Does a Cash Transfer Affect Elderly Labour Supply? Evidence from Age Discontinuities in Bolivia* PRELIMINARY: PLEASE DO NOT QUOTE WITHOUT PERMISSION

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March 29, 2013

Abstract

We study the effect of a cash transfer to elderly individuals on their labour supply decisions by using data on households in Bolivia. We identify the effect thanks to a regression discontinuity design since a policy reform makes the eligibility for the transfer discontinuous at age 60. We find that the transfer decreases the probability of being employed by 5-10 percentage points although it is not statistically significant. In addition, the transfer decreases employment by 20-44 percentage points for females in urban areas and it is highly significant while it is not for the remaining subgroups of individuals by gender and residence in an urban area. The empirical evidence informs policy decisions about the labour market for elderly individuals by suggesting *i*) that the cash transfer program that the Bolivian government setup only marginally affects labour supply on average and *ii*) that policy design should account for heterogeneous responses by different subgroups of individuals.

JEL Classification: H53, H55, J22, J26
Keywords: Bolivia, cash transfer, old-age workers, regression discontinuity, Renta Dignidad, retirement

*Thanks for helpful comments to Mariano Bosch and Fernanda Brollo. All errors are our own.

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1 Introduction

The design of labour market policies for elderly individuals scores high in policy-makers' agendas worldwide since it helps elderly individuals in developed countries to smooth consumption over time and, similarly, it decreases poverty levels in developing countries. Recent reforms in developed economies (Holzmann, 1997; Holzmann and Hinz, 2005) and in developing ones (Mesa-Lago, 2004) are driven inter alia by demographic changes, as well as by efficiency and distributional objectives (Barr and Diamond, 2008; World Bank, 1994). Overall, this confirms the pressing need for additional knowledge on the response by elderly individuals to labour market policies that are targeted at them.

In this paper we study the effect of a cash transfer to elderly individuals on their labour supply decisions. We set out to answer the question empirically by using a representative dataset of Bolivian households that contains information on individuals' employment and on their eligibility for a cash transfer that the government introduced. Its objective was to offer an old-age subsidy to all its elderly citizens regardless of their social security contribution. If an old-age worker benefits from an intervention that makes leisure cheaper with respect to consumption, or that similarly decreases the motivation to work, it may decrease the probability of staying on in a job. The intervention may either come in the form of a cash transfer, an increase in retirement benefits (Danzer, 2010), or as a decrease in the minimum retirement age (de Carvalho Filho, 2008; Bertrand *et al.*, 2003). Conversely, an individual's labour supply may not be affected by the intervention if the individual prefers a greater overall income, which may decrease poverty (Dethier *et al.*, 2010; Case and Deaton, 1998), and also increase consumption (Martinez, 2004). In addition, the intervention may increase children's education level (Martinez, 2004; Edmonds, 2006) or health (Duflo, 2003), have an impact on family arrangements (Edmonds *et al.*, 2005) or on the labour supply by prime-aged adults in a household (Ardington *et al.*, 2009; Bertrand *et al.*, 2003). The competing mechanisms that may drive the overall effect of a cash transfer for old-age workers on their labour supply highlight the importance of addressing the question empirically.

We identify the effect thanks to a regression discontinuity design by exploiting the eligibility rule for the transfer that is discontinuous at age 60. We estimate the effect as the difference in the estimated probability of being employed between the group of individuals who receive the transfer as they are 60 or barely older at the time of the survey and those who do not receive it as they are barely younger. We give a causal interpretation to the estimates under

the reasonable assumption that an individual's day or month of birth is "as if" randomised in a small neighbourhood of the age cutoff. Thanks to this research design, we can tease out the effect of confounders, as it is negligible for those individuals who are in a small neighbourhood on either side of the age 60 threshold. In contrast, estimates from a linear probability model may capture the joint effect of the transfer and of the correlation between the eligibility status for the transfer and unobserved variables that are correlated with eligibility, such as bank savings, financial literacy or health status.

In the empirical analysis regression discontinuity design estimates show that the transfer decreases the probability of being employed by 5-10 percentage points although it is not statistically significant. In addition, the effect of the transfer is highly significant and decreases the probability of employment by 20-44 percentage points for the subsample of females in urban areas, while it is not significant for the remaining subgroups. The results are robust to several econometric specifications and also when we account for the correlation between the labour supply decisions of the spouses in a household.

We offer a novel contribution to the literature that studies labour market policies for old-age workers *i*) by illustrating that the effect of receiving a cash transfer on employment is not clear-cut ex-ante since individuals may face competing incentives and *ii*) by estimating the causal effect of the transfer thanks to a discontinuous eligibility rule based on age and finding a heterogeneous response by different subgroups of individuals. This suggests that the design of policy reforms should carefully account for potentially heterogeneous behavioural responses to a policy.

The structure of the rest of the paper is as follows. Section 2 describes the institutional setting and the data. This sets the ground for the research design in section 3 and the empirical analysis in section 4. Finally, section 5 discusses the results and concludes.

2 Institutional setting and data

In this section we illustrate the policy intervention that generates the exogenous variation to identify the effect of a cash transfer for old-age workers on their labour supply, and the data on Bolivian households that we use to estimate it.

4.6% of the population in Bolivia is older than 65 and about two-thirds live in poverty, several of whom are subsistence farmers. In addition, life expectancy at birth is 67.9 years and mortality rate is about 6.76 deaths per thousand (CIA World Factbook, 2012). This overall

suggests a slightly younger population with lower life expectancy and lower living standards than other countries in South America.

Bolivia was the first country in South America in providing an annual cash transfer to all its citizens who were 65 or older, as part of a broader social and economic reform agenda. The purpose of the transfer was to offer an old-age subsidy to the whole elderly population regardless of their social security contribution (Bonosol).[1] The first payment was done in May 1997 and it consisted in 1,300 Bolivianos (248 US$) indexed to the value of the US dollar. This figure was approximately 25% of low per capita income and 85% of the average income of those living in extreme poverty (von Gersdorff, 1997).[2]

From 1998 to 2001 the transfer decreased to 420 Bolivianos (60 US$), due to financing problems (Bolivida). In 2002 the program was restored almost unchanged with respect to the first version. However, additional financing problems led to new statutory changes in the interventions for elderly individuals that were enacted on 1st February 2008 (Renta Dignidad).[3] The main differences with respect to Bonosol are *i*) a decrease from 65 to 60 in the age threshold at which individuals are eligible for the transfer and *ii*) a decrease in the amount paid. All individuals who are 60 or older, which is a key element in our research design, receive 2,400 Bolivianos (344 US$) per year except those who were retired or worked in the public sector who receive 1,800 Bolivianos (approx. 258 US$). Overall, the take-up rate of the transfer was greater than 90%, thanks inter alia to the several options to cash the payment that were available to the individuals who were eligible for the transfer.[4]

We estimate the effect of the transfer on old-age workers' labour supply decisions by using a repeated cross-section of survey data on households in Bolivia in the years 2008-2009 from the Instituto Nacional de Estadistica in Bolivia (Encuesta de Hogares). They contain information on labour supply, demographics and health at the individual and at the household level. They also contain survey weights to correct the estimates for under- or over-sampling, thus making our results representative of the population. We choose the time of the interview for the household survey in November 2008 as the time cutoff for the eligibility to receive the

[1] See Ley de Pensiones, no. 1732, 29 November 1996, article 12.

[2] See Willmore (2006, 2007) for additional information about pension reforms in Bolivia.

[3] See Ley de Pensiones, no. 3791, 28 November 2007, article 12.

[4] The beneficiaries of Renta Dignidad received the money payment by either going to a bank or authorised military enclosure with the ID card and two photocopies the month after turning 60. Alternatively, arrangements were in place to obtain the money at home. Individuals obtaining 2,400 Bolivianos (about 80% of the beneficiaries) could choose the frequency of the payment, while for the others the transfer was added automatically to the payment ballots that they usually received on a monthly basis.

transfer, rather than the day the cash transfer program was enacted in February 2008, so as to observe the employment status of both eligibles and non-eligibles in the survey data.

We proxy labour supply for old-age workers with their employment status as they tend to coincide under the reasonable assumption that a negligible share of old-age workers are unemployed or dropped out of the labour force before reaching the retirement age. We define the outcome of interest in the empirical analysis as a dummy that is equal to 1 if an individual was employed the week before the individual was interviewed in the household survey and it is equal to 0 otherwise. The response rate is of 100% among individuals who answered the survey. [5] We restrict the sample size to those individuals whose employment condition was observed up to a month after they turned 60 as up to a month may elapse between the 60th birthday and the payment of the first cash transfer. The data sample that we use in the empirical analysis contains the individuals who are 40 or older and younger than 80. This leads to a final sample of 8,055 observations in the years 2008-2009. About 2,500 of them are in the treatment group that consists of individuals who are 60 or older in 2008, thus eligible for the transfer, while the remaining ones are in the control group. We also use data in the same age range from the surveys in 2006 and 2007 to assess the robustness of the estimates.

Table 1 shows summary statistics of employment, our outcome of interest, and of the baseline characteristics of the individuals in the survey. The table shows across columns means separately for the subgroup of individuals who were at least 60 at the time of the yearly household, and were hence eligible for the transfer, and for younger individuals who were hence not eligible. We also assess whether the difference in the summary statistics by subgroup of individuals is statistically significant by reporting the p-value of the test of the null hypothesis that the difference is zero. The table shows from left to right summary statistics for three age groups with a decreasing age band around age 60 so as to compare similar individuals either just younger or just older than 60. The summary statistics in columns (1)-(3) show that 81% of individuals in the age range 50-59 were employed while 63% of those in the age range 60-69 were and the difference is statistically significant. The size and the significance of this difference is unchanged if we decrease the age band, as columns (4)-(9) show. Nevertheless, this difference may be driven by other factors than obtaining the transfer, as we also observe significant differences in the percentage of different ethnic groups and completed levels of education, urban residents, married individuals and number of children. However,

[5] Except individuals who are younger than 7, as they do not have to answer questions related to employment.

when we decrease the age band to compare individuals who are just above and just below the age 60 cutoff the differences in the baseline characteristics disappear, as columns (4) to (9) in the table show. This highlights the importance of assessing whether the correlation between the receipt of the cash transfer and employment has a causal interpretation.[6]

3 Research design

We define the outcome variable Y as a dummy that is equal to 1 if an individual is employed and 0 otherwise. We also define the dummy "receiving a cash transfer" as the treatment of interest. Our objective is to identify the effect of the treatment on individuals' employment. However, we cannot simultaneously observe an individual's employment condition in the event that the individual receives the transfer (treatment) and in the counterfactual event in which she does not receive it (control) due to the fundamental problem of causal inference. Hence, we exploit a policy change that makes the eligibility for the transfer depend on age A and it is discontinuous at a cutoff age $\bar{A}$ to identify the effect of being eligible for the transfer on employment thanks to a regression discontinuity design (RDD). We let the dummy D_A be equal to 1 for treated individuals and 0 otherwise. We estimate the effect as the difference in the mean probability of employment between the individuals who are 60 or barely older ($D_A = 1$) and those who are barely younger than 60 ($D_A = 0$).[7]

$$Y = \alpha + \beta D_A + f(A - \bar{A}) + U_1 \tag{1}$$

Our parameter of interest in equation (1) is β, that we interpret as the effect of the transfer on employment and $f(A - \bar{A})$ is the RDD polynomial in the difference between age A and the age cutoff $\bar{A}$ that captures the potential non-linear relationship between age and employment. The identifying assumption is that individuals on the left of the age cutoff $\bar{A}$ are similar to those on the right of it, for example, in their socio-economic background. In other words, whether an individual's birthday is earlier or later than the cutoff date to be eligible for the transfer can arguably be seen as a stochastic shock due to nature since an individual's day or month of birth cannot be manipulated or precisely forecasted. Although the identifying assumption is untestable, if the individuals who are barely younger or older than the age cutoff have similar baseline characteristics, being on either side of the age cutoff is due to chance,

[6] We provide further evidence on the balance of individuals' baseline characteristics in section 4.1.

[7] See Thistlethwaite and Campbell (1960) and Trochim (1984) for the early development of the RDD. See instead Imbens and Lemieux (2008) and Lee and Lemieux (2010) for a survey of the most recent the advances in the theory as well the recent increase in the number of applications of RDD in economics.

which supports the validity of the identifying assumption. In the empirical analysis in section 4.1 we discuss further the validity of the research design and also present evidence in support of the identifying assumption.

However, a public policy that was in place before and after the policy change may confound the RDD estimates of the effect of the transfer if the policy has the same eligibility rule. For example, the minimum retirement at age 60 for females in Bolivia confounds the effect of the transfer with the effect of the minimum retirement age policy. Hence, we tease out the effect of time-invariant policies that may confound the RDD estimates by jointly exploiting *i*) the discontinuous variation in the eligibility for the transfer at age $\bar{A} = 60$ and *ii*) the variation in time T around the year $\bar{T} = 2008$ in which the policy change occurred. We identify the effect of the transfer as the double difference in the probability of employment: the first is between eligibles for the transfer and non-eligibles and the second is over time before the policy change is introduced and after it, thanks to a difference-in-discontinuity design, hereafter diff-in-disc (Grembi *et al.*, 2011).

$$D = \begin{cases} 0 \text{ if } A < 60, T < 2008 \\ 0 \text{ if } A < 60, T \geq 2008 \\ 0 \text{ if } A \geq 60, T < 2008 \\ 1 \text{ if } A \geq 60, T \geq 2008 \end{cases} \tag{2}$$

We define the treatment indicator D as a dummy that is equal to 1 if an individual is 60 or older in 2008 or later and 0 otherwise, as equation 2 shows. We also let $D_T = I\{T \geq 2008\}$ be an indicator function that is equal to 1 if the calendar year is greater or equal to 2008 and 0 otherwise. Hence, we can re-write the treatment indicator as $D = D_T * D_A$. Finally, we define $A^* = A - 60$ as age rescaled to be zero at age 60.

$$Y = \sum_{k=0}^{p}(\delta_k A^{*k}) + D_A \sum_{k=0}^{p}(\gamma_k A^{*k}) + D_T\Big[\sum_{k=0}^{p}(\alpha_k A^{*k}) + D_A \sum_{k=0}^{p}(\beta_k A^{*k})\Big] + U \tag{3}$$

The coefficient β_0 of the interaction between the year cutoff and the age cutoff ($D_T * D_A$) in equations (3) is the diff-in-disc estimator that captures the effect of the transfer under the identifying assumptions that individuals are on the left of the age cutoff $\bar{A}$ rather than on the right of it by chance.

In the empirical analysis we estimate equations (1) and (3) by using a second order polynomial in the distance of an individual's age from the age 60 cutoff and also by fitting local linear regressions. We add individuals' baseline characteristics such as gender, ethnicity, education whether the household is in an urban area, marital status and health status to increase the

precision of the estimates. We also correct standard errors by using the sampling weights in the survey. We restrict the number of observations in the survey to those within an optimal bandwidth on either side of the age threshold, that we obtain thanks to the data-driven choice rule in Imbens and Kalyanaraman (2009). Finally, we assess whether the estimates are robust if we vary the size of the age bandwidth.

We estimate the effect as the difference in the estimated mean probability of employment between those individuals who were 60 or older at the time of the survey and those individuals who turned 60 after. Since not all individuals who are eligible for the transfer claim it, we use a fuzzy RDD and we interpret the estimates as an intention to treat effect. This is the product between the effect of being eligible for the transfer on the transfer take-up (first stage) and the effect of receiving the transfer on employment (structural equation). However, the extent of the fuzziness is low as approximately 90% of eligible individuals claim the transfer.

Nevertheless, individuals' responses to the transfer may vary with the characteristics and the labour market choices of the members within the same household. Hence, we also assess whether the effect of the transfer on the labour supply decision of an old-age worker in a household varies with that of the spouse *i*) by defining a household as the unit of observation and *ii*) by studying the effect of the cash transfer to both spouses in a household on whether both spouses are employed.

$$D_{HH} = \begin{cases} 1 \text{ if } A_F \geq 60, A_M \geq 60 \\ 0 \text{ otherwise} \end{cases} \tag{4}$$

$$Y_{HH} = \alpha_{HH} + \beta_{HH} D_{HH} + f_{HH}(A_F - \bar{A}, A_M - \bar{A}) + U_{HH} \tag{5}$$

We define the outcome Y_{HH} as dummy that is equal to 1 if both spouses are employed and zero otherwise. Similarly, we define the treatment indicator D_{HH} as a dummy that is equal to 1 if both spouses in the household are 60 or older and 0 otherwise, as equation (4) shows. $f_{HH}(\cdot)$ in equation (5) is the RDD polynomial in the distance of age of both the spouses (A_F^*, A_M^*) from the cutoff age $\bar{A}$. The research design is a RDD with two forcing variables, the age of each spouse, or a vector forcing variable (Imbens and Zajonc, 2011). Hence, the parameter β_{HH} measures the effect that both spouses in the household are 60 or barely older with respect to being barely younger on employment under the identifying assumption that the age of either spouse is as if randomised in a neighbourhood of the age cutoff. If β_{HH} is different from zero, the cash transfer program has an effect of the employment condition of both spouses. Conversely, if it is zero either the transfer has no effect on any spouse, or on one

of the two but not on both. We estimate the following cubic polynomial in both spouses' age rescaled at 60: $A_F^* + A_M^* + A_F^{*2} + A_M^{*2} + A_F^* A_M^* + A_F^{*3} + A_M^{*3} + A_F^{*2} A_M^* + A_F^* A_M^{*2}$. We also estimate a specification in which the running variable is the Euclidean distance in both spouses' age from 60, i.e. $\sqrt{(A_F^*)^2 + (A_M^*)^2}$, because the polynomials in the earlier specification may overfit the data if the bandwidth is small and the two specifications lead to similar results under fairly general conditions (Imbens and Zajonc, 2011). We add baseline characteristics to increase the precision of the estimates and we correct standard errors by using the sampling weights in the survey. We also assess the robustness of the estimates by using four different age bandwidths and by changing the order of the polynomial.

4 Results

In this section we firstly discuss the validity of the research design and the empirical evidence in support of it (section 4.1). Then we present the main results of the empirical analysis (section 4.2) and finally we illustrate two tests to assess the robustness of the results (section 4.3).

4.1 Validity of the research design

A research design to identify the effect of the cash transfer is valid if it mimics a controlled experiment in which the treatment consists in receiving the transfer and the assignment of individuals to the treated group or to the control group is random. Hence, we assess empirically whether, similarly to a controlled experiment, *i*) individuals' baseline characteristics, e.g. gender, are balanced at the age cutoff since eligibility for the transfer is discontinuous at the age 60 threshold and *ii*) the distribution of age is smooth at the cutoff.[8]

The value of individuals' baseline characteristics is predetermined at the time of the household survey in 2008, which we choose as the eligibility time cutoff to observe eligibles and non-eligibles in the survey data. Hence, if the day and month of birth is "as if" locally randomised around that date, the baseline characteristics of the individuals turning 60 on that date and of those who are barely younger are balanced at the age 60 cutoff. This holds if we consider each characteristic separately, e.g. the share of males, and also by considering all characteristics jointly. Otherwise, the effect of receiving the transfer is confounded by the correlation between, for example, gender and the eligibility status for the transfer, thus invalidating the randomised

[8]See Lee and Lemieux (2010) for a discussion of the validity of a RDD and for the approaches that are available to assess it empirically.

design at the age threshold. The top panel in table 2 shows RDD estimates of the difference between the treatment and control group in the mean value of each baseline characteristic. We obtain them by using the same specification as in equation (1) and by regressing a baseline characteristic on a dummy equal to 1 if an individual is 60 or older and the RDD polynomial in the difference between age and age 60. Small and not significant estimates of the difference in baseline characteristics suggest that they are balanced at the cutoff. The bottom line in the same table shows instead p-values to test the null hypothesis that the difference at a cutoff in the value of all baseline characteristics jointly is zero. We use a system of seemingly unrelated regressions with as many regressions as are the baseline characteristics. We regress a baseline characteristic on a dummy equal to 1 if an individual is 60 or older and the RDD polynomial in each regression. P-values different from zero in the table suggest that the baseline characteristics are jointly balanced. The results are unchanged if we vary the age bandwidth and use both local linear regressions and a second order polynomials.

Similarly, a randomisation of individuals to either side of the age cutoff 60 leads to a smooth distribution of age at the cutoff value. In contrast, a jump in the distribution at a cutoff suggests potential manipulation of age, that individuals self-report in the survey. We assess whether the distribution jumps discontinuously at the cutoff by plotting a histogram of age with age measured such that the integer part of a number counts years while the decimal part fractions of years. We use a bin width that is equal to 30 or 90 days to obtain histogram bins that contain an arbitrarily small number of individuals separately to the left and right of a cutoff and no bin contains the cutoff value. Visual inspection of the histograms in Figure 1 suggests no suspicious jumps in the height of the bins in the histogram at the cutoff, hence supporting the validity of the research design.

4.2 Main results

We plot the estimated probability of receiving the transfer as a function of the RDD polynomial in age in the top panel in Figure 2 by indicating the fitted values as solid lines and the 95% confidence interval as dashed lines. The figure confirms that eligibility for the transfer increases discontinuously at the age 60 cutoff and the estimated effect is statistically significant. We also plot the estimated probability of employment as a function of age in the bottom panel in the figure. We observe that the effect of the transfer on employment at the cutoff is negative but not statistically significant.

In addition to plots of the estimates, Table 3 shows RDD estimates of the effect of the cash

transfer on individuals' employment for the full sample in the years 2008-2009. We show estimates from local linear regression in columns (1)-(4) and from second order polynomials in the distance of age from the threshold and their interactions in columns (5)-(8). Columns (4) and (8) present the results for the optimal bandwidth according to the choice rule in Imbens and Kalyanaraman (2009) and we vary the age bandwidth to assess the sensitivity of the results in the remaining columns. The table shows that the estimated effect of the cash transfer on employment is negative and about 5-10 percentage points. However, the estimates lose statistical significance when we decrease the bandwidth to the optimal one.

In addition, we assess whether the effect of receiving the transfer on the probability of being employed varies if we estimate it by using subsamples of individuals who differ in the following baseline characteristics: gender and urban residence. The top panel in Table 4 shows the results for the subgroup of females by urban residence and the bottom panel for the subgroups of males. Both local linear regressions and second order polynomials show that the estimated probability of being employed decreases between 20 and 27 percentage points for females in urban areas, and it is highly statistically significant. The results are robust to different choices of the bandwidth. Conversely, the effect for the remaining groups of individuals is small and not statistically significant.

Table 5 shows instead diff-in-disc estimates of the effect of the transfer on employment, that we obtain by using observations in the years 2006-2009 to estimate equation (3). We present local linear regression in columns (1)-(4) and second order polynomial estimates in the distance of age from the threshold, their interactions and the time interactions in columns (5)-(8). The point estimates are in line with the results that we presented earlier. The top panel shows that the estimated effect of the transfer is negative but not significant as the bandwidth approaches the optimal one for the full sample. The rest of panels show that the effect is only significant for the subgroup of females in urban areas. Therefore, we can rule out the possibility that the results are driven by other public policies which were simultaneous in place before and after the policy change.

Finally, table 6 shows estimates of the effect of the cash transfer on whether both spouses in a household are employed, that we obtain thanks to a vector RDD. We present in the top panel estimates from both a cubic and a second order polynomial in the distance of age for both spouses from the cutoff age. We also show estimates from both a cubic and a second order polynomial in Euclidean distance of age from the cutoff in the bottom panel. Both

specifications show a small and not significant effect, thus suggesting that the labour supply decision of an old-age worker in a household is independent of the spouse's decision.

4.3 Robustness checks

First, we perform a set of placebo tests to evaluate whether the policy-relevant age cutoff at 60 is the only at which we observe a change in the probability of being employed. In Table 7 we replicate the baseline estimates at fake thresholds at age 55 and 65, as they are not policy relevant, and hence we do not expect significant estimates if the research design is valid. Small and not statistically significant estimates provide evidence in favour of the robustness of our results.

Second, we check whether 2008, the year in which the transfer program was introduced, is the only one in which we observe a change in the probability of being employed. We define as our treatment group the individuals who turned 60 in 2007, the year before the introduction of the transfer. The control group includes instead individuals who are younger than 60 in 2007 as well as all individuals in 2006. We use this definition of the treatment in estimating equation (3) by using data in the period 2006-2007. Table 8 shows that no estimate of the treatment effect is significant except one, thus suggesting that confounding factors that are invariant over time do not influence individuals' labour supply decisions at age 60.

5 Discussion

We study the effect of a cash transfer program on the labour supply decisions of elderly workers by using data on Bolivia. We identify the effect thanks to a regression discontinuity design since the eligibility rule for the transfer is discontinuous at age 60. We find that the transfer decreases the probability of being employed by 5-10 percentage points although it is not statistically significant. In addition, the probability of employment decreases by 20-44 percentage points for females in urban areas and it is highly significant, while it is not significant for the remaining groups.

Overall, the cash transfer program that the Bolivian government set up to help poor old-age individuals may mildly contribute to decrease labour market participation of elderly workers, as all the point estimates in the empirical analysis are different from zero and negative. However, the transfer is a small share of an individual's labour income and hence it may fail to induce elderly workers to leave the labour market on average, as the estimates are not sig-

nificant when we use a small age bandwidth. If we instead focus our attention on subgroups of individuals by gender and whether they live in an urban area, we observe that the labour supply response to the receipt of the transfer tends to vary by subgroup. The mechanism driving the heterogeneity across different groups of individuals in the response to the transfer may be explained by the heterogeneity in preferences over leisure and consumption across groups. Similarly, the institutional setting in the labour market in Bolivia, the structure of the economy and their interaction with individuals' preferences may help to explain why females in urban areas react to the policy more strongly than females in rural areas. However, we cannot tease out these channels in the empirical analysis without a structural model that carefully accounts for individuals' and households' choices.

In future research we plan to extend the paper by fully scrutinising two assumptions on which we rely in the empirical analysis. Firstly, we assume that measuring individuals' employment status a month after old-age workers have started to receive the transfer offers a policy-relevant outcome to assess the short-term effect of the transfer. On the one hand observing individuals' employment status at a later point in time with respect to the 60th birthday is preferable to assess the effect of the transfer. On the other it weakens the validity of the research design based on age discontinuities as the later the outcome is observed with respect to the time of the treatment, i.e. turning 60, the greater is the share of the treated individuals in a given year. In addition, the statistical design of the household survey in Bolivia is such that researchers cannot so far exploit repeated observations of the same individual over time. Secondly, we assume that changes in the composition of a household are negligible in the short-run. However, we plan to further assess this assumption by analysing the determinants of households' composition thanks to repeated observations of time-invariant and time-variant characteristics of a synthetic household that we obtain by matching information of different but similar households (Verbeek, 2008).

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Table 1: Summary statistics by individuals' eligibility status for the cash transfer at age 60 or older

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Age group 50-69 by eligibility status			Age group 55-64 by eligibility status			Age group 57-62 by eligibility status		
	No 50-59	Yes 60-69	P-val diff.	No 55-59	Yes 60-64	P-val diff.	No 57-59	Yes 60-62	P-val diff.
Employed (1 week before the survey)	0.81	0.63	0.00	0.79	0.66	0.00	0.80	0.66	0.00
Transfer take-up	0.00	0.83	0.00	0.01	0.76	0.00	0.01	0.71	0.00
Male	0.49	0.48	0.33	0.50	0.49	0.42	0.52	0.50	0.25
Quechua ethnicity	0.31	0.33	0.05	0.31	0.32	0.30	0.30	0.32	0.29
Aymara ethnicity	0.30	0.28	0.16	0.33	0.30	0.11	0.34	0.26	0.00
Other ethnicity	0.35	0.34	0.38	0.31	0.34	0.16	0.30	0.37	0.02
No education	0.45	0.59	0.00	0.50	0.56	0.01	0.51	0.55	0.13
Primary educ.n	0.18	0.15	0.01	0.19	0.16	0.04	0.19	0.14	0.04
Secondary educ.n	0.37	0.26	0.00	0.31	0.29	0.13	0.30	0.31	0.45
Urban residence	0.62	0.54	0.00	0.58	0.56	0.18	0.58	0.60	0.27
Poverty	0.45	0.45	0.40	0.45	0.46	0.29	0.44	0.44	0.42
Extreme poverty	0.25	0.20	0.00	0.25	0.22	0.04	0.25	0.20	0.04
Married	0.78	0.67	0.00	0.76	0.71	0.01	0.75	0.71	0.09
No. children	1.90	1.04	0.00	1.58	1.15	0.00	1.48	1.23	0.01
Ill last month	0.30	0.41	0.00	0.35	0.38	0.14	0.36	0.38	0.25
No. observations	2,395	1,648		1,096	933		648	553	

Note: The table shows summary statistics of the variables in a representative survey of households in Bolivia that was held in November 2008 and 2009. The summary statistics are reported separately for individuals who are younger than 60 at the time of the survey and are hence not eligible for the cash transfer and for those who are 60 or older and are hence eligible. Columns (1)-(2) show summary statistics for subsamples of individuals in the age group 50-59, and column (3) shows the p-value of null hypothesis that the difference in means between the group of eliglibles and non-eligibiles is equal to zero. Columns (4) to (9) show the same information as in the first three columns but for individuals in smaller age groups centered at age 60. The last row shows the number of observations. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis.

Table 2: Balance of individuals' baseline characteristics at the age 60 cutoff determining eligibility for the cash transfer

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local linear regression by age bandwidth				Second order polynomial by age bandwidth			
	20	10	5	3	20	10	5	3
Each baseline characteristic separately								
Male	-0.002	-0.017	-0.068	-0.045	-0.030	-0.036	-0.033	-0.037
S.e.	0.027	0.037	0.052	0.066	0.034	0.047	0.064	0.081
Quechua ethnicity	0.011	-0.006	0.015	0.049	0.027	0.028	0.073	0.079
S.e.	0.025	0.035	0.047	0.059	0.031	0.042	0.057	0.069
Aymara ethnicity	-0.030	-0.054	-0.080*	-0.003	-0.060**	-0.072*	-0.063	-0.041
S.e.	0.024	0.034	0.048	0.061	0.031	0.042	0.060	0.076
Other ethnicity	0.024	0.066*	0.079	-0.022	0.048	0.066	0.011	-0.030
S.e.	0.026	0.036	0.049	0.062	0.033	0.045	0.061	0.078
No education	0.012	0.008	0.044	0.103	0.017	-0.002	0.071	0.150*
S.e.	0.027	0.037	0.051	0.065	0.034	0.046	0.063	0.080
Primary educ.n	-0.012	-0.033	-0.043	-0.033	-0.034	-0.030	-0.046	-0.026
S.e.	0.020	0.027	0.038	0.047	0.025	0.034	0.045	0.055
Secondary educ.n	-0.000	0.026	-0.001	-0.070	0.017	0.032	-0.025	-0.123*
S.e.	0.025	0.034	0.046	0.058	0.031	0.042	0.057	0.073
Urban residence	-0.026	0.037	0.058	-0.012	0.008	0.058	-0.012	-0.063
S.e.	0.026	0.037	0.051	0.065	0.033	0.046	0.062	0.079
Poverty	0.009	0.001	-0.023	-0.030	0.011	-0.003	-0.070	-0.024
S.e.	0.027	0.037	0.052	0.066	0.034	0.047	0.064	0.081
Extreme poverty	-0.036	-0.044	-0.042	-0.006	-0.027	-0.024	-0.020	0.033
S.e.	0.022	0.031	0.044	0.057	0.028	0.039	0.055	0.069
Married	-0.038	-0.010	-0.024	-0.020	-0.023	-0.006	-0.039	0.011
S.e.	0.025	0.034	0.046	0.057	0.031	0.042	0.056	0.070
No. children	-0.296***	0.017	0.004	-0.046	-0.037	-0.026	-0.105	-0.096
S.e.	0.079	0.107	0.144	0.187	0.097	0.130	0.177	0.211
Ill last month	0.015	-0.037	0.015	-0.005	0.009	-0.022	-0.007	-0.013
S.e.	0.026	0.036	0.050	0.064	0.033	0.045	0.062	0.080
All baseline characteristics jointly								
P-value	0.067	0.949	0.877	0.859	0.893	0.997	0.949	0.615
χ^2 stat.	18.682	4.597	5.946	6.203	5.701	2.318	4.609	9.079
No. observations	8,055	4,043	2,029	1,201	8,055	4,043	2,029	1,201

Note: The table shows regression discontinuity design (RDD) estimates of the difference in the baseline characteristics between individuals who are 60 or barely older and those who are barely younger than 60 at the time of the survey. We use the full sample of individuals in the household survey in 2008 and 2009. The running variable is an individual's age: its integer part measures age in years while the decimal one measures fractions of years. The estimates in each row are obtained by regressing an individual's baseline characteristic on a dummy equal 1 if an individual was 60 or older at the time of the survey and 0 otherwise and on the RDD polynomial in the distance of age from the age threshold. We use both a local linear regression and a second order polynomial in age. We use the sample weights in the survey to correct standar errors and we vary the age bandwidth to assess the sensitivity of the estimates. In the top panel we evaluate separately whether each baseline characteristic is balanced. In the bottom panel we instead assess whether all baseline characteristics are jointly balanced. The p-values are obtained by *i*) regressing all baseline characteristics on the dummy indicating eligibility for the transfer (age 60 or older) and the RDD polynomial in a system of seemingly unrelated regressions and *ii*) testing the hypothesis that the dummy indicating eligibility is not significant in any regression. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all variables are in Table 1.

Table 3: Regression discontinuity estimates of the effect of the cash transfer program on employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local linear regression by age bandwidth				Second order polynomial by age bandwidth			
	20	10	5	3	20	10	5	3
	-0.104***	-0.099***	-0.101**	-0.052	-0.092***	-0.085	-0.046	-0.05
S.e.	0.022	0.031	0.043	0.056	0.028	0.039	0.055	0.070
F-stat. first stage	2034.11	578.88	176.14	89.29	2209.46	524.65	154.14	75.42
No. obs.	8,055	4,043	2,029	1,201	8,055	4,043	2,029	1,201

Note: The table shows regression discontinuity design (RDD) estimates of the effect of the cash transfer on employment for the full sample of individuals in the household survey in 2008 and 2009. The running variable is an individual's age: its integer part measures age in years while the decimal one measures fractions of years. The outcome variable is a dummy that is equal to 1 if an individual was employed the week before the household survey was held and 0 otherwise. The estimates are obtained by regressing the outcome on a dummy equal 1 if an individual was 60 or older at the time of the survey and 0 otherwise and on the RDD polynomial in the distance of age from the age threshold. We use both a local linear regression and a second order polynomial in age. We include in the regressions the following covariates: gender, ethnicity, education level, urban residence, poverty indicators, marital status and health status. We use the sample weights in the survey to correct the standard errors. We also vary the age bandwidth to assess the sensitivity of the estimates. We report the f-statistics from the first stage regression of the dummy indicating eligibility for the transfer on the the dummy indicating age 60 or older as the design is fuzzy. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all variables are in Table 1.

Table 4: Regression discontinuity estimates of the effect of the cash transfer on employment for subgroups by gender and urban residence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local linear regression by age bandwidth				Second order polynomial by age bandwidth			
	20	10	5	3	20	10	5	3
	Females in urban areas							
	-0.217***	-0.254***	-0.268***	-0.218*	-0.200***	-0.246***	-0.167	-0.267*
S.e.	0.048	0.069	0.096	0.123	0.061	0.085	0.116	0.153
F-stat. first stage	1113.46	290.44	92.77	30.87	1,192.60	273.40	257.33	25.64
No. obs.	2,404	1,134	535	312	2,404	1,134	535	312
	Females in rural areas							
	-0.137***	-0.100	-0.146	-0.152	-0.130**	0.131	-0.146	-0.162
S.e.	0.051	0.067	0.094	0.124	0.063	0.083	0.120	0.152
F-stat. first stage	589.51	237.36	75.39	27.38	668.81	225.17	66.70	23.83
No. obs.	1,781	959	481	278	1,781	959	481	278
	Males in urban areas							
	-0.084**	-0.060	0.030	0.101	-0.062	0.009	0.076	0.147
S.e.	0.041	0.054	0.076	0.106	0.052	0.070	0.102	0.132
F-stat. first stage	591.57	191.38	62.54	74.97	608.40	435.52	58.07	104.03
No. obs.	2,123	1,020	505	319	2,123	1,020	505	319
	Males in rural areas							
	0.055	0.064*	0.036	0.042	0.056	0.058	0.051	-0.004
S.e.	0.029	0.037	0.046	0.053	0.034	0.048	0.057	0.051
F-stat. first stage	398.94	123.10	38.63	16.03	527.97	109.55	34.73	14.16
No. obs.	1,747	930	508	292	1,747	930	508	292

Note: The table shows regression discontinuity design (RDD) estimates of the effect of the cash transfer on employment for subgroups of individuals' by gender and urban residence in the household survey in 2008 and 2009. The top panel shows the effect for females in urban areas and in rural ones and the bottom panel for males. The running variable is an individual's age: its integer part measures age in years while the decimal one measures fractions of years. The outcome variable is a dummy that is equal to 1 if an individual was employed the week before the household survey was held and 0 otherwise. The estimates are obtained by regressing the outcome on a dummy equal 1 if an individual was 60 or older and 0 otherwise and on the RDD polynomial in the distance of age from the age threshold. We use both a local linear regression and a second order polynomial in age. We include in the regressions the following covariates: gender, ethnicity, education level, urban residence, poverty indicators, marital status and health status. We use the sample weights in the survey to correct the standard errors. We also vary the age bandwidth to assess the sensitivity of the estimates. We report the f-statistics from the first stage regression of the dummy indicating eligibility for the transfer on the the dummy indicating age 60 or older as the design is fuzzy. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all the variables are in Table 1.

Table 5: Difference-in-discontinuity estimates of the effect of the cash transfer on employment for subgroups by gender and urban residence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local linear regression by age bandwidth				Second order polynomial by age bandwidth			
	20	10	5	3	20	10	5	3
				Full sample				
	-0.084**	-0.113**	-0.053	-0.007	-0.097**	-0.080	0.025	0.008
S.e.	0.037	0.051	0.071	0.089	0.047	0.064	0.087	0.109
No. obs.	15,919	7,781	3,780	2,222	15,919	7,781	3,780	2,222
				Females in urban areas				
	-0.221***	-0.328***	-0.439***	-0.372**	-0.231***	-0.364***	-0.276*	-0.387*
S.e.	0.068	0.097	0.136	0.174	0.087	0.120	0.166	0.214
No. obs.	5,091	2,378	1,105	649	5,091	2,378	1,105	649
				Females in rural areas				
	-0.219**	-0.275**	-0.146	-0.170	-0.287**	-0.241	-0.173	-0.241
S.e.	0.094	0.124	0.183	0.224	0.117	0.156	0.225	0.253
No. obs.	3,174	1,657	831	470	3,174	1,657	831	470
				Males in urban areas				
	0.076	0.079	0.226*	0.316**	0.103	0.130	0.301	0.439**
S.e.	0.066	0.091	0.127	0.156	0.084	0.115	0.154	0.197
No. obs.	4,597	2,148	1,028	641	4,597	2,148	1,028	641
				Males in rural areas				
	0.057	0.068	0.136	0.103	0.024	0.073	0.123	-0.099
S.e.	0.052	0.067	0.088	0.096	0.070	0.085	0.098	0.091
No. obs.	3,057	1,598	816	462	3,057	1,598	816	462

Note: The table shows estimates of the effect of the cash transfer on employment that tease out the effect of time-invariant policies that may confound the RDD estimates thanks to a difference-in-discontinuity design (Grembi *et al.*, 2011) and data on individuals in the household survey in 2006-2009. An example of such time-invariant confounders is the minimum retirement age at 60 for females in Bolivia. We identify the effect of the transfer by using a double difference in the probability of employment: the first is between eligibles for the transfer and non-eligibles and the second is over time before the policy change is introduced and after it. The top panel shows estimates for the full sample. The central panel shows the effect for females in urban areas and in rural ones and the bottom panel for males. The running variable is an individual's age: the integer part of the variable measures age in years while the decimal part measures fractions of years. The outcome variable is a dummy that is equal to 1 if an individual was employed the week before the household survey was held and 0 otherwise. The estimates are obtained by regressing the outcome on a dummy equal 1 if an individual was 60 or older at the time of the survey in 2008 or later and 0 otherwise and on the difference-in-discontinuities polynomial in the distance of age from the age threshold. We use both a local linear regression and a second order polynomial in age. We include in the regressions the following covariates: gender, ethnicity, education level, urban residence, poverty indicators, marital status and health status. We use the sample weights in the survey to correct the standard errors. We also vary the age bandwidth to assess the sensitivity of the estimates. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all the variables are in Table 1.

Table 6: Vector regression discontinuity estimates of the effect of the cash transfer to both spouses in a household on whether both spouses are employed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cubic polynomial by age bandwidth				Second order polynomial by age bandwidth			
	20	10	5	3	20	10	5	3
	Vector forcing variable							
	-0.120**	-0.085	-0.065	-0.127	-0.125**	-0.115	-0.072	-0.065
S.e.	0.060	0.084	0.137	0.205	0.052	0.075	0.118	0.172
	Euclidean distance in age from the age 60 cutoff							
	-0.150*	-0.082	-0.012	0.199	-0.126	-0.150	-0.183	0.039
S.e.	0.088	0.152	0.260	0.431	0.077	0.132	0.226	0.333
No. observations	2,488	1,046	381	175	2,488	1,046	381	175

Note: The table shows estimates of the effect of the cash transfer on employment by defining a household as the unit of observation and by exploiting a vector regression discontinuity design (RDD) to assess the effect of the cash transfer to both spouses in a household on whether they are employed (Imbens and Zajonc, 2011). We define the outcome as dummy that is equal to 1 if both spouses were employed the week before the household survey and zero otherwise. We define the treatment indicator as a dummy that is equal to 1 if both spouses in the household were 60 or older at the time of the survey and 0 otherwise. We regress the outcome on the treatment indicator and on the RDD polynomial in the distance between the age of both spouses from the cutoff age. The RDD has two forcing variables, the age of each spouse, or a vector forcing variable. In the top panel we estimate both a cubic and a second order polynomial in both spouses' age rescaled at 60 and in the bottom panel we estimate a specification in which the running variable is the Euclidean distance in both spouses' age from 60. We include in the regressions the following covariates: gender, ethnicity, education level, urban residence, poverty indicators, marital status and health status. We use the sample weights in the survey to correct the standard errors. We also vary the age bandwidth to assess the sensitivity of the estimates. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all the variables are in Table 1.

Table 7: Placebo test of the effect of the cash transfer on employment at irrelevant age thresholds

	Age 65 threshold								*Age 55 threshold*							
	Local linear regression by age bandwidth				Second order polynomial by age bandwidth				Local linear regression by age bandwidth				Second order polynomial by age bandwidth			
	20	10	5	3	20	10	5	3	20	10	5	3	20	10	5	3
								Full sample								
	-0.041	0.017	0.000	-0.012	0.001	0.031	-0.015	0.020	-0.007	-0.011	-0.058	-0.058	-0.010	-0.006	-0.061	-0.072
S.e.	0.027	0.037	0.052	0.068	0.034	0.046	0.066	0.084	0.019	0.025	0.036	0.046	0.023	0.032	0.044	0.056
No. obs.	6,618	3,189	1,648	1,008	6,618	3,189	1,648	1,008	9,485	4,877	2,395	1,428	9,485	4,877	2,395	1,428
								Females in urban areas								
	-0.048	0.090	-0.018	-0.057	0.048	0.111	-0.106	-0.175	-0.047	-0.024	-0.101	-0.129	-0.046	-0.002	-0.107	-0.142
S.e.	0.054	0.075	0.103	0.144	0.069	0.095	0.136	0.169	0.042	0.057	0.075	0.095	0.052	0.071	0.090	0.115
No. obs.	1,917	844	434	275	1,917	844	434	275	2,901	1,447	700	419	2,901	1,447	700	419
								Females in rural areas								
	-0.032	0.036	0.062	0.119	0.001	0.053	0.093	0.243	0.010	0.015	0.007	-0.003	0.030	0.017	-0.022	0.058
S.e.	0.061	0.086	0.119	0.148	0.077	0.107	0.146	0.174	0.045	0.062	0.083	0.110	0.054	0.078	0.106	0.142
No. obs.	1,530	787	421	253	1,530	787	421	253	2,019	1,069	538	324	2,019	1,069	538	324
								Males in urban areas								
	-0.112*	-0.009	-0.028	-0.122	-0.046	-0.014	-0.056	-0.091	0.045	0.054	0.045	0.033	0.057	0.042	-0.017	0.032
S.e.	0.057	0.084	0.119	0.148	0.073	0.105	0.149	0.180	0.029	0.037	0.056	0.067	0.036	0.048	0.068	0.093
No. obs.	1,690	745	357	202	1,690	745	357	202	2,636	1,326	663	383	2,636	1,326	663	383
								Males in rural areas								
	0.015	-0.048	0.008	0.047	-0.021	-0.037	0.008	0.021	-0.012	-0.047	-0.066	-0.075	-0.025	-0.064*	-0.066	-0.114**
S.e.	0.040	0.047	0.063	0.073	0.047	0.057	0.074	0.080	0.025	0.031	0.039	0.046	0.031	0.033	0.047	0.046
No. obs.	1,481	813	436	278	1,481	813	436	278	1,929	1,035	494	302	1,929	1,035	494	302

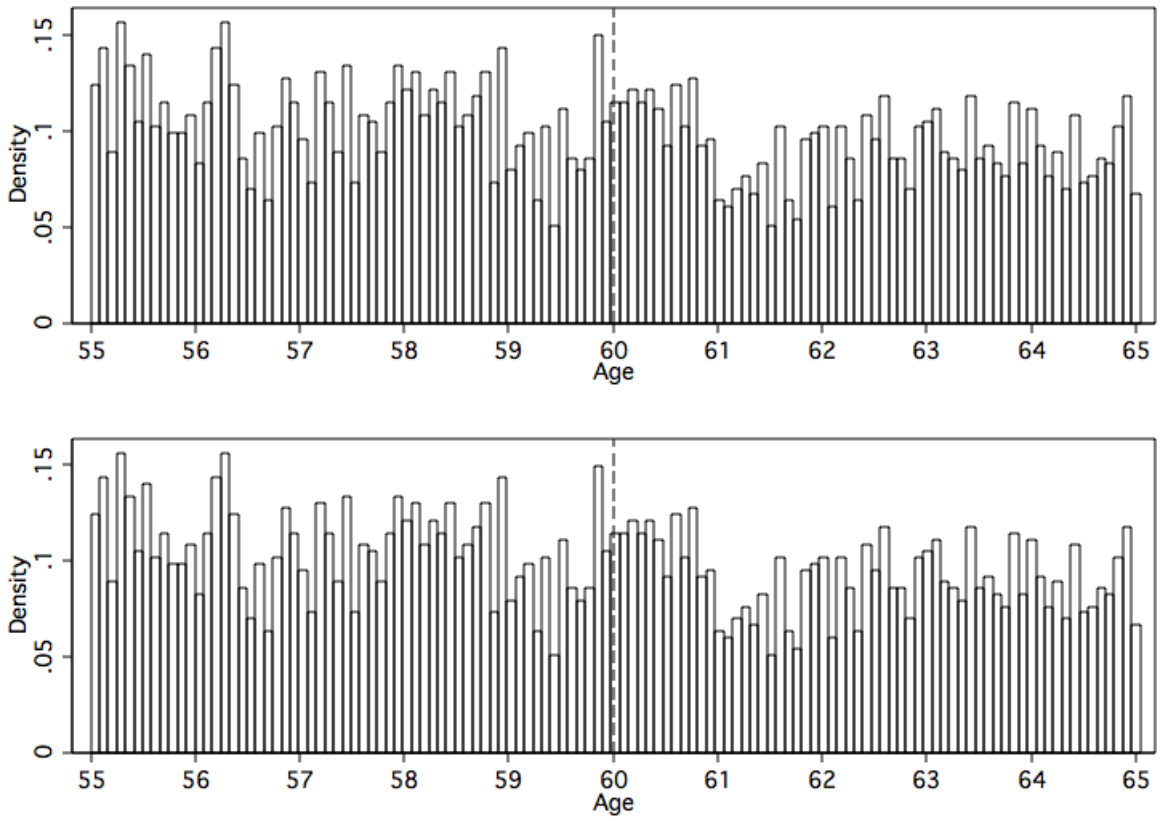
Note: The table shows regression discontinuity design (RDD) estimates of the effect of the cash transfer on employment at the age thresholds 65 and 55. They are not policy relevant as the threshold age to be eligible for the transfer is 60. Hence we do not expect significant estimates if the research design is valid. The top panel shows estimates for the full sample and the remaining panels for subgroups by gender and urban residence. The running variable is an individual's age: its integer part measures age in years while the decimal one as fractions of years. The outcome variable is a dummy that is equal to 1 if an individual was employed the week before the household survey was held and 0 otherwise. The estimates are obtained by regressing the outcome on a dummy equal 1 if an individual's age was greater or equal to the age threshold at the time of the survey and 0 otherwise and on the RDD polynomial in the distance of age from the age threshold. We use both a local linear regression and a second order polynomial in age. We include in the regressions the following covariates: gender, ethnicity, education level, urban residence, poverty indicators, marital status and health status. We use the sample weights in the survey to correct the standard errors. We also vary the age bandwidth to assess the sensitivity of the estimates. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all the variables are in Table 1.

Table 8: Falsification test of the effect of the cash transfer in the year before it was introduced

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local linear regression by age bandwidth				Second order polynomial by age bandwidth			
	20	10	5	3	20	10	5	3
				Full sample				
	-0.009	-0.059	-0.154	-0.207	-0.040	-0.106	-0.196	0.008
S.e.	0.058	0.080	0.111	0.138	0.075	0.100	0.133	0.166
No. observations	7,864	3,738	1,751	1,021	7,864	3,738	1,751	1,021
				Females in urban areas				
	0.095	-0.072	-0.094	-0.053	-0.019	-0.065	-0.065	0.029
S.e.	0.093	0.135	0.190	0.251	0.123	0.168	0.238	0.317
No. observations	2,687	1,244	570	337	2,687	1,244	570	337
				Females in rural areas				
	-0.095	-0.113	-0.292	-0.761**	0.010	-0.328	-0.268	-0.299
S.e.	0.149	0.197	0.269	0.324	0.187	0.240	0.326	0.424
No. observations	1,393	698	350	192	1,393	698	350	192
				Males in urban areas				
Urban subsample	0.009	-0.059	-0.041	-0.215	-0.077	-0.060	-0.307	0.049
S.e.	0.092	0.140	0.199	0.237	0.131	0.182	0.234	0.288
No. observations	2,474	1,128	523	322	2,474	1,128	523	322
				Males in rural areas				
Rural subsample	-0.030	-0.008	-0.126	0.013	-0.071	-0.034	-0.003	0.241
S.e.	0.083	0.114	0.155	0.142	0.118	0.143	0.169	0.150
No. observations	1,310	668	308	170	1,310	668	308	170

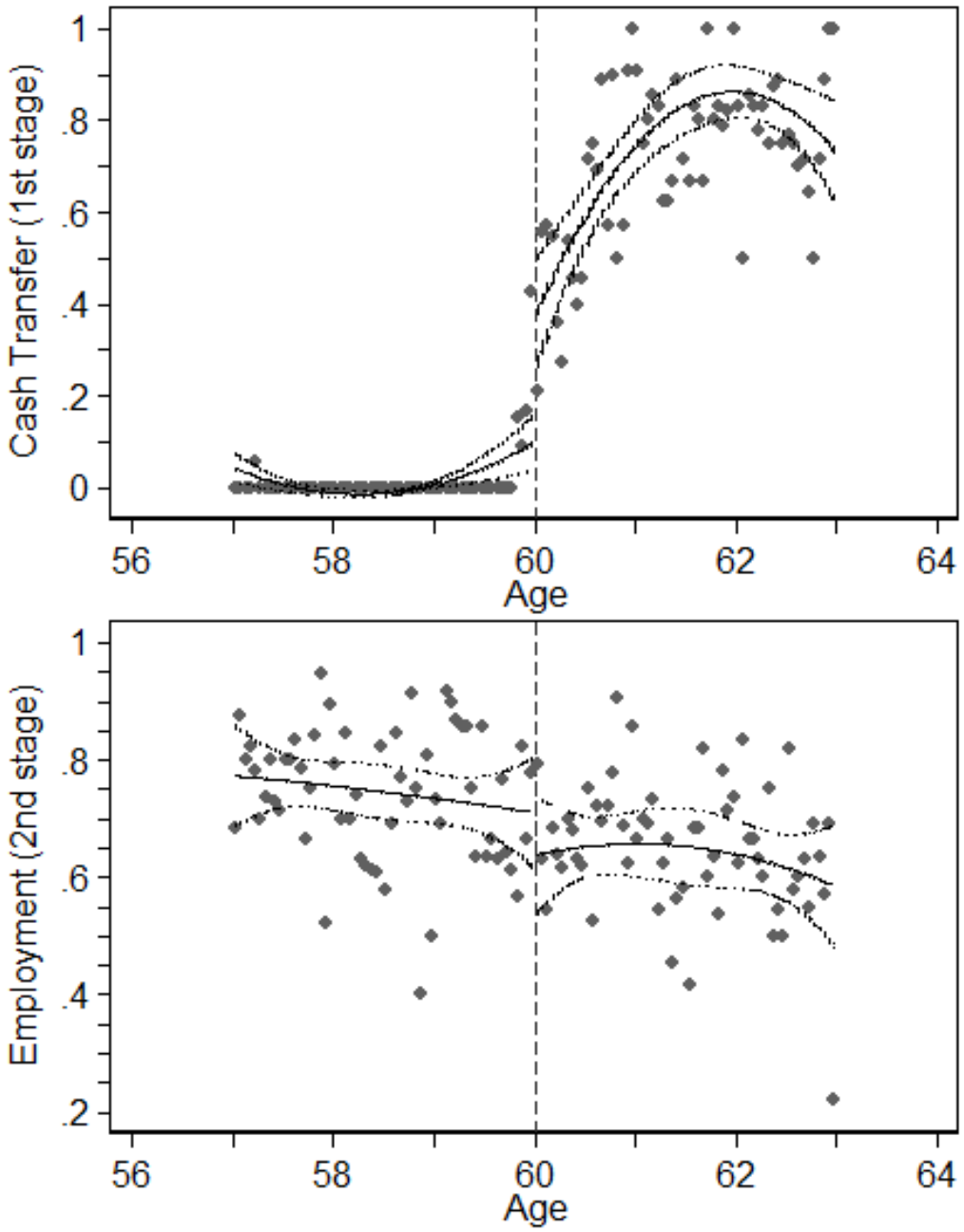
Note: The table shows estimates of the effect of the cash transfer on employment in the year before the cash transfer was introduced for the full sample and separately for subgroups by gender and urban residence. We use a difference-in-discontinuity design to tease out the effect of time-invariant policies that may confound the RDD estimates (Grembi *et al.*, 2011) and data on individual in the household survey in 2006-2007. An example of such time-invariant confounders is the minimum retirement age at 60 for females in Bolivia. We identify the effect of the transfer as the double difference in the probability of employment: the first is between eligibles for the transfer and non-eligibles and the second is over time before the policy change is introduced and after it. The running variable is an individual's age: the integer part of the variable measures age in years while the decimal part measures fractions of years. The outcome variable is a dummy that is equal to 1 if an individual was employed the week before the household survey was held and 0 otherwise. The estimates are obtained by regressing the outcome on a dummy equal 1 if an individual was 60 or older at the time of the survey in 2007 and 0 otherwise and on the difference-in-discontinuities polynomial in the distance of age from the age threshold. We use both local a linear regression and a second order polynomial in age. We include in the regressions the following covariates: gender, ethnicity, education level, urban residence, poverty indicators, marital status and health status. We use the sample weights in the survey to correct the standard errors. We also vary the bandwidth to assess the sensitivity of the estimates. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all the variables are in Table 1.

Figure 1: Histograms of individuals' age



Notes: The figure shows histograms of individuals' age, the integer part of which measures age in years while the decimal part measures fractions of years. The histogram in the top panel is obtained by using a bin width of 30 days, while in the bottom one the bin width is 90 days. If the distribution of age is smooth at the age 60 cutoff, it offers support to the validity of the regression discontinuity design to identify the effect of the cash transfer on employment (McCrary, 2008). Visual inspection of the size of the bins of histograms at the age 60 cutoff suggests that age is smooth at the cutoff. Section 2 offers additional information about the institutional setting and section 4.1 discusses the validity of the research design.

Figure 2: Plots of the first and second stage in a fuzzy regression discontinuity design of the effect of the cash transfer on employment



Note: The top panel in the figure shows plots of smoothed polynomials (lines) of the dummy indicating the eligibility for the transfer in the distance of age from the age 60 cutoff (vertical dotted line) and the estimated probability of receiving the transfer by age (dots). The bottom panel shows plots of smoothed polynomials of the dummy indicating whether an individual was employed (at the time of the survey) in age rescaled at 60 and the estimated probability of employment by age. We use the subgroup of individuals in the household surveys in 2008 and 2009 who is 57 or older and younger than 62 when the policy is enacted. The width of the bins to compute then mean values is 90 days. The solid lines indicate fitted values of second order polynomials and the dashed lines the 95% confidence intervals. The outcome variable is a dummy that is equal to 1 if an individual was employed the week before the household survey was held and 0 otherwise. The running variable is an individual's age: its integer part measures age in years while the decimal one as fractions of years. We use the sample weights in the survey to correct the standard errors. Section 2 offers additional information on the institutional setting and on the data, section 3 on the research design and section 4 on the empirical analysis. Summary statistics of all the variables are in Table 1.