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Money is not enough: Unintended negative effects of cash transfer design

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Abstract

The effectiveness of cash transfer programs to foster social mobility in the medium and long run is still unclear. Using a RDD we found that after six years of exposure to the Ecuadorean cash transfer, living conditions of beneficiaries are worse off than non-beneficiaries. We argue that it is the mechanism to evaluate continuity that incentivizes households to remain poor. Continuity is evaluated every 4-6 years based solely on a proxy-means score and not on whether households are on a path towards escaping poverty. Furthermore, households do not know how the score pis estimated and their proximity to the cutoff. This creates uncertainty on the side of beneficiaries, who take long-term suboptimal decisions to maximize their short-term utility. We also estimate the effect of the old-age pension's branch of the program, whose beneficiaries do not face uncertainty about their continuity, finding no negative effects for that branch.

Key words: cash transfer, program design, long-term impact, proxy-means-test, Ecuador

JEL codes: I38, H53, C14, D81

1. Introduction

Cash transfers have been created as mechanisms to help poor and vulnerable households meet their immediate needs, but also as instruments to break the intergenerational transmission of poverty. Cash transfers are expected to affect household consumption and investment decisions including the accumulation of human capital using education and health services. Human capital investments are expected to increase the employability of the next generation, achieve better living conditions and break the poverty trap.

There is abundant evidence of the positive impacts of cash transfers in the short-run, for both the unconditional and conditional provision of the transfer.[1] Most studies have found an increase in household consumption and a reduction of monetary poverty (Bastagli et al., 2016; Fiszbein & Schady, 2009; Wydick, 2018). The evidence on human capital accumulation, measured by school attendance or enrolment, is encouraging in most countries (Baird, McIntosh, & Özler, 2019; Bastagli et al., 2016; Fiszbein & Schady, 2009), and the transfers likewise reduce the prevalence of child labor (Baird, McIntosh, & Özler, 2019; Millán, Barham, Macours, Maluccio, & Stampini, 2019).

Hence, the evidence is solid for short run gains particularly with respect to schooling, but also in relation to other dimensions such as fertility and marriage decisions of young adults (Baird et al., 2019; Millán et al., 2019). However, if future generations are to benefit from cash transfer investments, program effects should be long-lasting and their effects measurable in the medium and long term. Within six to eight years after program participation, many studies show evidence of a sustained positive effects in terms of human capital accumulation. However, some studies also show that short run effects fade out in the medium run (Araujo, Bosh, & Schady, 2016; Millán,

[1] Unconditional Cash Transfers (UCT) provide money with no conditions attached. Conditional Cash Transfers (CCT) require beneficiaries to comply with certain conditionalities often related to school enrolment, health check-ups and nutrition.

Barham, Macours, Maluccio, & Stampini, 2019; Paredes-Torres, 2017; Wydick, 2018). Evidence of a sustained effect on human capital in the long-run is even less clear-cut (Millán et al., 2019; Molina-Millan, Barham, Macours, Maluccio, & Stampini, 2016). Most studies have not been able to find lasting positive effects in either physical or cognitive development. Intergenerationally, there is no clear evidence of any improvement in the employability and income of young adults that received the transfer while growing up (Millán et al., 2019).[2]

Several studies have analyzed to what extent program design matters for the outcomes. Studies on the differences between conditional and unconditional cash transfers show, for instance, that conditional cash transfers tend to have larger effects on human capital than unconditional transfers (Baird et al., 2019; Fiszbein & Schady, 2009; Wydick, 2018), and that the effects are more sustainable in the long run (Wydick, 2018). However, program design not only refers to whether the households must take some action, but also the duration of the transfer, the amount, the periodicity, the targeting and enforcing mechanisms, or the lifecycle period during which the transfer will be delivered.[3] Experimental evidence from the *Subsidios* program in Bogota, Colombia, shows that children that participated in the "savings" treatment, where a portion of the transfer payment was deferred until high school graduation, had a higher probability of taking the graduation exam compared to the group that received the same amount but without delayed payments (Millán et al., 2019). However, the effect was only found among children in upper secondary school, but not for those that received the transfer during lower secondary school (Millán et al., 2019). Consequently, the expectations created by the program as well as the lifecycle timing of it can increase the impact.

[2] Some authors claim that the lack of effects could be due to beneficiaries still attending school, which can delay their incorporation into the labor market (Millán et al., 2019; Molina-Millan et al., 2016).

[3] Conditions beyond the program itself also influence the effects. For instance, in countries where employment opportunities are limited the returns might be smaller (Wydick, 2018).

The effects on expectations can impact their behavior and decisions in relation to the labor market, savings and consumption, and the accumulation of human capital.

This paper focuses on the expectations that can arise due to program design. It answers the question how the design and application of a targeting mechanism can create unintended incentives that eventually lead to negative program outcomes using the Ecuadorian *Bono de Desarrollo Humano* (BDH) cash transfer program as the study case.

The BDH provides, among others, transfers to poor households (mothers) with children. Although the program is labeled as a conditional cash transfer, the conditionalities are not enforced. Hence, the incentives households face are similar to an unconditional transfer. Program eligibility is determined by a score of living condition variables (proxy-means test) that predict the level of household consumption. The transfer is delivered monthly during a period of four to six years, after which households must undergo the proxy-means test again. Program continuity depends solely on the household's poverty status. In other words, maintaining the benefit is conditional on remaining poor. Households are not aware of the exact calculation of the score, so they do not know how far they are from the eligibility threshold and, therefore, how large their window of improvement is without being expelled from the program.

Using data from the social registry of Ecuador from 2008 to 2014, we estimate the effects of the BDH for poor households with children (mother's program) on several living condition indicators. We find that the mother's program induces negative outcomes in terms of asset possession and other structural living conditions. We identify the population groups and dimensions that drive the negative results, finding that although small positive effects are found for certain groups, the general effect is negative. We conclude that the design of the BDH creates uncertainty and punishes improvements in living conditions with the expulsion from the program. Furthermore, the stakes of

losing the transfer are enhanced by the increase of the transfer value over time in both absolute and relative terms. This might have led households to maximize their chance to remain beneficiaries in exchange for larger long-term returns. Such decisions are not beneficial for the long-term utility of households, but they make sense in the context of a large discount rate of future flows, which has been documented for people in poverty (Haushofer & Fehr, 2014; Wydick, 2018). We test the uncertainty hypothesis by estimating the effects of the old-age pension's branch of the program, whose beneficiaries do not face uncertainty about their continuity. We found that when no uncertainty is faced, there are no negative effects.

This paper contributes to the literature by adding to the evidence of medium-term effects of cash transfer programs and to the discussion of the role that the mechanisms to assess continuity might have in the medium and long-term. We particularly argue and present evidence that proxy-means testing is not a good tool for assessing continuity on cash transfer programs as it creates uncertainty in families and incentivizes them to remain poor to maximize their chance to keep the benefit. Such incentives eliminate the short-term gains and have negatively impacted Ecuadorean poor families as they maximize their likelihood to keep the benefit in exchange of long-term investments that might improve their livelihood and their fight against their poverty trap.

The remainder of this paper is structured as follows. The second section provides details on the case study and the design of the BDH. We then introduce the data and the methodology. We describe the results of the mother's program in the fourth section. The fifth section is a discussion of the results of the fourth section. We test the uncertainty hypothesis using data for the old-age BDH program in the sixth section. Finally, we highlight the main conclusion in the last section.

2. The case study - Bono de Desarrollo Humano

In 1998 the Ecuadorean government implemented a cash transfer program designed to compensate households for the elimination of fuel and electricity subsidies. The subsidies, most of which were highly regressive[4] (Palacios, Jácome, Patiño, & Cisneros, 2017), were never removed but the cash transfer program stayed in place.

In 2003, the government decided to target the program towards the poor and other vulnerable groups, resulting in three program modalities: One for poor families with children (mother's program), one for people with disabilities, and one targeted to the elderly (old-age pension program). Under the first modality, families were asked to send minors to school and have monthly health checks, but these conditions were neither monitored nor enforced. Program eligibility was based on a multidimensional score, called RS score[5], that predicted the consumption level of households. New rounds of the proxy-means testing procedure took place in 2008 and 2014[6]. The eligibility threshold for the mother's program was equivalent to the poverty line in the 2003 and 2008 waves. In 2014, it was equivalent to the extreme poverty line (SIISE, 2009, 2014).

While the old-age pension was initially meant for all elders, after 2006 only the poor elders were eligible for the social pension (Decreto ejecutivo 1824. September 1st 2006). In 2009/2010 the threshold for the old-age pension was raised to 53 (the cutoff for the mother's program remained at 36.6) and elderly with a contributory pension were no longer eligible (Acuerdo Ministerial No.293. Ministerio de Inclusión Económica y Social. March 31st 2010). In 2012, eligibility for the old-age pension did no longer depend on the RS score, but later in the same year new inclusions were banned due to financial constraints. Despite the ban, recipients kept receiving the pension as long

[4] Gasoline subsidies are particularly concentrated among the rich households (Palacios et al., 2017).
[5] In 2003 the score was called SELBEN index, but since 2008 it has changed its name to RS index/RS score.
[6] A new wave has started in 2018 but has not been concluded at the submission of this paper.

as they were not public servants earning more than USD 280 a month and did not receive a contributory pension.

Table 1 Number of total beneficiaries by year

Modality	Period	Active beneficiaries	Entered after	Left after	Net inclusions for the next period
Mother's program	Jan-2009	1,001,919	535,551	356,412	179,139
	Dec-2010	1,181,058	105,094	82,945	22,149
	Dec-2012	1,203,207	3,044	761,689	(758,645)
	Dec-2014	444,562			
Old-age pension	Jan-2009	272,913	247,287	23,301	223,986
	Dec-2010	496,899	153,335	62,085	91,250
	Dec-2012	588,149	16,081	57,411	(41,330)
	Dec-2014	546,819			

Source: Statistics of the Ministry of Social Inclusion of Ecuador

In January 2009, the mother's program of the BDH had around one million beneficiaries, and the old-age pension about 0.3 million (Table 1). After the changes introduced throughout 2009 and 2010 (recalibration of the PMT in 2009 based on new survey data from 2008 and increase of old-age pension threshold), 179,139 more families were registered in the mother's program by the end of 2010, and 223,986 persons in the old-age pension. In late 2012, new inclusions were banned for the old-age pension, and in 2014 the eligibility criteria for the mother's program was considerably tightened. The latter resulted in the exclusion of more than 750,000 families (Table 1).[7]

The amount of the transfer has changed over time mostly due to political decisions. In 2003, the transfer was a monthly sum of USD 15 per family for the mother's program and USD 11.50 per individual for the other two modalities. In 2007, the monthly transfer was raised to USD 30 for all three programs, then further increased to USD 35 in 2009 and to USD 50 in 2012. In 2018 a variable amount was added for the mother's program depending on the number of children in the household

[7] Exclusions in the old-age pension program were primarily due to the decease of the beneficiaries.

and their age (with a maximum transfer of USD 150 only for new inclusions), while the old-age and disability pension remained at USD 50 per month.

Previous studies on the impact of the BDH program towards mothers have shown positive short-run effects with respect to school enrolment (Araujo & Schady, 2008) and a reduction of child labor (Edmonds & Schady, 2012). Positive effects on the cognitive or physical development of children as well as learning outcomes were only found for very specific groups of the population (Paxson & Schady, 2010; Younger, Ponce, & Hidalgo, 2009) or were non-existent (Ponce & Bedi, 2010). In terms of consumption, there is evidence of an increase in the share of food intake in total consumption (Schady & Rosero, 2008)[8].

With respect to medium and long-term effects, existing evidence is less encouraging. High-school completion rates for women have increased by two percentage points, but no effects were found for men (Araujo et al., 2016). Likewise, the program had no effect on cognitive and learning dimensions and did not result in increased university attendance (Araujo et al., 2016). In addition, Paredes-Torres (2017) found that all short-run gains in human capital accumulation had disappeared in the medium term contesting the results of Araujo, Bosh, & Schady (2016). In terms of employability, previous studies could not establish any program effect on occupation rates (Araujo, Bosch, Maldonado, & Schady, 2017; Paredes-Torres, 2017), except for a reduction in the likelihood of having a formal job (Araujo et al., 2017). Finally, although there is evidence of positive upward mobility in the medium term (Mideros & Gassmann, 2017), this cannot be explained by the cash transfer. On the contrary, there is evidence of negative impacts on the general living conditions of beneficiaries (Molina Vera & Oosterbeek, 2017; Sánchez, 2017).

[8] No evidence was found for an increase of undesirable consumption like alcohol and tobacco (Nabernegg, 2012).

Our aim is to find an explanation for the negative results obtained by Molina Vera & Oosterbeek (2017) and Sánchez (2017). We hypothesize that the design of the program creates incentives that affect beneficiaries' expectations. They maximize the probability to remain beneficiaries in exchange of long-term investments. This decision is consistent with a large discount rate of future cash flows among poor households (Carvalho, 2010; Haushofer & Fehr, 2014; Lawrance, 1991). The latter implies that they prefer short over long-term gains. This is socially suboptimal as future generations are less likely to break the poverty trap.

3. Data and Methodology

The data in this paper stem from the social registry (*Registro Social)* of Ecuador, which oversees the proxy-means testing mechanism. Two main sources of information are used: the social registry household surveys used for the calibration of the RS score and the lists of beneficiaries from administrative data. Data for the social registry surveys are only collected in high-poverty areas[9]. Hence, the analysis in this paper is representative for households living in those areas and households that directly approach the government to assess their eligibility despite not living in territories selected by the government.

We use the social registry surveys of 2008 and 2014[10] and the list of actual beneficiaries at four points in time: January 2009, December 2010, December 2012 and December 2014. The unit of analysis is the household.[11] Despite new survey data for the social registry was collected in 2008, the January 2009 beneficiary list was still based on the 2003 survey because of delays with the

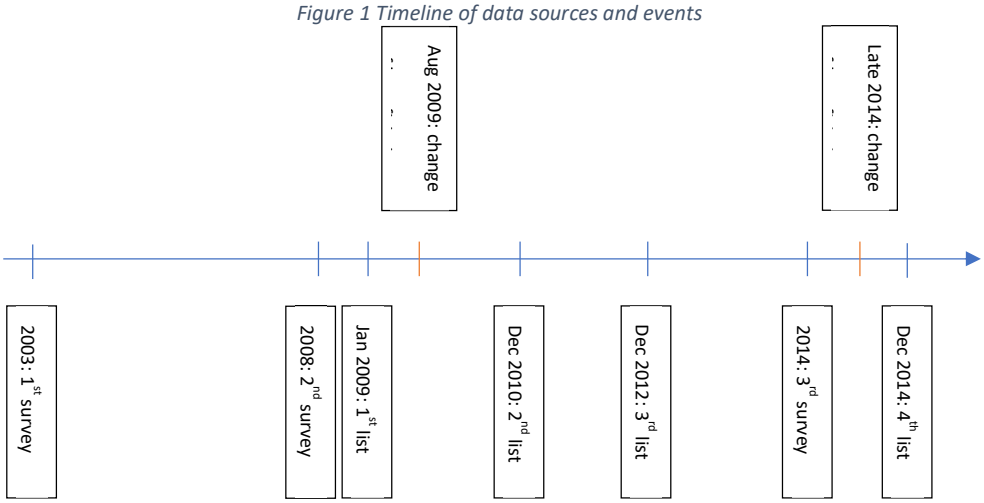
[9] Identified by a small area estimation (SAE) of poverty using the Population Census and the Living Conditions National Survey.

[10] The 2003 survey had to be excluded from the analysis. The lack of identification numbers created a large imbalance between treated and control observations because observations could not be matched longitudinally.

[11] Due to the lack of some individual identifiers, all information is aggregated at the household level using the ID of the household head or the spouse.

calibration and implementation of the new threshold until August 2009 (Molina Vera & Oosterbeek, 2017). The January 2009 list allows us to identify all households that were exposed to the treatment before 2009 and lets us identify different samples for the subsequent analysis. An illustrated timeline of data sources and events is presented in Figure 1.

Figure 1 Timeline of data sources and events



We measure the effects of losing and gaining the cash transfer on living conditions because they might be non-symmetrical (Buser, Oosterbeek, Plug, Ponce, & Rosero, 2017; Molina Vera & Oosterbeek, 2017). For this purpose, we construct two samples of households with children younger than 15 years old in the 2008 social registry survey data, the baseline for our analysis. *Sample M1* are those households surveyed in 2008 and 2014 that *did not receive* the transfer between 2003 and 2009. With this sample we aim to measure the effects in 2014 of receiving the transfer since mid-2009. *Sample M2* are households surveyed in 2008 and 2014 that *did receive* the transfer between 2003 and 2009. With this second sample we aim to measure the effects in 2014 of having lost the transfer in 2009. A household is labeled as beneficiary of the mother's program in the period 2003-2009 if it was listed in the January 2009 administrative list. A household is labeled as beneficiary of the mother's program in the period 2009-2014 if it appeared in either the 2010 or 2012 administrative beneficiary lists.

Given that non-eligible people are less likely to have their identification document number registered in the RS survey[12], the likelihood of matching baseline and follow up records of this group is smaller compared to the eligible group. This creates an artificial imbalance between treated and non-treated households when the 2008 and 2014 surveys are merged. We solved this by keeping only eligible and non-eligible observations with the same likelihood of being followed unconditional on eligibility or treatment status. A detailed explanation of the correction process is reported in Annex 1.

Table 2 Samples for analysis of mother's program

	# of households	# of people (in 2008)
Sample M1 (receivers 08-14)	202,300	960,925
Sample M2 (excluded 08-14)	315,917	1,796,425

Source: Registro Social Ecuador

Table 3 Distribution of the final sample

Sample M1: Households interviewed in 2008, evaluated in 2014 that had not received the transfer before 2009 (shaded areas refer to targeting errors)

	Above the cutoff	Below the cutoff	Total
Non-beneficiary of BDH	46,711	5,957	52,668
Beneficiary of BDH	8,550	141,082	149,632
Total	55,261	147,039	202,300

Sample M2: Households interviewed in 2008, evaluated in 2014 that had received the transfer before 2009

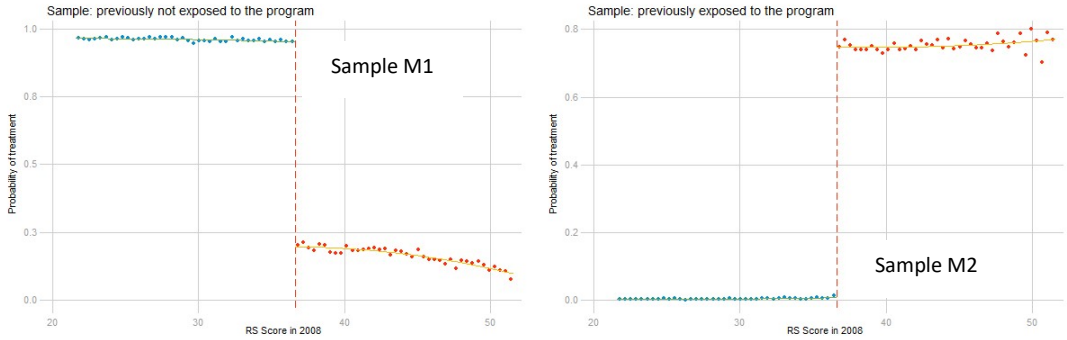
	Above the cutoff	Below the cutoff	Total
Kept the benefit	10,905	270,577	281,482
Lost the benefit	33,240	1,195	34,435
Total	44,145	271,772	315,917

Source: Registro Social Ecuador

[12] ID numbers are registered during the in-person survey or by the office after the assessment of eligibility. Households not deemed eligible will not be contacted and are therefore less likely to complete the information regarding their ID number.

Table 2 shows the final number of households included in each sample for which we have all the required information, that are observed in both periods of time and that have the same likelihood of being followed.

Figure 2 Probability of receiving the BDH in function of the RS Score in 2008 (of being excluded of the program for sample M2)



Source: Registro Social Ecuador

In Table 3 samples M1 and M2 are further disaggregated according to the households' position with respect to the eligibility threshold in 2008, and the households' treatment status between 2009 and 2014. In 2008 the eligibility threshold was established at 36.6 points of the RS score. The treatment status and the position relative to the cutoff point are highly correlated, though not perfectly explained by each other. In sample M1, 92.8% of the households are correctly targeted, and in sample M2 96.2% of the households are. As shown in Figure 2, there is a distinct discontinuity around the eligibility threshold with respect to the probability of receiving the transfer, which will be exploited in the subsequent analysis.

3.1. Empirical strategy

We use a regression discontinuity design (RDD). Given the change in the probability of treatment at the cutoff (Figure 2), if the transfer had an effect, a discontinuity should also be found in the outcome variables (y_i) at the same point. If all eligible households received the transfer, that discontinuity could be measured by the following model:

$$y_i = \alpha + f(s_i) + \beta T_i + u_i \quad (1)$$

where y_i is the outcome variable of interest, which for this study is a series of outcomes reflecting the living conditions of households. The most important outcome is the RS score in 2014 as a summary measure of well-being and consumption, as well as its components. s_i is the score/assignment variable rescaled so the cutoff equals to 0. s_i stands for the RS score. $f(s_i)$ is a function of s_i that captures the relationship between s_i and y_i. T_i is a dummy variable that takes the value of 1 if household *i* received the treatment between the baseline and the follow up survey according to their respective sample. For Sample M1, $T_i = 1$ if household *i* received the mother's program transfers between 2009 and 2014; and for Sample M2, $T_i = 1$ if household *i* lost the mother's program transfer between 2009 and 2014. α and β are parameters to be estimated, whereby β estimates the marginal treatment effect; and u_i is an error term, which represents all variables related to the outcome that were not included in the model.

Because not all eligible households receive the transfer, the fuzzy regression discontinuity design is required (Cameron & Trivedi, 2005). T_i cannot be thought of as locally exogenous in (1), and, hence, the estimation of β would lead to bias and inconsistent results. To solve this, we use D_i as an instrument for T_i, which leads us to estimate the following system of equations:

$$\begin{aligned} y_i &= \alpha + f(s_i) + \beta T_i + u_i \\ T_i &= \delta + g(s_i) + \gamma D_i + v_i \end{aligned} \quad (2)$$

Where D_i is a dummy variable that takes the value of 1 if household *i*'s RS score lies on the eligible side of the distribution of the score variable; $g(s_i)$ is a function of the conditional relationship between s_i and D_i; δ, γ, are parameters to be estimated and v_i is an error term.

We fit a non-parametric local Wald estimator (Imbens & Kalyanaraman, 2009; Imbens & Lemieux, 2008) so $f(s_i)$ and $g(s_i)$ are better fitted and the risk of mis-parametrization is minimized (Jacob, Zhu, Somers, & Bloom, 2012).

Non-parametrically, Imbens & Kalyanaraman (2009) and Imbens & Lemieux (2008) express the RDD Wald estimator as[13]:

$$\hat{\beta}_{wald} = \frac{lim_{s_i \uparrow 0} E[y_i|s_i] - lim_{s_i \downarrow 0} E[y_i|s_i]}{lim_{s_i \uparrow 0} E[T_i|s_i] - lim_{s_i \downarrow 0} E[T_i|s_i]} \quad (3)$$

$f(s_i)$ and $g(s_i)$ are fitted using local linear regressions above and below the cutoff (G. Imbens & Kalyanaraman, 2012)[14]. For stability testing, the local Wald estimator is fitted for half and double the optimal bandwidth, which did not affect our conclusions (see Supplementary Annex 2). We also run the estimation parametrically using a third-degree polynomial of s_i on both stages, which does not affect the conclusions either.

The main outcome is the RS score in the year 2014, which is the assignment variable (s_i) in the baseline. This might raise some concerns of endogeneity. However, this only poses a threat when both are determined simultaneously (Bajari, Hong, Park, & Town, 2011). For our application there is no simultaneity as the past score determines the future, but not otherwise. Further, although s_i is endogenous (i.e. $E[u_i|s_i] \neq 0$), we only require D_i to be conditionally exogenous in the vicinity of the cutoff. That is, as long as s_i captures all the endogeneity of D_i, it does not matter if $E[u_i|s_i] \neq 0$. Other studies use the same variable as assignment variable (score) and outcome, but in different times (see Jacob & Lefgren (2004), Matsudaira (2008) and Lee (2008) as examples and Lee & Lemieux

[13] Cameron & Trivedi (2005), Imbens & Kalyanaraman (2009) and Imbens & Lemieux (2008) write the estimator as the superior limit minus the inferior limit because they consider the treatment to be given above the cutoff. However, for the current analysis, the treatment is delivered to those below the cutoff. Nevertheless, even if run in the other way, signs cancel off and the resulting parameter is the same.

[14] We use the code developed by Nichols (2011) for Stata, which also provides an algorithm to choose the optimal bandwidth based on Imbens & Kalyanaraman (2009).

(2010) for a deeper review), none of which raises a major concern; Jacob & Lefgren (2004) explain this in the following sentence: "*Because treatment* [(intention to treat in our case)] *is perfectly correlated with observable characteristics* [(the assignment variable)]*, it is orthogonal to unobservable characteristics* [(the error term)]*"* (Jacob & Lefgren, 2004, p. 230).

The estimated parameter will identify the marginal treatment effect (MTE). This is the effect of receiving the transfer at the cutoff given that people follow the assignment to treatment determined by D_i. This is seen as a strong limitation as external validity is limited. Nonetheless, the external validity of results might be larger if the estimation error of the assignment variable is large (Jacob et al., 2012; Lee & Lemieux, 2010). That is, the RDD estimator can be thought as a weighted average treatment effect among those represented around the cutoff point. The distribution of weights depends on how exactly the assignment variable measures the underlying condition that it is supposed to quantify (Lee & Lemieux, 2010). The higher the uncertainty of the estimation of the underlying condition, the more generalizable results are because more heterogenous people would be represented around the cutoff point (Jacob et al., 2012; Lee & Lemieux, 2010).

For our case, the RS score is expected to predict the level of consumption and the cutoff point to be an estimation of the poverty line. However, due to the estimation errors, people with the same RS score can have different levels of consumption and therefore be heterogenous among them. Those in the second and third quintiles of the RS score of 2008 (around the cutoff point) are represented by people of all quintiles of consumption (SIISE, 2009) with extra weight among those of the low consumption quintiles[15]. This implies that there is a high level of uncertainty about the level of consumption of those around the cutoff point, so people with quite different levels of consumption

[15] Quintile 2 of the RS score is comprised of 28% of people of the 1st quintile of consumption, 35% of the second, 25% of the third, 9% of the fourth and 2% of the fifth. For the third quintile of the RS score the composition is 11%, 29%, 33%, 21% and 6%, respectively. SIISE (2009) does not report the correlation with the per capita consumption.

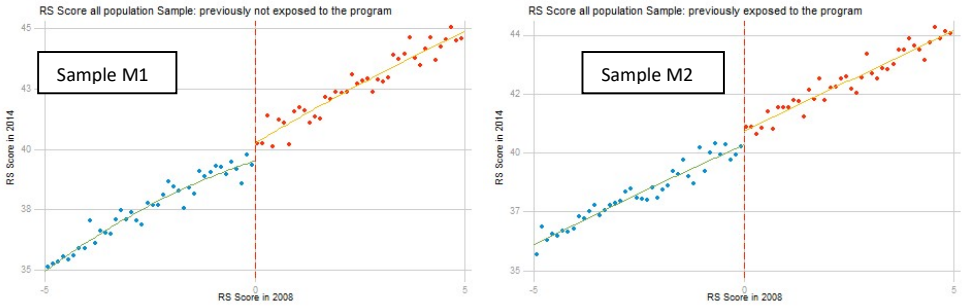
are included in the estimation and the estimated effects can be considered as a weighted random assignment design (Jacob et al., 2012; Lee, 2008).

The fact that the RS score has an error of estimation might also raise concerns about its usefulness as outcome variable. However, as long as the estimation error is continuous at the cutoff point, there is no major concern. We also run the estimation for the deterministic components of the RS score to better understand the results we obtain, so no estimation error can influence on them.

4. Results

Eligible households show a lower welfare index in 2014 compared to non-eligible households for samples M1 or M2 (Figure 3). Formally, households exposed to the transfer showed significantly worse results in 2014 than those not exposed to it (Table 4). The results of sample M1 show the impact in 2014 of receiving the program between 2009 and 2014; and those for sample M2 show the effects in 2014 of exclusion in 2009. Negative results are concentrated among people identified as mestizo[16], in urban areas and in the coast regions (Table 4). Results remain stable for double and half the bandwidth (see Supplementary Annex 2) and all robustness tests validate the strategy (see Annex 2).

Figure 3 Discontinuity at the cutoff on the RS Index in the follow up survey for each sample



Source: Registro Social Ecuador

[16] Given that the mestizo population represents the larger proportion in the Ecuadorian population (72.2% in the last census in 2010 and 73% on the used sample), their results heavily impacts the general estimation.

Although the overall result is negative and significant, not all groups have negative outcomes. The effect for households in the Amazon region is positive, though sensitive to the bandwidth selection. The coefficients for indigenous and rural populations are also positive, but not significant.

Most of the significant effects measured for sample M2 have the reverse sign for sample M1 (Table 4), implying symmetric effects for most groups, although smaller in size. Contrary to most groups, the impact of receiving the transfer is not significant for the indigenous population, but the effect of losing the transfer is negative and significant. This lack of symmetry is similar to that reported by Buser et al. (2014) that found that exclusion impacted negatively on the physical growth of children, while inclusion did not show any significant impact on the same dimension in a four-year timeframe.

Table 4 Non-parametric estimation of the marginal effect of receiving BDH on RS welfare score

		Receiving 2008 (Sample M1)	Losing 2008 (Sample M2)
National	National	-0.92**	0.64**
Ethnicity of HH head	Indigenous	1.17	-2.44*
	Afroecuadorean	-0.04	-0.20
	Montubio	-1.15	1.23+
	Mestizo	-0.87**	0.81**
	White/other	-1.48	1.16
Sex of HH head	Male	-0.94**	0.68**
	Female	0.19	0.56
Area of residence	Urban	-1.23**	0.85**
	Rural	0.47	0.24
Education of HH head	None	0.27	0.31
	Primary	-0.32	0.46
	Secondary	-1.39**	0.69+
	Superior	-0.86	1.39
Geographical region	Southern highlands	-0.97	0.40
	Central highlands	-0.36	-0.07
	Northern highlands	-5.15	5.03+
	Southern coast [1]	-1.03**	0.96**
	Northern coast	-1.44+	0.47
	Amazon	4.78*	-1.59
Receives other BDH transfer	No other BDH program	-1.11**	0.68**
	Receives other BDH program	1.75	0.22
Number of children in the household	No children	-0.89	0.21
	1 child	-1.49**	0.8*
	2 children	-0.92*	0.7+
	+2 children	-0.28	0.59

**1% significance, *5% significance, +10% significance

[1] It includes the Galapagos Islands

Source: Registro Social Ecuador

Because negative results concentrate on the Coast, in Table 5 we estimated the effects for sample M1 separately for households living on the Coast (74.7% of the sample) and for those living in other regions of the country. None of the estimated coefficients is significant for the subsample of households living in other areas. On the other hand, most of the coefficients that were negative at the national level, are also negative for the Coast with larger absolute values, implying that the coastal region is driving the negative results at the aggregate level.

Table 5 RDD estimation of the effects on the RS welfare score in 2014 of receiving the transfer in 2008 separated by region

		Other regions	Coast
Regional	Regional	-0.19	-1.1**
Ethnicity of HH head	Indigenous	1.63	0.19
	Afroecuadorean	2.82	-0.11
	Montubio	-3.81	-1.07
	Mestizo	-0.22	-1.09**
	White/other	-1.37	-1.63
Sex of HH head	Male	-0.27	-1.1**
	Female	1.86	0.02
Area of residence	Urban	-1.00	-1.35**
	Rural	0.71	0.30
Education of HH head	None	7.29	-0.58
	Primary	-0.16	-0.34
	Secondary	0.01	-1.68**
	Superior	-0.22	-0.76
Receives other BDH transfer	No other BDH program	-0.10	-1.37**
	Receives other BDH program	-0.15	2.55
Number of children in the household	No children	-0.72	-0.86
	1 child	-0.18	-1.59**
	2 children	-0.44	-1.06*
	+2 children	1.11	-0.40

**1% significance, *5% significance, +10% significance

Source: Registro Social Ecuador

Even though the construction of the RS score varies slightly across waves, the main dimensions remain comparable over time and can be summarized in the following broad categories: dwelling conditions (construction materials and access to basic services); geographic surroundings (poverty incidence in the household's vicinity and rurality); demographics of the household (number of people, number of children younger than 15 and the level of instruction of the head of the household); asset ownership (number and type of fixed assets the household possesses); and, schooling (number of children 5 to 15 that attend school and whether they attend to a private or

public school). Using the score weights of the 2008 wave, we reconstructed the different dimensions for the analysis of the RS components.[17] A detailed list of the dimensions considered in the 2008 proxy-means score is presented in Supplementary Annex 1.

Table 6 Effects of receiving the BDH on the components of the RS18 score between 2008 and 2014 (Sample M1)

		Dwelling	Assets	Demographics	Schooling
National	National	-0.25	-0.52**	0.01	-0.07
Ethnicity of HH head	Indigenous	0.18	1.48*	-0.10	-0.06
	Afroecuadorean	-0.05	0.04	-0.32	-0.17
	Montubio	0.14	-2.01**	-0.11	-0.27+
	Mestizo	-0.27	-0.49**	0.03	-0.04
	White/other	-0.86	-0.53	0.56*	-0.07
Sex of HH head	Male	-0.27	-0.45**	-0.01	-0.07
	Female	0.83	-1.59*	0.49+	0.11
Area of residence	Urban	-0.51*	-0.7**	-0.01	-0.09+
	Rural	0.89*	0.15	0.08	-0.01
Education of HH head	None	0.41	-1.93	0.45	0.12
	Primary	0.01	-0.11	0.09	-0.08
	Secondary	-0.43+	-0.69**	-0.23**	-0.12+
	Superior	-0.02	-1.13*	0.12	0.04
Geographical region	Southern highlands	0.01	0.21	0.05	-0.41*
	Central highlands	0.06	-0.19	-0.11	-0.16
	Northern highlands	-0.60	-1.82	-0.86	-0.13
	Southern coast[1]	-0.29	-0.76**	0.02	-0.03
	Northern coast	-0.55	-0.41	0.01	-0.06
	Amazon	2.85*	1.39	0.73+	0.38
Receives other BDH transfer	No other BDH program	-0.38+	-0.53**	-0.02	-0.08+
	Receives other BDH program	1.31	-0.11	0.08	0.01
Number of children in the household	No children	-0.15	-0.16	-0.08	-0.10
	1 child	-0.7*	-0.52	-0.06+	-0.02
	2 children	-0.15	-0.85**	-0.03	-0.03
	+2 children	0.40	-0.40	0.15+	-0.14*

**1% significance, *5% significance, +10% significance

[1] It includes the Galapagos Islands

Source: Registro Social Ecuador

Most of the negative coefficients are found in relation to the possession of assets, such as certain appliances or vehicles (Table 6). This implies that if truthfully answered, households that receive the transfer are less likely to accumulate assets. Even though not all assets are productive per se, their accumulation stands for a better standard of living. A positive effect was only measured for

[17] We do not consider the geographic surroundings dimension as we are interested in the in-house evolution.

[18] A higher RS component implies a larger overall RS score and consequently "better" living conditions. In terms of dwelling, it means better construction materials and improved access to basic services (utilities). In terms of assets, it means owning a larger quantity of house assets. For the demographics component to grow, family size should be smaller, the level of education of the household head should be higher and/or the number of children younger than 15 should be smaller. Finally, for the schooling component to grow, it is necessary for children to attend school or change from a public to a private school.

indigenous households, although the results are not stable when the bandwidth is changed. Hence, the latter needs to be treated with caution.

BDH receipt has a positive effect on dwelling conditions in the Amazon region and in rural households, but not in urban areas. The dwelling component of the RS score includes variables indicating the availability of basic services such as piped water, access to a sewage system and garbage disposal services, none of which depend entirely on the household but partially on local governments. However, the dimension also includes variables that measure the quality of the dwelling such as the materials of construction, the condition of the construction materials and overcrowding.

With respect to household demographics and schooling, the effects of the BDH are limited. Noteworthy are the negative effects on schooling in the Southern highlands and for households with more than two children, although these findings are not stable when doubling or halving the bandwidth. When disaggregating the schooling indicator, no effect, either positive or negative, could be identified on the rate of attendance (Supplementary Annex 2 Panel A) just as reported by Paredes-Torres (2017). Nonetheless, there might be some effect on whether children are sent to a public or a private school.

Taking a closer look at the variables included in the dwelling dimension (Supplementary Annex 2 Panel A), we see that the effect of the BDH on the water source is negative in urban and positive in rural areas.[19] Both effects are stable independent of bandwidth choice, although the effect in the rural area is only significant at the 10% level. One would expect that an improvement in the water source would also result in other improvements that are directly linked to water access such as

[19] Households can improve their water source score in three ways: by moving to another house with better connection (private investment), by local government expansions of the pipelines (public investment) or by connecting to an already existing public pipeline (private investment with public intervention if connected through a meter).

shower and toilet facilities. BDH households in rural areas show indeed a significant improvement in shower, but not toilet facilities. Given that local government extensions of water pipes benefit both beneficiaries and non-beneficiaries living in the same neighborhood and that the option to connect to an existent pipeline seems to be the more likely in urban areas, the most likely explanation for these positives effects in rural areas is that households have moved to another house or they have indeed invested in their facilities. Next to the impact on water and sanitation facilities, BDH receipt had a stable and positive result for overcrowding (less overcrowded) in the Southern Highlands and Amazon region, which could be due to migration of household members.

Table 7 Effect sign on specific asset items

	TV set	Computer	Washing machine	Blender	Refrigerator	DVD player	Car
National	- -		- -				
Montubio	-	- -	- -	-		-	- -
Mestizo	-						
Indigenous		-		+ +			
Urban area							- -
Southern coastline [1]	- -	- -	- -				
Southern highlands		+ +	+ +				
Amazon region					+		

- -: 5% significant negative effect, ++: 5% significant positive effect, -: 10% significant negative effect, +: 10% significant positive effect.

[1] It includes the Galapagos Islands

Source: Registro Social Ecuador

A closer analysis of the sub-components of the asset dimension indicates that BDH receipt has mixed effects on the possession of individual assets and that the effects vary by population group. A summary of disaggregated impacts is provided in Table 7.[20] The results indicate that the impact of BDH transfers in montubio households, those living along the southern coast and those in urban areas drive the negative results. On the other hand, positive impacts are more likely to be found for

[20] Conclusions do not change with the selection of bandwidth (Supplementary Annex 2 Panel A).

households in the Amazon region, in the southern highlands and for the indigenous population, although many of them are not significant or stable.

5. The behavioral root of the results (discussion)

The results above have shown that the mothers' program of the BDH has limited positive impacts for a few specific groups of the population and for a small number of outcomes after five years of program exposure. The overall balance of the Ecuadorean cash transfer for mothers after 20 years of implementation is discouraging considering the evidence presented here and elsewhere (see Araujo et al. (2017); Araujo et al., 2016; Paredes-Torres, 2017). This contradicts much of the evidence on other cash transfer programs, that show certain positive medium and long run effects (Bastagli et al., 2016; Millán et al., 2019).

Most cash transfer programs share certain commonalities, but details in the design (and implementation) may change the extent and direction of impact. In our case, the design of the transfer creates perverse incentives for beneficiaries effectively punishing improvements in living conditions. Every four to six years households are interviewed for the assessment of their living conditions. Both, initial benefit eligibility and continuity depend entirely on this assessment, which is reflected in the households' RS score. While the targeting method may be appropriate for program intake, it is inadequate when used to assess program continuity. It incentivizes households to remain poor (or lie to the authorities[21]) in order to keep receiving the transfer. Furthermore, the inclusion of assets in proxy-means scores implies taxing those assets (Banerjee, Hanna, Olken, & Sumarto, 2020). In a recent experimental study, Banerjee et al. (2020) found that the ownership of assets does not change when its specific ownership is asked on the proxy-means survey. However, this only

[21] Although lying about their living conditions might be a better scenario because people are in fact better off, this prevents authorities to assess the transfer's impacts and might even lead the government to eliminate the program altogether.

compares between two slightly different ways of proxy-means testing and not whether proxy-means testing is an adequate mechanism to evaluate continuity and whether it creates general disincentives towards asset ownership or other negative behavior.

Although people do not completely understand how the eligibility mechanism works, they speculate that certain changes in their employability, their income sources, or their living conditions might affect their continued benefit eligibility (Palacio, 2017). Moreover, beneficiaries do not know their RS score, which means that they cannot assess how much they can improve their situation without being removed from the program. To illustrate this, imagine two households at the border of the cutoff, neither of them knowing how close they are to the threshold. One of the households saves part of the transfer to improve their shower facility and the other prefers to use it for daily consumption. After five years, the first one is more likely to be removed from the program because it has improved its structural living conditions. Another example of perverse incentives due to the composition of the RS score is school attendance. A household's score is smaller when none of the children attends school. However, as soon as one child attends school, the RS score increases, and if they are sent to a private school, the score further increases. Hence, of two households with similar living conditions where one household sends their children to school and the other does not, it is the household where children do attend school that is more likely be removed from the program. Not only does the program not enforce the schooling conditionality, it also incentivizes beneficiaries not to send children to school to increase their chances to remain in the program. While it is unlikely that BDH recipients kept their children from going to school, the program design punishes households that have increased their use of schooling services.

The implicit message of the BDH design is that if beneficiaries improve their living conditions, they might be expelled from the program. BDH recipients know from the experience of other households

that benefit entitlements can be revoked in subsequent assessment rounds (Palacio, 2017). Since scores are not disclosed and not easy to replicate, people do not know whether their window to improve and remain as beneficiaries is wide or narrow. Hence, the disincentives might be transversal to the whole distribution of the RS score. As a reference, Araujo et al. (2016) found experimentally that after ten years, children with longer exposure to the transfer[22] showed no increase in school enrolment or cognitive or knowledge tests for any section of the consumption distribution.

In summary, it seems that the mothers' program of the BDH has not been designed with the long-term goal of improving household livelihoods and breaking the intergenerational transmission of poverty. Instead, the program (unintentionally) created incentives for families to remain poor. Removing families from the program that managed to slightly improve their living conditions interrupts their path towards a better life for themselves and for the next generation. The accumulation of long-term human, financial or social capital is neither encouraged nor rewarded. Beneficiaries are only aware that every time they are surveyed, or even without a survey, they might lose the transfer because either their assumptions about exclusion are true[23] or the government changes unilaterally the eligibility criteria.

Given this uncertainty, people refrain from making certain investments, such as the accumulation of human capital, that could improve their living conditions in the medium or long term in order to increase their chance of program continuation. This decision can be viewed in relation to the time preferences of the poor which have been consistently estimated to be lower than for the non-poor (Azariadis, 1996; Haushofer & Fehr, 2014; Lawrance, 1991). People with lower income value future income flows less, and therefore are less likely to invest. The mother's program is targeted at people

[22] Controls received the transfer later in time and about half of the accumulated amount.

[23] Some of the hypotheses people have about how to lose the transfer are obtaining a formal job or augmenting their patrimony (Palacio, 2017), which is confirmed by Araujo et al., (2017) in terms of labor market and is consistent with the evidence presented above.

below the poverty line, whose time preference for future flows is expected to be low. Even though the transfer itself might increase their preference for future flows, the uncertainty about keeping the transfer might affect this mechanism and impede human capital accumulation at a larger scale.

On the other hand, the government has permanently focused on improving the targeting of the program and has increased the amount transferred as a function of political cycles which has led to higher stakes of losing the transfer. These higher stakes due to increased transfer values mean that the incentives to remain as beneficiary became even larger.

6. Uncertainty and the old-age pension modality

To test our hypothesis that program design created perverse incentives and unintended effects, we exploit the fact that beneficiaries of the BDH old-age pension did not face the same risk of losing the transfer if they were not receiving any contributory pension or earning a monthly wage above USD 280 as a public servant. If uncertainty about program continuity plays a role in the decision-making rationale of households, we would expect to find positive or no impacts for the old-age pension's modality. To test the role of uncertainty, we apply a RDD estimation for the old-age pension modality of BDH, where each person 66 or older in our sample is entitled to receive a monthly non-contributory pension.

6.1. Sample and method

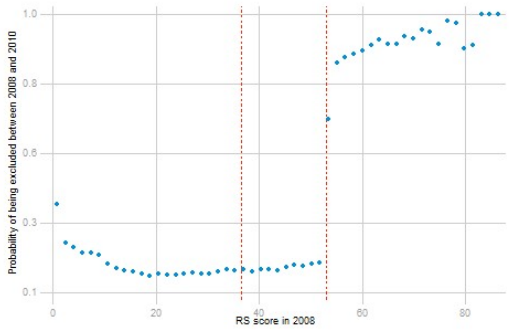
Using the social registry survey 2008, we created a sub-sample (*Sample O1*) consisting of households where the oldest member was at least 50 years old and whose RS score was below 53 in 2008. Below the 53 threshold the likelihood of keeping the transfer showed no discontinuity (Figure 4).[24] As a

[24] Note that the threshold was entirely abolished for a short period in 2012.

reference we also include a vertical line at 36.6, which is the eligibility criteria for the mother's program.

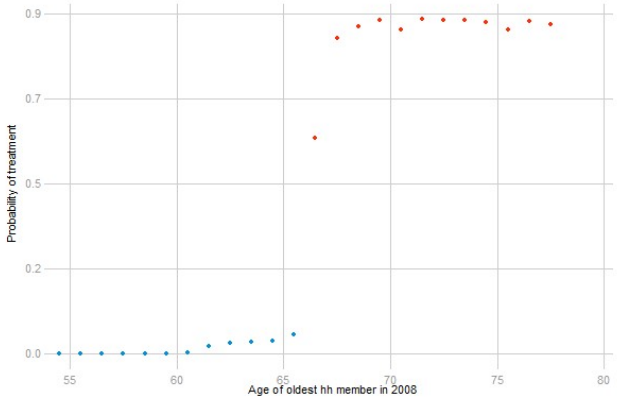
Within our sample, eligibility for the old-age pension depends on the age of the oldest household member (66 years or older). Table 8 shows the distribution of households in *Sample O1* according to the age criteria and whether they received the pension (treatment). Overall, 91.7% of households were correctly targeted. 16.8% of potentially eligible households did not receive the transfer and only 1% of non-eligible households received the transfer. Figure 5 shows that the exclusion error is the highest for the 66 year-olders.

Figure 4 Likelihood of losing the old-age pension between 2008 and 2010 in function of RS score of 2008



Source: Registro Social Ecuador

Figure 5 Likelihood of treatment for the old-age pension



Source: Registro Social Ecuador

Table 8 Distribution of sample to evaluate old-age pension (Sample O1). Number of households. (shaded areas refer to targeting errors)

	Oldest member < 66	Oldest member >= 66	Total
Non-treated	297,571	43,209	340,780
Treated	2,989	213,819	216,808
Total	300,560	257,028	557,588

Source: Registro Social Ecuador

The empirical strategy is equal to the one used for the mother's program, though the assignment variable is age instead of the RS score. Given that age does not explain assignment perfectly either, we fit the following system of equations:

$$\begin{aligned} y_i &= \rho + h(a_i) + \theta TO_i + w_i \\ TO_i &= \varepsilon + k(a_i) + \tau DO_i + z_i \end{aligned} \tag{4}$$

Where, y_i is the same set of outcome variables as for the mother's program; a_i is the age of the oldest member of household i rescaled in a way that 0 represents 66; TO_i is a dichotomous variable that equals 1 if household i has one or more beneficiaries of the old age pension program; and DO_i is a dichotomous variable that equals 1 if household i's oldest member is 66 years old or older.

Instead of a non-parametric approach, we fitted (4) using 2SLS with clustered standard errors, using as clustering variable age in years within the window of 6 years above and below the cutoff following the method recommended by Lee & Card (2008). $h(a_i)$ and $k(a_i)$ are fourth-degree polynomials of age interacted with TO_i and DO_i, respectively. We chose a fourth-degree polynomial as it was the specification that passed the test suggested by Lee & Card (2008).

We fitted a parametric estimation because a non-parametric estimation relies on having a continuous assignment variable (Frandsen, 2017; Lee & Card, 2008), but age in years is discrete. Our parametric estimation adjusts its standard errors to take into account clustered correlation for observations at each age (Lee & Card, 2008). This estimation leads to consistent estimators of the treatment effect and its standard error as long as the estimation error of $E[y_i|DO_i = 1, a = 0]$ is

equal to the estimation error of $E[y_i|DO_i = 0, a = 0]$ (Lee & Card, 2008). That is, whether the expected uncertainty about the estimation of $E[y_i|DO_i = 1, a = 0]$ and $E[y_i|DO_i = 0, a = 0]$ is the same, which requires an adequate model specification above and below the cutoff. The latter is tested by the specification test suggested by Lee & Card (2008).

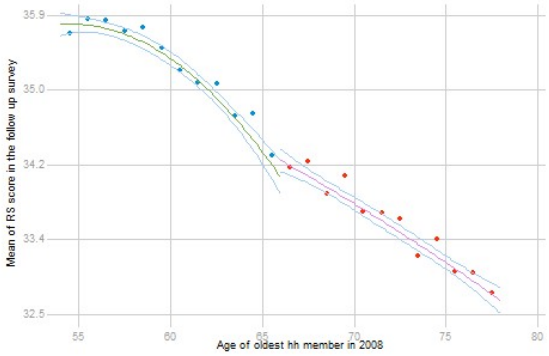
For θ to be consistently estimated, the identifying assumption is that the cutoff is exogenous, which in turn will make DO_i locally exogenous after controlling for a_i. If the cutoff is exogenous, the observations just below and above the cutoff are as good as locally randomly selected after controlling for a_i (Imbens & Lemieux, 2008). This means that $E[w_i|DO_i, a_i] = E[w_i|a_i]$ as a_i is the only systematic determinant of DO_i (Jacob et al., 2012), so after controlling for a_i, DO_i is exogenous and θ can be consistently estimated.

6.2. Results

Figure 6 shows the relationship between the age of the oldest member of the household in 2008 and the RS score in 2014. Graphically, eligible households show no noticeable difference with those not eligible to it. We confirm this econometrically in Table 9, where we present the marginal treatment effects. The sample is limited to households whose oldest member is between 60 and 72 years old.[25] As a robustness test, we also include the results for double the window in column 4. Results show no significant or stable effect for any of the subpopulations, even though the amount, the periodicity and the delivery mechanism of the old-age pension is the same as for the mother's program.

[25] The construction of age in years is arbitrary. To check the robustness of the results to other definitions of age, we estimated the models using age in semesters and thirds of a year. We could not use age in days because the discontinuity in the likelihood of treatment was not significant. Conclusions do not change and results are available upon request from the authors.

Figure 6 Relationship between age of the oldest member in 2008 (assignment variable) and the RS score in 2014 (outcome variable)



Source: Registro Social Ecuador

Table 9 Treatment effects of old pension program on RS score in 2014

		Treatment effect (window=6)	F-stat Instrument relevance (window = 6)	N (window = 6)	Treatment effect (window=12)
National	National	-3.36	16.21	201,573	-0.14
Ethnicity of HH head	Indigenous	4.24	18.51	27,165	1.04**
	Afroecuadorean	2.54	16.83	7,937	-3.86**
	Montubio	7.86	20.88	24,052	0.59
	Mixed	-1.68	15.53	135,078	-0.24
	White/other	17.32	9.94	7,341	-0.69
Sex of HH head	Male	-2.29	17.23	146,989	-0.23
	Female	15.45	13.57	54,584	0.14
Area of residence	Urban	-1.74	11.97	117,290	-0.19
	Rural	2.46+	27.19	84,283	0.06
Education of HH head	None	1.2*	21.70	41,069	0.36
	Primary	-3.35	15.69	141,599	-0.15
	Secondary	0.20	8.96	16,401	0.05
	Superior	3.89*	7.52	2,504	2.68
Geographical region	Southern highlands	3.44**	34.14	20,550	0.34
	Central highlands	35.55	22.69	33,006	-0.09
	Northern highlands	-4.38	5.42	15,756	-0.42
	Southern coast [1]	-1.99	10.50	79,697	-0.03
	Northern coast	-1.23	22.03	42,441	0.10
	Amazon	-31.80	12.47	10,123	-0.46+
Receives other BDH transfer	No mother BDH program	-72.09	17.23	152,738	-0.18
	Receives mother BDH program	-0.06	12.75	48,835	0.11
Number of children in the household	No children	1.05**	26.69	124,424	-0.01
	1 child	-3.55	10.91	38,987	0.44
	2 children	0.33	23.67	21,190	-0.05

**1% significance, *5% significance, +10% significance

Controlled for: ethnicity, geographic location, rurality, sex of the household head, age and age squared of the household head, education level of the household head.

Standard errors clustered at each age of the oldest member of the household (eligibility variable)

[1] Includes the Galapagos Islands

Source: Registro Social Ecuador

We expanded our analysis to sub-dimensions of the RS score (Table 10) finding that, in general, results are highly unstable and no significant effects, either positive or negative, were found for the different outcome variables.

Table 10 Treatment effects of old pension program on several outcomes in 2014

	Treatment effect (window=6)	F-stat Instrument relevance (window = 6)	N (window = 6)	Treatment effect (window=12)
RS score	-3.36	16.21	201,573	-0.14
School enrolment children 5-17	-0.02	8.93	79,746	0.01*
School enrolment children 15-17	-0.09	15.14	33,070	0.03*
Labour supply of adults	-0.09	10.98	167,187	0.07*
RS score (dwelling component)	-1.61	16.21	201,573	-0.16
RS score (assets component)	-0.78	16.21	201,573	0.11
RS score (demographic component)	-0.08	16.21	201,573	-0.07**
RS score (schooling component)	-0.05	16.24	197,824	-0.03**

**1% significance, *5% significance, +10% significance

Controlled for: ethnicity, geographic location, rurality, sex of the household head, age and age squared of the household head, education level of the household head.

Standard errors clustered at each age of the oldest member of the household (score variable)

These results support our hypothesis that program design affects the outcomes and the uncertainty about future benefit receipt may induce sub-optimal household decisions in line with a preference for short-term income flows. Even though the results for the old-age pension are not encouraging with respect to long-term improvements in living conditions of the elderly, the program has no unintended effects, but likely helps maintain existing living standards. The comparison of the two program modalities has shown that within the same country and context, a monthly transfer of the same amount can, as a function of design differences, have both negative or null effects. Other factors might play a role in explaining the different outcomes, but results are suggestive considering the evidence of other cash transfer programs outside our study case (Bastagli et al., 2016; Millán et al., 2019).

7. Conclusions

Evidence of long-run effects of cash transfers on social mobility is still being developed, and although some positive effects remain identifiable in the long-run for several countries, some short-run

effects have faded out. In addition, there is evidence of differences in results across countries both in size and direction of effects. This means that the cash transfer itself is only one part of the story. Other aspects of social protection programs such as program design and context also play a role for the achievement of the intended outcomes or the lack thereof.

This paper studied the Ecuadorean cash transfer program targeted towards poor families, specifically mothers and their children. The accumulated evidence shows some positive effects in the short run in school attendance and child labor, though in the medium and long run the effects in schooling disappear and negative results are reported in access to a formal job and living conditions. Our analysis confirmed the negative results in living conditions, and found that after five years of exposure, treated households are, on average, worse off in terms of asset ownership and certain dwelling characteristics. Negative effects accumulate in urban areas and the coastal region of the country, while limited and highly selective positive effects were found for the rural population, households in the Amazon region and the indigenous population. Furthermore, we found no evidence of significant impacts in school enrolment or other mechanisms of human capital accumulation.

We argued that these results derive from the design of the program, in particular the mechanism used to evaluate program continuity. Under the current design, beneficiaries are evaluated every four to six years solely in terms of their proxy-means score, which is built to predict the family's level of consumption. This discourages people to invest or improve, and in fact, punishes these achievements by expelling them from the program if their projected consumption surpasses the eligibility threshold. A particularly perverse incentive is created by the inclusion of school attendance as a predictor of consumption as households that send their children to school will have a larger score and therefore be more likely to be removed from the program. Consequently, while programs

in other countries condition the continuity on human capital accumulation, the Ecuadorean's design conditions it on households remaining poor.

Furthermore, families do not know what their score is and how far they are from the cutoff. Consequently, the incentive to remain below the eligibility threshold and the uncertainty about the distance to the cutoff are key factors explaining the negative results. Disincentives and uncertainty together with a high discount rate of future flows among poor households prevent people from making long-term investments that could reduce the likelihood of intergenerational poverty transmission. Moreover, every time the transfer value increases but the design remains unchanged, the incentives are further amplified as the stakes of losing the transfer get higher.

We also evaluated the old-age pension modality of the BDH to test the uncertainty hypothesis because its beneficiaries face little risk of losing the transfer and the conditions for losing it are clearer. Consequently, beneficiaries face less uncertainty about the transfer's continuity. Although both programs transfer the same amount of money in the same periodicity and using the same delivery mechanisms, they have different outcomes in terms of living conditions. The old-age pension of the BDH showed no significant effects, neither positive nor negative, while the mother's transfer showed mostly negative results.

The accumulated evidence suggests a change in the program's design, so both the government and households make a long-term commitment to meet certain goals and milestones to achieve a long-term objective of breaking the poverty trap. The new design must reward and incentivize beneficiaries to achieve such goals and assure them that while these goals are met, they will continue to receive the transfer. Some of those goals can be related to problems such as teen pregnancy and marriage, high school dropouts, little use of initial education and malnutrition, all of

which are highly prevalent in the country (INEC, 2019a, 2019b). Evidence from other programs outside Ecuador show that such results can be achieved.

Our case study shows that all aspects of the design inherently create conditions, and all conditions affect incentives and expectations. Under a design that conditions beneficiaries to remain poor to maintain the benefit without any other commitment, results are, not surprisingly, negative. Cash transfer programs are not only about money, and if wrongly designed, they can turn a costly program from a mechanism to achieve positive social mobility to a mechanism to perpetuate poverty.

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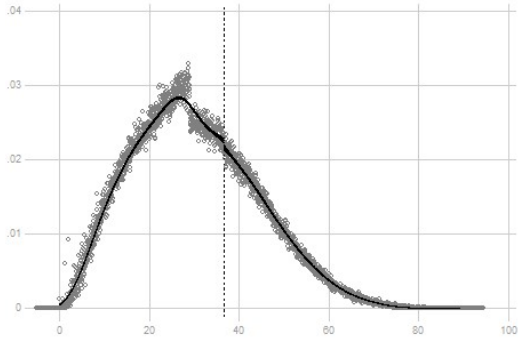
Annex 1 Sample correction for likelihood of following observations

To estimate the effects of the cash transfer program, we need to observe households in the baseline and in the follow up survey. However, not all people report their identification number, so matching both survey waves cannot be done perfectly. Those below the cutoff point are more likely to report their ID because it is required in order to cash out the transfer. This does not mean that there is more attrition among control households, but that their registries cannot be matched as easily as for the treated observations.

Once we select households matched in both periods of time, a significant discontinuity is created in the density of the RS score right after the cutoff point of eligibility (Figure 7).

Figure 7 McCrary test of continuity in the density of RS score by sample



Source: Registro Social Ecuador

This discontinuity is usually interpreted as a sign of likely manipulation of the eligibility score variable (McCrary, 2008). However, even though people might have the incentive to lie about their welfare and wealth, it is impossible that they do it in a way that they fall exactly behind the cutoff. When the survey is collected, the weights to construct the score are not known, and even if they were, they are not easy to interpret and use in such a way. Consequently, there is another phenomenon that is causing the discontinuity.

Before keeping only observations that can be matched between both waves, the distribution of the score in the baseline is smooth and has no discontinuity; however, after the selection is performed, the discontinuity appears.

There are people who report their ID on the day of the survey, and others whose ID is collected when they are informed that they are eligible for the program. The latter causes the imbalance in the density between people above and below the cutoff point because non-eligible people will not be re-contacted to fill in their IDs. In addition, it is not possible to know whose ID was registered when.

To solve this problem we took the observations above the cutoff point as a reference of the likelihood of reporting the ID during the surveying process and modelled such likelihood based on the characteristics of households as reported on Table 11 via a logit model.

Table 11 Logistic model to estimate the likelihood of being followed based on baseline characteristics for observations above the cutoff point

		Baseline 2008 (coefficients)
RS score at baseline	RS Score	-0.01
	RS Score ^ 2	0**
Age and sex of the head	Age of the head	0.05**
	Age of the head ^2	0**
	Male	-0.69**
Marital status head of the household	Married	0.17**
	Widow	0.45**
	Separated	-0.08**
	Single	-0.14**
Education of HH head	Literacy center	-0.01
	Primary school	0.06*
	Basic Education	-0.09*
	Secondary school	-0.07**
	High school	-0.07+
	Technical education	-0.03
	University	-0.17**
	Graduate education	-0.12*
	Adult basic education	0.13*
	Unknown education	0
Paid labor	Paid labor (head of the household)	0.1**
Ethnic background	Afroecuadorean	0
	Montubio	0.08**
	Mestizo	-0.05**
	White	-0.08**
Province of residence	Province 2	0.61**
	Province 3	0.64**
	Province 4	0.05+
	Province 5	0.77**
	Province 6	0.23**
	Province 7	0.34**
	Province 8	0.71**

		Baseline 2008 (coefficients)
	Province 9	0.86**
	Province 10	-0.17**
	Province 11	0.39**
	Province 12	1.03**
	Province 13	0.88**
	Province 14	0.73**
	Province 15	0.03
	Province 16	0.15**
	Province 17	-0.28**
	Province 18	0.54**
	Province 19	0.68**
	Province 20	-1.98**
	Province 21	0.13**
	Province 22	0.33**
	Undelimited regions	0.61**
Constant	Constant	-1.34**
	N	**667,674**

Source: Registro Social Ecuador

**1% significance, *5% significance, +10% significance

After fitting the model, we estimated the likelihood of reporting an ID during the survey interview for those households below and above the cutoff point. We kept the observations that show the same likelihood of reporting an ID during the interview on both sides of the cutoff point ([pr|X]>=0.5 for samples M1 and M2). Therefore, we keep only those households whose characteristics are locally similar and correct the discontinuity in the density. Both conditions are met as discontinuities in exogenous variables are not systematic and the discontinuity in the density is no longer significant for all the samples.

Annex 2 Robustness checks

Given the challenges of correctly identifying the Regression Discontinuity Design (RDD), we check for the robustness of the results by testing several conditions underlying our analysis. First, we formally test for the probability of receiving the transfer at the cutoff threshold. Table 11 presents the results of the first stage, which measures the jump in the probability of treatment. Results are significant and the statistics of the F-test exceed the value of 10, which is a rule of thumb for an instrument's relevance. The results in Table 12 are representative for the national samples, but estimations for subpopulations show the same behavior and are available upon request.

Table 12 Relevance test of instrument

	Sample M1	Sample M2	Sample O1
Coefficient	-0.73	0.75	0.90
F-test	16,947	11,139	263

Source: Registro Social Ecuador.

Samples M1 and M2 based on non-parametric model; sample O1 based on parametric 2SLS estimations.

Secondly, we test whether the cutoff point is exogenous, which is one of the identifying assumptions of the RDD. It implies that the threshold is selected based only on the criteria that the score is aiming to reflect. In our case it means that the cutoff for the mother's program was chosen only as a proxy estimation of the consumption's poverty line and not aiming to benefit any specific group of people beyond a consumption criterion. For the old-age pension, it means that it only aims to separate elders from others. This assumption might be violated if, for instance, the cutoff is selected because the density of indigenous people changes strongly at the cutoff or because people from certain geographic areas are bunched right above or below the cutoff. To test this condition, we estimate whether baseline characteristics show a significant jump at the cutoff point. If observables show no discontinuities at the cutoff, unobservables will also be assumed to be continuous and therefore $E[u_i|D_i, s_i] = E[u_i|s_i]$. We find no evidence of structural discontinuities in baseline characteristics for any of the samples (Table 13).

Table 13 Exogeneity tests on baseline characteristics

		Sample M1	Sample M2	Sample O1
Outcome variables in the baseline	RS Score	0.00	0.00	2.68
	BDH mother's program			0.04
	School enrolment 5-15 years old	0.02	0.01	-0.01
	School enrolment 15-17 years old	0.05	0.01	0.01
	% Adults (18-64 yrs old) working	0.00	-0.01	-0.08
	Dwelling quantitative deficit	-0.04	0.00	0.02
	Dwelling qualitative deficit	0.04	0.01	0.02
	Dwelling RS components	0.03	-0.27*	1.00
	Assets RS components	-0.03	0.13	0.38
	Demographics RS components	0.08*	-0.05	-0.16
	School attendance RS components	-0.05	0.04	0.01
	Dependency ratio	0.06+	0.01	0.00
	Household size	0.00	0.02	-0.19
Ethnicity of HH head	Indigenous	0.01	0.00	0.02
	Afroecuadorean	0.01	0.01	-0.03
	Montubio	0.01	-0.02+	0.04
	Mestizo	-0.04+	0.00	0.02**
	White/other	0.01	0.01	-0.06
Area of residence	Urban	-0.02	-0.02	0.08
Education of HH head	None	-0.01	0.00	0.18
	Primary	0.03	-0.03	0.00
	Secondary	-0.02	0.04+	-0.15
	Superior	0.00	-0.01	-0.03
About the HH head	HH head male	0.00	0.00	-0.12
	Age of the HH head	0.06	0.08	5.05
	Age square of the HH head	-21.07	8.29	473.02
Geographical region	Southern highlands	0.00	0.01	-0.07
	Central highlands	0.02	-0.01	0.03
	Northern highlands	0.00	0.00	-0.03
	Southern coast [1]	0.00	0.03	-0.07
	Northern coast	-0.02	-0.03	0.18
	Amazon	0.00	0.00	0.00

**1% significance, *5% significance, +10% significance; estimations for M1 and M2 based on non-parametric estimations, and for O1 on parametric 2SLS.

[1] It includes the Galapagos Islands

Source: Registro Social Ecuador

If households manipulate the score to increase their likelihood of eligibility, the identifying assumption of RDD can be violated. This can be tested by checking whether observations bunch right above or below the cutoff point, which can create a discontinuity in the density of observations to either side. Age is not susceptible to manipulation, but we estimated the Frandsen's test anyway, which is appropriate when the scoring variable is discrete (Frandsen, 2017; Lee & Card, 2008). Frandsen's test corrects for the limited support that a discrete variable has in contrast to a continuous one (Frandsen, 2017). The result show that the null hypothesis of no discontinuity in the density of age cannot be rejected with a 0.29 probability using a k parameter equal to 0, which is

the strictest case.[26] To test the likelihood of RS score manipulation for the mother's program, we estimated the McCrary test (McCrary, 2008), which shows that the null hypothesis of continuous density cannot be rejected for either sample (M1 and M2) (Table 14).[27]

Table 14 McCrary test of no manipulation of score

`	Sample M1	Sample M2
Coefficient	-0.01	-0.03
z-stat	-0.67	1.54

Source: Registro Social Ecuador

Table 15 Non-parametric RDD estimation on the intertemporal change of the RS score.

	Sample M1	Sample M2
Wald Estimator	-0.92	0.64
z-stat	-3.16	2.68
Window	3.73	3.57
N	202,300.00	315,917.00
Wald Estimator (50)	-1.18	0.70
z-stat (50)	-2.87	2.08
Window (50)	1.87	1.78
N (50)	202,300.00	315,917.00
Wald Estimator (200)	-0.75	0.70
z-stat (200)	-3.67	4.07
Window (200)	7.46	7.14
N (200)	202,300.00	315,917.00

Source: Registro Social Ecuador

To test the stability of our results, we used different bandwidths given that results should not be sensitive to the bandwidth selection or to small changes in specification. Results of the models using half and double the bandwidth are available in Supplementary Annex 2. As an additional stability test, we estimated the effects of the mother's program on the intertemporal change of the RS score at the national level (Table 15). This eliminates any fixed effect. The conclusions hold independent of the selected technique and bandwidth.

[26] For a better interpretation of the test, please see Frandsen (2017)

[27] The tests were fitted using the final samples. That is, those households with all the necessary information that could be matched between the two waves of the proxy-means survey and whose likelihood of longitudinal match was the same. A description of how the likelihood of longitudinal match was estimated is reported in Annex 1.

Some tests cannot be run in a non-parametric setting but are nevertheless relevant. We wanted to know whether the effects of being included in the program are significantly different from the effects of being excluded in absolute terms. We use a third-degree polynomial of the RS score on the baseline interacted with T and D to allow maximum flexibility of $g(s_i)$ and $f(s_i)$. The window of observations below and above the cutoff point is +/- 10 points of the RS score in the baseline. Estimations with half and double that window rendered the same conclusions and are available upon request. The analysis was run for samples M1 and M2 (Table 5).

Table 16 Parametric RDD estimation for testing heterogenous effects

	M1 (Joint estimation)	M2 (Joint estimation)	-M1=M2 Test
	1	2	3
Coefficient	-0.94	0.58	0.37
z-stat	-9.25	6.00	7.95
N	215,222	215,222	215,222

Source: Registro Social Ecuador

Columns 1 and 2 of Table 5 present the results of the joint estimation of heterogenous effects of receiving the program (M1) and of losing it (M2). For this estimation, we interacted D with a dummy variable that indicates whether households were previously exposed or not to the program. Likewise, we interacted that variable with T to have two separate treatment variables. Both results show that households that received the transfer are worse off than those that did not, just as we estimated before. In column 3 we present a test of equality of both coefficients in absolute terms, which is rejected at the 99% confidence level. The latter lets us conclude that people who lose the program do not recover from the negative effects incurred as beneficiaries, or that the negative results are smaller for those households already used to the program if no negative effects happened before 2008.

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