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

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

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-

* Enrico Miglino: Department of Economics, University College London (email: enrico.miglino.16@ucl.ac.uk). Nicolás Navarrete H: Department of Economics, City, University of London (email: nicolas.navarrete-hernandez@city.ac.uk). Gonzalo Navarrete H: M.D., onco-geriatrics, Hospital Clínico de la Universidad de Chile. Pablo Navarrete H: Department of Architecture and Urban Studies, Politecnico di Milano; Institute of Housing, Universidad de Chile; Latin America and the Caribbean Centre, London School of Economics (email: pablojavier.navarrete@polimi.it). We thank the Chilean National Institute of Pensions and the Ministry of Health for the data provided. We also thank Clément de Chaisemartin, Sascha O. Becker, Dolores de la Mata, Gabriel Facchini and seminar participants at the University of Warwick, UCL, PSE, UAB, Universidad de Chile, Universidad Diego Portales, Universidad Alberto Hurtado and the Royal Economic Society for their useful comments. We would also like to thank Claudio Allende for his outstanding research assistance. The authors gratefully acknowledge the financial support of CAF-development Bank of Latin America.

[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:

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

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.

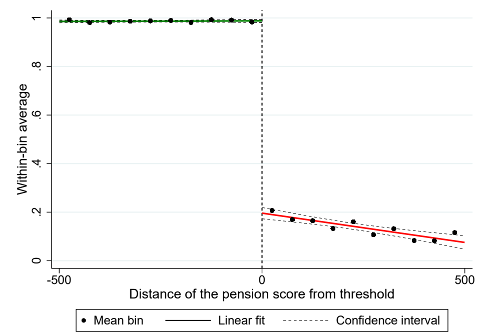
Panel B of Table 1 shows that being in the treatment group increases average monthly pension income by USD 103 and total income by USD 102 over the four years following the first application (27% of the minimum wage).[13] In the

[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.

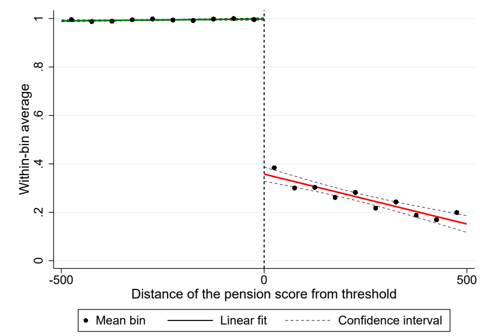
[12]Control group applicants who re-submit an application tend to be those with a lower social security score and those who live in larger households (Appendix Section C).

[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

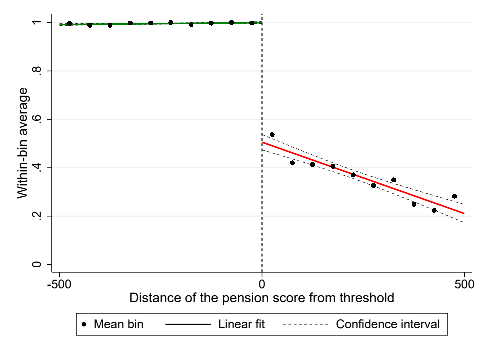
(a) Received a pension within 1 year



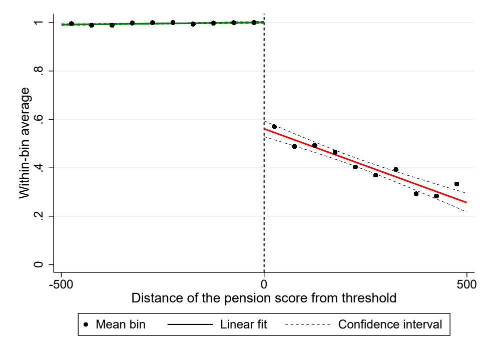
(b) Received a pension within 2 years



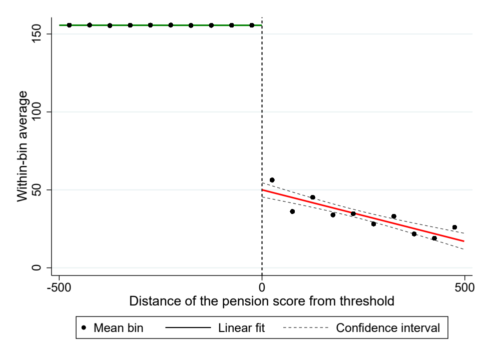
(c) Received a pension within 3 years



(d) Received a pension within 4 years



(e) Pension income



(f) Applicant's total income

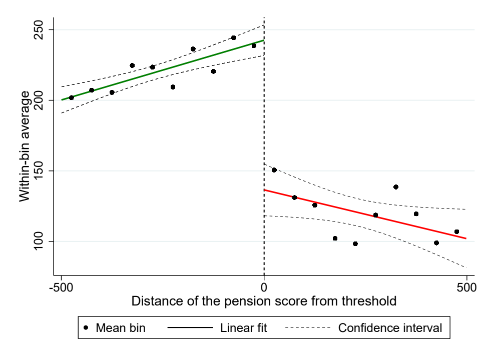


Figure 1. : First-stage effects.

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

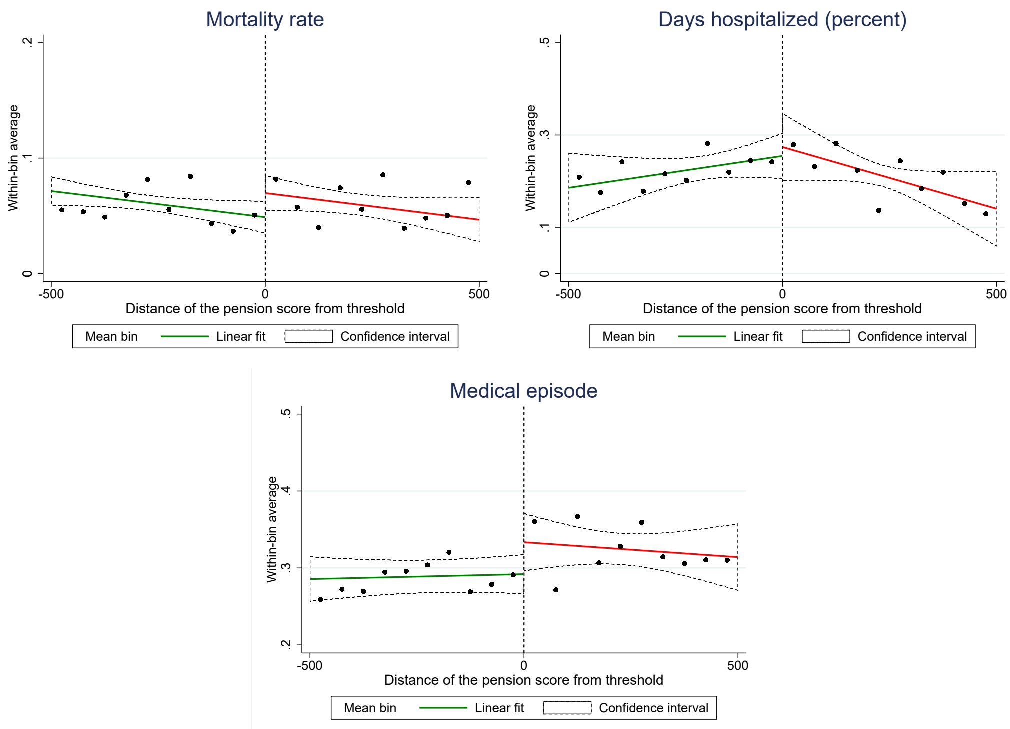


Figure 2. : Effect of the basic pension on mortality, percentage of days hospitalized and medical episodes

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.

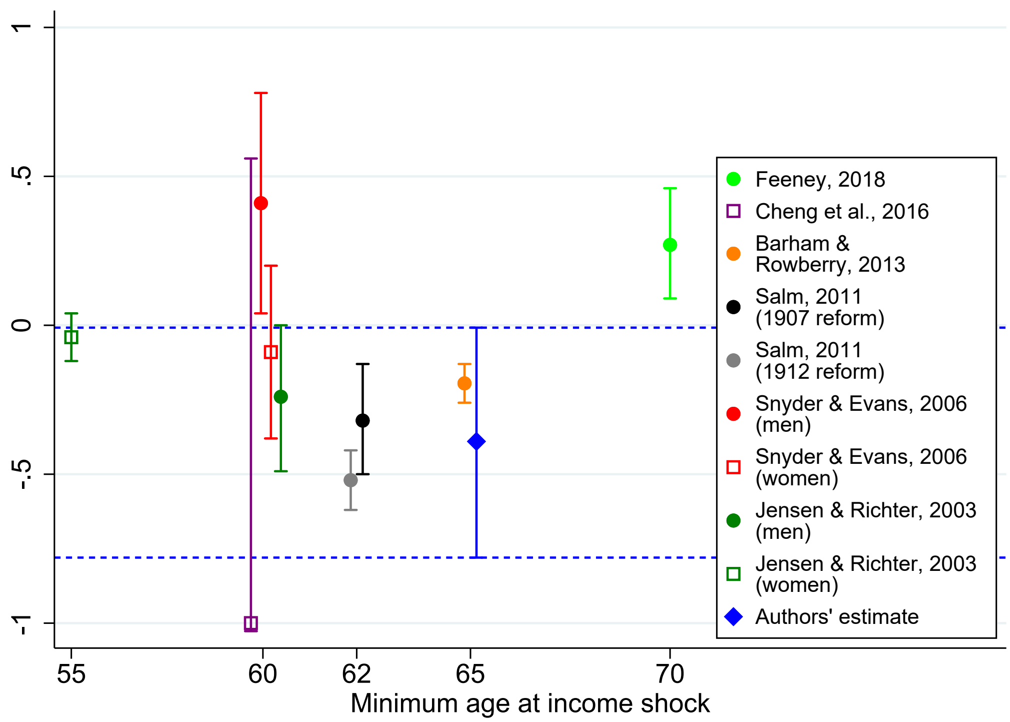


Figure 3. : Estimates of income-mortality elasticity of elderly

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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