Assessing the Effect of Conditional Cash Transfers in Children Chronic Stunting: The Human Development Bonus in Ecuador

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Resumen

Las políticas de erradicación de la pobreza basadas en Transferencias Monetarias Condicionadas (TMCs) son ampliamente utilizadas en los países en desarrollo, siendo la desnutrición crónica uno de los indicadores de resultado más estudiados. Sin embargo, los escasos análisis empíricos para Ecuador no concuerdan sobre los efectos de las TMCs en esta variable. En este contexto, y en base a una encuesta de gran escala, en este artículo propongo una estrategia de dos etapas para evaluar el efecto del programa Bono de Desarrollo Humano en la desnutrición crónica infantil. Primero, replico el índice de elegibilidad, a través de Análisis de Componentes Principales y, posteriormente, implemento un diseño de Regresión Discontinua Difusa. Los resultados principales muestran que existe un efecto estadísticamente significativo de entre 18 % y 24 % del estatus de elegibilidad sobre la probabilidad de tratamiento; un efecto de la intención del tratamiento estadísticamente no significativo de -0,5 a -0,3 desviaciones estándar; y, finalmente, un efecto local promedio del tratamiento de -2,1 a -1,6 desviaciones estándar no distinto de 0, dependiendo del enfoque y la especificación. A pesar de que los test de falsificación sustentan el cumplimiento del supuesto de identificación del diseño de evaluación, las conclusiones deben interpretarse con cautela debido al periodo de levantamiento de la encuesta, los cambios implementados en el programa y, finalmente, la muestra analizada.

Palabras clave: desarrollo social, Transferencias Monetarias Condicionadas, desnutrición crónica, análisis de componentes principales, regresión discontinua.

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Abstract

Poverty eradication policies based on Conditional Cash Transfers (CCTs) are widely used in developing countries, being chronic stunting one of the outcome indicators most studied. However, the few empirical analyses for Ecuador do not agree on the effects of CCTs on this variable. In this context, and based on a large-scale survey, in this article I propose a two-stage strategy to evaluate the effect of the Human Development Bonus programme on children chronic stunting. First, I replicate the eligibility status index through Categorical Principal Component Analysis, and then implement a Fuzzy Regression Discontinuity Design. Main results show that there is a statistically significant effect of 18% to 24% of the eligibility status on the probability of treatment; a non-significant estimate of the intention-to-treat effect going from -0,5 to -0,3 standard deviations; and, a local average treatment effect ranging from -2,1 to -1,6 standard deviations not significantly different from 0, depending on the approach and the specification. Although, falsification tests support compliance with the identification assumption of the evaluation design, conclusions should be interpreted with caution given the surveying period, the changes implemented in the programme and, finally, the sample analysed.

Key Words: social development, Conditional Cash Transfers, stunting, principal component analysis, regression discontinuity design.

JEL Codes: I31, I38, I12, C38, C31

1 Introduction

A vast number of empirical questions about poverty tackling in developing countries depend on causal effects of government programmes. One widely implemented version of this type of intervention are the Conditional Cash Transfers (CCT). These transfers aim to achieve generalised social well-being by allowing vulnerable groups be responsible for their own development. In order to attain this objective, the strategy incentivises social investments through beneficiaries' co-responsibilities. Namely, school assistance, and health checks of the children pertaining to the recipients' households.

In Latin American these programmes became popular in the late nineties, and since then, several studies focused their attention on measuring the expected benefits. In between the most recognisable are the Mexican *PROGRESA*, The Brasilian *Bolsa Familia*, an the Colombian *Familias en Acción*. These and others programmes have been evaluated in the light of their aligned objectives, being the most commonly studied outcomes, school enrolment and assistance, chronic stunting and vaccine preventable diseases prevalence. The educational outcomes have evidenced stable and positive effects (Ponce and Bedi, 2010), while, conclusions on children health are more heterogeneous, particularly when focusing in malnutrition (Paxson and Schady, 2007). This, added to the fact that stunting (measured as the deviation of the ratio of height for age from a distribution of well-nourished children) can be paired to an indicator of structural poverty, evidences room for prioritised empirical research.

One particular interesting case is the Ecuadorian programme *Bono de Desarrollo Humano* (BDH) or Human Development Bonus, launched in 2003. Similar to others, this CCT allocates the transfer depending on a score that determines household eligibility status. The BDH has been evaluated on several outcomes, being stunting one of the more understudied mainly due to the lack of anthropometric information associated to recipient's registries. Indeed, even when stunting prevalence is an indicator of particular concern for Ecuador, only two important studies can be reported. This evidenced gap, and a recent story of intricate methodological and political transfer scheme changes, are strong indicatives of the need of research.

In this context, and relying on a recent large-scale household living conditions survey data, I outline a two-stage empirical strategy, attempting to answer if there is an effect to the BDH in chronic stunting for children under 5 years. The first stage recurs to a multivariate analysis method, Categorical Principal Component Analysis (PCA), to replicate the original eligibility index in the survey. Afterwards, and exploiting the discontinuities in the administration of the programme, the second stage implements a Regression Discontinuity in its fuzzy version, to estimate the effect.

This document is structured as follows; Section 2 reviews the main theoretical relations between poverty, CCTs and children nutritional outcomes, and evidences Latin American and Caribbean case studies results. Section 3 contextualizes the Ecuadorian BDH and specifies the motivation. Section 4 details data, methodological approach and implementation. Then, Section 5 reports the main estimates and finally Section 6 outlines conclusions, discusses their validity and suggests further research.

2 Literature review

2.1 Poverty, malnutrition and Conditional Cash Transfers

Multiple causes have been cited as the root of the poverty-trap, being one of the more largely documented an insufficient bad-quality diet. The first formal attempt to study the relationship between poverty and malnutrition was made in 1957 by Leibenstein, and since then, several theorisations have been developed (Dasgupta and Ray, 1987). The transversal idea to these theories is that poor people eat on a surviving basis because of their constrained budget; therefore, they cannot fully develop their capacities which makes them less productive, resulting in underpaid jobs, if hired at all. This diminished labour market participation traduces in less resources to bring back home, perpetuating the cycle (Banarjee and Duflo, 2011).

Regardless of the apparent simplicity, the underlying intricate issues are constantly studied on the hopes of stablishing strategies that can effectively tackle poverty by placing individuals well-being as the ultimate objective. The theorization that best represents this aim is Sen's capability approach, which understands poverty as the lack of expansion of the peoples' freedoms seen both as primary ends and principal means not just as the simple process of accumulation and economic growth. In other words, development is only achieved when substantive freedoms as health and education satisfy both their constitutive and instrumental roles. The author mentions five types of instrumental freedoms, from which two are more accurately related to the design of policy interventions concerned with nutritional status, a) social opportunities and b) protective security. The former referring to the need of societal arrangements to secure healthy living and the later to the importance of providing safety nets to reduce the probability of extreme vulnerability (Sen, 1999).

Governments of developing countries have acted as catalysers of these instrumental freedoms for many years, by designing focalised tools, being one of the most widely and recently used the cash transfers. For the present research, the schemes that require co-responsibilities from the beneficiaries are of interest, namely the CCTs. The existing literature on these matter concludes that there are two main purposes for implementing CCTs, equity in the short run by redistributing resources to the poorer households and efficiency in the long run by restoring the mismatch between parents' preferences and social benefits from human capital investment (i.e. alleviating transactional, imperfect information and opportunity costs) (De Janvry and Sadoulet, 2006). These two elements are meant to be achieved by the transfer, though, due to trade-offs they depend on the success of the alignment between programmes objectives and the good that is conditioned (Das et al., 2005).

The CCTs have the premise that while the supply side is met by services and goods offered, there is a need to promote demand through conditions to attain social investment. Therefore, it is not enough to incentive the demand through an income effect (increasing households available budget), but also via a price effect (the behaviours households ought to change to receive the benefit) (Bourguignon et al., 2002). The usual beneficiaries are households and the stimulated changes are regular school attendance and periodical health checks for the children. These co-responsibilities embody the substantive freedoms of this poverty cycle breakage strategy, education and health/nutrition (Rawlings and Rubio, 2003).

2.2 Children anthropometric status and well-being

Malnutrition is a dangerous phenomenon that has constantly affected vulnerable population in developing countries, since it's evident short-term physical consequences have been proved to perpetuate into the long-run on such deep levels that reversibility is hard to accomplish. Because of this, childhood is considered to be the stage where intervention can be most rewarding (Behrman and Hoddinott, 2005). Therefore, it is primordial to review both the determinants and the consequences of malnutrition to understand how programmes as CCTs are hypothesised to meddle this problem.

In general, the nutritional status is the product of the balance between the needs and the uses of food energy and other essential nutrients; but also the result of multiple physical, biological, cultural, psychosocial, economic and environmental elements (Figueroa, 2004). Given that it is a multidimensional problem the range of outcomes under study is broad, though children stunting (measured as height-for-age z-scores compared to WHO 2006 standards) is of major concern since it is considered an indicator useful to assess capabilities acquirement (Banarjee and Duflo, 2011). The documented causes of stunting (Figure 1) root form the interconnected linkages between the immediate lower level (children) and the intermediate and higher levels (household and community).

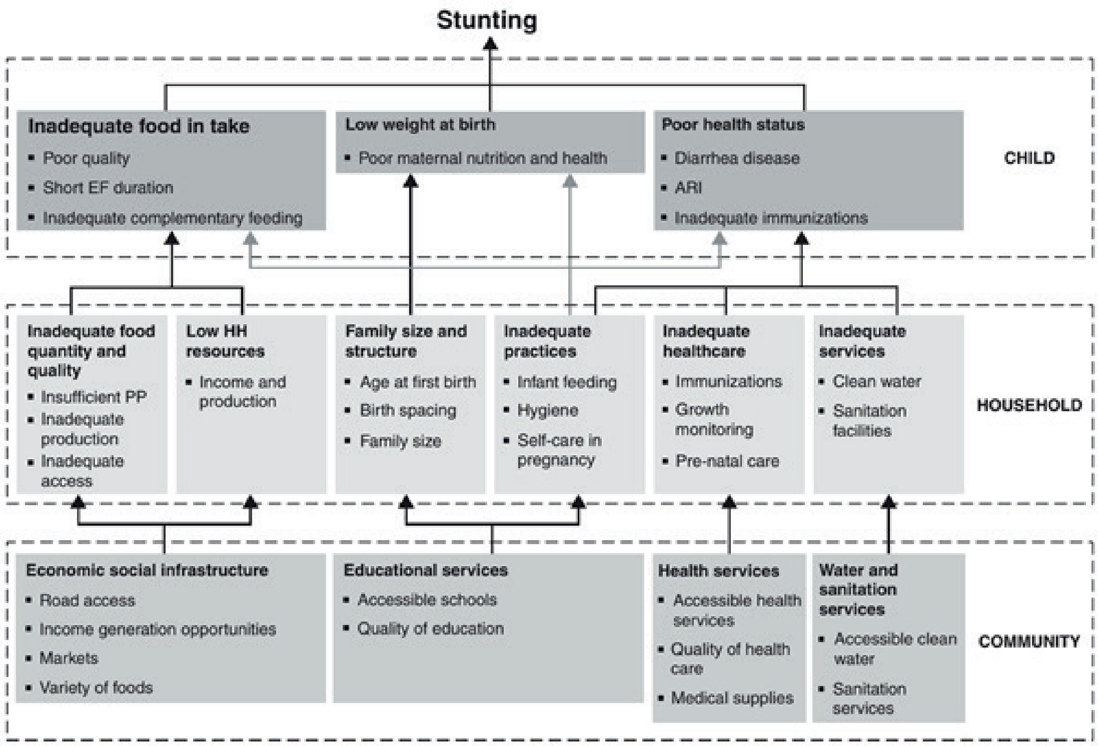

Figure 1: Causes of Stunting at Community, Household, and Individual level

Source: (World Bank, 2007)
Note: EF=exclusive breastfeeding, ARI=Acute respiratory infections, HH=household, PP=purchasing power

As evidenced, there are three proximate causes, inadequate food intake, low weight at birth and poor health status, each associated with problems at the household level, e.g. inadequate healthcare and sanitation practices, and at the community level, e.g. local economy and social infrastructure (World Bank, 2007). In the modelling of these determinants, a recurrent element is the conceptualisation of parental or caretakers' concern of immediate well-being of their children as well as their expectations of the value of investing in healthcare (Behrman and Hoddinott, 2005).

On the consequences side of the problem, empirical literature has reported that the failure of physical growth that leads to severe cases of undernutrition is highly correlated not only with a deficit in cognitive development but a higher probability of children mortality (Caulfield et al., 2004). For example, kids with iron deficiency are more prompt to have lower IQ, memory problems and difficulties to develop social skills (Paxson and Schady, 2007). Additionally, stunting increases the risk of long-term chronic illnesses and disability (Hoddinott et al., 2013) and permanently high prevalence of stunting reduces socio-economic growth jeopardising full physical and mental development of all individuals (Kabubo-Mariara et al., 2009).

In developing countries, the numerous constraints to interrupt the intergenerational vicious nutrition-based poverty cycle, worsen the presented scenario. The lack of instruments available for the parents to choose different patterns and for kids to take advantage of them is a permanent concern of governments. Indeed, CCTs have become a popular strategy among
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these nations since mid-nineties as part of their development agendas with the establishment of the health condition.

CCTs include several components as mediators to improve children nutritional status. The transfer itself is expected to translate into an increased nutritious consumption. As a companion for this objective, meetings for nutritional guidance are planned on a regular basis. Though, the most direct effect is implemented via the health checks condition. Specifically, through vitamins, supplements and growth monitoring. For instance, *PROGRESA*, one of the largest CCTs, which started in Mexico in 1997 (now called Oportunidades), includes i) *pláticas* (meetings) conducted by health professionals in order to improve the knowledge of the participants about child health; ii)*papilla* a differentiated supplement for pregnant and lactating women and for children under 5 years, and iii) growth monitoring which is mandatory before any benefit is given (Behrman and Hoddinott, 2005).

Likewise, other social strategies along Latin America and the Caribbean have implemented actions driven by a common concern on the permanently high children stunting. Simultaneously, impact evaluations to measure the effects of the strategies on efficiency and equity have been directed by private and public sectors. The results on children anthropometric outcomes are more heterogeneous than, for instance, educational outcomes (Ranganathan and Lagarde, 2012). In the following subsection, I present a brief review of the most preeminent studies in the field.

2.3 CCTs and children stunting: evidence

In Latin America and the Caribbean CCTs became popular since mid-nineties, being the first to implement them Brazil in 1995 with *Bolsa Escola* and *Programa de Erradicacao do Trabalho Infantil (PETI)*. Subsequently, researches focused on effect estimation came to be of academic and political concern (Fiszbein et al., 2009). Evaluations on children nutritional status have shown to be non-conclusive, and sensitive to sample size and estimation method (Paxson and Schady, 2007).

For example, Attanasio et al. (2005) showed that the Colombian programme *Familias en Acción*, introduced in 2001, improved nutritional status only for children under 24 months old. Through a Difference-in-Differences (DID) approach from a randomly selected sample of municipalities from treatment and control groups, the authors estimated a statistically significant increase in height-for-age z-score or HAZ of 0,161 standard deviations and a decrease in the probability of stunting of 0,069.

Behrman and Hoddinott (2005) based on a randomised trial found that *PROGRESA* in Mexico had significant positive effects on nutrition only after implementing child fixed-effects to control for unobserved heterogeneity. They reported an increase of approximately 1cm in mean growth per year of the treatment compared to the controls, for kids between 12 and 36 months.

In the Caribbean, a recognised large-scale programme is the Nicaraguan *Red de Protección Social*, which is focalised in rural areas and started in 2000. Maluccio and Flores (2004), through a DID approach, applied on an experimental dataset, estimated that the effect of *Red* was a decrease in the proportion of stunted children under 5, of about 5,3% and an increase in HAZ of 0,17 standard deviations.

Contradictorily, Morris et al. (2004) in their study of *Bolsa Alimentação* in Brazil, which was launched in 2001 as part of a larger programme, estimated that the gain in anthropometric measures is slightly larger for the non-exposed children. The authors reconstructed a dataset from original administrative registries and individually matched beneficiaries to excluded households. Their results, which they pair to an intention-to-treat estimate in a randomised trial, evidence a statistically significant reduction of the mean HAZ of 0,13 sd. for children under 7 years. Likewise, other countries have evaluated their strategies, though compared to the amount of researches focused on non-health or nutritional related outcomes, the number is diminished. Particularly, due to the difficulties to collect these type of information and given the intricate biological aspects that affect the analysis (Ranganathan and Lagarde, 2012).

3 Context and motivation

3.1 Ecuador and the Human Development Bonus

In Ecuador, the first attempt to implement a transfer dates from September 1998 with the launching of the *Bono Solidario* programme under the presidency of Jamil Mahuad. Initially, this transfer was thought to be a compensatory measure to the eventual reduction and elimination of gas and electricity subsidies, and did not included co-responsibilities. However, approximately one year after, an economic and financial crisis struck the country, and the transfer became a tool to retaliate the negative effects. Three groups of people were prioritised for the Bono reception, first, mothers in families with at least one child younger than 18 years, a monthly income less than 40 US$ and no permanent salary or social security benefits for the parents. Second, elders of 65 years old and more, with similar income restrictions as for the mothers. Third, persons with a disability percentage of 70% or more between 18 and 65 years. No other technical criteria was applied for beneficiaries' selection, therefore, the Bono was an auto-focalised benefit since people that believed should receive it, would go collect it from the government delegated offices (León, 2000).

In 2003 the *BDH* was implemented as an improved version of the *Bono Solidario*. Up to date, there are three distinguishable stages of the BDH. The type of beneficiaries were divided into the same three groups but conditions and technical selection criteria was included, while the amount of the benefit increased subsequently. In the first stage focalisation was implemented on the basis of 0 to 100 compound index named *Sistema de Selección de Beneficiarios* (SELBEN), which intended to identify nuclear families with limited capabilities to generate income; also, conditionality on education and health was introduced. The bond was given on a monthly basis and the amount was 15US$ for mothers and 7,5US$ for the other two groups. The total of the transfers came to represent 1% of the country's GDP and approximately 11% of the beneficiaries' expenses by 1999. By the year 2000 about 1,2 million people were recipients, corresponding to 45% of the Ecuadorian households (Vos et al., 2001).

The second stage initiated by the end of 2007 under the government of Rafael Correa. The process started with the registry of families located in areas with the highest poverty levels according to the 2001 Population and Housing Census, as a tool for tracking and monitoring potential recipients. Additionally, the index was updated, and both the database and the index were named *Registro Social* (RS). The amount was originally fixed at 30US$ for all types but then increased to 35US$ (Ponce, 2013). From this stage, the authorities directed efforts towards a more efficient system and the main institution in charge was the Coordinating Ministry of Social Development (MCDS).

The third stage started in 2013 with updates both on the registry[1] and the index renamed

[1] The mentioned registry updates were run by supply, though the database is constantly amended on demand.

Registro Social II (RSII). The amount increased to 50US$ per month and by the end of 2014, approximately 2'200.000 nuclear families and 7'500.000 individuals were part of the Registry from which 1'119.858 families were beneficiaries from the three types and 444.562 received the mother's one (MCDS webpage). Additionally, the first attempt to implement "graduation", a process in which recipients stop collecting the transfer since they are considered to have already acquired the capabilities to become responsible for their own development (according to the Ministerial Agreement No. 90197 of 28th March 2013), took place in this period.

The main goals of the programme remained similar through the stages, and were specifically to guarantee families a level of minimum consumption, decrease the prevalence of stunting and vaccine-preventable diseases for children under 5, promote school attendance between ages 6 and 16, and protect the elderly and persons with disabilities (Rosero and Martínez, 2007). Each index was calculated through a PCA, a multivariate analysis method that constructs a score on the basis of the observed covariance of certain selected variables. In this case, it is a 0 to 100 index based on variables that have high associations with per-capita consumption and which are taken from living standard measurement surveys pertaining to each period. Additionally, with each index change a different threshold was established which was paired up to a poverty line (Table 1).

Table 1: Indexes and eligibility criteria*

1	**SELBEN**	
	Quintile 1	$\leq 42{,}87$
	Quintile 2	$> 42{,}87$ & $\leq 50{,}65$
	Survey: 1999 Living Standard Measurement Survey	
2	**RS**	
	Extreme poverty	$\leq 25{,}5992$
	Poverty	$> 25{,}5992$ & $\leq 36{,}5987$
	No poverty	$> 36{,}5987$
	Survey: 2006 Living Standard Measurement Survey	
3	**RS II**	
	Extreme vulnerability	$\leq 24{,}087658$
	Vulnerability protection band	$\geq 24{,}087659$ & $\leq 28{,}20351$
	Vulnerability	$\geq 28{,}20352$ & $\leq 34{,}67905$
	No vulnerability	$\geq 34{,}67906$
	Survey: 2012 Households Socio-Economic Situation Survey	

*Shaded thresholds were considered eligible
Source: MCDS.

Likewise, other CCTs, the BDH is thought to address children chronic malnutrition via direct and indirect ways. The cash is expected to increase food consumption towards a more nutritious household diet. And, during health checks, kids are given supplements and their growth is monitored. Though, given the logistic and budget constraints, monitoring conditions is a challenge for the government, and only recently more accurate actions have been taken. Nevertheless, beneficiaries have always been informed of the importance of the co-responsibilities as a requirement for the monthly transfer. Moreover, several national surveys and public services directly ask recipients about their compliance.

3.2 Motivation

In the same way as with other interventions, the BDH has been evaluated in the light of its objectives; though, researches have mainly focused on school attendance, child labour, household consumption and labour indicators as outcomes (Gonzalez-Rozada and Llerena, 2011; Oosterbeek et al., 2008; Ponce, 2011; Rosero and Martínez, 2007; Schady, 2006). While, fewer studies have investigated children cognitive development and health indicators (Paxson and Schady, 2007; Ponce and Bedi, 2010) and even less, only malnutrition (León and Younger, 2007). The estimates, which correspond to the first stages of the BDH, point towards a slightly modest to null effect of the treatment on height-for-age.

As mentioned, there are only two relevant specific studies. First, León and Younger (2007) measured the impact of *Bono Solidario* using the 1999 Living standard Measurement Survey (LSMS) through and instrumented OLS with various controls. They modelled the differential effect between regular income and the cash transfer. The instruments were: a) an interaction of the three programme eligibility criteria (a dummy variable with value 1 for households with a monthly income less than 150US$, no workers in the formal sector and at least one mother with a child under 18, and 0 otherwise) b) a continuous variable of the traveling time to the collection point and c) a dummy with the value of 1 if the interview took place before April 1999, when the transfer amount was increased and 0 otherwise. They estimated that there is a significant though modest effect on children nutritional status both stunting (height-for-age) and underweight (weight-for-age) no different from a regular income effect.

The second study, by Paxson and Schady (2007), took advantage of the randomized introduction of the BDH, and computed the Intention-to-treat estimates for kids of ages 3 to 7 by the end of the period (baseline survey was collected October 2003 - September 2004 and follow-up September 2005 - January 2006). The authors estimated a 4 to 5 percent increase of a standard deviation (depending on model specifications) in the HAZ of 1.479 children (corresponding to 1602 nuclear families) in rural areas of six provinces. As mentioned one of the main reasons for this reduced number of researches is the lack of anthropometric information related to the transfer recipients, due to the costs it represents.

On top of these limited results, the discouraging evolution of an approximate 2% decrease in 8 years, from 25,6% in 2006 to 23,9% in 2014 (INEC, 2006, 2014) in national stunting prevalence, configures a permanent concern of the public, private and non-governmental sectors in Ecuador. Therefore, the measurement of the effect of CCT on children nutritional status is of great importance not only for theoretical matters but also to narrow the mentioned gap. In this sense, this research proposes an observational causal inference design using a recent important national survey attempting to analyse the effect of BDH on stunting of children under 5 years old in Ecuador, focusing on programme's third stage.

4 Empirical strategy

4.1 Data

The study sources out mainly from a secondary quantitative database, the Ecuadorian Living Standards Measurement Survey (LSMS), which is executed by the National Statistics Bureau (INEC). The LSMS, as part of the Ecuadorian Integrated Households Surveys System, collects information on the quality of life and well-being and represents an important tool for research and policymaking. Historically, six LSMS have been collected since 1994; however, the focus is in the last one collected from November 2013 to October 2014 (INEC, 2015). These databases can be found in the Institute´s web page:`http://www.ecuadorencifras.gob.ec/banco-de-informacion/`.

The main dimensions under study in the LSMS are income and expenditure, household production, housing, health, assets, public services access, education, among others. The survey is statistically representative for the 4 natural regions, 24 provinces, 9 planning zones and 4 main cities in urban and rural areas (INEC, 2015). The sample includes 28.970 households, 109.694 individuals and 11.473 children under 5 (INEC, 2014).

The indicator of children stunting, is estimated from the height and age collected on the questionnaire section 3 (Health), part E under "Anthropometrics". The height-for-age z-scores computed are compared to the distribution of the measurements for the standard sample established in 2006 by WHO in their Multicentre Growth Reference Study [2]. Point estimates under two standard deviations from the median of the reference are considered stunted. For this study, I computed results using the igrowup_stata package in a Stata 14 version (World Health Organization, 2006).

Regarding BDH reception, information is collected in section 7 (Economic Activities), part G of the survey under "Transfers and Money Aid". The type of bonus, the frequency, the amount, the co-responsibilities, among other topics are asked about the BDH in twelve questions. In the present study, only the type of BDH given to mothers will be considered in the analysis. The exclusion of other types is justified given the mentioned mediators that the BDH has to influence nutritional outcomes.

A critical part of the design is the initial reconstruction of the eligibility criteria index in the survey, which is the instrument for the later application of the regression discontinuity

[2]For children under 2 years the accurate measure is length-for-age z-scores, though to simplify I refer only to height

method. The RSII is a 0 to 100 index calculated through Nonlinear Principal Component Analysis (NLPCA) on 34 demographic, housing, education and asset possession variables, which categorises households according to their eligibility in the basis of a cutoff (Fabara, 2009). Almost all variables, but one, can be calculated directly from the LSMS, therefore, I used the 2010 Population and Housing Census to complete the exercise (further detail in the methodology section).

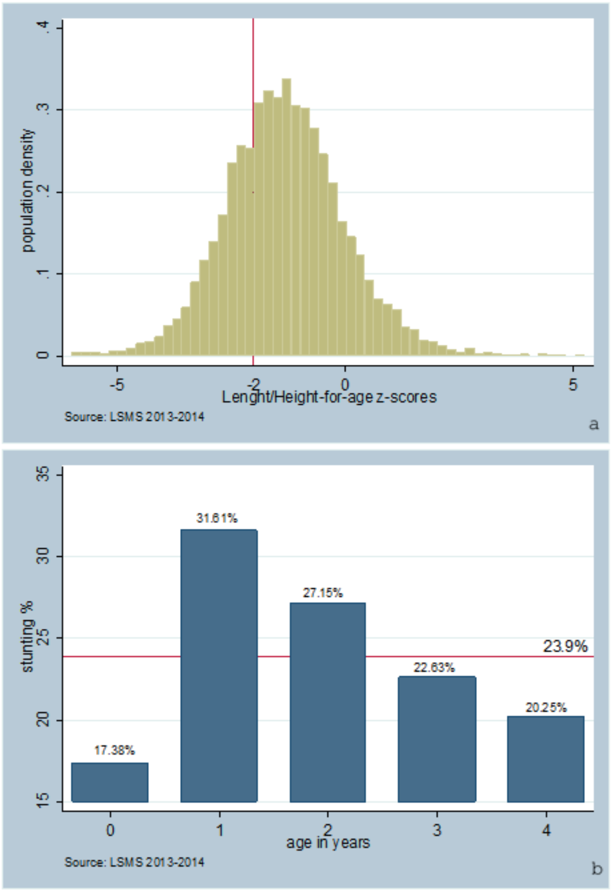

Figure 2: Children Stunting in Ecuador

Note: a) red line represents the -2 sd. threshold for underachievement in height-for-age z-scores compared to the standard distribution of well-nourished WHO sample. b) red line is the national mean stunting prevalence (23,90%). Ages 1 and 2 show higher values than the mean, specifically, 31,6% and 27,15%.

Descriptive statistics of the full LSMS sample show a 23,9% prevalence of children stunting, 19,9% in the urban areas and 32,8% in rural ones. Also, there is a stunting prevalence above the population mean for children of ages 1 and 2 (Figure 2). Regarding the transfer, from the total sample, 1'122.435 households receive BDH from all types (25,83% from total number of households), which represents 4'734.481 individuals (29,68% from total population). Within the BDH recipients' subset 672.714 households benefit from the mother's type (15,5% from total number of households), which represents 3'368.833 individuals (21,12% from total population) (INEC, 2014). When comparing the BDH beneficiaries' coverage from the LSMS to the official registries published by the MCDS, the percentages that the families represent from the total population [3] are either equal or very similar. Specifically, for all type of beneficiaries' the registries estimate shows a 6,99% coverage while the LSMS a 7,04%, and for the mother's type a 2,8% compared to a 4,2%. These percentages are also consistent between datasets when analysing by sex and ethnicity.

For the index replica, the complete set of surveyed individuals is employed to reflect the living conditions of the Ecuadorian population. Though, the analysis faces some other criteria which reduces sample's power. First, I kept only those who declared to be mother type recipient's households and non-beneficiaries. Afterwards, I selected only those surveyed from April 2014, since the RSII only started to be applied from that date. Lastly, the largest reduction was made by keeping only households with children under 5 years. The final sample is composed by 6.317 kids and 1.356 households, though, if we only consider the valid cases the sample is reduced by 143 individuals. Table 2 shows t-test for the stunting prevalence and the HAZ by BDH declared reception. The statistics evidence a significant difference in both measures when analysing by group.

Even though, the LSMS allows to group individuals into nuclear families, the construction of this unit was not clean (e.g. not all declared a family head), also, the LSMS focuses its design on individuals grouped into households. Because of this, I decided to do the analysis on the household level. The following subsection details the steps undertaken for the methodological design of the research including descriptive statistics and intermediate results.

4.2 Methodological approach and implementation

4.2.1 RSII index replica

The eligibility score is a proxy means testing index which is expected to be similar to the consumption poverty, but with a multidimensional approach based on Bourguignon and Chakravarty (2003). This type of index is the most commonly used in Latin American programmes. In Ecuador, as mentioned, the index changed depending on the living conditions of the population and was estimated through NLPCA. Compared to the traditional PCA, this method does not rely on the assumptions of linear relationships between numerical variables nor the multivariate normality of the data. Additionally, Optimal Scaling by

[3] The ratios where obtained by dividing the number of families receiving BDH to the total population, since MCDS makes public only the number of household but not the individuals they represent. For the registries ratio, I used the total population estimated for 2014 in the bases the 2010 population census; while for the LSMS I used the total weighted sample population.

Table 2: Stunting and HAZ average by BDH reception

Group	Observations	Stunting Prevalence	HAZ
Non-beneficiaries	4.197	22,90	-1,11
		(-0,006)	(-0,020)
Beneficiaries	1.977	39,20	-1,61
		(-0,011)	(-0,028)
N	6.174		
t		-13,487	14,560
Degrees of freedom	6172		
p-value		0,000	0,000

Note:t statistics for the null hypothesis of no significant differences between group means (beneficiaries and non-beneficiaries) in stunting prevalence and HAZ have associated p-values $<0,001$. Therefore, we can reject the null.

Alternating Least Squares was applied, because it allows the choice of both, measurement level and a number of sets. The combination of categorical optimal scaling level and one set of variables was chosen by the programme designing team. This specification lead to a Categorical Principal Components Analysis (CAPTCA) procedure, which was implemented by the SPSS algorithm named the same way (Fabara, 2009).

In Ecuador, the indexes were built based on variables highly correlated with the per-capita monthly consumption aggregate and optimally quantified in 2 dimensions. The matrix of the subjects in rows and these variables in the columns is the initial input for the calculation. The procedure itself consists of two iterative phases; first, parameter estimation and second, optimal scaling. In this way, scores with maximum heterogeneity between categories and maximum within-category homogeneity are assigned to subjects (Guerrero, 2002). Once the parameters (or categorical quantifications) are estimated, the category with the lowest quantification is given a value of 0 and the remaining quantifications are subtracted this original lowest weight. Finally, these new values are rescaled from 0 to 100 (SIISE, 2014).

The method was the same throughout the years, though the choice of variables changed towards a more structural and less income based measure of deprivation. The first index, SELBEN, was estimated with 27 variables with the 1999 LSMS data and was valid until 2007. Afterwards, in 2008, the RS index was calculated with the 2006 LSMS including 30 items. As part of the two Social Registry information gathering processes, a survey was implemented in early stages, called the Households Socio-Economic Situation Survey (HSSS). Once the first RS official database was collected in 2009, the 2006 LSMS index was updated on the basis of the first HSSS. For BDH's third stage, the RSII was calculated directly with the second HSSS, collected in 2012. The 2012 HSSS has statistical representativeness at national, urban and rural levels (3.076 households sample).

The estimation process of the original RSII index started with the merging of the 2012 HSSS database to a smaller one containing Unsatisfied Basic Needs (UBN) poverty percentages of disaggregated geographical units of the 2010 Census. Then, a set of individuals, assets, and housing variables, theoretically and empirically related to poverty, were correlated with the per-capita consumption to narrow this selection according to association levels. Those variables with associations higher than 0,11 were upwardly recoded assigning the value of 1 to their "worst" category. Then, the RSII index was built as a simple sum of the re-scaled category quantifications of 34 variables obtained with the CAPTCA as explained earlier (SIISE, 2014).

The replication strategy followed the exact procedure using the 2014 LSMS and the Census[4]. One of the variables could not be constructed from the questionnaire; therefore, only 33 variables were calculated. With this input, I run a version 2.0 CAPTCA algorithm in SPSS 23, attempting to be as close to the original. In Appendix 1, I present the main output of the analysis: iteration history, model summary and component loadings. The 2-dimension specification accounted for 36,5% (the original accumulated 33,7%) of the total variance and had a 62,2% correlation with the monthly per-capita aggregate consumption (formerly 62,4%). Figure 3 shows a histogram of the frequency of households by the created index.

Also importantly, cutoff choice was originally done by selecting values of the index that represented households with consumption poverty. For SELBEN the eligibility criterion was to keep the first two poverty quintiles and for the RS it was the point estimate of the poverty line consumption value from an OLS of the index and the logarithm of the aggregate per-capita consumption (Ponce, 2013). The RSII followed a similar approach to the last, but since the "graduation" strategy started, the critical value was estimated around the extreme poverty, specifically, 28,2. I kept the official threshold since computed pseudo-scores used the same methodology and it meant less manipulation of the design.

4.2.2 Effect estimation

The RDD, firstly discussed by Thistlethwaite and Campbell (1960) can be applied when there is precise knowledge of the rules determining treatment and when the following basic elements are present: an outcome, a continuous assignment covariate, a threshold and a treatment variable (Cook, 2008).

The identification assumption of the design, under the Rubin Causal Model (RCM) setup, is that the conditional expectations functions of the potential outcomes Y_1 and Y_0,

[4]Other approaches applied but discarded were: OLS coefficients of the aggregate consumption and all the variables in the HSSS as category weights, regression based estimated cutoff, nuclear families instead of households, complete sample without distinguishing by surveying-period, and different combinations of the previous.

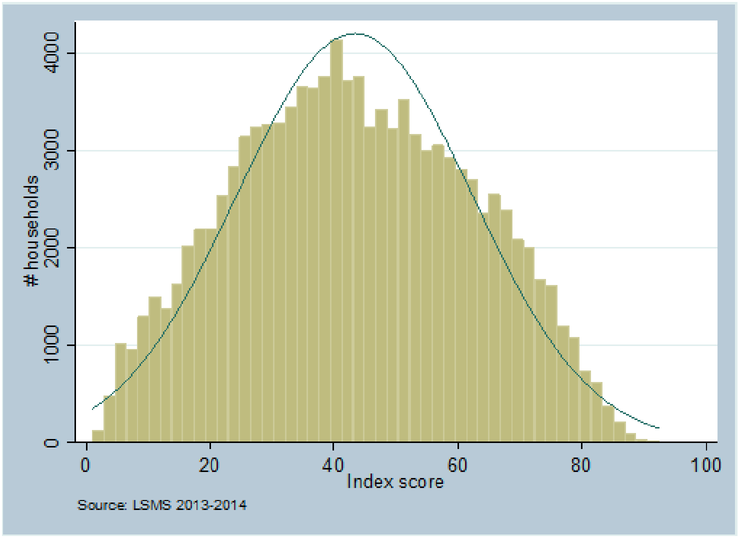

Figure 3: Pseudo-RSII index (full sample)

Note: The frequency histogram is overlaid with a normal density plot. Since they are both very similar and there are no significant deviations from the mean values of the sample, there is no skewness.

given the treatment status (D_i) and the assignment covariate (X), are continuous around the threshold (c) (Imbens and Lemieux, 2008). When the cutoff only partly influences the treatment exposure, the fuzzy design is appropriate (Hahn et al., 2001). In this context, and given the characteristics of the programme it was sensible to use this method. The following pairs up the empirical reasoning of the method to specificities of the case.

Following Angrist and Pischke (2008), in the fuzzy RDD the discontinuity can be seen as an instrumental variable for the treatment status in order to account for the potential bias derived from the probabilistic function, in the following way:

$$P(D_i = 1|x_i) = \begin{cases} g_1(x_i)\ if\ x_i\ \leq c \\ g_0(x_i)\ if\ x_i\ > c' \end{cases} \quad ,\ g_1(c) \neq g_0(c) \tag{1}$$

$$\text{assume } g_1(x_0) > g_0(x_0)$$

The interpretation of (1) is that there is a jump in the probability, with functional form $g(.)$, of receiving the transfer at the threshold $c = 28,2$. Beneficiaries ($D_i = 1$) should have at most an RSII value x_i of 28,2, while non-eligible households should have higher values. This association can also be written as:

$$E[D_i|x_i] = P(D_i = 1|x_i) = g_0(x_i) + [g_1(x_i) - g_0(x_i)]Z \tag{2}$$

$$Z = 1(x_i \leq c)$$

Here Z is a binary instrument [5] that takes the value 1 if the household has a score at least as lower as the value of the cutoff, and 0 otherwise. Importantly, the units under analysis are those very near to the threshold, to resemble an as-if random assignment. Taking into account the non-deterministic association between the index and the probability of the treatment, and following Imbens and Lemieux (2008), and Hahn et al. (2001), the identification result can be written as:

$$\begin{aligned} \alpha_{FRDD} &= E[Y_1 - Y_0 | X = c \text{ and unit } i \text{ is a complier}] \\ &= \frac{\lim_{x\downarrow c} E[Y|X=c] - \lim_{x\uparrow c} E[Y|X=c]}{\lim_{x\downarrow c} E[D|X=c] - \lim_{x\uparrow c} E[D|X=c]} \\ &= \frac{E[Y|Z=1] - E[Y|Z=0]}{E[D|Z=1] - E[D|Z=0]} \end{aligned} \tag{3}$$

Here $Y_{1,0}$ represent the observed outcome (height-for-age z-scores) for either treatment or control groups. The association between the index and HAZ is possible though it is assumed to be smooth, therefore a discontinuity in the conditional expectation is defined as a causal effect. The ratio in (3) is the variation in the outcome discontinuity to the variation on the treatment discontinuity. This non-parametric ratio represents the estimand, which is the local average treatment effect (LATE) and as Angrist and Pischke (2008) stated, it is local not only because is estimated for the compliers, but also because is for those around a specific vicinity. The implementation strategy follows two approaches, one parametric and one non-parametric. For the first, a 2SLS IV regression analysis was implemented. I defined 10, $\pm$ 1 vicinities around the threshold; then, with these bandwidths, I run different versions of the specification:

$$Y = \beta_0 + \beta_1 X + \beta_2 X^2 + \beta_3 W + \alpha D + u \tag{4}$$

where D is instrumented by Z, and α is the effect. Here W represents a vector of few controls empirically defined as determinants of children stunting in Ecuador (World Bank, 2007) that where not already part on the index itself. Specifically, age of the kids in days, sex, indigenous ethnicity, and mother´s height and education level. Tables with the results and plots are presented next, including also the first stage (treatment discontinuity) and the intention-to-treat estimates (outcome discontinuity). For this approach the bandwidth was chosen based in means test of selected covariates for treated and control just around the threshold, similar to Oosterbeek et al. (2008).

In fact, bandwidth choice is one of the challenges that the RDD faces, given the embedded bias-variance trade-off (i.e. smaller bandwidth leads to less bias but higher variance, and vice versa). This has motivated the development of non-parametric estimators, which root from

[5] Usual IV assumptions hold, namely ignorability, first stage and monotonicity.

the work simplified and represented in equation 3. Recent empirical literature on the matter has focused in nonparametric local polynomial estimators with complementary bandwidth choice procedures. These estimators are the results of weighted polynomial regressions above and below the threshold.

To perform these regression approximations a choice of bandwidth is required, generally based on selectors obtained by balancing the squared-bias and variance of the effect estimations. The mean weakness of this procedures is that the window selected is too "large", so that the validity of the assumptions of the distributional approximations cannot be ensured. This increases the probability of biased confidence intervals, which leads to over-rejecting the no treatment effect null hypothesis. The Cross-Validation (CV) bandwidth choice method, developed by Ludwig and Miller (2007), and the Mean Square Error optimal (MSE) by Imbens and Kalyanaraman (2012) are affected by this problem.

To address this issue, novelty work has been developed in Calonico et al. (2014), Calonico et al. (2016a); and Calonico et al. (2016b). The authors implemented a data-driven local polynomial RDD point estimator with bias-corrected confidence intervals. In the light of the exposed, the present study justifies the use of the lastly mentioned procedure. A simplified process of the proposal is as follows:

1. Bias-correction of the FRDD z-score estimator: instead of using the large-sample approximation for the standardised t-statistic, the procedure re-centers this statistic with an estimate of the leading bias.

2. Re-scaling the t-statistic: to complement the conventional bias correction performed in step 1 (which suffers from poor-finite sample performance due to low quality distributional approximation), the corrected t-statistic is re-scaled with a novel standard error specification attempting to account for the variability added by the estimated bias.

Regarding computation, I used the companion Stata commands, rdrobust, rdwselect and rdplot for different specifications mimicking the parametric analysis and checking for differences between them. For both approaches, I included up to quadratic polynomial transformations of the forcing variable as recommended in the research by Gelman and Imbens (2014).

The identification strategy of the RDD design is not directly testable since we never get to evidence the conditional expectation of the counterfactual outcomes, though there are indirect ways to address this. Specifically, falsification tests which stem from two general concerns; effects due to reasons other than the treatment, and manipulation of the forcing variable. Particular attention is given to balance checks of jumps in covariates, for which I reproduced the data-driven regression discontinuity plots developed by Calonico et al. (2015); and to density checks around the cut-off or sorting, for which I present McCrary (2008) and the Cattaneo et al. (2015) tests. In the next section, I show the main findings of the reviewed empirical design.

5 Results

5.1 Falsification test

The BDH has been reaching to a significant proportion of Ecuadorian households for thirteen years, and likewise, other social programmes, the possibility of the manipulation of the rules configure a permanent concern. This, not only due to government reasons, e.g. budget allocation; but also, because it represents a serious threat to evaluation purposes, e.g. the exogeneity of the eligibility index. For the RD design, the manipulation of the forcing variable increases the plausibility of the non-compliance of the assumed identification condition. Therefore, it makes sense to first present the results of this falsification check, along with others mentioned. Figure 4 illustrates the McCrary tests for the selected sample.

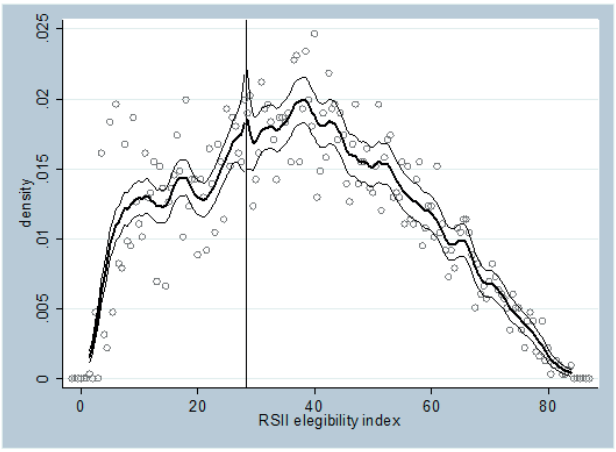

Figure 4: McCrary manipulation test

Note: with a binsize of 0,5 and a bandwidth of 3 the local density function of the households according to their RSII eligibility index does not shows significant differences around the threshold of 28,2.

This finely-gridded smoothed histogram shows that there is no apparent difference in density around the threshold. In fact, with a log discontinuity estimator of 0,046 and a standard error of 0,164, a t-test of the null hypothesis of continuity fails to reject. Likewise, the Cattaneo et al. (2015) test fails to reject the null hypothesis of no systematic manipulation (for all estimation methods) of the forcing variable on the base of t-statistics associated to p-values $> 0{,}1$, within the bandwidth (6,465; 8,895) (Table 3).

The underlying rationale in this case, where an index is reproduced, is that there is no apparent reason for households to under-report their assets or lie about their socioeconomic status to manipulate the RSII since the focus of the LSMS is not related to BDH reception (this could represent more of an issue for the actual Registry). Complementarily, covariate balance tests indicate no significant jump at the discontinuity. Figure 5 depicts Calonico

Table 3: CJX Manipulation test

Cutoff c = 28.2	Left of c	Right of c
Number of obs	2.139	4.182
Eff. Number of obs	652	1.012
Order loc. poly. (p)	2	2
Order BC (q)	3	3
Bandwidths (hl,hr)	estimated	estimated
Bandwidth values	6,465	8,895
Bandwidth scales	0,5	0,5
Method	T	P>T
Conventional	-0,5719	0,5674
Undersmoothed	-0,1293	0,8972
Robust Bias-Corrected	-0,0914	0,9272

Note: With a robust bias-corrected local polynomial of order 2 density estimator of -0,0914 (T) and an associated p-value>0,10 (P>T) we cannot reject the null hypothesis of no statistically significant differences of the densities around the threshold

et al. (2015) polynomial data-driven RD plots, with sample averages within bins and 95% confidence intervals.

The averages of the covariates around the cutoff are very similar, signalling an as good as random local assignment of the BDH. Therefore, we can expect for households with index scores just below and above 28,2 to be similar in all confounders, observed and unobserved. Also, an important step prior to the estimation is the analysis of the association of the treatment D and the instrument Z. A cross-tabulation (Table 4), as well as an OLS, allows us to check the level of compliance that the instrument induces, which is 46%. With an F-statistic of 1.787,39 and a p-value<0,001 we can reject the null hypothesis of no significant association.

This can also be aided by the graphical representation of the probability of the treatment given the RSII index. As seen in Figure 6, there is a jump at the threshold, meaning that households with indexes at least as low as 28,2 are about 14% percentage points more likely to be in the treatment group. Following estimates restrict the observations to the selected discontinuity samples.

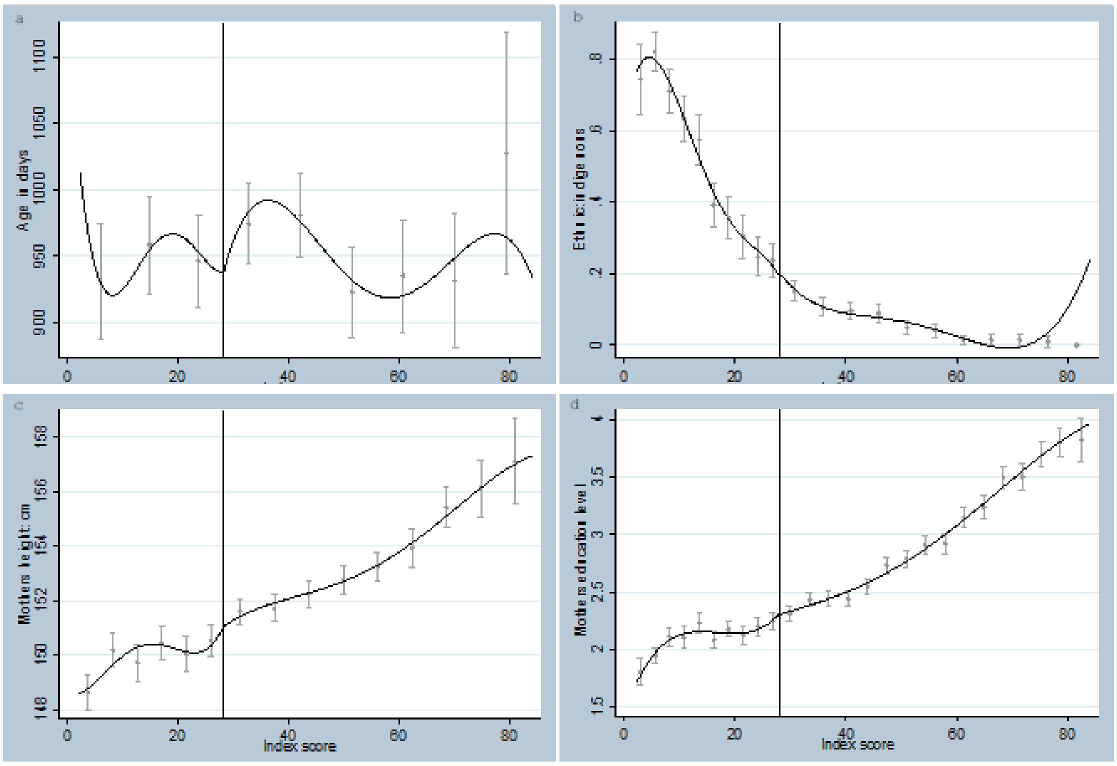

Figure 5: Covariate balance tests

Note: a, b, c and d show local polynomial functions of the forcing variable for the different covariates in the y-axis. Each point estimate is the mean of the covariate value within the bin in an equally-spaced partitioning (triangular kernel) of the RSII index. The plots evidence no jumps at the discontinuity of 28,2 of the estimates of the characteristics of the kids (age and indigenous ethnicity) nor the mothers (height and education level); which, empirically supports the thesis of the un-confoundedness of the effect estimate.

Table 4: BDH compliance

		Z		Total
		0	1	
D	0	3,517	795	4,312
	1	680	1,325	2,005
Total		4,197	2,12	6,317

Note: This cross-tabulation of the actual treatment (D) and the legibility status (Z) allows as to inspect the proportion of compliers in the total sample. This is calculated as follows: (D=1/Z=1) - (D=1/Z=0) = 0,63-0,16=0,46

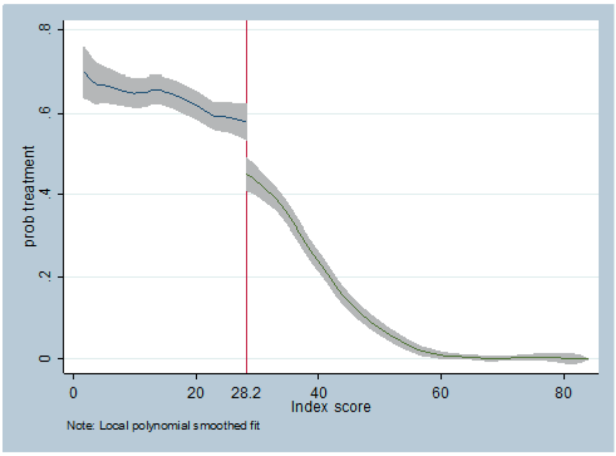

Figure 6: Probability of treatment

Note: there is a decrease in the probability Treatment (D) at the discontinuity cutoff 28,2, in around 14% given a local polynomial smoothed fit of the RSII index score.

5.2 Parametric estimates

After testing various specifications used in empirical studies in the thematic area, and specifically for the Ecuadorian case, three [6] were chosen: (1) linear (2) quadratic (3) quadratic with covariates, all with robust household clustered variance estimation. Estimates were computed for up to a ± 10 window; though, the vicinity selected was ± 3, since it was the one where most of the covariates shows not significant differences between eligibility groups (Appendix 2). The cross-tabulation of the instrument and the treatment for this discontinuity sample as well as the mean z-score by compliance status are included in Appendix 3. The first stage estimates are presented in Table 5.

I find that having a RSII score at least as low as 28,2 is translated in a significant increase in the probability of receiving the BDH transfer of around 18%-19%. The F-statistics for the null hypothesis that the instrument does not induces significant variation, have associated p-values$<$0,05, though the statistics range from 5,5 to 6,8 (Appendix 4), which are lower than the required minimum of 10 for non-weak instruments (Stock et al., 2002). Additionally, there is a concern about the statistical inference due to the reduced sample size which sums up to 662. In fact, these arguments justify the application of the alternative approach. Nonetheless, what is more important is that the instrument is uncorrelated with the error term in the HAZ equation, to ensure the local "randomization". It is arguable that given the arbitrary cutoff and the flexible functional form of the index, the last should not be correlated with unobserved confounders determining the anthropometric outcome.

[6] Additionally, one specification for different slopes was included ($X * Z$), though the first stage estimates were not significant. Therefore, not reported.

Table 5: First stage (parametric)

VARIABLES	(1)	(2)	(3)
Z	0,175*	0,176*	0,189**
	(0,0925)	(0,0928)	(0,0878)
X	0,0232	0,111	0,278
	(0,0268)	(0,501)	(0,475)
X2		-0,00154	-0,00435
		(0,00881)	(0,00833)
age (months)			7,75e-05**
			(3,23E-05)
sex (1: male)			0,0296
			(0,0378)
ethnicity (1: indigenous)			0,185***
			(0,0536)
mother's height			0,00211
			(0,0035)
mother's education level			-0,166***
			(0,0332)
Constant	-0,235	-1,472	-4,023
	(0,796)	(7,117)	(6,772)
Observations	662	662	662

Robust standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var: BDH treatment (D)

Note: Coefficients of Z in (1), (2) and (3) are parametric estimates of the effect of the eligibility status on the actual treatment. They are all significant and, for instance, the quadratic specification estimate can be interpreted as an increase in 19% percent in the probability of receiving BDH for households in the vicinity with values for the RSII index as lower as 28.2.

Given the randomness of the discontinuity sample we could describe the effects of Table 6, as those of the BDH over the stunting. Though, not all eligible households effectively receive the transfer, and some not eligibly end up receiving it. Therefore, the ITT is an underestimated effect, reason why we require to compute the ratio of this value to the first stage, also called compliers proportion. In this case the ITT effect of the BDH over stunting z-scores ranges from -0,41 to -0,34. Nevertheless, only for specification (3) the estimates are significant and at a 5% level.

The mentioned ratio is the LATE of the benefit on the HAZ for the bandwidth (25,2;31,2).

Table 6: Intention-to-treat (parametric)

VARIABLES	(1)	(2)	(3)
Z	-0,342	-0,340	-0,406**
	(0,214)	(0,214)	(0,197)
X	-0,0397	0,132	-0,369
	(0,0546)	(0,978)	(0,919)
X2		-0,00304	0,00522
		(0,0173)	(0,0162)
age (months)			-0,000249***
			(9,39e-05)
sex (1: male)			-0,289***
			(0,0900)
ethnicity (1: indigenous)			-0,283**
			(0,111)
mother's height			0,0451***
			(0,00877)
mother's education level			0,0161
			(0,0722)
Constant	-0,115	-2,547	-1,357
	(1,641)	(13,83)	(13,06)
Observations	662	662	662

Robust standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var: HAZ (Y)

Note: Coefficients of Z in (1), (2) and (3) are parametric estimates of the effect of the eligibility status on stunting z-scores. This intention-to-treat estimate is only significant for the last specification, where all but one covariate have significant associations with the dependent variable.

The estimates account for an average decrease of around 2 standard deviations in HAZ, though non-of them are significantly different from 0. For example, in specification (3) the LATE of -2,147 is the result of -0,406/0,189. The average z-score for those whom are to the left of the threshold in the specific band was -1,6 (stunting prevalence of 36,46%), which would become even more deviated when adding the found negative effect. Though, this interpretation is purely methodological since the estimate is not significant [7].

[7] Note that the estimates look very similar between models, which amounts to the argument of the fulfilment of the RDD identification strategy

Table 7: FRDD estimates (parametric)

VARIABLES	(1)	(2)	(3)
D	-1,951	-1,928	-2,147
	(1,503)	(1,499)	(1,410)
X	0,00553	0,346	0,228
	(0,0425)	(1,275)	(1,299)
X2		-0,00601	-0,00411
		(0,0225)	(0,0230)
age (months)			-8,26e-05
			(0,000166)
sex (1: male)			-0,225*
			(0,121)
ethnicity (1: indigenous)			0,113
			(0,299)
mother's height			0,0496***
			(0,0113)
mother's education level			-0,340
			(0,260)
Constant	-0,574	-5,385	-9,994
	-1,772	(18,13)	(18,15)
Observations	662	662	662

Robust standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var: HAZ (Y)
D instrumented by Z

Note: Coefficients of D in (1), (2) and (3) are estimates of the effect of BDH reception on stunting z-scores. None of these programme effect estimates are statistically significant.

5.3 Non parametric estimates

For this approach, I took advantage of the tools developed Calonico et al. (2014) and Calonico et al. (2016a) which allows to compare a) conventional estimates with conventional variance estimators, b) conventional but bias-corrected and c) bias-corrected robust non-parametric estimators, for different bandwidths and polynomial fits of the forcing variable for both, the effect estimates and the confidence intervals.

The logic for the especifications selection was similar to the parametric section, therefore the same three were used, though the difference (other than the method itself) is that the bandwidth for the CI was allowed to be data driven and particular to the especification [8]. The method for bandwidth selection procedure implemented was one common below and above the threshold Coverage Error Rate (CER), instead of the MSE since the former is optimal for statistical inference (Calonico et al., 2016a). Also, a tringular kernel regression was used for all cases.

Given the particularity allowed for each specification, the main bandwidths are three $\pm$5,252, $\pm$6,338 and $\pm$ 6,248. Consequently, each model has a different sample specifically 1.142, 1.350 and 1.331 all reported in the tables. Correspondingly, for the CI bandwidths and their sample.

As detailed in Table 8, the probability of treatment due to an eligible index increases significantly ranging from 0,18 to 0,24 for the robust procedure. Compared to the conventional non-parametric estimates, as well as to parametric, the values are larger. Signalling, that even after correcting for possible bias, the instrument still influences the probability of programme participation. In order visualize these first stage effects plots for each specification are depicted in Figure 7 [9].

For the ITT effects I find statistical inference differences between procedures, and a very similar path when comparing to the parametric results. The only significant robust estimate belongs to specification (3), which could be interpreted as the isolated effect of the instrument on the outcome. The 0,48 standard deviation decrease in HAZ refers to a main bandwidth of 6,3, with 627 observations below the threshold and 704 to the right (Table 9). Figure 8 accounts for this estimates.

Finally, the local polynomial LATE estimates are detailed in Table 10. None of the robust estimates are statistically distinguishable from 0, contrary to what happens with the conventional and bias-corrected for the last model, where the conventional p-values can lead to incorrect interpretation of the possible effect of the BDH. All estimates are lower than the parametric (for each specification), though all in average are close to a -2-standard deviation estimate.

[8] After comparing the CI reduction for each more complex specification between a particular one and a fixed one (corresponding to specification (1)), the largest percentages were associated to the non-fixed; therefore, each model has its own CI bandwidth.

[9] Software output estimates refer to what happens at the discontinuity. So, for instance, in (1) the original was negative since there is a negative jump due to the decrease in the probability of treatment to the right of the cutoff. Though, for comparability matters (with the parametric), the signs were inverted. The same case for the ITT estimates. The Effect estimates are left unaffected.

Table 8: First stage (non-parametric)

VARIABLES	(1)	(2)	(3)
Conventional	0,140**	0,185**	0,214***
	(0,063)	(0,0817)	(0,0776)
Bias-corrected	0,177***	0,205**	0,237***
	(0,063)	(0,0817)	(0,0776)
Robust	0,177**	0,205**	0,237***
	(0,0767)	(0,0918)	(0,0887)
Controls	NO	NO	YES
Observations	6,321	6,321	6,321
Obs. left main-bandwidth	549	637	627
Obs. right main-bandwidth	593	713	704
Obs. left bias-bandwidth	700	803	768
Obs. right bias-bandwidth	816	973	897
Conventional p-value	0,0261	0,0233	0,0059
Robust p-value	0,0208	0,0258	0,00747
Order Loc. Poly. (p)	1	2	2
Order Bias (q)	2	3	3
BW Loc. Poly. (h)	5.252	6.338	6.248
BW Bias (b)	7.272	8.553	8.015

Standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1

Note:(1), (2) and (3) present non-parametric estimates of the effect of the eligibility status on the actual treatment. The table shows conventional, bias-corrected and robust estimates, and the specificities of the sample since the bandwidths are data-driven. They are all significant and positive. For instance, the quadratic specification robust estimate can be interpreted as an increase in 24% percent in the probability of receiving BDH for households in the vicinity (1331 observations) with values for the RSII index as lower as 28.2

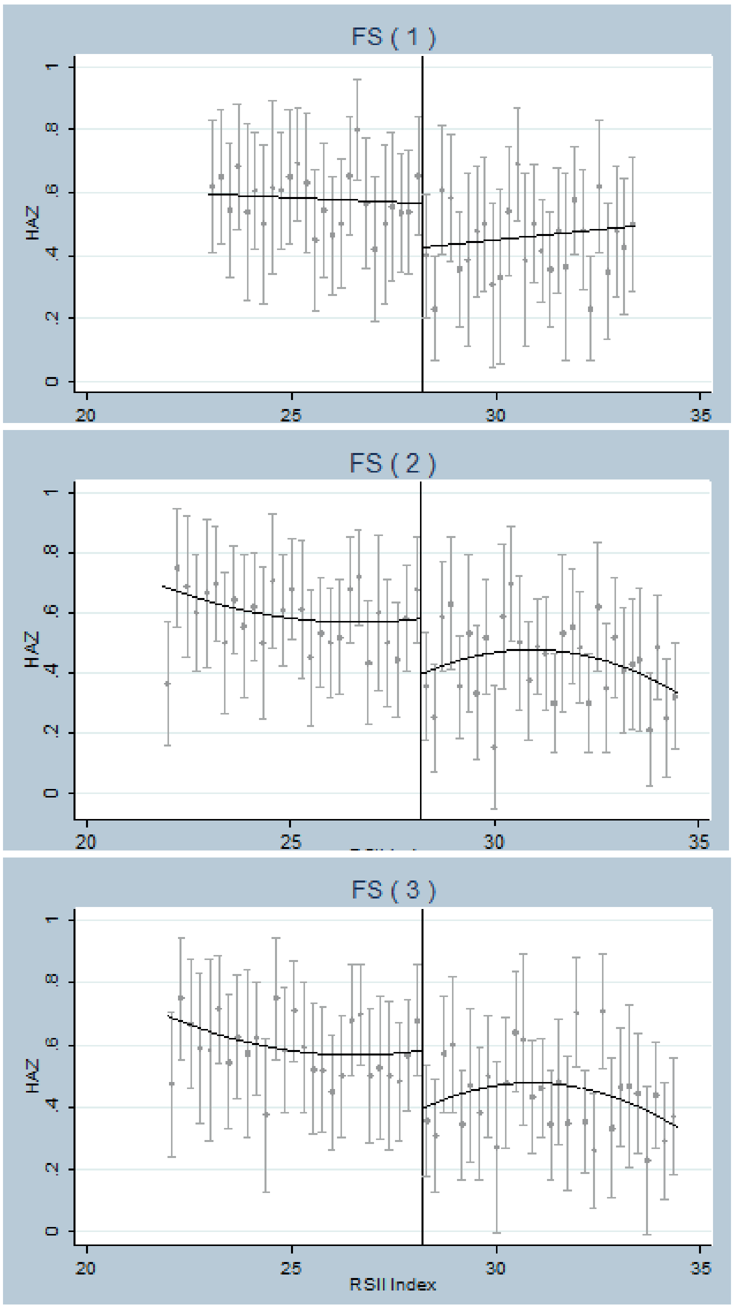

Figure 7: First stage (non-parametric robust)

Note: FS (1), FS (2) and FS (3) plot the robust estimates of the effect of the eligibility status on the probability of treatment, given the different specifications. All evidence the higher probability of treatment for those households reporting index scores to the left of the cutoff.

Table 9: Intention-to-treat (non-parametric)

VARIABLES	(1)	(2)	(3)
Conventional	-0,286*	-0,330	-0,434**
	(0,165)	(0,214)	(0,200)
Bias-corrected	-0,304*	-0,348	-0,484**
	(0,165)	(0,214)	(0,200)
Robust	-0,304	-0,348	-0,484**
	(0,202)	(0,240)	(0,227)
Controls	NO	NO	YES
Observations	6,321	6,321	6,321
Obs. left main-bandwidth	549	637	627
Obs. right main-bandwidth	593	713	704
Obs. left bias-bandwidth	700	803	768
Obs. right bias-bandwidth	816	973	897
Conventional p-value	0,0834	0,123	0,0297
Robust p-value	0,133	0,147	0,0327
Order Loc. Poly. (p)	1	2	2
Order Bias (q)	2	3	3
BW Loc. Poly. (h)	5,252	6,338	6,248
BW Bias (b)	7,272	8,553	8,015

Standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var= HAZ (Y)

Note:(1), (2) and (3) present non-parametric estimates of the effect of the eligibility status on stunting z-scores. The table shows conventional, bias-corrected and robust estimates, and the specificities of the sample since the bandwidths are data-driven. Statistical significance is not homogeneous, though they are all negative. For instance, the significant quadratic specification robust estimate can be interpreted as a decrease of 0,48 sd. in HAZ due to an eligible index for households in the vicinity (1.331 observations)

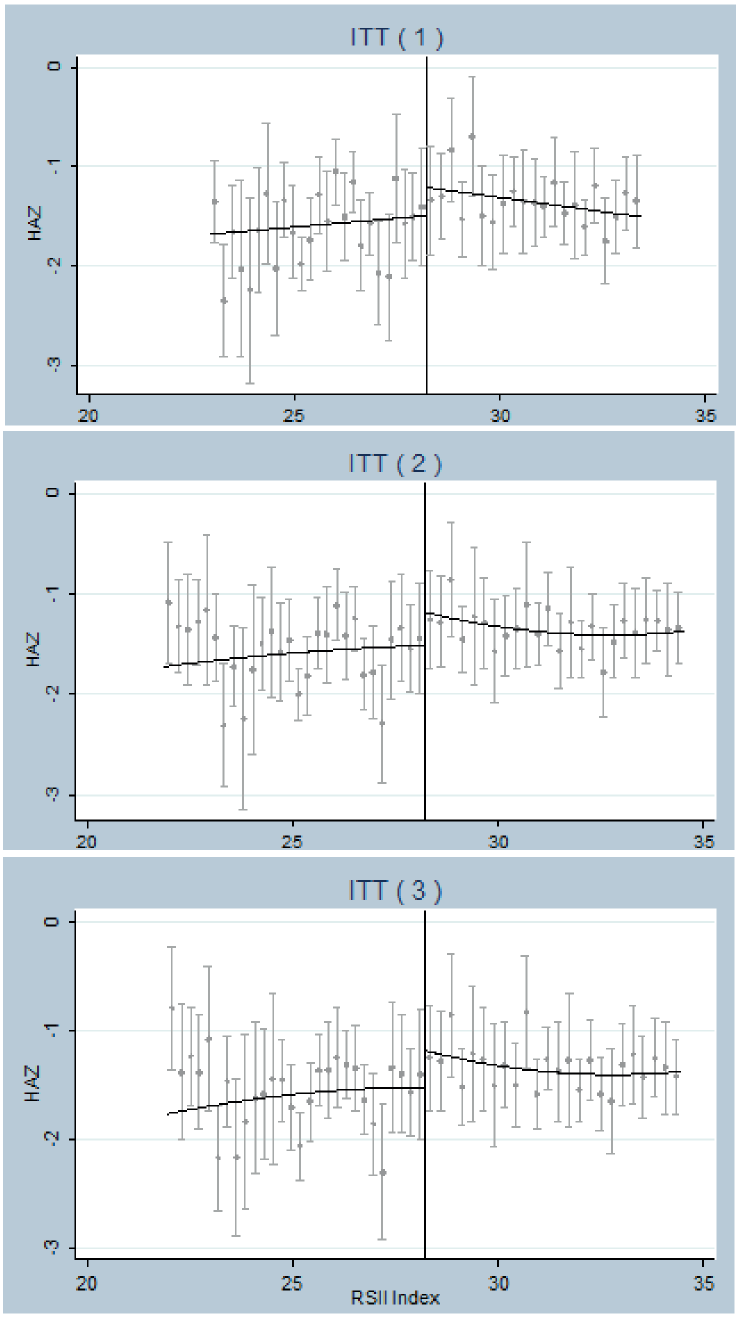

Figure 8: Intention-to-treat (non-parametric robust)

Note: ITT (1), ITT (2) and ITT (3) plot the robust estimates of the effect of the eligibility status on stunting z-scores, given the different specifications. All evidence lower HAZ values for households reporting index scores to the left of the cutoff

Table 10: FRDD estimates (non-parametric

VARIABLES	(1)	(2)	(3)
Conventional	-2,045	-1,783	-2,032*
	(1,409)	(1,299)	(1,170)
Bias-corrected	-1.624	-1.694	-2,042*
	(1,409)	(1,299)	(1,170)
Robust	-1,624	-1,694	-2,042
	(1,711)	(1,442)	(1,322)
Controls	NO	NO	YES
Observations	6.321	6.321	6.321
Obs. left main-bandwidth	549	637	627
Obs. right main-bandwidth	593	713	704
Obs. left bias-bandwidth	700	803	768
Obs. right bias-bandwidth	816	973	897
Conventional p-value	0,147	0,170	0,0824
Robust p-value	0,343	0,240	0,122
Order Loc. Poly. (p)	1	2	2
Order Bias (q)	2	3	3
BW Loc. Poly. (h)	5,252	6,338	6,248
BW Bias (b)	7,272	8,553	8,015

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dep var= HAZ (Y)

Note:(1), (2) and (3) present non-parametric estimates of the effect of BDH reception on stunting z-scores. The table shows conventional, bias-corrected and robust estimates, and the specificities of the sample since the bandwidths are data-driven. They are all negative, and particularly non-significant for the robust estimation

6 Conclusions and discussion

This study proposed a causal inference design attempting to evaluate the effect of the Ecuadorian social programme Human Development Bonus on children chronic malnutrition or stunting. The strategy implemented tries to overcome the challenge of the lack of anthropometric measures in the official participant's registries. The outcome selected is of particular interest, since its prevalence, has been stagnated for the last decade around 25%. Also, research of this topic is important given that the BDH dedicates considerable budget, 251 US$ million in 2016 according to MCDS.

Relying on the latest national Living Standard Measurement Survey, and in the richness of its questionnaire, I delineated a two stage quasi-experimental FRDD study. The first stage reproduces the eligibility index RSII, based on a Categorical Principal Component Analysis. The created 0 to 100 index accounts for 36,5% of the total variance of the selected variables and has a 62,2% correlation with the monthly per-capita aggregate consumption (similar to the original index). The official threshold was adopted; therefore, households with values at least as lower as 28,2 are considered eligible. For the second stage, I exploited the characteristics of the programme, mainly the continuous RSII and the cutoff, to implement a RDD on its fuzzy version given that the reception of the bonus showed to be not perfectly determined by the eligibility status (or instrument). The size of the sample for the evaluation had to be reduced due to intricate policy and methodological changes happening in the surveying period, trying to avoid extra noise in the results. Additionally, since the outcome was only registered for boys and girls up to 59 months old, the valid final sample was compressed to 6.174 observations.

The identification strategy of the RDD, was tested indirectly via manipulations tests and covariate balance. Both, empirically support the smoothness of the potential outcomes for the transfer recipients and non-recipients around the cutoff. Once the possibility of effect falsification was rejected estimates were drawn from two approaches a parametric two-stage least square instrumental variable regression analysis and a non-parametric local polynomial regression with robust bias-corrected estimates and confidence intervals.

For the IV estimates the bandwidth selected was ± 3, for which covariate average values by eligibility group showed to be not significantly different. Based on three specifications, linear (1), quadratic (2), and quadratic with controls (3), I found that the first stage accounted for an increase in the probability of treatment of around 18% to 19%. The effect of the instrument in the outcome, i.e. the intention-to-treat, showed a significant coefficient only for specification (3) with a -0,41 s.d. The ratio of these ITT (outcome discontinuity) to the first stage (treatment discontinuity), is the effect of the BDH for the compliers within the chosen bandwidth. None of these ratio estimates was found to be significantly different from 0 which could lead to fail to reject the null hypothesis of no effect. Though, even when the minimal RDD restrictions are likely to be met, under the fuzziness of the present design the last conclusion might seem a bit extreme.

Complementarily, the findings from the non-parametric procedure based on the work by Calonico, Cattaneo, Titiunik and others, account for estimates very similar to those from the IV method. With the same specifications as my parametric, but relying on data-driven bandwidths, the local polynomial robust estimates showed no statistically significant effect of the BDH over stunting z-scores. Though, as previously mentioned we ought to be cautious when concluding about the policy. A finding that could be emphasized is that the 10% level significant conventional non-parametric effect estimate for specification 3 ($\alpha =$ -2,032, p-value = 0,082) changed to non-significant when robust bias corrected ($\alpha =$ -2,042, p-value = 0,122). Consequently, if the present research would have only relied on a conventional variance estimator, there would have been incorrect indicative signs of a counterproductive effect of the transfer.

The RDD design itself is built upon the internal validity of the results for the compliers units in an arbitrary bandwidth. This could be argued for this case, under the evidence accounting for an as if random assignment around the threshold in the selected vicinity, without forgetting the challenges that the study faced. The external validity was tested by running the same models for the complete evaluation sample. The results for a parametric approach (Appendix 5) still showed a significant first stage, of around 0,14 to 0,15, not too far from the vicinity sample. Though, neither the ITT, nor the IV estimates showed similar patterns. Therefore, the national validity of the results is not likely. Analysing a wider context, we could attempt to make a light comparison with other Latin American programmes. For instance, the Colombian *Familias en acción*, as well as the Mexican *PROGRESA*, and the Nicaraguan *Red de Protección Social*, have shown significant positive effects over HAZ in several studies. While, the *Bolsa Alimentação* in Brazil has evidenced larger gains in anthropometric measures for non-treated children (detailed in section 2.3). The first ones relied on experimental evaluations which ensured no selection bias, while, the last implemented a meticulous matching of administrative data trying to resemble an experiment. This could be pair-wised to the intricate conditions under which the present research had to be conducted, and give some insight to the reasons of the findings.

Importantly, the previously mentioned programmes that found to be beneficial for children, where evaluated in a period of two to three years after the first implementation and the major gains in height for age were reported for those, in fact, under 24 or 36 months old. This is also the case for the estimates of the relevant studies of the effect of BDH on nutritional outcomes in Ecuador previously outlined. Even though, these initial effects of the programme cannot be estimated with the present design, I analysed if there might be a differentiated effect depending on the intensity of the treatment. I computed the difference between the years of the treatment a household received and the age of the kid. Around 89% of the children had received the transfer all their life, which was expected due to the date of the evaluation. Therefore, the number of individuals that reported less than full life treatment was only 34, from which only 16 had received it 2 to 3 years. Even though, I could not proceed to an evaluation with that number of observations, this is a suggestion for further research that could be done if data more robust could be accessible.

A second exercise I made to analyse this relationship between age and treatment effect, and also attempting to test the well evidenced argument that the earlier the intervention the better the expected outcomes (Behrman and Hoddinott, 2005), was to estimate results by single age groups. Given the advantages showed by the non-parametric approach and due to the related sample reduction, I computed local polynomial robust estimates for specification (3). Findings are still negative and non-significant, though; for instance, the group age of 0 to 1 years old has the lowest negative value (Appendix 6). This could incentive further discussion, since it is somehow aligned to the higher benefits empirically reported for the youngest children.

Furthermore, and in order to complement HAZ findings, empirical work in the area frequently analysis other anthropometric measures, for instance, weight-for-height z-scores (WAZ). This indicator named global stunting is not very stable, since it is highly dependent on short-term factors, which are even harder to account for in survey data. Though, to mimic other researches I estimated the effect of BDH over WAZ. The estimators are still non-significant and negative ranging from -1,5 to -1 (Appendix 7).

These results might at first seem controversial, though it is important trying to build upon the understanding of the paths that could be identified as mediators of this socio-biological outcome in the Ecuadorian context. According, to the LSMS from respondents in the vicinity ± 3, 95,82% know about the conditions and 99% of them report to follow these requirements. Then, seemingly the problem is not due to the lack of commitment, but perhaps the effectiveness of the services provided. Though, it is important to mention that individuals are incentivised to distort reality if asked about fulfilment of a particular settlement when their actions are not monitored. Therefore, I inspected expenditure made with the transfer, which though still self-reported is less likely to include false information, since the LSMS questionnaire gathers detailed household expenses. For the specific sample, the largest percentage is dedicated to a category that includes food and household equipment (53,73%), followed by lower values for education (32,54%) and health (9,55%) (Appendix 8). Though, it would be ideal to have more disaggregated categories, we can see that not many household resources are destined for health. Participants might adopt this attitude since they expect government services to be enough, though, if the aim is to gradually reduce the dependence of their development on government actions, they could and should change their priorities. This might be a compelling reason for the non-significant estimate results 11 years after the first implementation.

To sum up, given the fuzziness of the sample, the timing of the evaluation and the processes undertaken by the government during the surveying period, there is so much uncertainty that it is difficult to ensure a non-significant effect of the programme. Though, recent studies of the BDH effect, which also rely on survey data, have in fact found either negative or non-significant effects. Specifically, Gonzalez-Rozada and Llerena (2011) concluded that mothers receiving BDH experience longer periods of unemployment compared to those who do not; and Carranza and Méndez (2015) reported a non-significant effect over exclusive breastfeeding which is a determinant factor of stunting. In this sense, the research findings might not only reflect the embedded challenges of the design, but also a possible decrease in the intensity of the effects of the programme, and the need of revision of this policy scheme.

Moreover, and aiming to get conclusive results, it could be interesting to evaluate mediating indicators or intermediate effects. For example, the impact on diet composition (calories by type of food) aiming to search for pathways that determine final outcomes. Similarly, with health variables related to mothers, as breastfeeding or their consumption patterns.

Regarding the time issue, I suggest that on the basis of panel datasets, long term-effects over children health related outcomes should be estimated. This study would require the historical official participant's registry data, as well as information from government offices who monitor kids from BDH households. This enhances the importance of the institutional efforts that ought to be made to achieve good-quality interconnection registry data. Finally, and in the light of the recent changes in index calculation methodology, threshold, transfer amount and companion strategies, as the "graduation", there is motivation to research the effect of these complementary measures.

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Appendices

Appendix 1 CAPTCA results for RSII replica

Iteration History

Iteration Number	Variance Accounted For		Loss		
	Total	Increase	Total	Centroid Coordinates	Restriction of Centroid to Vector Coordinates
0^a	11,791936	0,000006	54,208064	53,763800	0,444264
9^b	12,046098	0,000005	53,953902	53,707942	0,245960

a. Iteration 0 displays the statistics of the solution with all variables, except variables with optimal scaling level Multiple Nominal, treated as numerical.
b. The iteration process stopped because the convergence test value was reached

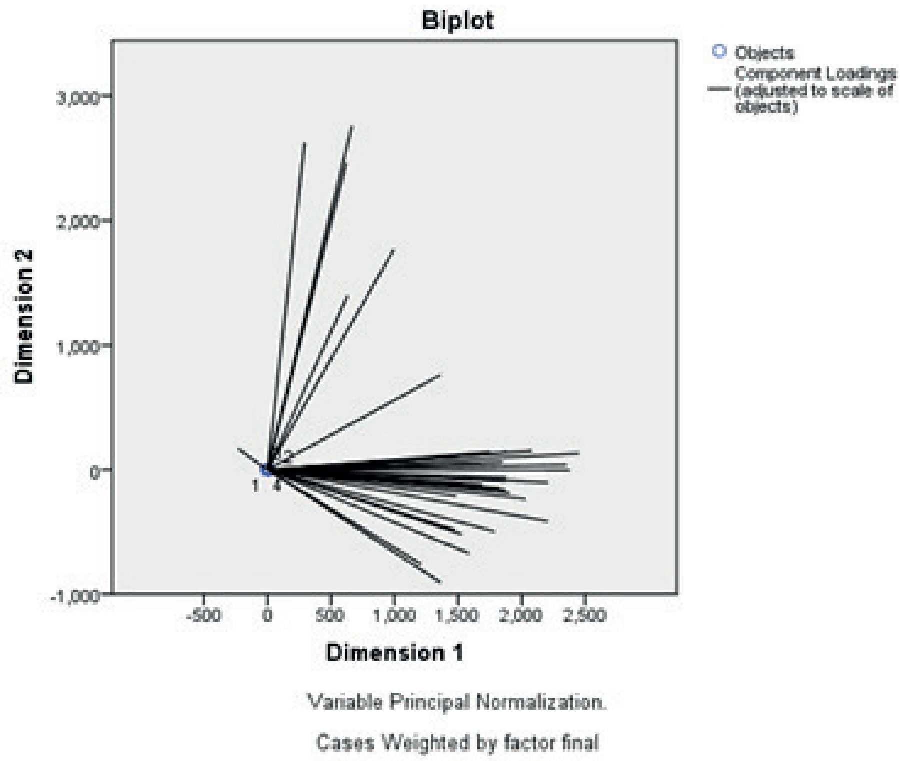
Model Summary

Dimension	Cronbach's Alpha	Variance Accounted For	
		Total (Eigenvalue)	% of Variance
1	0,918	9,122	27,644
2	0,678	2,924	8,860
Total	$0,946^a$	12,046	36,503

a. Total Cronbach's Alpha is based on the total Eigenvalue.

Component Loadings

	Dimension	
	1	2
type of flooring	0,743	0,012
shower access/location	0,577	-0,047
hygienic services	0,752	-0,002
walls material	0,53	0,018
water access	0,594	-0,02
roof material	0,552	0,044
housing condition	0,584	0,019
road access	0,594	0,044
basic services location	0,698	-0,032
overcrowding	0,43	0,24
mean poverty consumption by sector	0,775	0,041
rural/urban area	0,593	-0,051
# persons	0,092	0,828
household-head education level	0,603	-0,059
computer/laptop	0,698	-0,131
washing machine	0,564	-0,157
blender	0,466	-0,153
oven	0,593	-0,029
ironing equipment	0,501	-0,212
refrigerator/freezer	0,484	-0,164
telephone	0,657	0,048
automobile	0,487	-0,037
internet access	0,641	-0,074
type of education (children)	0,196	0,777
# children under 14 years	0,21	0,871
illiteracy	0,314	0,558
health insurance	0,535	-0,038
drinking water	0,203	-0,024
tv or dvd	0,382	-0,239
cable tv	0,469	-0,067
cell phones	0,431	-0,286
tenure status	-0,073	0,053
childcare	0,199	0,441



Appendix 2 Descriptive statistics by eligibility status (vicinity ±3)

VARIABLES	Eligible (mean)	Not eligible (mean)	p-value (t-test)
Age (days)	963,699	985,487	0,595
Sex (male)	0,508	0,529	0,591
Ethnics (indigenous)	0,225	0,192	0,301
Childcare centre assistance	0,291	0,325	0,349
Mother's Education	2,243	2,312	0,183
Father´s Education	6,234	6,378	0,242
Mother´s height (cm)	150,210	151,063	0,083
number of children	2,930	2,760	0,126
household size	5,486	5,321	0,320
urban area	0,243	0,324	0,021
Overcrowding	0,553	0,502	0,183
Illiteracy	1,067	1,105	0,673
RSII	26,727	29,716	0,000
Obs.	329	333	

Appendix 3 BDH compliance (parametric discontinuity sample)

		Z		Total
		0	1	
D	0	182	145	327
	1	151	184	335
Total		333	329	662

	Mean z-score	Std. Error	95% Conf. Interval	
D=0, Z=0	-1,109	0,098	-1,304	-0,915
D=1, Z=1	-1,860	0,075	-1,735	-1,437
D=1, Z=0	-1,170	0,091	-1,696	-1,338
D=0, Z=1	-1,430	0,107	-1,643	-1,218

Appendix 4 F-tests for instrument Z

	(1)	(2)	(3)
F statistic	5,44559**	5,48107**	6,84465***
p-value	0,0199	0,0195	0,0091

*** p<0,01, ** p<0,05, * p<0,1

Appendix 5 IV Effect estimates (complete sample)

	First stage		
	(1)	(2)	(3)
VARIABLES	Model 1	Model 2	Model 3
Z	0,147***	0,143***	0,146***
	-0,0239	-0,033	-0,0324
X	-0,0101***	-0,0105***	-0,00877***
	-0,000482	-0,00237	-0,00245
X2		4,92E-06	1,43E-05
		-2,26E-05	-2,33E-05
age (days)			3,39e-05***
			-8,14E-06
sex (1: male)			-0,0226**
			-0,0101
ethnicity (1: indigenous)			0,0461**
			-0,022
mother's height			-0,00135
			-0,000941
mother's education level			-0,0810***
			-0,00906
Constant	0,649***	0,659***	0,956***
	-0,0277	-0,0614	-0,156
Observations	6,321	6,321	6,321
R-squared	0,284	0,284	0,302

Robust standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var: BDH treatment (D)

	ITT		
	(1)	(2)	(3)
VARIABLES	Model 1	Model 2	Model 3
Z	-0,0744	-0,0503	-0,0491
	-0,06	-0,0748	-0,0708
X	0,0180***	0,0209***	0,00966*
	-0,00153	-0,00575	-0,00565
X2		-3,13E-05	1,78E-05
		-5,95E-05	-5,86E-05
age (days)			-0,000267***
			-3,32E-05
sex (1: male)			-0,0727**
			-0,0323
ethnicity (1: indigenous)			-0,313***
			-0,0489
mother's height			0,0477***
			-0,00293
mother's education level			-0,00331
			-0,0265
Constant	-1,927***	-1,989***	-8,528***
	-0,0756	-0,141	-0,454
Observations	6,321	6,321	6,321
R-squared	0,072	0,072	0,144

Robust standard errors in parentheses
*** p¡0,01, ** p¡0,05, * p¡0,1
Dep var: HAZ (Y)

IV estimate			
	(1)	(2)	(3)
VARIABLES	Model 1	Model 2	Model 3
D	-0,507	-0,352	-0,336
	-0,412	-0,523	-0,486
X	0,0129**	0,0172	0,00672
	-0,00542	-0,0105	-0,00924
X2		-2,96E-05	2,26E-05
		-6,14E-05	-6,34E-05
age (days)			-0,000256***
			-3,77E-05
sex (1: male)			-0,0803**
			-0,0345
ethnicity (1: indigenous)			-0,298***
			-0,0535
mother's height			0,0472***
			-0,00302
mother's education level			-0,0305
			-0,0485
Constant	-1,598***	-1,757***	-8,207***
	-0,333	-0,472	-0,736
Observations	6,321	6,321	6,321
R-squared	0,062	0,07	0,138

Robust standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var: HAZ (Y)
D instrumented by Z

Appendix 6 FRDD Non-parametric estimates by age group (specification 3)

Age	0	0 to 1	0 to 2	0 to 3	0 to 4
VARIABLES					
Conventional	-0,721	-0,732	-1984	-1758	-2,032*
	-1367	-0,753	-1298	-1298	-1170
Bias-corrected	-1174	-0,753	-1857	-1476	-2,042*
	-1367	-0,753	-1298	-1298	-1170
Robust	-1174	-0,753	-1857	-1476	-2042
	-1488	-0,811	-1460	-1463	-1322
Observations	1,055	2,249	3,532	4,9	6,321
Obs, left main-bandwidth	110	183	323	608	627
Obs, right main-bandwidth	95	177	340	695	704
Obs, left bias-bandwidth	141	265	401	772	768
Obs, right bias-bandwidth	139	279	448	924	897
Conventional p-value	0,598	0,331	0,126	0,176	0,0824
Robust p-value	0,43	0,353	0,203	0,313	0,122
Order Loc, Poly, (p)	2	2	2	2	2
Order Bias (q)	3	3	3	3	3
BW Loc, Poly, (h)	6020	4726	5612	7953	6248
BW Bias (b)	8415	7346	7393	10,4	8015

Standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var= HAZ (Y)

Appendix 7 Alternative outcome (global stunting measured as weight-for-age zscore)

IV effect estimate			
	(1)	(2)	(3)
VARIABLES	Model 1	Model 2	Model 3
D	-1460	-1468	-1517
	-1174	-1174	-1077
X	0,0259	-0,0908	-0,121
	-0,0382	-1096	-1090
X2		0,00206	0,00262
		-0,0194	-0,0194
age (days)			-3,86E-05
			-0,000126
sex (1: male)			-0,132
			-0,102
ethnicity (1: indigenous)			0,364
			-0,24
mother's height			0,0303***
			-0,009
mother's education level			-0,271
			-0,194
Constant	-0,513	1136	-2336
	-1505	-15,48	-15,28
Observations	662	662	662

Robust standard errors in parentheses
*** p<0,01, ** p<0,05, * p<0,1
Dep var: WAZ (Y)
D instrumented by Z

VARIABLES	(1)	(2)	(3)
Conventional	-1,613	-1,118	-1,184
	(1,144)	(0,976)	(0,861)
Bias-corrected	-1,034	-0,873	-1,067
	(1,144)	(0,976)	(0,861)
Robust	-1,034	-0,873	-1,067
	(1,365)	(1,074)	(0,973)
Observations	6,321	6,321	6,321
Obs. left main-bandwidth	550	677	637
Obs. right main-bandwidth	599	781	713
Obs. left bias-bandwidth	730	909	776
Obs. right bias-bandwidth	864	1133	914
Conventional p-value	0,159	0,252	0,169
Robust p-value	0,449	0,416	0,273
Order Loc. Poly. (p)	1	2	2
Order Bias (q)	2	3	3
BW Loc. Poly. (h)	5,314	6,942	6,342
BW Bias (b)	7,633	9,797	8,133

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Dep var: WAZ (Y)

Appendix 8 Categories of transfer expenditure

BDH transfer expenditures

years of treatment	Freq.	Percent	Cum.
health	32	9,55	9,55
education	109	32,54	42,09
clothing	8	2,39	44,48
savings	1	0,3	44,78
small business	2	0,6	45,37
food and household equipment	180	53,73	99,1
debt payment	3	0,9	100
Total	335	100	