Poverty Alleviation and Child Labor

By ERIC V. EDMONDS AND NORBERT SCHADY*

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Poor women with children in Ecuador were selected at random for a cash transfer equivalent to 7 percent of monthly expenditures. The transfer is greater than the increase in schooling costs at the end of primary school, but it is less than 20 percent of median child labor earnings in the labor market. Poor families with children in school at the time of the award use the extra income to postpone the child's entry into the labor force. Students in families induced to take-up the cash transfer by the experiment reduce their involvement in paid employment by 78 percent and unpaid economic activity inside their home by 32 percent. These declines in economic activity are accompanied by an increase in time in unpaid household services, but overall time spent working declines.

* Edmonds: Department of Economics, Dartmouth College, 6106 Rockefeller Hall, Hanover NH 03755 USA (email: eedmonds@dartmouth.edu); Schady: Inter-American Development Bank, 1300 New York Avenue, NW, Washington DC 20577 USA (email: norberts@iadb.org). The views in this paper do not necessarily reflect those of the Inter-American Development Bank, its Executive Directors, or the countries they represent. We appreciate the helpful comments of Caridad Araujo, Kathleen Beegle, Milo Bianchi, Francisco Ferreira, John Giles, Sylvie Lambert, Marco Manacorda, David McKenzie, Christopher Snyder, Zafiris Tzannatos, and especially Hilary Hoynes and our referees, as well as seminar participants at Cornell, Georgetown, Maryland, Paris School of Economics, Ohio State, University of California at Davis, and Vanderbilt and conference participants at the IZA/World Bank Employment in Development Conference, Juan Carlos III / UCW Conference on the Transition from School to Work, NBER Summer Institute, and NEUDC. We thank Isabel Beltran, Ryan Booth, and Jennifer Chong for help preparing the data.

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

In the recent literature on child labor responses to variation in economic status, reviewed in the next section, there is a debate on the extent to which child time allocation responds to income among poor households. There are many theoretical reasons why poverty may cause child labor. However, the time allocation of children and household incomes are joint outcomes of a single decision-making process, so establishing a causal relationship is difficult. Many correlates of family income influence the economic structure of the household, raising omitted variables concerns.

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 BDH randomly assigned eligibility for the cash transfer to some poor households and not to others. The BDH transfer is $15 per household per month, 7 percent of monthly expenditures for recipient households. The amount does not vary across eligible families. The transfer is paid to mothers, and does not have any conditions attached. We use the random assignment from the evaluation of the BDH as our source of variation in economic status.

We show that $15 a month given as the BDH reduces child labor. These results are consistent with a growing literature that documents that cash and in-kind transfers can reduce child work, but are novel in several ways. First, the BDH results in reductions in all kinds of work, including paid employment. The declines in paid employment are large, and are concentrated among children who were students at the time of random assignment. Lottery-induced take-up of the BDH is associated with a 41 percent decline in paid employment for the full sample, and a 78 percent decline for children who were students at baseline. The focus on paid employment is novel because earlier research has largely focused on unpaid economic activity in the family farm or business.

Second, households in the present study were not given a strong financial incentive to send children to school. This distinguishes our findings from those based on PROGRESA in Mexico and other conditional cash transfer programs, where the structure of the transfer changes the relative cost of schooling. The government of Ecuador carried out a social marketing campaign that stressed the importance of investments in human capital at the time the BDH was launched, but there was no attempt to enforce schooling attendance or other actions related to human capital. Some families in our data mistakenly report that they are supposed to send their children to school as a condition to receive the transfer. Schady and Araujo (2008) document that the change in schooling in the treated population is largest among these families. However, as we discuss below, it does not appear that the declines in paid employment we observe are a result of a misunderstanding of the requirements of the BDH.

Third, child labor declines with the BDH even though the size of the transfer is less than foregone child labor earnings. Fewer than 2 percent of children in paid employment report earning less than $15 a month and median earnings are $80 per child per month. We argue that the rigidities in hours in paid employment and in the length of the school day explain why paid employment declines despite the fact that the transfer does not cover foregone earnings. The transfer may push the optimal child labor supply for poor families to below that necessary to participate in paid employment. In fact, we observe that among the poorest students, there appears to be some movement from paid employment into unpaid, household based economic activity for fewer hours.

The rest of the paper proceeds as follows. Section I reviews the existing evidence on the income elasticity of child labor. The BDH program is described in section II, and BDH's impact on child labor is presented in section III. Section IV concludes with a discussion of the interpretation of our findings as well as its implications for future research on child labor.

I. Existing Evidence on Child Labor and Living Standards

An estimated 306 million children are economically active in the world (ILO 2010). These children are overwhelmingly in poor countries, and cross-country differences in GDP per capita alone can explain two-thirds of the cross-country variation in economic activity rates (Edmonds 2010). Perhaps the most researched question in the empirical child labor literature is whether there is a causal relationship between some measure of living standards and the time allocation of children. Many studies document that children are less likely to work in richer households and that children work less as a household gets richer.[1] This finding is not universal.[2] Several studies document important confounding factors that are correlated with both living standards and time allocation.[3] Attributing causation is difficult because child time allocation and economic status are joint outcomes of a single decision-making process.

There are several reasons why children might work more in poorer settings. We have in mind a single household decision-maker deriving utility from consumption today and the child's future well-being (produced by investing in child schooling today). This agent maximizes utility by allocating child time between schooling, paid employment and work inside the household, subject to a time and cash constraint. Schooling and paid employment are both difficult to do part-time. There are fixed costs associated with searching for and traveling to paid employment, and the formal labor market production technology may require coordination with other workers. Work inside the home is more flexible. Schooling is also inflexible, because it is difficult to make adequate school progress with sporadic attendance and awkward to leave after a few hours. As a result, it is rare to combine paid employment with schooling. Combining unpaid household based work with schooling is more feasible.

Preferences cause schooling to increase with income. Because of the rigidity in time in paid employment, a rise in demand for schooling reduces participation in paid employment as income rises holding wages constant. As time shifts from paid employment to school, work inside the household may rise as that's presumed to be more flexible. An emphasis on agent preferences as a key determinant of paid employment and child labor is commonplace, even in models without explicitly emphasizing schooling. Rosenzweig and Evenson (1977) put preferences for leisure in

[1] The following rely on cross-sectional variation: Behrman and Knowles 1999; De Carvalho Filho 2008; Chiwaula 2010; Cogneau and Jedwab 2008; Dammert 2006; Dammert 2008; Edmonds 2006; Edmonds, Pavcnik, and Topalova 2010; Wahba 2006. Studies using panel data include: Beegle, Dehejia, and Gatti 2006; Dillon 2008; Duryea, Lam, and Levison 2007; Edmonds 2005; Edmonds and Pavcnik 2005; Glewwe and Jacoby 2004; Jacoby and Skoufias 1997; Yang 2008.

[2] The following are a list of published studies that do not find support for a negative correlation between living standards and economic activity: Bhalotra and Heady 2003; Dumas 2007; Ersado 2005; Kambhampati and Rajan 2006; Psacharopoulos 1997; Ray 2000; Swaminathan 1998.

[3] There is a wide array of confounding factors documented in the literature. For examples, see: Basu, Das and Dutta 2007; Fafchamps and Wahba 2006; Kruger 2007; Manacorda and Rosati 2010; Mueller 1984; Rosenzweig and Evenson 1977; Schady 2004.

the utility function. Basu and Van (1998) is built on a luxury axiom that posits that children work only when the agent cannot otherwise overcome subsistence needs.

Liquidity constraints are implicit within our framework. They plan an important role in many models of child labor supply (e.g. Baland and Robinson 2000, Ranjan 2001) and their importance in child labor decisions has some empirical support (Edmonds 2006). Even if schooling were purely an investment, liquidity constraints can generate a link between income and child labor if liquidity constraints force families to choose less school than optimal given the market return and opportunity costs to schooling.

There are other reasons why increases in income could result in declines in child labor. Increases in income, including cash transfer income, could reduce the need for child labor for household self-insurance (Beegle, Dehejia, and Gatti 2006; de Janvry et al. 2005). Food, nutrition, books, pencils, notebooks, and transportation all might become more available with increases in income, and could increase the relative return to time in school. Higher income can affect changes in productivity in household based work. For example, income may induce families to specialize more, as the quality of products purchased in market may dominate the quality of products manufactured at home (Edmonds and Pavcnik 2006). That is, higher income may induce a decline in productivity in home production with implications for paid employment and schooling. Alternatively, additional income could increase productivity inside the household if it facilitates the accumulation of working capital that is complementary to child labor (Basu, Das, and Dutta 2010). The result might be less paid employment but more employment within the household.

There is considerable heterogeneity in how researchers define child labor in these studies (Edmonds 2008 is a review). Evidence on the link between paid employment and living standards is especially elusive. Paid employment is rare among children. Edmonds and Pavcnik (2005b) document that 1 out of 42 children between the ages of 5 and 14 in the 36 countries covered by the UNICEF MICS2 project work for pay. The fact that paid employment is rare and the joint determination of living standards and paid employment combine to create complicated econometric problems that make elusive compelling evidence on an income elasticity of paid employment. Studies of large-scale conditional cash transfer programs (CCTs) such as Mexico's PROGRESA sometimes find an impact of CCTs on paid employment (for example, Schultz 2004). However, CCTs change both the standard of living and the relative return to schooling.

Researchers since the origins of the human capital literature (e.g. Schultz 1960) have emphasized the primary importance of relative returns to education in time allocation decisions. Hence, without further assumptions, the CCT evidence is ultimately not informative about the relationship between living standards and paid employment.

II. Background on BDH and Its Evaluation

Ecuador has had a cash transfer program, the *Bono Solidario*, since 1998. Recipient households received $15 per month per family without conditions attached. *Bono Solidario* was meant to assist poor families during an economic crisis. It continued past the economic crisis and was poorly targeted. BDH replaced *Bono Solidario* beginning in mid-2003. A key difference between BDH and *Bono Solidario* is that *initial* eligibility for the BDH is explicitly means-tested based on a census conducted in 2001.[4] Only the poorest two quintiles of the population are eligible to receive the BDH's $15 per month.

In transitioning *Bono Solidario* to BDH, the government launched a social marketing campaign that stressed the importance of the BDH in supporting the human capital of poor children. The government paid for radio and newspaper advertisements emphasizing this. Local leaders were supposed to hold town-hall style meetings to explain the purpose of the BDH. Like the *Bono Solidario* program it replaced, BDH transfers have never been made explicitly conditional on specific investments in child human capital (for example, school enrollment). The government of Ecuador felt it did not have the technical capacity to monitor compliance with any such conditions.

Incorporation of new households, those who had not received the *Bono Solidario* previously, worked as follows. Program officials in local BDH offices were given a list of eligible recipients by the central government based on their scores on the means test. Local BDH officials then informed those who were eligible. To actually receive benefits, those who were told they were eligible had to visit in person any branch office of the largest network of private banks (Banred) or the National Development Bank (*Banco Nacional de Fomento*) on prearranged days, or arrange for monthly electronic transfers to accounts in their name. Benefit distribution started in the latter half of 2003, without commitments on the length of benefits or when eligibility would

[4] Starting in 2001, the government developed a family means test, called the Selben index. The Selben index was computed by first conducting a census of household assets, then using principal component analysis to create a single asset index.

be reassessed. There was no on-going means test in the program, so recipients did not need to do anything to establish on-going eligibility after they had been informed of their eligibility in 2003 based on the 2001 data collection. Eligibility was not reassessed between 2003 and 2008.

The rollout of BDH explicitly contained a randomized component in 4 of Ecuador's 24 provinces. Within provinces selected for the evaluation, parishes were randomly drawn. A parish is unit of local government with an average population of 26,503. Within selected parishes, a sample of 1,488 households was randomly selected into the evaluation. There were three filters for incorporation into the evaluation sample. First, households had to be BDH-eligible. Second, households that already received *Bono Solidario* transfers, and continued to be eligible for the BDH program, were excluded from the evaluation—for these households there was no policy change, and therefore nothing to evaluate. Third, households had to have at least one child age 6-17 at the time the means test data was collected.

Households in the evaluation sample were randomly assigned to a "treatment" group (lottery winners) and a "control" group (lottery losers) with a random number sorter. On average, there are 14 lottery winners age 11-16 at baseline and 13 lottery losers per evaluation parish.[5] Lottery winners would immediately be activated for transfers. Lottery losers were excluded for the duration of the program. In most parishes, there are more BDH-eligible households than those who were selected into the evaluation using the lottery. The process of informing eligible individuals and initiating the transfers did not differ in evaluation and non-evaluation parishes. Within evaluation parishes, it did not differ between lottery winners and other BDH-eligible households. Households, including those in the evaluation sample, were not told their scores on the means test. It would be difficult for an individual to assess their own eligibility ex-ante as eligibility was based on the national distribution of wealth, not local, and because the weights given to different components of the proxy means were not revealed. For this reason, it would also be difficult for households to manipulate their scores on the proxy means test to ensure eligibility.[6] Lottery losers were not informed that they were on the original list or that they had been removed from that list. A reasonable presumption is therefore that lottery losers assumed

[5] The poorest two-fifths of the population is eligible for the BDH. This does not correspond to two-fifths of each parish as living standards are not uniformly distributed, and the study areas are slightly wealthier than average in Ecuador (see the On-Line Appendix 1). The 27 study subjects per parish reflects the fact that the evaluation excluded subjects receiving *Bono Solidario* in the past and that our focus is primarily on the population age 11-16.

[6] See Camacho and Conover (2011) for evidence of widespread manipulation of a similar poverty index in Colombia, but only after the algorithm used to calculate the score had been made public.

that they were, in fact, ineligible for transfers. An important feature of this experiment is that, unlike the PROGRESA evaluation, randomization took place at the household level, rather than the community level. Within a parish, we observe both lottery winners and losers.[7]

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 the Catholic University of Ecuador, an organization that had no association with the BDH program and no responsibility for its implementation or evaluation. The baseline survey was collected between June and August 2003. The follow-up survey was collected between January and March 2005, after BDH recipients had been receiving transfers for more than a year. The survey instrument includes a roster of household members, basic socio-demographic characteristics of these members, detailed time allocation and schooling information for school-age children, employment status for adults, dwelling characteristics, household asset holdings, and an extensive module on household expenditures, following the structure of the 1998–99 *Encuesta de Condiciones de Vida* (ECV). We aggregate expenditures, including consumption of home-produced items into a consumption aggregate, appropriately deflated with regional prices of a basket of food items collected with the surveys.

Attrition between baseline and follow-up does not appear to be a substantive issue for our analysis. 94.1 percent of households were re-interviewed. Among households that attrited, most had moved and could not be found (4.2 percent); 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). Attrited children were less likely to be enrolled in school at baseline. They were older. There is no relation between assignment to the study groups and attrition. Baseline differences between attrited and other households in per capita expenditures, assets, and maternal education are small and insignificant.[8]

Our analysis focuses on comparing lottery winners to lottery losers. This is not equivalent to comparing BDH recipients to non-recipients as there was considerable non-compliance with the

[7] For additional information on the BDH and how the evaluation population differs from the rest of Ecuador, see the On-Line Appendix 1.

[8] In a regression of a dummy variable for attrited households on a dummy variable for lottery winners, with standard errors corrected for within-parish correlation, 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, 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.

experiment. 38 percent of children 11-16 at baseline in households that lost the BDH lottery received transfers. None should. 69 percent of children 11-16 in households that won the lottery took up the BDH. Despite non-compliance with the experimental design, winning the lottery increases the probability a child lives in a household receiving the BDH by 82 percent. The On-Line Appendix 2 compares the characteristics of BDH recipients that won the lottery and those that did not. The differences in observables between these two groups are not statistically significant individually or jointly.[9] Even so, there may be differences in unobservables, perhaps including a higher degree of "pushiness" among lottery losers who nevertheless received transfers. Because the results we present instrument BDH take-up with the lottery assignment, as discussed in greater detail below, they are relevant for children whose likelihood of receiving transfers was affected by the lottery. If treatment effects are heterogeneous, our estimates may not apply to other BDH-eligible households.

The lottery appears to have been successful in attaining balanced treatment and control samples. Schady and Araujo (2008) establish the validity of the randomization for children 5-16. Our key results are for children 11-16 at baseline, and we therefore focus our discussion on the validity of the randomization for children 11-16. Table 1 summarizes background characteristics of children and their families for the 1,883 children age 11 and older at baseline, separately for lottery winners and losers.[10] The top panel of table 1 summarizes work and schooling activities at baseline. Children can participate in multiple categories of work. Paid employment occurs outside of the child's home, while unpaid economic activity is work in the family farm or business. 42 percent of children are engaged in unpaid economic activity at baseline. Together, unpaid economic activity plus paid employment are defined as economic activity in this study. 53 percent of the sample is economically active at baseline. Unpaid household services include chores such as shopping, cleaning, and caretaking. The category "any work" combines economic activity and unpaid household services. The total hours and earnings statistics include non-participants in the activity coded as zeros. Children in paid employment work substantially more hours than do children in unpaid economic activity or unpaid household services. This can be seen in Table 1 by scaling up the hours by participation rates, which are three times larger in

[9] The F-statistic associated with the null that the mean differences in baseline characteristics reported in the On-Line Appendix 2 are jointly zero is 0.86 with a P-value of 0.67.

[10] Our analysis focuses on children above age 10 at baseline. Follow-up is on average 1.5 years pass the baseline, and school drop-out rates start to increase from age 12.

unpaid economic activity than in paid employment, and six times larger in unpaid household services. Conditional on participating, average hours in paid employment is 40 hours per week.

The last two rows of the work and schooling panel report information on schooling. Enrollment refers to the current school year at time of interview. Children can be enrolled and yet be coded as non-students. A non-student is either not enrolled in the current school year, or is enrolled but reports having quit or stopped attending for reasons other than teacher strikes or personal illness. At baseline, one-third of non-students work for pay, 44 percent work in unpaid economic activity, and 75 percent participate in unpaid household services. Two percent of subjects report being idle at baseline, where idle is defined as being a non-student who does not engage in any work. We do not discuss these idle children further. We do not see movements in idle status with the lottery, but we do not have power to measure small changes in the prevalence of idleness.

Most of the background characteristics in table 1 appear similar, but gender and total hours in unpaid household services (and thus combined hours) are not balanced between lottery winners and losers. Column 3 of table 1 reports the raw mean difference between columns 1 and 2 as well as whether the difference is significantly different from zero at 10 or 5 percent. The F-Statistic associated with a test of the hypothesis that the differences are jointly zero is 1.37, with a p-value of 0.11.

Girls spend a third more time in unpaid household services than do boys conditional on participating, and 60 percent more when non-participants are included (girls have higher participation rates). The differences in hours in unpaid household services (and total combined hours) between lottery winners and losers are related to the differences in gender composition between the two groups. The final column of table 1 mimics the empirical specification used throughout this study. It considers whether the differences in work and schooling between lottery winners and losers persist after including the controls used in all later tables. Specifically, the reported regression-adjusted difference is the coefficient on an indicator that the child's family won the BDH lottery obtained from a regression of the row variable (the dependent variable) on this lottery indicator, all of the controls listed in the table from age down, parish fixed effects, and the household composition controls listed in the note to the table. The difference in total hours worked in unpaid household services and the difference in total hours combined are not significant with regression adjustment. Gender appears to be the key characteristic that is responsible for the significant mean difference in hours worked—controlling for gender alone is enough to fail to reject the null that there is no difference between treatment and control populations in any of the variables for hours worked. The lack of balance in gender does not appear substantive in our analysis as we can never reject the null hypothesis that boys and girls are affected equally by the program.[11]

Several other studies have considered the impact of BDH transfers: Paxson and Schady (2010) show that transfers improve 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 has large effects on school enrollment rates. Though the transfers are small, the impact they have on children seems to be large.

III. Main Findings

The BDH lottery is independent of employment opportunities and the household's time allocation decision-making process. Hence, the lottery solves the problem of confounding factors and simultaneity that plague most of the literature on the impact of poverty on child labor. As discussed in section I, there are good theoretical reasons to expect that the transfer would result in declines in economic activity rates and paid employment.

Our empirical strategy is straightforward. The randomization was stratified by parish, so we include parish fixed effects throughout, λ_c, and cluster robust standard errors by parish. 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 control for the vector of baseline time allocation characteristics listed in table 1, Γ_{i0}, as well as the other child and household level characteristics listed in table 1, X_{i0}.[12] Age, household size, number of children 0-5, and number of school-age children are recoded as dummy variables throughout the empirical work and denoted λ_a.

[11] In an earlier version of this paper, we bifurcated the sample by gender and by urban-rural. Boys and girls appear to react to the lottery similarly, meaning we could not reject the null that changes in time allocation with the lottery were the same for boys and girls. We also found similar responses to the lottery 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.

[12] The only controls included not listed in Table 1 are counts of the number of household members by age and gender present at baseline and follow-up. These are not included in Table 1, because they would add an additional page in length to the Table.

Our reduced form results consider the impact of winning the BDH lottery. We regress a child labor indicator H_{ic1} for child *i* in parish *c* at follow-up (denoted as time period 1) on the controls above and an indicator for whether the child's family won the BDH lottery, l_i:

(1) $$H_{ic1} = \alpha + \lambda_c + \lambda_a + \beta X_{i0} + \delta\Gamma_{i0} + \gamma_r l_i + \varepsilon_{ic}$$

ε_{ic} is an error term that is 0 in expectation conditional on the other controls listed in equation (1).

The reduced form mimics the actual policy implementation of providing lists of BDH-eligible recipients to localities. The impact of being on the list will be different from the impact of receiving transfers because of imperfect compliance. To estimate the impact of lottery-induced BDH receipt, we replace l_i in equation (1) with an indicator for whether the family receives the BDH, and instrument for non-random receipt of the BDH with l_i.

Our study focuses on children age 11-16 at baseline, and some of the results we report below break down the sample between children who were students at baseline, and those who were not. For both groups, the lottery increases take-up by 33 percentage points. 2SLS results will be equivalent to multiplying the reduced form by 3.03 for both subgroups. The substantive assumption in interpreting this rescaling as the impact of the BDH for households whose likelihood of receiving the BDH was affected by the lottery is that the act of appearing on the BDH-eligible list (winning the lottery) had no effect on child labor decisions beyond its effect on take-up.

We use the 2SLS results to compute a counterfactual mean for non-receipt of the BDH. We cannot use the mean of the dependent variable at follow-up in the control because of contamination of the experimental design. Instead, the counterfactual mean is the predicted value of the second-stage regression for the treated population with the BDH indicator set to zero. The counterfactual means for most forms of work will be greater than the baseline means from table 1, because at follow-up study subjects are 1.5 years older on average. Dividing the estimated impact of the BDH by the counterfactual mean is not the average percentage impact of the BDH, but it is useful as a way of interpreting the magnitude of the estimated impact.

A. Overall results

Children in low-income households in Ecuador participate in different activities by age. Figure 1 plots participation rates at baseline in paid employment, unpaid economic activity, unpaid household services, and school enrollment against age at baseline for the pooled samples of lottery winners and losers. Participation in paid employment is extremely unusual before age 12. In contrast, child participation rates in unpaid economic activity and unpaid household services are similar for ages 8 through 16. Because the main focus of this paper is on the effect of cash transfers on paid employment, the estimates we report below are limited to children age 11-16 at baseline (and thereby 12 -17 at follow-up). Results for children ages 5-10 and all ages pooled are in the On-Line Appendix 3.

Our first set of results is reported in Table 2. The first row of Table 2 contains the first stage results alluded to above. Subsequently, each cell in columns 1 and 2 of table 2 is from a different regression. Each row indicates the dependent variable. The first column reports the reduced-form effect of the lottery on the row variable. The second column reports the 2SLS estimate of the effect of lottery-induced take-up of the BDH on the row variable. The third column reports the predicted value of the second stage regression for the treated population in the absence of BDH receipt.

The lottery reduces paid employment of children by 3.2 percentage points. The 2SLS results show declines in paid employment of 9.9 percentage points. Relative to the counterfactual participation rate of 24.3 percent, this implies a 41 percent reduction in paid employment. Unpaid economic activity declines by 19 percentage points. Relative to the counterfactual participation rate of 55.1 percent, this implies a 34 percent reduction in unpaid economic activity.

The change in paid employment appears to be driven mainly by changes in the extensive margin (participation), rather than the intensive margin (hours). The total hours worked in paid employment, conditional on working, is 40 hours. If that average were to stay fixed, a 9.9 percentage point reduction in participation implies reduction of 4.0 hours in paid employment. In practice, we observe 2.7 fewer hours worked in paid employment. The fact that the decline in paid employment is largely in the extensive margin is consistent with the strong concentration of hours worked in paid employment around 40 hours per week. Figure Two is a histogram of total hours worked in paid employment in the last seven days at follow-up for lottery losers (the control sample). The mode, median, and mean hours worked in paid employment is 40 hours per week. Where we see other minor spikes (5-10 hours, 20-24 hours, 45-50 hours) tends to be at multiples of 8 hours, consistent with hypothesis that there is some inflexibility in hours worked in paid employment within the day. The fact that the mass is so concentrated at 40 hours per week implies that most who work in paid employment do so full time.

There is some evidence that the decline in paid employment is at the lower part of the earning distribution. In table 2, the decline in participation in paid employment that results from the BDH implies a decline of earnings of $8.3 per month, but we observe an actual decline in earnings of only $1.8 per month. Given that most of the adjustment in paid employment is in the extensive margin, this smaller decline in earnings implies that those who stop paid employment with the transfer earn less than average.

B.Heterogeneity

This rigidity in paid employment makes it difficult to combine paid employment with schooling. In our data, fewer than 1 in 7 children in paid employment at baseline were enrolled in school. In contrast, out of every 20 children in unpaid economic activity at baseline, 13 were enrolled in school. If there are fixed costs of re-entry, those who are out of school and in paid employment at baseline are apt to stay out of school in the future. In fact, we observe in the control group that less than 1 in 10 children out of school and in paid employment had re-enrolled in school by follow up.

We consider whether the effect of the lottery and BDH varies with the child's student status at baseline with the hypothesis that non-students are unlikely to be affected by lottery-induced take-up of BDH. Non-students are less apt to be affected, because reenrollment is rare (our definition of non-student excludes transitory absences for health or teacher strikes). There may be fixed costs to re-entering school, the experience of contributing economically to the household may change perceptions of basic needs or reference income (as in Koszegi and Rabin 2006), or there may durable consumption plans that are difficult to reverse (Chetty and Szeidl 2010). To allow for heterogeneity with student status, we estimate equation (1), including an interaction of the lottery winning dummy with an indicator that the child was not a student at baseline. The results of this modification to our reduced form are in columns 1 and 2 of table 3. Column 1 contains the reduced form effect of winning the lottery on the row variable for students.[13] The effect of winning the lottery for non-students is the sum of column 1 and 2. The 2SLS results in columns 3 and 4 instrument for the BDH indicator and its interaction with the non-student at baseline indicator using the lottery dummy and its interaction with the non-student at baseline dummy. The counterfactuals in columns 5 and 6 are the predicted values of the 2SLS for students and non-students respectively after constraining the BDH dummy and its interactions to be zero.

Table 3 shows that the declines in paid employment are entirely concentrated in children who were students at baseline. The 2SLS estimates indicate that, among baseline students, there was a decline of 15 percentage points in paid employment as a result of the BDH, which is equivalent to 78 percent of the counterfactual mean. Among non-students, there is essentially no decline in paid employment associated with receipt of the BDH. In contrast, the decline in unpaid economic activity associated with the BDH is larger among non-students than students. For non-students (but not students) there is also a small decline in household chores. The BDH also results in larger increases in school enrollment among baseline students than non-students. It makes sense that patterns in school enrollment more closely mirror paid employment than unpaid economic activity, as paid employment tends to be for more hours and may lack the flexibility in hours that would make combining with schooling feasible.

The effects of lottery-induced BDH receipt on paid employment are concentrated in students. It is logical to suppose that its effects should be largest in poorer families where paid employment is most prevalent. We consider this in Figure 3. The sample is limited to students age 11-16 at baseline. We measure living standards with baseline per capita expenditures. Figure 3 contains two local-linear regressions. On the left axis, we plot the reduced form effect of the lottery on participation in paid employment in the last 7 days by baseline per capita expenditures. Specifically, the left axis is the change in participation rates in paid employment at follow-up (lottery winners – lottery losers). We also picture 90 percent confidence intervals around this reduced form treatment effect. On the right axis, we plot the participation rate in paid employment for the control sample by baseline per capita expenditures. This is the counterfactual for the lottery treatment. The figure suggests that the declines in paid employment associated

[13] On-Line Appendix 4 explores the validity of the randomization among baseline students. There are more females and more rural subjects among lottery winners. These are important controls in examining the effect of the lottery within students. The F-Statistic associated with the hypothesis that the differences between lottery winners and losers are jointly zero among students is 2.12 with a P-Value of 0.02 because of these two characteristics. For non-students, the F-Statistic for this test is 0.74 with a P-Value of 0.82.

with the lottery are somewhat larger amongst the poorest households, where paid employment is most prevalent.[14] Recall that to be eligible for this study, a child needed to live in a household in the poorest two-fifths of the population. The poorest quarter of our sample corresponds to the poorest 10 percent of the population in Ecuador. The declines in paid employment are only statistically significant for baseline per capita expenditures below 283 per person per year. This is 22.5 percent of the sample. Hence, the declines in paid employment are statistically significant in the poorest 9 percent of Ecuador's population.[15]

Figure 4 plots the impact of the lottery on participation in work and school as well as hours worked. Each curve is generated in a manner identical the reduced form treatment effect with the left axis in figure 3. Panel A reports changes in participation rates, and Panel B considers changes in hours.[16] Confidence bounds are not pictured to preserve readability. The figure shows that the lottery leads to increases in school enrollment for all but the wealthiest 5 percent of households at baseline. Unpaid household services increase throughout. Participation in unpaid work in the family farm or business decreases among all but the poorest households. In the poorest families, the lottery seems to induce a switch from paid employment to unpaid work in the family farm or business, but total hours still decline. This substitution to unpaid work is consistent with our hypothesis that the rigidity in paid employment hours is an important factor in why paid employment is so responsive to the transfer.

C. Interpretation

The discussion in section I emphasized that additional income may affect child time allocation through preferences for child schooling (or child welfare more generally), liquidity constraints, need for child labor with self-insurance, changing the return to education, or altering the productivity of child time in household-based production. We argue that preferences for child welfare combined with rigidity in hours in paid employment are the simplest explanation for the pattern of responses to the BDH we observe.

[14] Half of the population in paid employment in the control sample at follow-up comes from families with baseline per capita expenditures below \$380. Three-fourths come from families with baseline per capita expenditures below \$490.

[15] An obvious concern with comparisons of the type in Figure 3 is whether the randomization remains valid at such a fine partitioning of the data. In the On-Line Appendix 5, we plot the probability a child's household wins the lottery by baseline per capita expenditures. The probability a household is a lottery winner appears balanced throughout the baseline per capita expenditure distribution.

[16] We do not have data on changes in hours in school, so these are not included in the bottom panel. It is reasonable to assume that hours in school are fixed, conditional on participation.

Our results do not appear to be driven by an increase in the productivity of household-based production. Unpaid economic activity in the family farm or business increases among the poorest and decreases for less poor. For unpaid household services, we observe a small and insignificant increase in time in unpaid household services across the distribution of baseline per capita expenditures. As this increase does not seem to vary with the intensity of changes in other activities and does not seem to be associated with a decline in schooling, we do not think an increase in the productivity of time in unpaid household services is behind the declines in unpaid economic activity or paid employment. We view the change in unpaid household services as reflective of these declines rather than the cause.

The returns to education affect child time allocation by affecting the child's future welfare. Hence, it is difficult to separate whether we are capturing an effect of income on the return to education, which in turn induces more investment in child welfare, or preferences for child welfare. The On-Line Appendix 6 documents that there does not appear to be a significant increase in food expenditure associated with the transfer, so this is not a case where higher income raises the child's productivity in school as the family spends more on food.[17] We observe increases in school-related expenditure, so it is possible that more income leads to more complementary inputs. Of course, the additional expenditure may simply reflect the higher rates of school enrollment.

The simplest explanation for the complete pattern of results we observe is that higher income enables families to consume more child welfare. Additional income allows families to feel that they can afford to continue schooling. Paid employment is difficult to combine with schooling because of constraints in the minimum number of hours required to work. Hence, continuing in school induces families to choose unpaid economic activities over paid employment. Families whose children would otherwise have participated in unpaid economic activities also reduce their labor supply, and some of these families stop working altogether. For these families, the additional income raises their reservation wage enough to induce them to stop working and invest the entire time endowment in child welfare.

Why would $15 a month be enough to induce families to continue schooling? It could be the nature of preference for child welfare among the poor. Liquidity constraints seem a plausible

[17] Schady and Rosero (2008) show that the BDH increased the share of total expenditures on food, but not the amount spent on food. Households that were randomly assigned to receive the BDH had lower overall expenditures than those that were randomly assigned to the control group.

explanation, meaning families would have liked children to continue with school given current prices (wages, returns to education) but were unable to do so. They may have difficulty dealing with the lumpiness or the cash nature of school expenses, and they cannot borrow against future earnings. The size of the transfer is slightly more than the average increase in schooling costs between primary and secondary school, when many children end their schooling. However, liquidity constraints are unlikely to be the entire story as the largest declines in unpaid economic activity are among the least poor students, for whom there appears to be no substantive change in school enrollment. $15 could also be enough to enable households to self-insure against the types of shocks that occur with frequency. Hence, preference, liquidity constraints, and self-insurance are all difficult to rule out as explanations for the observed declines in paid employment and increases in schooling.

A potential concern with our results is that they reflect misunderstanding of the program. The BDH experiment was conducted at a time when the government of Ecuador was emphasizing the importance of the BDH for the accumulation of human capital. 25 percent of our sample (split evenly between treatment and control) think that BDH recipients are supposed to attend school. Schady and Araujo (2008) point out that, within the treated population, the increases in schooling over time are largest for those who erroneously believe that they are supposed to attend school to receive the BDH. Could mistaken beliefs about conditionality drive the changes in time allocation we report?

Two pieces of evidence suggest that this is not the case. First, the social marketing campaign emphasized the importance of schooling but did not address paid employment or any other component of child labor. In fact, as we show, baseline students frequently combine school and unpaid work in response to the transfer. Second, our results do not vary substantively with beliefs about the schooling requirements of the transfer. Beliefs about schooling requirements are not random, but we examine whether the impact of the lottery-induced take-up of the BDH on child labor varies with these beliefs (measured only at follow-up) in table 4. The impact of the lottery on paid employment and unpaid economic activity is not substantially larger among children in households that report mistaken beliefs about conditionality. The only margin where reported beliefs about schooling requirements substantively change the estimated effects of the BDH is in school enrollment.[18]

IV. Conclusion

This study considers child time allocation responses to a lottery in Ecuador where women with children were randomly assigned to receive $15 per month through the *Bono de Desarrollo Humano* (BDH) program. Winning this lottery is associated with an increase in income. Additional income allows families to feel that they can afford to continue schooling among those in school at baseline. Paid employment is difficult to combine with schooling because of rigidity in hours. Hence, continuing in school induces families to have children work in unpaid economic activities rather than paid employment. Although those that stop paid employment are below average earners, these families are typically giving up more in foregone child earnings than the amount of the transfer.[19] For families whose children would have engaged in unpaid economic activity absent the transfer, the additional income induces substantial exit from the economically active labor force.

The program effects we estimate are large in magnitude. Our 2SLS results suggest reductions in paid employment of 10 percentage points (from a counterfactual mean of 24 percent), and reductions in unpaid economic activity of 19 percentage points (from a counterfactual mean of 55 percent). Magnitudes are even larger for baseline students where we have a prior that the transfer should be most effective. We compare the magnitude of these full sample effects with those estimated for conditional cash transfer programs in Mexico, Nicaragua, and Colombia. Skoufias and Parker (2001) estimate the effect of the PROGRESA program in Mexico on child economic activity. They find that PROGRESA reduced economic activity among boys aged 12 to 17 by 3 to 5 percentage points, from a baseline value of 38 percent, and among girls by 2 percentage points, from a baseline value of 17 percent. Maluccio and Flores (2005) estimate that the *Red de Protección Social* program in Nicaragua reduced economic activity by 3-5 percentage points, although their analysis focuses on younger children, ages 7-12. Attanasio et al. (2010) find no evidence that the *Familias en Acción* program in Colombia had an effect on child work

[18] Of course, as Schady and Araujo (2008) emphasize, this is also the place where interpretation is most difficult since both treatment and control children who are attending school answer that they are supposed to attend school with the BDH. It is possible that children attending school simply respond that they are supposed to attend school, or that parents use misinformation about the transfer to coerce schooling out of children who otherwise might not attend.

[19] Less than 2 percent of children in paid employment report earning less than $15 a month.

in income-generating activities (as opposed to unpaid household services) in either rural or urban areas. The baseline values of the fraction working in income-generating activities in their sample are 15 percent in rural areas, and 13 percent in urban areas. The large magnitude of the effects we estimate is particularly surprising given the fact that the programs in Mexico, Nicaragua, and Colombia made transfers that were conditional on school enrollment, while the BDH transfers did not have strings attached to them.

There are a variety of reasons that could account for the large magnitude of the BDH effects on child time allocation, relative to others in the literature. A much larger fraction of the children in our sample work in paid employment than is the case in the studies described above, creating a larger margin for declines in Ecuador than elsewhere. In addition, our estimated treatment effects apply only to households for whom transfer take-up was affected by the BDH lottery. These households are likely to be less "pushy" than others (meaning they did not get BDH benefits except through the lottery), and the foregone earnings from switching children out of paid employment appear to be below average. It might be particularly easy to affect the time allocation of households such as these, and they may differ substantively from other households. We think this group, whose behavior was affected by the BDH lottery, is one of policy interest. At the moment, virtually every country in Latin America already has a cash transfer program, but most cover only a fraction of poor households (see Fiszbein and Schady 2009, especially pp. 67-80). In many contexts, then, households who are most "pushy" are likely to already have made their way into existing programs. Therefore, if the policy that is being considered is the expansion of the BDH or a similar program with less than full coverage of the poor, the parameters we estimate in this paper may be a reasonable approximation to the expected effects on child labor.

It is possible to interpret these findings as supporting some of the more controversial assumptions within Basu and Van's (1998) canonical model of child labor. Basu and Van rests on an ad-hoc characterization of preferences known as the luxury axiom. The luxury axiom treats preferences over child labor as lexicographic: child labor occurs if and only if families cannot cover their subsistence needs without child labor. This ad-hoc characterization of preferences has drawn substantial criticism. It implies that small changes in income can reduce child labor if it moves families from below to above subsistence, and that the increase in income does not need to cover foregone earnings. In the sample we study, we observe that a transfer valued at 7 percent of monthly expenditures is associated with a 41 percent reduction in paid employment. We argue that the key factor behind these large reductions in paid employment is the presence of rigidity in hours in paid employment. Basu and Van do not define child labor, but paid employment was the standard empirical implementation of child labor in studies at the time. We cannot say whether the transfer moves families from below subsistence to a point above, but classical preferences and a binding constraint on the minimum number of hours in paid employment appear to explain a set of results that are consistent with the luxury axiom characterization of preferences in Basu and Van.

Our main findings on paid employment do not seem to be driven by mistaken perceptions of BDH program requirements, but it is reasonable to suppose that the social context and the extent to which parents perceive the transfer as permanent or temporary are important. The government's social marketing campaign could have shaped agent priorities for spending. It is also plausible that government transfers are treated differently from found money or other sources of income, so that agents may prioritize spending them on goods with greater social returns, or there could be mental accounting issues involved. The fact that the transfer was new may also be important in explaining its impact. Agents with children on the cusp of transitioning between primary school to either secondary school or the labor market could respond to a surprise $15 a month differently than they would have responded to an entirely anticipated income source (regardless of whether they expect $15 a month to be transitory or permanent). For example, suppose that a parent had calculated that their child would need to end schooling at the end of primary because of the increase in school costs that accompany the transition to secondary school. $15 a month is enough to cover the expected increase in schooling costs between primary and secondary, and this "found money" might create the perception that the family can now afford to delay entry to paid employment in a way that they would not have if $15 had been included in the original budget for the household's optimization problem. The experiment is not designed to test whether the income elasticity of paid employment is sensitive to how the income is delivered.

Our findings highlight the central role that poverty plays in the child labor decision, and suggest the possibility of affecting large changes in child labor with relatively modest investments in poverty relief. Other studies have suggested the possibility of intergenerational poverty traps working through child labor itself (Emerson and Souza 2003), its impact on education (Barham et al. 1995), or the influence of child labor on occupational choice (Banerjee and Newman 1993). It is difficult to predict the magnitude of the resulting multiplier effects from child labor reductions, but taken together these studies and our results raise the prospect of large returns to poverty alleviation programs.

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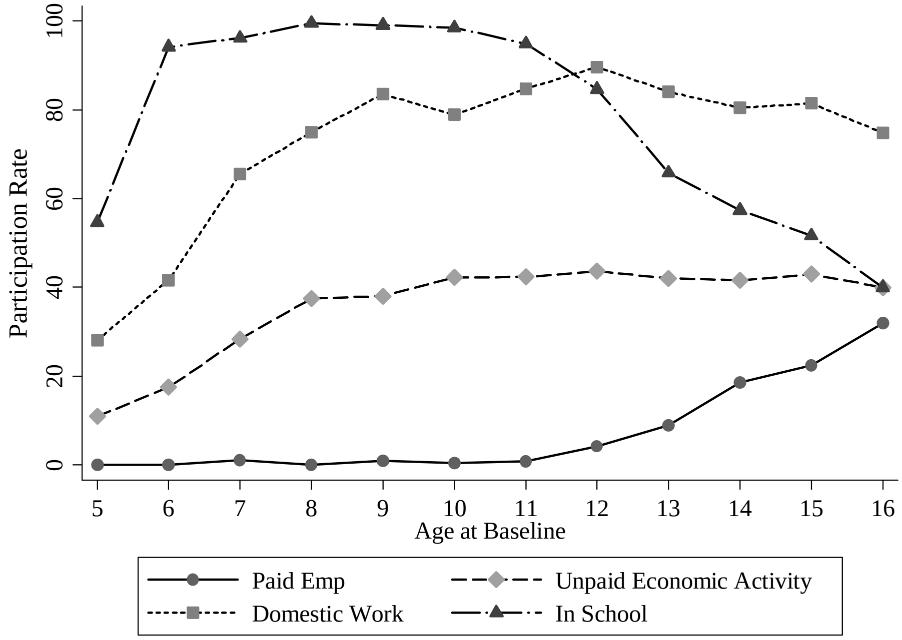


FIGURE ONE: PARTICIPATION IN WORK AND SCHOOL AT BASELINE BY AGE

Source: Authors' calculation from the BDH Evaluation data at baseline.

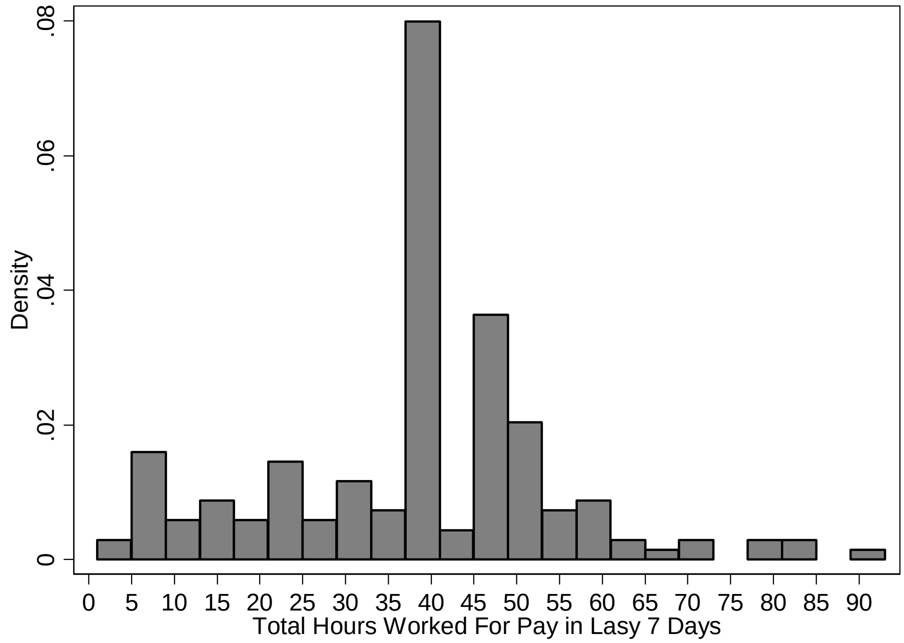


FIGURE TWO: THE DISTRIBUTION OF HOURS WORKED IN PAID EMPLOYMENT IN THE CONTROL POPULATION AT FOLLOW-UP

Source: BDH Evaluation control sample at follow-up data. We focus on the control population at follow-up for this figure rather than the baseline population, because the baseline population is on average 1.5 years younger than is the population at follow-up. Age 11-16 at baseline. The bin width is 5 hours.

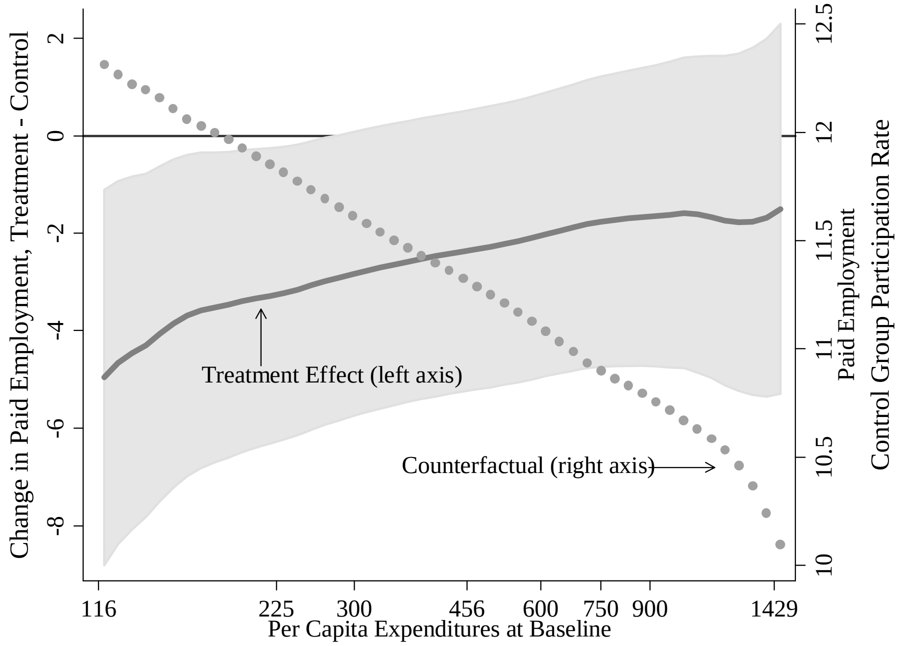
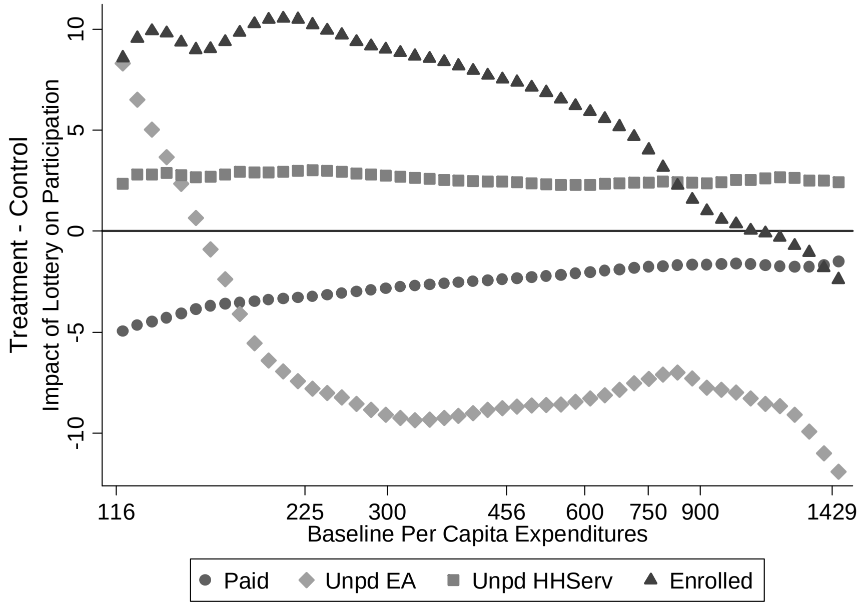
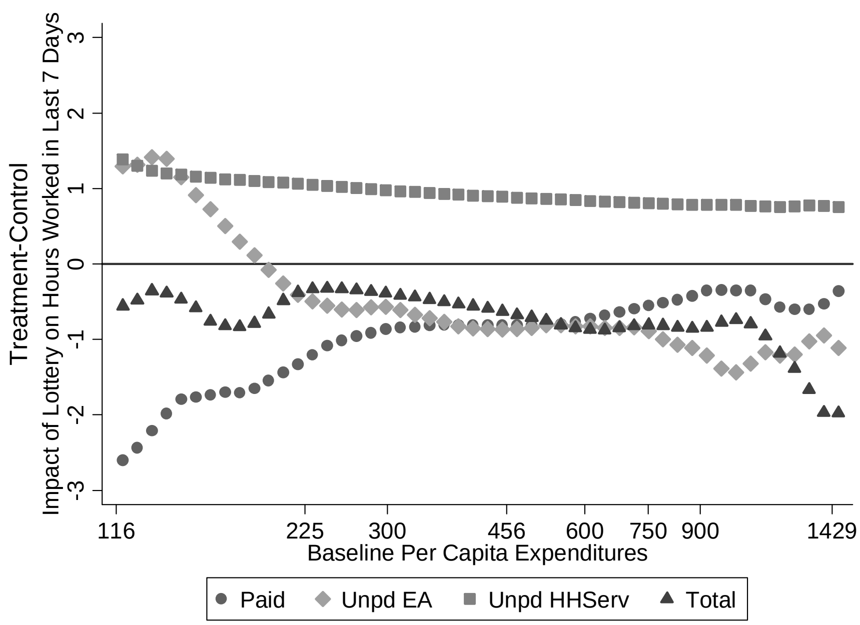


FIGURE THREE: IMPACT OF THE LOTTERY BY BASELINE PER CAPITA EXPENDITURE FOR BASELINE STUDENTS AGE 11-16

Authors' calculation from the BDH Evaluation data. The pictured solid curve is the change in participation rates in wage employment at follow-up (reduced form: lottery winners – lottery losers) by baseline annual per capita expenditures for students above age 10 at baseline. The pictured dotted curve is the mean incidence of wage employment among lottery losers at follow-up by baseline annual per capita expenditures for students above age 10 at baseline. The x-axis is pictured on a log scale. The y-axis is in percentage points (5 is 5 percentage points). 90 percent confidence interval for the change in participation rates is pictured.



PANEL A: PARTICIPATION IN WORK AND SCHOOL IN LAST 7 DAYS



PANEL B: TOTAL HOURS IN WORK AND SCHOOL IN LAST 7 DAYS

FIGURE FOUR: IMPACT OF LOTTERY ON WORK AND SCHOOLING BY BASELINE PER CAPITA EXPENDITURE FOR BASELINE STUDENTS 11-16

Source: Authors' calculation from the BDH Evaluation data. The pictured curve in panel A is the change in participation rates in each indicated activity at follow-up (reduced form: lottery winners – lottery losers) by baseline annual per capita expenditures for students above age 10 at baseline. The y-axis in Panel A is in percentage points (-5 means that lottery winners are 5 percentage points less likely to participate compared to the lottery losers) The pictured curve in Panel B is the change in total hours worked in the last week in each indicated activity at follow-up by baseline annual per capita expenditures for students above age 10 at baseline. The y-axis in panel B is in hours worked in the last week (-3 is a decline in 3 hours for lottery winners compared to lottery losers).The x-axis is pictured on a log scale. The change in participation in paid employment in panel A is the same as pictured in figure 3.

Table 1: Differences in Subject Characteristics at Baseline by Treatment Status
Children 11-16 at baseline

	Variable (b/se)	Lottery Winners	Lottery Losers	Mean Difference	Regression Adjusted Difference
	Sample Size	993	890	1,883	1,883
Work and Schooling	Paid Employment	0.14	0.14	0.00	0.00
		(0.01)	(0.01)	(0.02)	(0.02)
	Unpaid Economic Activity	0.42	0.42	0.00	0.01
		(0.02)	(0.02)	(0.03)	(0.02)
	Any Economic Activity	0.53	0.53	0.00	0.00
		(0.02)	(0.02)	(0.03)	(0.02)
	Unpaid Household Services	0.83	0.83	0.00	-0.01
		(0.01)	(0.01)	(0.02)	(0.02)
	Any Work	0.93	0.93	0.00	0.00
		(0.01)	(0.01)	(0.01)	(0.01)
	Total Hours Last Week, Paid Employment	4.09	4.02	0.08	0.13
		(0.39)	(0.40)	(0.48)	(0.51)
	Total Hours, Unpaid Economic Activity	6.12	5.51	0.60	0.40
		(0.35)	(0.33)	(0.52)	(0.44)
	Total Hours Last Week, Economic Activity	10.22	9.53	0.68	0.52
		(0.50)	(0.50)	(0.64)	(0.64)
	Total Hours Last Week, Unpaid Hh. Services	9.45	8.68	0.76**	0.29
		(0.29)	(0.28)	(0.37)	(0.41)
	Total Hours Last Week, Combined	19.67	18.21	1.44**	(0.80)
		(0.56)	(0.57)	(0.70)	(0.77)
	Total Earnings Last Month Paid Employment	9.85	9.76	0.18	0.44
		(1.05)	(1.13)	(2.04)	(2.01)
	School Enrollment	0.66	0.66	0.00	0.01
		(0.02)	(0.02)	(0.03)	(0.03)
	Non-student	0.35	0.34	0.00	-0.01
		(0.02)	(0.02)	(0.03)	(0.03)
Individual Characteristics	Age (at baseline)	13.43	13.45	-0.02	n/a
		(0.05)	(0.05)	(0.06)	
	Male	0.47	0.52	-0.05**	n/a
		(0.02)	(0.02)	(0.02)	
	Speaks Indigenous Language	0.10	0.08	0.02	n/a
		(0.01)	(0.01)	(0.02)	
	Has Disability	0.01	0.01	0.00	n/a
		(0.00)	(0.00)	(0.01)	
	Oldest Resident Child	0.65	0.66	-0.01	n/a
		(0.02)	(0.02)	(0.02)	
	Oldest Resident Girl	0.42	0.39	0.02	n/a
		(0.02)	(0.02)	(0.02)	
	Mother Present	0.94	0.93	0.00	n/a
		(0.01)	(0.01)	(0.02)	
	Mother's Years of Education	3.70	3.46	0.25	n/a
		(0.09)	(0.10)	(0.16)	
	Father Present	0.82	0.83	-0.01	n/a
		(0.01)	(0.01)	(0.03)	
Household Characteristics	Rural Locality	0.53	0.50	0.04	n/a
		(0.02)	(0.02)	(0.04)	
	# School Age Children Present	2.70	2.60	0.10	n/a
		(0.04)	(0.04)	(0.09)	
	# Children 0 to 5 Present	0.51	0.46	0.05	n/a
		(0.02)	(0.02)	(0.05)	
	Log of Per Capita Expenditures	6.00	6.01	-0.01	n/a
		(0.02)	(0.02)	(0.03)	
	Household Size	6.14	6.09	0.04	n/a
		(0.06)	(0.06)	(0.12)	

All characteristics are at baseline. Regression adjustment includes parish fixed effects and all controls from "Age" down to "Total Monthly Expenditure" marked with "n/a" plus counts of the number of men and women (separately) present at baseline and follow-up in each of the following age categories: under 5, 6-17, 18-44, 45-64, 65+. Household Size, Age, # school age, and # children are included as dummy variables in all regressions. For each characteristics, the standard error is reported on the line below the mean. Standard errors are corrected for clustering at the parish level. ** is significant at 5 percent. * is significant at 10 percent.

Table 2: Impact of Lottery on Work and Schooling in the Last 7 Days

Children age 11 - 16 at baseline

	Wins Lottery (Reduced Form)	Receives BDH (2SLS)	Counterfactual Mean (for (2))
	(1)	(2)	(3)
First Stage Results	0.327**		
Receives BDH	(0.034)		
Participation in the Last 7 Days			
Paid Employment	-0.032*	-0.099*	0.243
	(0.017)	(0.054)	
Unpaid Economic Activity	-0.061**	-0.187**	0.551
	(0.023)	(0.074)	
Economic Activity (Paid employment or Unpaid Economic Activity)	-0.080**	-0.245**	0.714
	(0.024)	(0.079)	
Unpaid Household Services	0.008	0.024	0.786
	(0.021)	(0.066)	
Any Work (Economic Activity or Unpaid Household Services)	-0.026**	-0.080*	0.965
	(0.013)	(0.041)	
Hours Worked in the Last 7 Days (including 0s)			
Paid Employment	-0.900	-2.737	9.006
	(0.821)	(2.494)	
Unpaid Economic Activity	-0.750*	-2.289*	8.234
	(0.409)	(1.369)	
Economic Activity (Paid employment + Unpaid Economic Activity)	-1.672*	-5.110*	17.264
	(0.900)	(2.871)	
Unpaid Household Services	0.381	1.166	7.382
	(0.353)	(1.083)	
Total Hours (Economic Activity + Unpaid Household Services)	-1.291	-3.945	24.647
	(0.992)	(3.112)	
Monthly Earnings from Paid Employment (including 0s)	-0.597	-1.829	16.933
	(1.946)	(5.886)	
School Enrollment	0.062**	0.190**	0.491
	(0.020)	(0.064)	

Each cell in columns 1 and 2 is the result of a different regression. All cells in column 1 contain reduced form estimates of the effect of winning the lottery on the row variable. Column 2 contains 2SLS estimates of the impact of the Bono. In this column, we instrument for Bono Receipt with an indicator for whether an individual won the Bono lottery. All regressions include parish fixed effects and controls for all characteristics listed in table 1 plus counts of the number of men and women (separately) present at baseline and follow-up in each of the following age categories: under 5, 6-17, 18-44, 45-64, 65+ . From table 1, Household Size, Age, # school age, and # children are included as dummy variables in all regressions. Robust standard errors in parenthesis are corrected for clustering at the parish level. * is significant at 10 percent. ** is significant at 5 percent. Column 3 contains counterfactual means for those induced to take-up the BDH by winning the lottery. These are the predicted value of the 2SLS results with BDH receipt set to 0 and all other characteristics as found in the lottery winner population. There are 1881 sampled children age 11-16 at baseline.

Table 3: Impact of the BDH on Work Participation and School Enrollment in the Last 7 Days by Baseline Student Status

Reduced Form and 2SLS Results. Children 11-16 at baseline.

	Wins Lottery (Reduced Form)		Receives BDH (2SLS)		Counterfactual Means	
		* Non-Student		* Non-Student	Student	Non-Student
	(1)	(2)	(3)	(4)	(5)	(6)
Impact on Participation in the Last 7 Days						
Paid Employment	-0.050**	0.051	-0.155**	0.164	0.198	0.337
	(0.020)	(0.035)	(0.065)	(0.124)		
Unpd Econ Activity	-0.056**	-0.014	-0.168**	-0.057	0.528	0.592
	(0.024)	(0.036)	(0.071)	(0.129)		
Econ Activity	-0.100**	0.058	-0.307**	0.184	0.687	0.775
	(0.029)	(0.041)	(0.092)	(0.141)		
Unpd Hh Serv	0.020	-0.035	0.064	-0.115	0.801	0.750
	(0.021)	(0.030)	(0.064)	(0.097)		
Any Work	-0.029**	0.008	-0.088*	0.025	0.976	0.947
	(0.014)	(0.024)	(0.045)	(0.083)		
School Enroll	0.068**	-0.015	0.205**	-0.043	0.674	0.145
	(0.024)	(0.038)	(0.080)	(0.130)		

1882 observations. Rows indicate dependent variable. Columns 1 and 2 contain reduced forms from the same regression. Column 1 contains the coefficient on the lottery indicator. Column 2 contains the coefficient of the interaction of the lottery indicator with an indicator that the child is not a student at baseline. Columns 3 and 4 contain 2SLS results from the same regression. Column 3 contains the coefficient on the indicator that the child's family receives BDH. Column 4 contains the coefficient on the interaction of the indicator that the child's family receives the BDH with an indicator that the child is not a student at baseline. Counterfactuals are predicted values from the 2SLS results for lottery winners in the absense of the Bono. See notes to Table 2 for a listing of all included controls. Robust standard errors are corrected for clustering at the parish level. * is significant at 10 percent. ** is significant at 5 percent.

Table 4: Impact of the BDH on Work Participation and School Enrollment in the Last 7 Days by Stated Beliefs on Schooling Requirements

Reduced Form and 2SLS Results. Children 11-16 at baseline.

	Wins Lottery (Reduced Form)		Receives BDH (2SLS)	
		*Thinks BDH Requires Schooling		*Thinks BDH Requires Schooling
	(1)	(2)	(3)	(4)
Impact on Participation in the Last 7 Days				
Paid Employment	-0.033*	-0.013	-0.110**	-0.007
	(0.017)	(0.030)	(0.054)	(0.042)
Unpd Econ Activity	-0.057**	0.006	-0.177**	0.027
	(0.022)	(0.032)	(0.071)	(0.043)
Econ Activity	-0.081**	0.011	-0.250**	0.042
	(0.024)	(0.040)	(0.081)	(0.054)
Unpd Hh Serv	0.003	0.039	0.026	0.052
	(0.024)	(0.035)	(0.071)	(0.053)
Any Work	-0.029**	0.017	-0.085**	0.032
	(0.013)	(0.031)	(0.042)	(0.043)
School Enroll	0.043**	0.072*	0.168**	0.084
	(0.021)	(0.042)	(0.066)	(0.060)

All regressions based on 1835 children observed in both baseline and follow-up periods. Sample size is smaller because of non-response to the question on the whether BDH requires schooling. Columns 1 and 2 contain reduced forms from the same regression. Column 1 contains the coefficient on the lottery indicator. Column 2 contains the coefficient of the interaction of the lottery indicator with an indicator that the child's guardian answered "yes" at follow-up when asked whether the BDH requires school attendance. Columns 3 and 4 contain 2SLS results from the same regression. Column 3 contains the coefficient on the indicator that the child's family receives BDH. Column 4 contains the coefficient on the interaction of the indicator that the child's family receives the BDH with an indicator that the child's guardian answered "yes" at follow-up when asked whether the BDH requires school attendance. See notes to Table 2 for a listing of all included controls. Robust standard errors are corrected for clustering at the parish level. * is significant at 10 percent. ** is significant at 5 percent.