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

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**Abstract**

Many developing countries are transitioning towards rental subsidies that, similar to the US Section 8 program, subsidize housing for low income families. I present the first evaluation of such programs in a developing country context, Chile. I create a data set that replicates voucher assignment in the Chilean rental subsidy and merge it with administrative data on baseline characteristics and a range of outcomes in December 2019, which I further complement with a survey implemented in 2020. I exploit cutoffs and tie-breaking rules in the assignment of the voucher to implement a local randomization regression discontinuity approach (Cattaneo & Frandsen, 2015). In the period prior to the pandemic, results are similar to the US literature: holding a voucher reduces overcrowding but does little to induce residential mobility to better neighborhoods for low income families. In contrast, in the first eight months following the COVID-19 outbreak of March 2020, my results show that rental vouchers had a broader impact on recipient households. They experienced less unwanted mobility and lower rent burden; moreover, shelter deprivation decreased among the elderly. Holding a voucher also affected how families were coping with the large unexpected income shock: they were less likely to be engaging in new activities to complement their incomes or to miss their rent payments. These results point to a previously underappreciated role of housing subsidies in helping poor households cope with negative income shocks.

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# 1 Introduction

In December 2013, the Chilean Ministry of Housing and Urbanism (MINVU) launched the first rental subsidy in Latin America (*"Subsidio de Arriendo"*). To date, several countries in the region, including Argentina, Mexico, Peru, Colombia, Paraguay, Uruguay and Brazil, have followed Chilean steps towards rental policies.

Almost ten years after the introduction of a rental voucher program in Latin America, the literature remains concentrated in the US. Even though the design of the Chilean rental subsidy was advised by the US Department of Housing and Urban Development (HUD) and inspired on the US rental voucher program, Section 8, differences in policy design, institutional context and governance system could have a profound impact on the experiences and outcomes of subsidized households (Colburn, 2021). Then, this paper asks how a rental voucher to rent a unit at the private market would affect families in poorer countries with smaller rental markets, higher levels of informality and where demand-side subsidies have promoted ownership for decades.

To answer this question, this research studies 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 larger voucher for a maximum of two years for the elderly.

In order to estimate the causal effect of this program, I begin by creating a data set that allows me to reconstruct voucher assignment, and merge it with a broad range of baseline covariates and post-treatment outcomes. I supplement administrative data with a survey that I implemented in 2020, which allows me to evaluate the program before the pandemic and also after the large unexpected income shock following the COVID-19 outbreak in March 2020. With this data in hand, I estimate treatment effects using the Local Randomization approach to Regression Discontinuity Designs (LRRD), developed in (Cattaneo & Frandsen, 2015).

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. Also, holding a voucher increased mobility of elderly households but it did not change how far they moved from their baseline location. In younger families, the voucher had the opposite effect; it did not affect the chances of moving yet voucher holders moved longer distances.

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 and 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 American countries, greatly hit by the pandemic have not been an exception.[1]

To understand how the Chilean rental subsidy offered relief to the unexpected income shock that came with the coronavirus pandemic, I implemented a survey between September and 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 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 a significant effects in times of economic shocks, providing income and housing security.

The contribution of this paper to the literature is twofold. First, it contributes to the empirical work that evaluates rental voucher programs. This literature shows that rental vouchers have been effective to reduce rent burden, overcrowding, and homelessness of

[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.

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)). However, this literature is focused on the US. Moreover, while many developing countries are moving towards rental policies [2], to the best of my knowledge, this is the first evaluation of a rental voucher program to rent houses in the private market in a developing country.

Secondly, this research contributes to a narrow literature on how housing policy affects housing security and 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 reduce evictions.[3] Importantly, previous evaluations have not been conducted during times of economic shocks.

The rest of the paper is organized as follows. The next section provides some background and 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 discusses the validity of the assumptions made in the LRRD approach. Then, Section 6 presents the results and Section 7 concludes.

## 2 Institutional Background and Policy Design

Chile has had a large influence in the design of housing policies in Latin America. In 1974, the Chilean Ministry of Housing and Urbanism (MINVU) introduced the first demand-side housing subsidy directed to ownership, which was later adopted by several countries, becoming the predominant housing policy in the region until today (Navarro, 2005). Four decades later, in 2013, MINVU introduced the first rental policy for low-income families, the program (in spanish, *"Subsidio de Arriendo"*). To date, many countries in Latin America

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

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

have followed Chilean steps towards rental policies, including Argentina, Mexico, Peru, Colombia, Paraguay, Uruguay, Brazil, among others.

Between 2014 and 2019, the Chilean rental subsidy assigned fifty thousand rental vouchers ($325 million) under two types of voucher schemes.[4] First, voucher holders in regular rounds are assigned $6,200 in fixed monthly installments of $180 that they can use over an eight-years period to pay rents up to the maximum rent payment standard, which is set nationally at $402.[5] Second, voucher recipients in elderly rounds are assigned $7,780 in monthly installments to cover up to 95 percent of monthly rents (up to $380) for two years.[6] Elderly rounds were created in 2017. When the first group of vouchers were about to expire in 2019, MINVU reformed the program to extend the subsidy for another two years, however, this has not been publicly announced or explained to current voucher recipients.

The design of the program was advised by the US Department of Housing and Urban Development (HUD) and inspired in the Housing Choice Voucher Program (Section 8), which is the largest federal housing subsidy program in the US. Similar to Section 8, the Chilean rental voucher policy assists families to pay their monthly rent in housing units that they find in the private rental market. Some important differences between Section 8 and the Chilean rental subsidy include their generosity, duration and eligibility. In particular, Section 8, is more generous[7] and lasts for a longer period of time yet families have a less time to search for a house before they lose their benefit; in Chile families have 24 months to lease-up. In addition, instead of using waiting lists to assign vouchers, MINVU calculates an application score to rank families and assign available vouchers to most vulnerable ones. Even so, the Chilean subsidy targets low and middle-income families, not only very low-income families, as in US. Section 2.0.1 explain voucher assignment in detail.

There is a large body of empirical work studying the effects of Section 8. The evidence suggests that the program reduces homelessness, rent burden and overcrowding, yet voucher recipients do not get access to significantly better locations. Further, com-

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

[5] Except for 30 out 346 counties located at the north and south of the country that have a higher maximum rent standard ($475).

[6] 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.

[7] Voucher holders in Section 8 pay thirty percent of their income towards rent and the government pays the rest up to the maximum payment standard, set locally for each Metropolitan Area. In 2019, the average monthly voucher per family was $890 (See https://www.huduser.gov/portal/datasets/assthsg.html).

pare to similar families without a subsidy, voucher holders may pay more for housing while quality standards remain unchanged (Mills et al. (2006), Kling et al. (2007), Ellen, Horn, and Schwartz (2016)). Regarding non housing outcomes, there is evidence showing that rental vouchers reduce employment in the short run - in the long run the evidence is mixed - and that children in the program perform better at school and have higher lifetime earnings (Schwartz et al. (2020), Chetty et al. (2016), Jacob and Ludwig (2012), Chyn et al. (2019), Mills et al. (2006)).[8]

The existing evidence may not be extrapolated to other contexts. Even within the US, most of this work is based in five large metropolitan areas[9] and results are mixed across evaluation sites. Moreover, lease-up rates across PHAs in the US vary between 35 to 100 percent, suggesting that differences in the administration of the program and the rental markets that each PHA serves affect the outcomes of the program (Andersson et al. (2016), Finkel and Buron (2001)). Indeed, there is evidence of heterogeneous treatment effects according to race, age and other demographic characteristics that may vary across rental markets (Schwartz et al. (2020), Chyn et al. (2019)).

Divergences in policy design, institutional context and governance system across countries may have an even more profound impact on the experiences and outcomes of subsidized households (Colburn, 2021). For instance, in 2013, rental housing represented no more than twenty percent of the housing stock in Chile and other Latin American countries, about half than the observed size for the US (Ross & Pelletiere, 2014).[10] Also, the fraction of low income families who owned their houses, even without housing assistance from the government, is higher in Chile than in the US.[11] This differences between the US and Chilean housing markets may well affect participation, lease-up rates and location decisions in the latter.

Some evidence has also pointed to the role social norms in the effectiveness of housing

[8]See (Collinson & Ganong, 2018) and Ellen et al. (2020) for a full review of the state of the literature.

[9]The experimental evidence is based on the Moving to Opportunity program in Baltimore, Boston, Chicago, Los Angeles, and New York City (Dumarey, Sket, Joseph, and Boquet (1975);Kling et al. (2007)) and the Welfare to Work program in Chicago (Mills et al., 2006). The few quasi-experimental studies have leveraged randomized waiting lists in Chicago (Jacob and Ludwig (2012); Chyn et al. (2019)) and New York (Schwartz et al., 2020)). Andersson, Kutzbach, Palloni, Pollakowski, and Weinberg (2016) and Gubits et al. (2016) expand their analysis to other states in the US but focus on alternative research questions.

[10]See Blanco, Fretes Cibils, and Muñoz (2014) for a comparative analysis between Latin American countries.

[11]Figure A2 in the Appendix shows own calculations for Chile in 2013 and 2017 and show that similar statistics are observed before and after the introduction of the rental voucher. Andrews and Sánchez (2011) reports tenure by household income in the US.

policies. In particular, (Barnhardt, Field, & Pande, 2017) provides experimental evidence on the long term effects of providing public housing in the periphery to slum dwellers in Ahmedabad, India. The authors found no socioeconomic improvement, no increase in tenure security, isolation from social networks and a reduction of informal insurance among winners. Moreover, the take up of the program was two thirds, one third of which exit the program to return to their previous slums to be close to their social networks. The authors concluded that social networks may play a more important role in housing programs for the poor in developing contexts. In Chile, in addition to social networks, some descriptive data suggests that strong preferences for ownership subsidies may have a negative impact in the rental subsidy (Selman, 2019).

To conclude, further research is needed to understand how these programs are doing outside of the US. As many developing countries move towards rental policies, it seems imperative to study their effects in families in poorer countries with smaller rental markets, higher levels of informality and where demand-side subsidies have promoted ownership for decades. This paper contributes to close this gap in the literature. The rest of this section explains the assignment rules that I exploit to estimate the causal effect of the Chilean rental subsidy.

### 2.0.1 Voucher Assignment

To apply to the program, families can go online or in person to any of the forty four PHAs (SERVIUs) across the country.[12] Minimum requirements vary between regular and elderly rounds. Regular rounds target eighteen or older-headed families with monthly income between $250 and $900[13] and who have US$180 in a private savings account for home-ownership. In contrast, elderly rounds are targeted to individuals sixty or older whose income is above $140 and savings are not required.

To assign available rental vouchers, MINVU gathers multiple administrative and self reported data from other government agencies and uses a complex formula to calculate an application score. Table A1 in the Appendix shows how this score is calculated. There are few differences between elderly and regular rounds; nonetheless, compared to a regular round, the same elder applicant can get 60 to 140 additional points in an elderly round.

[12]Some municipalities voluntarily participate in the application process.
[13]Families larger than three have higher income upper bounds.

Once the score is calculated, applicants are ranked over their score and families above certain cutoff are assigned a voucher. Importantly, the cutoff is determined by voucher availability, which is set by decree for each round before the start of the application process. This is not publicly announced and sometimes change for administrative or political decisions not made by the rental policy team at MINVU.

Vouchers are assigned using a national rolling application system. In practice, rounds are opened for two to nine months and MINVU can make one or multiple assignments during this period. Within the same round, applicants who are not selected in a previous assignment are ranked again with all new applicants for the next assignment.[14] In 2019, MINVU switched to regional voucher assignment i.e applicants are ranked by region, therefore, sixteen regional assignments occur at each assignment date.

The application score considers a social vulnerability component based on the national targeting system in Chile, used by several government agencies to assign social programs. A reform to the targeting instrument in 2016 forced MINVU to change their social vulnerability score formula. More specifically, the previous national system that assigned a continuous vulnerability score was replaced by the Household Social Registry (RSH), a seven category index based on families' position in a non-reported vulnerability score distribution: below 40th, 41th-50th, 51-60th, 61-70th, 71-80th, 81-90th and 91-100th percentiles.[15] Eligible families in the rental subsidy are within the first four groups of the RSH.

This reform had important consequences for voucher assignment in that it made the application score discrete (multiples of five), allowing ties at the cutoff, which demanded the establishment of a tie-breaking protocol. MINVU implemented a three-step procedure. First, families are sorted over their family size score and those with higher scores are assigned a voucher. Remaining ties are then sorted over their social vulnerability score. Again, higher scores are assigned a voucher. Finally, left standing vouchers are randomly assigned. I explain the implications of these issues for the empirical strategy in section 4.

[14]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.

[15]If anything, the reform made even harder to manipulate the application score. First, the score used to classify families is based on survey and administrative data on educational achievement, income, expenses, net worth, health, food security and living arrangements for several months before application. Second, specific cutoffs used by different programs in the country were replace by ranges of an unknown distribution. That said, many existent evaluations of policies in Chile use a regression discontinuity design based on the old continuous score and have not found evidence of manipulation; Aguirre (2020) in education policy and Navarrete and Navarrete (2016) in housing policy.

## 3 Data

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.

*Assignment Data.* I create a unique data set of all applicants, their scores, application dates, and rounds and assignment characteristics (dates, cutoffs, regional assignments, etc.) using different administrative data sets provided by MINVU and legal resolutions with information for each assignment. Reconstructing assignments required several meetings with the rental policy team at MINVU.

*Baseline Data.* I access application data that MINVU collects to determine applicants eligibility and vulnerability. In particular, I have socioeconomic and demographic characteristics, location[16] and some housing characteristics. In addition, I have survey data for applicants in regular rounds between March 2017 and October 2019, the relevant period of analysis. This survey was sent before assignments were announced and asked about housing and neighborhood experience, preferences, and beliefs about renting and residential mobility to all applicants. Response rate was 75 percent or higher.

*Before the Covid-19 outbreak.* I get outcomes from multiple sources. First, unit characteristics, household composition and location was obtained by MINVU from the Household Social Registry (RSH) and their own administrative data in December 2019. 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 in December 2019.[17] Finally, I linked all this data to geocoded data from different public sources to characterize neighborhoods: geocoded schools, health care centers, municipalities location as a proxy for high economic activity, and poverty, crime and density at the county level.

Using this data I created outcomes regarding crowding, residential mobility and neighborhood characteristics. Also, application to homeownership program and, only for regu-

[16]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.

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

lar rounds, I also measure the extensive (opened account) and intensive margins (balance) of private savings for homeownership.

*After the Covid-19 outbreak.* I partnered MINVU to implement a Follow up survey. Data collection took place between September and November 2020. The questionnaire included questions to measure crowding, residential mobility and neighborhood characteristics, subjective well-being, neighborhood satisfaction, income, employment, and questions to understand the effect of the voucher on how families were coping with the Covid-19 pandemic during the first eight months following the outbreak in March 2020.

# 4 Empirical Strategy

This research exploits the assignment rules of the Chilean rental subsidy to implement a multi-cutoff sharp regression discontinuity design. The regression discontinuity design (RDD) 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). The RDD is one of the most credible research designs in the absence of experimental treatment assignment, however, it is expected to have little external validity for populations away from the cutoff.

In the Chilean rental subsidy, applicants at each assignment $s \in S$ are ranked over their score $X_{i,s}$ and are assigned a rental voucher if $X_{i,s} \geq c_s$. There is full compliance at each assignment $s$, therefore, the sharp RDD design (Figure 1). The cutoff $c_s$ may vary across assignments since it is the value of the score of the applicant who got the last available voucher, set at the assignment level.[18] This multi-cutoff setting may have advantages for external validity that are discussed in further detail in Section 6.7 (Cattaneo, 2020).

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 $s \in S$ and $D_{i,s} = D_{i,s}(X_{i,s}) = I(X_{i,s} \geq c_s) \in \{0, 1\}$ the treatment indicator. Then, $Y_{i,s} = D_{i,s}Y_{i,s}(1) + (1 - D_{i,s})Y_{i,s}(0)$ is the observed outcome for individual $i$ (Rubin, 1974). As it is common in settings with multiple cutoffs, I pool the data and normalize the running variable around a unique cutoff $c_s = c = 0$ to analyze the

[18]Figure 2 in the Appendix presents the distribution of application scores and cutoffs in the pooled data.

data as in a single cutoff RDD (Cattaneo, 2018).

The standard estimation method to establish comparability between groups around the cutoff and estimate causal parameters in RDD settings has been the continuity-based approach (Lee & Lemieux, 2010). Assuming continuity of the regression functions $\mathbb{E}\{Y_i(1)|X_i = 0\}$ and $\mathbb{E}\{Y_i(0)|X_i = 0\}$ at the cutoff $X_i = 0$, this approach approximates the average outcome that units above the cutoff would have had in the absence of treatment. Then, the average treatment effect at the cutoff, $\tau_{Cont}$, 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 in this research[19], the continuity assumption in the standard approach fails to provide unbiased coefficients and confidence intervals in the smallest possible window $W_0 = [-c_k, c_k]$.[20] Therefore, this research uses the Local Randomization approach to RD analysis (LRRD), first introduced by Cattaneo and Frandsen (2015).[21]

The LRRD makes strong assumptions on the assignment mechanism near the cutoff instead of using modeling assumptions, as in the standard continuity approach (Branson & Mealli, 2018). In particular, 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. Potential outcomes may depend on the score only through treatment indicators inside $W_0$ and there should not be interference between units' potential outcomes ("Stable Unit Treatment Value Assumption" or SUTVA).[22] Under these assumptions, score ignorability $Y_{i,s}(X_{i,s}, D_{i,s}) = Y_{i,s}(D_{i,s})$ is guaranteed inside $W_0$, therefore, analyzing the data as if it were experimental data is straightforward.

The causal treatment effect in the LRRD, $\tau_{LR}$, is the difference between the average

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

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

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

[22]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 inference in the LRRD (Cattaneo & Frandsen, 2015).

outcome of treated and controls in the largest window around the cutoff where local randomization assumptions hold:

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

This approach may be valid only within a few units in a narrow window around the cutoff, therefore, it uses randomization inference to build exact confidence intervals and hypothesis tests that are robust in small finite samples.[23]

Similar to the problem of bandwidth selection in the continuity approach, the most important step in LRRD is window selection. In this paper, I use the data driven procedure developed in (Cattaneo & Frandsen, 2015). To select the window for assignment $s$ I assume fixed-margin treatment randomization at the assignment level. The selected window $W_s$ is the largest window such that the minimum p-value obtained through all balance tests in baseline covariates inside $W_s$, and any smaller window $W_k$, is above a predetermined significance threshold, in this case $\alpha^* = 0.1$. Balance tests are conducted using sharp null hypothesis tests of no difference in mean on pre-treatment covariates[24]

Once all $W_s$ are identified, I pool them together in the evaluation sample $W_0$ to estimate the causal effect of the rental voucher program using a fixed effect model that exploits the variation around of the cutoff at each assignment. Taking all the assignments together, the assumed assignment mechanism follows a stratified randomization design; given the rolling application system, this design can be thought to mimic a sequential stratified experimental design (Pocock & Simon, 1975) in which each assignment $s$ is a strata or block of applicants that are independently assigned to treatment and control groups.

The next section explains how the evaluation sample $W_0$ is built using the data from the Chilean rental subsidy and presents the results of different falsification tests to asses the validity of LRRD identification assumptions inside $W_0$.

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

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

# 5 Evaluation Sample Construction

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 and assignment rules in the evaluation, applicants prior to 2017 were excluded from the sample.

To build the evaluation sample $W_0$, I first I 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 of analysis.

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, each assignment in 2019 pool together all sixteen regional assignments in the country.[25]

Having a discrete running variable simplifies considerably the window selection procedure in Cattaneo and Frandsen (2015). 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.

In this minimum window, treatment and control units must be in different sides of the cutoff, regardless of the values of the running variable. Of there was no mass point at the cutoff, the minimum window was $W_s = [-5, 5]$. However, if treated and control units were in the mass point at $c_s = 0$, the normalized running variable required some transformation Cattaneo (2018). In particular, I used assignment rules of the program to transform the running variable: i) 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 ii) ties that are randomly broken are re-scaled to be in $W_s = [-1, 1]$. 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.[26] To implement the data driven window selection procedure, I first applied some sample restrictions. First, Cattaneo and Frandsen (2015) recom-

[25]Comparing cutoffs and scores between regular and elderly rounds may be misleading since elder applicants receive almost one hundred additional points for their age.

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

mends to have at least ten units at each side of the cutoff in the minimum window.[27] Doing this dropped 34,638 observations from the sample, most of which were in regular rounds in 2019; regional voucher assignment reform reduced the number of observations around the cutoff in each assignment, making it harder to meet this sample size restriction.

Second, in October 2018 MINVU added non voucher recipients from the previous round to the list of applicants to be ranked yet none of them was selected into treatment. Given that individuals applying in different periods may be different in unobservable characteristics, I kept treated and controls that have common support in application date. This excluded 3,562 observations from the expanded sample.

Third, the rolling application system explained in Section 2 may create two types of control units in any but the last assignment of each round: never treated and later treated i.e. those who were ranked more than once because they did not get the voucher in the first assignment they participated on.[28] Using both types of control units affects the interpretation of the estimated treatment effect, 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 larger sample. 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. Therefore, I dropped 4,300 duplicated observations from the sample in order to have only never treated in the control group,

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 March 2017 and October 2019. Columns 1 to 5 in Tables 4 and 5 show the number of participants, maximum and minimum score, number of available vouchers and cutoff for the assignments in the evaluation sample. 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.[29]

[27] In the minimum suggested window, assuming a discrete outcome, a minimum detectable effect of one standard deviation 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.

[28] In the evaluation sample the likelihood of waiting for another assignment can be thought as exogenous.

[29] 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.

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).[30] 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 is based on income, savings, distance (km) to the closest PHA, and dummy variables for tenancy, previous application to homeownership programs, online application, high poverty county (rate above 8.6 percent, the national average) or high density. I use covariates availables in regular and elderly rounds except for savings and the indicator variable for online applications, which are only available for regular rounds.

Figures A4 and A5 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. Therefore, the selected window for assignment $s$, $W_s$, includes dots closer to the y-axis that are above the threshold.

The evaluation sample $W_0$ pools all $W_s$ together, including 3,133 observations in eleven assignments: 1,356 in seven assignments in regular rounds and 1,777 in four assignments in elderly rounds. The maximum size of any $W_s$ in the evaluation sample is $W_s = [-15, 15]$. Columns 8 to 12 in Tables 4 and 5 show summary statistics for the assignments included in the evaluation sample.

## 5.1 Falsification Tests

To assess the validity of the LRRD assumptions inside the evaluation sample, $W_0$, I present two additional falsification tests. As in the continuity approach, 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.

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

#### 5.1.1 Density of the Running Variable

In this section I analyze 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 in that it would require anticipation of voucher availability, their own score and the entire score distribution to predict the value of the cutoff. [31] 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 examine for manipulation inside the evaluation sample $W_0$. For intuition, if applicants cannot control precisely their value of the score, the probability of success (treatment) $q$ is expected to be consistent with the assignment mechanism assumed in $W_0$. However, 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, complete randomization or $q3 = 0.5$. Columns 6 and 7 in Tables 4 and 5 show the values of $q1$ and $q2$ in regular and elderly rounds, respectively.

I present the results of the binomial tests with and without the December 2018 assignment since the exceptionally low value in December 2018 (1.6 percent in column 6) was caused by an administrative decision[32] exogenous to the application process during the year.

Table 6 shows the results for the three different assumed probabilities of success $q1$, $q2$ and $q3$ in regular and elderly rounds. The results show that the binomial test fails to reject manipulation if the assignment in December 2018 is included. However, if this data is excluded, then the probability of treatment in the evaluation sample in regular rounds is $q = 0.465$ percent and it is compatible with the overall probability of treatment $q2 = 0.43$ (p-value 0.06) and with complete randomization $q3 = 0.5\%$ (p-value 0.08).

In elderly rounds, Table 6 shows that the observed average probability of treatment in

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

[32] 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).

the evaluation sample was $q = 0.62$, compatible with the observed probability of treatment around the cutoff, $q1 = 0.60\%$ but not with the observed probability in the overall sample, $q2 = 0.26\%$, or complete randomization, $q3 = .05\%$.

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

#### 5.1.2 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[33]; being poor i.e. having an income below the national poverty line adjusted by family size[34]; and, using 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 7 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, average income was US$528, one fifth was under the poverty line and three fourth were tenants, paying on average $224 in rent, almost half 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: Santiago, Valparaiso, O'Higgins, and Maule, Biobio, Araucanica, and Los Lagos, all of them situated in the center and south of Chile.

In elderly rounds, Table 8 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.[35] Their average in-

[33]400 meters is the average size of a census track in Chile.

[34]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).

[35]The elderly are a particularly vulnerable population in Chile; main income source of sixty percent of all

come was \$239, almost half was 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.[36] 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, selecting covariates to analyze balance within each assignment can be challenging in that it requires variation between and within assignments in a small window around the cutoff.[37] To be as conservative as possible and maximize the number of baseline covariates I use the following two fixed effect models to test for balance in the evaluation sample:

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

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

Where $X_{i,s}$ is the vector of baseline covariates, $\gamma_s$ are assignment fixed effects, and $S_{i,s}$ are score components that determine tie-breaking rules. In regular rounds it includes dummy variables for family size and social vulnerability score, which created the observed ties.[38] In elderly rounds, I use dummy variables for score components that are more likely to break ties: number of elderly and age range scores.[39]

elder individuals in Chile is a subsidized pension, which is about \$220 per month. Moreover, differences between younger and elderly rounds can be amplified by the design of the program that may discourage participation of informal workers, or unemployed head of younger households.

[36]Few observations in elderly rounds in the evaluation sample had rent data. However, this result is consistent with table A9 in section 6.7 showing an average rent burden of 95 percent (N=8,018).

[37]Collinearity between covariates and assignment indicators in equation 5.1 are expected given that treatment and control groups are already balanced in the first set of covariates and this is a multi cutoff RDD. For instance, the fraction of poor families, female, spouse, geocoded location and county poverty rate vary across but not necessarily within assignments in regular rounds, and they all had children. In elderly rounds, all households were in the bottom 40 percent of the vulnerability index (RSH), had no children, and geocoded location, county poverty rate, tenancy and neighbors in previous rounds did not vary within each assignment. Finally, some variables are not available for the entire period of analysis e.g. rent is available only after September 2018 and regional assignments in 2019 eliminated variation by region within assignments.

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

[39]Family size and social vulnerability scores are all the same in elderly rounds: 40 and 180 points. Together, number of elderly and age range scores represented almost half percent of their total score.

Equation 5.1 and 5.2 use different hypothesis tests to analyze balance. In the fully interacted model in 5.1, coefficients $\tau_s$ and $\beta_s$ test the null of no effect within each assignment or strata ($H_0$). Covariates in $X_{i,s}$ in equation 5.1 vary across and within assignments. Then, to maximize the number of covariates, $\tau$ in equation 5.2 tests for the weaker 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 7 and 8 present the results of balance tests under $H_0$ and $H_0'$. Panel A 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 in (Young, 2019)).[40] Panel B show FE coefficients and p-values for the weaker balance test ($H_0'$). I present the results with and without controls.

Table 7 shows 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.[41]

To account for multiple testing problems the bottom panel presents the F-test of joint significance from regressing the treatment indicator on all covariates included in equation 5.1 and the Westfall-Young multiple-testing test of overall treatment irrelevance. These results support individual tests showing balance between treatment and control groups in regular rounds in the evaluation sample.

In elderly rounds, results in Table 8 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 treatment relevance and joint significance are rejected, suggesting that treated and controls were not different in these characteristics.

[40] 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.

[41] 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.

Finally, Figures A4 and A5 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 significant differences in baseline characteristics between units at different sides of the cutoff.

# 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, $X_{i,s} > 0$, $\gamma_s$ 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 5.1.2.

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, $\tau$, 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 $\tau$ 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, $\tau$, aggregating families with different vulnerability that have had their vouchers for different amounts of time.[42]

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 $\tau$ 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.

### 6.1 December 2019: Before the Coronavirus Pandemic

Tables 9 and 10 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 5.1.2. 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.

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

Consisitent with balance analysis in section5.1.2, 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 9 and 10 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.[43]

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 increase 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

[43]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).

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 9 and 10 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 9 shows that holding a voucher did not affect application to homeownership programs in regular rounds; coefficients are small and non significant. Furthermore, there was no effect on the extensive or intensive margins of savings. 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 10 show that the rental policy 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 11 and 12 present the results of equation 6.1 using outcomes from the follow-up survey. In section A1 in the Appendix I provide 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 11 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[44] 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.

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

Third, Panel B in Table 11 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[45], it did not have a significant effect on shelter deprivation[46]; Panel B in Table 11 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.[47]

*Elderly Rounds*

Two results in table 12 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 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*

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

[46] 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.

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

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 9), column 3 in Panel F in table 11 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 11 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 11 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 11 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.[48]

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 11 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,

[48]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.

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 12 shows that in addition to reducing food budget, cutting utility bills and using government relief benefits, in this more vulnerable population it was more common 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, $\tau$ 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 $\tau$.

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 A8 and A9 in the Appendix) to summary statistics in the evaluation sample (tables 7 and 8 in section 5.1.2).

Table A8 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 7 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 income 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 A9 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[49], 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 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

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

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[50] and high perceived costs of application. In particular, there was confusion about eligibility criteria, rules about where and how to apply[51] and how to meet the requirements.[52] Moreover, it

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

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

[52]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.

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.[53]

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

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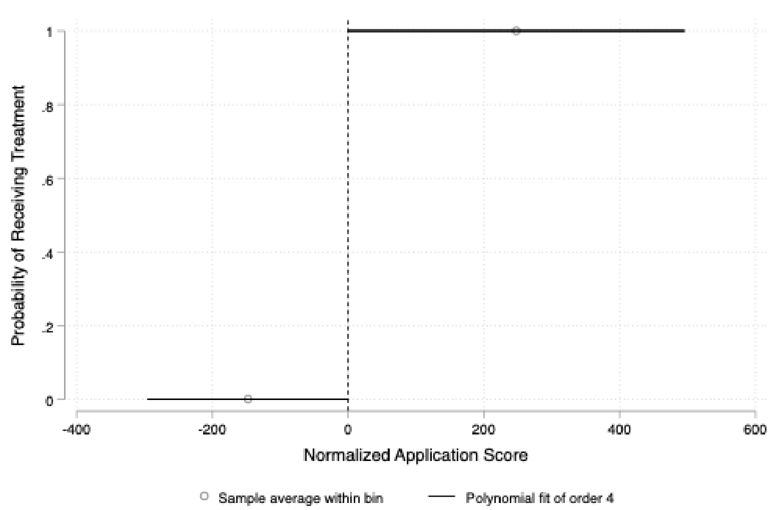
## Tables and Figures

Table 1: Program Descriptive Statistics

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

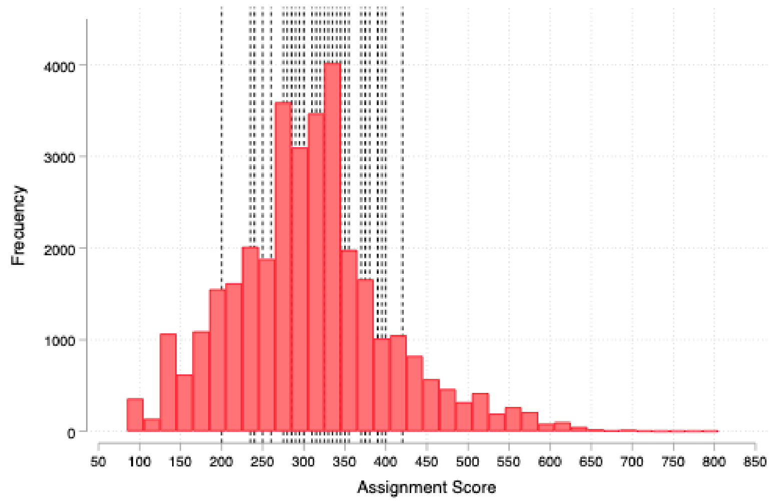
Note: This table presents descriptive statistics of each round of the program between 2014 and 2019. Columns 1 and 2 show the total number of applicants and number of vouchers assigned in each round. Columns 3-5 use data on all leases that voucher recipients activated between April 2014 and May 2020.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

Figure 1: Sharp RD Design

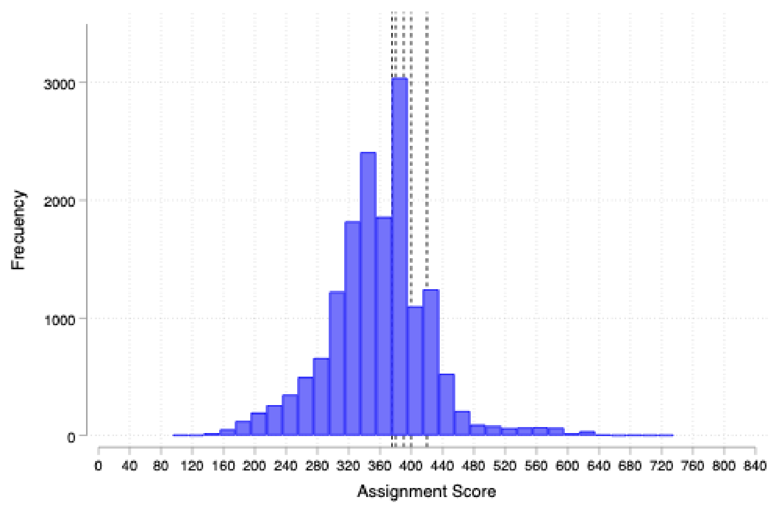



Note: This figure presents treatment probability for different values of the normalized application score.

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

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

Note: This table shows descriptive statistics for each assignment 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 $W = [-15, 15]$.

Table 5: Assignments in Elderly Rounds for Window Selection

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

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

Table 6: Density Test

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

Note: This table presents binomial tests to evaluate the presence of manipulation in the running variable in the evaluation sample. Three probability of success $q$ are used: $q1$ is the observed probability of assignment in a small window around the cutoff (column 7), $q2$ is the probability of treatment in the overall sample (column 6) and $q3$ tests for complete randomization in the evaluation sample ($q3 = 50\%$). The results are presented for the entire evaluation sample and excluding the assignment in December 2018, as explain in section 5.

Table 7: Balance in Baseline Characteristics in Regular Rounds

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

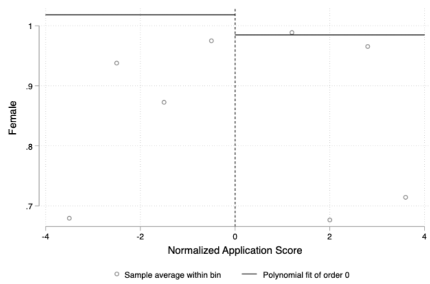
Note: This table presents summary statistics and balance tests between treatment and control groups in the evaluation sample. Columns 1 to 6 show summary statistics of baseline characteristics. Columns 7 to 10 show balance results from testing the more conservative null hypothesis ($H_0$) using the fully 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 5.1.2 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 8: Balance in Baseline Characteristics in Elderly Rounds

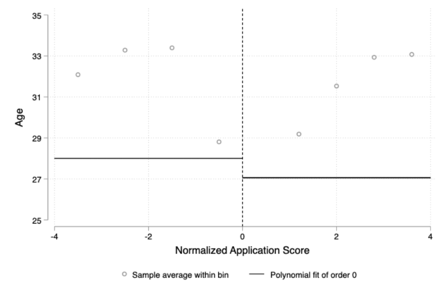
| | Summary Statistics | | | | | | Balance Test | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Pooled | Control | | Treated | | | | | | |
| | Mean | Mean | SD | Mean | SD | N | F-test (p) | Rand-t (p) | F-test (p) | Rand-t (p) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A: Interaction Terms (H0)* | | | | | | | | | | |
| Family income (US) | 239.44 | 235.37 | 97.23 | 241.99 | 106.30 | 1,777 | 0.667 | 0.744 | 0.643 | 0.903 |
| Poor (poverty line adjusted by family size) | 0.55 | 0.58 | 0.49 | 0.54 | 0.50 | 1,777 | 0.424 | 0.437 | 0.312 | 0.234 |
| Baseline application to ownership programs | 0.05 | 0.05 | 0.22 | 0.05 | 0.22 | 1,777 | 0.172 | 0.890 | 0.238 | 0.987 |
| KM to closest PHA | 12.61 | 12.69 | 18.06 | 12.56 | 19.12 | 1,681 | 0.381 | 0.171 | 0.421 | 0.184 |
| High density county | 0.51 | 0.52 | 0.50 | 0.51 | 0.50 | 1,777 | 0.855 | 0.538 | 0.853 | 0.409 |
| Female | 0.61 | 0.61 | 0.49 | 0.61 | 0.49 | 1,777 | 0.617 | 0.704 | 0.686 | 0.530 |
| Spouse/partner | 0.40 | 0.39 | 0.49 | 0.41 | 0.49 | 1,777 | 0.914 | 0.775 | 0.828 | 0.695 |
| Age at application | 75.62 | 74.66 | 6.30 | 76.22 | 6.85 | 1,777 | 0.130 | 0.939 | 0.087* | 0.769 |
| *Panel B: No Interaction Terms (H0′)* | | | | | | | | | | |
| Any neighbor in 400m previously applied | 0.71 | 0.73 | 0.44 | 0.69 | 0.46 | 1,285 | 0.993 | 0.368 | 0.979 | 0.296 |
| Rent (US) | 223.41 | 220.73 | 97.21 | 228.46 | 176.06 | 173 | 0.252 | 0.292 | 0.312 | 0.337 |
| Rent burden | 1.04 | 1.01 | 0.53 | 1.09 | 0.94 | 173 | 0.360 | 0.753 | 0.334 | 0.715 |
| Tenant in baseline | 0.55 | 0.58 | 0.49 | 0.53 | 0.50 | 1,654 | 0.605 | 0.508 | 0.558 | 0.518 |
| Geocoded location | 0.95 | 0.95 | 0.21 | 0.94 | 0.24 | 1,687 | 0.558 | 0.290 | 0.547 | 0.268 |
| County above national poverty rate | 0.35 | 0.35 | 0.48 | 0.34 | 0.47 | 1,777 | 0.496 | 0.695 | 0.494 | 0.704 |
| Santiago MSA | 0.29 | 0.25 | 0.44 | 0.31 | 0.46 | 1,564 | 0.073 | 0.873 | 0.071 | 0.945 |
| Assignment FE | | | | | | | Yes | Yes | Yes | Yes |
| Score components FE | | | | | | | No | Yes | No | Yes |
| | | | | | | | F-Test | Westfall Young | N | |
| Joint Significance Test (p-value) | | | | | | | 0.834 | 0.731 | 1,777 | |

Note: This table replicates Table 7 using data from elderly rounds. See Table 7 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01..

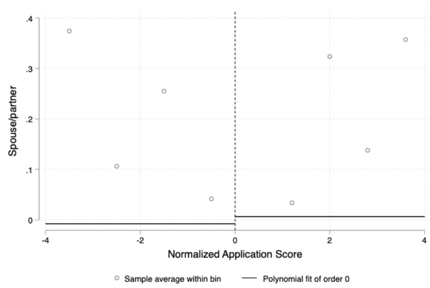
Figure 3: Balance in Baseline Characteristics in Regular Rounds



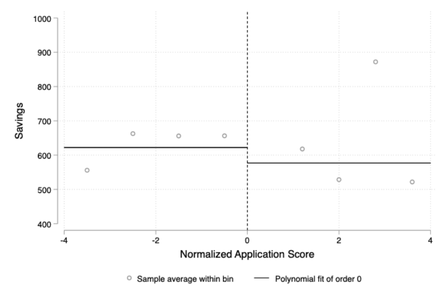

(a) Female



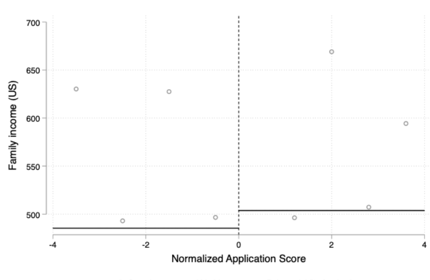

(b) Age



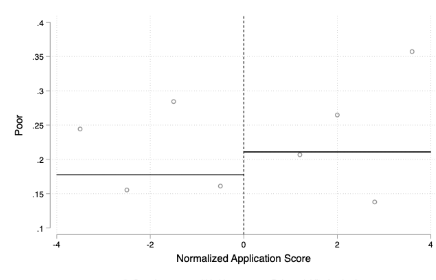

(c) Spouse/Partner



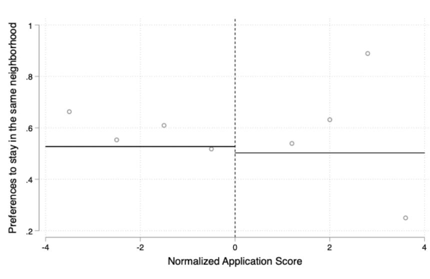

(d) Savings (UF)



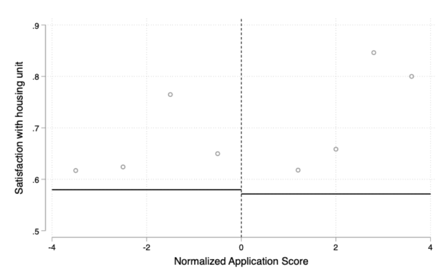

(e) Income (US)




(f) Income (US) below Poverty Line

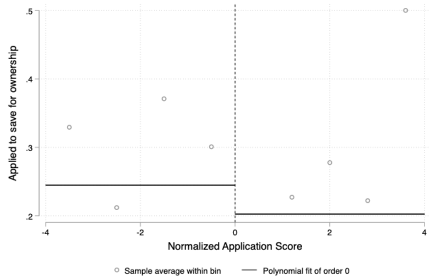



(g) Preferences Same Neighborhood

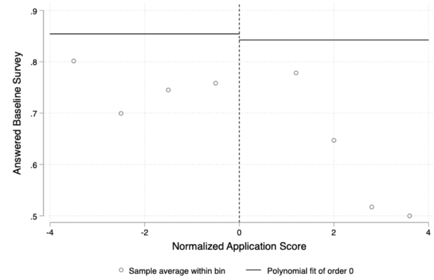



(h) Satisfaction Housing Unit

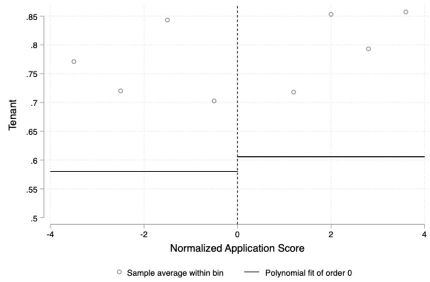
Figure 3: Balance in Baseline Characteristics in Regular Rounds



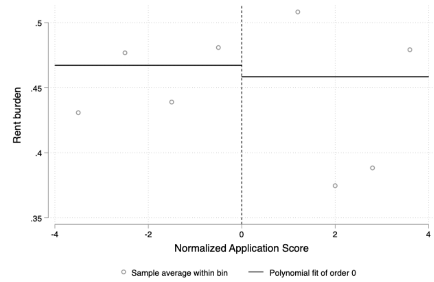

(i) Apply to save for Ownership



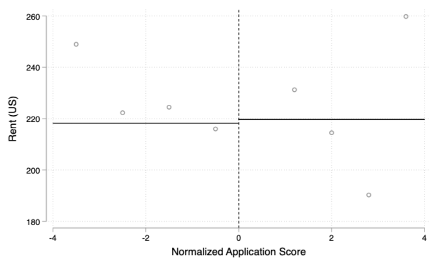

(j) Baseline Survey Response



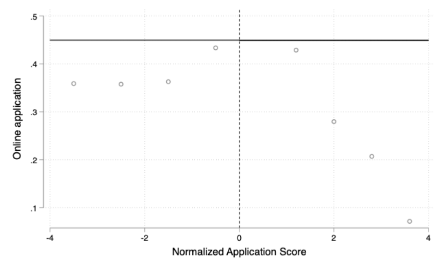

(k) Tenancy



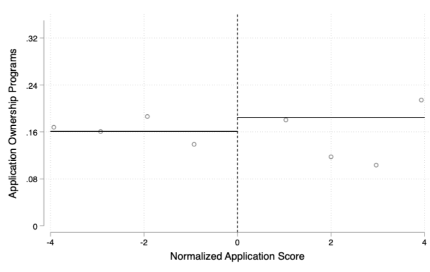

(l) Rent Burden



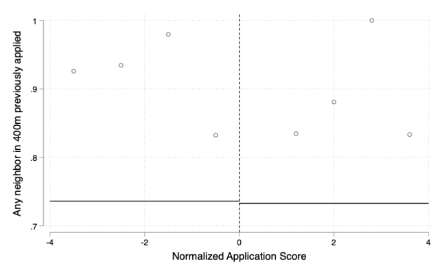

(m) Rent




(n) Online Application

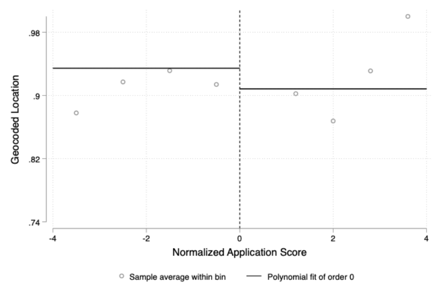



(o) Ownership Program Applications

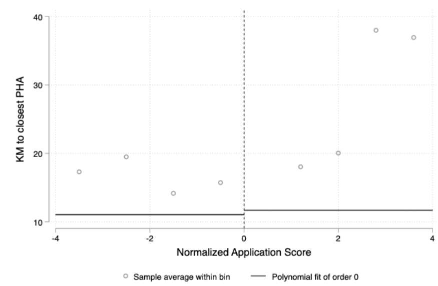



(p) Neighbors Applied in t-1

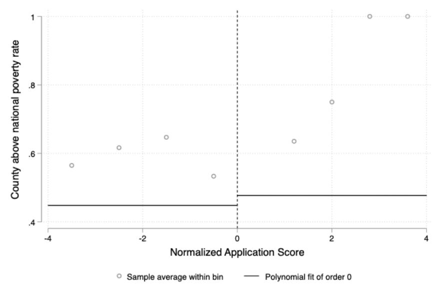
Figure 3: Balance in Baseline Characteristics in Regular Rounds



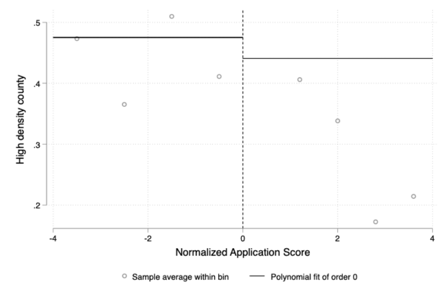

(q) Geocoded Location



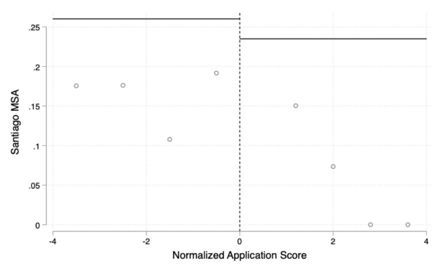

(r) Distance Closest PHA




(s) High Poverty County



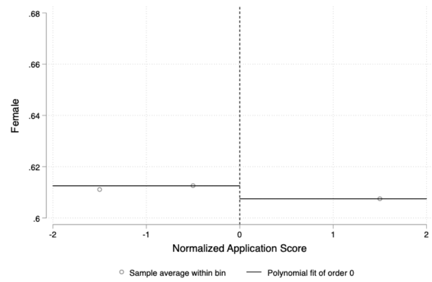

(t) High Density County



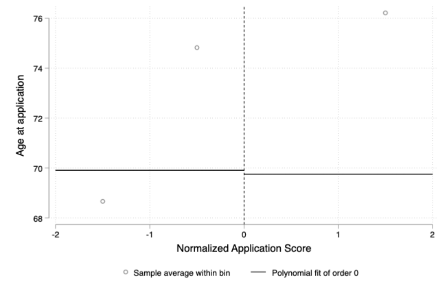

(u) Residence in Santiago MSA

Note: This figure show a graphical representation of balance tests performed in Table 7 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).

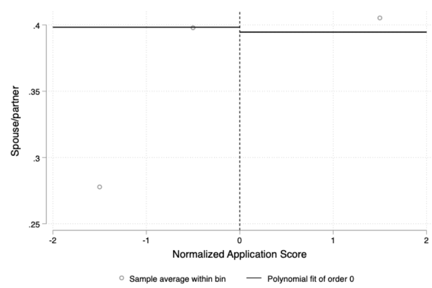
Figure 4: Balance in Baseline Characteristics in Elderly Rounds



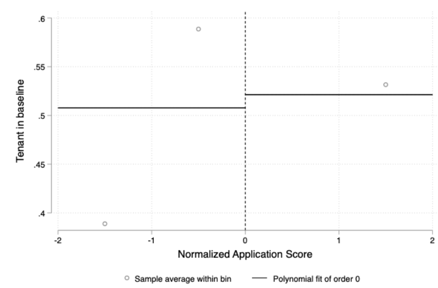

(a) Female



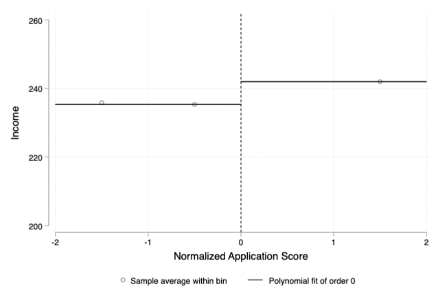

(b) Age



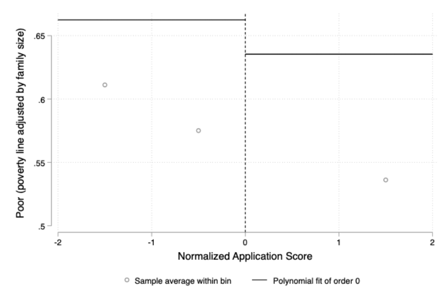

(c) Spouse/Partner



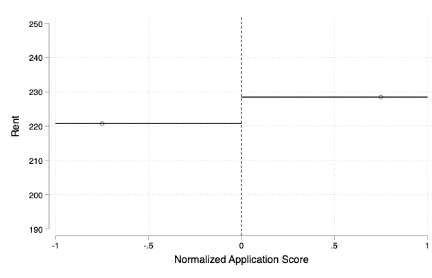

(d) Tenancy



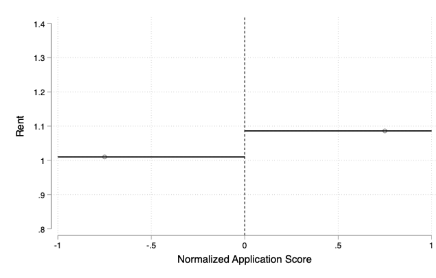

(e) Income




(f) Income Below Poverty Line

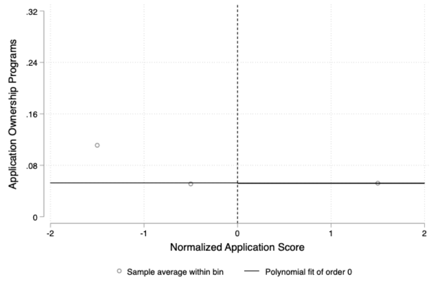



(g) Rent

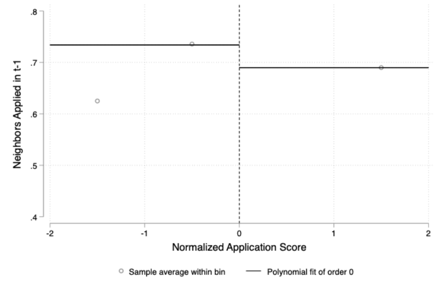



(h) Rent Burden

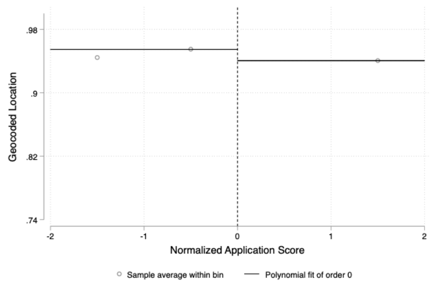
Figure 5: Balance in Baseline Characteristics in Elderly Rounds



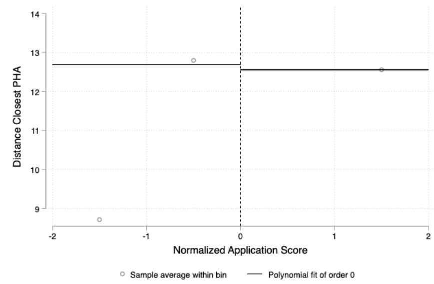

(a) Ownership Program Applications



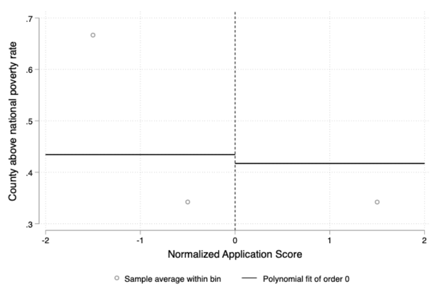

(b) Neighbors Applied in t-1



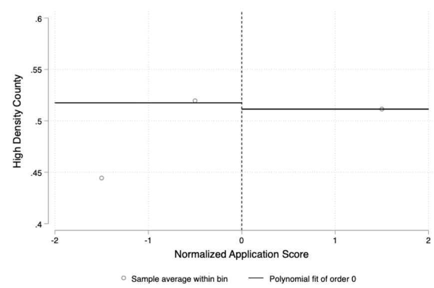

(c) Geocoded Location



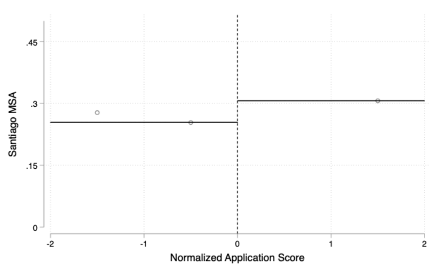

(d) Distance Closest PHA




(e) High Poverty County




(f) High Density County




(g) Residence in Santiago MSA

Note: This figure replicates Figure A4 using data from elderly rounds. See Figure A4 for details.

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

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

Note: This table presents estimates of equation 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 5.1.2. 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 10: Results Elderly Rounds Before the Covid-19 Pandemic: December 2019

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

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

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

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

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

Note: This table presents estimates of equation 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 5.1.2. 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 12: Results Elderly Rounds During the Covid-19 Pandemic: November 2020

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

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

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

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

## A1 Selective Attrition and Balance in the Follow up Sample

*Attrition*

I analyze the presence of selective attrition in the Follow-up Sample. In other words, whether the treatment affected differently the likelihood of responding the survey between units at different sides of the cutoff in the evaluation sample. It is worth mentioning that, while we could not provide monetary incentives, we did several things to reduce the chances of selective attrition.[54]

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.[55] In the evaluation sample, the response rate was 59.5 percent (706) and 27 percent (150) in regular and elderly rounds, respectively.[56] These rates of response are high for online surveys.

Figures A1a and A1b show response rates by assignment and treatment group. Except for some assignments, treated and controls show similar response rates. Moreover, it is not clear whether holding a voucher made it more or less likely to respond the survey.

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

$$Y_i = \alpha + \gamma_s Assignment_s + \tau_s D_{i,s} + \beta_s D_{i,s} x Assignment_s + \delta Z_i + \epsilon_{i,s} \tag{A1.1}$$

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

[54] 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.

[55] 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.

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

balance in section 5.1.2. 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 A2 and A3 show the estimates of $\tau_s$ and $\beta_s$ 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 A2 shows that all individual coefficients, $\tau_s$ and $\beta_s$, 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 A1b 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 A3 shows that while some individual coefficients are statistically significant at the 95 and 90 percent of confidence, the data rules out selective attrition in the overall sample.
*Balance* I analyze balance in the follow-up sample. Even in the absence of selective attrition, the strong assumptions made in the Local Randomization RD framework may not hold in a subset of individuals from the evaluation sample; excluding observations in different mass points around the cutoff may introduce bias. Compared to the continuity approach, the LRRD has the advantage of using a fixed sample, therefore, it is easier to test whether identification assumptions still hold in the sub-sample of follow-up respondents.[57]
Tables A4 and A5 replicate the balance analysis presented in section 5.1.2 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 A4 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

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

significance and the Westfall-Yang test of overall treatment relevance do not provide evidence of imbalance between treatment and control groups. Table A5 shows similar results for elderly rounds. While joint significance is rejected, there are significant differences in two covariates, age and tenancy.

Altogether, the data suggests that treatment did not affect follow up responses and Local Randomization assumptions are still valid within the sub-sample that responded the survey. Nonetheless, given the small sample sizes, specially in elderly rounds, the results in section A1 need to be taken with some caution.

## A2 Appendix Tables and Figures

Table A1: Application Score

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

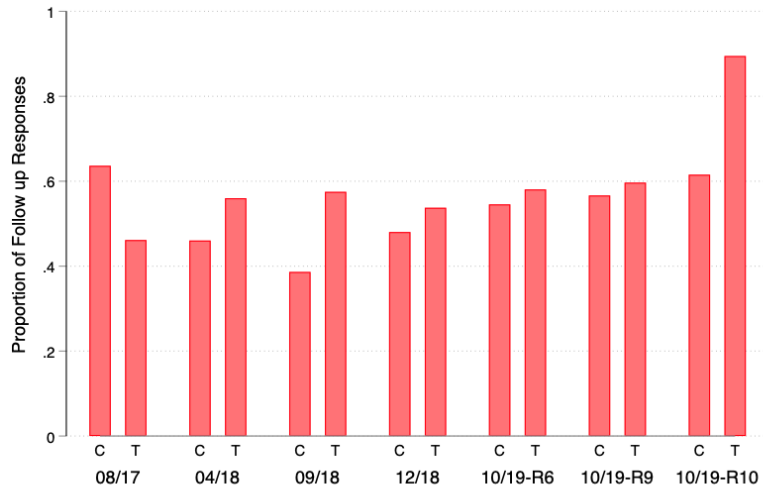
Note: This table presents all score components and how they are taken into consideration to calculate the application score. (*) Applicants are excluded in regular rounds. (**) Age by the end of the application year. Housing Vulnerability score is the sum of scores for crowding, housing quality, access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

Table A2: Follow Up Sample Attrition in Regular Rounds

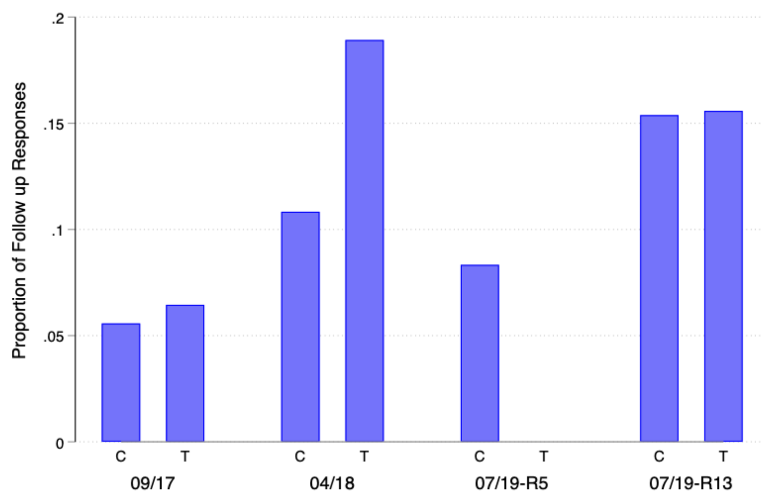
| | Survey Response (1) | Survey Response (2) |
|---|---|---|
| Treat*Assignment April 2018 | 0.081 | 0.082 |
| | (0.034)** | (0.029)** |
| Treat*Assignment Sept 2017 | -0.072 | -0.074 |
| | (0.079)* | (0.068)* |
| Treat*Assignment July 2019 Santiago | -0.079 | -0.086 |
| | (0.494) | (0.454) |
| F-Test (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 A1.1 to analyze the effect of treatment in the non-response of the follow-up survey. Baseline controls in the model in column 2 include income and distance to the closest PHAs and dummy variables for female, age, married, tenant, baseline application to homeownership programs, poor, online application, baseline survey response, living in a high density county, high poverty county. Bottom panel presents p-values for three different analysis of the null of joint significance: F-Test, Randomization-t Joint significance test and the Westfall-Young multiple-testing test of overall treatment irrelevance. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Figure A1: Follow up Sample Attrition by Assignment




(a) Regular Rounds




(b) Elderly Rounds

This figure

Table A3: 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 A2 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 A4: 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 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 7 for further details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A5: 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 8 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 7 for further details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

## Table A6: Results Regular Rounds December 2019 - Follow Up Sample

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

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

Table A7: Results Elderly Rounds December 2019 - Follow Up Sample

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

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

## Table A8: Baseline Characteristics Regular Rounds

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

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

## Table A9: 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 A8 for elderly rounds. See Table A8 for details.

Figure A2: Distribution of households by tenure in Chile in 2013

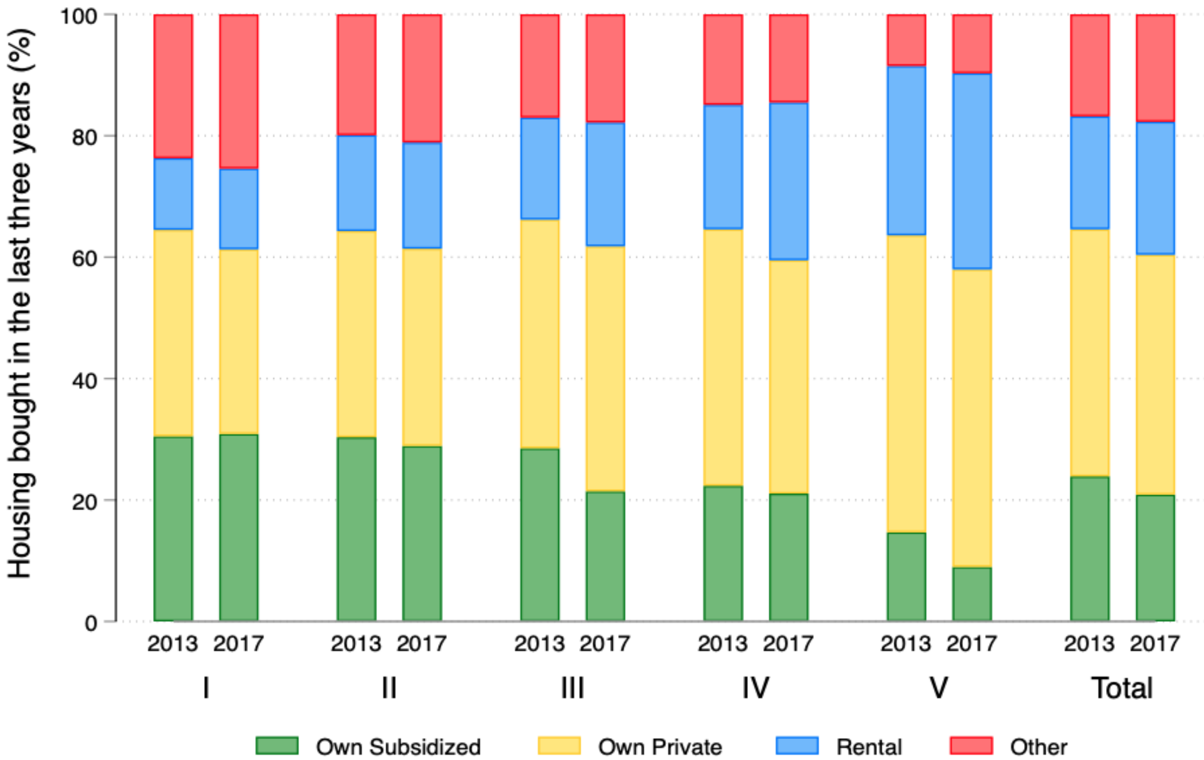
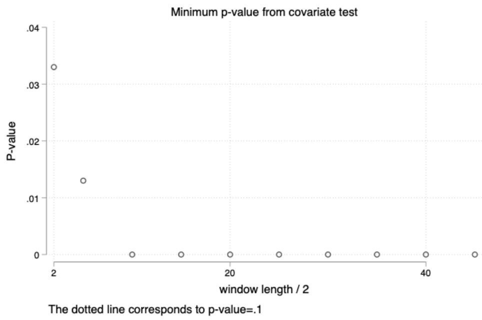
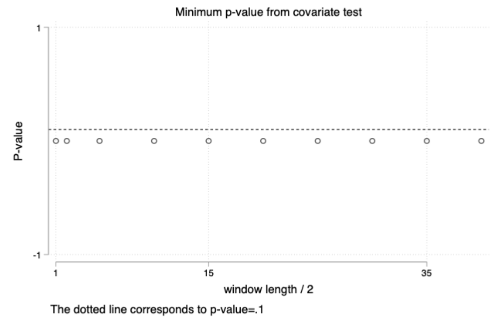


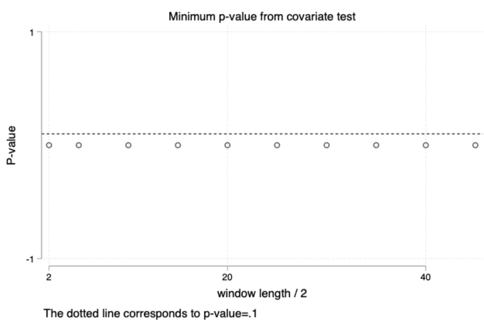
Figure A3: Window Selection by Assignment in Regular Rounds



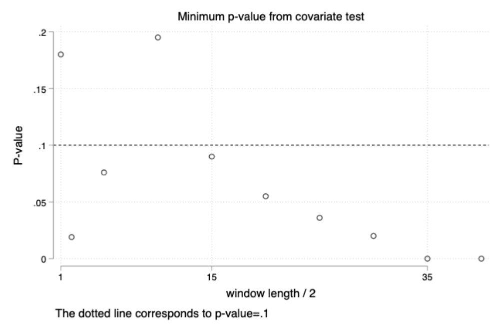

(a) April 2017, National



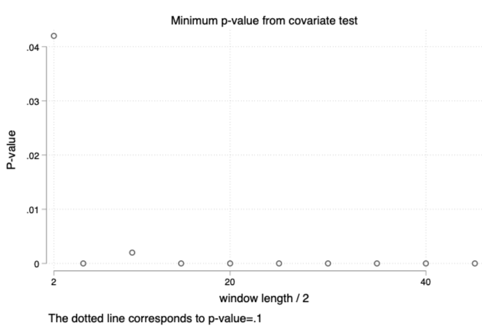

(b) May 2017, National



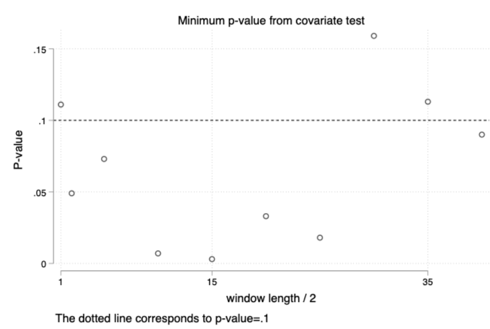

(c) June 2017, National



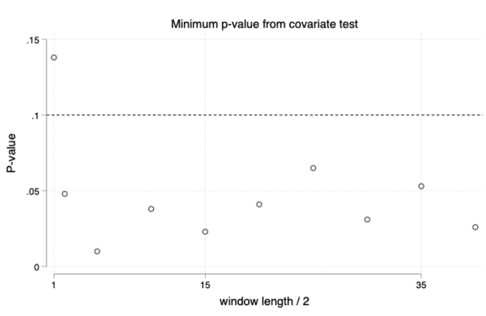

(d) August 2017, National



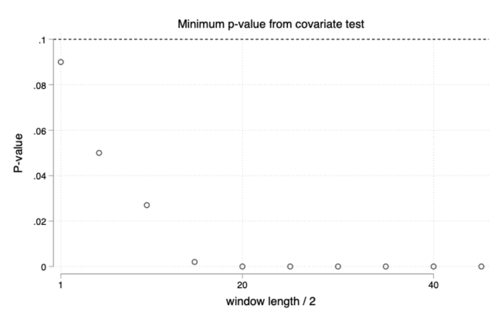

(e) October 2017, National




(f) April 2018, National



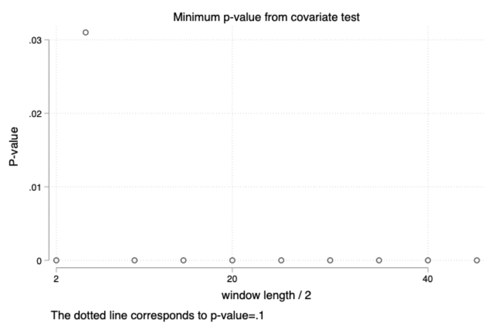

(g) June 2018, National



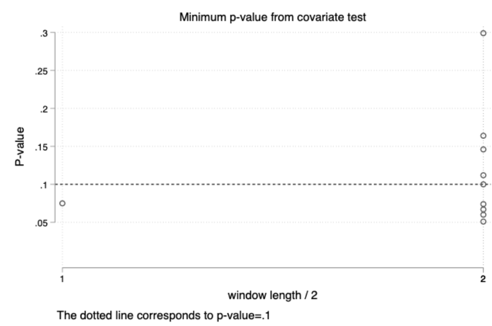

(h) September 2018, National

Figure A4: Window Selection by Assignment in Regular Rounds

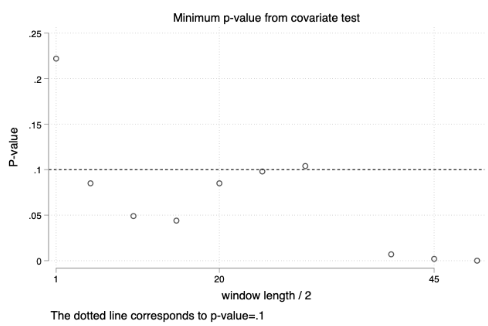
Figure A4: Window Selection by Assignment in Regular Rounds



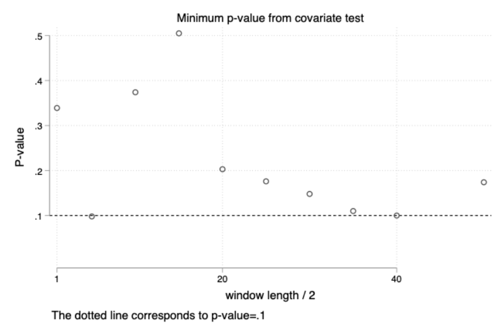

(a) November 2018, National



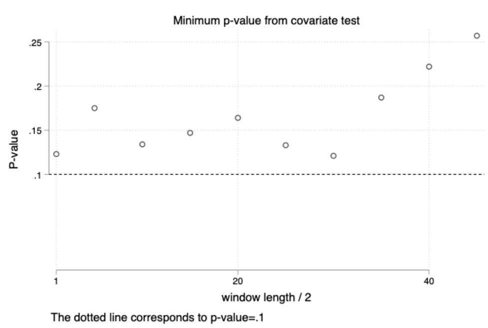

(b) December 2018, National




(c) October 2019, 6th Region



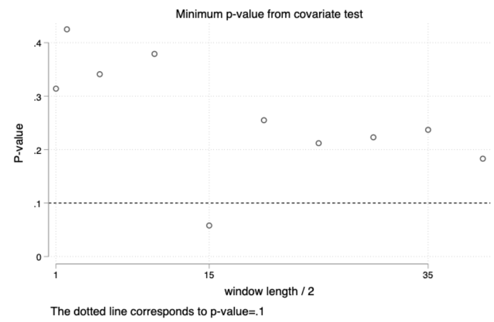

(d) October 2019, 9th Region



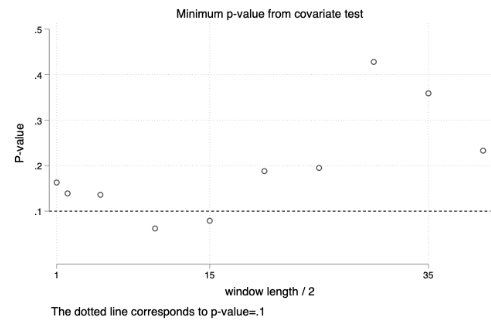

(e) October 2019, 10th Region

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 $\alpha^* = 0.1$.

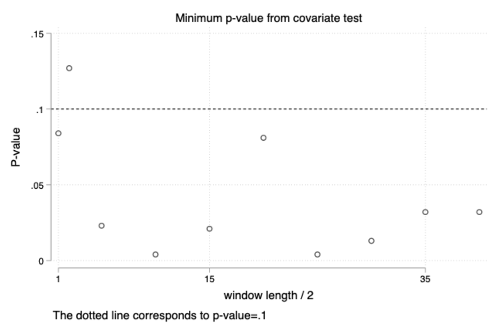
Figure A5: Window Selection by Assignment in Elderly Rounds



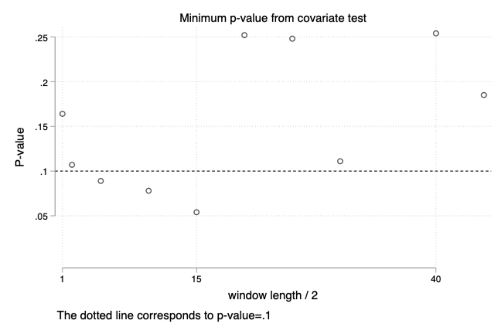

(a) September 2017, National



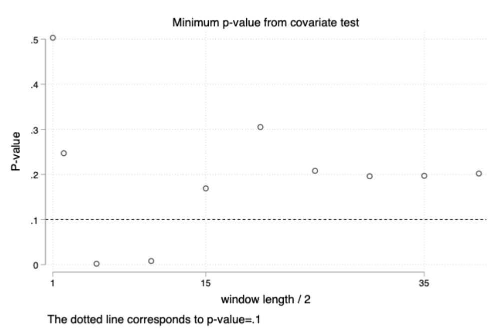

(b) April 2018, National




(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 $\alpha^* = 0.1$.