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Will elders provide for their grandchildren? Unconditional cash transfers and educational expenditures in Bolivia

Gustavo Canavire-Bacarreza[1] | **Alberto Chong**[2,3] | **Fernando Ríos-Avila**[4] | **Mónica Yáñez-Pagans**[5]

[1]Inter-American Development Bank, Washington, DC, USA
[2]Georgia State University, Atlanta, GA, USA
[3]Universidad del Pacifico, Lima, Peru
[4]The Levy Economics Institute of Bard College, New York, NY, USA
[5]The World Bank, Washington, DC, USA

Correspondence
Alberto Chong, Georgia State University, Atlanta, GA, USA.
Email: achong6@gsu.edu

Abstract
We take advantage of repeated cross-sectional household surveys and a sharp discontinuity created by the introduction of an unconditional cash transfer to elders in Bolivia, to evaluate its impact on educational expenditures on children within a household. We find positive and significant impacts of the program at the aggregate level. We also find that the program has stronger effects on indigenous populations as well as on female and rural populations. Our results are robust to a series of falsification tests, survey structure, model specification, and estimation methods.

KEYWORDS
Bolivia, children, education, expenditures, household, unconditional cash transfers

JEL CLASSIFICATION
H55; O15; I12; D12

1 | INTRODUCTION

Despite the fact that conditional cash transfers (CCTs) have been quite successful in achieving development goals, there have been recent discussions on how they compare with unconditional cash transfers (UCTs). It is unclear whether the former are a strong development strategy, as the latter may sometimes provide equal or even superior results depending on the area of application. For instance, UCTs have shown to be effective in reducing child labor, increasing rates of schooling, and improving health and nutrition (Baird et al, 2011). Given that UCTs are administratively easier to implement and tend to be less expensive than CCTs, a clear understanding of the key characteristics associated with the successful implementation of UCTs is rather relevant. Some recent research along these lines is provided by both Burlando (2014) and Aguero et al. (2015), who show that attaching conditions to transfers may be superfluous as long as a clear alignment of incentives occurs.

In this paper, we study the causal impact of UCTs on intra-household allocation in a context in which, given an alignment of incentives, conditions to transfer cash may not be critically needed. The specific UCT that we explore arises from an exogenous policy change implemented by the government of Bolivia, according to which old-age people are to be provided with a permanent unconditional transfer of cash. Similar to many developing countries, old-age people are among the most vulnerable groups in Bolivia, typically due to liquidity constraints that surge from inadequate pension systems and high informality levels. For the purpose of our research, we focus on the case of extended households where children and elders live together in the same premises, which is not uncommon in many developing countries.[1] It is reasonable to expect that elders living with children in the same household will develop stronger bonds and will become more invested in the welfare of the children in terms of human capital investments, such as education and health, which reinforces an alignment of incentives within the members of the household. As in most overlapping generations models, an intergenerational transmission of wealth from older generations to younger generations is expected to occur. This because of the alignment of incentives of the older generation with respect to the expectations of welfare improvement of younger generations. In the context of a standard rational expectations approach this may translate into increased incentives for intra-household reallocation of funds from the older generation to the younger generation in the short run, even when the older generation is under binding budget constraints (Barro and Sala-i-Martin, 2004; Azariadis, 1993). Interestingly, this idea has rarely been tested. Will older generations actually provide to younger ones in the short run, when they are still alive and under binding cash constraints? Will they do so unconditionally with the alignment of incentives?

There is relatively limited empirical evidence on the effects of old-age cash transfers and even less on the impact of old-age cash transfers on intra-household investment allocation. As mentioned earlier, the latter is of particular interest to researchers given the increasing use of this type of transfers in developing countries, the number of beneficiaries living within family households, and the potential dependence on cash transfers in poor households. Some related results are provided by Duflo (2003) and Edmonds (2006), who examine the South African old-age pension program and find evidence on the redistributive consequences of the transfer on food, clothing, housing, and overall better conditions of households with children. In Latin America, there is evidence that transfers have reduced poverty (Joubert and Todd, 2011; Barrientos, 2003). In Chile and Mexico, there is evidence that households that receive cash transfers deviate expenditures toward human capital investments and, in particular, health (Beherman, 2011; Amuedo Dorantes and Juarez, 2015).

The implementation of an old-age UCT program in Bolivia provides an excellent opportunity to study the causal effects of UCTs on intra-household income allocation toward children's human capital investments and, in particular, investment on education, along with health, on which there is little evidence. This program has been at the core of old-age population support strategies in the country and was first implemented at the end of 2000 in the name "Bonosol." However, its impacts, if any, are still grossly understudied. We take advantage of the fact that the probability of receiving this transfer changes discontinuously at the eligibility age and use this sharp discontinuity to parametrically identify conditional average intention-to-treat effects and study intra-household income allocation patterns with emphasis on private education investments.

We measure the impact of this program on child-level educational expenditures in the context of old-age program eligibility after controlling for socioeconomic and demographic characteristics.[2] The eligibility impact, which we allow to vary over households and children, is assumed to be a linear function of the ethnicity and gender of the potential recipient. In addition, given the high diversity in the country and the predominance of indigenous populations, we calculate a series of heterogeneous estimates. Our main finding is that UCTs to elders lead to substantial improvements in children's educational investments. We find that an unconditional transfer increases such investments by around 60 %. As far as ethnicity is concerned, the program leads to larger increases in schooling investments among indigenous households compared to their nonindigenous counterparts. We find similar results in the case of rural areas as well as in the case of women.[3]

Our paper is organized as follows: Section 2 shows evidence that private spending on education may be highly correlated with school-related outcomes. Section 3 briefly describes the old-age UCT program and its basic characteristics. Section 4 presents the data including basic statistics. Section 5 describes the methodological approach. Section 6 presents our results. Section 7 presents robustness tests, and Section 8 concludes.

2 | PRIVATE SPENDING ON EDUCATION AND SCHOOL OUTCOMES

As stated by the OECD, private spending in education refers to expenditures funded by private sources, namely, households and other private entities, and includes all direct expenditures on educational institutions and net of public subsidies, excluding expenditures outside educational institutions such as textbooks purchased by families, private tutoring for students, living costs of students, and out-of-pocket costs for transportation, uniforms, fees, and others. In short, this definition includes any private expenditures delivering or supporting educational services. Interestingly, private spending in schooling is rather large in Latin America, and in several countries of the region it ranks among the highest in the world. For example, the private spending in primary education reached 0.5% of GDP (2016) in Colombia, 0.31% of GDP (2014) in Chile, 0.30% of GDP (2014) in Argentina, and 0.27% of GDP (2014) in Mexico. In contrast, private spending in schooling in industrial countries ranges from 0.0001% of GDP in Norway (2016) to 0.04% of GDP in Belgium (2015) to 0.11% of GDP in the United States (2015).[4]

Among the Latin American countries, private spending in education is less dramatic in Bolivia. There are some indications that its role is rather important in terms of not only the out-of-pocket costs described earlier but also the so-called low-fee private schools, which are for-profit schools that are typically owned and managed by an individual or a group of individuals who provide basic education to children in low-income households for a relatively low fee that low-income families can afford (Phillipson, 2008; Inter-American Development Bank, 2017; World Bank, 2016). Although less prevalent than in other Latin American countries such as Peru, Argentina, or the Dominican Republic, low-fee private schools in Bolivia have the same basic characteristics as those in these countries. In particular, the low-fee private schools in Bolivia are mostly limited to primary and, in some cases, secondary education and are driven by small entrepreneurs. These schools have a relatively few academic staff and very precarious infrastructure. Many students come from low-income families in both rural and urban areas and seek quality alternatives to the public system (Verger, et al., 2016, 2017).

While not yet extensive, some evidence shows that private spending in education does have a bearing on schooling outcomes. For instance, Bourguignon et al. (2003) and Schultz (2004) find that CCT programs in Brazil and Mexico have a positive effect on school enrollment. Somewhat closer to our research, Carvalho (2012) explores an exogenous old-age social security reform in rural areas in Brazil and studies the effect of benefits on schooling for children living with or without elders and finds positive impacts on school enrolment.[5] For Bolivia, Martinez (2004) explored the possibility of positive spillovers from old-age UCT participation onto the child's education and finds preliminary positive effects on enrollment status, reporting an increase of 7 percentage points on the probability of enrollment. He argues that the effect tends to be driven by rural households with an increase in the probability of enrollment for older children of 14 percentage points.[6]

3 | BRIEF DESCRIPTION OF UCT PROGRAM FOR ELDERS

Bolivia is the poorest country in South America, which is evident from the fact that approximately two-thirds of the households are below the poverty line (World Bank, 2016). Traditionally, the elderly and children have been the most vulnerable and unprotected groups of the population. While 35% of adults live in poverty, nearly 55% of children and 60% of the elderly live so (World Bank, 2016). In fact, coverage in Bolivia is one of the lowest in the region as around 80% of the population does not have access to any type of pension system (Landa and Yanez-Pagans, 2007). Worse yet, the country has a rather complex multiethnic dimension reflected by the fact that it has the highest percentage of indigenous population in Latin America. Almost 50% of Bolivians self-report as belonging to an indigenous group.[7]

As part of the structural reforms of the 1990s, broadly supported by the World Bank and the International Monetary Fund, the government created an old-age UCT program that entitled all Bolivians aged 65 years and above to receive a flat, noncontributory, and UCT independently of their income levels. In short, the only eligibility rule was age.[8] The government determined that this pension would be financed with the dividends of the shares of privatized companies as part of the structural reform effort. After Bonosol (Bono Solidario) was first introduced in 1997 liquidity problems were so serious that in 1998 the program was put on hold. When it resumed, at the end of 2000, it was renamed Bolivida and the pension amount reduced from annuities of US$248 to US$60.

Figure 1 presents estimates by age of beneficiaries. Though take-up is high at the margin of eligibility, compliance is not perfect. Whereas 80% of the individuals of age over 65 years receive the benefit, the share is slightly lower as one gets closer to the cutoff age, which is mainly driven by the

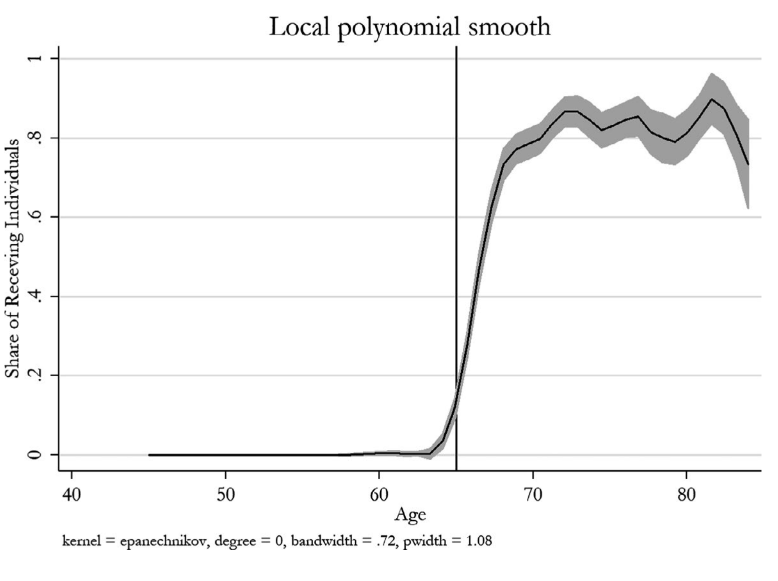

FIGURE 1 Share of people who received UCT by age

indigenous male population in rural areas. Low take-up rates are unsurprising because significant drawbacks in personal identification systems exist in rural areas. This is compounded by the availability of a very few financial centers. As a result, this situation tends to disproportionately affect the indigenous population.[9] However, it is reasonable to assume that, on average, age is truthfully reported and that it is identified with little error. To receive the benefit, interested elders must produce the Bolivian National Identification Card first. In this context, it is reassuring to observe that very few elders who are ineligible to benefit from this program are actually cashing in old-age UCT program.

4 | DATA

Our data come from a nationally representative household survey called the Living Standards Measurement Surveys (LSMS), which was first implemented by the World Bank and is currently managed by the Bolivian National Institute of Statistics.[10] The survey that we employ is for the period 1999–2002 and includes a comprehensive socioeconomic module including Bolivida receipt information at the individual level, as well as detailed data on expenditures for all members in the household who are at least 6 years old.[11] More importantly, the sample comprises all school-age children living in households with at least one person in the age range between 56 and 73 years. The school-age range we consider is 6–18 years. It should be mentioned that the minimum legal working age in Bolivia is 14 years. In addition, we exclude observations with outliers in expenditures, in particular, households whose total reported income is missing and those who belong to the top 1% of the income and educational expenditure distribution. Our preferred sample comprises 3,645 school-aged children and 1,038 eligible elders living in 1,915 households.[12]

Table 1 reports summary statistics. On average, households that report living with an eligible elder are different from those living with a soon-to-be eligible elder. Eligible households are slightly smaller and have more children than noneligible ones even though both have nearly the same number of school-age children, especially in later years. Noneligible and eligible households have different family characteristics. In particular, noneligible households show higher percentages of the presence of father and mother in the household as well as of parents' age. In addition, years of schooling of the parents and educational expenditures of children are higher for eligible households. Finally, both eligible and noneligible families allocate approximately 10% of their total income on children's schooling expenditures.

We also observe that the largest disparities appear in human capital between indigenous groups as well as between urban and rural people, where educational gender gaps among indigenous people are particularly large. It should be mentioned that the identification of indigenous people in Bolivia is not straightforward. Social class and ethnic elements are interrelated and difficult to disentangle, and, in general, the information used to define who the indigenous people are is based on a set of ethnolinguistic characteristics.[13] We use three specific questions from our survey data that are aimed at identifying ethnic groups: (1) Do you consider yourself as belonging to an indigenous group? (2) What languages do you speak? (3) In what language did you first learn to speak? The first two questions are collected exclusively for household members who are at least 12 years old, and the third one is collected for all members in the household. We follow Molina and Albó (2006) who employ the three criteria mentioned earlier to construct an ethnolinguistic matrix for Bolivia. We classify as "nonindigenous" those who do not self-identify as indigenous, whose first language is not indigenous, and who did not learn an indigenous language as children. On the contrary, we define as "indigenous" those who identify themselves as indigenous, whose first language is indigenous, and who learned an indigenous language as children. To simplify our empirical approach, all the other combinations are simply labeled as multiethnic.

TABLE 1 Summary statistics

	Noneligible (age ≤ 64)		Eligible (age ≥ 65)		
	1999–2000	2001–2002	1999–2000	2001–2002	Total
Panel A: Demographics					
Child age	12.5	12.6	12.1	11.8	12.4
Share men (%)	50.3	51.8	54.9	48.2	51.0
Number of school-age children in HH	2.6	2.6	2.3	2.8	2.6
Number of children under 5 in HH	0.5	0.6	0.6	0.6	0.6
Household size	6.0	6.1	5.8	6.3	6.1
Number of adults aged 19–64	2.8	2.9	1.9	1.9	2.6
Father present	0.7	0.7	0.6	0.5	0.7
Mother present	0.8	0.8	0.7	0.7	0.8
Panel B: Parents					
Father's years of education	5.2	4.8	6.0	5.3	5.1
Mother's years of education	3.9	4.0	5.4	5.1	4.3
Father's age	54.5	54.3	51.9	51.4	53.6
Mother's age	49.2	49.3	47.6	47.1	48.7
Panel C: Income, expenditure (in bolivares per month)					
Income per capita w/o Bono	478.1	505.7	432.5	501.6	489.1
Expenditure per capita	445.1	396.6	495.8	451.9	431.2
% Children with positive expenditure in education	78.5	77.1	82.9	83.4	79.3
Expenditure on education per child	51.6	34.5	62.1	46.2	44.5
Observations	1,067	1,540	386	652	3,645

To identify the effects of the old-age UCT on expenditures in education, we build a variable that captures expenditure on education for each child of school age as our key unit of analysis. Expenditures on education include tuition, uniforms, materials, fees, transportation, and school-related expenses for each student.[14] This measure captures heterogeneity across different age cohorts, as expenditures may differ across educational levels since intrahousehold human capital investment decisions may vary considerably at different stages of life. Specifically, to evaluate the impact of cash transfers to old-age individuals on education investment, our unit of analysis is children between 6 and 18 years of age. We analyze the impact on the decision to invest if the children have matriculated, including the amount spent.[15]

For the analysis of the treatment effect in the framework of our regression discontinuity approach, we differentiate between treatment intensity (total treatment) and marginal treatment status. Treatment intensity is defined by the number of adults older than 65 years of age who are currently living in the household. For example, a household with two senior people with one 64 years old and the other 80 years old has a treatment intensity of 1, while a household with two senior persons with one 65 years old and other 80 years old has a treatment intensity of 2. For the regression discontinuity approach, however, we focus on the marginal treatment status, which is based on the age of the household member closer to the cutoff age of 65 years. If the household member closer to the cutoff age is younger than 65 years, the household is considered marginally untreated. If he or she is 65 years old or older, the household is considered marginally treated. As an example, a household with two senior members with one who is 64 years old and the other who is 80 years old is considered marginally untreated, while a household with two senior individuals with one who is 65 years old and the other who is 80 years old is considered marginally treated. In this context, our results may be interpreted as the impact of having one additional recipient member, independent of how many other recipients the household has. This approach more clearly singles out the impact of receiving the UCT in the allocation of expenditures within the household, as it compares the households that are observationally identical, except for the presence of the additional eligible adult.

In Figure 2, we present histograms of age of reference in the household and show little divergence in the distribution of households with some just above and some just below the age threshold. We do observe that there is a large share of households that have an elder of exactly 65 years of age living in the household. Since our key variable is discrete, we cannot use McCrary (2008) but we can use a modification of this test, which is valid for discrete values (Frandsen, 2017) and was first implemented by Cattaneo et al. (2018). Under the null hypothesis that there is no manipulation of the running variable, the test provides *t*-statistics of 0.915 and a corresponding *p*-value of 0.36. Thus, we can say that we cannot reject the hypothesis that there is no such manipulation of the results. Figure 2a shows a histogram of individuals of age 50 years and above and compares the situation before and after the implementation of Bolivida, which as mentioned earlier occurred at the end of 2000. As observed in this figure, there is no evidence of an increase in the population distribution at the age of threshold of eligibility. Figure 2b shows no change in the share of households that are eligible around the cutoff age. In addition, the conditional probability of being eligible increases at such value, showing a significant discontinuity for actual beneficiaries. As it is reasonable to believe that age cannot be manipulated especially as potential beneficiaries should provide legal documents to government authorities in order for them to be allowed to participate, households are unlikely to strategically locate themselves around the eligibility age. As a result, we can confidently assert that the main assumption behind our regression discontinuity design holds, which we use to identify the average UCT intention-to-treat effects on children's schooling expenditures (Imbens and Lemiux, 2008).[16] It should be mentioned that we do not estimate the average treatment effect on the treated, as the use of the beneficiary variable in this setup is problematic. This is because pension receipts may be endogenous as there are slight differences between actual and estimated beneficiaries. As mentioned earlier, these may arise because of deficiencies

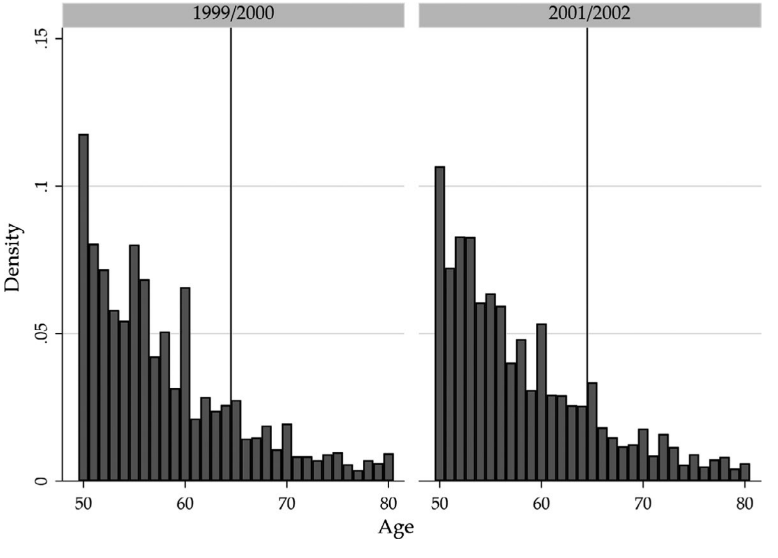

FIGURE 2 Histogram in age person closest to eligibility

in the government's identification documentation system, or because of lack of available funds to cover the fully targeted population, or even yet, because of difficulty reaching financial centers, either because of lack of transportation or because of excessive distance to them. All these factors may constrain eligible members to become recipients. As the exact constraining mechanisms are unclear, we decided that our empirical exercises should focus mainly on potential or intention-to-treat beneficiaries.[17]

Figure 3 shows the conditional expectation of children's schooling expenditures as a function of age of the household member closest to the cutoff age. Smoothing is performed separately for the period before and after 2001. Figure 3 makes clear that before 2001 there is no difference in the average educational expenditure per children. Afterward there is an important and statistically significant increase, which signals that regression discontinuity methods are appropriate. Next, we parameterize these relationships to quantify the effect of Bolivia on children's educational expenditure.

5 | EMPIRICAL STRATEGY

Our aim is to analyze the causal effect of the potential UCT received by elders on children's educational expenditures who live in the same household, taking advantage of the exogenous implementation of the Bolivida program in 2001 and related to the income shock families with an elder member may have experienced. Based on the characteristics of the program, a possible strategy may be to adopt a differences-in-differences approach, comparing educational expenditures per children, as a function of the age eligibility of the reference household member and variable indicating if Bolivida was already implemented. However, a simple comparison of educational expenditures by households below and above the cutoff age across time is likely to provide a biased estimate. As the cutoff age coincides with the retirement age, the household structure and distribution of potential retirees may not be random, and the presence of elder household members may have an impact on the allocation of time and resources among household members. As a consequence, it may be possible that potential beneficiary households may differ systematically from nonbeneficiary households. In this case, a simple differences-in-differences model may not be valid to account for potential systematic differences. To overcome unobserved differences between potentially beneficiary and nonbeneficiary households,

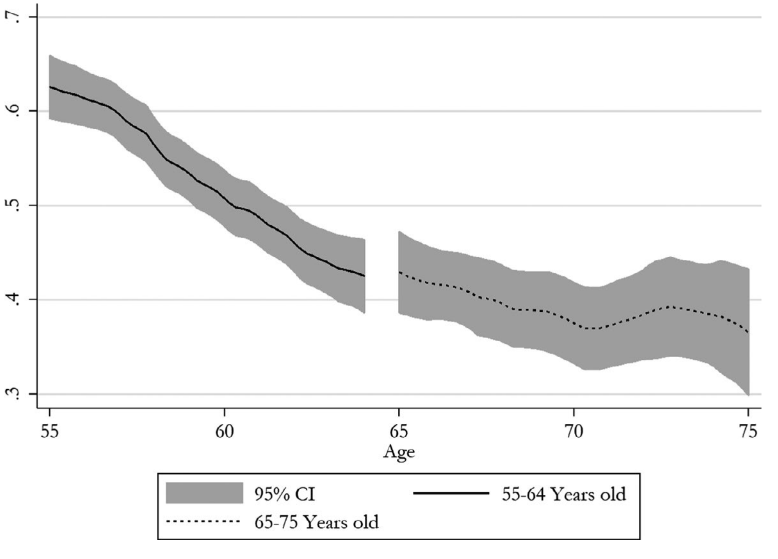

FIGURE 3 Share of households eligible around the threshold age

we take advantage of the fact that the probability that an individual receiving the cash transfer changes discontinuously at the age of eligibility. As such, we can apply a straightforward regression discontinuity design (Imbens and Lemiux, 2008). Furthermore, since we also have information across time, namely, individual information before and after the implementation of the program, an additional approach that we may apply is a combination of a differences-in-differences approach along with a sharp regression discontinuity design. This specification can be written as follows:

$$\begin{aligned}\log(\text{Educ exp}+1) = {} & a_0 + a_1 * I_{\text{age65}} + a_2 * I_{\text{yr}\geq 01} + a_3 * I_{\text{age65}} * I_{\text{yr}\geq 01} + b_\gamma * f_\gamma\left(\text{age}_{\text{ref}} - 64.5\right) \\ & + b_l * I_{\text{age65}} * f_l\left(\text{age}_{\text{ref}} - 64.5\right) + c_\gamma * I_{\text{yr}\geq 01} * g_\gamma\left(\text{age}_{\text{ref}} - 64.5\right) \\ & + c_l * I_{\text{yr}\geq 01} * I_{\text{age65}} * g_l\left(\text{age}_{\text{ref}} - 64.5\right) + \varepsilon,\end{aligned}$$

where the log of per child educational expenditure is estimated as a function of the age eligibility (I_{age65}) of the reference person in the household, an indicator of the year when the transfer was implemented ($I_{\text{yr}\geq 01}$) and an interaction of both age eligibility and year of implementation. In addition, we allow for the inclusion of flexible functional forms of the gap between the reference person age and the cutoff age ($\text{age}_{\text{ref}} - 64.5$), which are allowed to vary before and after the year of implementation (f_γ,f_l,g_γ,g_l), and for households with a reference, individual's age above or below the cutoff age. In this specification, a_3 is the parameter of interest, which identifies the impact of the UCT for school-age children living in households where the reference person is around the cutoff age.[18]

As the treatment state of a household is based on the age of the person closest to the cutoff age or reference individual, it may still be possible that other elder individuals may be living in the household. Given the fact that their presence is also likely to influence household expenditures on children possibly due to income shocks, our baseline specification includes a control for the presence of any household member older than 65 other than the reference person. In the context of the regression discontinuity approach, two additional factors should be considered: (1) the functional form for the age gap and (2) the bandwidth selection. With respect to the choice of the functional form, we consider the fact that the forcing variable, the age of the reference individual, is discrete. As a consequence, the use of nonparametric regressions is not viable. Instead, we estimate our empirical specifications using different polynomial functions, including logistic, linear, quadratic, and cubic functional forms. Due to the restricted variation in the forcing variable, we do not estimate higher-order polynomials, which helps us avoid overspecification. The selection of the order polynomial is based on the Akaike Information Criteria (AIC) as suggested in Lee and Lemieux (2010).

The second key issue is the choice of an appropriate bandwidth selection, as it determines the final sample used in the analysis. While larger bandwidths allow for the use of more data and thus potentially help us obtain more precise estimates, they can also reduce the accuracy of the estimates if the model is misspecified, which may introduce bias on the estimated treatment effects. To deal with this issue, we apply a cross-validation strategy described in Lee and Lemieux (2010) to select the appropriate bandwidth. Based on the cross-validation strategy and the AIC applied to the baseline model, we choose a linear model with a bandwidth of 9 years above and below the cutoff age.

Notice that consistent with a standard regression discontinuity approach we do find visual evidence on the impact of the UCT policy to elders on educational expenditures per child, as shown in Figure 4. While, as expected, Figure 4a shows that there is no discontinuity between 1999 and 2000, Figure 4b does show a significant increase in educational expenditures per child in households with an elder between 2001 and 2002. However, this evidence may not be enough to confirm our hypothesis, as until recently the official age at which people could retire was 65 years.[19] As the cash transfer was designed to transfer resources to individuals older than 65 years of age using data for 2001 and 2002 only, the latter may introduce bias as any

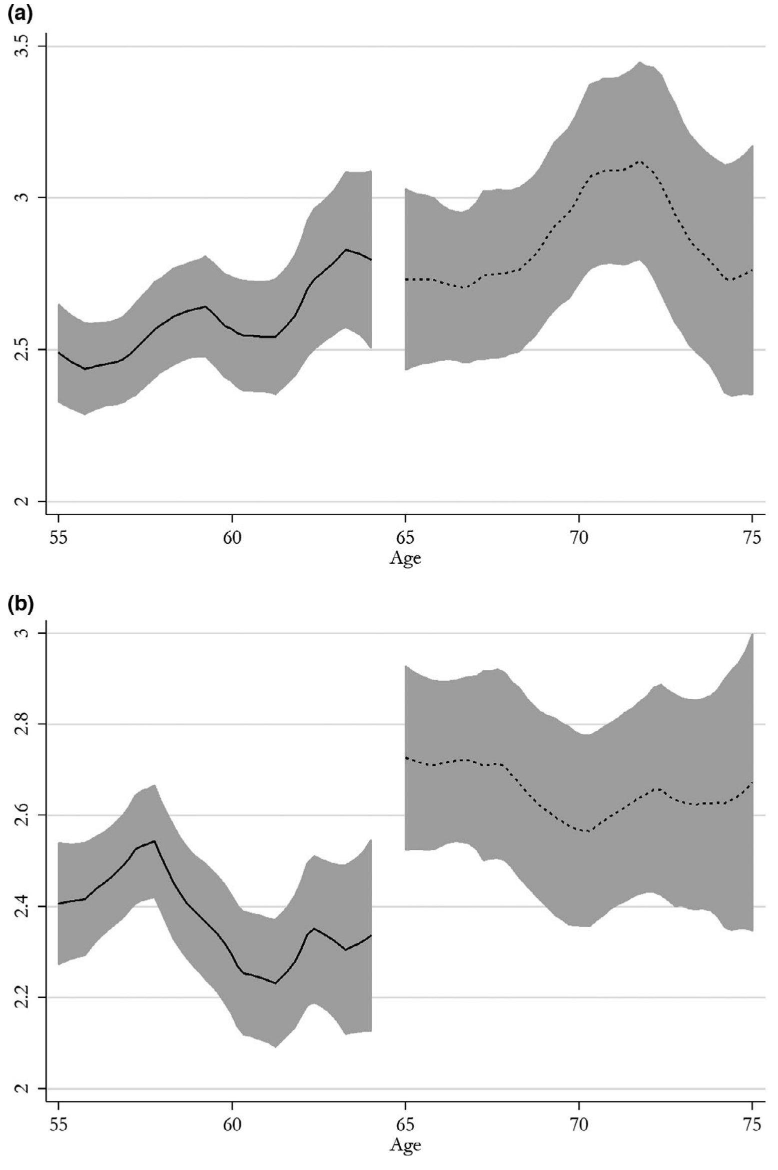

FIGURE 4 Average log expenditure on education per children: (a) 1999–2000 and (b) 2001–2002

statistic may be confounding the effect of retirement and the UCT under study. To control for this issue, we use a methodology that combines the regression discontinuity design and differences-in-differences methodology. As shown in Figure 4a, the fact that there is no discontinuity increase reassures us on the use of data for 1999 and 2000 to control for the retirement age effect based on the differences-in-differences strategy, while also considering the features of the regression discontinuity design to identify the impact of the old-age UCT.

6 | RESULTS

We depart from testing competing models to obtain an initial estimate as well as to establish the best specification for our estimation. As shown in Figure 5, and following the existing literature (Lee and

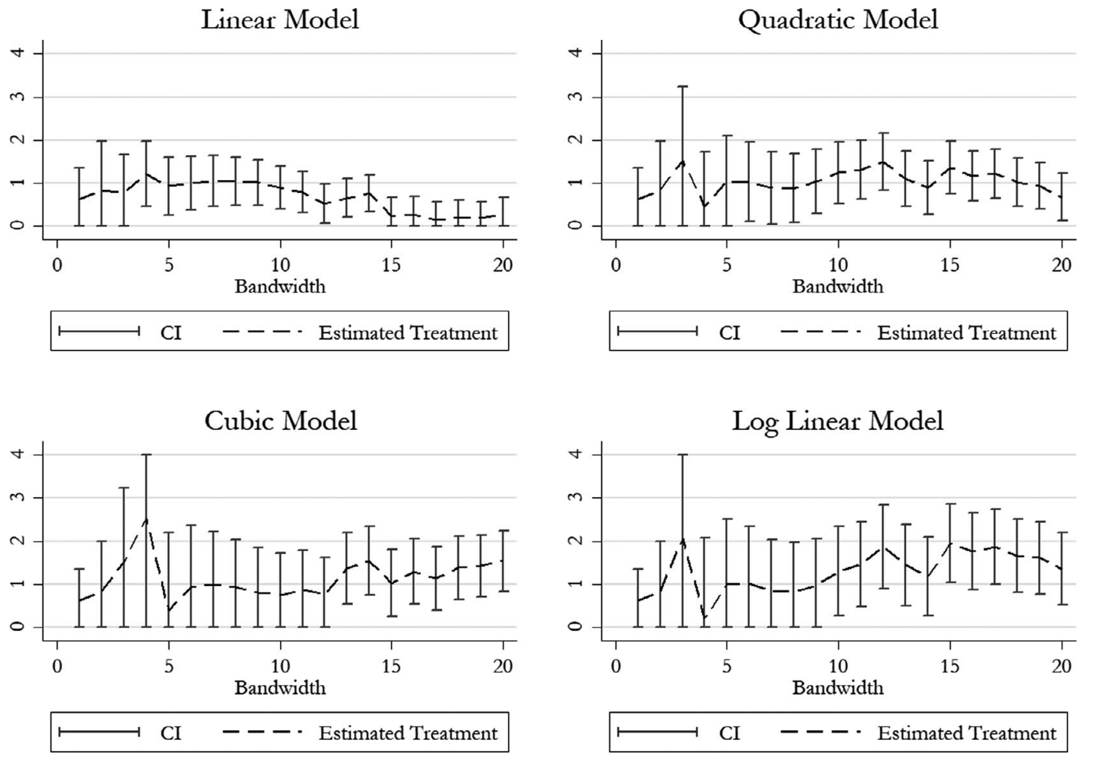

FIGURE 5 Base model polynomial estimations

Lemieux, 2010), we test quadratic, cubic, log linear, and linear specifications. For most specifications, the average effect lies below one. Following Lee and Lemieux (2010) as well as the Akaike Criteria, these findings indicate that the preferred empirical specification should be linear. Table 2 reports the basic results of our preferred specification without any controls.[20] Column 1 shows that, on average, the implementation of the cash transfer increased expenditures in education by about 73% in Bolivia. In addition, our results show that the probability of moving from zero expenditure in education to a positive size is about 12%, and for those households that have some level of expenditure the effect of the cash transfer reaches about 43% and it is still statistically significant at conventional levels. These results show that the UCT to elders has an effect on intrahousehold behavior pushing for larger expenditures in education. Moreover, these effects seem to be larger in households that are already

TABLE 2 Impact of UCTs on educational expenditures

	(1) Log expenditure	(2) Pr(expenditure > 0)	(3) Log expenditure if expenditure > 0
Treatment	0.732**	0.123*	0.426*
	(0.255)	(0.059)	(0.203)
N	3,645	3,645	2,889
R^2	0.025	0.014	0.031

Note: Standard errors in parentheses.

$^*p < 0.05$, $^{**}p < 0.01$.

spending on children education. However, these results may be biased since we are not controlling for other factors that may affect educational expenditures.

Table 3 presents findings when a broad set of controls are included, such as age, gender, number of children in the household, number of children aged 5 or less, number of adults, presence of a parent in the household, parents' education, whether the household is in an urban area, and whether the household is of indigenous background. For the most part we find analogous results and, if anything, we find stronger impacts. Yet, it is interesting to note that when controlling for indigenous background and the presence in an urban setting, the impact observed is reduced by roughly 10 percentage points with respect to the specification without such controls. These somehow suggest that additional heterogeneity analysis may provide further insights into our results. For instance, full sample estimates may hide some heterogeneity due to cultural, geographical factors, and, in particular, ethnicity, a particularly relevant element in a country such as Bolivia, as described earlier.

Table 4 presents heterogeneous impacts of the UCT of elders on children's educational expenditure by ethnicity, and also by region, gender, and age cohorts. The estimated parameters are reported separately for each group. On the one hand, after the implementation of the cash transfer, we find statistically significant and larger impacts for indigenous populations. The implementation of the cash transfers for indigenous beneficiaries increased expenditures in education by about 73%. On the other hand, the impact found for nonindigenous population not only is statistically insignificant but also reflects an effect of nearly half of the baseline results in Table 2. The multiethnic group, which includes mixed race population, also presents statistically insignificant results that lie in size somewhere in-between indigenous and nonindigenous. These results may be related with the fact that indigenous populations tend to face credit constraints and any liquidity shock modifies significantly the allocation of expenditures within households. A similar scenario is depicted when comparing rural and urban groups. In the former, we find, again, an impact larger than the baseline results and significant at the conventional levels. This heterogeneous effect reflects that implementation of the cash transfers for rural beneficiary households raised expenditures in education by about 75%. Nevertheless, urban populations show nonsignificant effects that are also smaller in size in comparison with the baseline results.

In addition to ethnicity and area of residence, the age of the children may turn out to be a relevant characteristic in relation to the outcome studied. Regarding age, we compute the heterogeneous effect for children in elementary school (aged 6–12 years) and those whose ages correspond to high school (13–18 years). The coefficients reported in column 3 of Table 4 show that the UCT to elders increases the level of expenditures for children in elementary school by 61%. In contrast to this finding, the estimated impact for children in high school is about 46%, but not statistically significant at any level. In addition, we estimate the average effects of eligibility for girls and boys separately.[21] The coefficients are reported in column 4 of Table 4. We find that conditional on eligibility, human capital expenditures on girls almost doubles and is strongly statistically significant. This is reflected in an increase in the level of educational expenditures of 97%. On the contrary, in the case of boys, this result is not statistically significant at any standard levels. This finding is reassuring as women have been historically neglected in terms of schooling in Bolivia.

We further explore the role played by gender in the intrahousehold allocation from elders to children living in the same household. In particular, we explore the affinity between gender of elders and children, as well as the affinity between gender of elders and age of children. Interestingly, we find evidence that educational expenditures on girls are driven by male elders, but not by female elders, as shown in Panel A of Table 5. This is the only statistically significant link between gender of elders and children living in the same household that we are able to detect as the rest of impacts are not statistically significant at conventional levels, and even in the particular case of male elder and children, we find a negative coefficient. This is rather remarkable, as it appears to show that male elders,

TABLE 3 Impact of UCTs on educational expenditures with controls

	1	2	3	4	5	6	7	8	9	10	11
	0.732**	**0.667*****	**0.607*****	**0.573*****	**0.601*****	**0.629*****	**0.612*****	**0.633*****	**0.637*****	**0.636*****	**0.586*****
Treatment	**−0.255**	**−0.224**	**−0.221**	**−0.22**	**−0.219**	**−0.219**	**−0.219**	**−0.219**	**−0.219**	**−0.218**	**−0.213**
Urban x dept		x	x	x	x	x	x	x	x	x	x
Household indigenous status			x	x	x	x	x	x	x	x	x
Age				x	x	x	x	x	x	x	x
Gender					x	x	x	x	x	x	x
Number of children in household						x	x	x	x	x	x
Number of children under age 5							x	x	x	x	x
Number of adults								x	x	x	x
Father/mother present									x	x	x
Father's/mother's age > 65										x	x
Father's/mother's education											x
N	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645
R^2	0.025	0.216	0.232	0.261	0.263	0.271	0.274	0.275	0.279	0.281	0.329

Note: Standard errors in parentheses.

$^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 4 Heterogeneous impacts

	(1) Indigenous status	(2) Urban status	(3) Age	(4) Gender
Nonindigenous	0.232			
	(0.350)			
Observations	1,480			
R^2	0.355			
Indigenous	0.727**			
	(0.370)			
Observations	1,157			
R^2	0.261			
Mix	0.553			
	(0.450)			
Observations	1,008			
R^2	0.300			
Rural		0.746**		
		(0.309)		
Observations		1,809		
R^2		0.204		
Urban		0.434		
		(0.289)		
Observations		1,836		
R^2		0.238		
6–12			0.612**	
			(0.266)	
Observations			1,799	
R^2			0.331	
13–18			0.462	
			(0.337)	
Observations			1,846	
R^2			0.352	
Girls				0.966***
				(0.309)
Observations				1,785
R^2				0.354
Boys				0.225
				(0.295)
Observations				1,860
R^2				0.321

Note: Standard errors in parentheses.

**$p < 0.05$, ***$p < 0.01$.

TABLE 5 Gender of elders in the household and children

A. Gender of child and gender of elder

	(1) Girl/female elder	(2) Girl/male elder	(3) Boy/female elder	(4) Boy/male elder
Treatment	0.566	1.484***	0.300	–0.005
	(0.469)	(0.429)	(0.489)	(0.388)
Observations	756	1,029	749	1,111
R^2	0.388	0.368	0.330	0.346

B. Age of child and gender of elder

	(1) Ages 6–12/female elder	(2) Ages 6–12/male elder	(3) Ages 13–18/female elder	(4) Ages 13–18/male elder
Treatment	0.800**	0.486	–0.116	0.734*
	(0.389)	(0.385)	(0.580)	(0.422)
Observations	758	1,041	747	1,099
R^2	0.364	0.345	0.369	0.373

Note: Standard errors in parentheses.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

having benefitted from a system that tilts toward males, realize the difficulties that female elders experience in Bolivia and at old age may be attempting to compensate gender-related inequities with the younger generations, which is fully consistent with our previous findings. In addition, we find that educational expenditures of girls aged 13–18 years may be driven by male elders while educational expenditures of boys aged 6–12 years appear to be driven by female elders. This is shown in Panel B of Table 5.

7 | ROBUSTNESS

We observe that there is a small share of the population who may be receiving the UCT despite being younger than the corresponding cutoff age of 65 years and that the share of people who are not receiving the benefit may be larger as we approximate to the cutoff age.[22] This may raise questions about the strength of using 65 years of age as the cutoff point, as well as on the bandwidth pertinence. To test this, we perform a series of falsification tests, which are presented in Table 6. We test alternative cutoff ages ranging from 63 to 67 years and control for analogous covariates as in our main results. We do not find any statistically significant results at conventional levels.

Similarly, Table 7 presents the results of using different bandwidth and alternative empirical specifications, which control for all the covariates as in Table 6. A major concern in regression discontinuity design is the high sensitivity of the estimates to the choice of bandwidth; thus, it is worth noting that whereas moving the bandwidth affects slightly the results, they remain stable and statistically significant for our preferred model, the linear specification. The quadratic and cubic models provide similar estimated treatment effects (smaller for quadratic larger for cubic), but for the quadratic

TABLE 6 Falsification tests

	(1) Cutoff age = 63	(2) Cutoff age = 64	(3) Cutoff age = 66	(4) Cutoff age = 67
Treatment	−0.042	0.112	0.295	0.213
	(0.191)	(0.199)	(0.232)	(0.237)
Observations	4,436	4,098	3,225	2,988
R^2	0.336	0.338	0.330	0.335

Note: Standard errors in parentheses.

specification, it is only significant using $h = 10$, while it is non-statistically significant in the cubic case.

As an additional robustness test, Table 8 reports findings that are not estimated using a regression discontinuity design along with a differences-in-differences approach, but rather a combination of a regression discontinuity design and a before–after design (Hoddinott and Skoufias, 2004; Borooah and Iyer, 2005). That is, these results are estimated separately for before and after the Bolivida UCT and conditional means are compared to assess the impact of the program. This specification is more flexible than the pooled one used previously, but it reduces the degrees of freedom as it needs more parameters to be estimated. The corresponding coefficients are somewhat smaller, but the results are analogous from the perspective of statistical significance.

In addition, Appendix 2 presents cross-validation tests of the regression discontinuity design including bandwidth selection. Finally, Appendix 3 shows the results of using weights provided by the National Statistics Institute. If anything the results tend to be more robust and slightly larger but not significantly different from our base results.[23]

TABLE 7 Sensitivity to bandwidth and specifications

	$h = 7$	$h = 8$	$h = 9$	$h = 10$	$h = 11$
Linear	0.447*	0.507**	0.586***	0.489**	0.451**
	(0.241)	(0.229)	(0.213)	(0.200)	(0.190)
N	2,696	3,100	3,645	4,223	4,728
R^2	0.345	0.334	0.329	0.336	0.330
Quadratic	0.513	0.434	0.426	0.679**	0.702**
	(0.347)	(0.326)	(0.306)	(0.290)	(0.278)
N	2,696	3,100	3,645	4,223	4,728
R^2	0.346	0.334	0.329	0.336	0.331
Cubic	0.585	0.656	0.654	0.397	0.516
	(0.513)	(0.462)	(0.421)	(0.392)	(0.369)
N	2,696	3,100	3,645	4,223	4,728
R^2	0.347	0.335	0.329	0.336	0.331

Note: Standard errors in parentheses.

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 8 Regression discontinuity before and after approach

	1	2	3	4	5	6	7	8	9	10
Treatment	**0.618*** (0.222)**	**0.550** (0.219)**	**0.520** (0.218)**	**0.542** (0.217)**	**0.571*** (0.217)**	**0.541** (0.217)**	**0.578*** (0.222)**	**0.586*** (0.221)**	**0.528** (0.238)**	**0.446* (0.228)**
Urban x dept	x	x	x	x	x	x	x	x	x	x
Household indigenous status		x	x	x	x	x	x	x	x	x
Age			x	x	x	x	x	x	x	x
Gender				x	x	x	x	x	x	x
Number of children in household					x	x	x	x	x	x
Number of children under age 5						x	x	x	x	x
Number of adults							x	x	x	x
Father/mother present								x	x	x
Father's/mother's age > 65									x	x
Father's/mother's education										x
Observations	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645
R^2	0.242	0.254	0.283	0.286	0.291	0.293	0.296	0.301	0.302	0.356

Note: Standard errors in parentheses.

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

8 | SUMMARY AND CONCLUSIONS

In this research, we take advantage of repeated cross-sectional household surveys and a sharp discontinuity created by the introduction of a UCT to elders to evaluate intention-to-treat impact on the educational expenditure on children within a household using a regression discontinuity design and a differences-in-differences approach.

Whereas it may be claimed that our main finding is not particularly surprising, namely, that UCTs to elders lead to substantial improvements in children's human capital investments, some of our additional results raise relevant questions that, with further research, may help better guide policy. For instance, we show that the UCT program for elders in Bolivia results in stronger impacts in indigenous, female, and rural populations, groups that tend to be relatively poorer in the society. Interestingly, this raises the question whether targeted unconditional transfers to these segments of the population will help optimize positive externalities societywide. Similarly, we show that specific affinity between the gender of elders and both the gender and age of the children living in the same household may matter, which may also have important implications policywise. Admittedly, while intriguing, these findings may be further confirmed with additional research. For instance, future work may focus on the actual recipients of the UCTs, which may allow us to estimate average treatment effects and may also focus on gender differences between elders and children within a household to better understand the dynamics of the fund transmission mechanism. In the near future, we hope to pursue further research along the lines described earlier.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available upon request.

ORCID

Gustavo Canavire-Bacarreza https://orcid.org/0000-0002-7801-1688
Alberto Chong https://orcid.org/0000-0002-5139-5386

ENDNOTES

[1] In many developing countries, the relationship between elders and children within a household is commonly blood based. While this is also true in Bolivia, in this paper, we take a more general approach by focusing on elders and children living in the same household, but without limiting the analysis to blood-based relationships. By doing this, we can include elders having emotional attachments not driven only by blood relationship. In fact, this arrangement is not uncommon in developing countries (e.g., Ruggles and Heggeness, 2008).

[2] It may be argued that educational expenditures are not an ultimate outcome variable. However, we believe that it is an important variable to study, as it is impacted fairly immediately after receiving the transfer, which makes it relevant from a public policy perspective. We are grateful to an anonymous referee and to the editor for raising this issue.

[3] Only a very few studies have examined the effects of unconditional cash transfers. They have focused on impacts on poverty (Escobar, et al., 2013); household expenditures (Martinez, 2004); cohabitation (Valencia, 2011); health, labor, and unintended impacts (Hernani-Limarino and Mena, 2015). Hernani-Limarino and Mena (2015) provide some basic evidence on education, but exploit a different exogenous shock for different years. In particular, they take advantage of a reduction in age of eligibility from 65 to 60 years in 2007.

[4] https://data.oecd.org/eduresource/private-spending-on-education.htm.

[5] On related work, Carvalho Filho and Litschig (2016) provide evidence on the long-run impacts of a temporary increase in federal transfers to local governments in Brazil. They find that children born after the extra funding had disappeared gained about 0.06–0.10 standard deviation across the entire score distribution of two nationwide exams at the end of the 2000s.

[6] As Martinez (2004) explains, his evidence is merely suggestive and exploratory. It should be mentioned that he employs different data periods and exogenous interventions that cannot be convincingly applied in the context of our intervention and period sample given the lack of adequate data.

[7] Furthermore, the ethnic dimension is closely correlated to poverty as 49% of the indigenous people live below the poverty line, but only 24% of the nonindigenous live so (http://www.ine.gob.bo).

[8] As in many Latin American countries, Bolivia implemented a broad array of structural reforms in the 1990s, which included the sell-off of state-owned companies to private investors and the change of the pension system from a public pay-as-you-go system to a privately managed one.

[9] No data allow assessing the extent to which different constraining mechanisms might be deterring elders to cash out of Bolivida.

[10] INE website: http://www.ine.gob.bo. The surveys employ a stratified two-stage sampling whose sampling frames are based on census data. The sampling frame for the baseline survey was constructed on the basis of the 1992 Census enumeration areas list. The follow-up survey uses an updated sampling frame that was constructed upon revised cartographic information compiled for the 2001 Census. While this may raise questions on the basis of the estimates, we perform robustness tests that show no statistical effect of this sampling framework. These results are available upon request.

[11] Approximately 18% of households are three-generations extended households.

[12] Appendix 1 compares the original full size of the sample available with our preferred sample. While the reduction in the number of observations is large, it is our preferred sample because we make sure that children and beneficiaries both live in the same household, which, in our view, is of critical importance given that it optimizes transmission incentives from beneficiary to children. Our preferred sample is based on the bandwidth used for the estimation of the regression discontinuity approach. Robustness tests were conducted using different sample sizes, and similar results were obtained.

[13] Ethnolinguistics refer to the study of language within ethnic groups or, more generally, to the relationship between language and culture (Mauro, 1995).

[14] For additional details see the household questionnaire for the 2001 Household survey, section 9 part E: Educational expenditures. We exclude from the calculations expenditures on photocopies due to inconsistent information reported and outliers in the sample.

[15] Additional estimations using household per student educational expenditure as well as including children in school but without expenditures were carried out, and the results remain robust.

[16] The density of age around the cutoff looks symmetric, which supports the validity of the assumption.

[17] While the surveys ask whether respondents received Bolivida, the answers are considered to be very unreliable. As it is an unconditional transfer, every adult should reply positively when hitting the corresponding age threshold. However, this is not the case for at least two reasons, lack of trust and the survey timing. In this context, an intention-to-treat approach is sensible.

[18] As indicated in Lee and Lemieux (2010), the estimation of the causal effect can be tested using separate regressions for samples above and below the cutoff age. While we provide results based on the pooled regression specification, we also tested results based on the estimations of separate regressions; results are similar.

[19] The share of individuals who receive a retirement pension is rather small. In fact, only 7.6% of the individuals older than 50 years of age are registered in formal pension funds (Source: INE http://www.ine.gob.bo).

[20] Results do not change regardless of the number of senior members in the household. Similarly, as the cutoff age coincides with the retirement age, we test excluding households with a retiree with a pension and our results also hold.

[21] Child ethnicity subsamples cannot be tested as not all variables used in the classification of ethnic groups were collected for children under the age of 12 years.

[22] We observe this graphically. The corresponding figure is available upon request.

[23] The household survey data collection process employs a two-stage sampling strategy. First, primary sampling units are chosen and then households are randomly selected. This may call for the use of weighting schemes, which are estimated by the National Statistical Office. Since our specification is based on a pooled cross-section, a differences-in-differences approach may yield biased estimates. This is because the sampling frame for the baseline surveys (1999 and 2000) was constructed on the basis of the 1992 Census enumeration list while the follow-up surveys (2001 and 2002) use an updated sampling frame that was constructed upon revised cartographic information compiled for the 2001 Census.

[24] For the purpose of this paper, we choose $c = 2$, but find similar results for other values of c.

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APPENDIX 1

Sample comparisons

	School-age children	School age children excluding outliers in expenditures	School-age children living with an eligible adult in the household[a]
Panel A: Demographics			
Child age	11.6	11.6	12.4
Share men (%)	51	51	51
Number of school-age children in HH	3.0	3.0	2.6
Number of children under 5 in HH	0.9	0.9	0.6
Household size	6.2	6.2	6.1
Number of adults aged 19–64	2.3	2.3	2.6
Father present	0.8	0.8	0.7
Mother present	0.9	0.9	0.8
Panel B: Parents			
Father's years of education	6.5	6.5	5.1
Mother's years of education	5.2	5.2	4.3
Father's age	42.2	42.2	53.6
Mother's age	39.5	39.5	48.7
Panel C: Income, expenditure (in bolivares per month)			
Income per capita w/o Bono	491.3	484.4	489.1
Expenditure per capita	446.0	439.7	431.2
% Children with positive expenditure in education	82.3	82.2	79.3
Expenditure on education per child	46.1	42.5	44.5
Observations	26,790	26,726	3,645

[a]Sample employed.

APPENDIX 2

Cross-validation of RDD and bandwidth selection

Two of the most important questions through the implementation of regression discontinuity design are the choice of the functional form used to capture the nonlinearity between the variable of interest and the dependent variable, and the choice of bandwidth. The bandwidth choice is crucial to balance the trade-off between bias and variance in the estimation of the causal effect based on RDD.

Following Imbens and Lemieux (2008) and Lee and Lemieux (2010), we use a leave-one-out cross-validation procedure and define the optimal bandwidth to the value that minimizes the following criteria:

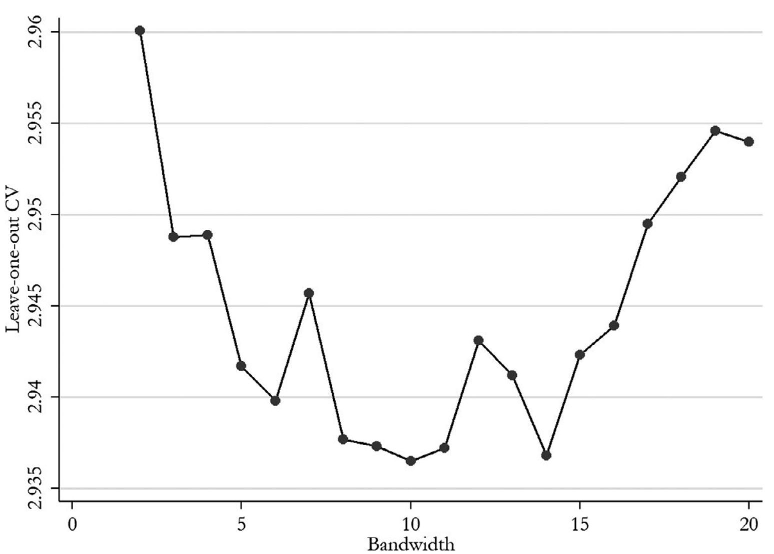
$$h_{cv} = \arg\min_{h} \sum_{i=th-c}^{th+c} \left(y - \hat{y}_{loo}(h)\right)^2$$

where *th* is the threshold used in the RDD specification, *c* is the amount of information close to the threshold used to estimate the CV statistics,[24] and $\hat{y}_{\text{loo}}(h)$ is the leave-one-out fitted value of the regression that uses all information that fall within the bandwidth:

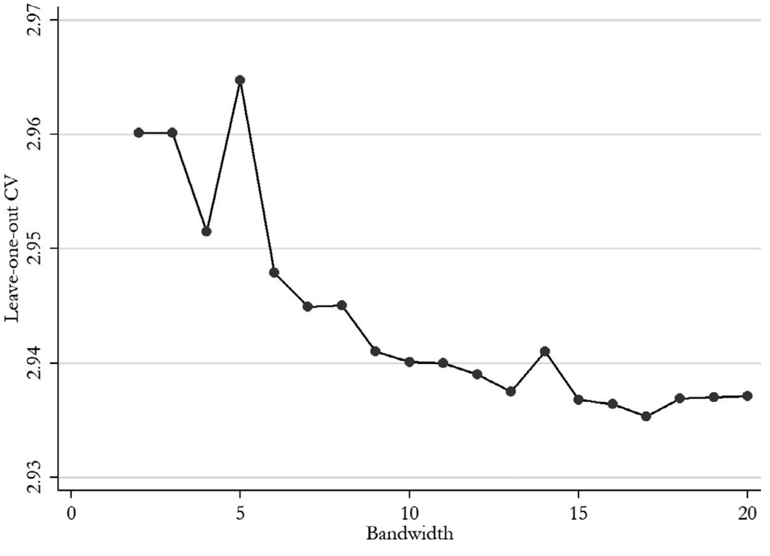
$$\text{age} \in \{\text{age} - \text{h}, \text{age} + \text{h}\}.$$

The results of the cross-validation statistics are shown in the figures that follow for the linear, quadratic, and cubic specifications. Based on this evidence, we select the linear model with bandwidth of 9 years above and below the cutoff point (age = 65 years).

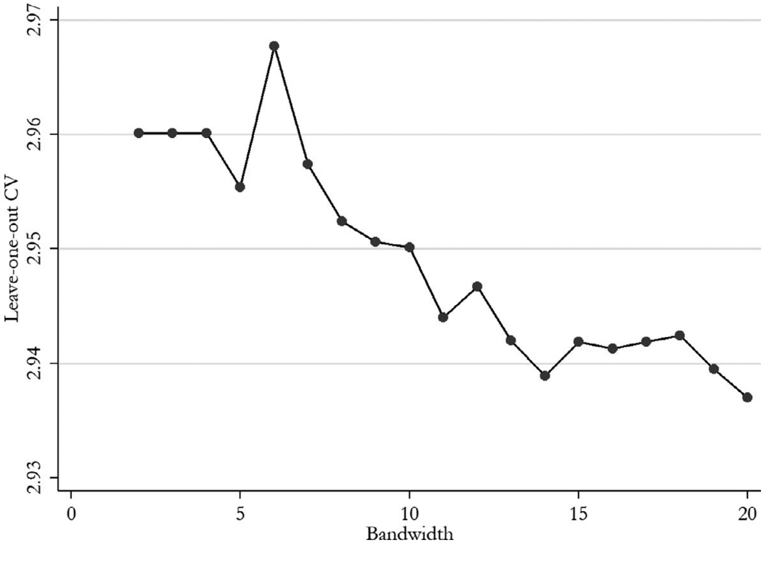
A. Linear



B. Quadratic



C. Cubic



APPENDIX 3

Results using weights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	0.766* **	**0.672** **	**0.663** **	**0.688** **	**0.738*** **	**0.738*** **	**0.771*** **	**0.751*** **	**0.749*** **	**0.749*** **	**0.665** **
Treatment	**(0.277)**	**(0.278)**	**(0.274)**	**(0.275)**	**(0.275)**	**(0.274)**	**(0.275)**	**(0.271)**	**(0.270)**	**(0.263)**	**(0.261)**
Urban x dept	x	x	x	x	x	x	x	x	x	x	x
HH indigenous		x	x	x	x	x	x	x	x	x	x
Age			x	x	x	x	x	x	x	x	x
Gender				x	x	x	x	x	x	x	x
Number of children in HH					x	x	x	x	x	x	x
Number of children under 5						x	x	x	x	x	x
Number of adults							x	x	x	x	x
Parent present								x	x	x	x
Father's/mother's age > 65									x	x	x
Parent's education										x	x
Log household income											x
N	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645	3,645
R^2	0.242	0.254	0.283	0.286	0.291	0.293	0.296	0.301	0.302	0.356	0.363

Note: Standard errors in parentheses.*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.