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Cash Transfers, Conditions, and School Enrollment in Ecuador

I nvestments in human capital in childhood are generally believed to be critical for adult well-being. Children who have higher educational attainment are more productive as adults, earn higher wages, and have better health status than children with less education. In country after country, governments have sought to devise effective policies to increase school enrollment. In this context, cash transfer programs have expanded dramatically in many developing countries, especially in Latin America. These are often conditional cash transfer programs, in which eligible households are given transfers conditional on compliance with certain requirements. In most countries, households are required to send school-aged children to school and to take younger children for regular visits to health centers, where they receive nutritional supplements and growth monitoring. The best known of these programs is *Oportunidades* (formerly Progresa) in Mexico, although similar programs have also been implemented in a number of other Latin American countries.

This paper evaluates the impact of a cash transfer program in Ecuador, the *Bono de Desarrollo Humano* (BDH), on school enrollment. This is a large program—in 2004, the BDH budget was approximately 0.7 percent of gross domestic product (GDP). Eligibility for BDH transfers is determined by a proxy means test known as the Beneficiary Identification and Selection

System, or Selben. In theory, the 40 percent of households with the lowest Selben score are eligible for monthly transfers.[1]

The analysis in this paper is divided into two parts. The first part assesses the overall impact of the BDH on school enrollment. During an expansion of the BDH coverage, program administrators undertook an experimental evaluation of the program's impact. A sample of households that had not previously received transfers was selected and randomly divided into treatment and control groups. Data on both groups were collected at baseline, before households started to receive BDH transfers, and follow-up, approximately one and a half years later. We use this experimental study design to show that BDH transfers significantly increased school enrollment. These results complement earlier findings of positive effects of cash transfer programs on enrollment in Mexico, Colombia, Nicaragua, and Honduras.[2]

The second part of the paper provides evidence on the relative importance of transfers and conditions (or perceived conditions) in explaining the positive BDH program effects on enrollment. Conditions attached to transfers introduce a kink in the budget constraint, and their effect depends on whether this compels households to behave differently than they would have done otherwise.[3]

The BDH was originally modeled on *Oportunidades*. Program administrators intended to condition transfers on school enrollment and attendance, among other things. Local elected leaders (the heads of the *Juntas Parroquiales*) were encouraged to hold town-hall-style meetings in which the BDH was presented as a compact between the state and beneficiaries—the state agreed to transfer resources to poor households, and these households, in turn, agreed to send their children to school. The BDH program also briefly aired a series of radio and television spots that explicitly linked transfers with school enrollment, and some BDH administrators appear to have stressed the enrollment requirements when they signed up households for transfers. In practice, however, the BDH did not monitor the schooling condition because of administrative constraints, and it thus did not penalize households whose children were out of school. Nevertheless, the BDH information campaign had an

1. The BDH transfer was $15 a month at the time these data were collected; it was doubled to $30 a month in early 2007.

2. On Mexico, see Schultz (2004) and Behrman, Sengupta, and Todd (2005); on Colombia, see Attanasio and others (2005); on Nicaragua, see Maluccio and Flores (2004); on Honduras, see Glewwe and Olinto (2004).

3. See Das, Do, and Özler (2005) for an illustration.

effect: in the follow-up survey used in this paper, approximately a quarter of respondents stated that they believed that sending children to school was a BDH program requirement.

We compare the impact of the program among conditioned households (that is, those who told survey enumerators that school enrollment was a BDH requirement) and unconditioned households (those who told enumerators that there was no enrollment requirement attached to transfers). Our estimates show that program effects on enrollment are only significant among conditioned households. Because exposure to the information campaign was not assigned randomly, these comparisons are not experimental. However, the effects we estimate are insensitive to adding a large number of controls, trimming the data, and sweeping out fixed differences between conditioned and unconditioned households. We therefore argue that the larger program effect among conditioned households most likely has a causal interpretation. These results complement evidence from a variety of structural and microsimulation models for Mexico and Brazil, all of which conclude that conditions attached to transfers explain the bulk of the effect of conditional cash transfer programs on school enrollment.[4]

The rest of the paper is structured as follows. The next section briefly describes the BDH, the sample frame, and the data. The paper then presents our results on overall program effects, while a subsequent section focuses on the differences between conditioned and unconditioned households. The final section concludes.

Background and Study Design

Ecuador is a lower-middle-income country. In 2004, its per capita GDP was $1,435 in constant 2000 U.S. dollars, or $3,595 in PPP-adjusted constant 2000 U.S. dollars, about half the population-weighted Latin American average. Inequality is high (the Gini coefficient is 0.44), although not especially so by Latin American standards. Poverty is widespread. An estimated 18 percent of the population lives on less than a dollar per person per day, and more than 40 percent live on less than two dollars per day.[5]

The net enrollment rate in primary school in Ecuador is 90 percent. The net enrollment rate in secondary school is substantially lower, however, at

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4. On Mexico, see Todd and Wolpin (2006); Attanasio, Meghir, and Santiago (2005); de Janvry and Sadoulet (2006); on Brazil, see Bourguignon, Ferreira, and Leite (2003).

5. World Bank (2004).

45 percent. Overall enrollment rates changed very little between 1990 and 2001: calculations based on the 1990 and 2001 population censuses indicate that the net primary enrollment rate increased from 88.9 to 90.1, while the net secondary enrollment rate rose from 43.1 to 44.7.[6] In part because of this stagnation in enrollment rates, the Ecuadorian government has given high priority to identifying policies that increase the coverage of the education system, especially at the secondary level.

There is no gender disparity in educational attainment in Ecuador; enrollment rates are marginally higher for girls than for boys. The mean level of schooling of the adult population ages fifteen and older is 6.5 years. On average, educational outcomes in Ecuador are comparable to those of other countries with similar income levels. As is the case in many other countries, however, educational outcomes vary by household socioeconomic status. For example, heads of households above the poverty line have approximately four more years of schooling than those below the poverty line in urban areas and three more years in rural areas.[7]

This paper focuses on the *Bono de Desarrollo Humano* (BDH) program, which grew out of an earlier program known as the *Bono Solidario.* The *Bono Solidario* was created in 1999, in the midst of an economic crisis. The purpose of the program was to make cash transfers to poor households, but eligibility criteria were not clearly defined. As a result, many of the households that received transfers were not poor, and many poor households were not covered by the program.

Since 2003, the BDH program has taken steps to retarget transfers to the poor. To this end, the government developed a composite welfare index based on information on household composition, education levels, dwelling characteristics, and access to services, aggregated by principal components. This index is known as the Beneficiary Identification and Selection System, or Selben. The Selben covers around 90 percent of households in rural areas in Ecuador and about the same percentage of households in selected urban areas that were judged to have a high incidence of poverty. Households surveyed by the Selben are ranked by their Selben score. In theory, 40 percent of households in Ecuador are eligible for $15 monthly transfers by the BDH, based on their low Selben scores. However, until recently the government did not have the budget to make transfers to all households in the first two Selben quintiles, so expansion of the coverage of benefits has been gradual.

6. Vos and Ponce (2005).
7. World Bank (2004).

BDH transfers are made to women, and they can be collected at any branch office of the largest network of private banks (*Banred*) or from the National Development Bank (*Banco Nacional de Fomento*). In terms of magnitude, the monthly $15 BDH transfer accounts for approximately 9 percent of the pre-transfer expenditures of the median household in the study sample. As a point of comparison, *Oportunidades* transfers accounted for about 20 percent of average household expenditures in Mexico.[8] Although the BDH program and the *Bono Solidario* program that preceded it have been the subject of much controversy and policy discussion in Ecuador, the impact of these programs on schooling has not been systematically evaluated.[9]

Sample Frame

The sample for the BDH program evaluation, which we use for our analysis in this paper, was drawn from the Selben rosters for four of the twenty-two provinces in the country: Carchi, Imbabura, Cotopaxi, and Tungurahua. All four provinces are in the highland region of the country. The sampling framework followed a two-stage process: parishes were randomly drawn from within these provinces, and a sample of 1,488 households was then selected from within the parishes. Of these households, 1,306 had school-aged children (ages six to seventeen) at the time of both the baseline and follow-up surveys, as well as baseline data for all of the variables used in this paper. (One exception is parental education, which is missing in a small number of cases in which the respondent did not know the education of a child's parent when the parent did not live in the household.) This sample of 1,306 households, which includes 2,875 school-aged children, is the basis for the analysis in this paper. None of the households in the sample had received BDH or *Bono Solidario* transfers prior to the program evaluation. At the request of program administrators, households near the cut-off between the first and second Selben quintiles were over-represented, as were households with older children; all of the households were drawn from the first and second Selben quintiles.

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8. Skoufias (2005).

9. León, Vos, and Brborich (2001) analyze the impact of the *Bono Solidario* program on consumption poverty, and León and Younger (2007) focus on the impact of the *Bono Solidario* on child health. Both papers find significant but modest program effects, but their analyses are not experimental. Rather, they are based on comparisons between *Bono Solidario* recipients and nonrecipients in the 1998–99 *Encuesta de Condiciones de Vida* (ECV), a nationally representative household survey, and may therefore be subject to biases associated with endogenous program placement or take-up.

At the time the evaluation was launched, the BDH budget was insufficient to cover all households in the first and second Selben quintiles. Half the households in the evaluation sample were randomly assigned to a treatment group that would be eligible for BDH transfers, and the rest were assigned to a control group that would not be eligible for transfers for the first two years. We refer to the first group as lottery winners and the second group as lottery losers. Lottery losers were taken off the roster of households that could be activated for BDH transfers. As shown below, however, a substantial share of these households nonetheless received BDH transfers, an issue we address in our estimates of program impact.

Because of the criteria for selection into the BDH evaluation, households in the study sample are poorer than other households in Ecuador. Table 1 reports the means and standard deviations for selected characteristics of households in the study sample at baseline, of all households in the parishes included in this study, and of all households in the country whose Selben score places them in the first or second quintile. The samples for these calculations are limited to households with children ages six to seventeen. Averages for the parishes in the study sample are based on the 2001 Population Census; national averages for households in the first two Selben quintiles are based on the 1998–99 *Encuesta de Condiciones de Vida* (ECV), a multipurpose household survey. Table 1 suggests that households in the sample are noticeably poorer than the average household in their parishes: a comparison of the first and second columns in the table shows that they are less likely to have water from a network, less likely to have a flush toilet, and more likely to have a dirt floor, while the heads of household have lower education levels. This is not surprising given that the means in the second column include both households in the first and second Selben quintiles and other households. A comparison of the first and third columns suggests that households in the evaluation sample are also somewhat poorer than the average household eligible for the BDH in Ecuador, perhaps because the parishes in the evaluation sample are poorer, on average, or because they are more unequal.

Data

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

T A B L E 1 .   **Comparison of the Evaluation Sample with National and Parish-Level Averages**[a]

Household characteristic	Impact evaluation sample at baseline	All households in evaluation parishes	All BDH-eligible households in Ecuador
A. Household-level variable			
Household size	5.77	4.19	5.84
	(1.84)	(1.51)	(2.26)
No. rooms in house	2.61	3.31	2.47
	(1.20)	(2.32)	(1.20)
Water from network	0.45	0.78	0.69
	(0.50)	(0.41)	(0.47)
Has toilet	0.24	0.72	0.66
	(0.43)	(0.45)	(0.49)
Has dirt floor	0.28	0.18	0.20
	(0.45)	(0.39)	(0.41)
Age of household head	44.96	42.70	43.89
	(9.33)	(12.94)	(12.83)
Education of household head	4.47	7.03	5.11
	(2.77)	(5.02)	(3.48)
Household head is male	0.85	0.77	0.82
	(0.36)	(0.42)	(0.38)
Household head is literate	0.83	0.91	0.81
	(0.38)	(0.29)	(0.39)
Household head is indigenous	0.16	0.17	0.11
	(0.36)	(0.38)	(0.33)
B. Child-level variable			
Age	11.41	11.72	10.78
	(3.09)	(3.46)	(3.12)
Mean years of completed schooling	4.41	5.08	3.76
	(2.60)	(3.03)	(2.65)

a.   The table presents means and standard deviations (in parentheses). Calculations for the first column are based on the impact evaluation sample; those for the second column are based on the 2001 population census; those for the third column are based on the 1998–99 *Encuesta de Condiciones de Vida* (ECV). All estimates are based on households with children aged six to seventeen.

June and August 2003, and the follow-up survey was collected between January and March 2005.

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, and work 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; durable goods and dwelling conditions were aggregated by

principal components into a composite indicator of household assets.[10] The main outcome measure in this paper is a dummy variable that takes the value of one if a child is enrolled in school in the current school year.[11]

The follow-up evaluation survey included a module on access to and perception of the BDH program. Ninety-seven percent of households in the sample had heard of the BDH program, and 61 percent stated that they received transfers. The survey also asked respondents whether they believed that households had to comply with any requirements or conditions to receive transfers. Respondents were not prompted for answers, but 27 percent stated that "ensuring that children attend school" was a prerequisite to receive BDH transfers. We refer to these households as conditioned households in the analysis; they include both lottery winners (55 percent of all conditioned households) and lottery losers (45 percent).[12]

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10. The indicator is based on the number of rooms in the house, dummy variables for earth floors, access to piped water, and access to a flush toilet (three variables), and count variables for the number of household durables based on responses to twenty-two separate questions in the survey. Results are similar when a simple count of household assets, rather than principal components, is used to aggregate these variables or if the measures of household conditions and assets are not aggregated at all but enter individually in the regressions. See Filmer and Pritchett (2001) for a discussion of these methods.

11. One concern is that households that received BDH transfers could have lied about their enrollment status. This concern is not particular to this evaluation—it could be an issue for any evaluation of a program that requires beneficiaries to comply with certain conditions and for which data are collected on the basis of household survey responses; see, for example, Schultz (2004) and Behrman, Sengupta, and Todd (2005) on *Oportunidades* and Ravallion and Wodon (2000) on the Food for Education program in Bangladesh. While we cannot fully rule out such concerns, they are unlikely to be a serious problem for our analysis for a number of reasons. During training, enumerators were instructed that they should not identify themselves as associated with the BDH program or its evaluation under any circumstances; if questioned, enumerators were to state that the information given by respondents would in no way affect eligibility for social programs, including the BDH program. Compliance with these instructions was verified during spot visits in the field. Moreover, questions about the BDH program were included in the last module of the survey, well after questions about household characteristics and (critically) schooling of children. Finally, households could also have lied about whether they had received BDH transfers. This appears not to have been the case, however, since their answers match up very closely with banking records on transfers, as shown in footnote 14.

12. The percentage of conditioned households in our sample is close to that found in other data sources. In a nationwide household survey carried out in December 2005, respondents were asked whether they believed that households had to comply with any requirements to receive BDH transfers: 26.0 percent answered that BDH beneficiaries were expected to ensure that children enroll in school and attend regularly. The question was worded in exactly the same way in the December 2005 nationwide survey and in the January–March 2005 survey used in this paper. One important difference is that the December 2005 survey asked this question only of households that reported receiving BDH transfers. In the January–March 2005 survey, 32.9 percent of

We obtained data from *Banred* on total BDH transfers collected by house-holds in the sample between January 2004 and July 2005. This allows us to construct two measures of BDH treatment—one based on household survey responses and one based on banking records.[13] Discrepancies between the two sources of data are minor, and the estimated program effects are similar regardless of whether treatment status is defined with the household data or the banking records.[14]

One possible concern with this evaluation is anticipation effects among households assigned to the control group. Households in this group were not told they would receive BDH transfers in the future. Nevertheless, although it is not easy for an individual household to learn its Selben score, some lot-tery losers may have concluded that they were eligible for transfers on the basis of their score. If consumption is smoothed over time, households in the control group may have increased their spending on schooling in anticipation of future transfers. Insofar as this is the case, the program effects reported here are likely to be lower-bound estimates of the BDH impact.

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

households receiving BDH transfers believed there was an enrollment requirement. We thank Mauricio León for help with calculations based on the December 2005 survey.

13. When a BDH beneficiary attempts to collect a transfer at a bank, her national identifi-cation number is used to check whether she is eligible for transfers, and a record is made of the amount of money she receives. The national identification numbers of respondents and other adults in the household were collected in the baseline and follow-up surveys, and the private banks in *Banred* provided data on the payments made to all persons with these identification numbers, on a monthly basis. A household is defined as treated using the banking records if a member withdrew BDH funds at least once. Transfers can be collected on a monthly basis, or they can accumulate for up to four months. In the study sample, 92 percent of households who ever withdrew transfers according to the banking records did so at least ten times over the nineteen-month period, and more than three-quarters of recipient households received a total amount equiv-alent to nineteen monthly transfers.

14. Of the 1,306 households in the sample, 493 (38 percent) are untreated by both mea-sures, and 672 (51 percent) are treated by both measures. Only 22 households (2 percent) appear as treated in the banking records but not the household survey, and 121 (9 percent) appear as treated in the household survey, but not the banking records. The national identification num-ber was used to merge the surveys and the banking records. Discrepancies could arise if enu-merators failed to collect the identification numbers of all household members or made errors copying the numbers. In addition, the banking records do not cover the small share of house-holds (approximately 2 percent) who collect transfers from the National Development Bank, rather than the consortium of private banks in *Banred.*

(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.[15] 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.

Descriptive Statistics at Baseline

We begin by assessing the extent to which the randomized experiment worked. Table 2 summarizes the baseline characteristics of lottery winners and losers and of transfer recipients and nonrecipients. The table shows that there are no significant differences between lottery winners and losers in any of a large number of variables. At baseline, lottery winners and losers are essentially indistinguishable in terms of enrollment, grade attainment, child work, gender, per capita expenditures, assets, parental education, and household size. These comparisons make clear that the random assignment to treatment and control groups was successful.

Although random assignment was successful, there is unfortunately a very imperfect match between assignment to a study group and receipt of BDH transfers. Program take-up among lottery winners was 78 percent; lack of information, the cost of traveling to a bank, and stigma may all have discouraged some households from receiving transfers. More worryingly, 42 percent of households assigned to the control group received transfers. The precise reasons for this substantial 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

15. 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, 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).

T A B L E 2 . **Baseline Differences between Lottery Winners and Losers and between BDH Recipients and Nonrecipients**[a]

Explanatory variable	Difference by lottery status		Difference by receipt of BDH transfers	
	Mean of lottery losers	Difference between lottery losers and winners	Mean of nonrecipient households	Difference between nonrecipient and recipient households
Probability that child is enrolled	0.770	−0.003	0.787	0.052***
	(0.423)	(0.021)	(0.409)	(0.017)
Child age	11.72	0.045	11.63	−0.173*
	(2.82)	(0.094)	(2.83)	(0.102)
Child is male	0.486	−0.030	0.497	−0.009
	(0.500)	(0.020)	(0.500)	(0.019)
Log of per capita expenditures	3.39	0.020	3.38	−0.040
	(0.541)	(0.023)	(0.521)	(0.033)
Asset index	0.041	0.015	0.023	−0.029
	(0.877)	(0.058)	(0.827)	(0.070)
Father's education	4.92	0.029	5.08	0.477**
	(2.73)	(0.147)	(2.62)	(0.187)
Mother's education	3.87	0.144	4.02	0.574***
	(2.93)	(0.150)	(2.90)	(0.213)
Household head is male	0.881	0.013	0.877	0.003
	(0.323)	(0.022)	(0.329)	(0.021)
Household head is indigenous	0.168	0.029	0.156	0.006
	(0.374)	(0.024)	(0.363)	(0.027)
Household head is literate	0.843	−0.032	0.875	0.044
	(0.364)	(0.021)	(0.331)	(0.032)
Household size	6.33	0.051	6.40	0.249**
	(1.98)	(0.123)	(1.87)	(0.122)

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.
a. All means refer to baseline values. Sample size is 2,875 for all variables except for father's education ($n = 2,748$) and mother's education ($n = 2,827$). Standard errors (in parentheses) are adjusted for within-parish correlation.

weeks, but withholding transfers from households that had already begun to receive them was judged to be politically imprudent.

Table 2 reveals clear differences between households that received BDH transfers and those that did not. Children in households that received transfers were significantly more likely to be enrolled at baseline than children in nonrecipient households; the fathers of these children had 0.48 more years of schooling, on average, and their mothers had 0.57 more years. Both of these differences are large, amounting to about one-fifth of a standard deviation, and significant. The heads of households that received transfers were also significantly more likely to be literate. Finally, households that received transfers

were larger than households that did not. Selection into the BDH program (as opposed to selection by the lottery) appears to be nonrandom.

Overall Estimates of BDH Program Impact

We begin our analysis with a reduced-form model that focuses on differences in outcomes between households randomly assigned to the treatment group (rather than households that received BDH transfers in practice) and households randomly assigned to the control group (rather than households that did not receive transfers):

$$(1) \qquad Y_{it} = \alpha_c + \mathbf{X}_{it-1}\beta + Z_i\delta + \varepsilon_{it},$$

where Y_{it} is a dummy variable that takes on the value of one if child i is enrolled in school at the time of the follow-up survey; α_c is a set of canton-level fixed effects; $\mathbf{X}_{it-1}$ is a vector of baseline child and household characteristics; Z_i is a dummy variable that takes the value of one if a family was a lottery winner; and ε_{it} is the regression error term. The parameter δ is an exogenous lottery effect that relies only on the random assignment.

It is also possible to run regressions in which the dependent variable is the change in enrollment between baseline and follow-up. Incorporating baseline enrollment in the estimation helps sweep out any unobserved heterogeneity across households that may have persisted despite random assignment. This alternative specification is as follows:

$$(2) \qquad Y_{it} - Y_{it-1} = \alpha_c + \mathbf{X}_{it-1}\beta + Z_i\delta + \varepsilon_{it}.$$

In both cases, we present results for four specifications, corresponding to different numbers of controls. The first specification includes no controls and is equivalent to unadjusted mean differences between lottery winners and losers; the second specification includes controls for child age and gender; the third specification includes an extended set of controls for baseline child and household characteristics; and the fourth specification supplements these controls with twenty-seven canton-level fixed effects.[16] The stability of the

16. The controls for child age are dummy variables corresponding to each year of age. The extended set of controls includes parental education, per capita expenditures, the composite measure of household wealth, dummy variables for whether paternal or maternal education is missing, dummy variables for whether a household head is indigenous, literate, and male (three separate variables), a dummy variable for rural households, and a measure of the number of household members.

coefficient on lottery winners—both as more controls are added and across specifications that define the dependent variable in terms of levels (as in equation 1) or changes (as in equation 2)—is an important check on the identification strategy.

As an alternative to reduced-form regressions, we use the randomized selection into study groups as an instrument for the actual receipt of BDH transfers:

$$(3) \qquad Y_{it} - Y_{it-1} = \alpha_c + X_{it-1}\varphi + T_i\lambda + v_{it},$$

where T_i is a dummy variable that takes the value of one if a household received BDH transfers and is instrumented with assignment by the lottery, Z_i. By construction, assignment into treatment and control groups is orthogonal with the error term, v_{it}, and therefore satisfies the exclusion restriction.[17]

Despite contamination of the control group and less-than-complete takeup by lottery winners, assignment by the lottery is a strong predictor of BDH treatment. In a regression of treatment on lottery status, with no controls, the coefficient is 0.347, with a robust standard error of 0.040; when the extended list of controls and canton fixed effects are added, the coefficient is 0.361, with a standard error of 0.037.

Main Results of Program Impact

The main results on the overall BDH program impact on enrollment are presented in tables 3 and 4. Table 3 reports the results from the reduced-form, linear probability model (ordinary least squares). In panel A, the dependent variable is enrollment at follow-up, while in panel B, the dependent variable is the change in enrollment between baseline and follow-up. In both cases, the main coefficient of interest is that on the dummy for lottery winners.

The table shows that the probability that a child is enrolled in school at the time of the follow-up survey is 3.2 to 4.0 percentage points higher among lottery winners than lottery losers. The estimates of program effects are stable across specifications, as one would expect given the random assignment by the BDH lottery. The stability of the coefficient on lottery losers does not stem from the lack of the explanatory power of the additional covariates. For example, in the specifications in panel A, the R squared rises from 0.001 in the specification with no controls to 0.256 in the specification that includes the full list of controls and canton fixed effects. Also, results are similar

17. Imbens and Angrist (1994).

T A B L E 3 . The Impact of BDH Transfers on School Enrollment: Reduced-Form Models[a]

Explanatory variable	(1)	(2)	(3)	(4)
A. Dependent variable: Enrollment at follow-up				
BDH lottery winner	0.032*	0.040**	0.037**	0.039**
	(0.019)	(0.017)	(0.016)	(0.016)
Child is male		0.050***	0.053***	0.054***
		(0.018)	(0.017)	(0.017)
Log of per capita expenditures			0.053***	0.078***
			(0.018)	(0.017)
Asset index			0.016	0.016
			(0.011)	(0.011)
Father's education			0.015***	0.015***
			(0.005)	(0.006)
Mother's education			0.022***	0.021***
			(0.005)	(0.005)
Household size			−0.001	−0.002
			(0.007)	(0.006)
Child age dummies	No	Yes	Yes	Yes
Canton dummies	No	No	No	Yes
R^2	0.001	0.175	0.237	0.256
B. Dependent variable: Change in enrollment between baseline and follow-up				
BDH lottery winner	0.035*	0.034*	0.035**	0.037**
	(0.018)	(0.017)	(0.017)	(0.018)
Child is male		0.030**	0.029*	0.031**
		(0.015)	(0.015)	(0.015)
Log of per capita expenditures			−0.025	−0.026
			(0.017)	(0.019)
Asset index			0.014	0.015
			(0.009)	(0.009)
Father's education			0.005	0.005
			(0.004)	(0.004)
Mother's education			0.000	0.000
			(0.003)	(0.004)
Household size			−0.005	−0.007
			(0.006)	(0.006)
Child age dummies	No	Yes	Yes	Yes
Canton dummies	No	No	No	Yes
R^2	0.002	0.046	0.054	0.064

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
a. The specifications in columns (3) and (4) also include dummy variables for whether father's education or mother's education is missing and for whether the household head is male, indigenous, or literate (three separate variables). Sample size is 2,875 in all regressions. Standard errors (in parentheses) are adjusted for within-parish correlation.

T A B L E 4 . The Impact of BDH Transfers on School Enrollment: Two-Stage Least Squares[a]

Explanatory variable	(1)	(2)	(3)	(4)
BDH lottery winner	0.101*	0.099*	0.100**	0.103**
	(0.052)	(0.050)	(0.050)	(0.048)
R^2		0.041	0.047	0.057

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

a. The dependent variable is the change in enrollment between baseline and follow-up. Specification (1) includes no controls; specification (2) includes a set of single-year child age dummies and a dummy for child gender; specification (3) supplements this with variables for baseline log per capita expenditures, the asset composite, father's education, mother's education, dummy variables for whether father's education or mother's education is missing, household size, and dummy variables for whether the household head is male, indigenous, or literate (three separate variables); specification (4) also includes a set of canton fixed effects. In all specifications, the dummy variable for whether a household received transfers is instrumented with random assignment by the BDH lottery. The sample size is 2,875 in all regressions. Standard errors (in parentheses) are adjusted for within-parish correlation.

when the dependent variable is defined in levels at follow-up (panel A) or in changes (panel B). We therefore focus on the specification in changes for the rest of the paper.

Table 4 presents comparable results from the structural, two-stage least squares model. In this case, the main coefficient of interest is on children in households that received BDH transfers, which is instrumented with the randomized assignment by the lottery. These regressions show that BDH transfers resulted in an increase in enrollment of approximately 10 percentage points among complier households.[18] This is a large effect compared with estimates for other programs in Latin America. For example, Schultz estimates that *Oportunidades* had an impact on the enrollment of children in first through eighth grades of between 3.4 and 3.6 percentage points, while Attanasio and others conclude that *Familias en Acción* had an impact of approximately 10 percentage points on the enrollment of children age twelve to seventeen in rural areas and 5 percentage points in urban areas, with no impact among younger children.[19] We do not have a satisfactory explanation for the large magnitude of the BDH effects. Households in our sample tend to be very poor—median per capita expenditures at baseline were just under two dollars per capita per day and less than about one dollar per capita per day for about 10 percent of the households in the sample. However, households

18. Additional results (unreported but available on request) show that the coefficients are similar if the *Banred* banking records are used to construct the variable for BDH transfers (rather than the answers provided by households in the follow-up survey, as in the table) or if estimation is done by joint bivariate probit, rather than two-stage least squares.

19. Schultz (2004); Attanasio and others (2005).

who benefited from *Oportunidades* in Mexico also appear to have been very poor. Hoddinott, Skoufias, and Washburn estimate that per capita expenditures in the *Oportunidades* control localities in 1998 were 189 pesos, or approximately US$0.61 a day at prevailing exchange rates.[20] Cross-country comparisons of expenditure levels are hazardous for a variety of reasons, including differences in purchasing power and in expenditure modules across surveys, but it does not appear at first glance that households in the BDH evaluation sample are a great deal poorer than those who benefited from *Oportunidades* in Mexico. Moreover, the amount of the transfer in *Oportunidades* accounted for approximately 20 percent of the consumption of the average household, versus 9 percent in our sample, which only makes the large magnitude of the BDH program effects on enrollment all the harder to explain. Another possible explanation for the difference in magnitudes in the estimated effects across countries is related to estimation choices. Instrumental variables estimates such as those we report for the BDH apply only to households whose likelihood of receiving transfers was affected by the BDH lottery—what Angrist, Imbens, and Rubin call compliers.[21] These compliers cannot be easily identified from the data without additional assumptions, but they may have characteristics that made their enrollment choices particularly sensitive to transfers.[22]

## Heterogeneity of Treatment Effects

We next assess possible heterogeneity of BDH treatment effects by school grade, gender, place of residence, maternal education, and per capita expenditures. We first focus on differences by school grade. We begin by estimating the rate of continuation from one grade to another, using information on the years of schooling completed by young adults in the baseline survey.[23] Specifically, we calculate the fraction of these young adults who report having completed exactly a given number of years of schooling—for example, the fraction of adults who have completed exactly one year of schooling, exactly two years, and so on. The continuation rate is then given by the difference in these proportions of adults across adjacent grades. Figure 1 summarizes the results. The figure reveals sharp drops in the continuation rate in

20. Hoddinott, Skoufias, and Washburn (2000, p. 49).
21. Angrist, Imbens, and Rubin (1996).
22. See Angrist (2004) for a discussion.
23. We define young adults as those aged nineteen to twenty-four, but our results are very similar with other age ranges.

F I G U R E 1 .   **Continuation Rate, by School Grade**

Continuation rate
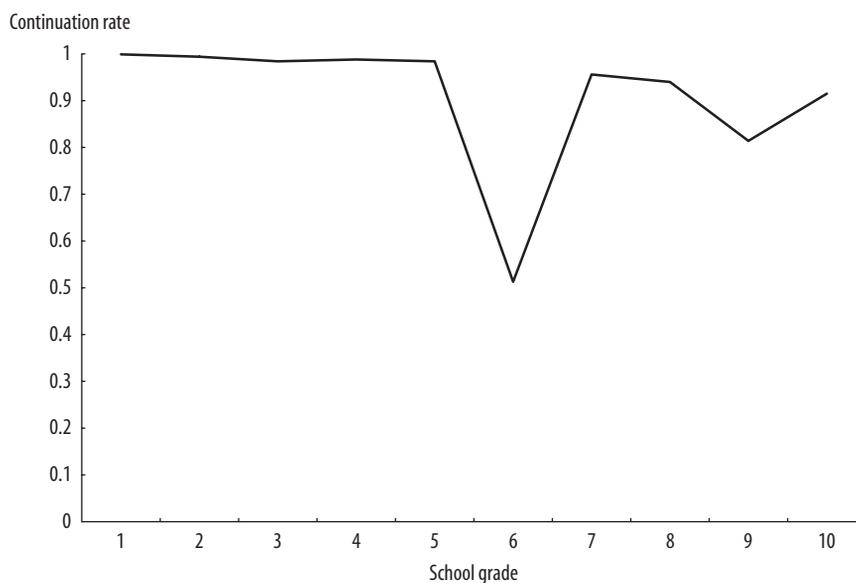
School grade

Source: Authors' calculations, based on the 2003 evaluation survey.

sixth grade (the last year of primary school) and ninth grade (the last year of
lower secondary school). One might therefore expect that the BDH effects
would be largest at these transition grades in which, absent the program, the
likelihood of dropping out of school is large.

To assess the heterogeneity of BDH program effects by school grade, we
use information on the highest grade attained by children at baseline. For chil-
dren who were enrolled in school, the highest grade attained and the grade in
which a child was enrolled at baseline are closely related. For example, we can
infer that a child who was enrolled in school and whose highest grade attained
was fifth grade was enrolled in sixth grade at baseline. We therefore generate
ten dummy variables, corresponding to the attainment of no grades through
the attainment of ninth grade at baseline.[24] We then run separate regressions

24. Both the baseline and follow-up surveys collected schooling information only for chil-
dren between the ages of six and seventeen; we thus have only a handful of children who have
attained more than nine grades at baseline.

T A B L E 5 .   **Heterogeneity of Treatment Effects, by Baseline Grade**[a]

Grade	Continuation rate	Coefficient
No schooling		−0.002
		(0.031)
First	0.999	0.001
		(0.020)
Second	0.994	−0.031
		(0.019)
Third	0.984	−0.031
		(0.041)
Fourth	0.988	0.009
		(0.043)
Fifth	0.984	0.113**
		(0.044)
Sixth	0.513	0.032
		(0.026)
Seventh	0.956	−0.007
		(0.049)
Eighth	0.940	0.125***
		(0.046)
Ninth	0.814	0.130*
		(0.068)

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.
a. The dependent variable in all specifications is the change in enrollment between the baseline and follow-up surveys. The variable "grade" refers to the highest grade completed at the time of the baseline survey. The continuation rate is calculated on the basis of adults aged nineteen to twenty-four at the time of the baseline survey. All specifications include a set of single-year child age dummies and a dummy for child gender. Standard errors (in parentheses) are adjusted for within-parish correlation. Sample size varies by grade.

that limit the sample to children who have achieved a given number of years of schooling at baseline. These regressions include the age and gender dummies, but they do not include the extended set of controls and canton fixed effects, given the small sample sizes for some grades.

The results in table 5 indicate that BDH program effects are concentrated among children who were likely to face transition grades between the two surveys. For children who had completed exactly five years of schooling at baseline, the BDH program effect is 0.113 (with a standard error of 0.044); for those who had completed eight years of schooling, it is 0.125 (with a standard error of 0.046). For all other children, the program effect on enrollment is insignificant at the 5 percent level or higher, although it is borderline significant for children who had completed exactly nine years of schooling at baseline. This pattern of results, whereby BDH program effects on enrollment are concentrated among children in school grades in which the prob-

T A B L E 6 . Heterogeneity of Treatment Effects, by Baseline Characteristics[a]

Baseline characteristic	Baseline enrollment	Coefficient	Test of equality (p value)
Gender			
Boys	0.784	0.027	0.631
		(0.022)	
Girls	0.753	0.042	
		(0.024)	
Place of residence			
Urban	0.784	0.049***	0.150
		(0.018)	
Rural	0.753	0.022	
		(0.019)	
Maternal education			
Low	0.693	0.028	0.479
		(0.022)	
High	0.866	0.042**	
		(0.016)	
Per capita expenditures			
Low	0.729	0.045**	0.417
		(0.021)	
High	0.807	0.024	
		(0.022)	

** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.
    a. The dependent variable in all specifications is the change in enrollment between the baseline and follow-up surveys. All specifications include a set of single-year child age dummies and a dummy for child gender. Sample size is 2,875 in all regressions. Standard errors (in parentheses) are adjusted for within-parish correlation.

ability of continuing to the next grade was relatively low, is similar to results for *Oportunidades* in Mexico, where program effects on enrollment are concentrated among children enrolled in sixth grade at baseline.[25]

We next focus on possible differences in BDH program effects based on gender, place of residence (urban or rural), maternal education (incomplete primary or less versus completed primary or more), and baseline per capita expenditures (above or below the median). The results of these calculations are presented in table 6. Each of these comparisons is based on a regression with a main effect (for example, a main effect for girls) and two interactions (for example, an interaction between lottery winners and girls and an interaction between lottery winners and boys). The table also includes a row for the baseline enrollment levels; these are useful for assessing whether larger BDH treatment effects are found among groups with the biggest margin for

25. Schultz (2004); de Janvry and Sadoulet (2006).

improvement. Finally, the last column of the table presents the results of an *F* test of the equality of coefficients on the two interaction effects. For example, in the first comparison, we report the *p* value on an *F* test of the equality of the coefficient on the interaction between lottery status and girls and the coefficient on the interaction between lottery status and boys.

As the table shows, we find no clear evidence of heterogeneity across these dimensions of the data. The point estimates are larger for girls than for boys, for urban than for rural areas, for more educated mothers than for less educated mothers, and for poorer households. None of these differences are statistically significant, however.

Program Effects among Conditioned and Unconditioned Households

The second part of the paper focuses on differences in outcomes between conditioned and unconditioned households, as described above. Recall that conditioned households are those that told enumerators that they believed the BDH transfers had a schooling requirement, whereas unconditioned households told enumerators that transfers did not have a schooling requirement. We begin by limiting the sample to lottery winners, and we then compare the outcomes for conditioned and unconditioned households after adjusting for observable differences in a variety of ways. The first estimator is based on a linear regression that includes a dummy variable for conditioned households (among lottery winners):

$$(4) \qquad Y_{it} - Y_{it-1} = \alpha_c + \mathbf{X}_{it-1}\eta + C_i\psi + \varepsilon_{it},$$

where C_i is a dummy variable that takes the value of one if the respondent told the enumerator that the transfers had a schooling requirement and zero otherwise.

Linear regression models rely heavily on extrapolation to impute potential (or counterfactual) outcomes. This may introduce biases if the distribution of the covariates $\mathbf{X}_{it-1}$ is very different for conditioned and unconditioned households. We therefore present results based on two alternative methods to correct for observable differences between conditioned and unconditioned households. Both of these are recent developments in the literature on matching estimators.[26] The first of these is a bias-adjusted matching estimator

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26. For a thoughtful review, see Imbens (2004).

proposed by Abadie and Imbens.[27] This estimator uses nearest-neighbor matching to match treated and control households on the basis of their covariates; it then adjusts for any remaining differences in the distribution of the covariates with regression techniques. Abadie and Imbens show that this bias-corrected matching estimator performs well in practice.[28] The second estimator relies on a reweighting scheme for the data, as proposed by Hirano, Imbens, and Ridder.[29] To calculate the weights, a dummy variable for conditioned households is regressed on their baseline characteristics (again for lottery winners):

$$(5) \qquad C_i = \alpha + \mathbf{X}_{it-1}\gamma + \omega_i.$$

This regression is then used to predict the probability that a household is conditioned, π_i, also known as the propensity score.[30] Hirano, Imbens, and Ridder show that giving conditioned households a weight of $1/\pi_i$ and unconditioned households a weight of $1/(1 - \pi_i)$ produces a fully efficient estimator of the Average Treatment Effect, with conservative standard errors.[31] To further adjust for observable differences between conditioned and unconditioned households, we also experiment with trimming the sample to remove the 10 percent of households with the lowest propensity score and the 10 percent with the highest score.

It is also possible to use information on lottery losers in the estimation. Since conditioned households are found among both lottery winners and losers, we can estimate a difference-in-differences specification:

$$(6) \qquad Y_{it} - Y_{it-1} = \alpha_c + \mathbf{X}_{it-1}\beta_1 + C_i\beta_2 + Z_i\delta_i + \left(Z_iC_i\right)\delta_2 + \varepsilon_{it}.$$

27. Abadie and Imbens (2002).

28. Abadie and Imbens (2002). Another recent application is McKenzie, Gibson, and Stillman (2006).

29. Hirano, Imbens, and Ridder (2003).

30. To avoid values of the propensity score below zero or above one, we estimate equation 5 by probit rather than ordinary least squares (OLS). The regression does not include canton fixed effects, as there are three cantons in which no household was conditioned. The probit algorithm automatically drops these from the sample when canton fixed effects are included in the regression, and no weights would be generated for these households. Our results are not sensitive to this choice. The coefficients for the weighted regressions reported below are very similar to those obtained when households in these cantons are dropped from the sample, weights are estimated on the basis of a regression that includes canton fixed effects, and weighted regressions are run on this smaller sample.

31. Hirano, Imbens, and Ridder (2003). Recent applications include Hirano and Imbens (2002); Chen, Mu, and Ravallion (2006); Filmer and Schady (2008).

The main parameter of interest is δ_2, which denotes the difference-in-differences estimate of the effect of the perceived enrollment requirement among lottery winners.[32] This specification allows us to sweep out any fixed differences between conditioned and unconditioned households, which could help reduce any remaining sources of bias. However, multiple differencing of this sort can also reduce the signal-to-noise ratio of the estimates and make the point estimates less precise.

Comparisons between Conditioned and Unconditioned Households at Baseline

We begin by comparing the characteristics of conditioned and unconditioned households separately for lottery winners and losers.[33] Table 7 reveals a number of significant differences between conditioned and unconditioned households at baseline. Children in conditioned households were more likely to be enrolled in school than children in unconditioned households; the parents of these children have more schooling and are less likely to be illiterate than their unconditioned counterparts; and conditioned households tend to have more household members. Some of these differences are large. For example, conditioned households have between 0.57 and 0.66 more years of paternal schooling and between 0.68 and 0.69 more years of maternal education than unconditioned households.

Table 8 summarizes baseline differences between conditioned and unconditioned households within the sample of lottery winners after matching and reweighting.[34] For the purpose of the table, all of the covariates have been normalized to have zero mean and unit standard deviation. The sample in panel A includes all lottery winners, while the sample in panel B trims the 10 percent of households with the highest propensity scores and the 10 percent with the lowest scores. The first and second columns report the standardized means of a given variable for conditioned and unconditioned households; the third column reports the raw difference between these values; the fourth column

32. Since the dependent variable in equation 6 is defined as changes in enrollment between baseline and follow-up, this model could also be written as a triple-difference specification, moving Y_{it-1} to the right-hand side of the equation, and interacting it with the parameters C_i, Z_i, and $Z_i C_i$.

33. The sample size for these comparisons and for the regressions in table 9 is slightly smaller than for our earlier analysis because in thirty-five households, including seventy-four school-aged children, respondents did not answer questions about knowledge and perceptions of the BDH program. Differences between households that did not answer these questions and other households in terms of baseline parental education, per capita expenditures, assets, child enrollment, age, and gender are not significant.

34. Abadie and Imbens (2002); Hirano, Imbens, and Ridder (2003).

T A B L E 7 . Baseline Differences between Conditioned and Unconditioned Households[a]

Explanatory variable	Lottery winners		Lottery losers	
	Mean of conditioned households	Difference	Mean of unconditioned households	Difference
Probability that child is enrolled	0.819	0.077***	0.823	0.068**
	(0.385)	(0.028)	(0.383)	(0.025)
Child age	11.60	−0.171	11.24	−0.565***
	(2.78)	(0.147)	(2.84)	(0.179)
Child is male	0.475	−0.016	0.480	−0.049
	(0.500)	(0.029)	(0.500)	(0.033)
Log of per capita expenditures	3.37	−0.024	3.55	−0.073
	(0.523)	(0.053)	(0.502)	(0.059)
Asset index	0.014	−0.037	−0.032	−0.072
	(0.836)	(0.071)	(0.781)	(0.090)
Father's education	5.39	0.659***	5.36	0.572*
	(2.70)	(0.247)	(2.37)	(0.291)
Mother's education	4.35	0.681**	4.29	0.689**
	(3.04)	(0.320)	(2.62)	(0.288)
Household head is male	0.877	−0.006	0.844	−0.028
	(0.329)	(0.030)	(0.363)	(0.031)
Household head is indigenous	0.147	−0.030	0.098	−0.057
	(0.355)	(0.043)	(0.298)	(0.041)
Household head is literate	0.877	0.046	0.920	0.061*
	(0.329)	(0.033)	(0.271)	(0.036)
Household size	6.44	0.145	6.51	0.344*
	(1.93)	(0.203)	(1.80)	(0.176)

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.

a. All means refer to baseline values. Sample size is 1,472 for lottery winners and 1,329 for lottery losers. Standard errors (in parentheses) are adjusted for within-parish correlation.

provides the average difference in the matched pairs, after matching with the bias-corrected matching estimator; the fifth column provides the average difference between conditioned and unconditioned households when conditioned households are given a weight of $1/\pi_i$ and unconditioned households are given a weight of $1/(1 - \pi_i)$.[35]

35. The matching estimates are based on matching with the four nearest neighbors, although our results are not sensitive to this choice. For example, the estimated program effect for the full sample of lottery winners reported in table 9 is 0.088 (with a standard error of 0.020). When matching is performed using only one neighbor, the value is 0.091 (with a standard error of 0.020), and when matching is performed using sixteen neighbors, the estimate is 0.086 (with a standard error of 0.020).

T A B L E 8 .   Baseline Differences between Conditioned and Unconditioned Households after Matching and Reweighting Lottery Winners[a]

	Average		Difference		
Explanatory variable	Conditioned	Unconditioned	Raw	Matching	Reweighting
A. Full sample					
Enrollment	0.124	−0.057	0.181	0.061	−0.070
Child age	−0.042	0.019	−0.061	0.019	0.027
Child is male	−0.023	0.010	−0.033	−0.046	−0.029
Log of per capita expenditures	−0.029	0.015	−0.044	−0.057	0.021
Asset index	−0.031	0.011	−0.043	−0.041	0.008
Father's education	0.194	−0.074	0.268	0.055	−0.044
Mother's education	0.180	−0.072	0.252	0.082	−0.033
Household head is male	−0.014	0.004	−0.017	0.009	0.018
Household head is indigenous	−0.056	0.024	−0.080	−0.042	−0.027
Household head is literate	0.094	−0.034	0.128	0.024	0.047
Household size	0.053	−0.020	0.073	0.030	−0.028
Mean difference			0.111	0.042	0.032
B. Trimmed sample					
Enrollment	0.148	0.018	0.129	0.043	0.040
Child age	−0.078	−0.022	−0.056	0.026	−0.030
Child is male	0.010	0.012	−0.002	−0.011	−0.004
Log of per capita expenditures	0.005	−0.002	0.008	−0.028	−0.001
Asset index	0.016	0.081	−0.065	−0.064	−0.040
Father's education	0.088	−0.042	0.129	0.068	0.005
Mother's education	0.058	−0.072	0.130	0.093	0.020
Household head is male	0.103	0.068	0.035	0.010	0.033
Household head is indigenous	−0.024	−0.002	−0.022	−0.034	0.016
Household head is literate	0.060	0.052	0.008	−0.008	−0.069
Household size	0.076	0.042	0.035	0.004	0.023
Mean difference			0.056	0.035	0.026

a. The sample is limited to lottery winners. All variables have been standardized to have zero mean and unit standard deviation. The first two columns present the means for conditioned and unconditioned households, respectively; the third column reports the difference in these raw means; the fourth column reports the difference after bias-corrected nearest-neighbor matching (Abadie and Imbens 2002); the fifth column reports the difference after reweighting with a transformation of the propensity score (Hirano, Imbens, and Ridder 2003). Sample sizes are 1,472 for the full sample and 1,178 for the trimmed sample.

Table 8 shows that the average raw difference between conditioned and unconditioned households in the full sample is equal to 0.111 standard deviations. Trimming removes approximately half of the average imbalance in the samples; the bias-corrected matching estimator removes approximately two-thirds of the difference in the covariates; and the reweighting scheme removes approximately three-quarters of the imbalance in the samples. In sum, both estimators make conditioned and unconditioned households very closely comparable in terms of their observed covariates, especially when they are combined with judicious trimming of the sample.

**T A B L E   9 .   BDH Effects on Enrollment at Follow-up, Conditioned
and Unconditioned Households**[a]

Method and sample	Coefficient	No. observations
A. Lottery winners only		
OLS		
Full sample	0.062*	1,472
	(0.034)	
Trimmed sample	0.059	1,178
	(0.038)	
Matching		
Full sample	0.076***	1,472
	(0.026)	
Trimmed sample	0.072***	1,178
	(0.028)	
Reweighting		
Full sample	0.079**	1,472
	(0.036)	
Trimmed sample	0.069*	1,178
	(0.039)	
B. Lottery winners and losers		
Difference-in-differences		
Full sample	0.060	2,801
	(0.051)	
Trimmed sample	0.072	2,236
	(0.061)	

   * Statistically significant at the 10 percent level.
   ** Statistically significant at the 5 percent level.
   *** Statistically significant at the 1 percent level.
   a. Standard errors (in parentheses) are adjusted for within-parish correlation.

The Effect of Conditions Attached to Transfers

Table 9 reports the results of comparing the school enrollment behavior of
conditioned and unconditioned households. The table includes the results for
the four different estimation methods, with and without data trimming. Panel
A includes lottery winners only and reports the coefficient on conditioned
households. Panel B includes lottery winners and losers and reports the coef-
ficient on the interaction between lottery winners and conditioned households.
All specifications include the extended list of controls and canton fixed effects.

   The table shows that the change in enrollment between baseline and follow-
up is 6 to 8 percentage points higher among households that believed the
BDH transfers had an enrollment requirement than among other households.
The parameters are robust to different estimation methods and to trimming of

the data. In some specifications, in particular the difference-in-differences specification, the point estimates are not significant at conventional levels. However, the point estimates from this specification are very close to those from the specifications in which the sample is limited to lottery winners.

Finally, we discuss one more possible concern with our estimates of the effect of the perceived condition on enrollment. Consider a case in which two kinds of households are eligible for BDH transfers—type A households, which enroll their children in school, and type B households, which do not. Despite this difference in behavior, both types of households receive transfers. Type A households may continue to believe that transfers are conditional, but type B households quickly realize that transfers are not conditional—after all, they continue to receive transfers even though their children are not in school. In this situation, the causality flows (at least in part) from school enrollment to awareness of the condition, and the parameter on the condition in the enroll-ment regression would not have a causal interpretation.

In an attempt to rule out this possibility, we performed the following vali-dation exercise. If causality flows from school enrollment to the perceived condition, we would expect that the association between the condition and enrollment would be highest for households with children enrolled in the earli-est grades in primary school at baseline, since enrollment rates are very high at that level of schooling. If, on the other hand, causality flows from the perceived condition to school enrollment, we would expect that the association would be highest for children enrolled in the transition grades, in which, absent the trans-fers, the likelihood of dropping out of school is large. As shown above, chil-dren who had completed exactly five or eight years of schooling at baseline were most likely to be enrolled in transition grades with a high dropout rate (sixth and ninth grades, respectively). The validation exercise therefore com-pares the effect of the condition on enrollment for children in lower primary grades and those in transition grades.

For this purpose, we use the difference-in-differences specification that includes both the lottery winners and the lottery losers. When the sample is lim-ited to children who had completed exactly five or eight years of schooling at baseline, the coefficient on the interaction between lottery winners and condi-tioned households is 0.194 (with a standard error of 0.113); when the sample is limited to children who had completed no more than four years of schooling at baseline, the coefficient is 0.021 (with a standard error of 0.037). The associa-tion between the perceived condition and enrollment is thus large and signifi-cant for children in transition grades (where the margin for positive program effects is large), but it is absent for those in lower primary grades (where this

margin is very small). We conclude from these checks that it is very unlikely that the results in table 9 are substantially biased by reverse causality.

Conclusion

Policymakers throughout the developing world have long sought to identify programs that build the human capital of the poor. Remarkably little is known, however, about the effect of policies on educational outcomes.[36] This paper adds to a growing literature illustrating that cash transfer programs in Latin America have had positive, and in some cases large, effects on school enrollment. The results for the *Bono de Desarrollo Humano* (BDH) program in Ecuador are based on the random assignment of households into treatment and control groups based on a lottery. Despite substantial contamination of the control group, our reduced-form estimates imply that school-aged children in households that were BDH lottery winners were 3.2 to 4.0 percentage points more likely to be enrolled in school than children in households that were lottery losers. The results are robust to defining the dependent variable in levels or changes, and they are insensitive to the addition of a large number of controls.

The results in this paper also contribute to an ongoing discussion about the extent to which the effects of conditional cash transfer programs on enrollment are a result of the income effects or the transfer conditions.[37] The *Economist* recently stated that "cash transfers, with strings attached, are a better way of helping the poor than many previous social programs."[38] To date, however, there is no experimental or quasi-experimental evidence on this point. In Ecuador, schooling requirements were announced, but never enforced. Nevertheless, approximately one-quarter of the households in our sample (as well as in other data) believed that parents were expected to enroll their children in school to be eligible for BDH transfers. We compare the impact of transfers among these conditioned households with those found among unconditioned households. These estimates suggest that the impact of the BDH program on enrollment was only significant among households that believed there was a

36. Glewwe (2002).

37. A recent five-day international conference on conditional cash transfers held in Istanbul, for example, featured a heated debate between advocates of conditional and unconditional cash transfers. See World Bank's website "Conference Sessions: Day 2 Session (June 27, 2006)" (http://info.worldbank.org/etools/ICCT06/D2_S2new.htm).

38. "Poverty in Latin America: New Thinking about an Old Problem," *Economist,* 17–23 September 2005, pp. 36–38.

schooling requirement associated with the transfers. Although these comparisons are not experimental, they are insensitive to the addition of a large number of controls, to estimates that difference out fixed unobservable differences between conditioned and unconditioned households, and to various ways of trimming and reweighting the data. We conclude that differences in the response to the BDH program between conditioned and unconditioned households are likely to have a causal interpretation.

In Ecuador, enrollment conditions were never monitored, and households that did not enroll their children in school were not penalized. Does this mean that administrators of cash transfer programs can simply announce a schooling requirement and expect that parents will enroll their children in school because they are afraid they could otherwise be disqualified from transfers? The results for Ecuador suggest that such a strategy may work in the short term—at least, for that fraction of households that can be convinced there is an enrollment requirement. We suspect, however, that the effect of an unenforced condition will gradually dissipate as households realize that they will not be penalized if they do not send their children to school and then adjust their behavior accordingly.

Comments

Ximena Peña: The well-measured success of the Mexican *Oportunidades* cash transfer program, formerly known as Progresa, has led many countries to design similar programs, including rigorous evaluation schemes. This paper evaluates the impact of Ecuador's *Bono de Desarrollo Humano* (BDH) on the school enrollment of children aged six to seventeen. Two special characteristics of the BDH implementation make its evaluation challenging and the results, in turn, quite interesting.

First, treatment is potentially endogenous since unmeasured characteristics may affect both the likelihood of receiving transfers and enrollment. The program originally followed an experimental design in which lottery winners received transfers and lottery losers were to be incorporated into the program at a later date, thereby providing a control. At baseline, there were no significant differences between lottery winners and losers in a wide set of covariates, including enrollment. Actual treatment, however, differed from the random assignment: 22 percent of lottery winners did not take up the program, while 42 percent of lottery losers received transfers. This translates into significant differences between treatment and control groups in several covariates at baseline, notably enrollment. In determining the impact of BDH on enrollment, the authors estimate the Intent to Treat and regress school enrollment at follow-up against the lottery outcome in addition to the appropriate controls. Their results suggest that the Intent to Treat increased enrollment by 3.2 to 4.0 percent. They get very similar results when they repeat the estimation using changes in enrollment between baseline and follow-up as the dependent variable. With the same dependent variable, they use the lottery outcome to instrument for actual treatment and estimate the effects using two-stage least squares. They find an increase in enrollment of between 9.2 and 11.4 percentage points. These results are large compared with other findings in the literature, especially since they are not focusing on the specific age groups for which impacts are highest. In other countries, transfers have the

strongest impact on enrollment at stages with high dropout rates; the authors similarly find that the impacts are highest among children facing transition grades (five or eight completed years of schooling) when school abandonment is high. Other than transition grades, impact is only significant at nine completed years of schooling. The program is thus ineffective at increasing enrollment at other stages of education.

Second, even though transfers were delivered with no strings attached, around one-fourth of the treatment group believed attendance was mandatory. Conditions on school enrollment and attendance were included in the program design and advertised at informational town-hall-style meetings, in radio and television spots, and at sign-up. In practice, however, conditions were never monitored and noncomplying households were not penalized. Nevertheless, the follow-up survey revealed that 27 percent of beneficiaries believed that school attendance was a prerequisite to receiving transfers. Using only lottery winners, the authors estimate the effect of conditionality on enrollment. Since the distribution of covariates differs at baseline between conditioned and unconditioned households, the authors control for selection on observables in several ways, including a bias-adjusted matching estimator, a reweighting scheme, and data trimming to remove 20 percent of the sample with the highest and lowest propensity scores. They find that enrollment is 5 to 8 percent higher in conditioned than in unconditioned households. The paper would benefit if these results were translated into a more easily comparable measure, such as net differences in enrollment rates or years of schooling.

Based on the wording of the perceived conditioning—namely, ensuring that children attend school—the program seems to have additional effects on attendance that are impossible to measure using existing data. This aspect should be included in a future follow-up survey, to facilitate the evaluation of other impacts of the program.

Exploring several factors would strengthen the paper's results. First, can the results be generalized? The four regions chosen for the study are in the highlands. Are they representative of the country as a whole? Second, the estimation strategy does not explicitly consider the effects of the precursor to the BDH, called *Bono Solidario,* which was a poorly targeted unconditional cash transfer program. Even though families who received *Bono Solidario* transfers were excluded from the sample, its effects are present. An effort should be made to address this. For example, was the program evenly spread in the regions evaluated? Third, the Colombian experience shows that the targeting instrument (Sisben) can be manipulated. It would be relevant to know whether this is a problem for Selben and whether that poses additional chal-

lenges for the present evaluation. Finally, the wrinkle in the BDH program implementation offers a unique opportunity to explore the importance of costly conditionality. While the presented results are interesting, I am left wondering about the implications for optimal program design. Is the additional impact on schooling worth the cost of having strings attached to the program?

The proliferation of evaluations of cash transfer programs has provided a lot of information on the impact on several outcomes, such a nutrition, child labor, and school enrollment. However, the generated information has not been used in the same measure to adjust program design. This is mainly due to the misalignment of the required changes and politicians' incentives. If academics do not push harder to use the body of results to fine-tune the programs, millions of dollars devoted to impact evaluations will continue to translate into academic publications only, and not into the desired maximum benefits for the poor.

Luis F. López-Calva: Norbert Schady and Maria Caridad Araujo's paper represents an important step forward in understanding the effects of conditional cash transfer programs in Latin America and the behavioral responses of households that receive them. This is not a trivial issue considering that almost 80 million people in Latin America and the Caribbean were beneficiaries of such subsidies in 2007.

The paper uses a randomized study design to analyze the impact of the *Bono de Desarrollo Humano* (BDH), a conditional cash transfer program, on school enrollment among poor children in Ecuador. The authors present two main results, namely, that the BDH program had a large, positive impact on the school enrollment of poor Ecuadorean children and that the program effects are significantly larger among a minority of households that believed the transfers had a school-enrollment requirement. The reason the paper establishes a group of people who "believed" the transfer was conditioned is that the conditionality was not really enforced, though the implementation created the perception among certain households that it would be.

The paper deals with a difficult set of problems that makes traditional impact-evaluation methodologies less appropriate. First, the program instituted a randomized design that was violated in practice, which could potentially affect the validity of the assessment. To address this challenge, the authors wisely exploit an originally random design as an instrument for intervention. The results are quite robust, and the instrument seems appropriate. Thus the data are not really experimental, but there is an initial lottery that can be used as an instrument, highly correlated with treatment. Second, as already mentioned, the program's conditionality was not enforced. Some households,

however, perceived the transfer to be subject to a specific behavior. While this represents a problem for the analysis from a traditional perspective, it opens the possibility of discussing new issues, previously unexplored in conditional cash transfer program evaluations, as explained below.

The econometrics consist mainly of setting up a reduced-form model, then adding controls and the instrument for treatment in a two-stage least squares model. The authors could strengthen the study by controlling for supply-side issues, to expand their exploration of the heterogeneous results among people receiving the transfer. The survey includes access to services, for example, so they may be able to examine the effect of distance to the school. While the fixed effects for the community may already be accounting for that factor, trying specifications in which supply-side characteristics vary across households could be useful, if available. Assessments of these interventions systematically assume that the supply side is homogeneous, but this assumption could obscure important factors in the program's outcome.

An important issue raised in the paper involves the quality of targeting under decentralized selection schemes. Ecuador's centralized design started with a randomized selection, which later was not followed by the operators in the field. One criticism of randomization is that under a limited budget, a program should start by including the poorest. These critics believe that a decentralized decision mechanism based on discretionary selection would best accomplish this goal, since field operators have more information for selecting the poorest among eligible households. For the case of Ecuador, Schady and Araujo compare the socioeconomic characteristics of lottery winners and losers in the original design with those of actual recipients and nonrecipients. They find that randomization would have resulted in a more progressive (or neutral) selection than the discretionary selection by operators.

One caveat that bears mentioning has to do with conditioned versus unconditioned households, in terms of their perceptions. Those beneficiaries whose perceptions are conditioned have obvious incentives to declare enrollment. The paper correctly addresses this concern given the existing information. The authors claim, however, that this problem arises in all databases like this, yet in other cases, like *Oportunidades* in Mexico after 2003, there is an administrative record that allows verification.

The evidence in the paper is generally convincing. The authors satisfactorily solve the methodological challenges, and the effects are economically relevant. They need to extend their evaluations beyond traditional impact-evaluation analysis, however. Their analysis could be used to address deeper questions, where feasible. For example, in the area of child labor, the normative argu-

ments in academic and policy realms can be summarized as a debate over preferences versus constraints. Conditional cash transfers relax the household's budget constraint but impose a condition on the behavioral response. This implies that, for several valid reasons, the desired response may not be socially optimal, or even individually welfare improving, from a standard long-run perspective. In policy circles, the question is whether the conditionality is justifiable, or whether the relaxation of the cash constraint is sufficient to generate the behavioral response the policymakers are trying to induce. Until recently, this question has been addressed empirically. Alvarez, Devoto, and Winters show that conditionality may play a role as an effective screening device, and it becomes a useful tool for policymakers.[1] They also show, however, that supply-side issues (related to health service providers in the case of *Oportunidades* in Mexico) may be a factor for dropouts from the program.

Schady and Araujo find that many so-called unconditioned households increased enrollment (the presumably desired result), while some conditioned households did not. What could explain the differential response? Do households that responded in the desired direction have access to other markets, like credit? Are there supply-side issues that can explain such differences? The impact evaluation literature assumes that supply-side issues, other context-related constraints, and access to other markets are either homogeneous across households or controlled for through appropriate specifications. Experiments like the one analyzed here, featuring a cash transfer and heterogeneous responses presumably as a result of perceptions, may be useful for better understanding the rationale for conditionality or even the need for it in the absence of certain local characteristics. In several countries, opposition to conditional cash transfer schemes has been based on one of two arguments—either a rights-based approach that challenges targeting and conditionality, or the notion that imposing conditions is paternalistic and ignores that households would respond to the transfers in a way that is individually and socially desirable. Evidence like that presented in this paper supports the view that conditionality may be a good idea given that, even when preferences are not an issue, financial constraints are not the only hurdles that beneficiaries must overcome. Even more important, understanding what factors are contributing to the differential response to transfers would help policymakers design more comprehensive interventions to enhance household welfare and achieve socially optimal long-run outcomes.

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1. Alvarez, Devoto, and Winters (2008).

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