The Medium Term Impacts of Cash and In-kind Food Transfers on Learning*

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Abstract

This paper studies the medium-term impact of early-life unconditional transfers on children's learning. Our context is the village-level randomized controlled trial of Mexico's Food Support Program (the *Programa de Apoyo Alimentario*, PAL) which assigned poor households to receive either cash, in-kind food transfers, or nothing (a control). We match children from this experiment to administrative data on primary school standardized tests taken 4 to 10 years after the experiment began, and we find that cash transfers led to a significant and meaningful decrease in test scores, while in-kind transfers did not impact scores. An analysis of mechanisms suggests that these counterintuitive findings stem from two factors: (1) children in cash localities attended lower quality schools and (2) that the cash transfers increased the returns to child labor inducing a substitution of labor and learning.

JEL Classification I12, I21, I38

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1 Introduction

Worldwide, means tested transfer programs have become one of the most common strategies to reduce poverty (Bastagli et al., 2019), and a robust body of evidence has demonstrated that transfers improve short-term outcomes (Fiszbein and Schady, 2009). In addition to addressing short-term needs, many transfer programs also aim to increase children's human capital as a means to improve life-long outcomes and promote intergenerational mobility. Our paper contributes to the small and growing literature on the medium- and long-run impacts of transfer programs (Barham et al., 2019, Araujo et al., 2016, Millán et al., 2020) by studying how unconditional cash and in-kind food transfers impacted the standardized test scores of primary school children in poor and remote areas of Mexico 4 to 10 years after transfers were first received.

Transfer programs can impact children's learning in various ways, both positively and negatively. First, transfers can improve nutritional intake during critical ages for mental and physical development (directly via food transfers or indirectly via cash), thus improving a key biological foundation for learning (Prado and Dewey, 2014, Almond et al., 2011). Second, transfers increase the household budget which allows parents more flexibility to invest in inputs to the learning process (Dahl and Lochner, 2012); for example, parents may buy more books for their children, reduce their work hours in order to spend more time on children's educational activities, or send their children to a better (and more expensive) school.

Third, transfers can impact child labor: greater resources can reduce the necessity for child labor and free up time for learning (Edmonds and Schady, 2012), or greater resources could lead to more child labor if, for example, the family invests in assets that are complementary to labor (Basu et al., 2010, Edmonds and Theoharides, 2019, de Hoop et al., 2019).[1] Fourth, transfers that are conditional on school attendance can increase enrollment and time in the classroom. While higher enrollment and more time in school can increase learning (Barham et al., 2017), it could also negatively impact students by increasing demands on a limited supply of teachers and school resources.[2] Finally, there are likely dynamic complementarities amongst inputs to a child's learning (Heckman and Cunha, 2007, Glewwe and Muralidharan, 2016). For example, if transfers lead to an improved biological foundation for learning, parents may subsequently increase or decrease investments in children's schooling depending on whether those investments complement or are substitutes for improved nutrition.

[1] Also, see de Hoop and Rosati (2014) and Dammert et al. (2018).

[2] If transfers induce lower ability students to enroll, the heterogeneity of student ability in the classroom may increase, leading to less effective teaching or adverse sorting of peers within class by ability (De Giorgi and Pellizzari, 2014, Duflo et al., 2011).

We investigate how these mechanisms contribute to the overall impact of cash and in-kind food transfers on children's learning in the context of the *Programa de Apoyo Alimentario* (PAL), one of Mexico's flagship anti-poverty transfer programs. We leverage the randomized controlled trial of PAL that was implemented during the program's roll-out in 2003. 208 villages in southern Mexico were randomized into three groups in which program-eligible households received either cash transfers, in-kind food transfers, or no transfers (a control). In-kind food transfers were of a similar value to the cash transfer, and both represented around 12% of pre-program household consumption. We follow approximately 4,000 children whose families were part of this experiment into primary school by merging individual-level experimental data with a nationwide census of standardized test scores in grades 3 through 6. Test score data spans the years 2007 through 2013, which allows us to study children's learning 4 to 10 years after transfers began. Unlike Mexico's other well-known anti-poverty program, *Progresa/Oportunidades*, PAL transfers were not conditional on school attendance.

Previous research (Cunha, 2014) has shown that in the short-run (approximately one year after transfers began), both in-kind and cash PAL transfers increased total household consumption by similar magnitudes, a result attributable to the fact that the cash transfer was largely inframarginal. However, certain in-kind food items were extramarginal and binding, and thus children receiving in-kind transfers consumed more micronutrients than children who received cash transfers. Pre-program, many children were deficient in key micronutrients - vitamin C, iron, and zinc - that have been shown to support brain development (Black, 2003) and the in-kind treatment induced more children to consume above the Recommended Daily Allowance (RDA) of these micronutrients. By linking individual-level information on consumption from the post-experiment survey in 2005 with test score results in primary education, we can thus directly assess whether improvements in the nutritional intakes translate into improvements in subsequent performance in school.

Unlike previous work on the effect of conditional and unconditional transfers on learning at the end of secondary school (e.g. Barham et al., 2019, Araujo et al., 2016), we study school performance in primary school where baseline enrollment was almost universal and treatment effects on learning outcomes are unlikely to be confounded by those on school enrollment and completion. We use both the household survey data that were collected as part of the experiment and administrative data that were collected concurrent with the primary school exams to study how PAL impacts other determinants of school performance, such as child labor, school quality, and parental time investment.

Our main finding is that 4 to 10 years after transfers began, relative to the control, in-kind food transfers had no impact on test scores while cash transfers negatively impacted test scores. Transfers, whether in-kind or in-cash, did not impact the likelihood that children took the tests, implying these results are not driven by sample selection. Students were tested in three subjects - math, Spanish, and a third subject which rotated yearly - and cash transfers led to reductions in scores in all three areas, with effect sizes varying between 0.12 and 0.16 standard deviations lower than students from control villages. The children in our sample varied in the age at which they were first exposed to transfers, with the oldest at 6 years of age and the youngest having been exposed since conception. While a limited sample size precludes precise comparisons, estimates suggest that the negative impacts on test scores in cash villages are concentrated among those over 2 years old when transfers began. We also find that indigenous students and those from especially poor families - two of the most disadvantaged groups in Mexico - experienced larger negative impacts on test scores for both in-kind and cash transfers.

We next explore the mechanisms that could be driving these differential impacts on test scores. First, we replicate results from (Cunha, 2014) and show that neither transfer modality improved several measures of the stock of child health, but children in in-kind communities increased their intakes of important micronutrients, such as zinc and iron. Second, we find that transfers induced children to work more, especially amongst students from cash localities. Third, we find that students from cash localities were more likely to attend community or indigenous schools, as opposed to general schools, which is likely a result of the fact that general schools in cash villages increased fees for materials, uniforms, and enrollment. General school are historically of better quality than community and indigenous schools, but the evidence suggests that parents substituted to cheaper schools when principals increased the costs of general schools. Finally, neither transfer modality seems to have impacted parental involvement in school related activities. In sum, we conclude that the lower test scores in cash villages were due to several factors: (1) principals in cash villages increased the fees for general schools, (2) children in cash localities moved to lower quality indigenous or community schools, and (3) that the cash transfers increased the returns to child labor which induced a substitution of labor and learning.

Our study contributes to several related literatures. First, we add to the literature that studies the design and implementation of transfer programs (Baird et al., 2011, Barrera-Osorio et al., 2011, Glewwe and Muralidharan, 2016). Consistent with the results in previous studies (Baird et al., 2016, Araujo et al., 2016, Baez and Camacho, 2011, Akresh et al., 2013), we find that unconditional transfers do not lead to long term improvements in human capital outcomes. By studying the behavioral responses to an unconditional transfer up to 10 years after its inception, our paper adds to the recent literature that studies the transmission channels through which conditional transfers affect long run outcomes (Barham et al., 2017, 2019). Previous work on PAL (Cunha et al., 2019) had found that the transfer modality had no differential effect on goods prices. We show that this is not the case for the price of education services. Second, we add to the literature that studies how micronutrient consumption - or lack thereof - in early life contributes to learning (Almond et al., 2011, Maluccio et al., 2009, Feyrer et al., 2017, Chong et al., 2016), by showing that improvements in the quality of micronutrient intakes in the first years of life are not sufficient to improve learning in the medium term.

The remainder of this paper is organized as follows: section 2 describes the PAL program and institutional features of primary education in Mexico; section 3 discusses our data and sample; section 4 presents the empirical strategy and results; section 5 discusses possible mechanisms through which PAL might affect learning outcomes; and section 6 concludes.

2 Background on Education in Mexico and the PAL Program

2.1 Primary education in Mexico

Public primary schools in Mexico include grades 1 though 6 and most are governed by the Federal Secretary of Education (*Secretaria de Educacion Publica*, SEP).[3] The remaining public primary schools are governed by CONAFE (*Consejo Nacional para el Fomento de la Educacion*), a decentralized agency responsible for providing educational services in rural and hard to reach communities with fewer than 2,500 inhabitants.

SEP schools (also known as *general* schools) typically have one classroom per grade and are staffed by teachers with open-ended contracts who have received post-secondary education (INEE, 2014). In contrast, CONAFE schools (also known as *community* schools) always have a single multigrade classroom, with a typical enrollment of 10-15 students per school. CONAFE instructors are generally young community residents between 15 and 29 years old who have completed upper secondary school yet do not have formal teacher training; they typically teach in the CONAFE school for only two years.[4]

Both SEP and CONAFE are required to offer non-Spanish speakers the option of attending schools which offer instruction in their indigenous language (known as *indigenous* schools).

[3] Less than 10% of all primary schools in Mexico are in the private sector (INEE, 2014).

[4] Only 2.6 percent of CONAFE teachers report having a college degree, while 19 percent report having only completed lower secondary education (INEE 2014). CONAFE teachers should receive between five and seven weeks of training, but more than half report four weeks of training or less (INEE 2014).

The large majority (66 percent) of the indigenous schools are multigrade. There are 68 officially recognized indigenous languages in Mexico, and the quality of these schools is often low, partly stemming from a low supply of trained indigenous-language teachers.

In 2013, general, community, and indigenous schools enrolled 93%, 1%, and 6% of Mexican public school students, respectively. However, in the more rural parts of Mexico we study, community and indigenous schools are more prevalent.

2.2 ENLACE tests

Between 2007 and 2013, all Mexican students in grades 3 through 9 were required to take a standardized test, the ENLACE (*Evaluación Nacional de Logro Académico en Centros Escolares)*. The test was administered at the end of each academic year and it assessed student knowledge in three areas: math, Spanish, and, starting in 2008, a third subject which rotated between Science (in 2008 and 2011), Ethics/Civics (in 2009 and 2013), History (in 2010), and Geography (in 2011). In the first year of implementation, ENLACE tests were normalized by subject and grade with a mean score of 500 and a standard deviation of 100; subsequent years' tests were graded relative to the base year to allow for the comparison of results over time. Nationwide, take-up of ENLACE was close to 90 percent.

Originally, teachers had no stake in the results of their student's ELACE test scores, but in 2008, ENLACE scores became one of the key criteria to measure teacher performance in *Carrera Magisterial* (CM) program. The CM is a national teacher incentive program which offered salary bonuses for taking professional development courses and agreeing to be subject to yearly evaluations (Santibañez et al., 2007). The use of ENLACE scores in the CM program possibly increased teacher effort, but as SEP required the use of external proctors, it is unlikely that teachers were able to directly manipulate student responses. Previous work has shown that ENLACE tests in primary education are correlated with later learning and labor market outcomes (Avitabile and de Hoyos, 2018, De Hoyos et al., 2018).

The ENLACE was not offered in 2014, and 2015 it was replaced by a new test, the PLANEA *(Plan Nacional para la Evaluación de los Aprendizajes)*. Unlike the ENLACE, the PLANEA only tested a random subset of students in schools and so is not useful for our analysis.

2.3 The PAL program and experiment

The *Programa de Apoyo Alimentario* began in 2004 with the aim of increasing the nutritional intake of poor families, with an emphasis on children and mothers. By 2009, it had expanded to operate in about 5,000 poor, rural villages throughout Mexico. Villages were eligible to receive PAL if they had fewer than 2,500 inhabitants, were classified as highly marginalized by the Census Bureau, and did not currently receive aid from either *Liconsa*, a subsidized milk program, or *Oportunidades*, a conditional cash transfer program. PAL villages were therefore typically poorer and more rural than the widely-studied *Oportunidades* villages.[5] Within eligible villages, households were eligible for the program if they fell below the threshold of a poverty index derived from observable characteristics of permanent income (Vazquez Mota, 2004).

PAL food transfer packages were chosen by nutritionists to provide a balanced diet of about 1,750 calories per day, per household (Campillo Garcia, 1998) and contained seven basic items (enriched corn flour, rice, beans, dried pasta soup, biscuits (cookies), fortified milk powder, and vegetable oil) and two to four supplementary items (including canned sardines, canned tuna fish, lentils, chocolate powder, packaged breakfast cereal, and corn starch). All the items were common Mexican brands and were by and large available in local stores. The transfer was not conditional on family size, it was delivered bimonthly (two food boxes at a time), resale of in-kind food transfers was not prohibited, and the wholesale cost to the government per box was about 150 pesos (approximately 15 U.S. dollars).

PAL experiment

Concurrent with the national roll-out of the program, a random sample of 208 villages in southern Mexico were chosen for inclusion in an experiment.[6] Villages were randomized into three treatment arms, in which eligible households received either a monthly in-kind food transfer (50 percent of villages), a 150 peso per month cash transfer (25 percent of villages), or nothing (the remaining 25 percent of villages). Approximately 89 percent of households in the in-kind and cash villages were eligible to receive transfers (and received them).

In addition to the randomization of transfer modality, the experiment also assigned all the cash villages and a randomly selected half of the in-kind villages to receive health, nutrition, and hygiene classes, which were designed to promote healthy eating and food preparation practices. In practice, few transfer recipients reported attending classes and - importantly - administrators confirmed that the conditionality of transfers on class attendance was never

[5]Villages were not incorporated in *Oportunidades* if they did not have health facilities and/or secondary schools in close enough proximity, as needed to fulfill the conditionality of *Oportunidades* transfers.

[6]The experiment was implemented in eight states: Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz, and Yucatan.

enforced; that is, no household was denied transfers for not attending classes.[7] Furthermore, qualitative research finds that the classes were held infrequently, were generally of low quality, and were not taken seriously by participants, suggesting that the classes did not likely impart new knowledge on program recipients that would impact their food consumption decisions (Rodriguez Herrero, 2005). As shown below, our main empirical results are robust to separating the in-kind villages into the group with classes and the group without.

Using data from the experimental sample, Cunha (2014) documents that both cash and in-kind transfers led to equally sized increases in total consumption (food plus non-food) and food consumption between in-kind and cash villages. However, several of the in-kind food items were extra-marginal, as evidenced by greater increases in consumption of those goods in in-kind villages compared to cash villages. Some of these extra-marginal foods were nutrient rich, such as fortified powdered milk and vitamin enhanced corn flour, and thus children in in-kind villages consumed more iron, zinc and vitamin C than children in cash villages.

3 Data and Sample

3.1 Data

Our data come from several sources: pre- and post-intervention surveys of individuals and households in PAL villages, student-level ENLACE test scores, student surveys from a subset of ENLACE test takers, and school-level data collected by the SEP.

PAL data

In each of the experimental PAL villages, approximately 33 households were selected to be surveyed pre- and post-intervention. The pre-intervention survey was administered in the last quarter of 2003 and the first quarter of 2004, and the post-intervention follow-up survey was conducted in the final quarter of 2005.[8]

[7] Based on household survey data, 76 percent of respondents attended a class in the in-kind villages assigned to receive classes and 69 percent attended a class in the in-kind villages assigned to not receive classes. In both cases, average attendance was roughly four classes over the course of the program. Furthermore, assignment to classes did not affect total food expenditure or the composition of food expenditure (results available from the authors).

[8] To ensure respondents would not wrongly conclude that responses could affect their eligibility for aid, surveys were administered by Mexico's National Institute of Public Health, a different agency than the one that administered PAL.

The pre-intervention data allow us to confirm the randomization was successful, as well as segment the population by various socio-economic characteristics. From the post-intervention survey, we use data from a 24-hour food recall for children and a time allocation module for both children and adults. The food recall was completed by the survey respondent, usually the female household head, for all children aged 2 to 6 at the time of the follow-up survey and allows us to calculate the quantity of macro- and micro-nutrients consumed. For all children 12 and older, both surveys asked the primary and secondary activity in the week prior to the interview. Therefore we can identify whether children attended school and/or worked. For those who reported working, the survey asks the number of hours worked.

Of the original 208 experimental villages, eight are excluded from our analysis. Two villages could not be resurveyed due to concerns for enumerator safety; two villages were incorporated in PAL prior to the pre-treatment survey; two villages were deemed ineligible for the experiment because they were receiving the conditional cash transfer program, *Oportunidades*, contrary to PAL rules; and two villages are geographically contiguous and cannot be regarded as separate villages.[9] Observable characteristics of excluded villages are balanced across treatment arms (results available upon request). Of the remaining 200 villages, three received the wrong treatment (one in-kind village did not receive the program, one cash village received both in-kind and cash transfers, and one control village received in-kind transfers). We include these villages and interpret results as intent-to-treat estimates.

ENLACE data

Our data allow us to study grade 3 through 6 ENLACE test scores for children in experimental villages who were born between 1998 and 2004.[10] The ENLACE identifies students through a government-issued identifier, the *Clave Única de Registro Poblacional* (CURP), which is formed via an algorithm which combines first name, last name, date of birth, sex, state of birth, and two randomly generated digits. The PAL surveys do not contain the CURP, but do contain all of its constituent demographics from which we generated a quasi-CURP which only lacks the random digits. Our data form an unbalanced panel of seven cohorts spanning seven academic years.

Each year, 20 percent of exam takers are randomly selected to complete the *ENLACE de Contexto,* a multiple choice survey asking about child labor, child and parental sociodemo-

[9] The contiguous villages are named "Section 3 of Adalberto Tejada" and "Section 4 of Adalberto Tejada," so they appear to be part of the same administrative unit.

[10] We observe ENLACE scores for a small number of children from PAL villages in grades 7 through 9, but exclude them from our analysis in order to focus on primary school outcomes.

graphic characteristics, child and parent expectations, and student perceptions about their peers, teachers and parental involvement.

School-level data

The Ministry of Education conducts two school censuses per year, known as Formato 911. These censuses identify the school type (*general*, *indigenous*, or *community*) and collect information on school characteristics, including the number of teachers, students, classrooms, and laboratories, whether a library is available, and the exact geocordinates of the school. The censuses also collect information on fees that students attending general schools pay for uniforms, materials and enrollment; this information is not reported for indigenous and community schools as materials and uniforms are provided by the government either in the form of in-kind or cash grants to the schools. Fees in general schools are by law not compulsory, but school principals often ask for payments in a manner which make parents perceive the contributions are compulsory.[11] ENLACE tests are matched to schools using a unique school identifier (*clave de centro de trabajo* CCT). The geocoordinates allow us to calculate the distance between a village and the school a student attended.

3.2 ENLACE take-up

Post-treatment, there were 5,444 children in PAL villages born between 1996 and 2005, and we match 69% of them write at least one ENLACE test. Our sample therefore includes 3,773 children from 200 villages for whom we observe a total of 11,006 ENLACE tests; Table 1 shows how these observations vary across academic grades and years.

There are several reasons why we may not observe a child from the PAL survey taking the ENLACE test. First, children's school attendance and taking of the ENLACE test could be differentially impacted by the PAL transfers; however, as we show in the results below, there is no evidence that this is the case. Second, the child's family could have migrated abroad before the child reached the end of the third grade.[12] Migration information is not available for the PAL sample, but surveys of participants in the 1997 experimental evaluation of the *Oportunidades* program, a similar social transfer program to PAL, reveal that 0.7 percent of control group households migrated to the U.S. within one year (Angelucci, 2015). Each household in our sample has on average one child between the ages of 0 and 6 and if we

[11] In order to discourage this behavior, in 2018 the Ministry of Education officially forbade school principals to ask for financial contributions.

[12] We observe students whose families moved within Mexico as the ENLACE was applied nationwide.

apply this migration rate in each of the 10 years between 2004 and 2013, we would expect to not observe ENLACE tests for around 6.8 percent of children.

Third, a child could have never enrolled in school or could have dropped out of school before reaching the end of third grade. We can estimate this potential source of attrition using the PAL follow-up survey in 2005: 7 percent of the children aged 8 to 12 (the typical age of children in grades 3 through 6) report not attending school (and there is no difference across treatment groups).[13]

Fourth, a student may have not been present on the day of the exam, or the exam was not offered at the school. We do not have data on absence rates in schools, but there is evidence that entire schools did not take the exam in certain years in areas where there is a strong representation of the National Educational Workers Syndicate (*Sindicato Nacional de Trabajadores de la Educación*), a trade union representing a large percentage of Mexican teachers. In fact, the union scheduled strikes and disruptions specifically on the days of the ENLACE test as a form of protest, and state-level variation in ENLACE take-up rates demonstrates their influence. For example, in Yucatan and Veracruz, where union membership is relatively low, take-up rates were constantly above 94 percent in all years between 2007 and 2013. In Chiapas, Guerrero and Oaxaca, however, where union membership is high, take-up varied considerably by year, with certain years having take-up rates of 60, 70, or 80 percent. Importantly, as these protests and disruptions are orthogonal to the original treatment assignment, including state and year fixed effects in our specifications will allow to account for potential biases induced by differences in the test take-up.

A final reason why we would not see a child's ENLACE test is that the merging algorithm was not accurate. However, we failed to merge only 2.8 percent students and there are no differences in merge rates across treatment groups. Further details about the merging algorithm and the possible reasons for attrition are provided in the Appendix.

3.3 Summary statistics

The first two panels in Table 2 present pre-intervention sample means, by treatment group, for child and household characteristics respectively. The sample of children does not include those born after the pre-intervention survey (as they do not have baseline data), and the

[13] Although students are in school they might not reach the third grade during our period of observation either because they start late or because they repeat the early grades more than once. According to the estimates provided by INEE (2013), 95 percent of the children who enrolled for the first time in primary education in 2010 complied with the statutory starting age, and repetition rates in early grade are remarkably low. Furthermore, late entry and repetition rates do not differ across experimental groups.

sample of households reflects one observation per child that we observe in the followup survey even if they were born after the baseline survey. Consistent with the random assignment, we find that characteristics are balanced across the three groups, with three exceptions: in cash localities, the share of boys is lower than in both in-kind and control localities; households in control localities are more likely to have an unmarried head than those in cash localities; and control households are more likely to have running water at home than those in cash villages.

Individual consumption data was only collected for children aged 1 through 4 years. As the 24-hour food recall module could overstate or understate actual consumption, it is difficult to draw definitive conclusions about child health, however, comparing caloric and micronutrient intake to Recommended Daily Allowances (RDAs) suggests that most children consume too few calories and that for many, those calories do not contain enough essential micronutrients. In particular, 89 percent of children consume fewer than the RDA of calories, and 32, 46, and 41 percent of children are not consuming the RDA of iron, vitamin C, and zinc, respectively. Households in our sample are large and poor, with a total per-capita expenditure of about 360 pesos per month, or $36 US dollars, and about 5.5. household members. Approximately 30 percent of households have at least one member who speaks an indigenous language. The household head has on average 7 years of education, about 40 percent of households have a dirt floor, and around 50 percent do not have running water.

The bottom panel in Table 2 presents the characteristics of the closest school to each PAL community. The closest school is typically a general school and on average it is located 1km from the center of the community. The average distance remains the same when the closest school is a community or an indigenous school. The student-teacher ratio is around 29, and repetition rates are around 9 percent in general schools and 18 percent in community and indigenous schools. On average, the yearly cost of the closest school is about 380 pesos, including contributions, materials' and uniforms' costs. As mentioned above, this cost is entirely driven by general schools, as indigenous and community schools are free of charge.

3.4 Post-experiment

The interpretation of our estimates depends in part on what benefits children received between the experiment and the observed ENLACE tests. Self-reports from the follow-up survey show that households in treatment villages (both in-kind and cash) reported receiving on average 12 months of PAL aid, however we do not have household-level information on the type of transfers received after the follow-up survey.

SEDESOL was able to provide us only with village-level administrative counts of the number of beneficiary households per year receiving PAL, *Oportunidades,* or *Liconsa* between 2005 to 2013. Figure 1 plots the average number of beneficiaries of PAL, *Oportunidades,* and *Liconsa* by year for the three experimental groups. While the average number of PAL beneficiaries fluctuated between 200 and 50 for in-kind villages and between 250 and 50 for cash villages, the control villages remained with an average number of beneficiaries below 50 during the entire period (left panel in Figure 1). The number of beneficiaries of *Oportunidades* and *Liconsa* steadily increased over time. Cash localities displayed on average a lower number of households that are beneficiary of the *Oportunidades* transfer (middle panel in Figure 1). This difference, which is very small in size,[14] is likely to partly reflect differences in population. In fact, both in 2005 and 2010 - years for which the population census is available - we find that the number of households in in-kind and cash localities is lower than in control ones (see columns 1 and 4 in Appendix Table A1), possibly as a result of differential migration. The share of beneficiary households in neither of the treatment groups is statistically different from the one in the control group (columns 3 and 6 in Appendix Table A1).

Overall, the evidence suggests that our estimates should be interpreted as the combined effect of differential exposure and differential take-up in treatment and control localities. If the receipt of other programs after the experiment is correlated with the treatment assignment to the villages and those programs affect student performance outcomes, then our results would reflect not just the impact of initial transfer modality, but also the dynamic response to that initial treatment.[15]

4 Empirical Strategy and Results

4.1 Empirical model

Our empirical framework leverages the randomization of transfer modality across villages, with the main models taking the following form, where $ENLACE_{ivgt}$ is the test score of child i, in village v, grade g, and school year t:

[14] On average cash and in-kind localities have 12 and 4 recipients fewer than the control group, that on average has 62 households that are *Oportunidades* beneficiaries. Since the follow-up survey, on which the merge is based on, samples 33 households (about 20 percent of the total number of households), differences in the number of *Oportunidades* beneficiaries are likely to lead to very small differences in terms of children who have to comply with the attendance conditionality in our sample.

[15] Our estimated impacts on learning are robust to controlling for number of beneficiaries of *PAL* and *Oportunidades* in the village (available upon request).

$$ENLACE_{ivgt} = \alpha + \beta_1 InKind_v + \beta_2 Cash_v + \gamma' X_i + \delta_t + \gamma_g + \varepsilon_{ivgt}$$

We normalize test scores within grade and year with respect to the mean and the standard deviation in the control group. In our preferred specification, X_i includes fixed effects for Mexican states, the child's age, and a set of individual, household and locality characteristics that showed imbalance at baseline, namely indicators for the child's gender, whether the head of household is married, whether the house has running water, and whether the closest school offers a morning shift. δ_t and γ_g are year and grade fixed effects. Standard errors are clustered at the village level, the unit of randomization. The parameters β_1 and β_2 represent the Intent to Treat (ITT) effects of living in in-kind and cash communities at the time of the follow-up survey. When testing the null hypothesis β_2-β_1= 0 we also present results based on Randomization Inference.[16]

We also consider several other outcomes as part of our investigation into the mechanisms behind the impact of PAL transfers on learning, and those models take the same form as the model for ENLACE described above.

4.2 Results

Table 3 contains our first set of results on the likelihood of a child taking the ENLACE test. Columns 1 and 2 show the impact on a student ever taking an ENLACE test (one observation per child), while columns 3 and 4 use a balanced panel of students in every year 2007 through 2013 (seven observations per child). All models use state fixed effect and the models using yearly observations use year fixed effects. The In-kind and Cash indicators are small and statistically insignificant in all models. Including covariates, the results in columns 2 and 4 show that older students, those with greater height-for-age, and those coming from households with a married head or a head with more years of education are more likely to take the ENLACE test. Furthermore, we do not observe any difference across groups in the probability that a child takes the test in a grade appropriate for her age (see Appendix Table A2) nor do we see significant differences in the characteristics of children for whom we have and do not have ENLACE tests (see Appendix Table A3). These results suggest

[16] All other hypothesis testing results based on Randomization Inference are in line with those presented and are available upon request.

that estimates of the effect of PAL on learning outcomes are not driven either by differential selection into taking the ENLACE test or the timing of taking the test.

Before presenting the impacts of PAL transfers on test scores, it is useful to see how test scores are correlated with observable characteristics. Table 4 contains estimates from regressions of test scores on child, household, and village characteristics, using only children from control villages. Several correlations stand out: having a general school in the village, as opposed to a community or indigenous school, is associated with approximately 0.5-0.6 s.d. higher test scores in all subjects; girls perform better than boys in all subjects, with the gap being largest in Spanish (0.23 s.d.); child height-for-age (a proxy for health status) is positively correlated with test scores; and having running water in the home (a proxy for household wealth) is associated with approximately 0.2 s.d. higher test scores. The fact that both an anthropometric measure and household wealth are positively correlated with learning outcomes is suggestive that transfers, whether in-kind or in-cash, could also have meaningful impacts on performance. State fixed effects reveal meaningful geographic variation in mean test scores, reflecting the heterogeneous nature of the population of southern Mexico.

Table 5 contains our main results on the impact of cash and in-kind transfers on test scores. We show two specifications for each subject. First, the models in columns 1, 3, and 5 show mean differences between treatment and control localities only controlling for state fixed effects. In-kind transfers did not meaningfully impact test scores: all coefficients are negative, yet very small in magnitude and not statistically significant. Cash transfers, on the other hand, caused a large drop in test scores relative to students from control communities: -0.19, -0.14, and -0.17 s.d. for math, Spanish, and the 3rd subject, respectively, with math and the 3rd subject being significant at the 5 percent level. The differential impact of in-kind and cash transfers is also significant for all three subjects, both using classical asymptotic theory and Randomization Inference.[17]

Columns 2, 4, and 6 add pre-program child and household characteristics and year and grade fixed effects. These controls do not meaningfully impact our estimates and the differences between the two treatment types are statically significant at the 10 percent level in all subjects. In addition, these results are not driven by the outcomes in specific years (see Figure 2) or grades (see Figure 3), although the negative impacts of cash transfers are larger for students in higher grades (5th and 6th grades, compared 3rd and 4th grades).[18]

[17]Appendix Tables A4 and A5 report main results separating villages into the three treatment arms verifying that results are robust. Similarly, when using the ENLACE test's official 4-item categorical classification of student performance (insufficient, sufficient, good, excellent), we find results fully consistent with those presented (Appendix Table A6).

[18]Similarly, we find no evidence of our results being driven by selection into taking ENLACE test in specific years (see Figure A1).

We next explore program impacts by the age at which children first received the program. A large literature has shown that nutrition interventions are the most impactful between conception and the second birthday (Pollitt et al., 1995), and children in our sample ranged from in-utero to six years of age at the time that transfers began. Figure 4 plots the treatment effects by age at the follow-up survey. For children younger than two, we find positive point estimates, but correspondingly large confidence intervals that prevent us from rejecting the null hypothesis of no impact.

We also test whether there is any treatment heterogeneity along two dimensions that are particularly relevant for southern Mexico: household expenditure and ethnicity. Table 6 contains estimates from models that interact treatment indicators with an indicator for "poor" households, defined as those with below median expenditure per capita. While coefficients and comparisons between them are imprecise, the negative coefficients on the interaction terms suggest that poor households experience larger declines across the three subjects for both cash and in-kind transfers.

Interacting treatment with an indicator for an indigenous household (defined as at least one member speaking an indigenous language), we find the negative impacts of PAL are even greater among the indigenous population: Table 7 shows the impact of cash and in-kind transfers on test scores are between 0.14 to 0.29 standard deviations lower among the indigenous compared to the non-indigenous students. Among non-indigenous students, in-kind transfers have no impact on learning outcomes and cash transfers have a negative (albeit insignificant) impact.

Finally, we estimate treatment effects across the test score distribution with quantile regressions. Figure 5 shows that the negative impacts of the cash treatment relative to both the treatment and control treatments is concentrated in the middle ventiles (the 35th to the 75th percentile) and the highest ventile of the distribution.

5 Mechanisms

To help understand the mechanisms through which the PAL transfers affected student learning, we adopt a simple learning production function (e.g. Glewwe and Miguel, 2007).

Assume there are two time periods in a young child's life: period 1 begins with conception and ends when the child enters primary school, while period 2 covers the primary school years. Further suppose that a child's academic knowledge in period 2, Y_{i2}, is a function $(f\,(.))$ of several factors, in addition to an unobserved component μ_i: the stock of health prior to entering school (HS_{i1}); parental health investment in the child prior to primary school (HI_{i1}), such as nutritional intake and vaccinations; the effort the student devotes to school related activities (SE_{i2}); the quality of the primary school (Q_{i2}); parental investments in the child during school (PI_{i2}).

$$Y_{i2} = f(HS_{i1}, HI_{i1}, SE_{i2}, Q_{i2}, PI_{i2}, \mu_i) \quad (1)$$

For each of the inputs described in eq. 1 there is well established evidence on the relationship with learning. Because of the self-productivity of human capital skills (Heckman and Cunha, 2007), child health before entering school is an important determinants of the health stock later in life and recent evidence shows that it a significant impact on learning outcomes.[19]Parental health investments might compensate or reinforce gaps in children's endowments (Becker and Tomes, 1976), since parents might decide to invest more either on children who are in worse health in order to minimize the gap with other siblings or on those who are in better health in order to maximize the overall return. Zinc and iron supplementation in early years has been found to be beneficial for cognitive outcomes (e.g. Powell et al., 2005, Feyrer et al., 2017). Student effort, either in the form of class participation or time spent on home assignments, can contribute to improved student performance. We do not have reliable measures of time spent by the students in those activities. We do, however, have measures of child labor, that has been shown to have detrimental effects on both education attainments and learning (Ravallion and Wodon, 2000, Beegle et al., 2009, Heady, 2003) School quality - a broad term that refers, among others, to the quality of teachers (Rockoff, 2004, Chetty et al., 2014) principals (Roland G. Fryer, 2017), peers (Duflo et al., 2011), class size (Angrist and Lavy, 1999) - is a key determinant of learning outcomes. Finally, a growing body of literature shows that parental time investments play a key role in improving child cognitive and socioemotional skills in the early years (Attanasio et al., 2018, Agostinelli and Sorrenti, 2018).

Our data allow us to test the impact of cash and in-kind transfers on each of these inputs to academic knowledge. However, we note that the results presented below should be interpreted as the reduced form impacts of PAL transfers, as they combine both the direct and the indirect effect of the program. In particular, there are likely dynamic complementarities among inputs (Heckman and Cunha, 2007). For example, children of PAL recipients might

[19]Figlio et al. (2014), using a sample of siblings in Florida, find that birthweight has a positive constant effect on test scores throughout the entire academic life. Bharadwaj et al. (2012) exploit school and birth records from Chile and Norway to find that low-weight children who receive extra medical care at birth have higher test scores.

attend better (and more expensive) schools (a higher Q_{i2}) because transfers increased the family budget, or, they may attend better schools because the returns to school quality are increasing in child health investments (HI_{i1}).

5.1 Health stock and health inputs

Similar to Cunha (2014), we use information from the evaluation follow-up survey in order to study whether PAL transfers impacted proxies for health status and parental health investment of children aged 0 to 6 at the baseline. We focus on four indicators: 1) the caregiver reported probability that the child was sick in the four weeks prior to the interview; 2) the child height per age, expressed as a z-score; 3) the child weight per age, expressed as a z-score; 4) whether the child was anemic or not, based on the analysis of a blood sample.[20]

Table 8 presents results for the whole sample and for only those children for whom we observe at least one ENLACE test score. There are no differences between treatment groups in either sample in terms of the probability of being sick, height-for-age, and weight-for-age. Children in both the in-kind and the cash group were about 3 percentage points less likely to be anemic than those in the control group; this is a large difference in relation to the prevalence of anemia (19 percent in the control group), but the effect is not statically different from zero.

Table 9 presents the impacts of PAL on consumption of calories, one macronutrient (protein), and five micronutrients (vitamin C, iron, zinc, calcium, and retinol). As with health outcomes, the results for the entire sample are very similar to results for the sample of children taking the ENLACE, which is consistent with the fact that the program did not appear to impact the probability of taking the exam. Only in-kind children display an increase in the caloric consumption, but the effect is not statistically different from zero. Neither transfer type impacted the intake of protein. There was a large and statistically significant impact of both in-kind and cash on the intake of vitamin C. When looking at iron and zinc, two key nutrients for brain development, we find large and statistically significant impacts for the in-kind modality, but not for cash. For zinc, the difference between the two treatment types is statistically significant at conventional levels. Similarly, the in-kind transfer has a significantly larger impact than the cash transfer on the intake of calcium and retinol. To quantify an overall impact of transfers on nutritional intakes, we construct the first principle component of the six nutrients. In-kind transfers led to non-trivial 0.1 standard deviation

[20] Anemia is a strong predictor of test scores, as evidenced by a 0.2 standard deviation difference in math and Spanish scores between control group students who had and did not have anemia as a young child.

larger impact on intake than did cash transfers (p-value = 0.09). Appendix Table A7 shows that our conclusions do not change if we use as outcomes indicators of whether a child consumed at least the Recommended Daily Allowance of each nutrient.

In sum, despite having better nutritional intake, children who had been exposed to the PAL program for at least 18 months did not have better measured health.

5.2 Student effort

There is compelling evidence that conditional cash transfers reduced child labor (Fiszbein and Schady, 2009), but the evidence on unconditional transfers is more mixed (Edmonds and Schady, 2012, Edmonds and Theoharides, 2019). We use two distinct data sources to study the impact of PAL on child labor.

First, information from the post-treatment survey identified attendance and labor outcomes of children aged 12 and 13 who were enrolled in primary education. These children would have been eligible to take the ENLACE in 2005, if the test had been in place at the time.[21] We study five outcomes: 1) only attending school (and not working); 2) attending school and working; 3) only working; 4) neither attending school nor working; 5) the number of hours of work (including zeros). Results are reported in columns 1 to 5 in Table 10. In the control group, 84 percent of the children attended school and did not work, 5 percent worked and did not attend school, 1 percent both worked and attended school, and 10 percent neither worked nor attended school. In cash localities we observe a 6.2 percentage point reduction in the share of students who reported only attending school in the week prior to the interview, as opposed to a null effect in the in-kind localities. The difference between the two treatment types is statically significant at 10 percent level. In cash localities, we observe an increase both in the share of students who report combining school and work, and in the share of those for whom work is the only activity. For the latter, the difference with the in-kind transfer localities is statistically significant at 5 percent level. Hours of work increased by 1.6 for children in the cash group, as opposed to a negative but very small effect in the in-kind group; the cash versus in-kind difference is large in terms of size - equivalent to the average number of hours in the control group - and is statistically significant (p-value=0.07).

Second, we use data from the *ENLACE de Contexto* to study students for whom we observe

[21] Because of the almost universal enrollment in primary education and the lack of any effect of PAL on the probability of attending a grade that is appropriate for student age, the results are unlikely to be driven by a selection effect. While 12 is the standard age for completing primary school in Mexico, being "over-age" is quite common and we include 13 year-olds in order to improve statistical power. Results restricted to 12 year-olds are qualitatively similar.

test scores.[22] Surveys between 2008 and 2013 ask students how many days on average they are involved in labor activities, and in addition surveys from 2011 to 2013 also ask how much time students dedicated to household chores in the week prior to the interview. Child labor hours are strongly correlated with test scores, for example, in the control group an additional day of work per week is associated with 0.10 s.d. reduction in the average ENLACE score of the three subjects.

The impacts of PAL on the labor outcomes of primary school children answering the *Enlace de contexto* are reported in columns 6 and 7 in Table 10. Students from both in-kind and cash localities increased the number of working day per week, with the effect being particularly large in cash villages (1.31 days per week). Similarly, there was a large increase in the share of children who report helping with household work, although the impacts are not statistically significant, owing in part to the limited sample size.

Basu et al. (2010) suggest that, in the presence of multiple factor market failures, the introduction or expansion of a productive asset could increase child labor. Looking at cross-sectional variation of the entire PAL sample pre-program, we see that the probability that a child age 12-13 exclusively attends school declines with the total number of animals owned by her family while the probability of working, either exclusively or in combination with school attendance, increases (see the top panel in Figure 6). In addition, the average number of hours that a child works steadily increases with the number of animals (see the bottom panel in Figure 6).

Using the experimental variation, and consistent with evidence from *Progresa/Oportunidades* transfers (Gertler et al., 2012), we find that among households with at least one child aged 8-13 PAL transfers increased the number of animals owned, and the effect is statistically larger under cash and under in-kind transfers (see column 1 in Appendix Table A9). Households from both treatment groups are more likely to report being involved in agricultural activities post-program (column 2 in Table A9), with the effect being statistically larger among those from cash localities. We also find that households in the cash group are more likely to report a higher number of family members involved in agricultural activities, and a higher number of hours farming, but in both cases differences are not statistically significant at conventional levels (columns 3 and 4 in in Table A9). Results presented in Skoufias et al. (2013) find that PAL did not lead to any change in the overall labor supply of adult males and females, but adult males substituted agricultural activities with non-agricultural ones. Also, Tagliati (2019) finds that among older children (age 15-16 at the baseline) both transfers

[22] We show in Appendix Table A8 that the pre-intervention characteristics of students observed in the *ENLACE de Contexto* are balanced across treatment groups.

increased child labor, with a stronger impact for cash transfers. Taken all together, the available evidence is consistent with the hypothesis that the transfer contributed to a partial reallocation of agricultural tasks from adults and older children to younger ones.

In summary, the results in this section show that the income effect generated by either type of unconditional transfer was not large enough to induce a reduction in child labor and an increase in school attendance. Less robust evidence suggests that in cash localities there was an increase in the return to labor of young children and a higher probability of combining work and school attendance, compared to in-kind localities.

5.3 School quality

Children in our sample have access to three types of schools that vary greatly in terms of quality and costs. We estimate the impact of PAL transfers on the quality of school a child attended in several ways. First, we estimate the differential likelihood that a child attended a typically higher quality general school (vis-à-vis an indigenous or a community one). Second, we look at several school-level indicators that impact the cost of schools: the distance from the community to the school and fees parents pay for materials, uniforms, and general attendance. Third, we look at student-teacher ratio as a proxy for resource congestion.

Results are reported in Table 11. The probability of attending a general school increased by 2 percentage points for children from in-kind localities, with respect to a baseline of 77 percent for children from control localities (column 1). For those from cash localities the probability of attending a general school decreased by 8.3 percentage points, although the effect is not statistically significant (column 1). The difference between in-kind and cash localities is statistically significant at 10 percent level (p-value=0.07). For both the in-kind and the cash groups, we observe a reduction in the probability of attending schools less than 5km from the community and between 5 and 10km away (columns 2 and 3 respectively). These reductions are compensated by an increase in the probability of attending schools between 10 and 30km, and those more than 30km away (column 4 and 5). However, for none of the distance outcomes are differences between in-kind and cash localities statistically significant. Results in column 5 rule out the possibility that the differential effect of in-kind and cash treatments on learning outcomes is driven by a differential effect of the transfer modalities on household internal migration.[23] There is a 10 percent reduction in the school fee paid by

[23] McKenzie and Rapoport (2011) find that internal migration in Mexico can have negative impacts on the education attainment of children from rural areas.

parents from cash localities, but the total school fees paid by parents are not statistically different across any treatment groups (column 6). Finally, column 7 shows that while the size of the increase is non trivial for students from cash localities, there is no significant difference in the student-teacher ratio of schools attended by students across treatment groups.

In principle, the income effect generated by the transfer should have led to an increase in the propensity to attend a more expensive (and possibly better quality) school option. We find a negligible positive effect on the propensity to attend a general school among students from in-kind localities, and a large negative effect for students from cash localities. In line with the evidence presented in the previous section, the income effect generated by the program does not seem large enough to alter medium-term investments in education by beneficiary households.

As a result of the income shock, school principals in general schools might ask for higher school fees in treatment than in control localities, pushing students towards alternative modalities, and the increase can potentially differ for in-kind and cash localities. In order to test this hypothesis, we use information on the fees charged during the period between 2007 and 2013 by the general schools that were closest to evaluation localities, based on the pre-program school roster in 2003. We present the results in Table 12. General schools located closest to cash localities display the highest values for all three types of fees (materials, uniforms, and enrollment). When we look at the total amount, general schools located closest to cash localities have fees that are 306 pesos higher than in general schools closest to in-kind localities, about 39 percent of the average fee in general schools closest to control schools.

In order to provide a quantitative assessment of how important school type might be in explaining the differential effect of in-kind and cash treatment on learning outcomes, we perform a causal mediation analysis (Conti et al., 2016, Carneiro et al., 2019). The bounding procedure is detailed in Appendix A2 and the results are presented in Table 13. For all three subjects, the differential treatment effect on the probability of attending a general school can explain a very large share of the differential treatment effect on learning and is always statistically significant. In fact, even after controlling for the quality of the micronutrient intake, the differential effect on school type can explain at least 40 percent of the differential treatment effect on math, 49 percent for Spanish and 32 percent for the third subject.

The results presented in this section provides clear evidence that, when distributed in cash, PAL induced children to enroll in lower quality school options, possibly as a result of the large increase in fees implemented in the closest general schools. This finding contrasts with that of de Hoop et al. (2019) who find that in the presence of a partial subsidy, children in the Philippines increase labor supply in order to cover education costs. However, in our setting, we can not rule out the possibility that for children who decided not to switch to a lower quality option, the increase in the number of working hours might be partly driven by the need to cover the large increase in fees.

5.4 Parental investment

There are different channels through which PAL might affect parental involvement in children's education. Depending on whether the income effect generated by the program is larger or smaller than the substitution effect driven by the potentially higher labor market returns, parents might work more or less. Changes in labor supply might alter the involvement in children's education, for instance through help and supervision of home assignments or increased participation in school activities or visits. All else equal, parents might reallocate the time they devote to a specific child, depending on whether they want to complement or compensate for differential investments in early nutrition.

In the *ENLACE de Contexto,* students are asked how often (never, rarely, sometimes, almost always, always) parents are involved in five activities that could impact learning: helping with homework, explaining topics that were not clear from school lectures, inviting them to review class material that was not clear, paying attention to student grades, and attending school meetings. We create an index of parental investment which is the sum of five binary variables indicating whether a student replied "always" or "almost always" to each question. We also construct the first principle component of the five indicator variables. Table 14 reports that the treatment effects on both summary indices are positive for both groups, but only significant for the cash group (at the 5 percent level). The difference between the two treatment groups is small and not statistically significant. These results suggest that the differential impact of the in-kind and cash transfers on learning is unlikely to be driven by differences in the amount of time parents invest in activities related to their children's learning.

6 Discussion and Conclusions

In this paper, we studied how unconditional cash and in-kind welfare transfers impacted the standardized test scores of primary school children from rural and marginalized areas in Mexico. We merged individual-level data from the randomized controlled trial of the *Programa de Apoyo Alimentario* with administrative panel data on standardized test scores taken 4 to 10 years after transfers began. Despite the fact that both transfer modalities increased household consumption, and that in-kind transfers induced children to consume more key micronutrients, we find that in-kind transfers did not impact student learning and cash transfers significantly reduced student learning.

There are several reasons why the improved micronutrient intake induced by in-kind transfers did not lead to improved learning in primary school. First, the improvement in micronutrients was not accompanied by a significant increase in the overall caloric intake. A high share of children were consuming below the recommended daily allowance of calories even after transfers and a sufficient caloric intake may be a necessary compliment to micronutrient intake. Second, most of the previous evidence on the beneficial effects of iron supplementation on cognitive outcomes is based on interventions that target individuals diagnosed with iron deficiency anemia (IDA) with high doses of iron. At the opposite, in our study all children from PAL beneficiary households are potentially exposed to improved intakes, but the doses do not seem sufficient to benefit those with IDA at the baseline. In fact, we did not observe any statistically significant reduction in anemia in the follow-up survey. Finally, previous evidence shows that zinc supplementation per se is not sufficient to improve cognitive development. Powell et al. (2005) find for a group of undernourished children in Jamaica that zinc supplementation increases cognitive development only when complemented by psychosocial stimulation. This result together with the evidence from other studies that analyze the combination of micronutrient supplementation and psychosocial stimulation (see Attanasio et al., 2018) is consistent with the hypothesis that nutrition interventions only improve the cognitive development of children who have adequate stimulation, which may not have been the case in the poor, rural villages we study.

The discussion above, however, cannot explain why children from cash villages had lower test scores. We find suggestive evidence that these lower scores are driven by students in cash villages attending lower quality schools and being more likely to work. Students attended lower quality community or indigenous schools, possibly because principals at the higher quality general schools increase school fees. Several pieces of evidence are consistent with the hypothesis that cash transfer increased the return to child labor and decreased the time and energy dedicated to learning, including the fact that the negative impacts of the cash modality on learning are more prominent among households with older children and those that are more likely to be credit constrained.

Overall, our results show that the PAL welfare transfers, even when they lead to increased nutritional intake in early childhood, were not sufficient to improve learning in primary school. One lesson for policy is clear: behavioral responses to government policies can be complex and counterintuitive, especially as they compound into the medium- and long-term horizons.

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Tables

Table 1: Number of Observations in the Sample by Grade and Year

Academic year	Grade				Total
	3rd	4th	5th	6th	
2007	335	209	30	0	574
2008	557	442	280	37	1,316
2009	557	518	411	261	1,747
2010	531	544	514	403	1,992
2011	517	519	525	513	2,074
2012	420	408	441	426	1,695
2013	329	428	417	434	1,608
Total	3,246	3,068	2,618	2,074	11,006

Notes: Observations are at the student-year level.

Table 2: Balance of Main Variables at Baseline

	Control	In-kind	Cash	Obs.	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value
	(1)	(2)	(3)		(4)	(5)	(6)
Child level characteristics							
Male	0.51	0.51	0.47	4,405	0.87	0.04**	0.02***
	(0.02)	(0.01)	(0.01)				
Age	3.10	3.17	3.17	4,448	0.28	0.39	0.97
	(0.05)	(0.04)	(0.06)				
Caloric intake (kcal, daily)	831.67	805.68	817.99	2,347	0.47	0.73	0.72
	(29.40)	(21.06)	(27.00)				
Iron consumption (mg, daily)	5.25	5.07	5.19	2,392	0.47	0.82	0.67
	(0.19)	(0.15)	(0.22)				
Zinc consumption (mg, daily)	3.85	3.77	3.62	2,392	0.75	0.40	0.48
	(0.21)	(0.12)	(0.15)				
Vitamin C consumption (mg, daily)	31.76	29.63	34.92	2,392	0.54	0.51	0.20
	(3.00)	(1.80)	(3.73)				
Z-score height for age	-0.25	-0.21	-0.27	2,719	0.79	0.91	0.71
	(0.12)	(0.10)	(0.13)				
Math grade (1-10 scale, previous school year)	7.86	7.93	7.83	3,352	0.33	0.67	0.20
	(0.05)	(0.04)	(0.06)				
Spanish grade (1-10 scale, previous school year)	7.92	7.92	7.82	3,363	0.97	0.21	0.16
	(0.05)	(0.04)	(0.06)				
Household level characteristics							
Indigenous household	0.33	0.26	0.27	5,444	0.48	0.57	0.93
	(0.09)	(0.05)	(0.07)				
Number of household members	5.93	5.66	5.74	5,444	0.31	0.54	0.77
	(0.21)	(0.16)	(0.23)				
Household head is married	0.87	0.91	0.92	5,444	0.13	0.04**	0.39
	(0.02)	(0.01)	(0.01)				
Years of education of household head	7.36	7.30	7.11	5,444	0.88	0.56	0.61
	(0.30)	(0.22)	(0.29)				
Dirt floor in the home	0.40	0.40	0.41	5,444	0.94	0.88	0.80
	(0.05)	(0.04)	(0.05)				
Running water in the home	0.65	0.52	0.42	5,444	0.08	0.01***	0.20
	(0.06)	(0.05)	(0.07)				
Monthly per capita total expenditure	382.52	362.34	356.23	5,444	0.47	0.38	0.81
	(22.46)	(16.54)	(20.07)				
Village level characteristics							
Distance to closest primary school (km)	1.09	1.06	0.91	200	0.95	0.66	0.59
	(0.33)	(0.18)	(0.21)				
Closest school is a general school	0.81	0.87	0.80	200	0.30	0.96	0.26
	(0.06)	(0.03)	(0.05)				
Closest school is a community school	0.06	0.03	0.10	200	0.41	0.45	0.10
	(0.04)	(0.02)	(0.04)				
Closest school is a indigenous school	0.13	0.10	0.09	200	0.52	0.55	0.97
	(0.05)	(0.03)	(0.04)				
Student-teacher ratio in closest school	28.53	29.38	28.51	192	0.60	0.99	0.66
	(1.22)	(1.04)	(1.66)				
Repetition rate in closest school	0.09	0.09	0.12	200	0.70	0.20	0.10
	(0.01)	(0.01)	(0.02)				
Repetition rate in closest community or indigenous school	0.17	0.19	0.18	174	0.55	0.82	0.72
	(0.03)	(0.02)	(0.03)				
Morning shift closest school	0.85	0.91	0.94	200	0.27	0.09*	0.38
	(0.05)	(0.03)	(0.03)				
Yearly parental expenditure per child in closest general school (fees, uniform, books)	378.77	450.19	398.21	200	0.26	0.76	0.39
	(46.05)	(42.80)	(42.47)				

Notes: *** p<0.01, ** p<0.05, * p<0.1 (1) Standard errors in parentheses, clustered at the village level. (2) Data are from the pre-intervention PAL survey and the 2003 Formato 911 school databases. (3) Child consumption data was only collected for children aged 1 to 4 in the pre-program survey. (4) Math and Spanish grades are self-reported recalls of the student's most recent report card, sample includes students currently attending school. (5) A household is defined as indigenous if one or more members speak an indigenous language. (6) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos. (7) Parental school expenditure data was only collected for general schools, as government grants cover all costs for community and indigenous schools; it is in 2003 nominal pesos.

Table 3: The impact of PAL on taking ENLACE tests

Outcome =	Took at least one ENLACE	Took at least one ENLACE	Took ENLACE	Took ENLACE
	(1)	(2)	(3)	(4)
In-kind	0.009	-0.003	0.000	-0.004
	(0.019)	(0.017)	(0.011)	(0.010)
Cash	-0.001	0.004	-0.003	0.004
	(0.027)	(0.023)	(0.016)	(0.012)
Z-score height for age		0.022***		0.015***
		(0.005)		(0.002)
Closest school is a general school		0.029		0.021
		(0.025)		(0.014)
Male		0.000		-0.007
		(0.014)		(0.006)
Age		0.100***		0.073***
		(0.004)		(0.002)
ln(Monthly per capita total expenditure)		-0.017		-0.005
		(0.012)		(0.005)
Age of household head		-0.002***		-0.001***
		(0.001)		(0.000)
Years of education of household head		0.011***		0.006***
		(0.002)		(0.001)
Household head is married		0.070***		0.022**
		(0.025)		(0.011)
Running water in the home		0.005		0.009
		(0.017)		(0.008)
State fixed effects	YES	YES	YES	YES
Year fixed effects	NO	NO	YES	YES
Outcome mean in control group	0.65	0.65	0.27	0.27
Observations	5,444	3,817	38,108	26,719
***Effect size:* In-kind - Cash**	0.01	-0.01	0.00	-0.01
H_0: In-kind = Cash (p-value)	0.69	0.74	0.83	0.48

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) The outcome in columns 1-3 are indicators for whether a student was observed to take any ENLACE test between 2007 and 2013, and regressions include one observation per child. The outcome in columns 4-6 vary by year, and regressions include one observation for each child in every year from 2007 to 2013.

Table 4: Explaining ENLACE test scores

Outcome =	Math (z-score)	Spanish (z-score)	3rd subject (z-score)
	(1)	(2)	(3)
General school	0.523***	0.666***	0.534***
	(0.121)	(0.143)	(0.100)
Male	-0.117*	-0.220***	-0.180***
	(0.063)	(0.053)	(0.049)
Z-score height for age	0.050**	0.049*	0.044**
	(0.025)	(0.025)	(0.020)
Age	-0.059	-0.080*	-0.037
	(0.048)	(0.046)	(0.042)
ln(Monthly per capita total expenditure)	-0.036	0.008	-0.069
	(0.065)	(0.069)	(0.061)
Age of household head	0.001	0.002	-0.000
	(0.003)	(0.002)	(0.002)
Years of education of household head	0.023**	0.020	0.020**
	(0.011)	(0.012)	(0.009)
Household head is married	-0.018	0.011	0.002
	(0.123)	(0.140)	(0.095)
Running water in the home	0.156**	0.201**	0.168**
	(0.075)	(0.089)	(0.072)
Guerrero	-0.334	-0.657**	-0.445**
	(0.225)	(0.271)	(0.196)
Oaxaca	0.500**	0.365	0.308
	(0.194)	(0.221)	(0.275)
Tabasco	-0.548***	-0.490***	-0.635***
	(0.135)	(0.146)	(0.126)
Veracruz	-0.603***	-0.677***	-0.747***
	(0.100)	(0.131)	(0.091)
Year and grade FE	YES	YES	YES
Observations	1,576	1,576	1,573
R-squared	0.109	0.150	0.128

*** p<0.01, ** p<0.05, * p<0.1.

(1) Standard errors are clustered at the village level.

(2) The ommited state is Chiapas.

(3) Sample includes only individuals from the control group.

Table 5: The impact of PAL on learning

Outcome =	Math (z-score)	Math (z-score)	Spanish (z-score)	Spanish (z-score)	3rd subject (z-score)	3rd subject (z-score)
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	-0.050	-0.049	-0.025	-0.026	-0.029	-0.029
	(0.078)	(0.073)	(0.086)	(0.081)	(0.075)	(0.071)
Cash	-0.192**	-0.182**	-0.158*	-0.156*	-0.161**	-0.156*
	(0.084)	(0.086)	(0.091)	(0.093)	(0.080)	(0.080)
State FE	YES	YES	YES	YES	YES	YES
Year FE	NO	YES	NO	YES	NO	YES
Grade FE	NO	YES	NO	YES	NO	YES
Pre-program controls	NO	YES	NO	YES	NO	YES
Observations	11,006	11,006	11,006	11,006	10,432	10,432
***Effect size:* In-kind - Cash**	0.14	0.13	0.13	0.13	0.15	0.13
H_0: In-kind = Cash (p-value)	0.05*	0.06*	0.07*	0.07*	0.05*	0.05*
H_0: In-kind = Cash (Randomization Inference p-value)	0.08*	0.09*	0.11	0.1	0.08*	0.08*

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) The 3rd subject was not administered in 2007, and it covered Science in 2008 and 2012, Ethics and Civics in 2009 and 2013, History in 2010, and Geography 2011.

Table 6: Heterogeneous impact of PAL on learning by household expenditure

Outcome =	Math (z-score)	Spanish (z-score)	3rd subject (z-score)
	(1)	(2)	(3)
In-kind	-0.001	0.051	0.062
	(0.071)	(0.073)	(0.067)
Cash	-0.130	-0.079	-0.085
	(0.081)	(0.083)	(0.076)
In-kind x poor	-0.089	-0.151	-0.181**
	(0.091)	(0.105)	(0.090)
Cash x poor	-0.101	-0.156	-0.144
	(0.108)	(0.119)	(0.102)
Poor	-0.075	-0.056	0.004
	(0.079)	(0.096)	(0.079)
Observations	11,006	11,006	10,432
***Effect size:* In-kind - Cash**	0.13	0.13	0.15
***H_0:* In-kind = Cash (p-value)**	0.07*	0.07*	0.03**
***Effect size:* In-kind x poor - Cash x poor**	0.01	0.01	-0.04
***H_0:* In-kind x poor = Cash x poor (p-value)**	0.89	0.95	0.64

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) All models include state fixed effects, year fixed effects, and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) "Poor" is an indicator variable equal to one for households with expenditure per capita below median.

Table 7: Heterogeneous impact of PAL on learning by indigenous ethnicity

Outcome =	Math (z-score)	Spanish (z-score)	3rd subject (z-score)
	(1)	(2)	(3)
In-kind	0.009	0.021	0.043
	(0.069)	(0.070)	(0.065)
Cash	-0.118	-0.106	-0.086
	(0.075)	(0.078)	(0.073)
In-kind x Indigenous household	-0.245	-0.207	-0.288*
	(0.153)	(0.176)	(0.159)
Cash x Indigenous household	-0.206	-0.137	-0.221
	(0.192)	(0.195)	(0.185)
Indigenous household	-0.291**	-0.375**	-0.199
	(0.141)	(0.170)	(0.156)
Observations	11,006	11,006	10,432
***Effect size:* In-kind - Cash**	0.13	0.13	0.13
***H_0:* In-kind = Cash (p-value)**	0.06*	0.06*	0.05**
Effect size: In-kind x indigenous - Cash x indigenous	-0.04	-0.07	-0.07
***H_0:* In-kind x indigenous = Cash x indigenous (p-value)**	0.81	0.61	0.61

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) All models include state fixed effects, year fixed effects, and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

Table 8: The impact of PAL on health outcomes

Outcome =	Person was ever sick in last 4 weeks		Z score height for age		Z score weight for age		Anemia	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In-kind	-0.023	-0.015	0.025	0.031	0.026	0.020	-0.021	-0.020
	(0.027)	(0.027)	(0.107)	(0.118)	(0.085)	(0.094)	(0.029)	(0.031)
Cash	0.001	0.002	-0.109	-0.082	-0.005	-0.001	-0.024	-0.022
	(0.032)	(0.033)	(0.136)	(0.145)	(0.099)	(0.111)	(0.030)	(0.033)
Restricted to those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	4,266	3,138	3,817	2,494	3,861	2,522	2,403	1,855
Outcome mean in control group	0.29	0.30	-0.32	-0.20	0.99	0.12	0.19	0.19
***Effect size:* In-kind - Cash**	-0.02	-0.02	0.13	0.11	0.03	0.02	0.00	0.00
***H_0:* In-kind = Cash (p-value)**	0.38	0.55	0.23	0.32	0.69	0.80	0.91	0.93
***H_0:* In-kind = Cash (Randomization Inference p-value)**	0.37	0.57	0.25	0.34	0.73	0.83	0.92	0.93

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) State fixed effects and the follosing pre-program controls included: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.
(3) Sample only includes individuals aged 6 or younger in 2003.

Table 9: The impact of PAL on nutrition

Outcome =	Energy (kcal)		Proteins		Vitamin C		Iron	
In-kind	48.128	38.504	1.760	1.374	25.221***	23.887***	1.058***	1.130***
	(41.529)	(46.073)	(1.760)	(1.931)	(5.209)	(5.617)	(0.385)	(0.421)
Cash	0.996	-5.474	1.933	1.726	25.257***	23.958***	0.547	0.538
	(48.650)	(52.738)	(1.990)	(2.237)	(7.627)	(8.681)	(0.421)	(0.453)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES	YES	YES	YES	YES
Restrc. to those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	2,381	1,856	2,419	1,880	2,419	1,880	2,419	1,880
Outcome mean in control group	967.54	980.10	32.48	33.48	31.89	32.37	6.81	6.79
***Effect size:* In-kind - Cash**	47.13	43.98	-0.17	-0.35	-0.04	-0.07	0.51	0.59
***H_0:* In-kind = Cash (p-value)**	0.21	0.26	0.92	0.84	1.00	0.99	0.20	0.14
***H_0:* In-kind = Cash (Randomization Inference p-value)**	0.22	0.28	0.92	0.85	1.00	0.99	0.21	0.17

Outcome =	Zinc		Calcium		Retinol		Principal component macro/micro nutrients	
In-kind	1.155***	1.098***	76.264***	80.725***	114.716**	107.959**	0.252***	0.247***
	(0.265)	(0.291)	(25.014)	(28.578)	(48.289)	(50.538)	(0.070)	(0.078)
Cash	0.518*	0.444	19.666	16.556	27.197	16.344	0.132*	0.119
	(0.270)	(0.296)	(30.679)	(34.199)	(54.694)	(56.472)	(0.076)	(0.084)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES	YES	YES	YES	YES
Restrc. to those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	2,419	1,880	2,419	1,880	2,419	1,880	2419	1,880
Outcome mean in control group	4.28	4.36	467.54	468.09	360.17	342.23	-0.32	-0.31
***Effect size:* In-kind - Cash**	0.64	0.65	56.60	64.17	87.52	91.61	0.12	0.13
***H_0:* In-kind = Cash (p-value)**	0.02**	0.02**	0.05*	0.04**	0.05*	0.04**	0.10*	0.09*
***H_0:* In-kind = Cash (Randomization Inference p-value)**	0.03**	0.04**	0.05*	0.04**	0.05*	0.05*	0.12	0.12

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.
(3) Sample only includes individuals aged 6 or less in 2003.
(4) Principal component includes proteins, iron, zinc, calcium and retinol.

Table 10: The impact of PAL on child labor

Outcome =	(1) Works & attends school	(2) Only works	(3) Only attends	(4) Neither works nor attends	(5) Hours of work (including 0)	(6) Average number of working days per week	(7) At least 1 hour per day of help with domestic work
In-kind	-0.001	-0.009	0.004	0.006	-0.653	0.683	0.176
	(0.009)	(0.017)	(0.040)	(0.029)	(0.863)	(0.420)	(0.166)
Cash	0.011	0.030	-0.062	0.021	1.555	1.313**	0.195
	(0.013)	(0.023)	(0.049)	(0.033)	(1.411)	(0.593)	(0.191)
State FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Grade FE	YES	YES	YES	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES	YES	YES	YES
Observations	986	986	986	986	988	310	113
Outcome mean in control group	0.01	0.05	0.84	0.1	2.06	1.12	0.21
***Effect size:* In-kind - Cash**	-0.01	-0.04	0.07	-0.02	-2.21	-0.63	-0.02
H_0: In-kind = Cash (p-value)	0.33	0.05*	0.09*	0.54	0.07*	0.30	0.91
H_0: In-kind = Cash (Randomization Inference p-value)	0.30	0.04**	0.10*	0.59	0.03**	0.37	0.96

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.
(3) The outcomes in columns (1) to (5) are based on the information collected in the 2005 follow-up survey and refer to the week prior to the survey; the sample includes children age 12 and 13 who are reported to be enrolled in primary school. The outcomes in columns (6) and (7) are based on the Enlace de Contexto which asks information on the average number of working days for all years between 2008-13 and information on household chores for years between 2011-13.

Table 11: The impact of PAL on school characteristics

Outcome =	General school	Km from village to school = [0,5)	Km from village to school = [5,10)	Km from village to school = [10,30)	Km from village to school >= 30	Total parental contributions	Student-teacher ratio
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
In-kind	0.022	-0.052	-0.037	0.031	0.058	-45.43	0.38
	(0.061)	(0.053)	(0.033)	(0.020)	(0.038)	-121.68	-1.49
Cash	-0.083	-0.074	-0.024	0.056	0.041	-30.83	2.16
	(0.075)	(0.067)	(0.034)	(0.038)	(0.047)	-154.31	-2.29
Observations	10,852	10,852	10,852	10,852	10,852	10,740	10,344
Outcome mean in control group	0.77	0.83	0.07	0.02	0.08	730.42	28.71
***Effect size:* In-kind - Cash**	0.10	0.02	-0.01	-0.03	0.02	-14.6	-1.77
H_0: In-kind = Cash (p-value)	0.07*	0.75	0.54	0.55	0.75	0.89	0.44
H_0: In-kind = Cash (Randomization Inference p-value)	0.07*	0.74	0.51	0.51	0.76	0.89	0.38

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) All models include state fixed effect, year fixed effects, and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and the whether the closest school offers morning shift.
(3) The outcome in column (1) is an indicator for a student attending a general school. The outcomes in column (2)-(5) are indicators for whether the driving distance from the center of the village to the school is within the specified range. The outcome in column (6) is the total cost that includes contributions to the school, materials and uniforms; this cost is by definition 0 in community and indigenous schools (see text). The outcome in column (7) is the average number of students per teacher in the school.

Table 12: The impact of PAL on general school fees

	(1)	(2)	(3)	(4)
Outcome =	Materials	Uniforms	Enrollment	Total
In-kind	18.070	-7.158	-33.172	-22.261
	(50.691)	(50.162)	(96.926)	(192.011)
Cash	111.118*	81.511	91.201	283.830
	(58.950)	(58.335)	(112.718)	(223.295)
State FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES
Observations	1,372	1,372	1,372	1,372
Outcome mean in control group	291.70	323.04	162.11	776.85
***Effect size:* In-kind - Cash**	-93.05	-88.67	-124.37	-306.09
H_0: In-kind = Cash (p-value)	0.06	0.07	0.19	0.10
H_0: In-kind = Cash (Randomization Inference p-value)	0.15	0.14	0.22	0.14

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Regressions are at locality level.

(2) Pre-program controls include quartile dummies for distance and schoolenrollment, as measured in 2003, and dummies for whether the school had a morning shift in 2003, and whether the information on the shift was not available.

(3) Outcome variables are based on the information from the Formato 911 for the years from 2007 to 2013.

Table 13: Mediation analysis on learning outcomes

Outcome =	Math			Spanish			3rd subject		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
In-kind	-0.098	-0.104	-0.108	-0.047	-0.054	-0.062	-0.053	-0.058	-0.058
	(0.080)	(0.070)	(0.071)	(0.089)	(0.075)	(0.075)	(0.077)	(0.070)	(0.071)
Cash	-0.233**	-0.171**	-0.173**	-0.171*	-0.104	-0.108	-0.192**	-0.137*	-0.137*
	(0.091)	(0.078)	(0.078)	(0.100)	(0.084)	(0.083)	(0.087)	(0.078)	(0.078)
Attends a general school		0.621***	0.619***		0.661***	0.659***		0.552***	0.552***
		(0.081)	(0.081)		(0.081)	(0.080)		(0.084)	(0.084)
Principal component of nutrients			0.011			0.024			-0.002
			(0.016)			(0.016)			(0.015)
Observations	5,988	5,988	5,988	5,988	5,988	5,988	5,985	5985	5985
***Effect size:* In-kind - Cash**	0.136	0.0671	0.0651	0.123	0.0501	0.0460	0.139	0.0786	0.0790
H_0: In-kind = Cash (p-value)	0.07*	0.32	0.33	0.123	0.49	0.53	0.055	0.23	0.23
Indirect Differential Effect through School Type:									
Lower Bound		0.059	0.055		0.065	0.06		0.055	0.044
Upper Bound		0.079	0.083		0.086	0.092		0.076	0.076
Indirect Differential Effect through Micronutrients:									
Lower Bound			-0,005			-0.002			-0.007
Upper Bound			0,009			0.011			0.005

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level. The lower bound and the upper bound are computed based on the results of 1,000 Montecarlo simulations.

(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) Sample is restricted only to children for which learning outcomes both intermediate outcomes are measured.

(4) Principal component includes proteins, iron, zinc, calcium and retinol.

Table 14: The impact of PAL on parental investment

Outcome =	Index of parental involvement in activities	Principal component of parental activities
	(1)	(2)
In-kind	0.459*	0.309*
	(0.266)	(0.183)
Cash	0.343	0.238
	(0.319)	(0.219)
Observations	283	283
Outcome mean in control group	3.19	-0.02
***Effect size:* In-kind - Cash**	0.12	0.07
H_0: In-kind = Cash (p-value)	0.68	0.71
H_0: In-kind = Cash (Randomization Inference p-value)	0.78	0.81

Notes: *** p<0.01, ** p<0.05, * p<0.1

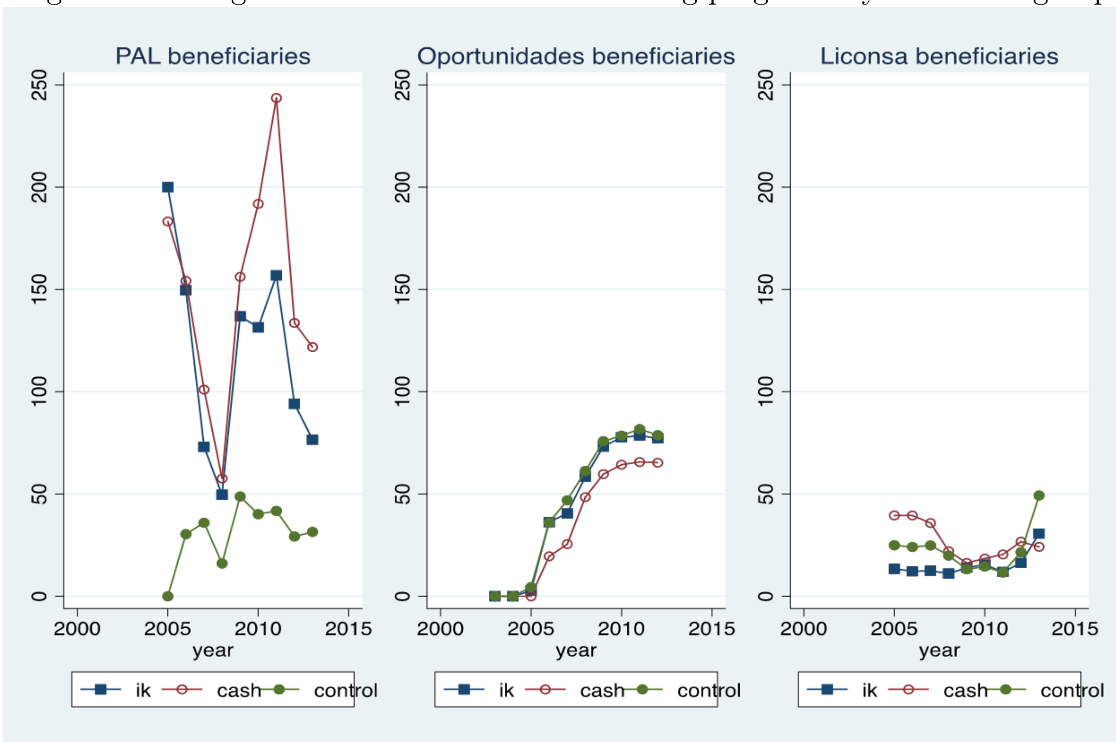
(1) Standard errors are clustered at the village level.

(2) All models include state, year, and grade fixed effects and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) Results based on the Enlace de Contexto for all years between 2008-13

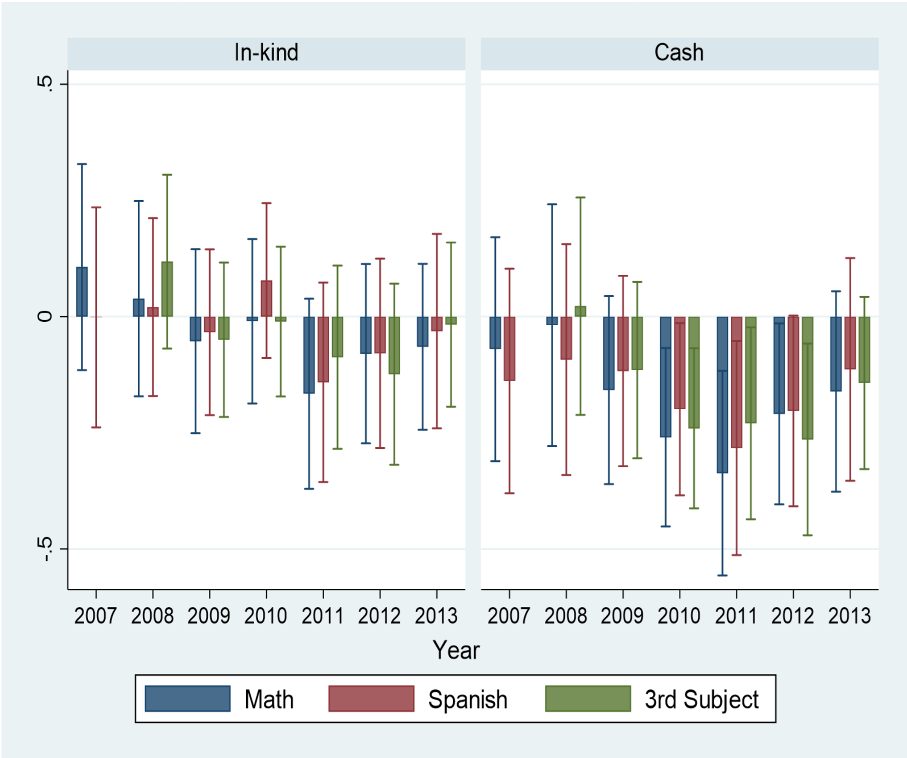
(4) The *Index of parental activities* equals the number of activities where parents are involved among the following: 1) helping with homework; 2) explaining topics that were not clear from the lecture; 3) inviting them to review class material that was not clear; 4) paying attention to student grades; 5) attending school meetings. The principal component of parental activities is the first component of the 5 indicators of parental activity.

Figure 1: Average number of households receiving programs by treatment group



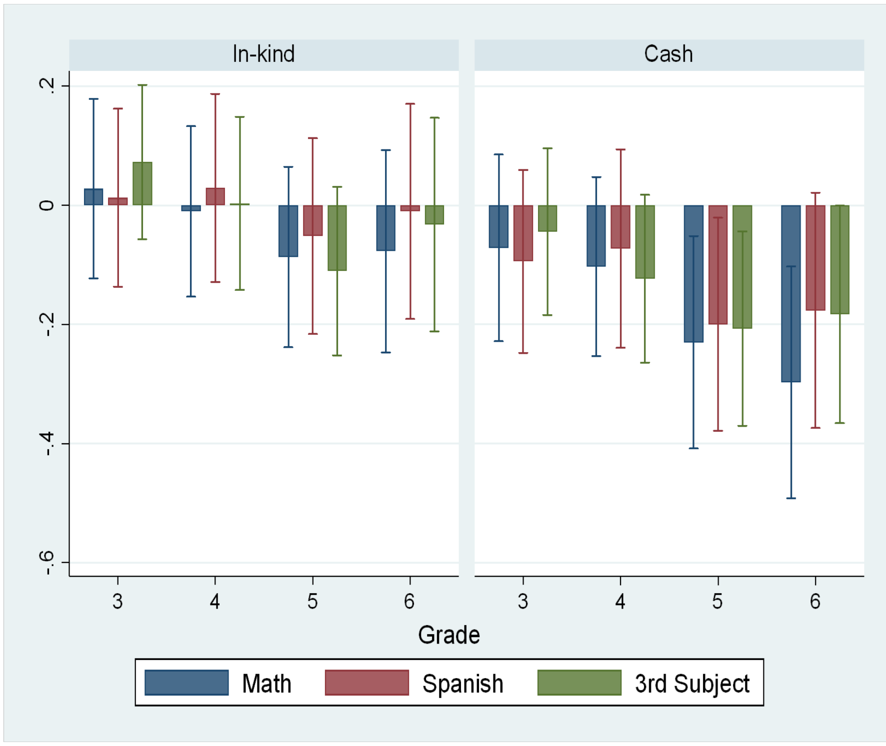
Notes: Year/group specific averages were obtained by averaging village level number of beneficiary households provided by SEDESOL.

Figure 2: Impact of PAL on learning by year



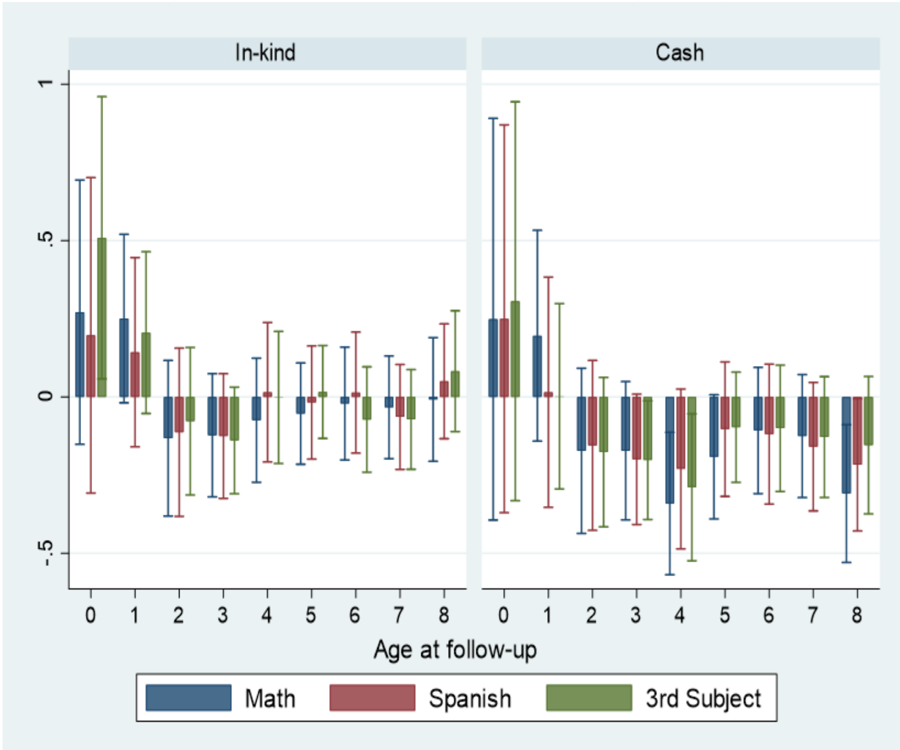
Notes: (1) Coefficients are from models that include pre-program controls, state and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 3: Heterogeneous impact of PAL on learning by grade



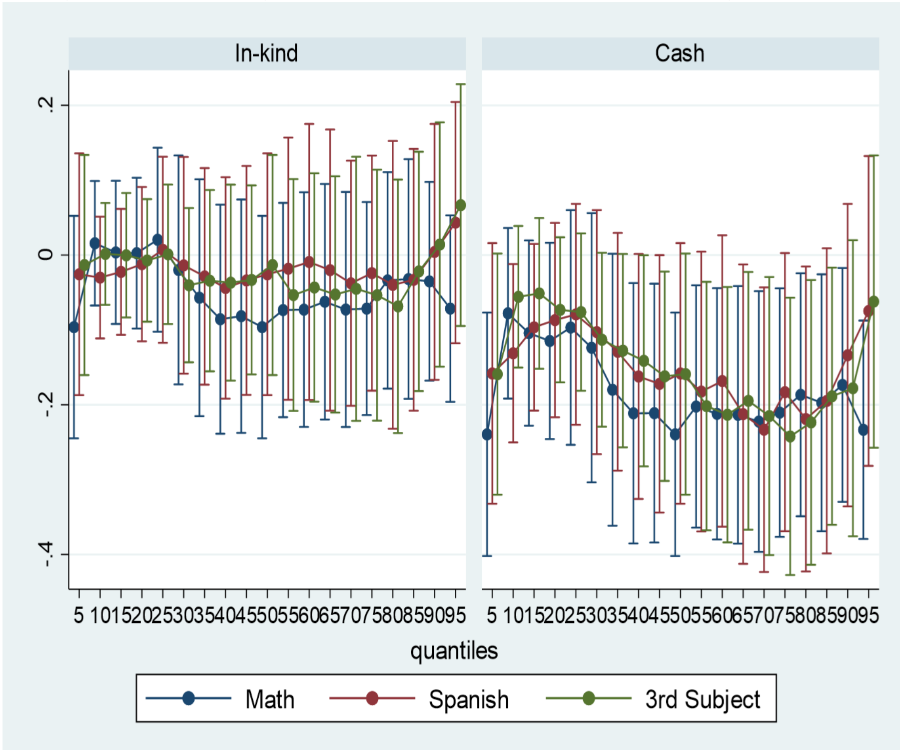
Notes: (1) Coefficients are from models that include pre-program controls, and state and year fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 4: Heterogeneous impact of PAL on learning by age at the follow-up



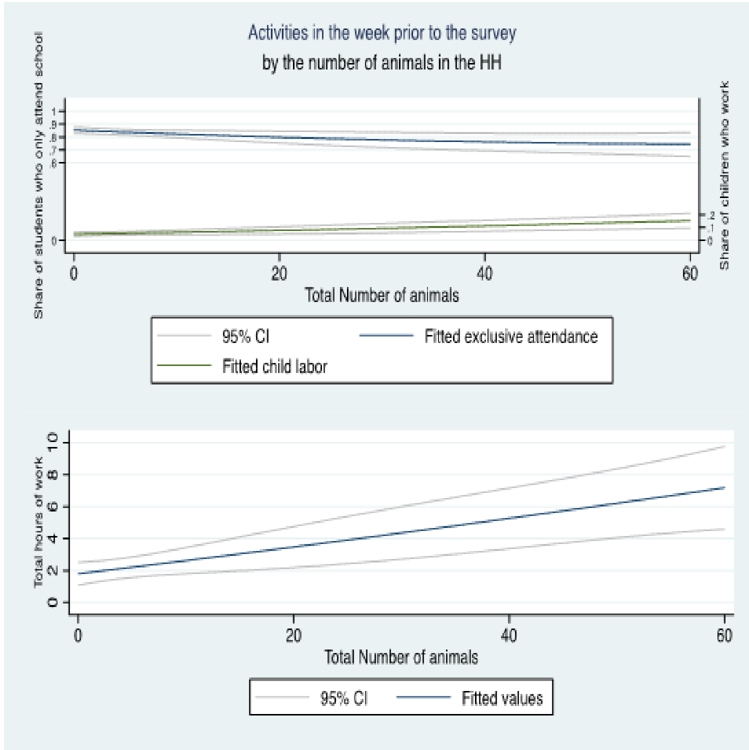
Notes: (1) Coefficients are from models that include pre-program controls, and state, year, and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 5: Heterogeneous impact of PAL on learning: quantiles



Notes: (1) Coefficients are from models that include pre-program controls, and state, year, and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 6: Child labor, school attendance and number of animals



Source: Baseline household survey. The sample is restricted to children age 12 and 13 who report being enrolled in primary school. The top panel plots the share of students who report attending school as the only activity and the share of those working (either exclusively or in combination with school attendance) vis-à-vis the total number of animals owned by the household. The bottom panel plots the number of hours of work (including 0s) vis-a-vis the total number of animals owned by the household.

Appendix

A1 Data Merge

Mexican citizens have a unique personal identifier, known as *Clave Única de Registro Poblacional, CURP*, formed by an algorithm combining name, surname, date of birth, sex, state of birth, plus two randomly generated digits. Using individual personal information collected both during the baseline and follow-up survey we were able to generate a quasi-*CURP* that differs from the real one only in the lack of the last two randomly generated digits. With the quasi-*CURPs* in hand, we were able to merge the baseline survey with the micro data from the ENLACE 3rd to 6th grade for the period 2007-2013, and the *ENLACE de contexto*. There are two potential explanations for the partial attrition of the ENLACE scores: (1) the exam is voluntary and students enrolled in primary might have not taken it, and (2) matching issues arose either because we could not generate a quasi-*CURP* or there were multiple individuals with the same identifier.

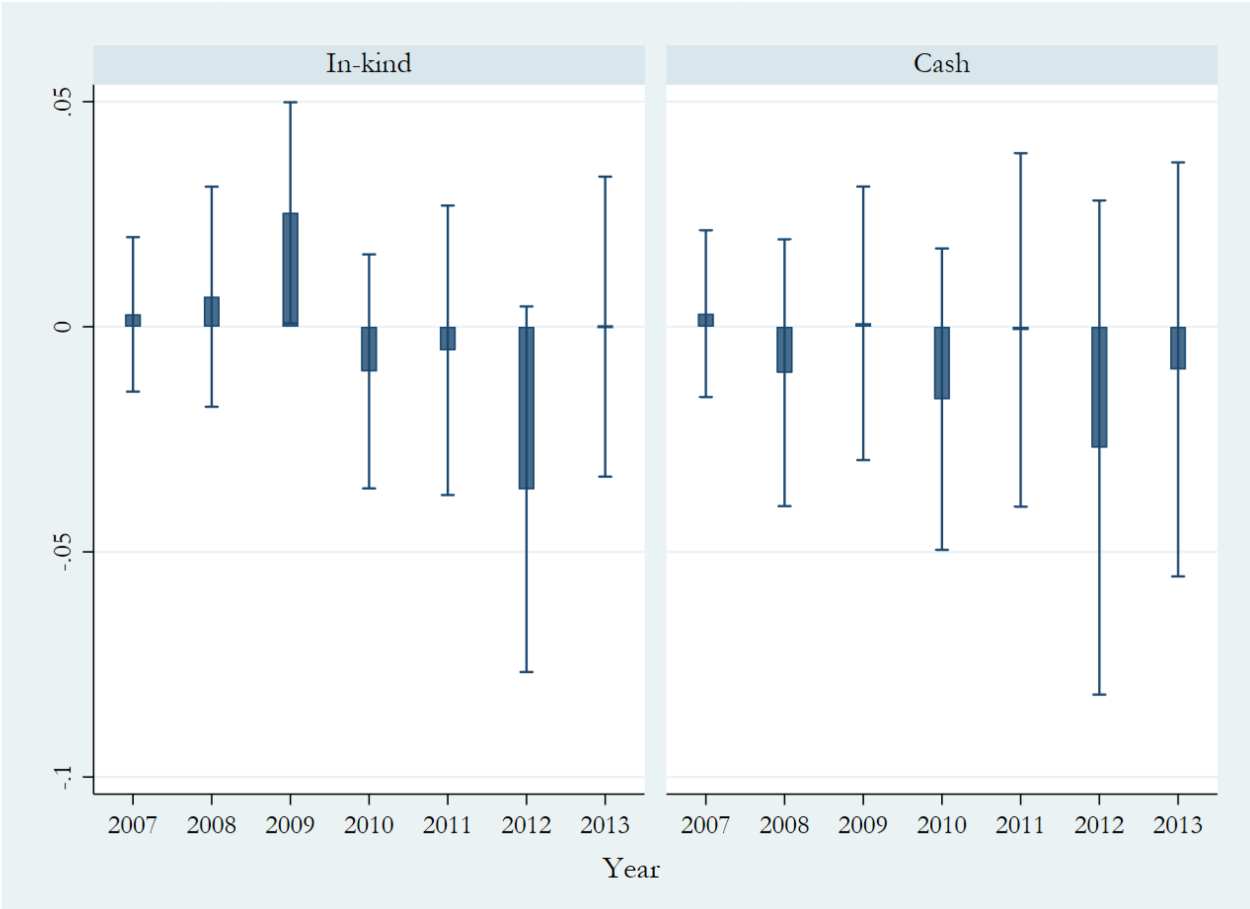
A2 Mediation Analysis

In this section we describe a standard mediation analysis to examine to what extent the differential impact of the in-kind and cash modalities on learning outcomes is driven by the differential impact on the type of school attended and the quality of micronutrient intakes. The assumptions under which one can decompose treatment effects estimates into different components are however very strong. This means that the results can only be interpreted as suggestive evidence of the importance of these mediators. In a standard mediation model where the outcome of interest is Y and the mediating factor (observed measured input) is M (it can be a vector of factors), the goal is to separately identify the interventions total indirect effect $((\gamma_{ik} - \gamma_{cash}) \cdot \delta)$ from the direct differential effect $(\beta_{ik} - \beta_{cash})$ from the following model:

$$\begin{aligned} Y_{ij} &= \alpha_0 + \beta_{ik} Ik_j + \beta_{cash} Cash_j + \delta M_{ij} + u_{ij} \\ M_{ij} &= \alpha_1 + \gamma_{ik} Ik_j + \gamma_{cash} Cash_j + e_{ij} \end{aligned}$$

where γ_{ik} and γ_{cash} are the ITT estimates of PAL on a particular mediator (type of school and quality of micronutrients), and the marginal effect of mediator on the learning outcomes. We estimate the model in steps using a Monte Carlo simulation approach. First, we estimate the coefficients by regressing the effect of in-kind and cash treatment assignments on each mediator. Second, we obtain estimates of δ from a regression of learning outcomes on treatment status (as in the ITT equation, controlling for the baseline regressors) and add one particular mediator at a time. We then compute the lower bound and upper bound of $((\gamma_{ik} - \gamma_{cash}) \cdot \delta)$ based on 1,000 Montecarlo repetitions. An interval that does not include zero indicates a significant indirect differential effect of that particular mediating variable on learning outcomes.

Figure A1: The impact of PAL on taking ENLACE tests by year



Notes: (1) Coefficients are from models that include pre-program controls and state fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Table A1: Post-experiment

	Year 2005			Year 2010		
Outcome=	Number of Households	HHs with Oportunidades	Share of Oportunidades recipients	Number of Households	HHs with Oportunidades	Share of Oportunidades recipients
	(1)	(2)	(3)	(4)	(5)	(6)
In-Kind	-58.33*	-1.760	0.00407	-57.97*	-7.800	-0.0175
	(34.89)	(4.155)	(0.00620)	(34.81)	(13.36)	(0.0468)
Cash	-65.77*	-1.184	0.00274	-63.78*	-17.02	-0.0811
	(36.24)	(2.798)	(0.00418)	(36.18)	(15.37)	(0.0553)
Observations	197	197	197	197	197	190
Outcome mean in control group	244.64	4.62	0.01	245.28	83.55	0.42
***Effect size:* In-kind - Cash**	7.44	-0.58	0.00	5.81	9.22	0.06
***H_0:* In-kind = Cash (p-value)**	0.78	0.67	0.51	0.83	0.49	0.19

Notes: *** p<0.01, ** p<0.05, * p<0.1.

(1) The number of households was obtained from Population Census. The number of households that receive Oportunidades was provided by SEDESOL. The share of households that receive Oportunidades is calculated as ratio of the former two.

(2) State fixed effects included.

(3) Three localities (out of 200) have been merged with bigger localities. In 2010, in 7 (out of 197) localities the number of households that receive Oportunidades was larger than the total number of households. In those cases we set the share equal to missing.

Table A2: Impact of PAL on the probability of being in the right age for grade

Outcome =	Appropriate age for grade	At least one appropriate age for
	(1)	(2)
In-kind	0.000	-0.007
	(0.006)	(0.018)
Cash	0.003	-0.021
	(0.007)	(0.027)
State FE	YES	YES
Year FE	YES	NO
Grade FE	YES	NO
Pre-program controls	YES	YES
Observations	11,006	5,444
Outcome mean in control group	0.24	0.62
***Effect size:* In-kind - Cash**	0.00	0.01
***H_0:* In-kind = Cash (p-value)**	0.60	0.57

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) Pre-program controls include gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.

(3) The dependent variable in (1) is an indicator function that equals one when a student has the appropriate age for the grade he/she is observed. The appropriate ages are defined based on the number of completed years of age at Dec 31st of the year observed. The appropriate ages are between 8 and 10 for grade 3, between 9 and 11 for grade 4, between 10 and 12 for grade 5, and between 11 and 13 for grade 6.

(4) The dependent variable in (2) is an indicator variable that equals one if the student was observed at least once with the appropriate age for his/her grade.

Table A3: Balance across treatment arms at baseline amongst the non-attrited sample

	Control	In-kind	Cash	Obs.	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value
	(1)	(2)	(3)		(4)	(5)	(6)
	Child level characteristics						
Male	0.53	0.53	0.47	3,773	0.91	0.04	0.01
	(0.02)	(0.01)	(0.02)				
Age	2.93	2.90	2.89	3,773	0.80	0.72	0.89
	(0.09)	(0.07)	(0.08)				
Caloric intake (kcal, daily)	826.78	814.55	846.26	1,840	0.75	0.66	0.40
	(31.19)	(21.93)	(30.54)				
Iron consumption (mg, daily)	5.13	5.09	5.26	1,875	0.89	0.70	0.56
	(0.22)	(0.16)	(0.24)				
Zinc consumption (mg, daily)	3.78	3.75	3.72	1,875	0.93	0.85	0.86
	(0.25)	(0.12)	(0.15)				
Vitamin C consumption (mg, daily)	33.19	30.88	35.01	1,875	0.59	0.74	0.38
	(3.68)	(2.12)	(4.15)				
Z-score height for age	-0.19	-0.17	-0.14	2,094	0.90	0.80	0.87
	(0.13)	(0.11)	(0.13)				
	Household level characteristics						
Indigenous household	0.30	0.25	0.24	3,773	0.63	0.59	0.92
	(0.08)	(0.05)	(0.07)				
Number of household members	5.80	5.63	5.68	3,773	0.54	0.71	0.86
	(0.22)	(0.16)	(0.22)				
Household head is married	0.90	0.91	0.92	3,773	0.43	0.16	0.41
	(0.02)	(0.01)	(0.01)				
Years of education of household head	7.67	7.43	7.43	3,773	0.51	0.55	0.99
	(0.30)	(0.23)	(0.28)				
Dirt floor in the home	0.35	0.38	0.36	3,773	0.67	0.89	0.75
	(0.05)	(0.04)	(0.04)				
Running water in the home	0.65	0.53	0.43	3,773	0.13	0.01	0.19
	(0.06)	(0.05)	(0.07)				
Monthly per capita total expenditure	386.97	360.12	362.98	3,773	0.42	0.49	0.92
	(27.69)	(18.23)	(21.54)				
	Village level characteristics						
Distance to closest primary school (km)	0.91	1.17	0.89	3,773	0.48	0.96	0.36
	(0.28)	(0.22)	(0.20)				
Closest school is a general school	0.76	0.79	0.73	3,773	0.76	0.76	0.51
	(0.08)	(0.05)	(0.07)				
Closest school is a community school	0.05	0.05	0.13	3,773	0.84	0.16	0.21
	(0.03)	(0.03)	(0.05)				
Closest school is a indigenous school	0.19	0.15	0.14	3,773	0.68	0.59	0.85
	(0.07)	(0.05)	(0.06)				
Student-teacher ratio in closest school	30.23	29.34	30.04	3,658	0.65	0.94	0.78
	(1.62)	(1.10)	(2.19)				
Repetition rate in closest school	0.10	0.09	0.13	3,773	0.91	0.27	0.20
	(0.01)	(0.01)	(0.03)				
Repetition rate in closest community or indigenous school	0.16	0.17	0.19	3,325	0.71	0.61	0.78
	(0.03)	(0.02)	(0.04)				
Morning shift closest school	0.78	0.85	0.93	3,773	0.41	0.07	0.20
	(0.07)	(0.05)	(0.04)				
Yearly parental expenditure per child in closest general school (fees, uniform, books)	338.23	406.01	377.42	3,773	0.32	0.58	0.68
	(49.20)	(47.47)	(50.63)				

Notes: *** p<0.01, ** p<0.05, * p<0.1 (1) Standard errors in parentheses, clustered at the village level. (2) Data are from the pre-intervention PAL survey and the 2003 Formato 911 school databases. (3) Child consumption data was only collected for children aged 1 to 4 in the pre-program survey. (4) Math and Spanish grades are self-reported recalls of the student's most recent report card, sample includes students currently attending school. (5) A household is defined as indigenous if one or more members speak an indigenous language. (6) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos. (7) Parental school expenditure data was only collected for general schools, as government grants cover all costs for community and indigenous schools; it is in 2003 nominal pesos.

Table A4: The impact of PAL on test taking - 3 treatment arms

Outcome =	Took at least one ENLACE	Took at least one ENLACE	Took ENLACE	Took ENLACE
	(1)	(2)	(3)	(4)
In-kind only	0.023	0.019	0.005	0.004
	(0.021)	(0.020)	(0.012)	(0.011)
In-kind & Education classes	-0.006	-0.017	-0.004	-0.009
	(0.022)	(0.019)	(0.012)	(0.011)
Cash	-0.001	-0.015	-0.003	-0.009
	(0.027)	(0.025)	(0.016)	(0.014)
State fixed effects	YES	YES	YES	YES
Year fixed effects	NO	YES	NO	YES
Pre-program controls	NO	YES	NO	YES
Observations	5,444	5,444	38,108	38,108
Outcome mean in control group	0.65	0.65	0.27	0.27
Effect size: **In-kind only - In-kind & Education classes**	-0.03	-0.04	-0.01	-0.01
H_0: **In-kind only - In-kind & Education classes (p-value)**	0.15	0.07*	0.40	0.19

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) The outcome in columns 1-3 are indicators for whether a student was observed to take any ENLACE test between 2007 and 2013, and regressions include one observation per child. The outcome in columns 4-6 vary by year, and regressions include one observation for each child in every year from 2007 to 2013.

(3) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

Table A5: The impact of PAL on learning - 3 treatments arms

Outcome =	Math (z-score)	Math (z-score)	Spanish (z-score)	Spanish (z-score)	3rd subject (z-score)	3rd subject (z-score)
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind only	-0.069	-0.052	-0.038	-0.018	-0.048	-0.032
	(0.094)	(0.085)	(0.102)	-0.092	(0.088)	(0.080)
In-kind & Education classes	-0.031	-0.046	-0.012	-0.034	-0.009	-0.027
	(0.088)	(0.085)	(0.094)	(0.091)	(0.088)	(0.084)
Cash	-0.192**	-0.182**	-0.159*	-0.156*	-0.162**	-0.156*
	(0.084)	(0.086)	(0.091)	(0.093)	(0.080)	(0.080)
State FE	YES	YES	YES	YES	YES	YES
Year FE	NO	YES	NO	YES	NO	YES
Grade FE	NO	YES	NO	YES	NO	YES
Pre-program controls	NO	YES	NO	YES	NO	YES
Observations	11,006	11,006	11,006	11,006	10,432	10,432
***Effect size:* In-kind only - In-kind & Education classes**	0.04	0.01	0.03	-0.02	0.04	0.01
H_0: In-kind only - In-kind & Education classes (p-value)	0.69	0.95	0.78	0.85	0.67	0.95

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

Table A6: The impact of PAL on learning - categorical classification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome =	Math Insuff.	Spanish Insuff.	3rd subject Insuff.	Math Excellent	Spanish Excellent	3rd subject Excellent	Levels Math	Levels Spanish	Levels 3rd subject
In-kind	0.011	0.012	0.006	-0.017	-0.003	-0.010	-0.052	-0.022	-0.027
	(0.025)	(0.026)	(0.017)	(0.028)	(0.030)	(0.023)	(0.063)	(0.063)	(0.052)
Cash	0.051*	0.054*	0.018	-0.072**	-0.050	-0.054**	-0.162**	-0.120*	-0.114*
	(0.031)	(0.032)	(0.020)	(0.032)	(0.034)	(0.025)	(0.073)	(0.071)	(0.058)
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Grade FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	11,006	11,006	11,006	11,006	11,006	11,006	11,006	11,006	8,737
***Effect size:* In-kind - Cash**	-0.04	-0.04	-0.01	0.06	0.05	0.04	0.11	0.10	0.09
H_0: In-kind = Cash (p-value)	0.14	0.11	0.48	0.03	0.06	0.03	0.06	0.06	0.08

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) Pre-program controls include gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.
(3) The 3rd subject was not administered in 2007 and it covered Science in 2008 and 2012, Ethics and Civics in 2009 and 2013, History in 2010, and Geography 2011.
(4) All dependent variables were created using categorical classification of the ENLACE for each subject. There are 4 categories: Insufficient, Sufficient, Good, Excellent. The dependent variables in columns (1)-(3) are indicator variables equal to 1 for test scores being insufficient, 0 otherwise. The dependent variables in columns (4)-(6) are indicator variables equal to 1 for test scores being excellent. The dependent variables in columns (7)-(9) takes the value between 0 and 3, with 0 being Insufficient and 3 being Excellent.

Table A7: The impact of PAL on nutrition - RDA outcomes

Outcome =	RDA energy (kcal)	RDA energy (kcal)	RDA protein	RDA protein	RDA vitamin C	RDA vitamin C	RDA iron	RDA iron	RDA zinc	RDA zinc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
In-kind	0.045	0.041	0.048	0.038	0.171***	0.153***	0.066**	0.068**	0.107***	0.110**
	(0.031)	(0.033)	(0.030)	(0.033)	(0.038)	(0.043)	(0.027)	(0.030)	(0.039)	(0.044)
Cash	-0.001	-0.001	0.050	0.049	0.084*	0.053	0.040	0.023	0.059	0.050
	(0.036)	(0.036)	(0.037)	(0.040)	(0.048)	(0.054)	(0.031)	(0.035)	(0.045)	(0.050)
Restrc. to those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Observations	2,419	1,880	2,419	1,880	2,419	1,880	2,419	1,880	2,419	1,880
Outcome mean in control group	0.22	0.21	0.78	0.79	0.46	0.46	0.76	0.76	0.54	0.54
***Effect size:* In-kind - Cash**	0.05	0.04	0.00	-0.01	0.09	0.10	0.03	0.04	0.05	0.06
H_0: In-kind = Cash (p-value)	0.12	0.16	0.94	0.71	0.05	0.03*	0.30	0.12	0.19	0.13

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) State fixed effects and the following pre-program controls are included: gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.
(3) Sample only includes individuals aged 6 or less in 2003.
(4) Outcome variables are indicator variables equal to 1 if value of the macro/micro nutrients exceeds the RDA.

Table A8: Balance of Main Variables at Baseline for Sample in *ENLACE de Contexto*

	(1)	(2)	(3)		(4)	(5)	(6)
	Control	In-kind	Cash	Obs.	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value
				Child level characteristics			
Male	0.51	0.50	0.41	461	0.91	0.24	0.17
	(0.503)	(0.501)	(0.495)				
Age at baseline	4.96	5.16	5.37	461	0.76	0.56	0.76
	(3.479)	(5.970)	(4.205)				
Caloric intake, kcal daily	1,055.00	827.16	890.91	196	0.09	0.23	0.56
	(615.177)	(507.731)	(397.841)				
Iron consumption, mg daily	5.67	5.58	6.82	199	0.90	0.42	0.36
	(4.748)	(3.692)	(6.158)				
Zinc consumption, mg daily	5.22	3.67	4.67	199	0.15	0.66	0.21
	(4.463)	(2.410)	(2.961)				
Vitamin C consumption, mg daily	51.86	25.66	55.16	199	0.03	0.87	0.11
	(63.703)	(37.474)	(81.977)				
Z score height for age	0.44	-0.37	0.22	203	0.04	0.59	0.11
	(1.553)	(1.346)	(1.493)				
				Household level characteristics			
Indigenous household	0.23	0.27	0.11	376	0.76	0.31	0.11
	(0.420)	(0.443)	(0.308)				
Number of household members	5.20	5.75	5.29	376	0.14	0.82	0.25
	(1.817)	(2.296)	(1.987)				
Married household head	0.88	0.93	0.91	376	0.10	0.38	0.31
	(0.333)	(0.250)	(0.292)				
Maximum years of education in HH	7.83	7.16	7.83	376	0.21	1.00	0.19
	(2.759)	(3.041)	(3.189)				
House has a dirt floor	0.28	0.32	0.24	376	0.66	0.80	0.37
	(0.449)	(0.469)	(0.432)				
House has plumbing	0.75	0.65	0.61	376	0.36	0.23	0.69
	(0.436)	(0.477)	(0.492)				
Total expenditure per capita in the household	434.44	344.28	401.61	376	0.10	0.55	0.20
	(248.866)	(231.506)	(236.781)				
				Village level characteristics			
Distance to closest primary school (km)	1.28	1.04	0.94	99	0.66	0.58	0.81
	(2.717)	(1.506)	(1.524)				
Closest school is a general school	0.81	0.78	0.75	99	0.71	0.63	0.83
	(0.389)	(0.416)	(0.444)				
Closest school is a community school	0.07	0.06	0.15	99	0.83	0.41	0.30
	(0.262)	(0.238)	(0.366)				
Closest school is a indigenous school	0.12	0.17	0.10	99	0.55	0.83	0.44
	(0.317)	(0.370)	(0.308)				
Student-teacher ratio in closest school	28.72	29.12	26.45	98	0.87	0.34	0.25
	(9.223)	(11.744)	(7.020)				
Repetition rate in closest school	0.10	0.10	0.11	99	0.88	0.65	0.68
	(0.086)	(0.067)	(0.155)				
Repetition rate in closest community or indigenous school	0.13	0.21	0.21	90	0.05	0.18	0.94
	(0.134)	(0.195)	(0.212)				
Morning shift closest school	0.74	0.86	0.93	99	0.17	0.05	0.38
	(0.417)	(0.317)	(0.245)				
Yearly expenditure per child in closest school (fees, uniform, books)	4,217.62	4,035.20	3,907.50	99	0.71	0.63	0.83
	(2,029.062)	(2,167.869)	(2,314.603)				

Notes: *** p<0.01, ** p<0.05, * p<0.1 (1) Standard errors in parentheses, clustered at the village level. (2) Data are from the pre-intervention PAL survey and the 2003 Formato 911 school databases. (3) Child consumption data was only collected for children aged 1 to 4 in the pre-program survey. (4) Math and Spanish grades are self-reported recalls of the student's most recent report card, sample includes students currently attending school. (5) A household is defined as indigenous if one or more members speak an indigenous language. (6) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos.

Table A9: The impact of PAL on proxies for the returns of child labor

Outcome =	Total number of animals	HH farmed or raised animals in past year	Number of HH members farming	Average number of hours farming
	(1)	(2)	(3)	(4)
In-kind	1.007	0.126***	0.108	3.162
	(1.253)	(0.044)	(0.070)	(3.428)
Cash	3.528**	0.196***	0.221**	7.965
	(1.546)	(0.050)	(0.091)	(5.211)
Observations	3,013	3,013	3,013	1,307
Outcome mean in control group	6.95	0.51	0.52	52.95
***Effect size:* In-kind - Cash**	-2.52	-0.07	-0.11	-4.80
H_0: In-kind = Cash (p-value)	0.04	0.09	0.15	0.33
H_0: In-kind = Cash (Randomization Inference p-value)	0.04*	0.12	0.14	0.33

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level. Sample restricted to those with ENLACE test scores.

(2) State fixed effects and the following pre-program controls included: gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.

(3) The outcome in column (1) is the total number of the number of small and large animals owned by the household; the outcome in column (2) is an indicator for whether any household member was involved either in farming or raising animals, the outcome in column (3) is the total number of household members who reported spending time farming; and the outcome in column (4) is the average number of hours spent farming among household members.