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Non-Contributory Pensions Number-Gender Effects on Poverty and Household Decisions*

Miguel Angel Borella-Mas, Mariano Bosch and Marcello Sartarelli**

Abstract

Non-contributory pensions, designed to reduce old-age poverty particularly in countries with low contributory coverage, may induce a variety of household behavioural responses. This paper tests whether they vary with beneficiaries number and gender in Bolivia, one of the countries with the lowest contributory coverage worldwide. Taking advantage of a discontinuity in eligibility at age 60 in the Renta Dignidad pension, we estimate these effects by using a bi-dimensional regression discontinuity design, with spouses' age as forcing variables. We find that, despite increasing income, the impact on poverty is mixed and not significant. Although potentially puzzling, this is rationalised by household responses. When receiving two pensions, household size increases due to beneficiaries' adult children working in the household and to grandchildren. In addition, female labour supply decreases weakly. When receiving one, instead, transfers to other households increase only if the beneficiary is male. Our results suggest that variation in beneficiaries number and gender plays a relevant role in explaining pension positive spillovers to households with no elderly.

Keywords: Consumption, labour supply, living arrangements, poverty, regression discontinuity, Renta Dignidad, social pension.

JEL classification numbers: D13, H2, J22, J26.

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1 Introduction

Bismarckian pension systems, based on contributions during workers' lives to finance old age pensions, provide coverage to a small fraction of elderly in low and middle income countries, as only 45%, 35% and less than 10% of workers save for a pension in Latin America, Asia Pacific and Africa (World Bank, 2015). The majority, instead, are in informal and unregulated jobs due to several factors, such as lack of government effort in incentivising pension savings or social norms. As a response to this and in the face of rapidly changing demographics, 60 countries have been paying non-contributory or social pensions to elderly over the last 20 years, regardless of their social security contribution history, in an attempt to prevent old age poverty (HelpAge, 2015).[1]

Motivated by the fact that, absent any pension, elderly tend to work as long as their health allows them in poor countries, the literature focuses primarily on social pension beneficiaries, whose well-being tends to increase due to a combination of lower labour supply and greater consumption, in line with theoretical predictions.[2] The motivation to study pension indirect effects stems, instead, from the relevance of extended families in household decisions in developing countries, with evidence showing that non-negligible benefits accrue to non-beneficiaries.[3]

[1] In relatively young countries, like Bolivia, Brazil or Chile, expenditure for these programs absorbs more than 1% of GDP and, given the demographic transition, it is likely to increase (Bosch *et al.*, 2013).

[2] Pensions tend to decrease labour supply in Latin America (de Carvalho Filho, 2008; Bosch and Guajardo, 2012; Hernani-Limarino and Mena, 2015) and in China (Chen *et al.*, 2015) and also increase consumption in Latin America (Martinez, 2004; Escobar Loza *et al.*, 2013; Galiani *et al.*, 2014) and in South Africa (Case and Deaton, 1998). Similarly, they tend to reduce poverty in South Africa (Ardington and Lund, 1995; Case and Deaton, 1998). Evidence on their impact on mental health in Mexico and Peru is, instead, mixed (Galiani *et al.*, 2014; Novella and Olivera, 2014; Salinas-Rodríguez *et al.*, 2014). See for a broader assessment of pension policies in Latin America and the Caribbean region Mesa-Lago (2004); Rofman *et al.* (2008); Bosch and Manacorda (2012); Bosch *et al.* (2013); Acosta *et al.* (2014); Rofman *et al.* (2014) and in 60 developing countries Holzmann and Hinz (2005).

[3] See Cox and Fafchamps (2008) for a survey of evidence on household decisions in the presence of extended families in developing countries and of the theory rationalising it. Evidence on the pension impact on adult children's labour supply (Bertrand *et al.*, 2003; Kassouf and Rodrigues de Oliveira, 2012; Juarez and Pfutze, 2014) and on living arrangements is mixed as extra family members may enter beneficiaries' household or, in contrast, leave them (Edmonds *et al.*, 2005; Posel *et al.*, 2006; Hamoudi and Thomas, 2014). In addition, the impact is positive on children's health (Duflo, 2003) and school-

Household decisions may also vary depending on the number of pensions obtained and on beneficiaries' gender. However, theoretical predictions are ambiguous as they depend on whether spouses' preferences differ by gender and on bargaining over costly decisions that one or more pensions afford. For example, changes in basic food consumption may be proportional to the number of pensions. Decisions over co-residence of extended family members may, instead, more markedly depend on spouses' preferences, relative bargaining power and on whether income is high enough to guarantee all members basic needs. Since the literature has, to the best of our knowledge, overlooked the number of pension beneficiaries - gender relationship, we are the first to tease out these effects empirically.[4]

We jointly estimate the effects thanks to rich survey data on household members' income, consumption, labour supply, socio-demographics and take-up of *Renta Dignidad*. This is a non-contributory pension enacted in Bolivia in 2008 and paying citizens aged 60 or older 200 bolivianos monthly, about 25% of per capita income or 60 US dollars in PPP (Bosch *et al.*, 2013). We identify the effects thanks to an age discontinuity in eligibility at 60 and to between-spouse age variation in households since, for example, of all households eligible for at least a pension, 38% receive two. This gives rise to a bi-dimensional regression discontinuity design (RDD) with spouses' age as forcing variables. In addition, as take-up is about 60%, we estimate a fuzzy RDD by instrumenting take-up by gender with pension eligibility.

Although the main aim of the pension was poverty reduction among elderly, we find that neither the number of beneficiaries nor the gender effect is significant and both are mixed in sign: positive if using the poverty rate (34% relative to households with no pension) and negative if using

ing (Martinez, 2004; Edmonds, 2006; Yanez-Pagans, 2008) and tends to be driven by pensions to females. Finally, the impact on private transfers tends to be negative, i.e. a crowding out effect (Jensen, 2004; Juarez, 2009; Amuedo-Dorantes and Juarez, 2013).

[4] Bertrand *et al.* (2003) show evidence of a negative pension-gender effect on young adults' labour supply in South Africa while Yanez-Pagans (2008) of a positive one on children's human capital investment in Bolivia. In addition to surveying the literature on the determinants of extended families and their decisions, Cox and Fafchamps (2008) also offer an in-depth discussion of gender as a relevant although under-researched factor in household decisions over extended families. Related evidence on developing countries shows that household decisions depend on spouses' relative bargaining power (Rangel, 2006; Bobonis, 2009; Attanasio and Lechene, 2014).

the extreme poverty rate (71%). By itself, this result may put a negative light on the pension objective, design or implementation. In contrast, assessing it in the light, firstly, of our results showing that, by way of household decisions, a substantial part of pension benefits is transferred to other households and, secondly, of the low pension amount leads to a richer understanding of the policy and of the role played by the number of beneficiaries and their gender.

When only the husband receives the pension, transfers from other households decrease by 236 bolivianos (245%) and those sent to other households increase by 98 bolivianos (350%). In contrast, when both spouses receive it, this effect is small and no longer significant while the probability of absorbing additional family members increases: grandchildren by 51 percentage points, pp hereafter, (268%) and adult sons living and working in the household by 55 pp (130%). We also find a negative but weakly significant female labour supply effect (30 pp or 50%) when the male spouse or both spouses receive the pension, while men labour supply changes little. Finally, the probability of purchasing medicines increases by 65 pp (140%) when at least a pension is received although it is independent of beneficiaries' number or gender.[5]

Importantly for our research design validity, the distribution of spouses' age and baseline characteristics are continuous at the 60 cutoff, which rules out sorting by individuals in or out of the pension. In addition, falsification tests obtained estimating the age 60 effect before the pension was enacted, in 2006 and 2007, exclude that results are driven by a spurious demographic effect at 60. Finally, results are robust to varying the time elapsed between when the pension is first received and when outcomes are observed, the RDD bandwidth and also to dropping individuals eligible for the Bonosol pension, that preceded Renta Dignidad and whose cutoff age was 65.

Overall, this paper makes three contributions to the literature and to inform policy decisions. First, we are the first ones to jointly test the number of beneficiaries and gender effects of a social pension by exploiting an

[5]Results also hold when testing quantity and gender pension effects jointly on several outcomes, as shown in Appendix B. By, instead, failing to jointly account for the number of beneficiaries and gender effects we would have only detected an increase in transfers to other households and in the probability of purchasing medicines when one or more pensions is received, as shown in Appendices C and D.

age discontinuity and between-spouse age variation in households. Second, we document that, even in the short-run, these effects lead to substantive changes in household decisions that proxy a welfare increase, such as a decrease in beneficiaries' labour supply and an increase in employment opportunities for their adult children. By also considering the non-significant poverty reduction, results suggests that the poverty rate, by itself, gives a potentially misleading pension assessment to inform policy decisions, unless it is complemented by household behavioural responses.

Finally, we show that a non-negligible part of the pension benefits spills over to non-beneficiary households by way of transfers to them. A dollar worth of pension reduces transfers received by 1.18 and increases those sent by 0.5, leading to a net increase in transfers sent by 1.68, only if a male obtains the pension. This also suggests that gender is an extra potential mechanism to explain variation in interhousehold transfers in a growing literature studying their determinants (Angelucci *et al.*, 2012; Kinnan and Townsend, 2012; Angelucci and Di Maro, 2015).

The structure of the rest of the paper is as follows. Section 2 describes the institutional setting and the data. Section 3 illustrates the conceptual framework motivating the questions we set out to answer. Section 4 describes the empirical research design and section 5 the results. Section 6 discusses the research design validity. Finally, section 7 discusses results and concludes. Additional results are available in the Appendix.[6]

2 Institutional setting and data

In 1997, before Renta Dignidad, the Bolivian government enacted Bonosol, a pension paying all citizens who were 65 or older 1,300 bolivianos per year, or about 27% of per capita income and 85% of income for those living in extreme poverty (von Gersdorff, 1997). It was part of a broader social and economic reform agenda with two main aims. The first was reducing high income inequality in the country, in the top quartile of the distribution of countries worldwide if measured using the GINI index (CIA World Factbook, 2014). The second was dealing with the consequences of the high share of informal workers, about 60% (World Bank, 2009), who

[6] The Appendix is available online at https://goo.gl/vYUqhA .

are not entitled to a contributory pension since they did not pay social security.[7]

However, in 1998 Bonosol was not paid as it was judged financially untenable. In 2001, instead, the pension, renamed Bolivida, resumed although its amount decreased to about 420 bolivianos in 2001-2002 while it increased to 1,800 bolivianos in 2003. Finally, the pension was discontinued in December 2007.[8] Renta Dignidad was enacted on 1st February 2008 with two main differences with respect to previous pensions. First, the age eligibility cutoff decreased to 60. Second, the amount paid increased to 2,400 bolivianos per year, except individuals obtaining a contributory pension and public sector employees, 19% of all Renta Dignidad beneficiaries, who received 1,800 bolivianos.[9]

We estimate number and gender of beneficiaries effects by using two repeated cross-sections of survey data in the years 2008 and 2009 from the National Statistics Institute in Bolivia (Encuesta de Hogares). The unit of observation in the data is a household and information about its members is collected either at the individual or at the household level.[10] Our treatments of interest are whether at least a spouse in a household obtained the pension and her/his gender, with respect to a counterfactual household in which no spouse received it. The pension eligibility rule and variation in spouses' age across households around the 60 age eligibility cutoff, as Figure 1 shows, offer a quasi-experimental increase in non-labour income to estimate our treatment effects of interest.[11]

[7] Contributory pensions only provide coverage to around 10% of the elderly population (Bosch *et al.*, 2013). A related phenomenon to low pension coverage and poverty is observing that household size in Bolivia, greater than 4, is greater than in other Latin American countries (Jelin and Díaz-Muñoz, 2003).

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

[9] Pensions were first paid up to a month after turning 60, either at a bank or authorized military enclosure subject to identity verification. Alternatively, arrangements were in place to obtain it at home. Individuals obtaining 2,400 bolivianos, about 80% of beneficiaries, could choose either monthly payments or a less frequent ones while, for the others, the pension was added automatically to payments obtained on a monthly basis. Additional information is available in Escobar Loza *et al.* (2013).

[10] The data can be downloaded from `www.ine.gob.bo:8081/Webine10/enchogares1.aspx`.

[11] Summary statistics available upon request show that of all households eligible for at least a pension, in 41.4% only the male is eligible, in 20.7% only the female is and in 37.9 % both are.

We study pension *contemporaneous effects* by setting at end of September the date at which we observe if spouses were 60 or older and, hence, eligible for the pension. The reason is that survey interviews were held yearly in November and up to a month elapsed between the time at which an individual turning 60 submitted the application for the pension and the time the pension was paid, as illustrated in Figure 1. Hence, we define as eligible individuals those who were born on 30th September 1948 or earlier and, implicitly, the household where they live although the pension targets individuals. Non-eligibles are, instead, those who were born later.[12]

We use the following measures of *income*: the total in a household and its counterpart in per capita terms in bolivianos, the country's currency, earned over the 12 months before the survey. Over the same period we also measure transfers received from or sent to other households in Bolivia.[13] In addition, we use two *poverty* measures obtained by setting a dummy equal to 1 if total income per capita is below the poverty threshold or the extreme poverty one.[14] *Food and non-food consumption* in a household are measured in bolivianos over the month before the survey was held. Medicines and medical services, instead, are measured as a dummy equal to 1 if their consumption is positive and 0 otherwise as their distribution peaks at zero.[15]

We measure *labour supply* for each adult member in a household, including elderly spouses and their adult children, thanks to a dummy set

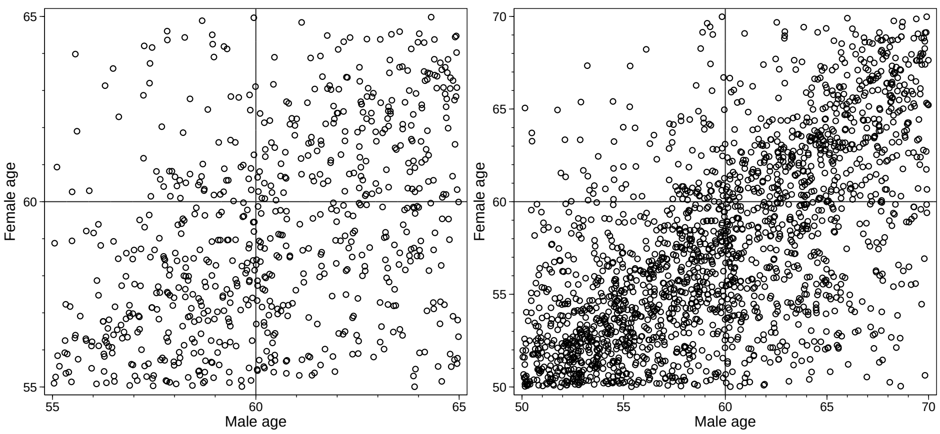
[12]In addition, as a robustness check, we also study pension *three months effects* by setting the end of June as the date in which we observe if one or more spouses was 60 or older.

[13]Interhousehold transfers exclude transfers received from abroad, i.e. remittances, or sent abroad, as they are measured separately in the survey.

[14]The National Statistics Institute in Bolivia set the poverty threshold at 418 bolivianos of total household income per capita and the extreme poverty one at 261 to obtain the poverty dummies in the survey data for 2008 and 2009. Threshold values were defined based on the cost of basic food and non-food consumption needs. Additional details about the survey design are available in `www.ine.gob.bo/pdf/EH/EH_2011.pdf`.

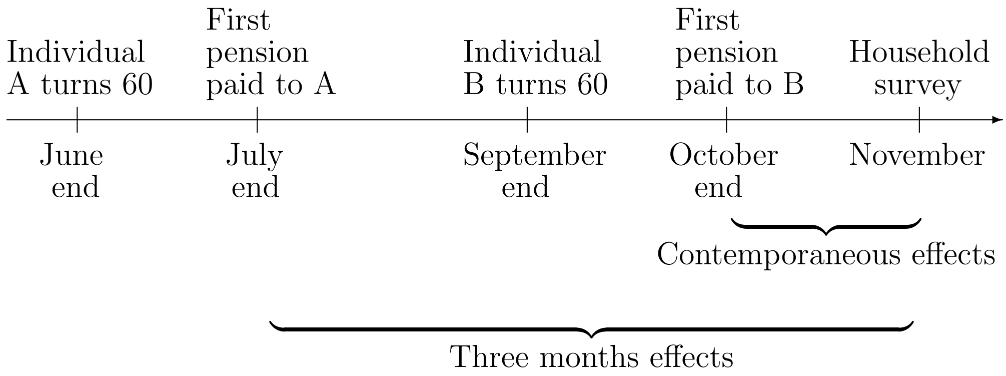
[15]Total food consumption is measured in *adult equivalent terms*. In particular, we use the OECD scale and divide food consumption by $(1+0.7*(N_{adults}-1)+0.5*N_{children})$. See chapter 2 in Haughton and Khandker (2009) for additional details.

Figure 1: Variation in spouses' age in a household



Note: The figure shows scatter plots of the variation in spouses age around the age 60 cutoff by gender. Aage is measured at the end of September to estimate pension *contemporaneous effects* since a month can elapse between the 60th birthday and the first pension payment due to administrative reasons and our outcomes of interest are measured in November. We use two different age intervals around the 60 cutoff: 5 and 10 years, shown on the left- and right-hand side in the figure respectively. We measured age as follows: the integer part measures age in years while the decimal one measures fractions of years. Data are from a representative survey of Bolivian households held by the National Statistics Institute in 2008 and 2009. Section 2 offers additional information on the institutional setting and on the data, section 4 on the research design and section 5 on the empirical analysis.

Figure 2: Illustration of the time at which pension eligibility and outcomes are measured



Note: The timeline shows that pension *contemporaneous effects* are defined by setting the end of September as the date in which we observe if spouses were 60 or older and hence eligible for a pension since a month can elapse between the 60th birthday and the first pension payment due to administrative reasons and we observe our outcomes of interest in November in a yearly survey of Bolivian households. In addition, we study *three months effects* by setting the end of June 2008 as the date in which we observe if one or more spouses was 60 or older. To do so, we also drop observations of individuals turning 60 between June and September as by the time outcomes are observed in the household survey in November these individuals have received the pension for less than three months. Section 2 offers additional information on the institutional setting and on the data.

equal to 1 in the survey data if a member worked over the week before the survey interview and 0 otherwise. In addition, we define a dummy to capture whether at least a spouse works and one to capture if they both work. Finally, we measure *living arrangements* in a household with an elderly couple by setting a dummy equal to 1 if adult sons or daughters live with in their elderly parents, a dummy to capture whether they work and, in addition, a dummy to capture whether elderly's grandchildren live in the household.

Table 1 shows summary statistics of our outcomes of interest and of baseline characteristics for households with spouses in a 40-80 age range but separately by whether they obtain at least a pension. The left-hand side panel shows means separately by whether both spouses in a household were 60 or older, hence eligible for two pensions. We also assessed whether differences between subgroups are significant and report the p-value of the null hypothesis that they are zero. The right-hand side panel, instead, shows means for the subgroup of households in which at least a spouse was 60.

The table shows that the difference between households with eligible individuals and others in pension take-up is positive and highly significant, as shown by low p-values in the third column in both panels. The difference varies between 60% and 90% depending on the number of pensions received, with zero take-up for non-eligibles. The poverty rate decreases in households with eligible individuals by about 10% and the extreme poverty one by 30%, although only the latter change is significant. Differences in income show a significant decrease in total income by about 25% for households with at least a pension while it is small and no longer significant in per capita terms.

When we move to difference in individual and household decisions, we find that beneficiaries' labour supply decreases, with a greater magnitude when two pensions are received. We also find that consumption of food, in monetary terms, and of medicines, measured as a dummy equal to 1 if medicines are purchased, increases significantly by 20-30% and 100% respectively. Finally, when we look at whether extended family members,

Table 1: Summary statistics separately by whether one or more spouses in a household were eligible for a pension

	Age band 40-80			Age band 40-80		
	Both 60-80	Both 40-60	p-val	At least one 60-80	Both 40-60	p-val
Pension take-up and poverty (0/1)						
Pension female	0.87	0.00	0.00	0.57	0.00	0.00
Pension male	0.91	0.00	0.00	0.80	0.00	0.00
1+ pension	0.94	0.00	0.00	0.87	0.00	0.00
2 pensions	0.84	0.00	0.00	0.50	0.00	0.00
Poverty	0.46	0.50	0.27	0.47	0.50	0.28
Extreme poverty	0.20	0.29	0.00	0.22	0.29	0.00
Household income (bolivianos)						
Total income	2864.74	3767.82	0.00	3046.38	3767.82	0.00
Total income per capita	837.99	837.21	0.99	849.41	837.21	0.83
Labour supply (0/1)						
Female works	0.45	0.65	0.00	0.52	0.65	0.00
Male works	0.69	0.94	0.00	0.74	0.94	0.00
1+ works	0.76	0.97	0.00	0.82	0.97	0.00
Both work	0.39	0.62	0.00	0.44	0.62	0.00
Food, non-food (bolivianos) and medical (0/1) consumption						
Total food consumption	342.36	277.87	0.00	330.58	277.87	0.00
Total non-food consumption	2248.43	4130.26	0.33	3154.36	4130.26	0.64
Medical serv. purchased	0.42	0.36	0.04	0.44	0.36	0.00
Medicines purchased	0.53	0.45	0.01	0.51	0.45	0.03
Living arrangements in elderly spouses household (0/1)						
Adult children	0.45	0.90	0.00	0.54	0.90	0.00
Adult male children work	0.55	0.40	0.00	0.52	0.40	0.00
Adult female children work	0.44	0.28	0.00	0.37	0.28	0.00
Grandchildren	0.32	0.14	0.00	0.29	0.14	0.00
Baseline characteristics (0/1)						
Survey year 2009	0.51	0.50	0.74	0.52	0.50	0.41
Urban residence	0.48	0.63	0.00	0.52	0.63	0.00
Educ. $\geq$ primary (5+ yy) male	0.43	0.71	0.00	0.48	0.71	0.00
Educ. $\geq$ primary (5+ yy) female	0.29	0.54	0.00	0.34	0.54	0.00
Educ. $\geq$ junior secondary (8+ yy) male	0.24	0.49	0.00	0.29	0.49	0.00
Educ. $\geq$ junior secondary (8+ yy) female	0.18	0.36	0.00	0.22	0.36	0.00
Educ. $\geq$ senior secondary (12+ yy) male	0.18	0.32	0.00	0.23	0.32	0.00
Educ. $\geq$ senior secondary (12+ yy) female	0.14	0.25	0.00	0.16	0.25	0.00
Quechua ethnicity male	0.35	0.33	0.33	0.34	0.33	0.58
Quechua ethnicity female	0.34	0.32	0.50	0.33	0.32	0.68
Aymara ethnicity male	0.27	0.29	0.59	0.30	0.29	0.51
Aymara ethnicity female	0.28	0.28	0.95	0.30	0.28	0.39
Sick previous month male	0.39	0.22	0.00	0.37	0.22	0.00
Sick previous month female	0.42	0.29	0.00	0.40	0.29	0.00
Health insurance male	0.76	0.63	0.00	0.74	0.63	0.00
Health insurance female	0.76	0.64	0.00	0.72	0.64	0.00
Contributory pension male	0.24	0.02	0.00	0.22	0.02	0.00
Contributory pension female	0.09	0.01	0.00	0.06	0.01	0.00
Extra Renta Dignidad pension	0.02	0.03	0.17	0.02	0.03	0.41
Observations	521	1,606	2,127	890	1,606	2,496

Note: The table shows in the panel on the left-hand side means for households with spouses in the age range 40-80 separately by whether both were 60-80. The panel on the right-hand side shows the same information except by whether at least one spouse was 60-80. We observed age at the end of September to study the pension *contemporaneous effects*, since our outcomes of interest are measured in November. We measured age as follows: the integer part measures age in years while the decimal one measures fractions of years. Data are from a representative survey of Bolivian households held by the National Statistics Institute in 2008 and 2009. Section 2 offers additional information on the institutional setting and on the data, section 4 on the research design and section 5 on the empirical analysis.

we find that the probability that adult children live with their elderly parents is lower by 40-50% in a household receiving one or more pensions, In contrast, the probability that adult children, females or males, live and work in the house is higher by about 25%, similarly to the probability that grandchildren live in the house, that doubles in magnitude.

The differences observed between households with eligible individuals and others in our outcomes of interest may be driven by observable factors as we observe, for example, that measures of completed education are higher in eligible households in the bottom panel of the table and, also to unobservable factors.This highlights the importance of assessing whether the correlation between pension eligibility and our outcomes of interest has a causal interpretation.

3 Conceptual framework

Renta Dignidad non-contributory pension in Bolivia consists in a monthly and life-long payment to all individuals who are 60 or older. Since the age condition is the only one an individual needs to fulfill to receive the pension, its recipients can afford greater consumption bundles than those not obtaining it, i.e. a pure income effect. Its size in a household with two elderly spouses may depend on the number of pensions received, that varies from zero to two. In addition, when one pension is received in a household, the beneficiary may be either the female or male spouse.

In a decision between leisure and consumption of a normal good, individuals experiencing a positive income shock can enjoy either the same consumption level as those not experiencing it but working less or higher consumption with no change in labour supply, or all combinations in between these two extreme cases. Evidence on beneficiaries' consumption and labour supply choices is in line with these predictions, with the magnitude depending on pension size. While the consumption response to an income shock tends to be positive, it may vary substantially by type of good or service. For example, non-durables tend to be elastic to income shocks in developing countries (Regmi *et al.*, 2001; Abler, 2010). As for poor households, the pension income shock may, in the presence of credit constraints limiting access to basic food or medicines, increase its consumption as soon as first pension payments are received.

Evidence on living arrangements shows that they respond to pension income shocks in order to improve, in addition to beneficiaries' living conditions, also that of their adult children and grandchildren indirectly, as extended families are frequently observed in developing countries. This can be rationalised as, on the one hand, beneficiaries are altruist towards other family members, whose improvement in living conditions increases beneficiaries' utility in Becker (1974) and the more recent research that this intuition induced. On the other, beneficiaries may rationally help younger members of their extended family in exchange for help when in need of assistance. This increases beneficiaries' utility thanks to an improvement primarily in own living conditions. In addition, beneficiaries may derive (non-)material benefits from strategically inducing to implement their decisions other family members, as it is shown in the case of bequests in Bernheim *et al.* (1985).

More recent research has focused on a family network, to expand the definition of extended family by including non-family members who are linked, by way of friendship, risk-sharing agreements or similar ties, to a program beneficiaries and may indirectly benefit from it. Evidence from developing countries shows that aid programs, be it conditional cash transfers or social pensions, generate positive spillover effects on households with no beneficiary (see for a survey Angelucci and Di Maro, 2015). Among several mechanisms to rationalise this evidence, one gaining increasing attention in the literature is the existence of informal agreements among households engaged in a long-term relationship, broadly defined as contracts with limited commitment and enforceability (Albarran and Attanasio, 2003; Dubois *et al.*, 2008). Related evidence from PROGRESA in Mexico shows that it has positive spillovers on consumption and investment decisions of non-eligible households (Angelucci and De Giorgi, 2009; Angelucci *et al.*, 2012).

In this paper we argue that, firstly, in a household with elderly spouses their welfare, proxied for example by whether household income is above the poverty line, may vary depending on the number of pensions they obtain and on the decisions that the pension income may induce. Assuming that spouses in a household have the same preferences over the allocation of the pension income, the size of the income effect for a normal and divisible good increases with the number of pensions. However, decisions over living arrangements, such as hosting a grandchild, are discrete in nature and may be taken, for example, only if both spouses obtain a pension.

Secondly, we argue that household behavioural responses to one or more pensions may depend on beneficiaries' gender. If the pension is received only by a spouse in a household, own labour supply may decrease. If, instead, both spouses receive a pension, labour supply may decrease for females more than for males in the presence of gender differences in labour supply elasticity, that can be driven by gender roles or productivity differences in field work in developing countries.[16] In addition, in both cases an individual's labour supply may also change thanks to the indirect effect of the spouse's pension and, for example, decrease in the presence of complementarity in spouses' leisure time. However, female labour supply is more likely to be influenced by male pension than vice versa, following the aforementioned argument on prevalent gender roles in developing countries.

Similar motives may drive changes in living arrangements. However, in such decisions, over which preferences by gender may differ markedly, prevalent gender roles in developing countries tend to shift decision power in the hands of the male spouse. Its magnitude may increase if the pension is received by the male only or, vice versa, decrease or even change sign if it is received by the female only, as related evidence on spouses' relative bargaining power shows (Rangel, 2006; Bobonis, 2009; Attanasio and Lechene, 2014).

While the foundations to understand intrahousehold bargaining between spouses were laid out in economics in the late 1980s (Chiappori, 1988, 1992), the impact of gender roles or differences is less clear-cut. This gap is highlighted in a survey of studies on the extended family in Cox and Fafchamps (2008) who suggest, as a possibility to fill it, testing predictions from evolutionary biology. For example, according to Hamilton (1964a,b) elderly may contribute to improve their adult children's employment opportunities and their grandchildren's education in an attempt to preserve a family's genes over time. In addition, their preferences over, for example, grandchildren's education may vary by elderly gender.

[16] Gender differences in labour supply elasticity are observed in developed countries (Blau and Kahn, 2007) and are rationalised theoretically using arguments based on gender roles (Albanesi and Olivetti, 2007).

Finally, a pension may jointly affect more than one household decision at the same time. The pure income effect induced by one or more pensions in a household may, for example, reduce poverty without modifying household size. In contrast, it may have no impact on poverty if household size increases, since pension benefits are shared with extended family members, or even reduce poverty if new living arrangements are more efficient. In addition, the pension may induce a joint effect on poverty and on living arrangements. While the implicit assumption in the design of non-contributory pensions is a mechanical poverty reduction ceteris paribus, it may not hold if household decisions affect extended family members.[17]

4 Research design

In this section we describe the empirical research design that we use to estimate whether the positive income shock induced by the Renta Dignidad pension and varying in beneficiaries' number and gender across households, influences their decisions and living conditions.

4.1 Regression discontinuity design

A linear regression of an outcome Y on a dummy P equal to 1 if an individual obtained the pension and 0 otherwise leads to spurious estimates if individuals' unobservable characteristics correlate with whether they receive a pension and how long they have received it for. We can, instead, identify the pension effect locally at age 60 thanks to a regression discontinuity design (RDD) that exploits the jump in pension eligibility in individuals' age A at 60.

However, not all eligible individuals receive the pension as they must apply for it. By considering the pension as the treatment, comparing out-

[17] In the related literature, the prevalent presumption was that consumption smoothing to a negative income shock is desirable from a welfare viewpoint (Townsend, 1994; Morduch, 1995). However, this is challenged theoretically by Chetty and Looney (2006) who point out that smoothing in the event of a negative shock may hide responses making a household worse-off in the future, for example if children are taken away from school to help with field work. Related empirical evidence from Thai households shows that the presence of kins in a household makes its decisions less dependent on interhousehold transfers thanks to the income kins can generate either through self-production or in the labour market (Kinnan and Townsend, 2012).

comes of eligible and non-eligible individuals close to the 60 cutoff under imperfect compliance with the treatment identifies the average effect of assignment into treatment or intention to treat effect (ITT) at the cutoff. With incomplete take-up, the ITT effect is lower in absolute value than the pension effect on those who actually receive it, the average treatment effect on the treated (ATT). This is why we estimate the pension effect by instrumenting pension receipt P with a pension eligibility dummy D equal to 1 if an individual is 60 or older and 0 otherwise in a "fuzzy" RDD (Hahn *et al.*, 2001; Imbens and Lemieux, 2008).[18]

The pension effect can be identified at the cutoff by dividing the "jump" in the relationship between pension eligibility and the outcome, i.e. the ITT, by the fraction of individuals actually obtaining the pension, as shown in equation (1).[19] This is the Local Average Treatment Effect (LATE) estimator, i.e. the ratio between the ITT or second stage in an instrumental variable setting, and the fraction of individuals taking the treatment among those eligible, or first stage, locally at the cutoff.

$$\beta = \frac{\lim_{\epsilon \downarrow 0} E[Y \mid A = 60 + \epsilon] - \lim_{\epsilon \uparrow 0} E[Y \mid A = 60 + \epsilon]}{\lim_{\epsilon \downarrow 0} E[P \mid A = 60 + \epsilon] - \lim_{\epsilon \uparrow 0} E[P \mid A = 60 + \epsilon]} \tag{1}$$

Receiving a pension may affect decisions taken by an individual, such as labour supply and consumption. However, in a household such decisions may also be influenced by an individual's spouse and may vary depending on whether no spouse, one or both spouses receive a pension and on the beneficiary's gender. Hence, to study the pension effect on decisions taken at the household level, we modify the research design defined in equation (1) as follows. Let A_F measure female spouse age in a household, the indicator $D_F = \{A_F \geq 60\}$ whether she is eligible for the pension and P_F be equal to 1 if she actually obtains it and 0 otherwise. We also define the same variables for the male (M) spouse in a household.

[18] Although not all individuals who were eligible for the pension claimed it, the extent of the fuzziness is low since more than 80% of those eligible in the age range 60-80 claimed the pension, as Table 1 shows, or about 60% when we take into account observable characteristics, as Table 2 shows.

[19] Hahn *et al.* (2001) proved the relationship between a treatment effect in an instrumental variables setting and in a fuzzy RDD.

$$\begin{aligned} Y_{hh} &= \beta_0 + \beta_1 P_M + \beta_2 P_F + \beta_3 P_M P_F + \\ &\quad + f(A_F - 60, A_M - 60, D_F, D_M) + U_1 \end{aligned} \tag{2}$$

$$\begin{aligned} P_i &= \delta_{0_i} + \delta_{1_i} D_M + \delta_{2_i} D_F + \delta_{3_i} D_M D_F + \\ &\quad + g_i(A_F - 60, A_M - 60, D_F, D_M) + U_{2,i} \quad , i = F, M \end{aligned} \tag{3}$$

The system in equations (2)-(3) shows the empirical specification that we use to estimate pension number of beneficiaries and gender effects. β_1 and β_2 in equation (2) capture the effect that only a spouse, male and female respectively, obtains the pension on Y_{hh}. $\beta_1+\beta_2+\beta_3$, instead, capture the effect that both spouses obtain a pension. Discontinuities in pension eligibility by spouse in their age at 60 gives rise to a bi-dimensional RDD with age by spouse rescaled at 60 as two forcing variables, that identifies our effects of interest locally at 60. Equation (3) shows, instead, the relationship between take-up for spouse i, own pension eligibility and spouse eligibility dummies, captured by δ_1 and δ_2, in a fuzzy RDD as take-up is incomplete.[20]

$$\begin{aligned} f(\cdot) &= \beta_4(A_M - 60) + \beta_5(A_F - 60) + \beta_6(A_M - 60)(A_F - 60) + \\ &\quad + \beta_7 D_M(A_M - 60) + \beta_8 D_F(A_F - 60) + \beta_9 D_M(A_F - 60) + \\ &\quad + \beta_{10} D_F(A_M - 60) + \beta_{11} D_M(A_M - 60)(A_F - 60) + \\ &\quad + \beta_{12} D_F(A_M - 60)(A_F - 60) + \beta_{13} D_F D_M(A_M - 60) + \\ &\quad + \beta_{14} D_F D_M(A_F - 60) + \beta_{15} D_F D_M(A_M - 60)(A_F - 60) \end{aligned} \tag{4}$$

The RDD polynomial $f(\cdot)$ in spouses' age rescaled at 60 used in equation (2) is defined in equation (4). Parameters $\beta_5 - \beta_8$ capture trends in husbands' and wives' own age and are allowed to differ by whether age is 60 or greater while $\beta_9 - \beta_{15}$ capture gender differences in age trends.

[20]We are the first ones in studying social pension beneficiaries number and gender effects thanks to a bi-dimensional RDD. In other fields, it has been used to assess the impact of test scores (Papay *et al.*, 2011), of taxation (Egger and Wamser, 2015) and of competition in the airline industry (Snider and Williams, 2014). Additional information about theory on multi-dimensional RDD is available in Zajonc (2012) and Bertanha (2014).

Polynomial $g(\cdot)$ in equation (3) is defined analogously.

The RDD *identifying assumption* is absence or imperfect sorting by individuals on either side of the age 60 cutoff. In other words, whether an individual is barely younger or older than 60, for example in February 2008 when the pension was enacted, is a stochastic shock due to nature. Although the identifying assumption is untestable, date of birth and, hence, age in a small neighbourhood of the 60 cutoff is arguably exogenous. We preview that the empirical distribution of age by gender is continuous at the 60 cutoff, thus offering support to our research design validity. In section 6.1, we will carefully discuss additional evidence in support of it, by also assessing whether individuals' age and baseline characteristics are continuous at the 60 cutoff.[21]

4.2 Estimation

We use rich individual-level data on Bolivian households including the day, month and year of birth to estimate equations (2)-(3). By using a local linear regression in spouses' age rescaled at the 60 cutoff and Two-Stage Least Squares (2SLS), we interpret our estimates as the average effect of the pensions number and beneficiaries' gender on *compliers* at the cutoff, i.e local LATE.[22]

Since data on our outcomes of interest were collected yearly at the beginning of November and individuals obtained the pension about a month after turning 60, we estimate the pension *contemporaneous effect* by setting the eligibility dummy D_i equal to 1 if spouse i turned 60 at the end of September and 0 otherwise. For the sake of robustness, we also estimate *three months effects* by setting the eligibility dummy to 1 for individuals turning 60 at the end of June and 0 otherwise, as illustrated in Figure 2. In addition, we drop observations of those individuals turning 60 between June and September, as they have been eligible for the pension for less than three months when outcomes are observed in the household survey in November.[23]

[21] See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for additional details about the identifying assumption in a regression discontinuity design.

[22] Local linear regressions provide a nonparametric way of consistently estimating the treatment effect in a RDD (Hahn *et al.*, 2001; Porter, 2003).

[23] By dropping individuals becoming eligible for the pension between June and

Estimating the number and gender of beneficiaries effects thanks to a fuzzy RDD with spouses' age as running variables requires choosing the bandwidth around the age 60 cutoff for each of running variable. If it is too small, estimates are imprecise. If, in contrast, it is too large, they may be biased as the comparison between the mean value of an outcome if age is on the right-hand side of the cutoff and the value on the left-hand side is little meaningful. Said differently, if the data sample chosen includes observations far away from the age cutoff, the slope of the polynomial in age is not properly accounted for.[24]

We select the optimal bandwidth by using the criterion in Papay *et al.* (2011), that is based on model cross-validation and consists in four steps . *Step 1:* an arbitrary bandwidth value is chosen to regress an outcome on each spouse's age, i.e. the running variables, and on their interaction, separately for subgroups of households in which both spouses were eligible for the pension, in which only one was, and in which nobody was. *Step 2:* predictions and prediction errors are obtained for all possible pairs of age values for male and female spouses by using estimates obtained in step 1 over a range of age values around the 60 cutoff. *Step 3:* steps 1-2 are repeated for approximately 1000 different pairs of bandwidth values. *Step 4:* mean squared prediction errors for each pair of bandwidth values are computed to select as optimal joint bandwidth the one minimizing the mean squared prediction error. In addition, we assess the sensitivity of the estimates to using bandwidth values different than the optimal one.[25]

In all specifications we use a rectangular kernel, i.e. we estimate a linear regression over a window of age rescaled at the 60 cutoff of width equal to the optimal bandwidth value on both sides of the cutoff.[26] We also correct standard errors by using the sampling weights in the survey. These are

September we compare households in which at least one spouse obtained the pension for at least 3 months and those receiving no pension at the time of the survey.

[24]Bandwidth choice corresponds to choosing the width of the age bins used to compute means of an outcome, that are then plotted to graphically visualise the discontinuity (Lee and Lemieux, 2010).

[25]We preferred the bandwidth choice criterion proposed in Papay *et al.* (2011) over the one proposed in Zajonc (2012) as the latter one is suitable to estimate a treatment effect locally at a discontinuity but only conditional on specific values of the running variables.

[26]A rectangular kernel has been chosen thanks to its simplicity with respect to more sophisticated ones, since kernel choice tends to have little impact in practice (Imbens and Lemieux, 2008).

the same when we use data at the individual and at the household level, as the number of households in the primary sampling unit in the survey varies from 80 to 350 based on the population density. Finally, we add as baseline characteristics dummies for ethnicity, education, health status and insurance, eligibility for a contributory pension, extra Renta Dignidad pensions received by other household members, survey year and residential area to increase the precision of the estimates.

5 Results

In this section we show estimates of the pension take-up and of the number and gender of beneficiaries effects on household decisions. Additional results assessing the robustness of the main results to several changes to our preferred specification and to the definition of key variables are shown in Appendix A. Similarly, we show that our results are robust to estimating pension effects jointly on several outcomes, in Appendix B. Finally, we show additional estimates of the effect of obtaining one or more pensions in a household, although without accounting for spouses gender, in Appendix C.

Estimates of contemporaneous pension effects on our outcomes of interest using equation (2) are shown in Table 2, that is divided into five panels by type of outcome. In each regression we report estimates of effects by spouse gender and, in addition of the effect of two pensions, one per each spouse, jointly with the p-value of the null hypothesis of no effect.[27] In last four rows in each panel we report the mean value of outcomes for households in which no spouse is eligible for a pension, the optimal age bandwidths used to select the data sample and the number of observations.[28]

Panel A shows that pension take-up increases discontinuously at 60 from 0% to about 60% when only a spouse is eligible and also when both spouses are. Similarly, it shows that the pension amount received by households

[27] We tested the effect of obtaining two pensions by setting as null hypothesis the linear restriction $\beta_1 + \beta_2 + \beta_3 = 0$ in equation (2).

[28] Additional information about the optimal bandwidth calculation is available in section 4.2.

Table 2: Contemporaneous pension effects

	Panel A: Take-up & poverty						Panel B: Income (bol.)			
	Take-up				Poverty (0/1)		HH income		Private transfers	
	(0/1)			bol. per HH		extreme	total	per capita	received	sent
	female (P_F)	male (P_M)	both							
P_F				219.369***	0.171	-0.208	1032.299	438.131	570.204	9.808
				(24.649)	(0.254)	(0.183)	(1817.989)	(480.835)	(745.162)	(26.044)
P_M				200.876***	0.127	-0.038	369.676	235.733	-235.933**	98.701**
				(11.046)	(0.147)	(0.139)	(1167.074)	(276.887)	(118.391)	(41.578)
$P_F * P_M$				-50.188	-0.361	0.138	-1439.396	-976.826	-308.479	-93.150
				(36.029)	(0.406)	(0.313)	(3007.621)	(752.060)	(939.128)	(91.157)
D_F	0.662***	0.049	0.059							
	(0.118)	(0.053)	(0.052)							
D_M	0.036	0.643***	0.042*							
	(0.027)	(0.075)	(0.025)							
$D_F * D_M$	-0.111	-0.085	0.372***							
	(0.138)	(0.112)	(0.093)							
$(A_F - 60)$	0.000	-0.000	-0.001	-0.377	-0.004	0.000	-18.228	-15.356	11.719*	-2.029
	(0.001)	(0.001)	(0.000)	(0.608)	(0.008)	(0.008)	(66.127)	(17.839)	(6.688)	(1.945)
$(A_M - 60)$	0.000	0.000	-0.000	0.021	-0.011*	0.002	-67.711	-2.511	9.001*	-3.241**
	(0.001)	(0.001)	(0.000)	(0.470)	(0.006)	(0.006)	(61.923)	(14.990)	(4.842)	(1.425)
$(A_F - 60) * (A_F - 60)$	0.000	0.000	-0.000	-0.024	-0.000	0.000	-5.701	-1.801	0.591	-0.281***
	(0.000)	(0.000)	(0.000)	(0.043)	(0.001)	(0.001)	(5.737)	(1.452)	(0.430)	(0.103)
Joint effect	0.586***	0.606***	0.473***	370.057***	-0.062	-0.108	-37.422	-302.962	25.791	15.359
P-value (joint eff.)	0.000	0.000	0.000	0.000	0.752	0.498	0.977	0.304	0.928	0.786
Mean value (ineligible HH)	0.000	0.000	0.001	97.568	0.493	0.291	3427.179	839.973	96.885	28.718
Bandwidth female	20	20	20	20	20	19	20	20	20	20
Bandwidth male	18	18	18	20	18	20	20	20	20	20
Observations	2,358	2,358	2,358	2,486	2,358	2,389	2,486	2,486	2,486	2,486

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	Panel C: Labour supply (0/1)				**Panel D: Consumption**				**Panel E: Living arrangements**			
	Female	Male	1+ spouse	Both	Total cons. (bol.)		Purchase (0/1)		Adult children (0/1)			1+ grandchild
					food	non-food	medic. serv.	medicines	in HH	male works	fem. works	(0/1) in HH
P_F	-0.022	0.019	-0.006	-0.063	61.709	2734.866	0.125	0.609*	-0.445	-0.153	0.200	-0.009
	(0.240)	(0.117)	(0.125)	(0.250)	(128.699)	(3033.443)	(0.227)	(0.341)	(0.281)	(0.299)	(0.281)	(0.230)
P_M	-0.265*	0.076	-0.053	-0.323*	-34.233	5203.801	0.322*	0.707***	-0.187	0.296	0.175	0.094
	(0.155)	(0.104)	(0.074)	(0.173)	(100.064)	(5277.716)	(0.183)	(0.256)	(0.160)	(0.254)	(0.234)	(0.148)
$P_F * P_M$	-0.069	-0.301	-0.160	0.095	-29.412	-7406.710	-0.342	-0.662	0.761	0.411	-0.673	0.429
	(0.414)	(0.242)	(0.235)	(0.432)	(205.237)	(8122.102)	(0.418)	(0.571)	(0.507)	(0.562)	(0.549)	(0.394)
$(A_F - 60)$	0.002	0.009	0.013	0.002	3.322	-550.656	-0.011	-0.085***	-0.003	-0.008	0.004	0.002
	(0.007)	(0.006)	(0.008)	(0.010)	(4.792)	(362.073)	(0.008)	(0.027)	(0.010)	(0.009)	(0.008)	(0.007)
$(A_M - 60)$	0.008	-0.002	0.002	0.011	7.022*	-497.456	0.005	-0.031**	-0.018***	-0.003	0.005	0.003
	(0.006)	(0.003)	(0.003)	(0.008)	(4.252)	(307.242)	(0.006)	(0.013)	(0.006)	(0.007)	(0.006)	(0.005)
$(A_F - 60)$ * $(A_F - 60)$	0.000	0.000	0.001	0.001	0.367	-51.550	-0.000	-0.009***	-0.001	-0.001	0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.001)	(0.381)	(35.150)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
Joint effect	-0.356*	-0.206	-0.219	-0.292	-1.936	531.957	0.105	0.655**	0.128	0.554**	-0.298	0.514**
P-value (joint eff.)	0.098	0.153	0.108	0.215	0.986	0.901	0.619	0.029	0.658	0.046	0.320	0.012
Mean value (ineligible HH)	0.608	0.878	0.920	0.574	324.556	4593.873	0.360	0.465	0.722	0.416	0.312	0.186
Bandwidth female	20	20	19	19	18	20	20	16	20	20	20	20
Bandwidth male	20	20	10	15	20	20	20	7	11	20	20	19
Observations	2,486	2,486	1,418	2,055	2,281	2,469	2,486	973	1,554	1,869	1,869	2,426

Note: The table shows estimates of pension take-up (P_i) as a function of the eligibility dummy (D_i) by spouse i in a household and of its effects on income, labour supply, consumption and living arrangements. We obtained them thanks to a fuzzy regression discontinuity design (RDD) exploiting the discontinuity in pension eligibility at age 60 by instrumenting pension take-up with eligibility for each spouse in a household and adding RDD polynomials in in their age (A_i) rescaled at 60. At the bottom of the panel estimates of the effect of two pensions are shown, along with the associated p-value. We observed age at the end of September to study contemporaneous effects because it can take up to a month since the 60th birthday to obtain the pension and our outcomes of interest are measured in November. We measured age as follows: the integer part measures age in years while the decimal one measures fractions of years. We included as covariates dummies for the survey year, ethnicity, education level, ethnicity, health and insurance status, contributory or extra Renta Dignidad pensions received in a household and urban residence. We corrected standard errors by using survey weights. The last four rows show mean values of outcomes for households with no pension, the optimal bandwidth used in the regressions and the number of observations. Significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data are from a representative survey of Bolivian households held by the National Statistics Institute in 2008 and 2009. Section 2 offers additional information on the institutional setting and on the data, section 4 on the research design and section 5 on the empirical analysis.

with one or more beneficiaries also increases significantly by 200-370 bolivianos with respect to households in which no pension is received, counterfactual households hereafter. The remaining two columns in panel A show estimates of contemporaneous effects on the probability that a household income is lower than the poverty line and the extreme poverty line, by using as outcomes a dummy equal to 1 if a household is poor. When only a pension is received, poverty increases by up to 17 percentage points, pp hereafter, or by 30% of the mean value of counterfactual households, hereafter shown in parenthesis. Extreme poverty, instead, decreases by up to 20 pp (66%) when one or more pensions are received. However, these effects are not significant.

Panel B in Table 2 shows estimates of pension effects on household income. The first two columns show that total income and total income per capita increase between 235 and 1030 bolivianos (10-50%), depending on who obtained the pension, although the effect is not significant. The last two columns show a decrease by 236 bolivianos (245%) in transfers received from other households and, in addition, an increase by 98 bolivianos (350%) in the transfers sent to other households when only the male obtained the pension.

Panel C in Table 2 shows estimates for labour supply. The probability that a wife works decreases by about 27 pp (43%) when her husband obtained the pension and by 36 pp (59%) when both obtained it, although these effects are weakly significant. Conversely, the effect on the probability that the husband works is not significant. In addition, the probability that one or more spouses in a household works tends to decrease when one or more pensions are received. However, the only significant effect, by about 21 pp (23%) albeit weakly, is observed when both spouses obtained it. Finally, a pension to the male decreases the probability that both spouses work by 32 pp (56%), although weakly, while the negative effect of receiving two pensions is not significant.[29]

Panel D in Table 2 shows estimates for consumption. The sign of the effect on total food consumption depends on beneficiary gender while the one on non-food consumption is positive. Although point estimates vary

[29]Results are similar when using measures of the intensive margin of household labour supply and are available upon request.

between 20% and 100% of the mean value for counterfactual households, they are not significant. In addition, the probability of purchasing medical services increases by 32 pp (89%) when only a male obtains the pension, although it is weakly significant. Finally, the probability of purchasing medicines increases between 60 and 70 pp (130-150%) when one or more pensions are received in a household.[30]

Finally, panel E in Table 2 shows estimates of pension effects on living arrangements. When only one spouse obtains the pension neither the probability of observing their adult male or female children working nor that that of observing grandchildren in that household changes significantly. In contrast, in households in which we observe a pension for both spouses, the probability that adult male children work increases by 55 pp (130%) and the one that grandchildren live with grandparents by 51 pp (268%).

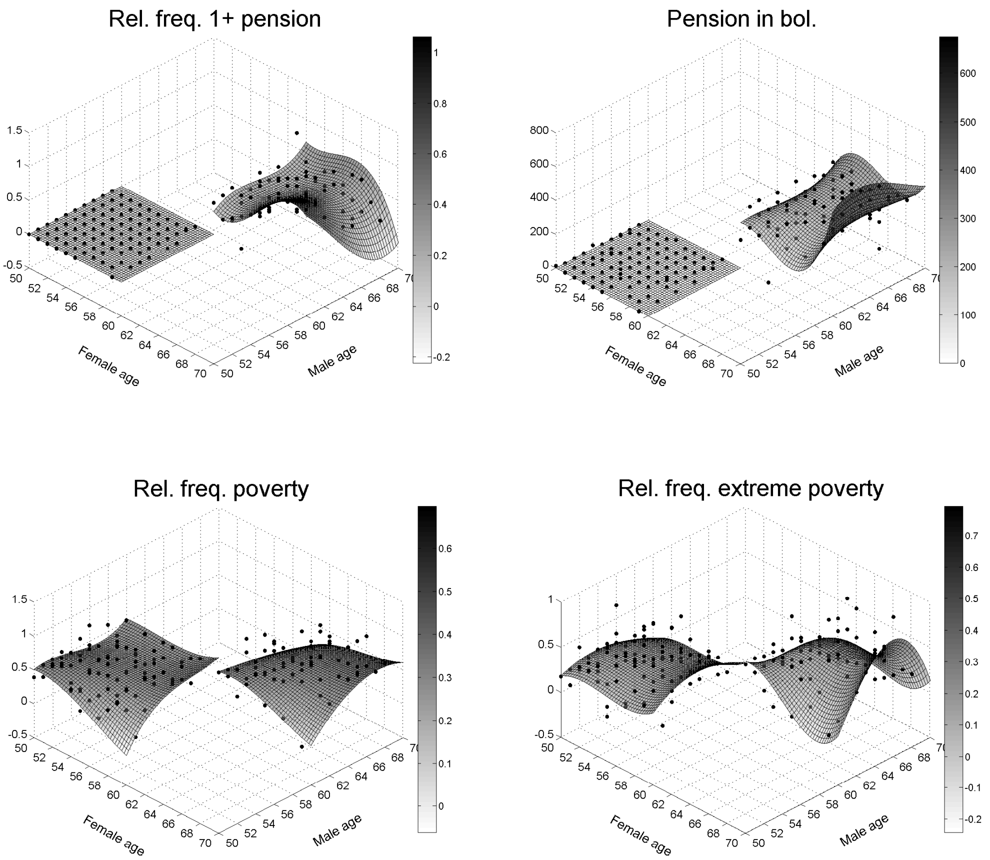
In addition, to visualise contemporaneous effects of receiving two pensions, we plotted as surfaces in Figure 3 estimated values, for several pairs of spouses' age values, of third order polynomials of our outcomes of interest in spouses' age. Estimates were obtained separately for households receiving two pensions on the right-hand side in each surface plot and for those receiving no pension on the left-hand one, with dots showing an outcome means for given pairs of spouses' age values. Panel A shows that take-up increases discontinuously at 60, from 0 to about 60%, and that pension income increases from 0 to about 200 bolivianos. Panel B to E in the same figure show the same information for our outcomes of interest as follows: income in panel B, labour supply in panel C, consumption in panel D and living arrangements in panel E. Overall, estimates are qualitatively in line with those shown in Table 2. However, their precision is not shown as it can only be assessed using bootstrapped confidence intervals, hence making analytical estimates in Table 2 easier to assess.

Had we, instead, estimated pension effects without accounting for beneficiary gender, we would have obtained similar results on labour supply

[30] We measure medical services or medicines consumption by using a dummy to test for a qualitative change in their consumption in poor households obtaining one or more pensions as they are services only consumed in the event of illness, whose monetary value follows a skewed distribution peaking at zero. Results are similar when using different measures of these household decisions and are available upon request.

Figure 3: Contemporaneous pension effects at the age 60 cutoff

Panel A: Take-up & poverty



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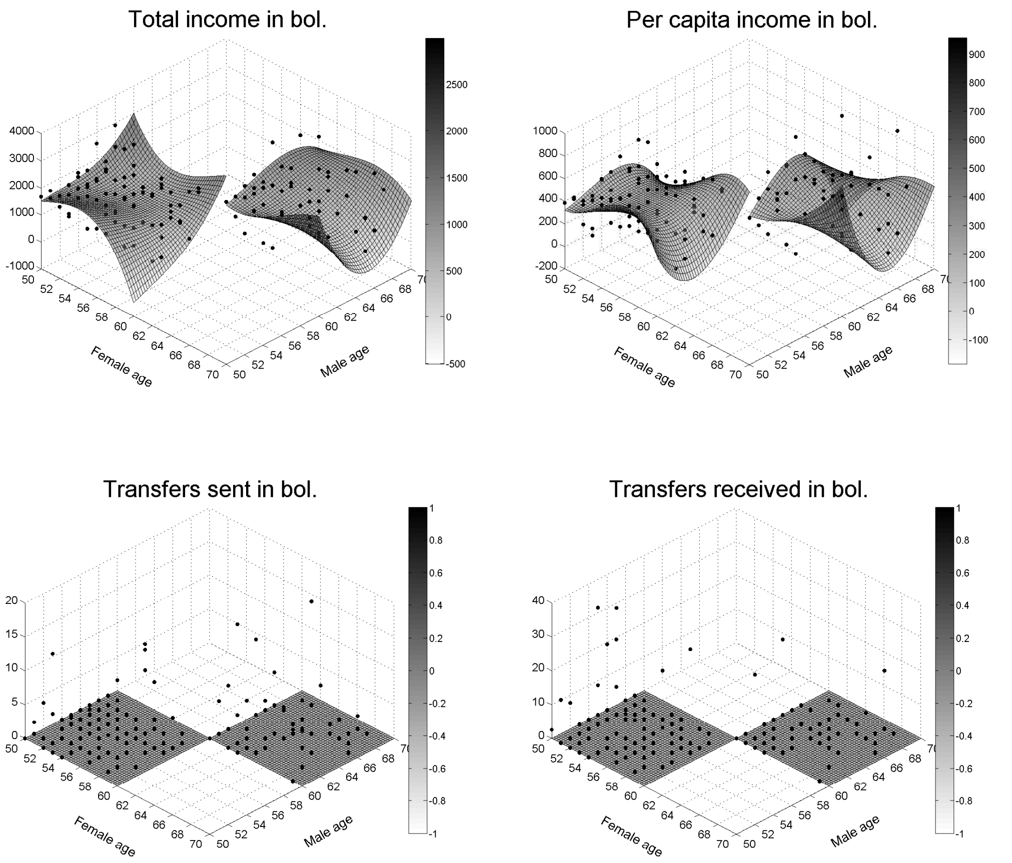
Panel B: Income



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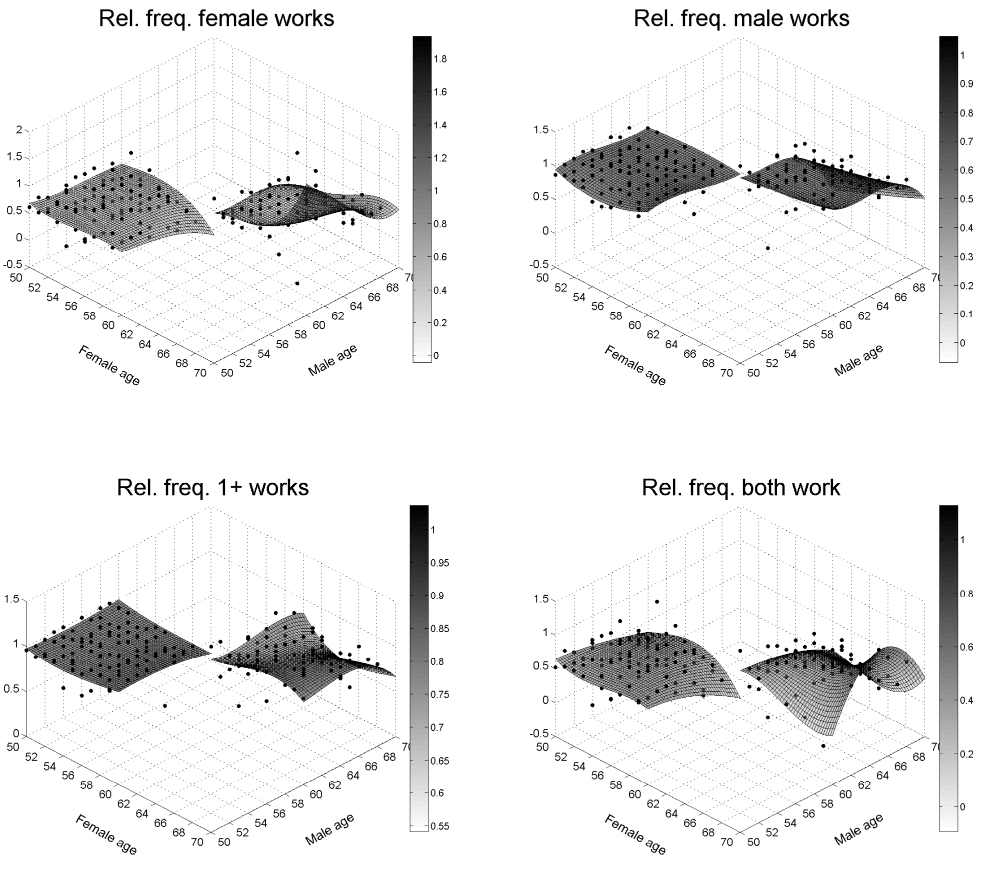
Panel C: Labour supply



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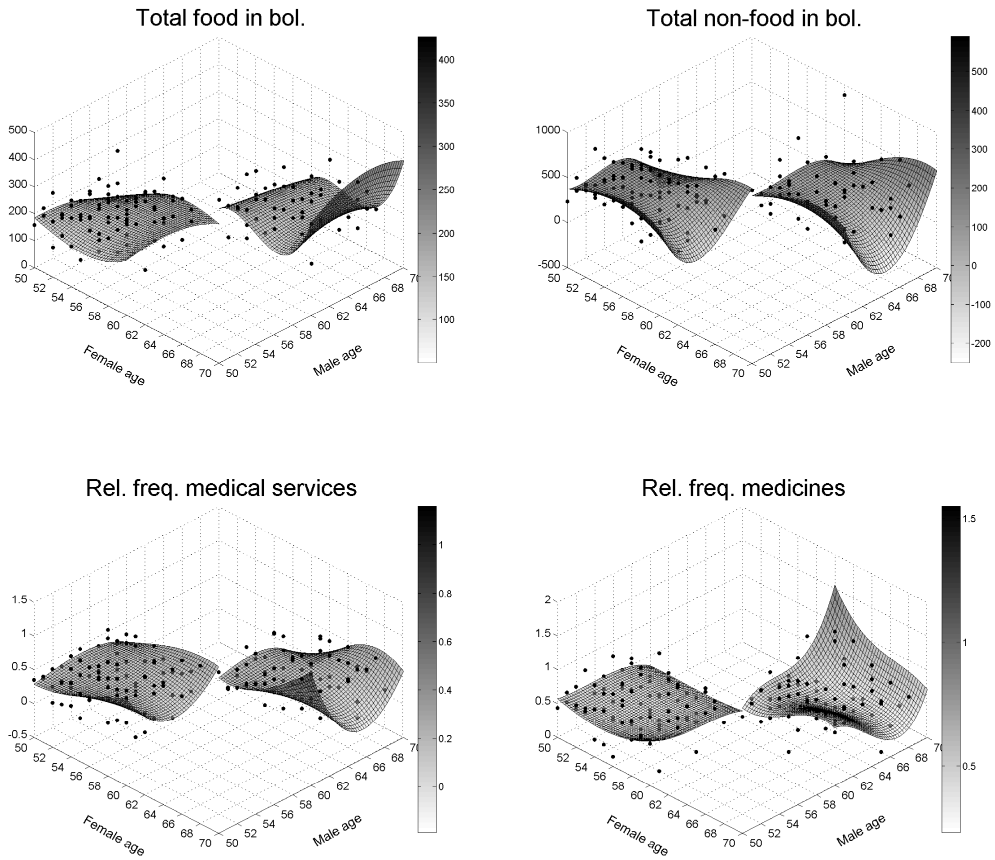
Panel D: Consumption



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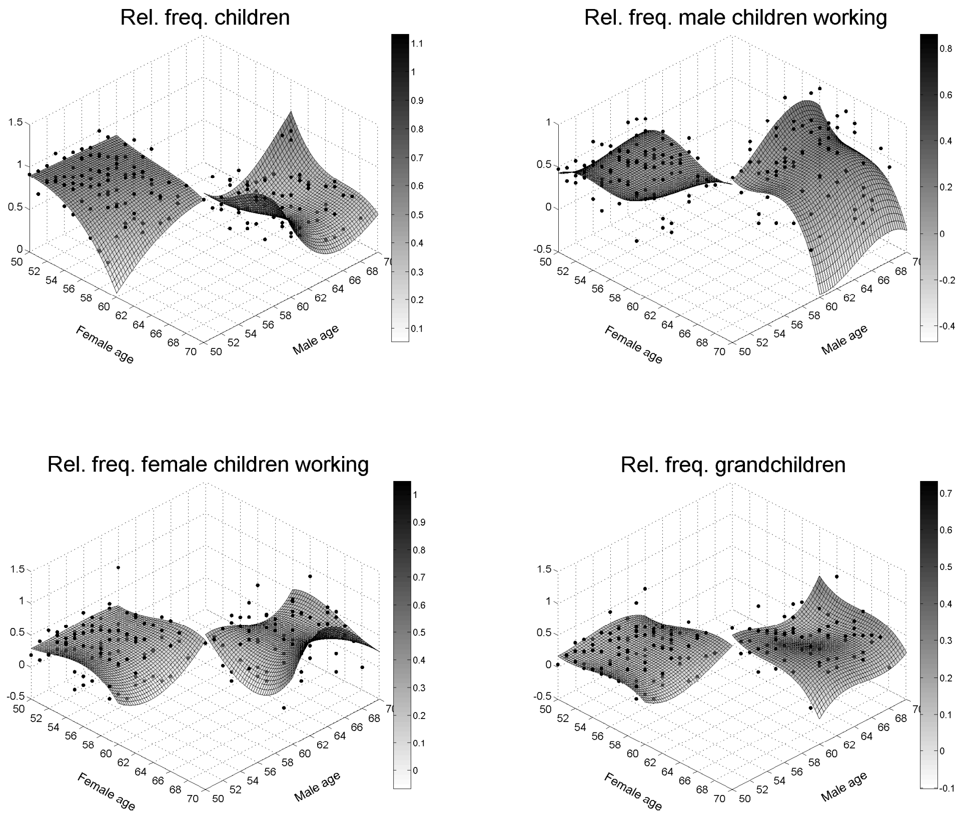
Panel E: Living arrangements



Note: The figure shows two surfaces obtained by estimating third order polynomials of an outcome in female and male spouse age separately for households receiving two pensions on the right-hand side in each surface plot and for those not receiving any on the left-hand side. In addition, dots show mean values of an outcome for a given female and male pair of age values. Five sets of household decisions are shown in different panels. Age is defined as follows: the integer part measures age in years while the decimal one measures fractions of years. We observed age at the end of September to study pension *contemporaneous effects* since a month can elapse between the 60th birthday and the first pension payment due to administrative reasons and our outcomes of interest are measured in November. Data are from a representative survey of Bolivian households held by the National Statistics Institute in 2008 and 2009. Section 2 offers additional information on the institutional setting and on the data, section 4 on the research design and section 5 on the empirical analysis.

decisions, although less precise, and on consumption, as shown in Appendix C. In contrast, we would have failed to uncover substantive effects on living arrangements involving beneficiaries' adult children and grandchildren. In addition, had we also failed to account for the fact that certain decisions, for example over living arrangements, may be taken jointly by spouses in a household and depend on their bargaining power, we would have mistakenly concluded that the pension has no effect on beneficiaries' labour supply, as shown in Appendix D.

6 Research design validity

In this section we firstly discuss our research design validity and then we present a falsification test.

6.1 Forcing variables and baseline characteristics

Consider an experiment in which subjects are barely younger or older than 60, the treatment consists in receiving a pension and the assignment of subjects to the treated group is random. A RDD identifies the effect of the pension locally at the age 60 cutoff by mimicking such an experiment in a quasi-experimental setting.

The untestable RDD *identifying assumption* is that individuals are unable to sort themselves into the treated group, or out of it, by manipulating, for example, their age or date of birth. We offer evidence in support of this assumption by assessing empirically, first, whether the age distribution by spouse gender is smooth at the age 60 cutoff and, second, whether individuals' baseline characteristics are balanced at the cutoff.[31]

Since an individual's date of birth is a predetermined characteristic, we expect a smooth age distribution at 60 for both females and males, in the absence of manipulation. In contrast, an age histogram showing that bins height on the right-hand side of the cutoff is, for example, greater than on the left-hand side suggests that individuals sorted themselves into the

[31] See Lee and Lemieux (2010) for a discussion of RDD identifying assumption and validity.

pension.[32]

The top panel in Figure 4 shows age histograms, separately by spouse gender in a household, with 90 days as bin width. This ensures that a jump can be detected visually as no bin contains 60, the cutoff value. Visual inspection suggests no suspicious jump in histogram bins height at the cutoff, hence supporting the validity of the research design. This result is confirmed by the density-based test of the null hypothesis of no manipulation in McCrary (2008), as shown in the bottom panel in Figure 4 by the overlapping confidence intervals of the difference in age density, shown as dashed lines, at the 60 cutoff.

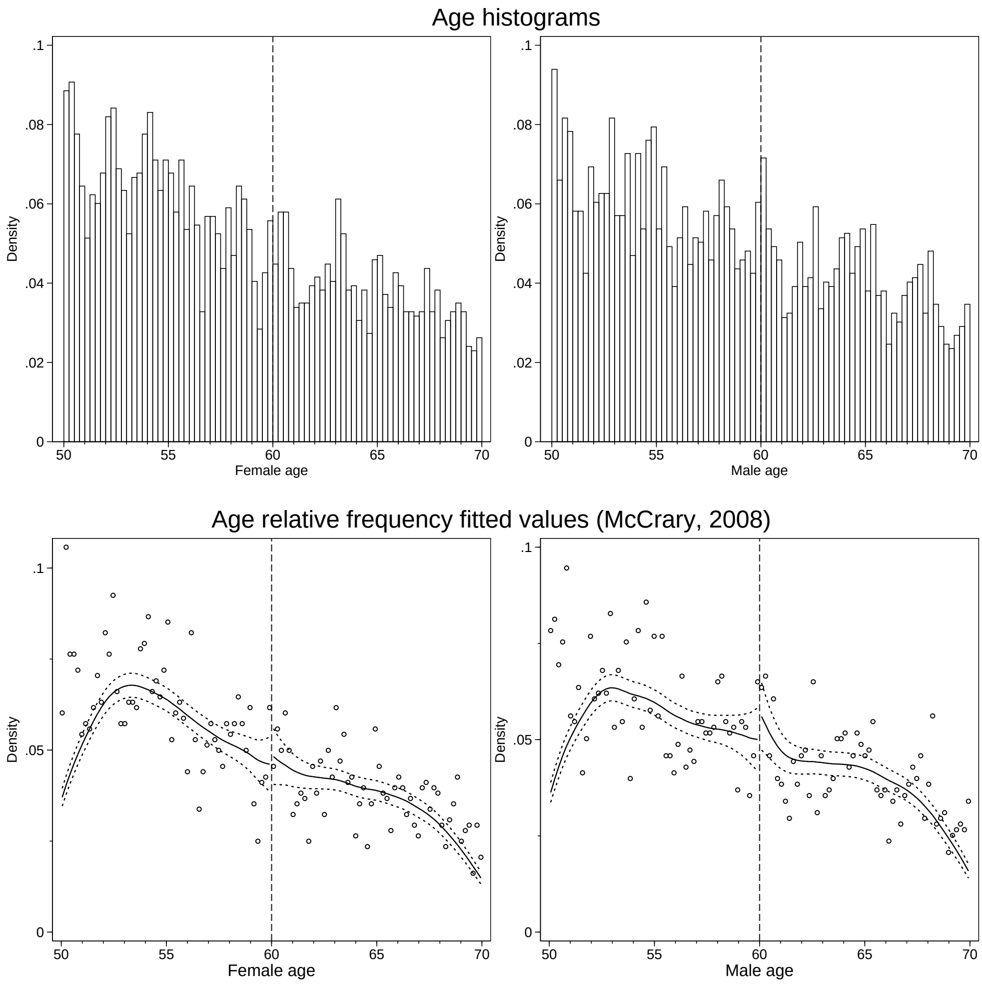
In addition, in the absence of manipulation, we expect no difference in individuals' baseline characteristics between those who were barely younger than 60 and those barely older, as such characteristics are predetermined.

Figure 5 shows fitted values of RDD polynomials in age of each baseline characteristic, estimated separately by whether individuals are at least 60, as well as by gender. Overall, differences in baseline characteristics at the cutoff are small and not significant, as shown by the overlapping confidence intervals dashed lines. Results are unchanged if we estimate differences in baseline characteristics at the 60 cutoff by regressing each of them on the RDD polynomial in spouses' age rescaled at 60 and separately by whether individuals are at least 60, as shown by small and non-significant estimates in Table 3.[33]

[32] See Camacho and Conover (2011) for an example of manipulation of a score based on demographics and income at a cutoff used to allocate individuals to a social program in Colombia. Individuals not truthfully reporting this information and complacent local politicians lead to the observed jump at the cutoff.

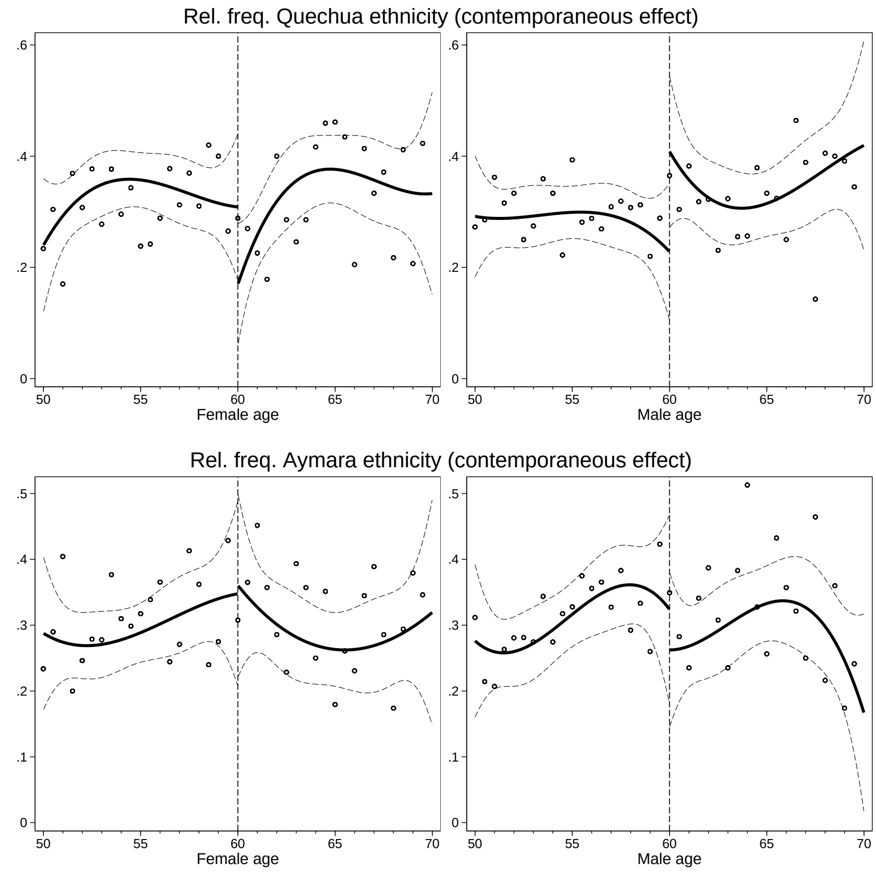
[33] We also tested the null hypothesis that differences in all baseline characteristics at the age 60 cutoff are jointly zero by estimating a system of seemingly unrelated regressions of each baseline characteristic on pension by gender dummies and the RDD polynomial in equation (2), and testing whether coefficients associated to the pension dummy by gender in each equation are jointly zero. Large p-values in Table A.3 in Appendix A suggest that baseline characteristics by spouse gender are jointly balanced, thus supporting the research design validity.

Figure 4: Age distribution



Notes: The top panel in the figure shows histograms of spouses' age observed contemporaneously to outcomes, in September, to estimate pension contemporaneous effects, because it can take up to a month since the 60th birthday to obtain the pension and outcomes are observed in November. Histograms bin width is 90 days. The bottom panel shows smoothed histograms of age, mean values as dots and 95% confidence intervals as dashed lines, obtained using the routine in McCrary (2008). We measured age as follows: the integer part measures age in years while the decimal one measures fractions of years. Data are from a representative survey of Bolivian households held by the National Statistics Institute in 2008 and 2009. Section 2 offers additional information on the institutional setting and on the data, section 4 on the research design and section 5 on the empirical analysis.

Figure 5: Balance of baseline characteristics at the age 60 cutoff



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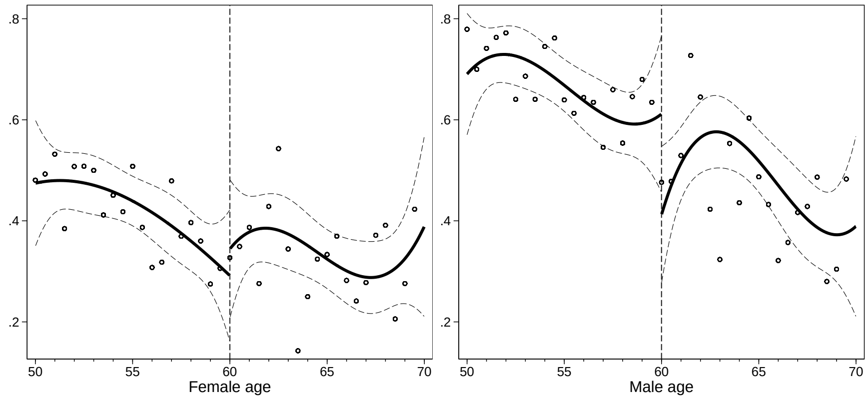
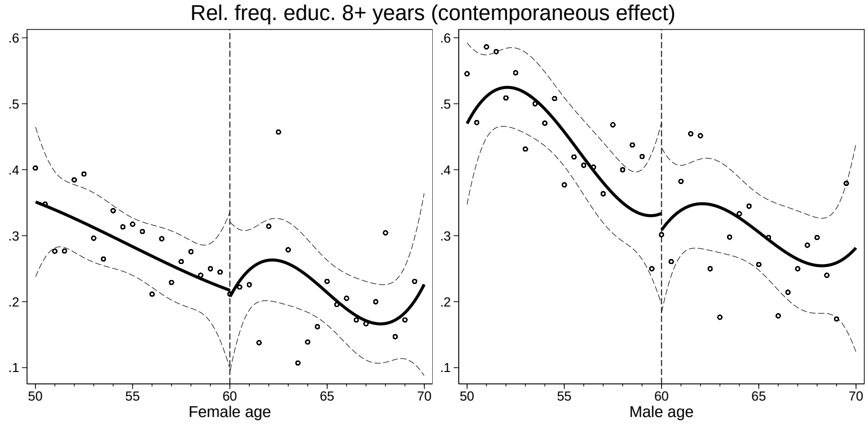
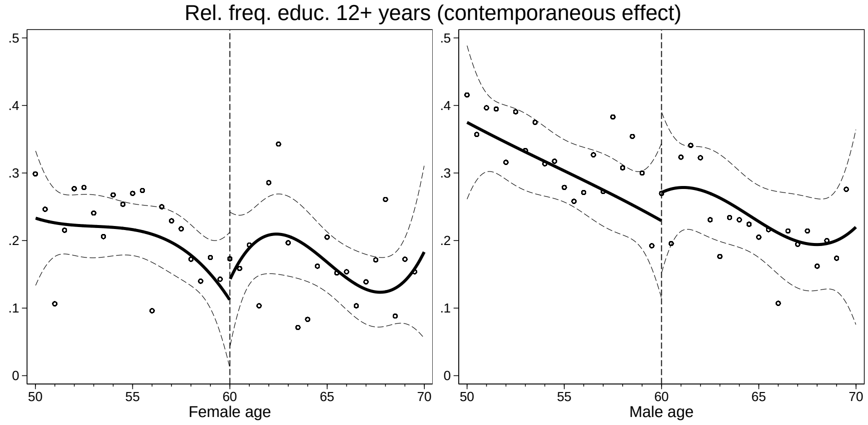
Rel. freq. educ. 5+ years (contemporaneous effect)



Rel. freq. educ. 8+ years (contemporaneous effect)



Rel. freq. educ. 12+ years (contemporaneous effect)



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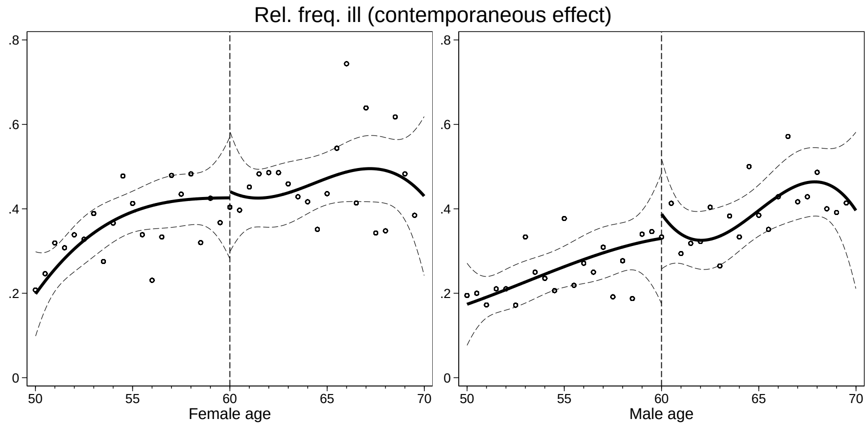

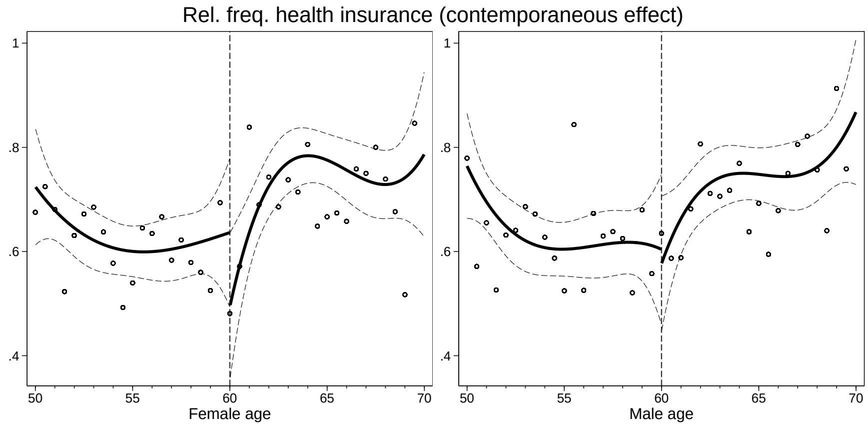

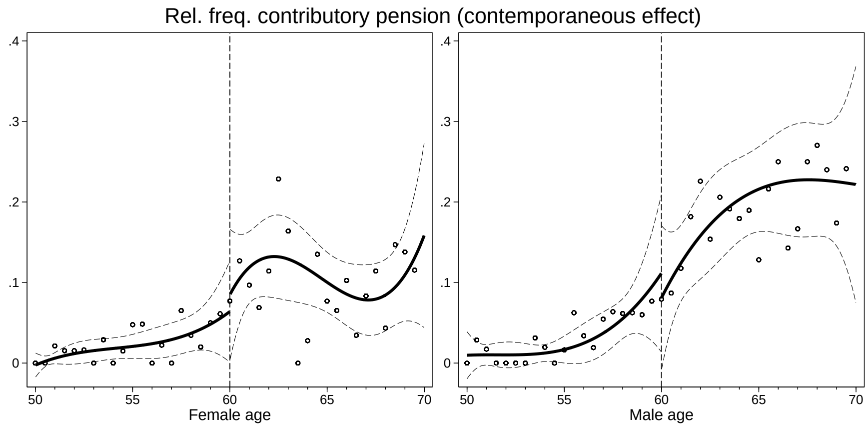

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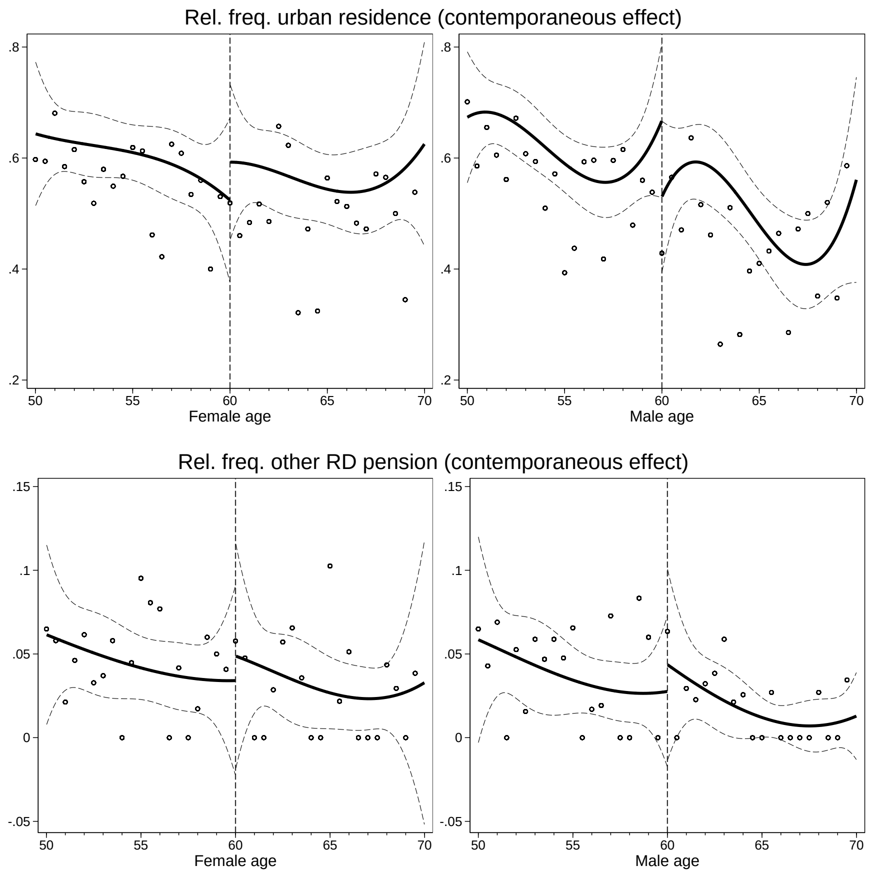

Note: The figure shows estimates of the difference in baseline characteristics at age 60 between individuals who were eligible for the pension as they were 60 or older and those who were not as they were younger at the end of September to assess whether baseline characteristics are balanced. We obtained estimates separately for females and males by regressing a baseline characteristic on the RDD polynomial in age rescaled at 60 separately by spouse and by whether an individual was at least 60. Continuous lines show predicted values and dashed ones 95% confidence intervals. We observed age at the end of September to study contemporaneous pension effects because it can take up to a month since the 60th birthday to obtain the pension and our outcomes of interest are measured in November. The integer part of age measures it in years while the decimal one in fractions of years. Data are from a representative survey of Bolivian households held by the National Statistics Institute in 2008 and 2009. Section 2 offers additional information on the institutional setting and on the data, section 4 on the research design and section 5 on the empirical analysis.

Table 3: Balance of baseline characteristics by spouse at the age 60 cutoff

Panel A: Females

	Ethnicity		Education (years)			Health		Contributory	Household characteristics	
	Quechua	Aymara	5+	8+	12+	sick spell	insurance	pension	1+ extra Renta Dig.	urban
P_F	0.389	-0.187	-0.100	-0.261	-0.169	0.034	-0.088	-0.071	0.142	-0.078
	(0.322)	(0.273)	(0.277)	(0.300)	(0.157)	(0.289)	(0.297)	(0.062)	(0.111)	(0.310)
P_M	0.137	-0.240	-0.147	0.116	0.003	-0.135	-0.341*	-0.095	0.034	-0.032
	(0.174)	(0.161)	(0.161)	(0.192)	(0.153)	(0.168)	(0.206)	(0.062)	(0.039)	(0.154)
$P_F * P_M$	-0.694	0.320	0.317	0.175	0.386	0.107	0.178	0.313*	-0.218	0.059
	(0.493)	(0.407)	(0.417)	(0.476)	(0.324)	(0.455)	(0.481)	(0.169)	(0.143)	(0.449)
$(A_F - 60)$	-0.013	0.020***	0.003	-0.001	-0.007	0.005	0.010	0.000	-0.005*	0.004
	(0.011)	(0.007)	(0.009)	(0.015)	(0.012)	(0.011)	(0.025)	(0.004)	(0.003)	(0.008)
$(A_M - 60)$	0.001	0.014**	-0.016**	-0.012	-0.011	0.013*	-0.001	0.003*	-0.002	-0.002
	(0.009)	(0.006)	(0.007)	(0.015)	(0.010)	(0.007)	(0.011)	(0.002)	(0.002)	(0.006)
$(A_F - 60) * (A_F - 60)$	-0.001	0.002***	0.000	0.001	-0.000	0.000	-0.000	0.000	-0.000*	0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)	(0.000)	(0.000)	(0.001)
Joint effect	-0.168	-0.108	0.070	0.029	0.220	0.006	-0.251	0.147	-0.043	-0.052
P-value (joint eff.)	0.472	0.524	0.703	0.898	0.242	0.977	0.343	0.214	0.398	0.789
Mean value (ineligible HH)	0.324	0.300	0.460	0.273	0.211	0.332	0.650	0.030	0.027	0.540
Bandwidth female	15	20	19	10	11	20	20	20	20	20
Bandwidth male	20	20	19	14	15	14	7	12	20	20
Observations	1,823	2,409	2,266	1,114	1,254	1,889	937	1,641	2,409	2,409

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Panel B: Males

	Ethnicity		Education (years)			Health		Contributory
	Quechua	Aymara	5+	8+	12+	sick spell	insurance	pension
P_F	0.342	0.018	0.415	0.148	0.214	0.004	-0.309	-0.054
	(0.312)	(0.281)	(0.309)	(0.301)	(0.265)	(0.279)	(0.310)	(0.216)
P_M	-0.345*	-0.156	0.046	-0.042	-0.173	0.098	-0.323	0.191
	(0.182)	(0.160)	(0.160)	(0.188)	(0.170)	(0.187)	(0.212)	(0.136)
$P_F * P_M$	-0.163	0.044	-0.656	-0.313	-0.149	-0.263	0.340	-0.160
	(0.522)	(0.414)	(0.452)	(0.463)	(0.420)	(0.437)	(0.493)	(0.339)
$(A_F - 60)$	0.022	0.023***	-0.006	0.005	0.012	0.001	0.017	0.012
	(0.024)	(0.007)	(0.008)	(0.015)	(0.008)	(0.015)	(0.027)	(0.008)
$(A_M - 60)$	0.014	0.012**	-0.013**	-0.012	-0.005	-0.024**	0.001	0.006
	(0.010)	(0.006)	(0.007)	(0.014)	(0.010)	(0.010)	(0.013)	(0.004)
$(A_F - 60) * (A_F - 60)$	0.003	0.002***	-0.000	0.001	0.001	-0.002	0.001	0.001
	(0.003)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)
Joint effect	-0.166	-0.094	-0.195	-0.207	-0.108	-0.161	-0.292	-0.023
P-value (joint eff.)	0.586	0.577	0.318	0.362	0.579	0.439	0.284	0.883
Mean value (ineligible HH)	0.314	0.305	0.642	0.370	0.271	0.300	0.655	0.092
Bandwidth female	20	20	20	11	12	19	16	20
Bandwidth male	7	20	20	17	20	9	7	9
Observations	939	2,409	2,408	1,286	1,445	1,196	894	1,203

Note: The table shows estimates of the difference in the baseline characteristics between individuals who obtain the pension (P_i=1) as they are 60 or older and those who do not (P_i=0) as they are younger than 60 at the time of the household survey. We obtained them thanks to a fuzzy regression discontinuity design (RDD) exploiting the discontinuity in pension eligibility (D_i) at age (A_i) 60 by spouse i. Running variables are spouses' age: their integer part measures age in years while the decimal one measures fractions of years. We regressed a spouse's baseline characteristic using 2SLS and instrumenting pension take-up at the end of September with eligibility and on the RDD polynomial in age rescaled at 60 to assess whether baseline characteristics are balanced. First stage estimates are shown in Panel A in Table 2. Data are from a representative survey of Bolivian households held by the National Statistics Institute in November 2008 and 2009. We corrected standard errors by using the survey weights. At the bottom of the panel estimates of the difference when both spouses obtain a pension are shown, along with the associated p-value. The last four rows show mean values of baseline characteristics for households with no pension, the optimal bandwidth used in the regressions and the number of observations. 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 and section 4 on the research design.

Although household characteristics for the same household cannot be measured over time due to the household survey repeated cross-section design, our estimates of the pension effect on household decisions, particularly household size, do not suffer from reverse causality since our RDD compares households with spouses barely younger than 60, i.e. control households, with households in which one more of them is barely older than 60, i.e. treated ones. By design, control and treated households should be very similar in observable predetermined characteristics, such as education, as the assignment to treatment locally at the 60 cutoff mimics that of an experimental design.

Earlier on in this section, we carefully assessed, firstly, whether predetermined characteristics are balanced by treatment status and, secondly, if self-selection into the pension, although not possible in theory, occurs de facto, which can be detected by testing whether the distribution of individuals' age is continuous at the 60 cutoff. Our findings confirm that households in the treated and control group are similar in a rich set of observable characteristics and that self-selection of non-eligible households into the pension is not a concern.

In addition, by estimating a fuzzy RDD we focus on the causal effect for compliers who, in our research design, are individuals who are 60 or older and receive one or more pension in a household. However, fuzzy RDD estimates and reduced-form estimates, not shown but available upon request, are qualitatively and quantitatively in line, as the contemporaneous take-up rate ranges from 60% to 90% and converges to about 100% in households in which one or more pensions have been received for three months or longer. Finally, results not shown in the paper but available upon request show that the probability that an extra extended family member, other than spouses' children or grandchildren, lives in a household receiving one or more pensions is small and not significant.

6.2 Falsification test

Finally, we assessed whether 2008, the year in which Renta Dignidad was introduced, and 2009, are the only ones in which a change in our outcomes of interest at age 60 is observed. We estimated the effect of turning 60 in 2006 and 2007, before the pension was enacted, using the reduced form of equations (2)-(3) and data for 2006 and 2007 by gender. Small and not significant estimates, shown in Table 4, of being 60 or older in 2006 or in 2007 exclude that our main results are driven by confounding factors at 60, such as demographic ones or pension anticipation effects, thus supporting the research design validity.[34]

7 Discussion and conclusion

First, our results enrich the existing evidence on non-contributory pensions in Bolivia. Escobar Loza *et al.* (2013) find that Renta Dignidad increases per capita income and consumption and decreases poverty, mainly driven by urban households. Differences with respect to our paper, that shows a small and non-significant effect on consumption and poverty, are potentially due to the fact that they do not account for spouse pension direct and indirect effects and, also, to the fact that they use data from an ad-hoc survey only on individuals within 5 years from the age 60 cutoff.

Changes in non-contributory pension policies in Bolivia, instead, are studied in Martinez (2004) and Hernani-Limarino and Mena (2015), respectively exploiting a suspension of Bonosol and the change to Renta Dignidad, that lowered the age eligibility cutoff from 65 to 60. Differently from Martinez (2004), we do not find evidence of credit constraints in home-production and consumption for poor households in rural areas, in results available upon request. We find, instead, evidence in line with credit constraints in medicines consumption, that is greater for beneficiaries, although we cannot learn whether it is driven by their health needs or by members of the extended family moving in with them. As for our evidence on the decrease in female labour supply, it is partly in line with the one in Hernani-Limarino and Mena (2015), although in our paper it is driven by spouse pension indirect effect rather than by own pension effect.

Second, finding that a universal pension has a small and non-significant short-term effect on poverty may look somewhat puzzling. However, it is

[34]The results are similar for three months pension effects, i.e. when setting the age dummy equal to 1 if an individual obtained the pension at the end of June, as shown in Table A.4.

Table 4: Falsification test of contemporaneous pension effects

	Panel A: Poverty			Panel B: Income				
	Poverty (0/1)			HH income			Private transfers	
		extreme		total	per capita		received	sent
D_{A_F}	0.257	0.380		-1396.541	-543.120*		-121.298**	21.510
	(0.173)	(0.283)		(984.290)	(282.405)		(60.193)	(17.246)
D_{A_M}	-0.032	-0.028		174.575	-169.685		-11.730	10.664
	(0.127)	(0.101)		(1001.162)	(261.106)		(47.951)	(16.154)
$D_{A_F} * D_{A_M}$	-0.354	-0.350		3216.969**	1043.700**		90.746	-2.162
	(0.218)	(0.307)		(1492.668)	(408.783)		(90.443)	(21.366)
$(A_F - 60)$	0.011	0.007		96.820	38.106*		-0.464	-0.747
	(0.011)	(0.010)		(73.874)	(20.520)		(3.710)	(2.424)
$(A_M - 60)$	-0.001	-0.002		33.075	23.089		3.708	-4.738**
	(0.007)	(0.008)		(53.690)	(14.994)		(2.802)	(2.409)
$(A_F - 60) * (A_F - 60)$	0.001	0.001		3.268	1.389		-0.013	-0.171
	(0.001)	(0.001)		(5.308)	(1.402)		(0.285)	(0.223)
Joint effect	-0.129	0.001		1995.002*	330.896		-42.282	30.011*
P-value (joint eff.)	0.274	0.995		0.093	0.360		0.497	0.063
Mean value (ineligible HH)	0.535	0.342		2808.632	681.445		51.899	24.074
Bandwidth female	20	19		20	20		20	20
Bandwidth male	18	20		20	20		20	20
Observations	1,176	1,158	8,196	1,229	1,229	8,196	1,229	1,229

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	Panel C: Labour supply (0/1)				**Panel D: Consumption**					**Panel E: Living arrangements**			
	Female	Male	1+ spouse	Both	Total cons. (bol.)		Purchase (0/1)			Adult children (0/1)			1+ grandchild
					food	non-food	medic. serv.	medicines		in HH	male works	fem. works	(0/1) in HH
D_{A_F}	-0.199	-0.065	0.057	0.141	183.233	-1646.808	-0.306	0.137		0.348	1.131***	-0.521***	-0.396
	(0.280)	(0.151)	(0.107)	(0.358)	(115.165)	(1013.305)	(0.260)	(0.333)		(0.283)	(0.206)	(0.145)	(0.243)
D_{A_M}	0.171	-0.026	-0.152	-0.080	37.865	775.809	0.089	-0.246		0.234*	0.461***	-0.012	-0.160
	(0.139)	(0.103)	(0.179)	(0.146)	(50.630)	(1362.315)	(0.143)	(0.269)		(0.131)	(0.161)	(0.151)	(0.156)
$D_{A_F} * D_{A_M}$	0.232	0.191	-0.036	0.086	-149.362	-550.634	0.120	0.186		-0.771**	-1.598***	0.925***	0.669**
	(0.324)	(0.199)	(0.242)	(0.391)	(134.836)	(1559.553)	(0.300)	(0.439)		(0.335)	(0.293)	(0.236)	(0.299)
$(A_F - 60)$	-0.014	-0.004	-0.004	-0.010	-3.500	129.411	0.023**	0.035		0.015	-0.016	-0.006	0.021**
	(0.011)	(0.006)	(0.011)	(0.014)	(4.561)	(87.435)	(0.010)	(0.040)		(0.015)	(0.012)	(0.012)	(0.010)
$(A_M - 60)$	-0.007	-0.005	-0.005	-0.010	-0.850	40.810	0.006	0.003		-0.013**	-0.004	0.017**	0.015*
	(0.009)	(0.005)	(0.006)	(0.011)	(6.336)	(64.214)	(0.008)	(0.020)		(0.007)	(0.009)	(0.008)	(0.008)
$(A_F - 60) * (A_F - 60)$	-0.001	-0.000	-0.001	-0.000	-0.235	5.088	0.001*	0.003		0.001	-0.001	-0.000	0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.446)	(5.810)	(0.001)	(0.005)		(0.001)	(0.001)	(0.001)	(0.001)
Joint effect	0.204	0.100	-0.131	0.146	71.736	-1421.634*	-0.096	0.077		-0.188	-0.006	0.392**	0.112
P-value (joint eff.)	0.168	0.336	0.337	0.341	0.305	0.094	0.436	0.737		0.278	0.974	0.022	0.445
Mean value (ineligible HH)	0.582	0.837	0.879	0.514	227.937	1020.327	0.316	0.499		0.753	0.370	0.272	0.211
Bandwidth female	20	20	19	19	18	20	20	16		20	20	20	20
Bandwidth male	20	20	10	15	20	20	20	7		11	20	20	19
Observations	1,229	1,229	678	1,001	1,096	1,227	1,229	455	8,195	763	962	962	1,201

Note: The table shows estimates of a falsification test of pension effects on income, labour supply, consumption and living arrangements obtained estimating the reduced form of equations (2)-(3) with data on 2006 and 2007, before Renta Dignidad was enacted. We regressed each outcome on dummies D_i equal to 1 if spouse i was 60 or older at the time of the survey and 0 otherwise, on their interaction and on the RDD polynomial in the difference in spouse age A_i from the 60 cutoff and interpret estimates as an intention to treat effect (ITT). We observed age at the end of September to study age 60 contemporaneous effects since our outcomes of interest are measured in November and we measured age as follows. Running variables are spouses' age: the integer part measures age in years while the decimal one measures fractions of years. We included as covariates dummies for survey year, ethnicity, education level, ethnicity, health and insurance status, contributory pension reception and urban residence. We corrected standard errors by using the survey weights. At the bottom of the panel estimates of the age 60 effect when both spouses were eligible are shown, along with the associated p-value. The last four rows show mean values of outcomes for households with spouses younger than 60, the optimal bandwidth used in the regressions and the number of observations. Significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data are from a representative survey of Bolivian households held by the National Statistics Institute in 2008 and 2009. Section 2 offers additional information on the institutional setting and on the data, section 4 on the research design and section 6 on the empirical analysis.

rationalised by the substantive and highly significant change in a number of household decisions benefiting elderly beneficiaries directly. In addition, benefits spillover to beneficiaries' extended family by increasing their adult children's labour supply and the probability that grandchildren are looked after by beneficiaries. Benefits also spillover to other households thanks to a net increase in domestic transfers sent to them. These results speak to the recent literature studying how decisions by a program's beneficiaries can both increase income and also improve decisions taken either by extended family members (Chetty and Looney, 2006; Kinnan and Townsend, 2012) or by non-beneficiary households (Angelucci and De Giorgi, 2009; Angelucci *et al.*, 2012; Angelucci and Di Maro, 2015), highlighting the relevant role played by elderly's positive income shocks.

Third, finding that beneficiary gender matters for female spouse labour supply and for interhousehold transfers, but not for other household decisions, such as purchasing medicines, contributes to the literature studying the role of bargaining power in intrahousehold decisions in developing countries (Rangel, 2006; Bobonis, 2009; Attanasio and Lechene, 2014) with novel evidence on an unexplored relationship between gender differences in spouses' bargaining power and interhousehold transfers. This mechanism is, instead, muted when a pension is obtained by both spouses, who seem to act cooperatively in the decision over the absorption of extended family members into the household.

Finally, the net increase in transfers to other households, when only the male obtains the pension, enriches the literaure studying the role of pension income in private transfers crowding out in developing countries, by uncovering gender as a novel mechanism explaining private transfers (Jensen, 2004; Juarez, 2009). In addition, these results enrich the growing evidence on the quantitative relevance of informal contracts between households (Albarran and Attanasio, 2003; Dubois *et al.*, 2008; Angelucci *et al.*, 2012) which, in poor countries, tend to compensate for the poor functioning of formal credit and of public institutions in charge of contracts design and enforcement.

From a policy viewpoint, these results suggest that age targeting may not necessarily result in short-run poverty reduction among elderly, particularly in countries with widespread poverty. Also finding that, when the beneficiary is a male, the pension is transferred to other households in Bolivia suggests that transfers are meant to help households that are at least as poor as pension beneficiaries or poorer ones, in line with related evidence on crowding out (Juarez, 2009) and on income mobility (Stampini *et al.*, 2015). This emphasises the importance of anticipating and quantifying at the design stage potentially unintended policy outcomes. Furthermore, these results contribute to the debate over the importance of carefully assessing the impact that relevant features of the institutional setting in developing economies, for example prevalent gender roles in a household, may have on individual and on household decisions. An illustrative example is targeting female spouses in conditional cash transfer for poor Mexican households, PROGRESA, as costs and benefits of gender targeting are hard to assess precisely unless all relevant behavioural mechanisms, such as women empowerment (Almås *et al.*, 2015), are carefully taken into account.

However, a few caveats should be noted. First, behavioural responses to the pension are observed in the short-run while less is known about their long-run impact, due to lack of data and to the increasing complexity in disentangling indirect from direct effects. Second, the effects estimated in this paper are local around the age 60 cutoff and may be different for beneficiaries at different ages. Finally, despite Renta Dignidad being a universal pension, take up at 60 is around 60%. While we cannot exclude that this is partially due informational frictions in the transition from Bonosol, a pension with age cutoff at 65 and discontinued in 2007, overall is in line with evidence on non-contributory pensions and on conditional cash transfers in other countries. Additional frictions potentially explaining why pension effects on poverty are muted are a government partial inability to implement a program in rural areas and negative self-selection of eligibles by, for example, education. Although similar hypotheses tend to hold in developing countries, we cannot test them due to small sample problems.[35]

In spite of our rich results and their contribution to the related literature and to inform policy decisions, we were unable to answer a number

[35] A broader caveat is that all evidence on non-contributory pensions is obtained by estimating partial equilibrium models. Except recent work on the relationship between cash transfers and sellers price-discriminating (Attanasio and Pastorino, 2015), very little is known about general equilibrium effects.

of highly relevant questions, that are left for future research, partly due to the inability to follow individuals over time in cross-sectional survey data. In particular, given the large spillovers found in this paper, it would be valuable to pin down who and through which channels indirectly benefits from social pensions in households with no beneficiary. Similarly, assessing whether decisions by households receiving one or more pensions soon after its enactment are different from those by households first receiving a pension years later, would be valuable to test whether learning effects play a relevant role in explaining household decisions induced by one or more pensions.

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