Rental Voucher Programs in Middle Income Countries: Quasi-experimental Evaluation of the Chilean Rental Subsidy

Javiera Selman *

November 2021

Click *here* for most recent version

Abstract

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

*Postdoctoral fellow, Economics Department and Murphy Institute, Tulane University. Email: javieraselman@tulane.edu. This research was made possible by a data-use agreement between the author and the Ministry of Housing and Urbanism of Chile. The opinions expressed are those of the author alone and do not represent the views of the Ministry. Thanks to Mapcity (www.mapcity.cl) for providing data used in a previous version of this study. I am indebted to my PHD advisors Rajeev Dehejia, Ingrid Gould Ellen and Tatiana Homonoff for their support and guidance, and to Daniel Waldinger and Kathy O'Regan for their useful comments. Thanks to seminar participants at the NYU Furman Center for Real Estate and Urban Policy, NYU-Wagner School, Tulane Economics Department, and to the UEA 11th EU Meeting participants and discussants. I am also grateful for the conversations at different stages of this projects with Hunt Alcott, Peter Bergman, Devin Bunten, Sewin Chan, Raj Chetty, Alejandro Ganimian, Brendan O'Flaherty and policy makers at MINVU. This research was supported by NYU-Wagner. All mistakes are my own.

1 Introduction

In December 2013, advised by the US Department of Housing and Urban Development (HUD) and inspired on the US rental voucher program Section 8, the Chilean Ministry of Housing and Urbanism (MINVU) launched the first rental voucher program in Latin America, the program *Subsidio de Arriendo* (Rental Subsidy).

Rental voucher programs are expected to provide neighborhood choice and increase residential mobility of low-income families towards better neighborhoods, where they would increase their chances of upward social mobility (Chetty et al., 2018).[1] In Chile, expectations were that the program would reduce overcrowding and applications to subsidized ownership programs providing housing in the periphery, specially among young families.[2] To date, several countries in the region, including Argentina, Mexico, Peru, Colombia, Paraguay, Uruguay and Brazil, have followed Chilean steps from providing ownership subsidies exclusively towards incorporating rental assistance to low-income families.[3]

There is a large literature based on evidence from the US Section 8 program showing that rental vouchers have not lived up to their promises. While they have proven to reduce overcrowding and improve other housing quality outcomes, families have generally not gained access to better environments. Differences in policy design and institutional context across countries could have a profound impact on the experiences and outcomes of subsidized households (Colburn, 2021), specially between high and low or middle income countries.

This paper presents the first evaluation of a rental voucher program in a middle income country, Chile. That is, a poorer, more unequal country, with higher levels of informality[4] and a smaller rental market than the US, and where demand-side subsidies have encouraged ownership for decades.

---

[1] As in the Chilean program, the goal of Section 8 in its beginnings was about improving housing conditions. Location was set as a goal later on.

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

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

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

In order to estimate the causal effect of the Chilean rental voucher program on overcrowding, residential mobility, neighborhood characteristics and application to homeownership policies, I exploit the assignment mechanism of the program. MINVU calculates an application score using multiple socioeconomic variables to rank families and assign all available vouchers according to vulnerability. I begin by creating a unique data set that links administrative individual data of all applicants to the program between 2017 and 2019 - including their geocoded address at the time of application and all variables used to calculate their application score - to individual and neighborhood outcomes in December 2019, obtained from different administrative and public data sources. In order to evaluate the program before and after the large unexpected income shock that followed the COVID-19 outbreak in March 2020, I supplement administrative data with a survey to all applicants between September and November 2020 that I designed and implemented it in partnership with MINVU. With this data in hand, I estimate treatment effects using the Local Randomization approach to Regression Discontinuity Designs (LRRD), developed by Cattaneo, Frandsen and Titiunik (2015).

I study the effect of two different rental voucher schemes: a modest monthly voucher for a maximum of three years available to all eligible applicants aged 18 or older and a generous monthly voucher for a maximum of two years for those aged 60 or older. The first voucher scheme is referred as regular rounds and the second scheme as elderly rounds by MINVU. In the period analyzed in this paper, 41,473 and 23,794 individuals applied to regular and elderly rounds of the program, respectively.[5] Of these, 1,356 and 1,777 just above and below the application score cutoff used to select voucher recipients are used in the evaluation.

Pre-pandemic data yielded results similar to evidence from US programs: holding a voucher reduced overcrowding but it did not provide better neighborhoods for low income families in Chile. Results vary between regular and elderly rounds. The voucher reduced overcrowding 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: while it did not affect the chances of moving, voucher holders moved longer distances. Further, application to homeownership programs increased among

[5] 23,561 and 5,927 individuals have been offered a voucher in regular and elderly rounds, respectively.

elderly voucher holders only. Among regular round participants, savings for ownership and applications to homeownership programs were unaffected by the voucher.

The coronavirus outbreak in March 2020 exposed the vulnerabilities of an already existing global housing crisis. High and increasing rents and low and stagnated wages leave low income families with almost no residual income to overcome unexpected income shocks (Ellen, 2020). Therefore, low-income families are highly vulnerable to the long-term negative effects of eviction on well-being (Collinson and Reed, 2018). Many countries have tried to provide income and housing security during the pandemic through eviction moratoriums and emergency rental assistance.[6] In this paper I leverage survey data to investigate whether rental vouchers affect how families cope with large aggregated shocks, like the one that came with the coronavirus pandemic.

During the first eight months of the Covid-19 pandemic, treatment effects around the cutoff show that the rental policy decreased unwanted mobility and rent burden. The data also suggests that voucher holders have responded differently to the unexpected economic shock. Voucher recipients in regular rounds were less likely to miss rent payments and to engage in new activities to complement their incomes. In addition, the data suggests that younger families were less likely to lend or give money to other family members and elder voucher holders became more prone to ask for a formal loan. Altogether, this paper suggests that rental vouchers can have a significant effects in times of economic shocks by increasing income and housing stability.

The contribution of this paper to the literature is twofold. First, it contributes to the empirical work that evaluates rental voucher programs. Overall, the existing evidence suggests that rental vouchers have been effective in reducing rent burden[7], overcrowding, and the homelessness of low-income households but have not been as successful at providing better environments for children to grow up in (Mills et al., 2006; Kling et al., 2007; Jacob and Ludwig, 2012; Chyn, Hyman and Kapustin, 2019; Chetty, Hendren and Katz, 2016; Schwartz et al., 2020). While many low and middle income countries are moving towards rental policies, most of the literature on housing vouchers is based on evaluations of the US program Section 8 in five large cities in the US and cannot be easily extrapolated to other contexts (Andersson et al., 2016). To the best of my knowl-

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

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

edge, this paper is the first evaluation of a rental voucher program offered to low income families to rent a unit in the private market outside of the US.[8]

Secondly, this research contributes to the very few empirical studies on housing policy effects on housing security and the response of low-income families to unexpected income shocks. The evaluation of the Welfare to Work program presented in Mills et al. (2006) shows that among families receiving social assistance, rental vouchers reduce the risk of homelessness and doubling up with other families. A recent observational study by Lundberg et al. (2020) suggests that public housing would be more effective than rental vouchers to reduce evictions. The evaluation of the Chilean rental voucher program differs from previous work in that it presents causal estimates of rental vouchers on behavioral responses during times of economic shocks in a broader population of low income families.

The rest of the paper is organized as follows. The next section provides some background, introducing the Chilean rental voucher program and comparing its differences to the US Section 8 program. Then, Section 3 describes the data and Section 4 explains the research design. Section **??** describes how the evaluation sample is built, discusses the validity of the research design and presents the econometric model that is used to estimate the causal effect of the program. Section 5 presents the results and Section 6 concludes.

2 Policy Context and Design

The Chilean rental voucher program subsidizes the rent paid by low-income households for units that they find in the private rental market. The design of the policy was advised by the US Department of Housing and Urban Development (HUD) and inspired by the US Housing Choice Voucher Program (Section 8), which is the largest federal housing subsidy program in the US, and the focus of the literature on rental voucher programs.

There are two main types of vouchers offered in Chile: regular rounds and elderly rounds of vouchers.[9] Regular rounds target eighteen or older-headed families with monthly income be-

[8] Barnhardt, Field and Pande (2017) evaluate public housing using a lottery among a group of slum dwellers in Ahmedabad, India, to win a rental subsidy to rent a unit administered by the government at the periphery. Fourteen years after the lottery, results show no socioeconomic improvement, no increase in tenure security, isolation from social networks and a reduction of informal insurance.

[9] In Chile, MINVU administers the application process, voucher assignment, leases and subsidy payments. Local housing authorities - SERVIUs in Chile- only provide information about the program, help in-person applicants and process paperwork to activate vouchers in MINVU's web platform.

tween US$250 and US$900,[10] with at least US$180 in private savings for homeownership[11] and who are up to the 70th percentile of the national vulnerability index to assign social benefits in Chile, the *Registro Social de Hogares* (RSH).[12] Voucher holders in these rounds receive US$6,200 in fixed monthly installments of US$180 to pay monthly rents up to the maximum rent payment standard, set nationally at US$402.[13] Voucher holders may space out the use of their total subsidy over an eight year period— although if used continuously, the subsidy lasts for about three years.

Elderly rounds target individuals sixty or older with incomes above $140. Savings are not required. In these rounds, the total subsidy amount and rent coverage vary slightly across four groups, based on their position in the national vulnerability index or RSH. Specifically, less vulnerable voucher holders (61-70th percentiles in the RSH) get a total subsidy of US$7,380 to cover up to 90 percent of their monthly rent, and the most vulnerable recipients (0-40th percentiles in the RSH) are assigned US$7,780 to cover up to 95 percent of rents below the maximum rent standard for two years.[14] In 2019, when the first group of vouchers was about to expire, MINVU extended the benefits for two more years, which was not publicly announced or explained to voucher recipients.

To apply to the Chilean program, families can go online or in person to any of the fifty PHAs (SERVIUs) across the country.[15] Rounds are opened for two to nine months and MINVU can make one or multiple assignments during this period, selecting one to three thousand voucher recipients in each assignment. MINVU uses a complex formula to calculate an application score for each applicant using multiple administrative and self reported data on socioeconomic and housing-related characteristics that they gather from other government agencies.[16]

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

[11]MINVU started asking for saving's requirements to applicants to homeownership programs in the 1990s. The amount asked in the rental voucher programs is about forty percent of what it is asked for the ownership programs for most vulnerable families.

[12]The RSH is administered by the Ministry of Social Development (MDS). The vulnerability index classifies families in seven groups through a score that the MDS calculates using survey and administrative data on educational achievement, income, expenses, health, food security and living arrangements. Groups are defined according to families' position in the national distribution of the score: below 40th, 41th-50th, 51-60th and 61-70th, 71-80th, 80-90th and 90-100th percentiles.

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

[14]Only three percent of voucher recipients are not in the most vulnerable group, none of them are included in this evaluation (Section 4.1).

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

[16]Table A.1 in the Appendix shows each component of the application score. There are few differences between elderly and regular rounds. Overall, the same elder applicant could get 60 to 140 additional points just by applying to

At each assignment, families are ranked by their application score. All applicants above the cutoff - the score of the family who gets the last available voucher- get the subsidy. Within the same round, the program follows a rolling application system. Applicants who are not selected in a previous assignment i.e. are below the cutoff, are re-ranked with all new applicants in the next assignment. Non-voucher recipients are re-ranked until the round closes. To be considered for the next round, they need to apply again.

The number of available vouchers for each assignment is set by decree before the start of each round of applications. Sometimes it might increase or decrease following administrative or political decisions made outside of the rental policy team at MINVU. Importantly, neither the initial quantity or any change to the number of available vouchers is publicly announced. Also unannounced, in 2019 MINVU switched to a regional voucher assignment system, replacing a unique national sorted list of applicants and unique cutoff per assignment by regional sorting of applicants and regional cutoffs per assignment.[17]

Families have two years to find a landlord willing to participate in the program.[18] Voucher recipients that are initially renting can stay in the same house, while those doubling up with other people have to rent a different unit. Landlords cannot be a family members and the rental unit needs to have at least three separated spaces and meet some legal requirements.[19]

Table 1 shows some descriptive statistics of the program. Between 2014 and 2019, MINVU received about ninety thousand applications (column 1) and spent US$325 million in the assignment of fifty thousand rental vouchers (column 2).[20]

Only four out of ten voucher recipients have used their voucher (column 4) in Chile, almost half of the average lease-up rate in the US.[21] Descriptive evidence shows that the chances of using their vouchers are particularly low for migrants, families living in the poorest housing conditions and in areas with tighter housing markets in Chile (Bogolasky, 2021). Studies analyzing administrative

an elderly instead of a regular round.

[17]The changed was made in response to a request made by two local housing authorities from two small regions in the extreme south of the country that complained of getting too few vouchers.

[18]In the US, voucher recipients spend an average of two years on a waiting list to receive a voucher. Once they get a voucher, they have two to four months to lease-up, or they lose the voucher to another family in the waiting list (Collinson and Ganong, 2018).

[19]Have a certificate of occupancy and a registration number at the Chileans tax office.

[20]Just for context, only in 2020 the two largest homeownership programs delivered forty thousand subsidies.

[21]The average lease up rate in the US is 70% but it varies between 35% and 100% across PHAs in the country (Finkel and Buron, 2001).

and survey data show that main barriers to voucher use include high prices, and lack of information and preferences of voucher recipients and landlords. In particular, families have strong preferences for homeownership and residential immobility, and landlords prefer non-subsidized tenants (Selman (2020); Flores and Selman, 2021).

Variation in lease-up rates across countries may reflect differences in both policy design and the rental market. Section 8 is more generous. Rent burden of voucher recipients in the US is fixed at thirty percent and the voucher covers the rest up to the maximum payment standard, set locally for each Metropolitan Area in the Country[22]. Furthermore, benefits do not expired in the US. Families receive assistance as long as they meet with the income requirements of the program.

In Chile, on the other hand, rent burden varies between 40 and 50 percent but it is not fixed. Instead, the amount of the voucher and maximum payment standard is the same for families living in different rental markets throughout the country, with only some exemptions.

Importantly, the size of the rental market in Chile and other Latin American countries is about half of the observed size of the US rental market (Ross and Pelletiere, 2014; Blanco Blanco, Cibils and Miranda, 2014). Rental housing represents twenty percent of the housing stock in Chile. Furthermore, while renting is particularly low among low-income families, ownership remains constant throughout the income distribution: more than sixty percent of families in the first income quintile are homeowners. This number is high even for Latin American countries and has not changed much over time (Andrews and Sánchez, 2011).[23]

3 Data

This paper uses a unique data set including administrative, survey and public data at three different moments in time: i) baseline data gathered at application; ii) outcome data collected in December 2019, before the pandemic; and iii) outcome data collected from September to November 2020, six to eight months after the Covid-19 outbreak in March 2020.

The data set includes individual data and neighborhood level data for each applicant. More specif-

[22]In 2020, the average rent paid by voucher holders was US$355 and the amount of the monthly voucher per family was US$810. See https://www.huduser.gov/portal/datasets/assthsg.html.

[23]Figure A.1 in the Appendix shows tenure by income quintile before and three years after the Chilean rental voucher program started.

ically, using geocoded data[24] and county codes at the time of application I link the administrative data to public geocoded information on neighborhood characteristics: geocoded schools, health care centers, municipalities and PHA location, and county characteristics such as poverty, crime and density. Therefore, two neighborhoods definitions are used in this research: county and the area around applicants location using a 1 or 2 kilometers radius.

Baseline Data. I access application data that MINVU collects to determine applicants eligibility and build the application score. In particular, I have socioeconomic and demographic characteristics, location 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 implemented in partnership with MINVU and included questions about housing and neighborhood experiences, preferences, and beliefs about renting and residential mobility. Answers were collected before assignments were announced.

Also, to replicate voucher assignments I use a unique data set of scores, application dates, and assignment characteristics (round type, dates, cutoffs, etc.) for all applicants to the program.[25]

Outcome data before the Covid-19 outbreak. I collect outcome data for December 2019 from multiple sources. First, unit characteristics, household composition and location were obtained from both the Household Social Registry (RSH) and MINVU's administrative data. Second, I have information on household application to the two largest homeownership programs, the *Fondo Solidario de Vivienda (DS49)* (Funding for Cooperative Housing) and *Subsidio Clase Media (DS1)* (Middle Class Subsidy) between January 2011 and December 2019.[26] Third, for regular rounds only, I also have data on private savings for homeownership.[27]

Outcome data after the Covid-19 outbreak. I partnered MINVU to implement a follow up survey between September and November 2020. The survey included questions to measure crowding, res-

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

[25]I build this data linking different administrative data sets and using public documents containing information for each assignment.

[26]The DS49 provides fully funded housing (no mortgage) for very low income families, who are only required US$300 in savings. The DS1 provides partial funding to low and middle income families. It gives a down payment that decreases with the price of the house and income of the family, available for low and middle income households who can finance the rest of the house with a mortgage loan or savings.

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

idential mobility, neighborhood characteristics, subjective well-being, health, housing and neighborhood satisfaction, income, employment, and asked how families were coping with the Covid-19 pandemic during the first eight months following the outbreak in March 2020.

The next section describes the empirical strategy to evaluate the Chilean rental policy.

4 Research Design

In 2017, following a reform to the national vulnerability index, the application score with which MINVU selects voucher recipients became discrete.[28] Confronted with ties, MINVU had to establish a tie-breaking protocol to handle mass points at the cutoff. A three-step procedure was implemented, selecting voucher recipients within tied applicants first by their family size score and then by their social vulnerability score. Left standing vouchers are randomly assigned.

To evaluate the Chilean rental voucher program I leverage the randomization at the cutoff. However, since only a very small number of regular vouchers are randomly assigned[29], in this research I exploit the assignment mechanisms of the program to implement a multi-cutoff sharp regression discontinuity design (RDD) and present the results of randomly assigned voucher and non-voucher recipients as a robustness check in Section 5.3.

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

Formally, applicants at each assignment $s \in S$ of the program are ranked over their score $X_{i,s}$. Applicants with a score above the cutoff, $X_{i,s} \geq c_s$, are selected into the program. The cutoff c_s vary across assignments since it is the value of the score of the applicant who received the last available voucher in assignment s.

[28] As explained in Section **??** and showed in Table A.1 in the Appendix, both the formula and the sources of information to calculate the application score of the rental voucher program changed with the reform. Before the reform, the score was continuous because the social vulnerability component consider a unique score for each possible value of the national vulnerability index. After the reform, this component has only four possible values, one for each group of the RSH in the target population.

[29] In elderly rounds, randomization has been more frequent than in regular rounds, since the majority of the applicants are in the most vulnerable group of the national vulnerability index (RSH).

Figure 1 shows the sharp discontinuity in treatment status at the cutoff and Figure 2 presents the distribution of application scores and cutoffs in the pooled data. 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.[30]

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[31], the continuity assumption in the standard estimation method used in regression discontinuity settings fails to provide unbiased coefficients and confidence intervals in the smallest possible window $W_0 = [-c_k, c_k]$.[32]

In this context, I estimate treatment effects using the Local Randomization approach to Regression Discontinuity Designs (LRRD), first introduced by Cattaneo, Frandsen and Titiunik (2015).[33] This research design makes it possible to get unbiased estimates in a larger sample size using the actual random variation at the cutoff and the quasi-random variation in a small window around the cutoff. U

4.1 Local Randomization Approach to Regression Discontinuity

Let $Y_{i,s}(1)$ and $Y_{i,s}(0)$ be the pair of potential outcomes under treatment and control in each assignment $s \in S$ and $D_{i,s} = D_{i,s}(X_{i,s}) = I(X_{i,s} \geq c_s) \in \{0,1\}$ the treatment indicator. Then, $Y_{i,s} = D_{i,s}Y_{i,s}(1) + (1 - D_{i,s})Y_{i,s}(0)$ is the observed outcome for individual i (Rubin, 1974).

The LRRD makes strong assumptions about the assignment mechanism near the cutoff (Branson and Mealli, 2018). Instead of modeling assumptions, like the ones used in the standard continuity

[30]Regional screening in 2019, imply that sixteen different voucher assignments occurred at each assignment date in 2019.

[31]The application score in the rental voucher program takes 131 unique values in regular rounds and 109 unique values in elderly rounds.

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

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

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

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

approach, the LRRD assumes that there exists a window $W_0 = [x - e, x + e]$ in which the distribution of the score is known and it is the same for all units, as in experimental data. Inside W_0, potential outcomes may depend on the score only through treatment indicators and there should not be interference between units' potential outcomes ("Stable Unit Treatment Value Assumption" or SUTVA).[34]

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

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

The LRRD approach may be valid only within a few units in a narrow window around the cutoff. Therefore, it uses randomization inference to build exact confidence intervals and hypothesis tests that are robust in small finite samples.[35]

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 by Cattaneo, Frandsen and Titiunik (2015) to select the window in which LRRD assumtions hold.[36] This window is the evaluation sample W_0 that I use to asses the causal effects of the rental voucher program in Chile. In particular, I compare the outcomes of individuals at different sides of the cutoff using regression analysis. Next, I explain how the evaluation sample is built, show the results of falsification tests to analyze the validity of the research design and present the econometric model

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

[35] 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 $Y_i(0) = Y_i(1) = Y_i$.

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

that is used to estimate the causal effects of the program.

4.2 Evaluation Sample

Following Cattaneo, Frandsen and Titiunik (2015), the evaluation sample in this research is the largest window such that the minimum p-value obtained through all balance tests in baseline covariates inside W_j, and any smaller window W_k (with $j > k$) is above a predetermined significance threshold, in this case $\alpha^* = 0.1$.[37]

Having a discrete running variable simplifies considerably the procedure since the minimum possible window is known: local randomization assumptions must hold in the window that contains the two mass points that are immediately above and below the cutoff.

In the Chilean rental voucher program, once a round is opened, every one or two months all applications are screened and available vouchers are assigned. It is at this moment in time that quasi-experimental variation takes place in the rental voucher program: applicants are ranked at each assignment and those above certain cutoff get a voucher and those below the cutoff do not. Therefore, I apply the data driven procedure in Cattaneo, Frandsen and Titiunik (2015) to select the window of analysis for each assignment s.

In these windows, treatment and control units must be in different sides of the cutoff, regardless of the values of the running variable. If there are ties at $c_s = 0$, the running variable require some transformation in $W_s = [0, 0]$. Importantly, any transformation that keeps the same order between mass points produces the same results (Cattaneo, 2018).[38]

To select the evaluation sample, I first build a data set in which each applicant appears as many times as they were screened and assigned to treatment or control groups during the period between March 2017 and September 2019. Initially, the data has 95,910 observations from 56,704 unique applicants that participated in one or more assignments in the period of analysis. I applied two data restrictions over this data before implementing the window selection procedure.

[37]Balance tests are conducted using sharp null hypothesis tests of no difference in mean on pre-treatment covariates and assuming fixed-margin treatment randomization at the assignment level. To be conservative, p-values in balance tests for window selection do not adjust for multiple testing.

[38]In the Chilean rental voucher program, the minimum window is $W_s = [-5, 5]$ when there is no mass point at the cutoff. On the other hand, when there are ties at the cutoff ($W_s = [0, 0]$), I use the assignment rules of the program (See Section 2) to transform the running variable: Ties that were broken using family size or social vulnerability score are re-scaled to be in $W_s = [-2, 2]$ and those remaining vouchers that were randomly assigned are re-scaled to be in $W_s = [-1, 1]$.

First, the control group excludes applicants who are participate in multiple screenings before getting the voucher, or later treated. This data restriction implied dropping 5,361 duplicated observations of later treated.[39] Second, following Cattaneo, Frandsen and Titiunik (2015), I keep assignments that have at least ten observations at each side of the cutoff c_s in the minimum window.[40] This sample restriction implied dropping 30,190 observations.[41]

Altogether, 49,834 observations (37,229 unique applications) in sixteen voucher assignments from March 2017 to September 2019 are used to select the set of W_s that are then to be pooled together in the evaluation sample W_0.[42]

4.2.1 ***Window Selection Results***

In total, 3,447 observations in eleven assignments are included in the evaluation sample W_0: 1,533 in seven assignments in regular rounds and 1,914 in four assignments in elderly rounds.[43] Ninety nine percent of the evaluation sample is in $W_0 = [-15, 15]$. [44] Tables 4 and 5 show summary statistics of the assignments included in the evaluation sample.

Columns 1 through 6 in Tables 6 presents summary statistics for the pooled sample (Column 1) in

[39] Individuals may be considered in more than one voucher screening per round until they either receive a voucher or the round closes. Mixing later treated and applicants who do not receive the voucher in any assignment (never treated) would affect the interpretation of the treatment effect: it would be a weighted average of the effects of holding a voucher (treated vs. never treated) and holding a voucher for a longer period of time (treated vs. later treated). While we may assume the likelihood of participating in more than one assignment is exogenous in a small window around the cutoff and obtain unbiased estimates of these effects, identifying the effect of the program (treated vs. never treated), which I argue that is the first order question for the first evaluation of the Chilean rental voucher program, would requires a larger sample size. See Section 2 for more details on how voucher recipients are selected.

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

[41] Most of the observations dropped are from regular rounds in 2019, after the regional voucher assignment reform was implemented. More specifically, only the regular assignments in October in the Los Lagos, Araucania and O'Higgins regions, and the elderly assignments in July in Santiago and Valparaiso regions are considered. Los Lagos, Araucania and O'Higgins are all regions located south from Santiago. Valparaiso and Santiago are in the center of the country and are the two most populated regions.

[42] One final sample restriction was to drop 3,562 duplicated observations of applicants from the first round in 2018 who were arbitrarily added by MINVU to the second round in 2018. They did not applied again and none of them was selected into treatment. I dropped these observations to keep applicants that have common support in application date.

[43] Assignment in June 2018 in regular rounds met balance criteria but I dropped it from the evaluation sample due to the small number of observations at the treatment group (13) and the relatively small size of the treatment compared to the control group (7%.).

[44] In total, 46% (91%) of the sample in regular (elderly) are applicants that were randomly assigned to treatment and control groups i.e. were in the smallest window possible. The largest window was observed for the assignment in August 2017, the control group had -45 points in the normalized running variable and the treatment group -1.

regular rounds and separately for the treatment (Column 2) and control group (Column 4) in W_0. The sample includes mostly families headed by young single mothers, whose average income is US$529.[45] One fifth of the families are under the poverty line and three fourth are initially tenants, paying almost half of their household income towards rent (US$224). Also, sixty one percent of the sample in regular rounds lives in high poverty counties. Summary statistics for elderly assignments inside of W_0, on the other hand, show that elder applicants are on average 75 years old, 60 percent are women, 40 percent has a partner. Rent burden in this group is roughly 100 percent.[46].

The multi-cutoff setup in the Chilean rental voucher program may introduce heterogeneity in the evaluation sample, reducing the local nature of traditional single cutoff RDD estimates (Cattaneo et al., 2016). To shed light on the external validity of this evaluation, Tables A.2 and A.3 in the Appendix show descriptive statistics for the full sample of voucher recipients in regular and elderly rounds, respectively (column4).

Indeed, there are few differences in baseline characteristics between all voucher recipients and the subset of observations in the evaluation sample in the period of analysis. In general, the evaluation sample over represents the effect of the voucher in families living in poorer counties in Chile, although not necessarily the poorest families within these counties.[47]

Below, I present evidence to support the validity of the LRRD assumptions in the evaluation sample. In particular, I show the results of balance tests in baseline covariates and a density test of the running variable to check for manipulation of the application score in W_0. Then, I detail the econometric model and present the results of the evaluation.

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

[46] Although there are few observations in elderly data with non-missing baseline rent, this is consistent with rent burden in the population of applicants (93 percent) using all observations with non-missing data (N=8,018) (See Table A.3 in the Appendix).

[47] The evaluation sample has a much larger fraction of families living in counties with high poverty rates (61% vs 22%), yet the average individual poverty rate is 12 percentage points lower than in the broader group of voucher recipients in regular rounds.

4.3 Falsification Tests

*4.3.1 **Balance Tests***

As it is common in this literature, window selection uses one set of pre-treatment covariates and another set is kept for further falsification tests of the evaluation sample. The chosen window for each assignment s, W_s, is the largest window around the cutoff in which baseline covariates are balanced. In other words, window selection guarantees that groups at both sides of the cutoff in window W_s for assignment s are balanced in terms of the first set of covariates. That is, income, distance (km) to the closest PHA[48], and indicator variables for tenancy, previous application to homeownership programs, geocoded location, high poverty counties[49] and high density counties. Savings amount and an indicator variable for online application are only available and used for the analysis of regular rounds.

Additional covariates consider in balance tests in this section include age and indicator variables for female, being married or partnered, having family income below the national poverty line adjusted by family size[50], and having neighbors in 400 meters (the average size of a census track in Chile) that applied to previous rounds of the program. Rent amount and rent burden are also included but are available only after September 2018. Also, for regular rounds only, I use baseline survey data to create dummy variables for survey response, strong preferences to stay in baseline neighborhood, high satisfaction with baseline housing unit, and having applied to the rental voucher program to save for ownership.[51] Importantly, both set of covariates include applicants' characteristics that are not observed by MINVU or considered in the selection of voucher recipients.

To analyze statistical balance of groups around the cutoff in each assignment included in the evaluation sample, I estimate the following fully interacted fixed effect model:

$$Z_{i,s} = \alpha + \tau_{1,s} D_{i,s} + \gamma_s Assignment_s + \beta_s D_{i,s} * Assignment_s + \delta S_{i,s} + \epsilon_{i,s} \tag{4.1}$$

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

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

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

[51] This information is available for those who responded the survey, that had a 75% response rate.

Where $Z_{i,s}$ is the vector of baseline covariates, γ_s are assignment fixed effects, and $S_{i,s}$ are score components that determine tie-breaking rules.[52] Coefficients $\tau_{1,s}$ and β_s in equation 4.1 test the null of no effect within each assignment or strata (H_0).

Note that estimating $\tau_{1,s}$ requires covariates that vary between and within assignments. Finding this variation in a small window around the cutoff including observations that were previously balanced in the first set of covariates is not trivial.[53] Therefore, for variables that vary between or within assignments, I use a modified version of equation 4.1, excluding the interaction term.[54]

Columns 7 to 10 in Tables 6 and 7 present the results of balance tests in regular and elderly rounds, respectively. Estimates of $\tau_{1,s}$ show very few statistically significant differences in baseline characteristics between groups above and below the cutoff in the evaluation sample in regular and elderly rounds, but only under OLS inference.[55] Importantly, we find balance in covariates that are not observed by MINVU during voucher assignment (e.g. survey variables, location characteristics, online application) and balance hold including or not specific score components used by MINVU to break ties. Moreover, the F-statistic of the joint significance test of all baseline covariates from a regression of the treatment indicator in all covariates also confirms balance.

4.3.2 ***Manipulation of the Running Variable***

Manipulation of the application score would be very costly for prospective applicants to the rental voucher program, if possible at all. Applicants would need to anticipate voucher availability, their own score and the entire score distribution.[56]

[52] In regular rounds $S_{i,s}$ includes dummy variables for family size and social vulnerability score. Together they explained seventy percent of the total score in these rounds. However, family size and social vulnerability scores do not vary in elderly rounds, therefore, I use dummy variables for score components that are more likely to break ties i.e. the number of elderly in the household and age.

[53] For instance, the fraction of poor families, female, spouse, geocoded location and county poverty rate vary across but not necessarily within assignments in regular rounds; furthermore, all families in regular rounds in W_0 have children. In elderly rounds, all households are in the bottom 40 percent of the vulnerability index (RSH) and have no children. Also, geocoded location, county poverty rate, tenancy and neighbors in previous rounds do not vary within each assignment. Finally, some variables are not available for the entire period of analysis e.g. rent is available only after September 2018 and regional assignments in 2019 eliminated variation by region within assignments.

[54] If we assume $\beta_s = 0$, $\tau_{1,s}$ tests for the null of zero weighted average effect in the pooled data (H_0'). This is a weaker yet commonly use tests in that H_0' could be zero if a specific linear combination of the effects in each assignment is zero, while H_0 is false (Young, 2019; Firpo, Foguel and Jales, 2020).

[55] There is a small statistically significant difference in age and survey response between treated and controls in regular rounds. In elderly rounds, treated are 1.12 years older than the control group. OLS and Randomization-t p-values tend to differ in large variance settings (or under the presence of outliers) which could be explained by differences between, not within assignments, and equation 4.2 includes assignment fixed effects.

[56] Not surprisingly, Tables 2 and 3 show no clear pattern between the number of participants and the number of available vouchers, or between available vouchers and the value of the cutoff.

To formally analyze manipulation in W_0 I use a binomial test of the probability of treatment inside the window W_0 (Cattaneo, 2018). For intuition, if applicants cannot precisely control their value of the score, the probability of success (treatment) q is expected to be consistent with the assignment mechanism assumed in W_0 i.e. randomization of available vouchers at the assignment level.

True q is always unknown (Cattaneo, 2018). Therefore, I use three different values: the observed average probability of assignment in a small window around the cutoff ($q1$); the observed average probability of assignment across all applicants ($q2$); and complete randomization ($q3 = 0.5$).[57]

Table A.4 in the Appendix shows no evidence of manipulation. The observed probability of treatment in the evaluation sample in regular rounds is $q = 0.453$ (column 5), similar to $q1$ and $q3$ and statistically compatible with $q2$ (p-value 0.182). In elderly rounds, the observed average probability of treatment in the evaluation sample is $q = 0.605$, statistically compatible with the observed probability of treatment in a small window around the cutoff $q1 = 0.60$.[58]

4.4 Econometric Model

Finally, to estimate the causal effects of the rental voucher program in the outcome $Y_{i,s}$ of applicant i in assignment s in the evaluation sample W_0, I use the following fixed effect model that exploits variation around the cutoff at each assignment.[59] There is no subscript for rounds because each assignment is unique to a round.

$$Y_{i,s} = \alpha + \tau Voucher_{i,s} + \gamma_s Assignment_s + \beta Z_{i,s} + \delta S_{i,s} + \epsilon_{i,s} \tag{4.2}$$

Voucher in equation 4.2 is an indicator variable for having an application score above the cutoff, $X_{i,s} > 0$, γ_s are assignment fixed effects, the vector $Z_{i,s}$ includes baseline covariates and $S_{i,s}$ comprises score components involved in tie-breaking rules used in Section 4.3.

[57] In regular rounds, the analysis excludes December 2018 assignment. The round had closed in October 2018 and, to spend remaining budget at the end of the year, Minvu pooled together all individuals in the last application process that had not been assigned a voucher, ranked them, and unexpectedly announced a complementary set of eighty vouchers. Hence, this assignment had a higher cutoff value and a disproportionate number of control units. This can hardly be thought as manipulation but it is likely to fail a density test, therefore are excluded in this density analysis.

[58] While a density test is the standard to analyze manipulation in discrete running variable LRRD, in the Chilean rental voucher program some disproportionate number of treated or controls per assignment may be consequence of administrative decisions rather than manipulation by applicants e.g. December 2018.

[59] This research design mimics a sequential stratified experimental design (Pocock and Simon, 1975) in which each assignment s is a strata or block of applicants that are independently assigned to treatment and control groups.

For the period before the pandemic, $Y_{i,s}$ includes overcrowding, residential mobility, savings for ownership[60], application the two largest homeownership programs in Chile and neighborhood characteristics.[61]

In November 2020, eight month after the COVID-19 outbreak, $Y_{i,s}$ comprises overcrowding, residential mobility, housing and neighborhood characteristics and satisfaction, tenure, rent burden, employment, income, health, and families' response to the economic hardship that came with the pandemic.

I estimate equation 4.2 separately for regular and elderly rounds. The parameter of interest, τ, is the normalized and pooled LRRD estimate of the effect of being assigned a voucher, or Intention to Treatment Effect (ITT). I cannot disentangle the heterogeneity by cutoff from the heterogeneity by treatment duration in the evaluation sample.[62] Therefore, despite having multiple cutoffs, this evaluation is focused on the normalized and pooled LRRD estimate of τ that recovers a double average: the weighted average of the average ITT effect within assignments.

The Local Average Treatment Effects (LATE) of using the rental voucher is not reported because of small sample sizes. If we expect those who have not used their voucher to behave similar to those who have, then this evaluation underestimates the effect of the voucher program in Chile.[63] As the program increases and raises its lease-up rates[64], future evaluations of the program could have enough statistical power to estimate the effect of using the voucher. Still, ITTs are relevant from a policy perspective in that evaluates the actual policy (offering a rental voucher), since lease-up cannot be enforced.

[60] Extensive (opened account) and intensive margins (balance) outcomes are included.

[61] Access to pre-schools, schools and health care services (primary care and hospitals) are measured using the distance to the closest service and available supply in one and two kilometers. Neighborhood school quality is measured by average standardized math and language sixth grade tests scores and the fraction of private, public and subsidized schools in one and two kilometers. Distance to commercial activity is approximated by the distance to the closest municipality. Total crime at the county level is measured in standard deviations from the national mean (z-score). Finally, to characterize income composition I include county poverty rate and the fraction of low income schools in the neighborhood i.e. the fraction of schools in which the majority of their students come from low income families.

[62] Each assignment occurs at a particular cutoff in a specific moment in time - and region, since 2019.

[63] LATE is τ adjusted by compliance rates in the treated group (Angrist and Pischke, 2008).

[64] In the evaluation sample is 30 and 50 percent in regular and elderly rounds, respectively.

5 Results

This section presents the results of the evaluation of the rental voucher program using equation 4.2. Tables 8 to 11 present the results separately for regular and elderly rounds in December 2019 and November 2020. In each table, specification 1 includes score components to control for tie-breaking rules, $S_{i,s}$, and Specification 2 controls for $S_{i,s}$ and baseline covariates, $Z_{i,s}$, used in Section 4.3. Since including covariates in $Z_{i,s}$ has efficiency gains and had little impact on the coefficients, this section focuses on the estimates of τ from Specification 2 (Column 8).

I report large-sample based inference (F-test) and Fisherian randomization inference, robust in small samples. Otherwise noted, p-values in parenthesis throughout this section are estimated using Fisherian randomization inference (Column 11).[65] The bottom panel shows the Westfall-Young multiple-testing test of overall treatment irrelevance.

5.1 Treatment Effects in December 2019 - Before the Coronavirus Pandemic

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

5.1.1 Housing, residential mobility and neighborhood characteristics

Panel A in Tables 8 and 9 show the effect of the voucher on housing-related outcomes. Holding a voucher reduced overcrowding in 4.2 percentage points (pp) in regular rounds and 2 pp in elderly rounds.[66] The reduction in overcrowding in elderly rounds is originated by voucher holders living in smaller families and having more available bedrooms. In contrast, a larger number of bedrooms is driving the decrease in overcrowding in regular rounds.

Turning to the effects of the rental voucher in residential mobility (Panel B), I observe large differences across round types. In regular rounds, holding a voucher slightly increased residential mobility (5.7 pp) but it had a large effect on distance: voucher holders moved longer distances

[65] I use the package randcmd in Stata to estimate Randomization-t exact test developed in (Young, 2019)). I use 1000 iterations, re-randomizing the data by assignment and following the assumed stratified experimental design.

[66] 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).

(14.5 km) and were 7.8 pp more likely to move to another county. In elderly rounds, on the other hand, the treatment largely increased mobility (24 pp) but there was no significant effect on distance. In other words, elderly voucher holders are more able to remain in the same environment.

Looking at the counterfactual (Column 2) these effects seem to be large. On the one hand, 56 percent and 68 percent of families in the control group in regular and elderly rounds did not move between application and December 2019, respectively. On the other hand, about thirty percent of movers in the control group in both round types moved one kilometer or less away from their initial location.

Finally, Panel C shows the effects of the voucher on neighborhood characteristics. The rental voucher did not significantly improve neighborhood characteristics in regular or elderly rounds. If anything, the few marginally significant coefficients suggest that families in regular rounds moved to areas farther away from pre-schools and elderly moved closer to municipalities and primary care.

5.1.2 ***Homeownership***

Panel D in Table 8 shows that holding a voucher did not affect application to homeownership programs in regular rounds; coefficients are positive but small and non significant. Furthermore, there was no effect on the extensive or intensive margins of savings i.e. both treated and controls kept their savings account opened with enough savings to apply at least to the fully funded homeownership program (DS 49) (Column 2).

For elderly rounds, on the other hand, the results in Table 9 show that the rental voucher increased application to homeownership programs in 4.4 percentage points, mostly through an increase of applications to the fully funded program (DS 49). Future research could explore how automatic renovation of the rental voucher program changes the cost-benefit analysis of different housing policy options in this population.

Overall, the evaluation of the program before the pandemic shows similar results to the previous literature focused on the Section 8 program: holding a voucher improved housing conditions but it did little to provide better neighborhoods for low income families. These results also suggest that voucher holders in elderly rounds, who receive a subsidy that reduce rent burden almost completely, are more effective in increasing both housing and neighborhoods quality indicators.

Nevertheless, holding a voucher does not discourage homeownership. I now turn to the results of the evaluation in the period after the COVID-19 outbreak in March 2020.

5.2 Treatment Effects in November 2020 - During the Coronavirus Pandemic

This section presents the results of equation 4.2 using the subset of applicants in W_0 who responded the follow-up survey. I begin by discussing survey attrition and the validity of local randomization assumptions in this smaller sample.

The response rate of the survey was high for an online survey -60 percent in regular rounds and 27 percent in elderly rounds. Section B in the Appendix analyzes differences between those who responded and not responded the survey in the evaluation sample and shows the results of balance tests in Section 4.3 for the subset of respondents at different sides of the cutoff in W_0.

Although the results in this period are obtained from a smaller sample and should be taken with some caution, the evidence does not suggest the presence of selective attrition and local randomization assumptions are still valid in the subset of observations inside W_0 who responded the survey. Therefore, coefficients in this section are interpreted as causal estimates of holding a voucher during the pandemic.[67]

Given the particularly small sample size in elderly rounds, the analysis in this section is focused on regular rounds and uses estimates for elderly rounds to highlight important differences across round types.

Results in this section are grouped by residential mobility, housing and household characteristics; neighborhood characteristics, housing and neighborhood satisfaction; and employment, income, health and household response during the Covid-19 crisis.

5.2.1 ***Residential mobility, Housing and Household characteristics***

Regular Rounds

[67] In addition, Table B.5 in the Appendix shows that the effects in December 2019 for the subset of voucher recipients in regular rounds are similar to the estimates for the full sample presented in the previous section. All coefficients are similar but some are not statistically significant in this smaller sample. In elderly rounds, on the other hand, Table B.6 in the Appendix shows that the effects in overcrowding and application to homeownership programs in December 2019 are larger in this smaller sample. The effects in residential mobility and neighborhood characteristics are similar, however, those who responded the survey in elderly rounds moved longer distances, similar to the observed effect in regular rounds.

Column 2 of Panel A in Table 10 shows that eight months into the pandemic, 85 percent of the control group was renting and the voucher had no significant effect on tenure.[68] The policy affected other important housing margins, nonetheless.

First, in a highly informal rental market[69] the voucher increased the probability of having a lease in 11 percentage points and decreased rent burden in 10 pp - the voucher reduced monthly out of the pocket rent payments in about US$45, while it did not affect the average rent amount paid by treated or controls (US$262) and income.

Results in crowding and residential mobility were similar to December 2019: overcrowding decreased in 5.2 pp and holding a voucher increased residential mobility in less than 10 pp. The magnitude of the treatment effects on distance were also similar (19 km) yet not statistically significant in this smaller sample (p-value 0.174).

Finally, compared to the control group, treated were about 9 pp. more likely to have an independent room for the kitchen[70], and a heating system. The coefficient for the probability of having a computer is positive, around 7 pp, but not statistically significant (p-value 0.147). Other housing expenses like cable TV, smart phone or Wifi had a small and not significant effect. The evidence suggests that holding a voucher increased housing-related consumption.

Elderly Rounds

Table 11 shows some important differences between regular and elderly rounds. First, the reduction in the amount of rent paid out of pocket in elderly rounds was US$111, more than twice as much as in regular rounds. Moreover, rent burden was 31 pp lower among the treated, three times the effect found in regular rounds. These results may be a driven by some combination of the larger size of the subsidy assigned to elder households and differences between applicants, including initial housing conditions and location.

5.2.2 ***Neighborhood characteristics and housing and neighborhood satisfaction***

Regular Rounds

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

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

[70] Which is one of the requirements to use the voucher in a certain unit.

The survey included several questions to measure access to amenities in the immediate neighborhood (4 blocks radius). Panel F in Table 10 shows similar results to those in December 2019. The voucher had no effect on neighborhood characteristics. Specifically, voucher holders did not have better access to childcare, schools, transportation, primary care centers, or were closer to family members, friends or their jobs than the control group. If anything, the data suggests that families were farther away from parks and were more exposed to gang fights, yet safety perceptions did not vary between treated and controls.

In addition, eight months into the pandemic, voucher holders in regular rounds were 6 pp more likely to be satisfied with their housing unit (p-value 0.106). However, they were less likely to be willing to ask their neighbors for childcare. While more research is needed to understand whether the reduction in network support for childcare reflects isolation, columns 4 and 8 in Table 10 suggest that this might be case: the coefficient for having close friends in the neighborhood is negative (-6.1 pp) but not statistically significant (p-value 0.173).

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 reduced the exposure to prostitution, destroyed property and graffiti of elderly households. Improvements in street safety could be driven by differences in lock-down's compliance in neighborhoods in which treated and controls lived, rather than changes in long-term neighborhood characteristics. While I cannot disentangle these results, Panel F suggests that this might be the case in that it shows non significant effects on perceived safety.[71]

Finally, elderly voucher holders were 24 pp more likely to be willing to ask for economic help from their neighbors in case of need. This would be consistent with shorter distances moved by those in elderly rounds.

Below, I present the effect of the rental policy on economic outcomes during the pandemic. This is the last set of outcomes of this evaluation.

[71] Furthermore, coefficients are negative for perceived safety outside the house.

5.2.3 *Employment, income, health and household responses during COVID-19 pandemic*

Regular Rounds

Panels C and D in Table 10 (column 2) give a sense of the size of the unexpected economic shock for young low income families in Chile. Roughly 77 percent of voucher recipients in the control group had partial or total income loss during the first eight months after the outbreak. Also, unemployment associated to the pandemic was 18 percent - mostly through suspended contracts of dependent workers and independent workers who could not go out to work during strict quarantines. In terms of mental health, 77 and 64 percent of the sample declared to feel depressed and worried, respectively.[72]

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

The rental voucher did not affect employment, income, or the need to respond to the economic crisis using these strategies. However, the policy did impact how families were coping with the consequences of the large unexpected shock that came with the COVID-19 pandemic i.e. there was an effect in the intensive margin.

The voucher had a positive effect in housing stability during the pandemic. Voucher holders were 4.4 pp less likely to move out because of the pandemic and 14 pp less likely to miss rent payments.[73] However, the voucher did not have a statistically significant effect on shelter deprivation[74] in the short run in regular rounds: eighteen percent in the control group was shelter deprived in November 2020. The effect of the voucher was non statistically significant and small but negative (-2.2 pp; p-value 0.339).[75] Importantly, this could change when the national eviction

[72] To distinguish from serious diagnoses, the survey included the Patient Health Questionnaire-4 (PHQ4) test, a four-questions screening for anxiety and depression. Results show that 16, 44 and 33 percent of the control group were considered to be normal, anxious and depressed using this test, respectively.

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

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

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

moratorium -in place since May 2020- is lifted in December 2021.

Two additional results are worth noting. First, although not statistically significant in this small sample, the effect of the voucher on engaging in new activities to complement family income, lending or giving money to other family members, and delaying monthly billing during this time were negative of sizes between 5 to 8 pp. and p-values smaller than 0.160, even smaller under OLS inference. Finally, the rental policy did not reduced perceived debt overload or the fraction of cases who felt depressed or anxious.

These results open interesting future research questions about the link between rental vouchers and mental health during a crisis. Also, in contrast to previous literature showing negative effects of rental policies on employment in the US (Jacob and Ludwig, 2012), I find a null effect of the voucher on income and employment. Whether these results differ due to the pandemic, lower costs of application for formal workers in the Chilean rental voucher program[76], the smaller and fix size of the Chilean voucher or other differences between the US and Chilean subsidy, are important research questions for future work.

Elderly Rounds

In addition to reducing food expenditure, cutting utility bills and using government relief programs, Panel D in Table 11 shows that the control group (column 2) in this more vulnerable and older population turned also to reducing health expenses.

Interestingly, while the fraction of these households that had use family savings was much lower than in control groups in regular rounds, among elder households the voucher made 13 pp. more likely to ask for a formal loan during this period— for which having a rental lease may be useful.

Similar to regular rounds, rental vouchers seems to contribute to reducing housing instability. The voucher caused a reduction on shelter deprivation of roughly 11.3 pp and coefficients for the effects of the voucher on moving out and missing rent payments are similar to regular rounds, the latter is not statistically significant.

This evidence suggests that rental subsidies may have an important role in the historic struggle in Latin American countries to eradicate informal settlements, moreover, to avoid large setbacks in

[76] As mentioned in Section 4.3, the design of the Chilean rental voucher program makes it easier for formal workers to apply. Baseline survey shows that 85 percent of all applicants were employees at the time of application.

this fight under periods of economic crisis.[77]

5.3 Robustness Checks

To asses the robustness of the quasi-experimental results presented in the previous section, in this section I present the estimates of equation 4.2 using actual voucher randomization: the sample of vouchers that were randomly assigned by MINVU.

This is a smaller sample to evaluate the effect of the voucher in regular rounds (839) and a larger sample in elderly rounds (2,312). Moreover, it contains observations from different assignments than those in the previous evaluation sample since I imposed less data restrictions -no minimum number of 10 observations at each side of the cutoff.

This exercise is useful for assessing the robustness of the estimates presented above to different research designs and to shed light on the external validity of the results to other assignments i.e. different voucher holders in specific moments in time and region. Table A.5 in the Appendix shows balance in this sub sample in regular and elderly rounds.

Tables A.6 and A.7 in the Appendix show the results for regular and elderly rounds using the sample of randomized voucher. Before the pandemic, results in these two samples look very similar. If anything, results are larger in the sample of randomized vouchers. However, conclusions remain the same.[78]

6 Discussion

This research coincided with the COVID-19 pandemic, and offered an unexpected opportunity not just to explore the effects of voucher programs in lower-income countries, but also to explore the effects of voucher programs when families are confronted with global economic shocks. Specifically, this paper studies the effect of the recently new rental voucher program in Chile on housing and neighborhood quality indicators of low income families in Chile, before and after the COVID-19 pandemic outbreak in March 2020.

[77] 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 — 74% according to governments statistics.

[78] I only present results from the period before the pandemic, since sample size in elderly rounds does not increase (181) and it decreases in half for regular rounds (430). In general, results after the COVID-19 outbreak were also similar, specially in elderly rounds. In regular rounds, estimates tend to be larger and while some of them are not significant anymore, others become significant in this sample. Results using the follow up survey are available upon requests to the author.

Results in the period before the pandemic 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 addition, the rental voucher had not decreased application to fully funded homeownership programs that normally provide housing at the periphery (Navarrete and Navarrete, 2016), as was the expectation of the government. Moreover, the voucher increased application among elderly voucher recipients —whose rents are almost fully subsidized— and that, according to this evaluation are not being pushed far away by the rental policy. Some combination of preferences for homeownership, benefit duration and access to more information through the interaction with the PHA might explain this result in elderly rounds.

In the eight months following the COVID-19 outbreak of March 2020, results show that the rental policy affected how families coped with the large unexpected income shock. Voucher holders were less likely to miss their rent payments, experience unwanted mobility and it decreased rent burden. Among the elderly, the voucher seemed to reduced shelter deprivation. Also, this research suggests that younger families were less likely to engage in new activities to complement their incomes and older families were more likely to ask for formal loans.

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

Differences in results for younger families and the elderly rounds suggest that the correlation between the size of the subsidy and the size of the effects might not be constant and may depend on voucher holder baseline vulnerability. Further research is needed to asses the elasticity of these effects to different voucher generosity using similar populations.

References

Andersson, Fredrik, John Haltiwanger, Mark J Kutzbach, Giordano E Palloni, Henry Pollakowski, and Daniel H Weinberg. 2016. "Childhood housing and adult earnings: A between-siblings analysis of housing vouchers and public housing." *NBER working paper*, , (w22721).

Andrews, Dan, and Aida Caldera Sánchez. 2011. "The evolution of homeownership rates in selected OECD countries: Demographic and public policy influences." *OECD Journal: Economic Studies*, 2011: 207–243.

Angrist, Joshua D, and Jörn-Steffen Pischke. 2008. *Mostly harmless econometrics.* Princeton university press.

Barnhardt, Sharon, Erica Field, and Rohini Pande. 2017. "Moving to opportunity or isolation? Network effects of a randomized housing lottery in urban India." *American Economic Journal: Applied Economics*, 9(1): 1–32.

Blanco Blanco, Andrés, V Fretes Cibils, and A Muñoz Miranda. 2014. "Rental housing wanted: Policy options for Latin America and the Caribbean." *Inter-American Development Bank. Fiscal and Municipal Management Division. IV. Title. V. Series*.

Bogolasky, Francisca. 2021. "The Paradox: When Poor Housing Conditions Act as a Barrier to Better Housing." PhD diss.

Branson, Zach, and Fabrizia Mealli. 2018. "The Local Randomization Framework for Regression Discontinuity Designs: A Review and Some Extensions." *arXiv preprint arXiv:1810.02761*.

Cattaneo, Matias D. 2018. "A Practical Introduction to Regression Discontinuity Designs: Volume II."

Cattaneo, Matias D, Brigham R Frandsen, and Rocio Titiunik. 2015. "Randomization inference in the regression discontinuity design: An application to party advantages in the US Senate." *Journal of Causal Inference*, 3(1): 1–24.

Cattaneo, Matias D, Rocío Titiunik, Gonzalo Vazquez-Bare, and Luke Keele. 2016. "Interpreting regression discontinuity designs with multiple cutoffs." *The Journal of Politics*, 78(4): 1229–1248.

Chetty, Raj, John N Friedman, Nathaniel Hendren, Maggie R Jones, and Sonya R Porter. 2018. "The opportunity atlas: Mapping the childhood roots of social mobility." National Bureau of Economic Research.

Chetty, Raj, Nathaniel Hendren, and Lawrence F Katz. 2016. "The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment." *American Economic Review*, 106(4): 855–902.

Chyn, Eric, Joshua Hyman, and Max Kapustin. 2019. "Housing Voucher Take-Up and Labor Market Impacts." *Journal of Policy Analisis and Management*, 38(1): 65–98.

Colburn, Gregg. 2021. "The use of markets in housing policy: a comparative analysis of housing subsidy programs." *Housing Studies*, 36(1): 46–79.

Collinson, Robert, and Davin Reed. 2018. "The effects of evictions on low-income households." *Unpublished Manuscript.[Google Scholar]*, 1–82.

Collinson, Robert, and Peter Ganong. 2018. "How do changes in housing voucher design affect rent and neighborhood quality?" *American Economic Journal: Economic Policy*, 10(2): 62–89.

Ellen, Ingrid Gould. 2020. "What do we know about housing choice vouchers?" *Regional Science and Urban Economics*, 80: 103380.

Ellen, Ingrid Gould, Keren Mertens Horn, and Amy Ellen Schwartz. 2016. "Why Don't Housing Choice Voucher Recipients Live Near Better Schools? Insights from Big Data." *Journal of Policy Analysis and Management*, 35(4): 884–905.

Finkel, Meryl, and Larry Buron. 2001. "Study on Section 8 voucher success rates. Volume I. Quantitative study of success rates in metropolitan areas." *prepared by Abt Associates for the US Department of Housing and Urban Development*, 2–3.

Firpo, Sergio, Miguel N Foguel, and Hugo Jales. 2020. "Balancing tests in stratified randomized controlled trials: A cautionary note." *Economics Letters*, 186: 108771.

Henriquez, Lysette. 2019. "Formalization: The Case of Chile." International Labour Organization.

Jacob, Brian A., and Jens Ludwig. 2012. "The effects of housing assistance on labor supply: Evidence from a voucher lottery." *American Economic Review*, 102(1): 272–304.

Kling, Jeffrey R, Jeffrey B Liebman, Lawrence F Katz, Source Econometrica, No Jan, B Y Jeffrey R Kling, Jeffrey B Liebman, Lawrence E Katz, and Wood Johnson. 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica*, 75(1): 83–119.

Kolesár, Michal, and Christoph Rothe. 2018. "Inference in regression discontinuity designs with a discrete running variable." *American Economic Review*, 108(8): 2277–2304.

Lee, David S, and David Card. 2008. "Regression discontinuity inference with specification error." *Journal of Econometrics*, 142(2): 655–674.

Lee, David S, and Thomas Lemieux. 2010. "Regression discontinuity designs in economics." *Journal of economic literature*, 48(2): 281–355.

Lundberg, Ian, Sarah L. Gold, Louis Donnelly, Jeanne Brooks-Gunn, and Sara S. McLanahan. 2020. "Government Assistance Protects Low-Income Families from Eviction." *Journal of Policy Analisis and Management*, 0(0): 1–21.

Mills, Gregory, Daniel Gubits, Larry Orr, David Long, Judie Feins, Bulbul Kaul, Michelle Wood, Amy Jones, et al. 2006. "Effects of housing vouchers on welfare families." *Washington, DC: US Department of Housing and Urban Development, Office of Policy Development and Research.*, 8(2010): 2.

Navarrete, Pablo, and Nicolás Navarrete. 2016. "Moving "away" from Opportunities?: Homeownership and Employment."

Navarro, Mario. 2005. "Land Lines." *L. LINES*, 37(6).

OECD. 2012. *OECD Economic Surveys: Chile. January 2012.*

Pocock, Stuart J., and Richard Simon. 1975. "Sequential Treatment Assignment with Balancing for Prognostic Factors in the Controlled Clinical Trial." *Biometrics*, 31(1): 103.

Ross, Lauren M, and Danilo Pelletiere. 2014. "Chile's new rental housing subsidy and its relevance to US housing choice voucher program reform." *Cityscape*, 16(2): 179–192.

Rubin, Donald B. 1974. "Estimating causal effects of treatments in randomized and nonrandomized studies." *Journal of educational Psychology*, 66(5): 688.

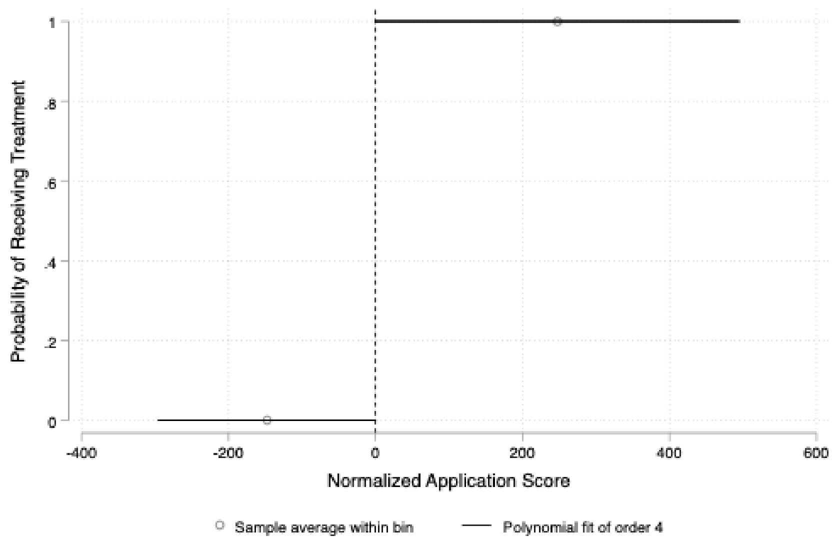
Schwartz, Amy Ellen, Keren Mertens Horn, Ingrid Gould Ellen, and Sarah A. Cordes. 2020. "Do Housing Vouchers Improve Academic Performance? Evidence from New York City." *Journal of Policy Analisis and Management*, 39(1): 131–158.

Selman, Javiera. 2020. "Recomendaciones para el Diseño de un Registro de Arrendadores para el Program Subsidio de Arriendo." Ministerio de Vivienda y Urbanismo de Chile.

Young, Alwyn. 2019. "Channeling fisher: Randomization tests and the statistical insignificance of seemingly significant experimental results." *The Quarterly Journal of Economics*, 134(2): 557–598.

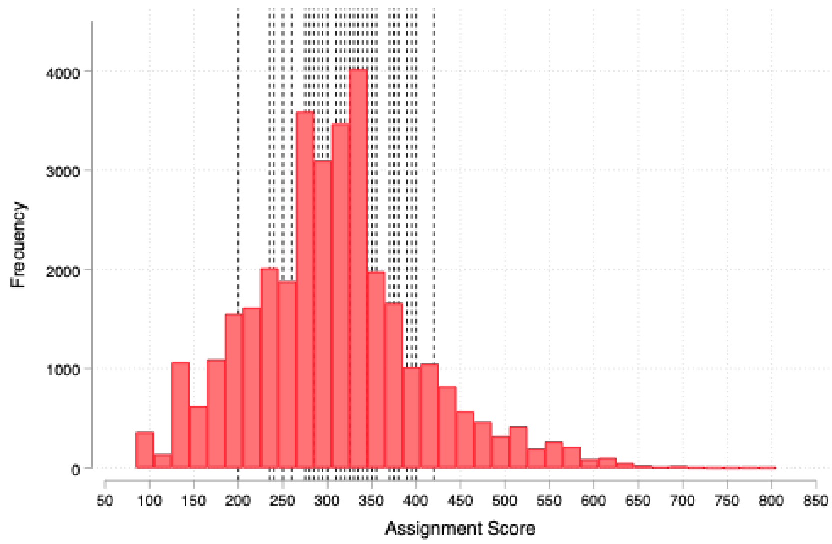
Figures and Tables

Figure 1: Sharp RD Design

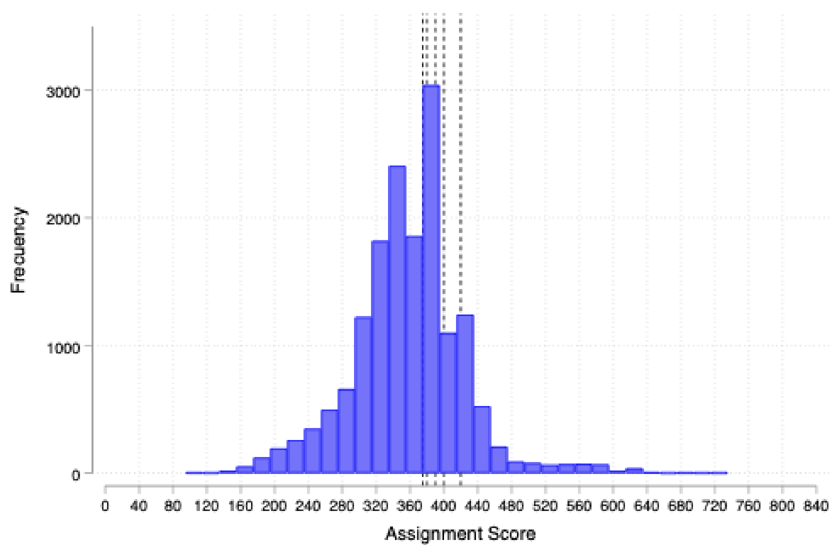


This figure presents treatment probability for different values of the normalized application score.

Figure 2: Multiple Cutoff Regression Discontinuity Design



(a) Regular Rounds



(b) Elderly Rounds

The figure presents the distribution of the application score in (a) regular and (b) elderly rounds in the pooled data. Vertical lines indicate multiple values of cutoff in the program.

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

This table presents descriptive statistics for each round of the program between 2014 and 2019. Regular and Elderly rounds are divided in Panel A and B, respectively. Columns 1 and 2 show the total number of applicants and number of voucher offers 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 by May 2020.

Table 2: Assignments in Regular Rounds

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

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 applicants that were screened. Column 2 and 3 present the maximum and minimum score among all applicants. 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 time of assignment. In June, August and October 2019 columns 1 to 4 aggregate all regional assignments and column 5 shows the average cutoff across all regions.

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

This table replicates the analysis in Table 2 using elderly rounds data. See Table 2 for details.

Table 4: Assignments in Evaluation Sample in Regular Rounds

Assignment Date	Region (1)	Applicants (2)	Cutoff (3)	Controls (4)	Treated (5)	Length (6)	Min pvalue (7)	Left (8)	Right (9)
24aug2017	National	1027	240	17	14	46	.18	-45	1
11apr2018	National	2591	285	61	68	2	.11	-1	1
28dec2018	National	5017	345	575	70	17	.19	-15	2
10oct2019	10	437	275	46	30	10	.11	-5	5
10oct2019	9	730	285	134	110	30	.44	-15	15
10oct2019	6	475	285	19	10	2	.12	-1	1

This table shows descriptive statistics of each assignment in the evaluation sample. Columns 1 describes the region where the assignment takes place)National before 2019). Column 2 shows the total number of applicants considered in window selection in each assignment and column 3 the cutoff. Columns 4 and 5 show the number of individuals below (control) and above (treated) the cutoff. Columns 7 to 9 describe the window selected in each assignment: the minimum p-value of all balance tests using covariates explained in Section 4.3 inside of the selected window, and the length of the window in terms of the minimum and maximum value of the running variable inside the window.

Table 5: Assignments in Evaluation Sample in Elderly Rounds

Assignment Date	Region (1)	Applicants (2)	Cutoff (3)	Controls (4)	Treated (5)	Length (6)	Min pvalue (7)	Left (8)	Right (9)
04sep2017	National	6280	380	314	792	15	.41	-5	10
11apr2018	National	2061	380	230	144	12	.14	-2	10
05jul2019	5	1248	380	105	15	2	.25	-1	1
05jul2019	13	1969	400	18	78	10	.4	-5	5

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

Table 6: Balance in Baseline Characteristics in Regular Rounds

	Summary Statistics						Balance Test			
	Pooled	Control		Treated						
	Mean	Mean	SD	Mean	SD	N	F-test (p)	Rand-t (p)	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Interaction Terms (H0)										
Tenant in baseline	0.75	0.75	0.44	0.76	0.43	1,533	0.309	0.642	0.575	0.596
Saving balance on application day (US)	652.50	666.10	1,163.23	613.86	604.99	1,533	0.200	0.098*	0.269	0.105
Family income (US)	529.34	528.36	191.64	532.12	195.02	1,533	0.602	0.190	0.403	0.208
Poor (poverty line adjusted by family size)	0.20	0.19	0.39	0.22	0.42	1,533	0.702	0.575	0.902	0.588
Online application	0.37	0.37	0.48	0.37	0.48	1,533	0.531	0.450	0.686	0.467
Baseline application to ownership programs	0.15	0.15	0.36	0.16	0.37	1,533	0.736	0.682	0.673	0.676
KM to closest PHA	17.72	16.77	22.25	20.49	26.46	1,380	0.601	0.837	0.578	0.820
High density county	0.40	0.42	0.49	0.37	0.48	1,533	0.894	0.613	0.782	0.607
Age at application	32.66	33.48	9.54	30.33	7.36	1,533	0.000***	0.241	0.000***	0.252
Preferences to stay in the same neighborhood	0.56	0.56	0.50	0.57	0.50	926	0.537	0.357	0.744	0.360
Satisfaction with housing unit	0.64	0.64	0.48	0.65	0.48	994	0.395	0.699	0.352	0.671
Applied to save for ownership	0.26	0.27	0.45	0.24	0.43	904	0.501	0.290	0.571	0.279
Any neighbor in 400m previously applied	0.89	0.90	0.30	0.85	0.35	708	0.981	0.878	0.981	0.878
Answered Baseline Survey	0.83	0.82	0.38	0.84	0.37	1,332	0.117	0.320	0.155	0.326
h0										
Female	0.89	0.88	0.33	0.90	0.30	1,533	0.452	0.470	0.229	0.875
Spouse/partner	0.15	0.16	0.37	0.11	0.32	1,393	0.898	0.143	0.010**	0.226
Rent (US)	224.14	224.20	106.97	223.92	114.27	936	0.836	0.099*	0.750	0.374
Rent burden	0.46	0.46	0.26	0.46	0.25	936	0.755	0.237	0.514	0.552
Geocoded location	0.90	0.91	0.29	0.88	0.33	1,501	0.104	0.685	0.099*	0.379
County above national poverty rate	0.61	0.58	0.49	0.69	0.46	1,259	0.577	0.354	0.636	0.370
Santiago MSA	0.17	0.18	0.39	0.12	0.32	1,091	0.140	0.503	0.177	0.503
Assignment FE							Yes	Yes	Yes	Yes
Score components FE							No	No	Yes	Yes
						N	F-Test			
Joint Significance Test (p-value)						1,533	0.735			

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. Panel A in columns 7 to 10 show balance results from testing the fully interacted model in equation 4.1 (H_0) and Panel B presents results under the weaker null hypothesis (H_0') excluding interaction terms from 4.1. See Section 4.3 for details. Columns 7 and 8 differ from columns 9 and 10 in the controls added in the model. Columns 7 and 9 presents inference using large-sample based inference (F-test) and columns 8 and 10 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). Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table 7: Balance in Baseline Characteristics in Elderly Rounds

	Summary Statistics						Balance Test			
	Pooled	Control		Treated						
	Mean	Mean	SD	Mean	SD	N	F-test (p)	Rand-t (p)	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Interaction Terms (H0)										
Family income (US)	243.00	237.38	102.15	246.66	114.89	1,914	0.228	0.069*	0.210	0.094*
Poor (poverty line adjusted by family size)	0.55	0.57	0.50	0.55	0.50	1,914	0.907	0.853	0.693	0.596
Baseline application to ownership programs	0.06	0.06	0.23	0.05	0.23	1,914	0.066*	0.761	0.146	0.727
KM to closest PHA	12.63	12.56	17.63	12.68	19.21	1,807	0.155	0.067*	0.147	0.070*
High density county	0.51	0.52	0.50	0.51	0.50	1,914	0.893	0.747	0.937	0.645
Female	0.60	0.60	0.49	0.60	0.49	1,914	0.096*	0.519	0.092*	0.359
Spouse/partner	0.40	0.39	0.49	0.41	0.49	1,914	0.998	0.839	0.995	0.992
Age at application	75.39	74.72	6.25	75.84	7.01	1,914	0.157	0.446	0.004***	0.437
h0										
Any neighbor in 400m previously applied	0.71	0.75	0.43	0.69	0.46	1,376	0.839	0.216	0.840	0.266
Rent (US)	225.31	222.01	96.70	231.54	175.17	179	0.312	0.469	0.268	0.410
Rent burden	1.04	1.01	0.54	1.11	0.94	179	0.141	0.965	0.094*	0.997
Tenant in baseline	0.54	0.54	0.50	0.53	0.50	1,914	0.487	0.515	0.523	0.492
Geocoded location	0.94	0.95	0.22	0.94	0.24	1,815	0.651	0.473	0.670	0.484
County above national poverty rate	0.34	0.34	0.47	0.34	0.47	1,914	0.460	0.471	0.459	0.443
Santiago MSA	0.28	0.25	0.43	0.30	0.46	1,640	0.086	0.976	0.100	0.925
Assignment FE							Yes	Yes	Yes	Yes
Score components FE							No	No	Yes	Yes
						N	F-Test			
Joint Significance Test (p-value)						1,914	0.602			

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

Table 8: Effect of Regular Voucher Before the Covid-19 Pandemic (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	1,533	2.897	1.300	-0.053	-0.040	0.488	0.480	-0.070	-0.054	0.338	0.348
Number of bedrooms	1,525	1.786	0.829	0.138	0.166	0.007***	0.007***	0.138	0.166	0.007***	0.006***
Number of people per bedroom	1,524	1.803	0.799	-0.174	-0.218	0.000***	0.000***	-0.181	-0.226	0.000***	0.000***
Overcrowing indicator	1,524	0.123	0.329	-0.043	-0.131	0.013**	0.014**	-0.042	-0.127	0.017**	0.020**
B. Residential Mobility											
Stayed in same unit	1,373	0.564	0.496	-0.071	-0.143	0.035**	0.037**	-0.057	-0.114	0.089*	0.088*
Distance (km)	1,373	6.600	36.547	14.367	0.393	0.068*	0.068*	14.538	0.398	0.064*	0.063*
Distance (km) (Movers)	614	15.133	54.205	26.408	0.487	0.097*	0.119	27.112	0.500	0.088*	0.101
Stayed 1km or less from application location	614	0.354	0.479	-0.054	-0.113	0.254	0.257	-0.052	-0.108	0.276	0.278
Moved to another county	616	0.152	0.359	0.068	0.189	0.070*	0.066*	0.078	0.217	0.041**	0.040**
C. Neighborhood Characteristics											
Distance to closest municipality	1,373	3.887	6.135	0.352	0.057	0.342	0.360	0.256	0.042	0.483	0.488
Distance to closest school (km)	1,373	1.102	3.223	0.441	0.137	0.056*	0.062*	0.365	0.113	0.110	0.124
Distance to closest Pre-Shcool (km)	1,373	1.186	3.835	0.537	0.140	0.027**	0.031**	0.440	0.115	0.062*	0.069*
Distance to closest Primary Care (km)	1,288	1.776	4.088	0.419	0.102	0.134	0.164	0.296	0.072	0.279	0.305
Number of Schools in 1Km	1,373	5.022	4.444	-0.323	-0.073	0.313	0.301	-0.169	-0.038	0.587	0.588
Number of Schools in 2Km	1,373	15.764	13.401	-1.364	-0.102	0.096*	0.092*	-0.698	-0.052	0.336	0.334
Number of Preschool in 1Km	1,373	3.097	2.620	-0.035	-0.013	0.846	0.849	0.040	0.015	0.821	0.817
Number of Health Care in 2km	1,373	5.175	4.688	-0.397	-0.085	0.142	0.135	-0.187	-0.040	0.447	0.437
Fraction of Public Schools 1Km	1,123	0.441	0.287	0.001	0.003	0.968	0.977	-0.004	-0.014	0.855	0.853
Fraction of Subsidized Schools 1Km	1,123	0.521	0.279	-0.006	-0.020	0.798	0.792	-0.001	-0.004	0.963	0.956
Fraction of Private Schools 1Km	1,123	0.038	0.115	0.005	0.041	0.521	0.541	0.005	0.043	0.510	0.526
Mat. SIMCE, 3 Closest School 2km	1,182	263.872	17.051	-0.748	-0.044	0.563	0.542	-0.689	-0.040	0.596	0.588
Mat. SIMCE, 3 Closest School 2km	1,182	250.244	18.504	-1.652	-0.089	0.239	0.235	-1.597	-0.086	0.257	0.255
Fraction of Low Income Schools 1km	1,123	0.591	0.338	0.039	0.115	0.119	0.114	0.033	0.098	0.178	0.180
Fraction of Low Income Schools 2km	1,193	0.569	0.275	0.018	0.065	0.347	0.359	0.013	0.048	0.470	0.469
County poverty rate	1,375	0.109	0.063	0.002	0.029	0.633	0.625	-0.001	-0.011	0.823	0.806
Total crime (County z-score)	1,375	1.185	1.663	-0.141	-0.085	0.200	0.208	-0.046	-0.028	0.579	0.576
D. Homeownership											
Application to Ownership Programs	1,533	0.313	0.464	0.033	0.071	0.254	0.244	0.013	0.028	0.559	0.537
Application to partially funded program (DS1)	1,533	0.228	0.420	0.013	0.032	0.617	0.627	0.000	0.001	0.987	0.987
Application to fully funded program (DS49)	1,533	0.125	0.331	0.026	0.079	0.170	0.169	0.017	0.051	0.375	0.364
Active ownership savings account	1,533	0.915	0.278	0.019	0.067	0.281	0.279	0.011	0.039	0.522	0.531
Balance in ownership savings account (US)	1,406	24.567	34.877	0.359	0.010	0.864	0.862	-0.210	-0.006	0.917	0.906
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)							0.015**				0.016**

This table presents estimates of equation 4.2 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 4.3. Large-sample based inference (OLS p-values) and Fisherian randomization inference (Randomization-t p-values from Young (2019)) are presented in columns 6, 7, 10 and 11. 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 9: Effect of Elderly Voucher Before the Covid-19 Pandemic (2019)

		Control			Specification 1				Specification 2		
	N	Mean	SD	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value	Treatment Effect	Treatment Effect (SD)	OLS p-value	Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Housing Conditions											
Household size Dec 2019	1,914	1.587	1.089	-0.204	-0.187	0.000***	0.000***	-0.206	-0.189	0.000***	0.000***
Number of bedrooms	1,819	1.344	0.729	0.494	0.677	0.000***	0.000***	0.491	0.673	0.000***	0.000***
Number of people per bedroom	1,813	1.242	0.591	-0.360	-0.610	0.000***	0.001***	-0.361	-0.610	0.000***	0.001***
Overcrowing indicator	1,895	0.032	0.176	-0.021	-0.117	0.013**	0.020**	-0.020	-0.116	0.015**	0.023**
B. Residential Mobility											
Stayed in same unit	1,715	0.682	0.466	-0.244	-0.523	0.000***	0.001***	-0.239	-0.513	0.000***	0.001***
Distance (km)	1,715	16.552	121.168	4.272	0.035	0.530	0.521	3.548	0.029	0.591	0.584
Distance (km) (Movers)	800	52.019	210.796	-16.576	-0.079	0.354	0.339	-15.872	-0.075	0.361	0.359
Stayed 1km or less from application location	800	0.290	0.455	-0.005	-0.012	0.891	0.890	-0.009	-0.020	0.816	0.806
Moved to another county	804	0.260	0.440	-0.018	-0.042	0.620	0.608	-0.016	-0.035	0.671	0.663
C. Neighborhood Characteristics											
Distance to closest municipality	1,715	3.906	7.455	-0.789	-0.106	0.029**	0.024**	-0.828	-0.111	0.020**	0.016**
Distance to closest school (km)	1,715	1.154	4.162	-0.207	-0.050	0.188	0.182	-0.208	-0.050	0.184	0.184
Distance to closest Pre-Shcool (km)	1,715	1.180	4.434	-0.243	-0.055	0.164	0.161	-0.246	-0.055	0.157	0.151
Distance to closest Primary Care (km)	1,637	1.666	4.258	-0.266	-0.063	0.134	0.124	-0.290	-0.068	0.101	0.087*
Number of Schools in 1Km	1,715	7.059	5.769	-0.208	-0.036	0.479	0.468	-0.162	-0.028	0.563	0.539
Number of Schools in 2Km	1,715	21.353	15.405	-0.434	-0.028	0.599	0.609	-0.182	-0.012	0.796	0.800
Number of Preschool in 1Km	1,715	3.710	2.952	0.008	0.003	0.959	0.955	0.028	0.010	0.861	0.850
Number of Health Care in 2km	1,715	6.523	5.699	0.006	0.001	0.984	0.989	0.103	0.018	0.707	0.700
Fraction of Public Schools 1Km	1,502	0.396	0.238	-0.001	-0.006	0.919	0.919	-0.003	-0.013	0.824	0.799
Fraction of Subsidized Schools 1Km	1,502	0.536	0.241	0.008	0.032	0.589	0.568	0.009	0.036	0.541	0.528
Fraction of Private Schools 1Km	1,502	0.068	0.140	-0.006	-0.045	0.414	0.392	-0.006	-0.040	0.458	0.430
Mat. SIMCE, 3 Closest School 2km	1,551	264.261	17.770	-0.487	-0.027	0.623	0.638	-0.618	-0.035	0.536	0.555
Mat. SIMCE, 3 Closest School 2km	1,552	251.724	18.221	-0.252	-0.014	0.806	0.827	-0.290	-0.016	0.780	0.798
Fraction of Low Income Schools 1km	1,502	0.455	0.336	-0.001	-0.002	0.969	0.968	0.001	0.002	0.967	0.959
Fraction of Low Income Schools 2km	1,558	0.437	0.267	-0.001	-0.005	0.937	0.931	0.000	0.001	0.986	0.986
County poverty rate	1,719	0.083	0.047	-0.001	-0.019	0.731	0.699	-0.000	-0.000	0.999	0.999
Total crime (County z-score)	1,719	1.737	1.984	-0.008	-0.004	0.942	0.950	0.029	0.015	0.758	0.765
D. Homeownership											
Application to Ownership Programs	1,914	0.122	0.327	0.044	0.135	0.011**	0.014**	0.044	0.134	0.001***	0.001***
Application to partially funded program (DS1)	1,914	0.070	0.255	0.018	0.069	0.186	0.211	0.018	0.072	0.098*	0.106
Application to fully funded program (DS49)	1,914	0.067	0.251	0.033	0.133	0.015**	0.015**	0.032	0.127	0.013**	0.016**
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)							0.015**				0.014**

This table replicates the analysis in Table 8 using elderly rounds data. See Table 8 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table 10: Effect of Regular Voucher During the Covid-19 Pandemic (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	616	0.851	0.357	-0.005	-0.015	0.880	0.854	0.006	0.017	0.862	0.848
Formal Lease	507	0.725	0.447	0.109	0.245	0.013**	0.019**	0.108	0.242	0.016**	0.024**
Total rent (unit)	497	261.550	90.581	-8.309	-0.092	0.402	0.403	-7.315	-0.081	0.451	0.454
Rent paid	481	239.388	103.740	-48.178	-0.464	0.000***	0.000***	-43.466	-0.419	0.000***	0.000***
Rent burden (rent paid)	429	0.498	0.257	-0.114	-0.441	0.000***	0.000***	-0.103	-0.401	0.000***	0.000***
Rent burden (rent amount)	445	0.553	0.253	-0.034	-0.133	0.217	0.243	-0.031	-0.121	0.259	0.291
Shelter deprivation (slum, shared room or other)	718	0.112	0.315	-0.029	-0.093	0.203	0.186	-0.022	-0.071	0.334	0.339
Lives with Parents/Grand parents	529	0.142	0.350	0.001	0.004	0.969	0.973	-0.015	-0.043	0.681	0.710
Living with grandchild	529	0.024	0.154	-0.008	-0.055	0.569	0.612	-0.008	-0.052	0.630	0.652
Spouse/Partner	526	0.308	0.462	0.057	0.123	0.259	0.276	0.025	0.054	0.605	0.602
Child borned since application	497	0.120	0.325	0.038	0.118	0.341	0.355	0.033	0.102	0.426	0.422
Household Size	659	3.322	1.478	-0.141	-0.095	0.267	0.280	-0.140	-0.095	0.259	0.272
Number of bedrooms	645	2.258	0.880	0.068	0.077	0.355	0.345	0.075	0.086	0.300	0.275
Number of people per bedroom	642	1.608	0.827	-0.177	-0.213	0.005***	0.005***	-0.177	-0.214	0.004***	0.005***
Overcrowding indicator	644	0.110	0.313	-0.055	-0.175	0.045**	0.030**	-0.052	-0.165	0.062*	0.047**
Pet Owner	529	0.011	0.103	-0.005	-0.050	0.582	0.632	-0.003	-0.026	0.731	0.873
Laundry Room	564	0.427	0.495	0.001	0.003	0.976	0.977	0.007	0.015	0.881	0.874
Kitchen Room	622	0.816	0.388	0.085	0.219	0.018**	0.015**	0.087	0.225	0.016**	0.012**
Hot water	644	0.877	0.329	-0.035	-0.106	0.314	0.344	-0.041	-0.126	0.236	0.253
Heat system	644	0.777	0.417	0.123	0.295	0.000***	0.001***	0.116	0.278	0.000***	0.001***
Cable TV	639	0.608	0.489	-0.011	-0.022	0.814	0.837	-0.025	-0.051	0.595	0.623
Wifi	639	0.599	0.491	0.011	0.023	0.811	0.807	0.017	0.035	0.715	0.723
Smart Phone Lease	638	0.682	0.466	0.006	0.013	0.887	0.875	-0.007	-0.014	0.882	0.876
Computer	641	0.519	0.500	0.052	0.104	0.264	0.245	0.068	0.137	0.144	0.147
B. Residential Mobility											
Stayed in same unit	565	0.590	0.492	-0.120	-0.245	0.014**	0.017**	-0.083	-0.168	0.089*	0.098*
Distance (km)	461	8.982	49.117	20.874	0.425	0.121	0.128	18.759	0.382	0.141	0.174
Number of moves from application	567	0.683	1.035	0.075	0.073	0.440	0.437	0.015	0.015	0.878	0.873
Less than 6 months current house	711	0.128	0.335	-0.024	-0.072	0.414	0.440	-0.037	-0.112	0.207	0.218
Between 6 months and 1 year current house	711	0.162	0.369	0.096	0.259	0.009***	0.011**	0.083	0.226	0.028**	0.032**
Between 1 and 2 years current house	711	0.222	0.416	0.069	0.167	0.085*	0.083*	0.062	0.149	0.126	0.125
2 or more years current house	711	0.487	0.500	-0.141	-0.282	0.001***	0.000***	-0.108	-0.215	0.012**	0.010**
Less than 6 months current neighborhood	694	0.093	0.290	-0.008	-0.028	0.751	0.784	-0.022	-0.075	0.396	0.433
Between 6 months and 1 year current neighborhood	694	0.138	0.345	0.061	0.176	0.080*	0.092*	0.046	0.134	0.193	0.197
Between 1 and 2 years current neighborhood	694	0.181	0.386	0.055	0.143	0.152	0.148	0.042	0.108	0.279	0.266
2 or more years current neighborhood	694	0.588	0.493	-0.108	-0.219	0.016**	0.013**	-0.066	-0.134	0.137	0.113
C. Employment and Income											
Work	524	0.692	0.462	-0.018	-0.038	0.715	0.695	-0.020	-0.042	0.686	0.669
Covid-19 unemployment	524	0.181	0.386	0.045	0.115	0.279	0.274	0.051	0.132	0.214	0.222
Debt overload	532	0.686	0.465	-0.084	-0.180	0.106	0.108	-0.069	-0.149	0.193	0.185
No income loss after COVID-19	534	0.225	0.418	0.057	0.137	0.213	0.204	0.057	0.135	0.230	0.215
D. Household Response During in Covid-19 Crisis											
Covid-19 response: moved out	528	0.064	0.246	-0.034	-0.137	0.114	0.130	-0.044	-0.180	0.060*	0.076*
Covid-19 response: delayed rent payments	460	0.264	0.441	-0.158	-0.357	0.000***	0.000***	-0.140	-0.317	0.001***	0.000***
Covid-19 response: others moved in	528	0.043	0.203	0.027	0.131	0.361	0.396	0.029	0.141	0.319	0.362
Covid-19 response: reduced food budget	528	0.547	0.498	-0.050	-0.101	0.341	0.352	-0.064	-0.128	0.237	0.256
Covid-19 response: reduced health expenses	528	0.340	0.475	0.024	0.050	0.628	0.624	0.033	0.070	0.499	0.492
Covid-19 response: reduced utilities expenses	528	0.458	0.499	-0.008	-0.017	0.875	0.880	-0.021	-0.041	0.701	0.708
Covid-19 response: delayed monthly billings	528	0.424	0.495	-0.061	-0.123	0.242	0.256	-0.079	-0.160	0.139	0.169
Covid-19 response: informal loan (family/friends)	528	0.365	0.482	-0.036	-0.075	0.475	0.460	-0.042	-0.086	0.418	0.406
Covid-19 response: formal loan or credit	528	0.180	0.384	-0.024	-0.062	0.553	0.563	-0.032	-0.083	0.422	0.433
Covid-19 response: sold expensive goods (vehicle, jewelry, etc.)	528	0.164	0.370	0.001	0.002	0.987	0.985	0.000	0.000	0.996	0.994
Covid-19 response: sold or rented real state/land	528	0.005	0.073	0.015	0.211	0.269	0.227	0.015	0.200	0.315	0.331
Covid-19 response: used family savings	528	0.466	0.500	0.027	0.055	0.607	0.623	0.025	0.050	0.644	0.635
Covid-19 response: new activities to generate more income	528	0.362	0.481	-0.075	-0.156	0.132	0.135	-0.082	-0.170	0.111	0.117
Covid-19 response: gave or lent money to family members	528	0.102	0.303	-0.044	-0.146	0.149	0.151	-0.048	-0.157	0.125	0.129
Covid-19 response: applied/used government emergency solutions	528	0.509	0.501	0.001	0.002	0.983	0.982	0.006	0.012	0.912	0.909
Covid-19 response: none	528	0.059	0.236	-0.021	-0.091	0.313	0.326	-0.024	-0.103	0.261	0.288
Covid-19 response: other	528	0.048	0.215	-0.022	-0.102	0.188	0.238	-0.012	-0.056	0.456	0.483
E. Virus Transmission and Mental Health											
At least one Covid-19 case- Home	505	0.040	0.195	-0.002	-0.011	0.915	0.926	-0.007	-0.038	0.726	0.727
At least one Covid-19 case- Family	505	0.246	0.431	0.005	0.013	0.911	0.909	0.019	0.043	0.700	0.690
At least one Covid-19 case- Friends	505	0.220	0.415	-0.033	-0.079	0.453	0.442	-0.006	-0.015	0.889	0.895
At least one Covid-19 case- Neighbors	505	0.195	0.397	-0.035	-0.089	0.391	0.377	-0.034	-0.085	0.422	0.398
At least one Covid-19 case- Work	505	0.184	0.388	-0.053	-0.137	0.195	0.207	-0.045	-0.116	0.270	0.295
At least one Covid-19 case- Other acquaintance	505	0.347	0.477	-0.010	-0.021	0.842	0.836	-0.012	-0.025	0.813	0.811
Do not know any COVID-19 case	505	0.297	0.457	-0.047	-0.103	0.331	0.327	-0.063	-0.138	0.201	0.202
Good health	517	0.595	0.492	0.034	0.070	0.513	0.544	0.003	0.006	0.957	0.963
Happy	503	0.708	0.455	0.012	0.027	0.802	0.802	-0.017	-0.037	0.729	0.736
Feel depressed	510	0.774	0.419	-0.057	-0.137	0.211	0.192	-0.060	-0.144	0.196	0.181
Feel worried	510	0.643	0.480	-0.051	-0.105	0.330	0.336	-0.067	-0.139	0.199	0.194
PHQ4 Test: Normal	510	0.159	0.366	-0.031	-0.085	0.421	0.419	-0.047	-0.128	0.243	0.241
PHQ4 Test: Anxiety	510	0.435	0.496	0.062	0.124	0.246	0.230	0.082	0.165	0.126	0.110
PHQ4 Test: Depression	510	0.331	0.471	0.038	0.080	0.462	0.466	0.038	0.080	0.463	0.467
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 10: *(Continuation)* Effect of Regular Voucher During the Covid-19 Pandemic (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)	695	0.578	0.494	0.005	0.011	0.907	0.916	0.009	0.019	0.836	0.849
Close to Schools (4 blocks)	694	0.574	0.495	-0.027	-0.055	0.544	0.578	-0.013	-0.026	0.774	0.799
Close to subway/bus (4 blocks)	694	0.635	0.482	0.045	0.093	0.311	0.307	0.063	0.131	0.155	0.157
Close to Park (4 blocks)	695	0.598	0.491	0.060	0.122	0.178	0.179	0.075	0.152	0.093*	0.082*
Close to Health Care (4 blocks)	694	0.455	0.498	0.017	0.034	0.711	0.715	0.042	0.083	0.359	0.374
Less than 15 min commute time to family	472	0.450	0.498	-0.030	-0.060	0.593	0.600	-0.061	-0.122	0.273	0.272
Less than 15 min commute time to friends	422	0.446	0.498	0.029	0.059	0.625	0.631	0.005	0.011	0.926	0.916
Less than 15 min commute time to school	449	0.511	0.501	0.047	0.094	0.402	0.403	0.020	0.040	0.722	0.716
Less than 30 min commute time to work	400	0.671	0.471	-0.038	-0.080	0.507	0.521	-0.043	-0.091	0.450	0.439
Street Alcohol Consumption	515	0.547	0.498	0.079	0.158	0.131	0.119	0.070	0.141	0.184	0.173
Street Drug Consumers	515	0.453	0.498	-0.023	-0.047	0.646	0.638	-0.020	-0.040	0.706	0.705
Street Drug Trafficking	515	0.276	0.448	-0.003	-0.007	0.951	0.956	-0.011	-0.025	0.819	0.800
Destroyed property	515	0.287	0.453	-0.002	-0.004	0.969	0.964	-0.004	-0.009	0.933	0.939
Graffiti	515	0.191	0.393	-0.019	-0.048	0.652	0.656	-0.016	-0.041	0.695	0.701
Gang Fights	515	0.182	0.387	0.076	0.198	0.074*	0.073*	0.075	0.194	0.084*	0.084*
People Carrying guns	515	0.204	0.404	0.015	0.036	0.722	0.716	0.031	0.077	0.428	0.416
Shooting	515	0.409	0.492	0.038	0.076	0.471	0.470	0.064	0.130	0.206	0.210
Prostitution	515	0.039	0.193	0.037	0.190	0.113	0.117	0.033	0.173	0.120	0.136
Feels safe walking at night	516	0.529	0.500	-0.035	-0.071	0.503	0.500	-0.031	-0.062	0.571	0.564
Feels safe inside the house at night	510	0.758	0.429	0.015	0.035	0.739	0.715	0.013	0.030	0.776	0.757
Victim of violence (physical)	508	0.121	0.326	-0.030	-0.091	0.350	0.365	-0.027	-0.082	0.417	0.414
Victim of robbery	482	0.327	0.470	-0.019	-0.041	0.707	0.709	-0.003	-0.006	0.958	0.953
G. Housing and Neighborhood Satisfaction											
Satisfaction current housing unit	708	0.775	0.418	0.061	0.145	0.085*	0.097*	0.058	0.140	0.098*	0.106
Satisfaction current neighborhood	686	0.801	0.400	-0.032	-0.081	0.364	0.366	-0.039	-0.098	0.266	0.258
Would ask neighbors for childcare	663	0.304	0.460	-0.114	-0.247	0.004***	0.008***	-0.098	-0.212	0.015**	0.027**
Has close friends in the neighborhood	665	0.443	0.497	-0.078	-0.156	0.086*	0.091*	-0.061	-0.123	0.188	0.195
Would ask neighbors for economic help	661	0.244	0.430	-0.055	-0.127	0.122	0.146	-0.050	-0.117	0.160	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)							0.029**				0.029**

This table presents estimates of equation 4.2 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 4.3. Large-sample based inference (OLS p-values) and Fisherian randomization inference (Randomization-t p-values from Young (2019)) are presented in columns 6, 7, 10 and 11. 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 11: Effect of Elderly Voucher During the Covid-19 Pandemic (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	143	0.833	0.376	0.042	0.113	0.562	0.579	0.046	0.123	0.533	0.579
Formal Lease	102	0.756	0.435	0.202	0.464	0.021**	0.025**	0.249	0.572	0.005***	0.008***
Total rent (unit)	108	247.048	89.442	64.530	0.721	0.006***	0.005***	55.336	0.619	0.019**	0.026**
Rent paid	109	230.955	94.634	-119.408	-1.262	0.000***	0.001***	-110.854	-1.171	0.000***	0.001***
Rent burden (rent paid)	93	0.550	0.249	-0.321	-1.292	0.000***	0.001***	-0.306	-1.230	0.000***	0.001***
Rent burden (rent amount)	92	0.617	0.238	0.133	0.559	0.039**	0.036**	0.107	0.448	0.117	0.124
Shelter deprivation (slum, shared room or other)	161	0.197	0.401	-0.092	-0.229	0.137	0.137	-0.113	-0.282	0.061*	0.063*
Lives with Parents/Grand parents	121	0.070	0.258	0.030	0.115	0.637	0.677	0.037	0.145	0.589	0.596
Living with grandchild	121	0.186	0.394	-0.112	-0.285	0.205	0.235	-0.114	-0.289	0.239	0.280
Spouse/Partner	121	0.279	0.454	0.029	0.064	0.765	0.750	-0.057	-0.126	0.492	0.491
Household Size	148	3.000	2.019	-0.988	-0.489	0.002***	0.001***	-1.085	-0.538	0.002***	0.000***
Number of bedrooms	143	2.245	1.329	-0.182	-0.137	0.418	0.450	-0.210	-0.158	0.412	0.444
Number of people per bedroom	141	1.416	0.676	-0.355	-0.525	0.006***	0.005***	-0.402	-0.594	0.003***	0.003***
Overcrowding indicator	145	0.115	0.323	-0.113	-0.351	0.094*	0.128	-0.140	-0.435	0.038**	0.031**
Pet Owner	123	0.295	0.462	0.119	0.258	0.165	0.183	0.125	0.271	0.163	0.176
Laundry Room	122	0.391	0.493	-0.026	-0.052	0.813	0.833	-0.074	-0.149	0.522	0.572
Kitchen Room	139	0.824	0.385	0.058	0.152	0.350	0.372	0.042	0.108	0.544	0.579
Hot water	147	0.852	0.359	0.119	0.333	0.047**	0.055*	0.101	0.280	0.107	0.094*
Heat system	145	0.717	0.455	0.022	0.048	0.805	0.775	0.049	0.109	0.570	0.574
Cable TV	146	0.593	0.496	-0.078	-0.158	0.420	0.425	-0.095	-0.192	0.281	0.274
Wifi	141	0.440	0.501	-0.037	-0.074	0.713	0.696	-0.006	-0.012	0.952	0.950
Smart Phone Lease	146	0.509	0.505	-0.035	-0.070	0.710	0.713	-0.067	-0.133	0.491	0.504
Computer	141	0.412	0.497	-0.030	-0.061	0.762	0.754	0.005	0.011	0.958	0.962
B. Residential Mobility											
Stayed in same unit	128	0.646	0.483	-0.221	-0.457	0.036**	0.038**	-0.226	-0.468	0.041**	0.044**
Distance (km)	112	58.332	353.328	-55.206	-0.156	0.474	0.749	-54.245	-0.154	0.525	0.783
Number of moves from application	128	0.750	1.313	-0.085	-0.064	0.759	0.730	-0.110	-0.084	0.692	0.670
Less than 6 months current house	161	0.049	0.218	-0.005	-0.021	0.923	0.870	-0.002	-0.010	0.963	0.954
Between 6 months and 1 year current house	161	0.115	0.321	0.013	0.041	0.837	0.851	0.010	0.030	0.884	0.885
Between 1 and 2 years current house	161	0.082	0.277	0.233	0.841	0.000***	0.000***	0.261	0.944	0.000***	0.000***
2 or more years current house	161	0.754	0.434	-0.241	-0.555	0.005***	0.002***	-0.269	-0.619	0.002***	0.002***
Less than 6 months current neighborhood	157	0.033	0.181	-0.005	-0.028	0.901	0.817	0.000	0.000	0.999	0.999
Between 6 months and 1 year current neighborhood	157	0.100	0.303	-0.018	-0.058	0.732	0.742	-0.015	-0.048	0.802	0.833
Between 1 and 2 years current neighborhood	157	0.067	0.252	0.183	0.728	0.001***	0.002***	0.196	0.778	0.001***	0.002***
2 or more years current neighborhood	157	0.800	0.403	-0.160	-0.398	0.039**	0.033**	-0.181	-0.449	0.025**	0.028**
C. Employment and Income											
Work	20	0.714	0.488	0.200	0.410	0.552	0.552	1.774	3.635	0.005***	0.048**
Covid-19 unemployment	20	0.286	0.488	-0.282	-0.578	0.330	0.429	-1.261	-2.585	0.002***	0.031**
Debt overload	124	0.733	0.447	-0.109	-0.243	0.266	0.267	-0.095	-0.213	0.354	0.347
No income loss after COVID-19	117	0.419	0.499	-0.018	-0.036	0.862	0.881	0.023	0.046	0.831	0.851
D. Household Response During in Covid-19 Crisis											
Covid-19 response: moved out	119	0.068	0.255	-0.043	-0.169	0.435	0.490	-0.044	-0.171	0.402	0.422
Covid-19 response: delayed rent payments	104	0.200	0.406	-0.156	-0.385	0.067*	0.077*	-0.192	-0.472	0.026**	0.025**
Covid-19 response: others moved in	120	0.159	0.370	-0.095	-0.258	0.220	0.226	-0.088	-0.238	0.295	0.296
Covid-19 response: reduced food budget	120	0.467	0.505	0.090	0.178	0.382	0.405	0.121	0.241	0.237	0.251
Covid-19 response: reduced health expenses	121	0.422	0.499	0.000	0.001	0.996	0.995	0.001	0.001	0.996	0.993
Covid-19 response: reduced utilities expenses	120	0.400	0.495	0.017	0.035	0.868	0.858	0.056	0.112	0.610	0.600
Covid-19 response: delayed monthly billings	120	0.378	0.490	-0.111	-0.226	0.260	0.246	-0.082	-0.167	0.465	0.450
Covid-19 response: informal loan (family/friends)	119	0.318	0.471	-0.102	-0.215	0.283	0.258	-0.094	-0.199	0.350	0.319
Covid-19 response: formal loan or credit	119	0.068	0.255	0.077	0.301	0.175	0.171	0.127	0.499	0.035**	0.033**
Covid-19 response: sold expensive goods (vehicle, jewelry, etc.)	120	0.114	0.321	0.015	0.047	0.832	0.817	-0.013	-0.041	0.867	0.853
Covid-19 response: used family savings	120	0.273	0.451	-0.002	-0.004	0.986	0.978	0.016	0.035	0.881	0.854
Covid-19 response: new activities to generate more income	119	0.227	0.424	0.007	0.016	0.939	0.931	-0.006	-0.013	0.951	0.952
Covid-19 response: gave or lent money to family members	119	0.091	0.291	-0.052	-0.177	0.424	0.419	-0.008	-0.027	0.919	0.932
Covid-19 response: applied/used government emergency solutions	120	0.500	0.506	-0.032	-0.064	0.760	0.731	-0.117	-0.232	0.270	0.253
Covid-19 response: none	119	0.091	0.291	0.085	0.292	0.170	0.170	0.095	0.326	0.137	0.135
Covid-19 response: other	119	0.159	0.370	-0.057	-0.154	0.496	0.497	-0.082	-0.223	0.356	0.374
E. Virus Transmission and Mental Health											
At least one Covid-19 case- Home	114	0.071	0.261	-0.020	-0.076	0.740	0.805	-0.040	-0.152	0.520	0.557
At least one Covid-19 case- Family	114	0.190	0.397	0.066	0.167	0.421	0.429	0.044	0.111	0.628	0.631
At least one Covid-19 case- Friends	114	0.143	0.354	-0.031	-0.088	0.665	0.643	-0.023	-0.064	0.766	0.783
At least one Covid-19 case- Neighbors	114	0.119	0.328	-0.026	-0.080	0.688	0.732	-0.023	-0.071	0.731	0.768
At least one Covid-19 case- Work	114	0.024	0.154	-0.011	-0.074	0.768	0.743	-0.016	-0.104	0.719	0.723
At least one Covid-19 case- Other acquaintance	114	0.286	0.457	-0.030	-0.065	0.746	0.773	-0.035	-0.077	0.700	0.715
Do not know any COVID-19 case	114	0.381	0.492	0.077	0.156	0.461	0.492	0.123	0.251	0.259	0.296
Good health	120	0.318	0.471	-0.029	-0.061	0.755	0.786	-0.085	-0.181	0.399	0.421
Happy	117	0.558	0.502	0.003	0.006	0.979	0.980	-0.033	-0.067	0.770	0.768
Feel depressed	108	0.842	0.370	-0.120	-0.324	0.172	0.166	-0.127	-0.344	0.184	0.187
Feel worried	108	0.605	0.495	-0.005	-0.010	0.966	0.966	0.023	0.046	0.843	0.856
PHQ4 Test: Normal	108	0.184	0.393	-0.063	-0.160	0.484	0.471	-0.087	-0.220	0.336	0.327
PHQ4 Test: Anxiety	108	0.395	0.495	0.066	0.133	0.558	0.561	0.052	0.106	0.655	0.650
PHQ4 Test: Depression	108	0.263	0.446	0.089	0.199	0.404	0.387	0.073	0.164	0.511	0.503
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: *(Continuation)* Effect of Elderly Voucher During the Covid-19 Pandemic (2020)

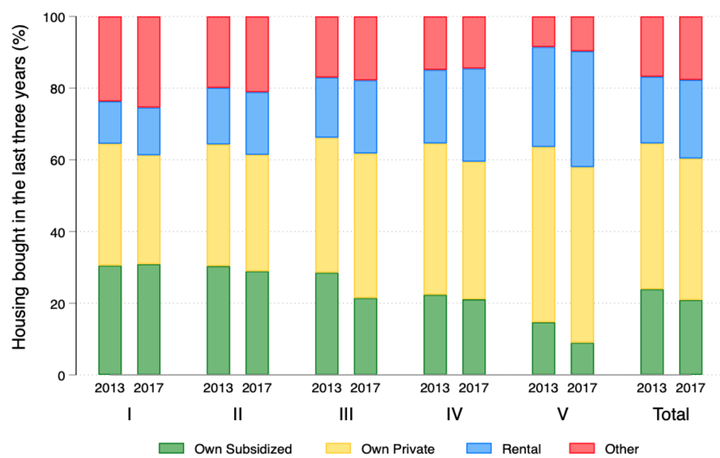
				Specification 1				Specification 2			
		Control		Treatment	Treatment	OLS	Rand-t	Treatment	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	Effect (SD)	p-value	p-value	Effect	Effect (SD)	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
F. Neighborhood Characteristics											
Close to childcare/pre-school (4 blocks)	154	0.357	0.483	0.014	0.029	0.883	0.894	-0.005	-0.009	0.964	0.894
Close to Schools (4 blocks)	154	0.643	0.483	-0.054	-0.111	0.563	0.545	-0.056	-0.115	0.562	0.562
Close to subway/bus (4 blocks)	155	0.825	0.384	-0.065	-0.169	0.399	0.409	-0.070	-0.182	0.365	0.377
Close to Park (4 blocks)	154	0.554	0.502	0.029	0.057	0.763	0.770	0.007	0.015	0.941	0.945
Close to Health Care (4 blocks)	154	0.518	0.504	-0.004	-0.008	0.966	0.962	-0.020	-0.040	0.826	0.834
Less than 15 min commute time to family	91	0.375	0.492	-0.155	-0.315	0.191	0.202	-0.195	-0.396	0.114	0.137
Less than 15 min commute time to friends	76	0.444	0.506	-0.044	-0.087	0.741	0.750	-0.113	-0.224	0.406	0.431
Street Alcohol Consumption	118	0.595	0.497	-0.010	-0.019	0.929	0.923	-0.033	-0.066	0.788	0.780
Street Drug Consumers	118	0.500	0.506	-0.020	-0.040	0.853	0.847	-0.034	-0.068	0.754	0.760
Street Drug Trafficking	118	0.190	0.397	-0.028	-0.069	0.726	0.726	-0.037	-0.093	0.633	0.644
Destroyed property	118	0.548	0.504	-0.268	-0.532	0.010***	0.012**	-0.265	-0.527	0.010**	0.011**
Graffiti	118	0.286	0.457	-0.113	-0.248	0.182	0.195	-0.111	-0.243	0.208	0.207
Gang Fights	118	0.167	0.377	-0.048	-0.127	0.505	0.506	-0.041	-0.109	0.521	0.515
People Carrying guns	118	0.190	0.397	-0.033	-0.083	0.651	0.624	-0.025	-0.062	0.739	0.725
Shooting	118	0.524	0.505	-0.154	-0.305	0.148	0.145	-0.231	-0.458	0.044**	0.039**
Prostitution	118	0.143	0.354	-0.128	-0.361	0.039**	0.060*	-0.140	-0.395	0.035**	0.046**
Feels safe walking at night	118	0.442	0.502	-0.098	-0.196	0.353	0.352	-0.097	-0.192	0.375	0.366
Feels safe inside the house at night	118	0.721	0.454	0.064	0.141	0.479	0.461	0.089	0.196	0.327	0.328
Victim of violence (physical)	106	0.056	0.232	0.005	0.020	0.937	0.905	0.009	0.039	0.889	0.875
Victim of robbery	99	0.333	0.479	0.016	0.034	0.879	0.871	-0.036	-0.076	0.741	0.742
G. Housing and Neighborhood Satisfaction											
Satisfaction current housing unit	153	0.807	0.398	0.080	0.201	0.219	0.209	0.089	0.224	0.164	0.175
Satisfaction current neighborhood	150	0.873	0.336	0.005	0.016	0.940	0.928	-0.013	-0.040	0.821	0.823
Would ask neighbors for childcare	137	0.412	0.497	0.146	0.295	0.135	0.126	0.136	0.274	0.180	0.169
Has close friends in the neighborhood	144	0.596	0.495	0.108	0.218	0.278	0.307	0.128	0.258	0.204	0.221
Would ask neighbors for economic help	145	0.240	0.431	0.243	0.563	0.003***	0.005***	0.241	0.559	0.006***	0.007***
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**				0.012**

This table replicates the analysis in Table 10 using elderly rounds data. See Table 10 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Appendix

A Additional Figures and Tables

Figure A.1: Tenure by Income Quintile in Chile



This Figure shows tenure distribution by income quintile in 2013 and 2017 in Chile using the 2017 National Socioeconomic Survey (CASEN).

Table A.1: 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

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 A.2: 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	

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 A.3: 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	

This table replicates the analysis in Table A.2 using elderly rounds data. See Table A.2 for details

Table A.4: Density Test

Assumed q	qi (1)	N (2)	Obs T (3)	Exp T (4)	Obs q (5)	p-value (6)
Regular (No December 2018 Assignment)						
q1	0.551	704	319	388	0.453	0.000
q2	0.428	704	319	301	0.453	0.182
q3	0.500	704	319	352	0.453	0.014
Elderly						
q1	0.604	1,914	1,158	1,156	0.605	0.926
q2	0.263	1,914	1,158	503	0.605	0.000
q3	0.500	1,914	1,158	957	0.605	0.000

This table presents the results of a 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 (50%). The results exclude the assignment in December 2018. See Section 4.3.2 for more details.

Table A.5: Balance in Baseline Characteristics in Sample of Randomized Voucher Recipients

	(1) Regular Rounds	(2) Elderly Rounds
Tenant in baseline	0.006	-0.023
	(0.043)	(0.022)
Saving balance on application day (US)	0.000	
	(0.000)	
Family income (US)	0.000	0.000
	(0.000)	(0.000)
Poor (poverty line adjusted by family size)	0.093	-0.014
	(0.057)	(0.028)
Online application	0.000	
	(0.044)	
Baseline application to ownership programs	0.018	0.036
	(0.066)	(0.050)
KM to closest PHA	0.001	0.000
	(0.001)	(0.001)
High density county	0.065	0.000
	(0.044)	(0.025)
Age at application	-0.000	0.001
	(0.003)	(0.002)
Any neighbor in 400m previously applied	0.008	-0.019
	(0.053)	(0.027)
Answered Baseline Survey	0.014	
	(0.043)	
Female	-0.016	0.007
	(0.042)	(0.022)
Spouse/partner	-0.068*	-0.022
	(0.036)	(0.022)
County above national poverty rate	0.067	-0.031
	(0.042)	(0.025)
Santiago MSA	-0.038	-0.043
	(0.052)	(0.027)
Family size score	0.000	
	(0.002)	
Social vulnerability score	-0.002	
	(0.007)	
Age score (Elderly rounds)		-0.001
		(0.001)
Constant	0.543	0.774***
	(1.231)	(0.164)
Observations	488	1,675
R-squared	0.282	0.221
Cutoff FE	YES	YES
F-test	0.995	0.791
p-value	0.463	0.671

This table presents balance tests using in sample of randomized vouchers assigned by MINVU to break ties. Column 1 and 2 splits the sample between regular and elderly rounds. Both include cutoff fixed effects. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A.6: Effect of Regular Voucher Before the Covid-19 Pandemic (2019): Randomization

				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	839	2.823	1.122	-0.071	-0.063	0.426	0.528	-0.080	-0.071	0.365	0.363
Number of bedrooms	833	1.502	0.723	0.284	0.393	0.000***	0.000***	0.284	0.393	0.000***	0.000***
Number of people per bedroom	833	2.116	0.895	-0.319	-0.356	0.000***	0.000***	-0.325	-0.363	0.000***	0.000***
Overcrowing indicator	833	0.293	0.455	-0.079	-0.173	0.001***	0.003***	-0.082	-0.179	0.001***	0.003***
B. Residential Mobility											
Stayed in same unit	745	0.510	0.500	-0.046	-0.092	0.276	0.492	-0.043	-0.086	0.299	0.559
Distance (km)	745	9.585	51.571	17.954	0.348	0.048**	0.027**	16.478	0.320	0.069*	0.061*
Distance (km) (Movers)	368	19.550	72.399	32.800	0.453	0.066*	0.041**	31.792	0.439	0.084*	0.076*
Stayed 1km or less from application location	368	0.313	0.465	0.000	0.001	0.995	0.829	0.005	0.012	0.929	0.987
Moved to another county	368	0.173	0.379	0.048	0.126	0.322	0.239	0.046	0.123	0.334	0.449
C. Neighborhood Characteristics											
Distance to closest municipality	745	3.390	4.843	0.475	0.098	0.306	0.558	0.490	0.101	0.298	0.582
Distance to closest school (km)	745	0.955	2.190	0.474	0.216	0.102	0.180	0.513	0.234	0.089*	0.164
Distance to closest Pre-Shcool (km)	745	0.986	2.619	0.562	0.215	0.064*	0.088*	0.590	0.225	0.055*	0.078*
Distance to closest Primary Care (km)	695	1.530	2.718	0.454	0.167	0.189	0.282	0.475	0.175	0.182	0.299
Number of Schools in 1Km	745	5.020	4.390	-0.493	-0.112	0.199	0.164	-0.560	-0.128	0.138	0.145
Number of Schools in 2Km	745	15.669	12.523	-1.545	-0.123	0.143	0.276	-1.862	-0.149	0.047**	0.084*
Number of Preschool in 1Km	745	3.333	2.788	-0.200	-0.072	0.373	0.346	-0.209	-0.075	0.336	0.328
Number of Health Care in 2km	745	5.112	4.286	-0.481	-0.112	0.157	0.307	-0.573	-0.134	0.063*	0.126
Fraction of Public Schools 1Km	618	0.466	0.285	0.012	0.043	0.664	0.726	0.017	0.060	0.543	0.409
Fraction of Subsidized Schools 1Km	618	0.508	0.279	-0.029	-0.105	0.292	0.361	-0.036	-0.127	0.203	0.141
Fraction of Private Schools 1Km	618	0.026	0.084	0.017	0.204	0.053*	0.080*	0.018	0.219	0.034**	0.041**
Mat. SIMCE, 3 Closest School 2km	652	262.852	16.786	1.260	0.075	0.388	0.566	1.238	0.074	0.407	0.477
Mat. SIMCE, 3 Closest School 2km	652	249.605	18.138	0.818	0.045	0.613	0.871	0.899	0.050	0.584	0.721
Fraction of Low Income Schools 1km	618	0.622	0.332	0.007	0.020	0.836	0.702	-0.000	-0.000	0.996	0.918
Fraction of Low Income Schools 2km	657	0.571	0.263	0.010	0.039	0.672	0.443	0.004	0.016	0.859	0.662
County poverty rate	745	0.104	0.056	0.001	0.014	0.859	0.730	-0.000	-0.004	0.954	0.905
Total crime (County z-score)	745	1.135	1.545	0.090	0.058	0.501	0.370	0.033	0.021	0.771	0.774
D. Homeownership											
Application to Ownership Programs	839	0.292	0.455	0.029	0.063	0.422	0.366	0.007	0.016	0.803	0.862
Application to partially funded program (DS1)	839	0.182	0.386	0.034	0.088	0.298	0.272	0.015	0.039	0.583	0.699
Application to fully funded program (DS49)	839	0.147	0.354	0.006	0.018	0.815	0.954	-0.004	-0.012	0.871	0.773
Active ownership savings account	839	0.901	0.299	0.014	0.046	0.530	0.621	0.005	0.017	0.820	0.954
Balance in ownership savings account (US)	763	21.997	31.787	2.470	0.078	0.301	0.290	1.313	0.041	0.568	0.614
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***

his table replicates the analysis in Table 8 for the sample of randomized vouchers in regular rounds. See Table 8 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A.7: Effect of Elderly Voucher Before the Covid-19 Pandemic (2019): Randomization

				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	2,312	1.556	1.080	-0.241	-0.223	0.000***	0.001***	-0.230	-0.213	0.000***	0.001***
Number of bedrooms	2,202	1.349	0.724	0.405	0.559	0.000***	0.000***	0.404	0.558	0.000***	0.001***
Number of people per bedroom	2,192	1.210	0.559	-0.309	-0.553	0.000***	0.001***	-0.302	-0.540	0.000***	0.001***
Overcrowing indicator	2,296	0.029	0.168	-0.017	-0.099	0.013**	0.005***	-0.017	-0.098	0.014**	0.002***
B. Residential Mobility											
Stayed in same unit	2,080	0.678	0.468	-0.212	-0.453	0.000***	0.000***	-0.211	-0.452	0.000***	0.000***
Distance (km)	2,080	20.052	132.068	2.978	0.023	0.625	0.697	3.845	0.029	0.528	0.661
Distance (km) (Movers)	965	62.237	227.289	-17.548	-0.077	0.247	0.077*	-14.213	-0.063	0.345	0.071*
Stayed 1km or less from application location	965	0.290	0.455	0.005	0.011	0.888	0.607	0.005	0.011	0.886	0.597
Moved to another county	968	0.262	0.441	-0.029	-0.065	0.394	0.546	-0.022	-0.050	0.510	0.495
C. Neighborhood Characteristics											
Distance to closest municipality	2,080	3.989	7.845	-0.846	-0.108	0.014**	0.009***	-0.825	-0.105	0.015**	0.006***
Distance to closest school (km)	2,080	1.208	4.432	-0.181	-0.041	0.324	0.109	-0.170	-0.038	0.360	0.160
Distance to closest Pre-Shcool (km)	2,080	1.212	4.598	-0.173	-0.038	0.364	0.118	-0.164	-0.036	0.390	0.179
Distance to closest Primary Care (km)	1,984	1.675	4.469	-0.143	-0.032	0.477	0.203	-0.147	-0.033	0.463	0.239
Number of Schools in 1Km	2,080	7.026	5.782	-0.074	-0.013	0.782	0.933	-0.055	-0.010	0.826	0.893
Number of Schools in 2Km	2,080	21.226	15.346	0.301	0.020	0.682	0.822	0.345	0.022	0.577	0.868
Number of Preschool in 1Km	2,080	3.760	3.071	0.007	0.002	0.964	0.954	0.001	0.000	0.996	0.780
Number of Health Care in 2km	2,080	6.480	5.596	0.456	0.082	0.088*	0.513	0.465	0.083	0.052*	0.486
Fraction of Public Schools 1Km	1,822	0.402	0.243	-0.004	-0.018	0.730	0.353	-0.006	-0.024	0.636	0.456
Fraction of Subsidized Schools 1Km	1,822	0.530	0.245	0.009	0.035	0.502	0.308	0.010	0.039	0.454	0.368
Fraction of Private Schools 1Km	1,822	0.069	0.141	-0.004	-0.030	0.561	0.976	-0.004	-0.026	0.613	0.793
Mat. SIMCE, 3 Closest School 2km	1,882	264.798	17.404	0.503	0.029	0.568	0.808	0.563	0.032	0.524	0.784
Mat. SIMCE, 3 Closest School 2km	1,884	252.294	17.899	0.284	0.016	0.755	0.780	0.377	0.021	0.680	0.754
Fraction of Low Income Schools 1km	1,822	0.459	0.338	-0.002	-0.005	0.919	0.237	-0.004	-0.013	0.790	0.355
Fraction of Low Income Schools 2km	1,889	0.441	0.267	0.002	0.008	0.873	0.382	-0.000	-0.001	0.975	0.630
County poverty rate	2,083	0.084	0.048	0.003	0.062	0.213	0.532	0.001	0.030	0.375	0.909
Total crime (County z-score)	2,083	1.713	1.992	-0.044	-0.022	0.655	0.852	-0.037	-0.018	0.658	0.820
D. Homeownership											
Application to Ownership Programs	2,312	0.117	0.322	0.026	0.081	0.068*	0.027**	0.025	0.078	0.025**	0.018**
Application to partially funded program (DS1)	2,312	0.067	0.250	0.008	0.034	0.456	0.247	0.007	0.026	0.479	0.291
Application to fully funded program (DS49)	2,312	0.065	0.247	0.024	0.097	0.027**	0.019**	0.024	0.096	0.021**	0.022**
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)							0.001***				0.011**

This table replicates the analysis in Table 8 for the sample of randomized vouchers in elderly rounds. See Table 8 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

B 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.[79]

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 application period in this paper.

[79] 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.

The response rate in this group was 57 percent, 60 percent (18,185) in regular rounds and 44 percent (3,023) in elderly rounds.[80] In the evaluation sample, the response rate was 59 percent (779) and 28 percent (171) in regular and elderly rounds, respectively.[81] These rates of response are high for online surveys.

Figures B.1a and B.1b 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.

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

This equation is similar to the fully interacted FE model in equation 4.1 used to analyze balance in Section 4.3. 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 B.1 and B.2 show the estimates of τ_s and β_s for regular and elderly rounds, respectively. The bottom panel presents the results (p-values) of three different analysis of the null of joint significance: F-Test, Randomization-t Joint significance test and the Westfall-Young multiple-testing test of overall treatment irrelevance (Young, 2019).

Table B.1 shows that all individual coefficients, τ_s and β_s, in the evaluation sample are not significant in regular rounds. Furthermore, joint significance of these coefficients is rejected by all three different tests in the bottom panel. The analysis suggests that there was not selective attrition between treated and controls in the follow up survey.

In elderly rounds, Table B.2 shows that while some individual coefficients are statistically significant at the 95 and 90 percent of confidence, the data suggests that there was no selective attrition in the overall sample. However, results for elderly rounds needs to be taken with some caution given the small sample sizes.

Balance

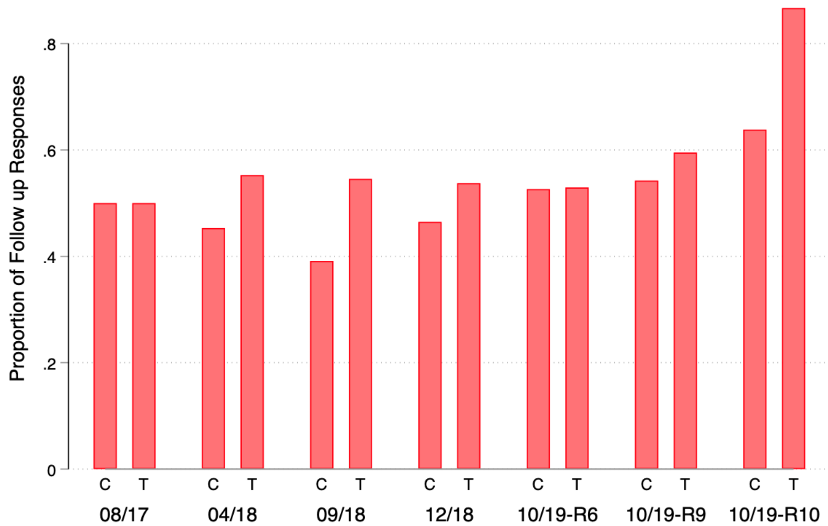
I analyze balance in the follow-up sample. Even in the absence of selective attrition, the strong assumptions made in the Local Randomization RD framework may not hold in a subset of individuals from the evaluation sample; excluding observations in different mass points around the cutoff may introduce 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.[82]

[80] 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.

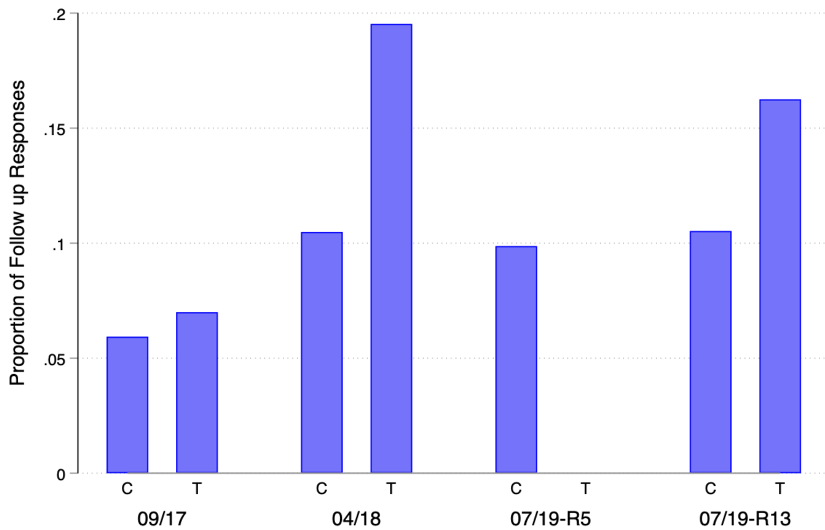
[81] These numbers exclude those who answer but did not recall applying to the program or applied for someone else, which was common in elderly rounds. I dropped fifty responses for this reason in elderly rounds.

[82] 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.

Figure B.1: Follow up Sample Attrition by Assignment



(a) Regular Rounds



(b) Elderly Rounds

This Figure shows response rates of the follow up survey. Panel (a) shows response rates of regular rounds and Panel (b) shows response rates of elderly rounds. In each Figure, C refers to the group of families who were not assigned a voucher (below the cutoff) and T to voucher recipients (above the cutoff).

Tables B.3 and B.4 replicate the balance analysis presented in Section 4.3 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 B.3 shows small differences in two baseline covariates, age and income, significant at 90 and 95 percent of confidence, respectively. Furthermore, the F-test of joint significance and the Westfall-Yang test of overall treatment relevance do not provide evidence of imbalance between treatment and control groups. Table B.4 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 B need to be taken with some caution.

Table B.1: Follow Up Sample Attrition in Regular Rounds

	Survey Response (1)	Survey Response (2)
Treat*Assignment April 2018	0.100	0.123
	(0.241)	(0.141)
Treat*Assignment August 2017	-0.100	-0.157
	(0.615)	(0.425)
Treat*Assignment September 2018	0.055	0.021
	(0.680)	(0.862)
Treat*Assignment December 2018	-0.027	-0.057
	(0.805)	(0.589)
Treat*Assignment October 2019 (R6)	-0.096	-0.137
	(0.484)	(0.284)
Treat*Assignment October 2019 (R9)	-0.047	-0.040
	(0.652)	(0.680)
Treat*Assignment October 2019 (R10)	0.129	-0.004
	(0.309)	(0.977)
F-Test (p-value)	0.111	0.184
Rand-t Joint Test (p-value)	0.115	0.188
Observations	1,533	1,533
Follow up responses	779	779
Assignment FE	Yes	Yes
Baseline covariates	No	Yes

This table shows estimates of equation B.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 two different analysis of the null of joint significance: F-Test and Randomization-t Joint significance test. Significance levels: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table B.2: Follow Up Sample Attrition in Elderly Rounds

	Survey Response	Survey Response
	(1)	(2)
Treat*Assignment April 2018	0.090	0.087
	(0.013)**	(0.015)**
Treat*Assignment Sept 2017	-0.080	-0.078
	(0.043)**	(0.044)**
Treat*Assignment July 2019 Santiago	-0.033	-0.037
	(0.712)	(0.682)
F-Test (p-value)	0.065	0.083
Rand-t Joint Test (p-value)	0.083	0.102
Observations	1,739	1,739
Follow up responses	156	156
Assignment FE	Yes	Yes
Baseline covariates	No	Yes

This table replicates the analysis in Table B.1 using elderly rounds data. 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. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table B.3: 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	0.76	0.43	0.76	0.43	779	0.518	0.456
Saving balance on application day (US)	613.08	613.64	595.00	611.78	624.78	779	0.163	0.191
Family income (US)	537.44	536.05	194.71	540.73	185.47	779	0.017**	0.022**
Poor (poverty line adjusted by family size)	0.18	0.18	0.38	0.19	0.40	779	0.888	0.888
Online application	0.42	0.42	0.49	0.40	0.49	779	0.111	0.115
Baseline application to ownership programs	0.14	0.14	0.35	0.15	0.35	779	0.392	0.403
KM to closest PHA	17.98	16.85	22.95	20.70	26.01	696	0.293	0.297
High density county	0.42	0.46	0.50	0.34	0.48	779	0.846	0.846
Age at application	32.16	33.10	8.73	29.94	6.50	779	0.033**	0.040**
Preferences to stay in the same neighborhood	0.59	0.60	0.49	0.59	0.49	592	0.730	0.716
Satisfaction with housing unit	0.65	0.64	0.48	0.68	0.47	629	0.808	0.847
Applied to save for ownership	0.28	0.30	0.46	0.22	0.42	579	0.169	0.166
Any neighbor in 400m previously applied	0.88	0.90	0.30	0.83	0.38	352	0.124	0.124
Answered Baseline Survey	0.89	0.89	0.32	0.88	0.32	779	0.992	0.992
No Interaction Terms (H0')								
Female	0.92	0.92	0.28	0.92	0.27	779	0.956	0.728
Spouse/partner	0.14	0.15	0.36	0.10	0.31	708	0.778	0.886
Rent (US)	230.80	230.06	125.25	232.86	125.58	484	0.292	0.601
Rent burden	0.47	0.47	0.28	0.47	0.26	484	0.605	0.859
Geocoded location	0.89	0.90	0.30	0.88	0.32	763	0.924	0.813
County above national poverty rate	0.62	0.58	0.49	0.71	0.45	624	0.728	0.623
Santiago MSA	0.15	0.17	0.38	0.09	0.29	520	0.663	0.590
Assignment FE							Yes	Yes
Score components FE							No	Yes
Assignment-Treat Interacted Terms							Yes	Yes
			N	F-Test				
Joint Significance (p-value)			779	0.486				

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

Table B.4: 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)	259.25	273.49	134.29	252.92	127.24	156	0.485	0.099*
Poor (poverty line adjusted by family size)	0.60	0.48	0.50	0.65	0.48	156	0.101	0.652
Baseline application to ownership programs	0.08	0.08	0.28	0.08	0.28	156	0.875	0.711
KM to closest PHA	13.20	14.22	19.65	12.76	17.31	149	0.861	0.074*
High density county	0.56	0.65	0.48	0.53	0.50	156	0.089*	0.573
Female	0.67	0.62	0.49	0.69	0.47	156	0.546	0.349
Spouse/partner	0.47	0.44	0.50	0.48	0.50	156	0.584	0.998
Age at application	73.12	73.04	5.63	73.16	7.47	156	0.487	0.000***
No Interaction Terms (H0’)								
Any neighbor in 400m previously applied	0.83	0.89	0.32	0.81	0.40	96	0.308	0.266
Tenant in baseline	0.66	0.62	0.49	0.68	0.47	156	0.026**	0.040**
Geocoded location	0.96	0.94	0.24	0.96	0.19	141	0.716	0.711
County above national poverty rate	0.24	0.29	0.46	0.22	0.42	156	0.668	0.630
Santiago MSA	0.39	0.42	0.50	0.38	0.49	141	0.122	0.134
Assignment FE							Yes	Yes
Score components FE							No	Yes
Assignment-Treat Interacted Terms							Yes	Yes
			N	F-Test				
Joint Significance			156	0.227				

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

Table B.5: Effect of Regular Voucher Before the Covid-19 Pandemic (2019): Follow Up Sample

					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	779	3.121	1.471	-0.151	-0.103	0.179	0.174	-0.149	-0.101	0.173	0.163
Number of bedrooms	776	1.969	0.892	0.106	0.119	0.144	0.154	0.114	0.128	0.113	0.127
Number of people per bedroom	775	1.767	0.887	-0.207	-0.233	0.001***	0.001***	-0.209	-0.236	0.001***	0.000***
Overcrowing indicator	775	0.120	0.325	-0.039	-0.120	0.105	0.115	-0.036	-0.111	0.130	0.144
II. Residential Mobility											
Stayed in same unit	693	0.551	0.498	-0.063	-0.126	0.161	0.160	-0.046	-0.092	0.307	0.305
Distance (km)	693	5.510	26.894	10.596	0.394	0.149	0.141	10.100	0.376	0.144	0.137
Distance (km) (Movers)	320	12.274	39.146	19.554	0.500	0.182	0.201	17.758	0.454	0.175	0.185
Stayed 1km or less from application location	320	0.342	0.476	-0.099	-0.209	0.098*	0.116	-0.097	-0.205	0.106	0.124
Moved to another county	321	0.141	0.349	0.087	0.250	0.066*	0.075*	0.100	0.287	0.034**	0.048**
III. Neighborhood Characteristics											
Distance to closest municipality	693	3.222	4.194	0.376	0.090	0.436	0.443	0.231	0.055	0.619	0.620
Distance to closest school (km)	693	0.951	1.607	0.592	0.368	0.106	0.103	0.433	0.269	0.199	0.191
Distance to closest Pre-Shcool (km)	693	0.973	2.030	0.510	0.251	0.141	0.146	0.315	0.155	0.326	0.350
Distance to closest Primary Care (km)	648	1.538	2.206	0.548	0.249	0.196	0.200	0.316	0.143	0.421	0.439
Number of Schools in 1Km	693	4.949	4.562	-0.807	-0.177	0.038**	0.046**	-0.474	-0.104	0.217	0.220
Number of Schools in 2Km	693	15.693	13.524	-2.472	-0.183	0.020**	0.029**	-1.154	-0.085	0.225	0.253
Number of Preschool in 1Km	693	3.031	2.641	-0.213	-0.080	0.371	0.375	-0.041	-0.016	0.860	0.880
Number of Health Care in 2km	693	5.004	4.524	-0.436	-0.096	0.213	0.248	-0.003	-0.001	0.993	0.996
Fraction of Public Schools 1Km	564	0.430	0.295	-0.004	-0.014	0.901	0.900	-0.016	-0.053	0.616	0.629
Fraction of Subsidized Schools 1Km	564	0.526	0.285	0.001	0.002	0.987	0.991	0.011	0.039	0.726	0.745
Fraction of Private Schools 1Km	564	0.044	0.117	0.003	0.030	0.761	0.734	0.005	0.041	0.677	0.653
Mat. SIMCE, 3 Closest School 2km	600	265.175	17.652	-2.900	-0.164	0.103	0.106	-2.593	-0.147	0.145	0.145
Mat. SIMCE, 3 Closest School 2km	600	250.995	19.123	-3.732	-0.195	0.045**	0.056*	-3.381	-0.177	0.072*	0.088*
Fraction of Low Income Schools 1km	564	0.562	0.345	0.073	0.211	0.039**	0.047**	0.057	0.164	0.095*	0.093*
Fraction of Low Income Schools 2km	605	0.550	0.284	0.051	0.181	0.057*	0.070*	0.037	0.129	0.152	0.154
County poverty rate	694	0.109	0.061	0.002	0.041	0.601	0.584	-0.001	-0.024	0.712	0.714
Total crime (County z-score)	694	1.310	1.693	-0.198	-0.117	0.187	0.178	0.017	0.010	0.872	0.855
IV. Homeownership											
Application to Ownership Programs	779	0.309	0.462	0.034	0.073	0.384	0.389	0.028	0.060	0.366	0.356
Application to partially funded program (DS1)	779	0.241	0.428	-0.011	-0.027	0.747	0.754	-0.014	-0.032	0.652	0.652
Application to fully funded program (DS49)	779	0.119	0.324	0.035	0.109	0.183	0.179	0.028	0.087	0.290	0.267
Active ownership savings account	779	0.918	0.275	0.007	0.024	0.777	0.789	-0.001	-0.003	0.966	0.967
Balance in ownership savings account (US)	714	24.077	32.337	-0.007	-0.000	0.998	0.996	-0.159	-0.005	0.946	0.952
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)							0.052*				0.018**

This table replicates the analysis in Table 8 including only individuals that responded the follow up sample. See Table 8 for details.

Table B.6: Effect of Elderly Voucher Before the Covid-19 Pandemic (2019): Follow Up Sample

				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	171	2.556	1.933	-0.726	-0.376	0.008***	0.009***	-0.712	-0.369	0.012**	0.013**
Number of bedrooms	169	1.919	1.309	0.165	0.126	0.431	0.426	0.156	0.119	0.484	0.473
Number of people per bedroom	169	1.416	0.757	-0.488	-0.644	0.000***	0.001***	-0.472	-0.624	0.000***	0.001***
Overcrowing indicator	170	0.079	0.272	-0.072	-0.265	0.138	0.157	-0.082	-0.302	0.089*	0.084*
II. Residential Mobility											
Stayed in same unit	159	0.719	0.453	-0.299	-0.660	0.001***	0.002***	-0.300	-0.661	0.001***	0.003***
Distance (km)	159	1.654	5.888	19.551	3.320	0.035**	0.016**	21.663	3.679	0.044**	0.022**
Distance (km) (Movers)	76	5.885	10.144	29.981	2.956	0.048**	0.036**	43.702	4.308	0.059*	0.031**
Stayed 1km or less from application location	76	0.500	0.516	-0.213	-0.413	0.179	0.174	-0.218	-0.422	0.180	0.195
Moved to another county	77	0.176	0.393	0.098	0.251	0.451	0.462	0.095	0.242	0.430	0.452
III. Neighborhood Characteristics											
Distance to closest municipality	159	4.186	7.023	-1.967	-0.280	0.172	0.186	-2.097	-0.299	0.120	0.130
Distance to closest school (km)	159	1.260	2.766	-0.385	-0.139	0.469	0.498	-0.426	-0.154	0.349	0.388
Distance to closest Pre-Shcool (km)	159	1.325	3.291	-0.513	-0.156	0.439	0.502	-0.613	-0.186	0.300	0.354
Distance to closest Primary Care (km)	152	1.870	3.283	-0.822	-0.250	0.204	0.236	-0.924	-0.281	0.097*	0.112
Number of Schools in 1Km	159	7.614	6.488	-0.712	-0.110	0.498	0.521	-0.832	-0.128	0.442	0.455
Number of Schools in 2Km	159	23.561	15.694	-1.514	-0.096	0.585	0.591	-1.221	-0.078	0.622	0.645
Number of Preschool in 1Km	159	4.000	3.111	-0.291	-0.093	0.589	0.607	-0.296	-0.095	0.571	0.574
Number of Health Care in 2km	159	7.895	6.860	-1.211	-0.177	0.274	0.285	-1.162	-0.169	0.252	0.261
Fraction of Public Schools 1Km	140	0.341	0.172	0.054	0.314	0.207	0.220	0.035	0.203	0.416	0.425
Fraction of Subsidized Schools 1Km	140	0.570	0.194	-0.025	-0.128	0.581	0.583	-0.016	-0.081	0.731	0.739
Fraction of Private Schools 1Km	140	0.089	0.119	-0.029	-0.245	0.236	0.251	-0.019	-0.160	0.419	0.408
Mat. SIMCE, 3 Closest School 2km	143	266.827	16.934	-3.640	-0.215	0.290	0.259	-4.127	-0.244	0.267	0.255
Mat. SIMCE, 3 Closest School 2km	143	255.943	16.547	-4.046	-0.245	0.255	0.251	-4.497	-0.272	0.244	0.235
Fraction of Low Income Schools 1km	140	0.371	0.311	0.030	0.096	0.645	0.647	0.007	0.024	0.909	0.900
Fraction of Low Income Schools 2km	143	0.410	0.265	-0.049	-0.184	0.356	0.329	-0.050	-0.190	0.283	0.253
County poverty rate	160	0.077	0.041	-0.004	-0.089	0.652	0.650	-0.002	-0.058	0.654	0.662
Total crime (County z-score)	160	2.316	2.158	-0.400	-0.185	0.334	0.351	-0.235	-0.109	0.572	0.596
IV. Homeownership											
Application to Ownership Programs	171	0.175	0.383	0.072	0.189	0.297	0.297	0.072	0.188	0.148	0.156
Application to partially funded program (DS1)	171	0.095	0.296	0.011	0.037	0.829	0.837	0.037	0.123	0.413	0.408
Application to fully funded program (DS49)	171	0.095	0.296	0.077	0.262	0.153	0.143	0.049	0.164	0.305	0.316
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)							0.021**				0.022**

This table replicates the analysis in Table 9 including only individuals that responded the follow up sample. See Table 9 for details.