Health effects of increasing income for the elderly: evidence from a Chilean pension program

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We estimate the effect of a permanent income increase on the health outcomes of the elderly poor. Our regression discontinuity design exploits an eligibility cut-off in a Chilean basic pension program that grants monthly payments to retirees without a contributory pension. Using administrative data we find that, four years after applying, basic pension recipients are 2.7 percentage points less likely to have died. Survey evidence suggests an increase in food consumption and visits to health centers as relevant drivers of the mortality reduction. (*JEL* I14, I38, J14)

Researchers and policymakers have documented large and ever-widening life expectancy inequalities across income groups in both developed and developing countries (Hoffman, 2008; Brønnum-Hansen and Baadsgaard, 2012; Tarkiainen et al., 2012). For instance, a recent OECD (2018) report shows that, at retirement age, high-income earners live longer than low-income earners: 1.6 years longer in the US, 3.6 in Chile, 3.25 in the UK and 2.9 in South Korea.[1]

Despite a large body of literature documenting that, at all ages, wealthier people enjoy better health on average (Marmot, 2005; Braveman et al., 2010; Waldron, 2013; Chetty et al., 2016), substantial debate remains on whether an income increase for the elderly poor can improve their health. For instance, unobserved characteristics (e.g. genetic factors) could explain both higher income and better health. Alternatively, better health could be the cause of higher income (reverse causality). Differences in health status may also be the result of cumulative conditions related to income inequalities at earlier ages (e.g. exposure to pollution).

The non-contributory pension program in Chile provides an ideal regression discontinuity (RD) design to identify the causal effect of a large permanent in-

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[1] In the report high-income earners are those who earn more than three times the average wage and low-income earners are those who earn half of the average wage or less.

come increase for the elderly poor on their health outcomes. Since 2008, Chileans who are aged 65 or over and do not have a contributory pension can apply to receive a governmental pension, which provides lifelong monthly payments of approximately 40% of the national minimum wage (basic pension). Upon receiving applications, the government calculates a *pension score* and assigns a basic pension to applicants who fall below the 60^{th} percentile (cut-off) of the score distribution.

Our study uses administrative data on basic pension applicants and their household members in 2011 and 2012.[2] This data is paired with their medical history from 2011 to 2016. We first note that the pool of applicants consists mostly of women without a history of regular paid employment (e.g. former stay-at-home mothers). As individuals can apply multiple times, we define applicants whose *first* application score fell *below* (*above*) the cut-off and within a certain bandwidth, as the intent-to-treat (ITT) 'treatment group' ('control group'). We show that density and balance tests cannot reject the hypothesis that the pension is as good as 'locally' randomly assigned between treatment and control group. We then implement an RD analysis to explore the causal ITT effects of the pension on applicants. To estimates the local treatment effect on the treated (TOT), we use the 'recursive' RD estimator suggested by (Cellini, Ferreira and Rothstein, 2010), which explicitly accounts for later successful applications by control group applicants.

Receiving a basic pension reduces applicants' probability of dying by 2.7 percentage points (pp.) within four years of applying, with an ITT income-mortality elasticity of -0.386. The decrease is statistically significant and remains unaffected when using nonparametric estimations and different sets of controls, bandwidths, and polynomial orders.

To shed light on the mechanisms behind this effect, we complement our RD estimation with the analysis of a longitudinal survey conducted by the Chilean Ministry of Labor (Ministerio Trabajo y Previsión Social, 2015). An increase in food consumption and more frequent visits to health centers appear to be relevant drivers of the improvements in recipients' health. Receipt of the basic pension is not associated with a significant change in health insurance coverage or labor supply.

The heterogeneity analysis shows strong health improvements for applicants living without working-age household members and no improvement for those living with working-age relatives. A plausible explanation for this last result is that younger relatives reduce their net transfers of income to applicants after pension payments begin. In line with this hypothesis, we also observe an increase in the fertility of working-age relatives of pension recipients, suggesting that transfers of income to applicants may have been diverted to child-raising expenditures.

Our paper provides causal evidence that a permanent income increase for the

[2]The program did not systematically collect information on applicants and household members before 2011, making it unfeasible to analyze earlier years.

elderly can improve their health at the present time. (Salm, 2011) finds that two pension increases in the early 1900s reduced the mortality rates of US veterans. In modern times, the evidence is mixed: studies have estimated negative (Jensen and Richter, 2003; Barham and Rowberry, 2013), insignificant (Cheng et al., 2016), or even positive (Snyder and Evans, 2006; Feeney, 2017, 2018) income elasticities of mortality.

The confidence interval of our estimate encompasses most of the previous negative point estimates of the income-mortality elasticity.[3] To reconcile our results with the positive estimates, note that (Snyder and Evans, 2006) and (Feeney, 2017) find that higher pension payments increase the probability of retirement, and that (Fitzpatrick and Moore, 2018) show that transition to retirement causes a significant rise in mortality, independently of whether income is affected. As the Chilean basic pension is given mostly to people that are already out of the labor force (e.g. former 'stay at home mothers'), it has a limited impact on retirement transitions. Our analysis is then better able to isolate the negative mortality effect of the permanent income increase from the positive mortality effect of the increase in transition to retirement.

The main policy implication of our results is that non-contributory pensions, intended to improve the living standards of the elderly poor, can also improve their health. Furthermore, a cost-benefit analysis suggests that the basic pension is a cost-effective measure to increase pension recipients' life expectancy. Our results are informative for policymakers who aim to introduce income transfers that target subpopulations similar to our treatment group, which is composed primarily of elderly, low-income women in a middle-income country. Income transfers directed to recipients with different characteristics may have different policy implications, as suggested by the large variance of mortality-income elasticities estimated in the literature.

The paper is organized as follows. Section I presents the basic pension program. Section II describes the data and explains the empirical strategy. Section III provides evidence for the validity of the RD assumptions. Section IV presents the results and the potential mechanisms behind the effects. Section V illustrates the cost-benefit analysis, and Section VI concludes.

I. The basic pension

Since 1980, Chile has had a full-capitalization pension system in which workers must contribute ten percent of their monthly wage into a private pension fund. Upon retirement, workers receive a pension that is dependent on the amount saved over their working life (*contributory pension*). Until recently, those who had never undertaken paid work received no pension.

[3](Lindahl, 2005), (Cesarini et al., 2016) and (Schwandt, 2018) showed mixed results regarding the impact of increases in wealth, such as lottery prizes, on mortality rates amongst the elderly. Although these studies belong to a related literature, the effects of unexpected wealth increases might differ from the effect of a permanent income increase guaranteed by the government.

This system was judged to be particularly unfair to stay-at-home mothers. To address this issue, President Bachelet signed ACT 20255 into law on March 11$^{\text{th}}$, 2008. This Act established that every citizen aged 65 or above with no retirement savings would be eligible for a pension consisting of lifelong monthly payments provided by the government (basic pension). The introduction of the basic pension took place across Chile simultaneously, and the first payments were delivered on July 1$^{\text{st}}$, 2008. Between 2011 and 2016, our period of analysis, basic pension payments were on average 166 US dollars in 2012 prices (80,961 Chilean pesos), corresponding to approximately 40% of the national minimum wage. Throughout the paper, we present all monetary values converted to 2012 US dollar prices for comparability.

The process for applying for the basic pension is free and identical across Chile. Applicants must apply to the Pension Institute by filling in a form in their municipality of residence. Then, the Pension Institute calculates a pension score that is comprised of two factors: household income from assets (e.g. contributory pensions from household relatives) and labor income from all household members. Administrative data shows that these two factors account for 60% and 40% of total household wealth, respectively. The pension score is then adjusted for household size and household members' disability status. To define a household, the Pension Institute follows the government definition: a group of people, related or not, who live in the same house and share income.

The pension score uses richer data and is computed differently from other governmental indices, such as the *social security score*.[4] The calculation of the pension score relies upon administrative information from public agencies (e.g. Revenue Service) and private companies (e.g. pension fund companies), as well as self-reported information. As the pension score requires information from several public and private offices, it is calculated only for people who apply for the pension.

Following the assigning of pension scores, the Pension Institute uses an arbitrary cut-off to determine basic pension recipients. The cut-off has gradually increased from covering the poorest 40% of the elderly population in July 2008, to covering the poorest 60% since July 2011. These gradual changes occurred at the same time nationwide.[5]

After the application decision, applicants observe only whether they will receive the basic pension and, if not, the reason for this decision. They can apply more than once, but they never observe the score assigned to them. The government initially considered reassessing basic pension recipients' eligibility every two years.

[4]The social security score ("Puntaje de la Ficha de Protección Social") is a proxy means test based on household composition, potential income and self-reported actual income that allows the government to assign social benefits. The social security score does not use administrative data on labor income or on income from other sources such as contributory pensions. For more details on the pension score see Appendix Section A.

[5]Appendix Figure A1 shows the timeline of the basic pension reform and the cut-off changes. We find little evidence of applicants delaying their applications to take advantage of the 5% cut-off increase in July 2011 (Appendix Section B).

This policy was never enacted and virtually all pension recipients continued to receive payments every month thereafter.

II. Data and empirical strategy

A. *Pension and health datasets*

Our analysis is based on administrative data provided by the Chilean government. First, we have access to all applications for the basic pension made in 2011 and 2012. For each applicant and each of the applicant's household members, the Pension Institute provided us with demographic information regarding their gender, age, town of residency, household social security score, unique identifier number (henceforth *ID number*), and unique identifier number for the household. This dataset also includes the pension score, application date, and the outcome of the application. The Pension Institute collected all the variables mentioned at the moment of application. It also provided us with the outcome of all applications submitted between 2013 and 2016 for those who applied between 2011 and 2012. We do not have access to applicants' data from previous years, as it was not systematically recorded before 2011.[6]

The applicant and household ID numbers allow us to identify the pension applicant in each household and perfectly match each applicant with all household members. Following the Chilean legal minimum working and retirement ages, we define male household members aged 16-64 and females aged 16-59 years as 'working-age household members', while male household members above 64 and females above 59 years of age as 'elderly household members'.

The Ministry of Health also granted us access to the medical history of each applicant and household member in the Pension Institute dataset from 2011 to 2016, which was perfectly matched using individuals' ID numbers. This dataset contains: the date and cause of any deaths; the date of any childbirth for female household members; the date and type of any vaccinations received; and the date, duration, and cause of any hospitalizations, in both private and public health institutions.

Our study analyzes only those applications submitted between July 1, 2011 and December 31, 2012. We do not use applications submitted prior to July 2011, as the 60$^{\text{th}}$ percentile cut-off point for eligibility was introduced by the government in July 2011 (Section I). The most recent health data to which we have access extends until December 2016. This allows us to measure health outcomes for up to four years from the date of application. As unsuccessful applicants can submit further applications, we count each applicant as a single observation and accommodate later changes in pension status using the 'recursive' RD estimator presented below.

[6]We also obtained household-level data on the factors that determined the pension score and the total household income generated for first applications submitted in 2012. Note that less than 1% of applicants in our working sample share a household with another applicant.

B. Regression discontinuity design

To estimate the causal effect of the basic pension on health outcomes, we use a regression discontinuity design. We estimate the local 'intent-to-treat' (ITT) effect, β_t^{ITT}, using the following equation:

$$(1)\quad y_{i,h,a+t} = \alpha + \beta_t^{ITT} D_{h,a} + g_0(Score_{h,a}) + D_{h,a} \times g_1(Score_{h,a}) + \boldsymbol{\gamma}' \boldsymbol{x_{i,h,a}} + u_{i,h,t+a}$$

where a is the date of the first application and t is the number of years since the first application. We analyze the outcome y up to four years after the first application, so we can consider the cross-section of first applications and estimate β_t^{ITT} at $t \in \{1, 2, 3, 4\}$. Our main tables report β_4^{ITT}, the ITT effect four years after the first application.[7] $\boldsymbol{x_{i,h,a}}$ is a vector of controls for potentially relevant determinants of the health outcomes, including: gender; whether the applicant is vaccinated for pneumonia and influenza; and month-of-application, health-district and age fixed effects. $Score_{h,a}$ is the distance of the first application score from the cut-off point, for the pension applicant of household h. In our preferred specification, g_j (j=0,1) is a polynomial of order 1 in $Score_{h,s}$. $D_{h,a}$ is an indicator equal to 1 if the applicant of household h obtained a pension score below the cut-off in their first application at date a, and 0 otherwise.

Each regression uses triangular kernels, such that the weight of each observation decreases with the distance from the cut-off. The sample is restricted to a bandwidth of 500 points on either side of the threshold. Standard errors are clustered at the province level.[8] We check the robustness of our results to different specifications using polynomials of order 2 in $Score_{h,a}$, nonparametric estimations, logistic regression, different sets of controls, and the mean-squared error optimal bandwidth approach proposed by (Calonico, Cattaneo and Titiunik, 2014).

C. Treatment effect on the treated

Equation (1) estimates the effect of the basic pension on applicants that were 'intended to be treated' at their first application. To estimate the effect of the pension on all applicants that were eventually treated within the four-year period, we need to account for the presence of serial applicants whose first application was rejected but who obtained a basic pension in a successive application. To identify the (local) effect of the treatment on the treated (TOT), we implement the 'recursive' RD estimator suggested by (Cellini, Ferreira and Rothstein, 2010)

[7] Appendix Figures H10 and H13 also show the ITT effect on mortality and fertility within each year following the first application date.

[8] There are 33 health districts and 54 provinces in Chile. The standard errors are clustered at the province level in our preferred specification, since health districts are not sufficiently high in number to employ the law of large numbers and make correct use of clustered standard errors. Provinces serve as a good proxy for health districts, while also being suitably high in number. Clustering at the health districts level does not change the results of our estimates.

and used by (Taylor, 2014), which explicitly accounts for the dynamic nature of the treatment.[9]

We can then write health outcomes for any year t as a function of the full history of application outcomes:

$$y_{i,h,t} = \sum_{s=0}^{4} \beta_s D_{h,t-s} + u_{i,h,t} \tag{2}$$

where $D_{h,t-s}$ is an indicator equal to 1 if the applicant of household h obtained a pension score below the cut-off in year $t-s$, and 0 if either the pension score was above the cut-off or they did not apply in that year. $u_{i,h,t}$ represents all other determinants of the outcome (with $E[u_{i,h,t}] = 0$). The TOT in year t is the effect of exogenously granting a pension to applicant i in year $t-s$ and controlling for the outcome of all successive applications (as though subsequent applications were not allowed). In Equation (2), this is β_s.

When deriving the TOT, it is important to clarify its relationship with the ITT effect in Equation (1). While the TOT is the effect of granting a pension for s years versus not receiving the pension at all for s years, the ITT is the effect of exogenously granting a pension in the first application and allowing unsuccessful applicants to apply again as they wish, potentially obtaining the pension at a later time. Thus, the ITT effect incorporates the effects of $D_{h,t-s}$ operating through the intermediate variables $\{D_{h,t-s+1}, ..., D_{h,t}\}$. The relationship between the ITT effect of $D_{h,t-s}$ on outcome $y_{i,h,t}$ and the corresponding TOT effect is:

$$\begin{aligned} \beta_s^{ITT} = \frac{dy_{i,h,t}}{dD_{h,t-s}} &= \frac{\partial y_{i,h,t}}{\partial D_{h,t-s}} + \sum_{j=1}^{s} \left(\frac{\partial y_{i,h,t}}{\partial D_{h,t-s+j}} \times \frac{\partial D_{h,t-s+j}}{\partial D_{h,t-s}} \right) \\ &= \beta_s^{TOT} + \sum_{j=1}^{s} \beta_{s-j}^{TOT} \pi_j \end{aligned} \tag{3}$$

where $\pi_h = \frac{dD_{h,t-s+j}}{dD_{h,t-s}}$ represents the effect of a successful first application on the probability of another successful application j years later. Since only those applicants who had a rejected first application will go on to apply again, we have $\pi_j < 0$ for all j. If $\beta_{s-j}^{TOT} \leq 0$ for all j years, this implies that $\beta_s^{TOT} \leq \beta_s^{ITT}$.

[9] The usual fuzzy RD is not the appropriate identification strategy in our case, as it assumes that *control* group applicants that receive the pension receive it for the same period as *treatment* group applicants (i.e. four years). With dynamic treatment effects, obtaining a pension in year $a+t$ (with $0 < t < 4$) does not have the same effect on the outcome in year $a+4$ as obtaining the pension at the first application in year a would have. To obtain the ITT effect, (Cellini, Ferreira and Rothstein, 2010) use the entire distribution of the running variable and control for the conditional expectation of the unobserved determinants of outcome given the running variable, by including a high-order polynomial of the running variable. Instead, we obtain the (local) ITT effect by focusing on a small window around the cut-off, as in the paper by (Taylor, 2014).

As in (Cellini, Ferreira and Rothstein, 2010), the identification of the TOT effects from Equation (3) is based on the assumption that the partial effect of a successful application in one year on outcomes in some later year depends only on the elapsed time (s) and not on the application history or the application year. Formally we assume that, although $\frac{\partial y_{i,h,t+s}}{\partial D_{h,t}}$ and $\frac{\partial D_{i,h,t+s}}{\partial D_{h,t}}$ may depend on s, they do not depend on application year or application history $\{D_{h,1}, ..., D_{h,t-1}, D_{h,t+1}, ..., D_{h,t+s-1}\}$. This is a more restrictive condition than the monotonicity and excludability assumptions required by a standard fuzzy RD, because the TOT effects of the pension within a certain period are assumed to be the same between those applicants successful at their first application and those successful at a later application. This would be violated if, for instance, conditional on control variables, serial applicants benefited more (or less) from the basic pension than first-time applicants. The assumption is not required to identify the ITT effects.

To obtain recursive formulas for the TOT effects in terms of β_t^{ITT} and π_t for all t, we can simply invert Equation (3):

$$\beta_t^{TOT} = \beta_t^{ITT} - \sum_{j=1}^{t} \pi_j \beta_{t-j}^{TOT} \tag{4}$$

The recursive estimator thus proceeds in two steps. First, we estimate the coefficients β_t^{ITT} and π_t using regression Equation (1) for each year $t \in \{1, 2, 3, 4\}$.[10] Second, we solve for β_t^{TOT} using recursive Equation (4) and obtain its standard error by the delta method.

D. *Descriptive statistics*

Appendix Table G1 reports descriptive statistics for applicants within 500 score points of the cut-off and at the moment of their first application, as well as for their working-age and elderly household members. There are 8,499 applicants in this bandwidth, representing 17.2% of the entire pool of 49,552 applicants.

This table shows that in our bandwidth 87.1% of applicants are female, which is the result of women being less likely to have a contributory pension. The average applicant's age is around 66.8. This suggests that applications are submitted shortly after reaching the minimum application age (75% of applicants are 65 years old) and that we observe the first application ever made for most of our sample. Regarding the typical household composition, the average applicant lives with at least one working-age household member and one elderly male person.

Pension applicants in the bandwidth are on average below the 40$^{\text{th}}$ percentile of

[10]To estimate π_t, we can use regression Equation (1) after replacing $y_{i,h,s+t}$ with $D_{h,s+t}$. Ideally we would estimate the ITT and TOT effects for each month after the first application. However, determining the standard errors in the TOT estimation becomes too computationally demanding.

the social security score distribution, which corresponds to 10,320 social security score points, an indication that applicants are poorer than the median Chilean.[11] Even though the pension score cut-off is set at the 60$^{\text{th}}$ percentile of the distribution, the average social security score for applicants close to the cut-off is well below the 60$^{\text{th}}$ percentile. This is not surprising, as the pension score considers a more comprehensive set of factors and sources than the social security score (see Section I).

III. RD validity

A. First stage

Panels 1a to 1d of Figure 1 display the probability of receiving a basic pension as a function of the distance of the first-application pension score from the cut-off, within each year following the first application (the 'First Stage'). Virtually all applicants in the bandwidth with a score below the cut-off in their first application (treatment group) received a basic pension in every year following the first application. Conversely, relatively few applicants in the bandwidth with a score above the cut-off in their first application (control group) received a basic pension during the year following the first application, but over the following years, gradually more applicants received the pension.[12] Panel A of Table 1 shows that treatment group applicants have a 78.5 pp. higher probability of receiving a basic pension within the first year, which falls to 42.7 pp. within four years following the first application. This dynamic first stage translates into treatment group applicants receiving pension payments for 2.42 more years than control group applicants.

Panel B of Table 1 shows that being in the treatment group increases average monthly pension income by USD 103 and total income by USD 102 over the four years following the first application (27% of the minimum wage).[13] In the

[11]It is unlikely that the basic pension affected applicants' eligibility for other government transfers. To the best of our knowledge, there are three government transfers that could be received by pension recipients' households aside from the basic pension: the rent subsidy 'Subsidio de arriendo de vivienda', the home renovation incentive program 'Programa de Protección al Patrimonio Familiar' and the household allowance 'Asignación Familiar'. The latter is provided to households whose main worker's monthly income is below 1,574 dollars (765,550 Chilean pesos). The basic pension does not affect eligibility for this, as the pension is by definition not received by a worker. The other two are provided to households with a social security score below the sixth decile of the social security score distribution (13,484 score points). While the basic pension can affect the social security score, it is unlikely to affect the eligibility for these two transfers, as our applicants are likely to be infra-marginal. Applicants at the cut-off have a social security score of 9,385 score points, which is around the third decile of the social-security-score distribution. The basic pension would not be sufficient to push applicants' income above the eligibility cut-off for the first two schemes in 2012.

[12]Control group applicants who re-submit an application tend to be those with a lower social security score and those who live in larger households (Appendix Section C).

[13]For these estimates, we use only data from applications in 2012 as we do not have non-pension income data for applications in 2011 (see Section II). Results on pension income remain very similar if we use data from applications in 2011. The monthly pension income increase is lower than the basic pension amount ($166) because 42.7% of control applicants obtain the pension at a later application

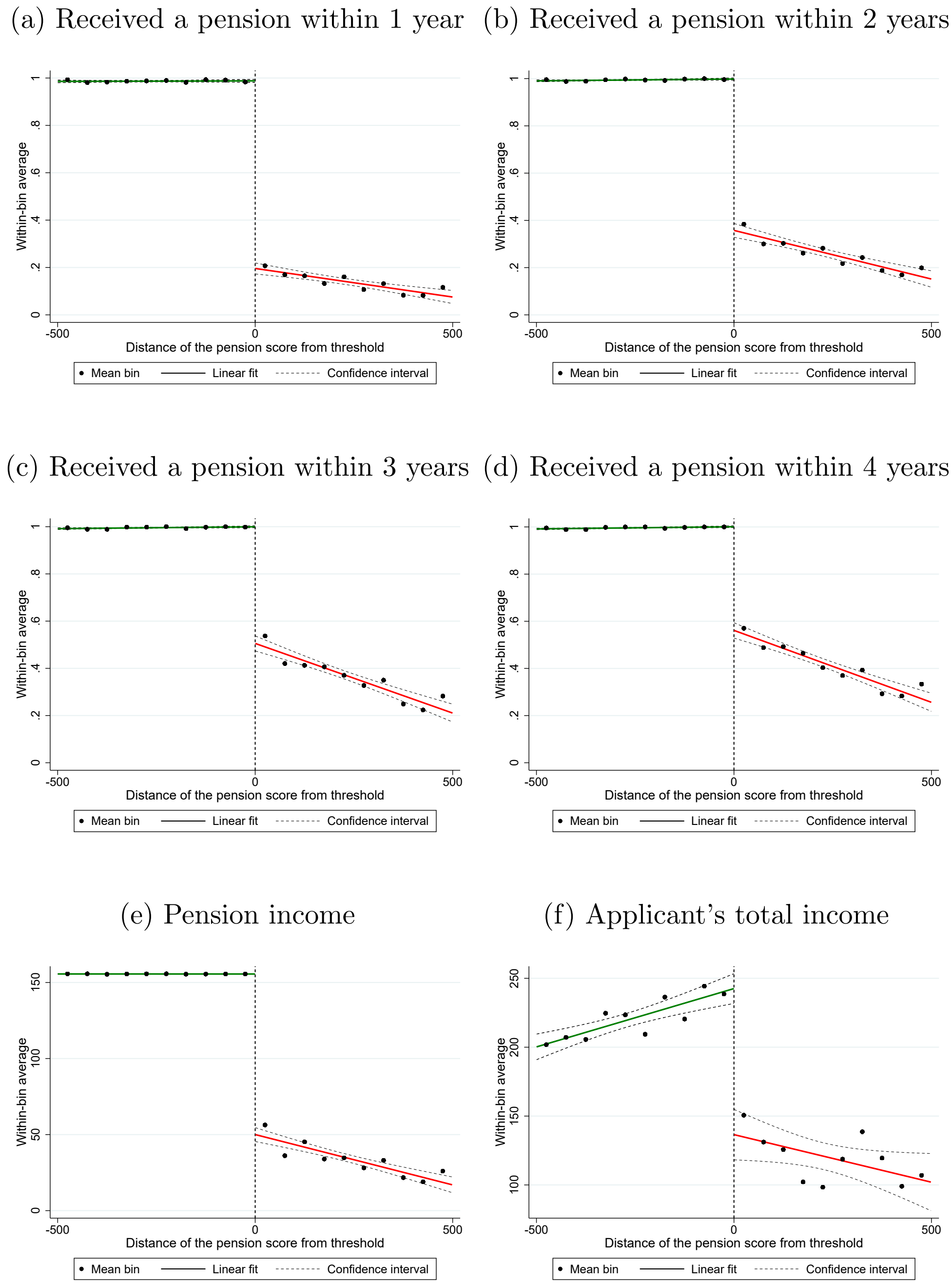

Figure 1. : First-stage effects.

Notes: These figures show the effect of the first-application pension score distance from the cut-off on the applicant's probability of receiving a basic pension within each year following their first application and the applicant's pension and total income. Income estimates are performed on applicants for 2012. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence intervals, respectively.

last two panels of Figure 1 we see that an applicant's total income increases below the cut-off because the pension income is constant and non-pension income is positively correlated with the pension score, but decreases above the cut-off because the decrease in average pension income dominates the increase in non-pension income.

Table 1—: First stage on the probability of receiving a basic pension by year and on income

Variables	ITT Coef. (1)	S.E. (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: time of pension receipt							
Pension in the 1^{st} year	0.785	(0.014)	56.486	0.000	500	8,499	0.203
Pension in the first 2 years	0.632	(0.017)	36.448	0.000	500	8,499	0.367
Pension in the first 3 years	0.483	(0.018)	26.847	0.000	500	8,499	0.517
Pension in the first 4 years	0.427	(0.021)	20.387	0.000	500	8,499	0.574
Years receiving payments	2.419	(0.051)	47.132	0.000	500	8,499	1.356
Panel B: income change (only for 2012 applicants)							
Pension income (2012 USD)	103.640	(3.302)	31.386	0.000	500	4,066	51.958
Total income (2012 USD)	102.148	(10.558)	9.675	0.000	500	4,066	141.062

Notes: This table reports results from OLS regressions of several dependent variables on a treatment dummy indicator and deviation of the pension score from the cut-off. In the first four rows, the dependent variable is a dummy indicator equal to 1 if the applicant received the basic pension within a particular year after their first application. In the fifth row, the dependent variable is the length of time in which the applicant received pension payments within four years from the first application. In the sixth and seventh rows the dependent variables are applicant's monthly average basic pension and total income within four years from the first application, respectively. Income estimates use only applicants in 2012, since we only have non-pension income for them and at the moment of aplication, and are expressed in 2012 US dollars. Column (1) and (2) report the treatment indicator coefficient and its standard error clustered at the province level, respectively. Column (3) and (4) report the t-statistic and p-value of the treatment dummy indicator coefficient, respectively. Column (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression sample, respectively. Column (7) reports the variable mean for control applicants at the cut-off.

B. Continuity of applicants' density and pre-determined covariates

Identification of the treatment effect requires that applicants do not manipulate their first-application pension score in order to receive the basic pension. For instance, this assumption would fail if more motivated applicants, who happen to be healthier, are able to adjust their pension score to fall below the cut-off. To formally confirm the absence of first-application score manipulations, we use the density of applicants in 10 score-point bins as the dependent variable in Equation (1) (McCrary, 2008). The test does not reject the null hypothesis of no disconti-

and because, on average, pension recipients receive the pension 2.4 months after their first successful application. This reduces their monthly pension income over four years, since pension payments are divided over 48 months. An applicant's total income includes both pension and non-pension income and takes into account the full trajectory of pension payments. As we do not observe the full trajectory of non-pension income over the four-year period after applying (we have access to non-pension income only at the moment of application), we assume that non-pension income remains stationary in real terms at its 2012 level (nominally changing with the inflation rate).

nuity in the density of applicants with a t-statistic of -1.019 and p-value of 0.309 (see Appendix Figure H1).

Table 2—: Balancing tests

Variables	ITT Coef. (1)	S.E. ITT (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Female	-0.016	(0.015)	-1.016	0.314	500	8,499	0.890
Age (years)	-0.372	(0.236)	-1.578	0.121	500	8,499	67.57
% days hospitalized	-0.096	(0.071)	-1.344	0.185	500	8,499	0.248
Influenza vaccination	-0.025	(0.020)	-1.281	0.206	500	8,499	0.357
Pneumonia vaccination	0.017	(0.008)	2.019	0.049	500	8,499	0.043
Household size	-0.008	(0.040)	-0.192	0.849	500	8,499	2.634
Social security score	64.69	(181.386)	0.357	0.723	500	8,499	9737
Elderly relative	0.016	(0.018)	0.872	0.387	500	8,499	0.693
Working-age relative	-0.004	(0.018)	-0.214	0.832	500	8,499	0.548
Child under 16	0.002	(0.004)	0.396	0.694	500	8,499	0.006
Municipal income	-2.465	(4.250)	-0.580	0.564	500	8,483	146.7

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

Identification of the treatment also requires comparable treatment and control groups in the RD design. Then, a series of pre-determined characteristics that could affect applicants' health should change smoothly at the cut-off (Lee and Lemieux, 2010). Appendix Figures H2 and H3 graphically shows that pre-determined covariates vary smoothly at the cut-off for applicants. Column (3) of Table 2 reports the results of the t-test performed on the coefficient β_t^{ITT} in Equation (1) (without controls), using as a dependent variable one of the 11 individual and household characteristics at the time of application. This table confirms the results and shows that only 1 out of the 11 estimations (pneumonia vaccinations) is significant at conventional levels. We do not believe that this represents a systematic difference between treatment and control groups around the cut-off, however we do include this variable among the controls in the main specification. Performing these regressions as seemingly unrelated regressions, we cannot reject the hypothesis that the coefficients are all equal to zero. For the covariates used to calculate the pension score, Appendix Table G2 shows that only 1 out of the 14 estimates (imputed income) is significant at the 10% level. The evidence presented above suggests that the basic pension is as good as (locally) randomly assigned around the cut-off, after conditioning on first-application pension score.

IV. Results

A. The effect of receiving a pension on applicants' health

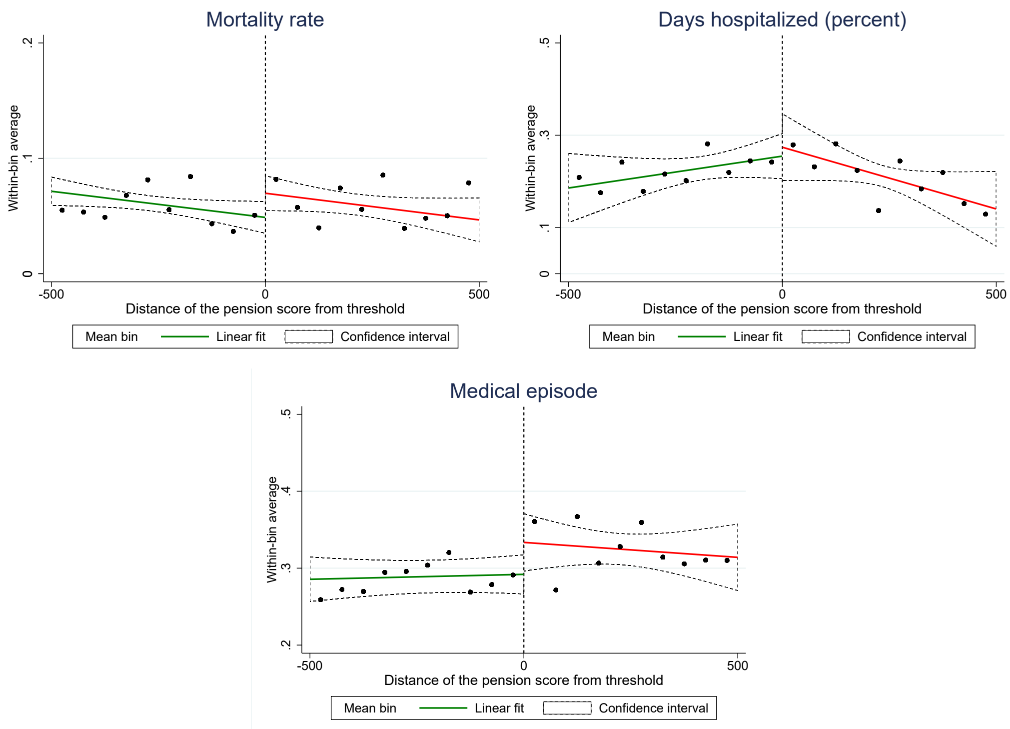

Figure 2. : Effect of the basic pension on mortality, percentage of days hospitalized and medical episodes

Notes: Each graph shows the average value of the corresponding variable conditional on the distance of the pension score from the cut-off. The circles represent averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence interval, respectively.

The top left panel of Figure 2 shows the causal effect of receiving a basic pension from the first application on the probability of dying within four years after applying (henceforth *mortality*). This panel indicates that applicants in the treatment group were less likely to die within four years of applying than applicants in the control group. Column (1) of Table 3 confirms this result and shows that receiving a basic pension significantly decreases the probability of dying by 2.7 pp. The ITT effect of the pension is a 2.0 pp. reduction (p-value=0.045) in the probability of dying from a baseline mortality at the cut-off of 7.0 pp.

Table 3—: Applicants' health outcomes over four years from application

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Mortality rate	-0.027	(0.013)	-0.020	(0.010)	0.045	500	8,499	0.070
% days hospitalized	-0.042	(0.066)	-0.006	(0.051)	0.905	500	8,499	0.274
Medical episode	-0.060	(0.024)	-0.039	(0.017)	0.025	500	8,499	0.333

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) reports the *treatment on the treated* coefficient as in Equation (4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the variable mean for control applicants at the cut-off.

Appendix Figure H8 suggests that the mortality effect manifests itself approximately one year after the first payment and grows almost monotonically over time, reaching a maximum at the end of the studied period.[14] This can have relevant policy implications in the context of a middle-income country: increasing income can improve the health of the elderly, even at a late stage in life.

Since the basic pension affects the probability of dying, we cannot estimate its causal effect on the raw number of days of hospitalization. To partially account for the survival bias, we divide the number of days of hospitalization by the number of days alive, excluding the final six months observed.[15] We find that the basic pension reduces the percentage of days spent in hospital, but the reduction is small and insignificant.

We summarize treatment effects on health outcomes by using as an outcome variable a dummy indicator equal to 1 if the applicant has either been hospitalized or died in the four years after applying (hereafter 'medical episode'). Column (1) of Table 3 shows that treated applicants are 6.0 pp. less likely to experience a medical episode in these four years, a result that it is statistically significant and not affected by the survival bias.

Appendix Section E shows that results on mortality and medical episodes remain significant when using different specifications and bandwidths. When including all available controls, the p-values are slightly higher, but the effects remain significant. Also, these effects are well powered according to the approach by (Gelman and Carlin, 2014) and do not seem to appear in other parts of the pension score distribution.

[14]Appendix Figure H10 confirms that the impact on mortality does not appear in the first year after the application, but rather becomes evident from the second year.

[15]The raw number of days of hospitalization for applicants on each side of the cut-off is not comparable, as those above the cut-off have fewer days available to be hospitalized due to their higher mortality rate. The survival bias would mechanically increase the point estimate (attenuation bias). Dividing the number of days in hospital by the number of days alive partially corrects the survival bias, as it compares shares rather than absolute numbers. Excluding the last six months observed prevents this variable from simply becoming an indicator of mortality.

B. Discussion on the mortality effect

Tables 1 and 3 show that the basic pension increases recipients' income by 72.4% (102/141) and reduces their mortality by 28% (0.020/0.07), respectively. Therefore, we estimate an ITT income-mortality elasticity of -0.386, which represents the percentage change in mortality over four years due to a 1 percent increase in income at the cut-off following a successful first application for the basic pension.[16]

Figure 3 shows that the confidence interval of our estimate encompasses all the negative income-mortality elasticity estimates obtained from previous papers. Our point estimate is slightly below the median negative elasticity estimated in the literature.[17] These include estimates for different countries and historical periods, such as Russia and Mexico in the late 1990s (Jensen and Richter, 2003; Barham and Rowberry, 2013), the United States in the 1900s (Salm, 2011) and women in the United States in the 1970s (Snyder and Evans, 2006). Although our analyzed time span is limited by data availability, it is similar to those used in other income-mortality elasticity estimates.[18]

The positive estimates by (Snyder and Evans, 2006) for men and by (Feeney, 2018) are notable exceptions. (Snyder and Evans, 2006) estimate that a notch in US social security payments for the cohorts 1916-1917, which reduced the later cohort's income, significantly *reduced* men's mortality rates in comparison to the wealthier cohort. They justify this result by showing that the poorer cohort retired *later*, reducing their social isolation and improving their health outcomes. (Feeney, 2017, 2018) exploits the age eligibility cut-off and the staggered introduction of a Mexican non-contributory pension across small rural towns, finding that this pension increases recipients' transition to retirement and mortality rates.

[16]As mentioned earlier, the percentage change in income takes into account baseline non-pension income and the full trajectory of pension payments received by control and treatment group applicants in 2012. Ideally, we would compute the elasticity using the full trajectory of non-pension income as well. However, we have no information on how non-pension income changes after the application, and so we assumed that non-pension income remains stationary in real terms at its 2012 level.

[17]As the majority of estimates in the literature are based on an individual measure of income (Snyder and Evans, 2006; Salm, 2011; Barham and Rowberry, 2013; Cheng et al., 2016), we use the applicant's income to compute the income-mortality elasticity. We use the ITT estimate for consistency with the majority of the estimates in the literature.

[18](Snyder and Evans, 2006), (Feeney, 2018) and (Barham and Rowberry, 2013) use comparable time spans, while (Jensen and Richter, 2003) and (Cheng et al., 2016) use shorter periods. (Salm, 2011) is the only paper to analyze a period longer than four years.

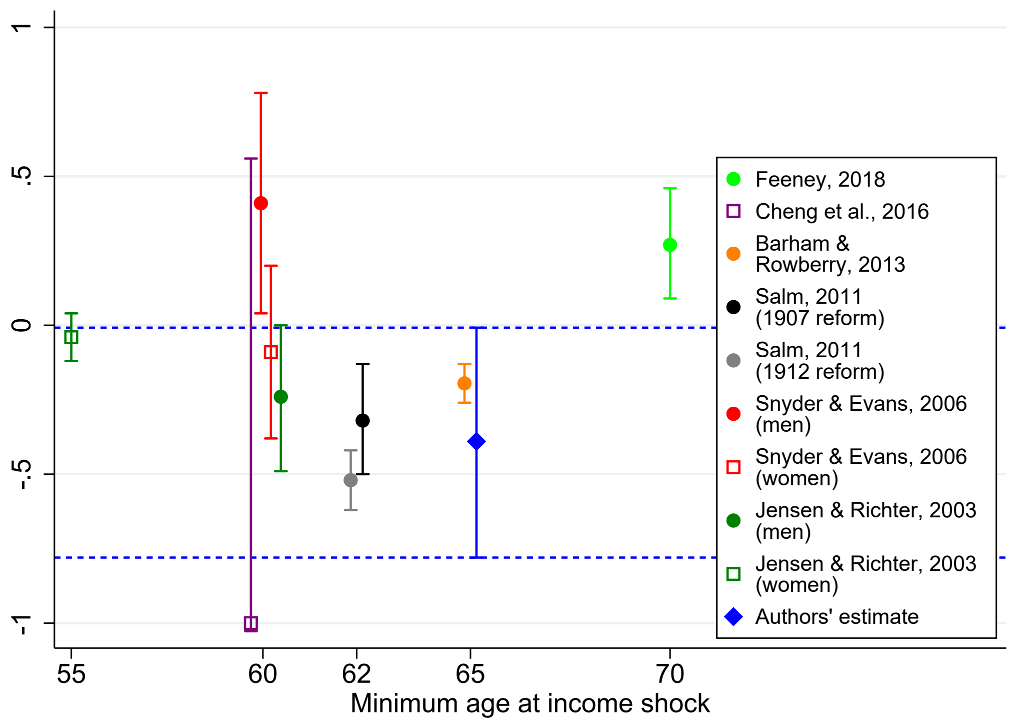

Figure 3. : Estimates of income-mortality elasticity of elderly

Notes: This graph plots point estimates and confidence intervals of income-mortality elasticity on the minimum age at which the income shock commenced. Empty squares indicate insignificant estimates. The dashed lines indicate the 95% confidence interval of our estimate. Elasticities in the other papers were computed using different measures of baseline income: (Feeney, 2018) household income; (Cheng et al., 2016) average per capita net income among potential beneficiaries; (Barham and Rowberry, 2013) average beneficiary income in rural areas; (Salm, 2011) average monthly earnings for non-farm employees; (Jensen and Richter, 2003) household income; (Snyder and Evans, 2006) individual income. Where possible, estimates were separated by gender.

Differences in 'pre-pension' labor market participation levels can explain the opposite sign of our estimate. Basic pension applicants cannot have a history of formal employment (e.g. former stay-at-home mothers), and so arguably the pension induced very limited labor supply effects, as shown in Section IV.D below. On the other hand, a high fraction of recipients in (Feeney, 2017, 2018) and (Snyder and Evans, 2006) were workers induced to retire because of the income increase.[19] (Fitzpatrick and Moore, 2018) showed that the transition to retirement causes a significant jump in mortality due to the fall in labor supply, independently of whether income is affected. There is also evidence that transition to retirement

[19] (Gelber, Isen and Song, 2016) studies the same pension notch as (Snyder and Evans, 2006) and also provides evidence of elderly labor supply responses to the pension increase.

is associated with changes in consumption patterns and lifestyles (Browning and Meghir, 1991; Fitzpatrick and Moore, 2018), along with social isolation (Snyder and Evans, 2006), and all of these factors are positively associated with mortality. Thus, our estimate can better isolate the negative mortality effect of the permanent income increase from the positive mortality effect of the increase in retirement.

C. *The heterogeneous effects of receiving a pension on applicants*

The pension may have different health effects depending on the recipient's characteristics. Appendix Table G3 shows that the effects are significantly negative for female applicants and insignificantly positive for males. However, as males constitute a small fraction of our sample, the standard errors are too large to detect a statistically significant difference in the effects across gender.

Following the medical literature on aging and mortality, which stresses the importance of living arrangements (Hawton et al., 2011; Garre-Olmo et al., 2013), we explore another potential pattern of heterogeneity: the household structure of the applicants. Living with children can result in stronger financial assistance for the elderly (Shi, 1993) and affect their compliance with social and health norms (Rogers, 1996; Manzoli et al., 2007). Reciprocal support between children and parents can last throughout the entire lifespan.

Table 4—: Applicant's health outcomes over four years from application by household structure

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: applicants not living with a working-age household member								
Mortality rate	-0.055	(0.020)	-0.044	(0.015)	0.006	500	3,647	0.094
% days hospitalized	-0.157	(0.074)	-0.085	(0.042)	0.047	500	3,647	0.309
Medical episode	-0.128	(0.051)	-0.091	(0.039)	0.023	500	3,647	0.352
Panel B: applicants living with working-age household members								
Mortality rate	-0.007	(0.013)	-0.004	(0.010)	0.686	500	4,852	0.049
% days hospitalized	0.053	(0.111)	0.053	(0.081)	0.518	500	4,852	0.245
Medical episode	-0.007	(0.050)	-0.000	(0.035)	0.990	500	4,852	0.318

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) reports the *treatment on the treated* coefficient as in Equation (4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the variable mean for control applicants at the cut-off.

Table 4 shows that treated applicants living without a working-age household member are strongly affected by the receipt of the basic pension, with a significant reduction in their mortality rate of 5.5 pp. The pension also significantly reduces their percentage of days spent in hospital by 0.157 pp., and the probability of a medical episode by 12.8 pp. Conversely, Panel B suggests that those living with at least one working-age household member remain unaffected by the receipt of the basic pension, with a small and insignificant reduction in their mortality rate and medical episodes. The difference between the coefficients in the two groups are statistically significant.[20] These heterogeneous results are in line with (Cheng et al., 2016), who report significantly larger beneficial health effects for pensioners living alone or with a spouse than for pensioners co-residing with other adults.

The insignificant effects on applicants living with working-age relatives could be the result of working-age relatives reducing their net transfers of income to applicants after pension payments began, as has been suggested by previous papers (Cox and Jimenez, 1992; Juarez, 2009). This seems a plausible mechanism, considering that the fraction of Chilean elderly people approaching retirement age who expect transfers from their children to finance their retirement is twice as large for those who live with working-age household members than for those who do not (36% and 18%, respectively). [21]

Which diseases drive the effects?

Appendix Table G7 shows that the effects appear to be driven mainly by a reduction in the probability of experiencing a medical episode caused by respiratory diseases or tumors.[22] Circulatory and digestive/nutritional medical episodes appear to play a less relevant role in health improvement for pension recipients, but we do not have sufficient power to find a significant difference with respect to the estimate for respiratory diseases in the sample of applicants living without working-age household members. As expected, the basic pension does not reduce the occurrence of medical episodes that are less directly connected to individual behavioral choices, such as transport accidents, although the probability of dying due to an accident is low in our sample.

[20]These subsamples appear to be locally comparable. First, test statistics for the McCrary test are -0.486 and -0.976 for applicants living with and without a working-age relative, respectively (Figure H9). Second, applicants living with and without a working-age relative have a significant imbalance in 0 out of 10 and 1 out of the 10 pre-determined covariates, respectively (Table G4). The mortality and medical episode results are robust to the use of different specifications (Appendix Tables G5 and G6), and remain significant at the 5% level when adjusting p-values by the number of hypotheses that we tested (Romano and Wolf, 2005*b*).

[21]The majority of people close to retirement age expect 'to finance their retirement with the help of the government': 50% of those who live with working-age household members, and 60% of those who do not. Transfers from children are the next most likely expected source of retirement income. These percentages are obtained using the 2004 and 2006 survey waves of the EPS survey (Ministerio Trabajo y Previsión Social, 2015), where we identify individuals who applied for the basic pension after 2008 and consider how they planned to finance their retirement.

[22]Applicants can have multiple causes for a medical episode. For instance, a person first hospitalized due to a respiratory disease and then due to a tumor would have both causes recorded for this analysis. The decrease in respiratory episodes does not appear to be driven by a significant increase in influenza or pneumonia vaccinations (Appendix Table G8).

D. Mechanisms behind the effects

Since our administrative data does not contain information on consumption, labor supply or health insurance coverage, we rely on survey evidence to shed light on the potential mechanisms underlying the estimated effects. We exploit the social benefits longitudinal survey (EPS) conducted by the Chilean Ministry of Labor (Ministerio Trabajo y Previsión Social, 2015), which is representative of the population aged 18 or older. We use the available EPS waves (years 2004, 2006, 2009, 2012 and 2015) which provide information on the respondent's age, health insurance affiliation, employment status, smoking and drinking habits, self-reported health, whether the respondent applied for and obtained the basic pension, and the number of times they had visited a health center in the last two years. We restrict the sample to the panel of 1,288 individuals who report to have applied for the basic pension between 2009 and 2015 and were sufficiently old to be pension recipients in 2015.[23] The EPS also consistently provides information about household income as well as monthly expenditures on food, clothes, utilities, transport, domestic services, medicine, and children's education. We then estimate the following fixed effect regression:

$$y_{i,s,a} = \alpha_0 + \beta Pension_{i,s,a} + \gamma_i + \gamma_s + \gamma_a + \varepsilon_{i,s,a}, \tag{5}$$

where $y_{i,s,a}$ is the outcome of interest for individual i in survey year s and at age a. $Pension_{i,s,a}$ is a dummy variable indicating whether individual i obtained the pension in survey year s and at age a. γ_i, γ_s and γ_a are individual, year-of-survey and age fixed effects, respectively. Standard errors are clustered at the individual level. β estimates the correlation of outcome y with receiving a basic pension after controlling for age, year of survey and unobserved time-invariant heterogeneity across applicants.

Panel A of Appendix Table G9 shows that when applicants were aged 60-64 (henceforth future applicants) less than 2% had private health insurance. Furthermore, full basic pension payments were not sufficient to purchase the cheapest private health insurance plan available at that time.[24] The table also shows that less than 16% of future pension recipients spent at least one hour in informal work in the week prior to the survey.[25] Amongst future recipients, 77% had visited a

[23]This is an unbalanced panel, as some individuals have a missing value for some survey questions, or were not surveyed in some particular EPS waves.

[24]According to the price comparison website 'Queplan' (`https://queplan.cl/`), in 2012 the cheapest private insurance plan in Chile had an average monthly cost of $175 for a 65-year-old and $218 for a 69-year-old. Moreover, public health insurance is free and available for all Chilean residents. Except for a few exceptions (e.g. members of the military force), every person without private health insurance is enrolled in the public system.

[25]The survey does not allow for distinguishing between formal and informal work (formal work being defined as a job eligible for mandatory social security payments). However, as a condition of eligibility for the pension, future pension recipients could not have been employed in formal work. We therefore interpret the fraction of future pension recipients doing at least one hour of work as the fraction of future pension recipients doing informal work.

health center in the last two years, but only 22% reported having bad health.

Panel A of Table 5 shows the results of the fixed effect regression analysis for variables measured at the individual level. The basic pension is not associated with a significant change in private health insurance coverage or employment status, which suggests that these factors play a minor role in the estimated mortality reduction. If anything, the basic pension income effect would be expected to incentivize retirement, and this should in turn *increase* mortality, according to the findings by (Fitzpatrick and Moore, 2018). On the other hand, the basic pension is associated with a significant increase in the probability of visiting a health center and in the actual number of health center visits during the preceding two years (by 6.62 pp. and 2.77 visits, respectively).[26] Since we estimate a negative insignificant impact on hospitalizations (Section IV.A), the increase in the number of visits to a health centre can be interpreted as an increase in outpatient care. The medical literature has shown that outpatient care is crucial in the prevention and treatment of most diseases, including respiratory diseases and tumors, and it is conducive to better health status and lower mortality (Rennard, 2004; Shi et al., 2005; Starfield, Shi and Mackinko, 2005). Panel B also shows that monthly household expenditure on drugs increases by 26% with the receipt of the pension. Although this increase is not statistically significant, it could indicate that the basic pension enhanced adherence to medical treatment.

We also find an insignificant decrease in self-reported 'bad' health, although the high fraction of 'middle' responses for self-reported health (around 50%) provides little variation across survey waves and may be an indication of inaccurate reporting (Greene, Harris and Hollingsworth, 2015).

Panel B of Table 5 shows that upon receiving the basic pension, both monthly household income and expenditure significantly increases by $131 and $115.6, respectively. The basic pension amount is slightly higher ($166), but it remains within the 95% confidence interval of the estimated household income increases.[27]

[26]In waves 2004 to 2009, respondents are asked how many times they visited a health center in the past two years and to select from the reasons provided: general consultation, consultation with a specialist, consultation with a dentist, emergency, laboratory exam, X-ray examination, surgery, and hospitalization. In waves 2012 and 2015 there is only one general question asking how many times they had visited a health center in the last two years. We aggregate the 2004 and 2009 questions in a single variable and assume it is comparable to the generalized question in 2012 and 2015. Results are qualitatively unchanged if we use more restrictive definitions of visits to a health center for the 2004 and 2009 waves. The increase in medical visits is insignificant if we focus only on visits to a GP in the last two years.

[27]We find a marginal propensity to consume equal to 0.88 for recipients' households, which is on the higher end of the range of previous empirical estimates (0-0.9) and is in line with evidence that consumers with low liquid assets show stronger consumption responses to income shocks (Agarwal and Qian, 2014; Carroll et al., 2017).

Table 5—: Fixed effect regressions for people who applied for the basic pension

Variables	Pension coefficient (1)	S.E. (2)	P-value (3)	Observations (4)
Panel A: individual level variables				
Private health insurance	-0.001	0.005	0.894	4124
Informal work	0.038	0.025	0.125	4166
Visited a GP	0.001	0.038	0.978	4217
Visited a health center	0.066	0.034	0.053	4199
Visits to health center	2.777	1.275	0.029	4199
Bad Health	-0.011	0.034	0.740	4217
Smoked, last month	-0.014	0.021	0.487	3509
Number of cigarettes, last month	5.303	5.233	0.311	3509
Drunk alcohol, last month	0.029	0.026	0.272	3509
Number of drinks, last month	0.193	0.176	0.272	3505
Panel B: household income and expenditure in 2012 US dollars				
Monthly income	130.501	44.561	0.003	4221
Total expenditures	115.568	51.787	0.026	4221
Food	25.805	10.966	0.019	4070
Clothes	7.280	3.350	0.030	4034
Utilities	64.486	49.446	0.192	4107
Transport	6.567	3.852	0.088	4037
Domestic services	0.448	0.930	0.630	4126
Drugs	7.077	4.491	0.115	3960
Children's education	6.314	3.039	0.038	4221

Notes: This table reports results from regressions of several dependent variables on a basic pension dummy indicator, as well as individual, survey wave and age fixed effects. Column (1) reports the basic pension dummy indicator coefficient. Columns (2) and (3) report the standard error, clustered at the individual level, and the p-value of the pension coefficient. Column (4) reports the number of observations used in the regression. 'Visited a health center' is a dummy variable equal to 1 if the individual had at least one appointment at a health center in the last two years. Income and expenditure variables are reported in 2012 US dollars. Total expenditures refers to the sum of the expenditures reported in the table. Data is from the panel survey conducted in 2004, 2006, 2009, 2012, and 2015 by the Ministry of Labor.

As in previous studies, the pension income increase is associated with a significant increase in household consumption of food (Duflo, 2000; Jensen and Richter, 2003; Salm, 2011), without a significant change in drinking or smoking habits (Cheng et al., 2016).[28] Higher nutrient intake can improve the functioning of the immune and respiratory systems (Chandra, 1997; Hu and Cassano, 2000), and can also reduce the risk of developing tumors and help the elderly to sustain invasive tumor treatments, such as chemotherapy (Hurria et al., 2011; Fiolet et al., 2018). This is particularly relevant considering that low-income elderly adults in

[28]Data on drinking and smoking behaviors is not available for the 2012 wave. We also observe a large but imprecisely estimated increase in expenditures on utilities. The vast majority of urban Chilean families already have access to electricity, potable water, and sewerage ($>$ 95%)Valenzuela and Jouravlev (2007); División de Acceso y Desarrollo Social (2019)). An increase in utilities may have been health conducive if, for instance, the basic pension was spent on heating during winter, but we are unable to test this hypothesis. Furthermore, less than 1% of households with a future applicant pay for a nurse to provide formal care, leaving little room for this as a potential mechanism.

Chile show a high prevalence (40%) of food insecurity. This is an index based on factors of insufficient food intake (e.g. going to bed hungry), insufficient food quality (e.g. low food variety), and anxiety and uncertainty about the food supply in the home (Atalah, Amigo and Bustos, 2014).

Finally, we see that expenditure on children's education significantly increases with the beginning of pension payments. Appendix Section F expands our RD analysis and provides additional evidence of spillover effects. The basic pension significantly increases the probability of having a child by 2.4 pp. for working-age household members and by 9.8 pp. for fertility-age women living with a pension recipient. On the one hand, the pension might have reduced the cost of raising children thanks to the help of more financially autonomous grandparents. On the other hand, since children can be considered as 'normal goods' (Becker, 1960), fertility ought to increase when higher income is available. Upon receiving the pension, recipients may have seen a reduction in transfers of income from their working-age relatives, as in (Cox and Jimenez, 1992) and (Jensen, 2003), or they may have transferred part of the pension amount to working-age household members, as in (Duflo, 2000).[29] In both cases, intra-household transfers of income between recipients and younger relatives could explain the presence of spillover effects on fertility and the absence of mortality effects on recipients living with working-age household members shown in Section IV.C.

V. Cost-benefit analysis

The estimated impact on mortality allows us to compute the basic pension cost that is necessary to increase the life expectancy of recipients and to compare it with the value of statistical life as estimated in the literature. For the basic pension program to pass a cost-benefit test in terms of life expectancy, the associated increase in the value of statistical life must exceed the monetary costs of the policy (Viscusi, 1994).

Table 6 shows that the basic pension increased recipients' life expectancy by around 4 months, and that it had an expected cost to government of $16,068.[30] Assuming the life expectancy gain is linear in the government transfer, the cost to government for an additional year of life was $50,697. To compare this with previous estimates of the value of statistical life, we multiply the cost by the

[29] This last hypothesis would need to be reconciled with survey evidence showing that only 4% of pension recipients share more than one-fifth of their pension with others (Ministerio Trabajo y Previsión Social, 2017).

[30] Life expectancy is measured by counting the observed years of life from the first application date until the observed date of death. If applicants are alive four years after the application date, we add the expected remaining years of life for their corresponding age-gender group in the Chilean population (Superintendencia de Pensiones, 2014). We assume that the expected years of life after the observed time span are the same for surviving pension recipients and for non-recipients, conditional on age and gender, and that the pension status remains unchanged. To measure expected cost, we multiply the pension amount received by ITT treatment and control applicants by the number of months that they receive the basic pension and are expected to live, discounted by an annual rate of 0.03. We cannot estimate the TOT effect, as we would need to estimate the probability of a successful application in each year after the first application, and data on successful applications after 2016 is not available.

average life expectancy for applicants close to the cut-off (20.09 years) and obtain a value of 1.01 million dollars. This is less than the value of statistical life at 62 estimated by (Aldy and Viscusi, 2008) for the US (5.02 million), and on the lower end of estimates for Chile, which range from 0.87 to 4.63 million dollars (Bowland and Beghin, 2001; Parada-Contzen, Riquelme-Won and Vasquez-Lavin, 2013). Our analysis suggests that the basic pension was cost-effective in increasing the life expectancy of recipients close to the cut-off, as its cost was not higher than most estimates of the value of statistical life reported in the literature.

Table 6—: Cost benefit analysis

Variables	ITT (1)	S.E. ITT (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Exp. lifetime income	16,068	(666)	24.12	0.000	500	8,499	15,070
Life expectancy	0.319	(0.146)	2.181	0.034	500	8,499	19.64

Notes: This table reports ITT effects on expected lifetime pension income (in 2012 US dollars) and expected life expectancy (including the observed four years since application date) on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the *intent-to-treat* coefficient, its standard error clustered at the province, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports variable mean for control applicants at the cut-off.

VI. Concluding remarks

Using a regression discontinuity design, this paper shows that permanently increasing the income of the elderly poor reduces their mortality rates within four years. In a longitudinal survey analysis, we find that the pension income increase is accompanied by an increase in recipients' food consumption and visits to health centers. Both of these factors are relevant in improving health outcomes: higher nutrient intake can help to improve the functioning of the immune and respiratory systems, while also preventing tumor development and allowing people to better sustain invasive treatments; and visits to health centers, which could be interpreted as outpatient care, can improve overall health status and lead to decreases in mortality from several causes.

Consistent with previous papers, the beneficial effects of the pension are concentrated on pensioners living alone or with their spouse. The absence of working-age household members appears to be an important factor in financial fragility for the elderly, making the income shock particularly beneficial for this group of applicants. The insignificant impact on applicants living with working-age household members could be result of reductions in net transfers of income to pension recipients. Evidence of spillover effects on the fertility of working-age relatives further suggests the presence of intra-household transfers that could explain the heterogeneity of the results.

Our study provides evidence that health inequalities in the elderly population are driven in part by contemporaneous income inequalities. In a cost-benefit analysis, we also show that the basic pension is a cost-effective measure to increase life expectancy, as the costs to government are lower than the benefits in terms of value of statistical life. The key policy implication is that non-contributory pension programs, intended to improve the living standards of the elderly poor, can effectively improve their health, and this should be taken into account when similar policies are considered for implementation.

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Online Appendix

Health effects of increasing income for the elderly: evidence from a Chilean pension program

Enrico Miglino, Nicolás Navarrete H., Gonzalo Navarrete H., Pablo Navarrete H.

APPENDIX A The pension score

The pension score was created solely to determine basic pension recipients and has no further use for other public agencies. This score is calculated as follows:

$$\text{(A1)} \qquad Pension\ score_g = \frac{\sum_{i}^{n_g}\{\,Y_{i,g} + YP_{i,g}\}}{IN_g} \times F$$

Where:

- $Y_{i,g}$ is the labor income for person i in household group g.
  - For elderly household members, the National Revenue Service provides this information. In cases where Revenue Service records do not show any income from a particular person, the Pension Institute uses the self-reported measure collected from the social security score.
  - For working-age household members, labor income is imputed using a variation of the Mincer equation (also referred to by its Spanish name, "capacidad de generar ingreso" or CGI), which includes gender, level of education, town of residence, among other variables. This number is estimated by the Ministry of Planning and the equation is not known to the public. In this way, the government avoids score manipulations by working-age household members not reporting their full income or leaving their employment.
- $YP_{i,g}$ is income from other pensions, government transfers, financial assets and any other income source not considered in $Y_{i,g}$ for person i in household group g. The National Revenue Service, the Ministry of Planing, banks and the private companies administering the pension funds provide this information. If these institutions do not show any record for a person, the

Pension Institute uses the self-reported measure collected from the social security score.

- IN_g is the household size of household g, adjusted by the level of disability of each household member. This index is computed as the sum of people in the household, with household members above the age of 65 and those in the national register of disabled persons adding an extra 0.4 and 1.3 points to this index, respectively.

- n_g is the number of people in the household group g.

- F is a transformation factor used to convert the results to the scale of the pension score. This factor is not publicly available and is not available to us.

For 2012 applicants, labor income from household members and income from assets represent on average 40% and 60% of the numerator of the pension score, respectively. This shows that wealth in the form of other pensions or financial assets seems to be the most relevant factor in the pension score for the average applicant, with labor income being relatively less important.

For applicants who submitted an application in 2011 or 2012, the pension score runs between 0 and 43,103 score points. To determine the 60$^{\text{th}}$ percentile for the Chilean population in 2011, the Pension Institute used data from the national household survey and estimated a pension score for each household in the survey. The cut-off then corresponds to the 60$^{\text{th}}$ percentile of the estimated pension score for the sample of households in the survey. There have been no updates to the pension score cut-off since July 2011, when the 60$^{\text{th}}$ percentile was estimated at 1,206 pension score points.

Overall, the majority of the elderly population who did not receive a contributory pension applied to receive a basic pension. In 2011, 64.3% of retirees without a contributory pension received a basic pension (Ministerio de Desarrollo Social, 2011) and an extra eight percent of those without a contributory pension submitted an unsuccessful application according to our records. Appendix Table G10 shows the characteristics of the elderly population without contributory pensions in 2011.

Pension payments

Monthly income from the basic pension has been adjusted yearly at a level that is around the inflation rate, except in 2009, when the increase was well above the inflation rate. Appendix Figure A1 shows the evolution of the cut-off and pension payments, along with their dates of changes. This figure also shows the years for which we have data.

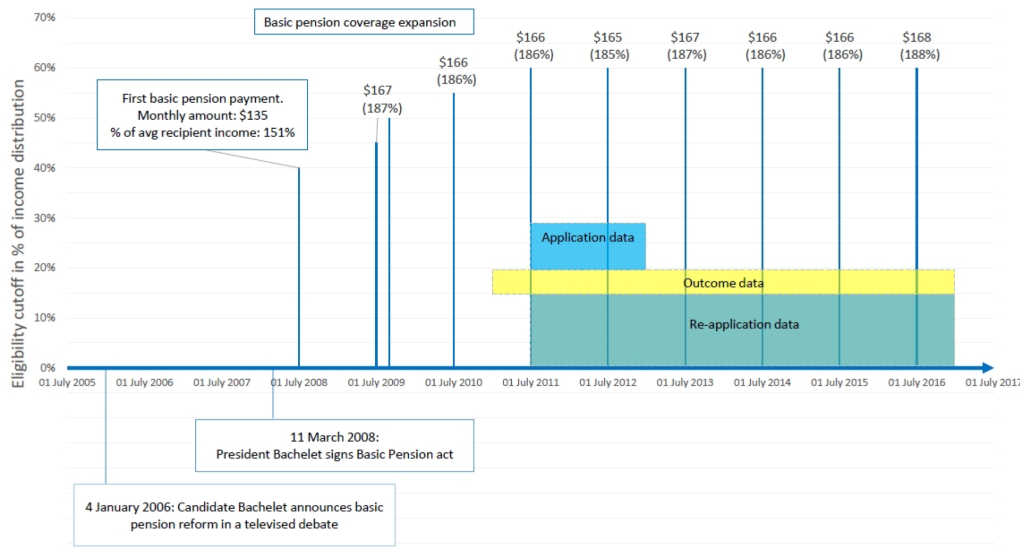


Figure A1. : Timeline of the basic pension reform

Notes: This figure shows the evolution of the basic pension reform, the expansion of its coverage and monthly payment amounts from 2008 onwards. Dates, eligibility cut-off points, and payment amounts are reported by the Chilean Pension Institute. Payments are in 2012 US dollars. To obtain payments in 2012 US dollar, we transformed the nominal value of the payments into 2012 Chilean pesos using the consumer price index and converted this amount into US dollars using the 2012 exchange rate. In parentheses, we report payments as percentages of the average recipient's income at the cut-off in 2012. The 'outcome data' horizontal bar represents the timeframe for which we have outcome data (January 2011 to December 2016). The 'application data' horizontal bar represents the timeframe in which we analyze the first applications of the applicants (July 2011 to December 2012). The 're-application data' horizontal bar represents the timeframe for which we have data on applications for the applicants that re-applied after a first application (July 2011 to December 2016).

Basic pension payments can be received by bank transfer or collected in person with an ID card. In our sample, 96% of recipients collect their pension in person. This indicates that the pension payments are effectively being received by applicants.

Basic pension payments cease if the recipient spends more than 90 days abroad in a single calendar year. The person can apply again, but they will need to prove 270 days of continuous residency in Chile in the year before applying. Payments also cease if the recipient does not collect any pension money within six months. In this case, recipients of the basic pension have another six months to request that the Pension Institute restore their payments. If this is not done, the basic pension expires and people in this category can apply again for a basic pension without any restriction. Finally, payments immediately cease when the pension recipient dies.

Less than 0.05% of recipients who obtained the basic pension between 2008 and

2015 stopped receiving it at some point (Subsecretaría de Previsión Social, 2015). All of these were for reasons unrelated to the pension score (e.g. emigration).

APPENDIX B Anticipating behavior

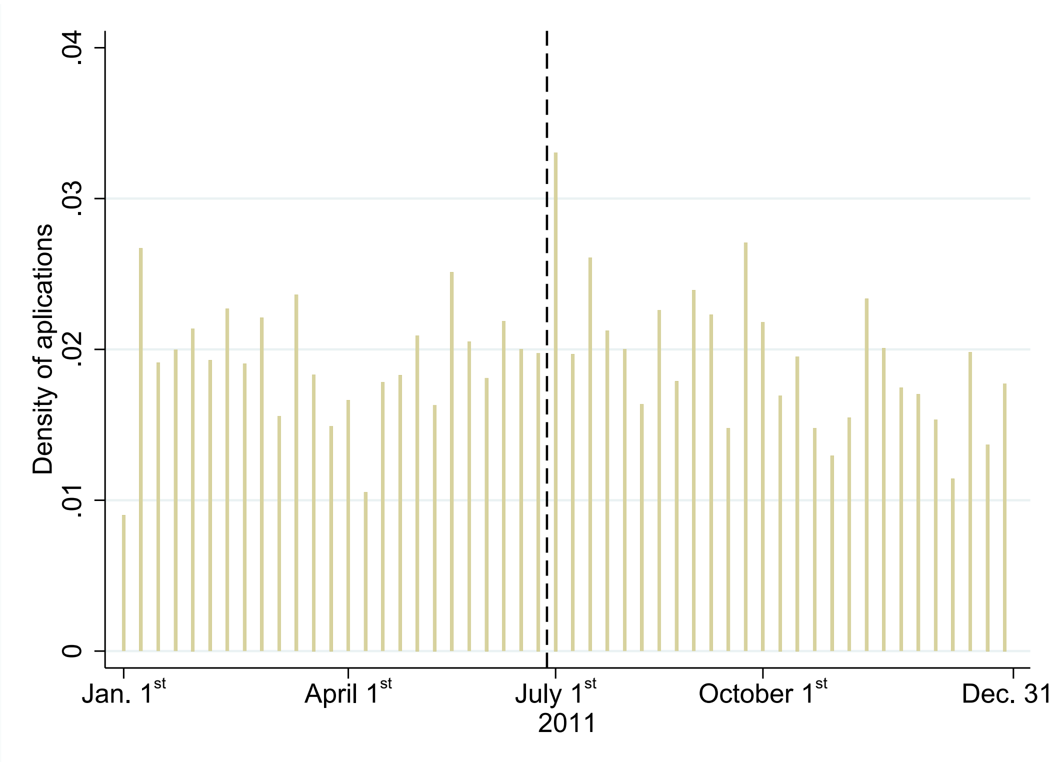


Figure B1. : Weekly density of applications over 2011

Notes: This figure shows the weekly density of applicants (both recipients and non-recipients) in 2011. The dashed vertical line represents the change in the pension score cut-off on July 1st, 2011.

The cut-off changes from covering 55% to covering 60% of the pension score distribution on July 1st, 2011 (Appendix Figure A1). This may have incentivized people to wait until this date to apply, in order to increase their probability of receiving a pension. Appendix Figure B1 shows an increase in the density of applications in the week beginning on July 1st, 2011, which is statistically significant according to the density test by (Cattaneo, Jansson and Ma, 2019). However, this increase appears to be transitory and disappears immediately after the first week of July. The absence of a strong anticipating behaviour can be rationalized by considering that the cut-off increase was not large, the monetary cost of applying is zero and individuals can apply multiple times without a penalty. Thus the increase in the number of applicants in the week beginning on July 1^{st} is arguably due to people stalling their application for only a short time or re-applying, and does not appear to affect the external validity of the main results. Our point estimates remain significant and of similar magnitude when we exclude applicants that applied in the first week of July 2011 (results are available upon request).

APPENDIX C Serial applicants

Figure 1 shows that few applicants below the cut-off did not receive the basic pension. This is explained by reasons unrelated to the pension score (e.g. not redeeming the pension in time). This figure also shows that a relevant number of applicants above the cut-off obtained a basic pension within four years. This is fully explained by non-recipients who submitted a subsequent application (henceforth referred to as serial applicants) that was successful.

To analyze the characteristics of serial applicants, we regress an indicator for whether the person is a serial applicant against baseline covariates. Column (1) of Appendix Table C1 presents a series of bivariate regressions in which each baseline characteristic is entered separately, while columns (2), (3), and (4) show estimations that regress on multiple covariates simultaneously. This table shows that applicants above the cut-off who are older and have a higher social security score are less likely to be serial applicants, while those in a larger household are more likely to apply more than once. This could be because: 1) older applicants might perceive a lower present value of the basic pension income (they expect to live for a shorter time); and, 2) wealthier people believe they are less likely to obtain the pension. In contrast, people in larger families might be more likely to see changes in their household composition or income. They may believe that these changes will affect their pension score which encourages them to reapply.

Table C1—: The effect of baseline covariates on the probability of applying multiple times

	(1)	(2)	(3)	(4)
Female	-0.076	-0.001	0.001	0.004
	(0.020)	(0.021)	(0.021)	(0.020)
Age (years)	-0.023	-0.019	-0.018	-0.016
	(0.001)	(0.001)	(0.001)	(0.001)
Social security score	-0.031	-0.026	-0.027	-0.025
	(0.001)	(0.001)	(0.002)	(0.002)
Days hospitalised	0.000	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Received influenza vaccination	0.017	0.034	0.037	0.014
	(0.013)	(0.013)	(0.013)	(0.014)
Received pneumonia vaccination	0.067	-0.001	-0.005	0.024
	(0.029)	(0.030)	(0.030)	(0.030)
Household size	0.022		0.021	0.023
	(0.006)		(0.010)	(0.010)
Elderly cohabitant	-0.116		-0.032	-0.030
	(0.014)		(0.017)	(0.017)
Working-age cohabitant	0.089		0.023	0.021
	(0.012)		(0.019)	(0.019)
Live with child under 16	0.106		0.009	-0.017
	(0.063)		(0.060)	(0.062)
Fertility age women	0.073		-0.027	-0.027
	(0.016)		(0.019)	(0.019)
FIXED EFFECTS	NO	NO	NO	YES
N	6,423	6,423	6,423	6,423

Notes: Using the sample of all applicants above the cut-off, this table reports results from OLS regressions of a binary indicator equal to 1 if the individual submitted at least another application within 4 years from the first application (and 0 otherwise) on several covariates. Column (1) reports coefficients of bivariate regressions. Columns (2), (3) and (4) report coefficients of multivariate regressions on the specified variables. Fixed effects are at the month-of-application and the health-district level. Standard errors are clustered at the province level. For ease of interpretation, the social security score is rescaled (divided by 1,000).

APPENDIX D Set of controls used in the robustness estimations

We test the robustness of our results by replicating them on several specifications. For the specification in which we use a polynomial of order 1 in score and other controls, we perform the regressions using the following control variables:

- Individual and household covariates: month-year of the first application fixed effect, age of application fixed effect, gender, social security score, and number of applicants in the household. We also use the following household characteristics prior to applying: dummy for whether the applicant lives with an elderly household member, dummy for whether the applicant lives with a working-age relative, dummy for whether the applicant lives with a person below 16 years of age, and household-size fixed effects.

- Health covariates six months before applying: percentage of days of hospitalization, dummy indicator for whether the applicant had been given a pneumonia vaccination, and dummy indicator for whether the applicant had been given an influenza vaccination.

- Geographical covariates: health service fixed effects, the number of health facilities per square kilometer, municipal income per capita, whether the town is rural or urban, and whether there is a hospital in the town.

APPENDIX E Sensitivity and placebo checks on the direct health effects

Appendix Table G11 shows that the causal effect of the basic pension on mortality and medical episodes remains qualitatively unchanged whether we use logistic regressions, non-parametric estimations, different sets of controls, or polynomials of order two in $Score_h$. When we include all controls, the p-values are slightly higher but remain small. Figure E1 also shows that the results do not change when we use different bandwidths around the cut-off, suggesting also that our results are not driven by observations far away from the cut-off.

Additionally, we implement the randomization inference method proposed by (Cattaneo, Frandsen and Titiunik, 2015) on the mortality estimate. This method randomly varies which observations are assigned to treatment and control in a window around the threshold where treatment status is as good as randomly assigned. After running this permutation test based on difference in means, we reject the null hypothesis of no mortality effect with a p-value < 0.001. We also set placebo thresholds along the score distribution at intervals of 25 score-points and perform reduced form estimates at every placebo threshold. Figure E2 compares these estimates and shows that the probability of obtaining a mortality estimate smaller than ours is as small as 0.0384. This result suggests that our estimated effect is not a random discontinuity that is likely to be observed in other parts of the score distribution.

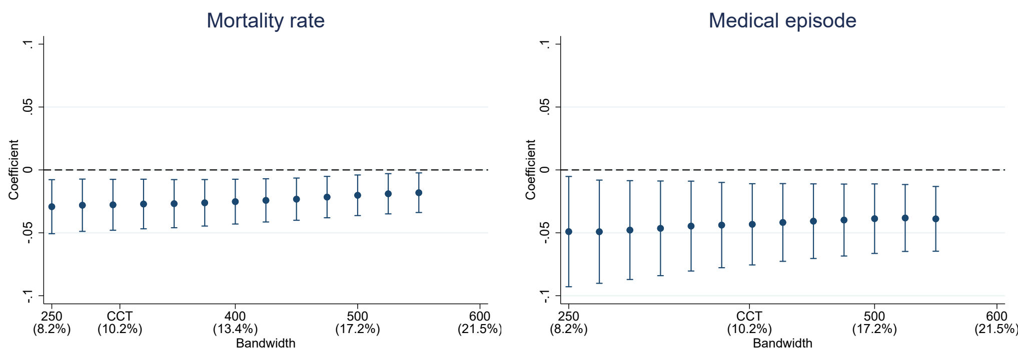


Figure E1. : Robustness of results for mortality and medical episodes using different bandwidths

Notes: Each graph shows the point estimate and the standard error of the ITT effect of the basic pension on applicants' mortality and medical episodes, using different bandwidths and all controls specified in regression Equation (1). The x-axis labels report the number of score points in each side of the bandwidth and, in parentheses, the percentage of total applicants that fall in the bandwidth. CCT is the optimal bandwidth using the approach proposed by (Calonico, Cattaneo and Titiunik, 2014).

Finally, according to the power calculation method suggested by (Gelman and Carlin, 2014), our mortality estimate appears to be well powered. Previous estimates in the literature find that the median income effect size on elderly mortality is 2.2 pp. and the average effect size is 2.7 pp. (Jensen and Richter, 2003; Snyder and Evans, 2006; Salm, 2011; Barham and Rowberry, 2013; Cheng et al., 2016; Feeney, 2018).[1] In our power estimations, we use our standard error for the mortality effect (0.97 pp.) and a statistical significance threshold of 0.05 (Gelman and Carlin, 2014). Using these numbers, we obtain a power of 0.62 for the median average effect size (0.8 for the mean effect size). This is reassuring considering that problems with the exaggeration ratio (expectation of the absolute value of the estimate divided by the effect size) 'start to arise when power is less than 0.5, and problems with the Type S error rate [probability that the estimate has an incorrect sign if significant] start to arise when power is less than 0.1' ((Gelman and Carlin, 2014), p.643).

[1] The literature finds these mortality effect sizes using different income shocks, in different populations and historical periods. Keeping this caveat in mind, we prefer to use the face value of these estimates rather than adjusting them using an arbitrary criterion.

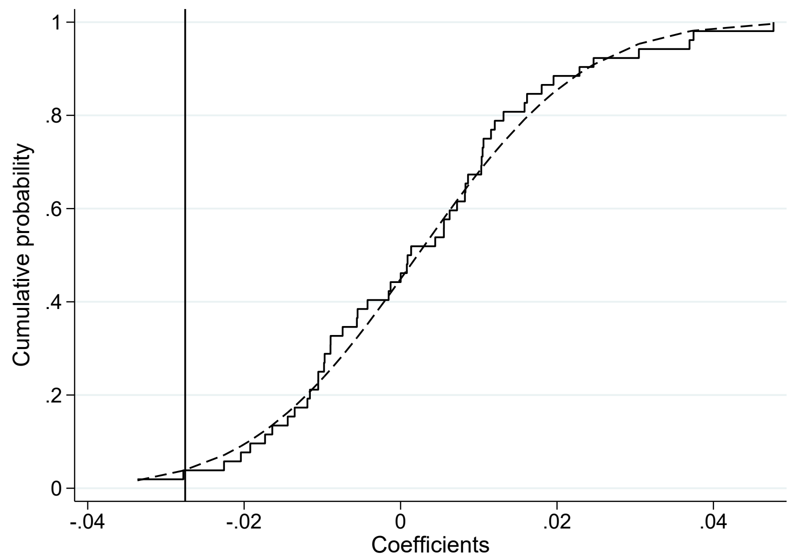


Figure E2. : Reduced-form effect of being below the cut-off on mortality: placebo estimates

Notes: This graph shows the cumulative distribution of reduced-form estimates on mortality, from placebo regressions in which the cut-off is set in different parts of the pension score distribution. Estimates are computed using the regression in Equation (1). Cut-offs are located every 25 points, starting from 306 (Calonico, Cattaneo and Titiunik's (2014) optimal bandwidth) up to 1606 score points, to make sure that we have observations in all points of the bandwidth. The cut-off is set at 1206 pension score points and the lowest pension score is zero. Therefore, placebo cut-offs are set between -900 and 400 pension score points from the cut-off. The solid line displays the empirical cumulative distribution of estimates and the dashed line displays fitted values of the cumulative distribution. The vertical line shows the coefficient estimated with our optimal bandwidth baseline specification.

APPENDIX F Spillover effects on applicants' household members

A Spillover results

This section provides causal evidence that a permanent income increase for the elderly poor can have spillover effects on the fertility of working-age household members. We are not aware of previous papers testing this directly, using administrative data and in a regression discontinuity design.

In Chile, the minimum legal age to claim contributory pension benefits is 65 for men and 60 for women, and the minimum legal working age is 15. Therefore, to analyze spillover effects, we define three exclusive groups of household members based on household members' age: 1) men above 64 and women above 59 years of age (elderly); 2) men aged 16-64 and women aged 16-59 years (working-age); and, 3) individuals below 16 years of age (school-age children). Given the small number of observations in this last group of household members (931), we focus the analysis on the first two groups.

Table F1—: Health outcomes over four years from application: household members by age

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: working-age household members								
% days hospitalized	0.012	(0.035)	0.012	(0.021)	0.575	500	8,047	0.100
Newborn child	0.024	(0.010)	0.017	(0.008)	0.035	500	8,047	0.033
Panel B: female household members of fertility age (16-40)								
% days hospitalized	0.007	(0.043)	-0.005	(0.033)	0.872	500	2,058	0.116
Newborn child	0.098	(0.036)	0.067	(0.028)	0.023	500	2,058	0.130
Panel C: elderly household members								
Mortality rate	0.012	(0.016)	0.011	(0.013)	0.397	500	5,722	0.125
% days hospitalized	0.060	(0.084)	0.026	(0.055)	0.635	500	5,722	0.274
Medical episode	0.061	(0.038)	0.045	(0.032)	0.164	500	5,722	0.376

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) reports the *treatment on the treated* coefficient as in Equation (4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

Panel A of Appendix Table F1 shows that working-age relatives of basic pension recipients do not see a change in the percentage of days spent in hospital. This is not surprising, considering that working-age relatives are young (40 years old on average) and are rarely hospitalized.[2] Panel C of this table shows that elderly household members were more likely to die than applicants (their average mortality rate, in column (7), is 12.5 percent), but this seems to be unaffected by having a relative who receives the basic pension.

Section IV.C shows that the household structure is a relevant determinant of the effect of the basic pension on recipients. One of the potential reasons is that families with a working-age household member pool income to different extents. To provide further evidence on the presence of intra-household transfers of income, we explore whether the fertility of working relatives living with recipients increases when pension payments begin. (Becker, 1960) suggests that children are normal goods, so their 'consumption' should increase when more income is available to

[2]Covariates seem to change smoothly at the cut-off for working-age and elderly household members. Panel A of Table G12 shows that 1 out of the 11 available covariates is significant for working-age household members. Panel B of Table G12 shows that 2 out of the 10 available covariates are statistically significant among elderly household members. Appendix Table G13 shows that adding covariates as controls does not change the results. Appendix Figure H11 also shows no discontinuity in the density of applicants' working-age household members (t-statistic of -0.013 and p-value of 0.999) or elderly household members (t-statistic of -1.576 and p-value of 0.115) at the cut-off.

parents. Panel A of Table F1 reveals that working-age relatives are 2.4pp. more likely to have a newborn child nine months after the pension application or later. As our data only identifies mothers and not fathers of newborn children, Panel B repeats the analysis focusing on fertility-age women (16-40 years of age) and estimate that they are 9.8 pp. more likely to have a newborn nine months after the application or later.[3] The ITT effect of the pension is a 6.7pp. increase (p-value=0.023) on the probability of having a newborn from a baseline probability of 13.0pp. Appendix Section F.B shows that fertility results remain statistically significant to a variety of robustness checks and are also in line with previous estimates in the literature.[4]

Our fertility results complement previous findings on the spillover benefits of non-contributory pensions on children's height, weight, school enrolment, and attendance (Duflo, 2000, 2003; Edmonds, 2006); and on working-age relatives' self-reported nutrition, sanitation, and employment (Case, 2004; Case and Menendez, 2007; Ardington, Case and Hosegood, 2009). The presence of spillover effects suggests that the benefits of pension policies could extend beyond the welfare of direct recipients and affect the life choices of younger generations.

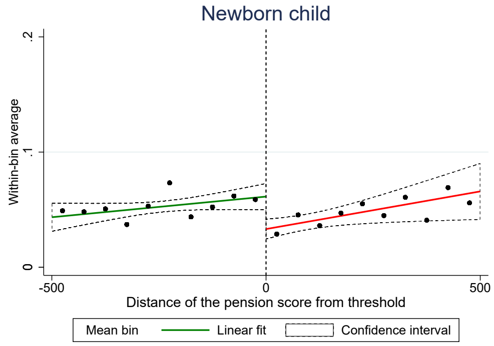
The significant *spillover* effect on the fertility rate of working-age household members, combined with the insignificant *direct* effect on recipients living with them, could be the result of intra-household transfers of income. As mentioned above, fertility is expected to increase when more income is available to parents (Becker, 1960).[5] On the one hand, working-age household members may have reduced their net transfers of income to applicants (current or expected future ones) after applicants started receiving the pension, and thus retained the necessary resources to raise a child. This would be consistent with previous evidence finding that social security benefits 'crowd out' 20%-30% of private transfers from younger generations to the elderly (Cox and Jimenez, 1992; Jensen, 2003), and the fact that a large fraction of recipients living with working-age relatives expect to finance their retirement with transfers from their children (see Section IV.C). On the other hand, recipients may transfer part of the pension to working-age household members, as documented in previous studies (Duflo, 2000, 2003; Ardington, Case and Hosegood, 2009). This hypothesis would need to be reconciled with survey evidence showing that 82% of pension recipients do not share any money with their relatives or friends, and only 4% share more than one-fifth of their pension with others (Ministerio Trabajo y Previsión Social, 2017).

[3] Appendix Figure H12 shows no discontinuity in the density of applicants' fertility-age female household members (t-statistic of -1.131 and p-value of 0.258). Appendix Table G14 shows that there is no imbalance out of 9 available covariates for female household members of fertility age.

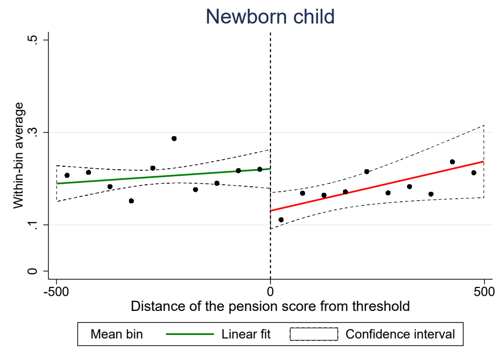
[4] According to our data, 49.9% of days spent in hospital by women of fertility age are due to pregnancy, childbirth and the puerperium. Hospitalizations for these reasons observe a significant increase if a family member receives a basic pension, in accordance with the positive effect on childbirth numbers. However, if we include days of hospitalization due to other causes, the estimation becomes less precise and we do not detect any significant effect. Results are available upon request.

[5] Alternatively, we could have considered working-age household relatives' consumption of other goods, such as food. Our administrative data does not contain consumption of these kinds of goods, and the EPS survey only contains household consumption without separating by household members.

(a) Working-age household members



(b) Female household members of fertility age



(c) Elderly household members

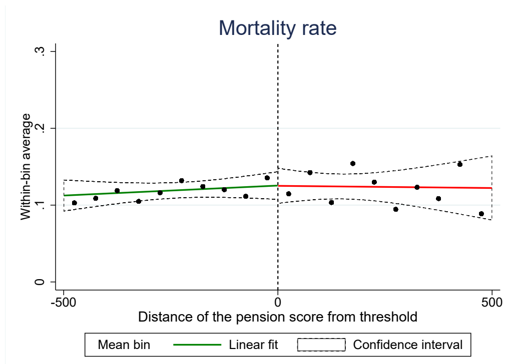


Figure F1. : Effect of the basic pension on mortality and fertility of household members

Notes: Each graph shows the average value of the corresponding variable conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

Alternatively, receipt of the pension could reduce the cost of raising a child (for example, financially autonomous healthy grandmothers may be more able to accompany children to and from school) and increase fertility, as highlighted in the previous literature (D'Addio and d'Ercole, 2006; Kalwij, 2010; Liu et al., 2018). Even though we cannot separate the causes of our fertility results – an increase in income versus a decrease in the costs of child-raising – the latter does seem less relevant in our context, given that most pension recipients do not have any job to quit that might grant them more free time to provide support for their grandchildren (arguably the main cause of the reduction in child-raising costs).

B Robustness of fertility results

This section explores the robustness and timing of the spillover effects on fertility and situates them in the context of the literature. Tables G12 and G14 show no imbalance in the probability of having a newborn before applying between the treatment and control groups. If we extend the analysis of the outcome up to 9 months after the application, we still find no evidence of imbalance between working-age (or women of fertility age) household members above and below the cut-off.

Appendix Tables G13 and G15 show that the results for working-age, female fertility-age, and elderly household members do not change when we use logistic regressions, non-parametric estimations, the optimal bandwidth approach proposed by (Calonico, Cattaneo and Titiunik, 2014), or different sets of controls, nor when we control for a polynomial of order 2 in $Score_h$. This also ensures that the null effect on elderly household members is not driven by the slight imbalance in this group.

Figure F2, shows that the fertility result remains positive and significant when using different bandwidths. Additionally, we implement the randomization inference method proposed by (Cattaneo, Frandsen and Titiunik, 2015) on the fertility estimate and reject the null hypothesis of no fertility effect with a p-value < 0.001. We also set placebo thresholds along the score distribution, at intervals of 25 score-points, and perform reduced form estimates. Figure F3 compares our estimate with the distribution of placebo estimates and shows that no estimate is higher than ours. This suggests that our estimated effect on fertility is not a random discontinuity that is likely to be observed in other parts of the score distribution. Finally, fertility estimates remain significant when adjusting our p-values for multiple hypothesis testing, with an adjusted p-value = 0.03 (Romano and Wolf, 2005*a*,*b*).

Figure F4 shows the timing of childbirths for women of fertility age, between six months before and four years after the first application. Treated and control women in fertility age have a similar fraction of newborn children until 9 months after the application, with a slightly higher fertility rate for control group women. 1.2 years after the application, the two lines intersect and the treatment effect on fertility starts accumulating over time.[6] The fraction of women of fertility age who have a newborn is not small in this time span: almost a quarter of treated women and a fifth of control women had a child four years after applications are submitted.

[6] In Appendix Figure H13 we can see that the impact on fertility is not significant in the first year after the application, but it becomes evident since the second year after the application.

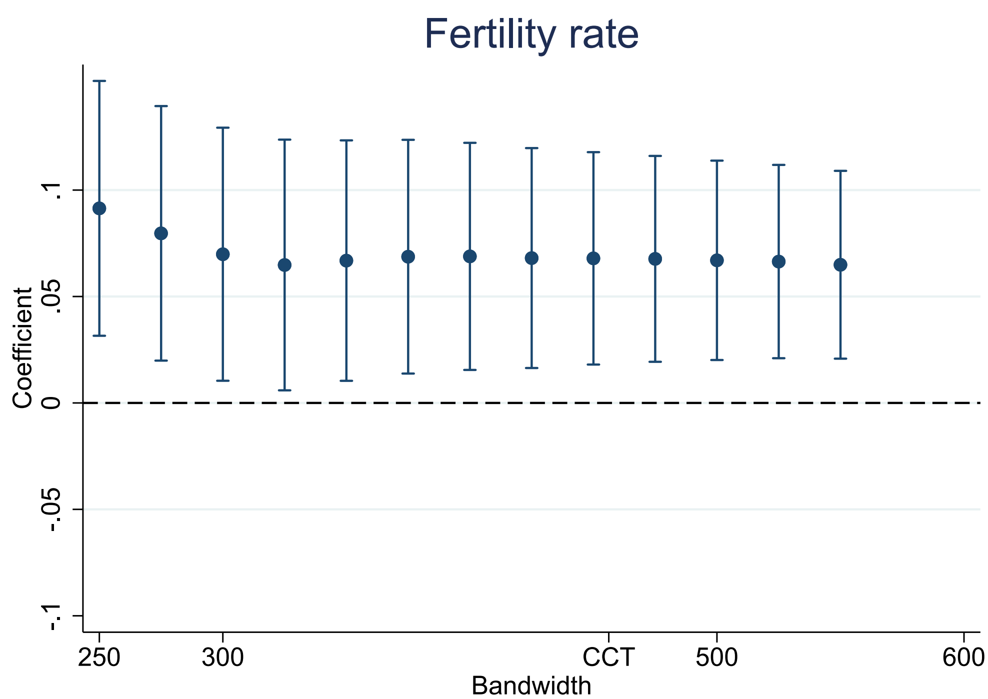


Figure F2. : Robustness of results for fertility using different bandwidths

Notes: This graph shows the point estimate and the standard error of the ITT effect of the basic pension on having a newborn child in the period from 9 months to 4 years after application for applicants' female household members of fertility age, using different bandwidths and all controls specified in regression Equation (1). The x-axis labels report the number of score points on each side of the bandwidth. CCT is the optimal bandwidth using the approach proposed by (Calonico, Cattaneo and Titiunik, 2014).

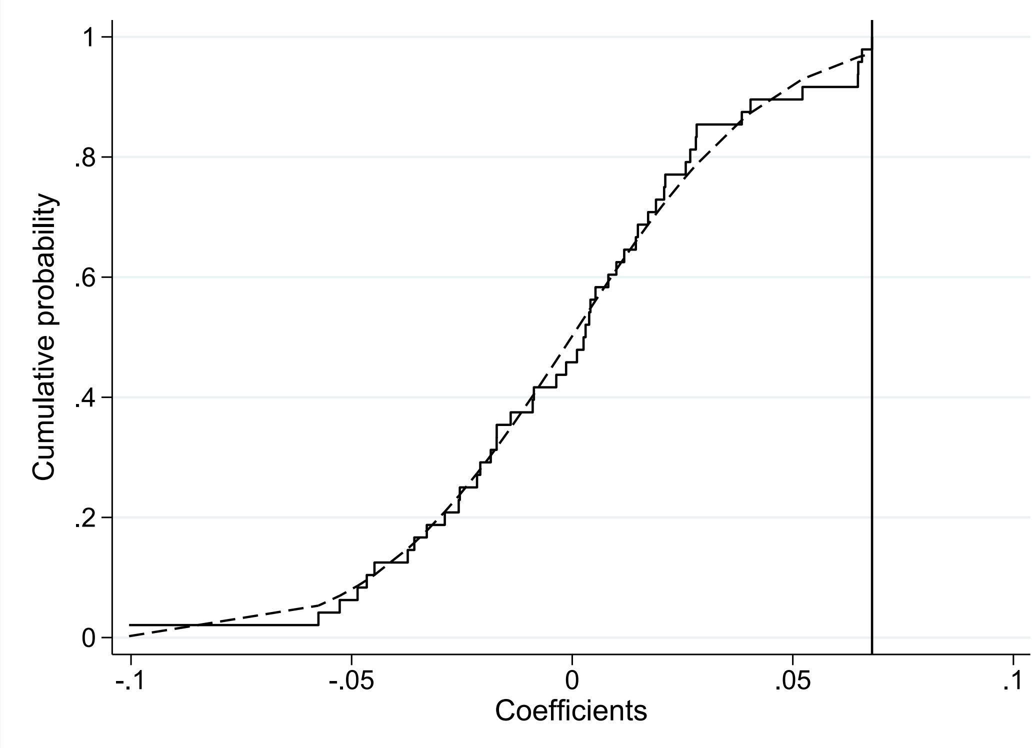


Figure F3. : Reduced-form effect of being below the cut-off on fertility: placebo estimates

Notes: This graph shows the cumulative distribution of reduced-form estimates on fertility, from placebo regressions in which the cut-off is set in different parts of the pension score distribution. Estimates are computed using regression Equation (1). Cut-offs are located every 25 score points, ranging from 456 (Calonico, Cattaneo and Titiunik's (2014) optimal bandwidth on fertility) to 1606, to ensure that we have observations in all points of the bandwidth. The lowest pension score is zero and the cut-off is set at 1206 pension score points. Then, placebo cut-offs are set between -750 and 400 pension score points from the cut-off. The solid line displays the empirical cumulative distribution of estimates, while the dashed line displays fitted values of the cumulative distribution. The vertical line shows the coefficient estimated with our optimal bandwidth baseline specification.

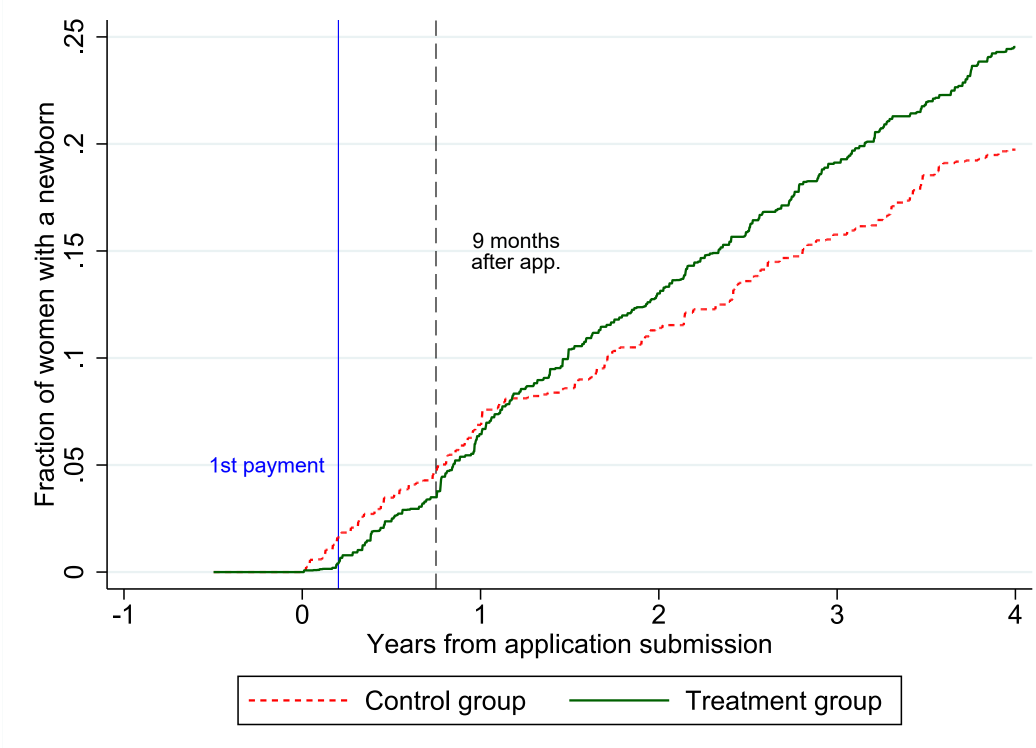


Figure F4. : Share of women of fertility age having a newborn between six months before applying and four years from date of application, adjusted by the deviation of the pension score from the cut-off.

Notes: This figure presents the share of women of fertility age that have a newborn in the treatment and control groups at each point in time following the first application. Shares are equal to $1 - \hat{S}(t)$, with $\hat{S}(t)$ being the $k_0(t)$ term in the Cox proportional hazard model: $k(t) = k_0(t)\exp(\beta_1 Score_h)$, with t being the time following the first application. Shares are estimated separately for the treatment and control groups in the 500 score-point bandwidth and using triangular weights.

C Discussion on the spillover effect on fertility

Following most of the literature, we estimate the income-fertility elasticity by dividing the ITT percentage change in newborns for women of fertility age by the ITT percentage income change for the recipients of income. In our case, the recipients of income are the applicants, and this calculation yields an income-fertility elasticity of 0.7. Alternatively, if we use the mother's income rather than recipient's income, the income-fertility elasticity is 0.76.[7] Figure F5 shows that

[7]The probability of having a newborn increases by 51% (0.067/0.130) for women of fertility age living with a pension recipient at the cut-off. As the basic pension increases recipients' income by 72.4 percent,

previous causal estimates of income-fertility elasticity are also positive, which is in line with the predictions of Becker's (1960) neoclassical model of fertility.[8] Our estimate is roughly in the middle of the range, but there is a considerable dispersion of fertility-income elasticities across studies.

the recipient's income-fertility elasticity is 0.7. For the estimate of mothers' income-fertility elasticity, we assumed perfect income pooling. In households with a woman of fertility age, the pension increases average monthly income per-capita by USD 26 over the four years following the first application, from an average monthly income of USD 34 for control group applicants. This leads to a mother income-fertility elasticity of 0.76. As before, these estimates take into account the full trajectory of income and are done using only first applicants from 2012.

[8]Children are generally considered 'normal goods' and their 'consumption' should increase with income. Our results, along with other recent empirical studies presented in Figure F5, help to explain the long-term puzzle of the negative cross-sectional correlation between income and fertility that is present in many parts of the world (see (Jones and Tertilt, 2008)).

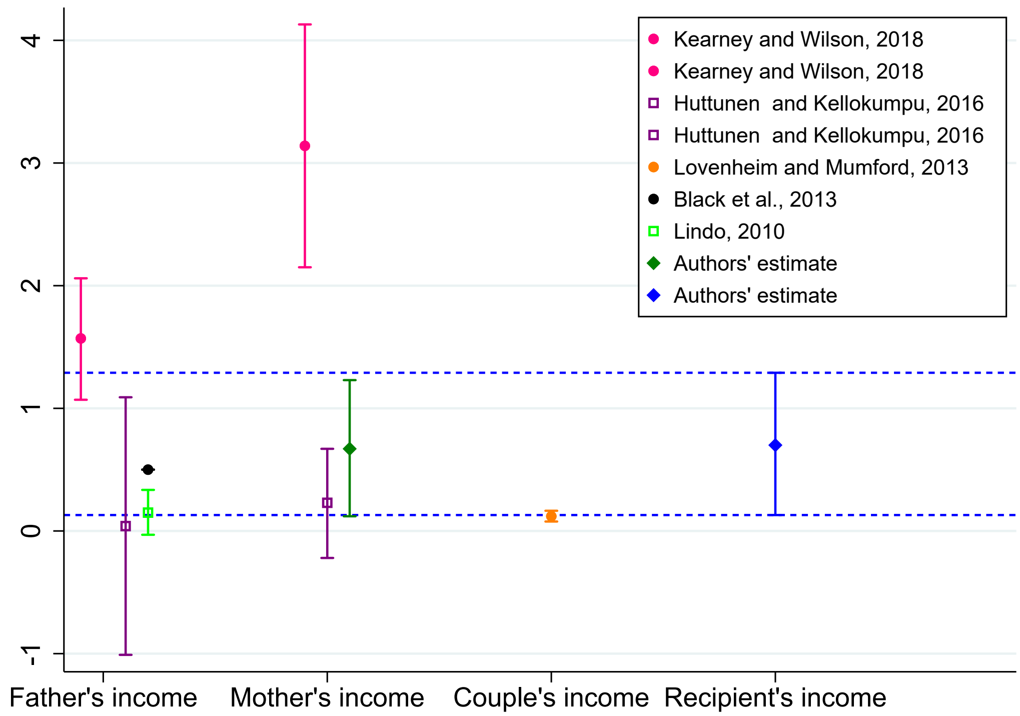


Figure F5. : Estimated income-fertility elasticity across different empirical studies

Notes: This graph plots point estimates and confidence intervals of income-fertility elasticity in different empirical studies. Empty squares indicate insignificant estimates. The dashed lines indicate the 95% confidence intervals of our estimates. The elasticities in the other papers are computed using income shocks on different household members: (Black et al., 2013) and (Lindo, 2010) estimate income-fertility elasticity using husband's income; (Kearney and Wilson, 2018) and (Huttunen and Kellokumpu, 2016) estimate mother's income-fertility elasticity and husband's income-fertility elasticity; and (Lovenheim and Mumford, 2013) estimate a fertility elasticity with respect to the house price. In several studies, it is not possible to calculate the income-fertility elasticity, because either baseline fertility or income are not reported. The confidence interval for (Black et al., 2013) is unavailable as the standard errors are not reported.

One explanation for the diverse pattern of estimates is that the nature of the income shock is very diverse across studies: mother's or father's job displacements in (Lindo, 2010) and (Huttunen and Kellokumpu, 2016); boosts in house prices in (Lovenheim and Mumford, 2013); economic booms in (Black et al., 2013) and (Kearney and Wilson, 2018); and the basic pension for elderly relatives in our case. Different shocks may also induce different impacts on household dynamics. For instance, job displacements might affect the probability of divorce and change women's career choices, while house price increases might be perceived as transitory income shocks with weaker effects on couples' decision to have a child, which is a permanent decision. Additionally, these studies are conducted in different countries, with different public provision of childcare, which could affect the relative 'price' of childbearing. For instance, (Huttunen and Kellokumpu, 2016) focuses on Finland which has a relatively generous welfare state compared to Chile and the US, the countries studied in our paper and the papers by (Lindo, 2010; Black et al., 2013; Lovenheim and Mumford, 2013) and (Kearney and Wilson, 2018).

APPENDIX G Additional tables

Table G1—: Characteristics of applicants, and their household members, at the moment of application and within 500 score points around the threshold

	Applicants (1)	Working-age household members (2)	Elderly household members (3)
Female	0.871	0.363	0.12
Age (years)	66.851	40.364	71.074
Social security score	9385.748	9576.395	9835.929
Household size	2.643	3.685	2.749
Working-age household member	0.571	1	0.434
Elderly household member	0.661	0.47	1
Child under 16	0.009	0.018	0.009
Days hospitalized	0.461	0.247	0.466
Influenza vaccination	0.32	0.089	0.347
Pneumonia vaccination	0.061	0.002	0.028
Urban town	0.762	0.737	0.77
Metropolitan region	0.373	0.348	0.368
Received a basic pension	0.799		
Observations	8,499	8,047	5,722

Notes: This table reports the mean of several covariates for applicants whose application score is within 500 score points from the cut-off and their household members. Column (1) reports means for applicants, Column (2) reports means for working-age household members, and Column (3) reports means for elderly household members. *Health covariates* are computed for the 6 months before applicants submit their first application.

Table G2—: Balancing tests on other covariates (2012 only)

Variables	ITT Coef. (1)	S.E. (2)	t stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: household measures							
Total household income	0.833	(10.163)	0.082	0.935	500	4,066	649.7
Imputed income	-25.000	(12.083)	-2.069	0.044	500	4,066	93.40
Labor income	27.940	(36.573)	0.764	0.449	500	4,066	246.5
All incomes from assets	-27.11	(36.282)	-0.747	0.459	500	4,066	403.1
Labor income factor	-0.013	(0.024)	-0.562	0.577	500	4,066	1.939
Needs index (IN)	-0.032	(0.021)	-1.539	0.130	500	4,066	2.021
Net working salary	-4.596	(19.870)	-0.231	0.818	500	4,066	187.8
Other labor income	36.160	(30.979)	1.167	0.249	500	4,066	20.10
Net pension income	5.339	(18.848)	0.283	0.778	500	4,066	357.0
Avg. no. of students	-0.021	(0.016)	-1.258	0.214	500	4,066	0.070
Panel B: income of household members							
Applicants' income	-1.464	(11.615)	-0.126	0.900	500	4,066	89.37
Elderly relatives' inc.	-17.44	(21.819)	-0.799	0.428	500	2,769	525.2
Work.-age relatives' inc.	-4.775	(31.926)	-0.150	0.882	500	2,309	290.0
Fert. age woman's inc.	0.956	(12.432)	0.0770	0.939	500	828	20.90

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. All estimations are computed using averages at household level due to data limitations. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. All income variables are expressed in 2012 US dollars.

Table G3—: Applicant's health outcomes over four years from application by gender

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: female applicants								
Mortality rate	-0.028	(0.011)	-0.022	(0.008)	0.013	500	7,403	0.063
% days hospitalized	-0.034	(0.062)	-0.005	(0.048)	0.908	500	7,403	0.263
Medical episode	-0.068	(0.030)	-0.047	(0.021)	0.026	500	7,403	0.328
Panel B: male applicants								
Mortality rate	0.010	(0.052)	0.014	(0.037)	0.710	500	1,096	0.129
% days hospitalized	-0.144	(0.258)	-0.019	(0.138)	0.890	500	1,096	0.363
Medical episode	0.005	(0.117)	0.034	(0.079)	0.669	500	1,096	0.382

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) reports the *treatment on the treated* coefficient as in Equation (4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

Table G4—: Balancing tests by household structure

Variables	ITT Coef. (1)	S.E. (2)	t stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: applicants not living with a working-age relatives							
Female	-0.014	(0.020)	-0.693	0.491	500	3,647	0.871
Age (years)	-0.680	(0.457)	-1.488	0.143	500	3,647	69.00
% days hospitalized	-0.270	(0.116)	-2.339	0.023	500	3,647	0.336
Influenza vaccination	-0.011	(0.028)	-0.387	0.701	500	3,647	0.360
Pneumonia vaccination	0.025	(0.016)	1.513	0.137	500	3,647	0.033
Household size	-0.016	(0.020)	-0.840	0.405	500	3,647	1.915
Social security score	-48.82	(207.017)	-0.236	0.815	500	3,647	9640.
Elderly relative	-0.022	(0.019)	-1.180	0.244	500	3,647	0.892
Child under 16	-0.004	(0.004)	-1.036	0.305	500	3,647	0.004
Municipal income	5.761	(5.048)	1.141	0.259	500	3,640	141.8
Panel B: applicants living with working-age relatives							
Female	-0.017	(0.021)	-0.780	0.439	500	4,852	0.906
Age (years)	-0.116	(0.314)	-0.369	0.713	500	4,852	66.38
% days hospitalized	0.048	(0.099)	0.488	0.628	500	4,852	0.174
Influenza vaccination	-0.036	(0.027)	-1.342	0.186	500	4,852	0.355
Pneumonia vaccination	0.010	(0.014)	0.681	0.499	500	4,852	0.052
Household size	0.008	(0.060)	0.136	0.892	500	4,852	3.227
Social security score	167.3	(255.827)	0.654	0.516	500	4,852	9823.
Elderly relative	0.043	(0.026)	1.646	0.106	500	4,852	0.528
Child under 16	0.007	(0.006)	1.045	0.301	500	4,852	0.007
Municipal income	-9.301	(5.746)	-1.619	0.112	500	4,843	151.0

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

Table G5—: Health outcomes, over four years from application, for applicants not living with working-age household members using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Mortality rate	No controls	-0.045	(0.016)	0.008	500	3,647
Mortality rate	Controls	-0.040	(0.015)	0.010	500	3,647
Mortality rate	Logit	-0.047	(0.015)	0.001	500	3,647
Mortality rate	Non-parametric	-0.045	(0.019)	0.021	500	3,647
Mortality rate	Optimal bandwidth	-0.050	(0.019)	0.010	374	2,704
Mortality rate	Quadratic	-0.065	(0.025)	0.013	500	3,647
Medical episode	No controls	-0.093	(0.036)	0.012	500	3,647
Medical episode	Controls	-0.086	(0.040)	0.036	500	3,647
Medical episode	Logit	-0.090	(0.037)	0.017	500	3,647
Medical episode	Non-parametric	-0.093	(0.034)	0.007	500	3,647
Medical episode	Optimal bandwidth	-0.116	(0.058)	0.053	294	2,124
Medical episode	Quadratic	-0.128	(0.066)	0.058	500	3,647

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section D. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

Table G6—: Applicants' health outcomes, over four years from application, for applicants living with working-age household members using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Mortality rate	No controls	-0.001	(0.014)	0.954	500	4,852
Mortality rate	Controls	-0.004	(0.010)	0.679	500	4,852
Mortality rate	Logit	-0.002	(0.010)	0.810	500	4,852
Mortality rate	Non-parametric	-0.001	(0.013)	0.949	500	4,852
Mortality rate	Optimal bandwidth	-0.012	(0.012)	0.317	364	3,382
Mortality rate	Quadratic	-0.017	(0.017)	0.312	500	4,852
Medical episode	No controls	-0.000	(0.032)	0.998	500	4,852
Medical episode	Controls	0.001	(0.038)	0.985	500	4,852
Medical episode	Logit	0.000	(0.036)	0.994	500	4,852
Medical episode	Non-parametric	0.000	(0.035)	0.990	500	4,852
Medical episode	Optimal bandwidth	-0.000	(0.035)	0.997	506	4,924
Medical episode	Quadratic	0.008	(0.053)	0.874	500	4,852

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section D. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

Table G7—: Medical episodes by cause over four years from application

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: applicants								
Circulatory	0.013	(0.016)	0.011	(0.012)	0.376	500	8,499	0.076
Respiratory	-0.030	(0.011)	-0.019	(0.008)	0.019	500	8,499	0.044
Tumour	-0.028	(0.015)	-0.021	(0.011)	0.067	500	8,499	0.054
Digestive or nutritional	-0.025	(0.016)	-0.020	(0.012)	0.097	500	8,499	0.098
Accidents	-0.002	(0.003)	-0.001	(0.002)	0.548	500	8,499	0.002
Panel B: applicants not living with a working-age household member								
Circulatory	-0.017	(0.026)	-0.011	(0.019)	0.544	500	3,647	0.099
Respiratory	-0.045	(0.012)	-0.031	(0.009)	0.001	500	3,647	0.050
Tumour	-0.048	(0.018)	-0.036	(0.014)	0.014	500	3,647	0.058
Digestive or nutritional	-0.009	(0.033)	-0.008	(0.026)	0.756	500	3,647	0.091
Accidents	0.002	(0.004)	0.001	(0.003)	0.600	500	3,647	0.001

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) reports the *treatment on the treated* coefficient as in Equation (4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

Table G8—: Vaccinations received in the four years after applying for applicants and applicants by household structure

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: applicants								
Influenza vaccine	0.010	(0.037)	-0.001	(0.025)	0.960	500	8,499	0.679
Pneumonia vaccine	0.027	(0.034)	0.009	(0.024)	0.721	500	8,499	0.306
Panel B: applicants not living with working-age household members								
Influenza vaccine	-0.001	(0.043)	-0.005	(0.031)	0.870	500	3,647	0.687
Pneumonia vaccine	0.008	(0.034)	-0.005	(0.025)	0.848	500	3,647	0.301
Panel C: applicants living with a working-age household members								
Influenza vaccine	0.012	(0.040)	-0.003	(0.026)	0.909	500	4,852	0.673
Pneumonia vaccine	0.040	(0.043)	0.019	(0.029)	0.510	500	4,852	0.311

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) reports the *treatment on the treated* coefficient as in Equation (4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

Table G9—: Characteristics of basic pension applicants when aged between 60 and 64

Variables	Recipients (1)	Non-recipients (2)	Difference (3)	P-value (4)
Panel A: individual level variables				
Private health insurance	0.017	0.018	-0.001	0.991
Informal work	0.156	0.228	-0.072	0.252
Visited a GP	0.589	0.655	-0.066	0.331
Visited a health center	0.769	0.793	-0.024	0.695
Visits to health center	11.097	8.862	2.235	0.174
Bad Health	0.220	0.276	-0.056	0.432
Smoked, last month	0.163	0.163	0.000	0.998
Number of cigarettes, last month	32.413	54.102	-21.689	0.437
Drunk alcohol, last month	0.106	0.265	-0.159	0.026
Number of drinks, last month	0.884	1.673	-0.790	0.077
Panel B: household income and expenditure in 2012 US dollars				
Monthly income	475.663	552.012	-76.349	0.380
Total expenditure	356.933	446.101	-89.168	0.075
Food	192.412	227.491	-35.079	0.212
Clothes	17.713	19.192	-1.479	0.742
Utilities	90.335	128.805	-38.47	0.086
Transport	30.082	40.699	-10.617	0.226
Domestic services	0.686	2.182	-1.496	0.354
Drugs	26.804	23.549	3.255	0.643
Children's education	10.445	4.995	5.451	0.119

Notes: This table reports the mean of the listed covariates for basic pension applicants at age 60-64. Column (1) reports means for applicants who eventually obtained the pension. Column (2) reports means for applicants who did not obtain the pension. Column (3) reports the difference between columns (1) and (2). Column (4) reports the p-value of a test of means differences between column (1) and (2). 'Visited a health center' is a dummy variable for whether the individual had at least one appointment at a health center in the last two years. Income and expenditure variables are reported in 2012 US dollars. 'Total expenditure' refers to the sum of the expenditures reported in the table. Data is from the panel survey conducted in 2004, 2006, 2009, 2012, and 2015 by the Ministry of Labor.

Table G10—: Characteristics of Chileans who are aged 65 or over and do not have a contributory pension

	All (1)	Basic pension recipients (2)	Basic pension non-recipients (3)
Female	0.720	0.721	0.718
	(0.449)	(0.448)	(0.450)
Age	73.55	73.94	72.83
	(6.706)	(6.614)	(6.811)
Household size	2.358	2.345	2.383
	(1.099)	(1.114)	(1.070)
Elderly household member	0.579	0.580	0.579
	(0.494)	(0.494)	(0.494)
Working-age household member	0.461	0.436	0.507
	(0.499)	(0.496)	(0.500)
Child household member	0.0755	0.0772	0.0723
	(0.264)	(0.267)	(0.259)
Metropolitan area	0.307	0.295	0.327
	(0.461)	(0.456)	(0.469)
Urban town	0.770	0.722	0.855
	(0.421)	(0.448)	(0.352)
Employed	0.0263	0.0156	0.0457
	(0.160)	(0.124)	(0.209)
Food from health service	0.380	0.434	0.285
	(0.486)	(0.496)	(0.451)
Public health insurance	0.946	0.977	0.892
	(0.225)	(0.151)	(0.311)
Received a basic pension	0.643	1	0
	(0.479)	(0)	(0)

Notes: Using data from the 2011 Chilean household survey (Ministerio de Desarrollo Social, 2011), this table reports the means and standard deviations (in parentheses) of several covariates for the Chilean population without a contributory pension in 2011. Column (1) reports statistics for the whole population, Column (2) reports statistics for elderly people with a basic pension and Column (3) reports statistics for elderly people without a basic pension.

Table G11—: Applicants' health outcomes in four years from the first application using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Mortality rate	No controls	-0.021	(0.010)	0.034	500	8,499
Mortality rate	Controls	-0.019	(0.010)	0.058	500	8,499
Mortality rate	Logit	-0.018	(0.009)	0.055	500	8,499
Mortality rate	Non-parametric	-0.021	(0.010)	0.045	500	8,499
Mortality rate	Optimal bandwidth	-0.028	(0.012)	0.029	306	5,048
Mortality rate	Quadratic	-0.035	(0.015)	0.021	500	8,499
Medical episode	No controls	-0.042	(0.018)	0.024	500	8,499
Medical episode	Controls	-0.037	(0.016)	0.029	500	8,499
Medical episode	Logit	-0.038	(0.016)	0.020	500	8,499
Medical episode	Non-parametric	-0.042	(0.023)	0.071	500	8,499
Medical episode	Optimal bandwidth	-0.043	(0.020)	0.033	398	6,605
Medical episode	Quadratic	-0.050	(0.027)	0.077	500	8,499

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section D. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

Table G12—: Balancing tests for working-age and elderly relatives

Variables	ITT Coef. (1)	S.E. (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: working-age relatives							
Female	0.030	(0.024)	1.240	0.221	500	8,047	0.358
Age (years)	-1.090	(0.656)	-1.661	0.103	500	8,047	40.96
% days hospitalized	-0.026	(0.033)	-0.794	0.431	500	8,047	0.094
Influenza vaccination	-0.015	(0.012)	-1.204	0.235	500	8,047	0.094
Pneumonia vaccination	-0.001	(0.003)	-0.271	0.788	500	8,047	0.004
Newborn child	0.007	(0.005)	1.514	0.137	500	8,047	0.006
Household size	0.007	(0.060)	0.121	0.904	500	4,836	3.228
Social security score	147.319	(261.230)	0.564	0.575	500	4,836	9857
Elderly relative	0.047	(0.026)	1.767	0.084	500	4,836	0.525
Child under 16	0.007	(0.006)	1.054	0.297	500	4,836	0.007
Municipal income	-8.321	(5.181)	-1.606	0.115	500	4,828	150.1
Panel B: elderly relatives							
Female	0.032	(0.016)	2.016	0.049	500	5,722	0.097
Age (years)	-0.608	(0.358)	-1.702	0.095	500	5,722	71.82
% days hospitalized	-0.022	(0.048)	-0.454	0.652	500	5,722	0.171
Influenza vaccination	-0.026	(0.029)	-0.899	0.373	500	5,722	0.364
Pneumonia vaccination	0.001	(0.006)	0.083	0.934	500	5,722	0.019
Household size	0.050	(0.050)	1.003	0.321	500	5,566	2.679
Social security score	96.419	(199.801)	0.483	0.632	500	5,566	1.0e+
Working-age relative	0.027	(0.024)	1.147	0.257	500	5,566	0.412
Child under 16	-0.000	(0.006)	-0.044	0.965	500	5,566	0.009
Municipal income	-2.603	(5.244)	-0.496	0.622	500	5,558	147.4

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

Table G13—: Health outcomes of family members, by age, over four years from application using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
	Panel A: working-age household members					
% days hospitalized	No controls	0.009	(0.023)	0.685	500	8,047
% days hospitalized	Controls	0.032	(0.030)	0.291	500	8,047
% days hospitalized	Logit	0.005	(0.019)	0.788	500	8,047
% days hospitalized	Non-parametric	0.009	(0.033)	0.781	500	8,047
% days hospitalized	Optimal bandwidth	0.014	(0.030)	0.649	260	3,889
% days hospitalized	Quadratic	0.028	(0.044)	0.528	500	8,047
Newborn child	No controls	0.028	(0.007)	0.000	500	8,047
Newborn child	Controls	0.016	(0.008)	0.050	500	8,047
Newborn child	Logit	0.057	(0.026)	0.034	500	8,047
Newborn child	Controls	0.016	(0.008)	0.050	500	8,047
Newborn child	Non-parametric	0.028	(0.007)	0.000	500	8,047
Newborn child	Optimal bandwidth	0.017	(0.008)	0.043	452	7,185
Newborn child	Quadratic	0.019	(0.010)	0.059	500	8,047
	Panel B: elderly household members					
Mortality rate	No controls	0.000	(0.013)	0.979	500	5,722
Mortality rate	Controls	0.012	(0.013)	0.379	500	5,722
Mortality rate	Logit	0.011	(0.012)	0.371	500	5,722
Mortality rate	Non-parametric	0.000	(0.015)	0.981	500	5,722
Mortality rate	Optimal bandwidth	0.009	(0.015)	0.547	402	4,596
Mortality rate	Quadratic	0.008	(0.020)	0.672	500	5,722
Medical episode	No controls	0.034	(0.030)	0.256	500	5,722
Medical episode	Controls	0.047	(0.033)	0.158	500	5,722
Medical episode	Logit	0.045	(0.032)	0.155	500	5,722
Medical episode	Non-parametric	0.034	(0.027)	0.208	500	5,722
Medical episode	Optimal bandwidth	0.047	(0.042)	0.268	407	4,657
Medical episode	Quadratic	0.062	(0.062)	0.320	500	5,722

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section D. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

Table G14—: Balancing tests for fertility-age female relatives

Variables	ITT Coef. (1)	S.E. (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Age (years)	-0.446	(0.466)	-0.958	0.343	500	2,058	29.58
% days hospitalized	0.000	(0.051)	0.006	0.995	500	2,058	0.103
Influenza vaccination	-0.013	(0.025)	-0.507	0.615	500	2,058	0.101
Newborn child	0.018	(0.018)	1.017	0.315	500	2,058	0.026
Household size	0.103	(0.175)	0.588	0.560	500	2,058	3.883
Social security score	396.901	(257.480)	1.541	0.130	500	2,058	9272.
Elderly relative	0.004	(0.057)	0.073	0.942	500	2,058	0.661
Child under 16	0.011	(0.016)	0.719	0.476	500	2,058	0.015
Municipal income	-17.838	(11.340)	-1.573	0.123	500	2,057	154.4

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

Table G15—: Fertility rate of fertility-age female family members 9 months or later after application using non-parametric estimations, different controls, optimal bandwidth and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Newborn child	No controls	0.091	(0.028)	0.002	500	2,058
Newborn child	Controls	0.052	(0.027)	0.062	500	2,058
Newborn child	Logit	0.068	(0.029)	0.020	500	2,058
Newborn child	Non-parametric	0.091	(0.029)	0.002	500	2,058
Newborn child	Optimal bandwidth	0.068	(0.030)	0.029	456	1,869
Newborn child	Quadratic	0.080	(0.034)	0.025	500	2,058

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section D. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

APPENDIX H Additional figures

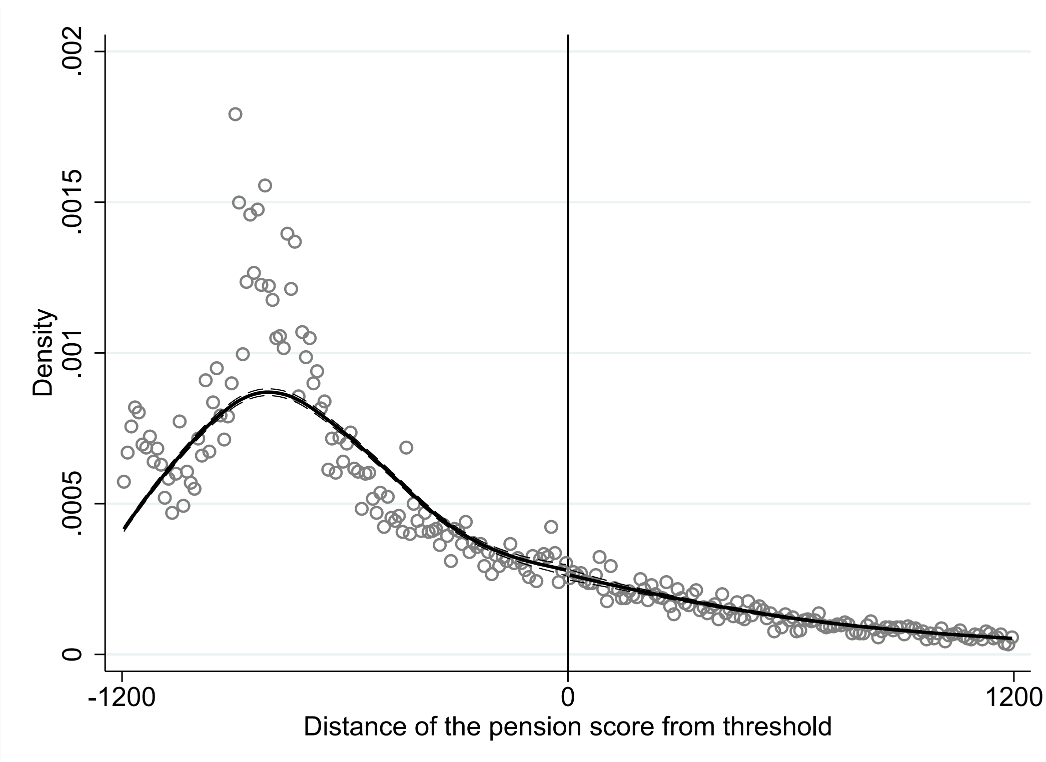


Figure H1. : McCrary test of applicants

Notes: This figure shows the density of applicants in 10 score-point bins. The solid line plots fitted values from a local linear regression of density on pension score deviations from the cut-off, separately estimated on both sides of the cut-off. The thin lines represent the 95% confidence interval.

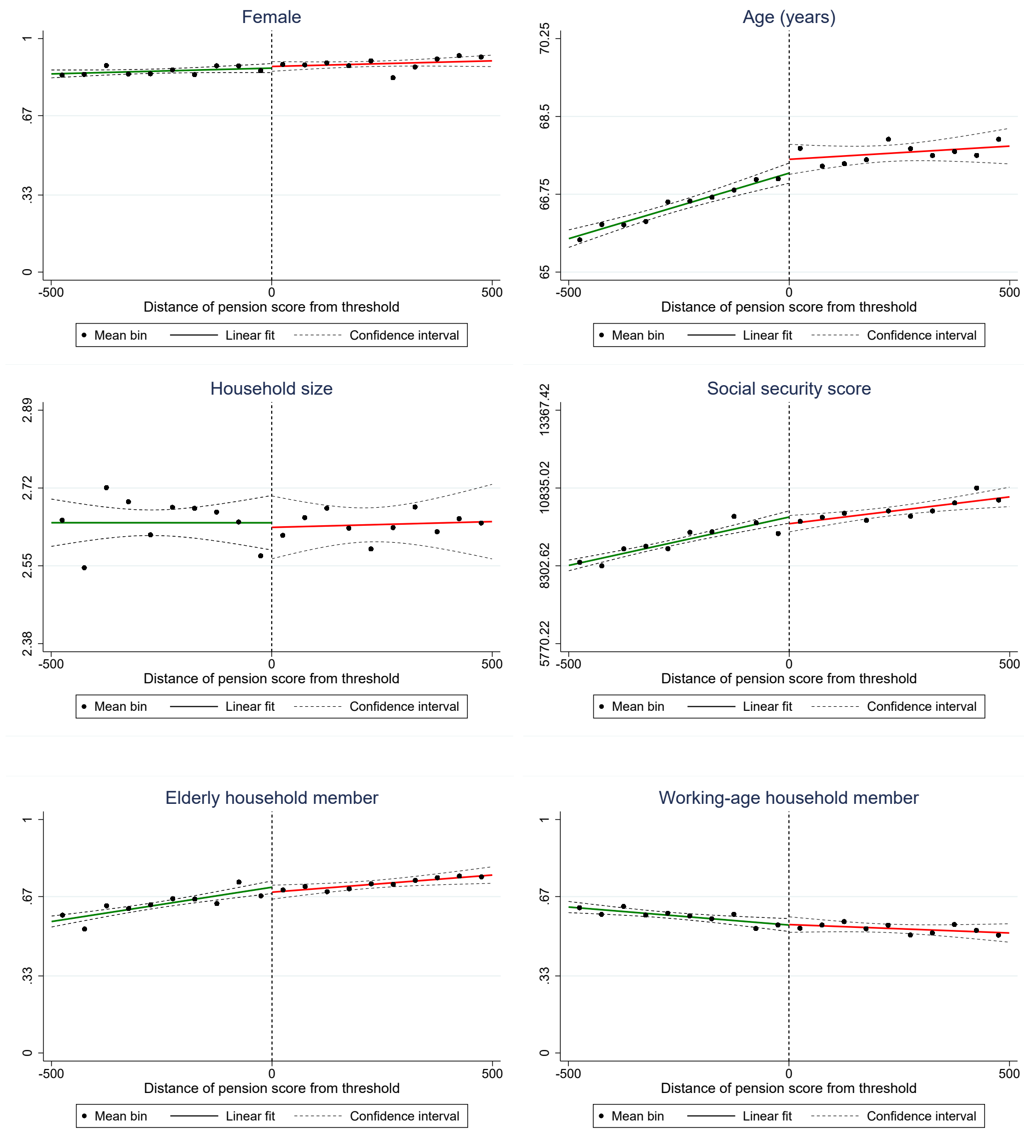


Figure H2. : Pre-determined covariates. RD plots, applicants

Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

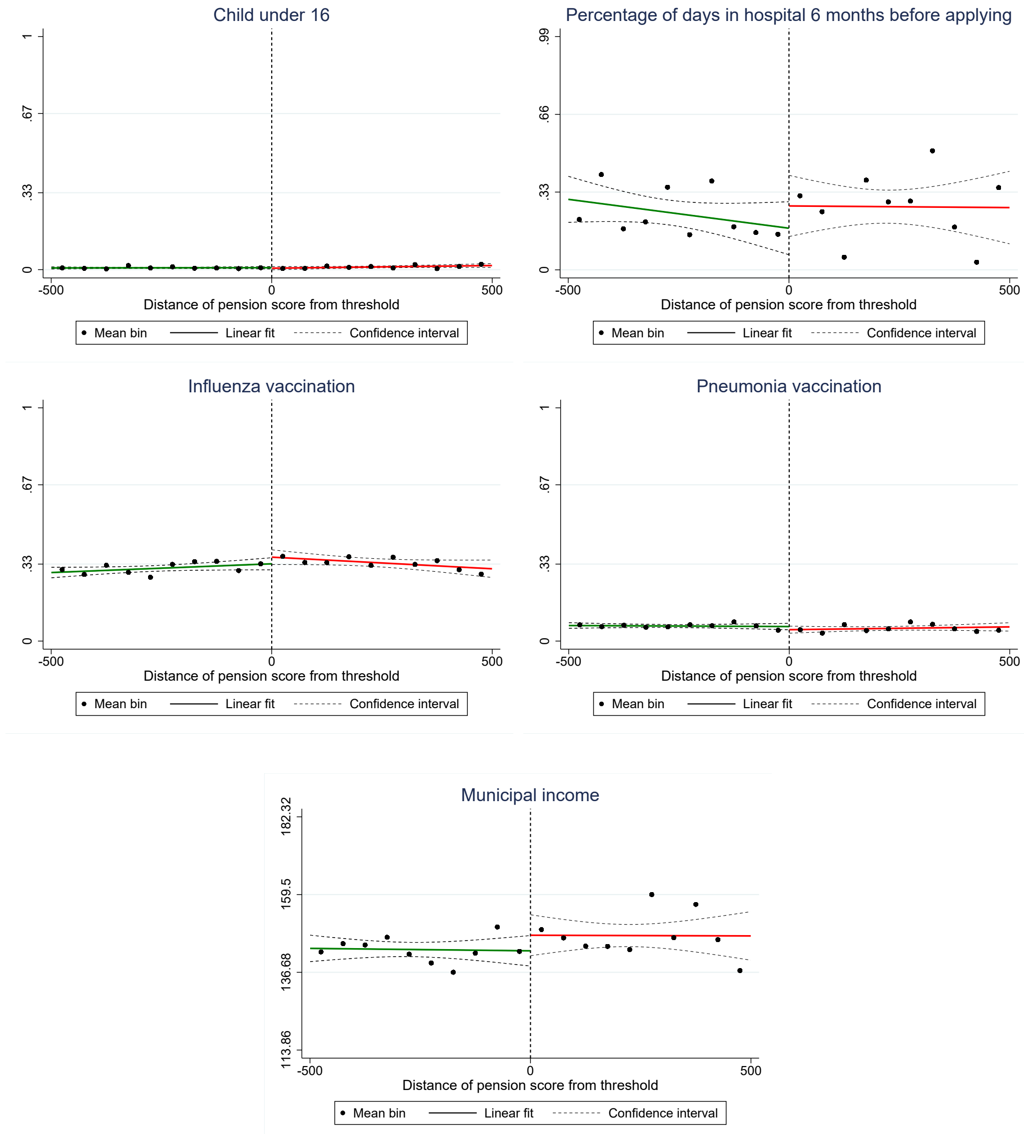


Figure H3. : Pre-determined covariates. RD plots, applicants

Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

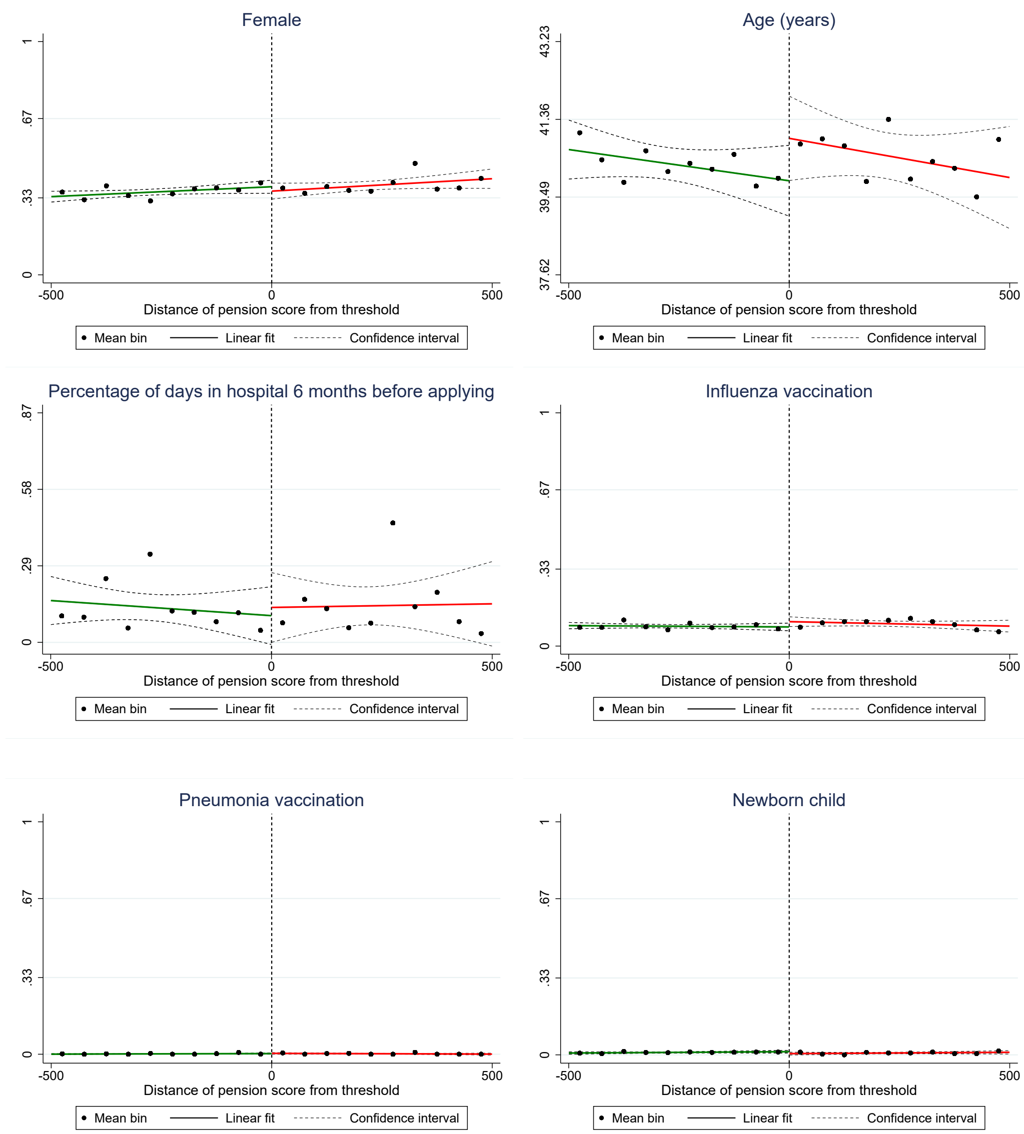


Figure H4. : Pre-determined covariates. RD plots, working-age household members

Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence interval, respectively.

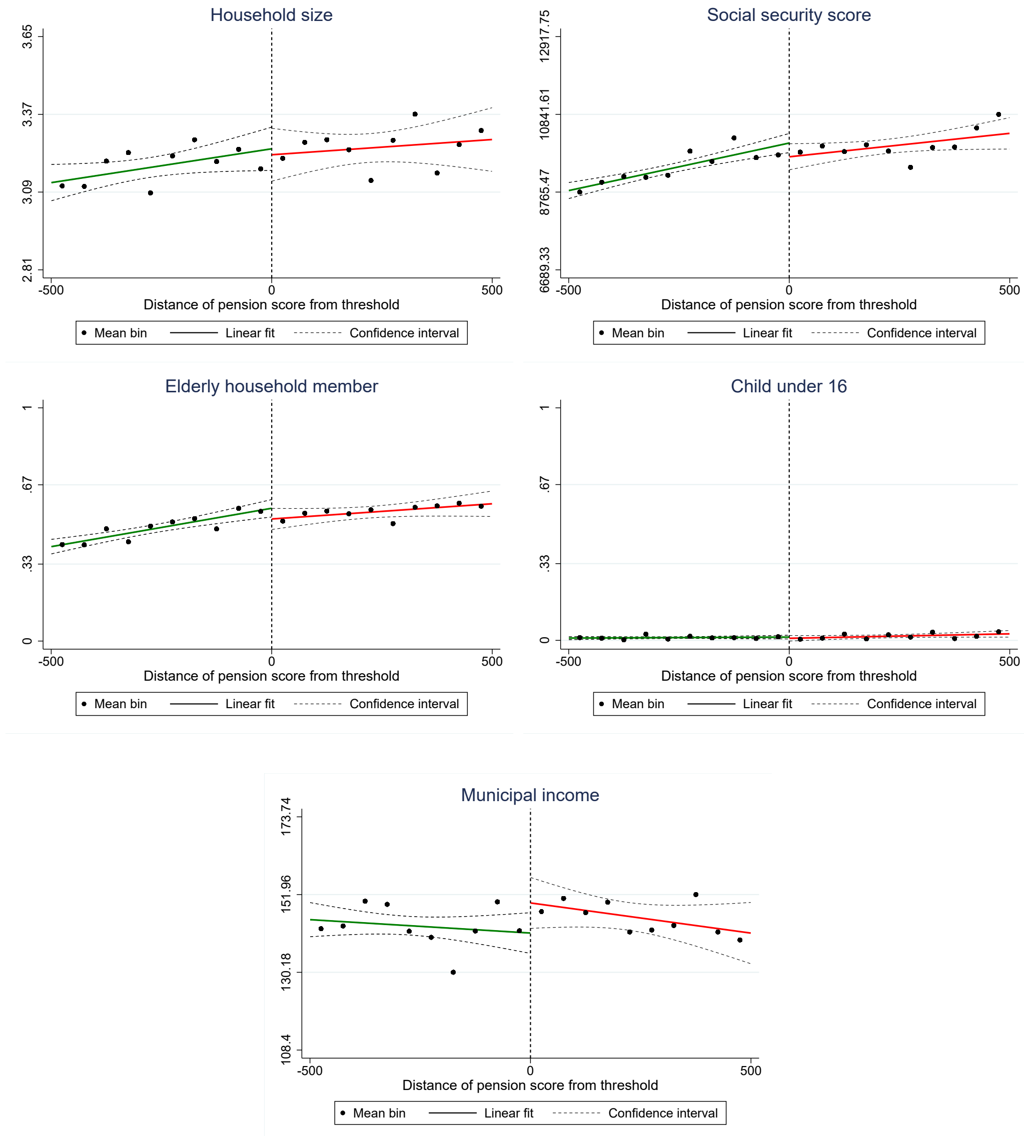


Figure H5. : Pre-determined covariates. RD plots, working-age household members

Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence interval, respectively.

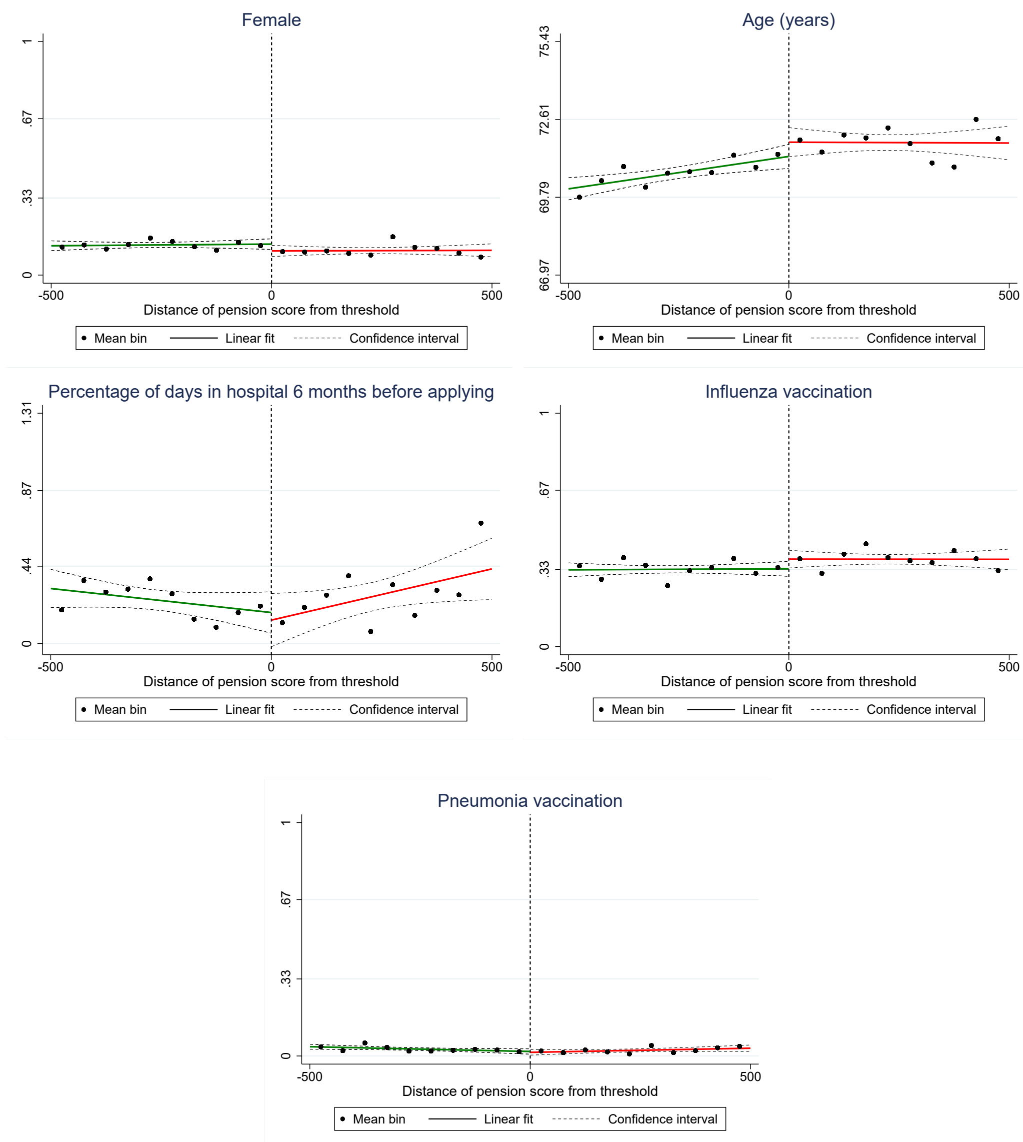


Figure H6. : Pre-determined covariates. RD plots, elderly household members

Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

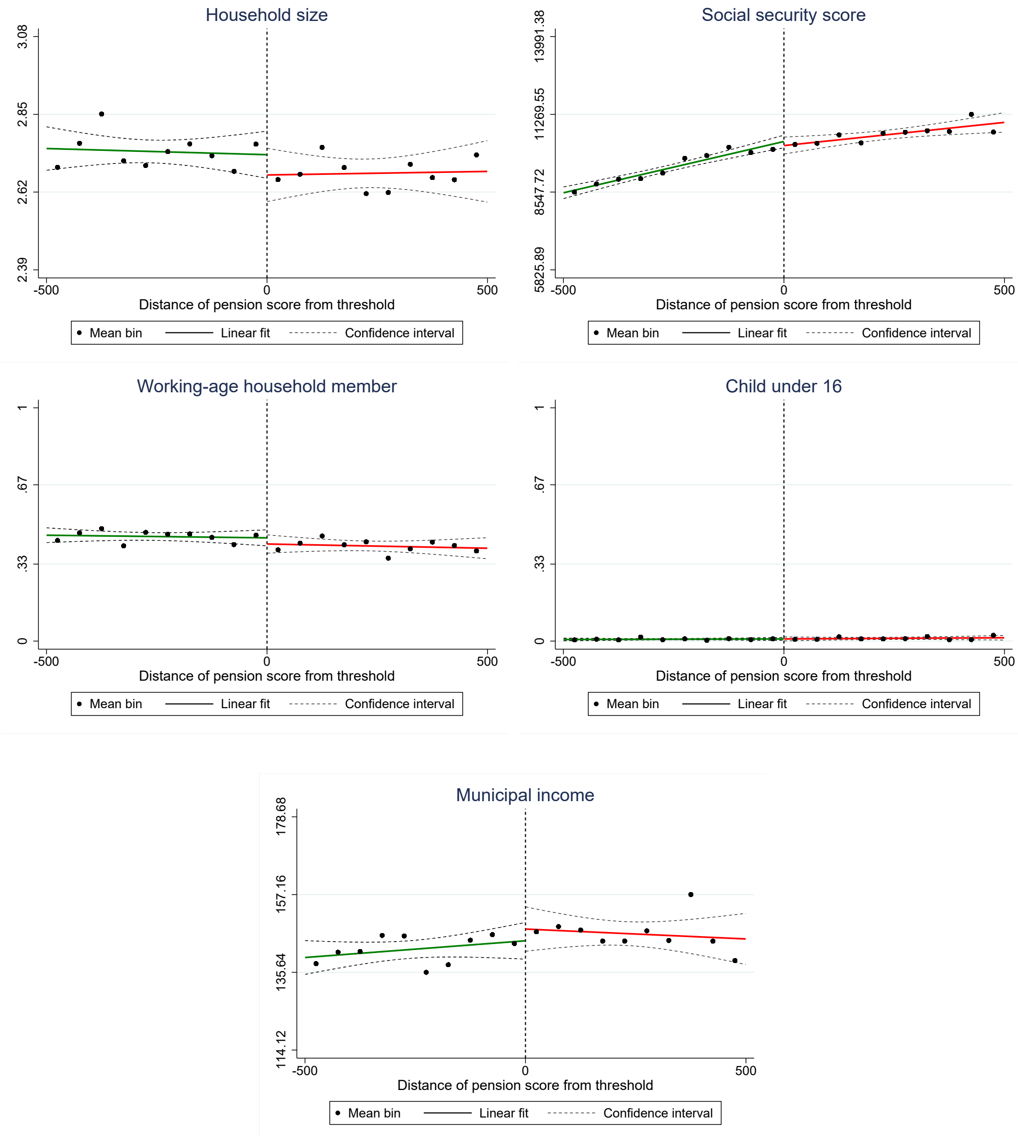


Figure H7. : Pre-determined covariates. RD plots, elderly household members

Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

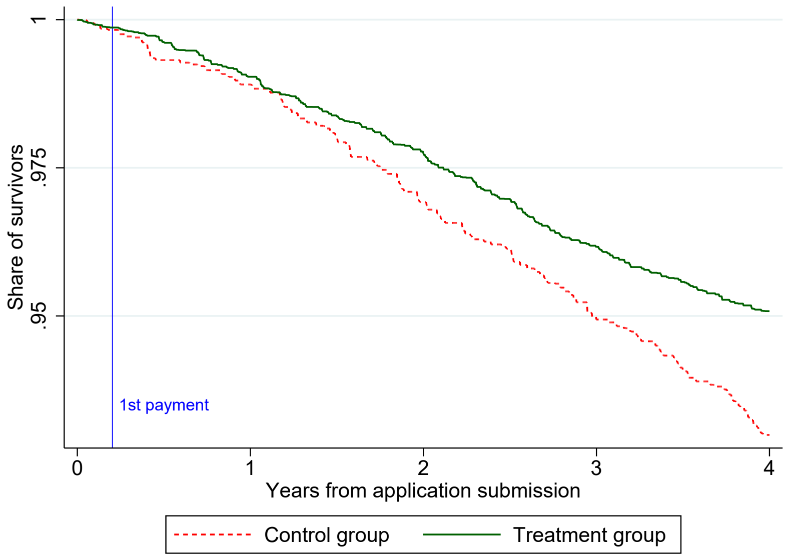
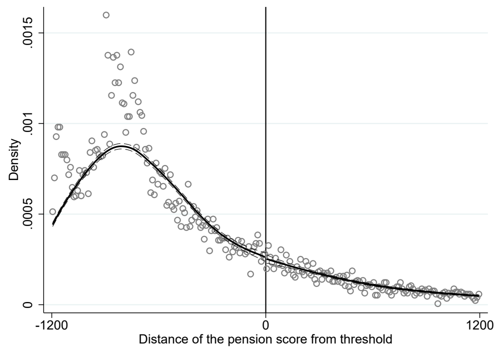


Figure H8. : Share of surviving applicants over 4 years from date of application, adjusted by the deviation of pension score from the cut-off.

Notes: This figure presents the share of survivors in the treatment and control groups at each point in time following the first application. Survival rates are equal to $1 - \hat{S}(t)$, with $\hat{S}(t)$ being the $k_0(t)$ term in the Cox proportional hazard model: $k(t) = k_0(t) \exp(\beta_1 Score_h)$, with t being the time elapsed after the first application. Survival rates are estimated separately for the treatment and control groups in the 500 score-point bandwidth and using triangular weights.

(a) Applicants living with working-age household members



(b) Applicants not living with a working-age household member

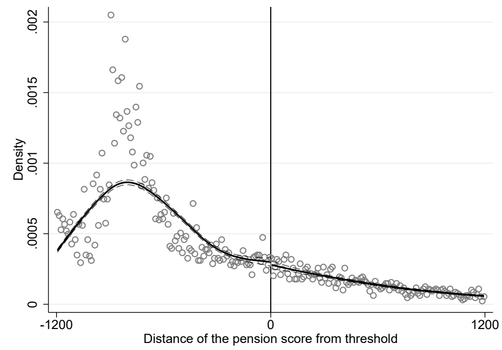


Figure H9. : McCrary tests by household structure

Notes: These figures show the density of individuals in 10 score-point bins. The solid line plots fitted values from local linear regressions of density on pension score deviations from the cut-off, separately estimated on both sides of the cut-off. The thin lines represent the 95% confidence intervals.

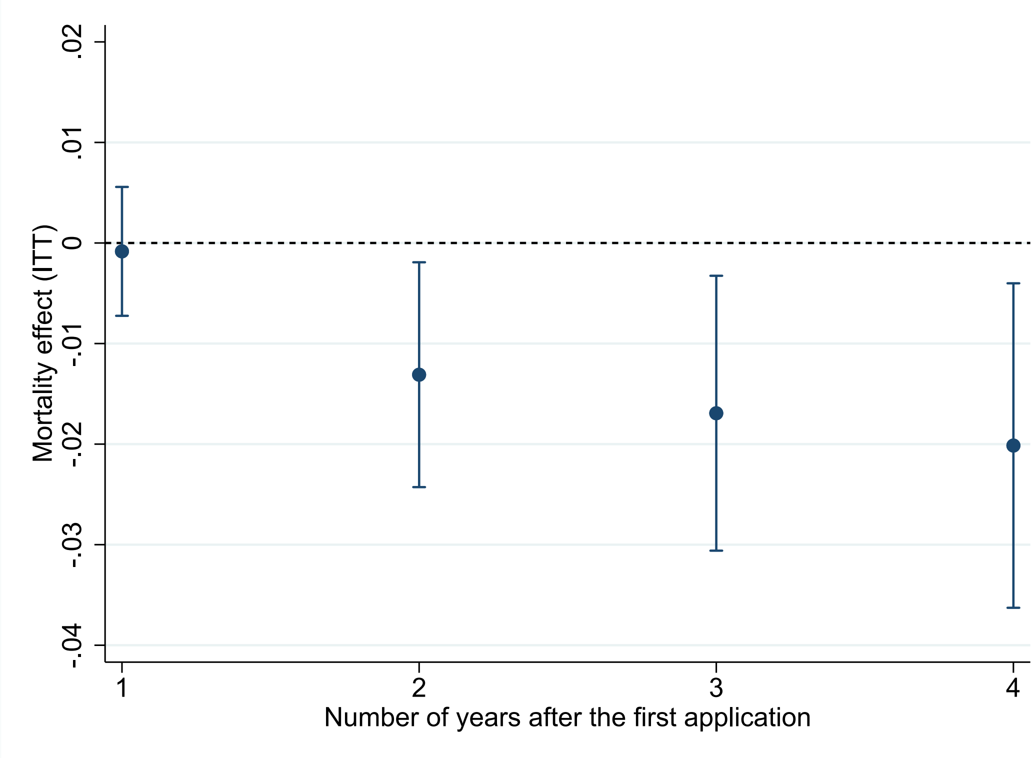


Figure H10. : Mortality by year

Notes: This graph represents the point estimate and 90% confidence intervals of the ITT effect of the basic pension on applicants' mortality in each of the four years observed after the first application.

(a) Working-age household members

(b) Elderly household members

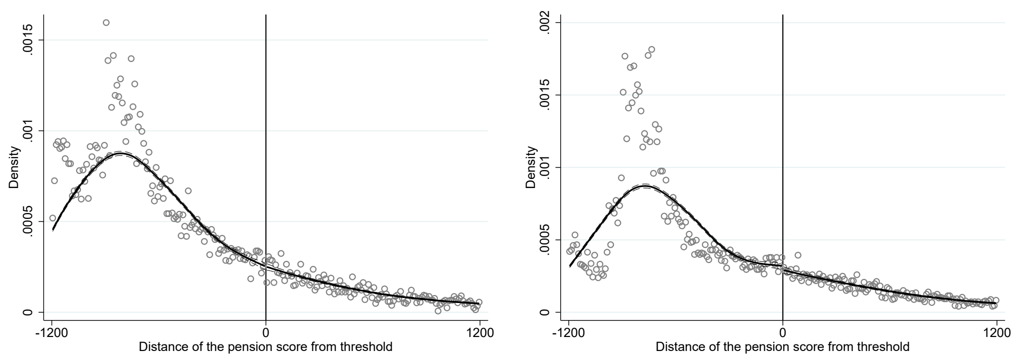


Figure H11. : McCrary tests of working-age and elderly household members

Notes: These figures show the density of individuals in 10 score-point bins. The solid line plots fitted values from a local linear regressions of density on pension score deviations from the cut-off, estimated separately on both sides of the cut-off. The thin lines represent the 95% confidence intervals.

Figure H12. : McCrary test on female fertility-age household members

Notes: This figure shows the density of applicants in 10 score-point bins. The solid line plots fitted values from a local linear regression of density on pension score deviations from the cut-off, separately estimated on both sides of the cut-off. The thin lines represent the 95% confidence interval.

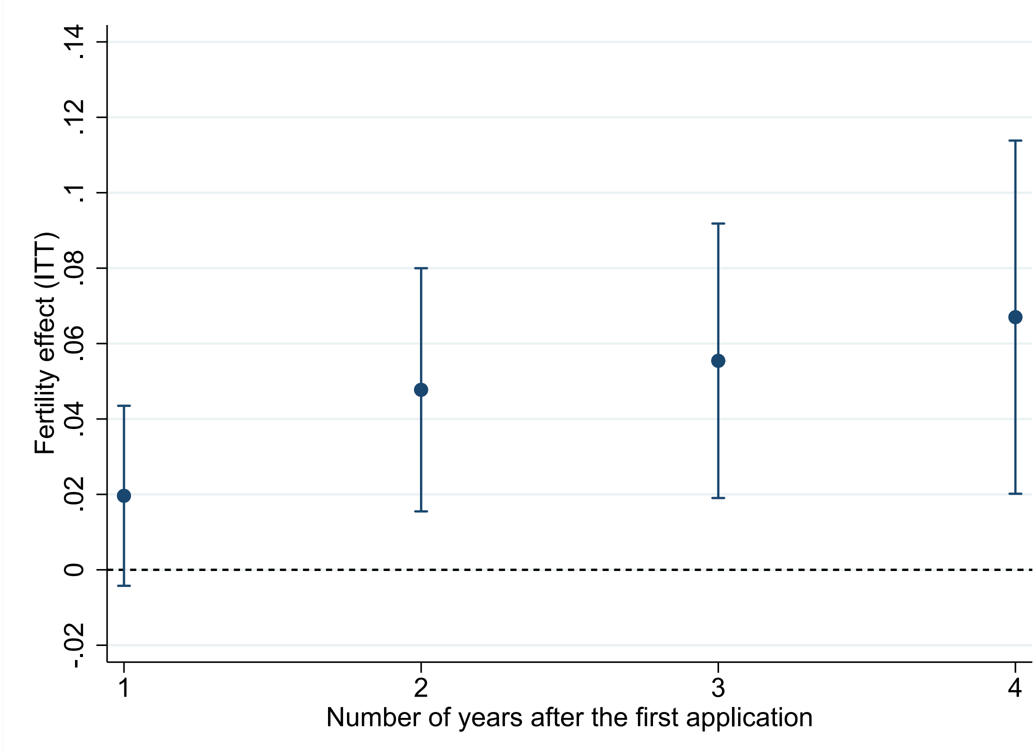


Figure H13. : Fertility by year

Notes: This graph represents the point estimate and 90% confidence intervals of the ITT effect of the basic pension on the probability of having a child for a female fertility-age family member of an applicant in each of the four years observed after the first application.

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