Now you receive it, now you don't: The effect of losing a cash transfer in Ecuador

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Abstract

The benefits of cash transfers have been extensively studied in the literature, but a significant gap remains concerning the consequences of losing these welfare payments. This paper evaluates the effects of losing the unconditional cash transfer Bono de Desarrollo Humano (BDH) on education and labor outcomes, using two waves of the Ecuadorian Registro Social (RS) database from 2014 and 2018. We follow the regression discontinuity design proposed by Dong (2019), controlling for sample selection due to the high level of attrition in our sample. Our results show that losing the BDH has a negative impact on school attendance and increases the probability of becoming a NEET (not in education, employment, or training), particularly among young women. In addition, the likelihood of joining the labor force increases, especially for men. We finally explore the reasons behind these effects, particularly strong on non-compulsory education aged individuals. These findings suggest that governments should exercise caution as households transition out of cash assistance programs.

JEL classification: *H53, J22, O12*

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1 Introduction

Cash transfers play a central role in the social protection policies of many developing countries. However, concerns about their financial sustainability raise questions about how to best design exit strategies for beneficiaries who are deemed no longer in need of the program. As we show in this study, the loss of Ecuador's Bono de Desarrollo Humano (BDH) had unintended negative consequences on school attendance and labor force participation, particularly among adolescents and young adults. Our empirical analysis uses an abrupt lowering of the eligibility threshold in 2014 to identify the affected beneficiaries. Our findings contribute to the limited literature on the loss of beneficiary status and highlight that short-term financial constraints can have severe effects on a country's long-term human capital accumulation.

The effects of receiving cash transfers on education and labor have been widely documented for almost 30 years. Most studies provide evidence of the effectiveness of these programs in improving educational outcomes, reducing child labor, among other benefits (Edmonds and Schady, 2012; Baird et al., 2014; Bastagli et al., 2019; Bosch and Schady, 2019). However, there is limited research on the consequences of households losing access to such monetary benefits. Policymakers use cash transfers to improve the living conditions of the most vulnerable populations. Nevertheless, ensuring that these gains persist beyond program participation remains a crucial challenge.

We address this gap in the literature by evaluating the impact of losing the BDH on school attendance, labor force participation, and the likelihood of falling into the NEET category (not in education, employment, or training). We employ a sharp regression discontinuity design with sample selection to address potential attrition bias (Dong, 2019). The 2014 recertification process redefined the target population to include only households living in extreme poverty—excluding those in poverty conditions. The bias arises from the lack of updated information in 2018 for some individuals who lost BDH eligibility in 2014. We estimate a policy-level causal effect (intensive margin) and establish boundaries within which the individual causal effect for always-participating compliers is identified.

Our main results indicate a significant negative effect of losing the BDH on school attendance, particularly among women of voluntary secondary school age (15–18 years). Furthermore, we find that the loss of benefits increases the likelihood of labor force participation for men, while women become more likely to fall into the NEET category. Specifically, school attendance decreases by 4.73 percentage points (p.p.) for women of secondary school age, labor force participation increases by 2.82 p.p. for men, and the probability of neither attending school nor working increases by 4.02 p.p. for women.

We argue that gender divisions of labor explain the differences in these effects, and we estimate the impact of losing the BDH on engagement in unpaid housework. These findings suggest that governments should carefully design cash transfer exit strategies to prevent setbacks in human capital accumulation and early labor force participation among adolescents and young adults. Additionally, the eligibility threshold for the program may be set too low, disproportionately affecting young women when they lose BDH benefits.

The remainder of this paper is organized as follows: Section 2 describes the BDH program and its main policy changes. Section 3 reviews the literature on cash transfer programs. Section 4 presents the data and descriptive statistics. Section 5 describes the regression discontinuity methodology with sample selection. Section 6 presents the results and robustness checks, and Section 7 discusses the results and concludes.

2 Background

The Ecuadorian government introduced the *Bono Solidario* (BS) subsidy during the financial crisis of 1998–2000 to compensate for the elimination of gas and electricity subsidies, which affected a large portion of households (Martinez, 2016). The program provided payments of \$15.10 to mothers of children aged 17 or younger and \$7.60 to elderly individuals without a fixed income. However, due to poor targeting, many non-poor households also received the benefit.

In 2003, the program was restructured as a conditional cash transfer (CCT) and renamed *Bono de Desarrollo Humano* (BDH), with a standardized payment of \$15 to all beneficiaries. However, conditions related to school attendance and regular health check-ups were not strictly enforced, leaving the BDH a de-facto unconditional cash transfer (UCT) (Paxson and Schady, 2010; Fernald and Hidrobo, 2011; Edmonds and Schady, 2012).

The eligibility criteria were updated, and the government conducted an extensive survey, *Sistema de Identificación y Selección de Beneficiarios de Programas Sociales* (SELBEN), to improve program targeting. Households were classified into quintiles based on a welfare index (the SELBEN Index), calculated using information on dwelling characteristics, access to basic services, employment status, education level, and school attendance. Households in the two lowest quintiles were eligible for the BDH (MIES, 2019).

Between 2007 and 2008, an additional round of the survey was conducted by the *Unidad de Registro Social* (URS), renamed *Registro Social* (RS). The payment increased to \$35. In 2014, a second RS survey was carried out, and an updated poverty score, known as the *Registro Social Index* (IRS), was estimated using principal components analysis. Additionally, the targeted population was updated. Initially, the program was designed to provide cash transfers to households living in poverty. Since 2014, only households living in extreme poverty have been considered eligible, receiving a payment of \$50, which excluded thousands of households from the program(Martinez, 2016; Araujo et al., 2017). It is this exclusion of previous beneficiary households that lies at the center of our estimation strategy. The most recent update to the RS occurred in 2018.

This study examines education- and labor-related outcomes. The Ecuadorian education system mandates attendance for children aged 5 to 14. Public education is free from early childhood through the completion of secondary school; however, the burden of all other education-related costs falls on households. There is a marked difference in school enrollment between boys and girls, even though the cost of education is generally similar. According to administrative data from the *Instituto de Estadística y Censos* (INEC) and the *Ministerio de Educación* (MINEDUC), girls represented 49.2% of overall compulsory school enrollment in the 2018–2019 academic year, but female enrollment in non-compulsory education (the last three years of high school) was less than 39%.

The legal minimum age for employment is 15, and households that support illegal child labor face criminal charges and the potential loss of parental rights. This study examines the effects of losing the Bono de Desarrollo Humano (BDH) on school attendance and labor force participation across four different school levels. However, due to the legal prohibition of child labor, labor force participation is analyzed only for adolescents and young adults. We also provide estimates of how these effects vary by gender and between rural and urban settings.

3 Existing Literature

The BDH is one of the earliest cash transfer programs in Latin America and has thus generated a considerable body of literature. Existing research has evaluated the effects of receiving the BDH on outcomes such as children's nutritional status (Leon and Younger, 2007), household food expenditure (Schady and Rosero, 2008), child development (Ponce and Bedi, 2010; Paxson and Schady, 2010; Fernald and Hidrobo, 2011), and child mortality (Moncayo et al., 2019).

A substantial body of literature has also examined the program's impact on human capital accumulation and employment among children and young adults. Bosch and Schady (2019) use a regression discontinuity approach based on the poverty score that determines eligibility for the BDH and other welfare programs. The results show that receiving the BDH did not discourage work but did reduce women's social security contributions. Meanwhile, Edmonds and Schady (2012) conclude that the BDH significantly decreases child labor, especially among children with a high likelihood of shifting from schooling to work. Additionally, the extra income from the BDH positively affects school enrollment, particularly for girls.

Using an experimental study, Schady et al. (2008) show that receiving the BDH increases school enrollment by 10 percentage points, with stronger effects among children who were less likely to continue schooling—those who had completed either compulsory primary or compulsory secondary school. This suggests that, without the program, dropout rates would have been higher for students transitioning from 5th to 6th grade or from 8th to 9th grade. Similarly, Araujo et al. (2017) find that receiving the BDH has small positive effects on secondary school completion, especially for women, but no effect on tertiary education continuation. The mentioned papers study the effects of receiving the BDH with a conventional regression discontinuity design, treating Ecuador's RS surveys as a poverty census. In the next section, we will explore this topic further.

The effectiveness of cash transfer programs on educational and labor outcomes has been studied in countries beyond Ecuador. Baird et al. (2011) conduct a randomized control trial among unmarried girls and young women aged 13-22 in Malawi to weigh how conditionality in cash transfers affects schooling outcomes. CCT recipients had a 2.2 percentage point higher attendance rate in 2009 compared to UCT recipients, although there was no effect on UCT beneficiaries. Attanasio et al. (2021) use a regression discontinuity design to assess the long-term impacts of Colombia's urban CCT Familias en Acción. They find a 1.7 percentage point increase in men's college enrollment and a 5.8 percentage point reduction in high school dropout rates for both men and women. Dervisevic et al. (2021) take advantage of the randomized rollout of the Pantawid Pamilyang Pilipino Program (4P) to study its effect on educational and labor outcomes for participants aged 12.5 to 14 at the time of the transfer. The study finds no significant impact on non-compulsory education enrollment, compulsory education attainment, or labor force participation.

Using welfare programs to improve human capital accumulation is not only a matter of concern for developing countries. While several of these programs focus on compulsory school-aged students, some of them seek to incentivize non-compulsory schooling outcomes. Among the former, the evidence on their effectiveness is mixed. Milligan and Stabile (2011) show that policy changes by province in the Canadian child benefit system improve child test scores, especially for boys from lower-income households. *Opportunity New York City*, modeled after Mexico's *Oportunidades* conditional cash transfer, did not have a significant impact on attendance and test scores of elementary and middle school students. However, positive effects were found among more proficient high school students (Riccio et al., 2010).

Regarding non-compulsory education, the literature presents promising results. Dearden et al. (2009) evaluated the *Education Maintenance Allowance* (EMA), a means-tested program that provides a weekly transfer to students from low-income households who stay in school after the statutory age in the United Kingdom. The findings suggest that the probability of remaining in full-time education for one year increased by 4.5 percentage points and by 6.7 percentage points for two years. Back to the United States, the *Quantum Opportunity Program* (QOP) was able to increase graduation rates and postsecondary education enrollment among low-performing high school students. The program aimed to promote participation in mentoring, tutoring, and community service activities in exchange for a financial reward of $1.25 per hour as well as reaching-a-milestone incentives such as obtaining their high school diploma. In general, the QOP benefited women more than it did men (Rodríguez-Planas, 2012). Positive school outcomes are also found by Pallais (2009) and Jackson (2010) in relation to the *American College Testing* (ACT) performance in Tennessee and college enrollment in Texas, respectively.

We evaluate the effect of losing the BDH on school attendance and workforce participation. To our knowledge, there is very limited empirical research examining the effects of losing beneficiary status on a cash transfer program. Pfutze (2019) studies Mexico's *Oportunidades* CCT and replicates its eligibility score. There are negative effects on school attendance for secondary school-aged students after losing the benefit. The results suggest that the poverty threshold used to identify potential beneficiaries might be set too low.

Buser et al. (2016) focuses on the BDH cash transfer program in Ecuador. Surveys were conducted in three major cities in 2011, two years after a major change in the eligibility score caused the exit of thousands of beneficiaries from the program. The authors find a negative impact on health outcomes (height-for-age and weight-for-height) for children under 5 years old. The main difference of our analysis relies on the availability of data at a national level with outcomes (such as school attendance or labor force participation) measured 4 years after leaving the program.

The work closest to ours is Palacios Mora et al. (2022), who use two waves of the RS surveys to analyze the effects of receiving or losing the BDH between 2008 and 2014 on the the 2014 RS score using RDD. Interestingly, they find that the benefit had a negative effect on households' well-being. Their study assumes that there is no inherent attrition in the data, but that a subgroup of households cannot be matched across surveys due to not reporting their national identification number in the survey. While the 2008 RS does have a high percentage of missing identification numbers (34.54%), this is not the case for the waves we use (2014 RS: 2.82%, and 2018 RS: 0.53%). However, we do find an inherent sample selection issue (attrition). We explain these important clarifications in the following sections.

Our research expands on this literature in several ways: i) We focus on the effect of a loss of benefit on one of the arguably most important long-term outcomes of cash-transfer programs, school attendance. ii) We show results for pratically the entire population of BDH benefiaciary households in 2014. iii) We employ a recently proposed, yet little noticed, identification method to account for sampler selection in a RDD framework. This last point should make working with administrative datasets much easier in the future. Additionally, we look at whether losing the cash transfer increases the likelihood of becoming part of the NEET group. This phenomenon is common in Latin America but remains understudied in the context of cash-transfers programs.

4 Data

We use confidential data from the URS. Two waves (2014 RS and 2018 RS) are merged using anonymized identification numbers. Eligibility for the BDH is determined by the IRS, which is recalculated with each new RS survey. After the 2014 survey, the BDH program underwent significant changes, resulting in 57% of previous beneficiaries being removed from the program due to new IRS scores exceeding the new eligibility threshold (MIES, 2017).

The methodology used to calculate the eligibility score was modified to align with the new targeting criteria, focusing exclusively on households living in extreme poverty. The IRS was calculated using principal components analysis (PCA), aggregating factors such as dwelling characteristics (e.g., wall and floor materials, access to water, sanitary systems) and household head characteristics, including age, education, and ethnicity. Households with an IRS score below 28.20351 were eligible to receive the cash transfer.

The URS collects these data through a two-step targeting process. First, it identifies geographic areas (known as census areas) to be surveyed based on poverty incidence.[1] Census areas with poverty incidences greater than 50% are selected. Then, the URS conducts surveys on all households living in these areas to create the RS database. Additionally, a demand-based registration process is available at *Ministerio de Inclusión Económica y Social* (MIES) offices. Households can apply for participation in the program, and once a sufficient number of applications are received from the same census area, the URS initiates the data collection process (Martinez, 2016, 2017).

The 2014 and 2018 field data collection processes were improved by the implementation of tablets and other technologies that allowed the field enumerator to link the identification number with administrative data from other departments (Martinez, 2017). These updates improved the number of observations lacking their identification numbers: 3.08% in 2014 RS and 0.53% in 2018 RS, compared to 34.54% in 2008 RS.

[1] Household's poverty condition rely on the Índice de Necesidades Básicas Insatisfechas developed by the Comunidad Andina de Naciones. Households are consider to live in extreme poverty conditions if they lack two or more of the following needs: 1) the dwelling has walls made of fragile materials or an earthen floor, 2) the dwelling lacks adequate basic services like running water or the sanitary system is not connected to the sewage system or a septic tank, 3) the household lives in critical overcrowding, meaning the average occupancy per room used for sleeping exceeds three people, 4) the household has high economic dependency, meaning there are more than three people for every employed individual, and the household head has not completed more than the first two years of primary education and 5) a child in the household between six and twelve years old does not attend school.

Table 1

Descriptive statistics

	All		Men		Women		Urban		Rural	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	Compulsory primary education (5 - 11 years)									
IRS score 2014	26.64	9.95	26.65	9.95	26.63	9.95	28.71	9.69	23.94	9.64
Observations	890,084		452,916		437,168		503,870		386,214	
Attending school	0.92	0.26	0.92	0.27	0.93	0.26	0.92	0.27	0.93	0.26
Observations	750,333		382,269		368,064		419,885		330,448	
	Compulsory secondary education (12 - 14 years)									
IRS score 2014	27.14	9.98	27.14	9.97	27.14	9.99	29.20	9.73	24.47	9.66
Observations	1,102,110		561,943		540,167		623,218		478,892	
Attending school	0.74	0.44	0.74	0.44	0.74	0.44	0.73	0.44	0.75	0.44
Observations	895,609		455,499		440,110		504,091		391,518	
	Voluntary secondary education (15 - 18 years)									
IRS score 2014	27.80	10.04	27.80	10.05	27.81	10.04	29.88	9.83	25.14	9.68
Observations	965,379		498,439		466,940		542,674		422,705	
Attending school	0.52	0.50	0.51	0.50	0.52	0.50	0.51	0.50	0.53	0.50
Labor force participation	0.31	0.46	0.44	0.50	0.18	0.38	0.31	0.46	0.31	0.46
NEET	0.21	0.41	0.10	0.30	0.33	0.47	0.22	0.41	0.21	0.41
Housework	0.17	0.37	0.03	0.18	0.30	0.46	0.17	0.37	0.16	0.37
Observations	736,761		374,660		362,101		416,830		319,931	
	Higher education (19 - 24 years)									
IRS score 2014	28.81	10.14	28.88	10.21	28.74	10.06	30.85	9.99	26.15	9.7
Observations	969,978		503,480		466,498		549,182		420,796	
Attending school	0.26	0.44	0.26	0.44	0.27	0.44	0.26	0.44	0.27	0.44
Labor force participation	0.48	0.50	0.68	0.47	0.29	0.45	0.48	0.50	0.48	0.50
NEET	0.29	0.45	0.11	0.31	0.47	0.50	0.29	0.45	0.29	0.45
Housework	0.23	0.42	0.03	0.17	0.44	0.50	0.24	0.42	0.23	0.42
Observations	694,559		346,548		348,011		399,123		295,436	

Note: IRS score calculated with the 2014 RS. Attending school, labor force participation, NEET and housework variables were obtained from the 2018 RS. The difference in the number of observations are the attrited observations (not observed in 2018).

For our analysis, we define the treatment group as individuals who received the BDH before 2014 but were excluded from the program after the methodological changes. The control group consists of individuals who continued to receive the transfer. Our sample is defined by individuals' self-reported BDH receipt status in the 2014 survey (recipient status was only asked for in 2014 RS). We then compare recipient status based on the eligibility score from the earlier wave (2008 RS, also known as SELBEN II) with individuals' responses in 2014. Households with an IRS score of 36.598715 or below in 2008 were eligible for the cash transfer. Among those eligible to receive the BDH in 2008, 80.41% reported receiving the cash transfer before 2014, while 79.54% of those ineligible in 2008 reported not receiving it by 2014. So, there is a close match between eligibility and beneficiary status.

Merging the 2014 and 2018 datasets presents challenges, particularly due to attrition. Many individuals who lost the BDH in 2014 were not surveyed in 2018, whereas a small fraction did not report their national identification number (and hence could not be matched). To address this issue, we employ the regression discontinuity design with sample selection proposed by Dong (2019), which we describe in detail further below. This novel approach allows us to examine the effects of losing the BDH on education and labor outcomes while accounting for potential biases caused by attrition or sample selection of any nature.

The study focuses on four education groups based on Ecuador's academic system: compulsory primary school (individuals aged 5 to 11), compulsory secondary school (individuals aged 12 to 14), voluntary secondary school (individuals aged 15 to 18), and higher education (individuals aged 19 to 24). Labor force participation and the likelihood of becoming part of the NEET group are analyzed for the latter two groups, as the legal working age in Ecuador is 15. Any effect of losing the BDH on labor force participation is expected to be concentrated among individuals aged 15 and above.

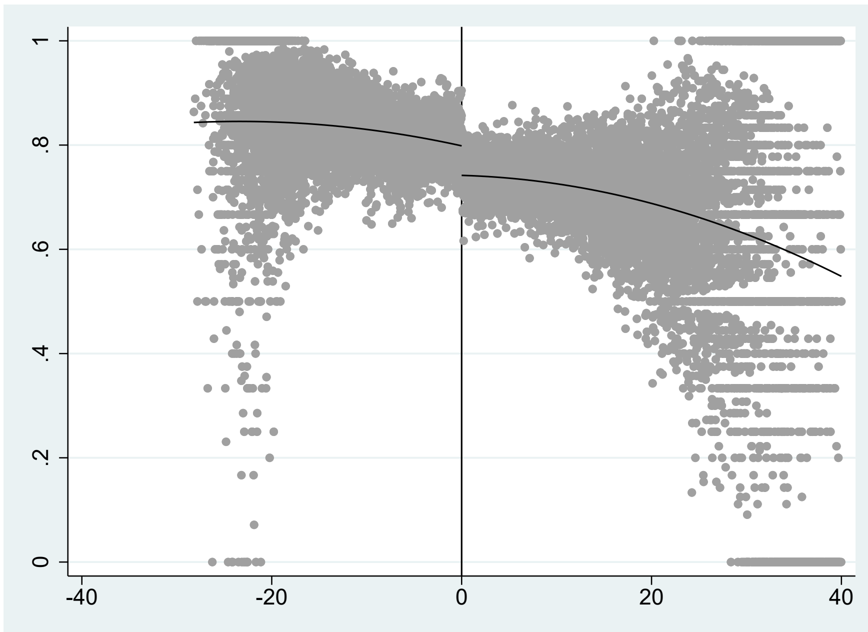
To ensure accuracy, we check for inconsistencies in reported ages across the two waves. If the age difference between 2014 and 2018 is less than four or greater than eight years, we classify the household as age-inconsistent and exclude it from the analysis.

Table 1 presents the summary statistics for the entire sample, as well as separately for male and female individuals and by area of residence. The difference in the number of observations for the IRS score and the dependent variables is due to attrition. Significant disparities are observed in labor force participation, NEET status, and housework between men and women. Furthermore, as expected, the IRS score is consistently higher in urban areas than in rural areas.

5 Methodology

We employ a sharp regression discontinuity design in our estimations. The initial sample consists of all individuals living in households that received the Bono de Desarrollo Humano (BDH) at the time their RS data was collected in 2014. Using this data, we determine whether households remained eligible for the BDH based on their IRS score in 2014. This score ranged from 0 to 84.26361, with an average of 29.16116 and a median of 28.75423 in our initial sample. The eligibility threshold was set at 28.20351. Although we cannot directly observe actual program participation in 2018, we are confident—based on the comparison of eligibility according to the IRS in 2008 and self-declared status in 2014—that eligibility closely reflected participation. An alternative interpretation of our results would be as an intention-to-treat effect (ITE).[2]

Figure 1: Sample selection around the threshold



Notes: The probability of being sampled in 2018 is discontinuous at the threshold (attrition), centered at 28.20351).

Our principal identification problem resides in the high number of attrition between the 2014 and 2018 RS rounds. Out of a total of 3'924,551 observations living in BDH recipient households in 2014, we are only able to observe 3'077,262 in 2018. Given the nature of RDD estimators, this would not constitute a problem as long as the attrition would not exhibit any discontinuity at the eligibility threshold. However, since households receiving the BDH (or some other benefit) need to be interviewed in each round to continue in the program, those that have lost eligibility may have no incentive to do so, or their identity number may go unreported. Figure 1 shows that these factors indeed translate into a discontinuous jump in the selection status at the eligibility threshold. Losing the cash transfer reduces the likelihood of being sampled in 2018. If unaddressed, and unless one is willing to assume that the treatment had the same average effect on attrited and non-attrited observations, this might induce selection bias into results obtained by a traditional sharp RDD.

[2]We have attempted to obtain actual program participation data. However, this would have required coordination between the RS and MIES to match their respective datasets (based on national ID) before providing us with an anonymized version. Additionally, by the time of writing this paper, Ecuador had undergone two changes in administration, and we were informed that the new staff at MIES did not have access to beneficiary information from previous administrations.

We address this problem by employing an RDD estimator proposed by Dong (2019) that explicitly controls for sample selection. While developed for the case of fuzzy RDD estimators, its adoption to the simpler sharp design is straightforward. Given that we deal with differential attrition to the left and the right of the eligibility threshold, we have four potential types of observations in our initial sample [3]: i) *Never Participants* would not be in the 2018 RS under either treatment or non-treatment; ii) *Always Participants* are in the 2018 RS (and thus observed) independent of treatment status; iii) *Quitters* are observed under no-treatment but are not in the 2018 RS round if treated; and lastly, iv) *New Participants* are observed under treatment but not under no-treatment (refer to Figure 2).

The method consists of first estimating the average outcome at the cutoff, adjusted for the magnitude of sample selection, from the left and the right of the eligibility threshold separately. Subsequently, one takes the difference between these two estimates to arrive at one for the treatment effect. Following the notation in Dong (2019), this estimator can be written for our sharp RDD as follows:

$$\begin{gathered} E[Y_1|S_1 = 1, R = r_0, C] - E[Y_0|S_0 = 1, R = r_0, C] = \\ \frac{\lim_{r\downarrow r_0} E[1(T=1)Y_1 S|R=r]}{\lim_{r\downarrow r_0} E[1(T=1)S|R=r]} - \frac{\lim_{r\uparrow r_0} E[1(T=0)Y_0 S|R=r]}{\lim_{r\uparrow r_0} E[1(T=0)S|R=r]}, \end{gathered} \tag{1}$$

where $T = 1$ denotes treatment and $T = 0$ no treatment; R is the running variable (the 2014 IRS score) with a treatment threshold at $R = r_0$. Furthermore, $Y_t = Y_1^* T + Y_0^*(1 - T)$ denotes the outcome of interest and $S_t = S_1^* T + S_0^*(1 - T)$ where $S_t \in \{0, 1\}$ the selection into the sample, with the subscript $t \in \{0, 1\}$ indicating treatment status.

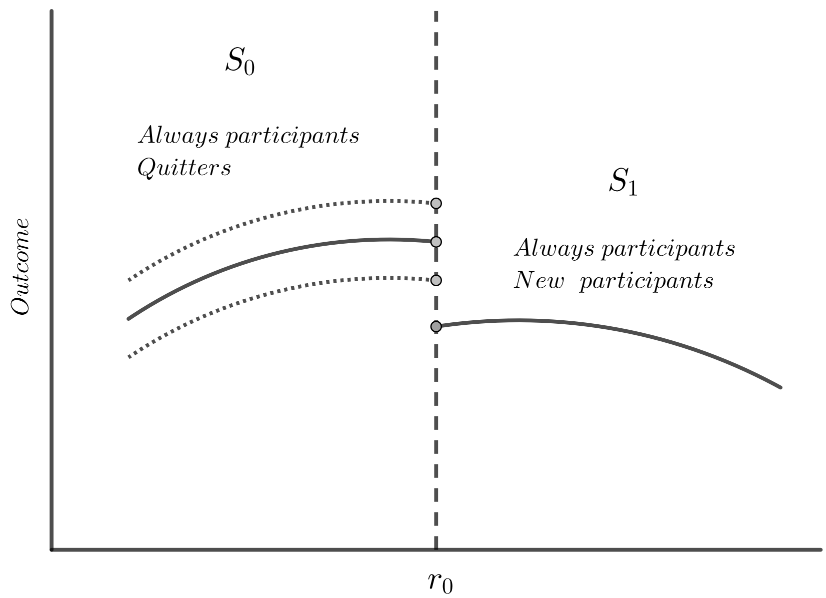
Equation 1 provides an estimator for the *intensive margin*. That is, how the observed outcome of interest is expected to differ between the treatment and control groups. The *extensive margin*, estimated as an RDD with S as the outcome variable, measures the impact of treatment on the probability of being observed. In our case, this would be the probability of being observed in the 2018 RS and is of no direct interest.

Equation 1 provides us with consistent estimates of the outcome of interest at either side of the discontinuity. However, the difference does not have a clear-cut interpretation without further assumptions. We will throughout impose the assumption of *monotonic selection*, such that $Pr(S_0 \geq S_1) = 1$. That is, no observation is more likely to be in the sample under treatment than under no-treatment. This means that we are ruling out new participants as defined above. In practical terms, we are assuming that households that formed part of the 2014 RS and are still observed in 2018 are always participants and that households not observed in 2018 who kept their eligibility are never participants.[4]

[3] This is the same terminology as in Dong (2019). Note that while it is very similar to that used in instrumental variable (or fuzzy RDD) settings, the terms refer to sample selection and not program participation.

[4] While of no importance to our identification strategy, there is no reason to believe that the proportion of never participants would change discontinuously at the threshold. Any significant effect at the extensive margin can thus be attributed to quitters.

Figure 2: Subgroups among compliers



Notes: The dotted lines represent the sharp bounds around the treatment effect for always participating compliers. Under the monotonic selection assumption, we do not observe new participants.

The estimated outcome at the threshold for the control group ($E[Y_0|S_0 = 1, R = r_0, C]$) contains always participants and quitters, while the one for the treatment group ($E[Y_1|S_1 = 1, R = r_0, C]$) under monotonic selection only contains always participants. The difference between the two does thus not have a straightforward interpretation. We cannot make any statement on the effect on quitters, and the intensive margin estimate could only be interpreted as an average treatment effect for always participants if we assume that the average outcome under no treatment (i.e. keeping benefit eligibility) is the same for them and quitters.

While not a completely unreasonable assumption, we can do better by constructing well-identified bounds around the treatment effect for always participants following Dong (2019). Here, the assumption of monotonic selection is crucial. Since it implies that we only observe quitters and always participants, we can estimate the proportion of the former in the control group with a standard RDD using S as the outcome variable (i.e. the *extensive margin* estimation). This proportion is denoted by q. Bounds are then constructed by assuming that the $(1 - q)$ largest and smallest outcomes, respectively, belong to always participants.[5]

[5] See Dong (2019) for a detailed discussion on estimation and inference.

Table 2
Results for School Attendance

	All	Male	Female	Urban	Rural
	Compulsory primary education (5 - 11 years)				
Standard RDD	-0.0123***	-0.0143***	-0.0094**	-0.0153***	-0.0063
s.e.	(0.0028)	(0.0041)	(0.0037)	(0.0035)	(0.004)
n	750,333	382,269	368,064	419,885	330,448
Intensive margin	-0.0102***	-0.0118**	-0.0088**	-0.0138***	-0.0064
s.e.	(0.0031)	(0.0047)	(0.0039)	(0.0040)	(0.0042)
Lower bound	-0.0962***	-0.1180***	-0.1185***	-0.1015***	-0.1149***
s.e.	(0.0090)	(0.0095)	(0.0082)	(0.0081)	(0.0081)
Upper bound	-0.0102***	-0.0118**	-0.0088**	-0.0138***	-0.0064
s.e.	(0.0031)	(0.0047)	(0.0039)	(0.0040)	(0.0042)
n	890,084	452,916	437,168	503,870	386,214
	Compulsory secondary education (12 - 14 years)				
Standard RDD	-0.0174***	-0.0254***	-0.0071	-0.0237***	-0.007
s.e.	(0.0041)	(0.0061)	(0.0049)	(0.0053)	(0.0062)
n	895,609	455,499	440,110	504,091	391,518
Intensive margin	-0.0197***	-0.0251***	-0.0111*	-0.0235***	-0.0086
s.e.	(0.0046)	(0.0061)	(0.0058)	(0.0058)	(0.0068)
Lower bound	-0.1256***	-0.1277***	-0.1228***	-0.1149***	-0.1355***
s.e.	(0.0069)	(0.0092)	(0.0088)	(0.0086)	(0.0100)
Upper bound	-0.0197***	-0.0251***	-0.0111*	-0.0235***	-0.0086
s.e.	(0.0046)	(0.0061)	(0.0058)	(0.0058)	(0.0068)
n	1,102,110	561,943	540,167	623,218	478,892
	Voluntary secondary education (15 - 18 years)				
Standard RDD	-0.0486***	-0.0404***	-0.0501***	-0.0498***	-0.0431***
s.e.	(0.0069)	(0.009)	(0.009)	(0.0087)	(0.0095)
n	736,761	374,660	362,101	416,830	319,931
Intensive margin	-0.0431***	-0.0373***	-0.0473***	-0.0436***	-0.0440***
s.e.	(0.0071)	(0.0087)	(0.0104)	(0.0100)	(0.0101)
Lower bound	-0.1198***	-0.115***	-0.1247***	-0.1132***	-0.1297***
s.e.	(0.0086)	(0.0107)	(0.0127)	(0.0124)	(0.0121)
Upper bound	-0.0431***	-0.0373***	-0.0473***	-0.0436***	-0.0440***
s.e.	(0.0071)	(0.0087)	(0.0104)	(0.0100)	(0.0101)
n	965,379	498,439	466,940	542,674	422,705
	Higher education (19 - 24 years)				
Standard RDD	-0.0177***	-0.0156**	-0.0194**	-0.017**	-0.0181**
s.e.	(0.0057)	(0.0078)	(0.0078)	(0.0074)	(0.0088)
n	694,559	346,548	348,011	399,123	295,436
Intensive margin	-0.0177***	-0.0134	-0.0181**	-0.0123	-0.0172**
s.e.	(0.0062)	(0.0096)	(0.0076)	(0.0078)	(0.0087)
Lower bound	-0.0338***	-0.0289***	-0.0348***	-0.0262***	-0.0357***
s.e.	(0.0067)	(0.0104)	(0.0083)	(0.0083)	(0.0096)
Upper bound	-0.0177***	-0.0134	-0.0181**	-0.0123	-0.0172**
s.e.	(0.0062)	(0.0096)	(0.0076)	(0.0078)	(0.0087)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

6 Results

We present our results in several steps. We begin with education-related outcomes, followed by labor market outcomes and NEET status, focusing on extensive margin effects. For each outcome and school level, we first report results from the standard regression discontinuity design (RDD), followed by results that account for potential selection bias. As discussed, these results can be interpreted as average treatment effects (ATEs)—in the first case, under the assumption that the treatment effect is the same for quitters and always participants, and in the second case, under the assumption that the average outcome without treatment (i.e., not losing the benefit) is the same for both groups. Similar results between these two approaches suggest that selection bias is likely not a major concern.

We then assess the robustness of our results using worst-case scenario bounds, assuming monotonic selection. Only one of the two reported bounds is relevant, depending on the sign of the extensive margin estimator, and we consider the statistical significance of these bounds in our analysis.

Finally, we test the validity of our specification by using individual and household head characteristics as outcome variables to show their smoothness at the threshold. We also check for any discontinuity in the running variable, which would indicate manipulation of the program's poverty score.

6.1 Standard RDD and extensive margin effects

Table 2 presents the standard RDD results and estimated intensive margin effects of losing the BDH on school attendance for the entire sample (column 1), by gender (columns 2 and 3), and by area of residence (columns 4 and 5). We find statistically significant negative effects across all educational levels, with the strongest impact observed for students in voluntary secondary education.

Moreover, as shown in the following tables, the results for the standard RDD and the intensive margin are very similar, suggesting that selection bias is likely a minor concern. Nonetheless, the more detailed discussion that follows will be framed in terms of the intensive margin results, which represent our preferred specification.

The magnitude of the effect is relatively small for individuals of compulsory school age, with a decrease of 1.02 percentage points (p.p.) for those in compulsory primary education and 1.97 p.p. for those in compulsory secondary education. With school attendance rates of 97.10% for the former group and 94.65% for the latter,[6] the relative decreases are 1.05% and 2.08%, respectively. These effects are more pronounced for boys than for girls.

In contrast, individuals of voluntary secondary school age experience a 4.31 p.p. decrease in attendance (5.74%, with an initial attendance rate of 75.12%) following the loss of the BDH. This negative effect is

[6] These are the expected values under no treatment ($E(Y_0|S_0 = 1)$), i.e., individuals who kept receiving the cash transfer payments. Appendix A shows the expected values for the control and treated group at the cutoff point for every outcome.

statistically stronger for women (4.73 p.p., or 6.29%) compared to men (3.73 p.p., or 4.97%). For individuals in the age group corresponding to higher education (19-24 years), the effect is a 1.77 p.p. decrease, and with an attendance rate of 21.61%, this represents an 8.19% relative decline. While the magnitude of this effect is similar for all four sub-groups, it is only statistically significant for the whole sample, women, and in rural areas.

Table 3

Results for Joining the Labor Force

	All	Male	Female	Urban	Rural
	Voluntary secondary education (15 - 18 years)				
Standard RDD	0.0187***	0.0321***	0.0068	0.0139**	0.0238***
s.e.	(0.0052)	(0.0086)	(0.0059)	(0.0062)	(0.008)
n	736,761	374,660	362,101	416,830	319,931
Intensive margin	0.0169***	0.0282***	0.0052	0.013**	0.0239***
s.e.	(0.0052)	(0.0089)	(0.0059)	(0.0064)	(0.0083)
Lower bound	0.001	0.0056	-0.0038	-0.001	0.0054
s.e.	(0.0056)	(0.0094)	(0.0063)	(0.0068)	(0.009)
Upper bound	0.1721***	0.0282	0.0922***	0.1628***	0.1874***
s.e.	(0.0034)	(0.0213)	(0.0039)	(0.0323)	(0.015)
n	965,379	498,439	466,940	542,674	422,705
	Higher education (19 - 24 years)				
Standard RDD	0.0149**	0.0151*	0.0123	0.0164*	0.0104
s.e.	(0.0065)	(0.0083)	(0.0078)	(0.0087)	(0.0092)
n	694,559	346,548	348,011	399,123	295,436
Intensive margin	0.0134*	0.0128	0.0083	0.0197**	0.0095
s.e.	(0.0069)	(0.008)	(0.0099)	(0.0084)	(0.011)
Lower bound	-0.0257***	-0.0475***	-0.0128	-0.0137	-0.0377***
s.e.	(0.0078)	(0.0106)	(0.0107)	(0.0093)	(0.0127)
Upper bound	0.0134*	0.0128	0.0083	0.0197**	0.0095
s.e.	(0.0069)	(0.008)	(0.0099)	(0.0084)	(0.011)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

These findings suggest that the loss of the BDH has a greater impact on individuals of voluntary secondary school age or higher education age, who can choose to pursue further non-compulsory education. The decision to opt out of voluntary education may be influenced by the opportunity costs associated with continuing education or gender norms that set the intrahousehold division of labor, a topic we explore further in the Discussion section.

Column 4 shows the estimated effects for individuals living in urban areas. There are significant negative effects of losing the BDH for three groups, with no significant effect observed for individuals of higher education age. Column 5 shows that the loss of beneficiary status negatively affects individuals of voluntary secondary school and higher education age in rural areas. There is no significant difference in the effect of losing the BDH between urban and rural areas for voluntary secondary school-aged individuals.

Table 3 reports the impact of losing the BDH on labor force participation. As mentioned before, we only include individuals of legal working age above 15 years. There is an increase in labor force participation of 1.69 p.p. for individuals of voluntary secondary school age (with a 15.53% participation rate, the relative increase is 10.89%), particularly among men (2.82 p.p. or 12.91% with a participation rate of 21.84%). Additionally, there is a positive effect on labor force participation among voluntary secondary school-aged students in rural areas.

For individuals of higher education age, we find a significant increase in labor force participation at the 10% level. This suggests that losing the BDH may push some individuals into the labor market at a younger age than they might otherwise enter. Among this age group, those residing in urban areas show a positive effect at the 10% significance level.

The estimated impacts of losing this welfare payment on the probability of not being in education, employment, or training (NEET) are shown in Table 4. We find statistically significant increases in NEET status for those of voluntary secondary school age, with a lower and statistically insignificant impact reported for individuals of higher education age.

The proportion of NEET among voluntary secondary school aged individuals increases by 2.43 p.p. This indicates a relative increase in the NEET status of 17.25%. There is a greater positive effect on females (4.02 p.p.; 20.99% relative increase) than on males (0.88 p.p.; 9.35% relative increase). Both urban and rural areas show significant increases in NEET status for individuals of voluntary school age; although, there is no statistical difference between these effects. Thus, we argue that individuals of voluntary secondary school age, particularly women, are less likely to engage in education and labor activities when their household leaves the program (NEET status).

6.2 Sharp bound estimates

We do believe that the close similarity of the standard RDD and the intensive margin estimates just presented lend support to the assumption that selection bias only induces minimal bias. It is nonetheless instructive to look at the worst-case scenario in which all quitters would have either the highest or lowest (depending on the sign of the estimate) outcome to the left of the threshold. As discussed before, we present sharp bounds (under the monotonic selection assumption) on the treatment effect of the loss of beneficiary status on school attendance, labor force participation, and becoming part of the NEET group for the always participating compliers. The third and fourth rows on each age subgroup panel present the lower and upper bounds that contain the true individual causal effect of losing the BDH on the outcome variable.

Table 4
Results for not in education, employment, or training (NEET)

	All	Male	Female	Urban	Rural
	Voluntary secondary education (15 - 18 years)				
Standard RDD	0.0263***	0.0072	0.0453***	0.0279***	0.0221***
s.e.	(0.0053)	(0.0055)	(0.009)	(0.007)	(0.0075)
n	736,761	374,660	362,101	416,830	319,931
Intensive margin	0.0243***	0.0088	0.0402***	0.0255***	0.0218***
s.e.	(0.0056)	(0.0057)	(0.0091)	(0.0068)	(0.0082)
Lower bound	0.0099*	-0.0009	0.0205**	0.012*	0.0064
s.e.	(0.0059)	(0.0061)	(0.0098)	(0.0073)	(0.0087)
Upper bound	0.1653***	0.1029***	0.0402	0.1695***	0.1586***
s.e.	(0.0036)	(0.004)	(0.0781)	(0.0265)	(0.0064)
n	965,379	498,439	466,940	542,674	422,705
	Higher education (19 - 24 years)				
Standard RDD	0.003	0.0015	0.0066	-0.0011	0.0076
s.e.	(0.0065)	(0.0056)	(0.0091)	(0.0087)	(0.0095)
n	694,559	346,548	348,011	399,123	295,436
Intensive margin	0.0007	0.0008	0.0106	-0.0078	0.0111
s.e.	(0.0066)	(0.0066)	(0.0114)	(0.0084)	(0.0103)
Lower bound	-0.0215***	-0.0077	-0.0241*	-0.0282***	-0.0133
s.e.	(0.0071)	(0.007)	(0.0126)	(0.0092)	(0.0113)
Upper bound	0.0007	0.1071***	0.0106	-0.0078	0.0111
s.e.	(0.0066)	(0.0089)	(0.0114)	(0.0084)	(0.0103)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

Losing this cash transfer has a negative effect on school attendance for all the age subgroups since the upper bounds on all the estimations are negative and almost all are statistically significant at conventional levels (Table 2). The coefficients of the upper bound increase from compulsory primary education to voluntary secondary aged individuals, supporting the hypothesis that losing the cash transfer has a greater effect on individuals with higher schooling opportunity costs. This is under the assumption that all quitters to the left of the threshold attend school.

On the other hand, the causal effect of losing the BDH on joining the labor force (Table 3) is not as clear-cut as the one on school attendance. The lower bounds for the voluntary secondary education-aged group are slightly above zero for the four results with statistically significant intensive margin effects; however, they are statistically insignificant. So, under the assumption that all quitters on the left side of the threshold are not in the labor force, we cannot rule out a null effect.

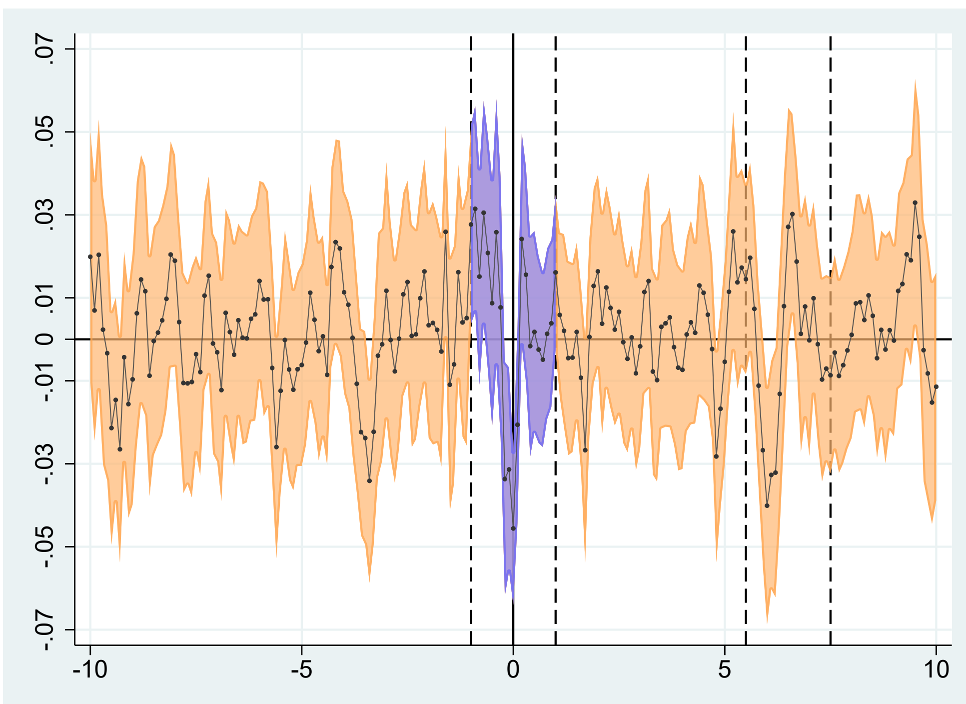
We find somewhat stronger results for becoming part of the NEET group (Table 4) for individuals of voluntary secondary education age. For three out of the four groups with statistically significant intensive margin estimates, we find statistically significant lower bounds (albeit in two only at the 10 percent level).

That is, under the assumption that none of the quitters to the left of the threshold is NEET. The strongest result is obtained for women who drop out of the program and who face an increase in their likelihood of being part of the NEET (2.05 p.p. at the lower bound for always participating compliers). This result is of particular importance and it is discussed below.

6.3 Robustness

We will now address two common robustness checks for RDD estimates. First, we run a placebo test, changing the cutoff to the left and to the right of the true threshold. If our approach is correct, we would not expect more statistically significant results than implied by random variation. If the test showed significant discontinuities, it would indicate that the observed effect at the real cutoff point is determined by factors other than the treatment. Secondly, we run our RDD at the true threshold on a number of other observable covariates. If our approach is correct, they should not exhibit statistically significant discontinuities.

Figure 3: Placebo test at different cutoff points



Notes: The blue-shaded area represent the excluded +/-1 area around the cutoff (vertical dashed lines, centered at 28.20351).

For the first exercise, we shift the threshold by increments of 0.1 within a window of 10 points on either side of the cutoff, starting at a distance of one point. We examine whether the estimated discontinuities are statistically significant at the 5% level. Random variation in the data would imply that around 5% of placebo thresholds reach that level of significance. Figure 3 shows the estimated coefficients and the 5% confidence interval for voluntary secondary school aged observations.

After excluding the area +/-1 around the cutoff, around 4.44% of the coefficients are statistically significant at the 5% level. And the magnitude of the effect is largest at the true threshold. Moreover, we find a group of statistically significant discontinuities between +5.5 to +7.5 placebo cutoff points, approximately (between 33.7 to 35.7 on the RS score). This cluster of statistically significant effects might be explained the presence of an eligibility threshold for a different program: The unconditional cash transfer *Mis Mejores Años* transfer payments to elder household members (eligibility threshold 34.67 (IRS 2014)). We ran the same test on the other school level groups and found similar results.

In the second robustness test, we evaluate the smoothness around the threshold for observable covariates that might have an independent effect on our outcome variables. We show results for individual characteristics and those of the household head. Table 5 shows the smoothness of these covariates at the cutoff point. Out of 72 estimations, we only found four coefficients with statistical significance at the 10% level and four at the 5% level. This is what would be expected by random variation.

We also test if there is any discontinuity in the density of the running variable at the threshold to check for possible manipulation of the program poverty score (third panel in Table 5). Across the four age groups, there is no evidence of manipulation of the 2014 IRS score.

Lastly, it is instructive to give a visual impression of the discontinuity identified. For the sake of brevity, we do so only for all the observations for the four school-levels. Figure 4 shows the change in the probability of school attendance at the threshold, based on a second-order polynomial and a standard RDD. A visual inspection reveals not only the discontinuities we discussed before,[7] but also that there are no suspicious functional fortm in the smoothed function around the threshold. The function form looks very similar to the left and the right of the discontinuous break.

7 Discussion and Conclusion: paid and unpaid work

In this study, we analyze the consequences of losing a cash transfer on education and labor outcomes in the Ecuadorian context, using two waves of RS data. The removal of poor, yet not extremely poor, beneficiaries from the BDH, as happened after the 2014 survey, resulted in consistently lower school attendance rates, regardless of age group, gender, or location. Particularly, women of voluntary secondary school age (15-18 years) faced a decrease in school attendance of 4.73 p.p., four years after leaving the program. We also observe that labor force participation increases among men by 2.82 percentage points, while the likelihood of becoming part of the NEET group increases by 4.02 percentage points for women. Although the BDH helps households overcome poverty, our results indicate that leaving the program has an unintended negative effect, particularly on young women.

[7] A decrease of school attendance of individuals right above the cutoff, i.e. individuals that left the program.

Table 5

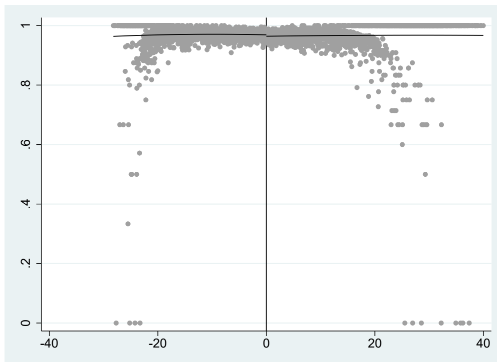
Smoothness of the conditional means of observable covariates

	Compulsory primary	Compulsory secondary	Voluntary secondary	Voluntary higher
	Individual Level Variables			
Women	-0.0058	0.0013	0.0063	-0.0066
	(0.0079)	(0.0087)	(0.0069)	(0.0077)
Urban	0.0180*	-0.0015	0.0013	-0.0016
	(0.0104)	(0.0089)	(0.0094)	(0.0069)
Ethinicity				
Mestizo	-0.0076	-0.0012	-0.0003	-0.0042
	(0.0159)	(0.0083)	(0.0068)	(0.0062)
Indigenous	-0.0190*	-0.0052	-0.0009	-0.0008
	(0.0115)	(0.0060)	(0.0048)	(0.0045)
Afroecuadorian	0.0167**	-0.0047	-0.0026	-0.0025
	(0.0077)	(0.0040)	(0.0032)	(0.0034)
Montuvio	0.0098	0.0121**	0.0027	0.0039
	(0.0105)	(0.0052)	(0.0051)	(0.0052)
White	-0.0029	-0.0004	0.0008	0.0041**
	(0.0050)	(0.0022)	(0.0017)	(0.0018)
	Household Level Variables			
Women	0.0092	-0.0226	0.0158	0.0052
	(0.0155)	(0.0175)	(0.0150)	(0.0152)
Employed	-0.0144	-0.0038	-0.0112	0.0104
	(0.0113)	(0.0109)	(0.0105)	(0.0103)
Ethinicity				
Mestizo	0.0245	-0.0051	-0.0148	-0.0030
	(0.0162)	(0.0179)	(0.0153)	(0.0157)
Indigenous	-0.0221**	-0.0042	0.0173	0.0018
	(0.0106)	(0.0121)	(0.0126)	(0.0092)
Afroecuadorian	0.0014	-0.0109	0.0050	-0.0066
	(0.0076)	(0.0089)	(0.0075)	(0.0078)
Montuvio	-0.0057	0.0203	0.0005	0.0061
	(0.0106)	(0.0133)	(0.0111)	(0.0106)
White	0.0006	-0.0085*	0.0015	0.0031
	(0.0046)	(0.0050)	(0.0045)	(0.0040)
Education				
No Education	0.0044	0.0028	-0.0139	-0.0164
	(0.0080)	(0.0094)	(0.0085)	(0.0100)
Primary	0.0036	0.0200	0.0366*	0.0188
	(0.0164)	(0.0167)	(0.0193)	(0.0206)
Secondary	-0.0085	-0.0236	-0.0161	0.0139
	(0.0154)	(0.0169)	(0.0144)	(0.0134)
Higher Education	0.0009	0.0035	-0.0010	0.0010
	(0.0037)	(0.0046)	(0.0034)	(0.0033)
	Density of the Running Variable			
IRS score 2014	-0.0010	-0.0006	0.0006	-0.0000
	(0.0008)	(0.0008)	(0.0008)	(0.0008)

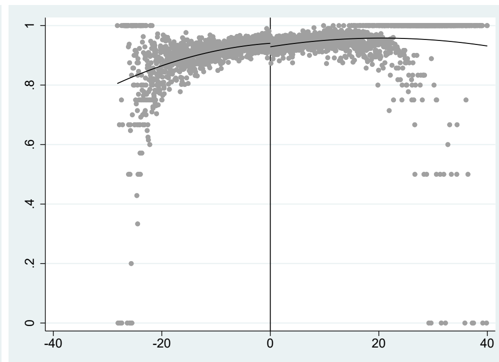
Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

The differences in effects between men and women are likely driven by the gender division of labor. In general terms, the costs associated with attending school appear to be similar for both men and women.[8] Nonetheless, the likelihood of joining the labor force increases for men after losing the BDH, while women face a decrease in the probability of both engaging in labor and attending school.

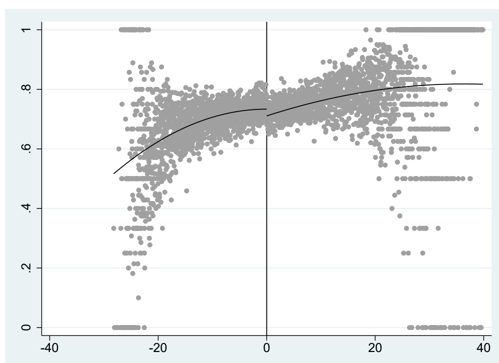
Figure 4: Probability of attending school around the threshold



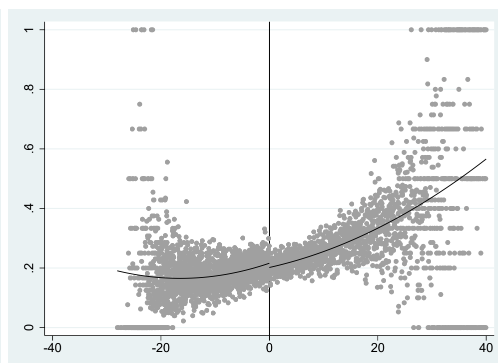
(a) Compulsory primary education



(b) Compulsory secondary education



(c) Voluntary secondary education



(d) higher education

Some NEETs may engage in household chores rather than being completely idle. Table 6 shows the effect of leaving the program on the probability of doing unpaid housework. Losing the BDH increases the likelihood of young women (15–18 years old) engaging in household chores by 3.06 percentage points at the intensive margin (a 17.16% relative increase with a participation rate of 17.83%), but no significant effects are found for men. The point estimate on the lower bound is still positive but not statistically significant. Therefore, under the worst-case assumption that all quitters to the left of the threshold are non-NEETs, we cannot rule out a null effect. However, the intensive margin reveals a negative unintended effect of losing the cash transfer by disproportionately pushing women into unpaid domestic work rather than education or employment. Our findings suggest that, after leaving the program, households' preferences shift from extending adolescents' and young adults' education to requiring their participation in paid and unpaid work to compensate for the loss of monetary transfers.

[8] We cannot explore this further due to data limitations, as the RS databases do not include information on household expenditures on goods or services.

Table 6
Results for housework

	All	Male	Female	Urban	Rural
	Voluntary secondary education (15 - 18 years)				
Standard RDD	0.0155***	-0.0007	0.0306***	0.0106*	0.0191***
s.e.	(0.0045)	(0.0038)	(0.0082)	(0.0062)	(0.0064)
n	736,761	374,660	362,101	416,830	319,931
Intensive margin	0.0163***	0.0005	0.0306***	0.0146**	0.019**
s.e.	(0.0047)	(0.0037)	(0.0085)	(0.0064)	(0.0084)
Lower bound	0.0056	-0.0034	0.0122	0.0043	0.0079
s.e.	(0.005)	(0.0039)	(0.0092)	(0.0068)	(0.0088)
Upper bound	0.1208***	0.0377***	0.2089**	0.1243***	0.1177***
s.e.	(0.0031)	(0.0026)	(0.0851)	(0.0046)	(0.0069)
n	965,379	498,439	466,940	542,674	422,705
	Higher education (19 - 24 years)				
Standard RDD	0.0006	-0.0022	0.0081	-0.0019	0.004
s.e.	(0.0063)	(0.0033)	(0.0093)	(0.0082)	(0.0094)
n	694,559	346,548	348,011	399,123	295,436
Intensive margin	-0.0008	-0.002	0.0071	-0.0067	0.0075
s.e.	(0.0064)	(0.0038)	(0.0107)	(0.0077)	(0.0103)
Lower bound	-0.0192***	-0.0045	-0.0259**	-0.0237***	-0.0124
s.e.	(0.0069)	(0.004)	(0.012)	(0.0082)	(0.0113)
Upper bound	-0.0008	0.0287***	0.0071	-0.0067	0.0075
s.e.	(0.0064)	(0.0025)	(0.0107)	(0.0077)	(0.0103)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

Overall, our results highlight the risks of removing beneficiaries from cash transfer programs at too low a threshold. On the policy side, Ecuador's BDH could adapt the Mexican *Prospera* and Colombian *Más Familias en Acción* systems by using different thresholds for entering and leaving the program. In this manner, households above the eligibility threshold would stop receiving the cash transfer, but those below a second vulnerability threshold would transition to a reduced support program (Medellín et al., 2015).

The effects of benefit loss from social programs in general, and cash-transfer programs in particular, is still an understudied and emerging area of research. The results presented in this study should be scrutinized in different settings to confirm their external validity. For the case of Ecuador's BDH, it would be of interest to study the longer-term effects of loss of beneficiary status and the likelihood of re-entering the program at a future date.

A Intensive margin and expected values under treatment and no treatment

Table A. 1
Results for School Attendance

	All	Male	Female	Urban	Rural
	Compulsory primary education (5 - 11 years)				
$E(Y_0S_0)$	0.9710***	0.9710***	0.9727***	0.9713***	0.9729***
s.e.	(0.0022)	(0.0033)	(0.0027)	(0.0029)	(0.0031)
$E(Y_1S_1)$	0.9608***	0.9592***	0.9639***	0.9575***	0.9664***
s.e.	(0.0023)	(0.0032)	(0.0028)	(0.0028)	(0.0033)
Intensive margin	-0.0102***	-0.0118**	-0.0088**	-0.0138***	-0.0064
s.e.	(0.0031)	(0.0047)	(0.0039)	(0.0040)	(0.0042)
n	890,084	452,916	437,168	503,870	386,214
	Compulsory secondary education (12 - 14 years)				
$E(Y_0S_0)$	0.9465***	0.9403***	0.9500***	0.9448***	0.9440***
s.e.	(0.0034)	(0.0046)	(0.0043)	(0.0043)	(0.0046)
$E(Y_1S_1)$	0.9269***	0.9153***	0.9389***	0.9214***	0.9354***
s.e.	(0.0032)	(0.0041)	(0.0039)	(0.0041)	(0.0046)
Intensive margin	-0.0197***	-0.0251***	-0.0111*	-0.0235***	-0.0086
s.e.	(0.0046)	(0.0061)	(0.0058)	(0.0058)	(0.0068)
n	1,102,110	561,943	540,167	623,218	478,892
	Voluntary secondary education (15 - 18 years)				
$E(Y_0S_0)$	0.7512***	0.7509***	0.7515***	0.7456***	0.7600***
s.e.	(0.0047)	(0.0059)	(0.0070)	(0.0070)	(0.0067)
$E(Y_1S_1)$	0.7081***	0.7135***	0.7043***	0.7020***	0.7160***
s.e.	(0.0051)	(0.0060)	(0.0075)	(0.0077)	(0.0081)
Intensive margin	-0.0431***	-0.0373***	-0.0473***	-0.0436***	-0.0440***
s.e.	(0.0071)	(0.0087)	(0.0104)	(0.0100)	(0.0101)
n	965,379	498,439	466,940	542,674	422,705
	Higher education (19 - 24 years)				
$E(Y_0S_0)$	0.2161***	0.1932***	0.2368***	0.2138***	0.2131***
s.e.	(0.0051)	(0.0084)	(0.0058)	(0.0066)	(0.0068)
$E(Y_1S_1)$	0.1984***	0.1798***	0.2186***	0.2015***	0.1959***
s.e.	(0.0036)	(0.0046)	(0.0053)	(0.0046)	(0.0054)
Intensive margin	-0.0177***	-0.0134	-0.0181**	-0.0123	-0.0172**
s.e.	(0.0062)	(0.0096)	(0.0076)	(0.0078)	(0.0087)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

Table A. 2

Results for Joining the Labor Force

	All	Male	Female	Urban	Rural
	Voluntary secondary education (15 - 18 years)				
$E(Y_0S_0)$	0.1553***	0.2184***	0.087***	0.1499***	0.1635***
s.e.	(0.0039)	(0.0059)	(0.0043)	(0.0051)	(0.0059)
$E(Y_1S_1)$	0.1721***	0.2466***	0.0922***	0.1628***	0.1874***
s.e.	(0.0034)	(0.0063)	(0.0039)	(0.004)	(0.0061)
Intensive margin	0.0169***	0.0282***	0.0052	0.013**	0.0239***
s.e.	(0.0052)	(0.0089)	(0.0059)	(0.0064)	(0.0083)
n	965,379	498,439	466,940	542,674	422,705
	Higher education (19 - 24 years)				
$E(Y_0S_0)$	0.5272***	0.7497***	0.2997***	0.513***	0.5445***
s.e.	(0.0055)	(0.0066)	(0.0084)	(0.0068)	(0.0084)
$E(Y_1S_1)$	0.5406***	0.7624***	0.308***	0.5327***	0.554***
s.e.	(0.0044)	(0.0051)	(0.0054)	(0.0052)	(0.0073)
Intensive margin	0.0134*	0.0128	0.0083	0.0197**	0.0095
s.e.	(0.0069)	(0.008)	(0.0099)	(0.0084)	(0.011)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

Table A. 3

Results for not in education, employment, or training (NEET)

	All	Male	Female	Urban	Rural
	Voluntary secondary education (15 - 18 years)				
$E(Y_0S_0)$	0.1409***	0.0941***	0.1915***	0.144***	0.1368***
s.e.	(0.0039)	(0.0041)	(0.0068)	(0.0057)	(0.0053)
$E(Y_1S_1)$	0.1653***	0.1029***	0.2317***	0.1695***	0.1586***
s.e.	(0.0036)	(0.0040)	(0.0060)	(0.0042)	(0.0064)
Intensive margin	0.0243***	0.0088	0.0402***	0.0255***	0.0218***
s.e.	(0.0056)	(0.0057)	(0.0091)	(0.0068)	(0.0082)
n	965,379	498,439	466,940	542,674	422,705
	Higher education (19 - 24 years)				
$E(Y_0S_0)$	0.2996***	0.1062***	0.4923***	0.3129***	0.281***
s.e.	(0.0053)	(0.0057)	(0.0100)	(0.0071)	(0.0078)
$E(Y_1S_1)$	0.3003***	0.1071***	0.5029***	0.3051***	0.292***
s.e.	(0.0038)	(0.0038)	(0.0056)	(0.0044)	(0.0069)
Intensive margin	0.0007	0.0008	0.0106	-0.0078	0.0111
s.e.	(0.0066)	(0.0066)	(0.0114)	(0.0084)	(0.0103)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

Table A. 4
Results for housework

	All	Male	Female	Urban	Rural
			Voluntary secondary education (15 - 18 years)		
$E(Y_0 S_0)$	0.1045***	0.0372***	0.1783***	0.1097***	0.0987***
s.e.	(0.0034)	(0.0027)	(0.0062)	(0.0049)	(0.0048)
$E(Y_1 S_1)$	0.1208***	0.0377***	0.2089***	0.1243***	0.1177***
s.e.	(0.0031)	(0.0026)	(0.0057)	(0.0046)	(0.0069)
Intensive margin	0.0163***	0.0005	0.0306***	0.0146**	0.019**
s.e.	(0.0047)	(0.0037)	(0.0085)	(0.0064)	(0.0084)
n	965,379	498,439	466,940	542,674	422,705
			Higher education (19 - 24 years)		
$E(Y_0 S_0)$	0.2478***	0.0307***	0.4683***	0.2609***	0.2291***
s.e.	(0.0053)	(0.0029)	(0.0091)	(0.0065)	(0.0078)
$E(Y_1 S_1)$	0.247***	0.0287***	0.4754***	0.2542***	0.2366***
s.e.	(0.0037)	(0.0025)	(0.0057)	(0.0042)	(0.0065)
Intensive margin	-0.0008	-0.002	0.0071	-0.0067	0.0075
s.e.	(0.0064)	(0.0038)	(0.0107)	(0.0077)	(0.0103)
n	969,978	503,480	466,498	549,182	420,796

Notes: Results show bias-corrected estimates conditional on compliers at the 2014 IRS score equal to 28.20351; Bootstrapped standard errors are in the parentheses; ***, ** and * denote statistical significance at the 1%, 5% and 10%, respectively

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