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Abstract[1]

This paper examines the efficacy of three conditional cash transfer (CCT) programs in Honduras, Mexico, and Nicaragua in mitigating the potential negative effects of an income shock caused by falling prices of coffee, an important cash crop to many CCT participants. A theoretical household model is developed that demonstrates both the positive potential of CCTs to mitigate negative shocks effects on early childhood development and the negative potential of CCTs to exacerbate the impacts of a negative shock to early childhood development if the conditionality encourages households to shift resources from younger to older children to sustain their school attendance. The experimental design includes both CCT and non-CCT households and communities with and without coffee production. The paper finds that in Mexico the CCT mitigated the negative shock on child height-for-age z-scores, while in Nicaragua coffee-producing households who participated in CCTs saw greater declines in z-scores. Findings for Honduras are largely inconclusive.

JEL Classification: H43, I12, I38, O15.
Keywords: Mexico, Nicaragua, Honduras, Conditional cash transfer, Early childhood development, Shocks

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1. Introduction

Conditional Cash Transfer (CCT) programs provide cash payments to mothers, the size of which is conditional on their household's utilization of health services and children's school attendance. CCTs have multiple goals that appear quite complementary, such as increasing school-age children's school attainment, decreasing poverty, enhancing women's autonomy and decision-making capacity, and improving early childhood development. While these programs have been shown to be successful in decreasing poverty, increasing consumption, and improving child cognitive development, their effect on early child nutritional status and development (ECD) is less clear (Fiszbein and Schady, 2009). In particular, CCTs have reportedly had limited success at improving height-for-age z-scores of young children, a common measure of nutritional status (Behrman and Hoddinott, 2005; Fernald, Gertler and Neufeld 2008; Maluccio and Flores, 2005; IFPRI, 2003), and have even been associated with reductions in z-scores when CCT households in Nicaragua experienced negative shocks (Maluccio, 2005).

One contending explanation for weak or negative ECD outcomes is that poor families face difficult tradeoffs in the human capital investment allocations for younger and older-school aged children, especially during periods of negative income shocks. Although CCTs typically separate payments for school attendance from other cash payments, as Schultz (2004) notes, these school fee payments typically constitute only 50 to 75 percent of the total cost of schooling (including both direct and indirect costs). Furthermore, older children may receive, or indeed require, an increased share of scarce family resources if they need additional food and clothing to attend school. Additionally, older children's attendance at school may be at the cost of time spent caring for younger siblings. In *Oportunidades*, mothers increased their time spent caring for younger children when older children increased their school attendance (Dubois and Rubio-Codina, 2010). However, if households do not have sufficient resources to cover the opportunity costs of income and time from the older child that attends a school, then increased schooling investments induced by CCTs could hinder improvements in ECD for younger children. This tradeoff would be more likely when CCT program transfers and/or family incomes are not sufficient to afford adequate investments in the health and nutritional investments in pre-school-age children, i.e. especially among CCT recipients under duress because of chronic low incomes and/or serious negative income shocks.

In the next section, we develop a basic household model which explores the potential tradeoffs between CCT-induced improvements in schooling of older children and family investments in early childhood development. The model sharpens our understanding of the issue. In particular, it identifies that school and ECD investment tradeoffs are most likely to occur when households face the following conditions: i) They have older children who would not have gone to school without the cash payment; ii) The cash payment is sufficient to get the child to attend; and iii) The transfers do not completely provide the necessary resources to send the older child to school. A more explicit dynamic treatment of the issue would also involve an additional participation premium for maintaining access to the future flow of benefits associated with the attendance condition of enrollment. This would only deepen the tradeoff against ECD. Overall, the role of a negative income shock then is twofold. It makes it more likely that a household will fall under the aforementioned conditions of facing tough tradeoffs on the margin between the necessary costs of school and ECD investments and that those tradeoffs will be heightened. It is worth pointing out that more generally the impact of the CCT on ECD can go the other way, based on the level of the transfer. In other words, it could also have positive impacts on ECD if the support is sufficiently large, or if the transfer increases during a time of covariate shock.

A substantive empirical literature links negative income shocks with declines in ECD (Alderman, Hoddinott and Kinsey, 2006; Paxson and Schady, 2005; Ferreira and Schady, 2008) as well as long-lasting effects of ECD on school performance and future income prospects (Hoddinott et al., 2008; Maluccio et al., 2009). As mentioned above, Maluccio (2005) examined the impact of a negative shock to coffee prices on the efficacy of a Nicaraguan CCT program. Although he found positive impacts on schooling for communities that experienced the shock, he also found that child height-for-age z-scores were significantly lower in CCT communities that experienced the shock. These paired results are consistent with our hypothesis that CCTs can be less effective in their ability to improve ECD outcomes because of the conflicting incentives of maintaining school attainment goals.

Building on Maluccio, this paper further explores the empirical relationship between the negative economic shock to coffee prices, CCT participation, and ECD outcomes as measured by height-for-age z-scores. Three CCT programs pursued simultaneously in Honduras, Mexico, and Nicaragua all provide evidence on ECD outcomes during the severe coffee price crisis of the early 2000s. Having datasets from several countries where rural households faced a similar negative income shock is especially valuable, because as suggested above the tradeoffs might vary substantively based on local conditions and the CCT's structure.

Of the three countries, Mexico is clearly the best off economically, with the highest incomes and initial height-for-age z-scores. Additionally, Mexico's CCT *Oportunidades* (formerly *Progresa*) was several years into its operation and had become a permanent national program in the years preceding the coffee crisis. Meanwhile, the programs in Honduras (PRAF) and Nicaragua (RPS) were modeled after *Progresa*, and were in pilot stages of operation that had just begun the year prior to the coffee price crisis. Two key differences in program designs could influence their impacts. First, they offered different levels of compensation to families for children's school attendance, with the provisions in RPS in Nicaragua being more generous than those offered by PRAF in Honduras. Second, RPS had a relatively fixed payment per household, while PRAF did not. *Oportunidades* had fixed nutrition and health transfers, but increased transfers for school related expenses in proportion to the number of school aged children with a maximum transfer size, although this maximum was rarely reached. Indeed, reviews of transfer data in 1998-99 show that in each period fewer than 10 households out of the thousands receiving payments were restricted. Further, perhaps in response to an IFPRI evaluation (Coady, 2001) noting the regressive potential of the cap, as the program grew over time additional payments were offered for children going on to secondary school, and caps were raised.

To date, few empirical studies have provided the type of consistent empirical comparison of program impacts across CCTs offered here (Hoddinott and Bassett, 2008; Hoddinott and Wiesman, 2008; Caldés et al., 2005). Examining how households react to shocks and how CCT impacts are influenced by these shocks across multiple programs and countries can increase our knowledge of how similar policies work in different environments.

The negative coffee price shock explored in this paper was quite severe. It began in 2000, and prices declined by almost 50 percent in 2001, with a small rebound in 2002 that left prices that year well below their 2000 level. This sharp decline in coffee prices was associated with declines in household consumption in coffee communities in the PRAF sample areas in Honduras (Coady, Olinto, and Caldés, 2005) and RPS areas in Nicaragua (Maluccio, 2005). Because coffee is a permanent crop, it is less susceptible to rapid changes in household participation, which reduces "endogeneity" problems that might arise in studying impacts for more flexible crop choices. Coffee also requires a large amount of temporary labor during the harvest season, so those without coffee land but living in coffee farming communities were also likely to be negatively affected by the coffee price shock.

All three programs analyzed here utilize a randomized experiment to test program efficacy. The randomization efforts in all three are well established as having created similar control and treatment groups (Hoddinott and Bassett, 2008). Although it was not explicitly part of the experimental design, coffee's geographical requirements of specific growing conditions, including higher altitudes and rich volcanic soils, make it such that only certain communities can farm coffee. Moreover, because we use baseline data on the presence of coffee, our coffee community measure is in no way endogenous to their CCT treatment status. Using a village's treatment status and coffee production status, we are able to construct in effect a 2x2 comparison of the impacts of both the CCT and the coffee shock. We find in the baseline year before the CCT that coffee communities receiving CCTs were similar to coffee communities not receiving CCTs. We find the same for non-coffee communities even though randomization was not designed to ensure that coffee and non-coffee growing communities were similar. Thus, we are able to propose a two-way difference-in-difference comparison: we contrast households in and out of the CCT program against those in coffee-producing communities and non-coffee-producing communities.

As a quick preview of the results, we find that the coffee price shock had a negative impact on ECD in all three countries. However, we find that *Oportunidades* in Mexico helped recipients to alleviate the shock's negative effects on height-for-age z-scores, while in Nicaragua and Honduras participation in RPS and PRAF may have exacerbated the negative impacts on z-scores. We further explore the potential impacts of older siblings and find significant evidence in Nicaragua (but not in Honduras) that is consistent with CCTs causing a reallocation of resources toward older children. These contrasting results could be the result of multiple influences, including heterogeneity in the severity of the shock, the fixed payment scheme in Nicaragua, and/ or lower cash payment for school participation in Honduras. Low payments might induce families in Nicaragua and Honduras to reduce younger children's consumption to maintain participation of older children.

2. Conceptual Framework

2.1 Problem Description

Conceptually, there are two effects of the coffee price shock on households. The first effect is the decline in income and household food consumption (Ferreira and Schady, 2008). The negative consequences on household consumption of the 2001 coffee price shock are well documented. Coady et al. (2004) found that between 2000 and 2002 households in Honduran coffee communities experienced a decline in consumption of around 5 percent. In Nicaragua, Maluccio (2005) found that control households in coffee communities saw declines in consumption on the order of 20 percent. However, these effects varied depending on household characteristics. There is also evidence that these declines were more likely to be seen in households that owned coffee land in Honduras (Coady et al., 2004), or were richer in Nicaragua (Gitter and Cáldes, 2009). A particular cause for concern is that the coffee shock led to similar declines in food consumption (Coady et al. 2004; Maluccio, 2005; Gitter and Cáldes, 2009), which likely has negative implications for early childhood development.

The second problem caused by the shock is that CCT program effectiveness may have been compromised. Previous studies of CCTs have shown that CCT effects on food consumption are weaker for lower-income households (Dammert, 2009). Thus, reductions in income may moderate program effects. Further, the structure of CCTs may not allow households to fully adapt coping strategies to negative shocks. For these types of common shocks, households may rely on being able to temporarily increase labor supply to maintain or at least smooth consumption. In fact, studies by de Janvry et al. (2006), Maluccio (2005), and Gitter and Barham (2009) show that households increase child labor in response to certain types of negative shocks.[2] However, because CCTs require children to attend school, and it may be these children who have some earning potential (or take on home duties) to help smooth shocks, compulsory school attendance may eliminate this coping mechanism. Gitter and Barham (2009) find that RPS increased school enrollment over 35 percent in the poorest households in Nicaraguan coffee communities, while its impacts were closer to 10 to 20 percent in other (better-off) households in coffee communities. School enrollment in non-coffee households was statistically unaffected by the program. This result implies that in households with both infants and school-aged children the poorest households in CCTs are less likely than the control group to smooth consumption through child labor to prevent declines in ECD.

In the next section we develop a theoretical model that incorporates shocks and a conditional cash transfer to take into account the main adjustments that households might make to their investments in human capital of younger children and school aged children.

2.2 Theoretical Model

Conditional cash transfer programs are designed to foster the human capital development of both young children (ages 5 and under) and older, school-aged children (ages 6-15). If consumption goods improving younger children's health are normal goods, then it follows that unconditional cash transfers should have a positive impact. However, it is important to remember that with finite resources, substitution between investment in younger and older children can occur as well and especially if transfers are conditioned in a manner that requires investment in older children. Below we propose a simple household model to show how transfers conditional on schooling can potentially negatively affect younger children's development because of this required investment.

[2] In less severe shocks such as recurring droughts (de Janvry et al. 2006; Gitter and Barham, 2009), the shock increases the opportunity costs of child labor, which may further diminish CCT program impact on schooling.

We build on previous work on stochastic shocks to production and school attendance (Jacoby and Skoufias, 1997; Krueger, 2006; and Gitter and Barham, 2009). Our model makes two additions. First, instead of considering only one school-age child, we model a household as having a young child (less than 5 years of age) and an older child of school age. Second, our older child can divide time in three ways: education, wage labor, and time spent on child care (z). Previous models simply allow for education and wage labor. Here, the younger child's human capital accumulation depends on a younger child-specific consumption good and on the total time invested by parents and older siblings in child care. The human capital accumulation of both children enters into overall household utility.

Suppose a household utility function is the sum of three parts. Each part represents a member or members of the household. We denote the adults in the household with the subscript, *a*, the younger non-school aged child with the subscript, *yc*, and the older school aged child with the subscript *o*. The household's utility is the sum of adult utility and human capital for younger and older children. The first part consists of a utility function for adults, *u*, with a single input: the adults' consumption (c_a). The second part is the young child's human capital function, *h*, which is a function of the young child's consumption, c_{yc}. A recent study of *Oportunidades* (Dubois and Rubio-Codina, 2010) shows that both older siblings and parents are responsible for taking care of younger children. This time spent with younger children will help develop their human capital and is included as time spent on childcare (*z*). The third part is the school age child's human capital, *H*, which has two inputs: that child's consumption, c_o, and time spent on education, *e*. We assume all three functions have diminishing marginal returns to any input. We also assume all three consumption goods are normal.

The household faces four constraints. The first is that the older child can divide his or her total time, represented by the numeraire unit 1, among three activities: education (*e*), childcare (z_o), and wage labor, L_o. Adults in the household divide their time between child rearing (z_a) and wage labor (L_a). As with the older children, we denote their total time as 1.

The total production of childcare (*z*) and income (*wL*) is the sum of the production of the adults and the older child. The older child's productivity in childcare is a fraction (α_z) of adults' productivity, where $0 \leq \alpha_z \leq 1$. This is consistent with findings in Dubois and Rubio-Codina (2010), that mothers are better caretakers than siblings. Similarly, in the wage market, older children earn a wage that is some fraction of adults' child care productivity, α_w, where $0 \leq \alpha_w \leq 1$. The relationship between α_z and α_w could depend on local conditions and child characteristics, such as gender and age. In coffee, for example, the fraction could be quite high given the challenges of harvesting and hauling coffee on inclined slopes.

The final constraint is a budget constraint. Households spend all of their money. We do not allow for borrowing or saving in this model.

$$U = u(c_a) + h(c_{yc},z) + H(c_o,e) \quad (1)$$

$$s.t.\ 1 = e + z_o + L_o$$

$$1 = z_a + L_a$$

$$z = z_a + \alpha_z z_o$$

$$L = L_a + \alpha_w L_o$$

$$wL = c_a + c_{yc} + c_o$$

We now discuss four potential forces that could affect human capital outcomes of the younger child, *h*.

- A negative shock to wages
- An unconditional cash transfer
- A cash transfer conditional on schooling
- A wage shock combined with a cash transfer conditional on schooling.

2.2.1 Negative Shocks to Wages and Younger Child Human Development

While the empirical portion of this paper examines the impact of a shock to coffee prices, this model captures that type of impact through a decline in wages. The impact of a decline in wages (w) on the younger child's human development (h) is the sum of the impacts on young child-specific goods (c_{yc}) and child care (z). Although previously cited literature suggests that wages and young children's development are positively correlated, a careful examination of equation (2) suggests cases may occur where the relationship is ambiguous or even negative.

$$\frac{\partial h}{\partial w} = \frac{\partial h}{\partial c_{yc}}\frac{\partial c_{yc}}{\partial w} + \frac{\partial h}{\partial z}\frac{\partial z}{\partial w} \quad (2)$$

Each addend on the right-hand side is the product of two terms. We assume that the first term of each addend is positive, i.e., that increased consumption and increased time invested in child care by parents and/ or older siblings improves the human capital accumulation of the younger child. The first addend is therefore a weakly increasing function of the local wage. It is weakly increasing because adults can distribute declines (increases) in consumption over themselves and each child, which might allow them to cushion younger children from shocks. A negative shock to wages should therefore weakly decrease the younger child's human capital. If c_{yc} is a normal good then the first term on the right hand side, $\frac{\partial h}{\partial c_{yc}}\frac{\partial c_{yc}}{\partial w}$, should be weakly positive.

The sign of the second addend depends on the final term, which is the effect of wages on time invested in child care. Standard economic theory suggests that the amount of hours worked is not always positively or negatively correlated with the wage (i.e., the labor supply curve may be backward bending). Therefore, childrearing time (*z*) is not always positively or negatively related to the wage. If older children are used to take care of younger children, then the tradeoff between school, child labor, and helping raise siblings is also dependent on the going wage. As both Krueger (2006) and Gitter and Barham (2009) show, decreasing wages may induce households to remove older children from school and send them to work to compensate for income losses. Conversely, because lower wages for older children are also potentially equivalent to a lower opportunity cost of schooling, lower wages might increase schooling. In the present model, lower wages might still increase schooling, but there is another margin: lower wages might also increase the older child's contribution to childrearing. If childrearing, *z*, is the alternative to wage labor and school, then we cannot sign the relationship between the production of the *z* good and wages. Because we cannot sign the second term in equation (2), we cannot definitively say whether declining wages decrease or increase the younger child's human development (*h*).

2.2.2 *The Impact of an Unconditional Cash Transfer on the Younger Child's Development*

Suppose that the household receives an unconditional cash payment (T) from the government. The budget constraint can now be written as

$$wL + T = c_a + c_{yc} + c_o \quad (3)$$

The full impact of the unconditional cash payment has two terms.

$$\frac{\partial h}{\partial T} = \frac{\partial h}{\partial c_{yc}}\frac{\partial c_{yc}}{\partial T} + \frac{\partial h}{\partial z}\frac{\partial z}{\partial T} \quad (4)$$

The impact of the unconditional cash transfer on the younger child's human capital will be the sum of the impact on consumption of the younger child specific good, c_{yc}, and the impact on the allocation of time toward child rearing, *z*. Based on the assumption that c_{yc} is a normal good, the first term on the right hand side will be positive. Unlike the above analysis on wages, the second term will also be positive. Because *T* is a direct payment, there is no substitution effect from changes in the opportunity costs of not working. Thus, $\frac{\partial z}{\partial T} \geq 0$. Because both terms are nonnegative, unconditional cash payments will improve early childhood development under the assumptions of the model.

2.2.3 The Impact of a Conditional Cash Transfer on Young Child Development

Next suppose that the household receives the transfer *CT* conditional on two factors. The first is that the older child must attend school full time, so that $e = 1$. Second is that an adult in the household must spend an amount of time *t* attending meetings and traveling to collect payments.[3] Some households may choose to ignore the conditions of the transfer. Those rejecting the transfer receive no transfer and are not affected by the program. However, in the three CCTs examined in this paper, most households accepted the cash transfer. The effects of the conditional transfer, *CT*, on production of human capital for the younger child, *h*, like the unconditional transfer, can be expressed as the sum of the impacts on the consumption of the good (c_{yc}) used as an input of younger child's human capital and the change in the household childcare, *z*.

$$\frac{\partial h}{\partial CT} = \frac{\partial h}{\partial c_{yc}} \frac{\partial c_{yc}}{\partial CT} + \frac{\partial h}{\partial z} \frac{\partial z}{\partial CT} \quad (5)$$

$$wL + CT = c_a + c_{yc} + c_0$$

$$CT > 0 \text{ if } e = 1 \;\&\; t > 0$$

$$CT = 0 \text{ if } e < 1 \text{ or } t = 0$$

$$1 = z_a + L_a + t$$

Intuitively, we would expect that increasing household income potential by providing a conditional cash transfer would improve younger child human development. This could happen if the transfer increased spending on the younger child's consumption good and/or if adults increase their production of child care to compensate for the older child's decreased participation in child care. If sufficiently large, the transfer may also encourage adults to decrease their labor supply and increase time devoted to child care.

However, the conditional transfer's effect on early childhood development is ambiguous for several reasons. First, Gitter and Barham (2009) point out that total consumption may fall as the result of accepting a transfer conditional on schooling if the transfer is large enough to be accepted but not large enough to compensate for the income lost by the reduction in child labor caused by sending their

[3] Because CCTs also offer education on childrearing, this may increase the productivity of the younger child human capital development (*h*). However, inclusion of a change in productivity does not change the basic results of the model.

child to school. This scenario is most likely in a household that is a) facing a serious income deficit and b) sufficiently risk averse to prefer the guaranteed income of the transfer over the variable income associated with the child or perhaps the mother working. Therefore the potential exists for cash transfers conditional on schooling to reduce spending on younger children, making the first term on the right-hand side of equation (5) negative. If the reduction in the young child's consumption good, c_{yc}, is sufficiently high, then cash transfers conditional on schooling may reduce the younger child's human capital.

Second, with certain transfer sizes adults may actually increase adult wage labor time to compensate for the loss of the labor of the older children, who are now in school. In this case a conditional transfer could also reduce child care. Additionally, households must expend time collecting the payment and meeting the requirements (t), which might further decrease the total time spent on child care. In either case, this may make the second term on the right hand side of equation (5) negative.

Complicating the relationship between CCTs and young childhood development further is that the impacts on older children's time will depend on whether children would have gone to school in the absence of a transfer. The decision on allocating the older child's time between wage labor, child care, and education will depend on the going local wage. In the next section we explore how CCTs affect younger children via impacts on the older child's time allocation problem. This third case occurs when transfers are sufficiently large to entice older children to go to school that otherwise would not have, but the transfer and local wages are sufficiently small perhaps during a negative shock that some households cannot maintain the same pre-transfer level of younger children's human capital.

2.2.4 A Wage Shock Combined with a Cash Transfer Conditional on Schooling

In this model, the main driver of income, in addition to the CCT transfer, is the going wage (w). During the time period we examine in the three countries, the going wage in rural areas was negatively affected by a severe shock to coffee prices. Based on previous findings of the relationship between shocks and the impacts on schooling of a CCT, we know that the potential negative impacts of a CCT on young children's development are heightened during shocks but, as shown below, the potential negative impacts of a CCT on younger children's development also depend on the (relative) size of the transfer.

Unfortunately it is very difficult to identify a threshold above or below which problems with ECD might emerge, mostly due to the fact that such a threshold may not be unique given variations in households, incomes, consumption preferences, costs of schooling, capacities for child care provision, and the like. Higher wages for workers may translate into older children going out and working rather than either getting an education or participating in child care. Alternatively, they may translate into adults making enough money that older children need not work: if the family has enough to eat, older children can be sent to school. In the same way, reduced wages can have a variety of effects. If earnings drop, older children are presumably less likely to attend school, but they may be sent out to work and/or required by adults to spend more time on child care. However, as shown in Gitter and Barham (2009), the levels of income at which older children engage in different activities are not consistent, in part because the shock can also negatively affect the potential returns to labor and hence actually lower the opportunity costs of their time spent working or doing child care to provide support for a parent working.

In this model, there are three relevant margins: the tradeoff between the older child's time spent on education as opposed to child care, the tradeoff between his or her time spent on education as opposed to working, and the tradeoff between adults working and child care. It is worth noting that all of these constraints might be shaped by gender characteristics and roles in potentially disparate ways. For example, girls might be more highly valued in child care, and boys might be more highly valued for some types of manual labor jobs. Thus, which margins bind could be shaped by household characteristics and allocation choices as well as by outside factors. Thus, while households accepting CCTs are modeled as accepting binding constraints on the first two margins, the degree to which these are detrimental to ECD outcomes is affected by various factors.

To examine the final relationship of interest—the effect of changes in wages on the impact of a conditional cash transfer on young child ECD via older children's time allocation—we consider three potential cases. In the first two cases, a corner solution exists in which the CCT does not change the older child's school attendance. If the child attends school regardless of the transfer (Case 1), the scenario reduces to the unconditional cash transfer and increases younger children's human capital. If the transfer is rejected (Case 2), then it has no direct impact on younger children's human capital. In the final case (Case 3), the child goes to school with the conditional transfer though s/he would not have in the absence of a transfer.

Below we denote three potential wage levels—w_1, w_2, and w_3—each corresponding to one of the above three cases. The optimal amount of time in school, $e(w,OCT)$, for the older child in school for each wage and offered conditional transfer (OCT) size is listed below. Only in Case 3 does the conditional cash transfer have an effect on schooling choice and hence on ECD. Note that the previous sections posited other avenues for CCTs to affect ECD outcomes, namely risk-averse households (or very poor ones) accepting CCT and adults doing less child care to compensate for income loss.

$$\textit{Case 1: } e(w_1, 0) = e(w_1, OCT) = 1 \qquad (6)$$
(CCT has no impact on education and increases ECD)

$$\Delta h = h(w_1, OCT) - h(w_1, 0) > 0$$

$$\textit{Case 2: } e(w_2, OCT) = e(w_2, 0) = 0 \textit{ (CCT has no impact on education or ECD)}$$

$$\text{Case 2: } \Delta h = h(w_2, OCT) - h(w_2, 0) = 0$$

$$\textit{Case 3: } e(w_3, OCT) - e(w_3, 0) = 1 \textit{ (CCT increases schooling and has an ambiguous effect on ECD)}$$

$$\Delta h = h(w_1, OCT) - h(w_1, 0) >?/< 0$$

In Case 3 the relationship between a CCT and young children's development is ambiguous, and there is a potential for negative impacts of a CCT on early childhood development. In equation (5) above we showed that the impact of a CCT on early childhood development (*h*) will be the sum of the impacts on spending on younger children and time spent in childrearing. In case 3 the older child attends school only with the CCT. However, as Schultz (2004) suggests, the CCTs studied in this paper may have made school payments that were less than what it would take to fully compensate for the loss of child labor plus the costs of schooling. Returning to the budget constraint ($wL + CT = c_a + c_{yc} + c_o$), once a conditional transfer (CT) is accepted, both older child labor is reduced and spending on that child is increased (c_o).

The impact of the CCT on the younger child's development depends on the size of the transfer and the household's initial income level. If the transfer is sufficiently large (on top of core household income) ($CT > CT_2$), then households can increase the consumption levels of both the younger and older child and compensate for adults' needing to spend more time in child care and less working themselves. If the transfer is small, then it will be rejected ($CT < CT_1$) and the CCT will have no impact. If CT is large enough to be accepted, yet small enough not to fully compensate ($CT_2 > CT > CT_1$) for the loss of child labor and increase in spending associated with the older child's schooling, then the impacts on younger child development could be negative. To compensate for this effect, adults may either increase their time spent working or reduce food consumption expenditures. Increases in labor reduce child care (z) and decreases in consumption could affect younger children. Either method of offsetting the loss of older child labor reduces inputs to early childhood development:

$$\Delta h = h(w_3, OCT) - h(w_3, 0) < 0 \text{ if } CT^2 > CT > CT^1 \quad (7)$$

To summarize, in the theoretical model described above, we have explored the relationships between wages, CCTs, and human capital development of young and school-age children. In some cases, CCTs have the potential to improve the human capital development of both younger and older children. However, in others, CCTs can have negative impacts on younger children's development by focusing human capital investment on older children. We find that this case is more likely to occur when wages are adversely affected by shocks, because this context could heighten the tradeoff for recipients who have chosen to participate even though the transfer does not fully substitute for foregone child earnings. Thus, we expect that we would be more likely to find a negative relationship between CCTs and ECD during a shock.

3. Data Summary and Descriptive Statistics

3.1 Program Description

The empirical analysis uses available datasets from Honduras's PRAF, Mexico's *Oportunidades*, and Nicaragua's RPS. The basic design of all three programs was similar. Cash payments were made to mothers in poor households on the condition that households utilize health services and older children attend school. (For a more detailed description of the programs see Hoddinott and Bassett, 2008). The average payment sizes were $27 and $28 a month in RPS and *Oportunidades,* respectively, though the share of household income accounted for by the transfer was considerably larger in Nicaragua. In Honduras, by contrast, the PRAF payment size was much smaller, with a maximum transfer size of $7.70 a month. Emulating a randomized control trial, all three CCTs created comparison control groups.

Another key difference in program design is the relationship between eligibility and family structure. This is particularly important when examining the relationship of interest between older and younger children's human capital investment. If payments are tied to family size, then CCTs could increase fertility, thereby mitigating over time some of the positive effects. RPS and *Oportunidades* placed limits on some aspects of the transfers and prevented increases in fertility. An additional component that might affect the tradeoff between school-aged and younger children is that *Oportunidades* educational transfers were not fixed. *Oportunidades* paid higher transfers for older children and additional children. Although *Oportunidades* ostensibly capped total education transfers, very few if any households hit the maximum. RPS had a fixed educational transfer with the exception of a small extra stipend for additional children's direct schooling cost.

Program design can also potentially have an impact on fertility if households receive additional benefits from having more children. In the cases of *Oportunidades* and RPS, household payments and eligibility were determined ex-ante of program implementation. Thus, they did not have positive fertility incentives. However, PRAF allowed households who had children after program implementation to become eligible. Consistent with their designs, only PRAF had measurable effects on fertility, increasing fertility 2-4 percentage points (Stecklov et al., 2007).

As mentioned above, these CCTs have had limited success in improving early childhood development as measured by young children's height-for-age z-scores. Hoddinott and Bassett (2008) summarize several studies (see Table 1 below) as showing that RPS and *Oportunidades* (PROGRESA) are associated with a non-statistically significant increase of about 0.15 standard deviations. In a different study, Hoddinott and Wiesmann (2008) show that households in *Oportunidades* increased the total number of calories consumed, while those in RPS increased calories from fruits and vegetables as well as the number of unique foods consumed. Thus, households in RPS and *Oportunidades* improved participants' nutritional intakes, but appear to have had only a limited impact on their children's height. Because individual consumption data are not available, it is difficult to go further than this with the nutritional analysis of ECD outcomes. Not surprisingly given the small size of the CCT transfer, Hoddinott and Wiesmann (2008) do not find similar program impacts from PRAF.

Mexico's *Oportunidades* preceded the other two programs, and much of the design of the other two programs builds on *Oportunidades* (Hoddinott and Bassett, 2008). Previous analyses of *Oportunidades'* impact on child height for age z-scores have given varied outcomes. In a review that includes Rivera et al. (2004), Gertler (2004), and Behrman and Hoddinott (2005), Hoddinott and Bassett (2008) conclude that "the most conservative estimates show that PROGRESA (*Oportunidades'* original name) has no effect on average height growth but does reduce stunting by ten percentage points." (p. 17) The only study we are aware of that examines the medium-term impacts of the program (from 1998 to 2003) finds no direct program effects but links payment amounts to increased child height (Fernald, Gertler, and Neufeld 2008).

As discussed in the introduction, the timing of these surveys is propitious for the proposed analysis. For RPS and PRAF, baseline data were collected in 2000, the year before coffee prices reached their nadir in 2001. Follow-up rounds were then collected in 2001 and 2002 at the height of the coffee price crisis. *Oportunidades* does not fit the mold quite as well, but still permits a reasonable identification strategy over the time period in question. Participating communities were split into treatment and control groups, and the leading edge of the treatment group began receiving payments in

March of 1998. About a year and half later, in November of 1999, the control group started receiving payments as well, meaning that all households received payments for most of the intervening period, though the randomization represents a household's having received around 20 months of additional payments in the past. We use data over the same timeframe as the other two studies, using a survey performed in 2000 as our baseline. This means that both groups had been receiving transfers (for two years or a few months in treatment and control groups, respectively) before our survey was taken. In our data, the treatment group averages 446 more pesos per capita in total transfers received than the controls (2315 vs. 1869).

Although the design of PRAF and RPS were both based on *Oportunidades*, *Oportunidades* is clearly the most established program. By 2000, the baseline year of PRAF and RPS, *Oportunidades* was a nationally distributed program, and by 2002 nearly one in five households in Mexico received CCT payments (Rawlings and Rubio, 2005). Conversely, PRAF and RPS were both pilot programs that were allowed to lapse after their initial phase, so households could not necessarily expect payments to continue, and indeed they did not.

Another difference is that PRAF included both demand- and supply-side interventions. On the demand side were the cash payments typical of CCTs, while on the supply side were improvements to local schools and health clinics. As a result, in the implementation, groups were created for the demand intervention only, the supply intervention only, both interventions, and a control group. Further complicating the evaluation was an uneven distribution of supply side payments and differences of timing in the collection of the data.

3.2 Describing the Shock

The shock of interest is the steep decline in coffee prices that coincided with the implementation of CCTs in each of the three countries. Figure 1 shows the change in coffee prices over the time period in question along with the years of data collection. The first round of data collection in all three programs occurred as coffee prices were falling. The second round of data was collected during the low point of coffee prices, from 2001 to 2003, when prices were only slightly above 60 cents per pound, less than one-third of their high point in 1997. The use of the decline in coffee prices as a shock is supported by previous analyses of the sample populations in Honduras (Coady et al., 2005) and Nicaragua (Maluccio, 2005) that show the shock caused substantial declines in total consumption.

As Maluccio (2005) notes, coffee is only grown at certain altitudes, and therefore is only feasible in certain communities. In Maluccio (2005) and Gitter and Barham (2009), coffee communities were identified utilizing surveys given to community leaders, which asked questions about the presence or importance of coffee crops.[4] On the other hand, Coady et al. (2005) use data at the household level. This approach would be difficult to implement for the Nicaraguan data due to limited data on where households earn their wages. To provide consistent estimates across the three programs, we utilize the community surveys on the presence of coffee in a community to define our measure of the shock. Communities are divided into coffee and non-coffee communities (i.e., those where coffee is grown and those where it is not). The community measure may not control for heterogeneous effects that depend on a household's participation in the coffee sector; however, it is a more inclusive measure because it encompasses households that may have experienced indirect impacts from the coffee shock through falling local wages.

The sample households in Honduras and Nicaragua have much higher participation rates in coffee farming than do those in the Mexico sample, and this is reflected in the CCT data. In Honduras 28 percent of households lived in communities that reported coffee as one of the two most important crops. Among *Oportunidades* households for which biometric data are available, 12 percent lived in a community with coffee cultivation. In Nicaragua, 50 percent of the RPS communities had a community leader reporting local coffee production.

3.3 Sample Statistics

We use the evaluation data for three CCTs: PRAF in Honduras, RPS in Nicaragua, and *Oportunidades* in Mexico. When we limited the sample to children who were under 4 at the time of the baseline survey and under 6 at final measurement, and to those for whom there were height-for-age z-scores, we are left with sample sizes of 2715 for PRAF, 491 for RPS, and 473 in Mexico. Because the econometric approach described in the next section takes advantage of the experimental design of each of the three CCTs, it is important that control and treatment groups be similar at the baseline. The first two columns of Table 2 compare height-for-age z-scores for these groups, and show that the control and treatment groups are not statistically different from each other at the baseline. Additionally, it is apparent from the table that the children targeted in *Oportunidades* have higher baseline z-scores than children targeted by the Nicaraguan or Honduran programs. This suggests that households there may be at a different basic consumption level and hence vulnerability to tradeoffs in human capital investment choices. The final four columns of Table 2 split up the sample by presence of coffee production in their community. Comparing coffee and non-coffee communities, the baseline

[4] Consistency checks show that coffee growing households were generally concentrated in coffee growing communities.

differences of z-scores are not statistically significant. In Table 3 we provide per capita transfer data, which is only available for the *Oportunidades* data set.

The dependent and independent variables' means and standard deviations are summarized in Table 4. Two noteworthy trends appear. First, in Nicaragua and Honduras the average child's height-for-age score decreased through the course of the study period in spite of the presence of CCTs. Second, mothers in the *Oportunidades* study have on average twice the number of years of schooling than mothers in PRAF or RPS. These differences, in addition to the higher initial z-scores, also point in the direction of divergent outcomes for Mexico relative to Honduras and Nicaragua. Table 5 divides the sample between coffee and non-coffee communities. There are initial baseline differences between the two groups in terms of height-for-age z scores. To correct for these differences, we utilize the change in z scores as our measure of interest. For the most part, children in coffee and non-coffee communities are otherwise similar in the baseline, although consumption is higher in coffee communities in PRAF and mothers are slightly less educated in RPS.

4. Econometric Specification and Difference-in-Difference Estimation

Evaluations of conditional cash transfer programs are popular among researchers at least in part because they were created with randomized control and treatment groups. Previously cited works as well as our own descriptive statistics show that randomization was successful in all three conditional cash transfer programs in creating control and treatment groups with similar child z-scores. In the previous section we showed that across coffee communities, treatment and control groups also had similar baseline statistics. This allows us to create a 2x2 matrix of the potential impact of on z-scores of treatment and coffee status.

The availability of ex-ante and ex-post observations of control and treatment groups allows for the estimation of "gold standard" difference-in-difference (DID) measures of program impacts (Schultz, 2004; Hoddinott and Skoufias, 2004; Parker and Skoufias, 2001). Our estimations are designed to explore the implications of the theoretical model presented in Section 2. We begin with a relatively simple DID framework to measure impacts of the coffee shock on child z-scores. In terms of the theoretical discussion, the relationship between wages and young child human capital is examined first. In this case, the coffee shock is meant to serve as a proxy for a change in wages, and human capital is measured by height-for-age z-scores. Next, we estimate the impact of the shock, the impact of the conditional cash transfers, and their combined impact on child z-scores, measures that mirror the final section of our theoretical discussion.

We begin with the shock measure to test if the coffee shock impacted child z-scores. The variable of interest is the change in the height-for-age z-score, Δh, of child i in community c's between the baseline survey and the post-treatment survey. By using the change in height-for-age z-score we remove the effects of child-invariant characteristics on overall height for age, though effects on trajectory might still be present.

$$\Delta h_{ic} = h_{ic}^{1} - h_{ic}^{0} \quad (8)$$

The first specification tests the impact of the shock on all children. Our econometric specification, below, includes a binary indicator to represent the shock. $Coffee_c$=1 if child i resides in a community that farmed coffee. In this case the α_1 coefficient indicates the difference between those affected by and not affected by the shock. Additionally the regression controls for a vector of child and household characteristics X_i: age at baseline, household size at baseline, per capita consumption at baseline, and maternal education in years. The error term is denoted as μ_{ic}. Because randomization took place at the community level, all error terms are clustered at the community level to control for correlation in the error terms at the community level. Additionally, we use this specification to test the shock's impact on household consumption to validate the effectiveness of the coffee variable as a measure of declining incomes.

$$\Delta h_{ic} = \alpha_0 + \alpha_1 Coffee_c + \beta_i X_i + \mu_{ic} \quad (9)$$

$Coffee_c$ = 1 if household is in a locality that farms coffee

μ_{ic} = time invariant error term

The second estimation equation (equation (10) below) expands the previous estimation to include the shock measure and the impact of conditional cash transfers as well as their interactive impacts. Specifically, to estimate the change in z-score, Δh_{ic}, we include the coffee and CCT variables along with a term that interacts them. As in the first estimation equation, the α_1 coefficient indicates the difference between those with and without the shock. However, because we are also controlling for treatment and control groups, the estimate of α_1 is now the impact of the shock on that part of the control group that was affected by the shock. The rest of the CCT impact measures are as follows. First is the measure of the impact of a CCT for non-coffee communities, which is captured by δ_1. For coffee-farming CCT treatment communities, the total impact of the CCT is $\delta_1 + \delta_2$. Finally, the difference in CCT impacts between coffee and non-coffee communities is δ_2. As in previous estimations, we also include a vector of individual and household characteristics, X_i. Due to the inherent heterogeneity of the programs and their distinctive country contexts, we estimate all equations separately for each of the three data sets.

$$\Delta h_{ic} = \alpha_0 + \alpha_1 Coffee_c + \delta_1 CCT_c + \delta_2 Coffee_c * CCT_c + \beta_i X_i + \mu_{ic} \qquad (10)$$

One additional issue arises in the comparison of the three data sets. In the case of *Oportunidades* both control and treatment households had received transfers by the beginning of the study period. Thus, for *Oportunidades* the binary CCT measure would proxy an intensity of treatment measure with treatment households receiving the payment for a longer time period. To get a better gauge of the intensity of treatment we utilize the per capita amount of transfers received instead of a binary variable for control and treatment, as we do with RPS and PRAF. Unfortunately, data are not available on total transfers received for RPS and PRAF recipients. Thus, we cannot develop a comparison using total transfers for them. The total transfer measure also has the advantage in the *Oportunidades* dataset of providing increased heterogeneity in the interaction term, which is smaller in *Oportunidades* due to fewer observations of coffee-producing households. We do not present the analyses using the binary CCT measure for *Oportunidades* because the sample sizes are rather small, but the results are not substantially different in terms of the basic effects.

In addition to estimates of impacts on children's z-scores, we also test impacts on per capita consumption. This helps to identify how households responded to both the shock and potential transfers. We further explore some hypotheses of the model by providing separate estimations by the child's gender and the presence of older siblings.

5. Results

Two core findings emerge in the empirical analysis. The first is that during the period of falling coffee prices, coffee communities were not strongly affected in Mexico, but the other two countries saw declines in child height-for-age z-scores and per capita consumption. The second is that the shock had heterogeneous impacts in the three countries across CCT recipients. Put differently, the efficacy of the three CCTs varied substantively. As in previous analyses, no overall positive treatment effect on height-for-age z scores was found; however, *Oportunidades* appeared to alleviate the negative impact of the shocks, while RPS appeared to exacerbate them. The amplification of the negative impacts of the shocks in RPS is higher in households with school-aged children, which is consistent with the theoretical model's hypothesis that investment in older children may divert resources away from young children especially during a negative shock. Finally, PRAF was shown to have little or no impact on ECD, which is consistent with previous analyses of the program and its relatively small cash transfers.

The first set of results in Table 6 estimates the impact of the shock independently of the CCT. In Nicaragua and Honduras, height-for-age z scores fell 0.2 and 0.1 standard deviations, respectively. The negative impact of the coffee shock measure was not statistically significant in Mexico, though the observed change in height for age was of roughly the same magnitude as the other two countries. The table also presents the impacts on per capita weekly consumption. Similarly, the results show declines in per capita consumption in Nicaragua and Honduras, but no impact in Mexico. In terms of magnitude, per capita weekly consumption declines were largest in Nicaragua, with a decline of over $1 (US$1 ~ 12 cordobas), followed by Honduras with around $0.50 (US$1 ~ 14 lempiras). In Mexico, the impact on per capita consumption was not significantly different from zero.[5]

Now that we have linked the coffee shock to a decrease in height-for-age scores, we examine the impact of CCT participation on that outcome. As shown in Table 7, the results are mixed. Having split out the effects using the interaction term, we find that in Mexico, the negative indicator for being in a coffee community that previously failed to achieve statistical significance is now both larger in size and statistically significant. By contrast, the coefficient on the interaction term with CCT treatment is positive. However, the total mean effect is still negative and statistically significant. Thus, being an *Oportunidades* recipient does not completely alleviate the negative effects of the shock.

The direct program effect for children in non-coffee communities also is small in size and statistically indistinguishable from zero, implying that the overall program effects on child height-for-age z scores were nonexistent in non-shock communities. This is consistent with the main published paper reporting results as of the 2003 survey we use (Fernald, Gertler, and Neufeld, 2008), which also failed to find a link between program participation and height for age, though transfer effects were identified.

The height-for-age z score results for the Honduran and Nicaraguan programs are less positive than for Mexico. In both cases, the estimated negative effect of being in a community that experienced the coffee price shock is less strong than it was before in the simple regression, and the overall effect is dominated by the interaction term with CCT treatment. In other words, the negative effects of being hit by the shock were amplified by being in a treatment community. In the Honduran data, these findings are not robust, as none of the coefficients are statistically significant. In Nicaragua, however,

[5] The coefficient of the coffee variable on per capita consumption in Mexico although not statistically significant is positive. One potential reason for this is the second data point comes from 2003, when coffee prices had started to recover. It is worth noting that in 1998 at the beginning of the coffee price crisis, coffee communities' consumption was almost 20 percent higher, but by 2000 it was lower than non-coffee communities' conumption. As it recovered from the shock, per capita consumption rose faster in coffee communities between 2000 and 2003. However, the damage was done: the negative coefficient on the coffee variable on child z-scores suggests lingering effects from the shock even as consumption had started to recover.

the interaction term is the only regressor of interest that is statistically significant, and it provides evidence that the shock took a larger toll on young children In CCT participant households.

One possible concern might be that, overall, the shock was larger in treatment communities in Nicaragua. In fact, the results provide evidence that the shock was equal across treatment and control communities, as impacts on per capita consumption were affected by both the treatment and the shock, but not by more than in the communities where both CCT and coffee shock occurred. Rather, we suspect that the result means that households in treatment villages are reallocating their resources to older children when faced with a shock. In Honduras, the impacts on consumption are less clear. This is not surprising, as Hoddinott and Weisman (2010) note that PRAF had little to no overall impact on consumption.

The gender effects across the three countries are examined in Table 8 by running separate regressions for males and females. The results again demonstrate significant variation across the three contexts. In Mexico the coffee shock seemed to have had the largest negative impact on boys, but the positive impact of the per capita transfers were also largest for boys hit by the shock. The overall impact of the shock was still negative given the mean amount of per capita transfers. The patterns of the CCT counteracting the negative effects of the coffee shock are evident for both males and females at similar magnitudes in Mexico. Meanwhile, in Nicaragua and Honduras, there are distinct gender differentials evident in the results. First, in Nicaragua, the negative effects on young girls only arise if they are in a treatment community that was hit by a negative shock. Otherwise, their z-scores would be neutrally affected by either the shock or the treatment. Honduras is exactly the reverse. Only the z-scores of younger boys are negatively affected by being in a family that receives the CCT and experiences the negative shock. This difference across these two countries is not readily explained by our analysis.

Finally, to probe the issue further, we separately consider the z-scores of young children in households with and without older siblings. The results presented in Table 9 support our theoretical hypothesis that in the context of a negative shock the costs associated with sending older children to school may negatively affect z-scores for younger children. First, note that in Mexico and Honduras, the negative shock as measured by being in a coffee-producing control community has a somewhat stronger negative effect on the development of children with older siblings. Second, note that in Nicaragua and Honduras, the coefficient estimates on the interaction between being in a coffee community and being a CCT participant are negative, and that the strongest and significant effect is for RPS households in Nicaragua where the younger child has older siblings.

In the case of Mexico, CCT program participation helps young children in coffee communities whether or not they have older siblings, though these protective effects are stronger for young children without older siblings. Paired with the negative coffee community coefficient, this outcome shows that in the Mexican case *Oportunidades* appears to help families compensate for difficulties that might otherwise have led them to distribute scarce resources toward older siblings at the expense of younger ones.

6. Discussion

We find that the different CCT programs have disparate outcomes with respect to ECD. In Mexico, the CCT appears to have offered a protective effect for ECD outcomes of program participants, substantively shielding young children from the effects of a negative coffee price shock. Meanwhile, in Honduras and Nicaragua, we find the opposite: either program participation has minimal impact (Honduras) or it actually amplifies the destructive effects of income shocks (Nicaragua) on younger children. Previous research, our model, and our empirical analysis of sub-samples with and without older siblings all point to a possible means by which this amplification could happen. Basically, if households agree to participate in CCTs, they commit to the education of their school-age children. This investment in human capital may, however, further constrain other consumption and investment opportunities by both limiting family earnings (through direct reduction of child labor and indirect reduction of adult labor through foregone child care) or by focusing investment more narrowly on school-age children at the expense of younger children. This impact will be based on CCT design, and the negative impacts are most likely to occur when transfer size is not linked to household structure. This finding is supported in Nicaragua, which has the most binding constraints on transfers and experiences the largest negative impacts of transfers on ECD outcomes.

Are ECD outcomes, in fact, constrained in some instances by CCT participation? A number of supporting facts lend credence to the story. First, the Nicaraguan CCT has been linked to a 22 percent increase in child schooling, while in Mexico the net effect on enrollment in primary education was about 1 percent (largely because a much higher percentage of Mexican children already attend primary school, as discussed by Rawlings and Rubio, 2005), although the impact on enrollment was substantially higher at the secondary level (Behrman et al., 2005). Furthermore, the gains in education from RPS were concentrated in the poorest households in the coffee communities. The potential for the constraints on family labor and expenditures to bind households in Nicaragua is therefore stronger than in Mexico, and likely even stronger in the poorest households hit by shocks in Nicaragua. Second, household income and consumption levels are much higher in Mexico than in Nicaragua, which makes it far less likely that there will be as significant a constraint on ECD expenditures among Mexican CCT recipients as there might be among Nicaraguan CCT recipients. Because of their lower reliance on coffee income, the coffee price shock also did not cut as deeply into family earnings, in percentage terms, as it did in Nicaragua. Finally, Mexican program participants had already been receiving payments from their CCT for a short time, and it is possible that this surplus enabled them to overcome the shock more effectively than in Nicaragua and Honduras.

The lack of significant results in the Honduran case is likely due to small transfer sizes. Participating households in Honduras received about 14 percent of what Nicaraguans got (Rawlings and Rubio, 2005), and previous research also reported difficulty identifying program effects due to problems with the seasons in which the surveys were conducted (Hoddinott and Wiesmann, 2008). Perhaps the seasonality issues might also provide a reason for our perplexing finding that the interaction of the program and the shock works to the advantage of girls but against boys. We are otherwise at a loss for an explanation for that outcome. Yet another curiosity is the fact that more children in the sample lack older siblings than have them. Since randomization was at the municipality level, it is odd that the sample would break down this way.

Omitted variable bias is also a possibility in any regression analysis. Our three-way difference-in-difference specification (differentiating across time, treatments, and coffee vs. non-coffee communities) reduces the potential for competing explanations, but it is possible that some unobserved phenomenon correlates with each of the dimensions across which we are taking differences. That said, an examination of the consumption data does not support the presence of an omitted variable influencing CCTs efficacy.

7. Conclusion and Policy Implications

Oportunidades has been the leader, along with *Bolsa Familia* (Brazil's CCT), in the global design of CCT programs. But, for the most part, little effort has been made to maximize program returns with respect to ECD. As other Latin American countries implement and experiment with their own CCTs, there is the potential to improve the efficiency and equity of these programs. This article suggests that there may be significant returns to further targeting households who need extra support to cope with negative income shocks. These types of shocks are common in the rural areas that are often the focus of CCTs, and they may interact with program conditions to have unintended negative effects on ECD outcomes.

Our main finding is that, especially in poorer countries, CCTs can hurt rather than help young children in recipient households experiencing a consumption shock. We attribute this to the conditionality of the transfers, which requires school-age children to attend school. When not attending school, older children generate important labor earnings or participate in child care, contributions which take on added significance when income is exogenously reduced. In Mexico, where primary school-age children tend to attend school regardless of the family's circumstances and baseline consumption levels are higher, this potentially negative effect is mitigated, but in Nicaragua and Honduras there are indications that the physical development of young children was impaired during hard times for CCT recipient households. This negative impact might be mitigated if funds are increased to compensate for lost child labor, including both wage labor and help with childrearing.

CCTs have demonstrated large and overwhelmingly positive effects in a number of contexts, yet these policies needs to be carefully attuned to the potential intra-family tradeoffs associated with tough choices during hard times. One way to avoid these tradeoffs is to allow higher transfers to families with more children. Pro-fertility effects can be avoided by tying payments to family structure at time of program entry. Most importantly, it is clear that future CCT designs should try to ensure that the long-term benefits of human capital accumulation for school-age children are not achieved at the expense of ECD outcomes for younger siblings. This might involve higher payments in times of shock in poorer countries.

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Table 1: The Impacts of CCTs on Early Childhood Development Indicators (from Hoddinott and Bassett, 2008)

	PROGRESA	RPS	PROGRESA	PRAF Demand interventions	PRAF Demand and supply interventions	BA 1	BA 2
Stunting (change in prevalence)	-7.3	-5.5	-6.0	-0.3	-0.8		
	(4.4)*	(3.0)*	(7.2)				
Height-for-age Z score	0.16	0.13	0.14	-0.02	0.02		
	(0.10)	(0.09)	(0.09)				
Underweight (change in prevalence)	1.0	-6.2	0.4	0.5	0.3		
	(4.1)	(2.5)**	(3.6)				
Weight-for-age Z score	0.07		0.10	-0.02	0		
	(0.9)		(0.83)				
Weight (kg/month)						-0.03	0.27
						(0.007)**	
Duration of exposure to intervention (years)	1	2	1	2	2	0.5	
Age range	< 3 years	< 3 years	< 5 years	< 5 years	< 5 years	6 months to 3 years	
Additional controls, comments	Child fixed effects, age, food prices		Child fixed effects, age, food prices			Child random effects, age, receipt of *Bolsa Escola*	

Source: *PROGRESA* results are new estimates using the same model as Table 6, column (5) and calculating changes in prevalences by taking parameter estimate and multiplying by 100. *RPS* results are taken from Tables 4.17, 4.19, and 4.20, Maluccio and Flores (2005). *PRAF* results are taken from Tables 58-63 in IFPRI (2003). *BA* 1 results are taken from Morris et al. (2004). BA 2 results are taken from Olinto (2005).

Notes:

1. Unless otherwise indicated, program impact is defined as the difference-in-difference estimate of the "intent to treat" effect.
2. Standard errors (in parentheses) for *PROGRESA* and *RPS* intent to treat results are corrected for heteroskedasticity and clustering at the *localidad* and *comarca* level respectively. * significant at the 10 percent level; ** significant at the 5 percent level.

Table 2. Child Height-For-Age Z-Score by CCT Treatment Status and Community's Coffee Production

		Total		**Non-Coffee Communities**		**Coffee Communities**	
Program	Year	Control	Treatment	Control	Treatment	Control	Treatment
Nicaragua (RPS)	2000*	-1.78	-1.78	-1.63	-1.54	-1.91	-1.97
	2002	-1.8	-1.64	-1.64	-1.46	-1.95	-1.8
Honduras (PRAF)	2000*	-2.34	-2.28	-2.40	-2.27	-2.23	-2.26
	2002	-2.37	-2.26	-2.45	-2.30	-2.25	-2.17
Mexico	2000	-1.63	-1.61	-1.63	-1.58	-1.60	-1.79
(*Oportunidades*)	2003	-1.46	-1.51	-1.43	-1.47	-2.04	-1.81

Table 3. Per Capita Transfers Received by Treatment Status and Coffee Production

Program	Total		Non-Coffee Communities		Coffee Communities	
	Control	Treatment	Control	Treatment	Control	Treatment
Oportunidades	1869	2315	1890	2289	1477	2483
	(890)	(1139)	(898)	(1156)	(648)	(1025)

Mean (SD) in pesos deflated by monthly Mexican CPI.

Table 4. Sample Statistics by Program

	Mexico (*Oportunidades*)	**Honduras (PRAF)**	**Nicaragua (RPS)**
Change in HAZ, 2000 – 2002†	0.14 (0.93)	-0.40 (1.12)	-0.16 (0.76)
Share Treated	0.46	0.57	0.52
Per capita transfers received	2076 (1036)	NA	NA
% in coffee communities	0.09	0.28	0.49
Treatment X coffee cmty	0.06	0.10	0.26
Age at baseline (in days)	821 (213)	601 (338)	48 (8)
Share Female	0.50	0.49	0.49
Per capita baseline consumption	25 (15)	14.66 (13.35)	3.05 (1.85)
Female Household Head Schooling (years)	4.5 (2.8)	2.25 (2.19)	1.79 (2.19)
Household Size	5.7 (2.4)	6.78 (2.46)	6.99 (2.89)
N	473	2715	491

Figures are Mean (SD). †Mexican figures are 2000 – 2003.

Table 5. Sample Statistics by Community Type

	Mexico (*Oportunidades*)		Nicaragua (RPS)		Honduras (PRAF)	
	Non-coffee	Coffee	Non-coffee	Coffee	Non-coffee	Coffee
Baseline HAZ (2000)	-1.61 (0.06)	-1.73 (0.21)	-1.50 (1.08)	-1.78* (1.10)	-2.19 (1.37)	-1.99* (1.35)
Share Treated	0.44 (0.02)	0.69** (0.07)	0.51 (0.50)	0.52 (0.50)	0.64 (0.48)	0.38 (0.48)
Per Capita Transfers	2066 (1038)	2172** (1030)	n.a	n.a	n.a	n.a
Age at baseline (in days)	826 (10.3)	773 (32.0)	581.1 (270.9)	582.0 (275.10)	594.94 (339.47	616.49+ (334.36)
Share Female	0.50 (0.02)	0.52 (0.08)	0.52 (0.50)	0.45 (0.50)+	0.49 (0.50)	0.49 (0.50)
Per capita baseline consumption	28.3 (1.72)	22.2 (2.35)	3010.41 (1910.43)	3091.33 (1799.41)	14.16 (12.45)	15.96* (15.41)
Female Household Head Schooling	4.46 (0.13)	4.69 (0.47)	1.94 (2.23)	1.63+ (2.15)	2.20 (2.15)	2.38 (2.28)
Household Size	7.16 (0.14)	7.57 (0.46)	7.16 (3.18)	6.81 (2.56)	6.85 (2.46)	6.60 (2.46)
N	431	42	248	243	1945	749

Statistics are Mean(SE). ** = coffee vs. non-coffee community difference significant at 1% level
* = significant at 5% level + = significant at 10% level

Table 6. Effects of Being in a Coffee Community on the Change in Consumption Expenditures and Height for Age

	Mexico (*Oportunidades*)		**Nicaragua (RPS)**		**Honduras (PRAF)**	
	HAZ	PCC	HAZ	PCC	HAZ	PCC
Coffee Community	-0.29 (0.18)	3.80 (3.00)	-0.22** (0.07)	-14.08** (4.66)	-0.10* (0.05)	-6.90* (3.22)
N	473	442	491	429	2714	2195
R^2	0.05	0.03	0.03	0.05	0.21	0.02

PCC (per capita consumption) units are in local currency and are weekly in Mexico, yearly in Nicaragua, and daily in Honduras. Coefficients with standard errors in parentheses. Regression also includes child and household characteristics.
** = significant at 1% level * = significant at 5% level + = significant at 10% level

Table 7. Effects of Program Participation Interacted with Coffee Shock on Change in Height-for-Age Z-Score

	Mexico (*Oportunidades*)	Nicaragua (RPS)	Honduras (PRAF)
	HAZ	**HAZ**	**HAZ**
Treatment †	0.01	0.08	0.01
	(0.05)	(0.10)	(0.05)
Coffee Community	-0.76	-0.07	-0.04
	(0.30)*	(0.10)	(0.06)
Treat* Coffee†	0.22	-0.27*	-0.11
	(0.10)*	(0.14)	(0.09)
N	473	491	2714
R^2	0.06	0.05	0.21

Coefficients with standard errors in parentheses. †Instead of a binary "treatment" variable, in regressions on the *Oportunidades* data we use the amount of per capita transfers, so effects represent the marginal impact of an additional 1000 pesos of transfers per capita over the course of the program up to 2003. The interaction term is thus the transfer amount multiplied by the binary indicator of whether the household is in a coffee community.
** = significant at 1% level * = significant at 5% level $^+$ = significant at 10% level

Table 8. Program, Shock, and Interacted Effects Differentiated by Child's Gender

	Mexico (*Oportunidades*)		Nicaragua (RPS)		Honduras (PRAF)	
	Boys	Girls	Boys	Girls	Boys	Girls
Treatment	-0.01 (0.06)	0.05 (0.06)	-0.08 (0.14)	0.22 (0.13)	-0.01 (.06)	0.03 (0.07)
Coffee Community	-1.04 (0.36)**	-0.05 (0.83)	-0.14 (0.14)	-0.05 (0.14)	-0.05 (0.08)	-0.05 (0.09)
Treat* Coffee	0.33 (0.11)**	-0.15 (0.45)	-0.08 (0.19)	-0.44* (0.20)	-0.25* (0.12)	0.05 (0.13)
N	236	237	252	239	1388	1326
R^2	0.04	0.10	0.05	0.07	0.25	0.16

** = significant at 1% level * = significant at 5% level $^+$ = significant at 10% level

Table 9. Program, Shock, and Interacted Effects Differentiated by the Presence of Older Siblings

	MEXICO Per capita transfers		Nicaragua (RPS)		Honduras (PRAF)	
	No Older Sibling	No Older Sibling	No Older Sibling	Has Older Sibling(s)	No Older Sibling	Has Older Sibling(s)
Treatment	0.18 (0.21)	-0.03 (0.04)	0.06 (0.17)	0.08 (0.12)	0.03 (0.06)	0.00 (0.06)
Coffee Community	-0.58 (0.46)	-1.07 (0.38)**	-0.20 (0.18)	0.01 (0.12)	0.00 (0.08)	-0.101 (0.09)
Treat* Coffee	0.38 (0.28)	0.29 (0.11)*	-0.15 (0.25)	-0.34* (0.16)	-0.15 (0.12)	-0.09 (0.13)
Mean Effect	0.10 (0.20)	-0.56 (0.23)*	n.a	n.a	n.a	n.a
N	139	334	172	319	1466	1248
R^2	0.05	0.09	0.05	0.06	0.21	0.20

** = significant at 1% level * = significant at 5% level $^+$ = significant at 10% level

Figure 1. Data Collection Timeline