# Poverty Alleviation and Child Labor[♣]

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*Does child labor decrease as household income rises? We argue that the even small increases in income can influence child labor among children most vulnerable to transitioning from schooling to paid work. We find support for this hypothesis in Ecuador, where poor families are selected at random to receive a cash transfer that is equivalent to slightly less than 1/10 of monthly income for recipient households. The additional income has small effects on child time allocation at peak school attendance ages and among children already out of school at baseline. For children most likely to leave school for paid work, additional income is associated with a decline in work for pay away from the child's home. Declines in work for pay are associated with diminished school drop-out rates, especially for girls. The continuation of schooling is matched by an increase in schooling expenditures that appears to absorb most of the cash transfer. However, total household expenditures do not increase with the transfer and appear to fall in households most impacted by the transfer because of the decline in child labor.*

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## 1. Introduction

More than one in five children in the world work. Most of these working children reside in poor countries. This paper is concerned with the relationship in poor countries between current family economic status and whether the child works. There are two distinct strands of research. The first considers whether working while young influences current economic status through the economic contribution of children to the household (Manacorda 2006) and child labor's impact on local labor markets (Basu and Van 1998). The second examines whether and why current economic status influences the decision to send children to work. Understanding the influences of economic factors on child time allocation is important for the design of child labor related policy and for understanding the political economy of existing child labor regulation (Doepke and Zilibotti 2005). This second strand of research is the focus of this study, which examines child time allocation responses to experimental variation in family income from a cash transfer program in Ecuador.

In the recent literature on child labor responses to variation in economic status, there is a debate on the extent to which child labor responds to income among poor households. Basu, Das, and Dutta (2007) is a recent discussion of the state of this literature. The theoretical literature has emphasized parental preferences (Basu and Van 1998) and liquidity constraints (Baland and Robinson 2000) as two reasons why there might be a strong causal relationship between poverty and child labor. Empirical evidence faces the problem of establishing that causation runs from variation in economic status to time allocation decisions. Many correlates of family income influence the economic structure of the household, and a large literature documents the impact of employment opportunities open to children on child time allocation (e.g. Fafchamps and Wahba 2006; Kruger 2007; Manacorda and Rosati 2007; Rosenzweig and Evenson 1977; Schady 2004).

This study considers how child time allocation in Ecuador responds to receipt of the Bono de Desarrollo Humano (BDH) cash transfer. The evaluation of the Bono de Desarrollo

Humano (BDH) program randomly assigned cash transfers to some poor households and not to others. The BDH transfer is $15 per month, slightly less than 1/10 the monthly income of recipient households, and does not come with any conditions attached although the program was launched simultaneously with a social marketing campaign aimed at promoting human capital. We use the random assignment from the evaluation of the BDH as our source of variation in economic status in this study. Our sample consists entirely of BDH-eligible households who are in the poorest two quintiles of Ecuador.

We find that random assignment of the BDH income is associated with less work for pay, less involvement in the family farm or business, reduced school drop-out rates, and fewer children working in some way without attending school. Relative to the control population, market work declines as schooling increases, but domestic work appears positively associated with the increase in income. This increase in domestic work is small and insignificant. These findings are consistent with a growing literature that has documented that cash and in-kind transfers can reduce child work and protect school enrollment (e.g. Attanasio et al. 2006; Edmonds 2006; Filmer and Schady 2008; Ravallion and Wodon 1998; Schultz 2004).

Our paper adds to this literature in a number of important ways. First, we provide a theoretical framework which predicts that there should be substantial heterogeneity in program effects. Using an adaptation of the Basu and Van (1998) model, we show in section 3 that the effect on child labor of the BDH transfer should be concentrated among children most vulnerable to transitioning between school and work. Specifically, our hypothesis is that for young children, for whom schooling is less expensive and the opportunity cost of time in school low, additional income is likely to have little effect on child time allocation. For older children, already withdrawn from school and working, the cash transfer is likely too small to affect their time allocation as re-entry is rare (10 percent of children out of schooling and working re-enter school in the control population) and the opportunity and direct costs of schooling increase with age. However, for children in school and not working but facing the dramatic increase in schooling costs that comes with completing primary school in Ecuador and the rise in opportunity costs that comes with age, their time allocation is potentially substantially altered by small changes in non-child labor income.

The findings for work for pay are strongly consistent with this theory. The decline in work for pay associated with BDH receipt on average is concentrated in children most vulnerable to transitioning from work to school. Older children in poor households, especially girls who are at the end of primary school or higher in the baseline survey, are most likely to transition to work for pay and stop schooling in the control sample follow-up survey, taken 1.5 years later. It is this population of older children finishing primary or higher that experience the largest declines in work for pay and increases in schooling with the BDH transfer. These findings for work for pay and schooling are consistent with the results from the PROGRESA program in Mexico, which appears to have an impact on school enrollment primarily among children making the transition from primary to lower secondary school (e.g. Schultz 2004; de Janvry and Sadoulet 2006).

Second, we show that the decreases in child labor occurred despite the fact that the transfer was substantially smaller than the sum of the direct and opportunity costs of attending school. While the transfer is $15 per family per month, it is associated with an additional $9 per month spent on education for children most vulnerable to transitioning from schooling to work for pay outside of their family. Average monthly wages for a child working for pay are $84 per month.[1] Thus, the forgone child labor income is greater than the additional education spending. Taken together, we do not see any significant increases in family expenditures associated with additional BDH income. In fact, total family expenditures appear to decline for those whose work for pay is most impacted by the BDH transfer, those most vulnerable to transitioning from school to work for pay. This empirical result is also a prediction of the Basu and Van (1998) model. Families send their children to work for pay when they cannot afford their desired alternatives such as schooling without the child's economic contribution. Our findings suggest

[1] For children engaged in paid employment, their wages are 40 percent of their family's monthly expenditures in our data.

that the BDH transfer of $15 per month allows families to continue the schooling of many of the children most vulnerable to leaving schooling for work for pay even though the transfer does not fully compensate families for the forgone child labor income.

The BDH program is described in the next section and we consider its effect on child labor in detail in section 3. The main findings are presented in section 4. We document that the randomized increase in income is associated with increased schooling and decreased work for pay in those most vulnerable to transitioning from school to work. The changes in work and school are large enough that the net effect of the transfer for this population is to decrease total expenditures. Section 5 concludes. Our findings highlight the importance of schooling costs in the decision to send the child to work and illustrate considerable scope for small, targeted changes in family income to have large effects on the child labor situation.

## 2. Background on the BDH program and its evaluation

Ecuador has had a cash transfer program, the Bono Solidario, in place since 1998. Recipient households received $15 per month per family. While the intent of Bono Solidario was to assist poor families during an economic crisis, the program continued well past the economic crisis and the program was poorly targeted. Bono Solidario was replaced by Bono de Desarrollo Humano (BDH) beginning in mid 2003. A key difference between BDH and Bono Solidario is that BDH is explicitly means-tested. Starting in 2001, the government invested into developing a family means test, called the Selben Index. Only families in the poorest two quintiles of the Selben index are eligible to receive BDH's transfer of $15 per month. Another difference between the Bono Solidario and the BDH is that the launch of the BDH was accompanied with a social marketing campaign that encouraged households to invest in the human capital of their children. However, unlike other transfer programs in Latin America, BDH transfers have never been made explicitly conditional on specific investments in child human capital (for example, school enrollment).

The rollout of BDH explicitly contained a randomized component in 4 of Ecuador's 24 provinces. Within provinces selected for the evaluation, parishes (counties) were randomly drawn. Within these parishes, BDH eligible households were randomly sorted into BDH recipient households (lottery winners) and non-recipients (lottery losers).[2] Households formerly receiving Bono Solidario transfers were excluded from the evaluation. Lottery losers were taken off the roster of households that could be activated to receive BDH transfers. An important feature of this experiment is that, unlike the PROGRESA evaluation, randomization is at the household level, rather than the community level. That is, within a community, we observe both lottery winners and lottery losers.

The main sources of data used in this paper are the baseline and follow-up surveys designed for the BDH evaluation. Both surveys were carried out by an independent firm that had no association with the BDH program, namely, the Catholic University of Ecuador. The baseline survey was collected between June and August 2003, and the follow-up survey was collected between January and March 2005.[3]

The survey instrument included a roster of household members and information on, among other things, the level of schooling attained, marital status, and languages spoken by all adults; school enrollment, grade progression, paid work, unpaid work, and household chores of all children ages six to seventeen; an extensive module on household expenditures, which closely followed the structure of the 1998–99 ECV; and a module on dwelling conditions, ownership of durable goods, and access to public services. We aggregated expenditures into a consumption aggregate, appropriately deflated with regional prices of a basket of food items collected at the time of the surveys.

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[2] This was done by assigning households in the sample a normally distributed random number with mean zero and standard deviation one, and assigning all households with values zero or higher to the treatment group, and those with values below zero to the control group. Random assignment was done jointly by staff from the BDH and the World Bank.

[3] The dataset used in this version of the paper differs slightly from earlier drafts. 85 individuals (3 percent of the original sample) have been dropped from the present analysis as their data on age, gender, or completed schooling was inconsistent over rounds of the survey. Their exclusion or inclusion has little consequence for our findings.

Attrition over the study period was low: 94.1 percent of households were reinterviewed. Among households that attrited, most had moved and could not be found (4.2 percent), while in a few cases no qualified respondent was available for the follow-up survey despite repeat visits by the enumerator (1.0 percent) or the respondent refused to participate in the survey (0.5 percent). There is no relation between assignment to the study groups and attrition, and baseline differences between attrited and other households in per capita expenditures, assets, maternal education, and paternal education are small and insignificant. Attrited children were less likely to be enrolled at baseline, although this is largely driven by the fact that they were older.[4] Attrition is most likely to introduce biases in estimation when there are large differences between attrited and other households or when attrition is correlated with treatment status, and there is little evidence that this is the case in our data.

The randomization appears to have been successful in attaining balanced treatment and control populations. Table 1 summarizes background characteristics of children and their families in treatment and control populations. These data are from the baseline survey data collected between June and August 2003 before households were assigned to treatment and control populations. Most of the background characteristics in table 1 appear similar. The control population looks a little more likely to be female and urban than does the treatment population, but these differences are not significant at 10 percent.[5]

There appears to be considerable leakage of BDH into the control population. By design of the experiment, the control population was not supposed to receive the BDH. In reality 39

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[4] In a regression of a dummy variable for attrited households on a dummy variable for lottery winners, the coefficient is 0.054, with a robust standard error of 0.057. In a simple regression of baseline enrollment on a dummy variable for attrited households, with standard errors corrected for within-parish correlation, the coefficient is –0.083, with a robust standard error of 0.038. When a set of unrestricted child age dummies is included in the regression, the coefficient on the dummy variable for attrited children becomes insignificant: The coefficient is –0.033, with a robust standard error of 0.034. On the other hand, a joint test shows that the age dummies are clearly significant (*p* value of less than 0.001). The attrition rate for children 10 to 16 at baseline was similar to the household rate: 93.9 percent of children 10-16 interviewed at baseline were recaptured at follow-up.

[5] In an earlier version of this paper, we bifurcated the sample by urban-rural. We found similar patterns in rural and urban areas. Although the magnitudes of the results were slightly larger in rural areas, the urban - rural differences were not statistically significant.

percent receive it. The precise reasons for this contamination are unclear. Conversations with BDH administrators suggest that the list of households that had been randomly excluded from the program was not immediately passed on to operational staff activating households for transfers. This situation was corrected after a few weeks, but withholding transfers from households that had already begun to receive them was judged to be politically imprudent. Also, 32 percent of households assigned to the treatment group did not take up the program; lack of information, the cost of traveling to a bank, and stigma may all have discouraged some households from receiving transfers. The imperfect correspondence between lottery status and treatment status means that our empirical work later will need to be an intent to treat type of analysis.

Several other studies have considered the impact of BDH transfers: Paxson and Schady (2008) show that transfers improved the health and development of preschool-aged children, and Schady and Rosero (2008) show that a higher fraction of transfer income is used on food than is the case with other sources of income. Most directly related to this paper, Schady and Araujo (2008) show that the program had large effects on school enrollment rates. Though the transfers are small - 8.9 percent of expenditures in the median household – the impact they have on children seems to be large.

Time allocation is available in the baseline BDH evaluation data for children 6 to 16. Table 2 summarizes their time allocation in the baseline data by the child's treatment status. There are no statistically significant differences in activities at baseline between the treatment and control samples. Three-fourths of children attend school, and more than three fourths work in domestic work such as cooking, cleaning, and other household chores. Less than half participate in market work. Most that do are engaged in unpaid family work. Less than one in ten children participate in market work for pay. One in five children work in either domestic or market work without attending school. 40 percent of these working children who do not attend school work in paid work where the modal and median hours worked is 40 hours per week. Less than 15 percent of children working for pay combine that work for pay with school. 59 percent of children working on the family farm or business also attend school. 63 percent of children participating in domestic work attend school. Below, we will find a close correspondence between changes in paid employment and schooling that we do not observe for other forms of work.

## 3. The effect of the BDH on child labor supply

### 3.1 Set-up

In this section, we examine the response of child labor supply to the BDH transfer in a simple version of the model of child labor supply developed in Basu and Van (1998, hereafter BV). We consider the case of a two person household, one adult and one child.[6] The BV model is built on two explicit assumptions. First, child and adult labor are perfect substitutes subject to a productivity shifter. One child worker is equivalent to $\alpha$ adult workers, $\alpha < 1$. This is known as the substitution axiom. $w_c$ is the child's labor income, $w_A$ is adult labor income (adult labor supply is inelastic). Equilibrium between the child and adult labor markets and the substitution axiom, together, imply $w_c = \alpha w_A$.

Second, child labor occurs only if family income is very low. Denote $s_i$ as the perceived subsistence level of family $i$. $c_i$ is the family's consumption. This second assumption, the luxury axiom, is written by BV as:

$$\begin{aligned} (c_i, 0) &\succ (c_i + \delta, 1) \text{ if } c_i \geq s_i \\ (c_i + \delta, 1) &\succ (c_i, 0) \text{ if } c_i < s_i \end{aligned} \quad \text{eq. (1)}$$

[6] The BV model abstracts from interesting issues of intrahousehold decision-making and sibling interactions . While adding such nuances to our model would unquestionably make it a more complete characterization of child time allocation decisions, the central point of our discussion – the transfer's effect should be largest for children who have yet to drop out of school but are at school transition ages - does not require these additional complexities.

for all $\delta > 0$.[7] Education and the influence of the returns to education on child labor are not explicit within the BV set-up. Implicit is the assumption that the optimal use of child time is in education or some other alternative to work.

We think the luxury axiom characterization of preferences is most appropriately applied to the decision to send children to work outside of the family farm or business. Across countries, work for pay outside of the child's family is less prevalent and associated with lower school attendance rates than other forms of work. This is true for Ecuador as well. Less than 15 percent of children engaged in work for pay also attend school. Across countries, the lower school attendance rates among children who work for pay outside of the household can be explained by differences in hours worked (Edmonds 2007). In Ecuador, average hours worked among children working for pay is clumped at 8, 40, and 60 hours per week whereas reported hours worked for other types of work appears much less grouped. Work for pay outside of the household may be associated with less flexibility in hours worked and greater intensity for all of the reasons typically offered for the lumpiness of formal wage employment in the adult labor market. Families may view work for pay outside of the home differently than other forms of work because of its greater intensity directly, the implied incompatibility with schooling, or discomfort with having children working outside of the home. Hence, we use participation in work for pay outside of the family farm or business as our definition of child labor in the empirical portion of this study. Of course, we examine the transfer's effect on other forms of work as well in our empirical analysis.

The discontinuity in child labor supply implied by the luxury axiom is central to BV's proof of the possibility of multiple equilibriums for child labor supply in an economy.[8] Hence, the luxury axiom has been the focus of a large body of research. Tests of the luxury axiom

[7] While, the BV model frames the child labor decision in the language of preferences, it is trivial to recharacterize the model as being one where child labor is driven by liquidity constraints, as in the Baland and Robinson (2000) model.
[8] Swinnerton and Rogers (1999) emphasize that the multiple equilibrium result implicitly depends on the assumption that the distribution of non-labor income (specifically, rents to capital ownership) is sufficiently unequal.

typically look at whether child labor is income elastic. A negative income elasticity of child labor follows out of any Becker (1965) style time allocation model where leisure or an alternative use of time outside of work is a normal good (see for example, Cigno and Rosati 2005). In fact, the luxury axiom as codified in equation (1) does not imply that child labor is income elastic for all levels of income. Child labor does not respond to increases in income when such increases leave the household unable to cover subsistence costs, for example. Edmonds (2005) argues that, if we accept that households will vary in their perceptions of subsistence costs, the luxury axiom implies that the relationship between child labor and family living standards should be flat, then decline rapidly in the range of perceived subsistence levels. He documents that the changes in child labor in Vietnam in the 1990s are consistent with what would be implied by the luxury axiom.

The baseline data in the BDH evaluation are consistent with the basic pattern implied by equation (1) of rapid declines in child labor after a certain standard of living is attained. Figure 1 contains the plot of participation rates in work for pay outside of the household for children 6-16 against the log of annual per capita expenditures in the baseline, pre-BDH, data. The curve in Figure 1 is the result of a non-parametric (local-linear) regression.[9] The curve does not represent a causal relationship and should not be taken as a prediction to what will happen to work outside of the child's household as they grow wealthier or receive the BDH transfer (as we discuss below). Nonetheless, the shape of the curve in figure 1 is striking. From the bottom of the distribution until monthly per capita expenditures are approximately $20 per person per month (when the log of annual per capita expenditures is 5.5), participation rates in work for pay outside of the household are roughly flat, consistent with the luxury axiom. After $20 per person per month, participation rates decline very rapidly. In the wealthiest households in the evaluation sample (drawn from the poorest two quintiles in Ecuador), participation rates in market work outside of the household are one third of what they are for the poorest families.

[9] We follow Fan and Gijbels (1995) nearest neighbor adaptive bandwidth selection rule for 60 nearest neighbors (approximately 2 percent of sample). We use a Gaussian kernel.

Heterogeneity in perceived subsistence levels is necessary to reconcile preferences such as equation (1) with the data and seems realistic. We model the family's perception of basic subsistence needs as having three components: the costs of maintaining a child *h*, the costs of educating the child k, and an idiosyncratic perception that is normally distributed with a mean 0 and variance 1. Both the maintenance and education costs vary with the child's age. Denote the age of the child in family *i* as $a_i$. Hence, the family's perception of its subsistence needs can be written as:

$$s_i = h(a_i) + k(a_i) - \varepsilon_i \qquad \text{eq. (2)}$$

with $\varepsilon_i \sim N(0,1)$. Subsistence needs are the income at which a family can afford to not have the child work outside of the home. The idiosyncratic error term may be influenced by factors such as child ability, parental valuation of education, discount rate heterogeneity, etc. We assume that both *h* and *k* are everywhere differentiable and increasing in child age.

The assumption that maintenance costs increase with age follows from the increasing nutritional and energy requirements associated with puberty, physical growth, and increased physical intensity of work with age (over the ages relevant for our discussion). Schooling costs include school fees, schooling inputs, transport costs, etc. Conceptually, schooling costs may also include the family's perceived opportunity cost of child time in schooling if this opportunity cost is not determined by adult wages. Opportunity costs would increase with age with the child's ability to work and contribute productively. Direct schooling costs also increase with age. Figure 2 plots schooling costs per child by age for children that attend school in our data. Primary school is technically free in Ecuador, but families still face costs for transport, uniforms, learning materials, and some other fees. Secondary school is not free and, at secondary school ages, direct schooling expenditures per child appear to be double their primary school age level (part of this difference may owe to differences in which families send older children to school).

The family chooses consumption $c_i$ and whether the child works in the formal labor market. $e_i$ is an indicator that is 1 if the child works in the formal labor market and 0 otherwise. The household then chooses whether the child works according to (1) and subject to the budget constraint:

$$c_i \leq e_i w_c + w_A + t_i \qquad \text{eq. (3)}$$

where $t_i$ is the household's non-labor income. Child labor supply and consumption then depend on whether adult labor income and non-labor income are enough to cover subsistence expenses. Liquidity constraints, as in Baland and Robinson (2000), are implicit within this model. Assuming non-satiation in consumption and the substitution axiom:

$$c_i = \begin{cases} (w_A + t_i) & \text{if } w_A + t_i \geq s_i \\ ((1+\alpha) w_A + t_i) & \text{if } w_A + t_i < s_i \end{cases} \quad \text{eq. (4)}$$

$$e_i = \begin{cases} 0 & \text{if } w_A + t_i \geq s_i \\ 1 & \text{if } w_A + t_i < s_i \end{cases} \qquad \text{eq. (5).}$$

An interesting implication of this setup is that increases in transfer or adult labor income can result in declines in consumption. Assume non-satiation in consumption so that the budget constraint (3) holds with equality: $c_i = (1+e_i\alpha) w_A + t_i$. Suppose $w_A < s_i$ so that without the transfer, the child works and expenditures are: $c_i = (1+\alpha) w_A$. Suppose that the transfer is sufficient to switch the family from having the child work to not: $w_A + t_i \geq s_i$ . Expenditures are then $c_i = w_A + t_i$. If $s_i - w_A \leq t_i \leq \alpha w_A$, then household expenditures fall with the transfer relative to what expenditures would be absent the transfer. The transfer does not need to fully cover the costs associated with keeping the child out of work. The family is still better off, because they can afford to live without child labor.

### 3.2 The effects of the cash transfer

The probability a child works for pay is the probability that the family's income absent child labor is below subsistence. That is:

$$\begin{aligned}\Pr(e=1) &= \Pr\left(w_A + t_i < s_i\right)\\ &= \Pr\left(w_A + t_i < h(a_i) + k(a_i) - \varepsilon_i\right)\\ &= \Pr\left(\varepsilon_i < h(a_i) + k(a_i) - w_A - t_i\right)\end{aligned}$$

Given the assumption $\varepsilon_i \sim N(0,1)$, we have

$$\Pr(e_i = 1) = F\left(h(a_i) + k(a_i) - w_A - t_i\right) \qquad \text{eq. (6)}$$

where *F* is the cumulative normal.

Three factors that influence whether the child works are emphasized in equation (6): the child's age, adult wages, and the transfer. Totally differentiating, we have:

$$d\Pr(e_i = 1) = f\left(h(a_i) + k(a_i) - w_A - t_i\right)\left(\left(\frac{\partial h(a_i)}{\partial a} + \frac{\partial k(a_i)}{\partial a}\right)da - dw_A - dt_i\right) \quad \text{eq. (7)}$$

where *f()* is the standard normal density and is assumed to be everywhere positive. Higher maintenance and education costs increase the probability that we observe a child working. Additional adult income or non-labor transfers reduce the incidence of child labor.

The change in schooling costs (associated with age) in equation (7) is: $\left(\partial k(a_i)/\partial a\right)da$ . Changes in schooling costs have the same effect on the incidence in child labor as does an equivalently valued increase in transfers or adult income. Put another way, the implication of our set-up is that families consider whether they can afford to not send their children to work. They come to the same conclusion whether they are sufficiently rich or alternative uses of the child’s time are sufficiently inexpensive. This point is important in interpreting the empirical results later in this study. The BDH transfer affects child labor by increasing incomes absent child labor or lowering the perceived costs of schooling (because of misperceptions in some households that receipt of the transfer requires schooling, Schady and Araujo 2008). For either reason, the BDH transfer causes the decision to forgo work to become relatively more affordable.

There is substantial heterogeneity between children in the effect of the transfer. Assume that the transfer does not influence equilibrium wages in the local labor market. Holding age constant, the change in the incidence of child labor in a family that starts receiving the transfer is then:

$$d\,\Pr\left(e_i = 1\right) = -f\left(h\left(a_i\right) + k\left(a_i\right) - w_A\right) \qquad \text{eq. (8)}$$

The magnitude of the reduction in child labor with the BDH transfer depends on subsistence costs and adult income. Child labor supply will be more elastic to the transfer for households closer to the margin in the sense of $w_A \approx s_i$. That is, for families where adult income is well above subsistence, the transfer will not influence child labor supply, because subsistence needs is not a motive for child labor. For very poor households, with adult income well below the subsistence level, it is unlikely that a small transfer such as the BDH will be sufficient to afford keeping the child out of the labor force. The effect of the transfer should be largest for families with adult incomes that are close to subsistence.

The effects of the transfer will vary with the child's age. Differentiating equation (8) with respect to age, we find:

$$\frac{\partial\left( d\,\Pr\left(e_i = 1\right) \big/ dt_i \right)}{\partial a} = -f'\left(h\left(a_i\right) + k\left(a_i\right) - w_A\right)\left(\frac{\partial h\left(a_i\right)}{\partial a} + \frac{\partial k\left(a_i\right)}{\partial a}\right). \text{ eq. (9)}$$

*f′* is the derivative of the density function at its argument (the difference between average subsistence costs and adult wages at age *a*). Both maintenance and schooling costs increase in age. Whether the impact of the transfer increases or decreases with age depends on the child’s working status at baseline. Suppose the child works for pay at baseline, $h\left(a_i\right) + k\left(a_i\right) > w_A$.

This implies that $f' > 0$ and thereby $\partial\left(d\,\Pr\left(e_i = 1\right)/dt_i\right)\big/\partial a < 0$. Among children who are working, the impact of the transfer is smaller as they age. Alternatively, suppose children are not working at baseline, $h(a_i)+k(a_i)<w_A$. The effect of the transfer then gets larger as children age: $\partial\left(d\Pr(e_i=1)/dt_i\right)/\partial a>0$. Given that schooling costs appear to increase dramatically starting at age 12 and with the primary to secondary school transitions, this discussion suggests that the impact of the BDH should be largest for children who do not work and are at least 10 at baseline. Moreover, as the value of the normal density *f* is greatest when its argument is closest to zero, the impact of the transfer on children at the transition ages should be largest for poorer children whose adult income absent child labor is closer to subsistence, $w_A \approx s_i$.

An examination of the baseline data is useful to identify what ages are most likely to be affected by the BDH transfer. Figure 3 pools baseline data for both the treatment and control populations and plots participation rates at baseline by type of activity and age.[10] Paid market work does not appear in the dataset until age 12 and does not exceed 20 percent of children until age 15. Schooling is nearly universal ages 6 through 11 and begins to decline rapidly thereafter, with the largest declines occurring between ages 12 and 13. The age patterns in schooling and paid market work are more vivid than those for work categories that take place inside the child's own home. Overall, unpaid market work occurs in the family farm or business, and by age 8, nearly 40 percent of children participate in unpaid market work. The prevalence of domestic work grows rapidly between age 6 and 9, and appears to be relatively stable thereafter.

Figure 3 has two implications for our empirical work. First, given the age patterns in child time allocation it seems that there is little scope for a transfer to affect child involvement in paid work at ages 11 and under. Hence, we focus our analysis on children age 10 and above at baseline. Second, it is striking how the timing of the decline in schooling matches the growth in work for pay. This reflects the indivisibility of time in paid employment apparent in the data. 8.5 out of every 10 children in paid employment do not combine that work with school. Given that much of the variation in child time allocation with age in this population is from increases in

[10] Figure 3 ends at age 16 in the baseline data, because the evaluation did not collect time allocation information for children above 17 in the treatment period.

work for pay and decline in schooling, we anticipate that the largest impact of the transfer will be on work for pay, rather than participation in unpaid market work in the child's home.

# 4. Main Findings

## 4.1 Empirical Methods

Our empirical strategy approach follows from equation (6). Adult wages are determined by local labor markets. We treat the parish as the labor market and include parish fixed effects $\lambda_p$. Maintenance and education costs vary by child age and possibly gender. We include a full set of age dummies $\lambda_a$ and a gender effect *g*. Non-labor transfers are affected by winning the BDH lottery. In our reduced form approach, we include an indicator for whether the family won the BDH lottery *l* as our measure of $t_i$:

$$e_{ip} = \alpha + \lambda_p + \lambda_a + \beta g_i + \gamma_r l_i + \varepsilon_{ip} \qquad \text{eq. (10)}$$

where $\varepsilon_{ip}$ is an error term that is 0 in expectation conditional on the other controls listed in equation (10). Standard errors are clustered at the parish level.

In our preferred specification, we estimate equation (10), replacing $l_i$ with an indicator for whether the family receives the BDH transfer as our measure of $t_i$ (the transfer does not vary among recipients). 39 percent of control households report receiving the BDH transfer, despite their exclusion from it in the evaluation design. Hence, take-up of the BDH transfer is non-random but the lottery is random by design. The lottery nearly doubles the probability that a household reports receiving the BDH transfer. Hence, we use the lottery indicator $l_i$ as an instrumental variable for $t_i$. The assumption in the instrumental variable specification is that the fact of winning the lottery does not itself influence child labor decisions beyond its effect on BDH take-up. With the inclusion of parish fixed effects, our empirical approach only captures effects of the BDH that are net of any spillovers to the control population.

We examine the impact of winning the BDH lottery on child time allocation for each of the different indicators of child time allocation described in table 2. For each outcome, *e*, we report reduced forms of winning the lottery and instrumental variables estimates of the impact of receiving the transfer. Our discussion of age trends above suggests that we would be unlikely to find an effect of the cash transfer at ages below 12. On average, there is 1.5 years between pre and post baseline data. Conservatively, we then expect the treatment to only be evident in children as young as 10 in baseline..

Our theoretical discussion suggests that the effect of the transfer should be largest on the child on the margin between schooling and work. We identify marginal children in three ways. First, children working at baseline are revealed to have subsistence needs above adult income absent the transfer. With age, subsistence needs increase. It becomes less likely that the value of the transfer will be sufficient to allow families to avoid child labor. Given the small value of the transfer, we expect its effect on work to be largest among children who are not working at baseline but are vulnerable to transitioning to work. Our first additional restriction is to look at children age 10 and above who attend school without participating in paid employment at baseline. Second, children are most likely to transition from school to work at the end of primary as direct schooling costs increase dramatically and perhaps there is a labor market return to primary. Our second additional restriction (in addition to greater than age 10 and older at baseline, in school, and not engaged in paid employment) is to focus on children near or beyond the end of primary. We limit the sample to children who do not work in paid employment and are in grade four or higher at baseline. (There are six grades in primary school in Ecuador.)

Third, we block children by the probability that they transition from this no work / schooling group to paid employment. Specifically, for children 10 and older, we restrict the sample to the control population. We regress an indicator that a child participates in paid work in the post round on age effects, gender effects, urbanity, parish fixed effects, baseline time allocation, and a second order polynomial in the natural log of per capita expenditures that we allow to vary with age*gender.  We use the predicted probabilities from this regression to divide the entire evaluation sample (treatment and control) into three equally sized groups: those with low, middle, and high probabilities of transitioning to work for pay.

### 4.2 At School to Work Transition Ages, Market Work and Work for Pay Decline with Additional Income

Children age 10 and older who receive the additional BDH transfer income are less likely to work in market work, work for pay, unpaid market work (children can participate in multiple activities). They are more likely to work in domestic work. The increase in domestic work is smaller than the decline in market work, and is similar in magnitude to the decline in work for pay outside of the child's family. The probability that a child works without enrolling in school decreases. The probability that a child enrolls in school increases. These findings are in table 3 for the full sample age 10+ and separately for boys and girls.

Each cell in table 3 contains the result from a different regression. For each population grouping (e.g. full sample, male, female, etc.), the first line contains the reduced form coefficient on the lottery winner indicator from estimating equation (10) with the dependent variable indicated by the column header. The second line contains the coefficient from a separate regression of the dependent variable indicated by the column on an indicator for whether the family receives the BDH, with random assignment used as an instrumental variable. Thus, winning the BDH lottery is associated with an 8 percentage point decline in the probability a child age 10+ at baseline works in market work in the post round, a 13 percent decline in the treatment population relative to the control population mean in the follow-up period. Slightly less than a third of those that win the BDH lottery do not take-up the transfer, and more than one third who should not receive the transfer do so. The impact of actually receiving the BDH, correcting for the endogeneity in this decision, suggests that receipt of the BDH reduces market work by 24 percentage points or 40 percent. The BDH income increases school enrollment by 18 percentage points or 30 percent. The increases in school enrollment relative to the control population are largest for girls. Female enrollment increases by 28 percentage points, a 50 percent increase over the control population. Girls experience larger declines in work for pay than boys, but overall the decline in market work for girls is smaller than boys. Hence, relative to boys, the BDH income is associated with more girls combining work with school. In the control population in the follow-up period, girls are 30 percent more likely than boys to work without school..

Changes in market work, especially work for pay, associated with BDH income increase substantially when we focus our analysis on children that are most likely to drop out of school and start working. Table 4 and all remaining tables mimic the structure of table 3. While we present reduced form and IV results in the tables, our discussion focuses on the effect of BDH receipt, the IV results. We limit the population used to estimate table 4 to children 10 and older who attend school and do not participate in paid employment at baseline. As discussed above, these children are most likely to be affected by the transfer. In fact, we observe a 31 percentage point decline in market work and an 8 percentage point decline in the probability that a child participates in work for pay. 8 percent of the control population (as restricted in table 4) work in market work in the follow-up period. Hence, this is a 100 percent decline in paid employment. The magnitude of the increase in domestic work (although not statistically significant) also increases. This implies a degree of substitutability between work for pay and domestic work that is not typically found in other datasets (Edmonds 2007). Much of the greater impact of the transfer in table 4 relative to table 3 comes from an increased response of boys to the BDH income in the more restricted sample.

Magnitudes are slightly larger when we focus on children who are most likely to complete primary school or higher during our evaluation period. Table 5 presents our main findings of the impact of the BDH transfer for children 10 and older, in school in grade 4 or higher, and not in paid employment at baseline. Market work declines by nearly 32 percentage points and work for pay by nearly 10 percentage points (again, a 100 percent decline as the mean paid employment participation rate is 10 percent in this subsample of the control population). School enrollment is increased in this group by nearly 20 percentage points relative to the comparable population that does not receive the transfer, a 25 percent increase relative to the control sample.

We see much smaller effects of the BDH transfer in populations that we expect to be further from the margin of subsistence. These findings are in table 6. Children under 10 at baseline experience much smaller changes in time allocation compared to children 10 and over. This is to be expected as perceptions of subsistence will generally be lower because of lower maintenance costs and direct and indirect schooling costs. Hence, younger children are more likely to be in school regardless of the transfer. Interestingly, associated with the BDH, we observe an increase in children under 10 that work without attending school. These children are young children, not yet of school age, engaged in domestic work.

Older children that are away from the end of primary school are also less likely to be impacted by the transfer. They face lower direct schooling costs (and perhaps indirect costs if there is a return to primary completion). Hence, they are less likely to transition out of school to work in the 1.5 years between the pre and post periods. Changes in time use associated with BDH receipt for this population are in the bottom part of table 6. We observe declines in unpaid market work (work in the family farm or business). These declines are smaller in magnitude than observed in table 5. However, this early primary school group does not experience the changes in work for pay observed in the population that completes primary school during this evaluation period. That is, for this relatively less schooling-advanced population, the transfer seems to forestall participation in the family farm and business, but this group is not especially vulnerable to transitioning to work for pay.

The analysis associated with tables 4 through 6 is based on inferring who is most likely to be transitioning from schooling to work for pay, based on baseline age and school / work status. An alternative is to estimate the child's transition probability directly using the control sample and taking into account the child's family's baseline living standards. These results are in table 7 where we trifurcate children into groups based on the probability that they transition from schooling to work for pay during the evaluation period. While the declines in market work are similar in all groups, this is driven by declines in work in the family farm or business in most of the population. It is only the group that is most vulnerable to transition where the decline in work is concentrated in work for pay. Similarly, it is only this group with the highest probability of transitioning from school to work where we see large schooling effects of the transfer as well. Thus, the effect of the transfer on child labor and schooling can be very large, but its effects seem fairly concentrated in one segment of the population.

### 4.3 School Expenditures Increase but Per Capita Expenditures Do Not Increase Significantly with Additional Income

The BDH transfer is $15 per family per month. Our estimates imply that families spend an additional $42 per child per year on schooling as a result of the transfer. This is $4.7 per month of the school year (the school year is 9 months). With 1.8 children per recipient household, more than half of the transfer is spent on the direct schooling costs of children. These findings are in table 8.[11]

Table 8 mimics table 3 in its construction. Each cell in the table comes from a different regression. Instrumental variables are presented for the full sample and the sample bifurcated by gender. The first column contains the simple difference in annual school expenses per child in the post sample. The second column looks at changes in annual schooling expenditure per child between the pre and post periods. Given random assignment, it is expected that the first

[11] The observation that such a large fraction of the transfer appears to be spent on schooling seems surprising given the other studies of the BDH find large effects of the transfer on the health and nutritional status of pre-school age children (e.g. Paxson and Schady 2008). In reality, the families selected into the two evaluations are very different. The data used by Paxson and Schady is based on a sample frame that explicitly excluded families with school-aged children, and no data were collected on these families. The sample used in our study, by contrast, includes families with school age and pre-school age children. However, in our sample 62 percent of children age 10 and above at baseline do not have any pre-school age children in the household. Hence, the present study and those on pre-school age children largely examine the impact of the BDH on different families.

difference results look qualitatively similar to the double difference results in column 2, and we see this. Hence, we focus on the first difference results in column 1 for discussion. The increase in schooling expenditures is largest for girls. This is similar to the patterns in school enrollment observed in table 3.

Total expenditures do not appear to increase substantively with the cash transfer despite greater school expenditures.[12] The dependent variable in columns 3 and 4 is total annual household expenditures. In the full sample, an additional $15 per month is associated with a $213 reduction in annual expenditures, or $18 less per month. The decline in total expenditures is largest for households with girls; girls also experience larger increase in school expenses (column 1) and the largest decline in work for pay (tables 3-5).

In fact, the decline in expenditures appears largest in families of children that were most vulnerable to transitioning from schooling to work for pay. This finding is explicit in table 9 which mimics the trifurcation of the data from table 7. Schooling expenditures per child increase by $77 per child per year with BDH receipt ($8.5 per month, $15.4 per household per month) among these children whose probability of attending school increases by 43 percentage points. Annual household expenditures decline by $430, $36 per month. Thus, while schooling expenses increase considerably relative to the control population with these children whose schooling status is protected with the transfer, overall their family seems to forgo considerable consumption in order to send children to school rather than work for pay.

These expenditure results are consistent with the results from table 7. In households most likely to transition to work, the BDH is associated with an additional $77 in schooling expenses per year, or $8.5 per month per child. There are 1.8 children per household, so total expenditures on education rise by $15. However, this increase in education experiences comes with a decline in income. The average monthly wage for a child in the control population working for wages in the post period is $84 per month. The probability a child works for pay declines by 37

[12] Schooling expenditures are included in total expenditures.

percentage points for children most likely to transition to work. On average, then, a 37 percentage point decline in the probability that a child works earning $84 per month costs a household $31 in forgone income per child or $56 for its 1.8 children. The additional transfer income of $15 per month implies that total household income declines by $41 per month assuming no other behavioral changes. This is $5 above the $36 decline directly estimated in the data in table 9, one hundredth of the standard error on the estimated $36 decline. It is of course important to note that although expenditures are lower than in the control population, expenditures are higher than they would be if the child had foregone paid employment in setting without the BDH transfer.

The model of section 3 posits a simple explanation for this surprising observation. Absent the transfer, families do not perceive themselves as able to cover their subsistence needs without child labor. The BDH transfer makes it more affordable for the family to continue the child's schooling, especially among families most likely to transition children from school to work.

## 5. Conclusion

Work for pay among children in Ecuador is concentrated in the poorest households, and children appear to transition from school to work for pay starting at age 12. We find that a randomly assigned cash transfer maintains school enrollment and leads to a decline in work for pay among children vulnerable to transitioning from school to work. The declines in work for pay and increases in schooling, relative to the control population, are largest for girls. The additional income appears to have little influence on child time allocation or schooling related expenditures for children below school transition ages or already working and out of school in our baseline data. Among children vulnerable to transitioning from school to work, we observe a substantial increase in school related expenditures. Most of the cash transfer appears to be spent on schooling in this population. Despite increased school expenditures, the decline in work for pay is large enough that total expenditures decline in families with children whose schooling is prolonged by the transfer. The decline in total expenditures is very close in magnitude to the forgone income implied by our estimates of the decline in work for pay.

The Basu and Van (1998) set-up offers a simple theoretical interpretation of these results. It posits that children work when families feel they cannot afford alternatives to that work. For families vulnerable to transitioning from school to work, the transfer improves their ability to afford schooling. Hence, they forgo the child labor income, total expenditures decline, and families are better off as a result.

It is striking that for children vulnerable to transitioning from school to work, families appear to use all of the transfer to support child schooling and defer the transition to work for pay. To the extent that this educational investment is productive and multiplier effects from education are substantive, our finding suggests potentially very high aggregate returns to small, well-targeted transfers. At least in our present case, the transfer does not even cover the full direct and opportunity cost associated with schooling.

But why would families invest so heavily to sustaining the education of these children at transition ages? The Basu-Van set-up frames the answer in terms of preferences, but why might preferences be such? The answer might depend on whether the transfer is perceived as transitory or permanent. If it is transitory, our findings are consistent with education as the highest return savings vehicle available to these poor families. If permanent, our findings may reflect nothing more than parental preferences (education or the absence of child labor brings utility directly) or something about liquidity constraints (education is the best long-term investment available and liquidity constraints were constraining investment). Of course, surrounding the transfer program is a social marketing campaign promoting investments in child human capital. It is possible that the results herein reflect a behavioral response to a combination of the cash transfer and the social marketing campaign. We do not have a comparable experiment to suggest how families would treat a similarly sized lottery award without the social marketing context. Understanding why families appear to prioritize schooling and the absence of child labor to the extent observed here is an interesting avenue for future research.

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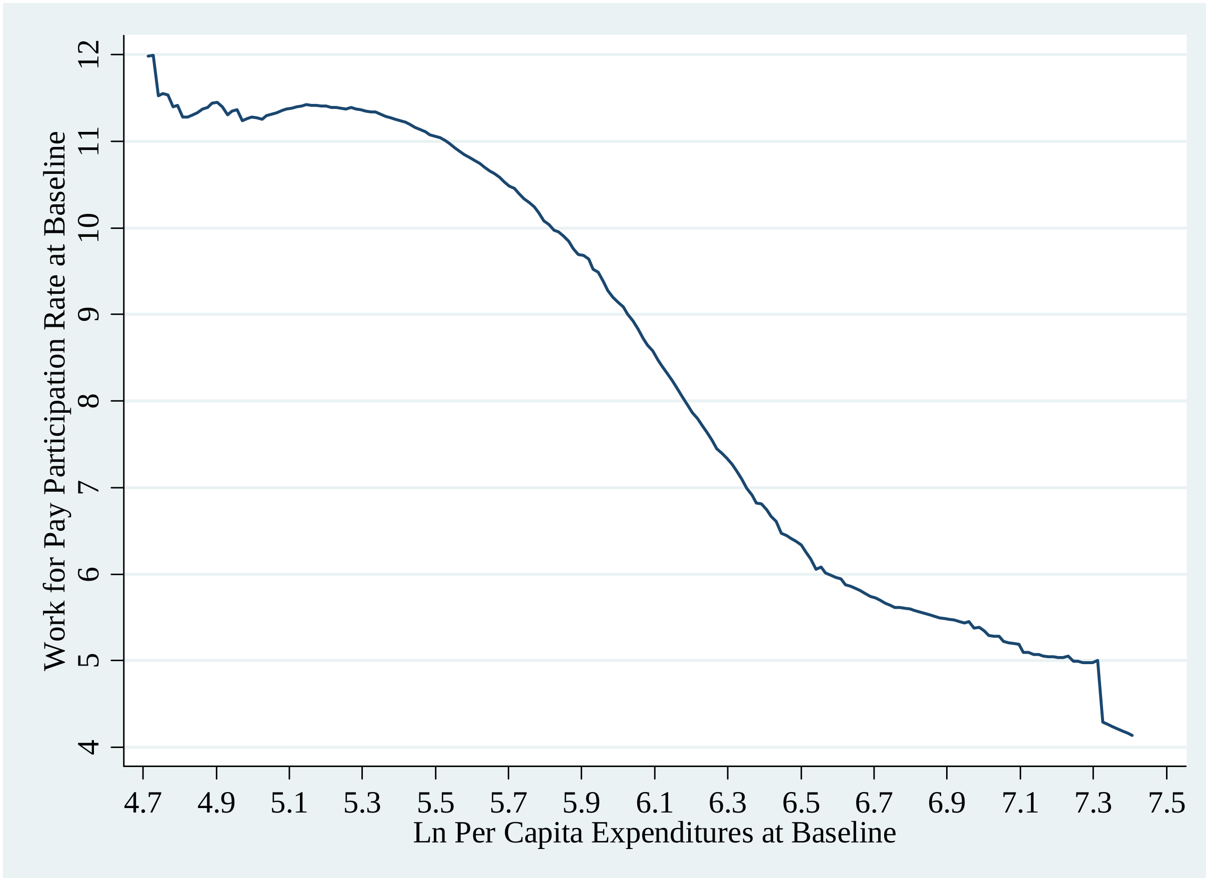


***Figure 1: Work for Pay outside of the Household and Per Capita Expenditures at Baseline***
Local Linear Regression with 28 (1% of sample) Nearest Neighbor Adaptive Bandwidth Selection

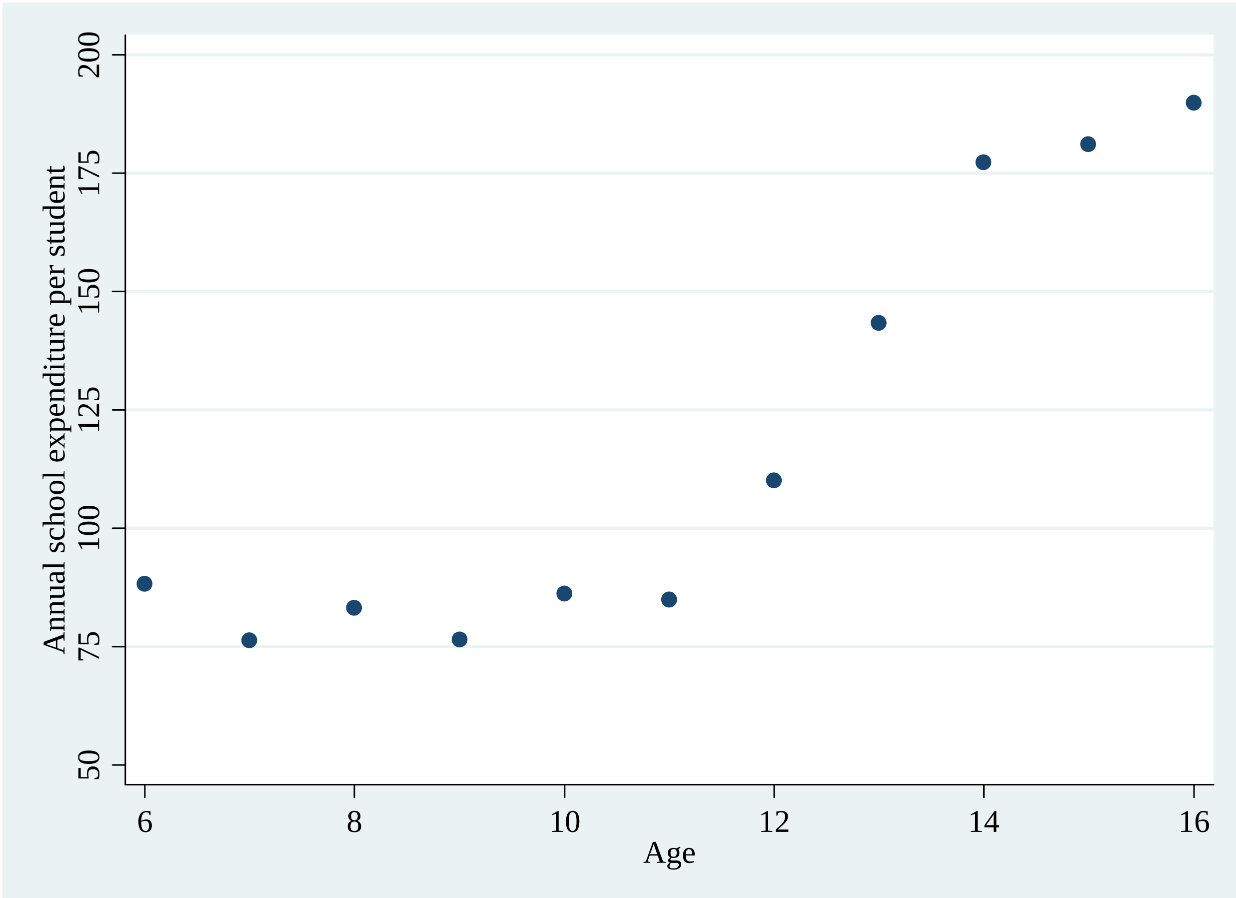

***Figure 2: Annual School Expenditure for Enrolled Students by Age at Baseline***

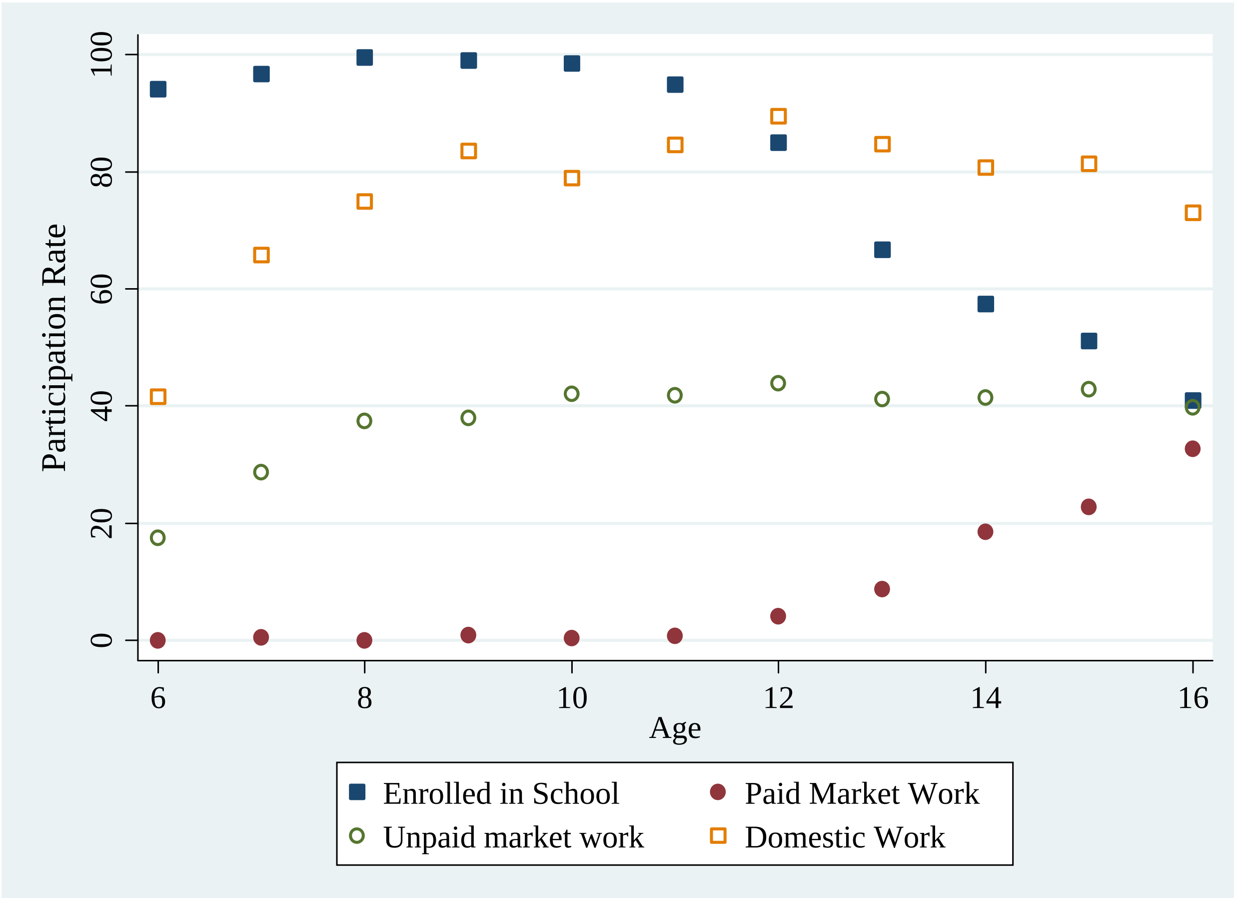


***Figure 3: School, Paid and Unpaid Market Work, and Domestic Work by Age at Baseline***

**Table 1: Child and Family Background Characteristics at Baseline**

| Variable | Treatment | Control |
|---|---|---|
| Parishes | 34 | 43 |
| Households | 685 | 598 |
| Children | 1456 | 1335 |
| Age | 11.65 | 11.59 |
| Male | 0.49 | 0.52 |
| Ever Married | 0.00 | 0.00 |
| Disabled | 0.01 | 0.01 |
| Indigenous | 0.10 | 0.08 |
| Father's Education | 4.76 | 4.61 |
| Mother's Education | 3.81 | 3.70 |
| Household Size | 6.34 | 6.29 |
| Log Per Capita Expenditures | 5.97 | 5.99 |
| Rural | 0.53 | 0.48 |
| Receives BDH | 0.68 | 0.39 |

Aside from the indicator that the child's family receives BDH, no treatment-control differences are significant at 10% or less. Sample restricted to BDH evaluation households with children 6 to 16 at baseline.

**Table 2: Child Time Allocation at Baseline**

| | Full Sample 6-16 | | Age 10 and older | |
|---|---|---|---|---|
| Variable | Treatment | Control | Treatment | Control |
| Enrolled in School | 0.77 | 0.77 | 0.71 | 0.71 |
| Highest Grade Completed | 4.62 | 4.59 | 5.67 | 5.60 |
| Any Market Work | 0.47 | 0.46 | 0.52 | 0.51 |
| Paid Market Work | 0.09 | 0.09 | 0.12 | 0.12 |
| Unpaid market work | 0.40 | 0.39 | 0.43 | 0.42 |
| Domestic Work | 0.80 | 0.79 | 0.82 | 0.83 |
| Works without school | 0.21 | 0.21 | 0.27 | 0.28 |
| Sample Size | 1456 | 1335 | 1083 | 994 |

No treatment-control differences are significant at 30 percent or lower.

Works without school indicate that a child participats in market work or domestic work without attending school

**Table 3: Impact of the BDH on Time Allocation**
Children 10 and older at baseline

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Market Work | Work for Pay | Unpaid Market Work | Domestic Work | Work w/o School | Enrolled in School |
| Full Sample (2077 children) | | | | | | |
| Randomization (Reduced Form) | -0.0778** | -0.0228 | -0.0667** | 0.0157 | -0.0634** | 0.0581** |
| | (0.0231) | (0.0166) | (0.0203) | (0.0196) | (0.0196) | (0.0222) |
| Receives BDH (2SLS) | -0.244** | -0.0716 | -0.209** | 0.0492 | -0.199** | 0.182** |
| | (0.0719) | (0.0537) | (0.0608) | (0.0634) | (0.0652) | (0.0764) |
| Male (1030 Children) | | | | | | |
| Randomization (Reduced Form) | -0.0941** | -0.0140 | -0.0778** | 0.0161 | -0.0338 | 0.0226 |
| | (0.0333) | (0.0304) | (0.0246) | (0.0279) | (0.0348) | (0.0377) |
| Receives BDH (2SLS) | -0.294** | -0.0439 | -0.243** | 0.0505 | -0.106 | 0.0707 |
| | (0.105) | (0.0962) | (0.0705) | (0.0900) | (0.108) | (0.118) |
| Female (1047 children) | | | | | | |
| Randomization (Reduced Form) | -0.0604** | -0.0288 | -0.0504* | 0.0180 | -0.0906** | 0.0875** |
| | (0.0255) | (0.0213) | (0.0268) | (0.0221) | (0.0272) | (0.0314) |
| Receives BDH (2SLS) | -0.191** | -0.0910 | -0.159* | 0.0569 | -0.286** | 0.276** |
| | (0.0798) | (0.0657) | (0.0865) | (0.0728) | (0.0845) | (0.105) |

Notes:

1 Each cell contains the result from a different regression. The column indicates the dependent variable. The coefficient labeled "randomization" is the reduced form coefficient on an indicator that the household won the BDH lottery (equation 10 in the text). Receives BDH reports the coefficient on an indicator that the child's family receives the BDH, using the random assignment as an instrumental variable.

2 All regressions include parish fixed effects, a vector of age dummies, and controls for gender and urbanity when feasible.

3 Standard errors in parentheses. Standard errors corrected for Parish level clustering.

4 * $p<0.10$, ** $p<0.05$

**Table 4: Impact of the BDH on Time Allocation for children not working and in school at baseline**

Children 10 and older, in school, and not engaged in paid employment at baseline

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Market Work | Work for Pay | Unpaid Market Work | Domestic Work | Work w/o School | Enrolled in School |
| Full Sample (1432 children) | | | | | | |
| Randomization (Reduced Form) | -0.0977** | -0.0263* | -0.0704** | 0.0263 | -0.0531** | 0.0462* |
| | (0.0274) | (0.0147) | (0.0244) | (0.0188) | (0.0221) | (0.0236) |
| Receives BDH (2SLS) | -0.314** | -0.0844* | -0.227** | 0.0845 | -0.171** | 0.149* |
| | (0.0839) | (0.0509) | (0.0696) | (0.0637) | (0.0725) | (0.0804) |
| Male (722 Children) | | | | | | |
| Randomization (Reduced Form) | -0.126** | -0.0306 | -0.0757** | 0.0392 | -0.0377 | 0.0284 |
| | (0.0362) | (0.0255) | (0.0330) | (0.0273) | (0.0301) | (0.0336) |
| Receives BDH (2SLS) | -0.416** | -0.101 | -0.251** | 0.130 | -0.125 | 0.0942 |
| | (0.107) | (0.0875) | (0.0988) | (0.0982) | (0.101) | (0.112) |
| Female (710 children) | | | | | | |
| Randomization (Reduced Form) | -0.0732** | -0.0307** | -0.0558* | 0.0265 | -0.0706** | 0.0675** |
| | (0.0306) | (0.0153) | (0.0298) | (0.0218) | (0.0301) | (0.0315) |
| Receives BDH (2SLS) | -0.219** | -0.0917* | -0.167* | 0.0791 | -0.211** | 0.202** |
| | (0.0896) | (0.0480) | (0.0858) | (0.0691) | (0.0874) | (0.0984) |

Notes:

1 Each cell contains the result from a different regression. The column indicates the dependent variable. The coefficient labeled "randomization" is the reduced form coefficient on an indicator that the household won the BDH lottery (equation 10 in the text). Receives BDH reports the coefficient on an indicator that the child's family receives the BDH, using the random assignment as an instrumental variable.

2 All regressions include parish fixed effects, a vector of age dummies, and controls for gender and urbanity when feasible.

3 Standard errors in parentheses. Standard errors corrected for Parish level clustering.

4 * p<0.10, ** p<0.05

**Table 5: Impact of the BDH on Time Allocation for children that do not work and are close to the end of primary school**

Children 10 and older, in school in grade 4 or higher, and not paid employment at baseline

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Market Work | Work for Pay | Unpaid Market Work | Domestic Work | Work w/o School | Enrolled in School |
| Full Sample (1233 children) | | | | | | |
| Randomization (Reduced Form) | -0.0998** | -0.0312** | -0.0661** | 0.0278 | -0.0650** | 0.0586** |
| | (0.0293) | (0.0160) | (0.0273) | (0.0202) | (0.0208) | (0.0229) |
| Receives BDH (2SLS) | -0.318** | -0.0996* | -0.211** | 0.0887 | -0.207** | 0.187** |
| | (0.0872) | (0.0553) | (0.0774) | (0.0665) | (0.0679) | (0.0796) |
| Male (609 Children) | | | | | | |
| Randomization (Reduced Form) | -0.114** | -0.0401 | -0.0506 | 0.0486 | -0.0628** | 0.0559 |
| | (0.0385) | (0.0297) | (0.0372) | (0.0331) | (0.0306) | (0.0354) |
| Receives BDH (2SLS) | -0.365** | -0.129 | -0.163 | 0.156 | -0.202** | 0.180 |
| | (0.113) | (0.0987) | (0.113) | (0.114) | (0.0990) | (0.115) |
| Female (624 children) | | | | | | |
| Randomization (Reduced Form) | -0.0954** | -0.0339** | -0.0755** | 0.0216 | -0.0665** | 0.0609* |
| | (0.0339) | (0.0171) | (0.0338) | (0.0216) | (0.0314) | (0.0312) |
| Receives BDH (2SLS) | -0.286** | -0.102* | -0.227** | 0.0648 | -0.200** | 0.183* |
| | (0.0971) | (0.0554) | (0.0934) | (0.0678) | (0.0916) | (0.0970) |

Notes:

1 Each cell contains the result from a different regression. The column indicates the dependent variable. The coefficient labeled "randomization" is the reduced form coefficient on an indicator that the household won the BDH lottery (equation 10 in the text). Receives BDH reports the coefficient on an indicator that the child's family receives the BDH, using the random assignment as an instrumental variable.

2 All regressions include parish fixed effects, a vector of age dummies, and controls for gender and urbanity when feasible.

3 Standard errors in parentheses. Standard errors corrected for Parish level clustering.

4 * $p<0.10$, ** $p<0.05$

**Table 6: Impact of the BDH on Time Allocation, Counterfactuals**
Various subsamples

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Market Work | Work for Pay | Unpaid Market Work | Domestic Work | Work w/o School | Enrolled in School |
| Children under 10 at baseline (714 children) | | | | | | |
| Randomization (Reduced Form) | -0.0289 | 0.00507 | -0.0353 | 0.0205 | 0.0211** | -0.00993 |
| | (0.0350) | (0.00666) | (0.0353) | (0.0400) | (0.00988) | (0.0130) |
| Receives BDH (2SLS) | -0.117 | 0.0205 | -0.143 | 0.0827 | 0.0853** | -0.0401 |
| | (0.125) | (0.0275) | (0.123) | (0.170) | (0.0423) | (0.0520) |
| Children 10 and older, in school in grade 3 or lower, and not in paid employment at baseline (199 children) | | | | | | |
| Randomization (Reduced Form) | -0.0853 | 0.00111 | -0.0912 | 0.0456 | 0.0335 | -0.0486 |
| | (0.0755) | (0.0143) | (0.0734) | (0.0610) | (0.0648) | (0.0648) |
| Receives BDH (2SLS) | -0.245 | 0.00319 | -0.262 | 0.131 | 0.0964 | -0.140 |
| | (0.222) | (0.0410) | (0.216) | (0.182) | (0.184) | (0.180) |

Notes:

1 Each cell contains the result from a different regression. The column indicates the dependent variable. The coefficient labeled "randomization" is the reduced form coefficient on an indicator that the household won the BDH lottery (equation 10 in the text). Receives BDH reports the coefficient on an indicator that the child's family receives the BDH, using the random assignment as an instrumental variable.

2 All regressions include parish fixed effects, a vector of age dummies, and controls for gender and urbanity.

3 Standard errors in parentheses. Standard errors corrected for Parish level clustering.

4 * $p<0.10$, ** $p<0.05$

**Table 7: Effect of BDH on time allocation by estimated transition probabilities**
Children 10 and older at baseline

| | (1) Market Work | (2) Work for Pay | (3) Unpaid Market Work | (4) Domestic Work | (5) Work w/o School | (6) Enrolled in School |
|---|---|---|---|---|---|---|
| Highest probability of transitioning from schooling to paid employment (692 children) | | | | | | |
| Randomization (Reduced Form) | -0.0792* | -0.118** | 0.00205 | 0.0390 | -0.144** | 0.137** |
| | (0.0433) | (0.0412) | (0.0334) | (0.0363) | (0.0420) | (0.0489) |
| Receives BDH (2SLS) | -0.245* | -0.365** | 0.00634 | 0.121 | -0.447** | 0.426** |
| | (0.134) | (0.130) | (0.104) | (0.115) | (0.140) | (0.162) |
| Middle third of transition probabilities (692 children) | | | | | | |
| Randomization (Reduced Form) | -0.0993** | 0.00171 | -0.106** | 0.0141 | -0.0400 | 0.0394 |
| | (0.0285) | (0.0222) | (0.0284) | (0.0289) | (0.0259) | (0.0294) |
| Receives BDH (2SLS) | -0.316** | 0.00543 | -0.336** | 0.0447 | -0.127 | 0.125 |
| | (0.0907) | (0.0707) | (0.0862) | (0.0936) | (0.0831) | (0.0958) |
| Lowest transition probability (693 children) | | | | | | |
| Randomization (Reduced Form) | -0.0679* | 0.0210 | -0.0815** | -0.00314 | -0.0436 | 0.0250 |
| | (0.0364) | (0.0136) | (0.0361) | (0.0314) | (0.0382) | (0.0407) |
| Receives BDH (2SLS) | -0.218** | 0.0674 | -0.262** | -0.0101 | -0.140 | 0.0804 |
| | (0.101) | (0.0493) | (0.106) | (0.100) | (0.120) | (0.133) |

Notes:

1. Each cell contains the result from a different regression. The column indicates the dependent variable. The coefficient labeled "randomization" is the reduced form coefficient on an indicator that the household won the BDH lottery (equation 10 in the text). Receives BDH reports the coefficient on an indicator that the child's family receives the BDH, using the random assignment as an instrumental variable.
2. All regressions include parish fixed effects, a vector of age dummies, and controls for gender and urbanity.
3. The full sample of children age 10 and older at baseline is trifurcated based on the predicted probability the child starts working in paid market work outside of the family's home during the post survey. The predicted probability is computed by using the control sample to regress an indicator that a child works for pay in the post round on parish fixed effects, urbanity, indicators for the child's time allocation at baseline and whether the child attended grade 5 or higher at baseline, gender effects, age effects, and a second order polynomial in baseline per capita expenditures that is allowed to vary for each age-gender group. The entire post population is then divided into three categories based on the resulting predicted probabilities from this regression. "Lowest" indicates the bottom 33 percent of predicted values, "middle" refers to the middle third of predicted probabilities, and "highest" refers to the top third of the population most likely to start working for pay between the baseline and post rounds.
4. Standard errors in parentheses. Standard errors corrected for Parish level clustering.
5. * p<0.10, ** p<0.05

**Table 8: Impact of BDH on Household Expenditures and Changes in Household Expenditures**
Children 10 and older at baseline

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Per Child School Expenditures | | Total Household Expenditures | |
| | First Difference | Difference in Differences | First Difference | Difference in Differences |
| Full Sample | | | | |
| Randomization (Reduced Form) | 13.28* | 13.85* | -68.01 | -53.67 |
| | (7.658) | (7.080) | (106.2) | (106.6) |
| Receives BDH (2SLS) | 41.64* | 43.40** | -213.3 | -168.5 |
| | (23.62) | (21.04) | (338.2) | (340.0) |
| Male | | | | |
| Randomization (Reduced Form) | 3.720 | 8.542 | -8.135 | 4.444 |
| | (9.568) | (8.630) | (102.9) | (106.6) |
| Receives BDH (2SLS) | 11.63 | 26.28 | -25.42 | 13.89 |
| | (29.91) | (26.39) | (321.6) | (333.0) |
| Female | | | | |
| Randomization (Reduced Form) | 18.56 | 15.46 | -112.0 | -87.45 |
| | (11.16) | (10.77) | (148.5) | (144.6) |
| Receives BDH (2SLS) | 58.57* | 48.85 | -353.5 | -277.0 |
| | (32.55) | (31.39) | (480.5) | (471.4) |

Notes:

1 Each cell contains the result from a different regression. The column indicates the dependent variable. The coefficient labeled "randomization" is the reduced form coefficient on an indicator that the household won the BDH lottery (equation 10 in the text). Receives BDH reports the coefficient on an indicator that the child's family receives the BDH, using the random assignment as an instrumental variable. Columns labeled first difference use only expenditure data from the follow-up survey. Columns labeled difference in differences report changes in expenditures from baseline to follow-up. See table 3 for sample sizes.

2 All regressions include parish fixed effects, a vector of age dummies, and controls for gender and urbanity when feasible.

3 Standard errors in parentheses. Standard errors corrected for Parish level clustering.

4 * p<0.10, ** p<0.05

**Table 9: Impact of BDH on Household Expenditures by estimated transition probabilities**

Children 10 and older at baseline

| | (1) Per Child School Expenditures | (2) Total Household Expenditures |
|---|---|---|
| Highest probability of transitioning from schooling to paid employment | | |
| Randomization (Reduced Form) | 24.90** | -138.7 |
| | (12.18) | (147.4) |
| Receives BDH (2SLS) | 77.19** | -430.0 |
| | (37.59) | (470.3) |
| Middle third of transition probabilities | | |
| Randomization (Reduced Form) | 12.76 | -73.07 |
| | (12.60) | (155.5) |
| Receives BDH (2SLS) | 40.60 | -232.5 |
| | (37.83) | (495.8) |
| Lowest transition probability | | |
| Randomization (Reduced Form) | 10.77 | 49.45 |
| | (14.96) | (135.5) |
| Receives BDH (2SLS) | 34.62 | 158.9 |
| | (49.16) | (430.2) |

Notes:

1 Each cell contains the result from a different regression. The column indicates the dependent variable. The coefficient labeled "randomization" is the reduced form coefficient on an indicator that the household won the BDH lottery (equation 10 in the text). Receives BDH reports the coefficient on an indicator that the child's family receives the BDH, using the random assignment as an instrumental variable. See table 7 for sample sizes.

2 All regressions include parish fixed effects, a vector of age dummies, and controls for gender and urbanity.

3 The full sample of children age 10 and older at baseline is trifurcated based on the predicted probability the child starts working in paid market work outside of the family's home during the post survey. The predicted probability is computed by using the control sample to regress an indicator that a child works for pay in the post round on parish fixed effects, urbanity, indicators for the child's time allocation at baseline and whether the child attended grade 5 or higher at baseline, gender effects, age effects, and a second order polynomial in baseline per capita expenditures that is allowed to vary for each age-gender group. The entire post population is then divided into three categories based on the resulting predicted probabilities from this regression. "Lowest" indicates the bottom 33 percent of predicted values, "middle" refers to the middle third of predicted probabilities, and "highest" refers to the top third of the population most likely to start working for pay between the baseline and post rounds.

4 Standard errors in parentheses. Standard errors corrected for Parish level clustering.

5 * $p<0.10$, ** $p<0.05$