0.123	0.123	0.123	0.124	0.124	0.124	0.151	0.151	0.151	0.151	0.150	0.150	0.150	0.331	0.331	0.331	0.331	0.334	0.334	0.334

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. LS regressions with RE at the household level. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. When specified, the following controls are included: head's age, age squared, education and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). Outliers trimmed at the top 0.5% of the dependent variable distribution.

Table 6: Total vs. Primary Grant Impacts on Share of Household Monthly Expenditures in Other Nondurables

	Share Children Clothing							Share Adult Clothing							Share Health Expenditures						
	Mod 1A	Mod 2A	Mod 3A	Mod 4A	Mod 5A	Mod 6A	Mod 7A	Mod 1B	Mod 2B	Mod 3B	Mod 4B	Mod 5B	Mod 6B	Mod 7B	Mod 1C	Mod 2C	Mod 3C	Mod 4C	Mod 5C	Mod 6C	Mod 7C
Treatment =1	0.008**	0.007**	0.003**	0.007**	0.006**	0.006**	0.003**	0.002*	0.002*	0.003**	0.002*	0.002+	0.002*	0.003*	-0.004**	-0.004**	-0.006**	-0.005**	-0.004**	-0.004**	-0.005**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Treatment * Children in Primary =1			0.007**							-0.002+							0.003				
			(0.001)							(0.001)							(0.002)				
Treatment * Children in Primary Only =1				0.001			0.005**				-0.000			-0.001				0.003+			0.003
				(0.001)			(0.001)				(0.001)			(0.001)				(0.002)			(0.002)
Children in Primary = 1			0.002+							-0.000							-0.004+				
			(0.001)							(0.000)							(0.002)				
Children in Primary Only =1				-0.003**			0.002				-0.000			-0.002				-0.002			-0.002
				(0.001)			(0.001)				(0.001)			(0.001)				(0.002)			(0.002)
Number Children in Primary			0.002**							-0.002*							0.001				
			(0.000)							(0.001)							(0.001)				
Number Children in Primary Only				0.004**			0.003**				-0.001**			-0.001+				-0.000			0.001
				(0.000)			(0.001)				(0.000)			(0.001)				(0.001)			(0.001)
Covariates	N	Y	Y	Y	N	Y	Y	N	Y	Y	Y	N	Y	Y	N	Y	Y	Y	N	Y	Y
Wave Dummies (98O, 99M)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	20718	20718	20718	20718	14332	14332	14332	20704	20704	20704	20704	14262	14262	14262	20758	20758	20758	20758	14270	14270	14270
Number of Households	10359	10359	10359	10359	7166	7166	7166	10352	10352	10352	10352	7131	7131	7131	10379	10379	10379	10379	7135	7135	7135
Proportion Children in Primary			0.599	0.354			0.510			0.602	0.355			0.513			0.603	0.354			0.514
Proportion Treat Hhs w/ Children in Primary			0.375	0.222			0.320			0.380	0.224			0.323			0.381	0.224			0.324
Mean Dependent Variable	0.027	0.027	0.027	0.027	0.024	0.024	0.024	0.018	0.018	0.018	0.018	0.019	0.019	0.019	0.016	0.016	0.016	0.016	0.017	0.017	0.017

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. LS regressions with RE at the household level. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. When specified, the following controls are included: head's age, age squared, education and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). Outliers trimmed at the top 0.5% of the dependent variable distribution.

Table 7: Total vs. Primary Grant Impacts on Household Investment in Productive Assets

	Ownership Draft Animals =1							Ownership Production Animals =1							Microenterprise Activity =1						
	Mod 1A	**Mod 2A**	**Mod 3A**	**Mod 4A**	**Mod 5A**	**Mod 6A**	**Mod 7A**	**Mod 1B**	**Mod 2B**	**Mod 3B**	**Mod 4B**	**Mod 5B**	**Mod 6B**	**Mod 7B**	**Mod 1C**	**Mod 2C**	**Mod 3C**	**Mod 4C**	**Mod 5C**	**Mod 6C**	**Mod 7C**
Treatment =1	0.036*	0.036**	0.051**	0.040**	0.036*	0.036**	0.049**	0.040*	0.034*	0.023	0.028+	0.040*	0.034*	0.018	0.027+	0.033*	0.036*	0.032*	0.032+	0.038*	0.039*
	(0.015)	(0.012)	(0.013)	(0.013)	(0.015)	(0.013)	(0.014)	(0.016)	(0.014)	(0.017)	(0.015)	(0.017)	(0.016)	(0.017)	(0.014)	(0.015)	(0.017)	(0.015)	(0.017)	(0.017)	(0.019)
Treatment * Children in Primary =1			-0.025*							0.018							-0.006				
			(0.012)							(0.013)							(0.008)				
Treatment * Children in Primary Only =1				-0.013			-0.029*				0.017			0.034*				0.003			-0.003
				(0.011)			(0.014)				(0.011)			(0.017)				(0.009)			(0.010)
Children in Primary = 1			0.009							-0.004							0.007				
			(0.011)							(0.012)							(0.007)				
Children in Primary Only =1				0.009			0.005				-0.019*			-0.025				-0.002			0.011
				(0.009)			(0.015)				(0.009)			(0.017)				(0.006)			(0.009)
Number Children in Primary			0.005							0.004							-0.000				
			(0.005)							(0.004)							(0.003)				
Number Children in Primary Only				0.003			0.008				0.009*			0.016*				0.001			-0.004
				(0.005)			(0.008)				(0.004)			(0.008)				(0.003)			(0.004)
Covariates	N	Y	Y	Y	N	Y	Y	N	Y	Y	Y	N	Y	Y	N	Y	Y	Y	N	Y	Y
Wave Dummies (98O, 99M, 99N)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	29214	29214	29214	29214	16536	16536	16536	29205	29205	29205	29205	16530	16530	16530	29223	29223	29223	29223	16545	16545	16545
Number of Households	9738	9738	9738	9738	5512	5512	5512	9735	9735	9735	9735	5510	5510	5510	9741	9741	9741	9741	5515	5515	5515
Proportion Children in Primary			0.623	0.347			0.463			0.623	0.347			0.462			0.623	0.347			0.462
Proportion Treat Hhs w/ Children in Primary			0.387	0.215			0.290			0.388	0.215			0.289			0.388	0.215			0.290
Mean Dependent Variable	0.281	0.281	0.281	0.281	0.247	0.247	0.247	0.739	0.739	0.739	0.739	0.709	0.709	0.709	0.075	0.075	0.075	0.075	0.075	0.075	0.075

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. LS regressions with RE at the household level. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. When specified, the following controls are included: head's age, age squared, education and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to , and ages 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor and bathroom); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). Baseline value of the dependent variable also included for ownership of draft and production animal equations.

Table 8: Total vs. Primary Grant Impacts on Secondary School Enrolment

	Secondary School Enrolment =1						First Year of Secondary School Enrolment =1					
	Mod 1A	**Mod 2A**	**Mod 3A**	**Mod 4A**	**Mod 5A**	**Mod 6A**	**Mod 1B**	**Mod 2B**	**Mod 3B**	**Mod 4B**	**Mod 5B**	**Mod 6B**
Treatment =1	0.125**	0.121**	0.161**	0.112**	0.130**	0.129**	0.130**	0.129**	0.120**	0.120**	0.136**	0.095*
	(0.024)	(0.021)	(0.032)	(0.036)	(0.032)	(0.048)	(0.024)	(0.020)	(0.033)	(0.033)	(0.029)	(0.048)
Treatment * Children in Primary =1			-0.053+			-0.002			0.008			0.060
			(0.031)			(0.061)			(0.032)			(0.054)
Children in Primary = 1			0.016			-0.055			-0.005			-0.046
			(0.029)			(0.061)			(0.030)			(0.053)
Number Children in Primary			0.029**			0.050+			0.024*			0.017
			(0.008)			(0.026)			(0.010)			(0.020)
Covariates	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Wave Dummies (98O, 99M, 99N)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	5931	5931	5931	1467	1467	1467	5868	5868	5868	1665	1665	1665
Number of Households	1977	1977	1977	489	489	489	3237	3237	3237	989	989	989
Proportion Children in Primary			0.807			0.612			0.812			0.663
Proportion Treat Hhs w/ Children in Primary			0.519			0.378			0.508			0.423
Mean Dependent Variable	0.764	0.764	0.764	0.774	0.774	0.774	0.629	0.629	0.629	0.630	0.630	0.630

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. Models A (secondary school enrolment): LS regressions on the balance panel of teenagers ages 12 to 16 with primary school completed; RE at the individual level. Models B (first year of secondary school enrolment): LS regressions on the pooled sample of teenagers ages 12 to 16 with primary school completed; RE at the household level. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. When specified, the following controls are included: age, sex, ethnicity (main language spoken) and baseline school enrolment; the number of teenagers ages 12 to 16 with primary completed in the household; baseline household demographics (number of children ages 0 to 7, 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). One teenager randomly selected in households with more than one teenager in the relevant age group.

Table 9: Total vs. Primary Grant Impacts on Children Morbidity (Ocurrence of Illness) - ENCEL Data

	Morbidity (Sick =1) - Children 0 to 35 Months at Baseline							**Morbidity (Sick =1) - Newborns**						
	Mod 1A	**Mod 2A**	**Mod 3A**	**Mod 4A**	**Mod 5A**	**Mod 6A**	**Mod 7A**	**Mod 1B**	**Mod 2B**	**Mod 3B**	**Mod 4B**	**Mod 5B**	**Mod 6B**	**Mod 7B**
Treatment =1	-0.026*	-0.021*	-0.034*	-0.028*	-0.028*	-0.027*	-0.040*	-0.055+	-0.065*	-0.039	-0.049	-0.073*	-0.084*	-0.037
	(0.011)	(0.010)	(0.017)	(0.012)	(0.013)	(0.012)	(0.017)	(0.030)	(0.029)	(0.045)	(0.034)	(0.037)	(0.037)	(0.046)
Treatment * Children in Primary =1			0.020							-0.036				
			(0.019)							(0.061)				
Treatment * Children in Primary Only =1				0.018			0.027				-0.035			-0.087
				(0.016)			(0.020)				(0.063)			(0.073)
Children in Primary = 1			0.013							0.131*				
			(0.018)							(0.066)				
Children in Primary Only =1				-0.009			0.010				0.111*			0.197**
				(0.014)			(0.023)				(0.053)			(0.076)
Number Children in Primary			0.003							-0.018				
			(0.006)							(0.023)				
Number Children in Primary Only				0.009			0.004				-0.018			-0.034
				(0.006)			(0.009)				(0.022)			(0.043)
Covariates	N	Y	Y	Y	N	Y	Y	N	Y	Y	Y	N	Y	Y
Wave Dummies (98O, 99M)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	11427	11427	11427	11427	7767	7767	7767	834	834	834	834	600	600	600
Number of Households	3809	3809	3809	3809	2589	2589	2589	417	417	417	417	300	300	300
Proportion Children in Primary			0.646	0.398			0.480			0.618	0.396			0.468
Proportion Treat Hhs w/ Children in Primary			0.415	0.254			0.306			0.385	0.242			0.285
Mean Dependent Variable	0.217	0.217	0.217	0.217	0.222	0.222	0.222	0.217	0.217	0.217	0.217	0.225	0.225	0.225

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. LS regressions with RE at the individual level. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. When specified, the following controls are included: age (in months), sex, mother's and father's age and education level, mother's ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). One child randomly selected in households with more than one child in the relevant age group.

Table 10: Total vs. Primary Grant Impacts on Children Morbidity (Ocurrence of Diarrhea) - INSP Data

	Diarrhea =1 Kids 12 to 23 (g1)					Diarrhea =1 Kids 24 to 35 (g5)					Diarrhea=1 Kids 36 to 59 (g4)				
	Mod 1A	Mod 2A	Mod 3A	Mod 4A	Mod 5A	Mod 1B	Mod 2B	Mod 3B	Mod 4B	Mod 5B	Mod 1C	Mod 2C	Mod 3C	Mod 4C	Mod 5C
Treatment =1	-0.082*	-0.232**	-0.149**	-0.098*	-0.227**	-0.050+	-0.046	-0.020	-0.072*	-0.040	-0.015	-0.033	-0.003	-0.028	-0.024
	(0.038)	(0.058)	(0.047)	(0.044)	(0.058)	(0.030)	(0.050)	(0.038)	(0.036)	(0.051)	(0.018)	(0.026)	(0.022)	(0.020)	(0.025)
Treatment * Children in Primary =1		0.233**			0.228**		-0.009			-0.078		0.028			-0.007
		(0.068)			(0.074)		(0.060)			(0.067)		(0.032)			(0.034)
Treatment * Children in Primary Only =1			0.157*					-0.087					-0.030		
			(0.068)					(0.058)					(0.033)		
Children in Primary = 1		0.012			0.001		-0.065			-0.031		0.016			0.021
		(0.077)			(0.088)		(0.060)			(0.074)		(0.031)			(0.040)
Number Children in Primary		-0.055+			-0.056		0.040			0.048		-0.007			-0.011
		(0.029)			(0.037)		(0.025)			(0.033)		(0.014)			(0.021)
Children in Primary Only =1			-0.017					0.025					0.012		
			(0.056)					(0.047)					(0.027)		
Number Children in Primary Only			-0.037					0.027					0.001		
			(0.027)					(0.023)					(0.014)		
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	528	528	528	421	421	570	570	570	447	447	1128	1128	1128	868	868
Proportion Treatment	0.473	0.473	0.473	0.466	0.466	0.426	0.426	0.426	0.427	0.427	0.399	0.399	0.399	0.386	0.386
Prop Children in Primary		0.616	0.421		0.549		0.575	0.360		0.459		0.623	0.393		0.510
Prop Treat Hhs w/ Children in Primary		0.290	0.191		0.249		0.249	0.158		0.201		0.261	0.159		0.206
Mean Dependent Variable	0.208	0.208	0.208	0.209	0.209	0.139	0.139	0.139	0.154	0.154	0.084	0.084	0.084	0.083	0.083

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. OLS regressionsl. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. The following controls are included in all regrssions: age (in months constructed using date of birth and date of interveiw), maternal weight, height, anaemia and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). One child randomly selected in households with

Table 11: Total vs. Primary Grant Impacts on Children Morbidity (Ocurrence of Respiratory Infections) - INSP Data

	Respiratory Infection =1 Kids 12 to 23 (g1)					Respiratory Infection =1 Kids 24 to 35 (g5)					Respiratory Infection=1 Kids 36 to 59 (g4)				
	Mod 1A	**Mod 2A**	**Mod 3A**	**Mod 4A**	**Mod 5A**	**Mod 1B**	**Mod 2B**	**Mod 3B**	**Mod 4B**	**Mod 5B**	**Mod 1C**	**Mod 2C**	**Mod 3C**	**Mod 4C**	**Mod 5C**
Treatment =1	0.005	-0.007	0.000	0.006	-0.004	-0.058	-0.083	-0.063	-0.063	-0.083	-0.010	-0.001	0.004	-0.014	-0.000
	(0.048)	(0.074)	(0.059)	(0.054)	(0.075)	(0.046)	(0.072)	(0.059)	(0.052)	(0.071)	(0.036)	(0.057)	(0.048)	(0.038)	(0.058)
Treatment * Children in Primary =1		0.019			0.017		0.042			0.039		-0.011			-0.024
		(0.099)			(0.109)		(0.096)			(0.105)		(0.067)			(0.075)
Treatment * Children in Primary Only =1			0.013					0.010					-0.031		
			(0.093)					(0.096)					(0.065)		
Children in Primary = 1		-0.026			0.053		-0.071			-0.020		-0.085			-0.048
		(0.092)			(0.104)		(0.078)			(0.094)		(0.066)			(0.074)
Number Children in Primary		0.066+			-0.003		0.044			0.012		-0.005			-0.016
		(0.036)			(0.045)		(0.034)			(0.052)		(0.028)			(0.037)
Children in Primary Only =1			0.004					-0.085					-0.024		
			(0.075)					(0.071)					(0.049)		
Number Children in Primary Only			0.058+					0.053					-0.020		
			(0.034)					(0.032)					(0.026)		
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	529	529	529	422	422	570	570	570	447	447	1128	1128	1128	868	868
Proportion Treatment	0.473	0.473	0.473	0.464	0.464	0.426	0.426	0.426	0.427	0.427	0.399	0.399	0.399	0.386	0.386
Prop Children in Primary		0.641	0.439		0.550		0.575	0.360		0.459		0.623	0.393		0.510
Prop Treat Hhs w/ Children in Primary		0.301	0.198		0.249		0.249	0.158		0.201		0.261	0.159		0.206
Mean Dependent Variable	0.484	0.484	0.484	0.498	0.498	0.479	0.479	0.479	0.477	0.477	0.421	0.421	0.421	0.429	0.429

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. OLS regressions. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. The following controls are included in all regrssions: age (in months constructed using date of birth and date of interveiw), maternal weight, height, anaemia and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). One child randomly selected in households with more than one child in the relevant age

Table 12: Total vs. Primary Grant Impacts on Children Nutrition (Anaemia) - INSP Data

	Anaemia =1 Kids 12 to 23 (g1)					Anaemia =1 Kids 24 to 35 (g5)					Anaemia =1 Kids 36 to 59 (g4)				
	Mod 1A	Mod 2A	Mod 3A	Mod 4A	Mod 5A	Mod 1B	Mod 2B	Mod 3B	Mod 4B	Mod 5B	Mod 1C	Mod 2C	Mod 3C	Mod 4C	Mod 5C
Treatment =1	0.055	0.140+	0.105	0.058	0.162+	-0.026	0.012	-0.025	-0.012	0.018	-0.090**	-0.137**	-0.113**	-0.089**	-0.127*
	(0.049)	(0.081)	(0.064)	(0.059)	(0.082)	(0.047)	(0.073)	(0.058)	(0.055)	(0.075)	(0.032)	(0.049)	(0.042)	(0.034)	(0.050)
Treatment * Children in Primary =1		-0.128			-0.180+		-0.068			-0.075		0.069			0.068
		(0.098)			(0.102)		(0.093)			(0.115)		(0.061)			(0.071)
Treatment * Children in Primary Only =1			-0.111					-0.006					0.052		
			(0.095)					(0.104)					(0.062)		
Children in Primary = 1		0.015			0.083		-0.086			-0.090		-0.066			-0.026
		(0.091)			(0.105)		(0.084)			(0.099)		(0.058)			(0.069)
Number Children in Primary		0.046			0.009		0.040			0.055		0.038			0.021
		(0.040)			(0.058)		(0.038)			(0.054)		(0.026)			(0.032)
Children in Primary Only =1			0.056					-0.006					-0.021		
			(0.069)					(0.068)					(0.047)		
Number Children in Primary Only			0.031					0.010					0.027		
			(0.040)					(0.039)					(0.022)		
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	437	437	437	344	344	483	483	483	374	374	954	954	954	726	726
Proportion Treatment	0.428	0.428	0.428	0.468	0.468	0.435	0.435	0.435	0.433	0.433	0.405	0.405	0.405	0.390	0.390
Prop Children in Primary		0.659	0.446		0.567		0.565	0.340		0.439		0.642	0.403		0.529
Prop Treat Hhs w/ Children in Primary		0.318	0.208		0.265		0.251	0.151		0.195		0.270	0.162		0.213
Mean Dependent Variable	0.606	0.606	0.606	0.608	0.608	0.435	0.435	0.435	0.441	0.441	0.265	0.265	0.265	0.265	0.269

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. OLS regressions. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. The following controls are included in all regrssions: age (in months constructed using date of birth and date of interveiw), maternal weight, height, anaemia and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). One child randomly selected in households with

Table 13: Total vs. Primary Grant Impacts on Children Nutrition (Stunting) - INSP Data

	Stunting =1 Kids 12 to 23 (g1)					Stunting =1 Kids 24 to 35 (g5)					Stunted =1 Kids 36 to 59 (g4)				
	Mod 1A	Mod 2A	Mod 3A	Mod 4A	Mod 5A	Mod 1B	Mod 2B	Mod 3B	Mod 4B	Mod 5B	Mod 1C	Mod 2C	Mod 3C	Mod 4C	Mod 5C
Treatment =1	-0.042	0.025	-0.073	0.084	0.179	-0.091	-0.355*	-0.372*	-0.032	-0.396+	-0.023	-0.290	-0.020	-0.386**	-0.332+
	(0.101)	(0.145)	(0.121)	(0.116)	(0.133)	(0.115)	(0.179)	(0.142)	(0.156)	(0.211)	(0.097)	(0.179)	(0.111)	(0.140)	(0.180)
Treatment * Children in Primary =1		-0.104			-0.173		0.358+			0.554**		0.364+			-0.116
		(0.186)			(0.227)		(0.183)			(0.189)		(0.193)			(0.249)
Treatment * Children in Primary Only =1			0.100					0.452**					-0.006		
			(0.199)					(0.161)					(0.187)		
Children in Primary = 1		0.158			-0.094		0.177			0.099		-0.154			-0.216
		(0.243)			(0.232)		(0.120)			(0.138)		(0.303)			(0.221)
Number Children in Primary		-0.049			0.299*		-0.017			0.034		-0.053			0.045
		(0.089)			(0.141)		(0.053)			(0.090)		(0.091)			(0.136)
Children in Primary Only =1			0.056					0.151*					-0.112		
			(0.119)					(0.066)					(0.142)		
Number Children in Primary Only			-0.049					-0.033					-0.034		
			(0.085)					(0.054)					(0.088)		
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	234	234	234	164	164	280	280	280	196	196	238	238	238	150	150
Proportion Treatment	0.141	0.141	0.141	0.134	0.134	0.111	0.111	0.111	0.122	0.122	0.164	0.164	0.164	0.140	0.140
Prop Children in Primary		0.662	0.415		0.518		0.693	0.471		0.561		0.714	0.395		0.547
Prop Treat Hhs w/ Children in Primary		0.094	0.047		0.067		0.082	0.064		0.082		0.118	0.046		0.067
Mean Dependent Variable	0.436	0.436	0.436	0.457	0.457	0.582	0.582	0.582	0.602	0.602	0.542	0.542	0.542	0.500	0.500

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. FE regressions. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. The following controls are included in all regrssions: age (in months constructed using date of birth and date of interveiw), maternal weight, height, anaemia and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). One child randomly selected in households with

Table 14: Total vs. Primary Grant Impacts on Children Nutrition (Underweight) - INSP Data

	Underweight =1 Kids 12 to 23 (g1)					Underweight =1 Kids 24 to 35 (g5)					Underweight =1 Kids 36 to 59 (g4)				
	Mod 1A	Mod 2A	Mod 3A	Mod 4A	Mod 5A	Mod 1B	Mod 2B	Mod 3B	Mod 4B	Mod 5B	Mod 1C	Mod 2C	Mod 3C	Mod 4C	Mod 5C
Treatment =1	0.132	0.032	0.055	0.095	0.035	-0.000	0.097	0.045	0.006	0.145	-0.062	-0.074	-0.066	-0.140	-0.118
	(0.091)	(0.069)	(0.078)	(0.120)	(0.109)	(0.075)	(0.097)	(0.093)	(0.102)	(0.106)	(0.090)	(0.142)	(0.092)	(0.113)	(0.137)
Treatment * Children in Primary =1		0.139			0.097		-0.132			-0.181		0.009			-0.055
		(0.116)			(0.209)		(0.147)			(0.195)		(0.170)			(0.244)
Treatment * Children in Primary Only =1			0.193					-0.074					-0.025		
			(0.206)					(0.151)					(0.158)		
Children in Primary = 1		-0.092			-0.154		0.105			0.028		0.085			-0.022
		(0.148)			(0.158)		(0.094)			(0.111)		(0.185)			(0.194)
Number Children in Primary		-0.108+			-0.083		-0.036			0.072		-0.060			-0.000
		(0.064)			(0.089)		(0.044)			(0.108)		(0.069)			(0.092)
Children in Primary Only =1			-0.100					-0.030					0.106		
			(0.085)					(0.052)					(0.127)		
Number Children in Primary Only			-0.117+					-0.012					-0.078		
			(0.064)					(0.044)					(0.064)		
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	234	234	234	164	164	280	280	280	196	196	238	238	238	150	150
Proportion Treatment	0.141	0.141	0.141	0.134	0.134	0.111	0.111	0.111	0.122	0.122	0.164	0.164	0.164	0.140	0.140
Prop Children in Primary		0.662	0.415		0.518		0.693	0.471		0.561		0.714	0.395		0.547
Prop Treat Hhs w/ Children in Primary		0.094	0.047		0.067		0.082	0.064		0.082		0.118	0.046		0.067
Mean Dependent Variable	0.137	0.137	0.137	0.134	0.134	0.196	0.196	0.196	0.230	0.230	0.139	0.139	0.139	0.133	0.133

Notes: +significant at 10%, *significant at 5%, **significant at 1%. SE in parentheses clustered at the community level. FE regressions. Children in Primaria =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary school, as predicted applying the program rules to baseline data. Children in Primary Only =1 if there is at least one child in the household that could be enrolled between third and sixth grade of primary and there are no children potentially enrolled in secondary school in the household. The following controls are included in all regrssions: age (in months constructed using date of birth and date of interveiw), maternal weight, height, anaemia and ethnicity (main language spoken); baseline household demographics (number of children ages 0 to 7, and 8 to 17, and number of adults ages 18 to 54 and over 55); baseline household characteristics and assets (ratio of household members per room, dirt floor, bathroom, ha of land owned and ownership of production animals); and baseline community characteristics (distance to urban center, distance to closest secondary school, male agricultural wage, (mobile) health center). One child randomly selected in households with