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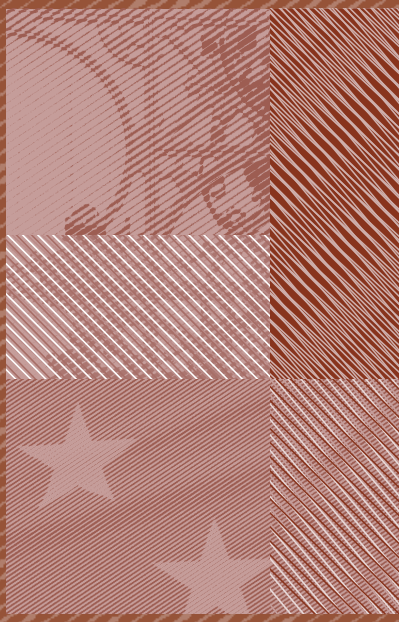

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Abstract

This paper studies the effects of cash versus in-kind transfers on child labor. Using data from a program which randomly transferred either cash or a basket of food to poor households in Mexico, I find that the cash transfer reduced children's work participation by a significantly larger margin than the in-kind transfer. Both transfers had large negative effects on child labor among recipients in the middle tertile of the income distribution. However, the in-kind transfer did not reduce child labor among children in the bottom tertile, whereas the cash transfer did. Moreover, transfer recipients in different income tertiles adjust child labor on different margins (extensive versus intensive). I show that the different margins of adjustment across the income distribution can be rationalized by a model in which preferences for schooling respect a luxury axiom and the household could forego child labor earnings only when the transfer pushes consumption above subsistence.

Keywords: cash transfers, in-kind transfers, child labor, schooling.

JEL classification: D61, H23, H43, I38, 022.

Resumen

Este documento estudia los efectos de transferencias en efectivo y en especie en el trabajo infantil y en la asistencia escolar de niños en municipios rurales de México. Utilizando datos de un programa de asistencia a hogares pobres que transfirió de manera aleatoria una transferencia en efectivo o una cesta de comida, se muestra que ambas transferencias reducen el trabajo infantil, aunque el efecto de la transferencia en efectivo es más elevado que el de la transferencia en especie. Al ser la transferencia en especie regresiva, no resulta efectiva para reducir el trabajo infantil entre hogares en la parte más baja de la distribución de la renta. Además, beneficiarios en diferentes cuantiles de la distribución de la renta reducen el trabajo infantil en distintos márgenes (extensivo frente a intensivo). El efecto heterogéneo del programa a lo largo de la distribución de la renta es coherente con un modelo en el que la educación es un bien de lujo y el hogar renuncia a la renta originada por el trabajo infantil solo cuando la renta familiar se sitúa por encima de cierto nivel de subsistencia.

Palabras clave: transferencias en especie, transferencias en efectivo, trabajo infantil, educación.

Códigos JEL: D61, H23, H43, I38, 022.

1 Introduction

Child labor is an extremely widespread phenomenon, involving over 200 million children under seventeen years of age (ILO, 2017). More importantly, it is often regarded as one of the main causes of the perpetuation of poverty in the developing world as it typically forces children to abandon school at an early stage, thus interfering with their human capital development. Several explanations for the existence of child labor in poor countries have been proposed in the literature.[1] However, there is ample consensus that child labor would decrease with improvements in household's economic conditions.[2] This view has motivated the use of poverty alleviation programs as means to contrast child labor. The vast majority of such programs provide benefits to vulnerable households which are delivered either in-kind or in-cash (Honorati et al., 2015). Although the literature has studied the effects of several social protection programs on child labour, little is known about the effectiveness of one transfer scheme against another. This paper investigates the relationship between child labor and the provision of in-kind versus cash transfers by exploiting experimentally induced variation in the transfer modality.

Most of the recent literature about the impact of poverty alleviation programs on child labor focused on a new generation of programs whose benefits are delivered conditional on children attending school. Conditional cash transfers are currently used in many low and middle income countries and they are often found to lead to increased school participation and lower levels of child labor (see Fiszbein and Schady, 2009 for a review). Although relatively less studied, there is evidence that conditional in-kind transfers can also change the time allocation of children. Ravallion and Wodon (2000) find that a food subsidy program conditional on children attendance at primary school increased school participation and reduced child labor in Bangladesh. Edmonds and Shrestha (2015) show that an in-kind stipend conditional on school attendance reduced child labor in Nepal, although the effect did not persist once the incentive was removed. However, the literature on conditional transfer programs can not inform about how child labor and schooling decisions respond to changes in household's economic conditions, since the conditionality requirement changes the opportunity cost of schooling. In other words, it is not possible to determine how much of the reduction in child labor is due to the transfer per se and how much it is driven by the program conditionality.

[1] Basu and Van (1998) develop a model in which child labor arises as a result of multiple equilibria: one in which children work because parental earnings in the labor market are low; and another in which children do not work because parental wages are high. Baland and Robinson (2000) show that child labor is inefficiently high when capital markets are imperfect since parents use child labor as a substitute for their inability to borrow against their children's future earnings. Similarly, Ranjan (2001) develops a model in which child labor arises due to credit constraints.

[2] Apart from the literature on poverty alleviation programs, which is reviewed in the main text, the relationship between changes in economic conditions and the time allocation of children has been studied empirically in an extensive number of settings, with mixed results. Edmonds (2005) finds that the reduction of child labor in Vietnam can be mainly explained by sustained economic growth. Schady (2004) documents that children exposed to macroeconomic crises in Peru are less likely to work. Kruger (2007) presents evidence that children in coffee producing regions in Brazil work more during economic booms. Soares et al. (2012) suggest that the contradictory results in the literature can be explained by different types of income shocks having income and substitution effects on child labor.

The empirical evidence about the child labor response to unconditional transfers, either in-kind or in-cash, is relatively more scarce. On one hand, Edmonds (2006) finds that child labor declines and schooling increases after an anticipated expansion of a pension scheme in South Africa. Edmonds and Schady (2012) document large reductions in child labor for families receiving a cash transfer in Ecuador. Similarly, other studies in Malawi and Mexico have found higher school attendance among children living in households that are given cash transfers (Baird et al., 2011; de Brauw and Hoddinott, 2011). On the other hand, to my knowledge there is no evidence in the literature about the impact of unconditional transfers in-kind on the time allocation of children.

The first contribution of this paper is to provide novel evidence about the effects of an unconditional transfer in-kind on child labor. Second, and more importantly, by exploiting experimental variation in the transfer modality, this is the first paper to study if the impact of welfare programs on child labor and schooling depends on whether the transfers are given in-kind or in-cash. I investigate these issues by studying how child time allocation responds to the Programa de Apoyo Alimentario (PAL), a governmental program providing either a cash transfer or a food basket to poor households in rural Mexico. The evaluation design of the program relies on an experimental trial in which approximately 200 villages were randomly assigned to receive either the cash transfer or the food basket, or to a control group that received nothing. In addition, pre- and post-intervention surveys collected information about the work participation and school attendance of children within the household, as well as about program take-up. Therefore, the PAL experiment provides a unique setting for the purpose of comparing the effects of cash versus in-kind transfers on child time allocation.

The empirical specification compares the change over time in the labor supply of participating children vis-a-vis the change over time in the labor supply of non-participating children. As the choice of participating in the program is endogenous, I use the random assignment of the treatment at the village level as an instrument for program participation. The results show that the cash transfer reduced participation in the labour market and increased schooling for children of high school age (15-16 years old at baseline).[3] The estimated impact of the cash transfer implies a reduction of about 10 working hours a week, and an increase in school attendance of 12 percentage points, as compared to children in the control group. On the extensive margin, the cash transfer reduced child labor by 9 percentage points, with even larger and significant effects when looking at paid employment. By contrast, the estimated impacts of the transfer in-kind are smaller and not statistically different from zero.

To shed more light on the significantly larger effect of the cash transfer vis-a-vis the in-kind transfer, the paper investigates the heterogeneity of the child labor response to the PAL program across the distribution of household income. This focus is motivated by existing evidence that the welfare gains for recipients of the PAL in-kind transfer are increasing with household income (Tagliati, 2018). This regressive effect of the food basket might thus explain the differential impact of the in-kind versus the cash transfer on child time allocation.

[3] As I will discuss in Section 5, for younger children neither the cash nor the in-kind transfer had an effect on child labor and schooling, which could be explained by binding child labour regulations.

This source of heterogeneity is studied not only empirically but also through the lens of a simple model which provides a theoretical reference to read the empirical results. The model shares important characteristics of the theoretical literature on child labor and, in particular, the idea that preferences for schooling are characterized by a luxury axiom (Basu and Van, 1998; Soares et al., 2012). Under this assumption, children work full-time when household income is not sufficient to guarantee that consumption is above subsistence. The model has three possible solutions for the child's time allocation (full-time work, work and schooling, full-time schooling) which depend on two income thresholds at which the household moves from one solution to another. This implies that households at different points of the income distribution adjust child labor decisions on different margins after receiving government transfers. Transfer recipients at the bottom of the income distribution reduce child labor only on the intensive margin. Recipients in the middle of the income distribution adjust child labor both on the intensive and on the extensive margin, while there is no effect for recipients at the top. As for the differential effects of cash versus in-kind transfers on child labor supply, theoretically the effect of a transfer in-kind is bounded above by the effect of an equal-value cash transfer, and strictly lower if the transfer in-kind is extra-marginal.[4]

Because the data lack several information that would be required to estimate the model, I rather test its predictions exploiting the randomized design of the program. I estimate the impact of the PAL program for children of high school age from households in the first, second and third tertile of a proxy measure of household baseline income.[5] The results are in line with the theoretical predictions. More precisely, I find that children from cash recipient households in the first income tertile work about 12 hours a week less than untreated children with similar socioeconomic background but, consistently with the model, there is no significant reduction on the extensive margin. For cash recipients in the middle of the income distribution, there are significant reductions over both the intensive and the extensive margin of child labor, while no effect is found for children in the top income tertile. Compared to the cash transfer, the transfer in-kind caused similar reductions in child labor for households in the middle tertile of the income distribution, but it was ineffective for children in the bottom tertile, a result which is consistent with the regressive nature of the PAL in-kind transfer. In terms of magnitude, the largest effects are found among households in the middle tertile: both transfers reduced child labour on the extensive margins by 22-23 percentage points, and by 8 to 14 hours a week on the intensive margin.

Finally, it is worth mentioning that, in addition to the literature on child labor, this paper is also related to the literature on the relative merits of cash versus in-kind transfers (see Currie and Gahvari, 2008 for a review). Whereas cash transfers are praised for having lower administrative costs and for being fungible, in-kind transfers might be preferred when there are externalities from consumption of some merit goods or to induce the non-poor to self-select out of social protection programs (Garfinkel, 1973; Nichols and Zeckhauser, 1982; Blackorby and Donaldson, 1988). Other theoretical works suggest that extra-marginal in-kind transfers provide lower disincentives to

[4] A transfer in-kind is said to be extra-marginal if consumption of the subsidized good under the transfer is larger than what the consumer would have consumed of that good under an equal-value cash transfer.

[5] As I will discuss in Section 3, to proxy for the unobserved income of the household, I construct an index of the socioeconomic status of the household using information on ownership of durables and housing characteristics.

work as compared to cash-equivalent transfers (Murray, 1980; Leonesio, 1988; Munro, 1989, Gahvari, 1994).[6] In the context of the PAL program, Skoufias et al. (2008) do not find any significant change in the labor supply of adults receiving either the cash or the in-kind transfer.[7] This paper contributes to this area of research by studying child time allocation responses to cash and in-kind transfers, documenting larger reductions in child labor among cash vis-a-vis in-kind recipients.

The rest of the paper is organized as follows. Section 2 presents the theoretical framework and the child labor model. Section 3 describes the PAL program and the data. Section 4 discusses the empirical strategy. The empirical results are presented in Section 5. Section 6 concludes.

2 Theoretical framework

2.1 Child labor under cash and in-kind transfers

Consider the maximization problem of a unitary household formed by one parent and one child.[8] Assume that the household has preferences over two consumption goods: c is a composite consumption good which is freely purchased in the market at a price p_c; z is a composite consumption good which is possibly subsidized in-kind by a government, and whose price is denoted with p_z. The household also values the time the child spends at school, which is denoted with s.[9] Hence, household utility is given by $U(c,z,s)$. The child's total time, T, is allocated between schooling and labor according to the equation $h+s=T$, where h denotes the hours of work of the child. As the focus of the paper is on child labor, I assume that parents always inelastically supply their time endowment in the labor market, and that parental labor supply is not affected by the provision of government transfers.[10]

The other actor in this economy is a government, which can either provide a transfer in-kind for good z, denoted with $\bar{z}$, or an equal-value cash transfer $\bar{x}=p_z\bar{z}$. In this setting, a transfer in-kind is

[6]There is an extensive literature studying the effects of either cash or in-kind transfers on adult employment. In advanced economies, negative effects on adult labor supply have documented among recipients of the Food Stamp Program (Fraker and Moffitt, 1988; Hagstrom, 1996; Hoynes and Schanzenbach, 2012), whereas Jones and Marinescu (2018) finds that a universal and permanent cash grant in Alaska had no effect on household employment. In developing countries, null or even positive effects on adult labor supply have being documented among recipients of unconditional cash transfers (Ardington et al., 2009; Haushofer and Shapiro, 2016; Salehi-Isfahani and Mostafavi-Dehzooei, 2018). On the contrary, other studies found that cash or in-kind benefits led to a reduction in work participation (Sahn and Alderman, 1996; de Carvalho Filho, 2008).

[7]Other studies using data from the PAL programs include Avitabile (2012), who studies the effect of the program on health behavior and health outcomes. Cunha (2014) studies the extra-marginality of the in-kind transfer and the program effects on household consumption and nutrition. Tagliati (2018) estimates a model of demand to quantify household welfare for in-kind recipients, finding that the in-kind transfer was on average more cost-efficient than the cash transfer but regressive. Cunha et al. (2019) show that, by lowering the residual demand of the subsidized goods, the in-kind transfer caused a reduction in local food prices.

[8]This section is largely based on the classical theory of labor supply under cash and in-kind transfers (Murray, 1980; Leonesio, 1988; Munro, 1989; Gahvari, 1994), but it is applied to child labor rather than to adult labor supply.

[9]The household values the schooling of the child because it might expect higher educated children to get larger future expected earnings. Returns to education in Mexico could indeed be substantial. Parker (1999) estimates that any additional year of schooling increases wages by approximately 8% in urban Mexico.

[10]In the context of the PAL program, Skoufias et al. (2008) show that neither the cash nor the in-kind transfer changed the work participation of the adult population. In Section 5.3, I provide further evidence about the lack of this mechanism across the distribution of household income.

said to be infra-marginal if household consumption of good z would be at least as large as $\bar{z}$ under a cash-equivalent transfer $\bar{x}$. On the contrary, if household demand for good z under $\bar{x}$ is strictly lower than $\bar{z}$, the transfer is extra-marginal.

Assuming that s is a normal good, a cash transfer increases schooling, and consequently decreases child labor, through a standard income effect. By definition, the same effect can be obtained by the provision of an infra-marginal transfer in-kind. If instead the transfer is extra-marginal, the household maximizes $U(c,z,s)$ subject to $z=\bar{z}$ and the budget constraint $p_c c \leq w(T-s)+y$, where w represents the wage that the child earns in the labor market and y is a source of income exogenous to child labor. Equivalently, one could write the household problem as the minimization of the following expenditure function subject to $z=\bar{z}$:

$$e(p_c,w,\bar{z},u) = \min_{c,s} \{p_c c + ws : U(c,\bar{z},s) \geq u\}. \tag{1}$$

The solution of this problem gives the "constrained" compensated demands for c and s, $c = c(p_c,w,\bar{z},u)$ and $s = s(p_c,w,\bar{z},u)$. Clearly, the child's labor supply can immediately be derived from the time constraint. Let u^* be the highest utility level attainable by the household from the solution of problem (1). An equivalent formulation is one in which the household faces prices p_c and p_z^* and chooses c, z and s to minimize the "unconstrained" expenditure function

$$e(p_c,p_z^*,w,u^*) = \min_{c,z,s} \left\{p_c c + p_z^* z + ws : U(c,z,s) \geq u^*\right\}. \tag{2}$$

Here, p_z^* is the virtual price of good z at which the household would freely choose to consume exactly $z=\bar{z}$. In other words,

$$\bar{z} = z(p_c,p_z^*,w,u^*). \tag{3}$$

It is straightforward to show that, in order to reach the utility level u^*, household income must be equal to $y^* = y + p_z^*\bar{z}$. Moreover, since the household would not have consumed $z=\bar{z}$ under an equal-value cash transfer, it must be that $p_z^* < p_z$. This implies that an extra-marginal transfer in-kind is equivalent to: (i) an implicit increase in household income from y to y^* (income effect); and (ii) an implicit reduction in the price of the transferred good from p_z to p_z^* ("price" effect). In the next subsection, I present a simple model and use these observations to study how income and price effects change child labor decisions upon the provision of an extra-marginal transfer in-kind.

2.2 The model

I assume that household utility is quasilinear in schooling and that the degree of substitutability between consumption goods can be represented by a generalized CES utility function, i.e. $U(c,z,s) = [c^\sigma + z^\sigma]^{\frac{\gamma}{\sigma}} + \alpha s$.[11] In order for $v(c,z) = [c^\sigma + z^\sigma]^{\frac{\gamma}{\sigma}}$ to be increasing and quasi-concave

[11] The model presented in this section, and particularly the functional form for household utility, are similar to Soares et al. (2012) but deeply differ in both motivation and objectives. While Soares et al. (2012) develop a model of child labor and schooling to disentangle income and substitution effects of changes in household economic conditions, I use a similar framework to study the heterogeneous response of child labor to cash and in-kind subsidies.

in c and z, let $\sigma < 1$ and $0 < \gamma \leq 1$. Moreover, assume that $\alpha > 0$ so that household utility is increasing in schooling. As before, the household budget constraint is given by $p_c c + p_z z \leq wh + y$, where p_c and p_z are respectively the prices of c and z; w represents the wage that a working child earns in the labor market; h denotes the hours of work of the child; and y is a source of income exogenous to child labor. More specifically, I assume that y is given by the sum of government transfers, $\bar{y}$, and parental labor earnings ε, that is $y = \bar{y} + \varepsilon$. In order to study how households with different income levels would respond to the provision of government transfers, I further assume that $\bar{y}$ is fixed and common across all households, while ε varies across households according to some unspecified distribution G. If the child's total time T is allocated between schooling and labor according to the equation $h + s = T$, the household problem can be expressed as follows

$$\begin{aligned} \max_{c,z,s} \left[c^{\sigma} + z^{\sigma}\right]^{\frac{\gamma}{\sigma}} + \alpha s \qquad & s.t \\ & p_c c + p_z z \leq w(T - s) + \bar{y} + \varepsilon. \end{aligned} \tag{4}$$

A convenient solution for this problem, which exploits the separability between s and $v(c,z)$, proceeds in two steps. In the first step, the objective function is maximized with respect to c and z, taking s as given and defining $m \equiv w(T - s) + \bar{y} + \varepsilon$ to be the household potential income for a given choice of s. Let $c(m)$ and $z(m)$ be the solutions for c and z as a function of m. In the second step, $c(m)$ and $z(m)$ are used into (4) to compute the "partial" indirect utility of the household conditional on s. Hence the problem becomes the one of choosing m and s to maximize

$$\begin{aligned} \max_{m,s} \left[c(m)^{\sigma} + z(m)^{\sigma}\right]^{\frac{\gamma}{\sigma}} + \alpha s \qquad & s.t \\ & m = w(T - s) + \bar{y} + \varepsilon. \end{aligned} \tag{5}$$

It is easy to verify that the first order conditions for this problem are

$$\pi \gamma m^{\gamma - 1} = \mu \tag{6}$$

$$\alpha \begin{matrix} > \\ = \\ < \end{matrix} \mu w, \tag{7}$$

where μ denotes the Lagrangian multiplier and π is equal to

$$\pi = \left[p_c^{\frac{\sigma}{\sigma - 1}} + p_z^{\frac{\sigma}{\sigma - 1}}\right]^{\frac{\gamma(1-\sigma)}{\sigma}}. \tag{8}$$

The solution of both steps of the maximization procedure are presented in Appendix *A*. Plugging (6) into (7) gives $\alpha \begin{matrix} \leq \\ > \end{matrix} \pi \gamma m^{\gamma - 1} w$, which relates the marginal utility of schooling to the marginal cost of foregone child labor earnings. Depending on the sign of this inequality, the solution to the household problem can lie in any of the following three cases.

Case 1: Work only If the marginal utility of schooling is lower than its marginal cost, i.e. if $\alpha < \pi\gamma m^{\gamma-1} w$, the child works full time (i.e., $h = T$ and $s = 0$). Plugging the budget constraint, which is given by $m = wT + \bar{y} + \varepsilon$, into the inequality above gives

$$\varepsilon < \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - wT - \bar{y} \equiv \varepsilon_L. \tag{9}$$

Case 2: Schooling only This case is represented by the inequality $\alpha > \pi\gamma m^{\gamma-1} w$, with $h = 0$ and $s = T$. Since the child does not work, the budget constraint is given by $m = \bar{y} + \varepsilon$. Plugging m into the inequality we have

$$\varepsilon > \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - \bar{y} \equiv \varepsilon_H. \tag{10}$$

Case 3: Work and schooling This case is represented by the equality $\alpha = \pi\gamma m^{\gamma-1} w$ with $h > 0$ and $s > 0$. Using the budget constraint, which is given by $m = wh + \bar{y} + \varepsilon$, we can find that $h = \frac{1}{w}\left[\left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - \bar{y} - \varepsilon\right]$ or, alternatively, we can derive that $\varepsilon = \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - wh - \bar{y}$. Since $0 \leq h \leq T$, we have a range of values of ε such that the child works and goes to school. In particular, $h > 0$ and $s > 0$ for

$$\varepsilon_L = \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - wT - \bar{y} \leq \varepsilon \leq \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - \bar{y} = \varepsilon_H.$$

Hours of work as a function of income From the solution to the model, we can derive the optimal choice for child labor supply h^* as a function of household income ε (net of child labor earnings and government transfers). This is given by

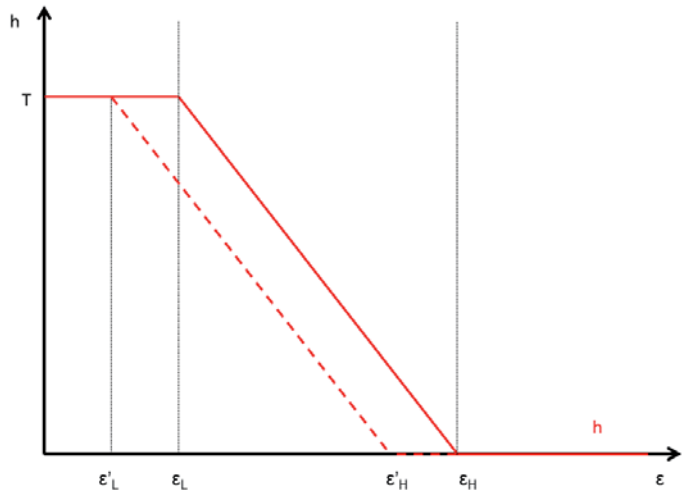
$$h^* = \begin{cases} T & if \quad \varepsilon < \varepsilon_L \\ \frac{1}{w}\left[\left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - \bar{y} - \varepsilon\right] & if \quad \varepsilon_L \leq \varepsilon \leq \varepsilon_H \\ 0 & if \quad \varepsilon > \varepsilon_H \end{cases} \tag{11}$$

and can be represented by the graph in Figure 1. The three cases discussed above correspond to the three regions in the graph identified by the thresholds ε_L and ε_H. If household income is below ε_L, the child works full time. If household income is between ε_L and ε_H, the child works and goes to school. Finally, for income levels above ε_H the child goes to school full time.

It is important to remark that the solution for the child labor supply depends crucially on the quasi-linearity of the utility function in schooling. This assumption about household preferences embeds the idea, common to some of the child labor literature, that household demand for child schooling follows a so called "luxury axiom" (Basu and Van, 1998; Soares et al., 2012). Under this assumption, for very low levels of household income, consumption falls below subsistence and, in

this case, the child's time is allocated entirely into the labor market to maximize utility from consumption. The threshold ε_L can thus be interpreted as the level of income below which household consumption is below subsistence. Only if household income is above ε_L the household is willing to forego at least part of what the child could earn in the labor market. Another implication of this assumption is that even relatively small changes in household income could have large effects on child labor if they allow households to move from below to above the subsistence threshold.

Figure 1: Labor supply as a function of household income



The effects of cash and in-kind transfers on child labor supply A cash transfer can be interpreted as an increase in the household allotment of government transfers $\bar{y}$. As it is apparent from equations (9) and (10), the thresholds ε_L and ε_H are both decreasing in $\bar{y}$. Hence, a cash transfer shifts to the left both thresholds in Figure 1, say to ε_L' and ε_H' . This determines a reduction in child labor but on different margins across the income distribution: (i) children in households whose income is between ε_L' and ε_L reduce the amount of working hours (change in the intensive margin of labor supply); and (ii) households whose income is between ε_L and ε_H decrease child labor over the intensive margin (in the region between ε_L and ε_H') and over the extensive margin (in the region between ε_H' and ε_H). For children in households whose income is above ε_H no change occurs. The effect on schooling could easily be obtained from the change in working hours: given that, in the model, a child can only work or go to school, we have that $\Delta s = -\Delta h$.

Consider now a transfer in-kind $\bar{z}$ whose market value coincides with the value of the cash transfer, i.e. $\bar{x} = p_z \bar{z}$. By definition, if the transfer is infra-marginal for a given household, it has exactly the same effect of an equal-value cash transfer. Instead, as discussed in Section 2.1, an extra-marginal transfer in-kind has both an income effect (i.e., it corresponds to an increase in income from $\bar{y} + \varepsilon$ to $\bar{y} + p_z^* \bar{z} + \varepsilon$) and a "price" effect (i.e., it corresponds to a reduction in the price of good z from p_z to p_z^*). This has two implications for child labor supply. First, the increase in income determines a left-shift of the thresholds ε_L and ε_H in Figure 1. However, since $p_z^* \bar{z} < p_z \bar{z} = \bar{x}$, the magnitude of the income effect of an extra-marginal transfer in-kind on labor supply is lower than the magnitude of the income effect of a cash transfer. Second, the price effect also shifts the thresholds since the term $\left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}}$ depends on p_z through the coefficient π shown

in equation (8). To determine the sign of a change in p_z, I differentiate ε_L (or, equivalently, ε_H) with respect to p_z which gives

$$\frac{\partial \varepsilon_L}{\partial p_z} = \left[\frac{\alpha}{\gamma}\frac{1}{w}\right]^{\frac{1}{\gamma-1}} \frac{\pi^{\frac{\gamma}{1-\gamma}}}{(1-\gamma)}\frac{\partial \pi}{\partial p_z}.$$

Given the restrictions $\alpha > 0$ and $0 < \gamma \leq 1$, which are required to ensure that utility is increasing in all arguments and concave in c and z, it is immediate that the sign of the derivative is equal to the sign of $\frac{\partial \pi}{\partial p_z}$. In Appendix A, I prove that $\frac{\partial \pi}{\partial p_z} < 0$, which implies that the (virtual) reduction in the price of good z determines a right shift in the thresholds ε_L and ε_H or, in other words, a positive effect on child labor. While the sum of the income and of the "price" effect is theoretically ambiguous, it is clear from the above analysis that, overall, labor supply would be higher under an in-kind transfer rather than under an equal-value cash transfer.[12]

In summary, the model presented in this section gives some testable implications on the effect of a cash transfer on child labor. It predicts a reduction on the intensive margin of child labor, for households at the bottom and in the middle of the income distribution, and no effect for households at the top. It further predicts a reduction on the extensive margin of child labor only for households in the middle of the income distribution. Regarding the effects of a transfer in-kind, if the income effect is larger than the "price" effect, we should expect similar effects but of lower magnitude.[13]

3 The PAL program and the data

3.1 The PAL program and experiment

PAL is a social protection program which operates in around 5,000 rural villages throughout Mexico. It was launched by the government at the end of 2003 with the objective of improving the living conditions of the targeted population. Eligibility to the program was determined through a two-stage procedure. First, villages were deemed eligible if: (i) they have a population of less than 2,500 inhabitants; (ii) they are highly marginalized, as defined by the National Council for Population criteria; (iii) they do not receive other transfer programs, such as Liconsa or Oportunidades;[14] (iv) they are accessible and close enough to a store managed by DICONSA, the governmental

[12] This result is consistent with the theoretical literature on adult labor supply under cash and in-kind transfers (Gahvari, 1994).

[13] If the cross-price elasticity between c and z is not too small, then a household would substitute away from c at a relatively large rate even for small changes in the price of z. In such a case, the implicit reduction in the price of good z, which is required in order for the household to optimally consume $\bar{z}$, is quite small and, therefore, the income effect is likely to be larger than the price effect. In the context of PAL, in which the in-kind transfer is a food basket and most of the household's budget is spent on food, the substitutability between subsidized and non-subsidized goods might be relatively high, in which case the income effect might dominate the price effect.

[14] Liconsa is a subsidized milk program. Oportunidades is the well-known conditional cash transfer program of Mexico. PAL villages were not included in Oportunidades because they did not have close enough health facilities and/or schools to comply with the conditional requirements of the program. As a consequence, PAL villages are, in general, poorer and more marginalized than rural villages in Oportunidades. As I will discuss in more detail, self-reported data on receipt of other transfer programs suggest that some PAL beneficiaries also received benefits from Oportunidades and Liconsa. In the empirical analysis, I include controls for receiving such programs and test the robustness of the results to the exclusion of households receiving scholarships from Oportunidades.

agency in charge of administering the program.[15] Second, within eligible villages, all households that scored below a means-test poverty threshold were offered the program.

Concurrent with its nationwide implementation, 206 villages, among the universe of eligible localities, were randomly selected in order to participate into an experimental trial. Each village was randomly assigned to one of three treatment arms: (i) an in-kind treatment arm (103 villages); (ii) a cash treatment arm (53 villages); (iii) a control group, which received nothing (50 villages). Villages in the in-kind treatment arm received a monthly food basket containing ten commodities, which were selected by nutritionists to provide a balanced diet.[16] Villages in the cash treatment arm were instead offered a monthly cash transfer of 150 pesos (approximately US$ 13), which corresponds to the purchasing cost of the food basket to the Mexican government in wholesale markets. However, the average cost of the basket in recipients' local markets was about 205 pesos (approximately US$ 18).[17] Hence, the face value of the transfer in-kind was on average 33 percent larger than the value of the cash transfer.

At face value, the in-kind transfer represented, on average, 11 percent of household's baseline total expenditure, whereas the cash transfer corresponded to about 13 percent of total expenditure at baseline. The transfers were not conditional on family size and, whenever possible, they were given to a female household member (typically the spouse of the household head). Estimates of child wages in PAL villages are not available, but it is possible to relate the size of the transfers to the average wage that a child working full-time could earn in other poor rural villages in Mexico. Based on this evidence, I estimate that the cash transfer represents between 25 to 30 percent of the average child wage, while the nominal value of the in-kind transfer represents between 33 to 40 percent.[18]

An additional feature of the program is the fact that, for a random half of villages in the in-kind treatment arm and for all villages in the cash treatment arm, the transfers were intended to be conditional on adult members' participation in monthly classes which covered topics related to healthy eating, nutrition and hygiene practices.[19] Classes were held by members of a Committee of Beneficiaries, which were previously selected among educated members within the village and

[15] Accessibility is defined as the village being within 2.5 km from a road. Similarly, a village is considered to be close to a DICONSA store if it is within 2.5 km from it.

[16] The commodities are corn flour, rice, beans, pasta soup, biscuits, fortified milk powder, vegetable oil, lentils, breakfast cereals and canned fish. The list of goods, the quantities transferred per month and the average value of the PAL box are reported in Table B1 in the Appendix.

[17] This is due to the fact that the government could exploit substantial economies of scale from procuring large quantities of goods in wholesale markets. These are only partially offset by the transportation, stocking and other administrative costs of the in-kind modality, which correspond to about 22 percent of the purchasing cost of the transfer. The administrative costs of the cash transfer are, instead, about 12 percent of the value of the subsidy (see Ventura-Alfaro et al., 2011 for more details about the costs of the two transfer modalities).

[18] Schultz (2004) estimates that the average monthly wage of a child working full-time in Oportunidades villages was about 380 pesos in 1999. Applying the growth rate either of the CPI or of the hourly wage in the manufacturing sector between 1999 and 2003, I estimate that the average nominal earnings of a child in 2003 amounts to 500-600 pesos (approximately US$ 44-53).

[19] Since one of the objectives of the experimental design was to study the effect of the classes over and above the effect of the in-kind transfer itself, some localities were randomly assigned to receive a pure unconditional in-kind transfer. Avitabile (2012) studied the effect of class participation on health outcomes, documenting improvements in the health behavior of women in the in-kind plus classes group as compared to women in the in-kind group.

who received special training for teaching the classes. However, although the courses were meant to be a mandatory requirements for the receipt of the transfer, no household was ever denied benefits for not attending (Skoufias et al., 2008).[20] In addition to the lack of enforcement, classes were taught also in villages in the in-kind without classes treatment arm. Because of the contamination of this program component, in the paper I pool together all villages that received transfers in-kind, irrespective of whether they were originally randomized in or out of class participation.

For the purpose of studying the effects of PAL on child labor, one might be worried that class participation might have had some direct effect on the time allocation of children. This might occur, for example, if parents were forced to reduce their participation in the labor market in order to attend the classes, possibly compensating the reduction in earnings with increased child labor. However, the fact that parents were only required to attend one class per month, and that class participation was not enforced (see Appendix B), makes this extremely unlikely. Indeed, previous evaluations of PAL did not find any effect of the program on adult labor supply (Skoufias et al., 2008). Another possibility is that this requirement fostered the interaction with other households in the village, and that parents attending the classes might be more likely to under-report child labor if this is stigmatized within the community. However, according to the Federal Labor Act (Ley Federal de Trabajo), child labor is regulated in Mexico up to age 15. As I will discuss, PAL had an effect on child labor only for children who were 16 or older at follow-up. As labor regulations are not binding for children in this age group, it is unlikely that the results are driven by differential "stigma effects" for class participants versus non-participants.

Finally, it is worth mentioning that the conditionality requirement of PAL is very different from those of other programs in the literature, which typically require kids from beneficiary households to comply with specific schooling requirements. While such programs provide a direct disincentive towards child labor by changing the relative price of schooling, this mechanism is not present in the context of PAL in which the only requirement is parental (as apposed to children) attendance to sporadic courses. Therefore, in the subsequent analysis I interpret the estimated impact of PAL on child labor and schooling as arising from unconditional transfers.

3.2 Data, sample and summary statistics

In each of the 206 villages included in the experiment, around 33 households were randomly selected to participate in pre- and post-intervention surveys. The baseline survey was conducted between October 2003 and April 2004, while follow-up data were collected from October to December 2005. The PAL transfers began to be delivered after the completion of the baseline survey. The survey provides information on school attendance, the main occupation and the total number of working hours in the last seven days for all individuals older than twelve. Data on household expenditure and ownership of assets are also available.

[20] According to the program rules, household were supposed to be excluded from the program if they missed more than two consecutive classes or a total of four classes in a year. Follow-up data confirm the lack of enforcement of such rules. Indeed, whereas households received on average 13 transfers since the start of PAL, they reported to have attended only 4 classes on average (see Appendix *B*).

The follow-up survey provides extensive information about the receipt of the PAL transfers, including the number of benefits received, their timing and the person beneficiary of the program within the household. About 90 percent of households reported to receive transfers from PAL in any treatment arm (see Appendix *A* for a detailed discussion about program take-up). Due to the lack of administrative data on household eligibility, it is not possible to determine if the remaining 10 percent of households did not participate because of ineligibility or imperfect compliance.

Of the original 206 experimental villages, nine villages were excluded from the analysis for various reasons: two localities were excluded because households started to receive PAL prior to the baseline survey; two villages are geographically contiguous, possibly violating the Stable Unit Treatment Assumption (SUTVA); two villages refused to participate in the program; one control locality was excluded because it received the in-kind treatment; two localities were dropped because all households in these villages were receiving Oportunidades, contrary to program rules.

Within the remaining 197 sample villages, I have dropped households with incomplete surveys, as well as attrited households and individuals. The estimation sample includes 2,590 children aged 12 to 16 at baseline who are observed in both surveys. The sample of households is derived selecting those 1,839 households with at least one child aged 12 to 16 at baseline. The choice to restrict the sample to this age group is dictated by two reasons. First, there is no information on the labor supply of children younger than 12. Second, by the age of 16 children should have progressed into high school (*preparatoria* or *bachillerato*), which usually ends around age 18. Since the follow-up survey was taken after one and a half year, and because enrollment into university after age 18 is extremely rare within this sample, choosing age 16 as a cutoff guarantees that a substantial number of children in this age group face a decision between enrolling or completing high school and start working.

Attrition was rather low in the sample, being around 12 percent at the household level and 21 percent at the child level. The attrition rate for 12-16 years old children is slightly lower for in-kind recipients than for the control group. However, there is no significant difference between the attrition rates of cash recipients and the control group, nor between the attrition rates of the cash and in-kind treatment groups (see Appendix *B*).

The first three columns in Table 1 show the means of selected household and children characteristics by treatment group. Columns 4 to 6 report the mean difference between any treatment group and another. As we can see from the top panel of Table 1, households in different treatment arms are overall balanced at baseline in terms of household composition, receipt of other welfare programs and per capita expenditure levels. Regarding children characteristics, the bottom panel of Table 1 shows that the randomization was somehow less successful. There are significantly less male children in the cash treatment group as compared to the control group. Moreover, children are significantly older in localities in the in-kind and cash treatment arms. Despite these differences, children in any of the treatment groups do not work significantly more than those in the control group, both on the intensive and on the extensive margin: although work participation and hours worked are slightly higher for cash and in-kind recipients, the difference with the control group is not statistically significant.

The statistics in Table 1 are also useful to characterize the targeted population. Households are quite large, with about six members on average. Around 17 percent of the households include one old member, and around 23 percent of them have at least one household member speaking an indigenous language. The sample is also quite poor and low educated. The average number of years of schooling of the household head is around four years. The average value of household monthly expenditure is around 2200 pesos (approximately US$ 190) and the share of food consumption is about 65 percent of total consumption. Sample children have completed six years of formal education on average. The average school attendance rate is slightly lower than 80 percent. Approximately 18 percent of the children in the sample worked in the last seven days. Among working children, the average number of weekly working hours is about 34.

3.3 Child labor and household income

For any individual who is at least 12 years old, the survey asks first to report the main activity in the last seven days;[21] it then asks if the respondent was involved in any working activity in addition to the main occupation in the last seven days. The measure of the extensive margin of child labor combines both answers and is defined as an indicator equal to one if either the child worked as the main activity or was involved in any other working activity in the last seven days. For all working individuals, the survey also asks to report the total number of working hours in the last seven days. This is the intensive margin measure of child labor used in the paper.

This definition of child labor, which is the preferred one throughout the paper, incorporates both market work and unpaid work in the family business (but it excludes housework). In some empirical results (see Section 5.1), I also distinguish between paid and unpaid work but, admittedly, in the absence of data on child earnings, this classification is only tentative. In particular, I define "unpaid work" as an indicator equal to one if the respondent reported "unpaid work in the family farm or business" as the type of working activity in the last seven days, and I define "paid work" as a residual category comprising all working activities others than "unpaid work in the family farm or business".[22] In the sample, about 11 percent of 12-16 years old children worked for pay at baseline, while about 7 percent worked in an unpaid activity in the family business, with no significant differences across treatment groups (see Table 1).

For easiness of exposition, the distinction between market and unpaid family work is not included in the simple model presented in Section 2.2. However, this might have implications for household response across the intensive versus extensive margins of child labor. Indeed, while hours of work in the family business can easily be adjusted, hours of work in the market might be indivisible and they are more likely to displace schooling. However, even in the sample of children

[21] The possible answers are: work, look for a job, student, housework/housekeeping, retired, unable to work.

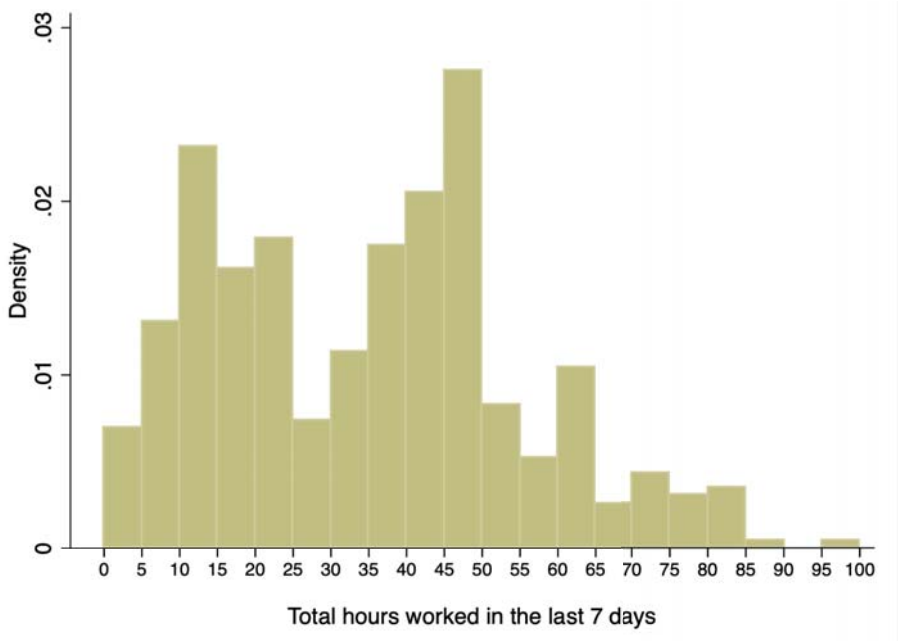
[22] It is possible that this classification overstates paid work since respondents who reported some working activities, such as "craftsman" or "construction worker", might be involved in them both outside the family business for pay or inside the family business without pay. Another reason to prefer a more comprehensive definition is that the survey asked only about the total number of weekly working hours, and therefore it is not possible to construct separate measures of the work intensity for occupations in and outside the family business.

Table 1: Baseline household and child characteristics by treatment group

	Control (1)	In-Kind (2)	Cash (3)	Diff. (2)-(1) (4)	Diff. (3)-(1) (5)	Diff. (2)-(3) (6)
			Panel *A*: household characteristics			
Number of household members	6.05	5.95	6.04	-0.09	-0.01	-0.09
	(2.22)	(2.12)	(2.19)	(0.24)	(0.28)	(0.25)
Number of children aged 0 to 5	0.62	0.57	0.57	-0.06	-0.05	-0.01
	(0.90)	(0.82)	(0.88)	(0.08)	(0.10)	(0.08)
Education of the household head	3.94	4.11	3.72	0.17	-0.22	0.40
	(3.53)	(3.49)	(3.61)	(0.30)	(0.35)	(0.32)
Indigenous household	0.27	0.23	0.17	-0.04	-0.10	0.07
	(0.44)	(0.42)	(0.37)	(0.08)	(0.09)	(0.07)
Old member in the household	0.15	0.18	0.18	0.03	0.03	0.00
	(0.35)	(0.38)	(0.38)	(0.02)	(0.03)	(0.03)
Female head	0.13	0.13	0.12	0.00	-0.01	0.01
	(0.34)	(0.34)	(0.33)	(0.03)	(0.03)	(0.02)
Received Oportunidades	0.17	0.11	0.11	-0.05	-0.06	0.01
	(0.37)	(0.32)	(0.31)	(0.06)	(0.07)	(0.05)
Received Liconsa	0.03	0.03	0.03	0.01	0.00	0.00
	(0.16)	(0.18)	(0.17)	(0.01)	(0.01)	(0.01)
Per capita monthly expenditure	420.99	393.30	398.86	-27.69	-22.13	-5.56
	(283.18)	(249.04)	(238.94)	(31.68)	(33.70)	(27.65)
			Panel *B*: children characteristics			
Male	0.59	0.55	0.53	-0.04	-0.05*	0.02
	(0.49)	(0.50)	(0.50)	(0.02)	(0.03)	(0.02)
Age	13.61	13.76	13.73	0.15**	0.12*	0.03
	(1.33)	(1.38)	(1.43)	(0.05)	(0.07)	(0.06)
Years of completed education	6.10	6.26	6.06	0.17	-0.04	0.21
	(2.28)	(2.20)	(2.39)	(0.19)	(0.24)	(0.21)
Attends school	0.81	0.76	0.78	-0.05	-0.03	-0.02
	(0.39)	(0.43)	(0.42)	(0.04)	(0.04)	(0.04)
Any work in the last seven days	0.17	0.18	0.20	0.02	0.04	-0.02
	(0.37)	(0.39)	(0.40)	(0.02)	(0.03)	(0.03)
Paid work in the last seven days	0.11	0.11	0.12	-0.00	0.01	-0.01
	(0.31)	(0.31)	(0.32)	(0.02)	(0.03)	(0.02)
Unpaid work in the last seven days	0.06	0.07	0.09	0.01	0.03	-0.02
	(0.24)	(0.25)	(0.28)	(0.02)	(0.03)	(0.03)
Hours of work in the last seven days	5.48	5.88	7.08	0.40	1.61	-1.21
	(15.02)	(15.25)	(16.85)	(1.02)	(1.33)	(1.17)

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Hours of work are equal to zero for children not working in the last seven days. Numbers in parentheses are standard errors, clustered at the village level, for the differences in columns (4) to (6) and standard deviations elsewhere.

Figure 2: Distribution of hours worked in the last seven days in the baseline sample



Notes: Hours of work are conditional on working in the last seven days. The sample includes 12-16 years old children at baseline.

not working in the family business, full-time market-related work is relatively rare as children are often employed as day laborers or in occupations which might have relatively flexible working hours, such as street sellers or domestic employees. This can be seen by looking at the distribution of working hours in the last seven days for the sample of working children at baseline, which is reported in Figure 2. Although about 10 percent of working children worked for 48 hours in the last week (i.e., the number of hours corresponding to a full-time job of eight hours per day within a working week of six days), more than 60 percent of children worked less than 40 hours. Indeed, the fact that there are not big spikes in the distribution of working hours might suggest that households are able to adjust the labor supply of their children on the intensive margin in response to changes in household income.

The PAL survey does not report information on household income nor on wages. In order to study the heterogeneity of the program impact across the income distribution, I use data on the characteristics of the dwellings and on the ownership of durables, and define an income index to be the first principal component within a Principal Component Analysis (PCA).[23] More specifically, housing characteristics include the material of the floor, walls and roof, the source of water, the type of sanitation facility, the presence of a kitchen, the number of rooms and bedrooms and the availability of electricity. The list of durables includes radio, television, video player, phone, computer, fridge, washing machine, stove, water heater, motorbike and car.

In the model presented in Section 2, whether a household adjusts child labor on the intensive or extensive margin in response to the provision of cash or in-kind transfers depends on its position

[23] Filmer and Pritchett (2001) show that indices constructed using PCA starting from data on ownership of durables and house characteristics provide good proxies for household income.

Table 2: Child labor by age and household income tertiles at baseline

Age	Any work			Hours of work		
	Bottom	Middle	Top	Bottom	Middle	Top
12	0.14	0.11	0.04	4.47	2.21	0.72
	(0.35)	(0.31)	(0.19)	(13.15)	(8.13)	(4.20)
13	0.21	0.11	0.07	7.04	3.39	1.39
	(0.41)	(0.32)	(0.25)	(16.95)	(12.85)	(7.01)
14	0.28	0.14	0.12	10.65	4.32	2.92
	(0.45)	(0.35)	(0.32)	(20.27)	(13.07)	(9.72)
15	0.40	0.24	0.19	12.99	8.54	5.47
	(0.49)	(0.43)	(0.39)	(19.09)	(18.75)	(14.08)
16	0.42	0.32	0.24	17.04	13.71	8.49
	(0.50)	(0.47)	(0.43)	(23.70)	(23.18)	(18.49)

Notes: "Any work" is an indicator for the child working in the last seven days. Hours of work are equal to zero for children not working in the last seven days. "Bottom", "Medium" and "Top" denote respectively, the first, second and third tertile of the baseline income distribution as proxied by the income index constructed as reported in Section 3.3. Numbers in parentheses are standard deviations.

within the income distribution as determined by income thresholds which are functions of both exogenous variables and of model parameters. Estimating such thresholds is not feasible since, apart from some stylized simplifications within the model, the survey lacks data on some variables (specifically, child wages) on which the thresholds depend. However, if preferences for child welfare are characterized by a luxury axiom, the impact of the transfers should vary across the distribution of household income as described by the model predictions. Hence, in order to mimic the structure of the model, I classify households into tertiles of the baseline income proxy discussed above. For simplicity of exposition, throughout the paper I refer to these three subsamples as to the “Bottom” group (first tertile), “Middle” group (second tertile) and “Top” group (last tertile).

Table 2 shows how child labor differs across the income distribution by looking at the average participation in the labor market and hours of work by income groups for children of different cohorts. Unsurprisingly, two clear patterns emerge. First, child labor supply increases with age: while only 10 percent of 12 years old kids work, by the age of 16 around 32 percent are engaged in some working activity. Child labor increases not only on the extensive margin, but also on the intensive margin: the average number of working hours goes from 3 hours for 12 years old kids up to 13 hours for 16 years old children. Second, there is a strong monotonic relationship between household income and child labor, with the difference between the poorest and the richest households becoming more pronounced as children get older. For example, 41 percent of 15-16 years old children in the “Bottom” group work (with an average of 15 working hours per week) against only 21 percent of children in the “Top” group (with an average of 7 working hours per week).

3.4 Child labor regulations and education system

Child labor in Mexico is regulated by the Ley Federal de Trabajo. The law prohibits any form of employment of children below age fourteen and of children older than fourteen but younger than

sixteen who did not finish compulsory school. There are additional restrictions to employment for 14-15 years old children, including mandatory medical examinations, exclusions from working in hazardous activities, and a limit of six working hours per day. Statistics in Table 2 seem to suggest that child labor laws might not be fully enforced, but they appear to limit somehow the work participation of children younger than 14.

With respect to the Mexican education system, compulsory school, which comprises primary and secondary education, should end around age 14 although it is not uncommon for children to delay completion because of grade repetition and late enrollment. After completion of secondary school, typically children either start high school (*preparatoria* or *bachillerato*) or they enter the labor force.

Because of the institutional framework, it is expected that the impact of PAL transfers on child labor might vary with the age of the child. Whereas for 12-14 years old children at baseline there are legal restrictions to child labor, for children who are 15-16 years old at baseline (and thus 16 to 18 at follow-up) child labor laws are not binding in the follow-up period. For this reason, in the empirical analysis I estimate the impact of PAL transfers separately on 12-14 years old children at baseline and 15-16 years old at baseline.

4 Empirical strategy

In order to estimate the effect of the PAL program on child labor, I compare the difference over time in the labor supply of children in each treatment group vis-a-vis the difference over time in the labor supply of children in the control group. The empirical specification employs a difference-in-difference estimation strategy controlling for individual fixed effects. More specifically, I estimate the following model

$$O_{ijt} = \alpha + \gamma Post_t + \delta^{Cash} Cash_i \times Post_t + \delta^{Kind} Kind_i \times Post_t + \lambda' X_{it} + \theta_i + \varepsilon_{ijt}, \qquad (12)$$

where O_{ijt} is the outcome of interest for child i in village j at time t; $Post_t$ is an indicator for the follow-up survey; $Cash_i$ and $Kind_i$ are indicators equal to one if child i lives in a household that received at least one cash or in-kind transfer, respectively; X_{it} is a vector of individual and household specific controls; θ_i represents individual fixed effects. The vector of controls X_{it} includes: the age of the child; the total number of household members and the numbers of children aged 0 to 5, 6 to 11 and 12 to 18 living within the household; a dummy for the presence of an old member; an indicator for the head of the household being female; the household income index; and indicators for receiving other governmental programs, including Oportunidades. Standard errors are clustered at the village level to account for potentially correlated shocks at the locality level.

The program participation variables, $Cash_i$ and $Kind_i$, are constructed from self-reported data on the receipt of transfers and are defined as indicators equal to one if the household received at least one PAL transfer since the start of the program. Because participation into the program might be correlated with individual unobservables, $Cash_i$ and $Kind_i$ are potentially endogenous in

the estimating equation.[24] To overcome this issue, I use an instrumental variable strategy, which requires identifying two variables which are correlated with the receipt of the transfer but that can be excluded from equation (12). Since the assignment of villages into the treatment arms was random, village-level treatment dummies can be used to instrument transfer receipt. In other words, I define Z_j^{Cash} and Z_j^{Kind} to be dummy variables taking the value one if the child lives in a village randomly assigned to receive cash or in-kind transfers, respectively, and I estimate equation (12) using a two-stage-least-square within estimator.

The parameters of interest are δ^{Cash} and δ^{Kind}, which measure, respectively, the impact of cash and in-kind transfers on the relevant outcome. More specifically, the instrumental variable approach identifies the local Average Treatment Effects of providing benefits in-cash or in-kind on child labor supply and the estimated parameters are interpreted as the effects of the intervention for the subpopulation of compliers. Identification requires some additional assumptions: (i) the stable unit treatment value assumption (SUTVA); (ii) the random assignment; (iii) independence; (iv) monotonicity; (v) relevance of the instrument.

The first assumption requires that the potential outcome for one unit is not affected by the assignment to treatment of another unit. As the PAL experiment was implemented at the village level, a possible violation of the SUTVA could arise if there are village-level spillover effects. However, these effects are unlikely since experimental localities are not close to each other and, as discussed in Section 3.2, the only two contiguous villages have been eliminated from the sample. The second assumption requires that individuals in villages receiving different treatments present similar characteristics. As we have seen in Table 1, the randomization was overall successful. In addition, the inclusion of individual fixed effects allows to control for any observed and unobserved characteristics fixed over time which might be correlated with the child's time allocation. The independence assumption requires that the instrument does not directly affect the outcome or the unobserved component of the participation rule.[25]As the instrument is the random assignment, it is unlikely to affect observed outcomes rather than by changing participation in the treatment. As for the monotonicity assumption, it suggests that children in those villages randomized into the program are more likely to select into the treatment. Finally, the last assumption requires the instrument to be sufficiently strong. In Appendix *C* I discuss the results of the first stage regressions.

It is also worth emphasizing that, in the presence of general equilibrium effects of cash and in-kind transfers on local prices, δ^{Cash} and δ^{Kind} would capture both the direct effect of the transfer

[24] As already discussed in Section 3.2, it appears that about 10 percent of respondents did not receive PAL transfers. This might be due to ineligibility to the program or to imperfect compliance, with households voluntarily dropping out of the program despite being eligible.

[25] The participation rule for cash recipients can be written as follows

$$Cash_i = \begin{cases} 1 & \text{if } Cash_i^* \geq 0 \\ 0 & \text{otherwise} \end{cases},$$

where $Cash_i^* = g(Z_j^{Cash}, v_i^{Cash})$ and v_i^{Cash} are unobserved variables determining the choice of participating in the program. The independence assumption requires that $(O_{ijt}, v_i^{Cash}) \perp Z_j^{Cash}$. A similar participation rule can of course be written for the in-kind recipients.

and the indirect effects of such price changes. It has been shown that the in-kind transfer provided by the PAL program caused a fall in the residual demand of the subsidized goods which in turn led to a reduction in the prices of such commodities (Cunha et al., 2019). Therefore, for households whose income depend on the production of close substitutes of PAL goods, the fall in the prices can be interpreted as a demand shock which might in turn affect household decisions about child labor and schooling.[26] Further discussion about these mechanisms are presented in Section 5.3 in which I look for heterogeneous effects of the transfers on child labor for households involved in agricultural and non-agricultural activities.

Hypothesis testing In the model presented in Section 2, the provision of government transfers would determine: (i) a reduction in the number of hours worked for children at the bottom and in the middle of the income distribution (i.e., those to the left of ε_H); (ii) a reduction in work participation only for children in the middle income group (i.e., those between ε_L and ε_H). After classifying households into income tertiles as described in Section 3.3, I estimate equation (12) separately for the "Bottom", "Middle" and "Top" income tertiles. Testing the model predictions require that: (i) the coefficients δ^{Cash} and δ^{Kind} are negative in the hours of work equation for both the "Bottom" and "Middle" income groups; (ii) the coefficients δ^{Cash} and δ^{Kind} are negative in the work participation equation only for the "Middle" income group. Therefore, in the next section I also provide the one-sided p-values corresponding to the predicted impact of the transfers.

Finally, another parameter of interest is the differential impact of cash versus in-kind transfer, $\delta^{Cash} - \delta^{Kind}$. As discussed in Section 2, if the nominal value of the transfer in-kind was equal to the value of the cash transfer, then the impact of a transfer in-kind would be bounded above by the impact of the cash transfer, i.e. $\delta^{Cash} \geq \delta^{Kind}$. However, if the nominal value of the transfer in-kind is larger than the value of the cash transfer, as it is the case for the PAL program, then the impact of the in-kind transfer can be either larger or smaller than that of the cash transfer depending on the extent of the extra-marginality of the in-kind subsidy. For this reason, instead of testing a one-sided hypothesis on $\delta^{Cash} - \delta^{Kind}$, I only present the p-values from the two sided null hypothesis that $\delta^{Cash} - \delta^{Kind} \neq 0$.

5 Results

Section 5.1 estimates the impact of PAL on different measures of child labor and schooling for children in different age groups. Section 5.2 tests the predictions of the model presented in Section 2 by studying the heterogeneous impact of PAL on child labor across the income distribution. Section 5.3 provides evidence against alternative mechanisms that could explain the heterogeneous impacts of PAL on child labor.

[26] Child labor could adjust in one direction or another depending on whether income or substitution effects prevail (Soares et al., 2012). On one hand, child labor might increase in order to partially compensate for the reduction in household earnings from food production. On the other hand, child labor might decrease as a result of the reduction in the opportunity cost of children's time.

The results of the first stage are reported in Appendix *C*. The instruments are very strongly correlated with household participation into the program. Indeed, depending on the model, the Kleibergen-Paap statistic ranges from 241 to 1715, while the Cragg-Donald statistic is always above 230.

5.1 Overall results

Table 3 presents the estimated impact of PAL on several measures of children's time allocation. The first column considers any type of working activity in the last seven days, whereas the second and third column consider, respectively, paid and unpaid activities. In the fourth column, child labor is measured on the intensive margin (hours of work in the last seven days). Finally, the last column shows results for school attendance. Three samples of children are considered in the analysis: the full sample of 12-16 years old at baseline (Panel *A*); 12-14 years old at baseline (Panel *B*); and 15-16 years old at baseline (Panel *C*). As discussed above, the rationale for choosing these specific age groups has to do with child labor regulations in Mexico, which prohibit work participation for all children younger than 14 and restricts it for 14-15 years old children. If such restrictions are binding, we should expect no effect on the work participation of younger kids and potentially some effect for children who, in the follow-up, are above the legal working age.

This is indeed what results in Table 3 suggest. Whereas PAL does not change the time allocation of children in the 12-14 years group, the program has large and statistically significant effects for children in the 15-16 years old group. In particular, children from cash recipient households reduce their participation in paid activities by 14 percentage points as compared to children in the control group. On the intensive margin, cash recipients work about 10 hours less, a reduction of about 50% as compared to the average weekly working hours of children in the control group at follow-up. Cash recipients are also 12 percentage points more likely to attend school, and about 9 percentage points less likely to work in any type of activity, although this latter estimate is not statistically different from zero. As for in-kind recipients, all estimates have the expected sign but are smaller in magnitude and are not statistically different from zero. As a result, when testing for differential effects of cash versus in-kind transfers on children's time allocation, I find that the cash transfer reduces employment in paid activities and hours of work among children in the 15-16 years group by a significantly larger margin than the in-kind transfer (p-values equal to 0.009 and 0.001, respectively; see the second to last row in Panel *C*).

Overall, the null impacts of the program for children younger than 15 at baseline are coherent with binding child labor regulations. Instead, older children typically face higher opportunity costs from not working, and they are therefore more likely to drop out of school without progressing into high school. In this respect, the schooling of children of legal working age is more likely to satisfy the luxury axiom as compared to the schooling of younger kids. If households value child welfare of older children but could not afford to forego their child labor earnings, government transfers could allow children who would have otherwise entered the labor force to stay a few extra years

Table 3: Estimated impact of PAL on child labor and schooling by age groups

	Any work	Paid work	Unpaid work	Hours of work	School attendance
	(1)	(2)	(3)	(4)	(5)
Panel A. All children					
Cash×Post	-0.014	-0.045	0.032	-1.731	0.016
	(0.041)	(0.031)	(0.036)	(1.460)	(0.042)
Kind×Post	-0.025	-0.009	-0.007	-0.616	0.024
	(0.032)	(0.028)	(0.022)	(1.002)	(0.031)
Mean in control group at follow-up	0.343	0.255	0.087	11.627	0.610
H_0: Cash×Post=Kind×Post, p-value	0.762	0.215	0.227	0.439	0.817
Observations	5018	4942	4942	4916	5010
Panel B. 12-14 years old children					
Cash×Post	0.017	-0.003	0.025	1.894	-0.033
	(0.051)	(0.036)	(0.042)	(1.869)	(0.049)
Kind×Post	-0.026	-0.008	-0.011	-0.324	0.019
	(0.039)	(0.031)	(0.025)	(1.102)	(0.038)
Mean in control group at follow-up	0.294	0.203	0.088	8.601	0.688
H_0: Cash×Post=Kind×Post, p-value	0.310	0.893	0.313	0.204	0.172
Observations	3448	3410	3410	3402	3448
Panel C. 15-16 years old children					
Cash×Post	-0.087	-0.145**	0.049	-10.130***	0.122*
	(0.062)	(0.058)	(0.042)	(2.799)	(0.065)
Kind×Post	-0.029	-0.016	0.004	-1.764	0.040
	(0.051)	(0.052)	(0.032)	(2.341)	(0.053)
Mean in control group at follow-up	0.467	0.389	0.084	19.543	0.408
H_0: Cash×Post=Kind×Post, p-value	0.289	0.009	0.296	0.001	0.118
Observations	1570	1532	1532	1514	1562

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 is an indicator for the child working in any activity (paid or unpaid) in the last seven days. The dependent variable in columns 2 (3) is an indicator for the child working in a paid (unpaid) activity in the last seven days. The dependent variable in column 4 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. The dependent variable in column 5 is an indicator for the child currently attending school. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. Panel *A* shows results for the full sample of children. Panel *B* show results for children aged 12 to 14 at baseline; Panel *C* for children aged 15 to 16 at baseline. Standard errors are clustered at the village level and are reported in parenthesis.

in school. Because of these observations, when testing the model's prediction across the income distribution I focus on the sample of 15-16 years old children and report the results for younger children in Appendix *C*.

Before turning to the analysis by income groups, I investigate the existence of differential effects for boys and girls. I estimate equation (12) including interaction terms between the transfer receipt dummies and an indicator for the child being male, and I instrument the transfer receipt dummies and the interaction terms with the village-level treatment dummies and their interactions

Table 4: Estimated impact of PAL on child labor and schooling by gender

	Any work	Paid work	Unpaid work	Hours of work	School attendance
	(1)	(2)	(3)	(4)	(5)
Cash×Post	-0.127	-0.117	0.020	-8.598**	0.131
	(0.088)	(0.077)	(0.050)	(3.481)	(0.085)
Kind×Post	0.028	0.044	0.011	-0.218	0.022
	(0.073)	(0.071)	(0.043)	(3.230)	(0.076)
(Cash×Post)×Male	0.087	-0.050	0.065	-1.859	-0.018
	(0.130)	(0.125)	(0.085)	(5.765)	(0.110)
(Kind×Post)×Male	-0.102	-0.121	-0.003	-3.325	0.042
	(0.111)	(0.098)	(0.071)	(5.178)	(0.102)
Mean in control group at follow-up	0.467	0.389	0.084	19.543	0.408
H_0: Cash×Post=Kind×Post, p-value	0.039	0.018	0.798	0.006	0.133
H_0: (Cash×Post)×Male=(Kind×Post)×Male, p-value	0.074	0.499	0.352	0.770	0.494
Observations	1570	1532	1532	1514	1562

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 15-16 at baseline. The dependent variable in columns 1 is an indicator for the child working in any activity (paid or unpaid) in the last seven days. The dependent variable in columns 2 (3) is an indicator for the child working in a paid (unpaid) activity in the last seven days. The dependent variable in column 4 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. The dependent variable in column 5 is an indicator for the child currently attending school. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey; (Cash×Post)×Male interacts Cash×Post with an indicator for the child being male; (Kind×Post)×Male interacts Kind×Post with an indicator for the child being male. Child controls, household controls and individual fixed effecs are included. Standard errors are clustered at the village level and are reported in parenthesis.

with the male dummy.[27] As the main effect of the program is on 15-16 years old children, I estimate the model on this age group only. The results in Table 4 show that there are no differential effects of either cash or in-kind transfers on child labor and schooling as the interaction terms between the receipt of the transfers and the male dummy are not statistically significant.

5.2 Heterogeneity by household income

In order to test the model predictions, households have been classified into three tertiles based on the value of the baseline income index (see Section 3.3). In what follows, I estimate the regression model in equation (12) separately for the "Bottom", "Middle" and "Top" income groups. Mapping the model predictions to the empirical framework, we would expect households in the bottom tertile, which are more likely to be below a "subsistence threshold", to reduce child labor only on the intensive margin after receiving government transfers. Households in the middle tertile, whose

[27] In other words, I estimate the following model $O_{ijt} = \alpha + \gamma Post_t + \delta_F^{Cash} Cash_i \times Post_t + \delta_F^{Kind} Kind_i \times Post_t + \delta_M^{Cash}(Cash_i \times Post_t) \times M_i + \delta_M^{Kind}(Kind_i \times Post_t) \times M_i + \lambda' X_{it} + \theta_i + \varepsilon_{ijt}$, where M_i is an indicator for child i being male and X_{it} includes the same control variables of the original model as well as their interactions with the male dummy. Using the same notation of Section 4, the full set of instruments is given by $\left\{Z_j^{Cash}, Z_j^{Kind}, Z_j^{Cash} \times M_i, Z_j^{Kind} \times M_i\right\}$.

pre-transfer income might guarantee consumption above subsistence but not sufficiently large to forego child labor earnings entirely, are expected to reduce child labor on both the intensive and extensive margins. Finally, no change is expected for households in the top income tertile.

Table 5 shows the impact of PAL on the extensive (columns 1 to 3) and intensive margin (columns 4 to 6) of child labor. Looking first at the program effect for cash recipients, we can observe that the pattern of response to the transfer across the income distribution is in line with the model predictions. First, child labor decreased on the extensive margin only for children in the "Middle" income group. The estimated impact of the cash transfer is very large and statistically significant at the 5% level: as compared to children of similar socio-economic background that received no transfer, cash recipients are 23 percentage points less likely to work. Second, both children in the bottom and in middle income groups experience a reduction on the intensive margin of about 12 to 14 hours per week when the household receives the PAL cash transfer as compared to control group children. Despite the estimates for the bottom and middle income group are very similar in magnitude, there is no significant reduction on the extensive margin for children in the bottom income group. This result is consistent with the model prediction and, as discussed in Section 3.3, with the fact that children in this population are not typically employed in occupations with indivisible working hours. Third, consistently with the model, there is no significant change on the extensive and intensive margins of child labor for relatively richer households.

As for the transfer in-kind, the estimates suggest a reduction on child labor only for children in the middle of the income distribution. On the extensive margin, children in in-kind recipient households are 22 percentage points less likely to work as compared to children in the control group. On the intensive margin, the estimated impact of PAL amounts to a reduction of approximately 8 hours per week. Both estimates are very similar in magnitude to the estimated impact of the cash transfer, and in fact the differences are not statistically different from zero (see second to last row of Table 5). Moreover, as found for the cash transfer, also the in-kind transfer has a null effect on child labor for households at the top of the income distribution. The main difference between the two transfer modalities is found for children in the bottom income tertile. The transfer in-kind does not have any effect on the child labor supply on either the extensive or the intensive margin. On the contrary, the cash transfer causes a significant reduction in working hours, with the difference between the estimated impacts of the two transfer modalities being significant at the 1% level.

Taken together, these results suggest that the impact of the cash transfer on child labor is more homogeneous across the household's income distribution than that of the transfer in-kind. A plausible explanation for this difference is related to the regressive nature of the transfer in-kind. Tagliati (2018) shows that the PAL food basket is more likely to be extra-marginal for households with low levels of income, and that the willingness to pay for the in-kind subsidy is increasing with household socioeconomic status. Therefore, welfare gains from the basket might not be large enough for poor households to forgo the additional earnings that could be generated from child labor.

The results discussed so far show reductions in the work participation of some groups of children receiving transfers in-kind or in-cash. Therefore, it is interesting to see if these changes in work participation are mapped into an increase in the school attendance of treated children. Unfor-

Table 5: Estimated impact of PAL on child labor supply by household income

	Any work			Hours of work		
	Bottom tertile	Middle tertile	Top tertile	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)	(4)	(5)	(6)
Cash×Post	-0.014	-0.233**	-0.059	-12.370**	-14.064**	-5.209
	(0.099)	(0.105)	(0.108)	(5.277)	(4.353)	(6.148)
Kind×Post	0.117	-0.218**	0.010	3.666	-8.212**	-1.388
	(0.099)	(0.093)	(0.070)	(4.767)	(4.148)	(5.025)
Mean in control group at follow-up	0.471	0.519	0.404	21.612	19.173	16.889
H_0: Cash×Post≤0, p-value	0.444	0.013	0.292	0.010	0.001	0.198
H_0: Kind×Post≤0, p-value	0.882	0.009	0.556	0.779	0.024	0.391
H_0: Cash×Post=Kind×Post, p-value	0.129	0.872	0.484	0.001	0.104	0.348
Observations	528	528	514	498	518	498

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 15-16 at baseline. The dependent variable in columns 1 to 3 is an indicator for the child working in the last seven days. The dependent variable in columns 4 to 6 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis.

tunately, the only available information about school participation is an indicator for whether the child is currently attending school. As the survey did not distinguish between school enrollment and attendance, nor it asked about attendance in a specific time period (e.g., in the last seven days), the outcome variable is likely to capture both an increase in the enrollment rate (change over the extensive margin) and an increase in the daily attendance of children (change over the intensive margin).

The analysis presented in Table 6 shows that, for children in the middle of the income distribution, both the cash and the in-kind transfers have a large effect on school attendance: the estimated increase in school attendance is 15 percentage points for the cash group and 11 percentage points for the in-kind group (column 2). Despite the low precision of the estimates, the p-values of the one-sided t- test confirm that the estimated coefficients are statistically greater than zero. In addition, consistently with the results found in the work participation analysis, the effect for children in the lowest income tertile receiving the cash transfer is both statistically larger than zero and very close in magnitude to the effect observed for children in the "Middle" income group. On the contrary, again the in-kind transfer does not have any effect on the poorest children. These results confirm that the impact of the cash transfer is more homogeneous across the income distribution than the impact of the in-kind transfer. Indeed, the null hypothesis that cash and in-kind transfers had the same effect on school attendance for children in the bottom tertile is rejected (p-value=0.079).

Table 6: Estimated impact of PAL on school attendance by household income

	School attendance		
	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)
Cash×Post	0.173	0.146	0.087
	(0.115)	(0.113)	(0.105)
Kind×Post	0.017	0.114	0.005
	(0.099)	(0.088)	(0.084)
Mean in control group at follow-up	0.309	0.389	0.574
H_0: Cash×Post≥0, p-value	0.065	0.098	0.202
H_0: Kind×Post≥0, p-value	0.431	0.098	0.477
H_0: Cash×Post=Kind×Post, p-value	0.079	0.733	0.240
Observations	520	526	516

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 15-16 at baseline. The dependent variable is an indicator for the child currently attending school. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis.

Robustness checks Next, I test the robustness of the results presented above in several ways. First, one might be concerned that the effect of the program on child labor might be at least partially induced by parental participation into the education classes offered by the PAL program. This might occur if child labor is stigmatized and, as a result of the increased parental interaction with other individuals in the village when attending the classes, parents changed their child labor decisions. One ideal framework to test this hypothesis would require having a group of villages receiving only cash or in-kind transfers, but not the classes. As discussed in Section 3, according to the original design of the program some households should have received only the in-kind transfer without the classes but, due to confounding of the treatment, these were taught also in those villages that should have received a purely unconditional transfer. Nevertheless, this latter group of households were significantly less exposed to this component of the program (see Table B2 in Appendix *B*). Hence, in order to check how the provision of the classes might have affected the results, I replicate the analysis presented above but excluding those in-kind villages that were randomized into the classes. As now the estimates for the in-kind treatment only take into account the effect on those households that were less exposed to the classes, if these had any effect on child labor we should observe a reduction of the estimated coefficients for the in-kind treatment group.

The results in Table 7 show that the estimates change very little as compared to those presented in Table 5. While the estimates for the number of working hours are essentially the same, those for the work participation model show, if anything, a larger impact of the in-kind transfer. Overall the results in Table 7 do not support the hypothesis that the impact of the PAL program on child labor might be due to the attendance of the classes rather than to the provision of the transfers.

Second, another potential concern is the fact that some households were receiving transfers from the conditional cash transfer program Oportunidades. While one of the eligibility rules re-

Table 7: Estimated impact of PAL on child labor supply by household income, robustness check excluding in-kind villages randomized into the classes

	Any work			Hours of work		
	Bottom tertile	Middle tertile	Top tertile	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)	(4)	(5)	(6)
Cash×Post	0.003	-0.232**	-0.055	-11.008**	-14.217**	-5.305
	(0.099)	(0.107)	(0.105)	(5.175)	(4.580)	(5.956)
Kind×Post	0.071	-0.231**	-0.009	4.118	-7.575*	-1.879
	(0.127)	(0.101)	(0.078)	(6.339)	(4.580)	(5.114)
Mean in control group at follow-up	0.464	0.528	0.404	21.294	19.549	16.889
H_0: Cash×Post≤0, p-value	0.510	0.015	0.299	0.017	0.001	0.187
H_0: Kind×Post≤0, p-value	0.713	0.011	0.452	0.742	0.049	0.357
H_0: Cash×Post=Kind×Post, p-value	0.551	0.996	0.654	0.015	0.078	0.394
Observations	402	396	386	384	386	376

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The sample includes children aged 15-16 at baseline in villages receiving either: (i) cash transfer; (ii) in-kind transfers without the classes; (iii) no transfer. The dependent variable in columns 1 to 3 is an indicator for the child working in the last seven days. The dependent variable in columns 4 to 6 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis.

Table 8: Estimated impact of PAL on child labor supply by household income, robustness check excluding households receiving Oportunidades

	Any work			Hours of work		
	Bottom tertile	Middle tertile	Top tertile	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)	(4)	(5)	(6)
Cash×Post	0.015	-0.178	-0.061	-10.510*	-13.394**	-6.320
	(0.107)	(0.111)	(0.111)	(5.545)	(4.640)	(6.432)
Kind×Post	0.113	-0.209**	0.015	5.032	-8.871**	-1.885
	(0.106)	(0.101)	(0.076)	(4.878)	(4.400)	(5.262)
Mean in control group at follow-up	0.500	0.489	0.419	23.375	18.556	17.927
H_0: Cash×Post≤0, p-value	0.555	0.055	0.292	0.029	0.002	0.163
H_0: Kind×Post≤0, p-value	0.856	0.019	0.580	0.849	0.022	0.360
H_0: Cash×Post=Kind×Post, p-value	0.289	0.732	0.456	0.001	0.215	0.295
Observations	470	492	484	446	480	470

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The sample includes children aged 15-16 at baseline who do not receive Oportunidades. The dependent variable in columns 1 to 3 is an indicator for the child working in the last seven days. The dependent variable in columns 4 to 6 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis.

quired households not to be part of other major welfare programs, around 12 percent of households were receiving transfers from Oportunidades. As the Oportunidades program provides cash transfers conditional on child participation into school, if there are systematic differences between treatment and control villages in the proportions of households receiving scholarships, the estimates for child labor might be capturing the effect of Oportunidades rather than the effect of PAL. To check the robustness of the results, in Table 8 I present the estimated impact of the PAL program after excluding households receiving scholarships from Oportunidades. As the estimates are similar to those in Table 5, we can conclude that the observed changes in the work participation of children are not driven by the provision of other welfare programs.

The results are also robust to correction for multiple hypothesis testing (Romano and Wolf, 2005; Romano and Wolf, 2016), which is presented in Table D1 of Appendix *D*. Moreover, when estimating equation (12) using hours of work as the dependent variable, estimation bias might occur as a result of censoring at zero. Table D2 in Appendix *D* shows that results are robust when using a Tobit model with individual fixed effects (Honoré, 1992).

5.3 Alternative mechanisms

Throughout the paper, the proposed interpretation for the observed impact of cash and in-kind transfers on child labor across the income distribution is given by household preferences for child welfare being characterized by a luxury axiom. In this section, I investigate alternative mechanisms that could generate similar patterns and provide evidence against such mechanisms.

One possibility is that cash and in-kind transfers affect differently households working in the agricultural or in the non-agricultural sector. On one hand, both transfers can have general equilibrium effects on local food prices. In an influential paper, Cunha et al. (2019) found that there was a significant reduction in the prices of the subsidized commodities in those villages receiving the PAL food basket. This could in turn affect the local demand for child labor. For example, producers of substitutes of the transferred goods might incur lower profits from the government provision of the in-kind transfer and, as a result, they might try to compensate the profit loss with increased earnings from child labor. On the other hand, cash or in-kind transfers might also free resources which are used to buy productive assets which are substitutes for child labor in the production of food. If the probability of being engaged in food production differs across the distribution of household income, then the underlying mechanism for the heterogeneous changes in the child labor decisions might be driven by the heterogeneity of the welfare effects of PAL transfers for producer versus non-producer households rather than by preferences for child welfare.

As food production is not directly observed, I investigate this possibility by estimating equation (12) with interaction terms between the receipt of PAL transfers and an indicator for the household being engaged in agricultural activities at baseline (*Agric*). The set of instruments include the village treatment dummies and their interactions with the agricultural activity dummy. Following Cunha et al. (2019), I define the variable *Agric* to take the value one if either: (i) at least one

Table 9: Estimated impact of PAL on child labor and schooling by baseline sector of activity of the household

	Any work	Hours of work	School attendance
	(1)	(2)	(3)
Cash×Post	-0.138	-10.353	0.115
	(0.147)	(6.832)	(0.141)
Kind×Post	-0.023	0.255	0.060
	(0.097)	(4.834)	(0.090)
(Cash×Post)×*Agric*	0.070	0.303	0.012
	(0.170)	(8.190)	(0.157)
(Kind×Post)×*Agric*	0.003	-2.272	-0.020
	(0.126)	(6.468)	(0.111)
Mean in control group at follow-up	0.467	19.543	0.408
H_0: Cash×Post=Kind×Post, p-value	0.402	0.103	0.693
H_0: (Cash×Post)×*Agric*=(Kind×Post)×*Agric* p-value	0.663	0.716	0.832
Observations	1570	1514	1562

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 15-16 at baseline. The dependent variable in columns 1 is an indicator for the child working in the week prior to the survey. The dependent variable in columns 2 are working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. The dependent variable in column 3 is an indicator for the child currently attending school. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey; (Cash×Post)×*Agric* interacts Cash×Post with an indicator for the household working in agricultural activities; (Kind×Post)×*Agric* interacts Kind×Post with an indicator for the household working in agricultural activities. Child controls, household controls and individual fixed effecs are included. Standard errors are clustered at the village level and are reported in parenthesis.

household member was employed in agricultural occupations in the last year at baseline; or (ii) the household consumed some food from their own production. The results of the estimation of this extended model are reported in Table 9. As can be seen, the interaction terms are not statistically significant. This suggests that the PAL cash and in-kind transfers did not have differential effects on schooling and on the extensive and intensive margins of child labor for households which worked in the agricultural sector at baseline versus those that did not.

Another possibility is that child labor is complementary or substitute with adult labor. In such a case, the observed changes in the work participation of 15-16 years old children would be driven by the effects of PAL transfers on adult labor supply. The results reported in Skoufias et al. (2008) do not support the existence of adjustments in the labor supply of adults on average, but there might exist heterogeneous effects across the income distribution. For such a mechanism to be the driver behind the results in Section 5.2 we would expect that, across tertiles of the income distribution, the estimated impact of PAL transfers on adult labor supply has the same (opposite) sign than the estimates in Table 5 if child labor is complimentary (substitute) with adult labor.

In order to shed light on this mechanism, I estimate equation (12) for the work participation and working hours in the last seven days of the adult population for each tertile of household income. To be consistent with the results shown in Section 5.2, I focus on working age (i.e., 25-65 years old)

Table 10: Estimated impact of PAL on adult labor supply by household income tertiles

	Any work			Hours of work		
	Bottom tertile	Middle tertile	Top tertile	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Female adults						
Cash×Post	0.087	0.033	0.092	-0.425	4.188	1.799
	(0.090)	(0.104)	(0.118)	(2.561)	(3.775)	(5.819)
Kind×Post	0.068	-0.121	0.059	-0.133	-1.148	5.162
	(0.079)	(0.095)	(0.088)	(2.735)	(3.600)	(5.125)
Mean in control group at follow-up	0.234	0.400	0.250	10.453	12.950	7.314
H_0: Cash×Post=Kind×Post, p-value	0.822	0.065	0.739	0.902	0.065	0.340
Observations	510	528	534	504	526	526
Panel B. Male adults						
Cash×Post	0.021	0.034	0.103	4.472	2.120	2.926
	(0.060)	(0.063)	(0.070)	(5.239)	(5.368)	(6.132)
Kind×Post	0.033	0.089	0.061	1.465	4.032	4.051
	(0.067)	(0.059)	(0.066)	(4.347)	(4.745)	(5.433)
Mean in control group at follow-up	0.930	0.955	0.900	38.902	41.548	44.225
H_0: Cash×Post=Kind×Post, p-value	0.847	0.374	0.402	0.516	0.668	0.824
Observations	426	424	432	386	396	410

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes working age adults (25-65 years old at baseline) within households with at least one child aged 15-16 at baseline. Panel *A* reports results for female adults, Panel *B* for male adults. The dependent variable in columns 1 to 3 is an indicator for the individual working in the week prior to the survey. The dependent variable in columns 4 to 6 is the number of working hours in the week prior to the survey. Hours of work are equal to zero for individuals not working in the last seven days. (Cash)×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Individual controls, household controls and individual fixed effecs are included. Bottom, middle and top tertiles refer to the baseline income distribution as proxied by the income score discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis.

individuals within households with at least one child aged 15-16 at baseline. In Table 10, I report separately the results for female (Panel *A*) and male (Panel *B*) members of the household as child labor could be complimentary or substitute with either one or another.[28] As can been, the cash and the in-kind transfer did not affect the labor supply of adults on neither the extensive (columns 1-3) nor the intensive margin (columns 4-6). The estimated impacts are small, never statistically different from zero and appear to be rather uniform across the distribution of household income. Therefore, there is no evidence that the estimated effect of the program on child labor could be explained by complementary or substitutability of child labor with adult labor.

[28]The estimation results obtained by pooling together male and female adult members do not differ from the disaggregated ones. Also, results do not change when restricting the sample of adult household members only to the household head and the spouse.

6 Conclusions

This paper studies child labor and schooling responses to cash and in-kind transfers. In order to understand if the effectiveness of poverty alleviation programs in contrasting child labor depends on the transfer modality, I exploit the experimental design of PAL, an unconditional transfer program which randomly provided either a food basket or cash to poor households in rural Mexico. The empirical results show that the cash transfer caused statistically larger reductions in child labor than the in-kind transfer among children of high school age. Whereas the estimated impact of the in-kind transfer is not statistically different from zero, cash recipients experienced a reduction of about 10 working hours a week as compared to children who did not receive benefits from PAL.

A strong focus of the paper is the heterogeneity of the program impact across the distribution of household income. Both the in-kind and the cash transfer reduced the work participation of children in the middle tertile of the income distribution by an estimated 22 percentage points as compared to non-recipients of similar socioeconomic status. None of the transfers affected child labor on the extensive margin for children in the bottom tertile. However, for this income group large reductions on the intensive margin are found for cash recipients but not for in-kind recipients. The differential effect of the two transfer schemes for very poor households can be explained by the regressivity of the PAL in-kind transfer (Tagliati, 2018). Moreover, the different margins of adjustment of child labor across the income distribution can be rationalized by a simple theoretical model in which household preferences for schooling are characterized by a luxury axiom (Basu and Van, 1998). In such a case, households whose initial income is below subsistence reduce child labor only on the intensive margin when receiving government transfers, while households above subsistence respond by decreasing child labor on both the intensive and extensive margins.

These results have important implications for policy analysis. First, both cash and in-kind transfers can be effective means to reduce child labor. However, cash transfers seem to be associated with larger and more evenly distributed reductions in child labor. The extent to which a transfer in-kind might reach the same objective as a cash transfer crucially depends on the extra-marginality of the transfer and on the distribution of the welfare gains across the income distribution. Second, the PAL program affected child time allocation exclusively among children above the legal working age, for which the opportunity cost of schooling is higher. If policy makers are interested in increasing enrollment into non-compulsory education, transfer programs targeted to poor households with children above the legal working age might have potentially large effects. Finally, the results in the paper are consistent with preferences for child welfare being characterized by a luxury axiom. In such a case, transfer programs whose size is sufficiently large to move household consumption from below to above subsistence might effectively reduce child labor even though the size of the transfer does not compensate entirely for the foregone child labor earnings.

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Appendix (for online publication)

Appendix *A*: Theoretical results

Model solution

In this appendix I present the solution of the model described in Section 2. The household problem is given by

$$\max_{c,z,s} [c^{\sigma} + z^{\sigma}]^{\frac{\gamma}{\sigma}} + \alpha s \qquad s.t$$
$$p_c c + p_z z \leq w(T-s) + \bar{y} + \varepsilon.$$

To simplify the analysis, it is convenient to exploit separability between s and other goods and solve the maximization problem into two steps. In the first step, I maximize the household's utility over c and z, taking the choice of s as given. Let $m \equiv w(T-s) + \bar{y} + \varepsilon$ for a given choice of s. The Lagrangian function can be written in the following way

$$L(c,z,\lambda) = [c^{\sigma} + z^{\sigma}]^{\frac{\gamma}{\sigma}} + \lambda(m - p_c c - p_z z)$$

where λ denotes the Lagrangian multiplier. Taking the first order conditions with respect to c and z gives

$$\frac{\gamma}{\sigma}[c^{\sigma} + z^{\sigma}]^{\frac{\gamma}{\sigma}-1} \sigma c^{\sigma-1} = \lambda p_c \tag{A1}$$

$$\frac{\gamma}{\sigma}[c^{\sigma} + z^{\sigma}]^{\frac{\gamma}{\sigma}-1} \sigma z^{\sigma-1} = \lambda p_z. \tag{A2}$$

Plugging (A1) into (A2) we get $c = \left(\frac{p_c}{p_z}\right)^{\frac{1}{\sigma-1}} z$. Defining $\theta \equiv \left(\frac{p_c}{p_z}\right)^{\frac{1}{\sigma-1}}$ and using the budget constraint $m = p_c c + p_z z$ we obtain

$$z(m) = \frac{m}{p_c\theta + p_z}, \tag{A3}$$

$$c(m) = \frac{\theta m}{p_c\theta + p_z}. \tag{A4}$$

In the second step, we plug (A3) and (A4) into the household's utility and maximize the following problem over s and m

$$\max_{s,m} \left[\left(\frac{\theta m}{p_c\theta + p_z}\right)^{\sigma} + \left(\frac{m}{p_c\theta + p_z}\right)^{\sigma}\right]^{\frac{\gamma}{\sigma}} + \alpha s \qquad s.t$$
$$m = w(T-s) + \bar{y} + \varepsilon.$$

Defining $\pi \equiv \left(\frac{\theta^{\sigma}+1}{(p_c\theta+p_z)^{\sigma}}\right)^{\frac{\gamma}{\sigma}}$, the Lagrangian for this problem can be written as

$$L(s,m,\mu) = \pi m^{\gamma} + \alpha s + \mu\left[w(T-s)+\bar{y}+\varepsilon-m\right],$$

where μ denotes the Lagrangian multiplier. Taking the first order conditions with respect to m and s gives

$$\pi\gamma m^{\gamma-1} = \mu \tag{A5}$$

$$\begin{matrix} > \\ \alpha \ = \ \mu w \\ < \end{matrix} \tag{A6}$$

Plugging (A5) into (A6) we get $\alpha \begin{matrix} \leq \\ = \\ > \end{matrix} \pi\gamma m^{\gamma-1} w$. Below I consider these three cases separately.

Case 1: $\alpha < \pi\gamma m^{\gamma-1} w$ Under this inequality, the marginal utility of schooling is lower than the marginal cost of foregone child labor earnings. Hence we have $h = T$, $s = 0$ and the budget constraint is given by $m = wT + \bar{y} + \varepsilon$. Plugging m into the inequality gives

$$\varepsilon < \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - wT - \bar{y} \equiv \varepsilon_L.$$

Case 2: $\alpha > \pi\gamma m^{\gamma-1} w$ Under this inequality, the marginal utility of schooling is higher than the marginal cost of foregone child labor earnings. Hence $h = 0$, $s = T$ and the budget constraint is given by $m = \bar{y} + \varepsilon$. Plugging m into the inequality we have

$$\varepsilon > \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - \bar{y} \equiv \varepsilon_H.$$

Case 3: $\alpha = \pi\gamma m^{\gamma-1} w$ This case is characterized by $h > 0$ and $s > 0$. The budget constraint is given by $m = wh + \bar{y} + \varepsilon$, so that we can find $h = \frac{1}{w}\left[\left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - \bar{y} - \varepsilon\right]$ or, alternatively, $\varepsilon = \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - wh - \bar{y}$. Since $0 \leq h \leq T$, we have a range of values of ε such that the child works and goes to school. In particular, $h > 0$ and $s > 0$ for

$$\varepsilon_L = \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - wT - \bar{y} \leq \varepsilon \leq \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - \bar{y} = \varepsilon_H.$$

Effects of a transfer in-kind

Next, I show how the threshold ε_L changes when the price of good z changes. The analysis for the threshold ε_H is identical. First, remember that the threshold is defined as $\varepsilon_L \equiv \left(\frac{\alpha}{\pi\gamma}\frac{1}{w}\right)^{\frac{1}{\gamma-1}} - wT - \bar{y}$, where π is a function of prices and model parameters given by

$$\pi = \left[\frac{\theta^{\sigma}+1}{(p_c\theta+p_z)^{\sigma}}\right]^{\frac{\gamma}{\sigma}} = \left[\frac{\left(\frac{p_c}{p_z}\right)^{\frac{\sigma}{\sigma-1}}+1}{\left(p_c\left(\frac{p_c}{p_z}\right)^{\frac{1}{\sigma-1}}+p_z\right)^{\sigma}}\right]^{\frac{\gamma}{\sigma}} = \left[p_c^{\frac{\sigma}{\sigma-1}}+p_z^{\frac{\sigma}{\sigma-1}}\right]^{\frac{\gamma(1-\sigma)}{\sigma}}.$$

To study whether a change in p_z determines a positive or negative shift in ε_L, I differentiate ε_L partially with respect to p_z , that is

$$\frac{\partial \varepsilon_L}{\partial p_z} = \left[\frac{\alpha}{\gamma}\frac{1}{w}\right]^{\frac{1}{\gamma-1}} \frac{\pi^{\frac{\gamma}{1-\gamma}}}{(1-\gamma)}\frac{\partial \pi}{\partial p_z}. \tag{A7}$$

Given that $\alpha > 0$, $0 < \gamma \leq 1$ and $\sigma < 1$, it is immediate that $\pi \geq 0$ and that the right hand side of (A7) has the same sign of $\frac{\partial \pi}{\partial p_z}$. Hence, differentiating π partially with respect to p_z gives

$$\begin{aligned}\frac{\partial \pi}{\partial p_z} &= \frac{\gamma(1-\sigma)}{\sigma}\left[p_c^{\frac{\sigma}{\sigma-1}}+p_z^{\frac{\sigma}{\sigma-1}}\right]^{\frac{\gamma(1-\sigma)}{\sigma}-1}\frac{\sigma}{\sigma-1}p_z^{\frac{1}{\sigma-1}} \\ &= -\gamma\left[p_c^{\frac{\sigma}{\sigma-1}}+p_z^{\frac{\sigma}{\sigma-1}}\right]^{\frac{\gamma(1-\sigma)}{\sigma}-1} p_z^{\frac{1}{\sigma-1}} < 0\end{aligned}$$

Given $0 < \gamma \leq 1$, it must be that $\frac{\partial \pi}{\partial p_z} < 0$.

Appendix *B*: PAL program and additional data analysis

Food basket

The list of goods and the quantities contained in the PAL box are reported in Table B1. To get a sense of the extent of the extra-marginality of the in-kind transfer, the second column of the table reports, for each commodity, the percentage of cash recipient households that consumed less than the subsidized quantity in the follow-up period.[1] As one can see, the basket combines commodities widely consumed by Mexican households (rice, beans, vegetable oil) and others which are consumed infrequently (pasta soup, cookies) or very rarely (breakfast cereals, corn flour, lentils, canned fish, powdered milk). The last two columns of Table B1 report the mean and standard deviation of the value of each commodity in the food basket. Summing across all items, the aver-

[1] As discussed in Section 2.1, the extra-marginality of an in-kind transfer is determined by looking at the counterfactual consumption of the subsidized goods under an equal-value cash transfer. Although the PAL cash transfer was less than an equal-value cash transfer, looking at consumption of cash recipients is more informative than pre-program consumption or post-program consumption of the control group.

Table B1: PAL food commodities

Commodity	Amount of the transfer (kg)	Percentage of cash recipients with consumption lower than the amount of the transfer	Baseline average value of the transfer (pesos)	Baseline SD of the value of the transfer (pesos)
Beans	2	0.08	20.85	3.64
Vegetable oil	1 (lt)	0.09	10.47	0.93
Rice	2	0.26	13.03	4.50
Pasta soup	1.2	0.55	16.23	2.21
Cookies	1	0.55	18.72	5.03
Canned fish	0.6	0.77	16.31	6.04
Corn flour	3	0.80	15.95	8.03
Lentils	1	0.87	10.80	6.20
Breakfast cereals	0.2	0.89	7.37	3.26
Powdered milk	1.92	0.90	75.45	60.25
Total			205.2	64.03

Notes: The table is taken from Tagliati (2018). Calculations in column 2 are based on self-reported post-program consumption for households in the cash treatment group. Calculations in columns 3 and 4 use the pre-program median unit value in a village and are based on 197 sample villages.

age value of the in-kind transfer (approximately 205 pesos, or US$ 18) is larger than the value of the cash transfer (150 pesos, or US$13). The variability in the value of the PAL box reflects the variation in the prices of PAL commodities across villages.

Program take-up and contamination of the conditionality requirement

In this section, I document the take-up of the program and the extent of contamination of the conditionality requirement using self-reported data on the receipt of the transfers.[2] All households were asked if they received any transfer from the PAL program, the periodicity of the delivery and the number of benefits they received. Moreover, conditional on having received at least one transfer, households were asked about their attendance to classes, the total number of classes attended and the topics covered among four possibilities: health, nutrition, hygiene, other topics.[3]

The first column of Table B2 shows that the percentage of households receiving at least one transfer was very high for all the three treatment types (i.e. cash plus classes - denoted with CC; in-kind plus classes - denoted with KC; and in-kind without classes - denoted with K). However, as reported in the bottom rows of the table, program take-up was significantly higher for the in-kind plus classes sample than for the cash sample. Take-up among households receiving the cash treatment is around 88%, while it is above 92% for households in in-kind villages. In-kind recipients also received more transfers, although the difference with respect to cash recipients is not statis-

[2]The analysis in this section replicates a similar analysis of the take-up and contamination of the PAL program which appears in Appendix *A* of Cunha (2014).

[3]One additional category refers to classes about the organization of the PAL program. However, since attendance to this type of classes was a mandatory requirements for all experimental villages, irrespective of whether they were randomized-in or out of the education component, I exclude them from the computation of class attendance.

tically different from zero. On average, households in the in-kind treatment arm received thirteen transfers since the start of the program, while households in the cash treatment arm received on average twelve (see column 2).[4]

Table B2: Program take-up and contamination

	At least one transfer	Number of transfers	At least one class	Number of classes
Cash+Classes (CC)	0.881	12.329	0.720	4.444
	(0.023)	(0.586)	(0.057)	(0.524)
Kind+Classes (KC)	0.937	13.388	0.852	4.865
	(0.016)	(0.444)	(0.028)	(0.371)
Kind (K)	0.919	13.115	0.712	4.225
	(0.028)	(0.318)	(0.046)	(0.524)
H0: CC = KC, p-value	0.045	0.152	0.039	0.512
H0: CC = K, p-value	0.293	0.241	0.914	0.768
H0: KC = K, p-value	0.589	0.618	0.010	0.320

Notes: Data are from the household survey and are self-reported. Standard errors are reported in parenthesis and are clustered at the village level.

The next columns show the extent of contamination of the conditionality requirement. Column 3 reports the percentage of households attending at least one class. Column 4 shows the average number of classes attended. A few comments are in order. First, 71% of households in the "in-kind without classes" group attended at least one session, suggesting that the treatment was indeed confounded. While this percentage is significantly higher for households in the "in-kind plus classes" group (around 85%), it is not significantly different from that of cash recipients, despite the fact that for the latter group class attendance was a mandatory requirement of the program. Second, the program rules envisaged compulsory attendance to monthly classes. However, the average number of sessions attended was very low, being around four in all treatment groups. The fact that the average number of benefits received was significantly higher than the number of classes attended suggests that, in practice, the conditionality requirement was not enforced.

Attrition

This section shows some descriptive statistics about attrition in the sample. Table B3 shows the attrition rates at the household and at the individual level by treatment group. While household attrition was around 15 percent in the control group, it was significantly lower for households in the cash and in-kind treatment groups, and approximately equal to 10 percent. Attrition at the child level is about 23 percent in the control and in the cash treatment arms, and about 19 percent in the in-kind treatment arm. There are no statistically significant differences in the attrition rates of the cash versus in-kind treatment groups (as reported at the bottom of Table B3, where I test for differential attrition between one treatment group and another).

[4]The variability in the number of transfers received is due to the different timing of implementation of the program, with the program reaching full coverage of eligible villages within a year.

Table B3: Attrition rates by treatment group

	Households	Full sample	Children 12-16 years old
Cash	-0.039**	-0.029	-0.013
	(0.019)	(0.018)	(0.027)
Kind	-0.045**	-0.043**	-0.045*
	(0.018)	(0.016)	(0.025)
Control	0.146***	0.199***	0.230***
	(0.015)	(0.013)	(0.020)
H0: Cash=Kind, p value	0.669	0.348	0.191
Observations	6625	30362	3683

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The table shows attrition rates at the household (column 1), at the individual (column 2) and at the child level (column 3) by treatment group. Standard errors are reported in parentheses and are clustered at the village level.

Appendix *C*: Additional results

First stage regressions

Table C1 shows the first stage regressions for the model estimated in Table 3. The estimation sample in Table 3 slightly varies depending on the missing values for the chosen dependent variable. Therefore, I report only the first stage corresponding to the model in column (1) of Table 3, in which the dependent variable is an indicator for the child working in the last seven days. Table C1 presents the first stage for the full sample of children (columns 1-2), the sample of 12-14 years old at baseline (columns 3-4), and 15-16 years old at baseline (columns 5-6).

Table C2 shows the first stage for the model estimated in Table 5. As before, I report only the results for the model in which the dependent variable is an indicator for the child working in the last seven days, corresponding to the model estimated in columns (1) to (3) in Table 5.

Table C1: First stage regressions by age groups

	All children		12-14 years old children		15-16 years old children	
	Received Cash	Received Kind	Received Cash	Received Kind	Received Cash	Received Kind
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument: Cash Village×Post	0.884***	-0.001	0.871***	-0.001	0.911***	-0.003
	(0.023)	(0.003)	(0.025)	(0.002)	(0.033)	(0.004)
Instrument: Kind Village×Post	-0.001	0.926***	-0.002	0.931***	-0.000	0.917***
	(0.002)	(0.018)	(0.002)	(0.018)	(0.003)	(0.021)
Post	0.008	0.018	0.016	0.013	-0.011	0.030
	(0.011)	(0.012)	(0.014)	(0.016)	(0.017)	(0.023)
Income Index	0.008**	-0.004	0.008**	-0.003	0.009	-0.006
	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)	(0.006)
Age	-0.004	-0.009	-0.009	-0.006	0.008	-0.017
	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.012)
Number of household members	0.011	-0.005	0.016**	-0.006	-0.001	-0.003
	(0.007)	(0.005)	(0.007)	(0.006)	(0.010)	(0.009)
Number of 0-5 children	-0.009	0.011	-0.018*	0.017	0.010	-0.003
	(0.010)	(0.009)	(0.010)	(0.011)	(0.016)	(0.014)
Number of 6-11 children	-0.010	0.013	-0.018**	0.017*	0.010	0.001
	(0.010)	(0.010)	(0.009)	(0.010)	(0.019)	(0.015)
Number of 12-17 children	-0.011**	0.003	-0.015**	0.006	0.003	-0.006
	(0.005)	(0.009)	(0.005)	(0.010)	(0.006)	(0.011)
Old member in the household	-0.017	0.034**	-0.018	0.032**	-0.013	0.031
	(0.016)	(0.014)	(0.016)	(0.013)	(0.021)	(0.034)
Head female	0.015	0.015	0.018	0.020	0.010	0.006
	(0.019)	(0.013)	(0.027)	(0.016)	(0.009)	(0.030)
Received Oportunidades	-0.015	-0.045**	-0.012	-0.038**	-0.022	-0.059
	(0.018)	(0.022)	(0.019)	(0.019)	(0.035)	(0.041)
Received Liconsa	-0.018*	0.013	-0.018	0.001	-0.020	0.039
	(0.010)	(0.017)	(0.013)	(0.017)	(0.018)	(0.031)
Received other welfare program	0.002	0.008	0.002	0.005	0.003	0.014
	(0.005)	(0.007)	(0.006)	(0.007)	(0.005)	(0.011)
Cragg-Donald Wald F statistic	2993.7		2048.5		899.0	
Kleibergen-Paap Wald F statistic	1714.8		1360.6		1182.1	
Joint F statistic for the instruments	726.6	1420.6	645.2	1414.3	427.8	1063.7
Observations	5018	5018	3448	3448	1570	1570

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. First stage regressions for the model estimated in column (1) of Table 3. Dependent variables are indicators for the household receiving at least one cash transfer (columns 1, 3 and 5) or at least one in-kind transfer (columns 2, 4 and 6). Instruments are indicators equal to 1 if the household lives in a village which was randomly assigned to receive transfers in-cash or in-kind. Standard errors are reported in parentheses and are clustered at the village level.

Table C2: First stage regressions by household income tertiles

	Bottom tertile		Middle tertile		Top tertile	
	Received Cash	Received Kind	Received Cash	Received Kind	Received Cash	Received Kind
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument: Cash Village×Post	0.907***	-0.026*	0.939***	-0.022	0.912***	0.006
	(0.040)	(0.016)	(0.037)	(0.015)	(0.049)	(0.007)
Instrument: Kind Village×Post	0.005	0.864***	0.002	0.888***	0.000	0.983***
	(0.006)	(0.040)	(0.003)	(0.032)	(0.009)	(0.010)
Post	-0.036	0.064	-0.014	0.007	-0.006	0.026
	(0.032)	(0.040)	(0.016)	(0.042)	(0.029)	(0.038)
Income Index	0.008	-0.017*	0.003	0.025	0.011	-0.004
	(0.009)	(0.010)	(0.007)	(0.016)	(0.011)	(0.005)
Age	0.010	-0.029	0.007	0.006	0.012	-0.019
	(0.011)	(0.021)	(0.010)	(0.023)	(0.015)	(0.022)
Number of household members	0.020	-0.016	-0.000	0.014	-0.024	0.000
	(0.017)	(0.018)	(0.004)	(0.020)	(0.025)	(0.004)
Number of 0-5 children	-0.025	0.002	-0.009	0.011	0.084*	-0.014
	(0.024)	(0.024)	(0.008)	(0.033)	(0.050)	(0.010)
Number of 6-11 children	-0.018	0.001	-0.010	-0.019	0.061	-0.003
	(0.018)	(0.023)	(0.008)	(0.030)	(0.056)	(0.007)
Number of 12-17 children	-0.004	-0.027	0.007	0.021	0.007	-0.001
	(0.007)	(0.023)	(0.011)	(0.023)	(0.014)	(0.004)
Old member in the household	0.028	0.038	0.014	0.038	-0.053	0.003
	(0.030)	(0.030)	(0.012)	(0.130)	(0.076)	(0.012)
Head female	0.040	0.007	0.001	-0.080	-0.017	0.038
	(0.032)	(0.031)	(0.009)	(0.080)	(0.019)	(0.035)
Received Oportunidades	0.017	-0.063	-0.002	-0.043	-0.162	-0.087
	(0.020)	(0.045)	(0.008)	(0.065)	(0.121)	(0.080)
Received Liconsa	0.004	0.084**	-0.002	-0.088	-0.065	0.124*
	(0.010)	(0.033)	(0.007)	(0.064)	(0.041)	(0.073)
Received other welfare program	0.002	0.038	0.011	0.018	-0.020	0.001
	(0.013)	(0.023)	(0.010)	(0.022)	(0.013)	(0.008)
Cragg-Donald Wald F statistic	254.8		230.6		429.1	
Kleibergen-Paap Wald F statistic	347.5		453.5		241.4	
Joint F statistic for the instruments	308.9	293.3	322.9	610.6	170.9	4668.6
Observations	528	528	528	528	514	514

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 15-16 at baseline. First stage regressions for the model estimated in columns (1)-(3) of Table 5. Dependent variables are indicators for the household receiving at least one cash transfer (columns 1, 3 and 5) or at least one in-kind transfer (columns 2, 4 and 6). Instruments are indicators equal to 1 if the household lives in a village which was randomly assigned to receive transfers in-cash or in-kind. Standard errors are reported in parentheses and are clustered at the village level.

Empirical results for 12-14 years old children

Table C3 is analogous to Table 4 in the paper and reports the estimated impact of the program on 12-14 years old children's time allocation by gender of the child. Table C4 is analogous to Table 5 in the paper and reports the estimated impact of the program on work participation and weekly working hours by tertiles of the household income distribution for 12-14 years old children. Table C5 is analogous to Table 6 in the paper and reports the estimated impact of the program on school attendance by income tertiles for 12-14 years old children. Table C6 is analogous to Table 9 in the paper and reports the estimated impact of the program on work participation and hours of work of 12-14 years old children for agricultural versus non-agricultural households.

Table C3: Estimated impact of PAL on child labor and schooling by gender, 12-14 years old children at baseline

	Any work	Paid work	Unpaid work	Hours of work	School attendance
	(1)	(2)	(3)	(4)	(5)
Cash×Post	0.102**	0.063	0.021	0.842	0.014
	(0.050)	(0.038)	(0.042)	(1.777)	(0.068)
Kind×Post	0.002	0.021	-0.033	-0.639	0.047
	(0.039)	(0.029)	(0.026)	(1.432)	(0.055)
(Cash×Post)×Male	-0.133*	-0.096	0.004	2.475	-0.080
	(0.080)	(0.063)	(0.048)	(3.048)	(0.064)
(Kind×Post)×Male	-0.035	-0.037	0.039	1.026	-0.048
	(0.065)	(0.051)	(0.039)	(2.294)	(0.051)
Mean in control group at follow-up	0.294	0.203	0.088	8.601	0.688
H_0: Cash×Post=Kind×Post, p-value	0.023	0.269	0.132	0.314	0.556
H_0: (Cash×Post)×Male=(Kind×Post)×Male, p-value	0.206	0.286	0.473	0.615	0.598
Observations	3448	3410	3410	3402	3448

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 12-14 at baseline. The dependent variable in columns 1 is an indicator for the child working in any activity (paid or unpaid) in the last seven days. The dependent variable in columns 2 (3) is an indicator for the child working in a paid (unpaid) activity in the last seven days. The dependent variable in column 4 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. The dependent variable in column 5 is an indicator for the child currently attending school. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey; (Cash×Post)×Male interacts Cash×Post with an indicator for the child being male; (Kind×Post)×Male interacts Kind×Post with an indicator for the child being male. Child controls, household controls and individual fixed effecs are included. Standard errors are clustered at the village level and are reported in parenthesis.

Table C4: Estimated impact of PAL on child labor supply by household income 12-14 years old children at baseline

	Any work			Hours of work		
	Bottom tertile	Middle tertile	Top tertile	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)	(4)	(5)	(6)
Cash×Post	0.080	-0.038	0.054	5.340	1.467	1.018
	(0.075)	(0.084)	(0.074)	(4.619)	(2.412)	(1.976)
Kind×Post	-0.003	-0.109	0.045	-1.155	-2.143	3.002*
	(0.053)	(0.068)	(0.063)	(2.215)	(2.158)	(1.723)
Mean in control group at follow-up	0.347	0.353	0.158	11.440	10.073	3.375
H_0: Cash×Post≤0, p-value	0.859	0.323	0.768	0.876	0.728	0.697
H_0: Kind×Post≤0, p-value	0.481	0.056	0.764	0.301	0.160	0.959
H_0: Cash×Post=Kind×Post, p-value	0.234	0.282	0.852	0.125	0.051	0.220
Observations	1186	1158	1104	1160	1144	1098

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 12-14 at baseline. The dependent variable in columns 1 to 3 is an indicator for the child working in the last seven days. The dependent variable in columns 4 to 6 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis.

Table C5: Estimated impact of PAL on school attendance by household income, 12-14 years old children at baseline

	School attendance		
	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)
Cash×Post	-0.152*	0.059	-0.061
	(0.080)	(0.075)	(0.058)
Kind×Post	0.003	0.075	-0.032
	(0.057)	(0.065)	(0.044)
Mean in control group at follow-up	0.571	0.662	0.867
H_0: Cash×Post≥0, p-value	0.970	0.216	0.855
H_0: Kind×Post≥0, p-value	0.477	0.123	0.767
H_0: Cash×Post=Kind×Post, p-value	0.037	0.754	0.570
Observations	1184	1156	1108

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 12-14 at baseline. The dependent variable is an indicator for the child currently attending school. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis.

Table C6: Estimated impact of PAL on child labor and schooling by baseline sector of activity of the household, 12-14 years old children

	Any work	Hours of work	School attendance
	(1)	(2)	(3)
Cash×Post	0.110	5.701	-0.146*
	(0.094)	(3.768)	(0.083)
Kind×Post	-0.057	-1.020	0.007
	(0.060)	(1.987)	(0.051)
(Cash×Post)×*Agric*	-0.111	-4.450	0.143
	(0.108)	(4.267)	(0.088)
(Kind×Post)×*Agric*	0.036	0.808	0.021
	(0.078)	(2.619)	(0.057)
Mean in control group at follow-up	0.294	8.601	0.688
H_0: Cash×Post=Kind×Post, p-value	0.035	0.060	0.054
H_0: (Cash×Post)×*Agric*=(Kind×Post)×*Agric* p-value	0.091	0.167	0.133
Observations	3446	3400	3446

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 12-14 at baseline. The dependent variable in columns 1 is an indicator for the child working in the week prior to the survey. The dependent variable in columns 2 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. The dependent variable in column 3 is an indicator for the child currently attending school. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey; (Cash×Post)×*Agric* interacts Cash×Post with an indicator for the household working in agricultural activities; (Kind×Post)×*Agric* interacts Kind×Post with an indicator for the household working in agricultural activities. Child controls, household controls and individual fixed effecs are included. Standard errors are clustered at the village level and are reported in parenthesis.

Appendix *D*: Additional robustness checks

Table D1 investigates the robustness of the results in Table 5 to correction for multiple hypothesis testing, using the procedure described by Romano and Wolf (2005, 2016). Since the results in Table 5 consider multiple outcomes (work participation and hours of work) and multiple treatments (cash versus in-kind), a total of twelve null hypothesis have been tested. In Table D1, I report the point estimates and standard errors from Table 5, as well as the unadjusted and adjusted p-values (within the curly brackets). As can be seen, the estimated coefficients for "Middle" and "Bottom" income households are still significant even after correcting the p-values for multiple hypothesis testing.

Next, I check for potential censoring bias in the estimating equation for hours of work. In the empirical analysis discussed in the paper, working hours have been coded as zero if the child was not working. It is well known that estimation of models with censored dependent variables using linear techniques might lead to estimation bias. In order to control for this issue, I use the Tobit model with fixed effects in panel data developed by Honoré (1992). In other words, I estimate the following equation

$$H^*_{ijt} = \zeta + \eta Post_t + \beta^{Cash} Z^{Cash}_j \times Post_t + \beta^{Kind} Z^{Kind}_j \times Post_t + \mu' X_{it} + \pi_i + \varepsilon_{ijt}, \qquad \text{(D1)}$$

where H^*_{ijt} is the latent variable for the number of working hours for child i in village j at time t; $Post_t$ is an indicator for the follow-up survey; Z_j^{Cash} and Z_j^{Kind} are dummy variables taking the value one if the child lives in a village receiving cash or in-kind transfers; X_{it} is a vector of individual and household specific controls; π_i represents individual fixed effects. Given that observations are censored, we observe $\{(H_{ijt}, Z_j^{Cash}, Z_j^{Kind}, X_{it}) : t = 1,2; i = 1,...,N; j = 1,...,J\}$ where $H_{ijt} = max\{H^*_{ijt}, 0\}$. This specification is similar to the one in equation (12) but with one difference. Since the properties of Honoré (1992)'s estimator with endogenous regressors have not been studied, I replace the variables for household participation in the program, $Cash_i$ and $Kind_i$, with the village-level treatment dummies previously used as instruments. Table D2 shows that the estimated treatment coefficients for households in the middle of the income distribution, and for cash recipients in the bottom of the income distribution, are still strongly significant even after controlling for censoring.

Table D1: Estimated impact of PAL on child labor supply by household income, p-values adjusted for multiple hypothesis testing

	Any work			Hours of work		
	Bottom tertile	Middle tertile	Top tertile	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)	(4)	(5)	(6)
Cash×Post	-0.014	-0.233**	-0.059	-12.370**	-14.064**	-5.209
	(0.099)	(0.105)	(0.108)	(5.277)	(4.353)	(6.148)
{Unadjusted p-value; adjusted p-value}	{0.889; 0.983}	{0.026; 0.074}	{0.583; 0.911}	{0.019; 0.059}	{0.001; 0.005}	{0.397; 0.794}
Kind×Post	0.117	-0.218**	0.010	3.666	-8.212**	-1.388
	(0.099)	(0.093)	(0.070)	(4.767)	(4.148)	(5.025)
{Unadjusted p-value; adjusted p-value}	{0.236; 0.558}	{0.019; 0.059}	{0.887; 0.983}	{0.442; 0.832}	{0.048; 0.132}	{0.782; 0.971}
H_0: Cash×Post≤0, p value	{0.444; 0.785}	{0.013; 0.029}	{0.292; 0.677}	{0.010; 0.017}	{0.001; 0.002}	{0.198; 0.506}
H_0: Kind×Post≤0, p value	{0.882; 0.936}	{0.009; 0.017}	{0.556; 0.879}	{0.779; 0.911}	{0.024; 0.063}	{0.391; 0.769}
Observations	528	528	514	498	518	498

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The sample includes children aged 15-16 at baseline. The dependent variable in columns 1 to 3 is an indicator for the child working in the last seven days. The dependent variable in columns 4 to 6 is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. Cash×Post denotes an indicator for receiving the cash treatment in the post-intervention survey; Kind×Post denotes an indicator for receiving the in-kind treatment in the post-intervention survey. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3. Standard errors are clustered at the village level and are reported in parenthesis. Curly brackets report on the left unadjasted p-values; and on the right p-values corrected for multiple hypothesis testing as described in Romano and Wolf (2005, 2016).

Table D2: Estimated impact of PAL on hours of work by household income, selection model

	Hours of work		
	Bottom tertile	Middle tertile	Top tertile
	(1)	(2)	(3)
Cash Village×Post	-21.921**	-30.559**	-15.736
	(11.010)	(12.173)	(20.917)
Kind Village×Post	3.843	-21.164*	3.645
	(8.404)	(12.218)	(22.651)
H_0: Cash Village×Post≤0, p-value	0.023	0.006	0.226
H_0: Kind Village×Post≤0, p-value	0.676	0.042	0.564
H_0: Cash Village×Post=Kind Village×Post, p-value	0.003	0.307	0.129
Observations	514	530	517

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample includes children aged 15-16 at baseline. The dependent variable is the number of working hours in the last seven days. Hours of work are equal to zero for children not working in the last seven days. Cash Village×Post is the interaction between a dummy for the household living in a village in the cash treatment arm and a dummy for the post-intervention period; Kind Village×Post is the interaction between a dummy for the household living in a village in the in-kind treatment arm and a dummy for the post-intervention period. Child controls, household controls and individual fixed effecs are included. "Bottom", "middle" and "top" refer to the tertiles of the baseline income distribution as proxied by the income index discussed in Section 3.3.

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