Policy Research Working Paper 9086

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

Ciro Avitabile
Jesse M. Cunha
Ricardo Meilman Cohn

WORLD BANK GROUP
Education Global Practice
December 2019

Policy Research Working Paper 0

Abstract

This paper studies the medium-term impact of early-life welfare transfers on children's learning. It studies children who were exposed to the randomized controlled trial of the Mexico's Food Support Program (the P*rograma de Apoyo Alimentario*, PAL), in which households were assigned to receive cash, in-kind food transfers, or nothing (a control). The children are matched with administrative data on primary school standardized tests, which were taken four to 10 years after the experiment began. The findings show that in-kind transfers did not impact test scores, while cash transfers led to a significant and meaningful decrease in test scores. An analysis of the mechanisms driving these results reveals that both transfers led to an increase in child labor, which is likely detrimental to learning. In-kind food transfers, however, induced a greater consumption of several key micronutrients that are vital for brain development, which likely attenuated the negative impacts of child labor on learning.

This paper is a product of the Education Global Practice. It is part of a larger effort by the World Bank to provide open access to its research and make a contribution to development policy discussions around the world. Policy Research Working Papers are also posted on the Web at http://www.worldbank.org/prwp. The authors may be contacted atcavitabile@worldbank.org.

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

Ciro Avitabile
University of Surrey and World Bank

Jesse M. Cunha
Naval Postgraduate School

Ricardo Meilman Cohn
Simon Fraser University

JEL Classification I12, I21, I38

Keywords: Welfare transfers, medium-term effects, learning outcomes

*The paper has been screened to ensure that no confidential information is revealed. We are indebted to Rafael de Hoyos for his contribution at an early stage of the project and the continuous feedback. We thank Tania Barham, Roberta Gatti, Federico Tagliati, seminar participants at ITAM, the World Bank DECRG for their comments. Authors' contact information: cavitabile@worldbank.org; jcunha@nps.edu; rmeilman@sfu.ca

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 lifetime 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 the 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, whether in-kind or in-cash, can impact children's learning in various ways; importantly, while most mechanisms suggest transfers will positively impact learning, there are several key ways in which transfers can harm learning. First, transfers can improve nutritional intake during critical ages for mental and physical development (directly via food transfers or indirectly via cash), thus providing 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 various 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, freeing up time for learning, or greater resources could lead to more child labor (De Hoop et al., 2017), for example, if the family invests in assets that are complementary to labor (Basu et al., 2010, Edmonds and Theoharides, 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 class can increase learning, 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 (Heckman and Cunha, 2007, Glewwe and Muralidharan, 2016) among all of these inputs to a child's learning; 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

[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 (Duflo et al., 2011a, De Giorgi and Pellizzari, 2014).

are substitutes for improved nutrition.

We investigate how some of 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-proverty transfer programs. To identify impacts, we leverage the randomized controlled trial that was implemented during the program's roll-out in 2003, in which 208 villages in southern Mexico were randomized into three groups: 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 household consumption pre-program. 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 from 3rd to 6th grade. 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), in-kind and cash PAL transfers both 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 which induced children receiving in-kind transfers to consume more micronutrients than children who received cash transfers. Many children were deficient in some key micronutrients that have been shown to support brain development (Black, 2003) - namely, vitamin C, iron, and zinc - 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. In addition, 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, in-kind food transfers had no impact on test scores while cash transfers negatively impacted test scores, relative to the control. Transfers, whether in-kind or in-cash, did not impact the likelihood that children took the tests, which rules out the possibility that these results are 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 among cash children are concentrated among those who were older than 2 when transfers began. We also find that indigenous students and those from especially poor familes - two of the most disadvantaged groups in Mexico - experienced larger negative impacts on test scores for both in-kind and cash transfers.

We next provide evidence on some of the potential mechanisms that are driving the results on test scores. As mentioned above, in-kind transfers led to greater consumption of key micronutrients in early childhood, which is vital for brain development and can positively impact children's ability to learn. When we look at other inputs in the learning production function, we find evidence of an increase in child labor, especially among students from cash localities. We also find that children in cash localities are less likely to go to general schools, preferring community or indigenous schools, that provide lower quality but are less expensive in terms of materials, uniforms and school contributions. We do not find any impact of the program in terms of parental involvement in school related activities. Overall, the results seem to suggest that PAL transfers induced greater child labor at the expense of learning, and that these negative consequences on learning outcomes were partly muted under in-kind food transfers.

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. 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). Third, we find results consistent with the empirical literature studying the impacts of unconditional transfers on child labor (Edmonds and Theoharides, 2019). Our finding that the impact of transfers on learning can be influenced by various inputs to the learning process highlights the complementarity between different types of human capital investment over the life cycle (Heckman and Cunha, 2007).

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 completed some type of tertiary 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*)*. 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 rural areas we study, community and indigenous schools are more prevalent reflecting their larger indigenous populations compared with urban centers.

[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).

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.

The ENLACE was designed as a low-stakes assessment that would not impact student grade progression, but it could be used to strengthen school accountability by informing parents and society at large about the levels and growth of students' knowledge. Originally, teachers had no stake in the results of the test; but in 2008, ENLACE scores became one of the key criteria to measure teacher performance in *Carrera Magisterial* (CM) program, 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).[5] The use of ENLACE 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 (Avitabile and de Hoyos, 2018, De Hoyos et al., 2018) has shown that ENLACE tests in primary education are correlated with later learning and labor market outcomes.

ENLACE was not offered in 2014, and 2015 it was replaced by a new test, PLANEA *(Plan Nacional para la Evaluación de los Aprendizajes)*. Unlike ENLACE, 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, especially 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

[5]From 2008 through 2010, the ENLACE scores of a teacher's students comprised 20 percent of the overall Carrera Magisterial score which was used to determine teacher bonuses, and this weight rose to 50 percent in 2011.

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.[6] 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; 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.[7] 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, however, few transfer recipients report attending classes and, importantly, administrators confirmed that the conditionality of transfers on class attendance was never enforced - that is, no household was denied transfers for not attending classes.[8] Fur-

[6]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.

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

[8]Based on the 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).

thermore, 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 communities. However, several of the in-kind food items were extra-marginal, as evidenced by greater increases in consumption of those goods in in-kind communities compared to cash communities. 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 communities were observed to consume more iron, zinc and vitamin C than children in cash communities.

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.

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.[9]

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 pre-intervention and follow-up survey ask both primary and secondary

[9]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.

activity in the week prior to the interview. Therefore we can identify whether children attend school and/or work. For those who report working, the survey asks the number of hours of work.

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 pretreatment 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.[10] 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.

For all children born between 1998 and 2004 - those who were 6 years old or younger at the time of the first PAL survey - we merge individual and household level information both from the baseline and the follow-up evaluation surveys with grades 3 through 6 ENLACE test score data between 2007 and 2013.[11] The ENLACE identifies students through the government-issued identifier, the *Clave Única de Registro Poblacional* (CURP), which is formed by an algorithm combining the first name, surname, date of birth, sex, state of birth, and two randomly generated digits. The PAL household surveys do not contain the CURP, but do contain all of its constituent demographics from which we generated a quasi-CURP (lacking the random digits). We thus 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 which elicits information about child labor, parental and student sociodemographic characteristics, student and parent expectations, and student perceptions about their peers, teachers and parental involvement.

The Ministry of Education conducts two school censuses per year, known as Formato 911, which are completed by an authorized school representative. The census identifies whether the school belongs to the *general*, *indigenous*, or *community* system and collects a large set of school characteristics, including the number of teachers, students, classrooms, and laboratories and whether a library is available. For general schools, it also provides information

[10]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.

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

on the cost that students have to sustain for uniforms, materials and school contributions.[12] This information is not reported for indigenous and community schools, since materials and uniforms are provided by the government either in the form of in-kind or cash grants to the schools. The average costs of the different items are provided by a school representative at least three years after the end of the PAL evaluation, thus ruling out the possibility of selective miss-reporting. We use information from the 2003 Formato 911 to assess whether treatment and control localities differed in terms of school supply prior to the rollout of PAL. For each student merged with the ENLACE test score, we have the unique school identifier (*clave de centro de trabajo* CCT) associated with each student test. Therefore, using information from the Formato 911 2007-2013, we can test whether the program affected the characteristics of the school where students attended primary education. In particular, we can potentially track the results of students whose families moved within Mexico after the follow up survey. In order to disentangle whether potential changes in distance are driven by increased willingness to spend on commuting or changes in residential location, we test the effect of PAL on 4 discrete measures: 1) attending a school within 5km from the community, 2) attending a school between 5 and 10km, 3) attending a school between 10 and 30km, 4) attending a school more than 30km from the community. Changes in the last outcome are possibly driven by changes in the household residential location.

3.2 ENLACE take-up

In the follow-up survey of the experimental evaluation 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. Thus, our final sample 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 potential reasons why we do not observe a child from the PAL survey taking the ENLACE test. First, children's school attendance and writing of the ENLACE test could be differentially impacted by the PAL transfers; however, as we show in the results below, there is fact no difference in these outcomes across treatment groups. Second, the child's family could have migrated abroad before the child reached the end of the third grade.[13] Migration information is not available for the PAL sample, but surveys of participants in

[12] According to the 2008 General Law of Education, parents are supposed to contribute on a voluntary basis according to their economic possibilities. However, school principals often require the payment and parents perceive the contributions as compulsory. In order to reduce this phenomenon, in 2018 the Senate asked the Ministry of Education to officially forbid the school principals to claim school contributions.

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

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 US 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 apply this migration rate in each of the 10 years between 2004 and 2013, we would expect as a rough approximation to not observe ENLACE tests for around 6.8 percent of children in our initial sample because they migrated abroad.

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 quantify 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).[14]

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 1.4 million 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. Details about the merging algorithm and the possible reasons for attrition are provided in the Data Appendix. Only for 2.8 percent of the individuals in our sample of interest we could not complete the merge due issues related to the algorithm, and the share does not differ across treatment and control groups.

[14] 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.

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 (they do not have baseline data), and the 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 cost of the closest school is 4,200 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 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* over time 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, that is very small in size,[15] 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 presented in this section 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] 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.

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:

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

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, which demonstrate that there is no differential impact across transfer modality on the likelihood of a child taking the ENLACE test. Columns 1-3 show the impact on a student ever taking an ENLACE test (one observation per child), while columns 4-6 use a balanced panel of students in every year 2007 through 2013 (seven observations per child). The slight difference in take-up rates across groups seen in column 1 disappears once state fixed effects are included (column 2), a result which likely reflects

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

the differential impact of teachers unions strikes across states described above (and that the randomization of PAL villages was not stratified by state). Including covariates, the results in columns 3 and 6 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. We also 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). These results suggest 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 simple mean differences between treatment and control localities with no controls. 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]

[17] Appendix Tables A3 and A4 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 A5).

Columns 2, 4, and 6 add pre-program child and household characteristics and state, 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).

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 0.137 to 0.288 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 for neither the cash nor the in-kind treatment is there significant evidence of differential treatment across the outcomes' 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 componet 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.[18]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., 2011b), 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

[18]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.

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 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

We use information from the evaluation follow-up survey in order to study whether PAL, either in the in-kind or cash modality, had an effect on proxies for health status and parental health investment of children aged 0 to 6 at the baseline. When looking at the health stock 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.[19]

The impacts of PAL on these variables are reported in Table 8. For each outcome, we present results for two specifications. In the first one, we present our baseline specification for the whole sample (column 1, 3, 5, 7), in the second one we restrict the sample to those children for which we observe at least one ENLACE test score during the time period between 2007 and 2013 (columns 2, 4, 6, 8). For the probability of being sick, height per age and weight per age, we find effects that are close to zero, both for the in kind and the cash modality, irrespective of whether we consider the full sample or the sample of children for which we observe at least one standardized test scores. We do find that both children in the in kind and the cash group were about 3 percentage point less likely to be anemic than those in the control group. The relative size is sizable when compared to the prevalence of anemia in the control group (19 percent), but the effect is not statically different from zero.

In Table 9 we look at the effect of PAL on the calorie consumption, the intake of one macronutrient (protein) and five micronutrients (vitamin C, iron, zinc, calcium, and retinol). For each outcome we present the two specifications discussed above. Only in-kind children display an increase in the calorie consumption, but the effect is not statistically different from zero. The program had no effect on the intake of protein. We do find a large and statistically

[19]Results not reported for space reasons show for the control group that the anemia status is a strong predictor of the performance test scores. In particular, children who were found with anemia in the follow-up survey scored on average 0.2 standard deviations lower than non-anemic children both in math and Spanish.

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. In order to provide an overall estimate of the program effect on nutritional intakes, we study the impact on the first component of the Principal Component Analysis (PCA) of the six nutrients. The in-kind modality shows a larger impact than the cash one with the difference between the two treatment types being non-trivial in terms of size (about 0.10 s.d.) and marginally significant (p-value = 0.09). The results for the overall sample and the sample of those with at least one ENLACE are remarkably similar and this is consistent with the fact that the program had no statistically significant impact on the probability of taking the exam. In order to rule out the possibility that the results are driven either by children with extremely low intakes or those with very high intakes, we study the treatment effects on the probability of having intakes equal or above the RDAs. Results presented in Appendix Table A6 are in line with those discussed above.

Overall, the evidence provided in this section suggests that children, who had been exposed to the PAL program for at least 18 months, did not exhibit better health status compared to those who were not exposed. However, they do display better nutritional intakes, especially if they receive the transfer in-kind.

5.2 Student effort

We use two distinct data sources to study the impact of PAL on child labor. First, we use information from the evaluation follow-up survey in order to study the attendance and labor outcomes of children age 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.[20] We focus on five different outcomes: 1) whether the child only reports having attended school the week prior the interview; 2) whether she was both working and attending school; 3) whether she was only working; 4) whether she was neither working nor attending; 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 aged 12 to 13 in primary education,

[20]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, over-age is quite common and we include 13 year old in order to improve statistical power. Results restricted to 12 year old are qualitatively similar.

are reported attending school as the only activity in the week prior the follow-up interview, 5 percent are reported only working, 1 percent combining both school attendance and work, 10 percent neither working nor attending. 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. When we look at the number of hours of work, we do observe an increase by 1.6 hours for children in the cash group, as opposed to a negative but very small effect in the in-kind group. The difference between the groups is large in terms of size - equivalent to the average number of hours in the control group, and statistically significant (p-value=0.07).

We use data from the *Enlace de contexto* to study the labor response for a subgroup of students for which we observe the test scores. While the information from the *Enlace de contexto* is only representative for the selected sample of exam takers, it provides direct evidence on the behavior of children who attend school.[21]All surveys between 2008 and 2013 ask students how many days on average they are involved in labor activities, while surveys from 2011 to 2013 also ask how much time students dedicated to household chores in the week prior to the interview. Although in principle student reported information might not accurately reflect actual labor activities, we do find a strong and statically significant negative association between results in the standardized test and number of working days reported in the *Enlace de contexto* (Appendix Figure A1). The impacts of PAL are reported in columns 6 and 7 in Table 10. We do observe increases in the number of working days among students from both in-kind and cash localities, with the effect being particularly large for the latter although the differential treatment is not statically significant at conventional levels. Similarly we do observe a large increase in the share of children who report helping with household work but the effects are not statistically significant, arguably for 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 result in increases in child labor. Information from the baseline household survey shows that the probability that a child age 12-13 exclusively attends school declines with the total number of animals owned by her family while

[21]We test whether characteristics are balanced across the three groups for the students we find in the *ENLACE de Contexto*. Table A7 reports pre-intervention sample means by treatment group. Overall, characteristics of students observed in *ENLACE de Contexto* are balanced with very few exceptions.

the probability of working, either exclusively or in combination with school attendance, increases (top panel in Figure 6). The average number of hours that a child works steadily increases with the number of animals (bottom panel in Figure 6). Consistent with previous results for *Progresa/Oportunidades* (Gertler et al., 2012), using evidence follow-up survey we find that among households with at least one child aged 8-13, PAL increased the number of animals owned, with an effect that is statistically larger for cash than in-kind ones (column 1 in Appendix Table A8). Households from both treatment groups are more likely to report being involved in agricultural activities after the program inception (column 2 in Table A8), with the effect being statistically larger among those from cash localities than in-kind ones. 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 A8). 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 increased child labor, with a stronger impact for cash transfers. Taken all together, the available evidence is consistent with the hypothesis that at least in households with primary school age children, 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 are highly suggestive that the PAL transfer, especially when distributed in cash, led to an increase in the return to labor of young children and a higher probability of combining work and school attendance. The fact that children in in-kind localities, despite the improved nutritional intakes, do not reduce their labor supply can potentially help explain why they did not experience any improvement in their test scores.

5.3 School quality

As discussed above, children in our sample have access to three types of school service, that are very different in terms of quality and costs. Using the information from the Formato 911 for the school associated to the individual score, we test whether the two transfer modalities had an impact on the probability of attending a general school (vis a vis an indigenous or a community one), and characteristics that make up for most of the direct costs of school attendance: school distance from the community of residence, the sum of school contributions, material and uniform costs, and the student teacher ratio. Finally, 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 point 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 decreased by 8.3 percentage point, 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). Both for the in-kind and the cash group, we observe a reduction in the probability of attending schools less than 5km from the community, and between 5 and 10km (columns 2 and 3 respectively). This reduction is compensated by an increase in the probability of attending schools between 10 and 30km, and those more than 30km (column 4 and 5). However, for none of the distance outcomes the difference between in-kind and cash localities is statistically significant. Results in column 5 rule out the possibility that the differential effect of in-kind and cash treatment on learning outcomes is driven by a differential effect of the transfer modalities on household internal migration.[22] Students from the in-kind localities attend schools that on average are 98 pesos more expensive than the ones attended by students from control localities, while those from cash localities attend schools that on average are 505 pesos cheaper (column 6), but the the difference between the two treatment types is not statistically significant at conventional levels. In column 7 we test whether the schools attended by students in cash and in-kind attend schools that differ in terms of student teacher ratio. We find a positive and not statistically significant effect on student teacher ratio both for in-kind and cash communities. This result lends little support to the hypothesis that the negative effect on the learning outcome of cash students is driven by a differential congestion effect of the transfer modalities.

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. We next argue that there are different, although complementary, explanations behind this result, that we are not able to test empirically. First, because of the increased return to child labor, the expected return from attending a better school might decrease. Second, child labor might be more socially accepted in community and indigenous schools than in general schools, an explanation that is potentially consistent with the fact that the average number of working days reported in the *Enlace de contexto* is higher in the former two than in the last (3.07 vs 1.76). Finally, as a result of the income shock, school principals in general schools might ask for higher school contributions in treatment than in control localities, pushing students

[22]McKenzie and Rapoport (2011) find for Mexico that migration can have negative impacts on the education attainments of children from rural areas.

towards alternative modalities. In in-kind localities, these three effects might be counteracted by the dynamic complementarity between improved micronutrient intakes in early years and later school quality. Results for the control group, presented in Table A9, show that the proxy for the quality of nutrition intakes prior school entry is positively and significantly correlated with the probability of attending a general school, even after conditioning on proxies for health stock such as height for age and anemia. In particular, one standard deviation increase in the principal component that summarizes the micronutrient intakes increases the probability of attending a general school by 2.5 percentage points.

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. Everything else being constant, 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 parents are involved in five activities that are potentially relevant for their performance: helping with homework; explaining topics that were not clear from the lecture; inviting them to review class material that was not clear; paying attention to student grades; and attending school meetings. For each of these activities, students can choose five options: never, rarely, sometimes, almost always, always. We create dummy variables that take the value one if students select almost always or always, 0 otherwise. We then create an index that sums the five dummies, and the first component from PCA of the five dummies. Table 12 reports that the treatment effects on both summary indexes are not statistically significant from zero for the households in the cash group, and marginally significant for those in the in-kind group. Nevertheless, the difference between the two treatment groups is small and not statistically significant.

Overall, the results presented in this section suggest that the differential effect of the in-kind and cash transfer 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, while cash transfers significantly reduced student learning.

There are many possible reasons why the improved micronutrients intake might have not led to long lasting impacts on test scores. First, for the children in our sample the improvement in micronutrients was not accompanied by a significant increase in the overall caloric intake. Due to the high share of children who were consuming less calories than the recommended daily allowance at the baseline, one possible explanation is that the quality of the intakes might not be sufficient to improve the cognitive function if the overall calorie consumption does not achieve a minimum threshold, and children do not improve their anthropometric measures. 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 potentially consistent with the hypothesis that nutrition interventions only improve the cognitive development of children who have adequate stimulation. This might not be the case for children in the PAL evaluation sample, where the quality of stimulation both at home and in school is likely to be poor. Moreover, our results show that parents did not increase the time they devote to their children, particularly in the cash group.

The explanations discussed above cannot fully explain the whole set of results in our study. In fact while in localities that received the transfer in-kind we observe an improvement in

micronutrients intake and no effect on test scores, children from cash localities displayed smaller improvements in intakes, compared to those from in-kind localities, and a non-negligible decline in test scores. We investigate the role of various determinants of student learning. On the one hand, the negative effects of the cash modality on learning are more prominent among households with older children and those that are more likely to be credit constrained. On the other hand, especially among children from cash localities, we do observe that the program led to an increase in child labor and to an increased probability of attending a lower quality school option. Both pieces of evidence are consistent with the hypothesis that, by increasing ability to buy productive assets, PAL increased the return to child labor and reduced the time and energy dedicated to learning. The detrimental effect of the unconditional transfer on learning was partly muted in in-kind localities possibly because the improvement in early nutrition increased parents' incentives to invest in their children. In our context, parents might be aware of the importance of attending school but not of learning outcomes. Even if they are aware of their importance, they might have imperfect information about the learning production function.

Overall, our results provide compelling evidence that an improvement in the quality of nutrition intakes in the first years of life, in the absence of improvements in the health stock, is not a sufficient condition for improved learning. When improvements in intakes are driven by policies - such as welfare transfers - that can trigger a variety of behavioral responses, their impacts on learning can potentially take any direction.

References

Agostinelli, F. and G. Sorrenti (2018, January). Money vs. time: family income, maternal labor supply, and child development. ECON - Working Papers 273, Department of Economics - University of Zurich.

Akresh, R., D. De Walque, and H. Kazianga (2013). *Cash transfers and child schooling: evidence from a randomized evaluation of the role of conditionality.* The World Bank.

Almond, D., B. Mazumder, and R. van Ewijk (2011, December). Fasting During Pregnancy and Children's Academic Performance. NBER Working Papers 17713, National Bureau of Economic Research, Inc.

Angelucci, M. (2015, March). Migration and Financial Constraints: Evidence from Mexico. *The Review of Economics and Statistics 97*(1), 224–228.

Angrist, J. D. and V. Lavy (1999). Using maimonides' rule to estimate the effect of class size on scholastic achievement. *The Quarterly journal of economics 114*(2), 533–575.

Araujo, M. C., M. Bosch, and N. Schady (2016, September). Can Cash Transfers Help Households Escape an Inter-Generational Poverty Trap? NBER Working Papers 22670, National Bureau of Economic Research, Inc.

Attanasio, O., H. Baker-Henningham, R. Bernal, C. Meghir, D. Pineda, and M. Rubio-Codina (2018, September). Early Stimulation and Nutrition: The Impacts of a Scalable Intervention. NBER Working Papers 25059, National Bureau of Economic Research, Inc.

Attanasio, O., S. Cattan, E. Fitzsimons, C. Meghir, and M. R. Codina (2018, July). Estimating the production function for human capital: results from a randomized controlled trial in Colombia. IFS Working Papers W18/18, Institute for Fiscal Studies.

Avitabile, C. and R. de Hoyos (2018). The heterogeneous effect of information on student performance: Evidence from a randomized control trial in Mexico. *Journal of Development Economics 135*(C), 318–348.

Baez, J. E. and A. Camacho (2011, May). Assessing the long-term effects of conditional cash transfers on human capital: Evidence from colombia. IZA Discussion Papers 5751, Institute for the Study of Labor (IZA).

Baird, S., C. McIntosh, and B. Özler (2011). Cash or Condition? Evidence from a Cash Transfer Experiment. *The Quarterly Journal of Economics 126*(4), 1709–1753.

Baird, S. J., C. Mcintosh, and B. Özler (2016, December). When the money runs out : do cash transfers have sustained effects on human capital accumulation ? Policy Research Working Paper Series 7901, The World Bank.

Barham, T., K. Macours, and J. Maluccio (2019). Experimental Evidence from a Conditional Cash Transfer Program: Schooling, Learning, Fertility, and Labor Market Outcomes After 10 Years. Technical report, mimeo.

Barrera-Osorio, F., M. Bertrand, L. L. Linden, and F. Perez-Calle (2011, April). Improving the Design of Conditional Transfer Programs: Evidence from a Randomized Education Experiment in Colombia. *American Economic Journal: Applied Economics 3*(2), 167–195.

Bastagli, F., J. Hagen-Zanker, L. Harman, V. Barca, G. Sturge, and T. Schmidt (2019). The impact of cash transfers: A review of the evidence from low- and middle-income countries. *Journal of Social Policy 48*(3), 569–594.

Basu, K., S. Das, and B. Dutta (2010). Child labor and household wealth: Theory and empirical evidence of an inverted-u. *Journal of Development Economics 91*(1), 8–14.

Becker, G. S. and N. Tomes (1976). Child endowments and the quantity and quality of children. *Journal of Political Economy 84*(4, Part 2), S143–S162.

Beegle, K., R. Dehejia, and R. Gatti (2009). Why Should We Care About Child Labor?: The Education, Labor Market, and Health Consequences of Child Labor. *Journal of Human Resources 44*(4).

Bharadwaj, P., K. V. Loken, and C. Neilson (2012). Early life health interventions and academic achievement. *IZA Discussion Paper*.

Black, M. M. (2003). Micronutrient deficiencies and cognitive functioning. *The Journal of nutrition 133*(11), 3927S–3931S.

Campillo Garcia, J. (1998). Official mexican standard (nom-169-ssa1-1998) for social food assistance to at-risk groups. *Government of Mexico manuscript, Available at http://bibliotecas. salud. gob. mx/gsdl/collect/nomssa/index/assoc/HASH015c.dir/doc.pdf*.

Chetty, R., J. N. Friedman, and J. E. Rockoff (2014, September). Measuring the impacts of teachers ii: Teacher value-added and student outcomes in adulthood. *American Economic Review 104*(9), 2633–79.

Chong, A., I. Cohen, E. Field, E. Nakasone, and M. Torero (2016, October). Iron deficiency and schooling attainment in peru. *American Economic Journal: Applied Economics 8*(4), 222–55.

Cunha, J. M. (2014). Testing paternalism: Cash versus in-kind transfers. *American Economic Journal: Applied Economics 6*(2), 195–230.

Dahl, G. B. and L. Lochner (2012, May). The impact of family income on child achievement: Evidence from the earned income tax credit. *American Economic Review 102*(5), 1927–56.

Dammert, A. C., J. de Hoop, E. Mvukiyehe, and F. C. Rosati (2018). Effects of public policy on child labor: Current knowledge, gaps, and implications for program design. *World Development 110*(C), 104–123.

De Giorgi, G. and M. Pellizzari (2014). Understanding social interactions: Evidence from the classroom. *The Economic Journal 124*(579), 917–953.

de Hoop, J. and F. C. Rosati (2014). Cash Transfers and Child Labor. *World Bank Research Observer 29*(2), 202–234.

De Hoop, J. J., J. Friedman, E. Kandpal, and F. C. Rosati (2017, September). Child schooling and child work in the presence of a partial education subsidy. Policy Research Working Paper Series 8182, The World Bank.

De Hoyos, R. E., R. Estrada, and M. J. Vargas (2018, May). Predicting individual wellbeing through test scores : evidence from a national assessment in Mexico. Policy Research Working Paper Series 8459, The World Bank.

Duflo, E., P. Dupas, and M. Kremer (2011a, August). Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in kenya. *American Economic Review 101*(5), 1739–74.

Duflo, E., P. Dupas, and M. Kremer (2011b). Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in kenya. *American Economic Review 101*(5), 1739–74.

Edmonds, E. V. and C. B. Theoharides (2019, August). The short term impact of a productive asset transfer in families with child labor: Experimental evidence from the philippines. Working Paper 26190, National Bureau of Economic Research.

Feyrer, J., D. Politi, and D. N. Weil (2017). The Cognitive Effects of Micronutrient Deficiency: Evidence from Salt Iodization in the United States. *Journal of the European Economic Association 15*(2), 355–387.

Figlio, D., J. Guryan, K. Karbownik, and J. Roth (2014, December). The Effects of Poor Neonatal Health on Children's Cognitive Development. *American Economic Review 104*(12), 3921–3955.

Fiszbein, A. and N. Schady (2009). *Conditional Cash Transfers: Reducing Present and Future Poverty*. The International Bank for Reconstruction and Development/The World Bank.

Gertler, P. J., S. W. Martinez, and M. Rubio-Codina (2012, January). Investing cash transfers to raise long-term living standards. *American Economic Journal: Applied Economics 4*(1), 164–92.

Glewwe, P. and E. A. Miguel (2007). The impact of child health and nutrition on education in less developed countries. *Handbook of development economics 4*, 3561–3606.

Glewwe, P. and K. Muralidharan (2016, June). *Improving Education Outcomes in Developing Countries*, Volume 5 of *Handbook of the Economics of Education*, Chapter 0, pp. 653–743. Elsevier.

Heady, C. (2003, February). The Effect of Child Labor on Learning Achievement. *World Development 31*(2), 385–398.

Heckman, J. and F. Cunha (2007, May). The Technology of Skill Formation. *American Economic Review 97*(2), 31–47.

INEE (2014). Panorama educativo de méxico. indicadores del sistema educativo nacional 2013 educación b'asica y media superior. Technical report.

Maluccio, J. A., J. Hoddinott, J. R. Behrman, R. Martorell, A. R. Quisumbing, and A. D. Stein (2009, April). The Impact of Improving Nutrition During Early Childhood on Education among Guatemalan Adults. *Economic Journal 119*(537), 734–763.

McKenzie, D. and H. Rapoport (2011, October). Can migration reduce educational attainment? Evidence from Mexico. *Journal of Population Economics 24*(4), 1331–1358.

Millán, T. M., K. Macours, J. A. Maluccio, and L. Tejerina (2020). Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. *Journal of Development Economics 143*, 102385.

Pollitt, E., K. Gorman, P. Engle, J. Rivera, and R. Martorell (1995, 05). Nutrition in early life and fulfilment of intellectual potential. *The Journal of nutrition 125*, 1111S–1118S.

Powell, C. A., H. Baker-Henningham, S. M. Grantham-McGregor, S. P. Walker, T. J. Cole, and J. M. M. Gardner (2005, 08). Zinc supplementation and psychosocial stimulation: effects on the development of undernourished Jamaican children. *The American Journal of Clinical Nutrition 82*(2), 399–405.

Prado, E. L. and K. G. Dewey (2014). Nutrition and brain development in early life. *Nutrition Reviews 72*(4), 267–284.

Ravallion, M. and Q. Wodon (2000, March). Does Child Labour Displace Schooling? Evidence on Behavioural Responses to an Enrollment Subsidy. *Economic Journal 110*(462), 158–175.

Rockoff, J. E. (2004, May). The impact of individual teachers on student achievement: Evidence from panel data. *American Economic Review 94*(2), 247–252.

Rodriguez Herrero, H. (2005). Qualitative evaluation of the programa de apoyo alimentario. final report. *CIESAS, Available at www.diconsa.gob.mx*.

Roland G. Fryer, J. (2017, May). Management and Student Achievement: Evidence from a Randomized Field Experiment. NBER Working Papers 23437, National Bureau of Economic Research, Inc.

Santibañez, L., J. F. Martinez, A. Datar, P. J. McEwan, C. M. Setodji, and R. Basurto-Davila (2007). Breaking ground: Analysis of the assessment system and impact of mexico's teacher incentive program" carrera magisterial." technical report. *RAND Corporation*.

Skoufias, E., M. Unar, and T. G. de Cossio (2013, December). The poverty impacts of cash and in-kind transfers: experimental evidence from rural Mexico. *Journal of Development Effectiveness 5*(4), 401–429.

Tagliati, F. (2019, October). Child labor under cash and in-kind transfers: evidence from rural Mexico. Working Papers 1935, Banco de España;Working Papers Homepage.

Vazquez Mota, J. (2004). Rules of operation for the programa de apoyo alimentario. *Government of Mexico manuscript, Available at www.diconsa.gob.mx*.

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

	(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	0.51	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 expenditure per child in closest school (fees, uniform, books)	4,196.94	4,552.17	4,177.83	200	0.30	0.96	0.26
	(295.41)	(173.22)	(283.11)				

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 3: The impact of PAL on taking ENLACE tests

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome =	Took at least one ENLACE	Took at least one ENLACE	Took at least one ENLACE	Took ENLACE	Took ENLACE	Took ENLACE
In-kind	0.066*	0.009	0.002	0.033	0.000	-0.002
	(0.039)	(0.019)	(0.017)	(0.021)	(0.011)	(0.009)
Cash	0.044	-0.001	-0.014	0.027	-0.003	-0.009
	(0.047)	(0.027)	(0.025)	(0.027)	(0.016)	(0.014)
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)			-0.014			-0.004
			(0.012)			(0.005)
Age of household head			-0.001***			-0.001***
			(0.001)			(0.000)
Years of education of household head			0.011***			0.006***
			(0.002)			(0.001)
Household head is married			0.071***			0.022**
			(0.025)			(0.011)
Running water in the home			0.004			0.009
			(0.017)			(0.008)
State fixed effects	NO	YES	YES	NO	YES	YES
Year fixed effects	NO	NO	NO	NO	YES	YES
Outcome mean in control group	0.65	0.65	0.65	0.27	0.27	0.27
Observations	5,444	5,444	3,817	38,108	38,108	26,719
***Effect size:* In-kind - Cash**	0.02	0.01	0.02	0.01	0.00	0.01
H_0: In-kind = Cash (p-value)	0.50	0.69	0.51	0.75	0.83	0.64

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 =	(1) Math (z-score)	(2) Spanish (z-score)	(3) 3rd subject (z-score)
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 =	(1) Math (z-score)	(2) Math (z-score)	(3) Spanish (z-score)	(4) Spanish (z-score)	(5) 3rd subject (z-score)	(6) 3rd subject (z-score)
In-kind	-0.044	-0.049	-0.016	-0.026	-0.022	-0.029
	(0.080)	(0.073)	(0.088)	(0.081)	(0.078)	(0.071)
Cash	-0.193**	-0.182**	-0.144	-0.156*	-0.173**	-0.156*
	(0.086)	(0.086)	(0.095)	(0.093)	(0.082)	(0.080)
State FE	NO	YES	NO	YES	NO	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**	0.15	0.13	0.13	0.13	0.15	0.13
H_0: In-kind = Cash (p-value)	0.05	0.06	0.10	0.07	0.03	0.05
H_0: In-kind = Cash (Randomization Inference p-value)	0.06	0.09	0.14	0.10	0.05	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 =	(1) Math (z-score)	(2) Spanish (z-score)	(3) subject (z-score)
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)
State, Grade, & Year FE	YES	YES	YES
Pre-program controls	YES	YES	YES
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) 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) "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

	(1)	(2)	(3)
Outcome =	Math (z-score)	Spanish (z-score)	subject (z-score)
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)
State, Grade, & Year FE	YES	YES	YES
Pre-program controls	YES	YES	YES
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) 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 8: The impact of PAL on health outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome =	Person was ever sick in last 4 weeks		Z score height for age		Z score weight for age		Anemia	
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)
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	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) 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 younger in 2003.

Table 9: The impact of PAL on nutrition

Outcome =	(1) Energy (kcal)	(2) Energy (kcal)	(3) Proteins	(4) Proteins	(5) Vitamin C	(6) Vitamin C	(7) Iron	(8) 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	Zinc	Calcium	Calcium	Retinol	Retinol	Principal component macro/micro nutrients	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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome =	Works & attends school	Only works	Only attends	Neither works nor attends	Hours of work (including 0)	Average number of working days per week	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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome =	General school	Distance between 0 and 5km	Distance between 5 and 10km	Distance between 10 and 30km	Distance more than 30km	Total cost	Student-teacher ratio
In-kind	0.022	-0.032	-0.055	0.029	0.057	98.704	0.381
	(0.061)	(0.056)	(0.038)	(0.020)	(0.038)	(433.096)	(1.485)
Cash	-0.083	-0.067	-0.026	0.052	0.041	-505.683	2.155
	(0.075)	(0.071)	(0.041)	(0.038)	(0.047)	(537.149)	(2.286)
State FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES	YES	YES	YES
Observations	10,852	10,852	10,852	10,852	10,852	10,740	10,344
Outcome mean in control group	0.77	0.81	0.08	0.02	0.08	5674	28.71
***Effect size:* In-kind - Cash**	0.10	0.04	-0.03	-0.02	0.02	604.4	-1.77
H_0: In-kind = Cash (p-value)	0.07	0.61	0.26	0.60	0.76	0.14	0.44
H_0: In-kind = Cash (Randomization Inference p-value)	0.07	0.60	0.35	0.56	0.77	0.14	0.38

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 the whether the closest school offers morning shift.
(3) Outcome variable in column (1) is an indicator variable that equals to 1 if student attends general SEP school. Outcome variables in column (2)-(5) are indicator variables that equal 1 if driving distance from village to school is within the specified range. Outcome variable in column (6) is the total cost that includes contributions to the school, materials and uniforms. Outcome variable in column (7) is the average number of students per teacher in the school.

Table 12: The impact of PAL on parental investment

Outcome =	(1) Index of parental activities [0-5]	(2) Principal Component
In-kind	0.459*	0.309*
	(0.266)	(0.183)
Cash	0.343	0.238
	(0.319)	(0.219)
State FE	YES	YES
Year FE	YES	YES
Grade FE	YES	YES
Pre-program controls	YES	YES
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

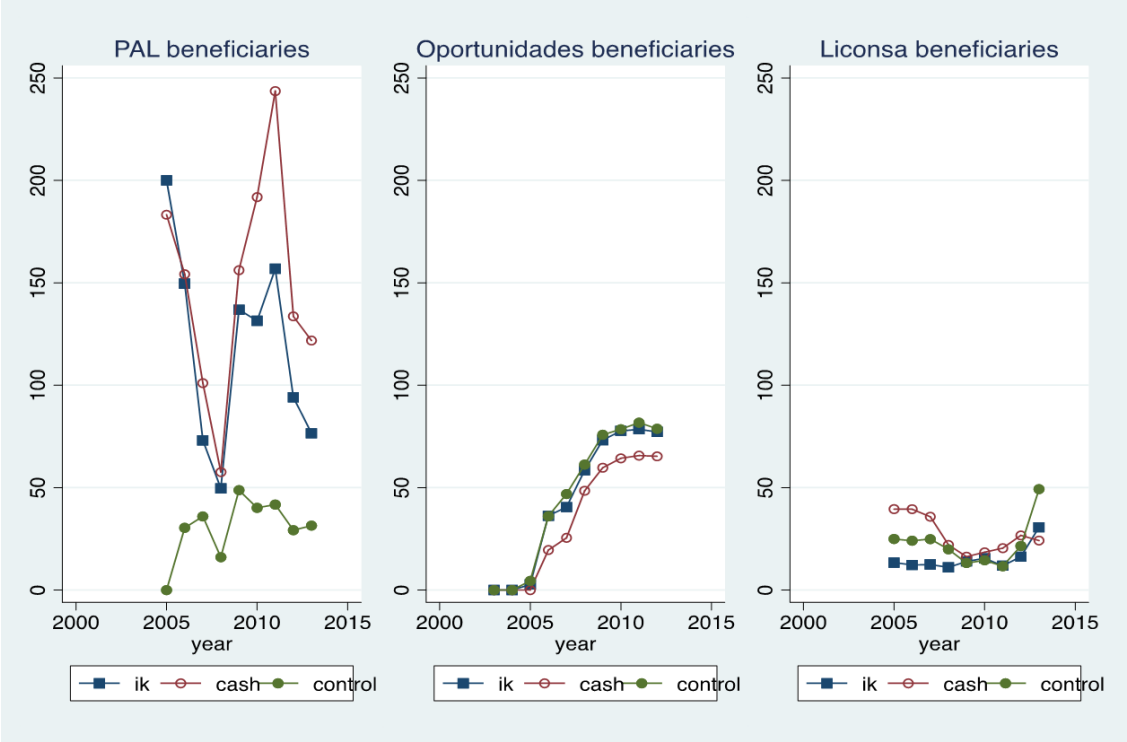
(1) Standard errors are clustered at the village level.

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

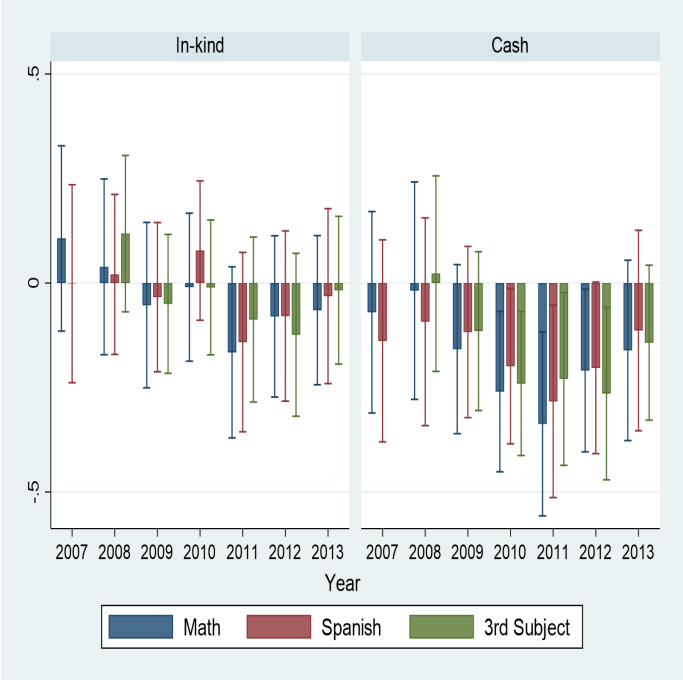
(4) The *Index of parental activities* equals the number of activities where parents are involved among the following: 1) helping with homework; 2) explaining topics that were not clear from the lecture; 3) inviting them to review class material that was not clear; 4) paying attention to student grades;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

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



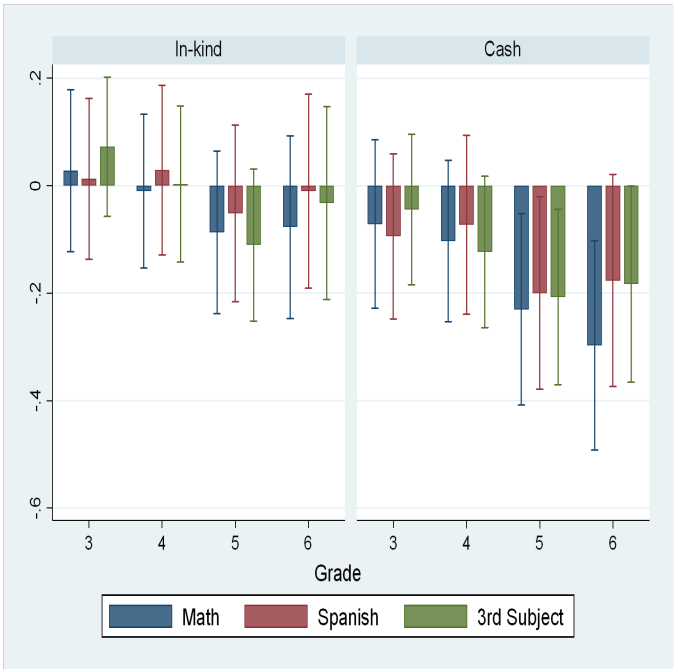
Notes: Year/group specific averages were obtained by averaging village level number of beneficiary households provided by SEDESOL.

Figure 2: Impact of PAL on learning by year



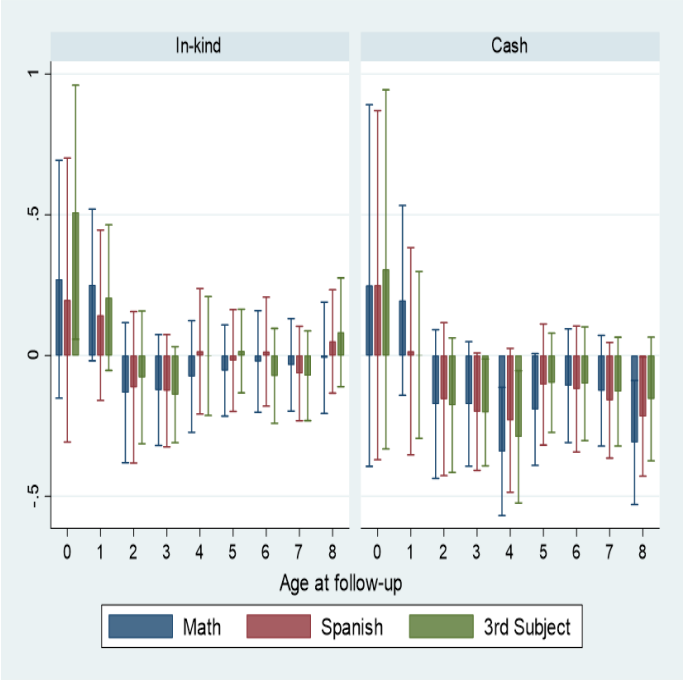
Notes: (1) Coefficients are from models that include pre-program controls, and state, year, and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 3: Heterogeneous impact of PAL on learning by grade



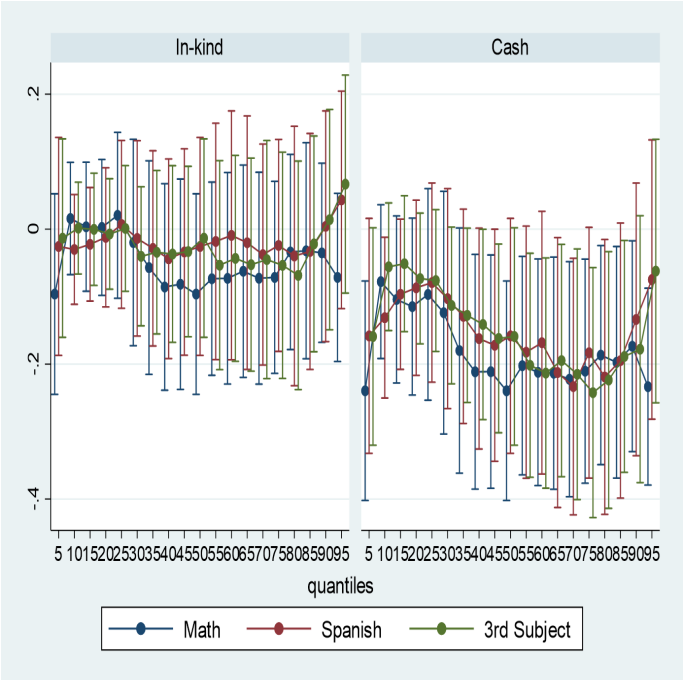
Notes: (1) Coefficients are from models that include pre-program controls, and state, year, and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

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



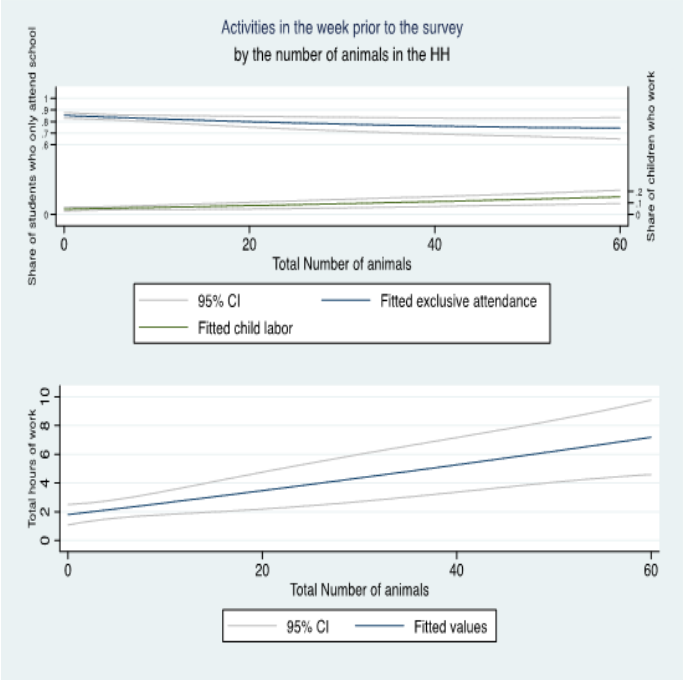
Notes: (1) Coefficients are from models that include pre-program controls, and state, year, and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

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



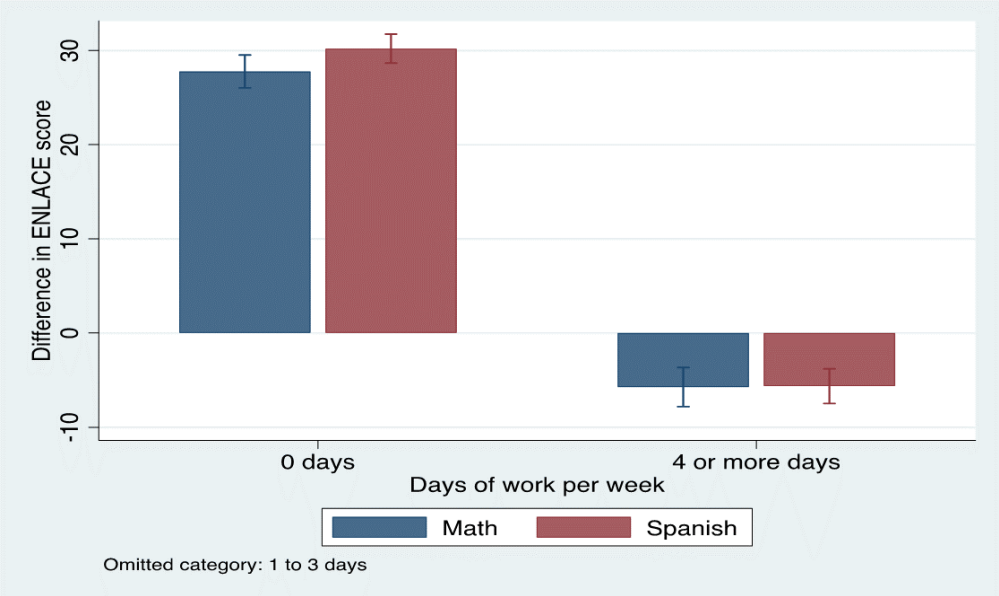
Source: Baseline household survey. The sample is restricted to children age 12 and 13 who report being enrolled in primary school. The top panel plots the share of students who report attending school as the only activity and the share of those working (either exclusively or in combination with school attendance) vis a 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

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.

Figure A1: Test scores and child labor



Source: 2010 ENLACE scores for all nationwide students in grades 3rd to 6th with ENLACE de Contexto. The graph plots the coefficients of an OLS regression that controls for student gender, age dummies, the dummy for speaking indigenous language, dummies for the number of household members and the number of assets at home, and grade fixed effects

Table A1: Post-experiment

	(1)	(2)	(3)	(4)	(5)	(6)
		Year 2005			Year 2010	
Outcome=	Number of Household	HHs with Oportunidades	Share of Oportunidades recipients	Number of Household	HHs with Oportunidades	Share of Oportunidades recipients
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)
State FE	YES	YES	YES	YES	YES	YES
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) 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

	(1)	(2)
Outcome =	Appropriate age for grade	At least one appropriate age for grade
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: The impact of PAL on test taking - 3 treatment arms

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome =	Took at least one ENLACE	Took at least one ENLACE	Took at least one ENLACE	Took ENLACE	Took ENLACE	Took ENLACE
In-kind only	0.067	0.023	0.019	0.030	0.005	0.004
	(0.041)	(0.021)	(0.020)	(0.023)	(0.012)	(0.011)
In-kind & educ	0.065	-0.006	-0.017	0.036	-0.004	-0.009
	(0.040)	(0.022)	(0.019)	(0.022)	(0.012)	(0.011)
Cash	0.044	-0.001	-0.015	0.027	-0.003	-0.009
	(0.047)	(0.027)	(0.025)	(0.027)	(0.016)	(0.014)
State fixed effects	NO	YES	YES	NO	YES	YES
Pre-program controls	NO	YES	YES	NO	NO	YES
Year fixed effects						YES
Observations	5,444	5,444	5,444	38,108	38,108	38,108
Outcome mean in control group	0.65	0.65	0.65	0.27	0.27	0.27
***Effect size:* In-kind only - In-kind & educ**	0.00	-0.03	-0.04	0.01	-0.01	-0.01
H_0: In-kind only - In-kind & educ (p-value)	0.94	0.15	0.07	0.75	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 A4: The impact of PAL on learning - 3 treatments arms

Outcome =	(1) Math (z-score)	(2) Math (z-score)	(3) Spanish (z-score)	(4) Spanish (z-score)	(5) 3rd subject (z-score)	(6) 3rd subject (z-score)
In-kind only	-0.046	-0.052	-0.012	-0.018	-0.024	-0.032
	(0.094)	(0.085)	(0.105)	-0.092	(0.089)	(0.080)
In-kind & educ	-0.043	-0.046	-0.020	-0.034	-0.021	-0.027
	(0.091)	(0.085)	(0.095)	(0.091)	(0.092)	(0.084)
Cash	-0.193**	-0.182**	-0.144	-0.156*	-0.173**	-0.156*
	(0.086)	(0.086)	(0.095)	(0.093)	(0.082)	(0.080)
State FE	NO	YES	NO	YES	NO	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 & educ**	0.00	0.01	-0.01	-0.02	0.00	0.01
H_0: In-kind only - In-kind & educ (p-value)	0.98	0.95	0.94	0.85	0.97	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 A5: The impact of PAL on learning - categorical classification

Outcome =	(1) Math Insuff.	(2) Spanish Insuff.	(3) 3rd subject Insuff.	(4) Math Excellent	(5) Spanish Excellent	(6) 3rd subject Excellent	(7) Levels Math	(8) Levels Spanish	(9) 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 A6: The impact of PAL on nutrition - RDA outcomes

Outcome =	(1) RDA energy (kcal)	(2) RDA energy (kcal)	(3) RDA protein	(4) RDA protein	(5) RDA vitamin C	(6) RDA vitamin C	(7) RDA iron	(8) RDA iron	(9) RDA zinc	(10) RDA zinc
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)
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
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) 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) 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 A7: 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 A8: The impact of PAL on proxies for the returns of child labor

	(1)	(2)	(3)	(4)
Outcome =	Total Number of animals	HH farmed or raised animals in past year	Number of HH members farming	Average number of hours farming
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)
State FE	YES	YES	YES	YES
Pre-program controls	YES	YES	YES	YES
Restrc. to those with ENLACE	YES	YES	YES	YES
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.

(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) Outcome variable in column (1) takes the value of the number of small and big animals owned by the household; outcome in column (2) takes the value 1 if any of the household member was involved either in farming or raising animals, outcome in (3) is the total number of household members who are reported farming; outcome in column (4) is the average number of hours spent farming among household members.

Table A9: School and Nutrition

	(1)	(2)
Outcome =	General school	General school
Male	-0.009	-0.010
	(0.018)	(0.019)
Height	0.010***	0.008***
	(0.002)	(0.002)
Anemia	-0.021	-0.058**
	(0.024)	(0.024)
Principal component nutrition score		0.025**
		(0.010)
State FE	YES	YES
Year FE	YES	YES
Pre-program controls	YES	YES
Observations	1,186	1,106

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.

(5) *General school* is an indicator variable that equals to 1 if student attends a general (SEP) school.