The impact of exposure to cash transfers on education and labor market outcomes

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Abstract

This paper studies the short and long-term effects of exposure to Bono de Desarrollo Humano (BDH), the main unconditional cash transfer program in Ecuador, on young people's education and labor market outcomes. Using individual administrative panel data and a regression discontinuity design, I estimate the short term impact of BDH, as well as the differential impact of a long exposure (10 years) versus a short exposure to BDH (five years). In the short-run, treated children experienced gains in enrollment and schooling, but those gains dissipated after five more years of treatment. This explains why after ten years of exposure, treated children aged 18-21 were not more likely to finish high school when compared to similar children who were only treated during the first five years of the program. Regarding labor market outcomes, BDH had a negative but not statistically significant impact on the probability of working among the young children who were treated either during five or ten years and did not increase job opportunities among young adults.

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 during college-going years in explaining gaps in socioeconomic attainment (Carneiro et al., 2002; Cunha et al., 2010; Heckman, 2000). Cash

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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 (UCT) to fully monitored and enforced conditional cash transfers (CCT) (Baird et al., 2014). A large body of evidence shows that CCTs improve health and education among beneficiary children (see Fiszbein et al. (2009) for a review and Saavedra and Garcia (2012) for a meta-analysis). The literature about UCTs is smaller but it has regained attention in the last few years, Hanlon et al. (2010) provide a review of the impacts of these programs on schooling and Benhassine et al. (2015) provide more recent evidence about the large effects of a labeled UCT on school participation in Morocco.

This paper studies the effects on young people's education and labor market outcomes of exposure to Bono de Desarrollo Humano (BDH), the main unconditional cash transfer program in Ecuador. BDH was launched in 2003; it targets women with young children (0-18 years) in the bottom 40% of a "Selben" score distribution. The "Selben" index is a multidimensional poverty index built using principal components analysis. In Ecuador, BDH has been subject to many short-term evaluations, showing important improvements in schooling, cognitive and socio-emotional development, and reductions in child labor among treated children (Edmonds and Schady, 2012; Paxson and Schady, 2007; Schady et al., 2008). However, this may not be enough to take these children out of poverty if they do not complete more years of schooling or if they learn little during classes. Furthermore, it is not clear that the short-term gains should persist through time.

Evidence of the impact of cash transfers in 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 obtained 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 terms of developing countries, the few existing papers that examine long-term effects focus mainly on Latin America (LA), firstly, because the eldest CCT programs started there (like Mexico's PROGRESA /Oportunidades in 1997), giving a sufficiently long time span to evaluate long-term effects. Secondly, because many programs in the region had rigorous designs and implementation which facilitated their evaluation. Since most of these programs are relatively young, the analysis of long-term effects in LA center on the study of outcomes measured at the end of high school or during early adulthood. Experimental evidence comes from Mexico's Progresa[1] (Behrman et al., 2011, 2005) and Nicaragua's CCT program Red de Protección Social (RPS) (Barham et al., 2013b,a), while non-experimental evidence comes from Colombia's CCT Familias en Acción [2] (Baez and Camacho, 2011) and more recently from Ecuador's CT program Bono de Desarrollo Humano (BDH). Schady et al. (2016) use a regression discontinuity (RD) design to compare the outcomes of individuals observed 10 years after BDH's implementation using the initial assignment to identify the long term effects. They found a modest intent to treat (ITT) effect of 2 to 3 percentage points increase on high school graduation for women in households that were eligible for transfers when they were in late childhood. Results on secondary school completion for men were smaller in magnitude and were not significant.

Molina-Millan et al. (2016) reviewed the evidence on the long-term impacts of CCTs in Latin America and conclude that, with the exception of schooling, there is little consistent evidence across outcomes for all the programs. Findings from the experimental literature include consistent evidence of impacts on schooling (in Mexico and Nicaragua), as well as some evidence of impacts on cognitive skills and learning (in Nicaragua), socio-emotional skills (in Mexico) and off-farm employment and income (in Nicaragua). The effects on other outcomes are generally not statistically different from zero, but this may be due to lack of power or the small difference in exposure between treatment and control groups, which is a common concern of experimental evaluations in which ethical considerations determine that transfers are not withheld from the control group for too long a period of time.

Considering that there was an important contamination of the original treatment and control groups, in this paper, I study the short and long-term effects of an ongoing unconditional cash transfer program, using administrative panel data to control for individuals' transitions in and out of the program over the years. This allows me to address the question of whether the short term effects of BDH persist or wear off in the long-run. To do this, I take advantage of the fact that BDH uses a proxy means test (PMT) – the Selben index - to select beneficiaries; and,

[1] Other studies about Progresa have used short-term estimates to extrapolate to long-run program impacts (Behrman et al., 2005; Todd and Wolpin, 2006; Attanasio et al., 2010). 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 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).

[2] Some differences between BDH and FA are that FA is a CCT while BDH is not. Furthermore, the size of the transfer in FA is smaller (17 dollars) and varies with the age and number of children in the household. Finally, FA expansion included first, a pilot stage applied to 22 municipalities between December 2000 and March 2001; second, an initial expansion to 306 municipalities between July and November 001; and third, a second expansion stage to include a further 303 municipalities between February and March 2002.

that it changed five years after the implementation of the program. The change to the PMT, which marked the beginning of the second phase of the program, meant that many BDH beneficiaries had to leave the program, while others remained on it for nearly five more years. My identification strategy relies on the fact that at the threshold of eligibility, the second assignment was independent from the first assignment. This allows me to disentangle the short term effects of BDH from the additional effect of five more years of treatment, hereafter the differential effect of a long exposure (ten years) versus a short exposure (five years).

I use a RD design to identify the differential effect of a long exposure versus a short exposure to BDH by comparing the outcomes observed at the end of phase two of children who were treated for 10 years versus children who were treated only for five years. The results are in line with the literature on short term effects of CTs, namely, treated children experience short term gains in enrollment and schooling by the end of phase one (Edmonds and Schady, 2012; Paxson and Schady, 2007; Schady et al., 2008), but those gains disappear after five more years of treatment by the end of phase two. This explains why children aged 18-21 by the end of phase two, who were around 8-11 years when BDH started, were not more likely to finish high school when compared to similar children who were treated only during phase one. In terms of labor market outcomes, BDH had a negative but not statistically significant impact on the probability of working among young children who were treated either during five or ten years and did not increase job opportunities among young adults.

This paper contributes to fill the gap in the literature about the short and long-term effects of social programs that lack a randomized design. 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 poverty index (that changes 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 the results from the RD design. Section 6 discusses some robustness checks using administrative data from a standardized exam taken at the end of high school and Section 7 concludes.

2 Institutional Background

Ecuador 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, 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. 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) 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 ages 5 to 15 years. 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 the Baccalaureate was 42.1%, and it increased considerably to 53.6% in 2008 and to 65.1% in 2014.[7] Much of the increase in the secondary enrollment rate has been attributed to a series of political and institutional changes and to the implementation of social programs like BDH.

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. 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.[8] 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).

[3] Source: World Bank World Development Indicators.

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

[5] Source: World Bank World Development Indicators

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

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

[8] 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.

By 2003, the BS had about 1 million beneficiary households and had been shown to suffer from poor targeting and significant resource leakages (World Bank, 2005). Unlike the BS, the BE was well targeted but its size was relatively small, approximately 150,000 households. Merging the BS and the BE to create the BDH involved two important changes: (i) re-targeting of the beneficiary population using a PMT called 'Selben', and (ii) an adequate system to monitor compliance with the conditions. The re-targeting proceeded smoothly. More than 500,000 households (50% of the original BS beneficiaries) were disqualified and stopped receiving benefits, and another 500,000 households were newly registered and started receiving benefits. By contrast, the monitoring of the conditions was never enforced (World Bank, 2005).

By 2008, BDH was the program with the highest relative coverage rate in LA, covering 40% of the population (Fiszbein et al., 2009). BDH was initially publicized as a CCT; women with children aged 0 to 18 years receiving the transfer were required to take their children for health checkups and to enroll them in school. Compliance with these 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 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 the mothers who were not satisfying the conditions as a first step to sanction non-compliance.[9]

In 2003 the amount of the monthly transfer was US$15 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). The size of the transfer does not depend on the number of children in the household, which may lead parents to choose the son in which they want to invest the most.

BDH uses a proxy means test called the "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 the "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

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

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 score, the same process is reproduced using a nationally representative survey. Once the cutoff is chosen; it is applied to select beneficiaries in the RS.[10]

From 2003 until 2008, hereafter the first phase of the program, BDH targeted women with children aged 0-18 years in the bottom 40% of the Selben distribution, namely, women 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 administration used the new score to select beneficiaries, with a new cutoff point of 36.5 points(Buser, 2015).[11]

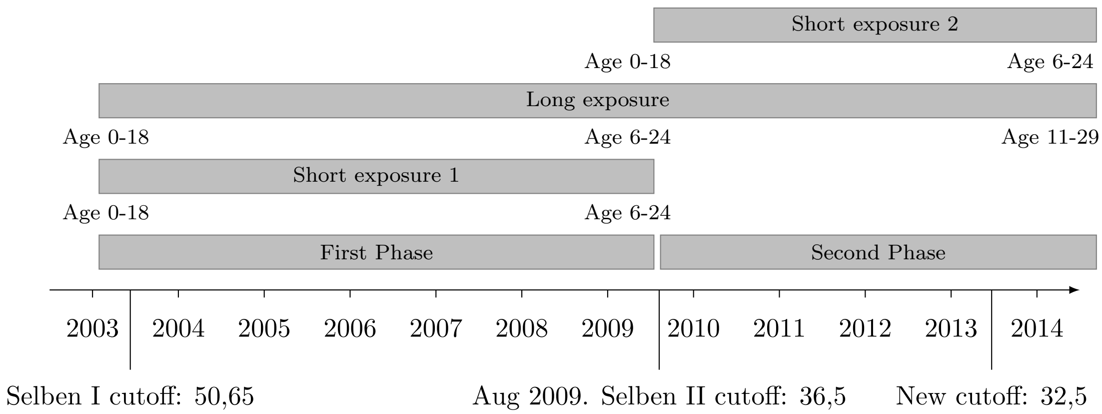
Figure 1 shows the chronology of BDH implementation and, specifically, the change in the targeting rule in 2009 that coincided with the collection of the second wave of the RS and marked the beginning of the second phase of the program. Figure 1 also shows the different lengths of treatment that exist 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 before 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 6 years older and by the end of phase two, they were approximately 11 years older. Their exact age depends on the date they were surveyed in the first, second and third wave of RS.

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

[11] 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.[12] 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. The last wave 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

[12] 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.aspx?source=2&series=SP.POP.TOTL&country=ECU)

out of the survey. The steps taken to build the panel with information from the three waves of the RS is explained in the next section.

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.

Finally, as the third wave of the RS is incomplete, I also use administrative data from the ENES exam for the years 2013 and 2014[13] and merge these data with the panel to estimate the impact of different lengths of exposure to BDH on high school graduation rates.

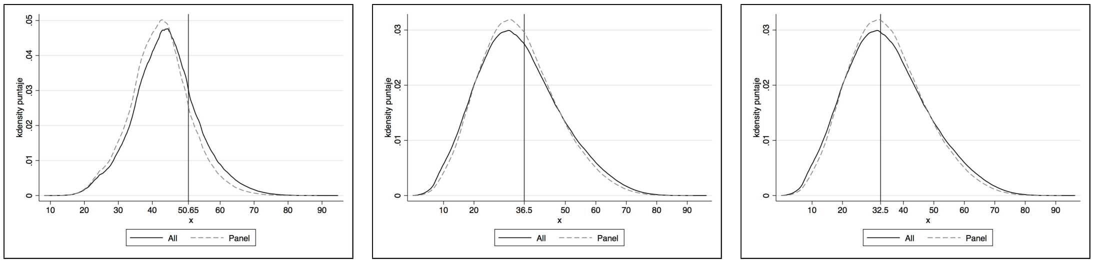
3.1.1 Construction of the panels

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 1,271,538 matches. Considering that each wave of the RS contains around 7 million observations, the amount of data that is lost is large. 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.[14].

By following the children instead of the mothers, I can rule out estimating the long-term impacts on children who did not benefit from the program from the beginning but moved in with a treated mother after a second marriage, and tell when children that stopped living with their mothers stopped receiving the benefits from the transfer. I address these complications by assigning eligibility status to children according to the current conditions of the household they lived in and by assigning the treatment to women who actually claimed the transfer each year. (See the Data Appendix and Paredes-Torres (2016) for a complete description of the process to build the panels).

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

[14] 400,000 individuals in the three-waves panel moved to (or from) other households (Paredes-Torres, 2016)



(a) First wave (b) Second wave (c) Third wave

Figure 2: Kernel density graphs of the Selben score in each wave of the Registro Social and the corresponding panel wave

3.1.2 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 Figure 2. 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. Table A.1 in the Appendix analyzes the link rates for individuals and households for the three-wave panel in more detail. 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.

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 (Paredes-Torres, 2016).

The three-waves panel allows me to track the trajectory of the individuals in terms of their educational attainment and eligibility status. 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

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.

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.

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). With BDH, the probability of treatment does not jump from 0 to 1 when the forcing variable crosses the threshold; in other words, the Selben score predicts substantial but not perfect changes in the probability of treatment. 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 A.1 in the Appendix 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.

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 very 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 and school year dummies because each wave of the RS was collected over more than one year.

4.4 Differential effect of a long exposure vs a short exposure

Eligibility after 2009 was determined by a new Selben II score, which was computed based on individual and household characteristics that were potentially affected by the exposure to BDH in phase one. A woman who was exposed to the transfer in phase one may have improved her poverty score enough to become ineligible in phase two. Since a significant number of individuals changed their eligibility status after the introduction of Selben II in 2009, and, given that the ones who did not change their status are more likely to be different (the poorer are likely to remain eligible during the two phases, while the richer are likely to remain ineligible), I am not able to compare the "always treated" and the "never treated" because their pre-treatment characteristics (and very likely their unobservable characteristics) are very different.

There are two other ways to estimate the long term effects of BDH. The first measures the impact of being treated only during phase one (and not in phase two) versus never being treated. However, this case is problematic because only the initial assignment would have been exogenous to pre-treatment characteristics while the second assignment (at the beginning of phase two) would have been endogenous. To compare these two groups based on the initial assignment, keeping enough observations at the threshold, 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 A.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 and school year dummies in the regressions.

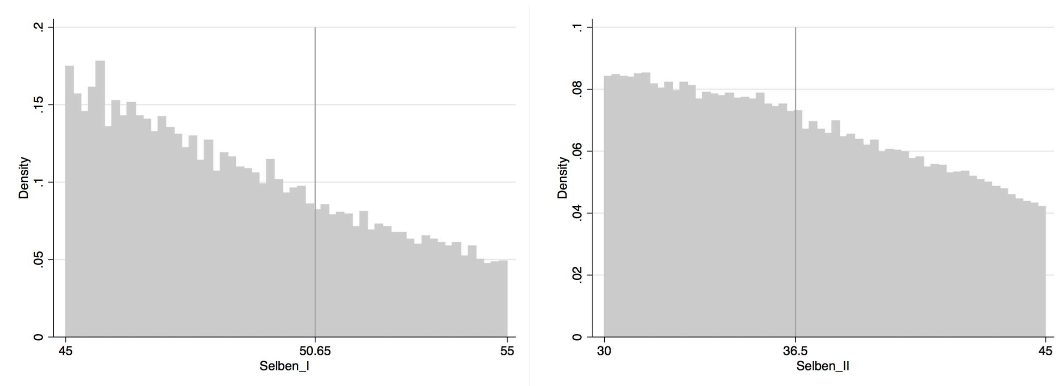
5 Results

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, 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[15] fail to reject the hypothesis that the difference in densities on the two sides of the cutoff is zero (See Table A.2 in the Appendix). 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 if among the former eligible children, 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

[15]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.

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



joint F-tests are reported in Tables A.3, A.4 and A.5 in the Appendix. In all the cases, at least for one of the specifications, the p-value on the omnibus F test was not significant.

5.1 Effects of a short exposure to BDH

Table A.3 in the Appendix 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 (See Table A.6 in the Appendix for results of the linear and quadratic specifications).

Table 2, under the odd numbered columns, reports the results of the IV regressions estimated at the end of phase one. Results of the first stage regressions are reported in Table A.9 of the Appendix, and results of the OLS regressions and ITT effects are reported in Table A.12 of the Appendix. Treated children aged 17 to 20 years by the end of the first phase experienced gains in most of the variables under analysis, and children aged 13 to 16 years increased their enrollment rates. These children were around 8 to 11 and 12 to 15 years old respectively when BDH started. In terms of enrollment, the gain for treated children in the 13 to 16 age range is 3.3 percentage points, while for children in the 17 to 20 age range, the impact is 6.7 percentage points. Both estimates are significant at the 1% level.

Table 2: IV Results. Short term effects of BDH by the end of phase one (odd columns). Differential effects of a long versus a short exposure to BDH measured by the end of phase two (even columns) for different age groups.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Phase I	Phase II	Phase I	Phase II	Phase I	Phase II	Phase I	Phase II	Phase I	Phase II
Variables	Age_5_8	Age_10_13	Age_9_12	Age_14_17	Age_13_16	Age_18_21	Age_17_20	Age_22_25	Age_21_24	Age_26_29
Enrollment	-0.000841	-0.00398	0.00647	0.00715	0.0331***	-0.0390*	0.0669***	0.0609**	0.0140	-0.00995
	(0.00569)	(0.00474)	(0.00459)	(0.0115)	(0.0102)	(0.0209)	(0.0213)	(0.0265)	(0.0252)	(0.0228)
Years of Education	0.0126	-0.0534	0.0845	-0.0494	0.0924	-0.152	0.276**	0.0315	0.0946	-0.158
	(0.0567)	(0.0707)	(0.0551)	(0.0662)	(0.0648)	(0.114)	(0.111)	(0.242)	(0.190)	(0.311)
Job	0.00281	-0.00269	-0.000385	-0.00205	-0.0138	0.0254	-0.0298	-0.0366	-0.0461*	0.0253
	(0.00468)	(0.00348)	(0.00338)	(0.00880)	(0.00901)	(0.0222)	(0.0211)	(0.0328)	(0.0270)	(0.0378)
Tenth grade				-0.0182	0.0214	-0.0112	0.0358**	0.0510*	0.0126	-0.0145
				(0.0164)	(0.0163)	(0.0161)	(0.0156)	(0.0284)	(0.0226)	(0.0386)
High School					-0.000216	-0.00414	0.0618***	0.00647	0.00790	-0.00398
					(0.00544)	(0.0214)	(0.0214)	(0.0335)	(0.0267)	(0.0411)
University						-0.0181	0.0439***	-0.000775	0.0125	-0.0347
						(0.0170)	(0.0169)	(0.0319)	(0.0261)	(0.0323)
Observations	13,836	19,661	24,986	28,159	26,895	20,885	21,181	11,970	16,265	7,458

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.Table reports the results of the IV regressions using a linear polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben cutoff (50,65 points) for "Phase I" columns and within +/-2.5 points from the Selben II cutoff (36.5 points) for "Phase II" columns.

As expected, results at the end of the first phase in terms of years of schooling were positive for the same age groups who experienced gains in enrollment. The group who gained the most is children in the 17 to 20 age range; these children were 12 to 15 years old when BDH started. For them, receiving the transfer during phase one caused an increase of 0.276 of a year, or 55 days more schooling based on a school year of two hundred days.[16] This effect is statistically significant at 5%. These results are in line with a higher probability of graduating from high school for children in the 17-20 age group of 6.2 percentage points (significant at 1% level), and a higher probability of 4.4 percentage points of being enrolled at university (significant at 1% level).

Also in line with the previous results, the likelihood of having a job was negative but not statistically significant for treated children aged 13 to 16 and 17 to 20 years old and negative and significant at the 10% level for children aged 21 to 24 years by the end of phase one.

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

In order to identify the differential effects of a long versus a short exposure to BDH, I tested among children who were treated in phase one, if those who were assigned to be eligible or not in phase two had balanced pre-treatment characteristics around the threshold. Results from the linear and quadratic specification show that those characteristics are well balanced including in terms of the Selben I score, meaning that the assignment in 2009 was independent from the first assignment in 2003. (See Table A.5 in the Appendix). 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. (See Table A.7 in the Appendix for results of the linear and quadratic specifications).

Table 2, under the even numbered columns, reports the differential effects of a long exposure to BDH (during phases one and two) versus a short exposure (during phase one). Results of the first stage regressions are reported in Table A.10 of the Appendix, and results of the OLS regressions and ITT effects are reported in Table A.13 of the Appendix. For the IV regressions, I follow the same cohorts of children that were used to estimate the short term effects of BDH aged 5 years more.[17] In

[16]The school year in Ecuador is composed of two five-month periods and its minimum duration is two hundred days.(Ministerio de Educación, 2012)

[17]The number of observations to estimate the effects by the end of phase one and to estimate the differential effect of a long versus short exposure to BDH are not the same because the number of individuals in the neighborhood around the Selben I cutoff is not exactly the same as the number of individuals close to the Selben II cutoff.

general, the initial positive effects observed at the end of phase one fade out by the end of phase two, except for the children aged 22 to 25 years.

In terms of enrollment, children aged 18 to 21 by the end of the second phase (who were 13-16 years old by the end of phase one) are the ones who saw their progress slow down the most. The reason for this could be that the increases in enrollment by the end of phase one did not translate into more years of schooling for this age group. Moreover, these children may have dropped out school when the families realized that conditions were not being monitored. On the other hand, children aged 22 to 25 still experienced gains in enrollment after five extra years of treatment. These children were 12 to 15 years old in 2003 and became ineligible when they reached 18, which means that most of them were ineligible during the second phase of the program. However, since they had experienced an increase in the number of years of schooling by the end of phase one and had become closer to finishing high school, it is likely that when they became ineligible for the transfer they kept studying because public education became free at the baccalaureate and undergraduate level in 2008.

In terms of years of schooling, five more years of treatment did not render additional gains for treated children. There are no statistically significant gains in terms of the likelihood of graduating from high school for children aged 18 to 21 years, or in the likelihood of finishing tenth grade for children aged 14 to 17 years. There is, however, a higher probability of finishing tenth grade of 5.1 percentage points (statistically significant at 10% level) among children aged 22 to 25. One reason for this could be that after education became free, parents who were receiving the transfer could afford to send their older children back to school, without affecting their younger children who were also able to attend public schools for free.[18] In Ecuador, and particularly in poor families, schooling increases with birth order while child labor decreases with birth order (Haan et al., 2013). It is very likely that children aged 22 to 25 years were the ones who had to drop out of school to find work at an early stage. Furthermore, in 2005 the government implemented a program to promote reintegration, which was successful in reducing the percentage of young people between 19 and 24 years old who did not complete the Baccalaureate from 46.5% in 2006 to 35.29% in 2012 (Mineduc, 2012).

Finally, regarding labor market outcomes, a longer exposure to BDH did not have a statistically significant effect on the probability of having a job among all age groups. The effect was negative for the younger age groups and positive for children of working age. Among children of working age, only those aged 22 to 25 years at the end of phase two experienced a negative effect which coincides with the positive and significant effects found on enrollment for this age group after ten years of

[18] Among children aged 22-25 in wave 3, only 13.45% did not have children below 18 years old living with them.

Table 3: IV Results. Effects of five years of exposure to BDH measured at the end of phase two

Variables	Phase II Age_10_13	Phase II Age_14_17	Phase II Age_18_21	Phase II Age_22_25
Enrollment	-0.00307	0.0160	-0.0661	0.0710
	(0.0146)	(0.0358)	(0.0624)	(0.0471)
Years of Education	-0.156	0.517**	0.370	0.931**
	(0.205)	(0.210)	(0.347)	(0.439)
Job	-0.0110	-0.0312	0.0300	0.0669
	(0.0127)	(0.0264)	(0.0660)	(0.0638)
Tenth Grade		-0.0213	0.0335	0.121**
		(0.0483)	(0.0485)	(0.0547)
High School			0.0170	0.0749
			(0.0636)	(0.0653)
University			0.0188	0.118**
			(0.0522)	(0.0570)
Observations	3,922	5,617	5,530	4,301

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the results of IV 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) who were not treated on phase 1.

treatment.

5.3 Results at the end of phase two among people who were not treated during phase one

Table A.4 in the Appendix 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 omnibus joint F-test favors the linear specification, which is also preferred based on the AIC criterion and the goodness of fit tests. (See Table A.8 in the Appendix for results of the linear and quadratic specifications).

Table 3 reports the short-term impacts of BDH by the end of phase two among children who were not treated during phase one. The results of the first stage regressions are reported in Table A.11 of the Appendix, and the results of the OLS regressions and ITT effects are reported in Table A.14 in the Appendix. Results show a positive effect on the number of years of schooling of 0.93 years, equivalent to 186 more days of schooling for treated children who were 22 to 25 years old by the end of phase two. Treated children in the same age group were also 12.1 percentage points (significant at 5% level) more likely to finish tenth grade and 11.8

percentage points (significant at 5% level) more likely to be enrolled at university. These children were 17 to 20 years at the beginning of phase two and should not have benefited from BDH for more than a year. This points to the earlier discussion about the possibility of parents investing in their older sons/daughters.

Children aged 14-17 years old by the end of phase two (who were around 9-12 years at the beginning of phase two) also saw an increase in their schooling of 0.52 years, equivalent to 104 more days in school. These results suggest that children who were close to finish primary or high school when they joined the program were the ones who responded more to the treatment; however, the gains showed clear delays because poor children face face higher dropout and grade repetition rates and experience delayed enrollment.

As in the previous section, the impact of BDH on the likelihood of having a job was negative for the younger age groups and positive for the older age groups; however, they were not statistically significant.

6 Robustness checks

Given the lack of results found at the end of phase two for high school graduation, and 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) 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. As in the previous section, I run similar IV 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. The results can be found on Tables A.15 and A.16 of the Appendix.

The results are very similar to the ones I obtained in Tables 2 and 3. There is no statistically significant impact on the likelihood of sitting the test for children who were exposed to BDH during the second phase of the program. Neither are there additional gains for being exposed to BDH for five more years for any of the age groups considered (22-25 and 26-29 years). There is only a negative and statistically significant impact on the likelihood of sitting the test for children in the 18-21 age range. It is worth noting that some of the individuals who take the test are adults who want to go to university, which is why I can report results for older age groups and not only for the 18-21 age group.

7 Conclusions

In this paper, I studied 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 people leaving or joining the program as time goes by, which may cause an attenuation of the program's impact and makes it difficult to say if the observed effects were achieved due to a long or a short exposure to the program. The main finding of the paper is that the short term effects of BDH (UCT) were important and similar in magnitude to those found for other CCT programs, but five more years of exposure to BDH did not render additional gains in terms of enrollment and schooling for most of the age groups. Consequently, the short term effects found in this paper should be considered as a lower bound of BDH's long-term effects, in particular for stock variables like years of education or the likelihood of finishing high school.

Thanks to the availability of individual-level administrative data that allowed me to build a series of short and long panels to control for individuals' transitions in and out of the program over the years, I was able to study the long term effects of BDH even in the presence of substantial contamination of the original treatment and control groups at the beginning of phase two. Furthermore, by following the children instead of the mothers, I avoided estimating the long-term effects on children who had not benefited from the program from the beginning, but had moved in with a treated mother after a second marriage. Furthermore, I could identify children that stopped living with their mothers at some point to avoid assigning them a wrong treatment status. Ignoring these family dynamics may cause an attenuation of the real effects of the program.[19]

The results from the IV regressions in this paper showed that the higher effects on enrollment and schooling were achieved by children that were close to complete primary or high school when they first joined the program. The observed short term gains disappeared after five more years of treatment for most age groups except for children aged 22 to 25 by the end of phase two, who started treatment in 2003 at

[19]Paredes-Torres (2016) documents the proportion of the population that left their original households between the first and third wave of the RS and their characteristics.

ages 12 to 15. The fact that education became free up to university level at public establishments could also explain why the effects on enrollment for this age group persisted.

There are several factors that can explain why BDH did not achieve its goal of improving educational attainment consistently in the long run. One is the lack of monitoring of the conditions. It seems plausible that at the beginning of the program people believed that they had to send their children to school to keep receiving the transfer, but with time they discovered that it was not a requirement and only continued to support children who were close to complete primary or high school. 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 could be that the transfer was not big enough to compensate for the wages that children close to complete high school could get in the labor market.

Regarding labor market outcomes, results showed a negative but not statistically significant effect on the probability of working particularly among young children. This is in line with the literature, which reports that cash transfers are a good mechanism for the reduction of child work. However, the positive short term effects found on schooling did not seem to give treated children an advantage in the labor market later on. This could be explained by the size of the samples used and also by the economic crisis that started around 2009 and persisted for several years.

The results from this paper stress the need for a redesign of BDH. Considering that public education is now free, it may be necessary to redirect the objectives of BDH. BDH may now focus on two critical groups: children aged 0-5 years and 15-18 years who are at higher risk of dropping out of school. Transfers should 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).

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A Appendix

Data Appendix

In this section I explain in a very concise way how I built the panel that links the three waves of RS. For more details see Paredes-Torres (2016). To merge individuals without id, who belong to a household where at least one member has a valid id, I used the 4 names (2 names and 2 last names) and a common household id as match keys. I focused first on the merge between waves 1 and 2 and then between waves 2 and 3 to maximize the number of matches in each case. I first merged wave 1 and wave 2 by individual id and build a household id (hhold) that was common to waves 1 and 2. The latter was built by concatenating the household id in wave 1 (idh1) and the household id in wave 2 (idh2). I assigned this new household id (hhold) to all the family members of the person with id in both waves. In this way, I was able to track the complete households and just needed to use names and last names to link the individuals inside those households. This exercise was repeated for waves 2 and 3.

To solve the problem of individuals leaving or joining the household as time goes by. I focused on the sample of households where I observed a change in the number of individuals with id. Then, if in wave 1 for example there was a household with two adults and each one shared the same first part of the common household identifier which is idh1 but not the second part which is idh2 because they got divorced, to identify their children I had to look for them in the two households that were formed after the divorce because it is not clear how many children if any moved with each parent. This is a simplified example of a household that separates into two but in the data, there are many possible scenarios and it was necessary to try all possible combinations. As before, this step was repeated for waves 2 and 3.

Finally, to assess the quality of the linkage, I used the acceptance sampling approach and followed the ANSI AQL tables to choose the size of the samples that were checked manually using the Stata command <clrevmatch>.

Table A.1: Number of individuals in the tree-wave panel with respect to the total on each of the three waves

	Total	**Likely linked**	**%**	**Actually linked**	**% Actual/Likely**
First wave of the RS					
househ.	1,583,617	1,022,164	65	1,068,188	105
indiv.	6,302,861	4,221,610	67	2,961,079	70
Second wave of the RS					
househ.	1,910,165	967,454	51	1,036,012	107
indiv.	8,068,957	4,447,300	55	2,961,079	67
Third wave of the RS					
househ.	1,758,401	984,356	56	1,179,668	120
indiv.	6,930,712	4,181,534	60	2,961,079	71

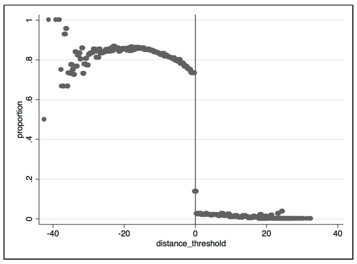
Notes: (i) Each wave has a total of 6.3, 8 and 6.9 million individuals respectively. Among them around 4.2, 4.5 and 4.2 million individuals belong to a family where at least one member has an id (called the "likely to be linked" sample). The individuals in the final three wave panel represent 70%, 67% and 71% of the "likely to be linked" sample on each wave. (ii) With households, the link rate is higher and exceeds 100% because over the years some households split, which increases the number of households. (iii) The size of the three-wave panel is bounded by the number of matches between waves 1 and 2 because less people reported an id in wave 1; however, there are other 1,26 million individuals that could be tracked through waves 1 and 2 but not through waves 1, 2 and 3; and other 1,48 million individuals that could be tracked only through waves 2 and 3.

Table A.2: Density tests for Selben I and Selben II

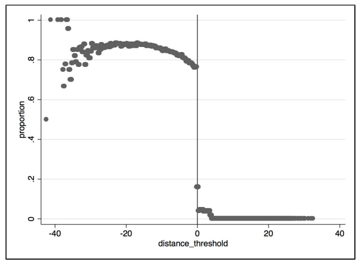
	left of SelbenI cuttof	right of SelbenI cuttof	left of SelbenII cuttof	right of SelbenII cuttof
Observations	651103	171121	555666	244332
Effective Observations	43763	36769	39043	40559
Bias corrected density	0.03	0.03	0.02	0.02
Standard error	0.00	0.00	0.00	0.00
Bandwidth values	1.54	1.55	2.00	2.27
Standard 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.

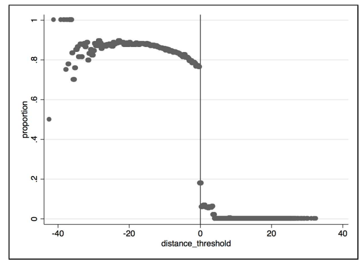
Figure A.1: Proportion of treated households with respect to the threshold by year



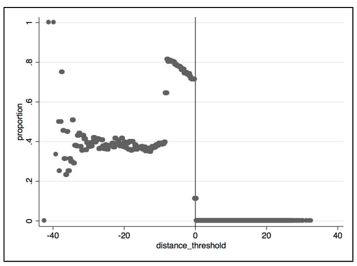
(a) 2005



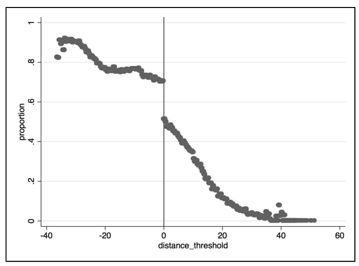
(b) 2006



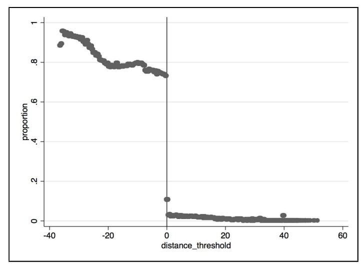
(c) 2007



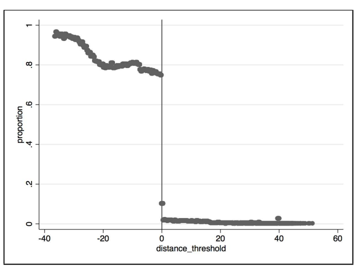
(d) 2008



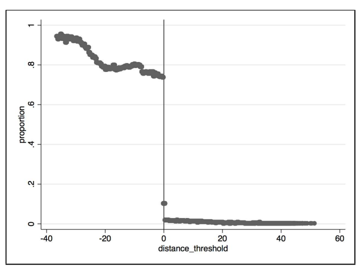
(e) 2009



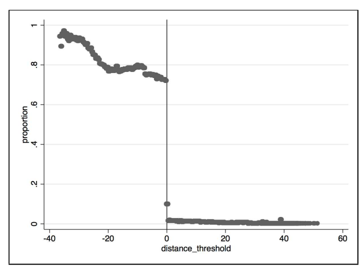
(f) 2010



(g) 2011



(h) 2012



(i) 2013

Notes: Graphs use RS data merged with administrative payments data. Sample is the same households in the balance tests. 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 data set contains 676,068 individuals while the 2007 and 2009 data sets contain 1,127,909 and 1,280,367 individuals respectively.

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

Variables	Pt_Est_1	Std_Err_1	Pt_Est_2	Std_Err_2
land	-0.0117**	(0.00546)	-0.0180**	(0.00818)
electricity	0.00138*	(0.000731)	0.000621	(0.00110)
no_exclusive_shower	0.00343	(0.00300)	0.00270	(0.00459)
Overcrowding	0.000363	(0.00630)	-0.000143	(0.00950)
members	0.00916	(0.0188)	0.00474	(0.0283)
totearners	0.000235	(0.0106)	-0.0111	(0.0159)
tothholdwork	0.00771	(0.00701)	0.000952	(0.0105)
below18	0.00678	(0.0132)	0.0286	(0.0199)
totstudy	0.0114	(0.0119)	0.0271	(0.0179)
educ_level_head	0.0190*	(0.0109)	0.0105	(0.0164)
job_head	0.00901**	(0.00451)	0.00267	(0.00677)
native_language_head	-0.000442	(0.00223)	5.01e-05	(0.00334)
retired_head	-0.000450	(0.000859)	0.000284	(0.00122)
years_educ_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.

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

Variables	Pt_Est_1	Std_Err_1	Pt_Est_2	Std_Err_2
selben1	-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)
no_exclusive_shower	0.00153	(0.00847)	-0.00725	(0.0126)
Overcrowding	-0.00290	(0.00856)	-0.00510	(0.0128)
members	0.0220	(0.0382)	0.0683	(0.0575)
totearners	-0.00413	(0.0159)	0.0280	(0.0234)
tothholdwork	-0.00189	(0.0140)	0.0120	(0.0211)
below18	0.0238	(0.0287)	0.0357	(0.0431)
totstudy	0.0161	(0.0268)	0.00128	(0.0402)
educ_level_head	-0.0176	(0.0387)	0.0198	(0.0583)
job_head	-0.000451	(0.00970)	0.0203	(0.0147)
native_language_head	-0.000207	(0.00365)	-0.000543	(0.00540)
retired_head	-0.00123	(0.00126)	-0.00281*	(0.00163)
years_educ_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.

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

Variables	Pt_Est_1	Std_Err_1	Pt_Est_2	Std_Err_2
selben1	-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)
no_exclusive_shower	-0.00264	(0.00495)	0.00254	(0.00737)
Overcrowding	-0.000476	(0.00548)	0.00724	(0.00818)
members	-0.0115	(0.0251)	-0.000694	(0.0375)
totearners	-0.00601	(0.0105)	-0.00974	(0.0157)
tothholdwork	-0.00797	(0.00879)	0.00444	(0.0132)
below18	-0.0155	(0.0193)	-0.0133	(0.0287)
totstudy	-0.00847	(0.0171)	-0.0134	(0.0254)
educ_level_head	-0.0236	(0.0225)	-0.0424	(0.0337)
job_head	-0.00344	(0.00606)	-0.000756	(0.00902)
native_language_head	-0.000198	(0.00226)	0.000904	(0.00323)
retired_head	0.000573	(0.000592)	0.000565	(0.000881)
years_educ_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.

Table A.6: IV regressions. Effect of a short exposure to BDH (during phase 1)

Variables	Lin_5_8	Quad_5_8	Lin_9_12	Quad_9_12	Lin_13_16	Quad_13_16	Lin_17_20	Quad_17_20	Lin_21_24	Quad_21_24
Enrollment	-0.000841	-0.00564	0.00647	-0.00308	0.0331***	0.0154	0.0669***	0.0946***	0.0140	-0.0450
	(0.00569)	(0.00841)	(0.00459)	(0.00640)	(0.0102)	(0.0151)	(0.0213)	(0.0331)	(0.0252)	(0.0385)
Constant	1.021***	1.026***	0.985***	0.991***	0.924***	0.952***	0.463	0.456	0.219	0.200
Years of Education	0.0126	0.0642	0.0845	0.0105	0.0924	0.0267	0.276**	0.415**	0.0946	-0.0596
	(0.0567)	(0.0846)	(0.0551)	(0.0795)	(0.0648)	(0.0943)	(0.111)	(0.170)	(0.190)	(0.288)
Constant	6.844***	6.810***	9.447***	9.484***	6.853**	7.078**	11.83***	11.81***	9.933***	9.882***
Job	0.00281	0.00521	-0.000385	0.000994	-0.0138	0.00353	-0.0298	-0.0627*	-0.0461*	0.0167
	(0.00468)	(0.00668)	(0.00338)	(0.00470)	(0.00901)	(0.0133)	(0.0211)	(0.0329)	(0.0270)	(0.0413)
Constant	-0.0182***	-0.0207**	0.00293	0.00333	0.0216	-0.00253	0.636**	0.643**	0.931***	0.950***
Tenth Grade			0.00304	0.00759	0.0214	-0.000109	0.0358**	0.0470*	0.0126	-0.00727
			(0.00327)	(0.00469)	(0.0163)	(0.0242)	(0.0156)	(0.0245)	(0.0226)	(0.0344)
Constant			0.557	0.554	1.025***	1.103***	0.950***	0.948***	0.841***	0.835***
High School					-0.000216	-0.00489	0.0618***	0.0941***	0.00790	-0.0346
					(0.00544)	(0.00810)	(0.0214)	(0.0332)	(0.0267)	(0.0407)
Constant					0.550	0.550	0.763***	0.758***	0.850***	0.837***
University					0.000527	-0.00521	0.0439***	0.00975	0.0125	-0.00743
					(0.00362)	(0.00525)	(0.0169)	(0.0260)	(0.0261)	(0.0397)
Constant					-0.00440	0.00156	0.763***	0.764***	0.430*	0.423*
Observations	13,836	13,836	24,986	24,986	26,895	26,895	21,181	21,181	16,265	16,265

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the results of IV regressions at the end of phase 1 for different age groups. Regressions include linear and quadratic polynomials of the distance to the cutoff. Sample is children with Selben score within +/-2.5 points from the Selben cutoff (50.65 points). The P-values from goodness of fit test are shown after standard errors. The goodness of fit test is obtained by jointly testing the significance of a set of bin dummies included as additional regressors in the model. The optimal order of the polynomial is chosen using Akaike's criterion (penalized cross-validation).

Table A.7: IV regressions. Differential effect of a long exposure (phases 1 and 2) versus a short exposure (phase 1)

Variables	Lin_10_13	Quad_10_13	Lin_14_17	Quad_14_17	Lin_18_21	Quad_18_21	Lin_22_25	Quad_22_25	Lin_26_29	Quad_26_29
Enrollment	-0.00398	-0.00288	0.00715	0.0260	-0.0390*	-0.0627	0.0609**	0.0913*	-0.00995	-0.0490
	(0.00474)	(0.00862)	(0.0115)	(0.0209)	(0.0209)	(0.0382)	(0.0265)	(0.0507)	(0.0228)	(0.0423)
Constant	0.992***	0.993***	0.836***	0.819***	0.325***	0.343***	0.144***	0.129***	0.00343	0.0281
Years of Education	-0.0534	-0.0801	-0.0494	-0.125	-0.152	-0.417**	0.0315	-0.498	-0.158	-0.390
	(0.0707)	(0.127)	(0.0662)	(0.121)	(0.114)	(0.205)	(0.242)	(0.471)	(0.311)	(0.559)
Constant	6.322***	6.340***	9.174***	9.227***	10.51***	10.71***	10.63***	10.92***	8.687***	8.774***
Job	-0.00269	-0.00213	-0.00205	-0.00445	0.0254	0.0989**	-0.0366	-0.0232	0.0253	0.0234
	(0.00348)	(0.00651)	(0.00880)	(0.0159)	(0.0222)	(0.0407)	(0.0328)	(0.0629)	(0.0378)	(0.0680)
Constant	0.00705	0.00641	0.117***	0.121***	0.489***	0.440***	0.694***	0.690***	0.846***	0.852***
Tenth Grade			-0.0182	-0.0143	-0.0112	-0.0350	0.0510*	-0.00721	-0.0145	-0.0695
			(0.0164)	(0.0301)	(0.0161)	(0.0289)	(0.0284)	(0.0550)	(0.0386)	(0.0689)
Constant			0.672***	0.667***	0.733***	0.753***	0.645***	0.671***	0.427***	0.446***
High School					-0.00414	-0.0251	0.00647	-0.0818	-0.00398	-0.0148
					(0.0214)	(0.0388)	(0.0335)	(0.0660)	(0.0411)	(0.0737)
Constant					0.605***	0.612***	0.624***	0.677***	0.359***	0.366***
University					-0.0181	-0.0397	-0.000775	0.00514	-0.0347	-0.0645
					(0.0170)	(0.0308)	(0.0319)	(0.0611)	(0.0323)	(0.0585)
Constant					0.170***	0.185***	0.225***	0.230***	0.0945*	0.109*
Observations	19,661	19,661	28,159	28,159	20,885	20,885	11,970	11,970	7,458	7,458

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the results of IV regressions at the end of phase 2 for different age groups. Regressions include linear and quadratic polynomials of the distance to the cutoff. Sample is children with Selben score within +/-2.5 points from the Selben II cutoff (36.5 points) who were eligible on phase 1. The P-values from goodness of fit test are shown after standard errors. The goodness of fit test is obtained by jointly testing the significance of a set of bin dummies included as additional regressors in the model. The optimal order of the polynomial is chosen using Akaike's criterion (penalized cross-validation).

Table A.8: IV regressions. Effect of a short exposure to BDH (during phase 2 only) among untreated children in phase 1

Variables	Lin_10_13	Quad_10_13	Lin_14_17	Quad_14_17	Lin_18_21	Quad_18_21	Lin_22_25	Quad_22_25
Enrollment	-0.00307	0.0225	0.0160	0.0862	-0.0661	-0.177	0.0710	0.136
	(0.0146)	(0.0290)	(0.0358)	(0.0575)	(0.0624)	(0.111)	(0.0471)	(0.0868)
Constant	1.005***	0.987***	0.958***	0.936***	0.474***	0.623***	0.0660	0.0424
Years of Education	-0.156	-0.274	0.517**	0.615*	0.370	0.310	0.931**	2.134**
	(0.205)	(0.466)	(0.210)	(0.340)	(0.347)	(0.569)	(0.439)	(0.872)
Constant	6.019***	6.127***	9.002***	8.995***	10.94***	10.91***	9.412***	8.976***
Job	-0.0110	-0.0171	-0.0312	-0.0748*	0.0300	0.00241	0.0669	0.237*
	(0.0127)	(0.0280)	(0.0264)	(0.0418)	(0.0660)	(0.108)	(0.0638)	(0.125)
Constant	0.00958	0.0166	0.0890*	0.102*	0.280***	0.324**	0.645***	0.583***
Tenth Grade			-0.0213	-0.0488	0.0335	0.0558	0.121**	0.254**
			(0.0483)	(0.0775)	(0.0485)	(0.0804)	(0.0547)	(0.106)
Constant			0.549***	0.558***	0.788***	0.748***	0.582***	0.534***
High School					0.0170	0.0552	0.0749	0.286**
					(0.0636)	(0.104)	(0.0653)	(0.133)
Constant					0.620***	0.579***	0.343***	0.267*
University					0.0188	0.00181	0.118**	0.208*
					(0.0522)	(0.0858)	(0.0570)	(0.108)
Constant					0.154**	0.174*	0.0709	0.0383
Observations	3,922	3,922	5,617	5,617	5,530	5,530	4,301	4,301

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the results of IV regressions at the end of phase 2 for different age groups. Regressions include linear and quadratic polynomials of the distance to the cutoff. Sample is children with Selben score within +/-2.5 points from the Selben cutoff (36.5 points) who were ineligible on phase 1. The P-values from goodness of fit test are shown after standard errors. The goodness of fit test is obtained by jointly testing the significance of a set of bin dummies included as additional regressors in the model. The optimal order of the polynomial is chosen using Akaike's criterion (penalized cross-validation).

Table A.9: First stage regressions. Short exposure to BDH (during phase 1)

Regression on:	Age_5_8	Age_9_12	Age_13_16	Age_17_20
Enrollment	0.740***	0.773***	0.783***	0.685***
	(0.0114)	(0.00806)	(0.00797)	(0.0102)
R2	0.567	0.607	0.618	0.497
Years of Education	0.740***	0.773***	0.783***	0.685***
	(0.0114)	(0.00806)	(0.00797)	(0.0102)
R2	0.567	0.607	0.618	0.497
Job	0.740***	0.773***	0.783***	0.685***
	(0.0114)	(0.00806)	(0.00797)	(0.0102)
R2	0.567	0.607	0.618	0.497
Tenth Grade		0.773***	0.783***	0.685***
		(0.00806)	(0.00797)	(0.0102)
R2		0.607	0.618	0.497
High School			0.783***	0.685***
			(0.00797)	(0.0102)
R2			0.618	0.497
University			0.783***	0.685***
			(0.00797)	(0.0102)
R2			0.618	0.497
N	13,848	24,986	26,895	21,181

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the instrument on treatment probability (first stage regressions) for each of the outcome variables for which IV regressions were estimated. Sample is children with Selben score within +/-2.5 points from the Selben cutoff (50.65 points). Regressions include a linear polynomial of the distance to the cutoff.

Table A.10: First stage regressions. Differential effect of a long exposure (phases 1 and 2) versus a short exposure (phase 1)

Regression on:	Age_10_13	Age_14_17	Age_18_21	Age_22_25
Enrollment	0.739***	0.720***	0.629***	0.517***
	(0.0105)	(0.00798)	(0.0108)	(0.0156)
R2	0.767	0.713	0.509	0.385
Years of Education	0.739***	0.720***	0.629***	0.517***
	(0.0105)	(0.00798)	(0.0108)	(0.0156)
R2	0.767	0.713	0.509	0.385
Job	0.739***	0.720***	0.629***	0.517***
	(0.0105)	(0.00798)	(0.0108)	(0.0156)
R2	0.767	0.713	0.509	0.385
Tenth Grade		0.720***	0.629***	0.517***
		(0.00798)	(0.0108)	(0.0156)
R2		0.713	0.509	0.385
High School			0.629***	0.517***
			(0.0108)	(0.0156)
R2			0.509	0.385
University			0.629***	0.517***
			(0.0108)	(0.0156)
R2			0.509	0.385
N	19,662	28,159	20,885	11,970

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the instrument on treatment probability (first stage regressions) for each of the outcome variables for which IV regressions were estimated. Sample is children with Selben score within +/-2.5 points from the Selben II cutoff (36.5 points) who were treated on phase 1. Regressions include linear and quadratic polynomials of the distance to the cutoff.

Table A.11: First stage regressions. Effect of a short exposure to BDH (during phase 2 only) among untreated children in phase 1

Regression on:	Age_10_13	Age_14_17	Age_18_21	Age_22_25
Enrollment	0.613***	0.550***	0.410***	0.463***
	(0.0301)	(0.0224)	(0.0238)	(0.0265)
R2	0.544	0.457	0.309	0.319
Years of Education	0.613***	0.550***	0.410***	0.463***
	(0.0301)	(0.0224)	(0.0238)	(0.0265)
R2	0.544	0.457	0.309	0.319
Job	0.613***	0.550***	0.410***	0.463***
	(0.0301)	(0.0224)	(0.0238)	(0.0265)
R2	0.544	0.457	0.309	0.319
Tenth Grade		0.550***	0.410***	0.463***
		(0.0224)	(0.0238)	(0.0265)
R2		0.457	0.309	0.319
High School			0.410***	0.463***
			(0.0238)	(0.0265)
R2			0.309	0.319
University			0.410***	0.463***
			(0.0238)	(0.0265)
R2			0.309	0.319
N	3,923	5,617	5,530	4,301

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the instrument on treatment probability (first stage regressions) for each of the outcome variables for which IV regressions were estimated. Sample is children with Selben score within +/-2.5 points from the Selben cutoff (36.5 points) who were untreated on phase 1. Regressions include linear and quadratic polynomials of the distance to the cutoff.

Table A.12: OLS, ITT and IV estimates of the effects of a short exposure to BDH (during phase 1)

	OLS	OLS	OLS	OLS	ITT	ITT	ITT	ITT	IV	IV	IV	IV
Variables	9_12	13_16	17_20	21_24	9_12	13_16	17_20	21_24	9_12	13_16	17_20	21_24
Enrollment	0.00172	0.00310	0.0124*	-0.000615	0.00499	0.0259***	0.0460***	0.00835	0.00647	0.0331***	0.0669***	0.0140
	(0.00179)	(0.00386)	(0.00719)	(0.00710)	(0.00356)	(0.00800)	(0.0147)	(0.0152)	(0.00459)	(0.0102)	(0.0213)	(0.0252)
Constant	0.988***	0.650***	0.381***	-0.0848*	0.991***	0.975***	0.395	0.224	0.985***	0.924***	0.463	0.219
Years Educ.	-0.0225	-0.121***	-0.233***	-0.498***	0.0676	0.0723	0.190**	0.0560	0.0845	0.0924	0.276**	0.0946
	(0.0211)	(0.0255)	(0.0378)	(0.0554)	(0.0426)	(0.0508)	(0.0766)	(0.114)	(0.0551)	(0.0648)	(0.111)	(0.190)
Constant	5.290***	9.976***	10.99***	10.24***	9.149***	6.777**	11.71***	10.39***	9.447***	6.853**	11.83***	9.933***
Job	-0.000108	0.00713**	0.0269***	0.0307***	-0.000305	-0.0108	-0.0207	-0.0275*	-0.000385	-0.0138	-0.0298	-0.0461*
	(0.00135)	(0.00340)	(0.00709)	(0.00763)	(0.00262)	(0.00708)	(0.0146)	(0.0162)	(0.00338)	(0.00901)	(0.0211)	(0.0270)
Constant	0.00269	0.225***	0.614***	0.132***	0.00401*	0.00738	0.606**	0.876***	0.00293	0.0216	0.636**	0.931***
Tenth Grade		-0.0276***	-0.0284***	-0.0540***		0.0183***	0.0238*	0.00386		0.0214	0.0358**	0.0126
		(0.00627)	(0.00533)	(0.00664)		(0.00569)	(0.0121)	(0.00982)		(0.0163)	(0.0156)	(0.0226)
Constant		0.841***	0.828***	1.042***		1.024***	0.995***	0.971***		1.025***	0.950***	0.841***
High School			-0.0384***	-0.0546***			0.0401***	-0.00234			0.0618***	0.00790
			(0.00719)	(0.00768)			(0.00881)	(0.00888)			(0.0214)	(0.0267)
Constant			0.706***	1.003***			0.698**	0.968***			0.763***	0.850***
University			-0.0312***	-0.0423***			0.0304***	0.00745			0.0439***	0.0125
			(0.00576)	(0.00733)			(0.0116)	(0.0157)			(0.0169)	(0.0261)
Constant			0.305***	-0.0959*			0.742***	0.440*			0.763***	0.430*
N	25,237	27,160	21,478	17,845	24,986	26,895	21,181	16,265	24,986	26,895	21,181	16,265

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the OLS, ITT and IV estimates of BDH's impact at the end of phase 1 for different age groups. Regressions use a linear polynomial of the distance to the cutoff. Sample is children with Selben score within +/-2.5 points from the Selben cutoff (50.65 points). The P-values from goodness of fit test are shown after standard errors. The goodness of fit test is obtained by jointly testing the significance of a set of bin dummies included as additional regressors in the model. The optimal order of the polynomial is chosen using Akaike's criterion (penalized cross-validation).

Table A.13: OLS, ITT and IV estimates of the effect of a long exposure (phases 1 and 2) versus a short exposure (phase 1)

	OLS	OLS	OLS	OLS	OLS	ITT	ITT	ITT	ITT	ITT	IV	IV	IV	IV	IV
Variables	10_13	14_17	18_21	22_25	26_29	10_13	14_17	18_21	22_25	26_29	10_13	14_17	18_21	22_25	26_29
Enrollment	0.00345	0.0873***	0.136***	0.0996***	0.0303***	-0.00353	0.00347	-0.0265**	0.0331***	-0.00148	-0.00398	0.00715	-0.0390*	0.0609**	-0.00995
	(0.00314)	(0.00687)	(0.00674)	(0.00557)	(0.00530)	(0.00349)	(0.00797)	(0.0124)	(0.0124)	(0.0118)	(0.00474)	(0.0115)	(0.0209)	(0.0265)	(0.0228)
Constant	0.986***	0.782***	0.184***	0.0785**	0.0195	0.989***	0.828***	0.332***	0.122***	0.0452**	0.992***	0.836***	0.325***	0.144***	0.00343
Years Educ.	0.0561	0.0564*	0.428***	0.336***	-0.536***	-0.0580	0.00558	-0.0591	0.132	0.0184	-0.0534	-0.0494	-0.152	0.0315	-0.158
	(0.0414)	(0.0334)	(0.0400)	(0.0548)	(0.0751)	(0.0504)	(0.0458)	(0.0684)	(0.114)	(0.157)	(0.0707)	(0.0662)	(0.114)	(0.242)	(0.311)
Constant	6.257***	9.022***	10.05***	9.758***	8.833***	6.378***	9.093***	11.03***	11.01***	9.981***	6.322***	9.174***	10.51***	10.63***	8.687***
Job	0.000443	-0.0268***	-0.0235***	-0.00123	-0.0838***	-0.00492*	-0.00217	0.00952	-0.00690	0.0281	-0.00269	-0.00205	0.0254	-0.0366	0.0253
	(0.00206)	(0.00513)	(0.00785)	(0.00755)	(0.00889)	(0.00267)	(0.00612)	(0.0132)	(0.0158)	(0.0194)	(0.00348)	(0.00880)	(0.0222)	(0.0328)	(0.0378)
Constant	0.00468	0.129***	0.519***	0.692***	0.832***	0.00973***	0.0559***	0.439***	0.688***	0.701***	0.00705	0.117***	0.489***	0.694***	0.846***
Tenth Grd.		0.0137	0.0502***	0.0172**	-0.0663***		-0.0128	-0.00206	0.0355***	-0.00727		-0.0182	-0.0112	0.0510*	-0.0145
		(0.00847)	(0.00619)	(0.00674)	(0.00927)		(0.0113)	(0.00980)	(0.0137)	(0.0198)		(0.0164)	(0.0161)	(0.0284)	(0.0386)
Constant		0.623***	0.686***	0.591***	0.477***		0.608***	0.835***	0.745***	0.679***		0.672***	0.733***	0.645***	0.427***
High School			0.0865***	0.0446***	-0.0648***			-0.00101	0.0120	0.00922			-0.00414	0.00647	-0.00398
			(0.00775)	(0.00792)	(0.0101)			(0.0129)	(0.0162)	(0.0213)			(0.0214)	(0.0335)	(0.0411)
Constant			0.516***	0.464***	0.374***			0.635***	0.620***	0.475***			0.605***	0.624***	0.359***
University			0.0595***	0.0816***	-0.0142*			-0.00801	0.00944	-0.00409			-0.0181	-0.000775	-0.0347
			(0.00543)	(0.00698)	(0.00815)			(0.0101)	(0.0150)	(0.0168)			(0.0170)	(0.0319)	(0.0323)
Constant			0.0951***	0.136***	0.124***			0.175***	0.217***	0.149***			0.170***	0.225***	0.0945*
N	21,991	31,476	23,822	16,798	11,082	21,800	31,197	23,508	14,297	8,947	19,661	28,159	20,885	11,970	7,458

Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the OLS, ITT and IV estimates of BDH's impact at the end of phase 2 for different age groups. Regressions use a linear polynomial of the distance to the cutoff. Sample is children with Selben score within +/-2.5 points from the Selben cutoff (36.5 points) who were eligible on phase 1 for the ITT estimates and children with Selben score within +/-2.5 points from the Selben cutoff (36.5 points) who were treated on phase 1 for the IV and OLS regressions.. The P-values from goodness of fit test are shown after standard errors. The goodness of fit test is obtained by jointly testing the significance of a set of bin dummies included as additional regressors in the model. The optimal order of the polynomial is chosen using Akaike's criterion (penalized cross-validation).

Table A.14: OLS, ITT and IV estimates of the effects of a short exposure to BDH (during phase 2 only) among untreated children in phase 1

Variables	OLS 10_13	OLS 14_17	OLS 18_21	OLS 22_25	ITT 10_13	ITT 14_17	ITT 18_21	ITT 22_25	IV 10_13	IV 14_17	IV 18_21	IV 22_25
Enrollment	-0.00459	-0.00735	-0.00956	-0.00734	-0.000251	0.0195	-0.0115	0.0290	-0.00307	0.0160	-0.0661	0.0710
	(0.00479)	(0.0135)	(0.0186)	(0.0168)	(0.00879)	(0.0281)	(0.0375)	(0.0386)	(0.0146)	(0.0358)	(0.0624)	(0.0471)
Constant	1.007***	0.938***	0.523***	0.112	0.988***	0.885***	0.456***	0.160**	1.005***	0.958***	0.474***	0.0660
Years Educ.	-0.774***	-0.0940	-0.205**	-0.685***	-0.0953	0.161	0.0211	0.0135	-0.156	0.517**	0.370	0.931**
	(0.0771)	(0.0873)	(0.0963)	(0.147)	(0.161)	(0.168)	(0.195)	(0.318)	(0.205)	(0.210)	(0.347)	(0.439)
Constant	5.858***	10.70***	12.58***	12.03***	5.965***	10.27***	12.01***	12.06***	6.019***	9.002***	10.94***	9.412***
Job	0.00250	0.000702	0.0184	-0.0330	0.00719	-0.0310	0.0579	-0.00222	-0.0110	-0.0312	0.0300	0.0669
	(0.00393)	(0.00977)	(0.0192)	(0.0208)	(0.00866)	(0.0196)	(0.0389)	(0.0467)	(0.0127)	(0.0264)	(0.0660)	(0.0638)
Constant	-0.00183	0.185**	0.0840	0.519***	0.0109	0.110**	0.293***	0.648***	0.00958	0.0890*	0.280***	0.645***
Tenth Grade		-0.0123	-0.00803	-0.0963***		-0.00954	-0.00554	0.0251		-0.0213	0.0335	0.121**
		(0.0192)	(0.0119)	(0.0168)		(0.0384)	(0.0235)	(0.0369)		(0.0483)	(0.0485)	(0.0547)
Constant		0.768***	0.991***	0.911***		0.746***	0.969***	0.862***		0.549***	0.788***	0.582***
High School			-0.00955	-0.118***			-0.00423	0.00802			0.0170	0.0749
			(0.0179)	(0.0200)			(0.0352)	(0.0446)			(0.0636)	(0.0653)
Constant			0.903***	0.795***			0.801***	0.717***			0.620***	0.343***
University			-0.0415**	-0.0443**			-0.00359	0.0288			0.0188	0.118**
			(0.0165)	(0.0197)			(0.0338)	(0.0438)			(0.0522)	(0.0570)
Constant			0.328***	0.257**			0.298***	0.290***			0.154**	0.0709
Observations	1,813	2,663	3,030	2,482	1,783	2,579	2,907	1,974	3,922	5,617	5,530	4,301

Notes: Robust standard errors clustered at county level shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Table reports the OLS, ITT and IV estimates of BDH's impact at the end of phase 2 for different age groups. Regressions use a linear polynomial of the distance to the cutoff. Sample is children with Selben score within +/-2.5 points from the Selben cutoff (36.5 points) who were ineligible on phase 1 for the ITT estimates and children with Selben score within +/-2.5 points from the Selben cutoff (36.5 points) who were untreated on phase 1 for the IV and OLS regressions. The P-values from goodness of fit test are shown after standard errors. The goodness of fit test is obtained by jointly testing the significance of a set of bin dummies included as additional regressors in the model. The optimal order of the polynomial is chosen using Akaike's criterion (penalized cross-validation).

Table A.15: Effects of a five year exposure to BDH measured at the end of phase two using ENES data

VARIABLES	Lin_18_21	Quad_18_21	Lin_22_25	Quad_22_25
Took the ENES	-0.0436	-0.116	0.0447	0.0417
	(0.0609)	(0.103)	(0.0370)	(0.0674)
Constant	0.343***	0.369***	0.0758	0.0779
Observations	5,530	5,530	4,301	4,301

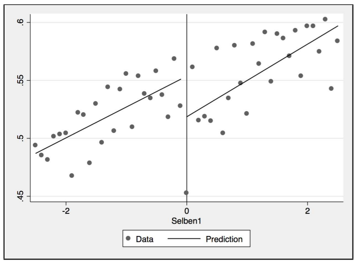
Notes: Robust standard errors clustered at county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Table reports the coefficients of IV regressions using a linear and a quadratic polynomial of the distance to the cutoff. Sample is children with a score within +/-2.5 points from the Selben II cutoff (36.5 points) who were untreated on phase 1.

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

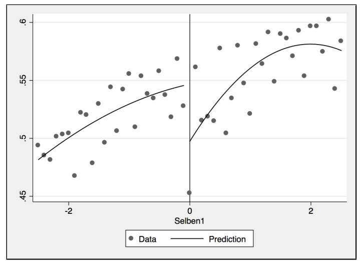
VARIABLES	Lin_18_21	Quad_18_21	Lin_22_25	Quad_22_25	Lin_26_29	Quad_26_29
Took the ENES	-0.0596***	-0.0622*	0.00548	-0.0253	-0.00415	0.0176
	(0.0200)	(0.0360)	(0.0210)	(0.0410)	(0.0171)	(0.0313)
Constant	0.323***	0.313***	0.0769***	0.0950***	0.0329	0.0141
Observations	20,885	20,885	11,970	11,970	7,458	7,458

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 IV regressions using a linear and a quadratic 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) who were treated on phase 1.

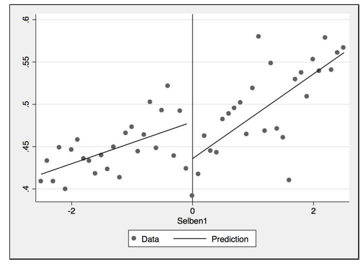
Figure A.2: Impact of BDH exposure on different variables measured at the end of phase 1, phase 2 and differential impact of long (10 years) vs short exposure (5 years).



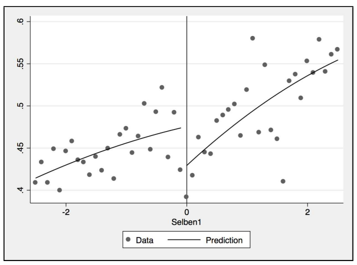
(a) Impact on enrollment. Children 17-20 by end of phase 1 (Linear)



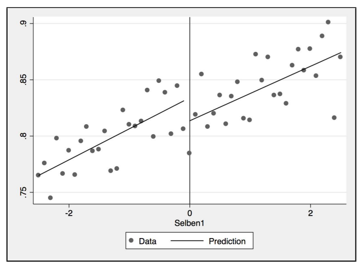
(b) Impact on enrollment. Children 17-20 by end of phase 1 (Quadratic)



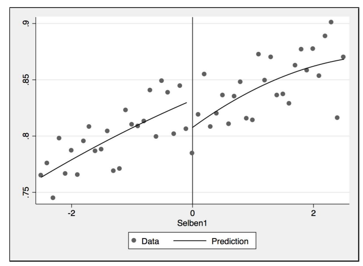
(c) Impact on HS graduation. Children 17-20 by end of phase 1 (Linear)



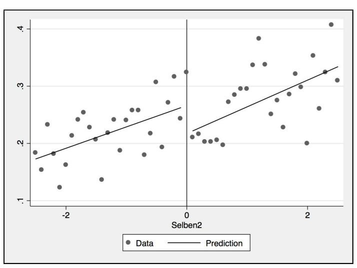
(d) Impact on HS graduation. Children 17-20 by end of phase 1 (Quadratic)



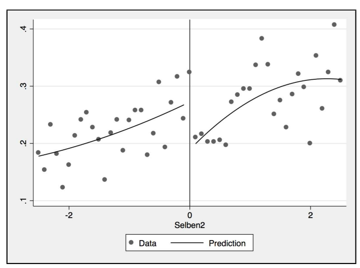
(e) Impact on 10 EGB. Children 17-20 by end of phase 1 (Linear)



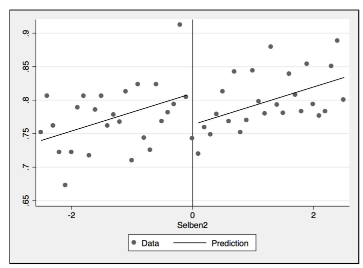
(f) Impact on 10 EGB. Children 17-20 by end of phase 1 (Quadratic)



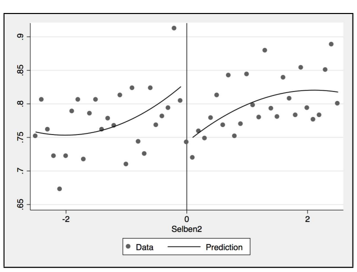
(g) Impact on university. Children 22-25 by end of phase 2 (Linear)



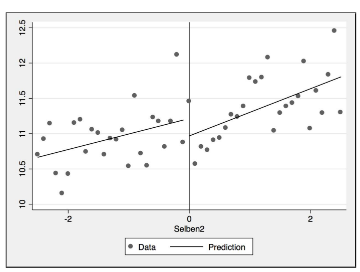
(h) Impact on university. Children 22-25 by end of phase 2 (Quadratic)



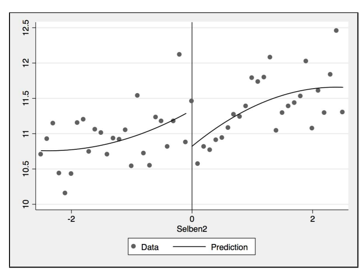
(i) Impact on 10 EGB. Children 22-25 by end of phase 2 (Linear)



(j) Impact on 10 EGB. Children 22-25 by end of phase 2 (Quadratic)



(k) Impact of 10 vs 5 years on years of educ. Children 22-25 by end of phase 2 (Linear)



(l) Impact of 10 vs 5 years on years of educ. Children 22-25 by end of phase 2 (Quadratic)