The long-term effects of cash transfers on education and labor market outcomes

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Abstract: This paper investigates whether the effects of a cash transfer program persist or wear off in the long-run. I study the first two phases of Bono de Desarrollo Humano (BDH) in Ecuador, each of which lasted about five years. I use a regression-discontinuity design and a change in the eligibility rule at the beginning of the second phase of the program to disentangle the effects of a short- versus long-exposure to the program. Most of the gains in enrollment and schooling were achieved in the short-run among children that started treatment when they were about to start elementary school, eleventh grade or Baccalaureate. However, an extended exposure to BDH was not enough to keep raising children's education. Regarding labor market outcomes, BDH had a negative (not statistically significant) impact on the probability of working among young children but did not increase job opportunities among young adults in the long-run.

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1 Introduction

It has been widely accepted in the literature that factors operating during early childhood play a more important role than tuition, school reforms, job training or family credit constraints in explaining gaps in socioeconomic attainment (Carneiro et al., 2002; Cunha et al., 2010; Heckman, 2000). Cash transfers targeted to the poor have become the most popular tool in developing countries to encourage investment in the health and education of young children. These programs range from pure unconditional cash transfers to fully monitored and enforced conditional cash transfers (CCTs) (Baird et al., 2014).

This paper studies the effects on young people's education and labor market outcomes of a continuous exposure to Bono de Desarrollo Humano (BDH), the main unconditional cash transfer program in Ecuador. While the previous literature on the long term effects of cash transfers, study how well the original treatment and control groups perform on several dimensions of interest after several years of a program's implementation. The question I address in this paper is whether cash transfers continue to be effective after several years targeting the same population.

Several studies have shown that CCTs are effective in increasing enrollment rates among beneficiary children in the short run (see Fiszbein et al. (2009) for a review and Saavedra and Garcia (2012) for a meta-analysis). However, large gains on school participation have also been found as a result of unconditional cash transfer programs like the ones documented by Benhassine et al. (2015) in Morocco for an unconditional cash transfer labeled as an education support program. The impact of cash transfers is overall much stronger on schooling than it is on child work (de Janvry et al., 2006). In fact, there is mixed evidence regarding the latter, with some studies finding significant reductions on child work among children most vulnerable to transitioning from schooling to work (Edmonds and Schady, 2012; Skoufias and Parker, 2001; Baird et al., 2014) and others finding no effects (de Janvry et al., 2006).

BDH targets women with young children aged 0 to 18 years old using a proxy means test score called "Selben", which is built using principal components analysis. In Ecuador, BDH has been subject to many short-term evaluations, showing important improvements in enrollment, cognitive and socio-emotional development, and reductions in child labor among treated children (Edmonds and Schady, 2012; Schady et al., 2008). However, improvements in enrollment may not be enough to take these children out of poverty if they do not complete more years of schooling. Furthermore, it is not clear that the short-term gains will persist through time.

While there is sufficient evidence of the impact of cash transfers in the short run, evidence about their impacts on the long run is sparse. In a recent study, (Aizer et al., 2016) analyzed the long-term impact of the Mothers' Pension program in the US on longevity, educational attainment, nutritional status, and income in adulthood. In the study, the boys of mothers whose application to the program was accepted lived one year longer than boys of mothers whose application was rejected. They also accumulated one third more years of schooling and had a higher income in adulthood. Also for the US, Hoynes et al. (2016) used the roll-out of the most important near cash safety net program in the US, the Food Stamp Program (now SNAP), to evaluate its long-term impact. They found that access to food stamps in utero and in early childhood leads to significant increases in educational attainment and earnings.

In developing countries, the few existing papers that examine long-term effects focus mainly on Latin America. Since most of these programs are relatively young (the oldest CCT program, PROGRESA /Oportunidades, started in 1997), the analysis has focused on the study of outcomes measured at the end of high school or during early adulthood. Most of the experimental evidence comes from PROGRESA /Oportunidades (Behrman et al., 2011, 2005; Kugler and Rojas, 2018), and Nicaragua's CCT program Red de Protección Social (Barham et al., 2013b,a). For both countries, there is consistent evidence of impacts on schooling as well as some evidence for Nicaragua of impacts on learning, off-farm employment and income. Other studies have also used short-term estimates to extrapolate to long-run program impacts (Behrman et al., 2005; Todd and Wolpin, 2006; Attanasio et al., 2012). However, there are some concerns about the results that stem from extrapolations; one is that short-term evaluations may reveal only temporary improvements in the outcomes of interest, which may vanish as time goes by. Another concern is that they may fail to detect any impact because the time span between the treatment and follow-up may be too short (King and Behrman, 2009).

On the other hand, non-experimental evidence about the long-term impacts of cash transfers is relatively scarce. One example is the study by Baez and Camacho (2011) about Colombia's CCT program Familias en Acción [2], that studies the ef-

[2] Some differences between BDH and Familias en Acción are that Familias en Acción is a CCT while BDH is not. Furthermore, the size of the transfer in Familias en Acción is smaller (17 dollars) and varies with the age and number of children in the household.

fects of Familias en Acción on school completion and learning outcomes at the end of high school using matching techniques and regression discontinuity design for identification. The authors found that treated children were on average between 4 and 8 percentage points more likely to graduate from high school but no significant effects were found on cognitive tests. In addition to this study, a contemporaneous paper by Araujo et al. (2016) studies the impact of Ecuador's Bono de Desarrollo Humano (BDH) after 10 years of its implementation. The authors used data from a randomized evaluation in 2003 and survey data collected in 2014 to test the impacts of BDH on various cognitive tests and found no effects on those tests. They also used administrative data and a RD design to evaluate the effects of BDH on school attainment and employment status of young adults, aged 19-25 years old in 2013/14. They found a modest impact on high school completion, between 1 and 2 percentage points for young adults living in treated households and no effect on employment amongst young adults.

This paper differs from that of Araujo et al. (2016) because I am not interested in knowing how well the original treatment and control groups performed after 10 years of BDH's implementation. The question I address in this paper is whether children who are treated for a longer period of time continue achieving better outcomes compared to children who were treated for a shorter period of time. In other words I study whether treatment continues being effective in the long-run. My identification strategy relies on the fact that at the threshold of eligibility, the second assignment to treatment (in 2008/9) was independent of the first assignment (in 2003). This allows me to disentangle the short term effects of BDH measured at the end of phase one from the additional effect from being treated also during phase two. Consequently, I estimate the differential effect of a short exposure to BDH (treatment during phase one) versus a long exposure (treatment during phases one and two).

I build a unique dataset that follows individuals (not households) through the three waves of the Registro Social (RS), which allows me to control for transitions in and out of the program and account for family dynamics that may also introduce bias to the estimates. More importantly, I assigned the eligibility status to households according to the Selben score that was in effect in each phase of the program and assigned the "treated" status to children living with women who actually claimed the transfer each year. This generates the variation in lengths of exposure to the program that I exploit in this paper.

The results show that treated children experienced important short-term gains in enrollment and high school graduation by the end of phase one. Most of the gains in enrollment and schooling were achieved among children that started treatment when they were about to start elementary school, eleventh grade or Baccalaureate. However, an extended exposure to BDH was not enough to keep raising children's education. This explains why 18 year olds that were treated during the two phases of the program were not more likely to finish high school when compared to similar children that were only treated during the first phase of the program. Regarding labor market outcomes, BDH had a negative yet not statistically significant impact on the probability of working among young children and did not increase job opportunities among young adults in the long-run.

This paper contributes to fill the gap in the literature about the long-term effects of cash transfers with emphasis on long duration programs and investigates whether these programs loose their effectiveness after several years targeting the same population. It highlights the use of individual panel data to produce reliable quasi-experimental evidence of the differential effects of long exposure to social programs that use a proxy means test (that may change over time) to target beneficiaries, an approach that is very common in social programs in developing countries.

In the next section, I explain the institutional background and operational aspects of BDH since its implementation. Section 3 discusses the three sources of administrative data used in this paper and the methodology used to build the panels. Section 4 presents the empirical strategy used to identify the different impacts of BDH. Section 5 presents evidence on the validity of the identification strategy. Section 6 presents the results from the RD design. Section 7 discusses some robustness checks using administrative data from a standardized exam taken at the end of high school and Section 8 concludes.

2 Institutional Background

Ecuador is a middle-income country that has experienced significant progress in terms of poverty reduction in the last decade. In 2005, Ecuador's GNI per capita was $7,310 in PPP-adjusted current U.S. dollars.[3] At that time 84.3% of the rural population lived in poverty as well as 35.1% of the urban population.[4]. By 2013,

[3]Source: World Bank World Development Indicators.

[4]Source: Sistema Integrado de Indicadores Sociales del Ecuador (SIISE) (http://www.siise.gob.ec)

Ecuador's GNI per capita was US$10,720 in PPP-adjusted current U.S. dollars.[5] However, poverty remained a major concern for policy makers with 57.8% of the rural population and 24.8% of the urban population living in poverty.[6]

The reduction in poverty has been accompanied by a marked improvement in the main educational indicators as well as in a more modest reduction of child labor. The Ecuadorian educational system has three phases: (i) initial education, for children ages 3 to 5 is not compulsory; (ii) basic general education, is compulsory and lasts 9 years for children ages 5 to 15 years, and (iii) the Baccalaureate lasts 3 years for children ages 15 to 18 years and is not compulsory. Since 2012, at the end of the Baccalaureate, students have to pass a standardized exam (ENES exam) to apply to university. Until October 2008, public education was free only up to the tenth year of Basic General Education for children aged 5 to 15 years[7]. In October 2008, the new constitution declared that public education should be free from the first year of basic education up to the undergraduate level from the 2008-2009 school year onwards.

Primary education is almost universal and most of the recent efforts of the government have focused on raising the secondary education enrollment rates. Gender differences in educational attainment are small when compared for all males and all females (García-Aracil and Winter, 2006). By 2003, the net enrollment rate for basic general education was 84.1% and increased up to 90.4% in 2008 and up to 95.7% in 2014. While the net enrollment rate for baccalaureate was 42.1% in 2003; 53.6% in 2008 and 65.1% in 2014.[8]

Despite the progress made in terms of schooling since 2003, youth unemployment, among individuals between 15 and 24 years of age, remained the highest among all the age groups even though it decreased from 22.6% in 1998 (Tokman, 2000) to 9.7% in 2014 (García et al., 2016). On the other hand, the rate of child work for children ages 5 to 14 years has decreased significantly since 2007, when 8% of children in said age range reported having a job, compared to 2014, when only 3% of children ages 5 to 14 worked[9].

[5] Source: World Bank World Development Indicators

[6] Source: SIISE (http://www.siise.gob.ec)

[7] Education was free at public schools, however most schools charged so called "voluntary contributions" at the beginning of the school year. Those contributions were eliminated after the reform in 2008.

[8] Source:http://www.siise.gob.ec/agenda/index.html?serial=13

[9] Source:Compendio Estadístico INEC, 2014. Using data from the labor force survey ENEMDU. http://www.ecuadorencifras.gob.ec/compendio-estadistico-2014/

BDH was implemented in 2003, when the incumbent government merged two existing cash transfer programs: Bono Solidario (BS) and Beca Escolar (BE) and became the first program to use a proxy means test to target the poorest families in Ecuador.[10] BDH uses a proxy means test called "Selben index", which correctly predicts that 95% of households in the poorest quintile are eligible for the benefits and erroneously excludes 5 percent of them (Fiszbein et al., 2009). The Selben index is computed every five years using the information contained in a Social Registry called "Registro Social" (RS), which is a census of poor households that contains individual level data of all potential BDH beneficiaries. The index is computed using a principal components analysis that assigns numerical values to categorical variables. After obtaining the weights for each variable, the values are added using a linear transformation, and a score ranging from 0 to 100 is computed. To make sure that BDH targets only the population in the bottom 40% of the Selben distribution, the same process is reproduced using a nationally representative survey. Once the cutoff is chosen; it is applied to select beneficiaries in the RS.[11]

The amount of the monthly transfer was US$15 in 2003 for individuals with families in the lowest 20% of the Selben distribution, and US$11.50 for those located in the next 20% of the distribution (equivalent to 12% and 9% of the minimum wage in 2003 respectively). Since then, the amount has increased progressively. In 2007, the transfer increased to US$30 (18% of the minimum wage) for individuals in the bottom 40% of the Selben distribution. In 2009, the transfer was raised to US$35 (16% of the minimum wage) and in 2013 it was set at US$50 per month (16% of the minimum wage).

BDH was initially publicized as a CCT; eligible women with children aged 0 to 18 years were required to take their children for health checkups and to enroll them in school.[12] Compliance with the conditions was supposed to be monitored every two months; however, due to lack of administrative capacity, this did not happen. Nevertheless, the program used radio and television spots to explicitly link transfers

[10] BS was an unconditional transfer to compensate poor families for the elimination of gas and electricity subsidies in 1998, and targeted mothers with earnings below 40 USD per month, people with disabilities, and senior citizens. By 2003, it had about 1 million beneficiary households (World Bank, 2005). The program had an open inscription process that relied on the identification of needy families by parish priests, who were considered to have reliable knowledge of whom among their parishioners was poor. BE was a CCT program that started in the late 1990s and consisted of monthly transfers of 5 USD per child (up to two children per household), conditional on children's enrollment in school and a 90% attendance rate (Carrillo and Ponce Jarrín, 2009).

[11] This is why more than 40% of the population in the RS fall below the chosen cutoffs.

[12] The amount of the transfer does not depend on the age or number of other eligible children in the household, which may lead parents to choose the child in whom they want to invest the most.

with the conditions, and some BDH administrators stressed the importance of school enrollment at the implementation stage (Schady et al., 2008). Furthermore, in 2008, the Ministry of Social Inclusion (MIES) started a process of notifying some mothers who were not satisfying the conditions; however, no further action was taken to sanction non-compliance.[13]

By 2014, BDH had 10 years of operation and people who were eligible during the two phases of the program could have received the transfer for a maximum of 10 years. From 2003 until 2008, hereafter the first phase of the program, BDH targeted women with children aged 0-18 years scoring less than 50.65 points (Ponce and Bedi, 2010). In 2009 a new Selben score was computed (Selben II), using most of the variables used in the estimation of the previous score and the information from the second wave of the RS (Ministerio Coordinador de Desarrollo Social, 2009). From August 2009 until March 2013, the new eligibility cutoff was set at 36.5 points(Buser, 2015).[14]

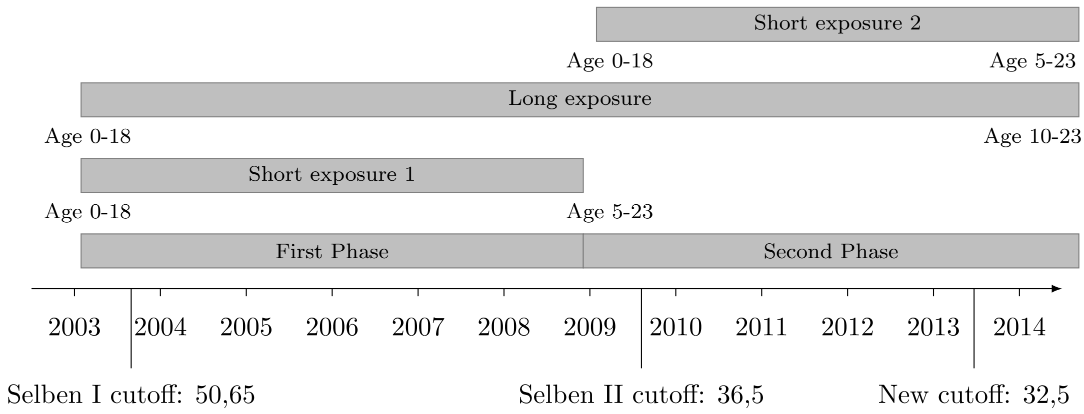
Figure 1 shows the chronology of BDH implementation and, specifically, the change in the targeting rule in 2009 that coincided with the collection of the second wave of the RS and marked the beginning of the second phase of the program. Figure 1 also shows the different lengths of treatment that exist and the children of interest in this study. Regarding the former, a short exposure means that individuals were eligible during one of the phases of the program either phase one or two; while long exposure means that individuals were eligible during the two phases of the program. Finally, individuals who were not eligible for the transfer may remain in this state during phase one, phase two or both.

The group of interest in this study are children aged 0-18 years when the program was launched in 2003, who grew up being exposed to BDH during phase one, phase two or both. At the end of phase one, these children were approximately 5 years older and by the end of phase two, they were approximately 10 years older. Their exact age depends on the date they were surveyed in the first, second and third wave of RS. It is worth noting that for the analysis of the differential effects of BDH, the minimum age at the end of phase two is 10 years and the maximum age is 23 years because older children would not be eligible for treatment at the beginning of phase two.

[13] See Executive Decree No. 347-A of April 25 of 2003 published in the Official Registry No. 76 on May 7 of 2013.

[14] Another change in the eligibility rule happened on March 2013 (according to the ministerial agreement No. 197 of 28 March 2013), when the beneficiaries whose score was between 32.5 and 36.5 points were excluded from the program.

Figure 1: Timeline of the implementation of BDH



3 Data and descriptive statistics

3.1 Description of the Data

Three sources of administrative data were used in this study, the Registro Social (RS), BDH payment data, and administrative data on the ENES exam (Examen Nacional para la Educación Superior). The RS is considered a census of the poor because the first wave was conducted in 215 cantons of the 223 registered in the census of 2010, and therefore covers most of the poor areas of the country. Furthermore, by 2008 the total number of households in Ecuador was estimated to be 3,392,851 and the second wave of the RS covers 2,393,377 of those households (Ponce and Falconí, 2011). It contains relevant information about BDH recipients and potential recipients, namely, individual socio-economic information at the family and individual level, the ID number of the members (when available) and the Selben score assigned to each household.

To date, there have been three waves of the RS. The first wave covered 6,303,352 individuals and was collected between 2001 and 2007; however, most of the information was collected before 2003.[15] The second wave covered 8,068,957 individuals and was collected between 2007 and 2013 with most of the surveys completed in 2008. The third wave was mostly collected in 2014, but data collection started in 2013 covering a total of 6,930,701 individuals.

[15]Ecuador's population was 12,628,596 inhabitants by the year 2000, 14,447,600 inhabitants by 2008 and 15,661,312 inhabitants by 2013 according to the World Bank data (http://databank.worldbank.org/data/reports)

The steps taken to build a panel that follows individuals across the three waves of RS using probabilistic record linkage is described in detail in the Appendix. The three-waves panel allows me to identify the individuals that were treated only during phase one, two or both and to track the trajectory of the individuals in terms of their educational attainment. By following the children instead of the mothers in the RS panel, I make sure that I will not estimate the long-term impacts of BDH on children that recently joined the household of a woman who was treated since 2003, but on children who lived with a treated woman since 2003 and may be currently living or not with her.

Table 1 shows the number of individuals who changed their eligibility status after the introduction of Selben II in 2009. Given that the cutoff point for eligibility moved from 50.65 to 36.5, I expected to see a substantial number of beneficiaries leaving the program. In fact, 20% of the people on the panel who were initially eligible to receive the transfer left the program by 2009 because they no longer met the selection criteria, and 28% of ineligible individuals became eligible. Looking specifically at the individuals around the Selben threshold established in 2003 (namely +/- 5 points from the 50.65 points cutoff), almost 35% of the individuals who were originally ineligible changed their status to eligible and 45% of those who were eligible became ineligible, which points to significant contamination of the original assignment groups.

The second source of administrative data is data on BDH payments. This information is collected on a monthly basis and gives an account of the amount and periodicity with which beneficiaries collect their transfers from the different financial institutions. Compliance rates are high, over 90 percent every year. This means that 90% of the people assigned to treatment cashed the transfer at least once per year. I merged the RS panel and payments data and assigned the actual "treated" status to children who were living with women who claimed the transfer each year.

It is worth mentioning that since the last wave of the RS contains around 1.1 million fewer observations due to attrition and because, during the data collection period, the last 2,458 sectors to be visited were excluded (leaving 735,479 individuals out of the survey). As a robustness check, I use administrative data from the ENES exam for the years 2013 and 2014,[16] to estimate the impact of different lengths of exposure to BDH on the likelihood of taking the exam, which is a good proxy for high school graduation, and compare these results with the ones obtained using the

[16]The ENES standardized exam did not exist before 2012. Data is only available from 2012.

Table 1: Eligibility status for individuals after the the introduction of Selben II in 2009

		2009 Ineligible	Eligible	Total
2003	Ineligible	148,417	58,274	206,691
		72%	28%	100%
	Ineligible +/- 5p	88,642	47,529	136,171
		65%	35%	100%
	Eligible	433,123	1,735,492	2,168,615
		20%	80%	100%
	Eligible +/- 5p	173,275	213,518	386,793
		45%	55%	100%
	Total	581,540	1,793,766	2,375,306
		24%	76%	100%
	Total +/- 5p	261,917	261,047	522,964
		50%	50%	100%

Notes: The table shows the transition matrix of the eligibility status to BDH before and after the introduction of Selben II. The total is 2,375,306 individuals instead of 2,961,043 because in the first wave not all the households had kids below age 18.

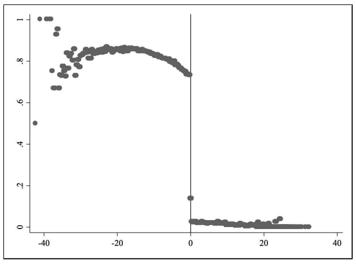
three-waves panel of the RS.

4 Empirical Strategy

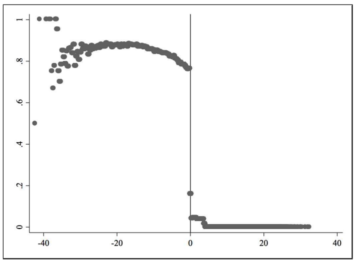
4.1 Regression discontinuity design

The discontinuity in the assignment rule allows me to recover the local causal effects of exposure to BDH by comparing the outcomes of similar individuals who are just below the threshold (hence eligible for the transfer) and just above the threshold (not eligible for the transfer). Given that the Selben score predicts substantial but not perfect changes in the probability of treatment, meaning that the probability of treatment does not jump from 0 to 1 when the forcing variable crosses the threshold, I use a Fuzzy RD design. In the Fuzzy RD design, the treatment effect is obtained by dividing the jump in the outcome variable (Y) at the threshold to the jump in the treatment probability at the threshold as in an instrumental variable approach or the analogous Wald estimator. The different graphs in Figure 2 show the jump in the treatment probability at the Selben I threshold (50.65 points) and at the Selben II threshold (36.5 points) for each year starting in 2005, the first year for which payment data is available.

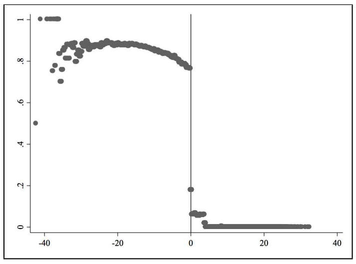
Figure 2: Proportion of treated households with respect to the threshold by year



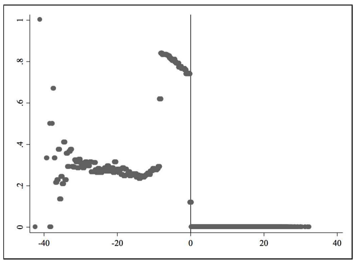
(a) 2005



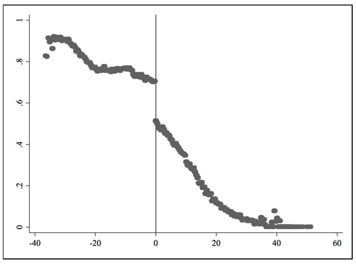
(b) 2006



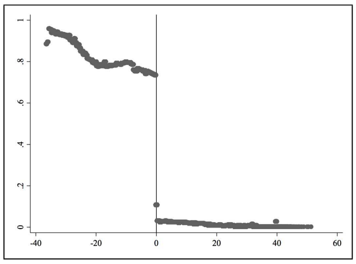
(c) 2007



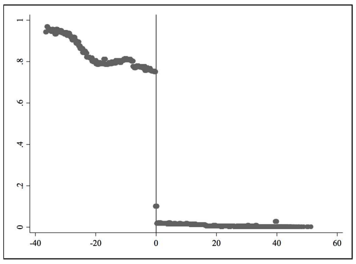
(d) 2008



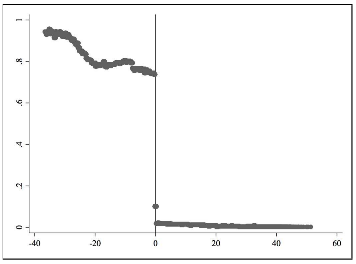
(e) 2009



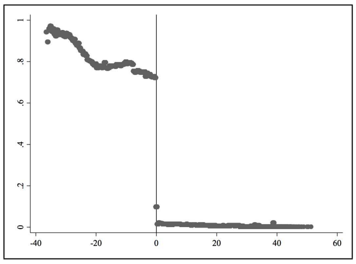
(f) 2010



(g) 2011



(h) 2012



(i) 2013

Notes: The graphs use RS data merged with administrative payments data. The sample is individuals that were surveyed before 2003 in the first wave and before 2009 in the second wave. The graphs show the proportion of households that received the treatment each year. The cutoff for the years 2005-2008 is 50.65 points and the cutoff for the years 2009-2013 is 36.5 points. The change in the assignment rule happened in August 2009 but the 2008 graph also exhibit some adjustment. In particular the 2008 payments dataset contains 676,068 individuals while the 2007 and 2009 datasets contain 1,127,909 and 1,280,367 individuals respectively.

In the next section, I explain how I estimate the impact of BDH at the end of each phase of the program, as well as the differential impact of a long exposure (during phases one and two) versus a shorter exposure to BDH (during phase one).

4.2 Estimation

Several non-parametric methods have been proposed in the literature to estimate the local average treatment effect (LATE). One corresponds to the series estimation approach, which consists of the inclusion of polynomial functions of the forcing variable and provides estimates of the regression function over all the values of the forcing variable. The other non-parametric approach is kernel regressions. In the simplest case of the rectangular kernel, one computes the local average of the outcome (Y) in the closest bin to the left and right of the cutoff point and compare those means to get the RD estimate. However, Hahn et al. (2001) argue that if the true model is upward sloping on both sides of the threshold, the RD estimate from kernel regression would be biased; moreover, any attempt to reduce the bias by reducing the bandwidth size would lead to very imprecise estimates in the absence of a large number of observations near the cutoff. To solve this problem Hahn et al. (2001) suggest running local linear regressions at each side of the threshold instead of computing local averages within the closest bins. Hahn et al. (2001) also proved that this approach reduces bias by one order of magnitude.

In the case of BDH, eligible individuals are located on the left-hand side of the cutoff, meaning that only people with a lower Selben score can benefit from the transfer. Following Lee and Lemieux (2010), the regression model on the left-hand side of the cutoff point ($S \leq c$) is:

$$Y = \alpha_l + f_l(S - c) + \epsilon \tag{1}$$

Y is the outcome variable, $f_l(.)$ and $f_r(.)$ are functional forms of the Selben score (S) that measures the distance to the cutoff c, α_l is the intercept. The regression model at the right hand side of the cutoff point ($S > c$) is:

$$Y = \alpha_r + f_r(S - c) + \epsilon \tag{2}$$

It is preferable to estimate the treatment effect with a pooled regression on both sides of the threshold. The advantage of this approach is that it directly yields estimates and standard errors of the treatment effect τ:

$$Y = \alpha_r + \tau T + f(S - c) + \epsilon \tag{3}$$

where $\tau = \alpha_l - \alpha_r$ and $f(S - c) = f_r(S - c) + T[f_l(S - c) - f_r(S - c)]$. The treatment status T is instrumented by D, which is a binary variable that takes a value of 1 when the Selben score is below the cutoff and 0 otherwise. It is important to let the regression function differ on both sides of the cutoff point by including interaction terms between T and S. In the linear case where $f_l(S - c) = \beta_l(S - c)$ and $f_r(S - c) = \beta_r(S - c)$, the pooled regression is:

$$Y = \alpha_r + \tau T + \beta_r(S - c) + (\beta_l - \beta_r)T(S - c) + \epsilon \tag{4}$$

The simplest fuzzy RD estimator uses only D as instrument without polynomial interactions of $f(S - c)$ with D. In this case, I allow for interaction terms in the first and second stage.

4.3 Short exposure

I first estimated the effects of a short exposure to BDH at the end of phase one. For that, I compare the outcomes (observed at the end of phase one) of children who were marginally eligible or not based on their proximity to the Selben score cutoff of 50.65 points set on 2003. Likewise, to estimate the effects of a short exposure to BDH, during phase two only, I restrict the sample to children who were not treated during phase one and compare the outcomes of children observed at the end of phase two with a Selben II score close to the cutoff for eligibility fixed at 36.5 points in 2009.

The analysis of the effects of each phase of the program is particularly important in this setting because BDH was publicized as a CCT. However, due to lack of administrative capacity the conditions were never enforced so it is likely that most of the impact was achieved in the first phase of the program when the transfer was believed to be conditional at least for a short period of time (De Brauw and Hoddinott, 2011; Baird et al., 2014; Benhassine et al., 2015). Furthermore, in October 2008, education became free in all public schools and universities in Ecuador, which may have caused the transfer to cease to have an effect on eligible children, since education became free for all eligible and ineligible children. It is important to contrast the results obtained in each of the phases separately to be able to identify the possible reasons why the effects of BDH may be different during phase one and two.

To estimate the effects of a short exposure to BDH, I estimate equation 4, where Y is the outcome variable observed at the end of phase one or at the end of phase two depending on the case. I instrument treatment with individual eligibility using the corresponding cutoff depending on whether I am evaluating the effects at the end of phase one or at the end of phase two. I try several bandwidths (+/-2.5, +/-5 and +/-7.5 points with respect to the cutoff) and test the robustness of the estimates to the inclusion of higher order polynomial terms. I estimate regressions for different age groups and include county fixed effects, a gender dummy and school year dummies because each wave of the RS was collected over more than one year.

It is worth noting that given that each wave of the RS was collected over several years, in the analyses of the short term effects at the end of phase one, I restrict the sample to individuals that appear in the first and second wave of the RS but were surveyed before 2003 in the first wave. Similarly, for the analysis of the short term effects at the end of phase two, I restrict the sample to individuals that I follow across the second and third wave of the RS that were surveyed before 2009 in the second wave. This is important because this is the sample on which I perform the balance tests of pretreatment characteristics and also because in this way I avoid having cases where people respond to the first survey very late and shortly after respond to the following survey or cases where the information on the contrary is widely spaced.

4.4 Differential effect of a long exposure vs a short exposure

A significant number of individuals changed their eligibility status after the introduction of Selben II in 2009. As a consequence, comparing the outcomes of children from treated households versus untreated households ten years later using the original assignment in 2003 would lead to underestimation of the treatment effect of the program, since both groups were exposed to the treatment at some point in time.

There are two other ways to estimate the long term effects of BDH. The first measures the impact ten years later of being treated only during phase one versus never being treated. However, this case is problematic because to avoid contamination of the original groups, transfers would have to be withheld from everyone during phase two (akin to a phase in design) which did not happen with BDH. The other way to estimate long-term effects is to measure the differential impact of a long exposure (during phases one and two) versus a short exposure to BDH (during phase one). To do this, I compare the outcomes observed at the end of phase two of children who were marginally treated (or not) during phase two among children who were treated during phase one. Since most of these individuals were eligible during phase one, and at the same time were very close in terms of their outcomes in 2008, I would be comparing two groups of people that are very similar in terms of observable and unobservable characteristics as shown on the balance tests (see Table 5 on the Appendix).

To estimate the differential impact of a long exposure (during phases one and two) versus a short exposure to BDH (during phase one), I restrict the sample to individuals who were treated during the first phase of the program and estimate equation 4, where Y is the outcome variable observed at the end of phase two and $(S-c)$ is a function of the Selben II score (S) that measures the distance to the cutoff (36.5 points).

As with the short term effects, I instrument treatment with individual eligibility and try several bandwidths (+/-2.5, +/-5 and +/-7.5 points with respect to the cutoff) as well as low polynomials of the distance to the cutoff to check the robustness of the RD estimates to different specifications. I estimate separate regressions for different age groups and include county fixed effects, a gender dummy and school year dummies in the regressions. I restrict the sample to individuals that I follow across the second and third wave of the RS that were surveyed before 2009 in the second wave.

5 Assessing the validity of the identification strategy

For the identification strategy to be valid, individuals should not be able to precisely manipulate their Selben score. This is unlikely in this setting because the Selben index is a complex "composite index" and its methodology has never been disclosed. People do not know the weights associated with their responses when they are surveyed, making it very difficult for them to determine which answers will make them end up on the left side of the cutoff. Furthermore, the change in the methodology in 2009, which involved the use of new variables to build the index,

Figure 3: Frequency distribution of Selben I and Selben II score

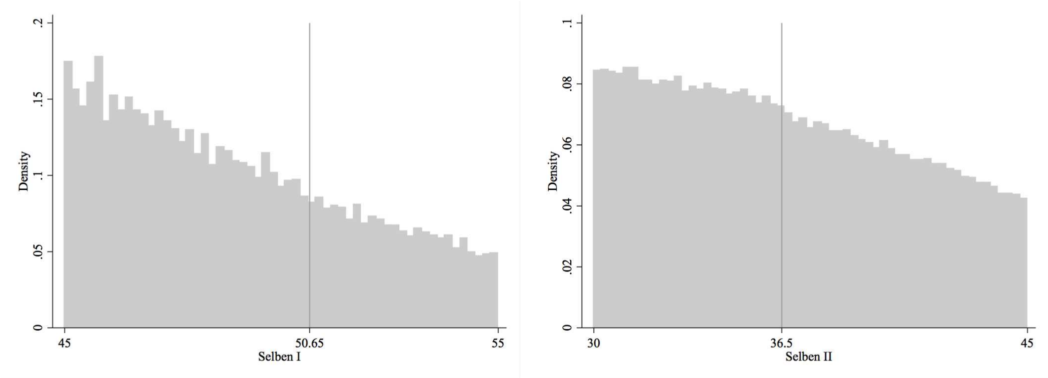


Table 2: Density tests for Selben I and Selben II

	Left of Selben I cuttof	Right of Selben I cuttof	Left of Selben II cuttof	Right of Selben II cuttof
Observations	651103	171121	555666	244332
Effective Observations	43763	36769	39043	40559
Bias corrected density	0.03	0.03	0.02	0.02
Standard error	0.00	0.00	0.00	0.00
Bandwidth values	1.54	1.55	2.00	2.27
Standard error test	0.00		0.00	
p-value	0.64		0.94	

Notes: Density tests based on Cattaneo et al. (2016). This local polynomial density estimator does not require pre-binning of the data as opposed to McCrary's test.

made cheating even more difficult. Figure 3 shows that there is no evidence of bunching or manipulation of the Selben I or Selben II scores. Moreover, the density tests shown in Table 2 fail to reject the hypothesis that the difference in densities on the two sides of the cutoff is zero.[17] These results rule out possible self-selection or non-random sorting of units into eligible or ineligible groups.

For identification, it is also important that there are no imbalances in baseline characteristics. To test for balance in the pre-treatment characteristics, I use a regression discontinuity approach and estimate local regressions of different polynomial orders using the observations within +/-2.5 points of distance to the cutoff. The variables were chosen from the list of variables used to estimate the Selben score.

I did this with the people who were surveyed before 2003 in the first wave of the RS, and with the people who became eligible for phase two, who were surveyed before 2009 in the second wave. I also tested whether among the former eligible children,

[17] Cattaneo et al. (2016) propose a set of manipulation tests based on a novel local polynomial density estimator, which does not require pre-binning of the data as opposed to McCrary's test.

Table 3: Balance tests for pre-treatment characteristics (2002) for the analysis of a short exposure during phase one

	Linear specification		Quadratic specification	
Variables	Pt. Est	Std. Err	Pt. Est	Std. Err
Does the household own land	-0.0117**	(0.00546)	-0.0180**	(0.00818)
Electricity	0.00138*	(0.000731)	0.000621	(0.00110)
Does not have exclusive shower	0.00343	(0.00300)	0.00270	(0.00459)
Overcrowding	0.000363	(0.00630)	-0.000143	(0.00950)
Household members	0.00916	(0.0188)	0.00474	(0.0283)
Total earners	0.000235	(0.0106)	-0.0111	(0.0159)
Members who work	0.00771	(0.00701)	0.000952	(0.0105)
Members below 18 years	0.00678	(0.0132)	0.0286	(0.0199)
Members studying	0.0114	(0.0119)	0.0271	(0.0179)
Education level of the head	0.0190*	(0.0109)	0.0105	(0.0164)
Does the head have a job	0.00901**	(0.00451)	0.00267	(0.00677)
Does the head speaks native languages	-0.000442	(0.00223)	5.01e-05	(0.00334)
Is the head retired	-0.000450	(0.000859)	0.000284	(0.00122)
Years of education of the head	0.0471	(0.0463)	0.0434	(0.0694)
Joint F-test	19.50		10.32	
P-value	0.1467		0.7386	
Observations	94,965		94,965	

Notes: Robust standard errors shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Sample is households surveyed before 2003 in the first wave of RS that have an adult woman and children below age 18.

those who were considered eligible or not for the second phase of the program had balanced characteristics. Results of the linear and quadratic specifications as well as the omnibus joint F-tests are reported in Tables 3, 4 and 5. In all the cases, at least for one of the specifications, the p-value on the omnibus F test was not statistically significant.

Table 3 shows that the pre-treatment characteristics of marginally eligible children in 2002 (around +/-2.5 points of the cutoff) present some minor imbalances in the linear specification that disappear in the quadratic specification. The quadratic specification performs better in terms of the omnibus joint F-test, but the linear specification was preferred in terms of the Akaike information criterion (AIC) and the goodness of fit test performed by jointly testing the significance of a set of bin dummies included as additional regressors in the model in order to select the optimal order of the polynomial.

Table 4 shows that among people who were not treated before 2009, the pre-treatment characteristics of the two comparison groups were balanced, including in terms of the Selben I score for both the linear and quadratic specifications. The

Table 4: Balance tests for pre-treatment characteristics (2008) for the analysis of short exposure during the second phase of the program

	Linear specification		Quadratic specification	
Variables	Pt. Est	Std. Err	Pt. Est	Std. Err
Selben I score	-0.0977	(0.192)	0.122	(0.289)
Land	-0.00466	(0.00706)	-0.0116	(0.0107)
Electricity	0.000447	(0.00140)	0.00167	(0.00212)
Does not have exclusive shower	0.00153	(0.00847)	-0.00725	(0.0126)
Overcrowding	-0.00290	(0.00856)	-0.00510	(0.0128)
Household members	0.0220	(0.0382)	0.0683	(0.0575)
Total earners	-0.00413	(0.0159)	0.0280	(0.0234)
Members who work	-0.00189	(0.0140)	0.0120	(0.0211)
Members below 18 years	0.0238	(0.0287)	0.0357	(0.0431)
Members studying	0.0161	(0.0268)	0.00128	(0.0402)
Education level of the head	-0.0176	(0.0387)	0.0198	(0.0583)
Does the head have a job	-0.000451	(0.00970)	0.0203	(0.0147)
Does the head speaks native languages	-0.000207	(0.00365)	-0.000543	(0.00540)
Is the head retired	-0.00123	(0.00126)	-0.00281*	(0.00163)
Years of education of the head	0.0240	(0.0966)	0.133	(0.145)
Joint F-test	4.77		12.88	
P-value	0.9939		0.6113	
Observations	22,811		22,811	

Notes: Robust standard errors shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.Sample is households surveyed before 2009 in the second wave of RS that have an adult woman and children below age 18.

omnibus joint F-test favors the linear specification, which is also preferred based on the AIC criterion and the goodness of fit tests.

Finally, I tested whether children who were treated in phase one, and were assigned to be treated or not in phase two based on their Selben II score, had balanced pre-treatment characteristics at the beginning of phase two. Results from the linear and quadratic specification show that those characteristics are well balanced including in terms of the Selben I score, meaning that in the neighborhood of the cutoff point, assignment in 2009 was independent of the first assignment in 2003. As shown in Table 5. Both specifications performed well in terms of the omnibus joint F-test, while the linear specification was slightly preferred based on the AIC criterion and the goodness of fit test.

Table 5: Balance tests for pre-treatment characteristics (2008) for the analysis of long vs short exposure to BDH

	Linear specification		Quadratic specification	
Variables	Pt. Est	Std. Err	Pt. Est	Std. Err
Selben I score	-0.0108	(0.0809)	-0.0926	(0.121)
Land	-0.00157	(0.00417)	-0.00212	(0.00622)
Electricity	0.000776	(0.000881)	0.000221	(0.00138)
Does not have exclusive shower	-0.00264	(0.00495)	0.00254	(0.00737)
Overcrowding	-0.000476	(0.00548)	0.00724	(0.00818)
Household members	-0.0115	(0.0251)	-0.000694	(0.0375)
Total earners	-0.00601	(0.0105)	-0.00974	(0.0157)
Members who work	-0.00797	(0.00879)	0.00444	(0.0132)
Members below 18 years	-0.0155	(0.0193)	-0.0133	(0.0287)
Members studying	-0.00847	(0.0171)	-0.0134	(0.0254)
Education level of the head	-0.0236	(0.0225)	-0.0424	(0.0337)
Does the head have a job	-0.00344	(0.00606)	-0.000756	(0.00902)
The head speaks native languages	-0.000198	(0.00226)	0.000904	(0.00323)
Is the head retired	0.000573	(0.000592)	0.000565	(0.000881)
Years of education of the head	-0.0445	(0.0580)	-0.0849	(0.0864)
Joint F-test	7.23		5.71	
P-value	0.9508		0.9842	
Observations	56,872		56,872	

Notes: Robust standard errors shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Sample is households surveyed before 2009 in the second wave of RS that have an adult woman and children below age 18.

6 Results

In this section, I present results of the 2SLS regressions in equation 4 that uses the treatment assignment as instrument for the actual treatment. Considering that there is some variation in treatment duration across households, I restrict the sample to households that were treated for at least three years in each of the phases of the program. Given that the percentage of observations that meet this requirement is close to 99% of the sample, I present the results for this sample and checked that the results for the complete sample are almost identical.

6.1 Effects of a short exposure to BDH measured at the end of phase one

6.2 Education outcomes

Table 6 reports on the short term effects of BDH on enrollment and years of education. Columns 1 and 3 show the results of the reduced form regressions (ITT), while columns 2 and 4 show the results from the 2SLS regression in equation 4. All the regressions include time and county fixed effects, control for gender and are estimated on the sample of individuals that are located within +/- 2.5 points of the Selben I cutoff.

To save space, I report the results only for children aged 10 to 21 years by the end of phase one. These children were between 5 and 16 years old when the first phase began in 2003. By choosing these age groups, I am able to observe the effects of BDH on children who began treatment at all the juncture ages in their academic progress which are age 5, 12 and 15. These are juncture ages because at age 5 parents decide to enroll their children at elementary school, at age 12 they decide to enroll them in high school and at age 15 they decide whether or not to enroll them in Baccalaureate. Furthermore, I study children aged 5 to 16 years in 2003, because the analysis focuses on children who were treated for at least three years during phase one and even when children aged 17 and 18 could benefit from the transfer, they were only exposed to it for two years maximum.

The results of the reduced form regressions in column 1 show negative effects of BDH on enrollment for some age groups, one reason is that the program did not reach all the households with a Selben score below the threshold of eligibility. Figure

Table 6: Short term effects of BDH on enrollment and years of education by the end of phase one.

	Enrollment		Years of Education		
	(1)	(2)	(3)	(4)	
Ages	ITT	2SLS	ITT	2SLS	N
Effect on 10 year olds	-0.00518	0.0130	0.00678	0.212**	5561
	(0.00422)	(0.00836)	(0.0468)	(0.0886)	
Pre- treatment mean	0.992	0.992	5.160	5.160	
Effect on 15 year olds	-0.0201*	0.0255	0.0180	0.147	6464
	(0.0121)	(0.0224)	(0.0662)	(0.117)	
Pre- treatment mean	0.886	0.886	8.863	8.863	
Effect on 16 year olds	0.0172	0.0127	0.0512	-0.0454	5861
	(0.0149)	(0.0264)	(0.0830)	(0.146)	
Pre- treatment mean	0.839	0.839	9.291	9.291	
Effect on 17 year olds	-0.00243	0.0872**	-0.107	0.170	5449
	(0.0183)	(0.0357)	(0.0911)	(0.174)	
Pre- treatment mean	0.723	0.723	9.791	9.791	
Effect on 18 year olds	0.000474	0.0836*	0.103	0.321	5343
	(0.0210)	(0.0437)	(0.105)	(0.214)	
Pre- treatment mean	0.539	0.539	10.42	10.42	
Effect on 19 year olds	-0.0220	0.0511	-0.110	0.296	5099
	(0.0215)	(0.0431)	(0.111)	(0.227)	
Pre- treatment mean	0.433	0.433	10.54	10.54	
Effect on 20 year olds	0.00882	0.111**	0.105	0.0170	4698
	(0.0219)	(0.0456)	(0.124)	(0.257)	
Pre- treatment mean	0.379	0.379	10.73	10.73	
Effect on 21 year olds	-0.0732***	0.0608	-0.110	0.441	4335
	(0.0222)	(0.0483)	(0.142)	(0.323)	
Pre- treatment mean	0.331	0.331	10.70	10.70	
County and time FE	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben I cutoff (50,65 points).

2 shows that before 2009, take up was always below 80% in the area closer to the threshold. Once I correct for the endogeneity of take up by instrumenting the actual treatment with the assignment to treatment, the effects turn positive for all the age groups.

According to the 2SLS estimates reported in column 2, BDH was effective in raising the enrollment rates of 10 to 21 year olds. However, the effect was statistically significant only for 17, 18 and 20 year olds who were around 12, 13 and 15 years when the program started in 2003. Enrollment increased by 8.7 percentage points by the end of phase one among 17 year olds and by 8.3 and 11 percentage points for 18 and 20 year olds respectively.

Given that when the program started most people believed that one of the conditions to receive the transfer was to send children to school, I expected to see positive effects on enrollment, however, what is more important is whether these children completed more years of schooling. Columns 3 and 4 report on the short term effects of BDH on years of education by the end of phase one. The reduced form specification reported in column 3 show some negative coefficients that turn positive after instrumenting the actual treatment with the assignment to treatment. Only 10 year olds, who were 5 years old when the first phase began experienced an increase in their years of education by 0.21 years, for children aged 15, 17 ,18 and 19 years, the effects were positive but not statistically significant.

The observed gains in enrollment are in line with a higher probability of graduating from high school for 18 year olds. For this group, being treated for at least three years during phase one increased the likelihood of graduating by 8.7 percentage points by the end on phase one (See column 2 of Table 7). This increase is equivalent to an 18% raise relative to a 48.7% pre-treatment high school graduation rate. Treated individuals aged 17 and 19 years by the end of phase one also experienced a higher probability of graduating from High School, although not statistically significant.

Furthermore, there was a positive effect of BDH on the likelihood of having some college education among 19 year olds at the end of phase one. These young people were 14 years old when the program started in 2003 and were old enough to benefit from free education at college level. The likelihood of having some college education increased by 7.4 percentage points for this group, relative to a pre-treatment rate of 17.3%, which implies a 43% raise. For 20 and 21 year olds, the effect of BDH was also positive but not statistically significant.

Table 7: Short term effects of BDH on high school graduation and on the likelihood of having some college education by the end of phase one.

	High School		Has some college		
	(1)	(2)	(3)	(4)	
Ages	ITT	2SLS	ITT	2SLS	N
Effect on 17 year olds	-0.0478***	0.0249			5449
	(0.0166)	(0.0306)			
Pre- treatment mean	0.186	0.186			
Effect on 18 year olds	0.00984	0.0870**			5343
	(0.0211)	(0.0441)			
Pre- treatment mean	0.487	0.487			
Effect on 19 year olds	-0.0350*	0.0369	-0.0290	0.0737**	5099
	(0.0203)	(0.0419)	(0.0178)	(0.0355)	
Pre- treatment mean	0.573	0.573	0.173	0.173	
Effect on 20 year olds			-0.00882	0.0323	4698
			(0.0206)	(0.0417)	
Pre- treatment mean			0.228	0.228	
Effect on 21 year olds			-0.0375*	0.0482	4335
			(0.0213)	(0.0466)	
Pre- treatment mean			0.246	0.246	
County and time FE	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben I cutoff (50,65 points).

6.3 Labor market outcomes

Table 8 shows the results of the ITT and 2SLS regressions of the effect of BDH on the probability of having a job by the end of phase one. In line with previous literature (Skoufias and Parker, 2001; de Janvry et al., 2006; Attanasio et al., 2012), the sign of the job coefficients is contrary to the sign of the enrollment coefficients for 17, 18, 20 and 21 year olds. The 2SLS estimates reported in column 2 show that the likelihood of having a job was negative but not statistically significant for most age groups and negative and statistically significant for 21 year olds. By the end of phase one, BDH reduced the employment rate of this age group by 12.5 percentage points relative to a pre-treatment rate of 56% , which implies a 22% reduction for this group. The magnitude of this effect is comparable to the 9.9 percentage points decrease in paid employment reported by Edmonds and Schady (2012) who evaluate the impacts of BDH after after 17 months of BDH's implementation on children aged 11–16 at baseline.

6.4 Effects of a short exposure to BDH measured at the end of phase two

6.5 Education outcomes

In terms of education outcomes, what makes the second phase of the program different from the first phase is that there was an increase in the years of education attained by treated children while by the end of phase one (with the exception of the effects on 10 year olds), I only observed effects on enrollment. One possible explanation is that the size of the transfer at the beginning was smaller, besides, it seems that parents were only looking to meet the conditions to get the cash transfer, but since the condition was on enrollment and not on passing the courses, there was not effects on years of education.

By contrast, in phase two, not only parents received a bigger cash transfer, but also, education became free after tenth grade with the reform of 2008. This may have reinforced the effect of BDH especially at the juncture ages of 12 and 15 years, when children decide whether or not to enroll at high school and at the first year of Baccalaureate, respectively.

Columns 1 and 2 in Table 9 report on the short term effects of BDH on enrollment

Table 8: Short term effects of BDH on the likelihood of having a job by the end of phase one.

	Has a job		
	(1)	(2)	
Ages	ITT	2SLS	N
Effect on 15 year olds	0.0136	0.0195	6465
	(0.00896)	(0.0166)	
Pre- treatment mean	0.0670	0.0670	
Effect on 16 year olds	-0.0142	-0.00178	5868
	(0.0117)	(0.0211)	
Pre- treatment mean	0.101	0.101	
Effect on 17 year olds	0.0177	-0.0287	5453
	(0.0163)	(0.0321)	
Pre- treatment mean	0.200	0.200	
Effect on 18 year olds	0.00423	-0.00627	5350
	(0.0198)	(0.0411)	
Pre- treatment mean	0.337	0.337	
Effect on 19 year olds	0.00693	0.0344	5105
	(0.0209)	(0.0418)	
Pre- treatment mean	0.449	0.449	
Effect on 20 year olds	-0.00196	-0.0600	4703
	(0.0217)	(0.0455)	
Pre- treatment mean	0.507	0.507	
Effect on 21 year olds	0.0288	-0.125***	4345
	(0.0214)	(0.0474)	
Pre- treatment mean	0.562	0.562	
County and time FE	Yes	Yes	
Controls	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben I cutoff (50,65 points).

Table 9: Short term effects of BDH on enrollment and years of education by the end of phase two.

	Enrollment		Years of Education		
	(1)	(2)	(3)	(4)	
Ages	ITT	2SLS	ITT	2SLS	N
Effect on 10 year olds	0.00597	-0.00765	-0.128	-0.899**	468
	(0.0146)	(0.0293)	(0.151)	(0.384)	
Pre- treatment mean	0.988	0.988	4.960	4.960	
Effect on 15 year olds	0.0347	0.0949	0.0247	0.445	1289
	(0.0251)	(0.0643)	(0.155)	(0.423)	
Pre- treatment mean	0.919	0.919	9.103	9.103	
Effect on 16 year olds	-0.0650**	-0.0694	-0.346**	0.398	1381
	(0.0285)	(0.0704)	(0.166)	(0.399)	
Pre- treatment mean	0.869	0.869	9.458	9.458	
Effect on 17 year olds	-0.0105	0.0480	-0.0143	1.204**	1482
	(0.0346)	(0.0952)	(0.181)	(0.512)	
Pre- treatment mean	0.728	0.728	10.11	10.11	
Effect on 18 year olds	-0.0376	-0.0708	-0.254	-0.141	1483
	(0.0386)	(0.110)	(0.190)	(0.555)	
Pre- treatment mean	0.447	0.447	10.70	10.70	
Effect on 19 year olds	0.0314	-0.224**	0.293	-0.267	1450
	(0.0375)	(0.106)	(0.197)	(0.521)	
Pre- treatment mean	0.344	0.344	10.97	10.97	
Effect on 20 year olds	-0.0205	0.193	-0.347	1.882**	1334
	(0.0371)	(0.129)	(0.223)	(0.825)	
Pre- treatment mean	0.279	0.279	11.12	11.12	
Effect on 21 year olds	-0.0421	0.0790	-0.0365	-0.158	1261
	(0.0366)	(0.175)	(0.255)	(1.204)	
Pre- treatment mean	0.256	0.256	11.20	11.20	
County and time FE	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben II cutoff (36.5 points).

Table 10: Short term effects of BDH on high school graduation and on the likelihood of having some college education by the end of phase two.

	High School		Has some college		
	(1)	(2)	(3)	(4)	
Ages	ITT	2SLS	ITT	2SLS	N
Effect on 17 year olds	0.00363	0.0240			1482
	(0.0292)	(0.0764)			
Pre- treatment mean	0.168	0.168			
Effect on 18 year olds	-0.0492	-0.224*			1483
	(0.0384)	(0.115)			
Pre- treatment mean	0.505	0.505			
Effect on 19 year olds	0.0178	0.0446	0.0477	-0.0516	1450
	(0.0380)	(0.102)	(0.0311)	(0.0870)	
Pre- treatment mean	0.624	0.624	0.167	0.167	
Effect on 20 year olds			-0.00727	0.381***	1334
			(0.0360)	(0.130)	
Pre- treatment mean			0.212	0.212	
Effect on 21 year olds			-0.0549	-0.115	1261
			(0.0369)	(0.177)	
Pre- treatment mean			0.248	0.248	
County and time FE	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben II cutoff (36.5 points).

by the end of phase two. My preferred specification is in column 2 and shows that BDH did not have a significant positive effect on enrollment for any of the age groups. If anything, there was a significant reduction on the enrollment rate of 19 year olds by 23 percentage points.

Columns 3 and 4 show the impacts of BDH on years of education by the end of phase two. Results from the 2SLS regressions show a positive and statistically significant effect on years of schooling for 17 and 20 year olds. The total years of education for 17 year olds increased by 1.2 years, relative to a pre-treatment mean of 10.11 years for this age group. While, for 20 year olds, schooling increased by 1.9 years, relative to a pre-treatment mean of 11.12 years of education.

Table 10 shows the short term effects of BDH on high school graduation rates and the likelihood of having some college education by the end of phase two. The 2SLS estimates reported in column 2 show a negative and statistically significant effect on the high school graduation rate among 18 year olds. Children who are close to finishing elementary school or tenth grade are less likely to drop out of school at least until they reach that goal. 18 year olds were around 13 years when the second phase of the program began and they were two years away from finishing tenth grade; so, receiving the transfer may not have been enough incentive to prevent some of them from leaving school. I also included 17 and 19 year olds to allow for the possibility of early and late high school graduations but the effects were not statistically significant for these groups.

On the other hand, column 4 shows that 20 year olds in 2014, who were 15 when the second phase began were in fact 38.1 percentage points more likely to have some college education. This is equivalent to a 180% increase relative to the pre-treatment rate.

6.6 Labor market outcomes

Table 11 reports on the short term effects of BDH on the employment rate of 15 to 21 year olds by the end of phase two. Column 2 shows that BDH had a negative and statistically significant effect on the employment rate of 17 year olds. The probability of working drops by 12.5 percentage points for this group, relative to a pre-treatment rate of 14.3% which is equivalent to a 87% reduction. This is consistent with the results reported in Table 9 that showed a statistically significant increase of 1.2 years of education for this age group accompanied by a positive effect, although not statistically significant, in enrollment.

6.7 Differential effects of a long versus a short exposure to BDH

6.8 Education outcomes

In this section, I discuss the differential effects of a short exposure to BDH (during phase one) versus a long exposure to BDH (during phases one and two). I focus on children aged 10 to 23 years at the end of phase two, who were treated during phase one and were 5 to 18 years old when they were assigned to be treated (or not) at the beginning of phase two. Since I restrict the sample to children that received

Table 11: Short term effects of BDH on the employment rate by the end of phase two.

	Has a job		
	(1)	(2)	
Ages	ITT	2SLS	N
Effect on 15 year olds	-0.0141	0.0528	1289
	(0.0176)	(0.0461)	
Pre- treatment mean	0.0480	0.0480	
Effect on 16 year olds	0.0163	-0.0117	1381
	(0.0207)	(0.0531)	
Pre- treatment mean	0.0770	0.0770	
Effect on 17 year olds	0.0163	-0.125*	1482
	(0.0269)	(0.0726)	
Pre- treatment mean	0.143	0.143	
Effect on 18 year olds	0.00301	-0.0260	1483
	(0.0371)	(0.107)	
Pre- treatment mean	0.311	0.311	
Effect on 19 year olds	0.0173	0.0928	1450
	(0.0396)	(0.107)	
Pre- treatment mean	0.440	0.440	
Effect on 20 year olds	0.0453	-0.165	1334
	(0.0421)	(0.150)	
Pre- treatment mean	0.523	0.523	
Effect on 21 year olds	-0.0575	0.240	1261
	(0.0427)	(0.218)	
Pre- treatment mean	0.587	0.587	
County and time FE	Yes	Yes	
Controls	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben II cutoff (36.5 points).

treatment for at least three years in each of the phases of the program, the final sample contains children aged 10 to 21 years by the end of phase two.

Table 12, shows the differential effects of BDH on enrollment and years of education by the end of phase two. Column 2 shows the results from the 2SLS regressions in equation 4. It shows that none of the age groups were significantly more likely to be enrolled in an educational establishment after being treated during the two phases of the program when compared to similar children that were treated only during the first phase.

Column 4 shows the results for years of education. The general results are not very encouraging either. However, children who began treatment in 2008 at age 5 increased their years of education in 0.28 years, relative to a pre-treatment mean of 4.96 years for that age group, which is equivalent to a 6% increase in schooling.

The differential effects of BDH on high school graduation and on the likelihood of having some college education are reported in Table 13. There were no statistically significant gains in terms of the likelihood of graduating from high school for 17 to 19 year olds. The reason is that these children were between 7 and 9 years when the first phase began and between 12 and 14 years when the second phase began. Since children who are close to finish tenth grade are less likely to drop out from school, I expect that most of them would remain at school despite not receiving the transfer, hence the non-statistically significant differences reported here, another interpretation could be that without the transfer they would have done much worse, but after being treated during two phases, their outcomes were at least as good as those of children treated during one phase. Likewise, there were no significant effects on the probability of having some college. This probability was even negative for 21 year olds, probably because households treated during the two phases are the most vulnerable ones.

6.9 Labor market outcomes

The last wave of the RS collects information on the occupation sector in which people work. This allows me to classify the different sectors into two general groups, agricultural work and white collar jobs. In this way it is possible to investigate the effects of BDH not only on the probability that young adults work but also on the type of work they have.

In general, being treated during the two phases of the program reduced the likeli-

Table 12: Differential effects of BDH on enrollment and years of education

	Enrollment		Years of Education		
	(1)	(2)	(3)	(4)	
Ages	ITT	2SLS	ITT	2SLS	N
Effect on 10 year olds	-0.00379	0.0166	0.00307	0.280**	1787
	(0.00650)	(0.0110)	(0.0609)	(0.114)	
Pre- treatment mean	0.988	0.988	4.960	4.960	
Effect on 11 year olds	0.00869*	-0.00686	0.0399	-0.159*	3401
	(0.00447)	(0.00788)	(0.0479)	(0.0927)	
Pre- treatment mean	0.987	0.987	5.917	5.917	
Effect on 12 year olds	0.00381	-0.00729	-0.0550	-0.0670	7002
	(0.00447)	(0.00811)	(0.0406)	(0.0759)	
Pre- treatment mean	0.980	0.980	6.817	6.817	
Effect on 13 year olds	0.00397	-0.0147	0.000286	0.0401	7470
	(0.00557)	(0.0103)	(0.0484)	(0.0890)	
Pre- treatment mean	0.971	0.971	7.677	7.677	
Effect on 14 year olds	-0.00218	0.0119	0.0316	0.0613	7629
	(0.00731)	(0.0142)	(0.0528)	(0.100)	
Pre- treatment mean	0.949	0.949	8.481	8.481	
Effect on 15 year olds	-0.0130	-0.00875	8.24e-05	-0.0803	6767
	(0.00923)	(0.0183)	(0.0631)	(0.117)	
Pre- treatment mean	0.919	0.919	9.103	9.103	
Effect on 16 year olds	-0.0257**	0.0353	-0.165**	-0.0312	7084
	(0.0112)	(0.0229)	(0.0679)	(0.140)	
Pre- treatment mean	0.869	0.869	9.458	9.458	
Effect on 17 year olds	-0.0108	-0.0239	-0.0563	-0.0606	6678
	(0.0156)	(0.0320)	(0.0752)	(0.151)	
Pre- treatment mean	0.728	0.728	10.11	10.11	
Effect on 18 year olds	0.0139	-0.0135	-0.106	-0.217	5734
	(0.0187)	(0.0386)	(0.0878)	(0.182)	
Pre- treatment mean	0.447	0.447	10.70	10.70	
Effect on 19 year olds	-0.0189	-0.0289	0.0275	-0.0322	5409
	(0.0185)	(0.0426)	(0.0933)	(0.211)	
Pre- treatment mean	0.344	0.344	10.97	10.97	
Effect on 20 year olds	0.0301*	-0.0613	0.0137	-0.291	5127
	(0.0176)	(0.0411)	(0.103)	(0.234)	
Pre- treatment mean	0.279	0.279	11.12	11.12	
Effect on 21 year olds	0.0209	-0.0501	-0.0429	-0.182	4605
	(0.0183)	(0.0434)	(0.118)	(0.281)	
Pre- treatment mean	0.256	0.256	11.20	11.20	
County and time FE	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben II cutoff (36.5 points).

Table 13: Differential effects of BDH on high school graduation and on the likelihood of having some college education

	High School		Has some college		
	(1)	(2)	(3)	(4)	
Ages	ITT	2SLS	ITT	2SLS	N
Effect on 17 year olds	0.0101	-0.0201			6678
	(0.0122)	(0.0251)			
Pre- treatment mean	0.168	0.168			
Effect on 18 year olds	-0.0151	0.00686			5734
	(0.0184)	(0.0379)			
Pre- treatment mean	0.505	0.505			
Effect on 19 year olds	-0.00511	0.0155	-0.00358	0.0206	5409
	(0.0187)	(0.0430)	(0.0142)	(0.0331)	
Pre- treatment mean	0.624	0.624	0.167	0.167	
Effect on 20 year olds			0.0152	-0.0444	5127
			(0.0155)	(0.0362)	
Pre- treatment mean			0.212	0.212	
Effect on 21 year olds			-0.00815	-0.0845**	4605
			(0.0183)	(0.0426)	
Pre- treatment mean			0.248	0.248	
County and time FE	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben II cutoff (36.5 points).

Table 14: Differential effects of BDH on the employment rate

	Has a job		Agriculture job		White collar job		
	(1)	(2)	(3)	(4)	(5)	(6)	
Ages	ITT	2SLS	ITT	2SLS	ITT	2SLS	N
Effect on 15 year olds	0.00515	-0.00410	0.0192	-0.183	-0.0188	-0.0783	6767
	(0.00719)	(0.0139)	(0.0823)	(0.126)	(0.0900)	(0.148)	
Pre- treatment mean	0.0480	0.0480	0.293	0.293	0.233	0.233	
Effect on 16 year olds	0.0141	-0.0109	0.0150	0.105	-0.143**	-0.0461	7084
	(0.00861)	(0.0175)	(0.0493)	(0.0883)	(0.0707)	(0.107)	
Pre- treatment mean	0.0770	0.0770	0.239	0.239	0.256	0.256	
Effect on 17 year olds	-0.00332	0.00744	0.00185	0.0278	-0.000628	0.0194	6678
	(0.0124)	(0.0251)	(0.0361)	(0.0730)	(0.0450)	(0.0913)	
Pre- treatment mean	0.143	0.143	0.194	0.194	0.270	0.270	
Effect on 18 year olds	-0.00152	0.0275	0.0231	-0.0398	0.00414	0.0290	5734
	(0.0170)	(0.0353)	(0.0244)	(0.0452)	(0.0340)	(0.0614)	
Pre- treatment mean	0.311	0.311	0.151	0.151	0.317	0.317	
Effect on 19 year olds	0.0128	-0.00477	0.00411	0.0377	0.0313	-0.0539	5409
	(0.0183)	(0.0425)	(0.0189)	(0.0425)	(0.0287)	(0.0666)	
Pre- treatment mean	0.440	0.440	0.144	0.144	0.337	0.337	
Effect on 20 year olds	0.00931	0.0221	0.00679	-0.0381	0.0432	0.0108	5127
	(0.0183)	(0.0422)	(0.0187)	(0.0408)	(0.0280)	(0.0624)	
Pre- treatment mean	0.523	0.523	0.129	0.129	0.362	0.362	
Effect on 21 year olds	-0.00188	0.0333	0.0402**	0.00300	-0.0612**	0.0633	4605
	(0.0188)	(0.0439)	(0.0168)	(0.0402)	(0.0275)	(0.0626)	
Pre- treatment mean	0.587	0.587	0.127	0.127	0.377	0.377	
County and time FE	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table reports on the results of ITT and 2SLS regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben II cutoff (36.5 points).

hood of having a job among the younger age groups. However, this effect was not statistically significant. Similarly, there were no significant effects on the probability of having a white collar job or a job in the agricultural sector as shown in Table 14.

These results are consistent with a recent study by Hahn et al. (2018) that examines the long-term effects of a stipend program that made secondary education free for rural girls in Bangladesh and find no apparent effect of the stipend on the likelihood of women working in adulthood.

Table 15: Short term effects of BDH by the end of phase two using ENES data

	Enes exam		
	(1)	(2)	
Ages	ITT	2SLS	N
Effect on 17 year olds	-0.0313	0.0559	1482
	(0.0376)	(0.100)	
Effect on 18 year olds	-0.0251	-0.134	1483
	(0.0393)	(0.112)	
Effect on 19 year olds	-2.21e-05	0.0478	1450
	(0.0365)	(0.0986)	
County and time FE	Yes	Yes	
Controls	Yes	Yes	

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the coefficients of ITT and 2SLS regressions. Sample is children who took the ENES exam and has a Selben score within +/-2.5 points from the Selben II cutoff (36.5 points) who were untreated on phase 1.

7 Robustness checks

7.1 Results using ENES exam data

Considering that an important number of counties were left out of the third wave of the RS, in this section, I use administrative data on the ENES exam (Examen Nacional para la Educación Superior) to test the robustness of the results regarding high school graduation. In Ecuador, students have to pass a standardized exam (ENES) in order to go to university. It is compulsory for all students enrolled in the last year of high school at private and public schools; hence, taking the test is a good predictor for high school graduation.

The exam was first administered in 2012, so it is possible to merge the 2013 and 2014 ENES databases to the last wave of my three-wave panel using the students' IDs. Of the 336,791 students in the ENES dataset, 168,481 were also in the RS. As in the previous section, I run similar 2SLS regressions to the ones I estimated before using as the outcome of interest a binary variable that takes the value of one if the child was on the ENES dataset, which implies that she was in the final year of high school, and zero if not.

As seen in Table 10, when the RS sample is used, being treated during the second phase of the program significantly reduced the probability of graduating from school among 18-year-olds by 22.4 percentage points, whereas when using the ENES exam sample, the result was a reduction of 13.4 percentage points that was not statistically

Table 16: Differential effects of a long versus a short exposure measured by the end of phase two using ENES data

	Enes exam		
	(1)	(2)	
Ages	ITT	2SLS	N
Effect on 17 year olds	-0.0352**	-0.0239	6678
	(0.0172)	(0.0352)	
Effect on 18 year olds	0.0198	-0.0353	5734
	(0.0188)	(0.0387)	
Effect on 19 year olds	0.00128	-0.0666	5409
	(0.0180)	(0.0421)	
County and time FE	Yes	Yes	
Controls	Yes	Yes	

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. P-values from goodness of fit test after standard errors. Table reports the coefficients of ITT and 2SLS regressions. Sample is children who took the ENES exam and has a Selben score within +/-2.5 points from the Selben II cutoff (36.5 points) who were treated on phase 1.

significant, as shown in column 2 of the Table 15.

On the other hand, Table 16 reports on the results of the differential effects of a long versus a short exposure to BDH on the probability of high school graduation. The results were in line to those reported in Table 13 that showed not statistically significant effects for 17, 18 and 19 year olds.

8 Conclusions

This paper studies the short and long-term effects of a cash transfer program that uses a proxy means test to select beneficiaries. The results provided here are more informative from a policy perspective than those of studies that look at how well the original treatment and control groups perform after several years. Said framework is common in the evaluation of short-duration programs but may not be optimal for the evaluation of long duration programs because is hard to rule out the contamination of the original treatment and control groups as time goes by, which may cause an attenuation of the program's impact and makes it difficult to derive policy implications regarding the optimal duration of this kind of programs.

The question I address in this paper is whether cash transfers continue to be effective after several years targeting the same population. For this, I used individual-level social registry data that allowed me to identify children that were treated during one or the two phases of the program and track their performance in terms of education and labor market outcomes. With this information and knowing that at the threshold of eligibility, the second assignment to treatment (in 2008/9) was independent of the first assignment (in 2003), I was able to disentangle the impact of a short exposure to BDH (treatment during phase one) versus a long exposure (treatment during phases one and two).

The short term effects of BDH at the end of phase one were positive and statistically significant on enrollment among children that began treatment at ages 12, 13, and 15 but no effects on years of education were found, except among children aged 5 years old when the first phase began. By contrast, the results by the end of phase two (among children who were treated only during phase two) showed positive effects on years of education among children that began treatment at ages 12 and 15, and effects in enrollment among 15 year olds. The positive effects in enrollment observed at the end of phase one dissipated among children observed at the end of phase two and are consistent with the fact that soon after implementation, people realized the authorities were not monitoring the conditions. On the other hand, the fact that BDH was effective in raising the years of education of children that were only treated during phase two may have to do with the education gratuity in place since 2008, which benefited children who were about to start the eleventh grade (15 years) when the second phase of the program began.

The lack of differential effects on education and labor market outcomes among children that were treated during the two phases versus those treated during just the first phase, is not explained by an attenuation of BDH effects for all the age groups in phase two. In fact, the analysis of the short term effects at the end of phase two showed positive and important effects of BDH at juncture ages. This contradicts the hypotheses that educational gratuity or lack of monitoring of the conditions attenuated the effects of BDH during phase two. A more plausible reason for the lack of differential effects is that once children reach the education level they have planned to achieve, or alternatively, once they reach certain age when working is more profitable, the unconditional transfer does not provide enough incentive to keep them at school.

The only group that experienced positive differential effects in years of education after being exposed during the two phases of the program versus children that were exposed during phase one were children aged 0 when BDH began in 2003. Children who started treatment at an older age did not benefit from the transfers because once cognitive gaps appear, the process can not be reversed only with the help of cash transfers. It is likely that these children were already lagging behind perhaps with low grades and attendance problems, which explains why BDH did not have positive effects on their education.

Regarding labor market outcomes in the short-run, results were not conclusive about whether the negative effect on the likelihood of having a job among treated 17 year olds at the end of phase two was caused by a concurrent raise in enrollment for this age group. Moreover, being exposed to BDH for two phases versus just one did not give treated children an advantage in the labor market by the end of phase two. The reason could be that children exposed during the two phases of the program did not achieve more years of education after all. It is possible that people treated during the two phases could be more vulnerable, and that other things that are not captured by the Selben score like social networks or lack thereof, important to access the job market, could be attenuating the effects of BDH on labor market outcomes.

The lack of monitoring of the conditions is one of the factors that may explain why BDH did not achieve its goal of improving educational attainment consistently in the long run. Had the transfer been conditioned on school registration and on grade progression it is likely that better results would have been found in terms of years of education. In a study for Ecuador, Schady et al. (2008) found that the short-term gains from BDH were significantly larger among households who believed that there was a school enrollment requirement attached to transfers. Evidence from micro-simulation models for Mexico and Brazil also conclude that conditions attached to transfers explain the bulk of the effect of CCT programs on school enrollment (Bourguignon et al., 2002; Todd and Wolpin, 2006). Another reason for the lack of lasting effects is that the transfer was not big enough to compensate for the wages that older children could get in the labor market. Finally, when there are other children at home, there is no way to prevent parents from spending the transfer on the older children instead of those below 18 years. This would also cause an attenuation of the program's impact.

Looking strictly to the effects of BDH on education and labor market outcomes, the results from this paper stress the need for a redesign of BDH. Attanasio et al. (2012) argue that for the case of PROGRESA, a revenue neutral change that increases the grants for secondary school children while eliminating it for primary school children would have positive effects on enrollment on the latter and minor effects on the former. I would expect similar results for the case of BDH. Transfers should also take into account the number of children in the household and should increase with age in order to reduce the opportunity cost from work for children aged 15 to 18 years. Furthermore, the government should set a limit for the maximum number of years that families can remain in the program (possibly five years).

Finally, it is worth noting that even when BDH became inefficient in the long-run in improving the educational outcomes of children, it does not mean necessarily that this applies also to health outcomes. Future research should look at the long-term effects of BDH on other outcomes to inform any reform to the program.

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9 Appendix

Data Appendix

The main challenge in linking the three waves of the RS is that many people do not report an ID (particularly children in the first wave of RS). I built two short panels (two-waves panel) and one long panel (three-waves panel). The size of the three-waves panel is bounded by the number of matches between waves 1 and 2 because fewer people reported an ID in wave 1. Merging the three waves of the RS by ID renders only 1,271,538 matches. For this reason, I use probabilistic record linkage to match the individuals in the three waves of the RS covering the period from 2001 to 2014. For that, I used the 4 names (2 names and 2 last names) and a common household ID as match keys. The algorithm takes into account the fact that some family members leave or join the household as time goes by.[18].

Descriptive statistics

The main panel contains 2,961,079 individuals linked throughout the three waves of the RS. The two short panels that follow individuals through waves 1 and 2, and through waves 2 and 3, contain 4,631,690 and 5,439,749 individuals respectively. In each wave of the RS, the sample in the long panel reproduces quite well the distribution of the Selben score as shown in Figures 4, 5 and 6. In general, the curves of the total RS population and the sample follow a normal distribution that almost overlaps, especially for the first and third wave.

It is important to bear in mind that around 30 variables were involved in the estimation of the Selben index, among them, characteristics of the household head, features of the house, access to services, assets, etc. This is why it is considered a good measure in order to characterize households. The fact that the Selben distributions of the panel and the corresponding wave are very similar means that the panel represents households of all socio-economic backgrounds that are in the RS. As such, on average, people who did not report an ID (and could not be matched because of it) are not disproportionately poorer. Only in the second wave does the panel contain a slightly greater number of poorer households.

[18] 400,000 individuals in the three-waves panel moved to (or from) other households. For more details on the dataset see Paredes (2016)

Figure 4: Kernel density graphs of the Selben score in the first wave of RS and and the corresponding panel wave

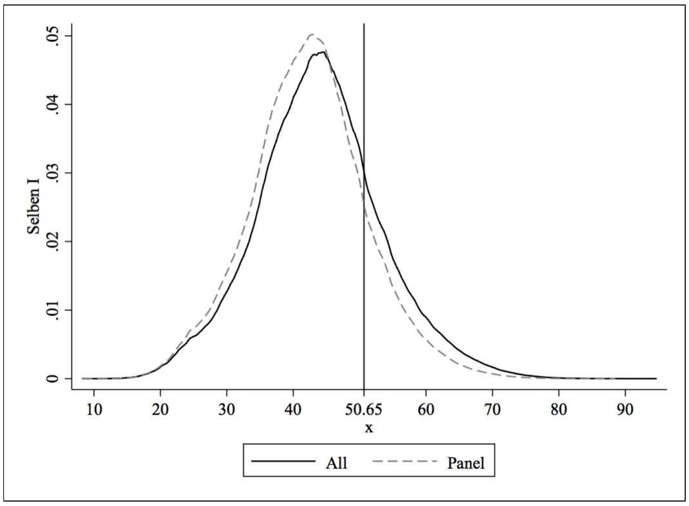


Figure 5: Kernel density graphs of the Selben score in the second wave of RS and and the corresponding panel wave

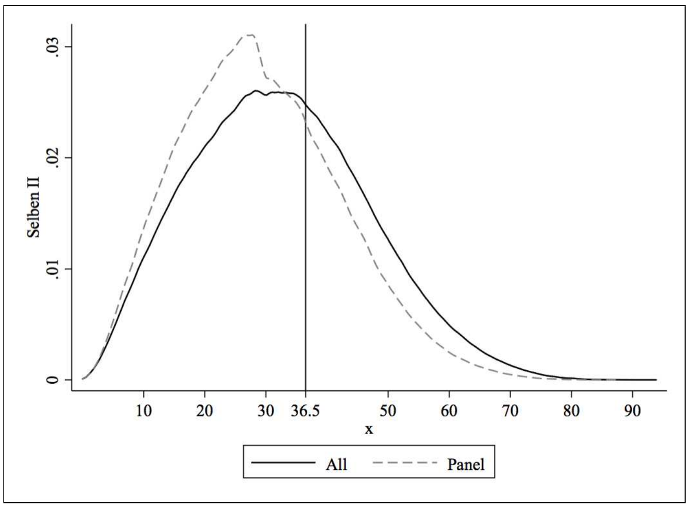
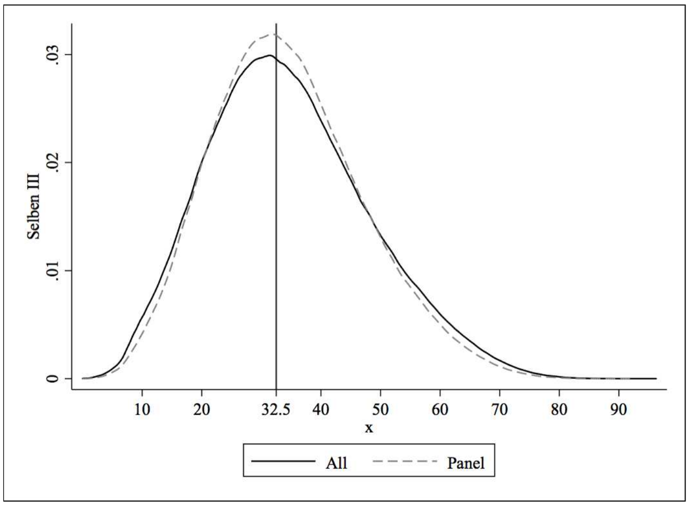


Figure 6: Kernel density graphs of the Selben score in the third wave of RS and and the corresponding panel wave



To correct for the fact that people with a valid ID are more likely to be in the panel, I constructed sample weights so that the panel's totals on key characteristics match the totals of the corresponding wave of the RS. This process is known as raking or sample-balancing. I included gender, education level, highest grade completed, the number of years of education, birth year, employment, province, county and type of house as auxiliary variables. [19].

[19] See Paredes (2016) for more details