Rental Voucher Programs in Middle Income Countries: Quasi-experimental Evidence from Chile

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Abstract

Many low and middle income countries are transitioning towards rental subsidies that, similar to the US Section 8 program, subsidize housing for low income families. I present the first evaluation of such programs on housing and neighborhood quality in a middle income country, Chile. I create a data set that replicates voucher assignment of applicants to the Chilean rental subsidy between 2017 and 2019 and merge it with administrative data on baseline characteristics and a range of outcomes in December 2019, which I further complement with a survey implemented in 2020. I exploit cutoffs and tie-breaking rules in the assignment of the voucher to implement a local randomization regression discontinuity approach. In the period prior to the pandemic, results are similar to the US literature: holding a voucher reduces overcrowding but does little to induce residential mobility to better neighborhoods for low income families. In contrast, in the first eight months following the COVID-19 outbreak of March 2020, my results show that rental vouchers had a broader impact on recipient households. They experienced less unwanted mobility and lower rent burden; moreover, shelter deprivation decreased among the elderly. Holding a voucher also affected how families were coping with the large unexpected income shock: they were less likely to be engaging in new activities to complement their incomes or to miss their rent payments. These results point to a previously underappreciated role of housing subsidies in helping poor households cope with negative income shocks.

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1 Introduction

In December 2013, advised by the US Department of Housing and Urban Development (HUD) and inspired on the US rental voucher program Section 8, the Chilean Ministry of Housing and Urbanism (MINVU) launched the first rental subsidy in Latin America, the "Rental Subsidy" (*Subsidio de Arriendo*). Expectations were that rental vouchers would provide neighborhood choice and increase residential mobility of low-income families towards better neighborhoods, where they would increase their chances of upward social mobility (Chetty, Friedman, Hendren, Jones, & Porter, 2018).[1] To date, several countries in the region, including Argentina, Mexico, Peru, Colombia, Paraguay, Uruguay and Brazil, have followed Chilean steps towards rental policies.

There is a large literature on rental vouchers that shows that the program has not lived up to its promise i.e. rental vouchers have reduced overcrowding and improved other housing quality outcomes, yet families have not gotten access to better environments. However, differences in policy design, institutional context and governance system across countries, specially between high and low or middle income countries, could have a profound impact on the experiences and outcomes of subsidized households (Colburn, 2021), yet there is no empirical evidence outside of the US.

This paper studies the effects of a rental voucher program on overcrowding, residential mobility, and neighborhood characteristics in a middle income country, Chile. That is, a poorer, more unequal, with higher levels of informality[2], and smaller rental markets than the US, and where demand-side subsidies have promoted ownership for decades.[3]

In order to estimate the causal effect of the Chilean rental subsidy, I exploit the assignment mechanism of the program. MINVU calculates an application score using multiple socioeconomic variables to rank families and assign all available vouchers according to vulnerability. I begin by creating a data set that allows me to reconstruct voucher assignment of applicants to the program between 2017 and 2019, and merge it with a broad range

[1]When Chile joined the OECD in 2010, the international organization argued that the Chilean housing policy, focused on homeownership as in the rest of Latin America, have pushed low income families to the periphery (OECD, 2012), squeezed the rental market, reduced residential (and labor) mobility and increased concentration of poverty; therefore, Chile needed to move towards rental policies.

[2]GDP per capita in the US (US$55,753) was almost four times the Chilean GDP per capita (US$15,091) in 2019; the Gini coefficient was 0.444 in Chile in 2017 and 0.411 in the US in 2016 (www.data.worldbank.org); and informality accounts for 29% of the employment in Chile (Henriquez, 2019).

[3]In 1974, Chile introduced the first demand-side housing subsidy directed to ownership, which was later adopted by several other countries and became the main housing policy in the region (Navarro, 2005).

of baseline covariates and outcomes measured in December 2019. I supplement administrative data with a survey that I implemented between September and November 2020 to all applicants in the period of analysis. Hence, I evaluate the program before and after the large unexpected income shock that followed the COVID-19 outbreak in March 2020. With this data in hand, I estimate treatment effects using the Local Randomization approach to Regression Discontinuity Designs (LRRD), developed in (Cattaneo & Frandsen, 2015).

I study the effect of two different rental voucher schemes: a modest voucher for a maximum of eight years available to all eligible applicants aged 18 or older and a larger voucher for a maximum of two years for those aged 60 or older. Before the coronavirus pandemic, I found similar results to previous evidence in the US: holding a voucher reduced overcrowding but it did not provide better neighborhoods for low income families in Chile. In particular, overcrowding decreased in 4.3 pp (40 percent) in regular or younger rounds and in 2 pp (65 percent) in elderly rounds, but there was no change in neighborhood characteristics such as distance to schools and primary care, school quality, crime and poverty rates. Also, holding a voucher increased mobility of elderly households but it did not change how far they moved from their baseline location. In younger families, the voucher had the opposite effect; it did not affect the chances of moving yet voucher holders moved longer distances.

The coronavirus outbreak in March 2020 exposed an already existing housing crisis throughout the world. High and increasing rents and low and stagnated wages leave low income families with almost no residual income to overcome unexpected income shocks (Ellen, O'Regan, & Ganz, 2020); therefore, are highly vulnerable to the long-term negative effects of eviction on well-being (Collinson & Reed, 2018). Many countries - Latin America has not been the exception- have tried to provide income and housing security during the pandemic through eviction moratoriums and emergency rental assistance.[4] In this paper I use survey data to understand whether long-term rental vouchers that were in place by the time that the pandemic hit offered relief to the unexpected income shock that came with the coronavirus pandemic.

Treatment effects around the cutoff show that the rental policy decreased unwanted

[4]For instance, Chile announced 150,000 three-months rental vouchers to the middle class to cover up to US$330 of rents no higher than US$800. Similarly, Mexico has now a three-months rental assistance program. The amount of the benefit varies by family type.

mobility, rent burden and, among the elderly, reduced shelter deprivation during the first eight months of the Covid-19 pandemic. Moreover, the data suggests that voucher holders have responded differently to the unexpected economic shock. Voucher recipients in regular rounds were less likely to engage in new activities to complement their incomes, to lend or give money to other family members or to miss rent payments. Elder voucher holders were also less likely to miss rent payments and more prone to ask for a formal credit. Although the results in this period are obtained from a small sample and should be taken with some caution, this paper suggests that long-term rental vouchers can have a significant effects in times of economic shocks, reducing income and housing instability.

The contribution of this paper to the literature is twofold. First, it contributes to the empirical work that evaluates rental voucher programs. Overall, the evidence suggests that rental vouchers have been effective to reduce rent burden, overcrowding, and homelessness of low-income households but have not been as successful to provide neighborhood choice in general, or better environments for children to grow up in in particular (Mills et al. (2006), Kling et al. (2007),Jacob and Ludwig (2012), Chyn, Hyman, and Kapustin (2019), Chetty, Hendren, and Katz (2016), Schwartz, Horn, Ellen, and Cordes (2020)). However, while many low and middle income countries are moving towards rental policies, this literature is based on evaluations of the US program Section 8 in five large cities in the country and cannot be easily extrapolated to other contexts (Andersson, Kutzbach, Palloni, Pollakowski, & Weinberg, 2016). To the best of my knowledge, this paper is the first evaluation of a rental voucher program offered to low income families to rent a unit in the private market outside of the US.[5]

Secondly, this research contributes to the literature on how housing policy affects housing security and the response of low income families to unexpected income shocks. The few available studies have include the Welfare to Work experiment presented in Mills et al. (2006) that showed that long-term rental subsidies reduced the risk of homelessness and doubling up. And more recently, an observational study by Lundberg, Gold, Donnelly, Brooks-Gunn, and McLanahan (2020) suggests that public housing would be more

[5]Barnhardt, Field, and Pande (2017) evaluates public housing -a rental subsidy to rent a unit administered by the government in a specific location- at the periphery offered to a group of slum dwellers in Ahmedabad, India. Results show no socioeconomic improvement, no increase in tenure security, isolation from social networks and a reduction of informal insurance after fourteen years. Furthermore, the take up of the program was low and many families exited the program to return to slums to be close to their social networks.

effective than rental vouchers to reduce evictions.[6] Importantly, this research differs from previous work in that it measures behavioral responses during times of economic shocks.

The rest of the paper is organized as follows. The next section provides some background, introduce the Chilean rental subsidy and compares its differences to the US rental voucher program. Then, Section 3 describes the data and Section 4 explains the empirical strategy, how the evaluation sample is built and discusses the validity of the research design. Then, Section 5 presents the results and Section 6 concludes.

2 Policy Context and Design

The Chilean rental subsidy was advised by the US Department of Housing and Urban Development (HUD). Inspired in the Housing Choice Voucher Program (Section 8), the Chilean policy subsidize the rent paid by low-income households for units that they find in the private rental market. In this section I describe the design of the rental subsidy implemented in Chile. Also, to understand how this evaluation differs from previous literature focused in Section 8, I explain the main differences between the Chilean and US rental voucher programs and rental market characteristics.

2.1 The Chilean Rental Subsidy

There are two main types of voucher schemes. First, in regular rounds, MINVU targets eighteen or older-headed families with monthly income between US$250 and US$900,[7] and at least US$180 in private savings for homeownership. Voucher holders in these rounds receive US$6,200 in fixed monthly installments of US$180 to pay monthly rents up to the maximum rent payment standard, set nationally at US$402.[8] Voucher holders may use their subsidy over an eight-year period or three years without interruptions.

Second, in elderly rounds, MINVU targets individuals sixty or older with incomes above $140. Savings are not required. In these rounds, the total subsidy amount and rent coverage slightly vary across four groups defined using the National Multidimensional

[6] The authors argue that rental vouchers help families to pay rents on time but do not make them less vulnerable to being evicted.

[7] Families larger than three have higher income upper bounds. Average family income of applicants is US$568.

[8] Except for 30 out of 346 counties located at the north and south of the country where maximum rent standard is US$475.

Vulnerability Index, the Household Social Registry (RSH), created by the Ministry of Social Development (MDS).[9] Specifically, less vulnerable voucher holders get a total subsidy of US$7,380 to cover up to 90 percent of their monthly rent and most vulnerable recipients are assigned US$7,780 to cover up to 95 percent of rents below the maximum rent standard for two years.[10] In 2019, when the first group of vouchers was about to expire, MINVU extended the benefits for two more years, which was not publicly announced or explained to voucher recipients.

To apply to the program, families can go online or in person to any of the forty four PHAs (SERVIUs) across the country.[11] Then, MINVU gathers multiple administrative and self reported data on socioeconomic and housing-related characteristics from other government agencies and uses a complex formula to calculate an application score for each applicant. I explain score components in greater detail in section 4.

To select voucher recipients in each assignment, families are ranked over their score. Then, all applicants above the cutoff - the score of the family who gets the last available voucher- get the subsidy. The number of available vouchers is set by decree for each assignment in each round before the start of the application process. This number is not publicly announced and may increase or decrease for administrative or political reasons, normally not decided by the rental policy team at MINVU. In 2019, also unannounced, MINVU switched to regional voucher assignment i.e. replace national sorting of applicants by regional rankings with regional cutoffs.

The program follows a rolling application system. Rounds are opened for two to nine months and MINVU can make one or multiple assignments during this period. Within the same round, applicants who are not selected in a previous assignment are ranked again with all new applicants for the next assignment. To be considered for the next round, non-voucher recipients need to apply again to the program.

[9] Chile has a national targeting system to assign social benefits. Using survey and administrative data on educational achievement, income, expenses, health, food security and living arrangements, MDS creates a vulnerability score for families in the registry. In 2016, a reform changed how the score was built and MDS stopped informing the continuous score and replaced it by a seven category index, the Household Social Registry (RSH). Eligible families in the rental subsidy are within the first four groups of the RSH i.e. below 40th, 41th-50th, 51-60th and 61-70th percentiles. Many existent evaluations of policies in Chile use a regression discontinuity design based in the old continuous score and have not found evidence of manipulation; Aguirre (2020) in education policy, Navarrete and Navarrete (2016) in housing policy and Carneiro, Galasso, and Ginja (2018) in welfare programs.

[10] Only three percent of voucher recipients are not in the most vulnerable group, none of them are included in this evaluation (Section 5.1).

[11] Some municipalities may voluntarily help families in the application process.

Families have two years to find a landlord willing to participate in the program, who cannot be a family member. The unit has to meet some legal requirements[12] and needs to have at least three separated spaces. Voucher recipients that are initially renting can stay in the same house, while those doubling up have to rent a different unit. Homeowner households cannot apply to the program.

Table 1 show some descriptive statistics of the program. Between 2014 and 2019, MINVU received about ninety thousand applications and spent US$325 million in the assignment of fifty thousand rental vouchers.[13] In this period, six out of ten vouchers have been left unused.

2.2 The Chilean vs. the US Rental Voucher Program

The US Housing Choice Voucher Program (Section 8) is the largest federal housing subsidy program in the US and the focus of the literature evaluating rental voucher programs. Most of the studies take place in five large cities in the US[14] with high lease-up rates[15] and show that while the program reduces homelessness, rent burden[16] and overcrowding, it fails to live up to its promise of providing access to better neighborhoods to low income families (Collinson and Ganong (2018) and Ellen et al. (2020)).

Section 8 aims to provide assistance only to very low-income families.[17] Voucher holders pay thirty percent of their income towards rent and the government pays the rest up to the maximum payment standard, set locally for each Metropolitan Area. In 2020, the average rent paid by voucher holders was US$355 and the amount of the monthly voucher per family was US$810.[18]

[12] Have a certificate of occupancy and a registration number at the IRS.

[13] Just for context, only in 2020 the two largest homeownership programs assigned forty thousand subsidies, ten thousand to buy and thirty thousand to build houses.

[14] Most of the experimental evidence is based on the Moving to Opportunity program comparing voucher holders to public housing recipients (Dumarey, Sket, Joseph, and Boquet (1975);Kling et al. (2007)), the Welfare to Work program in Chicago comparing voucher holders to welfare recipients (Mills et al., 2006), and the Family Options Study using families living in emergency shelters as the counterfactual (Gubits et al., 2016). The few quasi-experimental studies have leveraged randomized waiting lists in voucher assignment in Chicago (Jacob and Ludwig (2012); Chyn et al. (2019)) and New York (Schwartz et al., 2020).

[15] On average, seventy percent of issued vouchers are used in the US but this can vary between thirty five and a hundred percent Finkel and Buron (2001)

[16] Although compared to similar families without a subsidy, voucher holders may pay more for housing while quality standards remain unchanged (Mills et al. (2006), Kling et al. (2007) and Ellen, Horn, and Schwartz (2016)).

[17] Poverty line for a family of three in the US in 2020 was $21,720 and the annual household income of voucher recipients was US$14,693.

[18] See https://www.huduser.gov/portal/datasets/assthsg.html.

Compared to the Chilean rental subsidy, Section 8 is more generous. The fraction of family income spent in monthly rent is fixed at thirty percent in the US program. In Chile, on the other hand, average rent burden after the subsidy is not fixed and higher (between 40 and 50 percent). Furthermore, families in Section 8 receive assistance as long as they meet income requirements of the program. Nevertheless, after two years in a waiting list to receive a voucher (Collinson & Ganong, 2018), families have only two to four months to lease-up or they loose their voucher to another family in the waiting list. Families may be up to In other words, it is harder to get and be able to use a rental voucher in the US but once you lease-up, benefits are higher.

Another important difference between the US and Chile is the size of the rental market. Rental housing in Chile and other Latin American countries represents no more than twenty percent of the housing stock, about half of the observed size of the US rental market (Ross and Pelletiere (2014);Blanco, Fretes Cibils, and Muñoz (2014)). Not surprisingly, almost all landlords have had only one unit in the program. Also, the fraction of low income families who are homeowners is much lower in the US than in Latin American countries Andrews and Sánchez (2011).[19]

These divergences in policy design and rental market characteristics may affect lease-up, residential mobility and neighborhood choice, impacting the experiences and outcomes of subsidized households (Colburn, 2021). For example, social norms and preferences involved in residential mobility and tenure decisions of voucher holders may play a more important role in mediating the effects of housing programs for the poor in societies with higher preferences for homeownership or to remain closer to social networks, (Barnhardt et al., 2017). This highlights the need of new evidence outside of developed countries as many low and middle income countries adopt rental policies.

3 Data

This paper uses a unique data set including administrative, survey and public data at three different moments in time: i) baseline data gathered at application; ii) outcome data collected in December 2019, before the pandemic; and iii) outcome data collected from

[19] Figure A1 in the Appendix shows that above sixty percent of families in the first income quintile in Chile owns their houses, and there has not been much variation in tenure after the introduction of the rental voucher.

September to November 2020, six to eight months after the Covid-19 outbreak in March 2020.

Baseline Data. I access application data that MINVU collects to determine applicants eligibility and build the application score. In particular, I have socioeconomic and demographic characteristics, location[20] and some housing characteristics. In addition, I have survey data for applicants in regular rounds between March 2017 and October 2019, the relevant period of analysis. This survey was sent before assignments were announced and included questions about housing and neighborhood experiences, preferences, and beliefs about renting and residential mobility.

Also, to replicate voucher assignments I create a unique data set of all applicants, their scores, application dates, and assignment characteristics (round type, dates, cutoffs, etc.) linking different administrative data sets and complementing this with public documents containing information for each assignment.

Outcome data before the Covid-19 outbreak. I collect outcome data for December 2019 from multiple sources. First, unit characteristics, household composition and location were obtained from both the Household Social Registry (RSH) and MINVU's administrative data. Second, I have information on household application to any homeownership program between January 2011 and December 2019. Third, for regular rounds only, I also have data on private savings for homeownership.[21]

I create outcome variables for overcrowding, residential mobility and dummy variables to measure application to the two largest homeownership programs in Chile, the *Fondo Solidario de Vivienda (DS49)* (Funding for Cooperative Housing) and *Subsidio Clase Media (DS1)* (Middle Class Subsidy). The DS49 provides fully funded housing (no mortgage) for very low income families, who are only required US$300 in savings. The DS1 provides partial funding to low and middle income families.[22] Further, using data on private savings for homeownership for regular rounds only, I create extensive (opened account) and intensive margins (balance) outcomes.

[20]This project uses a unique geocoded data of baseline and outcome location of all applicants to the program collected from multiple data sources provided by MINVU and complemented with survey data to analyze its quality.

[21]Savings accounts are required to apply to homeownership programs but they can be used to buy any house in the private market.

[22]It gives a down payment that decreases with the price of the house and income of the family, available for low and middle income households who can finance the rest of the house with a mortgage loan or savings.

I link this data to public geocoded information on neighborhood characteristics: geocoded schools, health care centers, municipalities location as a proxy for high economic activity, and poverty, crime and density at the county level. I create different indicators of neighborhood quality. First, access to pre-schools, schools and health care services (primary care and hospitals) are measured using the distance to the closest service and available supply in one and two kilometers. Second, neighborhood school quality is measured by average standardized math and language sixth grade tests scores and the fraction of private, public and subsidized schools in one and two kilometers. Third, distance to commercial activity is approximated by the distance to the closest municipality. Fourth, total crime at the county level is measured in standard deviations from the national mean (z-score). Finally, to characterize income composition I include county poverty rate and the fraction of low income schools in the neighborhood i.e. the fraction of schools in which the majority of their students come from low income families.

Outcome data after the Covid-19 outbreak. I partnered MINVU to implement a Follow up survey between September and November 2020. The survey included questions to measure crowding, residential mobility, neighborhood characteristics, subjective well-being, health, housing and neighborhood satisfaction, income, employment, and asked how families were coping with the Covid-19 pandemic during the first eight months following the outbreak in March 2020.

4 Empirical Strategy

To evaluate the Chilean rental subsidy, this research exploits the discontinuity at the application score of the family who receives the last available voucher (Section 4.1) to implement a multi-cutoff sharp regression discontinuity design (RDD).

The RDD is one of the most credible research design in the absence of experimental treatment assignment. Identification is based in a simple and intuitive idea: when there is a discontinuous change in the probability of treatment by just surpassing a threshold, observations in a small window around that cutoff can be considered "as good as randomly assigned" to treatment and control groups (Lee & Card, 2008).

In the Chilean rental subsidy, applicants at each assignment $s \in S$ are ranked over their score $X_{i,s}$ and are assigned a rental voucher if $X_{i,s} \geq c_s$.[23] The cutoff c_s may vary across assignments in that it is the value of the score of the applicant who got the last available voucher for assignment s. There is full compliance at each assignment s. Figure 1 shows the sharp discontinuity at the cutoff and Figure 2 presents the distribution of application scores and cutoffs in the pooled data.[24]

After the creation of the Household Social Registry (RSH) (Section 4.1) in 2016, the application score became discrete (multiples of five) and, confronted with ties, MINVU established a tie-breaking protocol to handle mass points at the cutoff. A three-step procedure was implemented, assigning vouchers within tied applicants over their family size score, then over their social vulnerability score and then, if there are still left standing vouchers, using randomization. I explain the implications of these issues for the empirical strategy in Section 5.1.

When the support of X_i is finite and has just a few number of mass points, as it is the case in this research[25], the continuity assumption in the standard estimation method used in regression discontinuity settings fails to provide unbiased coefficients and confidence intervals in the smallest possible window $W_0 = [-c_k, c_k]$.[26] Hence, this research uses the Local Randomization approach to RDD analysis (LRRD), first introduced by Cattaneo and Frandsen (2015).[27] I explain the local randomization approach to RDD and how this method is used to evaluate the Chilean rental subsidy in the next section.

[23]Table A1 in the Appendix shows each component of the application score. There are few differences between elderly and regular rounds. Overall, the same elder applicant can get 60 to 140 additional points just by applying to an elderly instead of a regular round. I do not adjust scores to make them comparable between round types since less than two percent of applicants to regular rounds are aged sixty or older; therefore, I do not compare schemes across similar groups of households.

[24]Tables 2 and 3 show the number of participants, maximum and minimum application score, available vouchers, and cutoff for each assignment date in regular and elderly rounds of the program.

[25]The application score in the rental subsidy takes 131 unique values in regular rounds and 109 unique values in elderly rounds.

[26]The standard estimation method to establish comparability between groups around the cutoff and estimate causal parameters in RDD settings has been the continuity-based approach (Lee & Lemieux, 2010). Assuming continuity of the regression functions $\mathbb{E}\{Y_i(1)|X_i = 0\}$ and $\mathbb{E}\{Y_i(0)|X_i = 0\}$ at the cutoff $X_i = 0$, this approach approximates the average outcome that units above the cutoff would have had in the absence of treatment. Then, the average treatment effect at the cutoff, τ_{Cont}, is

$$\tau_{Cont} = \mathbb{E}\{Y_i(1) - Y_i(0)|X_i = 0\} = \lim_{x \downarrow c} \mathbb{E}\{Y_i(1)|X_i = 0\} - \lim_{x \uparrow c} \mathbb{E}\{Y_i(0)|X_i = 0\}$$

When the running variable is discrete, specification bias in the average treatment effect ($\mathbb{E}\{Y_i(0)|X_i = c\} - \mathbb{E}\{Y_i(0)|X_i = c_k\}$) is no longer negligible. In practice, in the smallest window possible the continuity approach will consider each mass points as a bin and local polynomial methods would extrapolate from the closest mass point on either side to the cutoff.

[27]See Branson and Mealli (2018) for a review of alternative estimation methods in RDD settings. A common practice in the presence of discrete running variables, but in contexts with large number of mass points, has been the use of clustered standard errors by the running variable (Lee & Card, 2008) (Kolesár & Rothe, 2018).

4.1 Local Randomization Approach to Regression Discontinuity

Let $Y_{i,s}(1)$ and $Y_{i,s}(0)$ be the pair of potential outcomes under treatment and control in each assignment $s \in S$ and $D_{i,s} = D_{i,s}(X_{i,s}) = I(X_{i,s} \geq c_s) \in \{0, 1\}$ the treatment indicator. Then, $Y_{i,s} = D_{i,s}Y_{i,s}(1) + (1 - D_{i,s})Y_{i,s}(0)$ is the observed outcome for individual i (Rubin, 1974). As it is common in multi-cutoffs RDD, I pool and normalize the running variable around a unique cutoff $c_s = c = 0$ and analyze the data as in a single cutoff RDD (Cattaneo, 2018).

The LRRD makes strong assumptions about the assignment mechanism near the cutoff (Branson & Mealli, 2018). Instead of modeling assumptions, like the ones used in the standard continuity approach, the LRRD assumes that there exists a window $W_0 = [x - e, x + e]$ in which the distribution of the score is known and it is the same for all units, as in experimental data. Inside W_0, potential outcomes may depend on the score only through treatment indicators and there should not be interference between units' potential outcomes ("Stable Unit Treatment Value Assumption" or SUTVA).[28]

Under these assumptions, score ignorability $Y_{i,s}(X_{i,s}, D_{i,s}) = Y_{i,s}(D_{i,s})$ is guaranteed inside W_0; therefore, analyzing the data as if it were experimental data is straightforward. Hence, the causal treatment effect under the LRRD, τ_{LR}, is the difference between the average outcome of treated and controls in the largest window around the cutoff where local randomization assumptions hold. More formally,

$$\tau_{LR} = \bar{Y}_{i \in W_0}(1) - \bar{Y}_{i \in W_0}(0) \approx \mathbb{E}\left\{Y_i(1) - Y_i(0) | X_i \in W_0\right\}$$

The LRRD approach may be valid only within a few units in a narrow window around the cutoff. Therefore, it uses randomization inference to build exact confidence intervals and hypothesis tests that are robust in small finite samples.[29] Similar to the problem of

[28] Depending on the outcome, different applications within the same unit could result in interference. However, this is not observed in the data -those doubling up applied while living with homeowners, who are not eligible in the program. Also, the presence of peer effects in participation or leasing-up could result in interference. However, the size of the program and national distribution of its participants makes this very unlikely in a sample near the cutoff using different cutoffs. Finally, since the cutoff is a function of the distribution of scores and available vouchers, there might be interference between earlier and later treated within one round, while not between ever treated and never treated, which, as I will explain in the next section, is what I use in this evaluation. Importantly, SUTVA violation does not invalidate inference in the LRRD (Cattaneo & Frandsen, 2015).

[29] Randomization inference assumes fixed potential outcomes but random assignment mechanism. The Fisher sharp null hypothesis used in randomization inference tests for zero treatment effect for any unit i.e. $H_0 : Y_i(0) = Y_i(1)$, and it is exact in that it uses observed outcomes to impute potential outcomes under

bandwidth selection in the continuity approach, the most important step in LRRD is window selection. In this paper, I use the data driven procedure developed in (Cattaneo & Frandsen, 2015).[30] Specifically, the selected window for assignment s (W_s) is the largest window such that the minimum p-value obtained through all balance tests in baseline covariates inside W_s, and any smaller window W_k (with $s > k$) is above a predetermined significance threshold, in this case $\alpha^* = 0.1$. Balance tests are conducted using sharp null hypothesis tests of no difference in mean on pre-treatment covariates and assuming fixed-margin treatment randomization at the assignment level.[31] Once W_s is selected for each assignment, I pool all of them in the evaluation sample W_0.

Having a discrete running variable simplifies considerably the window selection procedure in Cattaneo and Frandsen (2015). The minimum possible window is known i.e. local randomization assumptions must hold in the window that contains the two mass points that are immediately above and below the cutoff. In this window, treatment and control units must be in different sides of the cutoff, regardless of the values of the running variable.

In the Chilean rental subsidy, when there is no mass point at the cutoff, the minimum window is $W_s = [-5, 5]$. However, ties at $c_s = 0$ require some transformation of the normalized running variable in $W_s = [0, 0]$ (Cattaneo, 2018), for which I use the assignment rules of the program (See section 4.1). More specifically, ties that were broken using family size or social vulnerability score are re-scaled to be in the $W_s = [-2, 2]$ window and those remaining vouchers, which were randomly assigned, are re-scaled to be in $W_s = [-1, 1]$. Importantly, any transformation that keeps the same order between mass points produces the same results (Cattaneo, 2018).

Considering the rolling application system and the "as good as radom" assignment in a small window around each C_s, this research design mimics a sequential stratified experimental design (Pocock & Simon, 1975) in which each assignment s is a strata or block of applicants that are independently assigned to treatment and control groups.

Hence, in order to estimate the causal effects of the rental voucher program in the outcome $Y_{i,s}$ of applicant i in assignment s, I use the following fixed effect model that

treatment and control, such that $Y_i(0) = Y_i(1) = Y_i$.

[30] I use the package rdwinselect in Stata to implement window selection in LRRD settings (Cattaneo & Frandsen, 2015).

[31] To be conservative, p-values in balance tests for window selection do not adjust for multiple testing.

exploits variation around the cutoff at each assignment.[32]

$$Y_{i,s} = \alpha + \tau Voucher_{i,s} + \gamma_s Assignment_s + \beta Z_{i,s} + \delta S_{i,s} + \epsilon_{i,s} \tag{4.1}$$

Where $Voucher$ is an indicator variable for having an application score above the cutoff, $X_{i,s} > 0$, γ_s are assignment fixed effects, the vector $Z_{i,s}$ includes baseline covariates and $S_{i,s}$ comprises score components involved in tie-breaking rules. Outcomes in $Y_{i,s}$ are described in Section 6.

I estimate equation 4.1 separately for regular and elderly rounds. In regular rounds $S_{i,s}$ includes dummy variables for family size and social vulnerability score.[33] However, in the evaluation sample of applicants to elderly rounds, family size and social vulnerability scores do not vary; therefore, I use dummy variables for score components that are more likely to break ties i.e. the number of elderly in the household and age.

The parameter of interest, τ, is the normalized and pooled LRDD estimate of the effect of being assigned a voucher, or Intention to Treatment Effect (ITT). This coefficient τ recovers a double average: the weighted average of the average treatment effects within assignments.

Since each assignment occurred at a particular cutoff in a specific moment in time - and region, since 2019-, I cannot disentangle the heterogeneity by cutoff from the heterogeneity by treatment duration in the evaluation sample. Hence, despite having multiple cutoffs, this evaluation is focused on the normalized and pooled LRDD estimate of τ. Furthermore, given the small sample size and low lease up rate in this LRRD application (30 and 50 percent in regular and elderly rounds, respectively) I do not present estimates of the Local Average Treatment Effects (LATE) of using the rental voucher. However, ITTs are relevant from a policy perspective in that lease-up cannot be enforced. Further, since there is full compliance in the use of the voucher in the control group, the average treatment effect on the treated is τ adjusted by compliance rates in the treated group (Angrist & Pischke, 2008).

Next, I describe the window selection process and construction of the evaluation sample W_0.

[32] There is no subscript for rounds because each assignment is unique to a round.

[33] Together they explained seventy percent of the total score in these rounds.

5 Empirical Analysis: Evaluation Sample Construction

This section explains how the evaluation sample is built. First, I describe the data used in the window selection procedure. Second, I present window selection results and describe baseline characteristics of the evaluation sample. Third, I present additional falsification tests to asses the validity of the identification assumptions of the local randomization approach to regression discontinuity inside of W_0. Finally, I discuss the external validity of the results presented in Section 6.

5.1 Data Construction for Window Selection

I evaluate the effect of the Chilean rental subsidy using applicants in regular and elderly rounds of the program between March 2017 and September 2019. I first expand the data to have each of these applicants as many times as they were sorted and assigned to treatment or control groups during the period of analysis. Then, I apply the data driven procedure in (Cattaneo & Frandsen, 2015) to select the window of analysis for each assignment s and build the evaluation sample W_0.

Two sample restrictions are implemented. First, following Cattaneo and Frandsen (2015), I keep assignments that have at least ten observations at each side of the cutoff c_s in the minimum window.[34]

Second, the rolling application system explained in Section 4.1 may create two different control groups: applicants who never receive a voucher (never treated) and applicants who receive the voucher in a later assignment of the round they applied to (later treated) i.e. are ranked more than once in one round.[35] Although the likelihood of participating in more than one assignment can be thought as exogenous in a small window around the cutoff, using both control groups together affects the interpretation of the treatment effect. The estimand τ in equation 4.1 would be a weighted average of two effects: holding a voucher (treated vs. never treated) and holding a voucher for a longer period of time (treated vs. later treated). The small sample size around the cutoff cannot be used to precisely identify these two effects in a fully interacted model; therefore, I keep only never treated units in

[34]This guarantees that a randomization-based test of the sharp null of no treatment effect in the minimum window would have 60-80 percent of statistical power under the following assumptions: discrete outcome, a minimum detectable effect of one standard deviation and significance levels of 0.05-0.15.

[35]On average, applicants who get the voucher participated in two assignments. The median number of assignments in the treatment group is one.

the control group. I argue that estimating the effect of holding a voucher is the first order question for the first evaluation of the Chilean rental subsidy.

Altogether, to select the evaluation sample I use 53,410 observations (42,293 unique applications) from March 2017 to September 2019. Columns 1 to 5 in Tables 4 and 5 show the number of participants, maximum and minimum score, number of available vouchers and cutoff for the assignments used in window selection in regular and elderly rounds, respectively.

In 2019, only a few assignments had enough sample size at both sides of the cutoff to meet the first sample restriction.[36] More specifically, only the regular assignments in October in the Los Lagos, Araucania and O'Higgins regions, and the elderly assignments in July in Santiago and Valparaiso regions are included in the evaluation sample W_0.[37]

5.2 Window Selection Results

As it is common in this literature, pre-treatment covariates are divided in two sets, one for window selection and another for further falsification tests of the evaluation sample. In this research, for each assignment, treatment and control groups in W_s are balanced in terms of income, distance (km) to the closest PHA[38], and indicator variables for tenancy, previous application to homeownership programs, high poverty[39] and density counties. Savings and an indicator variable for online application are only available for the analysis of regular rounds.

The second set of covariates used in additional falsification tests in Section 5.2.1 includes age and indicator variables for female, being married or partnered, having neighbors in 400 meters that participated in previous rounds of the program[40] and having family

[36]Initially, the expanded data had 95,910 observations from 56,704 unique applicants that participated in one or more assignments in the period of analysis. Implementing the first sample restriction implied dropping 34,638 observations, most of which from regular rounds in 2019, after the regional voucher assignment reform. The second sample restrictions further reduce the number of observations by 4,300. In addition, I dropped 3,562 duplicated observations of applicants from a previous round who were arbitrarily added by MINVU to the regular assignment of October 2018. They did not applied again and none of them was selected into treatment, therefore, I dropped this observations to keep treated and controls that have common support in application date.

[37]Los Lagos, Araucania and O'Higgins are all regions located south from Santiago. Valparaiso and Santiago are in the center of the country and are the two most populated regions. Valparaiso is one hour from Santiago to the coast.

[38]This is a proxy for location characteristic and access to formal information about the rental subsidy and other housing policy options.

[39]Higher than the national head count ration, 8.6 percent (Casen, 2017).

[40]400 meters is the average size of a census track in Chile.

income below the national poverty line adjusted by family size.[41] Also, for regular rounds only, I use baseline survey data to create dummy variables for survey response, strong preferences to stay in baseline neighborhood, high satisfaction with baseline housing unit and had applied to the rental program to save for ownership.

The chosen window for assignment s, W_s, is the largest window around the cutoff in which the first set of covariates is balanced. Figures A3 and A4 in the Appendix present the results of the window selection procedure. Each dot in the graphs represents the minimum p-value (Y-axis) of all balance tests in windows of increasing lengths (X-axis) and the line indicates the threshold for statistical significance $\alpha^* = 0.1$. Conditional on balance in the minimum window [-1,1], W_s includes all consecutive dots above the threshold. After the first dot fells below $\alpha^* = 0.1$, this window and all larger ones are considered in violation of the LRRD assumptions and excluded from the analysis.

The evaluation sample (W_0) pools all W_s together. Columns 6 to 10 in Tables 4 and 5 show summary statistics for the assignments included in the evaluation sample. In total, 3,133 observations in eleven assignments are included in W_0: 1,356 in seven assignments in regular rounds and 1,777 in four assignments in elderly rounds. The maximum size of the windows W_s in the evaluation sample is $W_s = [-15, 15]$.

Table 6 presents summary statistics for the pooled sample (Column 1) in regular rounds and separately for treatment (Column 2) and control group (Column 4). The sample includes mostly families headed by young single mothers, whose average income is US$528.[42] One fifth of the families are under the poverty line and three fourth are initially tenants, paying almost half of their household income towards rent (US$224). Forty percent of the sample lives in high poverty counties and almost ninety percent applied from seven out of the sixteen regions in Chile[43], all of them in the center and south of the country.

Summary statistics for elderly assignments inside of W_0 show that elder applicants are on average 76 years old, 61 percent are women, 40 percent has a partner, and have no children at home. Rent burden in this group is roughly 100 percent[44] and about half

[41] In 2017 poverty line adjusted by family size was US$210, US$342, US$455, US$556 for a family of one, two, three and four, respectively. The national poverty rate was 8.6 percent, varying from 2.1 percent in Magallanes to 17.2 percent in the Araucania region.

[42] Average household income in Chile was US$1,302 in the last National Socioeconomic Survey (CASEN); in the first four income deciles was, respectively, US$140, US$400, US$540 and US$655 (CASEN 2017).

[43] In Santiago, Valparaiso, O'Higgins, Maule, Biobio, Araucania and Los Lagos.

[44] Although there are few observations in elderly data with non-missing baseline rent, this is consistent with rent burden in the population of voucher recipients (95 percent) using all observations with non-missing data

applied from two regions, Santiago and Valparaiso, the largest cities in the center of the country (Table 7).

5.2.1 Falsification Tests and External Validity

To further study the validity of LRRD assumptions inside W_0, this section analyzes balance in additional baseline covariates and examines the density of the running variable to check for manipulation of the application score.

Regression discontinuity designs are expected to have little external validity for populations away from the cutoff, (Cattaneo, Keele, & Vazquez-bare, 2016). To asses the external validity of this evaluation, this section compares summary statistics between the group of applicants in the evaluation sample and all voucher recipients in the period of analysis in regular and elderly rounds.

Balance Tests. To analyze statistical balance between treatment and control groups around the cutoff in each assignment (where quasi-experimental variation occurs) I use the following fully interacted fixed effect model:

$$Z_{i,s} = \alpha + \tau_{1,s} D_{i,s} + \gamma_s Assignment_s + \beta_s D_{i,s} * Assignment_s + \delta S_{i,s} + \epsilon_{i,s} \tag{5.1}$$

Where $Z_{i,s}$ is the vector of baseline covariates, γ_s are assignment fixed effects, and $S_{i,s}$ are score components that determine tie-breaking rules, as in equation 4.1.

Coefficients $\tau_{1,s}$ and β_s in equation 5.1 test the null of no effect within each assignment or strata (H_0). To identify these coefficients, all covariates in $Z_{i,s}$ need to vary across and within assignments, restricting the number of variables that can be used in balance tests.[45]

For this reason, I present two specifications of equation 5.1 in Tables 6 and 7. Panel

(N=8,018) (See Table A3 in the Appendix

[45] Despite the large set of baseline characteristics available, finding covariates that vary between and within assignments in a small window around different cutoff is challenging. For instance, the fraction of poor families, female, spouse, geocoded location and county poverty rate vary across but not necessarily within assignments in regular rounds; furthermore, all families in regular rounds in W_0 have children. In elderly rounds, all households are in the bottom 40 percent of the vulnerability index (RSH) and have no children. Also, geocoded location, county poverty rate, tenancy and neighbors in previous rounds do not vary within each assignment. Finally, some variables are not available for the entire period of analysis e.g. rent is available only after September 2018 and regional assignments in 2019 eliminated variation by region within assignments; to control for rental market characteristics and avoid colinearity between region characteristics and assignment FE, $Z_{i,s}$ includes only county level variables.

A shows the results of balance tests using equation 5.1 for covariates that vary between and within assignments and Panel B presents the results of a weaker yet commonly used hypothesis test. Specifically, Panel B presents the results of equation 5.2 for variables that vary between or within assignments, not both; therefore, $\tau_{2,s}$ tests the null of zero weighted average effect in the pooled data (H_0').[46]

$$Z_{i,s} = \alpha + \tau D_{i,s} + \gamma_s Assignment_s + \delta S_{i,s} + \epsilon_{i,s} \tag{5.2}$$

Tables 6 and 7 present the results of $\tau_{1,s}$ and $\tau_{2,s}$ for regular and elderly rounds, respectively.[47] The bottom panel reports two omnibus tests of balance: the F-statistic of the joint significance test from a regression of the treatment indicator in all covariates and the Westfall-Young multiple-testing test of overall experimental relevance. The latter uses randomization inference to test for statistical significance of the experiment as a whole when there are multiple estimating equations or treatment effects.

The results of $\tau_{1,s}$ and omnibus tests show no statistically significant differences in baseline characteristics between treatment and control groups in regular and elderly rounds. Further, Panel B shows only a few statistically significant differences using this weaker hypothesis test and only under OLS inference.[48]

Density Test. Manipulation of the application score would be very costly for applicants, if possible at all. Applicants would need to anticipate voucher availability, their own score and the entire score distribution. Not surprisingly, Tables 4 and 5 show no clear pattern between the participants and the number of available vouchers, or between available vouchers and the value of the cutoff.

To analyze manipulation inside the evaluation sample W_0 I use a binomial test of the probability of treatment inside the window W_0 (Cattaneo, 2018). For intuition, if applicants cannot precisely control their value of the score, the probability of success (treatment) q is

[46] H_0' is weaker in that H_0' could be zero if a specific linear combination of the effects in each assignment is zero, while H_0 is false (Young (2019) Firpo, Foguel, and Jales (2020)).

[47] Also, graphical analysis in Figures A3 and A4 in the Appendix shows the distribution of covariates around the cutoff in the pooled data i.e. $\tau_{2,s}$.

[48] In regular rounds, there is a small difference in age (treated are 1.6 years younger), which is significant only using OLS inference. In elderly rounds, treated are six percentage points more likely to live in Santiago, significant at the 90 percent of confidence, using OLS p-values only. Importantly, OLS and Randomization-t p-values tend to differ in large variance settings (or under the presence of outliers) which could be explained by differences between, not within assignments, and equation 4.1 includes assignment fixed effects and county level controls for rental market characteristics.

expected to be consistent with the assignment mechanism assumed in W_0 i.e. randomization of available vouchers at the assignment level.

Given that true q is unknown, I use two different values: the observed probability of assignment in a small window around the cutoff ($q1$) and complete randomization or $q2 = 0.5$. Table 8 shows no evidence of manipulation. The observed probability of treatment in the evaluation sample in regular rounds ($q = 0.465$ percent) is similar to both $q1$ and $q2$ and statistically compatible with complete randomization $q2 = 0.5$ (p-value 0.08). In elderly rounds, the observed average probability of treatment in the evaluation sample is $q = 0.62$, compatible with the observed probability of treatment in a small window around the cutoff, $q1 = 0.60$ but not with complete randomization, $q3 = .05$. In sum, there is no evidence suggesting the presence of manipulation of the application score.

External Validity. Tables 6 and 7 and Tables A2 and A3 in the Appendix show descriptive statistics for the evaluation sample and all voucher recipients, respectively.

There are few differences in baseline characteristics between all voucher recipients and the subset of observations in the evaluation sample in the period of analysis. The main difference is the two to three times as large fraction of families living in counties with high poverty rate in the evaluation sample. Nonetheless, other county characteristics such as density and average rent are similar. Furthermore, average individual poverty rate is 14 percentage points lower than in the broader group of voucher recipients in regular rounds. However, the multi-cutoff design averages treatment effects from assignments with different individual poverty rates, from zero in the assignment with the lowest cutoff (240 points) and thirty percent in the assignment with the highest cutoff (355 points).

The evidence presented in this section suggests that having a multi-cutoff regression discontinuity design introduced heterogeneity in the evaluation sample, reducing the local nature of RDD estimates. That said, this evaluation represents closer the effect of the voucher in families living in poorer counties -yet not necessarily the poorest families.

6 Results

This section presents the results of the evaluation of the rental voucher program using equation 4.1 explained in Section 4:

$$Y_{i,s} = \alpha + \tau Voucher_{i,s} + \gamma_s Assignment_s + \beta Z_{i,s} + \delta S_{i,s} + \epsilon_{i,s}$$

Where τ is the Intention to Treatment Effect (ITT) of holding a rental voucher. For the period before the pandemic, $Y_{i,s}$ includes overcrowding, residential mobility, neighborhood characteristics, savings for ownership and application to homeownership programs. In November 2020, eight month after the COVID-19 outbreak, $Y_{i,s}$ comprises overcrowding, residential mobility, housing and neighborhood characteristics and satisfaction, tenure, rent burden, employment, income, health, and families' response to the economic hardship that came with the pandemic.

Tables 9 to 12 present the results of equation 4.1 for regular and elderly rounds, respectively. Specification 1 includes score components to control for tie-breaking rules $S_{i,s}$, and Specification 2 uses $S_{i,s}$ and baseline covariates $Z_{i,s}$ used in Section 5.2.1.

Including covariates in $Z_{i,s}$ had little impact on the coefficients, therefore, unless otherwise specified, this section focuses on the estimates of τ from Specification 2 (Column 8). I use large-sample based inference (F-test) and Fisherian randomization inference, robust in small samples. Otherwise noted, p-values in parenthesis are estimated using Fisherian randomization inference (Column 11).I use the package randcmd in Stata to estimate Randomization-t exact test developed in (Young, 2019)). I use 1000 iterations, rerandomizing the data by assignment and following the assumed stratified experimental design. The bottom panel shows the Westfall-Young multiple-testing test of overall treatment irrelevance.

6.1 Treatment Effects in December 2019 - Before the Coronavirus Pandemic

Results before the pandemic, in December 2019, are divided two groups: i) housing, residential mobility and neighborhood characteristics, and ii) application to homeownership policies and private savings for ownership.

6.1.1 Housing, residential mobility and neighborhood characteristics

Panel A in Tables 9 and 10 shows the effect of the voucher on housing-related outcomes. Holding a voucher reduced overcrowding in 4.1 percentage points (pp) in regular rounds and 2 pp in elderly rounds.[49] Separately, the effects on family size and number of bedrooms suggest that the reduction in elderly rounds comes from voucher holders living in smaller families and having more available bedrooms, while the effect in regular rounds (or younger families) is associated to an increase in the number of bedrooms only.

Turning to the effects of the rental voucher in residential mobility (Panel B), I observe larger differences across round types. Holding a voucher did not increase mobility among families in regular rounds yet it did change the location chosen by movers: voucher holders moved longer distances (32 km or 0.64 standard deviations) and were 7.6 pp more likely to move to another county. In elderly rounds, on the other hand, the treatment largely increased mobility (24 pp) but there was no significant effect on distance.

Looking at the counterfactual (Column 2) these effects seem to be large. On the one hand, in December 2019, 55 percent and 68 percent of families in the control group in regular and elderly rounds did not move since application. On the other hand, about thirty percent of movers in the control group in both round types moved one kilometer or less away from their initial location.

Finally, Panel C shows the effects of the voucher on neighborhood characteristics. The rental policy did not significantly improve neighborhood characteristics in regular or elderly rounds. If anything, the few marginally significant coefficients suggests that families in regular rounds moved to areas with a larger fraction of low income schools[50] and elderly moved closer to municipalities and health care centers.

Overall, the evaluation of the program before the pandemic shows similar results to the previous literature focused on the Section 8 program: holding a voucher improved housing conditions but it did little to provide better neighborhoods for low income families.

6.2 Homeownership

Panel D in Table 9 shows that holding a voucher did not affect application to homeownership programs in regular rounds; coefficients are small and non significant. Furthermore, there was no effect on the extensive or intensive margins of savings i.e. both treated and

[49] Overcrowding is defined as more than two family members sleeping together in one bedroom. This is the definition applied by MINVU to evaluate applicants to the program. In Chile, however, overcrowding is considered mild if there are between 2.5 and 3.5 individuals per bedroom, high if it exceeds 3.5 and critical if 5 or more people share the same bedroom (Casen 2017).

[50] OLS inference in Column 10 suggests that families in regular rounds also moved farther away from schools and pre-schools.

controls kept their savings account opened and kept enough savings to apply at least to the fully funded homeownership program (DS 49) (Column 2).

For elderly rounds, on the other hand, the results in Table 10 show that the rental voucher increased application to homeownership programs in 4.5 percentage points, mostly through an increase of applications to the fully funded program (DS 49).

It it worth noting that rents are almost fully subsidized for voucher recipients in elderly rounds. Moreover, while the previous section suggests that the rental policy does not push elder households far away, social housing is normally located at the periphery (Navarrete & Navarrete, 2016). Some combination of preferences for homeownership and better access to information through the interaction with the PHA might explain these results. Future research could explore the effect of automatic renovation of the rental subsidy on homeownership application in that it might have changed the cost-benefit analysis of different housing policy options.

6.3 Treatment Effects in November 2020 - During the Coronavirus Pandemic

This section presents the results of equation 4.1 using the sample of applicants in W_0 who responded the follow-up survey. Respectively, the response rate was 59.5 percent (706) and 27 percent (150) in regular and elderly rounds in the evaluation sample.

In Section A2 in the Appendix I analyze attrition and show that there was no selective attrition in the response of the follow-up survey. In other words, local randomization assumptions are still valid in this subset of observations inside W_0, therefore, coefficients in this section are causal estimates of holding a voucher during the pandemic. Furthermore, Table A8 shows that the effects in December 2019 for the subset of voucher recipients in regular rounds are similar to results in the previous section.[51]

While the response rate in elderly rounds was high for an online survey, and there is no evidence of selective attrition, results for this round type should be interpreted with some caution.[52] Therefore, the analysis in this section is focused on regular rounds and highlights some observed differences between regular and elderly rounds.

Results in this section are grouped in residential mobility, housing and household char-

[51] All coefficients are similar but some are not statistically significant in this smaller sample.

[52] Table A9 shows that the effects in overcrowding and application to homeownership programs in December 2019 are larger in this smaller sample. The effects in residential mobility and neighborhood characteristics are similar except for distance, which is significant and similar to the observed effect in regular rounds.

acteristics; neighborhood characteristics, housing and neighborhood satisfaction; health; and employment, income and household response during the Covid-19 crisis.

6.4 Residential mobility, Housing and Household characteristics

Regular Rounds

Column 2 of Panel A in Table 11 shows that eight months into the pandemic, 86 percent of the control group was renting and the voucher had no significant effect on tenure. [53] The policy affected other important housing margins, nonetheless.

First, in a highly informal rental market[54] the voucher increased the probability of having a lease in 12 percentage points and decreased rent burden in 10 pp - the voucher reduced monthly out of the pocket rent payments in about US$50, while it did not affect the average rent amount paid by treated or controls (US$260).

Results in crowding and residential mobility were similar to December 2019: overcrowding decreased in 4 pp and holding a voucher did not affect residential mobility. The magnitude of the treatment effects on distance were also similar yet not statistically significant in this smaller sample.

Finally, compared to the control group, treated were about 9 pp. more likely to have an independent room for the kitchen, a heating system and/or a computer at home, suggesting that holding a voucher increased housing-related consumption. Other housing expenses like cable TV, smart phone or Wifi were not affected by the voucher

Elderly Rounds

Table 12 shows some important differences between regular and elderly rounds. First, the reduction in the amount of rent paid out of pocket in elderly rounds was US$106, more than twice as much as in regular rounds. Moreover, rent burden was 30 pp lower among the treated, three times the effect found in regular rounds. These results may be a driven by some combination of the larger size of the subsidy assigned to elder households and differences between applicants, including location.

[53] Importantly, survey data at baseline shows that eighty percent in both the treatment and control groups were tenants when they applied to the program. Homeowners are not eligible to the rental subsidy.

[54] Baseline survey shows that 35 percent of those who were tenants at baseline did not have a rental lease.

6.5 Neighborhood characteristics and housing and neighborhood satisfaction

Regular Rounds

The survey included several questions to measure access to amenities in the immediate neighborhood (4 blocks radius). Panel F in table 11 shows similar results to those in December 2019. The voucher had no effect on neighborhood characteristics. Specifically, voucher holders did not have better access to childcare, schools, transportation, parks, primary care centers, or were closer to family members, friends or their jobs than the control group.

In addition, eight months into the pandemic, voucher holders in regular rounds were less likely to be willing to ask their neighbors for childcare, yet safety perceptions and housing and neighborhood satisfaction did not vary between treated and controls. More research is needed to understand whether the reduction in network support for childcare reflects isolation generated by the farther away moves by the treatment group in regular rounds.

Elderly Rounds

Similar to the effects in regular rounds, eight month into the pandemic treated and controls in elderly rounds had similar access to primary care, parks, transportation, family and friends. However, Table 12 suggests that the voucher reduced the exposure to prostitution, destroyed property and graffiti of elderly households. It is possible that improvements in street safety could be driven by differences in lock-down's compliance in neighborhoods in which treated and controls lived, rather than changes in long-term neighborhood characteristics. Indeed, Panel F shows non-positive effects on perceived safety.

Finally, elderly voucher holders were 23 pp more likely to be willing to ask for economic help from their neighbors in case of need; this coefficient in regular rounds is negative and not statistically significant.

6.6 Employment, income and household response during the Covid-19 crisis

Regular Rounds

Column 2 in Panel C and D in Table 11 gives a sense of the size of the unexpected economic shock for young low income families in Chile. Roughly 77 percent of voucher recipients in the control group had partial or total income loss during the first eight months after the outbreak. Also, unemployment associated to the pandemic was 16 percent - mostly suspended contracts of dependent workers and independent workers who could not go out to work during strict quarantines.

Fifteen different strategies to cope with the pandemic were asked in the survey. Ninety four percent had to turned into new income sources, cut spending or increasing debt to adapt to the new economic circumstances. The most used strategies were included turning to some government emergency relief program, using family savings, reducing food expenditure and cutting utility bills (Column 8).

Regarding the effects of the rental voucher, the policy impacted how families were coping with the consequences of the large unexpected economic shock that came with the COVID-19 pandemic. Voucher holders were 9 pp. and 5 pp. less likely to engage in new activities to complement family income and lend or give money to other family members, respectively.

Furthermore, the voucher had a positive effect in housing stability during the pandemic. Voucher holders were 4 pp less likely to move out because of the pandemic (p-value 0.100) and 13 pp less likely to miss rent payments (p-value 0.003).[55] However, the voucher did not have a statistically significant effect on shelter deprivation[56] in the short run in regular rounds: eighteen percent in the control group was shelter deprived in November 2020 and the non statistically significant estimated effect of the voucher is -2 pp (p-value 0.523).[57] This could change as the national eviction moratorium -in place since May 2020- is lifted in December 2021. Moreover, these results pool together families who did and did not leased-up with the voucher (ITT).

In addition, the rental policy reduced perceived debt overload in 18.6 pp. More specifically, the fraction of young families declaring being overwhelmed about their debts decreased from 68 to 50 percent (p-value 0.074).

[55]Positive effects of rental vouchers in housing stability have been previously documented in the US in periods of no economic crisis (Mills et al., 2006).

[56]In poor countries, this includes homelessness, renting a room, doubling up and, importantly, living in slums. Since the pandemic started, the number of families living in slums has increased in 74 percent in Chile (Techo, 2021).

[57]According to survey data, baseline shelter deprivation was 22 percent.

Interestingly, opposite to previous literature showing negative effects of rental policies on employment in the US (Jacob & Ludwig, 2012), Panel C shows that the voucher did not impact income and employment. Whether these results differ due to the pandemic, lower costs of application for formal workers in the Chilean rental subsidy[58], the smaller size of the Chilean voucher or other differences between US and Chilean subsidy, are interesting research questions for future work.

Elderly Rounds

Panel D in Table 12 shows that in addition to reducing food expenditure, cutting utility bills and using government relief programs, the more vulnerable and older population in elderly rounds turned to reducing health expenses more frequently than to using family savings. Further, among elder households the voucher made 16.2 pp. more likely to ask for formal credits, for which having a rental lease may be useful.

As in regular rounds, rental vouchers seems to contribute to reducing housing instability during the pandemic in elderly rounds. The voucher cause an important reduction on shelter deprivation of roughly 20 pp (p-value 0.016). Coefficients for the effects of the voucher on moving out and missing rent to cope with the pandemic are similar to regular rounds, although not statistically significant using randomization inference.

This evaluation suggests that rental vouchers in middle income countries may provide housing stability to low-income families after unexpected large income shocks by reducing unwanted mobility and shelter deprivation. This result points to a previously underappreciated role of housing subsidies in helping poor households coping with negative shocks.

Differences between regular and elderly rounds throughout this section suggests that the correlation between the size of the subsidy and the size of the effects might not be constant and may depend on voucher holder and location characteristics. Further research is needed to asses the elasticity of these effects to different voucher generosity using similar populations.

Next, I present the effect of the rental policy on health during the pandemic. This is the last set of outcomes in this evaluation.

[58] As mentioned in section **??** the design of the Chilean rental subsidy makes it easier for formal workers to apply. Baseline survey shows that 85 percent of all applicants were employees at the time of application.

6.7 Health

Regular Rounds

Panel E presents health-related outcomes. Treated and controls were not different in overall health self evaluation, or exposure to the virus (Column 2). In terms of mental health, 78 and 65 percent of the sample declared to feel depressed and worried, respectively. To distinguish from serious diagnoses, I use the Patient Health Questionnaire-4 (PHQ4) test, a four-questions screening for anxiety and depression. Results show that 17, 43 and 33 percent are considered normal, anxious and depressed using this test, respectively.

While marginally significant, the voucher reduced the fraction of cases who felt depressed in 6.5 pp (p-value 0.109), while increased in 8.2 pp. the number of cases who were anxious according to the PHQ4 test (p-value 0.117). These results open interesting future research questions about the link between rental vouchers and mental health during a crisis. There are no important differences between elderly and regular rounds in health outcomes.

7 Discussion

This research evaluates a rental voucher program on housing and neighborhood quality of low income families in a middle income country, Chile, and answers how holding a rental voucher affects how low income families cope with a large unexpected income shock.

To answer these questions I provided evidence on the Chilean rental subsidy, the first rental voucher program implemented in Latin America, which design was advised by HUD and based on the Section 8 program in the US. The similarities to the US program and the large number of countries in Latin America that are currently moving away from homeownership policies towards rental assistance programs make the Chilean rental voucher an interesting case study.

I analyzed two different voucher schemes in the program: a modest voucher for younger families and a large voucher for elderly households. I employ a local randomization regression discontinuity approach (Cattaneo & Frandsen, 2015) to estimate the causal effects of the Chilean rental policy on multiple outcomes that I get from administrative data in December 2019 and survey data collected between September and November 2020.

Before the pandemic,the results are similar to those of the existing literature for the US: holding a voucher seemed to improve housing conditions by reducing overcrowding yet it did little to provide better neighborhoods for low income families. In the eight months following the COVID-19 outbreak of March 2020, results show that the rental policy decreased unwanted mobility, rent burden and, among the elderly, reduced shelter deprivation. Also, the policy affected how families coped with the large unexpected income shock. More specifically, voucher holders were less likely to engage in new activities to complement their incomes or to miss their rent payments.

These findings point to a previously underappreciated role of housing subsidies in helping poor households cope with negative income shocks, which could be particularly relevant for developing countries. In particular, under high levels of informality and social inequalities not only make unexpected income variations more likely to occur, and their potential negative effects bigger, but also undermine the effectiveness of government response during a crisis.

For instance, Chile announced 100,000 three-months emergency fixed rental vouchers of US$330 in July 2020. The benefit targeted a broader eligible population, including current and past voucher recipients, and premised to subsidize 40 percent of rents up to US$800. After extending the application period twice due to the low number of applicants, MINVU assigned 43,128 vouchers and argued that the lack of information[59] and high perceived costs of application explained this result.[60] Moreover, it exposed the lack of digital infrastructure of the government to manage a large online application process and respond to high demands for information that followed the announcement of the emergency rental subsidy.

The evidence provided here suggests that in developing countries like Chile, long-term rental voucher programs complemented with cash transfers might be more appropriate than the use of emergency short-term rental policies in periods of national economic dis-

[59]Although it had what it was likely the largest advertisement campaign ever made in the country for a social benefit.

[60]In particular, there was confusion about eligibility criteria, rules about where and how to apply, and requirements. For instance, they needed copy of their current rental lease, otherwise, families could send a letter from their landlords acknowledging the rental agreement. Furthermore, PHAs were closed across the country and Wifi is not broadly available across the country.

tress.[61] Furthermore, this research suggest that rental subsidies may have an important role in the historic struggle in Latin American countries to eradicate informal settlements, moreover, to avoid large setbacks in this fight under periods of economic crisis.[62]

To conclude, interesting and useful future lines of work to improve housing policy in developing countries, would be the study of the effects of rental vouchers on homelessness and informal settlements and the comparative study of the effects of emergency versus long-term rental voucher programs in periods of crisis.

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[61]See Ellen et al. (2020) for a discussion of housing policy response during the Covid-19 pandemic in the US.

[62]Since the beginning of the pandemic, the Chilean government has reported high concern about the observed increased in the number of families living in slums.

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Tables and Figures

Table 1: Program Descriptive Statistics

	Applicants (1)	Voucher Recipients (2)	Ever Lease-up May-20 (3)	Lease-up Rate May-20 (4)	Active Leases May-20 (5)
Panel A. Regular Rounds					
1-2014 Regular	5023	5004	1994	40%	85
2-2014 Regular	2045	2045	906	44%	180
2015 Regular	3525	3001	1391	46%	624
2016 Regular	11892	10576	4676	44%	2858
2017 Regular	13634	8785	3809	43%	2809
1-2018 Regular	8350	3002	1345	45%	1122
2-2018 Regular	9175	4238	1816	43%	1619
2019 Regular	10584	7536	2775	37%	2694
Total Regular Rounds	64228	44187	18712	42%	11991
Panel B. Elderly Rounds					
2016 Elderly (Pilot)	630	630	326	52%	247
2017 Elderly	6292	1871	945	51%	747
1-2018 Elderly	5858	2068	1110	54%	974
2-2018 Elderly	4526	939	440	47%	394
2019 Elderly	7118	1049	471	45%	453
Total Elderly Rounds	24424	6557	3292	50%	2815
Total Program	88652	50744	22004	43%	14806

Note: This table presents descriptive statistics of each round of the program between 2014 and 2019. Columns 1 and 2 show the total number of applicants and number of vouchers assigned in each round. Columns 3-5 use data on all leases that voucher recipients activated between April 2014 and May 2020. Column 3 presents the total number of voucher recipients that ever used their vouchers, even if they were not using it in May 2020. Column 4 presents the lease up rate i.e. column 3 divided by column 2. Column 5 shows the number of those who leased up in the program that had an active lease in May 2020.

Table 2: Assignments in Regular Rounds

Assignment Date	N (1)	Min Xi (2)	Max Xi (3)	Vouchers (4)	Cutoff (5)
26apr2017	2,090	85	665	956	300
17may2017	2,214	85	720	996	275
21jun2017	2,373	85	720	1,000	275
24jul2017	2,343	85	705	999	240
24aug2017	2,495	85	685	1,000	240
27sep2017	2,714	85	650	999	235
19oct2017	3,085	85	695	1,933	200
13dec2017	5,751	85	790	900	395
11apr2018	2,591	85	695	1,500	285
01jun2018	6,848	85	755	1,500	370
21sep2018	3,399	125	700	1,000	355
26oct2018	4,162	125	800	1,000	375
20nov2018	7,174	125	800	2,157	350
28dec2018	5,017	125	345	80	345
03jun2019	4,657	85	700	1,985	331
19aug2019	5,076	85	680	1,990	297
10oct2019	6,607	85	740	3,559	273
Total	68,596	85	800	23,554	317

Note: This table shows descriptive statistics for each assignment date that occurred between April 2017 and October 2019 in Regular Rounds. Column 1 shows the total number of participants that were sorted and assigned to treatment and control. Column 2 and 3 present the maximum and minimum score among all participants. Column 4 indicates the number of available vouchers and column 5 the value of the cutoff. After regional assignments were implemented in 2019, sixteen different assignments occurred at each assignment date. In June, August and October 2019 columns 1 to 4 aggregate all regional assignments and column 5 shows the average cutoff.

Table 3: Assignments in Elderly Rounds

Assignment Date	N (1)	Min Xi (2)	Max Xi (3)	Vouchers (4)	Cutoff (5)
04sep2017	6,280	135	730	1,859	380
11apr2018	2,063	175	645	1,000	380
25jun2018	3,789	175	860	999	420
19oct2018	8,084	145	710	997	420
05jul2019	7,098	105	740	1,033	394
Total	27,314	105	860	5,888	401

Note: This table replicates the analysis in Table 2 in elderly rounds. See Table 2 for details.

Table 4: Assignments in Regular Rounds for Window Selection

	Sample in Window Selection							Evaluation Sample					
Assignment Date	N (1)	Min Xi (2)	Max Xi (3)	Vouchers (4)	Cutoff (5)	P(T=1) (6)	P(T=1,w15) (7)	N (8)	Min Xi (9)	Max Xi (10)	Vouchers (11)	Cutoff (12)	P(T=1) (13)
26apr2017	1,232	85	665	956	300	0.776	1.000	0			0		
17may2017	1,427	85	720	996	275	0.698	1.000	0			0		
24jul2017	1,700	85	705	999	240	0.588	1.000	0			0		
24aug2017	1,854	85	685	1,000	240	0.539	0.774	24	225	240	13	240	0.542
27sep2017	1,981	85	650	999	235	0.504	1.000	0			0		
19oct2017	3,084	85	695	1,933	200	0.627	0.243	0			0		
11apr2018	2,591	85	695	1,500	285	0.579	0.403	138	285	285	75	285	0.543
01jun2018	6,847	85	755	1,500	370	0.219	0.318	0			0		
21sep2018	2,845	125	700	1,000	355	0.351	0.720	84	345	355	40	355	0.476
20nov2018	7,094	125	800	2,157	350	0.304	0.445	0			0		
28dec2018	5,017	125	345	80	345	0.016	0.097	717	330	345	80	345	0.112
10oct2019	1,649	85	715	774	282	0.469	0.454	393	270	300	169	283	0.430
Total	37,321	85	800	13,894	312	0.372	0.488	1,356	195	355	377	320	0.278

Note: This table shows descriptive statistics for each assignment date (not application dates) before and after the window selection procedure is applied. Columns 1 to 7 present the restricted sample considered in window selection according to section 5.1. Columns 8 to 13 show the selected sample of windows in which Local Randomization assumptions held or evaluation sample. Column 6, 7 and 13 describe the density of the running variable around the cutoff. Column 6 presents the probability of treatment (total number of participants (column 1) divided by the total number of available vouchers (column 4)). Column 13 presents the same probability but using observed quantities in the evaluation sample. Column 7 presents the probability of treatment in the window $W = [-15, 15]$.

Table 5: Assignments in Elderly Rounds for Window Selection

	Sample used in Window Selection							Evaluation Sample					
Assignment Date	N (1)	Min Xi (2)	Max Xi (3)	Vouchers (4)	Cutoff (5)	P(T=1) (6)	P(T=1,w10) (7)	N (8)	Min Xi (9)	Max Xi (10)	Vouchers (11)	Cutoff (12)	P(T=1) (13)
04sep2017	6,280	135	730	1,859	380	0.296	0.679	1,176	380	390	853	380	0.725
11apr2018	2,061	175	645	1,000	380	0.485	0.350	388	380	390	148	380	0.381
19oct2018	4,522	145	710	935	420	0.207	0.765	0			0		
05jul2019	3,226	105	740	435	392	0.135	0.374	213	380	400	92	388	0.432
Total	16,089	105	740	4,229	394	0.263	0.604	1,777	380	400	1,093	381	0.615

Note: This table replicates the analysis in Table 4 using data for elderly rounds. See Table 4 for further details.

Table 6: Balance in Baseline Characteristics in Regular Rounds

	Summary Statistics						Balance Test			
	Pooled	Control		Treated						
	Mean	Mean	SD	Mean	SD	N	F-test (p)	Rand-t (p)	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Interaction Terms (H0)										
Tenant in baseline	0.74	0.73	0.44	0.75	0.43	1,356	0.720	0.636	0.744	0.584
Saving balance on application day (US)	637.73	645.39	606.80	617.83	613.13	1,356	0.232	0.119	0.301	0.118
Family income (US)	528.15	526.71	190.68	531.89	192.65	1,356	0.715	0.276	0.304	0.281
Poor (poverty line adjusted by family size)	0.19	0.18	0.39	0.22	0.41	1,356	0.724	0.568	0.916	0.586
Online application	0.38	0.39	0.49	0.37	0.48	1,356	0.539	0.555	0.501	0.572
Baseline application to ownership programs	0.16	0.16	0.36	0.16	0.37	1,356	0.636	0.706	0.665	0.698
KM to closest PHA	18.22	17.26	22.69	20.75	26.75	1,233	0.615	0.873	0.689	0.851
High density county	0.40	0.41	0.49	0.37	0.48	1,356	0.913	0.614	0.819	0.602
Age at application	31.09	31.49	7.06	30.04	6.82	1,356	0.000***	0.528	0.000***	0.524
Preferences to stay in the same neighborhood	0.56	0.56	0.50	0.56	0.50	838	0.380	0.339	0.526	0.330
Satisfaction with housing unit	0.65	0.65	0.48	0.64	0.48	899	0.463	0.613	0.456	0.605
Applied to save for ownership	0.27	0.28	0.45	0.24	0.43	819	0.415	0.221	0.362	0.196
Any neighbor in 400m previously applied	0.88	0.90	0.31	0.85	0.36	618	0.963	0.851	0.964	0.854
Answered Baseline Survey	0.74	0.74	0.44	0.72	0.45	1,356	0.232	0.217	0.319	0.210
Panel B: No Interaction Terms (H0')										
Female	0.91	0.91	0.29	0.92	0.27	1,065	0.063*	0.371	0.248	0.957
Spouse/partner	0.13	0.13	0.34	0.11	0.31	1,110	0.496	0.228	0.574	0.430
Rent (US)	224.47	224.59	109.70	224.08	116.50	810	0.895	0.161	0.800	0.409
Rent burden	0.46	0.47	0.26	0.46	0.25	810	0.707	0.225	0.546	0.600
Geocoded location	0.91	0.91	0.28	0.90	0.30	1,287	0.171	0.955	0.270	0.588
County above national poverty rate	0.61	0.58	0.49	0.70	0.46	1,094	0.290	0.293	0.353	0.350
Santiago MSA	0.16	0.17	0.38	0.12	0.32	963	0.226	0.720	0.289	0.801
Assignment FE							Yes	Yes	Yes	Yes
Score components FE							No	Yes	No	Yes
							F-Test	Westfall Young	N	
Joint Significance Test (p-value)							0.211	0.489	1,356	

Note: This table presents summary statistics and balance tests between treatment and control groups in the evaluation sample. Columns 1 to 6 show summary statistics of baseline characteristics. Columns 7 to 10 show balance results from testing the more conservative null hypothesis (H_0) using the fully interacted model in equation 5.1. Columns 11 to 16 show balance test under a more weaker null hypothesis (H_0') using the FE coefficient in equation 5.2. See section **??** for details. Columns 7, 9, 12 and 14 presents inference using large-sample based inference (F-test) and columns 8, 10, 13 and 16 present Fisherian randomization inference p-values (Randomization-t exact test). I use the package randcmd (1000 iterations) to calculate randomization inference p-values in Stata (Young, 2019). The bottom panel presents the F-test of joint significance from regressing the treatment indicator on all baseline covariates (excluding survey variables not available for the elderly) and the Westfall-Young multiple-testing test of overall treatment irrelevance. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table 7: Balance in Baseline Characteristics in Elderly Rounds

	Summary Statistics						Balance Test			
	Pooled	Control		Treated						
	Mean	Mean	SD	Mean	SD	N	F-test (p)	Rand-t (p)	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Interaction Terms (H0)										
Family income (US)	239.44	235.37	97.23	241.99	106.30	1,777	0.667	0.744	0.643	0.903
Poor (poverty line adjusted by family size)	0.55	0.58	0.49	0.54	0.50	1,777	0.424	0.437	0.312	0.234
Baseline application to ownership programs	0.05	0.05	0.22	0.05	0.22	1,777	0.172	0.890	0.238	0.987
KM to closest PHA	12.61	12.69	18.06	12.56	19.12	1,681	0.381	0.171	0.421	0.184
High density county	0.51	0.52	0.50	0.51	0.50	1,777	0.855	0.538	0.853	0.409
Female	0.61	0.61	0.49	0.61	0.49	1,777	0.617	0.704	0.686	0.530
Spouse/partner	0.40	0.39	0.49	0.41	0.49	1,777	0.914	0.775	0.828	0.695
Age at application	75.62	74.66	6.30	76.22	6.85	1,777	0.130	0.939	0.087*	0.769
Panel B: No Interaction Terms (H0')										
Any neighbor in 400m previously applied	0.71	0.73	0.44	0.69	0.46	1,285	0.993	0.368	0.979	0.296
Rent (US)	223.41	220.73	97.21	228.46	176.06	173	0.252	0.292	0.312	0.337
Rent burden	1.04	1.01	0.53	1.09	0.94	173	0.360	0.753	0.334	0.715
Tenant in baseline	0.55	0.58	0.49	0.53	0.50	1,654	0.605	0.508	0.558	0.518
Geocoded location	0.95	0.95	0.21	0.94	0.24	1,687	0.558	0.290	0.547	0.268
County above national poverty rate	0.35	0.35	0.48	0.34	0.47	1,777	0.496	0.695	0.494	0.704
Santiago MSA	0.29	0.25	0.44	0.31	0.46	1,564	0.073	0.873	0.071	0.945
Assignment FE							Yes	Yes	Yes	Yes
Score components FE							No	Yes	No	Yes
							F-Test	Westfall Young	N	
Joint Significance Test (p-value)							0.834	0.731	1,777	

Note: This table replicates Table 6 using data from elderly rounds. See Table 6 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01..

Table 8: Density Test

Assumed q	qi (1)	N (2)	Obs T (3)	Exp T (4)	Obs q (5)	p-value (6)
Regular (All)						
q1	0.488	1,356	377	661	0.278	0.000
q2	0.372	1,356	377	505	0.278	0.000
q3	0.500	1,356	377	678	0.278	0.000
Regular (No December 2018 Assignment)						
q1	0.551	639	297	352	0.465	0.000
q2	0.428	639	297	273	0.465	0.060
q3	0.500	639	297	320	0.465	0.082
Elderly						
q1	0.604	1,777	1,093	1,073	0.615	0.344
q2	0.263	1,777	1,093	467	0.615	0.000
q3	0.500	1,777	1,093	888	0.615	0.000

Note: This table presents binomial tests to evaluate the presence of manipulation in the running variable in the evaluation sample. Three probability of success q are used: $q1$ is the observed probability of assignment in a small window around the cutoff (column 7), $q2$ is the probability of treatment in the overall sample (column 6) and $q3$ tests for complete randomization in the evaluation sample ($q3 = 50\%$). The results are presented for the entire evaluation sample and excluding the assignment in December 2018, as explain in section 5.1.

Table 9: Results Regular Rounds Before the Covid-19 Pandemic: December 2019

					Specification 1				Specification 2		
		Control		Treatment	Treatment	OLS	Rand-t	Treatment	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	Effect (SD)	p-value	p-value	Effect	Effect (SD)	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing Conditions											
Household size Dec 2019	1,356	2.896	1.285	-0.084	-0.065	0.297	0.336	-0.094	-0.073	0.224	0.179
Number of bedrooms	1,349	1.773	0.809	0.123	0.152	0.022**	0.007***	0.124	0.154	0.018**	0.007***
Number of people per bedroom	1,348	1.810	0.797	-0.185	-0.232	0.000***	0.000***	-0.190	-0.238	0.000***	0.000***
Overcrowing indicator	1,348	0.112	0.316	-0.042	-0.132	0.023**	0.010**	-0.043	-0.135	0.019**	0.009***
B. Residential Mobility											
Stayed in same unit	1,226	0.549	0.498	-0.055	-0.111	0.108	0.375	-0.049	-0.098	0.152	0.548
Distance (km)	1,226	6.510	34.271	15.474	0.452	0.057*	0.061*	15.562	0.454	0.055*	0.063*
Distance (km) (Movers)	562	14.430	49.935	29.408	0.589	0.072*	0.066*	29.220	0.585	0.072*	0.061*
Stayed 1km or less from application location	562	0.340	0.474	-0.041	-0.086	0.397	0.272	-0.040	-0.084	0.407	0.304
Moved to another county	564	0.154	0.362	0.066	0.182	0.088*	0.429	0.077	0.213	0.048**	0.168
C. Neighborhood Characteristics											
Distance to closest municipality	1,226	3.887	6.138	0.358	0.058	0.352	0.808	0.284	0.046	0.451	0.725
Distance to closest school (km)	1,226	1.143	3.370	0.412	0.122	0.087*	0.360	0.358	0.106	0.136	0.690
Distance to closest Pre-Shcool (km)	1,226	1.222	3.995	0.486	0.122	0.054*	0.178	0.404	0.101	0.100	0.608
Distance to closest Primary Care (km)	1,148	1.801	4.219	0.355	0.084	0.222	0.765	0.241	0.057	0.397	0.969
Number of Schools in 1Km	1,226	4.893	4.402	-0.152	-0.034	0.642	0.323	-0.050	-0.011	0.876	0.917
Number of Schools in 2Km	1,226	15.366	13.271	-0.922	-0.069	0.275	0.108	-0.410	-0.031	0.579	0.435
Number of Preschool in 1Km	1,226	3.048	2.611	0.041	0.016	0.827	0.664	0.093	0.035	0.608	0.673
Number of Health Care in 2km	1,226	5.060	4.628	-0.231	-0.050	0.402	0.149	-0.068	-0.015	0.786	0.513
Fraction of Public Schools 1Km	992	0.441	0.288	-0.005	-0.016	0.836	0.472	-0.008	-0.027	0.727	0.377
Fraction of Subsidized Schools 1Km	992	0.523	0.279	-0.003	-0.010	0.898	0.537	0.000	0.002	0.985	0.480
Fraction of Private Schools 1Km	992	0.036	0.112	0.008	0.068	0.319	0.950	0.007	0.066	0.347	0.875
Mat. SIMCE, 3 Closest School 2km	1,047	263.865	17.347	-1.204	-0.069	0.369	0.432	-1.209	-0.070	0.369	0.441
Mat. SIMCE, 3 Closest School 2km	1,047	250.165	18.602	-1.942	-0.104	0.181	0.115	-1.866	-0.100	0.200	0.149
Fraction of Low Income Schools 1km	992	0.599	0.338	0.035	0.102	0.175	0.089*	0.031	0.093	0.208	0.108
Fraction of Low Income Schools 2km	1,058	0.580	0.273	0.010	0.037	0.598	0.291	0.008	0.028	0.679	0.333
County poverty rate	1,228	0.111	0.064	0.001	0.018	0.771	0.220	-0.001	-0.014	0.779	0.584
Total crime (County z-score)	1,228	1.153	1.653	-0.096	-0.058	0.396	0.184	-0.016	-0.010	0.845	0.442
D. Homeownership											
Application to Ownership Programs	1,356	0.319	0.466	0.037	0.079	0.218	0.381	0.015	0.033	0.514	0.544
Application to partially funded program (DS1)	1,356	0.230	0.421	0.019	0.044	0.505	0.396	0.003	0.006	0.905	0.570
Application to fully funded program (DS49)	1,356	0.131	0.337	0.025	0.073	0.220	0.935	0.015	0.046	0.440	0.818
Active ownership savings account	1,356	0.919	0.273	0.017	0.063	0.330	0.677	0.012	0.043	0.507	0.970
Balance in ownership savings account (US)	1,248	24.555	35.254	0.537	0.015	0.810	0.600	-0.111	-0.003	0.959	0.755
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES
Westfall-Young Multiple Testing (p-value)							0.002***				0.002***

Note: This table presents estimates of equation 4.1 using outcomes measured in December 2019. Columns 2 and 3 show the average of the outcome in the relevant counterfactual group. Treatment effects are presented in outcome units and in standard deviations. Specifications 1 include score components to control for tie-breaking rules and specification 2 include score components and baseline covariates used in section **??**. Standard errors are clustered by applicant given the use of expanded data and Large-sample based inference (OLS p-values) and Fisherian randomization inference (Randomization-t p-values from Young (2019)). The bottom panel shows the Westfall-Young multiple-testing test of overall treatment irrelevance considering all outcomes together. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table 10: Results Elderly Rounds Before the Covid-19 Pandemic: December 2019

				Specification 1				Specification 2			
		Control		Treatment	Treatment	OLS	Rand-t	Treatment	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	Effect (SD)	p-value	p-value	Effect	Effect (SD)	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing Conditions											
Household size Dec 2019	1,777	1.583	1.052	-0.251	-0.238	0.000***	0.000***	-0.250	-0.238	0.000***	0.000***
Number of bedrooms	1,688	1.345	0.720	0.489	0.679	0.000***	0.001***	0.486	0.675	0.000***	0.001***
Number of people per bedroom	1,682	1.241	0.592	-0.373	-0.630	0.000***	0.001***	-0.373	-0.630	0.000***	0.001***
Overcrowing indicator	1,761	0.031	0.173	-0.021	-0.119	0.011**	0.016**	-0.020	-0.118	0.013**	0.013**
B. Residential Mobility											
Stayed in same unit	1,595	0.679	0.467	-0.245	-0.523	0.000***	0.000***	-0.240	-0.514	0.000***	0.000***
Distance (km)	1,595	17.872	127.016	0.759	0.006	0.907	0.574	0.291	0.002	0.964	0.660
Distance (km) (Movers)	752	55.588	219.653	-24.454	-0.111	0.160	0.363	-23.409	-0.107	0.164	0.344
Stayed 1km or less from application location	752	0.286	0.453	-0.013	-0.029	0.747	0.729	-0.020	-0.045	0.619	0.798
Moved to another county	755	0.274	0.447	-0.033	-0.074	0.401	0.195	-0.025	-0.057	0.506	0.203
C. Neighborhood Characteristics											
Distance to closest municipality	1,595	3.918	7.565	-0.823	-0.109	0.031**	0.011**	-0.877	-0.116	0.019**	0.008***
Distance to closest school (km)	1,595	1.181	4.340	-0.215	-0.049	0.202	0.161	-0.220	-0.051	0.185	0.131
Distance to closest Pre-Shcool (km)	1,595	1.223	4.626	-0.258	-0.056	0.170	0.137	-0.267	-0.058	0.149	0.112
Distance to closest Primary Care (km)	1,525	1.693	4.422	-0.278	-0.063	0.142	0.116	-0.305	-0.069	0.103	0.072*
Number of Schools in 1Km	1,595	7.160	5.756	-0.276	-0.048	0.368	0.941	-0.208	-0.036	0.478	0.792
Number of Schools in 2Km	1,595	21.528	15.395	-0.500	-0.032	0.561	0.729	-0.140	-0.009	0.849	0.562
Number of Preschool in 1Km	1,595	3.758	2.952	-0.067	-0.023	0.690	0.766	-0.034	-0.011	0.839	0.621
Number of Health Care in 2km	1,595	6.535	5.597	0.045	0.008	0.886	0.427	0.182	0.033	0.519	0.249
Fraction of Public Schools 1Km	1,401	0.397	0.238	0.001	0.003	0.957	0.783	-0.001	-0.005	0.939	0.799
Fraction of Subsidized Schools 1Km	1,401	0.537	0.240	0.005	0.020	0.740	0.503	0.006	0.025	0.684	0.527
Fraction of Private Schools 1Km	1,401	0.065	0.133	-0.006	-0.042	0.471	0.438	-0.005	-0.036	0.531	0.480
Mat. SIMCE, 3 Closest School 2km	1,445	264.401	17.687	-0.694	-0.039	0.506	0.864	-0.841	-0.048	0.422	0.899
Mat. SIMCE, 3 Closest School 2km	1,446	251.918	18.097	-0.527	-0.029	0.622	0.865	-0.571	-0.032	0.596	0.936
Fraction of Low Income Schools 1km	1,401	0.459	0.332	-0.007	-0.020	0.735	0.799	-0.006	-0.017	0.760	0.746
Fraction of Low Income Schools 2km	1,451	0.444	0.265	-0.004	-0.016	0.787	0.941	-0.004	-0.015	0.783	0.842
County poverty rate	1,598	0.085	0.048	-0.001	-0.021	0.709	0.993	-0.000	-0.005	0.901	0.979
Total crime (County z-score)	1,598	1.725	1.962	-0.015	-0.008	0.896	0.913	0.040	0.020	0.686	0.515
D. Homeownership											
Application to Ownership Programs	1,777	0.123	0.328	0.050	0.152	0.005***	0.005***	0.046	0.141	0.001***	0.003***
Application to partially funded program (DS1)	1,777	0.070	0.256	0.019	0.075	0.173	0.182	0.018	0.069	0.127	0.232
Application to fully funded program (DS49)	1,777	0.069	0.253	0.037	0.148	0.008***	0.006***	0.034	0.136	0.010**	0.004***
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES
Westfall-Young Multiple Testing (p-value)							0.006***				0.006***

Note: This table replicates the analysis in Table 9 for elderly rounds. See Table 9 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table 11: Results Regular Rounds During the Covid-19 Pandemic: November 2020

				Specification 1				Specification 2			
		Control		Treatment	Treatment	OLS	Rand-t	Treatment	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	Effect (SD)	p-value	p-value	Effect	Effect (SD)	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing and Household Characteristics											
Tenancy	559	0.853	0.355	0.007	0.021	0.838	0.988	0.015	0.041	0.683	0.933
Formal Lease	463	0.723	0.448	0.118	0.263	0.010***	0.001***	0.115	0.256	0.013**	0.003***
Total rent (unit)	454	259.991	91.776	-8.844	-0.096	0.401	0.170	-6.437	-0.070	0.522	0.461
Rent paid	437	236.165	103.016	-47.368	-0.460	0.000***	0.000***	-41.786	-0.406	0.001***	0.000***
Rent burden (rent paid)	386	0.491	0.261	-0.109	-0.417	0.000***	0.001***	-0.099	-0.378	0.001***	0.001***
Rent burden (rent amount)	403	0.549	0.259	-0.032	-0.123	0.266	0.126	-0.028	-0.108	0.322	0.369
Shelter deprivation (slum, shared room or other)	651	0.110	0.313	-0.025	-0.080	0.285	0.142	-0.018	-0.059	0.437	0.229
Lives with Parents/Grand parents	477	0.153	0.360	-0.015	-0.041	0.699	0.689	-0.025	-0.069	0.506	0.818
Living with grandchild	477	0.018	0.134	-0.004	-0.027	0.790	0.549	-0.003	-0.019	0.852	0.782
Spouse/Partner	474	0.293	0.456	0.070	0.154	0.174	0.080*	0.036	0.079	0.461	0.243
Child borned since application	450	0.129	0.336	0.031	0.093	0.459	0.201	0.027	0.081	0.529	0.253
Household Size	600	3.305	1.421	-0.178	-0.125	0.161	0.071*	-0.162	-0.114	0.195	0.089*
Number of bedrooms	586	2.215	0.822	0.064	0.078	0.395	0.616	0.066	0.080	0.379	0.468
Number of people per bedroom	584	1.630	0.843	-0.190	-0.226	0.003***	0.005***	-0.182	-0.216	0.004***	0.002***
Overcrowding indicator	586	0.111	0.314	-0.045	-0.143	0.104	0.041**	-0.040	-0.128	0.144	0.064*
Pet Owner	477	0.003	0.055	0.002	0.028	0.858	0.546	0.001	0.023	0.858	0.651
Laundry Room	513	0.422	0.495	-0.003	-0.006	0.955	0.431	0.004	0.008	0.937	0.655
Kitchen Room	566	0.806	0.396	0.094	0.237	0.012**	0.027**	0.093	0.235	0.013**	0.052*
Hot water	585	0.877	0.329	-0.042	-0.129	0.240	0.136	-0.051	-0.154	0.160	0.124
Heat system	585	0.791	0.407	0.104	0.255	0.001***	0.000***	0.095	0.233	0.002***	0.000***
Cable TV	580	0.619	0.486	-0.032	-0.066	0.500	0.984	-0.041	-0.084	0.397	0.898
Wifi	580	0.589	0.493	0.019	0.038	0.700	0.629	0.020	0.041	0.677	0.759
Smart Phone Lease	580	0.684	0.466	0.004	0.010	0.923	0.755	-0.012	-0.025	0.799	0.891
Computer	582	0.504	0.501	0.072	0.143	0.137	0.233	0.082	0.163	0.091*	0.090*
B. Residential Mobility											
Stayed in same unit	511	0.578	0.495	-0.103	-0.207	0.042**	0.036**	-0.065	-0.131	0.199	0.161
Distance (km)	421	9.506	51.382	12.948	0.252	0.253	0.286	10.324	0.201	0.317	0.514
Number of moves from application	513	0.697	1.038	0.048	0.046	0.639	0.507	-0.011	-0.011	0.912	0.990
Less than 6 months current house	645	0.122	0.327	-0.015	-0.046	0.609	0.945	-0.027	-0.082	0.366	0.590
Between 6 months and 1 year current house	645	0.167	0.373	0.075	0.202	0.044**	0.046**	0.065	0.174	0.094*	0.169
Between 1 and 2 years current house	645	0.236	0.425	0.060	0.142	0.155	0.091*	0.052	0.122	0.219	0.118
2 or more years current house	645	0.475	0.500	-0.121	-0.241	0.007***	0.006***	-0.090	-0.180	0.043**	0.029**
Less than 6 months current neighborhood	632	0.090	0.286	-0.003	-0.010	0.911	0.710	-0.016	-0.055	0.543	0.834
Between 6 months and 1 year current neighborhood	632	0.136	0.343	0.047	0.138	0.176	0.242	0.035	0.103	0.326	0.522
Between 1 and 2 years current neighborhood	632	0.194	0.396	0.052	0.132	0.201	0.092*	0.037	0.094	0.358	0.179
2 or more years current neighborhood	632	0.581	0.494	-0.097	-0.196	0.037**	0.031**	-0.057	-0.114	0.220	0.137
C. Employment and Income											
Work	477	0.703	0.458	-0.022	-0.049	0.657	0.514	-0.020	-0.044	0.692	0.614
Covid-19 unemployment	477	0.161	0.368	0.061	0.166	0.147	0.120	0.061	0.165	0.150	0.149
Debt overload	481	0.683	0.466	-0.087	-0.186	0.102	0.074*	-0.073	-0.158	0.179	0.116
No income loss after COVID-19	482	0.232	0.423	0.053	0.125	0.266	0.162	0.055	0.130	0.256	0.190
D. Household Response During in Covid-19 Crisis											
Covid-19 response: moved out	476	0.067	0.251	-0.033	-0.133	0.129	0.146	-0.042	-0.168	0.077*	0.102
Covid-19 response: delayed rent payments	415	0.248	0.433	-0.140	-0.324	0.001***	0.002***	-0.127	-0.294	0.004***	0.003***
Covid-19 response: others moved in	476	0.049	0.216	0.025	0.115	0.415	0.221	0.027	0.123	0.372	0.248
Covid-19 response: reduced food budget	476	0.532	0.500	-0.041	-0.082	0.455	0.341	-0.055	-0.109	0.332	0.311
Covid-19 response: reduced health expenses	476	0.315	0.465	0.033	0.070	0.512	0.530	0.040	0.086	0.427	0.296
Covid-19 response: reduced utilities expenses	476	0.453	0.499	-0.008	-0.016	0.884	0.453	-0.022	-0.045	0.690	0.341
Covid-19 response: delayed monthly billings	476	0.422	0.495	-0.052	-0.106	0.332	0.304	-0.062	-0.126	0.258	0.309
Covid-19 response: informal loan (family/friends)	476	0.361	0.481	-0.044	-0.091	0.405	0.392	-0.055	-0.114	0.306	0.215
Covid-19 response: formal loan or credit	476	0.177	0.383	-0.014	-0.037	0.735	0.763	-0.024	-0.062	0.564	0.584
Covid-19 response: sold expensive goods (vehicle, jewelry, etc.)	476	0.162	0.369	-0.002	-0.006	0.948	0.506	0.004	0.010	0.924	0.601
Covid-19 response: sold or rented real state/land	476	0.006	0.078	0.016	0.205	0.277	0.125	0.016	0.209	0.297	0.217
Covid-19 response: used family savings	476	0.465	0.500	0.023	0.046	0.672	0.321	0.020	0.039	0.720	0.397
Covid-19 response: new activities to generate more income	476	0.367	0.483	-0.081	-0.168	0.114	0.234	-0.087	-0.180	0.100*	0.226
Covid-19 response: gave or lent money to family members	476	0.107	0.310	-0.051	-0.164	0.113	0.326	-0.054	-0.176	0.092*	0.269
Covid-19 response: applied/used government emergency solutions	476	0.498	0.501	-0.009	-0.017	0.873	0.999	-0.009	-0.018	0.872	0.890
Covid-19 response: none	476	0.064	0.246	-0.026	-0.107	0.250	0.406	-0.027	-0.109	0.250	0.288
Covid-19 response: other	476	0.043	0.203	-0.024	-0.117	0.137	0.093*	-0.018	-0.087	0.267	0.169
E. Virus Transmission and Mental Health											
At least one Covid-19 case- Home	455	0.042	0.201	-0.005	-0.024	0.816	0.441	-0.007	-0.034	0.748	0.396
At least one Covid-19 case- Family	455	0.245	0.431	0.005	0.011	0.926	0.746	0.025	0.058	0.612	0.341
At least one Covid-19 case- Friends	455	0.226	0.419	-0.032	-0.077	0.470	0.293	0.000	0.001	0.992	0.992
At least one Covid-19 case- Neighbors	455	0.197	0.398	-0.032	-0.082	0.449	0.229	-0.029	-0.072	0.513	0.392
At least one Covid-19 case- Work	455	0.197	0.398	-0.061	-0.154	0.147	0.111	-0.051	-0.128	0.222	0.342
At least one Covid-19 case- Other acquaintance	455	0.348	0.477	-0.020	-0.042	0.700	0.426	-0.023	-0.049	0.658	0.443
Do not know any COVID-19 case	455	0.284	0.452	-0.038	-0.084	0.443	0.925	-0.065	-0.145	0.190	0.313
Good health	466	0.616	0.487	0.017	0.034	0.756	0.995	-0.010	-0.020	0.854	0.582
Happy	454	0.731	0.444	-0.012	-0.027	0.806	0.940	-0.035	-0.079	0.488	0.632
Feel depressed	459	0.780	0.415	-0.064	-0.155	0.172	0.126	-0.065	-0.157	0.177	0.109
Feel worried	459	0.646	0.479	-0.059	-0.124	0.269	0.227	-0.071	-0.147	0.189	0.135
PHQ4 Test: Normal	459	0.172	0.378	-0.047	-0.124	0.241	0.772	-0.061	-0.162	0.134	0.491
PHQ4 Test: Anxiety	459	0.433	0.496	0.064	0.129	0.240	0.285	0.082	0.165	0.136	0.117
PHQ4 Test: Depression	459	0.328	0.470	0.033	0.070	0.533	0.617	0.031	0.067	0.550	0.529
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES

Table 11: Results Regular Rounds During the Covid-19 Pandemic: November 2020

		Control		Specification 1				Specification 2			
	N	Mean	SD	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
F. Neighborhood Characteristics											
Close to childcare/pre-school (4 blocks)	632	0.586	0.493	0.014	0.029	0.755	0.834	0.016	0.032	0.737	0.730
Close to Schools (4 blocks)	631	0.568	0.496	-0.005	-0.010	0.912	0.706	0.007	0.015	0.877	0.729
Close to subway/bus (4 blocks)	631	0.634	0.482	0.039	0.082	0.387	0.314	0.052	0.108	0.248	0.148
Close to Park (4 blocks)	632	0.602	0.490	0.044	0.090	0.339	0.201	0.055	0.111	0.237	0.159
Close to Health Care (4 blocks)	631	0.451	0.498	0.024	0.049	0.602	0.838	0.045	0.090	0.335	0.317
Less than 15 min commute time to family	424	0.458	0.499	-0.039	-0.079	0.498	0.596	-0.064	-0.128	0.265	0.233
Less than 15 min commute time to friends	378	0.435	0.497	0.038	0.077	0.535	0.419	0.012	0.024	0.842	0.745
Less than 15 min commute time to school	417	0.505	0.501	0.049	0.098	0.399	0.726	0.024	0.048	0.678	0.960
Less than 30 min commute time to work	363	0.673	0.470	-0.030	-0.064	0.610	0.954	-0.042	-0.090	0.473	0.728
Street Alcohol Consumption	464	0.536	0.499	0.082	0.164	0.130	0.397	0.076	0.152	0.164	0.258
Street Drug Consumers	464	0.432	0.496	-0.026	-0.052	0.621	0.305	-0.023	-0.047	0.667	0.492
Street Drug Trafficking	464	0.281	0.450	-0.015	-0.032	0.762	0.344	-0.019	-0.042	0.702	0.374
Destroyed property	464	0.287	0.453	0.015	0.032	0.769	0.851	0.013	0.029	0.796	0.847
Graffiti	464	0.196	0.397	-0.022	-0.055	0.621	0.494	-0.017	-0.044	0.680	0.909
Gang Fights	464	0.189	0.392	0.067	0.170	0.136	0.792	0.071	0.181	0.117	0.493
People Carrying guns	464	0.211	0.409	0.004	0.009	0.928	0.516	0.026	0.064	0.515	0.904
Shooting	464	0.404	0.491	0.049	0.099	0.364	0.719	0.082	0.166	0.117	0.139
Prostitution	464	0.041	0.199	0.036	0.183	0.138	0.898	0.036	0.182	0.121	0.696
Feels safe walking at night	465	0.535	0.500	-0.051	-0.102	0.346	0.728	-0.043	-0.086	0.442	0.723
Feels safe inside the house at night	459	0.752	0.433	0.013	0.030	0.779	0.477	0.014	0.032	0.766	0.457
Victim of violence (physical)	458	0.122	0.328	-0.028	-0.086	0.397	0.218	-0.025	-0.077	0.457	0.367
Victim of robbery	438	0.329	0.471	-0.020	-0.042	0.705	0.343	-0.001	-0.003	0.981	0.703
G. Housing and Neighborhood Satisfaction											
Satisfaction current housing unit	644	0.770	0.421	0.048	0.113	0.183	0.111	0.043	0.103	0.231	0.172
Satisfaction current neighborhood	626	0.801	0.400	-0.038	-0.095	0.294	0.926	-0.045	-0.114	0.202	0.808
Would ask neighbors for childcare	604	0.290	0.454	-0.103	-0.227	0.012**	0.028**	-0.090	-0.198	0.031**	0.116
Has close friends in the neighborhood	606	0.424	0.495	-0.070	-0.142	0.131	0.379	-0.057	-0.115	0.237	0.702
Would ask neighbors for economic help	604	0.238	0.427	-0.050	-0.117	0.174	0.123	-0.047	-0.109	0.205	0.173
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES
Westfall-Young Multiple Testing (p-value)							0.048**				0.047**

Note: This table presents estimates of equation 4.1 using outcomes measured in the follow-up sample implemented in September-November 2020. Columns 2 and 3 show the average of the outcome in the relevant counterfactual group. Treatment effects are presented in outcome units and in standard deviations. Specifications 1 include score components to control for tie-breaking rules and specification 2 include score components and baseline covariates used in section **??**. Standard errors are clustered by applicant given the use of expanded data and Large-sample based inference (OLS p-values) and Fisherian randomization inference (Randomization-t p-values from Young (2019)). The bottom panel shows the Westfall-Young multiple-testing test of overall treatment irrelevance considering all outcomes together. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table 12: Results Elderly Rounds During the Covid-19 Pandemic: November 2020

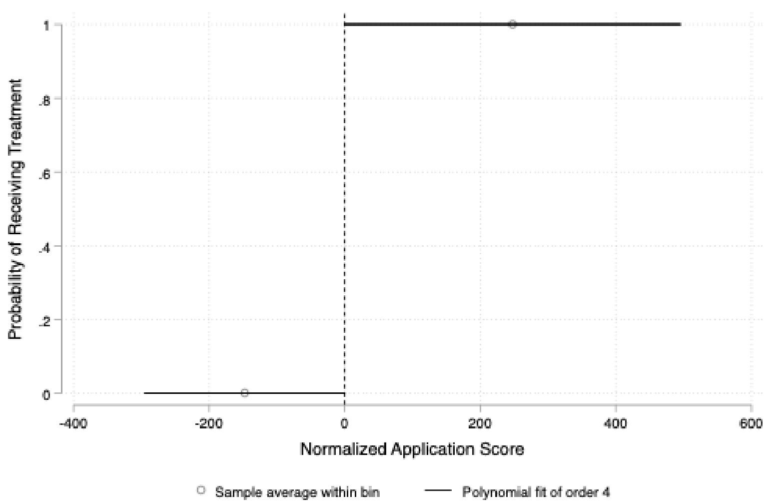
				Specification 1				Specification 2			
		Control		Treatment	Treatment	OLS	Rand-t	Treatment	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	Effect (SD)	p-value	p-value	Effect	Effect (SD)	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing and Household Characteristics											
Tenancy	124	0.826	0.383	0.041	0.107	0.598	0.758	0.039	0.101	0.614	0.758
Formal Lease	88	0.771	0.426	0.157	0.369	0.073*	0.083*	0.194	0.455	0.032**	0.060*
Total rent (unit)	94	251.556	85.050	49.142	0.578	0.032**	0.016**	46.075	0.542	0.049**	0.024**
Rent paid	94	222.323	96.211	-112.204	-1.166	0.000***	0.000***	-106.155	-1.103	0.000***	0.002***
Rent burden (rent paid)	81	0.548	0.262	-0.305	-1.161	0.000***	0.001***	-0.300	-1.144	0.000***	0.001***
Rent burden (rent amount)	80	0.639	0.222	0.107	0.481	0.100	0.044**	0.085	0.384	0.227	0.236
Shelter deprivation (slum, shared room or other)	140	0.208	0.409	-0.098	-0.239	0.126	0.054*	-0.117	-0.287	0.055*	0.016**
Lives with Parents/Grand parents	107	0.077	0.270	0.036	0.135	0.585	0.404	0.061	0.225	0.426	0.298
Living with grandchild	107	0.205	0.409	-0.109	-0.267	0.248	0.396	-0.101	-0.247	0.341	0.420
Spouse/Partner	107	0.282	0.456	-0.001	-0.003	0.990	0.899	-0.080	-0.176	0.338	0.481
Household Size	128	3.000	1.874	-1.046	-0.558	0.002***	0.004***	-1.086	-0.579	0.003***	0.003***
Number of bedrooms	124	2.196	1.293	-0.142	-0.110	0.539	0.603	-0.124	-0.096	0.639	0.628
Number of people per bedroom	122	1.451	0.693	-0.386	-0.558	0.006***	0.004***	-0.419	-0.605	0.004***	0.003***
Overcrowding indicator	126	0.133	0.344	-0.117	-0.339	0.105	0.128	-0.135	-0.392	0.068*	0.064*
Pet Owner	109	0.300	0.464	0.123	0.265	0.188	0.390	0.098	0.212	0.313	0.432
Laundry Room	103	0.342	0.481	0.024	0.049	0.842	0.479	-0.036	-0.076	0.766	0.845
Kitchen Room	120	0.814	0.394	0.062	0.157	0.323	0.365	0.030	0.077	0.682	0.750
Hot water	128	0.826	0.383	0.121	0.315	0.051*	0.018**	0.091	0.238	0.167	0.090*
Heat system	126	0.778	0.420	-0.003	-0.007	0.973	0.692	0.012	0.029	0.889	0.993
Cable TV	127	0.543	0.504	-0.028	-0.056	0.785	0.919	-0.056	-0.111	0.560	0.641
Wifi	123	0.419	0.499	-0.040	-0.080	0.705	0.764	0.000	0.001	0.997	0.976
Smart Phone Lease	127	0.511	0.506	-0.052	-0.102	0.605	0.663	-0.123	-0.244	0.224	0.303
Computer	122	0.372	0.489	-0.032	-0.064	0.763	0.936	0.029	0.060	0.791	0.680
B. Residential Mobility											
Stayed in same unit	112	0.643	0.485	-0.226	-0.465	0.045**	0.027**	-0.236	-0.487	0.048**	0.055*
Distance (km)	97	65.767	378.527	-66.793	-0.176	0.469	0.762	-69.042	-0.182	0.491	0.712
Number of moves from application	112	0.810	1.383	-0.023	-0.017	0.935	0.947	-0.073	-0.053	0.806	0.742
Less than 6 months current house	140	0.057	0.233	-0.005	-0.022	0.924	0.911	-0.004	-0.017	0.938	0.952
Between 6 months and 1 year current house	140	0.094	0.295	0.039	0.131	0.556	0.371	0.031	0.106	0.634	0.412
Between 1 and 2 years current house	140	0.057	0.233	0.220	0.942	0.000***	0.001***	0.240	1.029	0.000***	0.001***
2 or more years current house	140	0.792	0.409	-0.253	-0.619	0.005***	0.003***	-0.267	-0.653	0.002***	0.001***
Less than 6 months current neighborhood	136	0.038	0.194	-0.004	-0.020	0.932	0.902	-0.000	-0.002	0.995	0.910
Between 6 months and 1 year current neighborhood	136	0.077	0.269	-0.003	-0.011	0.954	0.704	0.000	0.001	0.995	0.731
Between 1 and 2 years current neighborhood	136	0.038	0.194	0.184	0.947	0.002***	0.002***	0.184	0.947	0.004***	0.003***
2 or more years current neighborhood	136	0.846	0.364	-0.177	-0.486	0.032**	0.025**	-0.184	-0.505	0.025**	0.047**
C. Employment and Income											
Work	15	0.600	0.548	0.382	0.698	0.330	0.382	1.046	1.910	0.023**	0.097*
Covid-19 unemployment	15	0.400	0.548	-0.485	-0.886	0.117	0.060*	-0.811	-1.480	0.000***	0.035**
Debt overload	109	0.725	0.452	-0.084	-0.186	0.431	0.257	-0.078	-0.172	0.494	0.289
No income loss after COVID-19	102	0.421	0.500	-0.020	-0.040	0.855	0.540	-0.002	-0.003	0.989	0.725
D. Household Response During in Covid-19 Crisis											
Covid-19 response: moved out	104	0.077	0.270	-0.040	-0.148	0.507	0.537	-0.038	-0.142	0.475	0.385
Covid-19 response: delayed rent payments	90	0.161	0.374	-0.128	-0.343	0.163	0.252	-0.159	-0.426	0.080*	0.215
Covid-19 response: others moved in	105	0.179	0.389	-0.096	-0.247	0.246	0.345	-0.079	-0.202	0.394	0.369
Covid-19 response: reduced food budget	105	0.450	0.504	0.095	0.188	0.376	0.640	0.128	0.255	0.238	0.421
Covid-19 response: reduced health expenses	106	0.400	0.496	0.011	0.022	0.922	0.825	-0.014	-0.028	0.904	0.649
Covid-19 response: reduced utilities expenses	105	0.425	0.501	0.013	0.026	0.905	0.979	0.051	0.101	0.662	0.785
Covid-19 response: delayed monthly billings	105	0.375	0.490	-0.102	-0.208	0.321	0.351	-0.077	-0.158	0.516	0.454
Covid-19 response: informal loan (family/friends)	104	0.359	0.486	-0.136	-0.279	0.181	0.099*	-0.107	-0.220	0.332	0.203
Covid-19 response: formal loan or credit	104	0.051	0.223	0.098	0.438	0.077*	0.048**	0.162	0.725	0.011**	0.006***
Covid-19 response: sold expensive goods (vehicle, jewelry, etc.)	105	0.077	0.270	0.054	0.202	0.462	0.437	0.037	0.139	0.640	0.713
Covid-19 response: used family savings	105	0.282	0.456	-0.022	-0.048	0.833	0.952	-0.001	-0.003	0.991	0.861
Covid-19 response: new activities to generate more income	104	0.256	0.442	0.008	0.018	0.935	0.887	0.007	0.017	0.943	0.836
Covid-19 response: gave or lent money to family members	104	0.103	0.307	-0.044	-0.145	0.495	0.824	-0.013	-0.041	0.877	0.798
Covid-19 response: applied/used government emergency solutions	105	0.487	0.506	-0.073	-0.145	0.505	0.753	-0.150	-0.296	0.160	0.287
Covid-19 response: none	104	0.103	0.307	0.079	0.257	0.228	0.462	0.084	0.273	0.227	0.442
Covid-19 response: other	104	0.154	0.366	-0.041	-0.112	0.646	0.949	-0.079	-0.217	0.389	0.681
E. Virus Transmission and Mental Health											
At least one Covid-19 case- Home	101	0.079	0.273	-0.034	-0.124	0.572	0.617	-0.044	-0.160	0.490	0.498
At least one Covid-19 case- Family	101	0.184	0.393	0.053	0.134	0.528	0.819	0.021	0.052	0.825	0.999
At least one Covid-19 case- Friends	101	0.158	0.370	-0.044	-0.119	0.548	0.748	-0.041	-0.112	0.595	0.881
At least one Covid-19 case- Neighbors	101	0.132	0.343	-0.016	-0.046	0.800	0.946	-0.016	-0.046	0.808	0.824
At least one Covid-19 case- Work	101	0.026	0.162	-0.003	-0.015	0.951	0.610	-0.008	-0.049	0.868	0.714
At least one Covid-19 case- Other acquaintance	101	0.289	0.460	-0.061	-0.132	0.534	0.460	-0.068	-0.147	0.492	0.525
Do not know any COVID-19 case	101	0.368	0.489	0.121	0.248	0.273	0.364	0.166	0.340	0.146	0.351
Good health	107	0.275	0.452	-0.011	-0.025	0.907	0.970	-0.052	-0.116	0.609	0.806
Happy	104	0.538	0.505	-0.054	-0.106	0.637	0.826	-0.084	-0.166	0.492	0.737
Feel depressed	95	0.853	0.359	-0.100	-0.278	0.273	0.180	-0.106	-0.294	0.276	0.207
Feel worried	95	0.618	0.493	0.054	0.110	0.644	0.890	0.079	0.160	0.514	0.666
PHQ4 Test: Normal	95	0.147	0.359	-0.004	-0.012	0.962	0.671	-0.044	-0.123	0.618	0.837
PHQ4 Test: Anxiety	95	0.382	0.493	0.020	0.042	0.864	0.568	0.013	0.025	0.919	0.643
PHQ4 Test: Depression	95	0.265	0.448	0.084	0.188	0.456	0.330	0.054	0.120	0.651	0.558
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES

Table 12: Results Elderly Rounds During the Covid-19 Pandemic: November 2020

	N	Control Mean	Control SD	Treatment Effect	Specification 1 Treatment Effect (SD)	Specification 1 OLS p-value	Specification 1 Rand-t p-value	Treatment Effect	Specification 2 Treatment Effect (SD)	Specification 2 OLS p-value	Specification 2 Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
F. Neighborhood Characteristics											
Close to childcare/pre-school (4 blocks)	134	0.396	0.494	0.012	0.024	0.906	0.798	0.007	0.014	0.948	0.798
Close to Schools (4 blocks)	134	0.646	0.483	0.002	0.003	0.988	0.684	-0.001	-0.003	0.988	0.745
Close to subway/bus (4 blocks)	135	0.857	0.354	-0.084	-0.239	0.280	0.204	-0.094	-0.267	0.223	0.209
Close to Park (4 blocks)	134	0.604	0.494	-0.041	-0.082	0.682	0.723	-0.054	-0.109	0.599	0.946
Close to Health Care (4 blocks)	134	0.562	0.501	-0.005	-0.010	0.961	0.962	-0.002	-0.004	0.984	0.761
Less than 15 min commute time to family	79	0.393	0.497	-0.104	-0.209	0.412	0.318	-0.161	-0.324	0.220	0.101
Less than 15 min commute time to friends	67	0.458	0.509	-0.083	-0.163	0.560	0.403	-0.160	-0.314	0.267	0.236
Street Alcohol Consumption	105	0.579	0.500	-0.033	-0.065	0.775	0.587	-0.038	-0.076	0.769	0.544
Street Drug Consumers	105	0.500	0.507	-0.089	-0.176	0.436	0.270	-0.081	-0.159	0.475	0.340
Street Drug Trafficking	105	0.158	0.370	-0.046	-0.123	0.564	0.612	-0.034	-0.093	0.670	0.617
Destroyed property	105	0.526	0.506	-0.318	-0.629	0.003***	0.004***	-0.305	-0.603	0.005***	0.015**
Graffiti	105	0.263	0.446	-0.143	-0.322	0.106	0.048**	-0.145	-0.325	0.114	0.063*
Gang Fights	105	0.158	0.370	-0.080	-0.218	0.272	0.165	-0.059	-0.159	0.352	0.203
People Carrying guns	105	0.184	0.393	-0.064	-0.162	0.385	0.170	-0.047	-0.120	0.550	0.316
Shooting	105	0.526	0.506	-0.231	-0.457	0.035**	0.153	-0.293	-0.579	0.012**	0.107
Prostitution	105	0.132	0.343	-0.126	-0.369	0.052*	0.054*	-0.141	-0.410	0.043**	0.045**
Feels safe walking at night	105	0.410	0.498	-0.056	-0.112	0.620	0.975	-0.052	-0.104	0.659	0.831
Feels safe inside the house at night	105	0.718	0.456	0.033	0.072	0.727	0.592	0.048	0.106	0.611	0.503
Victim of violence (physical)	95	0.061	0.242	0.002	0.010	0.971	0.988	0.009	0.038	0.897	0.985
Victim of robbery	89	0.333	0.479	0.015	0.031	0.901	0.735	-0.045	-0.093	0.711	0.531
G. Housing and Neighborhood Satisfaction											
Satisfaction current housing unit	132	0.816	0.391	0.047	0.121	0.481	0.231	0.052	0.132	0.432	0.209
Satisfaction current neighborhood	130	0.875	0.334	0.029	0.086	0.702	0.699	0.003	0.009	0.962	0.949
Would ask neighbors for childcare	117	0.419	0.499	0.144	0.289	0.172	0.354	0.113	0.227	0.292	0.358
Has close friends in the neighborhood	124	0.591	0.497	0.129	0.259	0.227	0.313	0.126	0.253	0.232	0.244
Would ask neighbors for economic help	125	0.214	0.415	0.236	0.569	0.007***	0.007***	0.237	0.571	0.012**	0.004***
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES
Westfall-Young Multiple Testing (p-value)							0.013**				0.054*

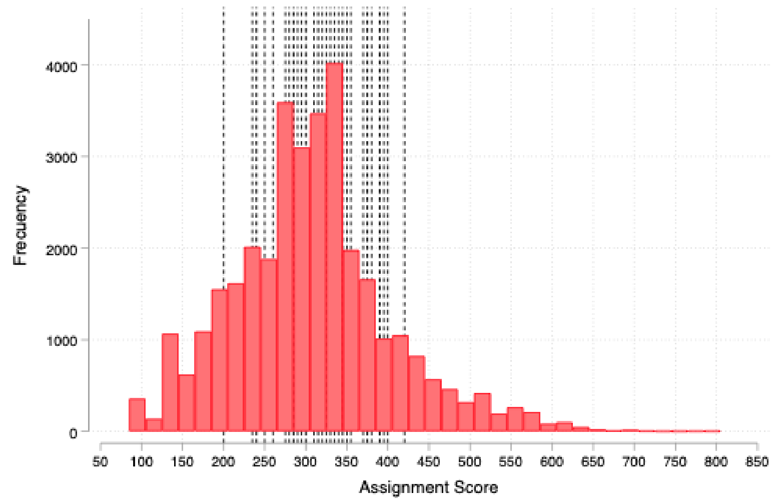
Note: This table replicates the analysis in Table 11 for elderly rounds. See Table 11 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Figure 1: Sharp RD Design

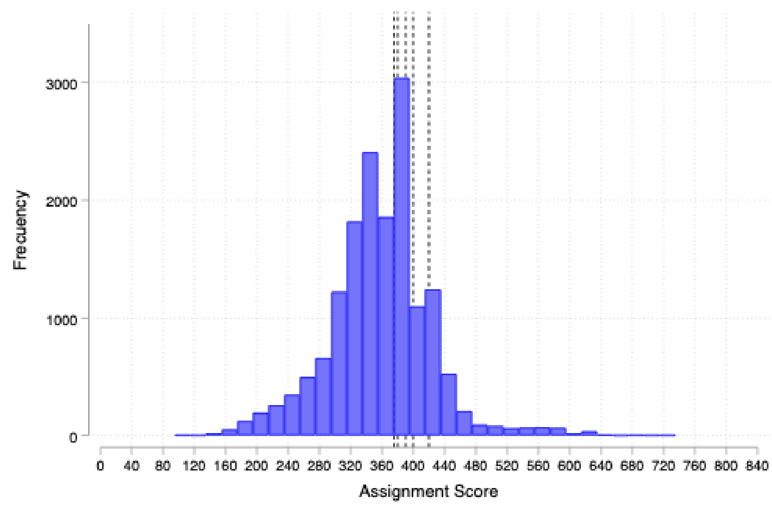


Note: This figure presents treatment probability for different values of the normalized application score.

Figure 2: Multiple Cutoff Regression Discontinuity Design



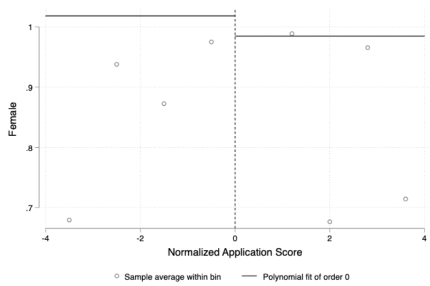
(a) Regular Rounds



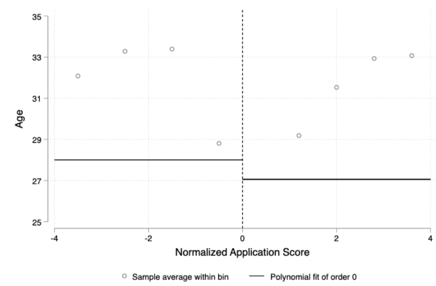
(b) Elderly Rounds

Note: This figure presents the distribution of scores in regular (2.A) and elderly (2.B) rounds in the pooled data. Black vertical lines indicate the values the cutoff has taken over time.

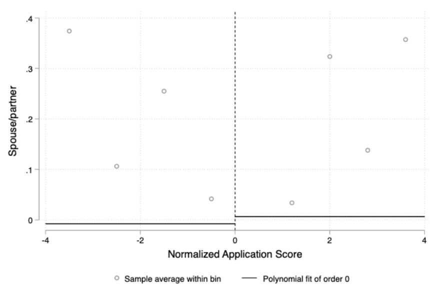
Figure 3: Balance in Baseline Characteristics in Regular Rounds



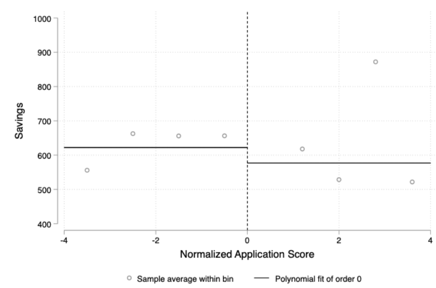
(a) Female



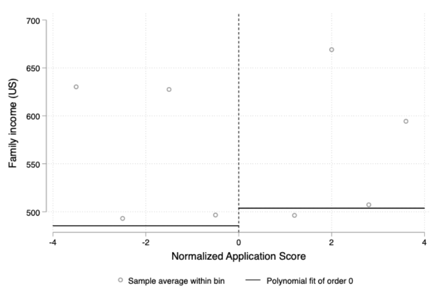
(b) Age



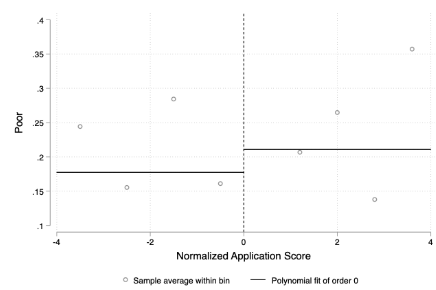
(c) Spouse/Partner



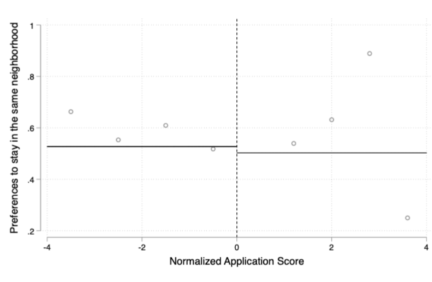
(d) Savings (UF)



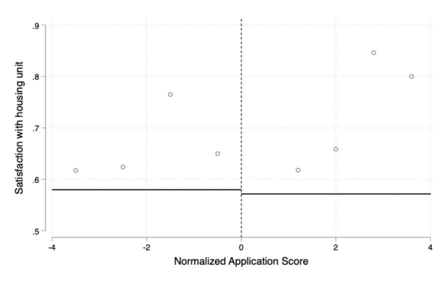
(e) Income (US)



(f) Income (US) below Poverty Line

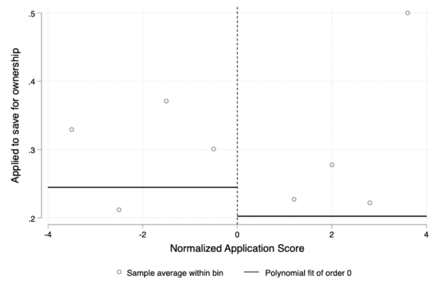


(g) Preferences Same Neighborhood

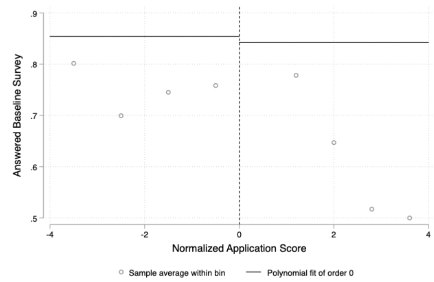


(h) Satisfaction Housing Unit

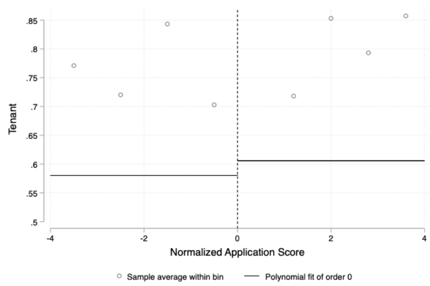
Figure 3: Balance in Baseline Characteristics in Regular Rounds



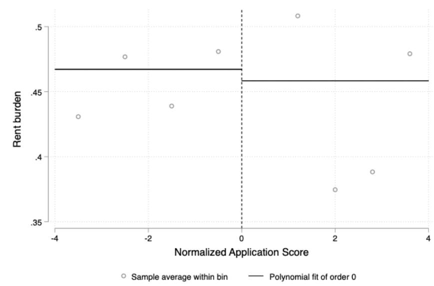
(i) Apply to save for Ownership



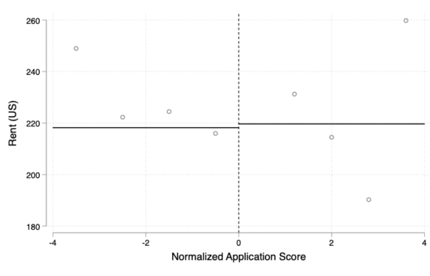
(j) Baseline Survey Response



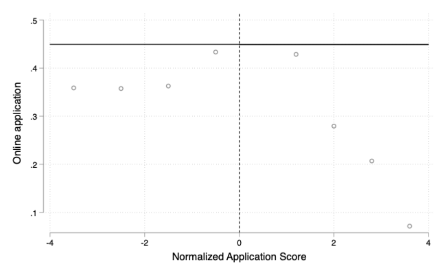
(k) Tenancy



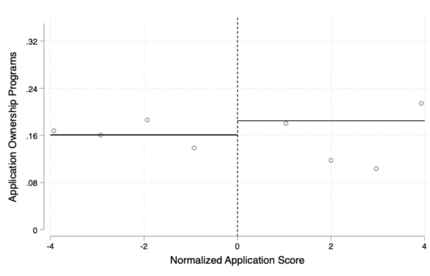
(l) Rent Burden



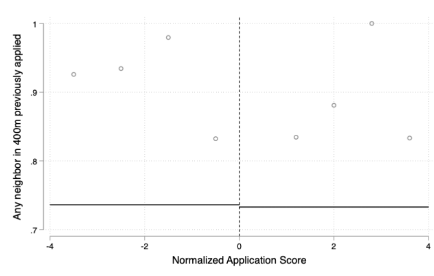
(m) Rent



(n) Online Application

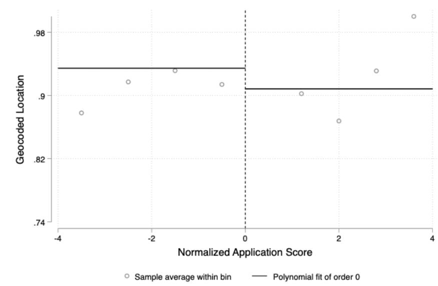


(o) Ownership Program Applications

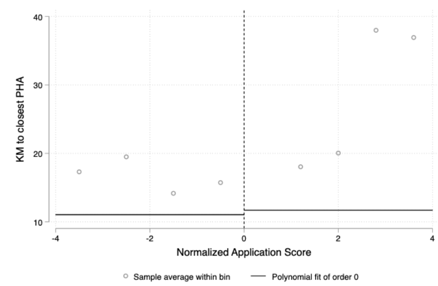


(p) Neighbors Applied in t-1

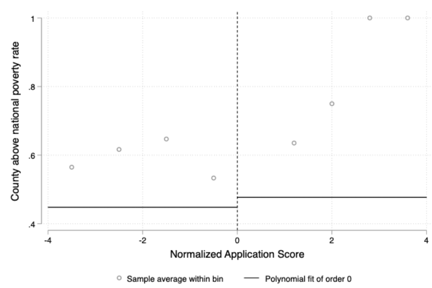
Figure 3: Balance in Baseline Characteristics in Regular Rounds



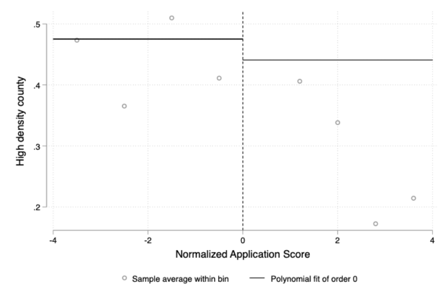
(q) Geocoded Location



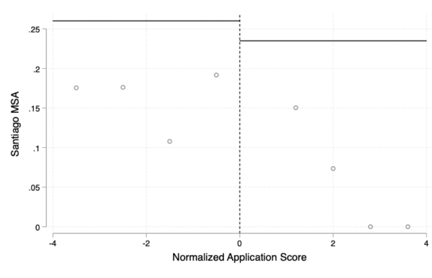
(r) Distance Closest PHA



(s) High Poverty County



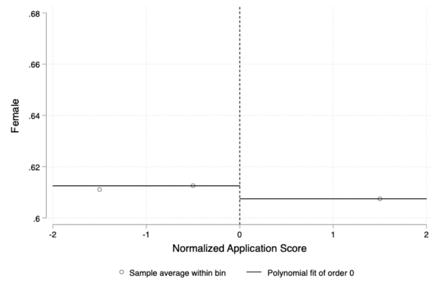
(t) High Density County



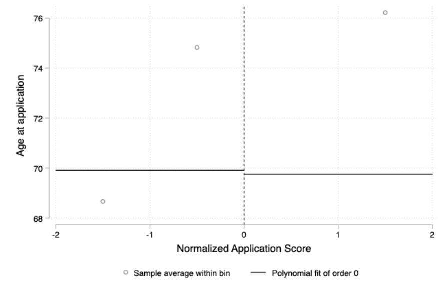
(u) Residence in Santiago MSA

Note: This figure show a graphical representation of balance tests performed in Table 6 for baseline covariates. The X-axis shows the normalized pooled running variable and Y-axis the value of the corresponding baseline covariate. Grey dots represent average baseline covariate in each mass point included in the evaluation sample. These graphs are created using the package rdplot developed by Calonico, Cattaneo and Titiunik (2015).

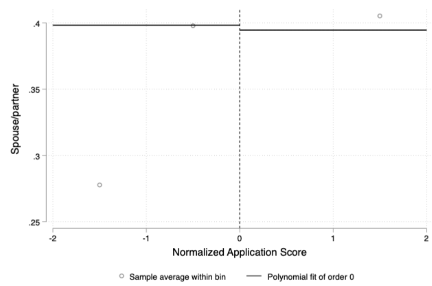
Figure 4: Balance in Baseline Characteristics in Elderly Rounds



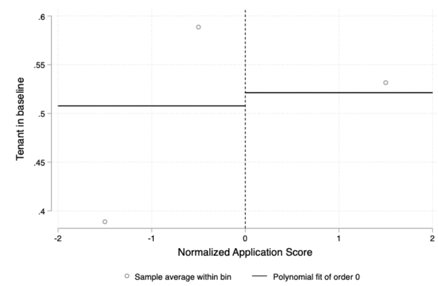
(a) Female



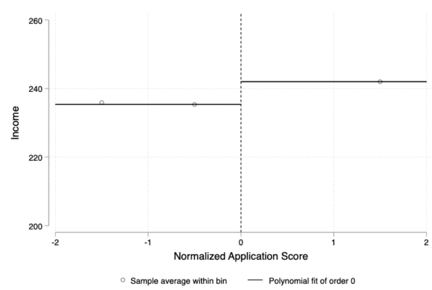
(b) Age



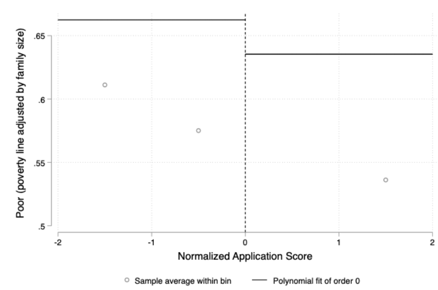
(c) Spouse/Partner



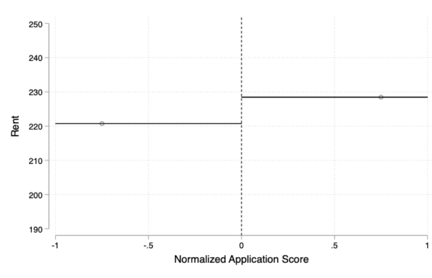
(d) Tenancy



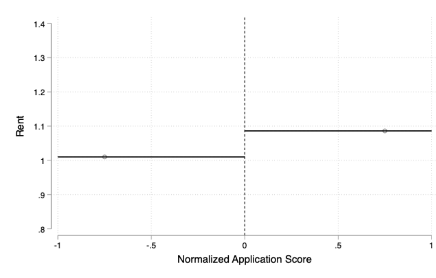
(e) Income



(f) Income Below Poverty Line

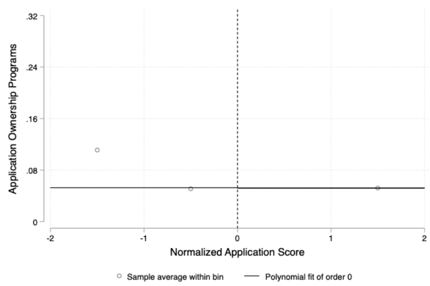


(g) Rent

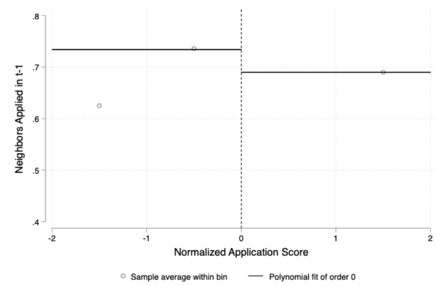


(h) Rent Burden

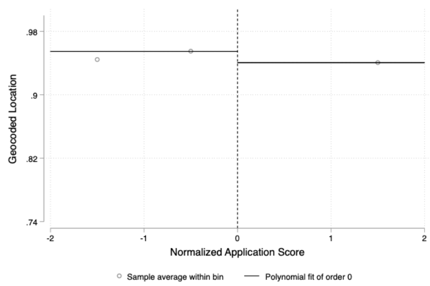
Figure 5: Balance in Baseline Characteristics in Elderly Rounds



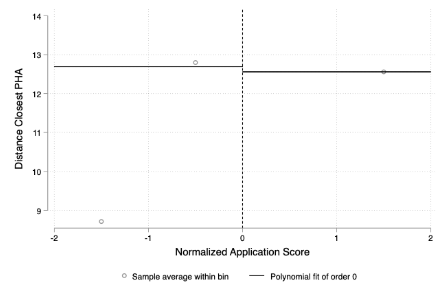
(a) Ownership Program Applications



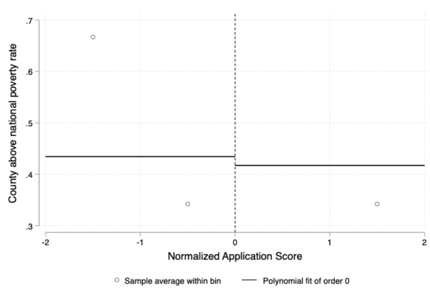
(b) Neighbors Applied in t-1



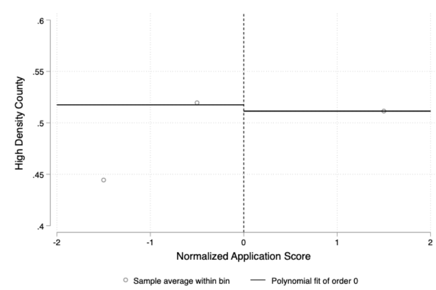
(c) Geocoded Location



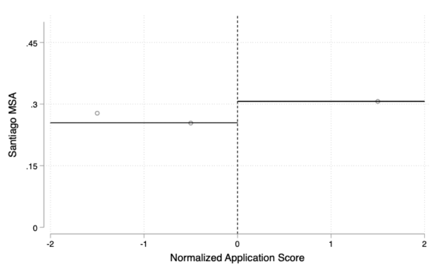
(d) Distance Closest PHA



(e) High Poverty County



(f) High Density County



(g) Residence in Santiago MSA

Note: This figure replicates Figure 3 using data from elderly rounds. See Figure 3 for details.

A1 Appendix Tables and Figures

Figure A1: Distribution of households by tenure in Chile in 2013

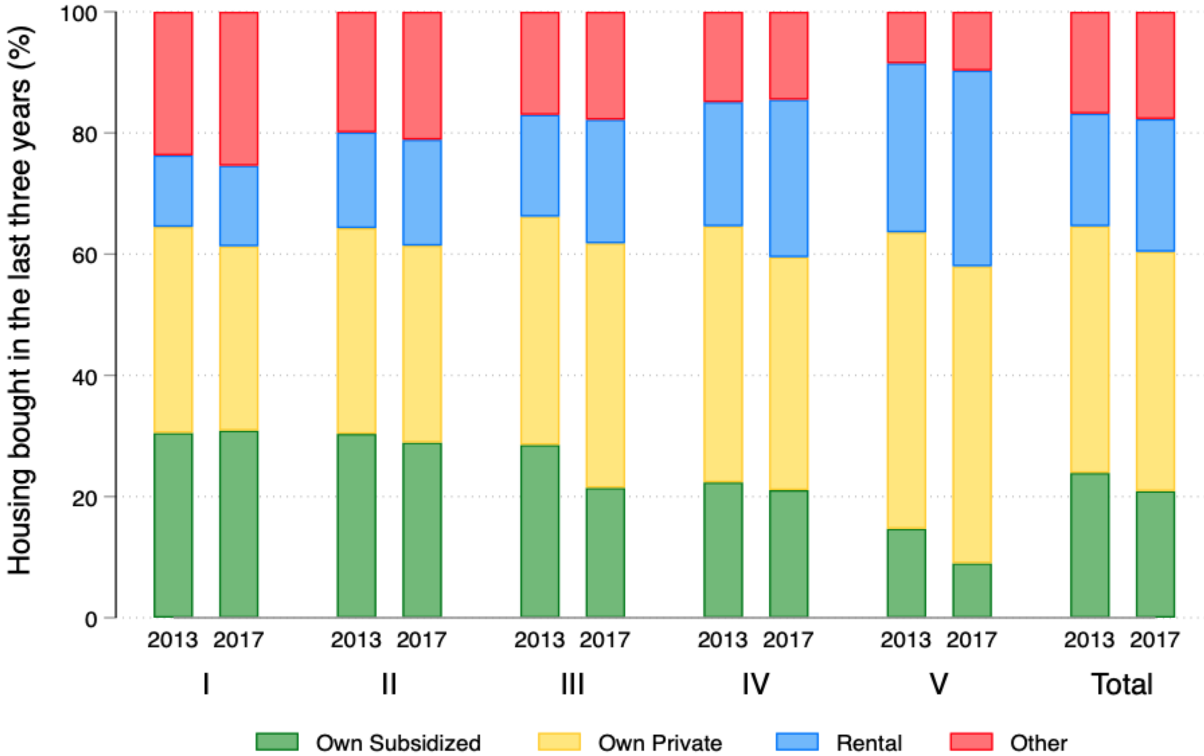


Table A1: Application Score

	Score Component	Regular Rounds	Differences in Elderly Rounds
1	Household member*	40 each	-
2	Children under 5 **	30 each	-
3	Children between 6 and 18	20 each	-
4	Elderly*	30 each	60 each
5	Single Parent of 18 or younger children	0 or 35	-
6	Physical discapacity	30 each	-
7	Tortured in dictatorship (applicant, partner)	0, 100, 200	-
8	Military Service	20 each	-
9	Gendarmerie Service (applicant, partner)	0, 40, 80	-
10	Previous Applications (max 3)	0, 20, 40, 60	-
11	Social Vulnerability		
	2014-2016	(13484-FPS Score)/100	-
	RSH Reform	0, 45, 90, 135, 180	-
12	Housing Vulnerability	0, 20, 40, 60, 80, 100, 120, 140, 160	-
13	Applicant's age (60-64, 65-69, 70-74, >75)	No	20, 40, 60, 100

Note: This table presents all score components and how they are taken into consideration to calculate the application score. (*) Applicants are excluded in regular rounds. (**) Age by the end of the application year. Housing Vulnerability score is the sum of scores for crowding, housing quality, access to reliable water and basic sanitation.

Table A2: Baseline Characteristics Regular Rounds

	All Applicants								Voucher Recipients						
	Pooled	Non-Recipients		Recipients		Est. Difference			Never Leased-up		Leased-up		Est. Difference		
	Mean	Mean	SD	Mean	SD	Coeff	FE Coeff	N	Mean	SD	Mean	SD	Coeff	FE Coeff	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	
I. Baseline Characteristics															
Tenant in baseline	0.69	0.71	0.45	0.67	0.47	-0.040***	-0.059***	39,385	0.65	0.48	0.71	0.45	0.065***	0.076***	23,553
Saving balance on application day (US)	563.01	579.90	684.81	550.84	628.32	-29.061***	-61.242***	39,385	574.74	638.85	513.80	616.69	-60.943***	-54.295***	23,553
Family income (US)	580.88	598.59	207.84	568.13	217.68	-30.454***	-44.994***	39,385	571.31	216.72	562.87	218.26	-8.436***	-8.984***	23,553
Poor (poverty line adjusted by family size)	0.25	0.14	0.35	0.32	0.47	0.180***	0.215***	39,385	0.31	0.46	0.34	0.47	0.034***	0.019***	23,553
Online application	0.34	0.35	0.48	0.33	0.47	-0.016***	-0.053***	39,362	0.37	0.48	0.27	0.45	-0.096***	-0.094***	23,542
Baseline application to ownership programs	0.14	0.13	0.33	0.14	0.35	0.016***	0.009*	39,385	0.14	0.35	0.15	0.36	0.016***	0.003	23,553
KM to closest PHA	14.51	13.72	18.97	15.08	20.46	1.358***	1.104***	35,602	14.39	19.81	16.27	21.47	1.881***	1.550***	21,183
High density county	0.45	0.47	0.50	0.43	0.50	-0.044***	-0.058***	39,385	0.47	0.50	0.35	0.48	-0.120***	-0.112***	23,553
Age at application	34.69	35.65	11.39	34.00	9.54	-1.649***	-1.981***	39,385	34.17	9.65	33.75	9.41	-0.424***	-0.343***	23,553
Preferences to stay in the same neighborhood	0.54	0.53	0.50	0.54	0.50	0.005	0.004	23,308	0.53	0.50	0.55	0.50	0.023***	0.040***	13,650
Satisfaction with housing unit	0.59	0.60	0.49	0.58	0.49	-0.018***	-0.028***	24,460	0.56	0.50	0.62	0.49	0.058***	0.067***	14,306
Applied to save for ownership	0.27	0.28	0.45	0.26	0.44	-0.021***	-0.023***	22,785	0.25	0.43	0.27	0.44	0.019**	0.027***	13,338
Any neighbor in 400m previously applied	0.76	0.76	0.43	0.76	0.43	-0.003	-0.006	26,330	0.76	0.43	0.74	0.44	-0.027***	0.022***	15,576
Answered Baseline Survey	0.69	0.70	0.46	0.68	0.47	-0.027***	-0.023***	39,385	0.67	0.47	0.68	0.47	0.002	0.015**	23,553
Female	0.83	0.83	0.38	0.83	0.38	0.001	0.007	39,362	0.84	0.37	0.82	0.38	-0.016***	-0.011**	23,542
Spouse/partner	0.25	0.24	0.43	0.25	0.44	0.017***	-0.004	39,385	0.25	0.43	0.26	0.44	0.014**	0.003	23,553
Rent (US)	242.16	237.45	99.44	245.21	101.87	7.762***	-3.211	13,152	247.63	102.60	239.37	99.85	-8.255***	-0.209	7,994
Rent burden	0.46	0.44	0.24	0.48	0.26	0.036***	0.037***	13,149	0.48	0.25	0.47	0.26	-0.004	0.011*	7,993
Geocoded location	0.90	0.91	0.29	0.90	0.30	-0.009***	-0.006	39,385	0.90	0.30	0.89	0.31	-0.009**	-0.007	23,553
County above national poverty rate	0.21	0.20	0.40	0.22	0.41	0.022***	0.025***	39,385	0.19	0.39	0.27	0.45	0.083***	0.075***	23,553
Santiago MSA	0.23	0.23	0.42	0.23	0.42	-0.004	-0.021***	39,385	0.28	0.45	0.13	0.34	-0.150***	-0.124***	23,553
Chilean	0.91	0.93	0.26	0.91	0.29	-0.023***	-0.026***	39,362	0.90	0.30	0.92	0.27	0.023***	0.011***	23,542
Children younger than 18 in the household	0.84	0.69	0.46	0.94	0.24	0.241***	0.258***	39,385	0.93	0.25	0.93	0.25	0.001	0.002	23,553
II. Score															
Application score	325.93	252.54	66.21	378.77	90.98	126.228***	152.525***	39,385	374.98	93.36	376.26	89.03	1.277	-3.418***	23,553
Social vulnerability score	158.25	140.02	47.03	171.38	22.98	31.360***	37.999***	39,385	170.47	24.01	171.61	22.65	1.140***	1.496***	23,553
Family size score	68.60	49.95	20.66	82.03	37.40	32.079***	35.252***	39,385	80.18	37.44	82.88	37.31	2.701***	0.711	23,553
Assignment FE						No	Yes						No	Yes	

Note: This table shows summary statistics for the entire population of applicants. Columns 1 to 5 show statistics for the pooled sample and separately for those who were and were not assigned a voucher. Columns 6 and 7 show estimated correlations between baseline covariates and treatment status using OLS regressions; column 7 includes assignment fixed effects. Columns 9 to 14 show unconditional means and standard deviation of baseline characteristics for the population of voucher recipients by lease up status: families that ever and never leased up with their subsidies. Columns 13 and 14 show estimated OLS regression adjusted differences in means between these groups, column 14 includes assignment fixed effects. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A3: Baseline Characteristics Elderly Rounds

	All Applicants								Voucher Recipients						
	Pooled	Non-Recipients		Recipients		Est. Difference			Never Leased-up		Leased-up		Est. Difference		
	Mean	Mean	SD	Mean	SD	Coeff	FE Coeff	N	Mean	SD	Mean	SD	Coeff	FE Coeff	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	
I. Baseline Characteristics															
Family income (US)	264.14	270.91	158.21	244.77	123.95	-26.134***	-28.619***	22,515	242.66	118.73	247.02	129.52	4.361	3.511	5,887
Poor (poverty line adjusted by family size)	0.60	0.60	0.49	0.60	0.49	0.001	0.005	22,515	0.59	0.49	0.62	0.49	0.030**	0.029**	5,887
Baseline application to ownership programs	0.06	0.07	0.25	0.06	0.23	-0.010**	-0.015***	22,515	0.06	0.23	0.06	0.23	0.001	-0.001	5,887
KM to closest PHA	13.52	13.34	19.08	14.06	20.20	0.719**	0.842***	21,299	12.76	18.27	15.62	22.17	2.857***	2.386***	5,561
High density county	0.50	0.51	0.50	0.46	0.50	-0.043***	-0.037***	22,515	0.52	0.50	0.40	0.49	-0.126***	-0.109***	5,887
Female	0.61	0.63	0.48	0.55	0.50	-0.081***	-0.082***	22,431	0.53	0.50	0.57	0.50	0.035***	0.033**	5,876
Spouse/partner	0.38	0.38	0.49	0.37	0.48	-0.008	-0.022***	22,515	0.36	0.48	0.39	0.49	0.026**	0.016	5,887
Age at application	70.39	68.68	5.70	75.30	6.88	6.629***	6.864***	22,515	75.62	6.91	74.89	6.83	-0.730***	-0.804***	5,887
Any neighbor in 400m previously applied	0.81	0.82	0.38	0.77	0.42	-0.054***	-0.010	14,542	0.77	0.42	0.75	0.43	-0.021	-0.002	3,818
Rent (US)	211.91	215.61	103.04	192.88	116.79	-22.729***	-22.014***	8,018	183.88	111.03	206.08	123.69	22.203***	25.362***	1,305
Rent burden	0.93	0.94	0.58	0.88	0.61	-0.067***	-0.068***	8,018	0.84	0.57	0.93	0.66	0.090**	0.102***	1,305
Tenant in baseline	0.61	0.64	0.48	0.54	0.50	-0.105***	-0.060***	22,515	0.53	0.50	0.54	0.50	0.013	0.027**	5,887
Geocoded location	0.95	0.95	0.22	0.94	0.23	-0.002	-0.003	22,515	0.95	0.23	0.94	0.23	-0.002	-0.000	5,887
County above national poverty rate	0.17	0.16	0.37	0.19	0.39	0.026***	0.028***	22,515	0.17	0.38	0.20	0.40	0.028***	0.020**	5,887
Santiago MSA	0.28	0.28	0.45	0.25	0.43	-0.035***	-0.023***	22,515	0.31	0.46	0.18	0.39	-0.124***	-0.098***	5,887
Chilean	0.98	0.98	0.15	0.98	0.14	0.004*	0.003	22,431	0.98	0.15	0.98	0.12	0.006*	0.005	5,876
Children younger than 18 in the household	0.04	0.03	0.16	0.08	0.26	0.049***	0.053***	22,515	0.06	0.25	0.09	0.28	0.023***	0.028***	5,887
II. Score															
Application score	356.87	330.68	49.00	431.91	52.38	101.227***	102.288***	22,515	433.97	53.18	428.90	50.77	-5.062***	-3.116**	5,887
Age score (Elderly rounds)	55.69	47.64	23.89	78.75	27.52	31.114***	32.324***	22,515	79.69	27.20	77.35	27.85	-2.342***	-2.450***	5,887
Number of elderly score	50.42	48.63	20.49	55.54	13.49	6.909***	5.765***	22,515	55.97	13.01	55.09	13.95	-0.877**	-1.320***	5,887
Assignment FE						Yes	Yes						Yes	Yes	

Note: This table replicates the analysis in Table A2 for elderly rounds. See Table A2 for details.

A2 Selective Attrition and Balance in the Follow up Sample

Attrition

I analyze the presence of selective attrition in the Follow-up Sample. In other words, whether the treatment affected differently the likelihood of responding the survey between units at different sides of the cutoff in the evaluation sample. It is worth mentioning that, while we could not provide monetary incentives, we did several things to reduce the chances of selective attrition.[63]

The follow-up survey was sent by email to all individuals who applied to the program between March 2014 and May 2020 who had a valid email. In total, 60,926 surveys were sent, 37,338 of whom applied between 2017 and 2019, the relevant period in this paper. The response rate in this group was 57 percent, 60 percent (18,185) in regular rounds and 44 percent (3,023) in elderly rounds.[64] In the evaluation sample, the response rate was 59.5 percent (706) and 27 percent (150) in regular and elderly rounds, respectively.[65] These rates of response are high for online surveys.

Figures A5a and A5b show response rates by assignment and treatment group. Except for some assignments, treated and controls show similar response rates. Moreover, it is not clear whether holding a voucher made it more or less likely to respond the survey.

More formally, to analyze selective attrition, I estimate the following linear probability model, separately for elderly and regular rounds.

$$Y_i = \alpha + \gamma_s Assignment_s + \tau_s D_{i,s} + \beta_s D_{i,s} x Assignment_s + \delta Z_i + \epsilon_{i,s} \tag{A2.1}$$

This equation is similar to the fully interacted FE model in equation 5.1 used to analyze

[63] First, to enhance confidence, the email was sent from the same institutional email used to send the baseline survey. In addition, the email included a link to MINVU's Web site where the survey was acknowledge and its goals were explained. Second, we provided non monetary incentives to respond the survey. Policy changes during the pandemic created high information demands; PHAs were closed while MINVU announced different changes to its programs to adapt to the current crisis. Furthermore, in July 2020, at the peak of the pandemic, MINVU announced 150k emergency rental subsidies, available also to already voucher recipients of elderly and regular rounds. In this context, we created a blog with short and simple answers to frequently asked questions and provided survey respondents with the opportunity of sending their own questions at the end of the survey, which we responded through the blog. We received more than 10k questions during the data collection period.

[64] In this period the elderly applied to the program in person only, having lower quality contact information in the data set. We tried to reach out to the elderly using text messages but phones were also not valid or updated. I do not report this data.

[65] These numbers exclude those who answer but did not recall applying to the program or applied for someone else, which was common in elderly rounds. I dropped fifty responses for this reason in elderly rounds.

balance in section **??**. Here, however, the dependent variable Y_i is an indicator variable taking the value of one for those who responded the follow up survey and zero for the rest. Z_i include baseline covariates used in balance tests in the previous section.

Tables A4 and A5 show the estimates of τ_s and β_s for regular and elderly rounds, respectively. The bottom panel presents the results (p-values) of three different analysis of the null of joint significance: F-Test, Randomization-t Joint significance test and the Westfall-Young multiple-testing test of overall treatment irrelevance (Young, 2019).

Table A4 shows that all individual coefficients, τ_s and β_s, in the evaluation sample are not significant in regular rounds. Furthermore, joint significance of these coefficients is rejected by all three different tests in the bottom panel. This analysis suggest that there was not selective attrition between treated and controls in the follow up survey.

In elderly rounds, on the other hand, Figure A5b shows that no treated unit answered the follow up survey in the assignment in Valparaiso in July 2019. Therefore, nine observations in the control group who had valid survey data were dropped, keeping only 141 observations in elderly rounds in the follow up sample. In this small sample, table A5 shows that while some individual coefficients are statistically significant at the 95 and 90 percent of confidence, the data rules out selective attrition in the overall sample.

Balance I analyze balance in the follow-up sample. Even in the absence of selective attrition, the strong assumptions made in the Local Randomization RD framework may not hold in a subset of individuals from the evaluation sample; excluding observations in different mass points around the cutoff may introduce bias. Compared to the continuity approach, the LRRD has the advantage of using a fixed sample, therefore, it is easier to test whether identification assumptions still hold in the sub-sample of follow-up respondents.[66]

Tables A6 and A7 replicate the balance analysis presented in section **??** for the follow up sample. Given the smaller sample sizes, I just present randomization inference results in this section.

In general, the results are similar in the evaluation and follow-up samples. In regular rounds, table A6 shows small differences in two baseline covariates, age and income, significant at 90 and 95 percent of confidence, respectively. Furthermore, the F-test of joint

[66]In the continuity approach, outcomes are analyzed using different bandwidth, therefore, it would be harder to study non-linearities caused by attrition in the follow-up survey.

significance and the Westfall-Yang test of overall treatment relevance do not provide evidence of imbalance between treatment and control groups. Table A7 shows similar results for elderly rounds. While joint significance is rejected, there are significant differences in two covariates, age and tenancy.

Altogether, the data suggests that treatment did not affect follow up responses and Local Randomization assumptions are still valid within the sub-sample that responded the survey. Nonetheless, given the small sample sizes, specially in elderly rounds, the results in section A2 need to be taken with some caution.

Table A4: Follow Up Sample Attrition in Regular Rounds

	Survey Response (1)	Survey Response (2)
Treat*Assignment April 2018	0.081	0.082
	(0.034)**	(0.029)**
Treat*Assignment Sept 2017	-0.072	-0.074
	(0.079)*	(0.068)*
Treat*Assignment July 2019 Santiago	-0.079	-0.086
	(0.494)	(0.454)
F-Test (p-value)	0.183	0.170
Rand-t Joint Test (p-value)	0.208	0.188
WP Mult-Test Rand-t (p-value)	0.059	0.055
Observations	1,654	1,654
Follow up responses	141	141
Assignment FE	Yes	Yes
Baseline covariates	No	Yes

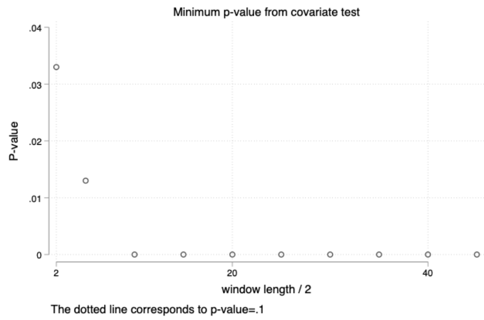
Note: This table shows estimates of equation A2.1 to analyze the effect of treatment in the non-response of the follow-up survey. Baseline controls in the model in column 2 include income and distance to the closest PHAs and dummy variables for female, age, married, tenant, baseline application to homeownership programs, poor, online application, baseline survey response, living in a high density county, high poverty county. Bottom panel presents p-values for three different analysis of the null of joint significance: F-Test, Randomization-t Joint significance test and the Westfall-Young multiple-testing test of overall treatment irrelevance. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A5: Follow Up Sample Attrition in Elderly Rounds

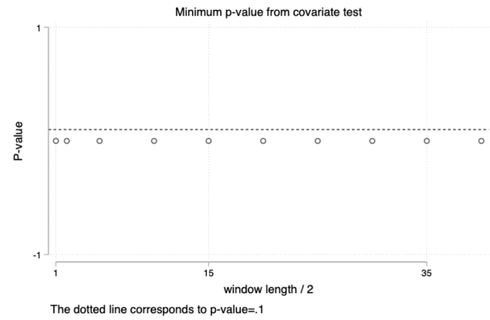
	Survey Response (1)	Survey Response (2)
Treat*Assignment April 2018	0.100	0.128
	(0.244)	(0.129)
Treat*Assignment August 2017	-0.275	-0.269
	(0.210)	(0.227)
Treat*Assignment September 2018	0.089	0.051
	(0.511)	(0.683)
Treat*Assignment December 2018	-0.043	-0.072
	(0.695)	(0.499)
Treat*Assignment October 2019 (R6)	-0.064	-0.118
	(0.647)	(0.374)
Treat*Assignment October 2019 (R9)	-0.069	-0.052
	(0.509)	(0.600)
Treat*Assignment October 2019 (R10)	0.180	0.071
	(0.221)	(0.628)
F-Test (p-value)	0.107	0.101
Rand-t Joint Test (p-value)	0.114	0.109
WP Mult-Test Rand-t (p-value)	0.715	0.481
Observations	1,356	1,356
Follow up responses	706	706
Assignment FE	Yes	Yes
Baseline covariates	No	Yes

Note: This table replicates the analysis in Table A4 for elderly rounds in the follow-up sample. Controls in column 2 include income and distance to the closest PHAs and dummy variables for female, age, married, tenant, baseline application to homeownership programs, poor, and living in a high density county, high poverty county.

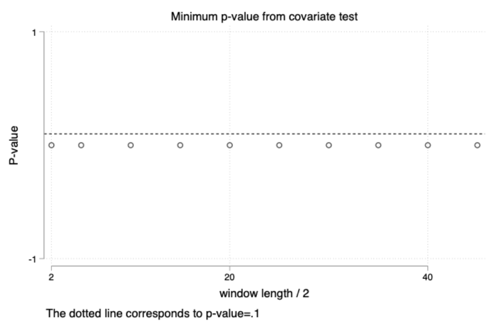
Figure A2: Window Selection by Assignment in Regular Rounds



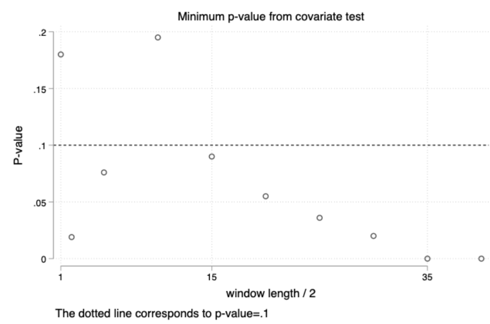
(a) April 2017, National



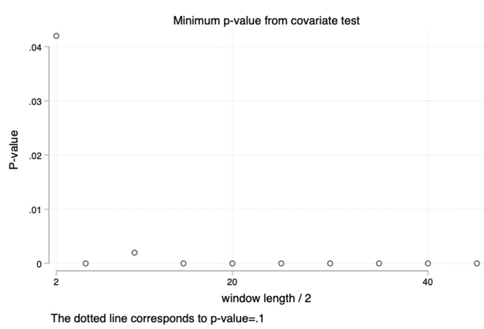
(b) May 2017, National



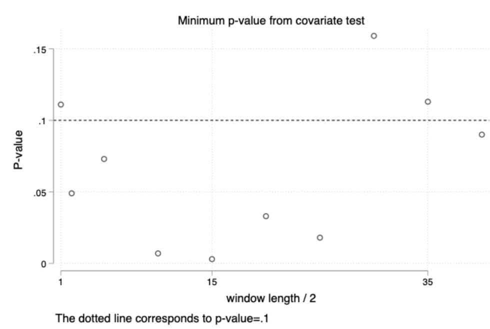
(c) June 2017, National



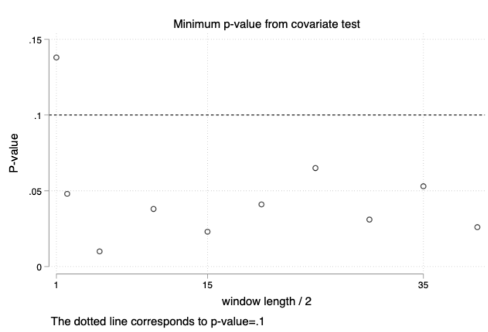
(d) August 2017, National



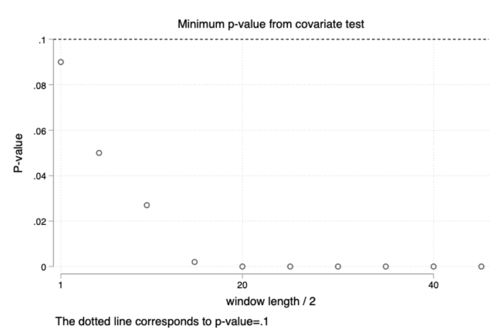
(e) October 2017, National



(f) April 2018, National



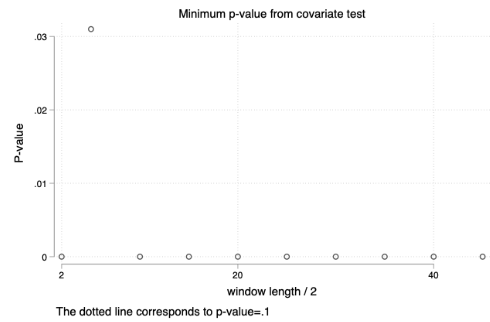
(g) June 2018, National



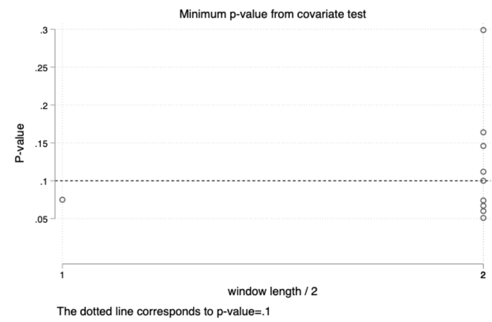
(h) September 2018, National

Figure A3: Window Selection by Assignment in Regular Rounds

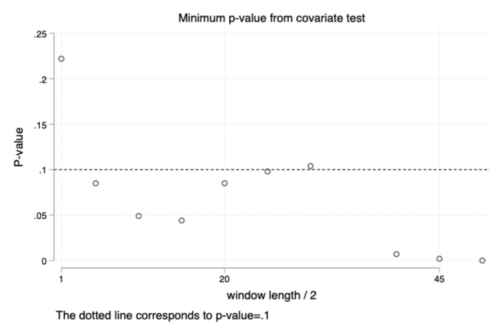
Figure A3: Window Selection by Assignment in Regular Rounds



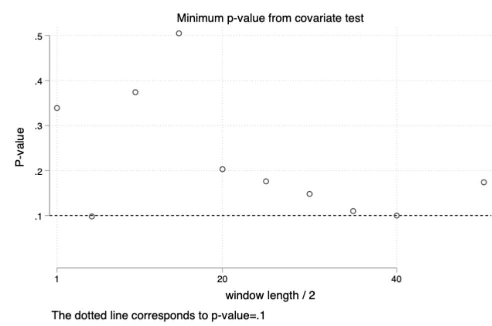
(a) November 2018, National



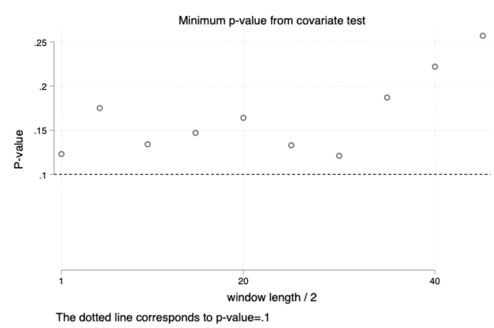
(b) December 2018, National



(c) October 2019, 6th Region



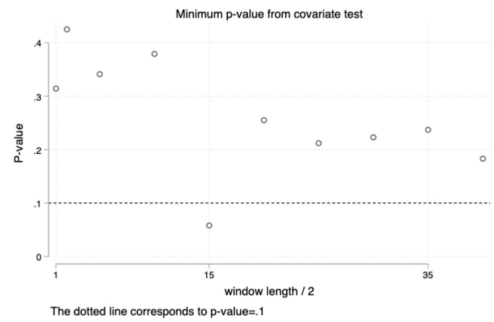
(d) October 2019, 9th Region



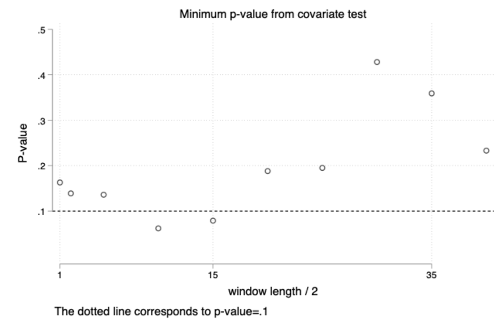
(e) October 2019, 10th Region

Note: These figures present the results of window selection procedure. Each graph shows the minimum p-value of all balance tests conducted in windows of different lengths (showed in the X-axis). The horizontal line shows the minimum significance accepted at $\alpha^* = 0.1$.

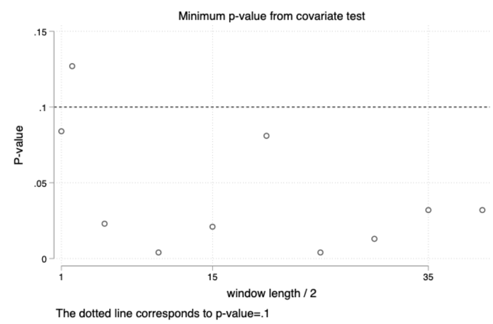
Figure A4: Window Selection by Assignment in Elderly Rounds



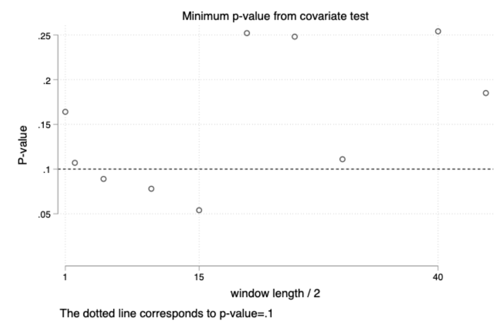
(a) September 2017, National



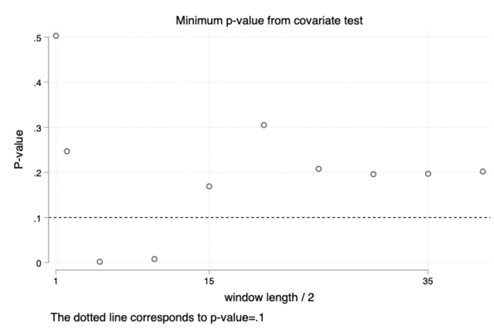
(b) April 2018, National



(c) October 2018, National



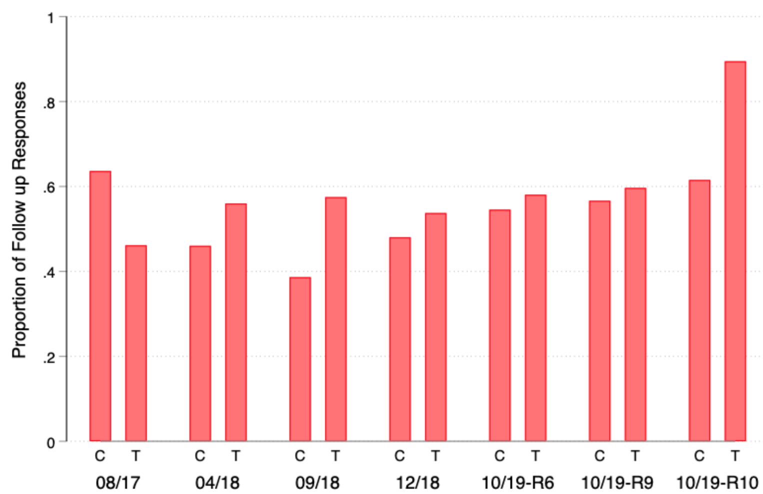
(d) July 2019, 5th Region



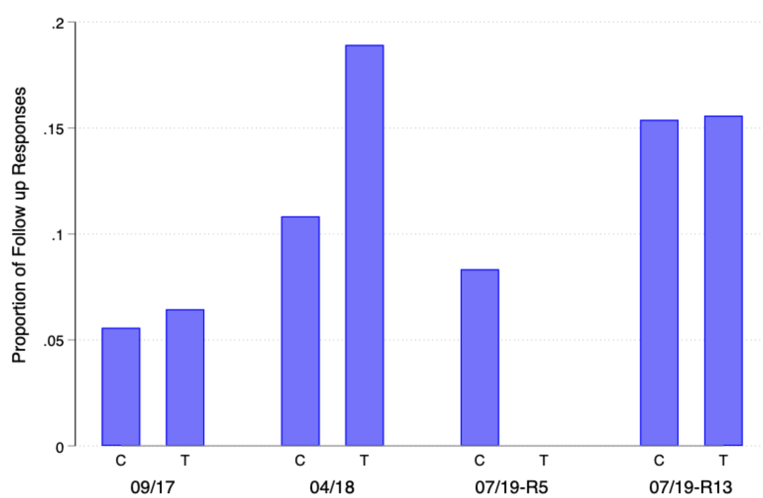
(e) July 2019, 13th Region

Note: These figures present the results of window selection procedure. Each graph shows the minimum p-value of all balance tests conducted in windows of different lengths (showed in the X-axis). The horizontal line shows the minimum significance accepted at $\alpha^* = 0.1$.

Figure A5: Follow up Sample Attrition by Assignment



(a) Regular Rounds



(b) Elderly Rounds

This figure

Table A6: Balance in Baseline Characteristics in Regular Rounds-Follow Up Survey

	Summary Statistics						Balance Test	
	Pooled	Control		Treated			Joint Test (1)	Joint Test (2)
	Mean	Mean	SD	Mean	SD	N	Rand-t (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interaction Terms (H0)								
Tenant in baseline	0.76	0.75	0.43	0.76	0.43	706	0.488	0.414
Saving balance on application day (US)	620.70	624.70	584.60	611.86	631.06	706	0.140	0.173
Family income (US)	537.90	536.42	194.19	541.16	185.25	706	0.027**	0.035**
Poor (poverty line adjusted by family size)	0.17	0.16	0.37	0.19	0.39	706	0.142	0.142
Online application	0.43	0.44	0.50	0.40	0.49	706	0.124	0.128
Baseline application to ownership programs	0.15	0.15	0.35	0.15	0.36	706	0.406	0.415
KM to closest PHA	18.43	17.37	23.43	20.75	26.17	644	0.314	0.305
High density county	0.42	0.45	0.50	0.35	0.48	706	0.786	0.804
Age at application	30.97	31.45	6.81	29.91	6.39	706	0.049**	0.054*
Preferences to stay in the same neighborhood	0.60	0.60	0.49	0.59	0.49	543	0.712	0.705
Satisfaction with housing unit	0.65	0.64	0.48	0.67	0.47	576	0.776	0.803
Applied to save for ownership	0.28	0.30	0.46	0.23	0.42	532	0.174	0.161
Any neighbor in 400m previously applied	0.88	0.91	0.28	0.83	0.38	318	0.134	0.131
Answered Baseline Survey	0.89	0.90	0.30	0.88	0.33	706	0.983	0.980
No Interaction Terms (H0')								
Female	0.94	0.94	0.24	0.94	0.24	549	0.697	0.983
Spouse/partner	0.12	0.12	0.33	0.10	0.31	582	0.489	0.639
Rent (US)	230.55	229.69	129.85	232.81	129.00	431	0.351	0.561
Rent burden	0.47	0.47	0.29	0.47	0.27	431	0.423	0.666
Geocoded location	0.91	0.91	0.28	0.91	0.28	660	0.632	0.996
County above national poverty rate	0.62	0.58	0.49	0.72	0.45	554	0.720	0.703
Santiago MSA	0.14	0.17	0.37	0.10	0.29	473	0.804	0.718
Assignment FE							Yes	Yes
Score components FE							No	Yes
Assignment-Treat Interacted Terms							Yes	Yes
				F-Test	Westfall Young	N		
Joint Significance (p-value)				0.258	0.216	706		

Note: This table replicates the analysis in Table 6 using only observations that responded the follow up sample. Given the smaller sample sizes this table presents only results from randomization inference and exclude the results of the weaker test H_0' if the test for the stronger null hypothesis H_0 is available. See Table 6 for further details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A7: Balance in Baseline Characteristics in Elderly Rounds-Follow Up Survey

	Summary Statistics						Balance Test	
	Pooled	Control		Treated			Joint Test (1)	Joint Test (2)
	Mean	Mean	SD	Mean	SD	N	Rand-t (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interaction Terms (H0)								
Family income (US)	246.73	256.93	105.71	241.79	112.30	141	0.027**	0.868
Poor (poverty line adjusted by family size)	0.59	0.50	0.51	0.63	0.48	141	0.058*	0.238
Baseline application to ownership programs	0.08	0.07	0.25	0.08	0.28	141	0.384	0.970
KM to closest PHA	13.31	13.96	19.71	13.00	17.80	135	0.646	0.196
High density county	0.57	0.65	0.48	0.54	0.50	141	0.144	0.392
Female	0.69	0.65	0.48	0.71	0.46	141	0.300	0.577
Spouse/partner	0.45	0.41	0.50	0.46	0.50	141	0.637	0.684
Age at application	73.72	72.87	5.69	74.13	7.21	141	0.318	0.004***
No Interaction Terms (H0′)								
Any neighbor in 400m previously applied	0.83	0.88	0.33	0.81	0.40	85	0.229	0.249
Tenant in baseline	0.65	0.61	0.49	0.66	0.48	141	0.060*	0.043**
Geocoded location	0.96	0.93	0.25	0.97	0.18	127	0.416	0.362
County above national poverty rate	0.26	0.30	0.47	0.24	0.43	141	0.864	0.931
Santiago MSA	0.40	0.43	0.50	0.39	0.49	127	0.170	0.115
Assignment FE							Yes	Yes
Score components FE							No	Yes
Assignment-Treat Interacted Terms							Yes	Yes
				F-Test	Westfall Young	N		
Joint Significance				0.428	0.514	141		

Note: This table replicates the analysis in Table 7 using only observations that responded the follow up sample. Given the smaller sample sizes this table presents only results from randomization inference and exclude the results of the weaker test H_0' if the test for the stronger null hypothesis H_0 is available. See Table 6 for further details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A8: Results Regular Rounds December 2019 - Follow Up Sample

		Control		Specification 1				Specification 2			
	N	Mean	SD	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing Conditions											
Household size Dec 2019	706	3.134	1.487	-0.188	-0.126	0.111	0.109	-0.177	-0.119	0.124	0.114
Number of bedrooms	703	1.942	0.855	0.096	0.112	0.203	0.198	0.099	0.116	0.188	0.186
Number of people per bedroom	702	1.791	0.901	-0.220	-0.244	0.001***	0.000***	-0.216	-0.240	0.001***	0.000***
Overcrowing indicator	702	0.120	0.326	-0.036	-0.111	0.143	0.150	-0.033	-0.103	0.171	0.169
II. Residential Mobility											
Stayed in same unit	641	0.530	0.500	-0.044	-0.088	0.333	0.310	-0.033	-0.065	0.475	0.454
Distance (km)	641	5.683	27.555	11.345	0.412	0.134	0.089*	10.653	0.387	0.130	0.091*
Distance (km) (Movers)	305	12.076	39.250	21.498	0.548	0.149	0.111	19.358	0.493	0.145	0.112
Stayed 1km or less from application location	305	0.343	0.476	-0.115	-0.243	0.055*	0.058*	-0.114	-0.240	0.059*	0.062*
Moved to another county	306	0.144	0.352	0.087	0.246	0.074*	0.074*	0.100	0.285	0.037**	0.042**
III. Neighborhood Characteristics											
Distance to closest municipality	641	3.275	4.312	0.410	0.095	0.410	0.402	0.265	0.061	0.577	0.561
Distance to closest school (km)	641	0.998	1.674	0.554	0.331	0.143	0.144	0.403	0.241	0.235	0.251
Distance to closest Pre-Shcool (km)	641	1.036	2.121	0.461	0.217	0.198	0.201	0.270	0.127	0.404	0.422
Distance to closest Primary Care (km)	599	1.592	2.296	0.491	0.214	0.261	0.263	0.267	0.116	0.497	0.511
Number of Schools in 1Km	641	4.827	4.519	-0.620	-0.137	0.114	0.116	-0.320	-0.071	0.407	0.387
Number of Schools in 2Km	641	15.341	13.478	-1.989	-0.148	0.067*	0.081*	-0.759	-0.056	0.430	0.421
Number of Preschool in 1Km	641	2.995	2.670	-0.131	-0.049	0.588	0.572	0.032	0.012	0.895	0.883
Number of Health Care in 2km	641	4.961	4.559	-0.298	-0.065	0.405	0.408	0.116	0.025	0.726	0.745
Fraction of Public Schools 1Km	516	0.429	0.294	-0.012	-0.041	0.712	0.723	-0.021	-0.070	0.518	0.539
Fraction of Subsidized Schools 1Km	516	0.530	0.284	0.003	0.009	0.936	0.944	0.010	0.035	0.753	0.749
Fraction of Private Schools 1Km	516	0.041	0.114	0.009	0.082	0.416	0.433	0.011	0.093	0.352	0.383
Mat. SIMCE, 3 Closest School 2km	551	264.812	17.886	-2.639	-0.148	0.150	0.144	-2.326	-0.130	0.202	0.195
Mat. SIMCE, 3 Closest School 2km	551	250.448	19.434	-3.333	-0.172	0.081*	0.082*	-2.970	-0.153	0.125	0.126
Fraction of Low Income Schools 1km	516	0.574	0.347	0.054	0.154	0.135	0.150	0.040	0.116	0.240	0.240
Fraction of Low Income Schools 2km	556	0.566	0.283	0.033	0.117	0.229	0.236	0.019	0.069	0.446	0.466
County poverty rate	642	0.112	0.062	0.002	0.026	0.743	0.752	-0.002	-0.033	0.606	0.606
Total crime (County z-score)	642	1.277	1.705	-0.132	-0.078	0.389	0.396	0.056	0.033	0.606	0.584
IV. Homeownership											
Application to Ownership Programs	706	0.309	0.462	0.036	0.079	0.369	0.367	0.025	0.054	0.442	0.447
Application to partially funded program (DS1)	706	0.241	0.428	-0.010	-0.024	0.779	0.785	-0.018	-0.042	0.561	0.564
Application to fully funded program (DS49)	706	0.117	0.322	0.036	0.112	0.200	0.176	0.027	0.084	0.334	0.311
Active ownership savings account	706	0.920	0.272	0.008	0.031	0.727	0.729	0.004	0.014	0.870	0.870
Balance in ownership savings account (US)	649	24.398	32.363	-0.538	-0.017	0.834	0.838	-0.966	-0.030	0.691	0.736
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES
Westfall-Young Multiple Testing (p-value)							0.009***				0.009***

Note: This table replicates the analysis in Table 9 using the follow-up sample. See Table 9 for details.

Table A9: Results Elderly Rounds December 2019 - Follow Up Sample

		Control		Specification 1				Specification 2			
	N	Mean	SD	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing Conditions											
Household size Dec 2019	150	2.545	1.751	-0.821	-0.469	0.004***	0.004***	-0.744	-0.425	0.010**	0.012**
Number of bedrooms	148	1.870	1.260	0.187	0.148	0.388	0.417	0.194	0.154	0.395	0.437
Number of people per bedroom	148	1.472	0.785	-0.553	-0.705	0.000***	0.000***	-0.517	-0.658	0.000***	0.000***
Overcrowing indicator	149	0.091	0.290	-0.083	-0.285	0.105	0.107	-0.083	-0.285	0.110	0.123
II. Residential Mobility											
Stayed in same unit	140	0.700	0.463	-0.277	-0.598	0.004***	0.007***	-0.284	-0.614	0.005***	0.008***
Distance (km)	140	1.817	6.261	20.615	3.293	0.043**	0.013**	23.551	3.762	0.059*	0.033**
Distance (km) (Movers)	66	6.049	10.478	30.705	2.930	0.048**	0.017**	46.870	4.473	0.068*	0.035**
Stayed 1km or less from application location	66	0.533	0.516	-0.256	-0.496	0.134	0.143	-0.259	-0.502	0.155	0.174
Moved to another county	67	0.188	0.403	0.056	0.138	0.684	0.677	0.061	0.152	0.623	0.629
III. Neighborhood Characteristics											
Distance to closest municipality	140	3.803	5.794	-1.257	-0.217	0.347	0.370	-1.372	-0.237	0.214	0.226
Distance to closest school (km)	140	1.314	2.887	-0.337	-0.117	0.580	0.588	-0.291	-0.101	0.583	0.595
Distance to closest Pre-Shcool (km)	140	1.395	3.456	-0.449	-0.130	0.563	0.607	-0.462	-0.134	0.503	0.531
Distance to closest Primary Care (km)	134	1.835	3.303	-0.571	-0.173	0.437	0.486	-0.640	-0.194	0.319	0.359
Number of Schools in 1Km	140	7.120	5.770	-1.091	-0.189	0.282	0.267	-1.160	-0.201	0.267	0.256
Number of Schools in 2Km	140	22.600	14.938	-2.209	-0.148	0.443	0.453	-1.943	-0.130	0.436	0.456
Number of Preschool in 1Km	140	3.680	2.965	-0.574	-0.194	0.246	0.256	-0.572	-0.193	0.264	0.272
Number of Health Care in 2km	140	7.640	6.663	-1.215	-0.182	0.311	0.294	-1.012	-0.152	0.354	0.361
Fraction of Public Schools 1Km	123	0.331	0.164	0.084	0.509	0.072*	0.077*	0.064	0.388	0.184	0.182
Fraction of Subsidized Schools 1Km	123	0.574	0.193	-0.050	-0.256	0.288	0.293	-0.040	-0.205	0.424	0.434
Fraction of Private Schools 1Km	123	0.095	0.125	-0.034	-0.274	0.169	0.172	-0.024	-0.195	0.313	0.335
Mat. SIMCE, 3 Closest School 2km	126	267.871	17.419	-4.696	-0.270	0.215	0.227	-5.497	-0.316	0.194	0.200
Mat. SIMCE, 3 Closest School 2km	126	257.280	17.075	-5.720	-0.335	0.137	0.145	-6.300	-0.369	0.147	0.160
Fraction of Low Income Schools 1km	123	0.365	0.310	0.066	0.213	0.345	0.367	0.031	0.101	0.645	0.655
Fraction of Low Income Schools 2km	126	0.411	0.276	-0.034	-0.124	0.543	0.545	-0.041	-0.149	0.404	0.418
County poverty rate	141	0.079	0.043	-0.000	-0.002	0.990	0.993	0.000	0.001	0.992	0.993
Total crime (County z-score)	141	2.332	2.248	-0.606	-0.270	0.162	0.164	-0.424	-0.188	0.336	0.329
IV. Homeownership											
Application to Ownership Programs	150	0.164	0.373	0.120	0.321	0.088*	0.094*	0.093	0.250	0.082*	0.083*
Application to partially funded program (DS1)	150	0.091	0.290	0.033	0.113	0.534	0.503	0.049	0.168	0.291	0.286
Application to fully funded program (DS49)	150	0.073	0.262	0.105	0.402	0.052*	0.049**	0.065	0.247	0.191	0.199
Assignment FE				YES	YES	YES	YES	YES	YES	YES	YES
Score components FE				YES	YES	YES	YES	YES	YES	YES	YES
Baseline Covariates				NO	NO	NO	NO	YES	YES	YES	YES
Westfall-Young Multiple Testing (p-value)							0.010**				0.010**

Note: This table replicates the analysis in Table 10 using the follow-up sample. See Table 10 for details.