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

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Abstract

Chile is one of many developing countries that are moving towards rental subsidies that, similar to the US Section 8 program, subsidize housing for low income families. Research in the US has found that Section 8 vouchers improve the quality of housing but do not improve neighborhood quality. I present the first evaluation of such programs in the developing country context. I construct a data set that merges administrative data on baseline characteristics with voucher assignment 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 eligibility to implement a local randomization regression discontinuity approach (Cattaneo & Frandsen, 2015). In the period prior to the pandemic, results are similar to 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 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 decreased unwanted mobility and a lower rent burden. It also affected how families were coping with the large unexpected income shock: voucher holders were less likely to be engaging in new activities to complement their incomes or to miss their rent payments. This results point to a previously underappreciated role of housing subsidies in helping poor households cope with negative income shocks.

1 Introduction

The Housing Choice Voucher Program (Section 8) is the largest federal assistance program in the US. Currently, more than two million families receive rental assistance through this policy. The available evidence suggests that the program improves housing conditions, yet the policy does not necessarily provide access to better neighborhoods for low income families (Collinson, Ellen, & Ludwig, 2020).

In December 2013, advised by the US Department of Housing and Urban Development (HUD), the Chilean Ministry of Housing and Urbanism (MINVU) launched the rental voucher program (*"Subsidio de Arriendo"*), the first rental subsidy in Latin America. Similar to the US policy, the Chilean program provides families with a voucher to rent a unit in the private rental market. However, Chile has a relatively small rental market, popular home-ownership policies, high levels of informality, and scarce residential mobility, all of which may affect the ability of rental vouchers to improve well being of low income families in the country. Therefore, it is difficult to predict the impact of this program without further research.

In this paper, I study the effect of the Chilean rental subsidy on a variety of outcomes including housing, residential mobility, and neighborhood characteristics using a unique data set of administrative and survey data. Specifically, I study the effect of two different rental voucher schemes: a modest voucher for a maximum of eight years available to all eligible applicants and a large voucher for elderly applicants that can be renewed every two years.

In order to estimate the causal effect of this program, I begin by constructing an administrative data set that allows me to reconstruct the treatment eligibility and assignment, and link these with a broad range of post-treatment outcomes. I supplement administrative data with a survey that I implemented, which allows me to evaluate the program before the pandemic and also after the large unexpected income shock following the COVID-19 pandemic outbreak in March 2020. With this data in hand, I use a Regression Discontinuity Design (RDD) to evaluate the program. Specifically, I estimate treatment effects using the Local Randomization approach to Regression Discontinuity Designs (LRRD), developed in (Cattaneo & Frandsen, 2015) to take into account that the running variable is discrete, taking on a limited number of values near the eligibility threshold. Intuitively,

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this method is equivalent to the standard continuity approach when the running variable is discrete, but in order to deal with a discrete running variable LRRD makes stronger identification assumptions. It uses randomization inference to identify a small window around the cutoff where these assumptions hold, and then estimate the treatment effects within that window.

Using data from December 2019, 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. Furthermore, 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.

Throughout the world, the coronavirus outbreak in March 2020 exposed an already existing housing crisis. High and increasing rents and low and stagnated wages leave low income families with almost non residual income to overcome unexpected income shocks, therefore, these households are highly vulnerable to being evicted (Ellen, O'Regan, & Ganz, 2020). Evictions have long-term negative effects Collinson and Reed (2018) that have motivated important efforts to provide housing security during the pandemic through eviction moratorium and rental assistance. Latin America, greatly hit by the pandemic has not been an exception, as several countries introduced some sort of emergency rental policies to avoid evictions.[1]

To understand how the Chilean rental subsidy offered relief to the unexpected income shock that came with the coronavirus pandemic, I implemented a follow up survey in September-November 2020. Results show that the rental policy decreased unwanted 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

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

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. Albeit the results in this period are obtained from a small sample and should be taken with some caution, this paper suggests that rental vouchers can have different effects under economic distress, providing income and housing security in times of economic shocks.

The contribution of this paper to the literature is twofold. First, it contributes to the empirical work that evaluates rental voucher programs. The literature, mostly focused on the US, shows 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 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, & Cordes, 2020)). Outside of the US, (Barnhardt, Field, & Pande, 2017) provides experimental evidence on the long term effects of a rental assistance program in the city of Ahmedabad in India. They found that fourteen years later families had better housing but there was no effect on tenure security, family income or human capital, moreover, voucher winners were isolated from family and social networks. It is important to remark that this program was offered only to slum dwellers and it did not offer a voucher to be used freely in the private rental market. Instead, it was more closely related to public housing or project-based Section 8 in the US. Thus, to the best of my knowledge, while many developing countries are moving towards rental policies [2], process that might be furthered accelerated by the current pandemic, this is the first evaluation of a rental voucher program in a developing country.

Secondly, this research contributes to a narrow literature on how housing policy affects the response of low income families to unexpected income shocks. The few available studies 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 effective than rental vouchers to

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[2]Including Argentina, Mexico, Peru, Colombia, Paraguay, Uruguay and Brazil, among others in Latin America.

reduce evictions.[3]

The rest of the paper is organized as follows. The next section introduce the Chilean rental subsidy. Then, Section 3 describes the data and Section 4 explains the empirical strategy in detail. Section 5 shows how the evaluation sample is built and provides evidence on the validity of the assumptions made in the LRRD approach. Then, Section 6 presents the results and Section 7 concludes.

2 The Chilean Rental Subsidy

The Chilean Ministry of Housing and Urbanism (MINVU) launched the rental subsidy in December 2013. Six years later, MINVU has spent $325 million dollars in the assignment of fifty thousand rental vouchers.

The Chilean rental subsidy provides a voucher to rent a unit at the private market. Vouchers can only be used in housing units that do not exceed the national maximum rent standard, $402, and meet certain quality standards, in particular, that are not owned by family members and have at least three separated spaces. Only 30 out of 346 counties in the country, at the very north and south, have a higher maximum rent standard ($475).[4]

There are two types of voucher schemes. First, regular rounds, which assign $6,200 in fixed monthly installments of $180. Voucher holders in regular rounds have eight years to use their subsidy, which is designed to cover at least 30 percent (and maximum 80 percent) of monthly rents. Second, in 2017 MINVU created rounds targeting elder people only. In this elderly rounds a larger subsidy is assigned but for a shorter period of time. More specifically, voucher holders receive $7,780 in monthly installments to cover up to 95 percent of monthly rents i.e. up to $380, for up to two years.[5] In both cases, MINVU collects the copay from voucher recipients and pay the entire monthly rent directly to landlords. Table 1 shows the number of applicants, assigned vouchers and lease up rate in every regular and elderly round of the program.

To apply to the program, families can go online or in person to any of the forty four

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

[4]The amount of the subsidy and rental cap has increased over time. In 2014 these were $110 and $329, respectively.

[5]The total amount and coverage slightly vary across four groups of vulnerability. Less vulnerable voucher holders get a total subsidy of $7,380 to cover up to 90 percent of rent and most vulnerable ones are assigned $7,780 and have 95 percent coverage. Only three percent of voucher recipients are in not in the latter.

PHAs (SERVIUs) across the country. Minimum requirements vary between rounds. While regular rounds target 18 or older-headed families with monthly income between $250 and $900[6] and who have US$180 in a private savings account for homeownership, elderly rounds only require applicants to be 60 or older and have an income above $139. In both rounds, applicants who are in the top 30 percent of the National Vulnerability Index are excluded.[7]

Voucher Assignment

To assign available vouchers, MINVU uses multiple administrative and self reported data to calculate an application score among eligible applicants. Then, applicants are ranked over their score and all families above a certain cutoff, are assigned a voucher. Voucher availability is set by decree for each round before the start of the application process. This is not publicly announced and sometimes may change for administrative or political decisions made by people outside of the Rental Policy team at MINVU.

Applicants get 20 to 180 points in twelve dimensions (thirteen in elderly rounds) that are aggregated into a total application score.[8] There are few differences between elderly and regular rounds.[9] that imply that, compared to a regular round, the same elder applicant can get 60 to 140 additional points in an elderly round.

Starting in 2016 assignments follow a rolling application system. In practice, rounds are opened for two to nine months and MINVU makes one or multiple assignments during this period. After the first assignment, applicants who fall below the cutoff in a previous assignment within the same round are pooled and ranked again with all new applicants for the next assignment.[10] In 2019, MINVU moved from national to regional voucher assignment; while all assignments happen in the same day, applicants are ranked by region, therefore, each assignment date has 16 regional assignments.

The National Social Vulnerability Index was reformed in 2016 and MINVU had to change the formula for one of the components of the application score, the social vul-

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[6]Families larger than three have higher income upper bounds.

[7]This index is used to assign most of public assistance in Chile. The score to build this index is based on survey and administrative data on educational achievement, income, expenses, net worth, health, food security and living arrangements.

[8]Table A1 in the Appendix shows each dimension that is considered to build the application score.

[9]Elderly rounds give additional points for age and do not exclude the applicant to assign family size and number of elderly scores.

[10]In the last assignment, in addition to voucher recipients, all non recipients from the entire round are announced. Reapplication is needed to be considered for the next round.

nerability score (See Table A1 in the Appendix). Before the reform the social vulnerability score was calculated using $(13484 - Index)/100$, where $Index$ was a continuous multidimensional index taking values between 2,000 and 15,000 points. Hence, the social vulnerability score included in MINVU's application score was a continuous number between 0 and 114.12. After the reform, MINVU made the social vulnerability score discrete by giving 180, 135, 90 or 45 points to each of vulnerability group reported in the new National Social Registry (RSH), that classified families in five groups according to their position in a new vulnerability index distribution: below 40th and between 41th-50th, 51-60th, 61-70th, 71-80th, 81-90th and 91-100th percentiles.

This reform was important in that it made the total application score discrete, including only values that are multiples of 5. Moreover, it generated ties at the cutoff and MINVU established a three-step tie-breaking protocol. First, families are sorted again over their family size score and those with higher scores are assigned a voucher. Second, remaining ties are sorted again over their vulnerability score and those with higher scores are assigned a voucher. Third, if there are remaining ties, which happens often, residual available vouchers are assigned randomly. I explain the implications of these issues for the empirical strategy later in the paper.

3   Data

I evaluate the effect of the Chilean rental subsidy using applicants to the program during the period between March 2017 and September 2019. In order to use families that have had the same application requirements, application score formula and assignment rules in the evaluation, applicants prior to 2017 were excluded from the evaluation sample. Also, I restricted to assignments that occurred at least two to three months prior to the first evaluation period in December 2019.[11]

This paper uses a unique data set including administrative, survey and public data for three different periods: baseline, before and after the Covid-19 outbreak.

Baseline Data. I accessed application data that MINVU collects to determine applicants eligibility and vulnerability. In particular, I have socioeconomic and demographic charac-

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[11]Average time to lease is between three and four months.

teristics, location[12], some housing characteristics, application score and each score component. In addition, I have survey data to applicants in regular rounds between March 2017 and October 2019, the relevant period of analysis. This survey, sent before assignments were announced, asked about housing and neighborhood experience and preferences, and beliefs about renting and residential mobility to all applicants. The response rate to this survey was 75 percent or higher.

Before the Covid-19 outbreak. I use outcomes measured in December 2019 that I get from multiple sources. First, unit characteristics, household composition and location was obtained by MINVU from the Social Household Registry (RSH) and their own administrative data. Second, I have information on household application to any homeownership program between January 2011 and December 2019 and, only for regular rounds, data on private savings for homeownership.[13] Finally, I linked all this data to geocoded data from different public sources to characterize neighborhoods: geocoded schools, health care centers, municipalities location (that are commonly located in areas with high economic activity) and poverty, crime and density at the county level.

Using this data I created pre-pandemic outcomes measuring crowding, residential mobility and neighborhood characteristics. Also, application to any homeownership program, and separated by fully and partially funded homeownership policies. For regular rounds only, I can also estimate the effect of the voucher on the extensive (opened account) and intensive margins (balance) of private savings for homeownership.

After the Covid-19 outbreak. To estimate the effect of holding a voucher during the coronavirus pandemic, I partnered MINVU to implement a Follow up survey. Data collection took place between September and November 2020. The questionnaire included outcomes previously studied in the literature (subjective well-being, neighborhood satisfaction and employment), and questions to understand the effect of the voucher on how families were coping with the Covid-19 pandemic during its first eight months.

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[12]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.

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

4 Empirical Strategy

This research exploits the sharp change in the probability of being assigned a voucher at the cutoff c_s to implement a Regression Discontinuity Design (RDD). At each assignment $s \in S$ there is a group of applicants who are ranked over their score $X_{i,s}$ and, if $X_{i,s} \geq c_s$, they are assigned a rental voucher; the cutoff may vary across s since c_s is the value of the score of the applicant who got the last available voucher in assignment s. There is full compliance at each assignment s i.e. those above the cutoff hold a voucher and those below the cutoff cannot have one, unless they reapply to the program and get one in the future.

Figure 1 shows the probability of voucher assignment along the running variable and Figure 2 presents the distribution of the application scores and cutoffs in the pooled data. Given the sharp jump of the probability of treatment at the cutoff and the presence of multiple cutoffs, the research design used in this paper is more accurately described by a Multi-cutoff Sharp Regression Discontinuity Design (Cattaneo, 2018).

As it is commonly done in settings with multiple cutoffs, the running variable is normalized and all data is pooled together around a unique cutoff centered at zero ($c_s = c = 0$). I discuss the implications of having local estimates at different points of the score distribution in section 6.7.

The Regression Discontinuity Design 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). Thus, it is one of the most credible research designs in the absence of experimental treatment assignment. Nonetheless, it is expected to have little external validity for populations away from the cutoff. In section 6.7 I discuss the advantages that a multi-cutoff design may have for external validity.

More formally, let $Y_{i,s}(1)$ and $Y_{i,s}(0)$ be the pair of potential outcomes under treatment and control in each assignment $s \in S$, respectively, $D_{i,s} = D_{i,s}(X_{i,s}) = I(X_{i,s} \geq c) \in \{0,1\}$ is the treatment indicator and $Y_{i,s} = D_{i,s}Y_{i,s}(1) + (1 - D_{i,s})Y_{i,s}(0)$ is the observed outcome for individual i.[14]

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[14]Rubin (1974) and Neyman (1990).

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 and 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

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

However, when the support of X_i is finite and has just a few number of mass points, as it is the case of the application score in the rental subsidy[15], we need an alternative estimation method. More specifically, the continuity assumption in the standard approach fails to provide unbiased coefficients and confidence intervals in the smallest possible window $W_0 = [-c_k, c_k]$ since 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.[16]

In this context, I use the Local Randomization approach to RD analysis (LRRD). First introduced by Cattaneo and Frandsen (2015)[17], 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. In other words, the LRRD makes assumptions on the assignment mechanism near the cutoff instead of using modeling assumptions as in the standard continuity approach (Branson & Mealli, 2018).

In the LRRD, potential outcomes may depend on the score only through treatment indicators inside W_0 and there should not be interference between unit's potential outcomes ("Stable Unit Treatment Value Assumption" or SUTVA).[18] 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

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[15]The application score takes 131 unique values in regular rounds and 109 unique values in elderly rounds.

[16]In practice, in the smallest window possible the continuity approach will consider each mass points as a bin (or observation) and local polynomial methods would extrapolate from the closest mass point on either side to the cutoff. This implies that the confidence interval would have empirical coverage below 95 percent and it is not solved by clustering standard errors by the values of the running variable -which has been a common practice in RDD with discrete running variables - when the number of mass points is small (Lee & Card, 2008) (Kolesár & Rothe, 2018).

[17]See (Branson & Mealli, 2018) for a review of alternative estimation methods in RDD settings.

[18]Since the cutoff in the rental subsidy is a function of the distribution of scores and available voucher, there might be interference between earlier and later treated, while not between ever treated and never treated, which is what I use in this evaluation. That said, SUTVA violation does not invalidate LRRD which could still be estimated under weaker assumptions (Cattaneo & Frandsen, 2015).

data as if it were experimental data is straightforward. In this setting, the causal treatment effect, 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:

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

Due to its stronger assumptions, the Local Randomization approach is not always applicable or may be valid only within a few units in a narrow window around the cutoff. Therefore, this approach uses randomization inference to build confidence intervals and hypothesis tests that are robust in small finite samples.[19]

Similar to the problem of bandwidth selection in the continuity approach, the most important step in the Local Randomization framework is window selection. In this paper I use the data driven procedure developed in (Cattaneo & Frandsen, 2015) to select this window. To implement this procedure I first need to make assumptions about the randomization mechanism, choose relevant pre-treatment covariates, define a minimum number of observations in the smallest window possible, a test statistic and a significance level threshold α^* to test for balance between units at different sides of the cutoff. Once the procedure is implemented, window W_s for assignment s would be the largest window such that the minimum p-value obtained in all balance tests in W_s and any smaller window W_k with $k > s$ around c_s is above the significance threshold.

In this evaluation, balance tests are conducted using sharp null hypothesis tests of no difference in mean on pre-treatment covariates and $\alpha^* = 0.1$.[20] Also, since the sharp discontinuity in the probability of getting the rental voucher at the cutoff is observed at the assignment level, I test for local randomization assumptions at each assignment separately, assuming fixed-margin randomization of treatment assignment inside W_s. Taking all assignments together, the assumed assignment mechanism in this evaluation follows a stratified randomization design: inside each block of applicants, units are randomized holding fixed the number of units assigned to treatment inside the window. For intuition, given the rolling application system, this design can be thought to mimic a sequential stratified

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

[20]To be conservative, p-values from balance tests do not adjust for multiple testing.

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.

Having a discrete running variable simplifies considerably the window selection procedure in that the minimum window is known. If local randomization assumptions hold, they must do it in the window that contains the two mass points that are immediately above and below the cutoff, regardless of the values of the running variable.

Once all W_s are identified, I pooled them together in W_0 to estimate the causal effect of the rental voucher program using a fixed effect model that exploits the variation around of the cutoff by assignment. The next section describes in detail how I use the LRRD to evaluate the Chilean rental subsidy.

5 Implementation of the LRRD to evaluate the Rental Subsidy

This section explains in further detail how the evaluation sample, W_0, is built using the Local Randomization approach and shows the results of different falsification tests to asses the validity of the identification assumptions.

5.1 Evaluation Sample

To build the evaluation sample, W_0, I first expanded the data to have each applicant as many times as she was sorted and assigned to treatment or control groups during the eight rounds of the program in the period between March 2017 and October 2019. In this period, 82 assignments occurred in 21 different moments in time. 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. To simplify exposition, assignments in 2019 pool together all sixteen assignments for each region in the country.[21] Comparing cutoffs and scores between regular and elderly rounds may be misleading since elder applicants receive almost 100 additional points for their age.

The window selection procedure in Cattaneo and Frandsen (2015) requires treated and control units to be in different sides of the cutoff in the minimum window, regardless of the values of the running variable. Since application score is discrete, the minimum

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[21]In practice, what changed with regional assignment was that the announced national list of voucher recipients was now organized by region and score, instead of using score only. Therefore all assignments were announced the same day by MINVU; PHAs do not participate in the assignment process.

window is the one including the two mass points that are immediately above and below the threshold. In the rental subsidy, if there was no mass point at the cutoff, the minimum window is $W_s = [-5, 5]$. However, if there were ties i.e. treated and control units were in the mass point at $c_s = 0$ in $W_s = [0, 0]$, the normalized running variable required some transformation Cattaneo (2018).

I used the assignment rules of the program to transform the running variable: ties in $W_s = [0, 0]$ that are broken with family size or vulnerability score are re-scaled to be in $W_s = [-2, 2]$ and ties that are randomly broken are re-scaled to be in $W_s = [-1, 1]$. Scores away from the cutoff are not modified i.e. the minimum window possible in that case was still $W_s = [-5, 5]$. Importantly, as pointed out in Cattaneo (2018), any transformation that keeps the same order between mass points would produce the same results.

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; on average, applicants participated in 1.2 assignments.[22] Before implementing the data driven window selection procedure, I applied several sample restrictions.

First, Cattaneo and Frandsen (2015) recommends for the minimum window to have at least ten units at each side of the cutoff.[23] When the running variable is discrete, balanced tests are implemented in length-increasing windows by adding one mass point at each side at each time. Therefore, I restricted the sample to the subset of assignments that had at least ten observations in each of the two mass points closest to the cutoff c_s. Doing this implied dropping 34,638 observations from the sample; 59 out of the 63 assignments that were dropped happened in regular rounds in 2019. This was not unexpected since regional assignments reduced the number of observations around the cutoff in each assignment, making it harder to meet this sample size restriction.

Second, in the assignment in October 2018 MINVU added (or automatically re-applied) non voucher recipients from the previous round to the list of applicants to be ranked. This only happened in this elderly round and none of them were selected into treatment. However, given that individuals applying in different periods may be different in unobservable

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[22]Most of those who participated in more than one assignment are from the regular round in 2017.

[23]This recommendation is based on power calculations in a model with discrete outcome and minimum detectable effect of one standard deviation. In the minimum suggested window, under these assumptions and significance levels of 0.05-0.15, a randomization-based test of the sharp null of no treatment effect would have 60-80 percent power.

characteristics, I kept treated and controls that have common support in application date. This excluded 3,562 observations from the sample.

Third, rolling application in the Chilean rental subsidy (Section 2) may create two types of control units in any but the last assignment of each round: never treated and later treated.[24] Using both types of control units affects the interpretation of the treatment effects, which would be the weighted average of two effects: having a voucher (treated vs. never treated) and having a voucher for a longer period of time (treated vs. later treated) (Goodman-Bacon et al., 2018). To precisely estimate these effects separately in a fully interacted model I would need a large sample. However, sample sizes are restrictive in any RD, and even more in the LRRD approach. Thus, I kept only never treated in the control group, dropping 4,300 duplicated observations from the sample.[25] While both effects are interesting, I argue that for the first evaluation of the program, estimating the effect of holding a voucher is the first order question.

Altogether, the sample to test for local randomization assumptions includes 53,410 observations from 42,293 applications to the rental subsidy in the period between April 2017 and October 2019. This sample contains nineteen assignments, fourteen in regular and five in elderly rounds. 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 these assignments. In October 2019, only the assignments from Los Lagos, Araucania and O'Higgins regions are considered and are pooled together. In July 2019 only the assignments in Santiago and Valparaiso are included.[26]

As explained above, the evaluation sample is selected based on balance tests performed in windows of increasing length at each assignment Cattaneo and Frandsen (2015).[27] As it is common in this literature, I divided the set of pre-treatment covariates in two, one for window selection and another for further falsification tests. Window selection included income, savings, distance (km) to the closest PHA, and dummy variables for tenancy, pre-

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[24]Later treated are applicants who were ranked more than once because they did not get the voucher in the first assignment they participated on. Importantly, in the evaluation sample the likelihood of waiting for another assignment can be thought as exogenous.

[25]This did not change the number of unique applicants in the sample since later treated are also in the treatment group.

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

[27]Cattaneo and Frandsen (2015) is accompanied by the package rdwinselect in Stata to implement window selection in LRRD settings.

vious application to homeownership programs, online application, baseline county with poverty rate above the national average (8.6%) or high density. I tried to use the same set of covariates in regular and elderly rounds but there are two variables that are not available in regular rounds, savings and the indicator variable for online applications since elder applicants applied in person during the period of analysis.

Figures A2 and A3 in the Appendix present the results of the window selection procedure and illustrates how the selection occurs. Each graph shows the minimum p-value of all balance tests conducted in windows of different lengths (showed in the X-axis). If the estimated minimum p-value (represented by dots) for a certain window is below the horizontal line at $\alpha^* = 0.1$, that window and any observation in windows further away from the cutoff are considered in violation of the LRRD assumptions. Selected windows in assignment s are therefore those closer to the y-axis that are above the threshold.

I pooled together all observations in windows that are selected by this procedure in the evaluation sample, W_0. The length of the windows across assignments varies between 2 and 30 points, including mass points at 1,2,5,10 and 15 above and below the cutoff. The sample in W_0 includes 3,133 observations that participated in eleven assignments: 1,356 in seven assignments in regular rounds and 1,777 in four assignments in elderly rounds. Columns 8 to 12 in Tables 4 and 5 show summary statistics for the assignments included in the evaluation sample.

Two additional falsification tests to assess the validity of the LRRD assumptions inside the evaluation sample, W_0, are presented below.

5.2 Falsification Tests

In this section I analyze manipulation of the running variable and check whether there are other differences in baseline characteristics between treated and control units that could explain the change in treatment probability at the cutoff. In addition, the last segment of this section examines the validity of LRRD assumption in the sub-sample of follow-up respondents.

5.2.1 Density of the Running Variable

In this section I examine the density of the running variable to check for manipulation in the application score. In practice, manipulation in a small window around the cutoff, if possible, would be very costly for applicants given the complex application score formula, the use of current and past administrative and self reported data, non fixed voucher availability, rolling applications and national or regional level assignments that determine the cutoff at each assignment. In this context, manipulation would require anticipation of voucher availability, and knowledge about the own score and the entire score distribution to anticipate the value of the cutoff. [28] Not surprisingly, Tables 4 and 5 showed no clear relation between the number of participants and available vouchers, or between available vouchers and cutoffs or else.

I follow (Cattaneo, 2018) and use a binomial test to test for manipulation inside the evaluation sample W_0. For intuition, if applicants cannot control precisely their value of the score, the number of observed treated and control units is expected to be consistent with the assignment mechanism assumed inside the window W_0. More formally, I test for the null hypothesis that the probability of success q, or probability of treatment assignment, in the evaluation sample is compatible with the stratified experimental design assumed inside W_0.

The true probability of success is unknown, therefore, I present the results for three different values of q: the observed probability of assignment in a small window around the cutoff ($q1$), the observed treatment probability in the overall sample used in window selection ($q2$) and, as a benchmark, I test the null of complete randomization in the evaluation sample ($q3 = 50\%$). Columns 6 and 7 in Tables 4 and 5 show the values of $q1$ and $q2$ in regular and elderly rounds, respectively.[29]

Column 13 in Table 4 shows that the probability of treatment in the evaluation sample was 27.8 percent in regular rounds. However, looking at individual assignments, except for December 2018, treatment probability fluctuated between 43 and 54 percent. While the exceptionally low value in December 2018 (1.6 percent in column 6) was caused by an

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[28]If anything, applicants could anticipate that the larger the score (unknown for them), the more likely they are to get a voucher.

[29]Column 7 uses the maximum window length observed in the evaluation for regular and elderly rounds, respectively.

administrative decision exogenous to the application process during the year, including this assignment in the density test may suggest the presence of manipulation. Therefore, I present the results of the binomial tests with and without December 2018 data.[30]

Panel B in Table 4 shows the results the test for the three different assumed probabilities of success $q1$, $q2$ and $q3$. Columns 1 to 6 provide evidence including all assignments and columns 7 to 12 do the same analysis leaving December 2018 out of the sample. The results show that the binomial test fails to reject manipulation if the assignment in December 2018 is included. If this data is excluded, then the probability of treatment in the evaluation sample in regular rounds 46.5 percent and it is compatible with the overall probability of treatment $q2 = 42.8\%$ and with complete randomization $q3 = 50\%$ (with 90 percent of confidence).

In elderly rounds, Column 13 in Panel A and Panel B in Table 5 show that the observed average probability of treatment in the evaluation sample was 61.5 percent and that this is compatible with the observed probability of treatment around the cutoff, $q1 = 60.4\%$. Nonetheless, it is not compatible with the observed probability in the overall sample, $q2 = 26.3\%$, or complete randomization, $q3 = 50\%$.

The evidence showed in this section does not suggest that there was sorting around the cutoff in the evaluation sample.

5.2.2 Baseline Characteristics and Balance Tests

In this section I describe applicants in the evaluation sample and analyze balance between units at different sides of the cutoff. This is equivalent to the analysis of discontinuities in baseline covariates in the cannonical RD approach.

In addition to the set of covariates used in balance tests for window selection, this section analyzes age and indicator variables for female; being married/partnered; having neighbors in 400 meters that participated in previous rounds of the program[31]; being poor i.e. having an income below the national poverty line adjusted by family size[32]; and, using

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[30]MINVU decided to spend the annual residual budget on the assignment of 80 new vouchers. To do this, they pooled together and ranked all individuals that had not been assigned a voucher in the second round of 2018. This resulted in 98.4 percent of participants assigned to the control group (Column 6).

[31]400 meters is the average size of a census track in Chile.

[32]In 2017 poverty line adjusted by family size was US$210, $342, $455, $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. Average household income in Chile was US$1,302; US$140, US$400, US$540 and US$655 in the first four income deciles, respectively (CASEN 2017).

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baseline survey for regular rounds only, I include dummy variables for survey response, having strong preferences to stay in the same neighborhood, being satisfied with baseline housing unit and having applied to the voucher program to save for homeownership.

Columns 1 to 6 in Tables 6 show summary statistics of baseline characteristics of applicants in regular rounds in the evaluation sample. Families in regular rounds were mostly headed by young single mothers. Also, the data shows that average income was US$528, one fifth was under the poverty line and three fourth were tenants, paying on average $224 in rent or 46 percent of their household income. Regarding location, forty percent were living in high poverty counties and almost ninety percent of the sample was concentrated in six regions of the country: Santiago, Valparaiso, O'Higgins, and Maule, Biobio, Araucanica, and Los Lagos, all of them situated in the center and south of Chile.[33]

In elderly rounds, table 7 shows that applicants were on average 76 years old, 61 percent were women, 40 percent had a partner, and none of them lived with children. Elderly were much more vulnerable than younger families in the program.[34] Their average income was $239, 55 percent were poor, and 45 were somehow shelter deprived (doubling -up with family members, renting rooms or living in slums). Furthermore, while average rents were similar between rounds, rent burden was roughly 100 percent among tenants in elderly rounds.[35]

Interestingly, the elderly were more concentrated in larger cities (almost half resided in Santiago and Valparaiso regions) and much less likely (about half) to be living in a high poverty county.

Balance Tests

Despite the large set of baseline characteristics, analyzing balance within each assignment can be challenging in that I need covariates that vary between and within assignments in a small window around the cutoff.[36] To be as conservative as possible and be

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[33]Distribution by region is shown in tables A4 and A5.

[34]The elderly are a particularly vulnerable population in Chile; main income source of 1.7 million elder individuals in Chile (out of the 2.8 million counted in the last Census in 2017) is a subsidized pension, which is about $220 per month. That said, differences in economic vulnerability between younger and elderly rounds may be amplified by the design of the program since younger families have higher income requirements and it may be harder for most vulnerable younger families to meet minimum savings standards and provide proof of income, discouraging participation of informal workers, or unemployed head of younger households.

[35]Few observations in elderly rounds in the evaluation sample had rent data. However, this result is consistent with table A5 in section 6.7 that shows that the average burden from rents in elderly rounds (N=8,018) is 95 percent, about twice as high as in regular rounds.

[36]Collinearity between covariates and assignment indicators **??** are expected in this setting for a couple of

able to use a larger number of baseline covariates I use the following two fixed effect models to test for balance in the evaluation sample:

$$X_{i,s} = \alpha + \tau_s D_{i,s} + \gamma_s Assignment_s + \beta_s D_{i,s} * Assignment_s + \delta S_{i,s} + \epsilon_{i,s} \qquad (5.1)$$

$$X_{i,s} = \alpha + \tau D_{i,s} + \gamma_s Assignment_s + \delta S_{i,s} + \epsilon_{i,s} \qquad (5.2)$$

Where $X_{i,s}$ is the vector of baseline covariates, γ_s are assignment fixed effects, and $S_{i,s}$ includes score components that determine tie-breaking rules.[37]

In the fully interacted model (equation 5.1) coefficients τ_s and β_s are the treatment effects by assignment that I use to test the null of no effect within each assignment or strata (H_0). Then, τ in equation 5.2 is the FE coefficient that I use to test the weaker but rather commonly used null hypothesis that the weighted average effect in all assignments is zero (H_0'). This alternative null hypotheses is weaker in that the FE coefficient could be zero if an specific linear combination of the effects in each assignment is zero, while H_0 is false (Young (2019); Firpo, Foguel, and Jales (2020)).

Tables 6 and 7 present the results of the more conservative balance tests under H_0 for covariates that vary between and within assignments and H_0' for those that do not vary within some assignments. Columns 7 to 10 show p-values for joint tests of the null of zero effect in each assignment, H_0, using large-sample based inference (F-test) and Fisherian randomization inference (Randomization-t exact test (?)).[38] I present the results of these

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reasons. First, in the LRRD approach the selected window only contains observations that are very close to the cutoff and were already balanced in the first set of covariates, therefore, are likely to share other characteristics. Second, in a multi cutoff settings, those very close to a higher and lower cutoff may be different, highlighting the relevance of testing balance in each assignment separately. For instance, the fraction of poor families, female, spouse, available geocoded location and county poverty rate vary across but not necessarily within assignments. Furthermore, they all have children. In elderly rounds, all households were in the bottom 40 percent of the vulnerability index (RSH) and had no children. Also, available geocoded location, county poverty rate, tenancy and having a neighbor that previously participated in the program did not vary between treated and controls in every assignment. Third, some variables are not available for the entire period of analysis (rent is available only after September 2018) or would not vary because of the design of the program (regional assignments in 2019 eliminated variation by region within assignments).

[37]In regular rounds it includes dummy variables for family size and social vulnerability score that were more likely to generate ties (40 and 180, respectively). Together they explained 70% of the total score in these rounds. In elderly rounds, on the other hand, these scores did not vary across units, they all had 40 and 180 points. Therefore, I use dummy variables for scores that are more likely to break ties: number of elderly and age range scores, that represent 44 percent of their scores.

[38]I use the package randcmd to calculate randomization inference p-values in Stata. In particular, 1000 iterations are calculated re-randomizing the data by assignment each time, following the assumed stratified experimental design.

tests with and without controls. Columns 11 to 16 show FE coefficients and p-values for the weaker balance test (H_0') with and without controls. In addition, the bottom panel presents two test of joint significance. First, the F-test of joint significance from regressing the treatment indicator on all covariates[39]. Second, to account for multiple testing problems, I present the Westfall-Young multiple-testing test of overall treatment irrelevance.

The results in Table 6 show that under the more conservative test and using randomization inference, the evaluation sample is balanced in all covariates in regular rounds. The weaker test in columns 11 to 16, however, show imbalance in age (treated are 1.6 years younger) but in a multi-cutoff setting this may be explain by heterogeneity across assignments with different cutoffs.[40] Furthermore, the bottom panel shows that joint insignificance of all covariates and overall treatment insignificance are not rejected in favor of balance.

In elderly rounds, results in Table 7 show that all covariates are balanced under both H_0 and H_0' except for residing in Santiago, which is significant at the 90 percent of confidence under the weaker null hypothesis, H_0'. However, overall experimental insignificance and joint insignificance tests suggest that treated and controls were not different in these characteristics.

Finally, Figures A2 and A3 show graphical analysis of the difference in mean in pre-treatment covariate between treated and controls. Consistent with the results from the window selection procedure and FE estimates presented in this section, there are no large differences in baseline characteristics between units at different sides of the cutoff.

Before presenting the results of the evaluation I examine selective attrition in the follow-up survey and analyze balance in the subset of observations that responded the survey.

5.2.3   Selective Attrition and Balance in the Follow up Sample

To estimate the effect of holding a rental voucher during the pandemic I use the sample of individuals who responded the follow up survey. Therefore, in this section I analyze the extent to which LRRD assumptions are valid in this sub-sample and how the results should be interpreted.

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[39]I used variables included in fully interacted balance tests to use the entire sample.

[40]This may also explain the differences between OLS and Randomization-t p-values since these tend to differ in large variance settings or under the presence of outliers.

First, I analyze the presence of selective attrition. 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.[41]

Second, 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.[42]

Attrition

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.[43] In the evaluation sample, the response rate was 59.5 percent (706) and 27 percent (150) in regular and elderly rounds, respectively.[44] These rates of response are high for online surveys.

Figures 6a and 6b show response rates by assignment and treatment group. Except for

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[41]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.

[42]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.

[43]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.

[44]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.

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.

$$Y_i = \alpha + \gamma_s Assignment_s + \tau_s D_{i,s} + \beta_s D_{i,s} x Assignment_s + \delta Z_i + \epsilon_{i,s} \qquad (5.3)$$

This equation is similar to the fully interacted FE model in equation **??** used to analyze balance in section 5.2.2. 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 8 and 9 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 8 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 6b 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 9 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

Even in the absence of selective attrition, the strong assumptions made in the LRRD may not hold in this subset of observations, therefore, we analyze balance in the follow-up sample.

Tables 10 and 11 replicate the balance analysis presented in section 5.2.2 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 10 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 significance and the Westfall-Yang test of overall treatment relevance do not provide evidence of imbalance between treatment and control groups. Table 11 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 affected 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 5.2.3 need to be taken with some caution.

6   Results

This section presents the results of the evaluation. I use the following reduced form equation to estimate the effects of the rental voucher program

$$Y_{i,s} = \alpha + \tau Voucher_{i,s} + \gamma_s Assignment_s + \beta Z_{i,s} + \epsilon_{i,s} \tag{6.1}$$

where $Y_{i,s}$ is the vector of observed outcomes of applicant i assigned to treatment or control in assignment s. There is no subscript for rounds because each assignment is unique to a round. Moreover, I estimate equation 6.1 separately for regular and elderly rounds. $Voucher$ is an indicator variable for having an application score above the cutoff, $X_{i,s} > 0$, γ_s are assignment fixed effects, and the vector $Z_{i,s}$ includes baseline covariates and score components involved in tie breaking rules used in balanced tests in section 5.2.2.

Pre-pandemic outcomes in $Y_{i,s}$ use administrative data and include crowding, residential mobility, neighborhood characteristics, savings for ownership and application to homeownership programs. Post-pandemic outcomes use the follow-up survey and consider crowding, residential mobility, neighborhood characteristics, as well as tenancy, rent burden, unit characteristics, housing and neighborhood satisfaction, employment, health, and different possible strategies to respond to the economic hardship that came with the

pandemic.

The parameter of interest, τ, is the normalized and pooled RDD estimate of the effect of being assigned a voucher or Intention to Treatment Effect (ITT). The fixed effect coefficient τ in equation 6.1 recovers a double average: it is the weighted average across assignments 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 duration of treatment in the evaluation sample. Hence, while having multiple cutoffs, this section only reports the normalized and pooled RDD estimate, τ, aggregating families with different vulnerability that have had their vouchers for different amounts of time.[45]

Given the small sample, the analysis here is just focused on ITTs and does not show IV estimates of the Local Average Treatment Effects (LATE) of using the rental voucher. Nonetheless, I argue that ITTs are relevant from a policy perspective because lease-up cannot be enforced. Moreover, since there is full compliance in the use of the voucher in the control group, ITTs are also informative of the average treatment effect on the treated, which is τ adjusted by compliance rates in the treated group (Angrist & Pischke, 2008). Table 1 shows that compliance or lease up rates in the Chilean rental subsidy have been as low as 42 and 50 percent in regular and elderly rounds, respectively. In the evaluation sample,lease up rate was 30 in younger and 50 percent in elderly rounds.

The results of the evaluation are presented by period of analysis, before and during the pandemic. In addition, in each of these periods, the results are organized in broader topics. Before the pandemic, the results are grouped in i) housing conditions, residential mobility and neighborhood characteristics and ii) homeownership. Then, during the pandemic, the results are classified in i) residential mobility, housing and household characteristics; ii) neighborhood characteristics, housing and neighborhood satisfaction; and iii) employment, income, health and household response during the Covid-19 crisis. This section concludes with a discussion on the external validity of the results of the evaluation.

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[45]To avoid collinearity between region characteristics and assignment FE, $Z_{i,s}$ includes county level variables to control for rental market characteristics.

6.1 December 2019: Before the Coronavirus Pandemic

Tables 12 and 13 present the results for regular and elderly rounds, respectively. Each table includes descriptive statistics of the relevant counterfactual and the treatment effects using two different specifications of equation 6.1. Specification 1 includes score components to control for tie-breaking rules and specification 2 incorporates score components and baseline covariates used in section 5.2.2. Given the small number of observations by assignment (strata) I report large-sample based inference (OLS p-values) and Fisherian randomization inference (Randomization-t p-values from Young (2019)). Finally, since I use multiple outcomes the bottom panel in these tables shows the Westfall-Young multiple-testing test of overall treatment irrelevance considering all outcomes together.

Consisitent with balance analysis in section5.2.2, including baseline covariates had little impact on the coefficients but showed some gains in efficiency. Therefore, unless otherwise specified, this section describes estimates from specification 2 (columns 8 to 11).

6.1.1 Housing conditions, residential mobility and neighborhood characteristics

Panel A in Tables 12 and 13 shows the effect of the voucher on housing-related outcomes. Column 2 shows that overcrowding in the control group in regular rounds was 11.4 percent, almost four times higher than in elderly rounds, 3.1 percent.[46]

Holding a voucher reduced overcrowding in 4.1 percentage points (pp.) in regular rounds and 2 pp. in elderly rounds. Importantly, while the reduction in elderly rounds can be associated to voucher holders living in smaller households with more available bedrooms, younger families did not reduce their household size but were living in units with a larger number of bedrooms.

The rental policy affected residential mobility in a rather immobile population. Column 2 in Panel B shows that 55 percent of younger families and 68 percent of the elderly were living in the same unit since application. Moreover, about 30 percent of movers stayed one kilometer or closer to their initial location.

The voucher had different effects in regular and elderly rounds. While it did not in-

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[46]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).

crease residential mobility among younger families, it did change the location chosen within 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 increased residential mobility in 24 percentage points yet there was not a significant effect of the voucher on distance.

Panel C shows the effects of the voucher on neighborhood characteristics. To do this I created multiple outcomes using public data. 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/or two kilometers. Second, neighborhood school quality is measured by average standardized tests scores and the fraction of private, public and subsidized schools. 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 neighborhood 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.

Tables 12 and 13 show that the rental policy did not significantly improve neighborhood characteristics in regular or elderly rounds. If anything, the few marginally significant coefficients suggests that younger families moved farther away from schools and pre-schools and elderly were located closer to municipalities and health care centers.

Overall, results before the pandemic were similar to those found in previous literature focused on the Section 8 program: holding a voucher seemed to improve housing conditions but it did little to provide better neighborhoods for low income families.

6.2 Homeownership

Panel D in Table 12 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. Interestingly, most applicants kept their savings account opened and kept enough savings to apply to the fully funded homeownership policy (DS 49) (Column 2).

For elderly rounds, on the other hand, the results in Table 13 show that the rental pol-

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icy increased application to homeownership programs in 4.5 percentage points, mostly through higher (3.4 pp.) applications to the fully funded homeownership program (DS 49).

This is an interesting in that rents are almost fully subsidized for voucher recipients in elderly rounds and, according to this evaluation, the rental policy would allow them to stay closer to their initial location. On the other hand, fully subsidized homeownership are normally located at the periphery (Navarrete & Navarrete, 2016). Future research could look deeper into the mechanism behind this puzzling results.

Next, I present the results of the rental voucher on outcomes measured during the pandemic.

6.3 September -November 2020: After the Coronavirus Outbreak

Tables 14 and 15 presents the results of equation 6.1 using outcomes from the follow-up survey. In section 5.2.3 I provided evidence against the presence of selective attrition and showed the LRRD assumptions were still valid in the follow up sample. Hence, while acknowledging the small sample sizes, specially in elderly rounds, estimates in this section are interpreted as causal effects of holding a voucher during the pandemic.

Results are grouped in i) residential mobility, housing and household characteristics; ii) neighborhood characteristics, housing and neighborhood satisfaction; and iii) employment, income, health and household response during the Covid-19 crisis. Given the small number of individuals that responded the survey in elderly rounds, this section presents the results for regular and elderly rounds separately. Moreover, I describe findings for younger families in further detail and use the results in elderly rounds mostly to highlight differences between round types.

6.4 Residential mobility, housing conditions and household characteristics

Regular Rounds

Panel A in Table 14 shows that eight months into the pandemic, 85 percent of the control group was renting and the voucher had no significant effect on this outcome. The policy affected other important housing margins, nonetheless.

First, in this highly informal rental market[47] voucher recipients were 11 percentage points more likely to have signed a lease. Second, rent burden was reduced in 10 pp; the voucher reduced monthly out of the pocket rent payments in about US\$50, while average rent amount was not different between treated and controls (US\$260). Furthermore, there is some evidence suggesting that voucher holders were not living in housing units of lower quality eight months into the pandemic. In particular, the treated were living in less crowded homes and were more likely to have an independent room for the kitchen. Also, compared to the control group, the fraction of families that had a heat system and a computer at home was about 9 pp. higher among voucher holders. Other housing expenses like cable TV, smart phone or Wifi were not statistically different between these groups.

Third, Panel B in Table 14 shows a positive effect of the voucher in housing stability during the pandemic. In particular, voucher holders were 6.5 pp. more likely to move before March 2020 and 4 pp. less likely to declare that they were forced to move out as a result of the Covid-19 pandemic (Panel D). This last effect was significant at the 90 percent of confidence.

These results suggests that while the rental policy contributed to avoid unwanted mobility during the first semester of the crisis[48], it did not have a significant effect on shelter deprivation[49]; Panel B in Table 14 shows that 11 percent of the control group was shelter deprived and there was no statistical difference between treatment and control groups. Importantly, this could change as the national eviction moratory, in place since May 2020, is lifted in December 2021.[50]

Elderly Rounds

Two results in table 15 are highlighted here. First, the reduction in the amount of rent paid out of the pocket in elderly rounds was \$106, more than twice as much as in regular rounds. Moreover, rent burden was 30 pp. lower among the treated, three times the effect

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[47]Baseline survey shows that 35 percent of those who are tenants at baseline do not have a rental lease.

[48]Positive effects of rental vouchers in housing stability (in normal times) have been previously documented in the US (Mills et al., 2006).

[49]In developing countries shelter deprivation takes the form of homelessness, living in crowded rooms, doubling up with other family members and living in informal settlements. Indeed, since the pandemic started, the Chilean government has manifested concern about the increased in the number of families living in slums and on the streets.

[50]Looking at baseline data, shelter deprivation during the pandemic is four times higher than during application.

found in regular rounds. This is an interesting result in that the voucher in elderly rounds is about two times the voucher in regular rounds. Future work could explore the elasticity of these effects to different voucher generosity.

Second, in elderly rounds the voucher did cause an important reduction on shelter deprivation of 12 pp. (p-value 0.064). Altogether, the evidence suggests that the rental policy can provide housing stability by reducing unwanted mobility in regular rounds and shelter deprivation in elderly rounds.

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 around the house) of applicants to the Chilean rental subsidy. Similar to the results using administrative data before the pandemic (Panel B in table 12), column 3 in Panel F in table 14 suggests that the subsidy did not change the surrounding of younger voucher holders. More specifically, voucher holders did not have better access to childcare, schools, transportation, parks, primary care centers, family members, friends or jobs than the control group.

In addition, during the pandemic, safety perceptions and housing and neighborhoods satisfaction did not vary between treated and controls. However, when asked about their neighbors, younger voucher holders were less likely to be willing to ask them for childcare assistance in case of need. This may be explained by the mobility farther away observed in pre-pandemic results, but more research is needed to understand whether this result reflects isolation or else.

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 14 suggests that the voucher generated a reduction in their exposure to prostitution, destroyed property and graffiti. Importantly, estimated effects in 2019 showed that the elderly were more likely to be closer to a municipality, therefore, the

observed result could be explained by differences in lock-down's enforcement by neighborhoods in which treated and control lived, rather than changes in long-term neighborhood characteristics. Moreover, Panel F in table 14 shows non-positive effects of the policy on perceived safety.

In addition, elderly voucher holders were more likely to declare they would ask for economic help from their neighbors in case of need. This is also consistent with the results before the pandemic that suggested that the elderly in the treatment group were more likely to stay closer to their initial location.

Next, I discuss the effect of the rental policy on economic outcomes during the pandemic. This is last set of results of this evaluation.

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

Regular Rounds

Column 2 in Panel C and D in table 14 provides evidence on the size of the economic shock for low income families in Chile. The Covid-19 pandemic have generated partial or total income loss in roughly 77 percent of the control group. Furthermore, in the same group, the pandemic-related unemployment was 16 percent, which was mostly caused by contract suspensions by employers or independent workers that cannot go out to work during strict quarantines.

The rental policy did not have an effect on these outcomes. Moreover, Panel C shows the the voucher did not affect income and employment in general during the first eight months of the Covid-19 pandemic. This result is different from previous literature showing negative effects of rental policies on employment in the US (Jacob & Ludwig, 2012). However, whether they differ because of the pandemic, the smaller voucher for working families or other differences between Section 8 and the Chilean rental subsidy are interesting research questions for future work.[51]

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[51]In addition to be smaller, application rules in Chile make it easier for formal workers to apply, therefore, the large fraction of formal workers in the program; baseline survey shows that 85 percent of all applicants were employees at the time of application.

The data suggests that the voucher did affect other important margins during the pandemic. First, while only significant at the 90 percent, the rental policy reduced perceived debt overload in 8.7 pp.; the fraction of families declaring being overwhelmed about their debts decreased from 68 to 59 percent.

Second, the voucher affected how families were dealing with the consequences of the large economic shock during the pandemic. The survey asked about fifteen different strategies families may have implemented in response to the economic the crisis. Column 2 in Panel D of Table 14 shows that 94 percent of both the treatment and control group had turned to some strategy to adapt to the new economic circumstances.

This analysis show that the voucher did not prevent families from reducing food budget, using some government relief benefit, spending household savings or cutting utility bills in trying to overcome the unexpected economic shock that came with Covid-19, which were the most common ways in which families adapted in the first eight month of the pandemic. However, voucher holders were less likely to have done new activities to complement family income (9 pp.), lent or gave money to other family members (5 pp.), moved out of their houses (4 pp.) and missed rent payments (12 pp.).

Finally, I analyze the effects of the voucher on health related outcomes. Treated and controls were not different in overall health self evaluation, or exposure to the virus; both were living in neighborhoods where the chances of having a neighbor that had the virus (and knowing this information) were about 20 percent. 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. While not statistically significant, it is worth noting that a smaller fraction of voucher recipients reported to feel depressed (-6.5 pp, p-value 0.109) but they had higher anxiety results in the PHQ4 test (8.2 pp., p-value 0.117). These results open interesting future research questions about the link between rental vouchers and mental health during a crisis.

Elderly Rounds

Panel F in table 15 shows that in addition to reducing food budget, cutting utility bills and using government relief benefits, in this more vulnerable population it was more com-

mon to reduce health expenses than to use family savings, which they might not have. Moreover, elderly voucher holders were more likely (16.2 pp.) to ask for formal credits, for which having a rental lease may be useful. As in regular rounds, voucher holders were also less likely (12.6 pp.) to miss rent payments during the first semester of the pandemic.

Altogether, this evidence suggests that the rental policy may have contributed to provide income and housing security during the first eight month of the Covid-19 pandemic.

6.7 External Validity

The Regression Discontinuity Design is one of the most credible research designs in the absence of experimental treatment assignment. Nonetheless, it provides local treatment effects that have little external validity for populations away from the cutoff. When there are multiple cutoff, however, the heterogeneity introduced by averaging local estimates at different values of the running variable distribution in the pooled estimate of the treatment effect, τ in equation 6.1, has the potential to provide a richer estimand (Cattaneo, Keele, & Vazquez-bare, 2016). Whether this is the case depends on the data and the probability of observing the cutoffs included in τ.

In this section I provide descriptive evidence to analyze the extent to which the results of this evaluation can be extrapolated to a wider population of voucher recipients. To do this, I compare summary statistics for all applicants in the period between April 2017 and September 2019 (table A4 and A5 in the Appendix) to summary statistics in the evaluation sample (tables 6 and 7 in section 5.2.2).

Table A4 shows that similar to the evaluation sample, voucher recipients in regular rounds were mostly young single mothers: 83 percent were women, the average age was 34 (10 sd.), 26 percent had a partner and 94 percent had children in school age. Average family income of voucher recipients was US$568 and 32 percent was under the poverty line (adjusted by family size), however, it varied across cutoffs between 17 and 48 percent. While table 6 shows a lower average poverty rate in the evaluation sample, it included a lot of variation in socioeconomic conditions: poor families were as low as 0 percent at the lowest cutoff (240) and as high as 30 percent at the highest cutoff (355) in the evaluation sample. There is no difference in rent burden between the evaluation sample and the population of voucher recipients, in both cases families spent 46 percent of their in-

come in rents. Other housing characteristics are also similar: 70 (74) percent of all voucher recipients (evaluation sample) were tenants at baseline paying $245 ($224) on average in monthly rent. Also, there were no differences in application to homeownership programs at baseline, which was about 14 percent.

A significant difference between the population of voucher recipients and the evaluation sample was their location at baseline. Eighty percent of voucher recipients were concentrated in seven regions: Santiago (23 p.), Biobio (14 p.), Valparaiso (12 p.), Araucanica (9.5 p.), Maule (7.5 p.), Los Lagos (7 p.) and O'Higgins (6.5 p.). These are all in the center and south of the country. In the evaluation sample these same six regions concentrated 87.5 percent of the observations, however, the distribution was different. The fraction of families living in Araucania, the poorest region in the country, represented 26 percent of the sample. Accordingly, while families living in high density counties was similar in these two samples (43 vs. 40 percent), the proportion of families living in counties with high poverty rate in the evaluation sample was almost three times the rate among all voucher recipients (61 vs. 22 percent).

Nevertheless, despite living in poorer surroundings the proportion of families that declared that they would like to stay in the same neighborhood if they got a voucher is the same (about 55 percent). Also, satisfaction with current unit is higher in the evaluation sample than in the population of voucher recipients (65 vs. 58 percent).

Column 4 in table A5 shows that in elderly rounds the average voucher recipient was 75 years old (7 sd), 55 percent were women, 37 percent had a partner, and only 8 percent lived with children. In addition, elderly voucher recipients were more concentrated in larger cities[52], 54 percent of voucher recipients were renting at baseline, average rent was $193 and they were normally renting alone.

Main observed differences between the population of voucher recipients and the evaluation sample in elderly rounds include a larger proportion of women (61 vs. 55 percent), lower rent burden (88 vs 100 percent), fewer families with children (8 vs. 0 percent), and more households living in high poverty counties (17 vs. 35 percent).

The evidence provided in this section shows that although I use a Regression Discontinuity design, which are known for the lack of external validity, having a multicutoff RD

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[52]Almost half resided in Santiago and Valparaiso regions and 46 percent lived in high density counties at baseline.

introduced heterogeneity in the evaluation sample that made it more similar to the population of voucher recipients. However, this evaluation represents better the potential results of families living in poorer counties in the center and south of Chile.

7 Conclusion

research focuses on two relevant questions. What is the effect of rental vouchers on housing and neighborhood quality for low income families in developing countries? And what are the effects of rental vouchers during unexpected large income shocks for its recipients?

To answer these questions I provided evidence on the Chilean rental subsidy, the first rental voucher program implemented in Latin America, and 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's relatively new policy an interesting case study.

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

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 voucher program decreased unwanted mobility and rent burden and also affected how families coped with large unexpected income shocks. 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. This fact could be particularly relevant for developing countries where high levels of informality and social inequalities not only make unexpected income variations more likely, and their potential negative effects larger, but also undermine the effectiveness of government response during a crisis.

For instance, Chile announced 100,000 three-months emergency fixed rental vouchers of \$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 \$800. After extending the application period twice due to the low number of applicants, MINVU received 62,023 applications and assigned 43,128 vouchers overall. According to official sources at MINVU, these results were explained by lack of information[53] and high perceived costs of application. In particular, there was confusion about eligibility criteria, rules about where and how to apply[54] and how to meet the requirements.[55] Moreover, it exposed the lack of digital infrastructure inside MINVU to manage this large online application process and to respond to the high demand for information that followed the announcement of the emergency rental subsidy.

Further research is needed to compare the effects of emergency versus long-term rental voucher programs in periods of crisis. However, the evidence provided here suggests that in developing countries like Chile, larger 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 distress.[56]

Finally, 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. This research suggest that rental subsidies may have an important role in the historic struggle in Latin American countries to eradicate informal settlements and equally important, to avoid large setbacks in this fight under periods of economic crisis. Further understanding of the effect of the rental voucher on homelessness and informal settlements in developing countries would be an interesting future line of work.

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

[54]PHAs were closed across the country and Wifi is not broadly available across the country.

[55]Since it asked for a copy of current rental lease in a highly informal market, families could send a letter from their landlords acknowledging the rental agreement.

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

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8   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.

Figure 1: Sharp RD Design
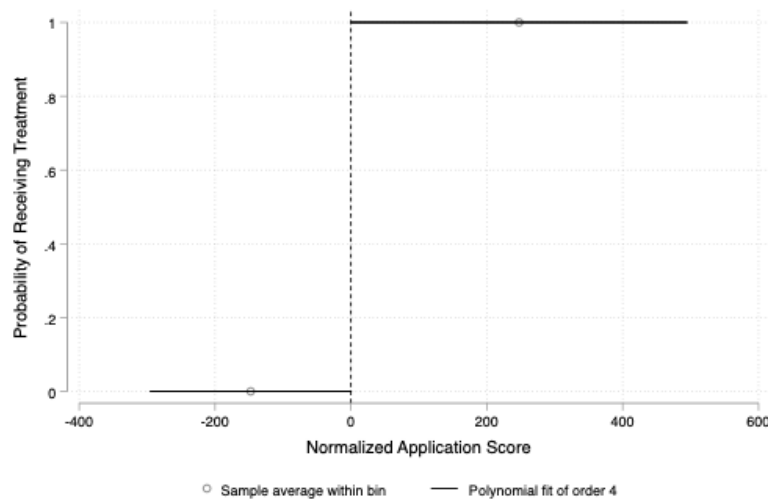
Note: This figure presents treatment probability for different values of the normalized application score.

Figure 2: Multiple Cutoff Regression Discontinuity Design
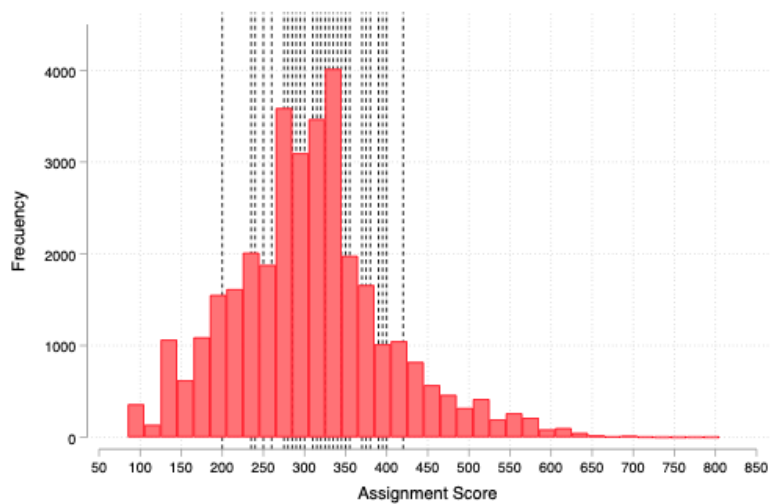
(a) Regular Rounds
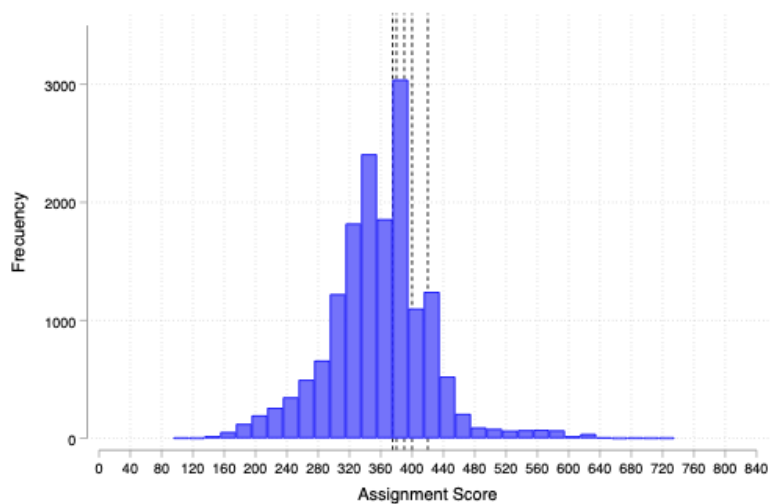
(b) Elderly Rounds

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.

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

Panel A: Window Selection

Assignment Date	N	Min Xi	Max Xi	Vouchers	Cutoff	P(T=1)	P(T=1,w15)	N	Min Xi	Max Xi	Vouchers	Cutoff	P(T=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(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

Panel B: Binomial Test for Manipulation of the Running Variable

| | All | | | | | | All but Dec 2018 | | | | | |
Assumed q	qi	N	Obs T	Exp T	Obs q	p-value	qi	N	Obs T	Exp T	Obs q	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
q1	0.488	1,356	377	661	0.278	0.000	0.551	639	297	352	0.465	0.000
q2	0.372	1,356	377	505	0.278	0.000	0.428	639	297	273	0.465	0.060
q3	0.500	1,356	377	678	0.278	0.000	0.500	639	297	320	0.465	0.082

Note: The top panel of this table shows descriptive statistics for each assignment 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. 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 $W = [-15, 15]$. The bottom panel 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.

Table 5: Assignments in Elderly Rounds for Window Selection

Panel A: Window Selection

Assignment Date	N	Min Xi	Max Xi	Vouchers	Cutoff	P(T=1)	P(T=1,w10)	N	Min Xi	Max Xi	Vouchers	Cutoff	P(T=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(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

Panel B: Binomial Test for Manipulation of the Running Variable

| | All | | | | | |
Assumed q	qi	N	Obs T	Exp T	Obs q	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
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 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: Interaction Terms (H0)				Balance Test: No Interaction Terms (H0')					
	Pooled	Control		Treated			Joint Balance Test (1)		Joint Balance Test (2)		Individual FE Test (1)			Individual FE Test (2)		
	Mean	Mean	SD	Mean	SD	N	F-Test (p)	Rand-t (p)	F-Test (p)	Rand-t (p)	Coeff	t-test (p)	Rant-t (p)	Coeff	t-test (p)	Rant-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Tenant in baseline	0.74	0.73	0.44	0.75	0.43	1,356	0.720	0.636	0.744	0.584	0.026	0.360	0.371	0.002	0.957	0.957
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	-45.380	0.238	0.228	-29.476	0.446	0.430
Family income (US)	528.15	526.71	190.68	531.89	192.65	1,356	0.715	0.276	0.304	0.281	18.264	0.153	0.161	-9.378	0.401	0.409
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	0.033	0.214	0.225	0.014	0.605	0.600
Online application	0.38	0.39	0.49	0.37	0.48	1,356	0.539	0.555	0.501	0.572	-0.000	0.989	0.955	0.016	0.599	0.588
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	0.024	0.297	0.293	0.022	0.349	0.350
KM to closest PHA	18.22	17.26	22.69	20.75	26.75	1,233	0.615	0.873	0.689	0.851	0.666	0.684	0.720	0.458	0.777	0.801
High density county	0.40	0.41	0.49	0.37	0.48	1,356	0.913	0.614	0.819	0.602	-0.034	0.278	0.276	-0.045	0.161	0.165
Age at application	31.09	31.49	7.06	30.04	6.82	1,356	0.000***	0.528	0.000***	0.524	-0.947	0.032**	0.033**	-1.426	0.001***	0.001***
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	-0.024	0.554	0.566	-0.040	0.325	0.345
Satisfaction with housing unit	0.65	0.65	0.48	0.64	0.48	899	0.463	0.613	0.456	0.605	-0.008	0.832	0.841	-0.012	0.756	0.774
Applied to save for ownership	0.27	0.28	0.45	0.24	0.43	819	0.415	0.221	0.362	0.196	-0.042	0.255	0.279	-0.055	0.132	0.134
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	-0.003	0.917	0.909	-0.001	0.971	0.970
Answered Baseline Survey	0.74	0.74	0.44	0.72	0.45	1,356	0.232	0.217	0.319	0.210	-0.012	0.685	0.666	-0.013	0.660	0.635
Female	0.91	0.91	0.29	0.92	0.27	1,065					-0.049	0.063*	0.085*	-0.049	0.248	0.260
Spouse/partner	0.13	0.13	0.34	0.11	0.31	1,110					0.019	0.496	0.479	0.019	0.574	0.583
Rent (US)	224.47	224.59	109.70	224.08	116.50	810					1.437	0.895	0.901	1.437	0.800	0.801
Rent burden	0.46	0.47	0.26	0.46	0.25	810					-0.009	0.707	0.711	-0.009	0.546	0.545
Geocoded location	0.91	0.91	0.28	0.90	0.30	1,287					-0.028	0.171	0.186	-0.028	0.270	0.294
County above national poverty rate	0.61	0.58	0.49	0.70	0.46	1,094					0.040	0.290	0.270	0.040	0.353	0.357
Santiago MSA	0.16	0.17	0.38	0.12	0.32	963					-0.043	0.226	0.232	-0.043	0.289	0.296
Assignment FE							Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Score components FE							No	No	Yes	Yes	No	No	No	Yes	Yes	Yes
Assignment-Treat Interacted Terms							Yes	Yes	Yes	Yes	No	No	No	No	No	No

	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 5.2.2 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: Interaction Terms (H0) | | | | Balance Test: No Interaction Terms (H0') | | | | | |
		Control		Treated			Joint Balance Test (1)		Joint Balance Test (2)		Individual FE Test (1)			Individual FE Test (2)		
	Pooled Mean	Mean	SD	Mean	SD	N	F-Test (p)	Rand-t (p)	F-Test (p)	Rand-t (p)	Coeff	t-test (p)	Rand-t (p)	Coeff	t-test (p)	Rant-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Family income (US)	239.44	235.37	97.23	241.99	106.30	1,777	0.667	0.744	0.643	0.903	4.693	0.378	0.368	5.394	0.307	0.296
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	-0.027	0.303	0.292	-0.025	0.346	0.337
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	0.003	0.811	0.753	0.004	0.762	0.715
KM to closest PHA	12.61	12.69	18.06	12.56	19.12	1,681	0.381	0.171	0.421	0.184	-0.679	0.513	0.508	-0.659	0.526	0.518
High density county	0.51	0.52	0.50	0.51	0.50	1,777	0.855	0.538	0.853	0.409	-0.028	0.288	0.290	-0.029	0.268	0.268
Female	0.61	0.61	0.49	0.61	0.49	1,777	0.617	0.704	0.686	0.530	0.011	0.670	0.695	0.010	0.693	0.704
Spouse/partner	0.40	0.39	0.49	0.41	0.49	1,777	0.914	0.775	0.828	0.695	-0.004	0.886	0.873	-0.002	0.952	0.945
Age at application	75.62	74.66	6.30	76.22	6.85	1,777	0.130	0.939	0.114	0.769	-0.152	0.621	0.659	-0.128	0.675	0.704
Any neighbor in 400m previously applied	0.71	0.73	0.44	0.69	0.46	1,285					-0.000	0.993	0.996	-0.000	0.979	0.972
Rent (US)	223.41	220.73	97.21	228.46	176.06	173					22.743	0.252	0.286	22.743	0.312	0.357
Rent burden	1.04	1.01	0.53	1.09	0.94	173					0.109	0.360	0.412	0.109	0.334	0.382
Tenant in baseline	0.55	0.58	0.49	0.53	0.50	1,654					0.014	0.605	0.605	0.014	0.558	0.558
Geocoded location	0.95	0.95	0.21	0.94	0.24	1,687					-0.007	0.558	0.565	-0.007	0.547	0.544
County above national poverty rate	0.35	0.35	0.48	0.34	0.47	1,777					-0.017	0.496	0.474	-0.017	0.494	0.491
Santiago MSA	0.29	0.25	0.44	0.31	0.46	1,564					-0.045	0.073*	0.088*	-0.045	0.071*	0.084*
Assignment FE							Yes		Yes			Yes	Yes		Yes	Yes
Score components FE							No		Yes			No	No		Yes	Yes
Assignment-Treat Interacted Terms							Yes		Yes			No	No		No	No

| | | | 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..

Figure 3: Balance in Baseline Characteristics in Regular Rounds
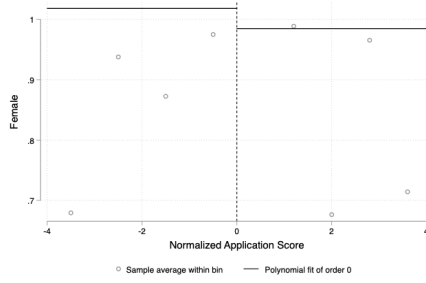
(a) Female
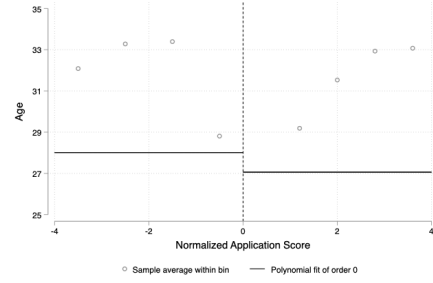
(b) Age
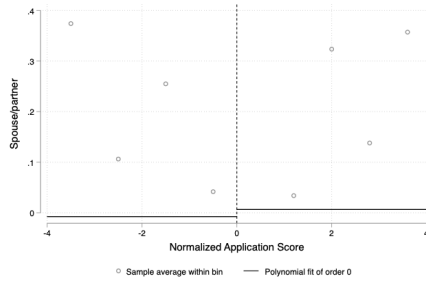
(c) Spouse/Partner
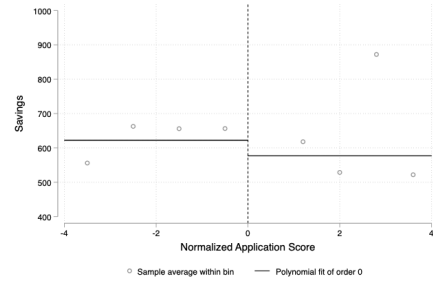
(d) Savings (UF)
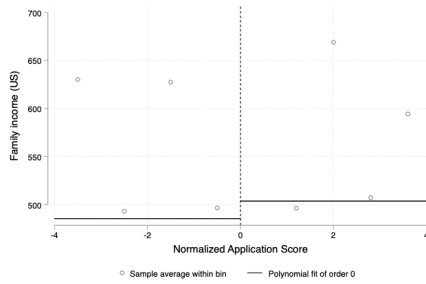
(e) Income (US)
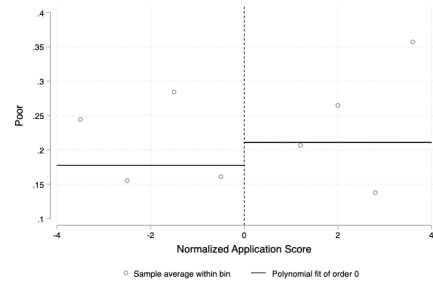
(f) Income (US) below Poverty Line
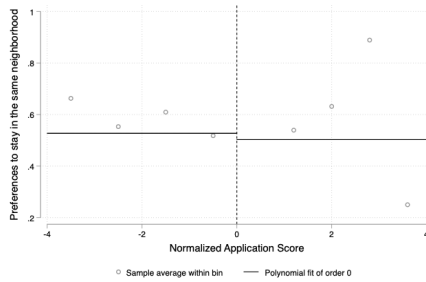
(g) Preferences Same Neighborhood
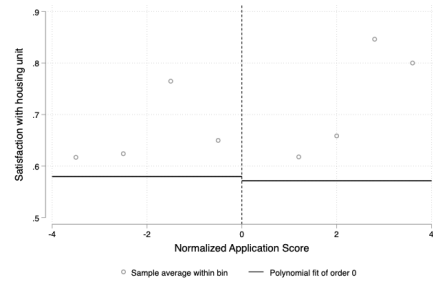
(h) Satisfaction Housing Unit

Figure 3: Balance in Baseline Characteristics in Regular Rounds
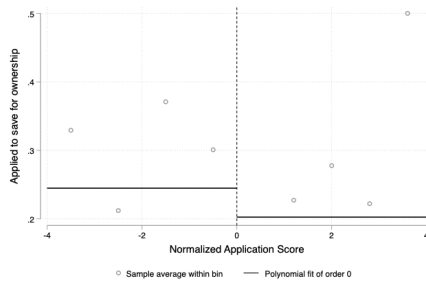
(i) Apply to save for Ownership
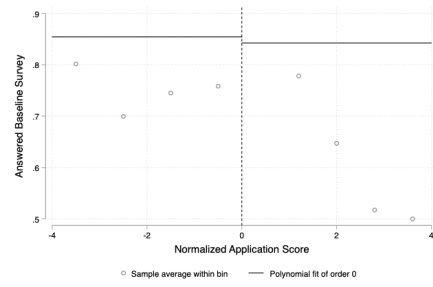
(j) Baseline Survey Response
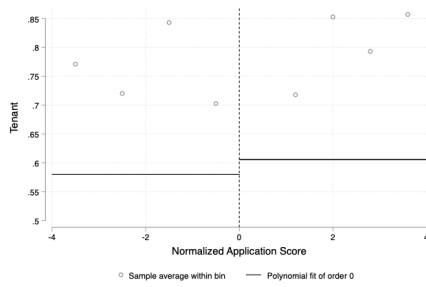
(k) Tenancy
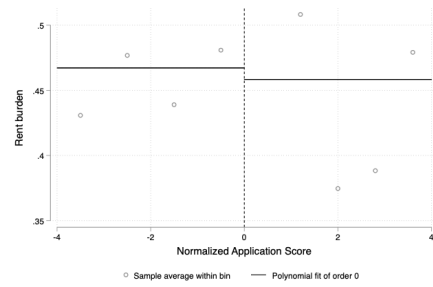
(l) Rent Burden
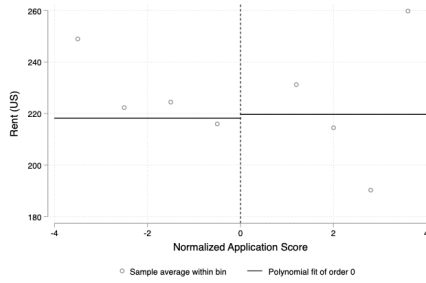
(m) Rent
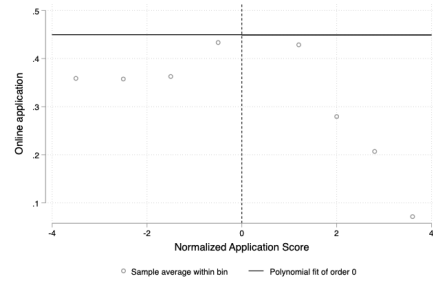
(n) Online Application
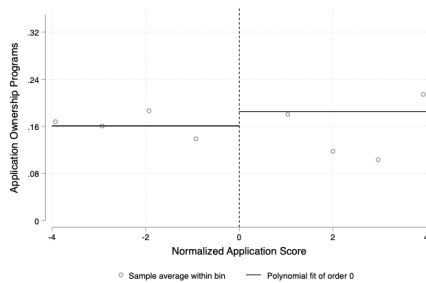
(o) Ownership Program Applications
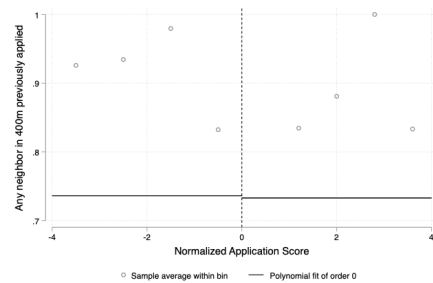
(p) Neighbors Applied in t-1

Figure 3: Balance in Baseline Characteristics in Regular Rounds
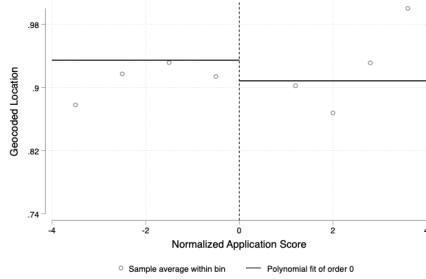
(q) Geocoded Location
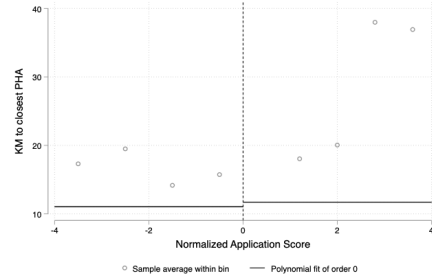
(r) Distance Closest PHA
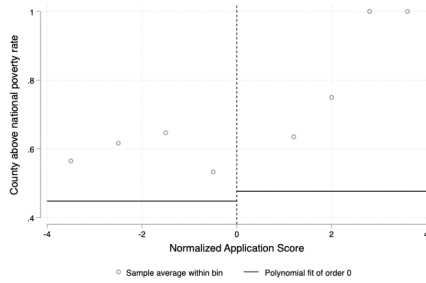
(s) High Poverty County
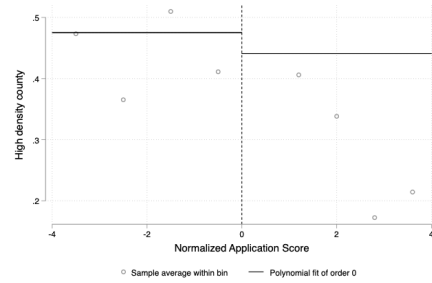
(t) High Density County
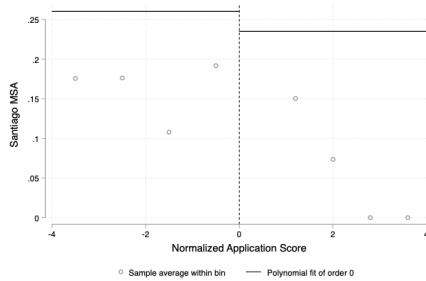
(u) Residence in Santiago MSA

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
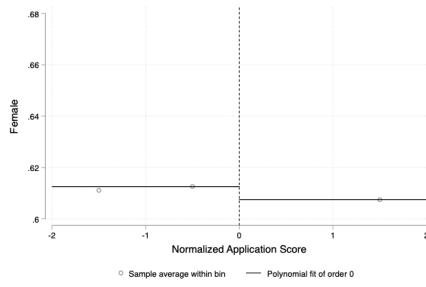
(a) Female
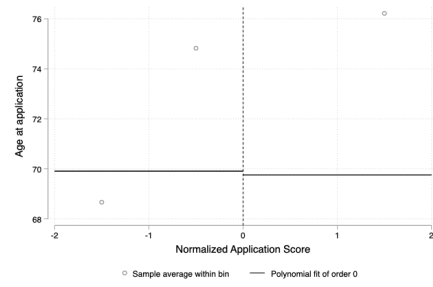
(b) Age
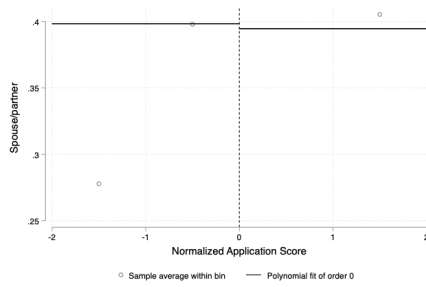
(c) Spouse/Partner
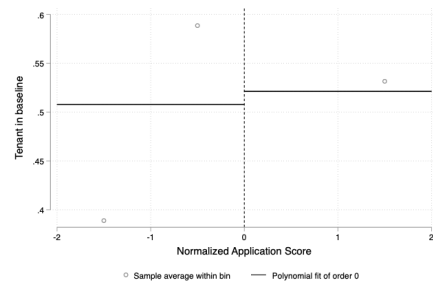
(d) Tenancy
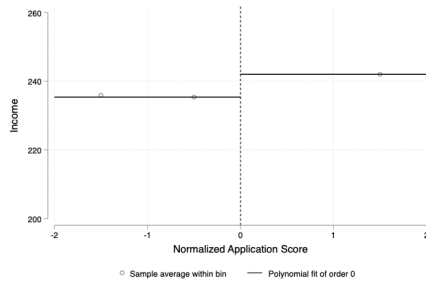
(e) Income
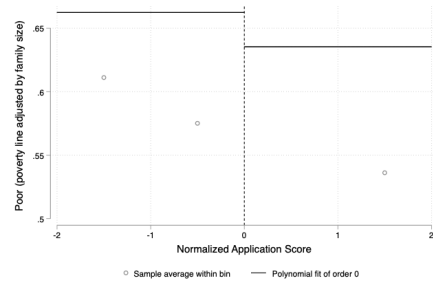
(f) Income Below Poverty Line
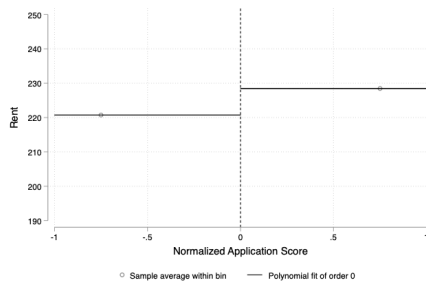
(g) Rent
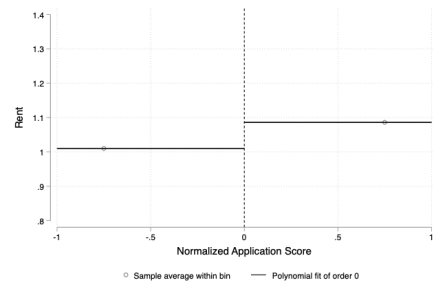
(h) Rent Burden

Figure 5: Balance in Baseline Characteristics in Elderly Rounds
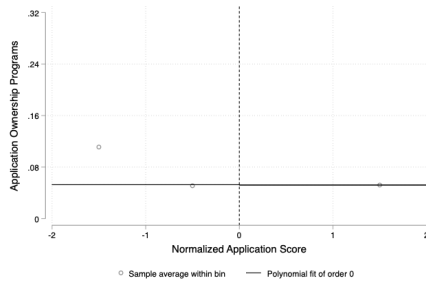
(a) Ownership Program Applications
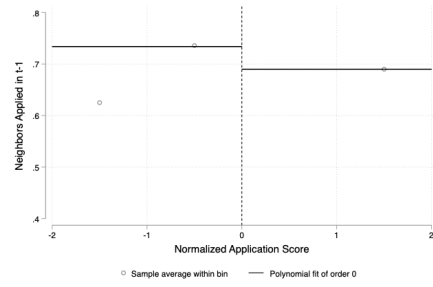
(b) Neighbors Applied in t-1
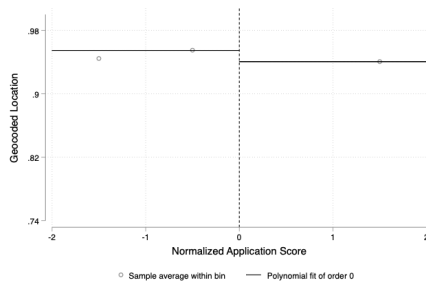
(c) Geocoded Location
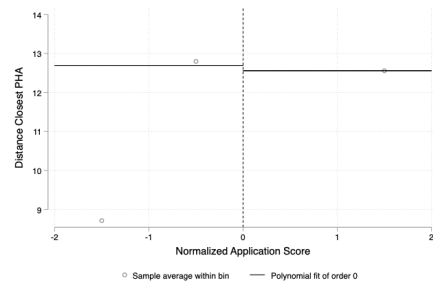
(d) Distance Closest PHA
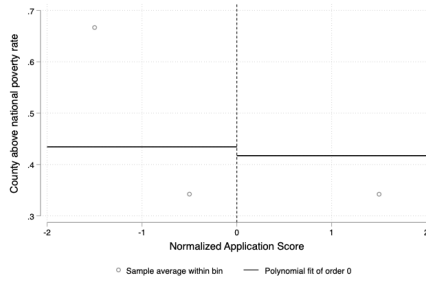
(e) High Poverty County
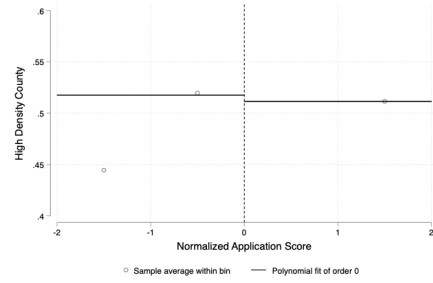
(f) High Density County
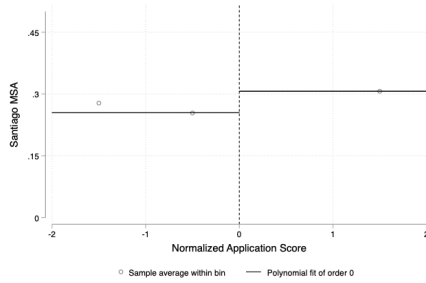
(g) Residence in Santiago MSA

Note: This figure replicates Figure A2 using data from elderly rounds. See Figure A2 for details.

Figure 6: Follow up Sample Attrition by Assignment
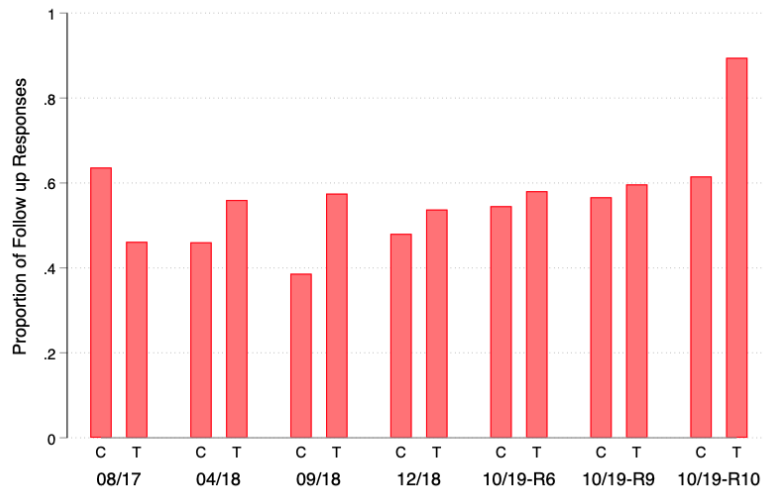
(a) Regular Rounds
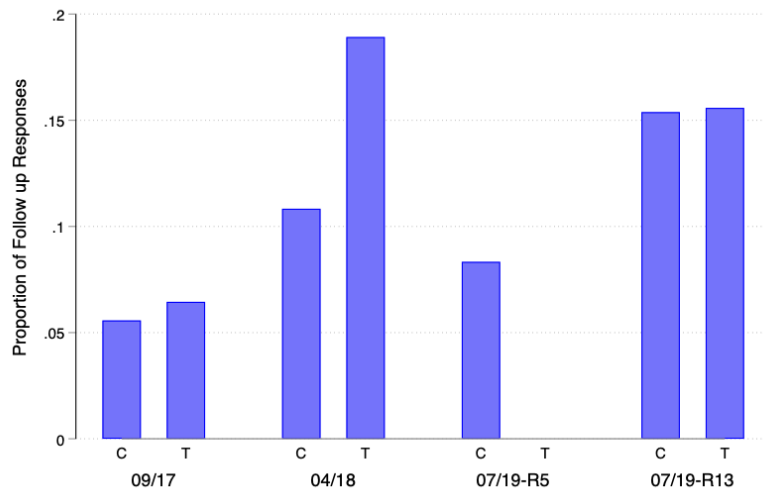
(b) Elderly Rounds

Table 8: 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 5.3 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 9: 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 8 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.

Table 10: 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 11: 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 12: Results Regular Rounds Before the Covid-19 Pandemic: December 2019

| | | 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	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 6.1 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 5.2.2. 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 13: Results Elderly Rounds Before the Covid-19 Pandemic: December 2019

	N	Control 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
					Specification 1				Specification 2		
	(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 12 for elderly rounds. See Table 12 for details. Significance
levels: * p<0.1; ** p<0.05; *** p<0.01.

Table 14: 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)
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 14: Results Regular Rounds During the Covid-19 Pandemic: November 2020

	N	Control Mean	SD	Treatment Effect	Spec 1 Treatment Effect (SD)	Spec 1 OLS p-value	Spec 1 Rand-t p-value	Treatment Effect	Spec 2 Treatment Effect (SD)	Spec 2 OLS p-value	Spec 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)	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 6.1 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 5.2.2. 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 15: Results Elderly 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)
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	105	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 15: Results Elderly Rounds During the Covid-19 Pandemic: November 2020

	N (1)	Control Mean (2)	Control SD (3)	Treatment Effect (4)	Spec 1 Treatment Effect (SD) (5)	Spec 1 OLS p-value (6)	Spec 1 Rand-t p-value (7)	Treatment Effect (8)	Spec 2 Treatment Effect (SD) (9)	Spec 2 OLS p-value (10)	Spec 2 Rand-t p-value (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 14 for elderly rounds. See Table 14 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Appendix

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.
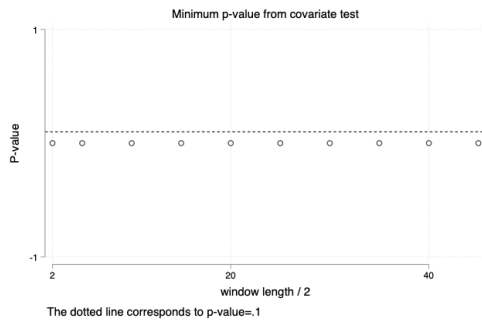
Figure A1: Window Selection by Assignment in Regular Rounds
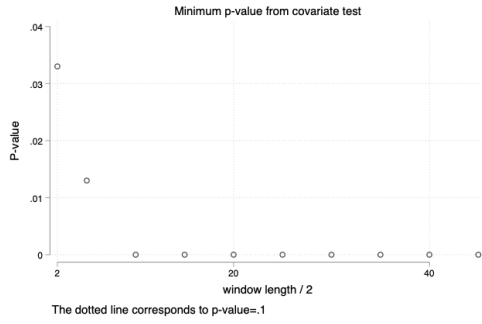
(a) April 2017, National
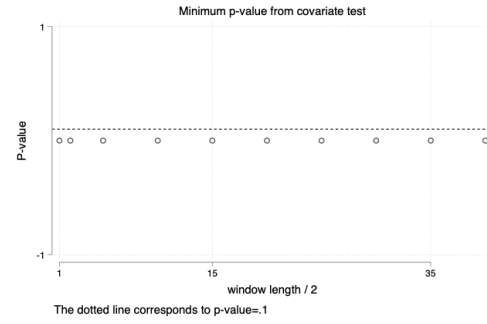
(b) May 2017, National

(c) June 2017, National
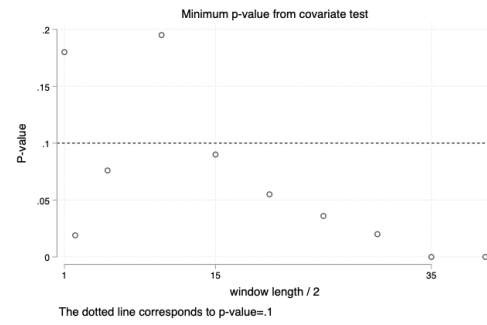
(d) August 2017, National
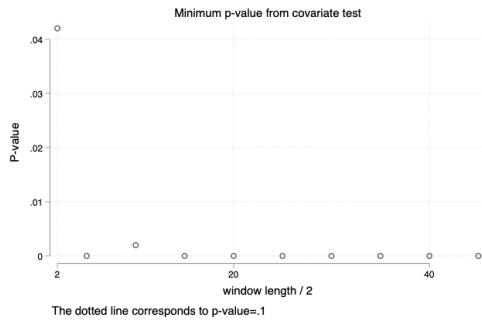
(e) October 2017, National
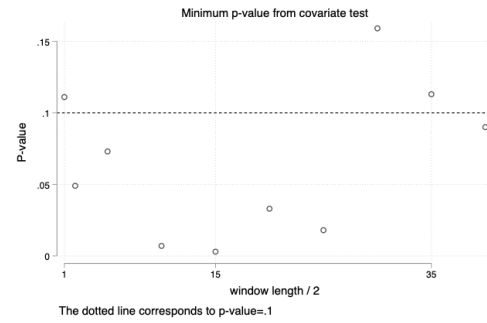
(f) April 2018, National
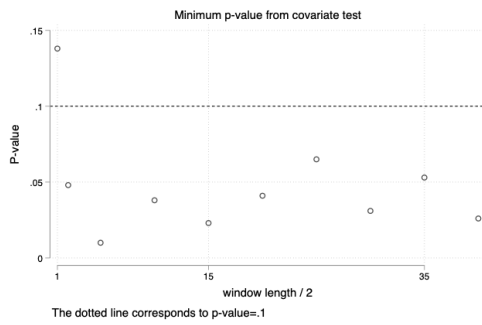
(g) June 2018, National
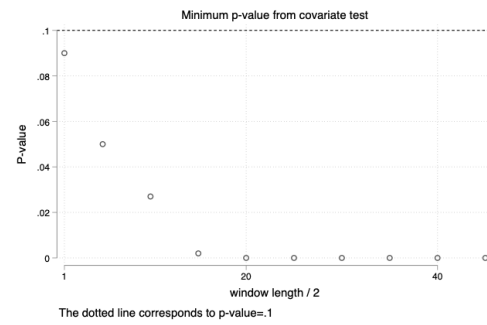
(h) September 2018, National

Figure A2: Window Selection by Assignment in Regular Rounds

Figure A2: Window Selection by Assignment in Regular Rounds
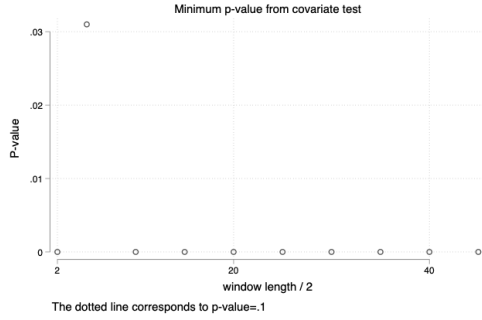
(a) November 2018, National
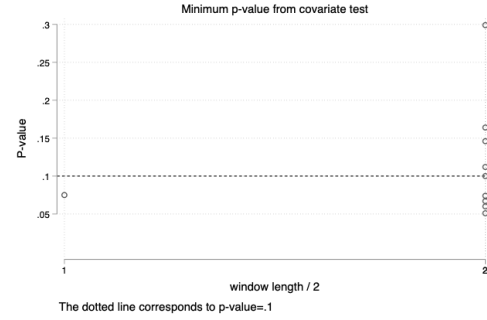
(b) December 2018, National
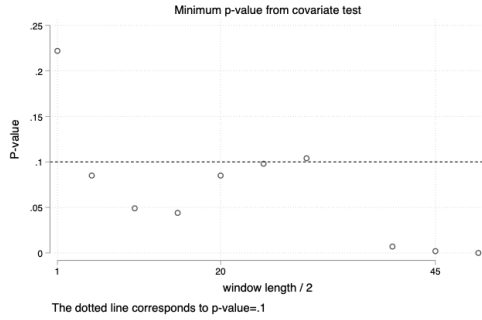
(c) October 2019, 6th Region
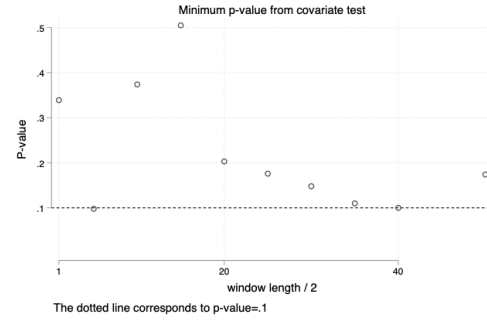
(d) October 2019, 9th Region
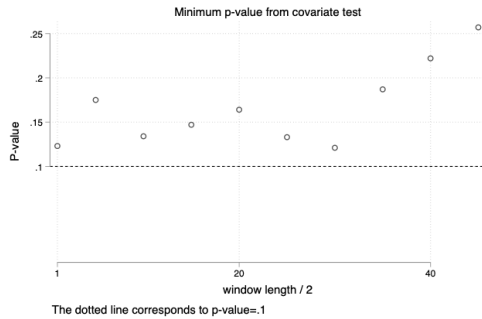
(e) October 2019, 10th Region

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 A3: Window Selection by Assignment in Elderly Rounds
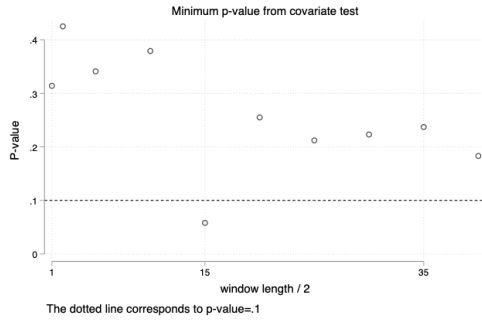
(a) September 2017, National
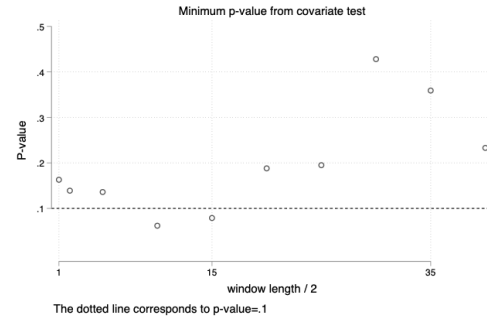
(b) April 2018, National
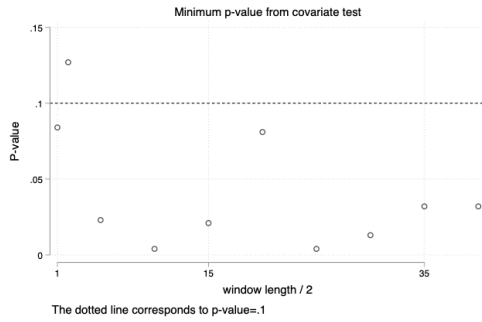
(c) October 2018, National
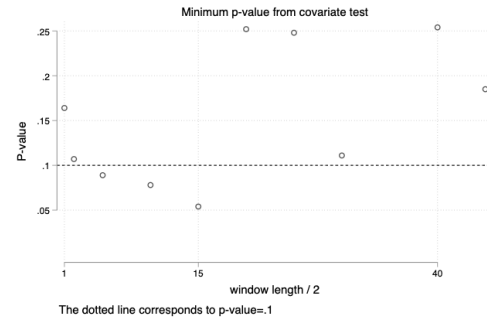
(d) July 2019, 5th Region

(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 $\alpha^* = 0.1$.

Table A2: Results Regular Rounds December 2019 - Follow Up Sample

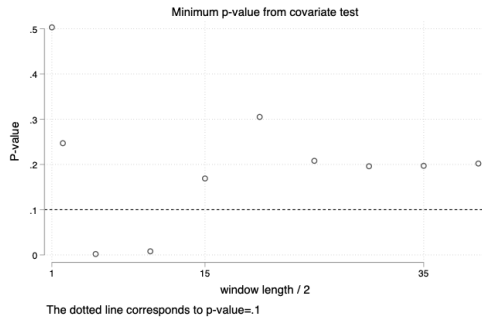|	N	Control Mean	SD	Specification 1 Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value	Specification 2 Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing Conditions											
Household size Dec 2019	706	3.134	1.487	-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 12 using the follow-up sample. See Table 12 for details.

Table A3: Results Elderly Rounds December 2019 - Follow Up Sample

	N	Control Mean	SD	Specification 1 Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value	Specification 2 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 13 using the follow-up sample. See Table 13 for details.

Table A4: Baseline Characteristics Regular Rounds

| | All Applicants | | | | | | | | Voucher Recipients | | | | | | |
| | Pooled | Non-Recipients | | Recipients | | Est. Difference | | | Never Leased-up | | Leased-up | | Est. Difference | | |
	Mean (1)	Mean (2)	SD (3)	Mean (4)	SD (5)	Coeff (6)	FE Coeff (7)	N (8)	Mean (9)	SD (10)	Mean (11)	SD (12)	Coeff (13)	FE Coeff (14)	N
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	0.44	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 A5: 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 A4 for elderly rounds. See Table A4 for details.