FLORENCIA LOPEZ BOO
Inter-American Development Bank

JOHN CREAMER
U.S. Census Bureau

Cash, Conditions, and Child Development: Experimental Evidence from a Cash Transfer Program in Honduras

ABSTRACT We explore the effects of a randomly assigned conditional cash transfer in Honduras (Bono 10,000) on early childhood development. We find significant impacts on cognitive development in children aged zero to sixty months, with an average effect of 0.13 standard deviations. We show differential impacts by type of transfer: zero- to five-year-old children from families receiving the health transfer, which targeted families with zero- to five-year-old children only, benefited significantly from the program, whereas zero- to five-year-olds in families receiving the education transfer, which targeted six- to eighteen-year-olds, perceived no benefit. In comparison with other programs, the effect of this impact is sizable (0.34 standard deviations, on average). Although the overall program appears to have slightly changed some behaviors that might affect children (namely, decreased probability of maternal employment and increased maternal self-esteem), we did not find heterogeneous impacts of the Bono across these variables. Results are explained mainly by differences in conditions: while the education component imposed conditions only on children of school age, the health transfer required regular health checkups of zero- to five-year-old children. The health transfer families were more likely to attend health checkups, which may have induced behavioral changes that improved children's health and cognitive development, including purchasing more nutritious food. These results imply that cash without well-targeted conditions might not be as effective for the development of young children.

JEL Codes: C93, J13, I25, I38

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Conditional cash transfer programs (CCTs) have been used in many settings to alleviate poverty and incentivize behavior changes in low-income families. These behavioral changes mainly include increasing school

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attendance and improved use of medical services. Attaching conditions to the cash is intended to lower the opportunity cost of activities that promote human capital development as compared to labor, which ultimately increases the probability that households will grow out of poverty.

CCTs have been shown to have effects that extend beyond those directly implied by the conditions. The literature indicates that in addition to affecting school attendance and the frequency of health checkups, CCTs influence other variables, such as adult labor supply, household consumption patterns, and child nutrition.

Nonetheless, the debate continues about the direction and magnitude of the effects—or lack thereof. In relation to child nutritional status, for example, evidence on the effects of CCTs is mixed.[1] The impact of CCTs on other domains of child development (beyond nutrition) has been less studied. Changes, unintended or not, during sensitive and critical periods of early childhood will likely have a knock-on effect for the child later in life. This study attempts to address this gap in the literature

There are many pathways through which cash transfers may affect child development. The improvements may be explained by an income effect: additional cash available to the household may allow parents to invest in a better home environment or to purchase goods that directly influence child development (such as more nutritious food, better health care, and books). A second mechanism may be that social marketing—which is sometimes part of the cash transfer program or conditions—may induce behavioral change that results in better parenting practices, ultimately leading to improved child development. A third mechanism is that conditions pertaining to health or education depending on the age group may induce differing behavior. In this paper we study these hypotheses by analyzing the impact of a CCT program implemented in Honduras—namely, the Bono 10,000 program—on child development. This program distributed cash to poor and extremely poor households through two mutually exclusive components: a "health" transfer of U.S. $250, which targeted zero- to five-year olds without any older siblings and was conditional on health checkups, and an "education" transfer of U.S. $500, which targeted children between six and eighteen years of age and was conditional on school attendance.

1. Fiszbein and Schady (2009); Lagarde, Haines, and Palmer (2009); Araujo, Bosch, and Schady (2016).

We use data from the randomized evaluation of the program and show a significant improvement in younger children's human capital following the implementation of Bono 10,000, as measured by scores on the Ages and Stages Questionnaire (ASQ) administered to children under the age of five years. The program improves child development by 0.13 standard deviations, mainly through an effect on communication skills (average effect of 0.18 standard deviations). It also appears to have slightly changed certain behaviors that might affect children, such as a decreased probability of maternal employment[2] and increased maternal self-esteem,[3] although we did not find heterogeneous impacts of the Bono program across these variables.We did identify heterogeneous impacts by type of transfer: the impact on children from families receiving the health transfer was 0.34 standard deviations, on average, with an even larger impact on the problem-solving domain (half a standard deviation). However, children from families receiving the education component of the transfer did not benefit at all in terms of ASQ scores. These differential results seem to be explained by differences in conditions. While the education component imposed conditions only on school-aged children in the household, the health transfers required regular health checkups of the zero- to five-year-old children, for which we measured child development. The health transfer families were more likely to attend health checkups. This increased exposure to medical or paramedical advice may have encouraged healthy behaviors, including a shift in spending toward more nutritious food items, which may have contributed to the observed improvement in child health and cognitive development of 0.34 standard deviations.

These results complement those presented by Benedetti, Ibarrarán, and McEwan, who analyze the effects of Bono 10,000 on children between six and seventeen years old, finding that the program resulted in increased school attendance of approximately 4 percent, while child labor participation decreased slightly. In line with our paper, the authors find that children between zero and five years old in the treatment group were more likely to be regularly weighed and to attend checkups.

Our analysis builds on and extends that of Benedetti, Ibarrarán, and McEwan by further examining whether different conditionalities also affect

2. Hill and others (2005).
3. Fernandez and others (2008).

young children's human capital outcomes. Benedetti, Ibarrarán, and McEwan show that different household compositions reacted differently to the cash transfer, perhaps because the cash transfer was less binding in larger households because of the enforcement of the school conditions only for *one* six- to eighteen-year-old in the household. Their analysis focuses only on intermediate outcomes such as school attendance, child labor, and the use of health services. They do not examine whether the cash transfer has effects on human capital outcomes, particularly for younger children. We therefore extend the analysis to the younger children in the household (zero to five years of age) who received benefits *directly* through the cash transfer for health service usage or *indirectly* through the cash transfer subject to the school attendance of their older eligible sibling. Our results also support Benedetti, Ibarrarán, and McEwan's conclusion about the importance of conditions for program effectiveness.[4]

The rest of the paper continues as follows. The next section presents the literature review, which is followed by a presentation of the CCT program, the data, and the method used in the analysis. The final section presents the full results of the study with concluding remarks.

Literature Review

Evidence on the effect of cash transfers on child development is rather scarce and not particularly robust. A recent review of the literature finds eight studies that report on cognitive development outcomes.[5] Of these, only four papers report a statistically significant effect: three for Uganda and Nicaragua, where the authors find a positive and significant overall effect, and one for Ecuador, where the transfer had positive effects only in a subgroup of children (infants and toddlers in rural areas).[6] Because of the heterogeneity in the type of indicators used in each paper and the varying age groups on which they focus, it may be misleading to make comparisons of effect sizes.

Randomized evaluations from Ecuador and Nicaragua report robust estimates of the impact of cash transfer programs on child cognitive and language

4. Benedetti, Ibarrarán, and McEwan (2016).
5. Bastagli and others (2016).
6. See Bastagli and others (2016, table 7.7).

development. In Ecuador, the unconditional Bono de Desarrollo Humano (BDH) program had a significant heterogeneous impact on cognitive and behavioral outcomes among children from thirty-six to fifty-nine months old in the poorest households, with an effect size of 0.18 standard deviations.[7] For younger children treated at twelve to thirty-five months, the intervention resulted in more words being spoken at follow-up.[8] In Nicaragua, the Atención a Crisis program improved the cognitive development, language, and behavior of children zero to five years of age by 0.12 standard deviations.[9] The Red de Protección Social (RPS), another CCT program in Nicaragua, improved male children's achievement on cognitive assessments at age ten, but only if they were treated before turning one year old, as compared to those who were treated between one and two years old.[10] Overall, the results indicating a positive impact of cash transfers on cognitive development outcomes are far from conclusive, which is perhaps to be expected as it is harder theoretically to link cash transfers to cognitive outcomes. Impacts will also heavily depend on the design of the transfers, including the amount of transfer, the target population, conditionalities, and social marketing.

Particularly relevant to our findings, Benedetti, Ibarrarán, and McEwan show that the Bono 10,000 program resulted in increased school attendance, while child labor participation decreased slightly.[11] The authors find mixed results for the use of health services: children between zero and six years of age in the treatment group were more likely to be regularly weighed and to attend checkups, but the treatment did not seem to affect mothers' prenatal or postnatal use of health services.

7. Paxson and Schady (2010).
8. Fernald and Hidrobo (2011).
9. Macours, Schady, and Vakis (2012).
10. Barham, Macours, and Maluccio (2013). There are two studies in which no significant effect sizes were found for any measure of cognitive development, both of which evaluate the impact of the BDH unconditional cash transfer program in Ecuador (Fernald and Hidrobo, 2011; Paxson and Schady, 2010). Fernald and Hidrobo (2011) find that while there were no significant effects of the program on combining words and a language development test for the full sample, there was a statistically significant effect for infants and toddlers in rural areas on language development and ability to combine words. The authors suggest that this may be because of higher uptake in rural areas or a greater potential for impact of the educational elements of the program due to lower initial schooling levels of mothers. Parents of children in rural areas were also more likely to have ensured that their children received vitamin A or iron supplementation and were more likely to have bought their child a toy, all potential mechanisms that could explain the positive effect.
11. Benedetti, Ibarrarán, and McEwan (2016).

Intervention, Data, and Methodology

Bono 10,000 was a CCT program introduced in Honduras in 2010. The aim of the program was to break the intergenerational cycle of poverty by promoting investments in the human capital of children in poor households.[12] Incentives were set to increase the use of education and health services among these children. The program operated through 2014, when Honduras implemented a new CCT program known as the Bono Vida Mejor. The new program is targeted to the extreme poor and maintains the same health and education conditions, but the structure of the individual payments is now tied to the demographic structure of the household and to the requirement that each member comply with the conditions.

The Bono 10,000 program was structured as two types of transfers: the educational transfer (*bono educación*) provided a monetary transfer to eligible households with at least one child between the ages of six and eighteen years old who had not completed ninth grade, only if she or he was enrolled in school. In households with two or more children in that age group, the program required only one of them to fulfill the condition for the family to receive the transfer. The educational transfer amounted to 10,000 lempiras (U.S. $500) per year, regardless of the number of eligible children in the household.[13] A typical household received per capita transfers equal to 18 percent of median per capita consumption.[14]

If a poor household was ineligible for this transfer (because there were no children between six and eighteen years of age in the household), it was entitled to a health transfer (*bono salud*) as long as there was a child aged zero to five years in the household. The health component promoted demand for health services through an annual transfer of 5,000 lempiras (U.S. $250), conditional on the child attending regular health checkups, following Ministry of Health guidelines (at one, two, three, six, twelve, and eighteen months and then once a year from age two years onward). As with the educational component, if there were two or more children in that age group in the household, only one of them had to fulfill the condition of having regular checkups for the household to receive the transfer. The amount of the *bono salud* was the

12. Poor households were defined as (1) residing in a village declared as eligible by program administrators, based on poverty; and (2) being classified as poor based on a proxy means test.

13. That means that the education conditions were not enforced for all eligible children in households with multiple children.

14. Glewwe and Olinto (2004); Galiani and McEwan (2013).

same regardless of the number of children in the household who fell within the age group. Households could be eligible for either the *bono educación* or the *bono salud*, but not both.

The Bono 10,000 program was evaluated through an experimental design. The eligible units for the experimental evaluation consisted of 816 poor villages in Honduras where the government had not yet begun implementing the program. The evaluation sample consisted of 300 randomly selected villages, which were assigned to treatment and control groups (150 villages in each). A sample of households in each of those villages was randomly selected for a survey, but one village in the treatment group and three in the control group refused to participate. Thus the final sample included 4,416 households in 296 villages (see figure 1).

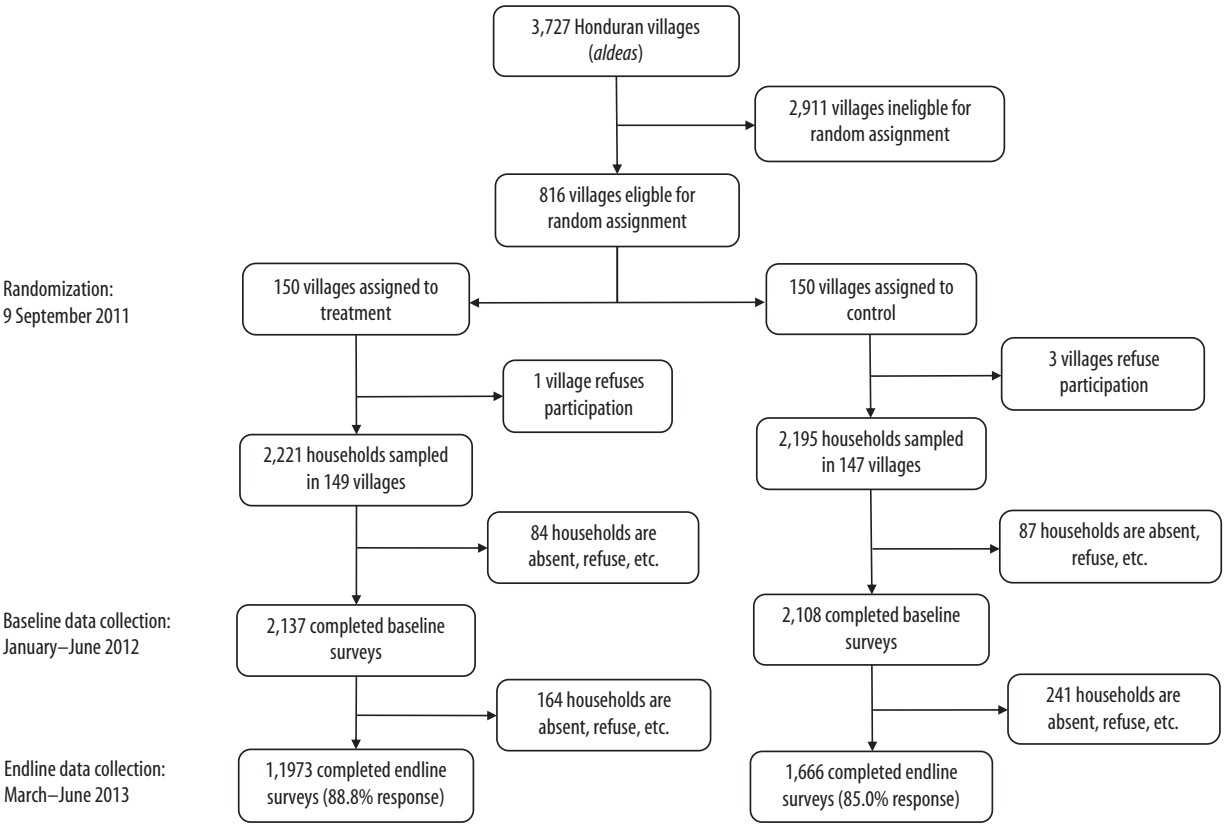
A baseline survey was conducted between January and June 2012, with a response rate of 96 percent (4,245 households). The questionnaire spanned a range of topics, including household assets, as well as individual characteristics of members such as education, labor market participation, and maternal and child health. A follow-up survey using the same questionnaire was conducted between March and June 2013, with a response rate of 89 percent.[15]

Table 1 presents the baseline treatment and control group means for a set of household and individual characteristics. The first columns are calculated for the full sample, while in the last columns we restricted the sample to those households that were also present in the follow-up survey. As expected from the randomization process, treatment and control groups are balanced. Within the non-attritor subsample, we observe statistically significant differences only in the proportion of dwellings with a dirt floor, which is slightly larger in the control group (37 percent versus 34 percent), and in the number of members between twenty-six and sixty-four years old.

Households receiving the CCT, on average, have 5.2 members and an annual per capita income of 975 lempiras at baseline. In terms of access to

15. Figure 1 shows selective attrition (89 percent for treatment villages versus 85 percent for control villages). To assess whether nonrandom attrition introduced observed differences across treatment and control groups, table A1 in the appendix reports the marginal effects of a probit regression of an indicator variable for attrition on a set of baseline observable characteristics. Apart from treatment status and the proportion of dwellings with a dirt floor, estimates are not statistically significant and are very close to zero, suggesting that attrition is uncorrelated with other observable characteristics of the household. Nevertheless, differential attrition raises the possibility of selection on unobservables. Therefore, as a robustness check we estimated bounds based on a trimming procedure (Lee, 2009), that is, trimmed upper and lower bounds of the mean level in treatment and control groups. The bounds show positive, nonzero bounds, meaning that the interpretation of the results is not affected by attrition (results available on request).

FIGURE 1. Bono 10,000 Experimental Design



Source: Benedetti, Ibarrarán, and McEwan (2016).

TABLE 1. Baseline Characteristics of Treatment and Control Groups

	Households in baseline survey			*Households in panel (baseline and follow-up surveys)*		
Variable	*Mean control*	*Mean treatment*	*Diff.*	*Mean control*	*Mean treatment*	*Diff.*
No. households	2,098	2,134		1,767	2,000	
No. individuals	13,055	13,408		11,340	12,689	
% Households receiving education transfer	—	79.80		—	79.70	
Household characteristics						
Household size	5.29	5.22	–0.06	5.32	5.22	–0.10
No. members 0–5 years old	0.84	0.82	–0.01	0.84	0.82	–0.02
No. members 6–18 years old	1.99	1.98	–0.01	2.01	1.98	–0.03
No. members 19–25 years old	0.61	0.62	0.01	0.62	0.63	0.01
No. members 26–64 years old	1.72	1.68	–0.04	1.73	1.67	–0.05*
No. members over 64 years old	0.19	0.19	–0.01	0.19	0.19	0.00
Years of education, household head	3.58	3.66	0.07	3.55	3.68	0.13
Years of education, spouse	4.00	4.05	0.05	4.00	4.03	0.03
Dirt floor in dwelling (yes = 1)	0.35	0.34	–0.01	0.37	0.34	–0.03**
Piped water in dwelling (yes = 1)	0.18	0.17	–0.02	0.18	0.17	–0.02
Dwelling has bathroom or latrine (yes = 1)	0.76	0.78	0.02	0.77	0.78	0.01
Electricity in dwelling (yes = 1)	0.69	0.66	–0.02	0.67	0.66	0.00
Landline or cell phone access (yes = 1)	0.85	0.87	0.02	0.85	0.87	0.01
Individual characteristics (children 5–18 years old)						
Male (1)	0.51	0.53	0.02	0.51	0.53	0.02
Age (in years)	11.40	11.35	–0.05	11.38	11.37	–0.02
Alphabet (yes = 1)	0.78	0.78	0.00	0.78	0.78	0.00
Attends school (yes = 1)	0.74	0.73	–0.01	0.74	0.73	–0.01
Years of education	3.95	3.87	–0.07	3.92	3.89	–0.03

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

services, 17 percent have access to piped water, 66 percent have electricity, and 87 percent have a landline or a cell phone. The household head of treated households has 3.6 years of education, on average. Children aged five to eighteen years living in the treated household have 3.9 years of education, on average, and 73 percent were attending school during the baseline period.

Child Development Measures

The third edition of the Ages and Stages Questionnaire (ASQ-3) was applied to measure the outcomes of interest. ASQ-3 is a screening test that can be used for children between the ages of one and sixty-six months, with different questionnaires designed for different age brackets. The ASQ is parent reported, and the thirty items can be completed by parents alone or administered by a

trained assessor. Five subscales measure skills in the communication, gross motor, fine motor, personal-social, and problem-solving (similar to cognitive) domains. The questionnaires are divided into two- to three-month age intervals.

There are three possible responses for each item on the test: always, sometimes, and never. A score of ten points is noted if the parent reports that the child *always* exhibits a behavior described in the questionnaire (for example, "When playing with sounds, does your baby make low-pitched noises?"); five points if the child *sometimes* exhibits or performs the described behavior; and zero points if the child *never* exhibits the behavior. Consequently, the maximum raw score is 60 for each subscale. Since fine motor skills were not measured in our survey, the maximum score is 240. Table 2 shows baseline scores by age and domain. The samples are balanced, and the only statistically significant differences found were for children aged twenty-four to thirty-five months (in the problem-solving domain) and children aged thirty-six to forty-seven months (in problem-solving and total scores). On average, children in the treated households had a total score of 190 at baseline. Figure A1 in the appendix shows the raw ASQ score distribution by treatment status, while figure A2 shows the same information by dimension. Figure A2 shows that deficits are largest in the problem-solving scale, which is aligned with the international literature. For our regression, ASQ test scores were standardized using international standards.

Methodology

The experimental nature of the data provides a credible identification strategy. Because of randomization at the village level, the treatment assignment is orthogonal to baseline observable and unobservable characteristics that may affect the outcomes. Therefore, we first present the mean difference in the follow-up period between control and treatment groups. In equation 1, Y_{ihj} represents the standardized outcome of child i in household h and village j measured at the follow-up period, and T_j is a dummy indicator of whether the child lives in a treatment village. We also include a vector $\mathbf{X}_{ih}$ of individual and household characteristics at baseline.

$$Y_{ihj} = \alpha_0 + \alpha_1 T_j + \alpha_2 \mathbf{X}_{ih} + \varepsilon_{ij}. \tag{1}$$

To exploit baseline characteristics in some specifications, the sample is restricted to the panel of children aged zero to five years in households that were interviewed for both the baseline and follow-up surveys. Apart from

TABLE 2. Mean ASQ-3 Score and Standard Deviation at Baseline, by Age Group and Treatment Status

	Domain				
Age group and sample	*Communication*	*Gross motor*	*Problem-solving*	*Personal-social*	*ASQ*
0 to 11 months					
Control	50.93	48.06	49.09	49.98	198.09
	(12.61)	(13.28)	(15.49)	(11.62)	(38.97)
Treatment	52.16	47.50	49.88	50.78	200.52
	(10.22)	(14.10)	(15.12)	(10.50)	(35.35)
12 to 23 months					
Control	40.89	51.21	40.84	49.41	181.60
	(14.38)	(12.74)	(15.43)	(11.55)	(40.01)
Treatment	40.64	50.26	41.59	49.6	184.24
	(15.13)	(13.48)	(15.66)	(10.97)	(40.43)
24 to 35 months					
Control	48.53	50.60	43.83	44.84	190.63
	(11.81)	(12.75)	(14.83)	(13.01)	(40.72)
Treatment	48.02	51.47	40.27*	46.22	186.90
	(13.03)	(12.36)	(16.49)	(12.19)	(41.24)
36 to 47 months					
Control	52.50	52.10	42.81	46.48	200.04
	(10.17)	(11.11)	(15.59)	(12.61)	(34.02)
Treatment	52.39	51.26	37.90**	46.52	189.11**
	(9.18)	(10.66)	(18.00)	(11.74)	(38.23)
48 to 60 months					
Control	51.96	52.62	33.66	48.19	188.71
	(11.17)	(11.73)	(15.67)	(11.86)	(37.41)
Treatment	53.56	53.43	35.30	47.36	189.51
	(9.85)	(10.84)	(17.62)	(12.43)	(37.48)
0 to 60 months					
Control	49.06	50.60	41.12	47.79	188.63
	(12.88)	(12.67)	(16.77)	(12.25)	(39.30)
Treatment	49.57	50.57	41.45	48.20	190.00
	(12.52)	(12.63)	(17.12)	(11.72)	(37.78)

* Difference between treatment and control groups is statistically significant at the 10 percent level.
** Difference between treatment and control groups is statistically significant at the 5 percent level.

average impacts, we also explore the presence of heterogeneous effects in different subgroups of the population, through the interaction of the treatment dummy variable and other individual- and household-level variables.

In each estimation, we report both original p values and Romano and Wolf's stepdown adjusted p values robust to multiple hypothesis testing.[16]

16. Romano and Wolf (2005).

TABLE 3. Average Impacts of the Bono 10,000 Program

Variable	*ASQ* (1)	*Communication* (2)	*Gross motor* (3)	*Personal-social* (4)	*Problem-solving* (5)
Treatment	0.128*	0.177*	0.047	0.120	0.165
	(0.056)	(0.069)	(0.078)	(0.063)	(0.081)
Control variables	Yes	Yes	Yes	Yes	Yes
Original *p* value	0.024	0.010	0.550	0.058	0.042
Romano-Wolf *p* value	0.099	0.057	0.567	0.114	0.114
No. observations	1,702	1,702	1,702	1,702	1,702
R^2	0.054	0.041	0.014	0.044	0.148

Note: Control variables include sex, age groups (in months), household size, and dummy variables for the following dwelling characteristics: piped water, bathroom or latrine, electricity, landline or cell phone access, and dirt floor. All controls are measured at baseline. Treatment is whether the zero- to five-year-old child lives in a treatment village. Robust standard errors adjusted for clustering within villages are in parentheses. Bootstrap replications: 1,500.

* Statistically significant at the 10 percent level.

Results

Our results indicate that the program had, on average, a positive effect on child development. According to the estimates presented in table 3, once we control for multiple hypothesis testing, Bono 10,000 significantly increased ASQ scores by 0.13 standard deviations (adjusted *p* value of 0.099) for zero- to five-year-old children living in a treatment village (that is, receiving either the *bono educación* or the *bono salud*) in relation to children living in a control village. This effect was driven mainly by the communication domain: on average, the program increased the standardized scores in this domain by 0.18 standard deviations (*p* value of 0.057). After adjusting *p* values, we do not find significant impacts on any of the other domains (gross motor, personal-social, or problem-solving skills), which is consistent with the literature showing that language is one of the strongest predictors of long-term outcomes, as well as one of the domains most sensitive to early childhood policy investment.[17]

We consider three potential hypotheses to explain our results. First, an income effect: the additional cash available to the household may have allowed parents to invest more in activities and goods that enhance child development. In addition to the material investments that the transfer allowed parents to make, an increase in their endowment may have reduced their levels of stress, which, in turn, may have increased their time, willingness, and

17. Berlinski and Schady (2015).

capacity to interact with their children in an age-appropriate manner. When we tested this hypothesis, we found that the program appears to have slightly changed some behaviors that might affect children (namely, a decreased probability of maternal employment and an increase in maternal self-esteem; see table A2 in the appendix), but we did not find heterogeneous impacts of the transfer across these variables (table A3). Moreover, we did not find impacts on other relevant material investments for children either (that is, the transfer did not affect either health and education expenditures—not reported). We do, however, discuss below how the health transfer changed the consumption of certain types of nutritious foods.

Second, behavioral change may have happened as a result of the social marketing associated with the transfers. If the program encouraged positive changes in parent behavior (such as buying books or play materials), we would expect these improvements in parenting practices to lead to enhanced child development outcomes. We can rule out this effect, however, because there was no social marketing whatsoever for the Bono 10,000 program.

Third, the two components of the program have different conditionalities and different target populations. Differing conditions imposed on families—ceteris paribus—will have differing effects on child development. Households in the education component received their transfer without any requirement with regard to their zero- to five-year-old (that is, the health conditions did not apply, nor were they mentioned to recipients). This could explain the lack of impact of the education cash transfer on the outcomes of the zero- to five-year-old children as compared with those in the health component. We explore this channel below.

Heterogeneous Effects

To further explore possible heterogeneous effects of treatment by child characteristics, table 4 presents the interactions between treatment status and type of transfer, age, and gender. To facilitate reading, we present only the estimates, standard deviation, and Romano-Wolf p value for the interaction terms. We do not find evidence of different impacts by gender, age group, or household size.

How Different Types of Transfers Affect Child Development

As households could only receive one of the transfers—either health or education—and were automatically disqualified for the smaller health transfer if they had an older child, an interesting discussion is which of the

TABLE 4. Heterogeneous Effects of the Bono 10,000 Program

Variable	*ASQ* *(1)*	*Communication* *(2)*	*Gross motor* *(3)*	*Personal-social* *(4)*	*Problem-solving* *(5)*
Gender					
Treatment	0.118	0.185	−0.003	0.096	0.187
	(0.071)	(0.087)	(0.099)	(0.085)	(0.110)
Treatment × Gender (male = 1)	0.022	−0.002	0.090	0.053	−0.046
	(0.078)	(0.094)	(0.120)	(0.102)	(0.135)
Control variables	Yes	Yes	Yes	Yes	Yes
Original *p* value (treatment)	0.136	0.029	0.951	0.308	0.211
Romano-Wolf *p* value (treatment)	0.434	0.139	0.951	0.510	0.504
Original *p* value (interaction)	0.777	0.981	0.456	0.605	0.731
Romano-Wolf p value (interaction)	0.985	0.985	0.960	0.977	0.985
No. observations	1,702	1,702	1,702	1,702	1,702
Age (in months)					
Treatment	0.024	0.041	0.107	0.121	−0.143
	(0.119)	(0.149)	(0.174)	(0.151)	(0.229)
Treatment × Age (in months)	0.003	0.004	−0.002	0.000	0.009
	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)
Control variables	Yes	Yes	Yes	Yes	Yes
Original *p* value (treatment)	0.842	0.782	0.538	0.424	0.531
Romano-Wolf *p* value (treatment)	0.948	0.948	0.948	0.931	0.948
Original *p* value (interaction)	0.308	0.305	0.660	0.984	0.14
Romano-Wolf *p* value (interaction)	0.764	0.764	0.875	0.980	0.526
No. observations	1,702	1,702	1,702	1,702	1,702
Household size					
Treatment	0.113	0.255	0.035	0.093	0.050
	(0.105)	(0.124)	(0.157)	(0.139)	(0.170)
Treatment × Household size	0.003	−0.013	0.002	0.006	0.021
	(0.018)	(0.021)	(0.025)	(0.024)	(0.029)
Control variables	Yes	Yes	Yes	Yes	Yes
Original *p* value (treatment)	0.190	0.049	0.920	0.409	0.412
Romano-Wolf *p* value (treatment)	0.563	0.217	0.923	0.794	0.794
Original *p* value (interaction)	0.861	0.533	0.950	0.809	0.465
Romano-Wolf *p* value (interaction)	0.995	0.953	0.995	0.995	0.953
No. observations	1,702	1,702	1,702	1,702	1,702

Note: Control variables include sex and age (in months) of the ASQ child, household size, birth order, and dummy variables for the following dwelling characteristics: piped water, bathroom or latrine, electricity, landline or cell phone access, and dirt floor. Regressions also include indicators for levels of each variable in the triple interaction. All controls are measured at baseline. Robust standard errors adjusted for clustering within villages are in parentheses. Bootstrap replications: 1,500.

TABLE 5. Effects of the Bono 10,000 Program by Type of Transfer

Type of transfer	*ASQ*	*Communication*	*Gross motor*	*Personal-social*	*Problem-solving*
Education component					
Treatment	0.082	0.141	0.022	0.076	0.082
	(0.060)	(0.072)	(0.080)	(0.071)	(0.087)
Control variables	Yes	Yes	Yes	Yes	Yes
Original *p* value	0.246	0.074	0.889	0.356	0.438
Romano-Wolf p value	0.573	0.258	0.883	0.727	0.762
No. observations	1,388	1,388	1,388	1,388	1,388
Health component					
Treatment	0.336***	0.362***	0.127	0.351***	0.519***
	(0.098)	(0.120)	(0.154)	(0.118)	(0.161)
Control variables	Yes	Yes	Yes	Yes	Yes
Original *p* value	0.000	0.002	0.371	0.004	0.001
Romano-Wolf *p* value	0.001	0.007	0.358	0.005	0.008
No. observations	314	314	314	314	314

Note: Control variables include sex and age (in months) of the ASQ child, household size, birth order, and dummy variables for the following dwelling characteristics: piped water, bathroom or latrine, electricity, landline or cell phone access, and dirt floor. Regressions also include indicators for levels of each variable in the triple interaction. All controls are measured at baseline. Robust standard errors adjusted for clustering within villages are in parentheses. Bootstrap replications: 1,500.

*** Statistically significant at the 1 percent level.

two transfers had higher effects on the zero- to five-year-old children. For instance, Benedetti, Ibarrarán, and McEwan show that health service usage was unchanged for children in households that received the education transfer.[18] In table 5, we find different effects for the different type of transfers. The impact on children in families receiving the health transfer was 0.34 standard deviations, on average. All domains of development (except gross motor skills) were positively and significantly affected, with the largest impact on the problem-solving domain (half a standard deviation), even after controlling for multiple hypothesis testing. In contrast, there are no significant effects associated with the education transfer.[19] These two components differ on several dimensions. First, the subsidy amount was U.S. $250 for households eligible for the health transfer, versus double that amount for the education transfer. Second, they mandated different conditions: the health component

18. Benedetti, Ibarrarán, and McEwan (2016).

19. Running a pooled regression with an interaction between treatment status and the education component (not reported) shows, first, that both components have significantly different effects (with the interaction being very significant); and second, that the education interaction with treatment has a *sizable* significant negative effect. The results in table 3 are thus clearly being driven by the majority of beneficiaries from the education component—and hence, the small average effects.

required health controls for zero- to five-year-old children without older siblings, while the education component required school attendance for one school-aged child in the household. This difference in conditions is pivotal for this paper because it means that the education component would affect young children only indirectly.

Regarding the difference in the transfer amount, a key question is whether households with older siblings are also much larger, in which case the household's per capita income could be smaller despite the larger transfer associated with the education component. The data reject this hypothesis (table A4): although the average household size is indeed larger for the education component than for the health component (5.6 members versus 3.7), in per capita terms the education transfer is 33 percent larger than the health transfer (U.S. $89.30 per capita versus U.S. $67.40).[20] We argue that positive correlations between the number of children and poverty rates effectively stack the deck against finding larger effects among children in smaller families (that is, with no six- to eighteen-year-olds). In Honduras and elsewhere, the literature typically finds larger effects among poor households.[21] However, cash only seems to play a role when it is contingent on conditions. In table A5, we compared the effect of the transfer on various types of items (food, health, and education expenditures). The health transfer families are indeed purchasing more nutritious food (such as milk and butter) and buying fewer harmful items (such as beer). Milk, for instance, is precisely the type of item that nurses or doctors might recommend to parents of young children on the checkup visits (the sole condition of the health component).

Table A4 also shows that families eligible for each version of the program are dissimilar not only in terms of household size but also in terms of household composition and educational attainment. Owing to the design of the intervention, the education component beneficiaries have, on average, 2.5 children aged six to eighteen years, while those in the health component have none. This might organically have a direct impact on parents' time spent with each child. Additionally, zero- to five-year-old children in the health component are mostly firstborns, while zero- to five-year-old children in the education component are not. Unfortunately, there are no data on time use or home environment with which to directly test these hypotheses

20. We addressed this issue by including fixed effects for household size and children's ages in table 4 and then interacting continuous household size and age terms with the (heterogeneous) treatment effects (lines not reported in the table for the sake of clarity).

21. Fiszbein and Schady (2009); Galiani and McEwan (2013).

(that is, that the treatment generated more time spent with children because of fewer children in the household or more time spent with firstborn children, or both). Years of education are also slightly different: the head of the household in the health component families has 1.1 more years of education than household heads in the education component, while the spouse in the health component has 1.4 more years of education than spouses in the education component.

We also looked at whether maternal employment and maternal self-esteem indicators collected in the survey could possibly be channels for the impact in the health transfer. We found no effect of maternal employment in either of the two samples, for both the health and education components. [22]

The different conditions attached to the two interventions seem to provide the most plausible explanation. Conditions imposed specifically on health checks induced parents of younger children to increase their use of health care services. Benedetti, Ibarrarán, and McEwan show that the only statistically significant effects on the use of health services occur in households with no children over the age of five years.[23] In such households, the treatment increased the probability that a young child's last visit to a health center was a checkup by seven percentage points, while the point estimate was smaller and statistically insignificant in households with one older child.[24] This increased access to health professionals may have influenced behaviors such as the spending shift toward more nutritious items, as well as other behaviors that could have resulted in healthier children and improved child cognitive development.

The aforementioned results could be generated by the differential application of conditionalities, but it is also plausible that simply labeling it a health transfer nudged households to seek medical care: for instance, a "labeled" cash transfer in Morocco—promoted as an education support program—produced

22. After controlling for multiple hypothesis testing, we did find a positive effect of the health component on one of the items of the Rosenberg self-esteem scale (namely, "Do you feel you have some good qualities?"). Still, because the Rosenberg test had many missing observations, we do not have a comparable sample (140 observations in this regression versus 314 in the rest), so we cannot attribute this to the transfer.

23. Benedetti, Ibarrarán, and McEwan (2016).

24. Even so, the household transfer increases sharply from 5,000 to 10,000 lempiras with the addition of just one child aged six to eighteen years who enrolls in grades one to nine. This is hopelessly collinear with the application of the health condition, but we note that it dramatically stacks the deck against finding effects of stronger health conditions (presuming that demand for health services increases with income).

large gains in attendance that were mostly unaffected by added conditions.[25] Additionally, the education transfer might have pushed parents' attention toward complying with the condition of the older child and, as a result, concentrating more attention on them rather than on the zero- to five-year-old siblings. The latter is consistent with the hypothesis of the limited bandwidth of parents.[26]

Concluding Remarks

Rigorous evidence of the effects of CCTs on child development is scarce. In this paper, we exploited the original randomized controlled trial (RCT) design of the Bono 10,000 impact evaluation to estimate the impact of living in a program village on the development of young children. This question was left unexamined by previous literature on this specific CCT program.[27]

We found an overall positive effect of the CCT, driven mainly by improvements on the communication subscale of the ASQ-3 test. The magnitude of the overall impact is in line with previous research.[28] The fact that communication is the most easily influenced domain also aligns with past studies.[29]

The program appears to have slightly changed some behaviors that might affect children (namely, a decreased probability of maternal employment and increased maternal self-esteem), but we did not find heterogeneous impacts of the program across these variables.

Importantly, we found substantial differences by type of transfer, with no effect on the group receiving the education component and a large effect on the ASQ-3 of the group receiving the health transfer. Problem-solving (the cognitive domain) was the main driver of the effect in the health group, pointing to an established fact in the literature: the impacts are usually the greatest in areas where children present the largest deficits. This is also the domain with the largest variance at baseline (figure A2).

The eligibility criteria of Bono 10,000 introduced variation across households in the likelihood that children were subject to education or health conditions. Households with any number of children eligible for the education

25. Benhassine and others (2015).
26. Mayer and others (2018).
27. See Benedetti, Ibarrarán, and McEwan (2016) and the papers cited therein.
28. Macours, Schady, and Vakis (2012).
29. Fernald and Hidrobo (2011).

component received the transfer if at least one child was enrolled in school. Therefore, school-aged children in larger families had a smaller chance of being subject to a binding enrollment condition, while zero- to five-year-olds did not have any conditions imposed on them. Households with younger children were subject to the health conditions *in the absence of children eligible for the education transfer*. In a household with children both under five and over five, none of the conditionalities of the transfer required any action to be taken with regard to the welfare of the zero- to five-year-old. The two types of subsidies encouraged different actions depending on the terms of the conditionalities, which may explain why we found differing effects on child development. On the one hand, the education condition may have caused parents to concentrate more on complying with the schooling condition instead of focusing on the youngest sibling in the household. On the other, it is a well-established fact in the literature that important outcomes, such as educational attainment, decline with birth order.[30]

The relatively large impact on the health group could have also been due to tighter enforcement of the mandated health checkups or a result of an increased attention to health care brought about by the health label of the transfer. Both explanations are consistent with the results of Benedetti, Ibarrarán, and McEwan, who show that children in this group were more likely to be regularly weighed and to attend checkups.[31] The education component, with a larger per capita cash transfer but without conditions for the age group studied in this paper, did not improve child development, whereas the treatment offering a smaller transfer per capita while attaching age-specific conditions did. This paper's main contribution to Benedetti, Ibarrarán, and McEwan's analysis is twofold. First, our paper focuses on human capital outcomes, as opposed to the intermediate outcomes previously investigated (namely, school attendance, child labor, and health services usage).[32] Second, we concentrate on the most relevant age period for human capital formation; that is, we look at the impacts on the development of very young children. Beyond evaluating the impact of a CCT on a rarely evaluated outcome (child development), this paper contributes to the literature on the positive relationship between the strength of conditions and final outcomes.[33]

30. Hotz and Pantano (2013).
31. Benedetti, Ibarrarán, and McEwan (2016).
32. Benedetti, Ibarrarán, and McEwan (2016).
33. Baird and others (2014).

Appendix: Supplemental Figures and Tables

FIGURE A1. Raw ASQ-3 Score Distribution by Treatment Status at Baseline

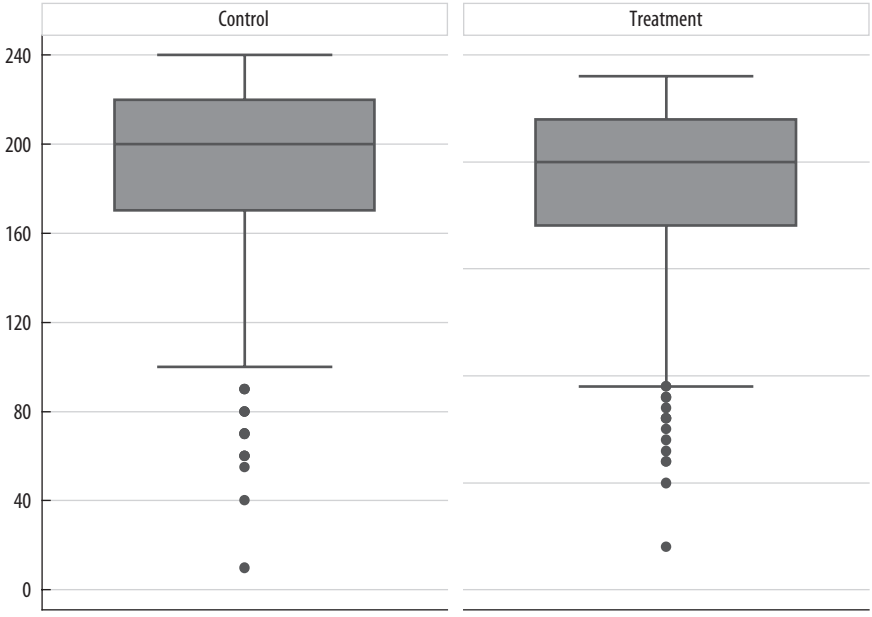


FIGURE A2. Raw ASQ-3 Score Distribution by Dimension and Treatment Status at Baseline.

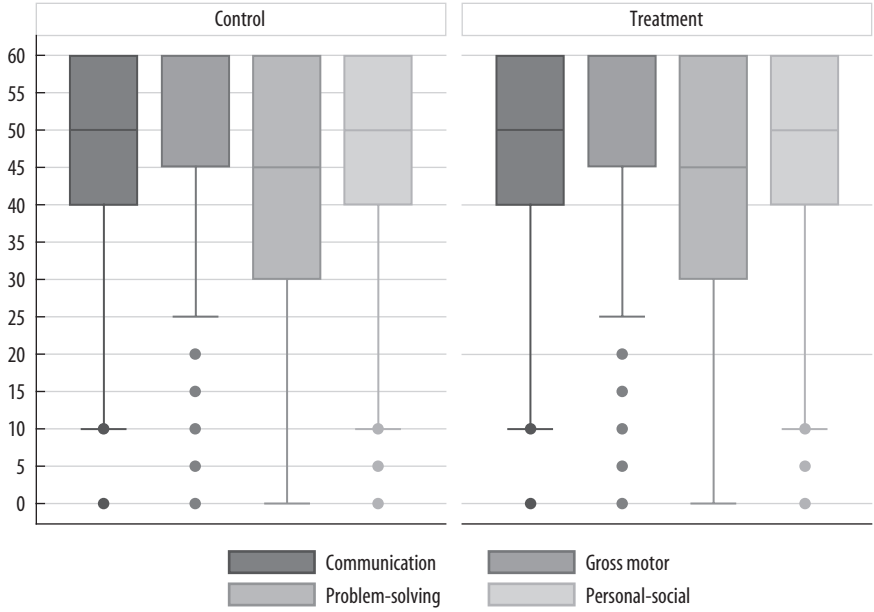


TABLE A1. Model for the Probability of Remaining in the Sample: Marginal Effects at Means

Variable	*dy/dx*
Treatment group (yes = 1)	–0.086***
	(0.011)
Household size	–0.005
	(0.015)
No. members 0–5 years old	0.009
	(0.016)
No. members 6–18 years old	0.004
	(0.015)
No. members 19–25 years old	0.002
	(0.016)
No. members 26–64 years old	–0.001
	(0.015)
No. members over 64 years old	–0.002
	(0.017)
Years of education, household head	–0.002
	(0.002)
Years of education, spouse	0.001
	(0.002)
Dirt floor in dwelling (yes = 1)	–0.022*
	(0.012)
Piped water in dwelling (yes = 1)	–0.022
	(0.015)
Dwelling has bathroom or latrine (yes = 1)	–0.014
	(0.013)
Landline or cell phone access (yes = 1)	–0.008
	(0.015)

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

TABLE A2. Impact of the Bono 10,000 Program on the Mother's Probability of Employment and Self-Esteem

	Employed mothers		*Rosenberg test question*[a]									
Variable	*No controls*	*Controls*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*
Treatment	−0.052*	−0.047*	0.070*	−0.088	0.015	−0.047	−0.133	−0.095	0.081	0.061	0.053	0.130*
	(0.028)	(0.027)	(0.041)	(0.088)	(0.054)	(0.059)	(0.100)	(0.095)	(0.070)	(0.088)	(0.092)	(0.068)
Original *p* value	0.062	0.084	0.085									0.057
Romano-Wolf *p* value	0.059	0.080	0.083									0.062
Controls	No	Yes	No	No	No	No	No	No	No	No	No	No
No. observations	1,657	1,651	791	776	785	787	778	772	783	781	780	780

Note: Robust standard errors are in parentheses. a. The Bono 10,000 evaluation survey included the ten questions from the Rosenberg self-esteem scale (questions 49 through 58 in the survey): 1. ¿En general estas satisfecho(a) contigo mismo(a)? 2. ¿En algunas ocasiones, piensas que no eres bueno(a) para nada? 3. ¿Sientes que tienes algunas buenas cualidades? 4. ¿Eres capaz de hacer las cosas tan bien como la mayoría de la gente? 5. ¿Sientes que no tienes mucho de que sentirte orgulloso(a)? 6. ¿De seguro que algunas veces te sientes inútil? 7. ¿Sientes que eres una persona de valor al igual que otras? 8. ¿Te gustaría tener más respeto contigo mismo(a)? 9. ¿Te sientes inclinado(a) a pensar que eres un(a) fracasado(a) en todo? 10. ¿Tienes una actitud positiva hacia tu persona?

* Statistically significant at the 10 percent level.

TABLE A3. Heterogeneous Effects of the Bono 10,000 Program on Maternal Employment and Self-Esteem

Variable	*ASQ (1)*	*Communication (2)*	*Gross motor (3)*	*Personal-social (4)*	*Problem-solving (5)*
Maternal employment					
Treatment	0.186***	0.220***	0.121	0.203**	0.211**
	(0.068)	(0.084)	(0.096)	(0.083)	(0.106)
Treatment × Mother employed (yes = 1)	−0.114	−0.058	−0.172	−0.142	−0.115
	(0.079)	(0.099)	(0.111)	(0.113)	(0.137)
Original *p* value (interaction)	0.150	0.560	0.123	0.208	0.403
Romano-Wolf *p* value (interaction)	0.465	0.633	0.465	0.486	0.633
Control variables	Yes	Yes	Yes	Yes	Yes
No. observations	1,628	1,628	1,628	1,628	1,628
Maternal self-esteem					
Treatment	0.181	0.090	0.376	0.720	−0.462
	(0.564)	(0.552)	(0.459)	(0.648)	(0.790)
Treatment × Maternal self-esteem (high = 1)	−0.135	0.008	−0.474	−0.628	0.541
	(0.569)	(0.552)	(0.468)	(0.655)	(0.799)
Original *p* value (interaction)	0.812	0.989	0.311	0.338	0.499
Romano-Wolf *p* value (interaction)	0.955	0.986	0.837	0.837	0.861
Control variables	Yes	Yes	Yes	Yes	Yes
No. observations	756	756	756	756	756

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

TABLE A4. Baseline Characteristics of Households Receiving Each Transfer

Household characteristic	*Mean education component*	*Mean health component*	*Difference*
No. households	1,707	430	1,277
No. individuals	11,104	2,304	8,800
Household size	5.60	3.71	1.89***
No. members 0–5 years old	0.73	1.18	−0.44***
No. members 6–18 years old	2.42	0.27	2.15***
No. members 19–25 years old	0.53	0.98	−0.44***
No. members 26–64 years old	1.80	1.19	0.62***
No. members over 64 years old	0.19	0.18	0.01
Years of education, household head	3.44	4.52	−1.08***
Years of education, spouse	3.76	5.12	−1.36***
Dirt floor in dwelling (yes = 1)	0.34	0.34	0.00
Piped water in dwelling (yes = 1)	0.17	0.16	0.00
Dwelling has bathroom or latrine (yes = 1)	0.79	0.74	0.04*
Electricity in dwelling (yes = 1)	0.67	0.65	0.02
Landline or cell phone access (yes = 1)	0.87	0.85	0.03

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

TABLE A5. Impact of the Bono 10,000 Program on Other Outcomes

Expenditure item	*Education component*	*Health component*
Clothes	3038.970	−668.384
	(2815.930)	(9015.433)
School supplies	1547.240	147.579
	(2454.982)	(103.809)
Tuition	−48.265	12337.300
	(36.562)	(8517.051)
Hospitalization	8.428	−234.624
	(44.844)	(172.701)
Other health expenditures	−2953.460	−13269.000
	(2064.481)	(13218.081)
Appliances and furniture	−4,513.212*	6195.000
	(2526.285)	(6128.636)
Vegetables	1944.520	−699.340
	(1993.651)	(1114.949)
Drinks	−1039.890	−53.956
	(767.449)	(904.031)
Water	−0.169	−1.359
	(0.514)	(1.610)
Flour	−2782.170	352.897
	(1811.602)	(2592.506)
Beans	−5.806	−665.406
	(403.453)	(660.533)
Eggs	−436.088*	−659.704
	(249.504)	(665.353)
Chicken	−1,308.661**	−658.162
	(611.231)	(665.194)
Milk	−291.554	3.708***
	(204.721)	(1.352)
Cheese	−285.149	−649.655
	(206.133)	(665.316)
Bananas	140.861	0.860
	(142.119)	(1.149)
Oranges	284.008	614.085
	(200.960)	(609.341)
Rice	−589.876	−47.944
	(563.577)	(907.020)
Flour	−154.444	−0.442
	(378.689)	(3.498)
Bread	0.330	612.181
	(0.458)	(616.970)
Pan dulce	−3.136	1.969*
	(203.144)	(1.169)
Corn flakes	−1,025.200*	−45.775
	(619.478)	(895.956)
Spaghetti	−631.915	516.710
	(1067.088)	(1643.654)

(*continued*)

TABLE A5. Impact of the Bono 10,000 Program on Other Outcomes (*Continued*)

Expenditure item	*Education component*	*Health component*
Milk powder	139.477	44.748
	(428.841)	(31.348)
Butter	272.676	6.135*
	(632.363)	(3.599)
Beef	–297.225	–43.466
	(354.734)	(899.504)
Pork	–729.447	–1324.170
	(514.712)	(934.895)
Tomato	276.396	5.589
	(489.249)	(3.588)
Onions	992.680**	–47.470
	(502.063)	(907.349)
Potato	124.566	3.012
	(675.968)	(2.118)
Cabbage	272.443	2.706**
	(631.791)	(1.300)
Yucca	283.313	–663.177
	(199.836)	(656.492)
Canned juice	–4.328	–659.996
	(203.049)	(660.946)
Soft drinks	–436.323*	612.447
	(249.936)	(616.865)
Tomato sauce	–444.484	–45.645
	(379.194)	(898.203)
Salt	700.203	1841.220
	(543.126)	(1360.549)
Beer	–7.125	–2.185*
	(349.658)	(1.257)
Cigarettes	–4.040	–662.826
	(202.624)	(660.973)
No. observations	1,394	314

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

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