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Will Elders Provide for Their Grandchildren?

Unconditional Cash Transfers and Educational Expenditures in Bolivia

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Abstract

This paper takes advantage of repeated cross-section household surveys and a sharp discontinuity created by the introduction of an unconditional cash transfer to elders. The paper evaluates the impact of these cash transfers on the educational expenditures for children within a household. The analysis finds positive and significant effects of the program at the aggregate level. It also finds that the program has stronger effects in indigenous populations and among female and rural populations. The results are robust with respect to a series of falsification tests, survey structures, model specifications, and estimation methods.

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Will Elders Provide for Their Grandchildren? Unconditional Cash Transfers and Educational Expenditures in Bolivia

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1 Introduction

Even though conditional cash transfers (CCTs) have been quite successful in helping achieve development goals, there has been recent debate on the merits of unconditional cash transfers (UCTs) in relation to conditional ones. It is unclear whether CCTs are a strongly dominating strategy, as UCTs may provide equal or even superior results in some cases. In fact, UCTs have been found to reduce child labor, increase schooling, and improve health and nutrition (Baird et al., 2011). Given that from a policy perspective they are administratively easier to implement and are less costly than CCTs, a clear identification of the key characteristics associated with the successful implementation of UCTs is relevant. There are some indications along these lines that are provided by Burlando (2014) and Agüero et al. (2005), who found that attaching conditions to transfers may be superfluous providing that 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 may not be needed. The specific UCT that we explore arises from an exogenous policy change implemented by the government of Bolivia, by which elderly adults are to be provided with a permanent unconditional transfer of cash. Similar to most developing countries, elderly adults are among the most vulnerable groups in Bolivia, typically due to liquidity constraints that surge from inadequate pension systems and high levels of informality. For the purposes of our research, we focus on the case of extended households where children and grandparents live together in the same residence, something not unusual in many developing countries. It is reasonable to expect that elders living with their grandchildren in the same household will develop stronger bonds and become more invested in the welfare of the children, particularly with respect to human capital investments, such as education and health, which reinforces an alignment of incentives among members of the household.

As in most overlapping generation models, an intergenerational transmission of wealth from older generations to younger generations is expected to occur given the alignment of incentives of the older generations with respect to the expectations of welfare improvement of younger generations. In the context of a standard rational expectations approach, this may translate to an increased incentive

for the intra-household reallocation of funds from the older generation to the younger in the short term, even when the older generation is facing budget constraints (Barro, 2004; Azariadis, 1993). Interestingly, this idea has rarely been tested. Will the older generations provide for the younger ones in the short term when they are still alive and under constraints? If so, will they do so unconditionally, given the alignment of incentives?

There is limited empirical evidence for 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, the effect of old-age transfers on intra-household investment allocation is of particular interest given the increasing use of these transfers in developing countries, the number of beneficiaries that live within family households and the potential dependence on the transfers in poorer households. Some related studies include Duflo (2003) and Edmonds (2006), who examine the South African old-age pension program and find evidence that the redistributive consequences of the transfer of food, clothing, and housing are better conditions for households with children. In Latin America, there is some evidence that transfers have reduced poverty (Joubert and Todd, 2011; Barrientos, 2003) as well as evidence regarding intra-household behaviors among households in Chile and Mexico that receiving transfers deviates expenditures towards human capital investments, particularly health (Amuedo-Dorantes and Juarez, 2015; Behrman, 2011).

The implementation of an old-age UCT program in Bolivia (Bolivida) provides an excellent opportunity to study the causal effects of UCTs on intra-household income allocation towards children's human capital investments, particularly that of education, for which there is, along with health, limited evidence. This program, first implemented at the end of 2000, has been the core of old-age population support strategies in the country. 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 age of eligibility and use this sharp discontinuity to parametrically identify conditional average intent-to-treat effects and study intra-household income allocation patterns with emphasis on education investments. We measure the impact of this program through child-level educational expenditures, along with old-age program eligibility, after controlling for socioeconomic and demo-

graphic characteristics. The eligibility effect, which we allow to vary across households and children, is assumed to be a log 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 also estimate a series of heterogeneous estimates. Our main finding is that unconditional cash transfers to elders lead to substantial improvements in children's educational investments. We find that unconditional transfers increased such investments by approximately 60 percent. With respect to ethnicity, eligibility leads to greater increases in schooling investments among indigenous households compared to their non-indigenous counterparts. We find a similar result for rural and female students.[1]

Our paper is organized as follows. Section 2 briefly describes the old-age unconditional cash transfer program and its basic characteristics. Section 3 presents the data and basic statistics. Section 4 describes the methodological approach, and Section 5 presents our results. Section 6 then presents robustness tests, and Section 7 concludes the paper.

2 Brief Description of UCT Program for Elders

Bolivia is the poorest country in South America, as illustrated by the fact that approximately two-thirds of the households are below the poverty line (The World Bank, 2016). Traditionally, the elderly and the youngest have been the most vulnerable and unprotected segments of the population. In fact, whereas 35 percent of adults live in poverty, nearly 55 percent of children and 60 percent of the elderly do so (The World Bank, 2016), and it is this latter group that is the most unprotected and vulnerable. Moreover, coverage in Bolivia is one of the lowest in the region, with approximately 80 percent of the population having no access to any type of pension system (Landa and Yanez-Pagans, 2008). In addition, the country has a rather complex multiethnic dimension reflected in the fact that it has the

[1] Only a handful of studies have examined effects of this unconditional cash transfer. They have focused on impacts on poverty (Escobar-Loza et al., 2013), household expenditures (Martinez, 2004), co-habitation (Valencia, 2011), health, labor and unintended impacts (Hernani-Limarino and Mena, 2015). The latter provides some basic evidence on education, but exploits a different exogenous shock for different years. In particular, they take advantage of a reduction in the age of eligibility from 65 to 60 years in 2007.

highest percentage of indigenous populations in Latin America, with almost 50 percent of Bolivians self-reporting as belonging to an indigenous group.[2]

As part of the structural reforms of the 1990s that were broadly supported by the World Bank and the International Monetary Fund, the government created an old-age unconditional cash transfer program that entitled all Bolivians aged 65 and older to receive a flat, noncontributory, unconditional cash transfer independent of their income levels. In short, the only eligibility rule was age[3] . 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. Whereas the original program, known as Bonosol (Bono Solidario), was first introduced in 1997, liquidity problems were so serious that in 1998, it had to be put on hold, and when it resumed, at the end of 2000, it was renamed Bolivida, and the pension was reduced from annuities of US$248 to US$60. Figure 1 presents estimates by beneficiaries. Though take-up is high at the margin of eligibility, compliance is not perfect. Whereas 80 percent of the individuals over 65 receive the benefit, the share is slightly lower as one approaches the cutoff, a situation that is mainly driven by the indigenous male population in rural areas. Low take-up rates are somewhat unsurprising given that, in rural areas, there are significant deficiencies in personal identification systems and there are few financial centers, a situation that tends to disproportionately affect the indigenous population [4]. Under the reasonable assumption that, on average, age is truthfully reported and that it is measured with little error, it is reassuring to observe that very few elders who are ineligible to benefit from this program are cashing out of Bolivida.

[2]Furthermore, the ethnic dimension is closely correlated to poverty as 49 percent of indigenous people live below the poverty line, but only 24 percent of the non-indigenous population do (http://www.ine.gob.bo).

[3]As in many Latin American countries, in the 1990s Bolivia implemented a broad array of structural reforms, 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.

[4]There are no data that allow assessing the extent to which different constraining mechanisms might be deterring elders to cash out of Bolivida.

3 Data

We employ a set of nationally representative cross-sectional Living Standards Measurement Surveys (LSMS) from 1999 and 2002 administered by the Bolivian National Institute of Statistics[5]. The surveys include a comprehensive socioeconomic module, Bolivida receipt information at the individual level, and detailed expenditures for all members in the household who are at least six years old[6]. The sample comprises all school-age children who live in households with at least one person in the age range of 56 to 73[7]. The school-age range considered is 6- to 18-years as the minimum legal working age in Bolivia is 14. The sample excludes households that belong to the top 1 percent of the educational expenditures distributions. The total sample includes 3,645 school-aged children and 1,038 eligible elders distributed among 1,915 households. The surveys employ a stratified two-stage sampling whose sampling frames are based on census data[8].

Table 1 reports summary statistics. On average, families living with an eligible elder are not substantially different from those living with a soon-to-be eligible elder, factors that have not changed significantly during our period of study. Eligible households are slightly smaller and have slightly more children than non-eligible ones, even though both have nearly the same number of school-age children, especially in later years. However, non-eligible and eligible households have different family characteristics. Particularly, a higher percentage of non-eligible households have both the father and mother living in the household, and they are of parental age. In addition, the parents' years of schooling and the children's education expenditures are higher for eligible households. Finally, both eligible and non-eligible families allocate approximately 10 percent of their total income to children's educational expenditures[9].

[5]INE website: http://www.ine.gob.bo.

[6]Approximately 18 percent of households are three-generation extended families.

[7]The preferred sample is defined based on the bandwidth used for the estimation of the regression discontinuity model. Robustness tests were conducted using slightly larger and smaller samples.

[8]The sampling frame for the baseline survey was constructed based on 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 regarding the bias of the estimates, we perform robustness tests that show no statistical effect of this sampling framework. These results are available upon request.

[9]We also observe that the largest disparities appear in human capital among ethnic groups as well as between urban and rural populations where educational gender gaps among indigenous people are particularly large.

To identify the effects of the unconditional cash transfer on expenditures in education, we build a variable that captures expenditures on education for each school age child as our key unit of analysis. Expenditures on education include education enrollment tuition and fees, uniforms, school materials, other school fees and contributions, transportation related expenditures and individual expenses for each student[10]. This measure captures heterogeneity across different age cohorts, as expenditures may differ across educational levels, and intra-household human capital investment decisions may vary at different stages of life. Therefore, to evaluate the impact on education investment, our unit of analysis is children between 6 and 18 years old[11]. Accordingly, we analyze the combined impact on the decision to invest, if the children have matriculated, and the amount spent.

In terms of treatment assignment, we identify the treatment status of a household based on the age of the household member closest to the cut-off age of 65 years. If the household member closest to the cut-off age is younger than 65, the household is considered untreated, and if the member is 65 years old or older, the household is considered treated. For example, a household with two senior people, one who is 64 and one who is 80, is considered untreated, while a household with two senior persons, one who is 65 and one who is 80, is considered treated. In this sense, the results can be interpreted as the impact of having one additional recipient member, and we can more clearly identify the impact of receiving the cash transfer on the allocation of expenditures within the household. Thus, we consider as treated those children who live in a treated household and consider as untreated those children living in an untreated household.

Panel (a) in Figure 2 displays a histogram of individuals at age 50 and above that compares before and after the implementation of Bolivida, which occurred at the end of 2000. There is no evidence of a sudden increase in the population distribution at the age of the threshold of eligibility. Panel (b) in the same figure indicates that there is no change in the share of households that are eligible around the cut-off age. In addition, the conditional probability of being eligible increases at such a

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

[11]Additional estimations using household per-student educational expenditure as well as including children in school but without expenditures were performed and the results remain robust.

value, indicating a significant discontinuity with respect to actual beneficiaries. As it is reasonable to believe that age cannot be manipulated, especially since beneficiaries are required to provide legal documents to authorities to participate, households are unlikely to strategically locate themselves around the age of eligibility. Thus, we can confidently assert that the main assumption behind our regression discontinuity design holds, and therefore, we use it to identify the average UCT intention-to-treat effects on children's schooling expenditures (Imbens and Lemieux, 2008)[12]. It is noted that we do not estimate the average treatment effect on the treated as the use of the beneficiary variable in this setup is problematic since the receipt of a pension may be endogenous given that we find slight differences between actual and estimated beneficiaries. As previously mentioned, these differences may arise because of deficiencies in the government's personal identification documentation system, because of a lack of available funds to cover the fully targeted population, or because of difficulties reaching financial centers, either due to the lack of transportation or the excessive distance. All these factors may constrain eligible members from receiving benefits[13].

Figure 3 presents the conditional expectation of children's schooling expenditures as a function of the age of the household member closest to the cut-off age. The smoothing is performed separately for the period before and the period after 2001. The graph makes clear that before 2001, there is no difference in the average education expenditure per child. After 2001, however, 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 Bolivida on children's educational expenditures.

4 Empirical Strategy

The aim of this paper is to analyze the causal effect of the potential unconditional cash transfer received by elders on the education expenditures of children who live in the same household, when

[12]The density of age around the cut-off appears symmetric, which supports the validity of the assumption.

[13]As the exact constraining mechanisms are unclear, we decided that our empirical exercises should focus mainly on potential beneficiaries.

taking advantage of the exogenous implementation of the Bolivida program of 2001, which is related to the income shock that 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 education expenditures per child as a function of the age of eligibility of the reference household member and the variable indicating whether Bolivida was already implemented. However, a simple comparison of education expenditures by households below or above the cut-off age across time is likely to provide a biased estimate. Since the cut-off age coincides with the retirement age, the household structure and distribution of potential retirees may not be random, and thus, the presence of elder household members may have an impact on the allocation of time and resources among household members. Therefore, it may be possible that potential beneficiary households will differ systematically from non-beneficiary households. In this case, a simple difference-in-difference model would not be valid to account for the potential systematic differences.

To overcome unobserved differences between potential beneficiary and non-beneficiary households, we exploit the fact that the probability that an individual receiving the cash transfer changes discontinuously at the age of eligibility. Accordingly, we apply a straightforward regression discontinuity design (Imbens and Lemieux, 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 apply is a combination of a differences-in-differences approach and a sharp regression discontinuity design. This specification is written as follows:

$$\begin{aligned} log(Educ\,exp+1) =& a_0 + a_1 * I_{age65} + a_2 * I_{yr\geq 01} + a_3 * I_{age65} * I_{yr\geq 01} + \\ & b_\gamma * f_\gamma(age_{reg} - 64.5) + b_l * I_{age65} * f_l(age_reg - 64.5) + \\ & c_\gamma * I_{yr\geq 01} * g_\gamma(age_{ref} - 64.5) + \\ & c_l * I_{yr\geq 01} * I_{age65} * g_l(age_{ref} - 64.5) + \epsilon \end{aligned} \tag{4.1}$$

where the log of per child education expenditure is estimated as a function of the age of eligibility

(I_{age65}) of the reference person in the household, an indicator of the year when the transfer was implemented ($I_{yr\geq 01}$) and an interaction of both age of eligibility and year of implementation. In addition, we allow for the inclusion of flexible functional forms of the gap between the reference person's age and the cut-off age (age_{ref}-64.5), both of which can 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 cut-off age. In this specification, a_3 is the parameter of interest as it identifies the impact of the unconditional cash transfer for school age children living in households where the reference person is near the cut-off age[14].

Since the treatment state of a household is based on the age of the person closest to the cut-off age or the reference individual, it is possible that there may be other elderly individuals living in the household. Given 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 are considered, (i) the functional form for the age gap (f_γ,f_l,g_γ,g_l) and (ii) the bandwidth selection. With respect to the choice of the functional form, we consider that the forcing variable, the age of the reference individual, is discreet, and thus, the use of non-parametric regressions is not viable. Instead, we estimate the 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 over-specification. The selection of the order polynomial is based on the Akaike information criteria (AIC) as suggested by Lee and Lemieux (2010).

The second key issue is the choice of an appropriate bandwidth selection, as it determines the final sample employed in the analysis. While larger bandwidths allow for the use of more data and, thus, potentially help 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 address

[14] 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 cut-off. While we provide results based on the pooled regression specification, we also provide results based on the estimations of separate regressions in Appendix 1 (Table 9).

this issue, we apply a cross-validation strategy, as described by 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 nine years above and below the cut-off age.

5 Results

We depart from testing competing models to obtain an initial estimate as well as establish the best specification for our estimation. As evidenced in Figure 5, and following the existing literature of Lee and Lemieux (2010), we test quadratic, cubic, log linear and linear specifications. For most specifications, the average effect is below one. Following Lee and Lemieux (2010) and the Akaike criteria, these findings indicate that the preferred empirical specification should be the linear specification. Table 2 reports the basic results of our preferred specification without any controls. Column one shows that, on average, the implementation of the cash transfer increased expenditures on education by approximately 73 percent in Bolivia. In addition, our results show that the probability of moving from a zero expenditure on education to a positive size is approximately 12 percent and, for those households that have some level of expenditure, the effect of the cash transfer reaches approximately 43 percent and remains statistically significant at conventional levels. These results indicate that unconditional cash transfer to elders influences intra-household behavior by promoting larger expenditures on education. Moreover, these effects are larger in those households that are already contributing to their children's education. However, these results may be biased as we are not controlling for other factors that may affect education expenditures.

Table 3 presents the findings when we include a broad set of controls, specifically, age, gender, number of children in the household, number of children aged five or under, number of adults, presence of a parent in the household, parents' education, urban or rural area, and indigenous or non-indigenous background. For the most part, we find analogous results and stronger impacts. However, it is interesting to note that when controlling for an indigenous background and an urban setting, the impact observed is reduced by approximately ten percentage points with respect to the specification

without such controls. These somehow suggest that additional heterogeneity analyses can provide further insights into the results presented herein. For instance, full sample estimates may hide some heterogeneity due to cultural and geographical factors, and, more particularly, ethnicity, a highly relevant element in a country such as Bolivia.

The identification of indigenous people in Bolivia is not straightforward as 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.

We use three specific questions from our survey data aimed at identifying ethnic groups: (i) do you consider yourself as belonging to an indigenous group? (ii) what languages do you speak? and, (iii) what language did you first learn to speak? The first two questions are intended exclusively for household members who are at least 12-years-old, and the third is intended for all members in the household. We follow Molina and Albó (2006), who employ the above three criteria to construct an ethnolinguistic matrix for Bolivia. We classify as non-indigenous those who do not self-identify as indigenous, whose first language was not indigenous, and who did not learn an indigenous language as children. We define as indigenous those who identify themselves as indigenous, whose first language was indigenous and those who learned an indigenous language as children. To simplify our empirical approach, all other combinations are labeled as multiethnic.

Table 4 displays the heterogeneous impacts of the unconditional cash transfer of elders on children's educational expenditures by ethnicity, region, gender, and age cohorts. The estimated parameters are reported separately for each group. After the implementation of the cash transfer, we find statistically significant and larger impacts for indigenous populations and smaller and statistically insignificant effects for non-indigenous people. Mixed race populations reveal statistically insignificant results that lie somewhere between the indigenous and non-indigenous populations. These results may be because indigenous populations tend to face credit constraints, and therefore, any liquidity shock modifies significantly the allocation of expenditures within households. A similar finding is noted when comparing rural and urban groups. While in the former we find a large and statistically significant impact, urban populations exhibit smaller and non-significant effects. In addition, we es-

timate the average effects of eligibility for girls and boys separately[15]. 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, while for boys, this result is approximately 22 percent and not statistically significant at any standard levels. This finding is reassuring since females have been historically neglected in schools in Bolivia.

In Table 5, we further explore the role played by gender in the intra-household allocation from elders to children. Specifically, we explore the affinity between the gender of grandparents and grandchildren as well as the impact of the age of the children on grandparents' preferences. We find evidence that educational expenditures on girls are driven by grandfathers, but not by grandmothers, as revealed in Panel (a). This is rather remarkable, as it indicates that male elders, having benefitted from a system that favors males, realize the difficulties that females have in Bolivia. Moreover, it is fully consistent with our previous findings. Furthermore, we find that grandfathers focus their attention on girls between the ages 13 and 18 years, unlike grandmothers, who appear to focus on younger children. This is presented in Panel (b) in Table 5.

6 Robustness

We observe that there is a small share of the population that may be receiving the unconditional cash transfer despite being younger than the corresponding cut-off age of 65 and that the share of people who are not receiving the benefit may be greater for those closer to the cut-off[16]. This may raise questions about the strength of using 65 years of age as the cut-off point and about the relevance of the bandwidth. To test for this, we perform a series of falsification tests, the results of which are displayed in Table 6. We test alternative cut-off ages ranging from 63 to 67 years of age and control for analogous covariates as in our main results. We do not find any statistically significant results at conventional levels.

[15]Child ethnicity sub-samples cannot be tested since not all variables used in the classification of ethnic groups were collected for children under the age of 12.

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

Similarly, Table 7 presents the results using different bandwidth and alternative empirical specifications that control for all the covariates as in Table 6. A major concern with the regression discontinuity design is the high sensitivity of the estimates to the choice of bandwidth, and thus, although moving the bandwidth slightly affects 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, albeit smaller for the quadratic and larger for cubic. However, for the quadratic specification, it is only significant when using $h = 10$, and it is not 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 are estimated using a combination of RDD and a before-after design (Hoddinott and Skoufias, 2004; Borooah and Iyer, 2005). That is, these results are estimated separately for the period before and the period after the Bolivida UCT, and conditional means are then compared to assess the impact of the program. This specification is more flexible than the pooled one previously employed, but it reduces the degrees of freedom as it requires that more parameters be estimated. Although the results are somewhat smaller, they are equally statistically significant.

Finally, the Appendix (Table 9) presents the results when using weights provided by the National Statistics Institute. If anything, the results tend to be more robust and slightly greater than, albeit not significantly different from, our base results[17].

7 Conclusions

In this research, we take advantage of repeated cross-section household surveys and a sharp discontinuity created by the introduction of an unconditional cash transfer to elders to evaluate the

[17]The household survey data collection process employs a two-stage sampling strategy. First, primary sampling units are chosen, and then, households are selected randomly. 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 diff-in-diff approach may yield biased estimates. This is because the sampling frame for the baseline surveys (1999 and 2000) was constructed based on the 1992 census enumeration list while the follow up surveys (2001 and 2002) used an updated sampling frame that was constructed upon revised cartographic information compiled for the 2001 census.

intent-to-treat impact on the educational expenditures related to children within a household by using a regression discontinuity design and a differences-in-differences approach. The eligibility effect, which we allow to vary across households and children, is assumed to be a linear function of the ethnicity and gender of the potential recipient. The main finding is that unconditional cash transfers to elders lead to substantial improvements in children's human capital investments. We also find that the program has stronger effects in indigenous populations as well as in female and rural populations. Our results are robust to a series of falsification tests, survey structures, model specifications and estimation methods.

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Table 1: Summary Statistics

	Non-Eligible (Age $\leq$ 64)		Eligible (Age$\geq$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 (percentage)	50.3	51.8	54.9	48.2	51.0
# School Age Children in HH	2.6	2.6	2.3	2.8	2.6
# 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
# Adults 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					
Yrs of Education Father	5.2	4.8	6.0	5.3	5.1
Yrs of Education Mother	3.9	4.0	5.4	5.1	4.3
Age Father	54.5	54.3	51.9	51.4	53.6
Age Mother	49.2	49.3	47.6	47.1	48.7
Panel C: Income, Expenditure (in Bs. 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

Table 2: Impact of UCTs on Educational Expenditures

	(1) Log (Expenditure+1)	(2) Pr(Expenditure>0)	(3) Log Expenditure if Expenditure > 0
Treatment	0.732**	0.123*	0.426*
	(0.255)	(0.059)	(0.203)
N	3645	3645	2889
R^2	0.025	0.014	0.031

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Impact of UCTs on Education Expenditures with Controls

	1	2	3	4	5	6	7	8	9	10	11
Treatment	0.732**	0.667***	0.607***	0.573***	0.601***	0.629***	0.612***	0.633***	0.637***	0.636***	0.586***
	-0.255	-0.224	-0.221	-0.22	-0.219	-0.219	-0.219	-0.219	-0.219	-0.218	-0.213
Urban × Dept		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
HH Indig			✓	✓	✓	✓	✓	✓	✓	✓	✓
Age				✓	✓	✓	✓	✓	✓	✓	✓
Gender					✓	✓	✓	✓	✓	✓	✓
Children HH						✓	✓	✓	✓	✓	✓
# Children<5							✓	✓	✓	✓	✓
# Adults								✓	✓	✓	✓
Parent Present									✓	✓	✓
Parent > 65										✓	✓
Parent educ											✓
N	3645	3645	3645	3645	3645	3645	3645	3645	3645	3645	3645
R^2	0.025	0.216	0.232	0.261	0.263	0.271	0.274	0.275	0.279	0.281	0.329

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Heterogeneous Impacts

	(1) Indigenous Status	(2) Urban Status	(3) Age	(4) Gender
Non Indigenous	0.232			
	(0.350)			
Observations	1480			
R^2	0.355			
Indigenous	0.727**			
	(0.370)			
Observations	1157			
R^2	0.261			
Mix	0.553			
	(0.450)			
Observations	1008			
R^2	0.300			
Rural		0.746**		
		(0.309)		
Observations		1809		
R^2		0.204		
Urban		0.434		
		(0.289)		
Observations		1836		
R^2		0.238		
6-12			0.612**	
			(0.266)	
Observations			1799	
R^2			0.331	
13-18			0.462	
			(0.337)	
Observations			1846	
R^2			0.352	
Girls				0.966***
				(0.309)
Observations				1785
R^2				0.354
Boys				0.225
				(0.295)
Observations				1860
R^2				0.321

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Characteristics of Elder and Child

A. Gender of Child and Gender of Elder	(1) Girl/Grandmother	(2) Girl/Grandfather	(3) Boy/Grandmother	(4) Boy/Grandfather
Treatment	0.566	1.484***	0.300	-0.005
	(0.469)	(0.429)	(0.489)	(0.388)
Observations	756	1029	749	1111
R^2	0.388	0.368	0.330	0.346

B. Gender of Elder and Age of Child	(1) Grandmother/6-12	(2) Grandfather/6-12	(3) Grandmother/13-18	(4) Grandfather/13-18
Treatment	0.800**	0.486	-0.116	0.734*
	(0.389)	(0.385)	(0.580)	(0.422)
Observations	758	1041	747	1099
R^2	0.364	0.345	0.369	0.373

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Falsification Tests

	(1) Age cut=63	(2) Age cut=64	(3) Age cut=66	(4) Age cut=67
Treatment	-0.042	0.112	0.295	0.213
	(0.191)	(0.199)	(0.232)	(0.237)
Observations	4436	4098	3225	2988
R^2	0.336	0.338	0.330	0.335

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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	2696	3100	3645	4223	4728
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	2696	3100	3645	4223	4728
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	2696	3100	3645	4223	4728
R^2	0.347	0.335	0.329	0.336	0.331

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Regression Discontinuity with Before and After Approach

	1	2	3	4	5	6	7	8	9	10
Treatment	0.618***	0.550**	0.520**	0.542**	0.571***	0.541**	0.578***	0.586***	0.528**	0.446*
	(0.222)	(0.219)	(0.218)	(0.217)	(0.217)	(0.217)	(0.222)	(0.221)	(0.238)	(0.228)
Urban × Dept	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
HH Indigenous status		✓	✓	✓	✓	✓	✓	✓	✓	✓
Age			✓	✓	✓	✓	✓	✓	✓	✓
Gender				✓	✓	✓	✓	✓	✓	✓
# Children in HH					✓	✓	✓	✓	✓	✓
# Children under 5						✓	✓	✓	✓	✓
# Adults							✓	✓	✓	✓
Parent present								✓	✓	✓
Father/Mother > 65									✓	✓
Father/Mother educ										✓
Observations	3645	3645	3645	3645	3645	3645	3645	3645	3645	3645
R^2	0.242	0.254	0.283	0.286	0.291	0.293	0.296	0.301	0.302	0.356

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

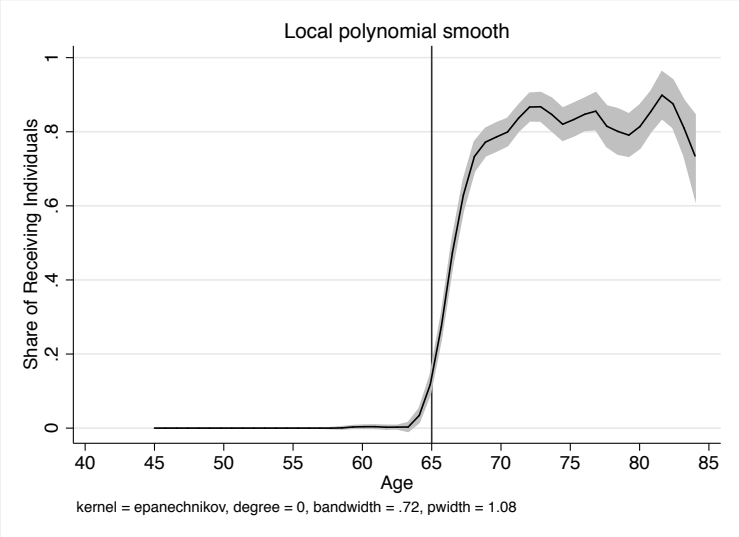


Figure 1: Share of People who Received UCT by Age

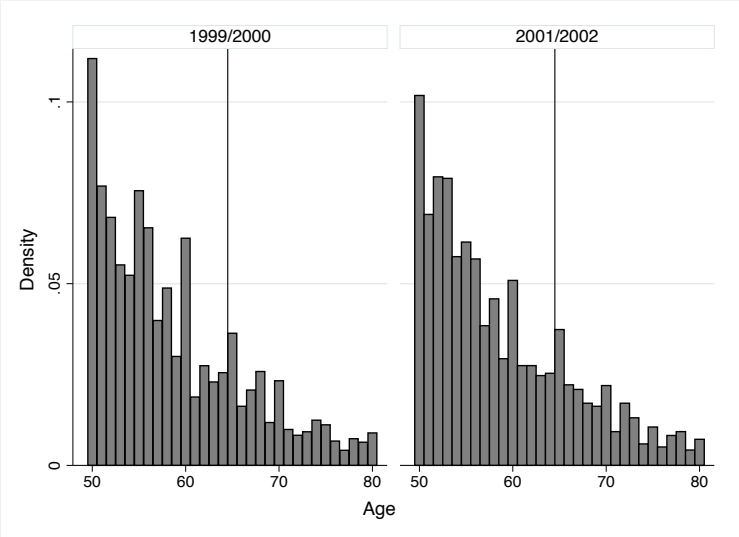


Figure 2: Histogram in Age Person Closest to Eligibility

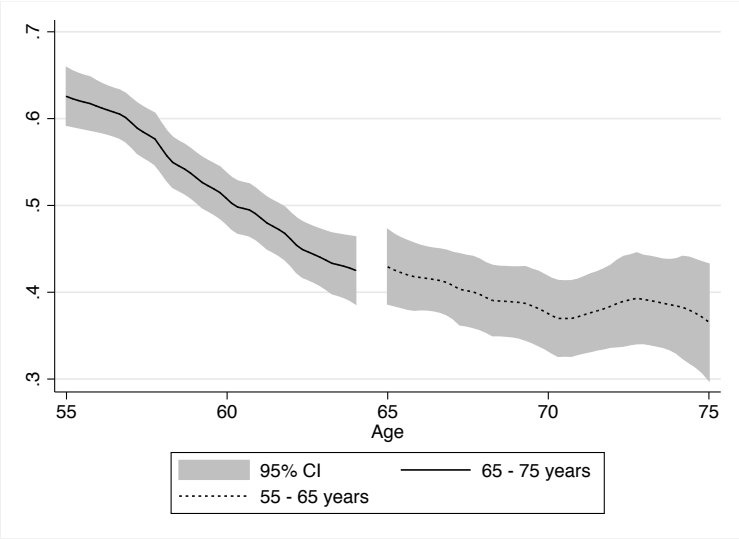
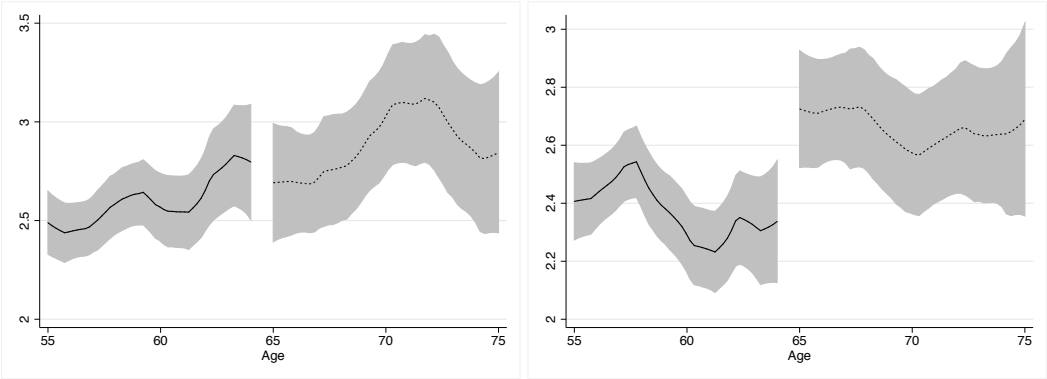


Figure 3: Share of Households eligible around the Threshold Age



(a) (b)

Figure 4: Average Log Expenditure on Education per children (1999/2000)

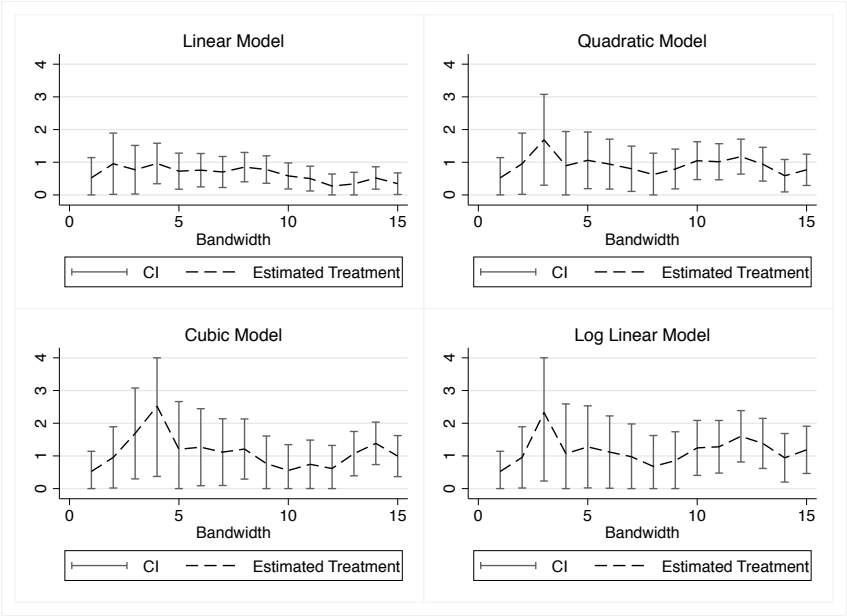


Figure 5: Base Model Polynomial Estimations

Table 9: Appendix: Results Using Weights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treatment	0.766***	0.672**	0.663**	0.688**	0.738***	0.738***	0.771***	0.751***	0.749***	0.749***	0.665**
	(0.277)	(0.278)	(0.274)	(0.275)	(0.275)	(0.274)	(0.275)	(0.271)	(0.270)	(0.263)	(0.261)
Urban × Dept	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
HH indigenous		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Age			✓	✓	✓	✓	✓	✓	✓	✓	✓
Gender				✓	✓	✓	✓	✓	✓	✓	✓
# Children in HH					✓	✓	✓	✓	✓	✓	✓
# Children under 5						✓	✓	✓	✓	✓	✓
# Adults							✓	✓	✓	✓	✓
Parent present								✓	✓	✓	✓
Father/Mother > 65									✓	✓	✓
Parent educ										✓	✓
Log Househol Inc											✓
N	3645	3645	3645	3645	3645	3645	3645	3645	3645	3645	3645
R^2	0.242	0.254	0.283	0.286	0.291	0.293	0.296	0.301	0.302	0.356	0.363

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$