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

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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 score 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, results show that rental vouchers had a broader impact on recipient households. Holding a voucher affected how families were coping with the large unexpected shock caused by COVID-19: they experienced less unwanted mobility and were less likely to miss their rent payments. Furthermore, they were less likely to cut food expenses or use emergency relief policies during this period. These results point to a previously underappreciated insurance role of rental subsidies in helping poor households cope with negative aggregated shocks.

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1 Introduction

In December 2013, advised by the US Department of Housing and Urban Development (HUD) and inspired on the US rental voucher program Section 8, the Chilean Ministry of Housing and Urbanism (MINVU) launched the first rental voucher program in Latin America, the *Subsidio de Arriendo* (Rental Subsidy). To date, several countries in the region have followed Chilean steps from providing ownership subsidies exclusively towards incorporating rental assistance to low-income families.[1]

Rental vouchers aim to provide neighborhood choice and increase mobility towards neighborhoods where low-income families can have more opportunities and experience upward social mobility (Chetty et al., 2018; Chyn and Katz, 2021). In addition, the Chilean rental voucher program was expected to reduce overcrowding and, by providing a more flexible housing policy option for young families, to discourage the high demand for fully funded subsidized housing that had shown negative effects on employment and segregation of low income families at the city periphery (OECD, 2012; Navarrete and Navarrete, 2016).

There is a large literature on rental voucher programs based on evidence from the US Section 8 program. This literature shows that rental voucher programs have not lived up to their promises of providing low income families with access to better neighborhoods, although they have reduced overcrowding, rent burden and improved other housing-related outcomes.

Differences in policy design and institutional context across countries could have a profound impact on the experiences and outcomes of rental voucher recipients (Colburn, 2021), specially between high and low or middle income countries. However, there is no causal evidence about how these programs are doing outside of the US.

This paper presents the first evaluation of a rental voucher program in a middle income country, Chile. Compared to the US, Chile is poorer, more unequal, has higher levels of informality[2], a smaller rental market, and large demand-side subsidies that have encouraged ownership for

[1] Argentina, Mexico, Peru, Colombia, Paraguay, Uruguay and Brazil, among others, have launched rental subsidies in recent years. Policy design varies across countries.

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

decades.[3]

I estimate the causal effect of the Chilean rental voucher program on overcrowding, residential mobility, neighborhood characteristics and application to homeownership policies and private savings to buy a house. To do this, I exploit the assignment mechanism of the program. In particular, MINVU calculates an application score using multiple socioeconomic variables to rank families according to their vulnerability. Applicants above a score cutoff get a voucher.

I use a unique data set linking administrative data of all applicants to the program from 2017 to 2019 to individual and neighborhood outcomes in December 2019, obtained from different administrative data sources. I supplement administrative data with a survey to all applicants between September and November 2020, implemented 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 evaluate the effect of two different rental voucher schemes, referred as regular rounds and elderly rounds by MINVU. Regular rounds target head of households aged 18 or older and provide a modest monthly voucher for about 36 months. Elderly rounds target people aged 60 or older and provide a more generous monthly voucher for 24 months. In the period analyzed in this paper, 41,473 and 23,794 families applied to regular and elderly rounds, respectively. Of these, 1,131 and 1,328 just above and below the application score cutoff are used in this evaluation.

Pre-pandemic data yielded results similar to the evidence from the US program Section 8: 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 6.1 pp (46%) in regular rounds and in 2 pp (59%) in elderly rounds. Also, holding a voucher had a large effect on mobility of elderly households and allowed voucher holder to remain closer to their baseline location. In younger families, the voucher had the opposite effect: while it had a small positive effect on the chances of moving, voucher holders moved longer distances. Neighborhood characteristics did not change significantly. If anything, younger families ended up farther away from schools. Further, application to homeown-

[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).

ership programs increased among elderly voucher holders. Among regular round participants, private savings to buy a house 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), vulnerable to 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. In this paper, I leverage survey data to investigate whether rental vouchers affect how families in regular rounds cope with large aggregated shocks, like the one that came with the coronavirus pandemic.

Treatment effects around the cutoff show that voucher holders had responded differently in the first eight months of the Covid-19 pandemic. The program had an important effect on housing stability: voucher recipients were less likely to miss rent payments and experienced unwanted mobility. Also, families were less likely to cut food expenses or use emergency income assistance.

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[4], crowding, and 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) However, 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 knowledge, this paper is the first evaluation of a rental voucher program offered to low income families to rent a unit in the private market outside of the US.[5]

[4]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).

[5]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.

Further, while the elder population is an important fraction of housing subsidy recipients in the US (Collinson, Ellen and Ludwig, 2020), most empirical evidence analyzes the effect of rental vouchers on families with children. The evaluation of the Chilean rental voucher shows that rental vouchers can have important positive effects on the elderly, contributing to close this gap in the literature.

Secondly, this research contributes to the very few empirical studies regarding 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 in normal times. This research focuses in a different research question: whether rental vouchers are able to provide housing security to low income families in times of large economic shocks. To the best of my knowledge, this is the first evaluation of a rental vouchers program during times of economic struggle and suggests a previously underappreciated insurance role of rental vouchers, reducing housing and income instability 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 5 describes how the evaluation sample is built and discusses the validity of the research design. Section 6 presents the results and robustness checks. Section 7 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. There are two main types of vouchers offered in Chile: regular rounds and elderly rounds of vouchers.[6] Regular rounds target 18 or older-headed families with monthly income between US$250 and US$900,[7] with at least US$180 in

[6]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.

[7]Families with 3 or more members have higher income upper bounds.

private savings to buy a house[8] and who are in the bottom seventy percentiles of the national vulnerability index, measured by the *Registro Social de Hogares* (RSH).[9] Voucher holders in these rounds receive US$6,200 in fixed monthly installments of US$180 to pay monthly rents up to the maximum payment standard, set nationally at US$402.[10] 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 60 or older with incomes above $140. Savings are not required. Benefits last for two years[11], although total subsidy and rent coverage vary slightly across four groups, based on their RSH group. Specifically, less vulnerable voucher holders (61-70th percentiles in the RSH) get 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 payment standard.[12]

To apply to the Chilean program, families can go online or in person to any of the fifty PHAs (SERVIUs) across the country.[13] Rounds are opened from two to nine months and MINVU makes monthly or bimonthly assignments, selecting up to 3,000 voucher recipients each time. MINVU uses an application score to screen applicants and assign vouchers to the most vulnerable families. Specifics of the assignment mechanism are explained in detail in Section 4.

Families have two years to find a landlord willing to participate in the program.[14] 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

[8]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.

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

[10]Except for 30 out of 346 counties located at the north and south of the country where maximum payment standard raises to US$475.

[11]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.

[12]Only four percent of voucher recipients are not in the most vulnerable group. Section 4.1.

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

[14]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).

unit needs to have at least three separated spaces and meet some legal requirements.[15]

Some differences between the US and Chilean rental voucher program may impact the effectiveness of the voucher. First, Section 8 is more generous. Rent burden of voucher recipients 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.[16] In Chile, on the other hand, the amount of the voucher and maximum payment standard is fixed.[17]

Second, while in Chile benefits last between 24 and 36 months, families in the US receive assistance as long as they meet program income requirements. Third, the size of the rental market in Chile and other Latin American countries is about half of the US rental market (Ross and Pelletiere, 2014; Blanco Blanco, Cibils and Miranda, 2014).[18] Furthermore, while renting is particularly low among low-income families, ownership remains constant throughout the income distribution in Chile (Andrews and Sánchez, 2011).[19]

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

Only four out of ten voucher recipients have used their vouchers (Column 4) in Chile, almost half of the average lease-up rate in the US (Finkel and Buron, 2001).[21] Descriptive evidence shows that the chances of using the voucher are particularly low for migrants, families living in the poorest housing conditions and in areas with tighter housing markets (Bogolasky, 2021).

3 Data

This paper uses a unique data set including administrative, survey and public data at three different moments in time: baseline data gathered at application; outcome data collected in

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

[16] 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.

[17] Effective rent burden in Chile is then not fixed, varying between 40 and 50 percent.

[18] Rental housing represents twenty percent of the housing stock in Chile.

[19] 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.

[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.

December 2019, before the pandemic; and outcome data collected from September to November 2020, six to eight months after the Covid-19 outbreak in March 2020.

Baseline Data. I access application data that MINVU collects to determine applicants eligibility and calculate the application score. I have socioeconomic and demographic characteristics, location and some housing characteristics. In addition, I have access to 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. On average, response rate in the period of analysis is 78%. Importantly, answers were collected before voucher recipients were announced.

Also, to replicate voucher assignments I build a unique data set of scores, application dates, and assignment characteristics (round type, dates, cutoffs, etc.) for all applicants to the program.[22]

The data set includes individual data and neighborhood level data for each applicant. More specifically, using geocoded data[23] and county codes at the time of application I link the administrative data to public geocoded information on neighborhood characteristics. I create county level variables such as poverty, crime and density. Also, using detailed geocoded data I measure access (number and distance) to schools, health care centers, municipalities and PHAs in a 2 kilometers radius.

December 2019: 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. I create the same neighborhood variables explained above using December 2019 geocoded location. Second, I have information on household application to the two largest homeownership programs, the *Fondo Solidario de Vivienda (DS49)* and *Subsidio Clase Media (DS1)* between January 2011 and December 2019.[24] Third, for regular rounds only, I also have data

[22] I build this data linking administrative data to data from public documents and confirming any disagreement directly with policy makers involved in voucher assignment at the time.

[23] 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.

[24] 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 pay-

on private savings to buy a house.[25]

November 2020: Outcome data after the Covid-19 outbreak. I partnered MINVU to implement a follow up survey between September and November 2020. The survey included questions to measure crowding, residential mobility, neighborhood characteristics, subjective well-being, health, housing and neighborhood satisfaction, income, employment, and behavioral responses 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 the Chilean rental voucher program, MINVU assigns available vouchers over an application score that they calculate for each applicant using a complex formula including multiple indicators. Table 1 describes the variables that are considered in the application score formula.

Vouchers are assign in several assignment periods, every one or two months, within each round.[26] The rolling application system implies that each applicant may participate in multiple assignment periods within a round.

Before 2019, all applicants in Chile were screened together at each assignment period, resulting in a unique national cutoff. In 2019, MINVU switched to regional screenings of applicants. Now, during each assignment period, applicants from each region are screened separately, having different cutoffs according to the number of vouchers available per region.[27]

The number of available vouchers and number of assignments periods per round is set by decree before the start of each round of applications. Following administrative or political decisions made outside of the rental policy team at MINVU, sometimes these quantities change afterwards. However, these changes are not announced to the public.

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ment 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.

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

[26] See Section 2 for a general description of the program.

[27] 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 who complained about getting too few vouchers.

In this research, I exploit this voucher assignment mechanism to evaluate the program. Specifically, I exploit the discontinuity in the probability of treatment at the score cutoff that takes place each time that MINVU screens applicants to implement a Sharp Multi-cutoff Regression Discontinuity Design.

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). Figure 1 shows the sharp discontinuity in treatment status at the cutoff and Figure 2 the distribution of application scores and cutoffs in the Chilean rental voucher program.

Instead of using the standard continuity approach, in this paper I estimate treatment effects using the Local Randomization approach to Regression Discontinuity Design (LRRD), first introduced by Cattaneo, Frandsen and Titiunik (2015). This approach makes stronger assumptions about the assignment mechanism near the cutoff (Branson and Mealli, 2018) and it provides unbiased robust estimates when the running variable is discrete and includes only a few mass points, as it is the case of the application score in the Chilean rental voucher program.[28]

In 2017, following a reform to the national vulnerability index (RSH) [29], the social vulnerability score added in the application score formula became discrete.[30] This reform changed the distribution of the application score: it became a discrete variable including multiples of 5, with a few mass points.[31]. Confronted with ties at the cutoff, MINVU had to establish a tie-breaking protocol. A three-step procedure was implemented, selecting voucher recipients according to their family size score and then by their social vulnerability score. Left standing vouchers are randomly assigned. Only a small number of regular vouchers have been randomly assigned.

[28] In settings with large number of mass points, it is common practice to use the continuity approach and estimate standard errors clustered by the running variable (Lee and Card, 2008; Kolesár and Rothe, 2018). See Branson and Mealli (2018) for a review of alternative estimation methods in RDD settings.

[29] Administered by another Ministry and used to assign most of the social benefits in Chile. See Section 2 for more details.

[30] Continuous social vulnerability component is replaced by a variable taking four values, one for each group of the RSH in the target population.

[31] The support of the application score is finite and includes 131 unique values in regular rounds and 109 unique values in elderly rounds.

Therefore, I use the sample of randomized vouchers to check the robustness of the main results in Section 6.3.

In this set up, the standard continuity approach fails to provide unbiased coefficients and confidence intervals in the smallest window possible.[32] The LRRD, on the other hand, uses the actual random variation at the cutoff and the quasi-random (or as good as random) variation in a small window around the cutoff. Next, I explain how the LRRD is used to estimate causal treatment effects.

4.1 Local Randomization Approach to Regression Discontinuity

In each screening $s_t \in S$ in assignment period t of round r, applicants are sorted over their score X_{i,s_t}.[33] Those with the highest score receive a rental vouchers.

The assignment cutoff, c_{s_t}, is the value of the score of the applicant who is offered the last available voucher. Applicants who are not offered a voucher ($X_{i,s_t} \leq c_{s_t}$) are screened again with all new applicants in the following assignment period in the same round. This happens until the round closes. To be considered for the next round, they need to apply again.

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

The LRRD assumes there exists a window $W_{s_t} = [x - e, x + e]$ in which the distribution of the score is known, and it is the same for all units, as in experimental data.[34] Inside W_{s_t}, potential outcomes may depend on the score only through treatment indicators and there should not be interference between potential outcomes of different units i.e. the stable unit treatment value

[32]In the standard continuity approach, the continuity assumption of the regression functions $\mathbb{E}\{Y_i(1)|X_i = 0\}$ and $\mathbb{E}\{Y_i(0)|X_i = 0\}$ at the cutoff $X_i = 0$ is used to approximate the average outcome that units above the cutoff would have had in the absence of treatment (Lee and Lemieux, 2010). 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, 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 of the cutoff.

[33]Since each assignment period is unique to a round, to simplify notation I do not use the sub-index r.

[34]There are no modelling assumptions as in the standard continuity approach.

assumption or SUTVA holds.[35]

Under these assumptions, score ignorability $Y_{i,s_t}(X_{i,s_t}, D_{i,s_t}) = Y_{i,s_t}(D_{i,s_t})$ is guaranteed inside W_0. Hence, as in experimental settings, the causal treatment effect under the LRRD, τ_{LR}, is the difference between the average outcome in the treated and control groups in the largest window around the cutoff in which local randomization assumptions hold. More formally,

$$\tau_{LR} = \bar{Y}_{i,s_t \in W_{s_t}}(1) - \bar{Y}_{i,s_t \in W_{s_t}}(0) \approx \mathbb{E}\left\{Y_{i,s_t}(1) - Y_{i,s_t}(0) | X_{i,s_t} \in W_{s_t}\right\}$$

Similar to the problem of bandwidth selection in the standard continuity approach, window selection is the most important step in LRRD. In this paper, I use the data driven procedure developed by Cattaneo, Frandsen and Titiunik (2015) to identify the largest window around the cutoff in which LRRD assumptions hold.[36] Note that the window selection procedure simplifies if the running variable, here the application score, is discrete. In this case, the minimum possible window is known, therefore, the local randomization assumptions must hold in the window that contains the two mass points that are immediately above and below the cutoff in each s_t.

To implement this procedure, treatment and control units must be in different sides of the cutoff in each W_{s_t}, regardless of the values of the running variable. If there are ties at $c_{s_t} = 0$, the running variable require some transformation in $W_{s_t} = [0, 0]$, although any transformation that keeps the same order between mass points produces the same results (Cattaneo, 2018).[37]

[35] In a rental voucher program, interference could happen between neighbors or through the interaction in the rental market. I argue that this is unlikely in the Chilean program. The program is still very small and vouchers are assigned either across applicants from large geographical units, nationally or regionally. In this context, general equilibrium effects are unlikely to occur in local rental markets. The low lease up rate is 43% makes this even more unlikely, given that families do not use their voucher for many reasons, including preferences for residential immobility, preferences for homeownership and the lack of information about voucher status and the lease-up process. In this context, it is unlikely to live nearby others who have won the voucher, more so in the same round. Descriptive data on applicants supports these ideas. Geocoded data shows that three out of four applicants do not live next to any other applicant from previous rounds of the program. Also, baseline survey data to applicants between 2017 and 2019 shows that only 3% of applicants know a neighbor that won the voucher in the past. It is reasonable then to assume that treatment received by one individual do not affect outcomes for another individual in the sample.

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

[37] I use the assignment rules of the program (See Section 2) to transform the running variable when there are ties at the cutoff ($W_s = [0, 0]$): 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 $W_s = [-1, 1]$. When there is no mass point at the cutoff, the minimum window is $W_s = [-5, 5]$.

Fore each s_t, the chosen window, W_{s_t}, is the largest window such that the minimum p-value obtained in balance tests in pre-treatment covariates in $W_{s_{k,t}}$ and any smaller window $W_{s_{j,t}}$ (with $j < k$), is above a predetermined significance threshold, α. In this paper, $\alpha^* = 0.10$.

The LRRD uses randomization inference, robust in small finite samples.[38] Also, it uses two set of covariates. One set of pre-treatment covariates is used for window selection and another set is used for further falsification tests. I use variables that do not enter the application score formula directly. Further, I build additional covariates that are not observed by MINVU during voucher assignment using administrative data from other government agencies or divisions inside MINVU, and also survey and geocoded data not available for them during assinment periods. I describe the variables used in the window selection procedure in Section 5. Finally, all chosen windows W_{s_t} are stacked together in the evaluation sample W_0 that I use to estimate the causal effect of the program using a fixed effect model that exploits variation around the cutoff in each s_t.[39]

Section 5 explains how W_0 is built using the data of the Chilean rental voucher program. Then, Section 5.3 presents the fixed effect model used to estimate causal treatment effects.

5 Evaluation Sample

To select windows W_{s_t} and build the evaluation sample W_0, I start by creating a data set that stacks together all screenings of applicants s_t between March 2017 and September 2019. Initially, the data has 95,910 observations (56,705 unique applicants) that participated in 82 screenings that occurred in 22 assignment periods.[40]

I implement three data restrictions to determine the subset of s_t that meet minimum conditions

[38] Randomization inference assumes fixed potential outcomes but random assignment mechanism. The Fisher sharp null hypothesis of zero (additive) treatment effect ($H_0 : Y_i(0) = Y_i(1)$) is exact in that it uses observed outcomes to impute potential outcomes under treatment and control, such that $Y_i(0) = Y_i(1) = Y_i$. Balance tests of no difference in means on pre-treatment covariates assume fixed-margin treatment randomization within screenings of applicants. To be conservative, p-values in balance tests for window selection do not adjust for multiple testing.

[39] 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.

[40] This includes the entire support of the running variable. The maximum window length in the evaluation sample is $[-15, 15]$. Initially, the data has 16,245 observation in this window; 11,930 and 4,315 observations from 9,082 and 4,133 unique applicants in regular and elderly rounds, respectively.

to be considered in the window selection procedure. First, I analyze the number of observations close to the cutoff in each screening s_t. Following Cattaneo, Frandsen and Titiunik (2015), I exclude those screenings of applicants were the minimum window around the cutoff has less than ten observations at each side of the cutoff c_{s_t}.[41] By doing this, I exclude 30,294 observations (14,504 unique applicants) from screenings that do not meet this criteria.[42]

Second, I analyze the type of control units close to the cutoff. The rolling application system generates two types of control units: i) later treated or applicants that receive the voucher at a later assignment period during round r and ii) never treated or applicants that do not receive the voucher in any assignment period in round r. The focus of this research is to evaluate the effect of the voucher and comparing later treated to voucher holders captures a different estimand: the effect of holding a voucher for a few more months (treatment timing). I lack statistical power to estimate both effects separately since most screenings have a small number of units in a small window around the cutoff. Furthermore, some screenings with only later treated close to the cutoff do not meet LRRD assumptions. For this reason, I exclude 7,071 unique applicants (26,773 observations) corresponding to nine screening of applicants with only later treated in the control group in a small window around the cutoff.[43]

Finally, I drop 2,992 observations from 2,174 applicants from the September 4th screening of elderly applicants to have common support between treated and controls at the cutoff in terms of application dates.[44]

The final data set to select W_{s_t} contains 35,848 observations from 30,610 unique applicants[45]

[41]This is done to have enough statistical power to test for balance in each assignment. Assuming a discrete outcome, a minimum detectable effect of one standard deviation and significance levels of 0.05-0.15, the randomization-based test of the sharp null of no treatment effect in the minimum window would have 60-80 percent of statistical power.

[42]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 have enough units in each side of the cutoff. 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.

[43]Regular screenings of April, May, July, August, September of 2017, September and November of 2018 and August 2019, and elderly screening of June 2018.

[44]This assignment period was the first national elderly round of the program. It was implemented in May 2017 without much advertisement. In June 30th, MINVU increased their advertisement efforts to increase application before closing the round in August. As a consequence, all non-voucher recipients were applicants from the last six weeks of the assignment period (July 1st - August 16th). Applicants before June 30th were dropped. Doing this does not changed the distribution in a small window arond the cutoff.

[45]Of these, 3122 and 2575 are in a small window around the cutoff $[-15, 15]$ in regular and elderly rounds,

that participated in 12 screenings (7 in regular and 5 in elderly rounds) in 9 voucher assignment periods (5 in regular and 4 in elderly rounds).[46] Columns 1 to 3 of Table 5 show the assignment period, region and cutoff of each screening s_t used in the selection window procedure.

I use the window selection procedure to analyze balance in the four smallest windows $W_{s_{j,t}}$ ($j = 1, 2, 3, 4$) around the cutoff in each screening of applicants s_t.

Finding covariates that vary between and within screenings in a small window around the cutoff is not trivial. The first set of covariates to implement the window selection procedure includes variables with larger variation that do not enter the application score formula. Specifically, it includes family income, and indicator variables for tenant, previous application to homeownership programs, having geocoded location and two dummy variables controlling for county characteristics: high density and PHA access.[47]. Savings to buy a house at the time of application and online application are included in balance tests in regular screenings only. In elderly rounds I use an indicator variable for taking documents to validate income in person to the PHA.[48]

The second set of covariates includes dummy variables for female, married, age between 25 and 35 for younger rounds and between 70 and 79 for elderly rounds, Chilean nationality, family poverty status[49], Santiago MSA, high poverty counties (above the national poverty rate), having a valid email address, and distance (km) to the closest PHA.

respectively.

[46]One last minor data restriction that had no implications in the window selection procedure was to exclude three applicants from the sample (farther from the cutoff), who had a score higher than the cutoff but were mistakenly assigned to the control group.In total, there were 380 of this mistakes in the period of analysis but 376 of these occurred in the regular screening in September 21st of 2018, which was excluded by the second sample restriction. The three remaining cases happened in October 19th of 2017 (1) and April 11th of 2018 (2). I confirmed with MINVU that these were not a problem in the data but actual individuals that were mistakenly not given a voucher.

[47]I create a dummy indicator for whether the county is one of the 53 counties (out of 343) that has a PHAs. This is a proxy for location characteristic and access to formal information about the rental voucher program and other housing policy options.

[48]Elderly rounds do not ask for savings, and online application was not available for elderly rounds in the period of analysis. In regular rounds income validation is not reported in the data for online applications.

[49]Female, married and age are strongly correlated with application score. Number of children and family size enter the formula directly. Hence, I created a dummy indicator of being between the 25 and 75 percentiles of the age distribution per round type, which has a weaker correlation with application score. Also, I included family adjusted poverty status that, for similar income level, varies across families of different sizes. 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.

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, knowing other applicants to the program, having access to a car and having neighbors perceived as high social class.[50]. Finally, I included rent amount and rent burden, available only after September 2018. Balance tests results in the evaluation sample are shown in Section 5.1.

Nine windows W_{s_t} were selected using the data driven procedure and stacked together in the evaluation sample, W_0. Columns 4 to 9 of Table 5 describes the screening of applicants included in W_0. In particular, the minimum p-value of all balance tests conducted using the first set of covariates, the number of units to the right of the cutoff (treated), the number of units to the left of the cutoff (controls), and the minimum and maximum values of the normalized score included in each W_s. Windows W_{s_t} have different lengths. The maximum length of W_{s_t} is $[-15, 10]$ and $[-5, 5]$ in the sample of regular and elderly rounds, respectively.

In total, W_0 has 2,459 observations (2,425 unique applicants), 1,131 (1,107) from five regular screenings in three assignment periods and 1,328 (1,318) from four elderly screenings in three assignment periods. Treated and controls in W_0 are balanced in both the first and second set of covariates.[51] Randomized vouchers in the smallest window possible $[-1, 1]$ represent 47.7% and 89.4% of the sample in regular and elderly rounds, respectively.

Next, I present falsification tests to analyze the validity of LRRD in the evaluation sample. Then, I show summary statistics of the evaluation sample and compare it to the broader population of voucher recipients to assess the external validity of the results of this evaluation.

5.1 Falsification Tests

As in the standard continuity approach, the LRRD is valid if the running variable is not manipulated and there are no other discontinuities in baseline covariates around the cutoff. In the

[50] Survey data has missing values yet having a valid email and response to baseline survey are balanced between treated and control units. See Section 5.1

[51] Windows were selected using the first set of covariates. Then, I analyzed window length and balance in terms of the second set of covariates. To be conservative, I adjusted one screening to use a smaller sample around the cutoff. Initially, the selected regular window $W_{December28th2018}$ had 80 treated and 749 control units. While the normalized score of treated units where at the cutoff and controls were up to 15 points away from the cutoff. I use the two (instead of four) increasing windows around the cutoff for this assignment period, excluding 454 observations from the control group that were outside of the window $[-5, 5]$ around the cutoff.

LRRD, the later means that treated and controls are balanced around the cutoff, as in experimental settings.

5.1.1 Balance Tests

In this section, I show the result of balance tests in the first and second set of covariates. Recall that the window selection procedure guarantees balance in the first set of covariates.

Given that quasi-experimental variation happens at each screening of applicants s_t, I estimate the following fully interacted fixed effect model to analyze statistical balance in the evaluation sample:

$$Z_{i,s_t} = \alpha + \tau_{1,s_t} D_{i,s_t} + \gamma_{s_t} * S_{s_t} + \beta_{s_t} D_{i,s_t} * S_{s_t} + \epsilon_{i,s} \tag{5.1}$$

Where Z_{i,s_t} is the vector of baseline covariates, D_{i,s_t} is an indicator variable for having an application score above the cutoff, γ_{s_t} are screenings of applicants fixed effects and $D_{i,s_t} * S_{s_t}$ is the interaction between treatment and screening of applicants S_{s_t}. To test for balance I test the null (H_0) of no treatment effect in each screening of applicants s_t: τ_{1,s_t} and β_{s_t} are equal to 0.

Columns 8 and 9 of Tables 6 and 7 present balance results in the evaluation sample in regular and elderly rounds, respectively. In both cases, the sample is balanced in the analyzed baseline characteristics.[52] Further, the bottom panel shows the results of a joint significance test of all covariates[53], confirming balance.[54]

Baseline survey response, geocoded location, female, married, Chilean, Santiago MSA, rent and rent burden do not vary across groups within all screenings of applicants. For these covariates, Columns 8 and 9 present a weaker yet commonly used balance tests, using a modified version of equation 5.1, that assumes $\beta_{s_t} = 0$. Then, τ_{1,s_t} tests for the null of zero weighted average

[52]Recall that Z_{i,s_t} includes covariates that are strongly correlated with application score while excluded from the application score formula, and also covariates that are not observed by MINVU during voucher assignment.

[53]I run a regression using treatment status as the dependent variables and all covariates that are available for the full sample. I replace missing values of distance to PHA with the observed distance in each screening and add a dummy to control for missing geocoded data.

[54]To further understand identification, Tables A.8 and A.7 analyze differences in the values of score components and total score between the evaluation sample and the randomized sample across regular and elderly rounds. There are very small differences, specially in elderly rounds. Looking at the application score formula, these differences do not have an economic meaning to the extent that they do not translate in significant household differences.

effect across all screening of applicants together in the pooled data (H_0'). [55]

5.1.2 Manipulation of the running variable

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

To analyze manipulation, I conduct a density test. Specifically, I do a binomial test of the probability of treatment in a small window around the cutoff (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.

Following Cattaneo (2018), I assume complete randomization ($q = 0.5$). Table 4 shows no evidence of manipulation. The observed treatment probability in the data used for window selection and in the subset of screenings included in the evaluation sample are not statistically different from 0.5.

5.2 Descriptive Statistics

Tables 7 and 6 show summary statistics of the evaluation sample W_0. Columns 1 to 3 present summary statistics of the pooled sample and Column 4 to 7 describe the control and treatment group in W_0.

The evaluation sample in regular rounds includes mostly Chilean families headed by young single mothers. Average income is US$530.[57] 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, 67% lives in high poverty counties.

In the evaluation sample in elderly rounds, 61% of elder applicants are women, 39% has a partner, and 54% is initially renting. Compared to the regular sample, the elderly have lower

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

[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.

[57] 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).

family income (average income is US$243) and are more likely to be under the poverty line. However, they live in denser, less poor counties, and are located closer to a PHA than those in regular rounds.

Voucher use among recipients in W_0 is 38%, lower than the national average (43%) (Table A.1 in the Appendix). The lease up rate varies both across regular and elderly rounds, and across s_t. Table A.2 shows the lease up rate of voucher recipients in each screening of applicants. On average, 29% of voucher holders in regular rounds have used their vouchers. In elderly rounds, the average lease up rate is 44.5%. In both cases, voucher use varies across assignment periods. In both cases, the maximum lease-up rate is observed in the assignment period in April 2018.[58]

An advantage of the multi-cutoff set up is that the evaluation sample includes treated and control units in close windows around different cutoffs, which may contribute to reducing the local nature of traditional single cutoff RDD estimates (Cattaneo et al., 2016). To assess the external validity of the results of this evaluation, Tables A.3 and A.4 in the Appendix show descriptive statistics for the full sample of voucher recipients in regular and elderly rounds, respectively.

Indeed, few and small differences are observed between all voucher recipients in the period of analysis and the subset of observations in the evaluation sample, specially in elderly rounds.[59] In regular rounds, main differences seemed to be explained by the exclusion of the screening of applicants in Santiago MSA, that did not have enough observations in the smallest window around the cutoff. Consistently, Santiago is underrepresented. Also, there is a larger fraction of families living in poor counties (67% vs 50%), yet the average proportion of families under the poverty line is similar (21% vs 25%).

In this sample I estimate causal effects using the following fixed effects model.

5.3 Econometric Model

I use the following equation to estimate the causal effect of the rental voucher program in outcome Y_{i,s_t} of applicant i in screening of applicants s_t from assignment period t.

[58] Recall that, in addition to the timing of the treatment, the cutoff and the regional distribution may vary across assignment periods.

[59] Further, Column 8 in Tables A.3 and A.4 in the Appendix shows overall very small differences between voucher recipients and non-voucher recipients in baseline covariates.

$$Y_{i,s_t} = \alpha + \tau D_{i,s_t} + \beta Z_{i,s_t} + \gamma_{s_t} + \epsilon_{i,s_t} \tag{5.2}$$

Where D_{i,s_t} is an indicator variable for having an application score above the cutoff, $X_{i,s_t} > 0$, Z_{i,s_t} is a vector of baseline covariates including covaraites used to asses balance in Section 5.1[60], and γ_{s_t} are screenings of applicants fixed effects. Note that each screening has a different cutoff and occurs in a specific moment in time (assignment period t).[61] Hence, I cannot disentangle the heterogeneity across different cutoffs from the heterogeneity introduced by having a voucher for different amounts of time.

For the period before the pandemic, outcomes Y_{i,s_t} includes overcrowding, residential mobility, savings for ownership[62], application to the main two homeownership programs in Chile and several neighborhood characteristics.[63]

In November 2020, eight month after the COVID-19 outbreak, Y_{i,s_t} 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 5.2 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). Specifically, τ recovers a double average: the weighted average of the average ITT effect within screenings of applicants s_t.

Local Average Treatment Effects (LATE) of using the rental voucher are not reported in the

[60] I use the subset of covariates that are available for the entire sample. In regular rounds, Z_{i,s_t} includes tenancy, savings, income, online application, previous applications to homeownership programs, non missing geocoded data, PHA in the county, high poverty county, high density county, female, chilean, family poverty status, age group, distance to closes PHA, married and baseline survey response. In elderly rounds I use the same set of controls, excluding online application, savings and baseline survey response.

[61] After the reform in 2019, in a specific region.

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

[63] 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 neighborhood income composition I include county poverty rate and the fraction of low income schools in the neighborhood i.e. the fraction of schools in which the majority of their students in low income families.

paper because of small sample sizes and low lease-up rate among voucher recipients.[64] As the program increases and raises its lease-up rate, future evaluations of the program could have enough statistical power to estimate the effect of using the voucher. Still, ITTs are the estimates of interest from a policy perspective in that lease-up cannot be enforced.

Next, I explain how I apply the window selection procedure to build the evaluation sample W_0.

The next section presents the results of the evaluation of the Chilean rental voucher program.

6 Results

This section presents the results of the evaluation of the rental voucher program using the fixed effect model in equation 5.2 in Section 5.3.

Tables 8 to 10 present the results for regular and elderly rounds before and during the pandemic. In each table, Specifications 1 and 2 include screening of applicants fixed effects. Specification 2 adds baseline covariates, $Z_{i,s}$, used in Section 5.1. I report large-sample based inference (F-test) (Column 8) and Fisherian randomization inference (Column 9), robust in small samples.

Otherwise noted, this section discusses the results in Column 7, estimating the ITT effects of holding a voucher (τ in equation 5.2) controlling for baseline covariates $Z_{i,s}$.[65] and Fisherian randomization inference.[66] The bottom panel in each table shows the Westfall-Young multiple-testing test of overall treatment irrelevance.

6.1 Treatment Effects in December 2019 (Before the Coronavirus Pandemic)

Two set of outcomes are created and analyzed using administrative data before the pandemic, in December 2019. The first group of outcomes assess the effect of the voucher on housing, residential mobility and neighborhood characteristics. The second group of outcomes measures

[64] 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 rental voucher program in Chile since LATE is τ adjusted by compliance rates in the treated group (Angrist and Pischke, 2008).

[65] Including covariates in $Z_{i,s}$ has efficiency gains and only little impact on the coefficients.

[66] 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 screening of applicants, as in a stratified experimental design.

the effect of the rental voucher on application to homeownership policies and private savings to buy a house.[67]

6.1.1 *Housing, residential mobility and neighborhood characteristics*

Panel A in Table 8 and 9 shows that, with respect to the control group mean in Column 2, holding a voucher reduced overcrowding in 6.1 pp (46%) in regular rounds and 2 pp (59%) in elderly rounds, respectively.[68] The reduction in overcrowding in elderly rounds is originated by voucher holders living in smaller families and having more available bedrooms. In contrast, results in regular rounds is driven exclusively by the increase in the number of bedrooms, not by a change in household size.

There are important differences in the effects of the rental voucher on residential mobility across regular and elderly voucher schemes (Panel B in Tables 8 and 9). In regular rounds, holding a voucher increased residential mobility in 7.6 pp (12%) and had a large overall effect on distance to initial location (15.7 km). Among the subset of movers i.e. excluding those who stayed in the same housing unit, voucher holders relocated farther away (27.5 km). Moreover, they were 6.9 pp (46%) more likely to move to another county. In elderly rounds, the effect of the voucher on mobility was larger (20 pp or 29.4%) and average distance was not affected by the voucher. Among mover, while not statistically significant (pvalue 0.150), the effect on distance is large and has a negative sign.

Panel C in Tables 8 and 9 shows the effects of the voucher on neighborhood characteristics. The rental voucher did not improve neighborhood characteristics in regular or elderly rounds. If anything, treated younger families with children in regular rounds moved to areas farther away from schools and pre-schools. In elderly rounds, on the other hand, neighborhoods characteristics remained the same except for the distance to the closest school, which is shorter as a result of the voucher

Altogether, despite the differences between the Chilean rental voucher and the US Section 8 program, the evaluation of the Chilean program before the pandemic shows similar results to

[67]Section 5.3 describes the outcomes in more detail.

[68]Overcrowding is defined as more than 2 family members sleeping together in one bedroom. This is one of the variables that MINVU uses to measure applicants' housing vulnerability. This is a more severe measure of overcrowding than the most commonly used in Chile. Three indicators are more brodaly used: mild (between 2.5 and 3.5 individuals per bedroom), high (3.5-5 individuals per bedroom) and critical (above 5) (Casen 2017).

previous literature focused in the US experience: holding a voucher improves housing conditions but it does not provide better neighborhoods for low income families. Further, this evidence suggests that while younger families receiving a less generous voucher end up farther away, the more generous elderly voucher scheme allow households to remain closer to their initial location.

6.1.2 *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 to buy a house i.e. both treated and controls kept their savings account opened with enough savings to apply to a homeownership program (Column 2).

In contrast, results in Table 9 show that the rental voucher increased application to homeownership programs in 3.2 pp (25.2%) among the elderly. Importantly, the effect is driven by an increase of the number of applications to the fully funded homeownership program (DS49) that provide housing at the periphery and has been associated to an increase in segregation of low income families (OECD, 2012).

I now turn to the results of the evaluation in the period after the COVID-19 outbreak in March 2020.

6.2 Treatment Effects During the Coronavirus Pandemic

This section uses a subset of applicants in the evaluation sample W_0 who responded an online survey implemented six to eight months following the Coronavirus pandemic outbreak, in March 2020. With this data, I use equation 5.2 to estimate the causal effects of holding a voucher during the pandemic. Given the few responses from elderly rounds, the following analysis focuses in regular rounds only.[69]

[69] Only 38% of the sample in elderly rounds had a valid email and 40% of these responded the survey, corresponding to 173 individuals. While there is no selective attrition and the sample is balance in baseline characteristics, sample size in two screening of applicants does not have the minimum number of observations at each side of the cutoff. Given the small sample left to do the analysis, I excluded elderly rounds altogether.

The survey was sent to applicants in the evaluation sample who had a valid email address: 88% in regular rounds.[70] Of these, 634 individuals in five regular screening of applicants s_t responded the survey (64% response rate).

Section B in the Appendix analyses selective attrition and balance in this sub-sample of applicants. The data shows no evidence that the treatment affected survey response. Further, local randomization assumptions hold in the subset of regular applicants inside W_0 who responded the survey.[71]

I use survey data to analyze the effect of the voucher on three sets of outcomes. The first one measures the effects on housing, household or family composition, residential mobility and neighborhood characteristics. With this survey, I am able to study additional housing and neighborhood outcomes, not available in administrative data, such as housing and neighborhood satisfaction, and housing consumption.

The second set of outcomes assess the effect of the voucher in health and subjective well being. The third and last set of outcomes includes employment, income during the pandemic, and measures families response to the large aggregated shock that came with the Covid-19 pandemic.

6.2.1 ***Housing, Residential mobility, Family Composition and Neighborhood Characteristics***

Eight months into the pandemic, 86% of the control group was renting and the voucher had no significant effect on tenancy (Panel A in Table 10).[72] Nonetheless, the policy affected other important housing margins.

In a highly informal rental market[73] holding a voucher increased the probability of having a lease in 12.6 percentage points (17%) and rent burden decreased in 12.6pp (25%). In particular, monthly out of the pocket rent payments decreased in US$48.5, while the average rent amount (US$261) and income remained the same.

[70] Section 5.1 shows that the evaluation sample is balanced in email availability.

[71] In addition, Table B.3 in the Appendix shows that the results in December 2019 for this subset of observations. While some coefficients are slightly smaller and others have larger p-values, overall the results look very similar to the estimates using the full sample of regular rounds in W_0 presented above.

[72] 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.

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

Compared to the control group, voucher recipients were about 8 pp. (10%) and 10 pp (13%) more likely to have an independent room for the kitchen[74] and a heating system, respectively.[75] Further, voucher holders were 8.4 pp (11%) more likely to be satisfied with their housing.

Results in crowding and residential mobility are similar to December 2019: overcrowding decreased in 6.4 pp and holding a voucher increased residential mobility in 9.4 pp (16%). Distance from initial location, on the other hand, is not statistically significant in this smaller sample.

This data suggests that reductions in overcrowding and increases in mobility rate do not fade over time. Distance, on the other hand, might decrease over time as those with stronger location preferences may take longer to search and move nearby. However, I cannot disentangle the effect of the pandemic from the long term effects of the voucher and, as shown below, the pandemic affected mobility and might have as well affected preferences for remaining close to social networks.

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. Voucher holders did not access neighborhoods with better characteristics.

Despite being 7.5 pp (12%) more likely to live close to a park, the effect of the voucher on access to childcare, schools, transportation and primary care centers was not statistically significant. Further, distance to work, family and friends did not change with the treatment.

Subjective coefficients regarding neighborhood satisfaction and safety perception are not statistically significant, nonetheless, they are all consistently negative. Furthermore, voucher holders were 7.7 pp (47%) more likely to have been recently exposed to gang fights in the neighborhood and 7.3 pp (25%) less likely to have a neighbor they could ask for childcare support, suggesting that the voucher may cause isolation from social networks.[76]

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

[75] Other housing expenses like cable TV, smart phone, computer or Wifi have a small and not significant effect.

[76] Coefficients for having close friends in the neighborhood or neighbors who they could for economic help are both negative, although not statistically significant. Isolation among rental policy beneficiaries in low income countries have been previously shown by (Barnhardt, Field and Pande, 2017).

6.2.2 *Health and Subjective Well Being*

I analyse the effect of the voucher on health related outcomes. The voucher did not affect overall physical health or happiness of voucher recipients.

The analysis of mental health outcomes shows mixed effects during this period. On the one hand, voucher holders were 8.1 pp (11.8%) less likely to feel worried (pvalue 0.114). On the other hand, they were 9.9 pp (24.6%) more likely to feel anxious according to the PHQ4 test.[77]

This data does not allow us to further understand this result. It might be that residential mobility increases anxiety but at the same time having the rent subsidized provide some peace of mind. Given the large burden on mental health imposed by the pandemic, specially in low income families, it is important to further understand the link between rental vouchers and mental health during a crisis.

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.

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

Panels C and D (Column 2) in Table 10 give us a sense of the size of the unexpected economic shock for young low income families in Chile. Roughly 88% of the control group suffered partial or total income loss after the outbreak in March 2020. Unemployment associated to the pandemic - mostly suspended contracts of dependent workers and independent workers who could not go out to work during strict quarantines - was 17%.

Fifteen different strategies to cope with the economic shock that came with the pandemic were surveyed. Almost the entire sample had to turned to some of these strategies to generate new income, cut spending or increase debt to adapt to the new economic circumstances. The data shows that the most common responses were to resort to government emergency assistance (58%) and the reduction of food expenses (59%). In addition, about half of the sample declares to have cut utility and other monthly bills and used family savings.

The rental voucher did not prevent the reduction of household income generated by the pan-

[77] 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 17, 40 and 31 percent of the control group were evaluated as normal, anxious and depressed using this test, respectively.

demic, although it reduced debt overload in 12.5 pp (18%). Interestingly, the voucher increased temporary unemployment in the beginning of the pandemic, yet it did not affect employment overall.[78] Further research is needed to explore whether the voucher allowed more single mother -main beneficiaries of regular rounds- to be at home with their kids when the pandemic first hit.

The voucher did not reduce the need to do extraordinary things to adapt to the new economic circumstances. However, the policy had important positive effects on the way in which families were coping with the consequences of the large unexpected shock. First, holding a rental voucher had positive effects in housing stability during the pandemic. Unwanted or emergency moves and delayed rent payments were 5.6 pp (72%) and 11 pp (45%) less likely among voucher holders, respectively.[79] Second, voucher holders were 12.6 pp (21.5%) and 8.5 pp (15%) less likely to reduce their food budget and rely on emergency relief policies, respectively.

These results point to a previously underappreciated insurance role of rental subsidies in helping poor households cope with negative aggregated shocks.

The next section analyzes robustness of the results of the evaluation using the sample of randomized vouchers.

6.3 Robustness Checks

In this section, I present estimates of equation 5.2 using the sample of vouchers that were randomly assigned by MINVU.

In the period of analysis, 2,400 elderly and 1,315 regular vouchers were randomly assigned. However, in some screenings of applicants only a few units were randomly assigned or randomization failed to provide a balance sample of treated and controls.[80] In other cases, there were only later treated in the control group. Dropping these screenings form the sample leaves

[78]Previous literature in the US (Jacob and Ludwig, 2012) show negative effects of rental policies on employment. In the Chilean rental voucher program the fixed (and smaller) amount of the subsidy does not change the marginal tax rate and therefore it is expected that employment is not affected by the policy.

[79]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).

[80]This happened mostly because MINVU had very few vouchers left to randomize over a large number of applicants at the cutoff or because they made some last minute change to the available number of vouchers or length of application periods. See Section 5 for more details.

1,187 elderly and 539 regular randomized vouchers to estimate the effect of holding a rental voucher, all of which are included in the smallest window possible around the cutoff in the evaluation sample. Then, estimates using this subset of observations is assessing robustness to window length.

Tables A.5 and A.6 in the Appendix show balance analysis in this sub sample in regular and elderly rounds, respectively. In both cases, there is no evidence of statistically significant differences between treated and controls. In addition, Tables A.9 and A.10 in the Appendix show the results for the period before the pandemic using the sample of randomized voucher in regular and elderly rounds. I analyzed this period only since the sample of regular applicants that responded the follow-up survey drops in more than half, having very few observations in some screenings of applicants for some of the outcomes.

Overall, although some coefficients are slightly different, results in Section 6.1 are robust to window length.

7 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 large 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.

Results in the period before the pandemic are similar to those of the existing literature for the US: holding a voucher seems to improve housing conditions by reducing overcrowding yet it does little to provide better neighborhoods for low income families.

In addition, the rental voucher did not discourage the application to homeownership programs of young families, as was the expectation of the government. Moreover, the voucher increased application among the elder —whose rents are almost fully subsidized in the rental voucher program.

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. Future research could explore how the recent reform that automatically renovates the rental voucher in elderly rounds may change the cost-benefit analysis of different housing policy options in the elder population.

In the eight months following the COVID-19 outbreak of March 2020, results show that the rental policy had an important effect on how young families with children were coping with the large unexpected income shock. Voucher holders were less likely to miss their rent payments or experience unwanted mobility. Also, they were less likely to reduce their food budget and rely on emergency relief policies during this period.

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 low and middle income 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.

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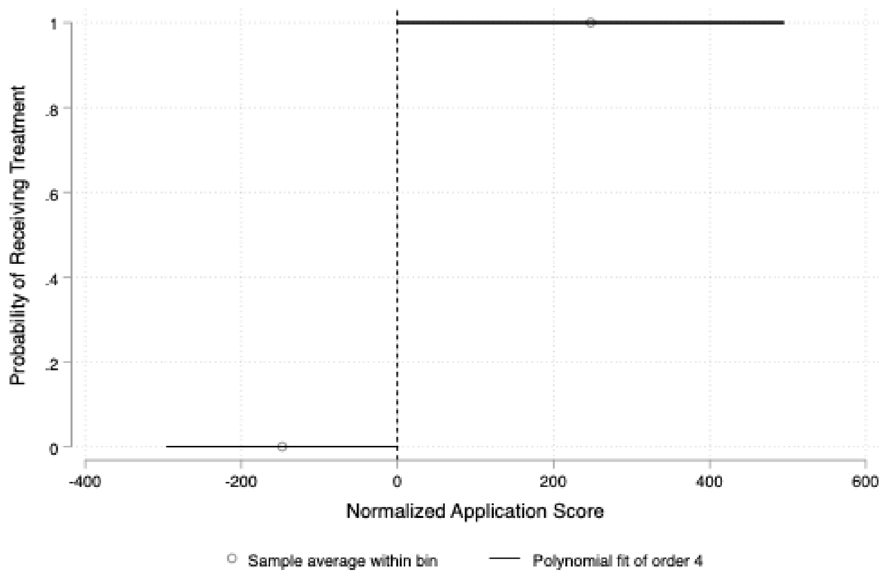
Figures and Tables

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

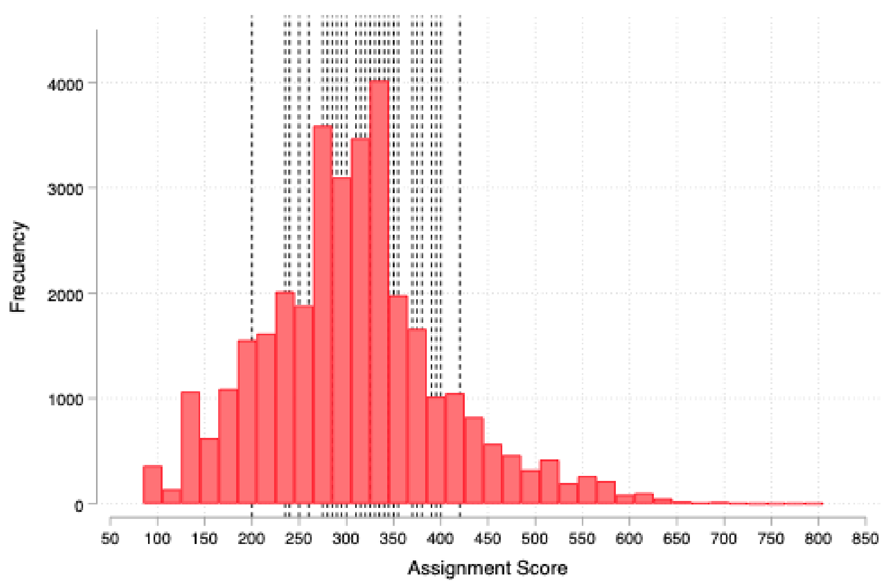


Figure 3: Regular Rounds

The figure presents the distribution of the application score in regular rounds in the pooled data. Vertical lines indicate multiple values of cutoff in the program.

1

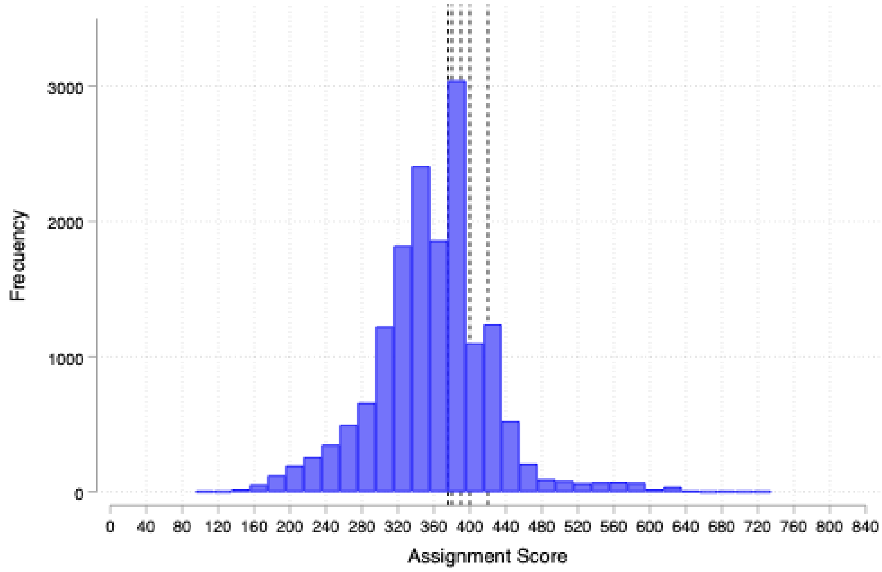


Figure 4: Elderly Rounds

The figure presents the distribution of the application score in elderly rounds in the pooled data. Vertical lines indicate multiple values of cutoff in the program.

Table 1: Application Score

	Score Component	Regular Rounds	Differences in Elderly Rounds
1	Household member[1]	40 per member	=
2	Children under 5[2]	30 per member	=
3	Children between 6 and 18	20 per member	=
4	Elderly*	30 per member	60 per member
5	Single Parent of 18 or younger children	35	=
6	Physical disability	30 per member	=
7	Tortured in dictatorship (applicant and/or partner)	100 per member	=
8	Military Service	20 per member	=
9	Gendarmerie Service (applicant and/or partner)	40 per member	=
10	Previous Applications (max 3)	20 per prev application	=
11	Social Vulnerability (RSH Index)[3]	0 (81-100th), 45 (71-80th), 90 (61-70th)	=
		135 (51-60th), 180 (40-50th)	=
12	Housing Vulnerability (sum of multiple scores)[4]	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. (1) Applicants are excluded in regular rounds. (2) Age by the end of the application year. (3) Includes crowding, housing quality, access to reliable water and basic sanitation. (4) Before the reform the formula was (13484-Family's FPS Score)/100, using the Social Vulnerability Card (FPS) instead of the RSH Index.

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: Density Test

	Binomial Test (q=0.5)				
Screenings	N (1)	Observed T (2)	Expected T (3)	Observed q (4)	p-value (5)
Window Selection	5,043	2,485	2,522	0.49	0.311
Evaluation Sample	2,801	1,405	1,400	0.50	0.880

This table presents the results of a binomial tests to evaluate the presence of manipulation in the running variable. The assumed probability of success q is 50%. See Section **??** for more details.

Table 5: Window Selection Results

Assignment Period	Screening (1)	Region (2)	Cutoff (3)	Controls (4)	Treated (5)	Length (6)	Min pvalue (7)	Left (8)	Right (9)
Regular Rounds									
19oct2017	1		200						
11apr2018	2		285	151	117	10	.12	-5	5
01jun2018	3		370						
28dec2018	4		345	295	80	7	.21	-5	2
10oct2019	5	6	285	97	49	20	.29	-10	10
10oct2019	6	9	285	153	122	30	.4	-15	15
10oct2019	7	10	275	48	19	4	.25	-2	2
Elderly Rounds	0								
04sep2017	1		380	377	279	15	.23	-10	5
11apr2018	2		380	275	110	10	.14	-5	5
19oct2018	3		420						
05jul2019	4	5	380	159	30	10	.18	-5	5
05jul2019	5	13	400	19	79	7	.28	-5	2

This table shows descriptive statistics of each assignment in the evaluation sample. Columns 1 describes the region where the assignment takes place after switching to regional screenings in 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 5.1 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 6: Balance in Baseline Characteristics in Elderly Rounds

	Summary Statistics							Balance Test	
		Pooled		Control		Treated			
	N (1)	Mean (2)	SD (3)	Mean (4)	SD (5)	Mean (6)	SD (7)	F-test (p) (8)	Rand-t (p) (9)
Tenant in baseline	1,328	0.54	0.50	0.55	0.50	0.53	0.50	0.451	0.471
Family income (UF)	1,328	6.22	2.81	6.19	2.74	6.29	2.92	0.611	0.633
PHA in county of residence	1,328	0.47	0.50	0.47	0.50	0.47	0.50	0.426	0.474
County above national poverty	1,328	0.32	0.46	0.34	0.47	0.28	0.45	0.262	0.398
High density county	1,328	0.54	0.50	0.52	0.50	0.58	0.49	0.877	0.875
Female	1,328	0.61	0.49	0.59	0.49	0.63	0.48	0.115	0.141
Age 70-79	1,328	0.56	0.50	0.57	0.49	0.54	0.50	0.300	0.324
Below family adjusted PL	1,328	0.56	0.50	0.57	0.50	0.55	0.50	0.864	0.872
KM to closest PHA	1,328	12.03	16.62	12.42	16.83	11.39	16.26	0.195	0.263
Valid email address	1,328	0.37	0.48	0.39	0.49	0.34	0.47	0.222	0.235
Spouse/partner	1,328	0.39	0.49	0.39	0.49	0.40	0.49	0.697	0.701
Chilean	1,328	0.98	0.13	0.98	0.14	0.99	0.12	0.078*	0.649
Previous app. to ownership subsidy	1,328	0.05	0.22	0.06	0.23	0.04	0.20	0.452	0.312
Income documents to PHA	1,328	0.99	0.12	0.99	0.11	0.98	0.13	0.948	0.492
Santiago MSA	1,328	0.27	0.44	0.23	0.42	0.34	0.47	0.847	0.066*
Geocoded location	1,328	0.96	0.20	0.95	0.21	0.97	0.18	0.324	0.483
SCREENING FE								Yes	Yes
Joint Significance F-Test (p)								0.120	0.170

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

Table 7: Balance in Baseline Characteristics in Regular Rounds

		Summary Statistics						Balance Test	
		Pooled		Control		Treated			
	N	Mean	SD	Mean	SD	Mean	SD	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tenant in baseline	1,131	0.75	0.43	0.75	0.43	0.74	0.44	0.978	0.978
Saving balance on application day (UF)	1,131	16.60	29.44	16.75	34.31	16.31	16.47	0.388	0.402
Family income (UF)	1,131	13.33	4.80	13.24	4.61	13.50	5.13	0.686	0.688
Online application	1,131	0.36	0.48	0.37	0.48	0.35	0.48	0.512	0.486
High density county	1,131	0.39	0.49	0.40	0.49	0.37	0.48	0.906	0.919
County above national poverty	1,131	0.67	0.47	0.65	0.48	0.71	0.46	0.648	0.672
PHA in county of residence	1,131	0.47	0.50	0.48	0.50	0.47	0.50	0.740	0.747
Previous app. to ownership subsidy	1,131	0.15	0.36	0.14	0.35	0.17	0.37	0.747	0.733
Age 25-35	1,131	0.60	0.49	0.58	0.49	0.64	0.48	0.103	0.128
Below family adjusted PL	1,131	0.21	0.41	0.20	0.40	0.22	0.42	0.733	0.744
KM to closest PHA	1,131	18.03	22.99	17.52	22.20	19.01	24.43	0.486	0.497
Valid email address	1,131	0.88	0.33	0.88	0.33	0.88	0.33	0.810	0.823
Want to stay same neighborhood	677	0.56	0.50	0.57	0.50	0.55	0.50	0.713	0.737
Satisfaction with housing unit	723	0.66	0.47	0.66	0.47	0.65	0.48	0.156	0.196
Does not know other applicants	659	0.53	0.50	0.51	0.50	0.57	0.50	0.095*	0.118
Access to car	655	0.34	0.47	0.35	0.48	0.31	0.47	0.537	0.562
(Perceived) High social class neighbors	702	0.49	0.50	0.49	0.50	0.49	0.50	0.627	0.667
Baseline Survey response	995	0.82	0.38	0.82	0.39	0.82	0.38	0.764	0.941
Geocoded location	1,131	0.89	0.31	0.90	0.30	0.88	0.32	0.146	0.890
Female	1,131	0.89	0.31	0.89	0.31	0.89	0.31	0.611	0.233
Spouse/partner	1,131	0.17	0.38	0.18	0.38	0.15	0.36	0.158	0.751
Chilean	1,131	0.94	0.24	0.93	0.26	0.97	0.18	0.007***	0.557
Santiago MSA	1,131	0.11	0.31	0.11	0.32	0.10	0.30	0.887	0.821
Rent	772	5.65	3.03	5.65	3.12	5.66	2.81	0.622	0.943
Rent burden	772	0.47	0.28	0.47	0.29	0.48	0.24	0.654	0.235
SCREENING FE								Yes	Yes
Joint Significance F-Test (p)								0.245	0.373

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 5.1 (H_0) and Panel B presents results under the weaker null hypothesis (H_0') excluding interaction terms from 5.1. See Section 5.1 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 8: Effect of Regular Voucher Before the Covid-19 Pandemic (2019)

				Specification 1			Specification 2		
		Control		Treatment	OLS	Rand-t	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	p-value	p-value	Effect	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Housing Conditions									
Household size Dec 2019	1,131	2.847	1.204	0.052	0.494	0.506	0.058	0.434	0.459
Number of bedrooms	1,121	1.750	0.820	0.194	0.000***	0.000***	0.203	0.000***	0.000***
Number of people per bedroom	1,121	1.816	0.770	-0.175	0.000***	0.000***	-0.182	0.000***	0.000***
Overcrowing indicator	1,121	0.133	0.340	-0.061	0.001***	0.001***	-0.061	0.001***	0.002***
B. Residential Mobility									
Stayed in same unit	1,003	0.587	0.493	-0.089	0.008***	0.009***	-0.076	0.021**	0.018**
Distance (km)	1,003	7.392	45.429	16.781	0.039**	0.029**	15.711	0.049**	0.037**
Distance (km) (Movers)	441	17.906	69.449	30.031	0.067*	0.058*	27.523	0.101	0.090*
Stayed 1km or less from application location	441	0.332	0.472	-0.051	0.283	0.277	-0.048	0.321	0.323
Moved to another county	441	0.150	0.357	0.073	0.052*	0.056*	0.069	0.051*	0.040**
C. Neighborhood Characteristics									
Distance to closest municipality	1,003	3.273	4.627	0.394	0.245	0.249	0.387	0.249	0.236
Distance to closest school (km)	1,003	0.896	1.797	0.441	0.027**	0.031**	0.446	0.032**	0.036**
Distance to closest Pre-Shcool (km)	1,003	0.976	2.286	0.489	0.028**	0.039**	0.493	0.028**	0.030**
Distance to closest Primary Care (km)	930	1.545	2.440	0.348	0.158	0.163	0.331	0.195	0.203
Number of Schools in 1Km	1,003	4.851	4.265	-0.297	0.330	0.340	-0.256	0.384	0.388
Number of Schools in 2Km	1,003	14.858	12.818	-0.997	0.231	0.235	-0.786	0.271	0.272
Number of Preschool in 1Km	1,003	3.008	2.535	-0.070	0.691	0.719	-0.045	0.790	0.800
Number of Health Care in 2km	1,003	4.959	4.567	-0.326	0.252	0.240	-0.250	0.308	0.293
Fraction of Public Schools 1Km	827	0.445	0.288	-0.008	0.712	0.687	-0.006	0.785	0.767
Fraction of Subsidized Schools 1Km	827	0.521	0.281	-0.000	0.984	0.984	-0.003	0.894	0.899
Fraction of Private Schools 1Km	827	0.034	0.105	0.008	0.262	0.270	0.009	0.241	0.241
Mat. SIMCE, 3 Closest School 2km	878	263.786	17.432	0.293	0.818	0.828	-0.090	0.943	0.951
Mat. SIMCE, 3 Closest School 2km	878	249.516	18.520	0.246	0.858	0.869	0.070	0.959	0.951
Fraction of Low Income Schools 1km	827	0.619	0.341	0.015	0.547	0.547	0.014	0.563	0.569
Fraction of Low Income Schools 2km	886	0.597	0.266	0.002	0.909	0.900	0.000	0.995	0.996
County poverty rate	1,003	0.115	0.064	-0.000	0.950	0.957	-0.002	0.548	0.554
Total crime (County z-score)	1,003	1.014	1.556	0.122	0.282	0.283	0.121	0.118	0.103
D. Homeownership									
Application to Ownership Programs	1,131	0.313	0.464	0.027	0.343	0.304	0.012	0.583	0.589
Application to partially funded program (DS1)	1,131	0.222	0.416	0.029	0.273	0.230	0.017	0.428	0.409
Application to fully funded program (DS49)	1,131	0.124	0.329	0.014	0.470	0.395	0.010	0.599	0.570
Active ownership savings account	1,131	0.911	0.285	0.010	0.565	0.534	0.008	0.644	0.644
Balance in ownership savings account (US)	1,033	24.132	31.862	1.071	0.597	0.580	0.365	0.838	0.852
SCREENING FE				YES	YES	YES	YES	YES	YES
CONTROLS (Z)				NO	NO	NO	YES	YES	YES
Westfall-Young Multiple Testing (p-value)						0.020**			0.020**

This table presents estimates of equation 5.2 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 5.1. 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	OLS p-value	Rand-t p-value	Treatment Effect	OLS p-value	Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Housing Conditions									
Household size Dec 2019	1,328	1.600	1.091	-0.144	0.012**	0.006***	-0.175	0.001***	0.001***
Number of bedrooms	1,252	1.358	0.735	0.443	0.000***	0.001***	0.443	0.000***	0.001***
Number of people per bedroom	1,247	1.243	0.597	-0.298	0.000***	0.001***	-0.317	0.000***	0.001***
Overcrowing indicator	1,314	0.034	0.182	-0.017	0.097*	0.110	-0.020	0.046**	0.049**
B. Residential Mobility									
Stayed in same unit	1,198	0.680	0.467	-0.201	0.000***	0.001***	-0.199	0.000***	0.001***
Distance (km)	1,198	16.123	116.539	1.389	0.838	0.843	-0.520	0.938	0.931
Distance (km) (Movers)	458	50.324	201.952	-19.348	0.249	0.262	-26.552	0.140	0.150
Stayed 1km or less from application location	458	0.301	0.460	-0.003	0.938	0.932	-0.002	0.956	0.960
Moved to another county	460	0.266	0.443	-0.022	0.589	0.583	-0.026	0.514	0.521
C. Neighborhood Characteristics									
Distance to closest municipality	1,198	3.777	7.220	-0.554	0.138	0.124	-0.481	0.189	0.189
Distance to closest school (km)	1,198	1.117	4.007	-0.130	0.473	0.511	-0.099	0.612	0.647
Distance to closest Pre-Shcool (km)	1,198	1.129	4.269	-0.155	0.451	0.486	-0.111	0.612	0.640
Distance to closest Primary Care (km)	1,137	1.614	4.103	-0.120	0.561	0.581	-0.081	0.704	0.721
Number of Schools in 1Km	1,198	7.056	5.731	-0.315	0.353	0.339	-0.536	0.097*	0.086*
Number of Schools in 2Km	1,198	21.328	15.354	-0.071	0.940	0.949	-0.744	0.361	0.363
Number of Preschool in 1Km	1,198	3.731	2.925	-0.136	0.470	0.474	-0.202	0.267	0.270
Number of Health Care in 2km	1,198	6.546	5.648	-0.145	0.675	0.658	-0.364	0.238	0.207
Fraction of Public Schools 1Km	1,050	0.405	0.241	-0.010	0.537	0.548	-0.004	0.796	0.809
Fraction of Subsidized Schools 1Km	1,050	0.529	0.244	0.007	0.660	0.658	0.004	0.823	0.814
Fraction of Private Schools 1Km	1,050	0.066	0.137	0.003	0.747	0.750	0.000	0.957	0.956
Mat. SIMCE, 3 Closest School 2km	1,087	264.174	17.522	-0.758	0.507	0.527	-0.874	0.455	0.478
Mat. SIMCE, 3 Closest School 2km	1,088	251.717	18.091	-0.129	0.914	0.909	-0.240	0.843	0.836
Fraction of Low Income Schools 1km	1,050	0.463	0.337	-0.025	0.275	0.277	-0.007	0.736	0.716
Fraction of Low Income Schools 2km	1,092	0.438	0.266	-0.023	0.198	0.199	-0.008	0.639	0.615
County poverty rate	1,200	0.084	0.048	-0.003	0.389	0.368	0.002	0.467	0.467
Total crime (County z-score)	1,200	1.698	1.965	0.096	0.451	0.461	-0.047	0.648	0.673
D. Homeownership									
Application to Ownership Programs	1,328	0.123	0.329	0.021	0.292	0.305	0.032	0.052*	0.052*
Application to partially funded program (DS1)	1,328	0.077	0.267	-0.002	0.891	0.916	0.005	0.687	0.690
Application to fully funded program (DS49)	1,328	0.061	0.240	0.027	0.095*	0.073*	0.030	0.044**	0.034**
SCREENING FE				YES	YES	YES	YES	YES	YES
CONTROLS (Z)				NO	NO	NO	YES	YES	YES
Westfall-Young Multiple Testing (p-value)						0.005***			0.005***

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	OLS	Rand-t	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	p-value	p-value	Effect	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Housing and Household Characteristics									
Tenancy	480	0.862	0.345	0.014	0.676	0.680	0.026	0.421	0.429
Formal Lease	394	0.733	0.444	0.119	0.007***	0.009***	0.126	0.007***	0.007***
Total rent (unit)	387	261.322	94.026	-3.780	0.705	0.727	-0.294	0.974	0.983
Rent paid	376	241.706	106.387	-51.372	0.000***	0.001***	-48.538	0.000***	0.001***
Rent burden (rent paid)	334	0.509	0.271	-0.137	0.000***	0.000***	-0.126	0.000***	0.000***
Rent burden (rent amount)	344	0.553	0.252	-0.041	0.108	0.122	-0.032	0.204	0.223
Shelter deprivation (slum, shared room or other)	552	0.180	0.385	-0.038	0.256	0.278	-0.023	0.470	0.473
Lives with Parents/Grand parents	410	0.143	0.351	-0.001	0.970	0.964	-0.010	0.785	0.797
Living with grandchild	410	0.023	0.151	0.004	0.832	0.861	0.012	0.508	0.493
Spouse/Partner	409	0.319	0.467	0.071	0.166	0.174	0.078	0.102	0.103
Child borned since application	380	0.133	0.340	0.050	0.238	0.231	0.045	0.289	0.294
Household Size	512	3.331	1.461	-0.161	0.201	0.203	-0.118	0.341	0.347
Number of bedrooms	496	2.223	0.897	0.028	0.705	0.696	0.037	0.618	0.609
Number of people per bedroom	496	1.653	0.843	-0.145	0.038**	0.037**	-0.147	0.035**	0.032**
Overcrowding indicator	498	0.129	0.336	-0.064	0.021**	0.025**	-0.064	0.030**	0.035**
Pet Owner	410	0.016	0.124	-0.012	0.278	0.271	-0.009	0.400	0.454
Laundry Room	428	0.416	0.494	-0.003	0.955	0.944	0.005	0.924	0.916
Kitchen Room	480	0.796	0.404	0.075	0.042**	0.038**	0.080	0.034**	0.030**
Hot water	496	0.850	0.357	-0.006	0.868	0.855	-0.023	0.508	0.509
Heat system	496	0.775	0.418	0.129	0.000***	0.001***	0.103	0.001***	0.002***
Cable TV	495	0.634	0.483	-0.016	0.718	0.688	-0.042	0.366	0.325
Wifi	493	0.564	0.497	0.021	0.648	0.655	0.004	0.935	0.930
Smart Phone Lease	491	0.641	0.480	0.052	0.250	0.255	0.038	0.406	0.422
Computer	495	0.497	0.501	0.053	0.260	0.243	0.041	0.383	0.354
B. Residential Mobility									
Stayed in same unit	441	0.591	0.493	-0.094	0.059*	0.056*	-0.094	0.065*	0.058*
Distance (km)	358	7.779	44.523	9.971	0.326	0.370	8.550	0.301	0.343
Number of moves from application	441	0.737	1.137	-0.001	0.992	0.996	0.009	0.930	0.930
Less than 6 months current house	546	0.141	0.348	-0.034	0.245	0.253	-0.036	0.215	0.225
Between 6 months and 1 year current house	546	0.147	0.354	0.088	0.011**	0.012**	0.086	0.018**	0.022**
Between 1 and 2 years current house	546	0.214	0.411	0.076	0.052*	0.055*	0.084	0.034**	0.037**
2 or more years current house	546	0.499	0.501	-0.130	0.003***	0.003***	-0.134	0.003***	0.002***
Less than 6 months current neighborhood	538	0.095	0.294	-0.007	0.784	0.786	-0.012	0.630	0.640
Between 6 months and 1 year current neighborhood	538	0.131	0.338	0.058	0.075*	0.081*	0.056	0.101	0.105
Between 1 and 2 years current neighborhood	538	0.188	0.391	0.042	0.249	0.277	0.042	0.256	0.283
2 or more years current neighborhood	538	0.586	0.493	-0.093	0.036**	0.041**	-0.085	0.057*	0.069*
C. Employment and Income									
Work	406	0.700	0.459	-0.018	0.710	0.721	-0.036	0.465	0.499
Covid-19 unemployment	406	0.170	0.376	0.056	0.172	0.206	0.068	0.100*	0.117
Debt overload	414	0.696	0.461	-0.124	0.016**	0.010**	-0.125	0.017**	0.009***
No income loss after COVID-19	415	0.215	0.411	0.070	0.129	0.149	0.060	0.209	0.232
D. Household Response During in Covid-19 Crisis									
Covid-19 response: moved out	411	0.077	0.267	-0.052	0.017**	0.022**	-0.056	0.014**	0.016**
Covid-19 response: delayed rent payments	364	0.241	0.429	-0.117	0.006***	0.007***	-0.108	0.012**	0.018**
Covid-19 response: others moved in	411	0.066	0.248	0.011	0.702	0.687	0.024	0.418	0.432
Covid-19 response: reduced food budget	411	0.587	0.493	-0.109	0.038**	0.044**	-0.126	0.018**	0.023**
Covid-19 response: reduced health expenses	411	0.363	0.482	-0.060	0.217	0.206	-0.067	0.180	0.175
Covid-19 response: reduced utilities expenses	411	0.467	0.500	-0.033	0.528	0.536	-0.030	0.574	0.587
Covid-19 response: delayed monthly billings	411	0.444	0.498	-0.076	0.137	0.127	-0.085	0.110	0.094*
Covid-19 response: informal loan (family/friends)	411	0.405	0.492	-0.054	0.283	0.294	-0.051	0.325	0.330
Covid-19 response: formal loan or credit	411	0.224	0.418	-0.054	0.192	0.208	-0.048	0.255	0.272
Covid-19 response: sold expensive goods (vehicle, jewelry, etc.)	411	0.158	0.366	0.011	0.758	0.744	0.023	0.527	0.504
Covid-19 response: sold or rented real state/land	411	0.004	0.062	0.015	0.238	0.256	0.017	0.174	0.193
Covid-19 response: used family savings	411	0.494	0.501	-0.013	0.808	0.800	-0.028	0.596	0.618
Covid-19 response: new activities to generate more income	411	0.347	0.477	-0.037	0.448	0.431	-0.046	0.363	0.334
Covid-19 response: gave or lent money to family members	411	0.116	0.321	-0.030	0.362	0.345	-0.029	0.399	0.396
Covid-19 response: applied/used government emergency solutions	411	0.583	0.494	-0.083	0.110	0.096*	-0.085	0.111	0.100
Covid-19 response: none	411	0.058	0.234	-0.003	0.895	0.890	0.002	0.926	0.932
Covid-19 response: other	411	0.050	0.219	-0.028	0.105	0.114	-0.013	0.437	0.438
E. Virus Transmission and Mental Health									
At least one Covid-19 case- Home	388	0.041	0.199	0.025	0.309	0.322	0.019	0.473	0.481
At least one Covid-19 case- Family	388	0.254	0.436	0.018	0.713	0.710	0.020	0.683	0.679
At least one Covid-19 case- Friends	388	0.197	0.398	-0.010	0.817	0.824	0.000	0.999	1.000
At least one Covid-19 case- Neighbors	388	0.180	0.385	-0.015	0.714	0.707	-0.012	0.782	0.778
At least one Covid-19 case- Work	388	0.193	0.395	-0.039	0.326	0.313	-0.062	0.144	0.144
At least one Covid-19 case- Other acquaintance	388	0.324	0.469	0.031	0.549	0.562	0.023	0.650	0.652
Do not know any COVID-19 case	388	0.307	0.462	-0.056	0.244	0.219	-0.037	0.441	0.403
Good health	397	0.586	0.494	0.063	0.225	0.203	0.057	0.277	0.257
Happy	382	0.702	0.458	0.046	0.339	0.335	0.029	0.566	0.551
Feel depressed	393	0.794	0.405	-0.069	0.130	0.141	-0.050	0.280	0.275
Feel worried	393	0.685	0.465	-0.094	0.067*	0.069*	-0.081	0.122	0.114
PHQ4 Test: Normal	393	0.169	0.376	-0.031	0.436	0.477	-0.034	0.423	0.449
PHQ4 Test: Anxiety	393	0.403	0.492	0.111	0.038**	0.039**	0.099	0.067*	0.061*
PHQ4 Test: Depression	393	0.310	0.464	0.049	0.332	0.353	0.028	0.583	0.611

Table 10: *(Continuation)* Effect of Regular Voucher During the Covid-19 Pandemic (2020)

				Specification 1			Specification 2		
		Control		Treatment	OLS	Rand-t	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	p-value	p-value	Effect	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
F. Neighborhood Characteristics									
Close to childcare/pre-school (4 blocks)	538	0.580	0.494	0.009	0.837	0.849	0.014	0.766	0.757
Close to Schools (4 blocks)	538	0.586	0.493	-0.065	0.149	0.145	-0.051	0.259	0.256
Close to subway/bus (4 blocks)	538	0.642	0.480	0.013	0.760	0.771	0.038	0.394	0.397
Close to Park (4 blocks)	538	0.612	0.488	0.063	0.146	0.153	0.075	0.080*	0.089*
Close to Health Care (4 blocks)	538	0.470	0.500	-0.021	0.641	0.646	0.001	0.974	0.972
Less than 15 min commute time to family	370	0.439	0.497	0.009	0.867	0.842	-0.005	0.937	0.938
Less than 15 min commute time to friends	325	0.458	0.499	0.031	0.604	0.617	0.045	0.459	0.472
Less than 15 min commute time to school	347	0.519	0.501	0.017	0.768	0.759	0.003	0.960	0.957
Less than 30 min commute time to work	310	0.713	0.454	-0.029	0.611	0.599	-0.025	0.670	0.666
Street Alcohol Consumption	397	0.544	0.499	0.027	0.606	0.614	0.021	0.694	0.709
Street Drug Consumers	397	0.435	0.497	-0.027	0.596	0.608	-0.034	0.515	0.524
Street Drug Trafficking	397	0.274	0.447	-0.026	0.579	0.591	-0.024	0.605	0.603
Destroyed property	397	0.327	0.470	-0.029	0.558	0.556	-0.028	0.564	0.581
Graffiti	397	0.210	0.408	-0.029	0.468	0.452	-0.039	0.362	0.337
Gang Fights	397	0.165	0.372	0.064	0.133	0.119	0.077	0.076*	0.056*
People Carrying guns	397	0.190	0.393	0.015	0.723	0.705	0.015	0.716	0.691
Shooting	397	0.387	0.488	0.043	0.413	0.421	0.047	0.368	0.382
Prostitution	397	0.060	0.239	0.005	0.842	0.847	0.012	0.629	0.624
Feels safe walking at night	398	0.550	0.498	-0.034	0.529	0.544	-0.033	0.545	0.561
Feels safe inside the house at night	394	0.765	0.425	0.031	0.496	0.504	0.025	0.573	0.589
Victim of violence (physical)	391	0.119	0.324	-0.023	0.449	0.437	-0.016	0.625	0.624
Victim of robbery	370	0.298	0.458	0.005	0.912	0.915	0.010	0.833	0.838
G. Housing and Neighborhood Satisfaction									
Satisfaction current housing unit	547	0.754	0.431	0.078	0.029**	0.028**	0.084	0.019**	0.020**
Satisfaction current neighborhood	526	0.802	0.399	-0.023	0.524	0.515	-0.031	0.378	0.392
Would ask neighbors for childcare	510	0.297	0.458	-0.081	0.046**	0.044**	-0.073	0.082*	0.081*
Has close friends in the neighborhood	513	0.436	0.497	-0.034	0.464	0.443	-0.019	0.688	0.672
Would ask neighbors for economic help	507	0.243	0.429	-0.036	0.348	0.348	-0.042	0.282	0.270
SCREENING FE				YES	YES	YES	YES	YES	YES
CONTROLS (Z)				NO	NO	NO	YES	YES	YES
Westfall-Young Multiple Testing (p-value)						0.000***			0.000***

This table presents estimates of equation 5.2 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 5.1. 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.

A Appendix: Additional Figures and Tables

Table A.1: Program Summary Statistics

Round	Applicants	Voucher Recipients	Lease-up (N) May-20	Lease-up (%) May-20
1-2014 Regular	5023	5004	1994	40%
2-2014 Regular	2045	2045	906	44%
2015 Regular	3525	3001	1391	46%
2016 Regular	11892	10576	4676	44%
2017 Regular	13634	8785	3809	43%
1-2018 Regular	8350	3002	1345	45%
2-2018 Regular	9175	4238	1816	43%
2019 Regular	10584	7536	2775	37%
Total Regular Rounds	64228	44187	18712	42%
2016 Elderly (Pilot)	630	630	326	52%
2017 Elderly	6292	1871	945	51%
1-2018 Elderly	5858	2068	1110	54%
2-2018 Elderly	4526	939	440	47%
2019 Elderly	7118	1049	471	45%
Total Elderly Rounds	24424	6557	3292	50%
Total Program	88652	50744	22004	43%

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 A.2: Lease-up rate by screening of assignment in the Evaluation Sample

	All	Apr 2018	Dec 2018	Oct 2019 O'Higgins	Oct 2019 Araucania	Oct 2019 Los Lagos
Regular	0.29	0.42	0.29	0.24	0.21	0.11
	All	**Sept 2017**	**Apr 2018**	**Jul 2019 Valparaiso**	**Jul 2019 Santiago**	
Elderly	0.44	0.45	0.57	0.43	0.24	

Table A.3: Summary Statistics Voucher Recipients and Non Recipients Regular Rounds

	All Applicants			Recipients		Non-Recipients		Difference
	N	Mean	SD	Mean	SD	Mean	SD	(7)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tenant in baseline	41,738	0.70	0.46	0.68	0.47	0.72	0.45	0.04
Saving balance on application day (UF)	41,738	14.47	16.64	14.18	16.18	14.84	17.21	0.65
Family income (UF)	41,738	14.80	5.47	14.56	5.56	15.12	5.32	0.57
Online application	41,738	0.34	0.47	0.34	0.47	0.34	0.47	0.00
High density county	41,738	0.45	0.50	0.43	0.50	0.47	0.50	0.03
County above national poverty	41,738	0.50	0.50	0.50	0.50	0.50	0.50	-0.01
PHA in county of residence	41,738	0.49	0.50	0.49	0.50	0.50	0.50	0.01
Previous app. to ownership subsidy	41,738	0.14	0.35	0.14	0.35	0.14	0.34	-0.01
Age at application	41,738	34.71	10.46	34.02	9.57	35.60	11.46	1.57
Below family adjusted PL	41,737	0.25	0.43	0.32	0.47	0.16	0.37	-0.16
KM to closest PHA	41,738	14.61	19.04	15.08	19.46	14.00	18.47	-1.08
Valid email address	41,738	0.86	0.35	0.85	0.36	0.87	0.33	0.02
Want to stay same neighborhood	24,771	0.54	0.50	0.54	0.50	0.53	0.50	-0.00
Satisfaction with housing unit	25,983	0.59	0.49	0.58	0.49	0.60	0.49	0.02
Does not know other applicants	24,250	0.62	0.49	0.61	0.49	0.62	0.49	0.01
Access to car	21,943	0.33	0.47	0.33	0.47	0.33	0.47	-0.01
(Perceived) High social class neighbors	25,541	0.51	0.50	0.51	0.50	0.51	0.50	-0.00
Baseline Survey response	36,342	0.79	0.41	0.78	0.41	0.80	0.40	0.02
Geocoded location	41,738	0.90	0.29	0.90	0.30	0.91	0.29	0.01
Female	41,713	0.83	0.38	0.83	0.37	0.83	0.38	-0.00
Spouse/partner	41,738	0.25	0.43	0.25	0.44	0.24	0.42	-0.02
Chilean	41,713	0.92	0.28	0.91	0.29	0.93	0.26	0.02
Santiago MSA	41,738	0.20	0.40	0.20	0.40	0.20	0.40	-0.00
Rent	17,877	6.12	2.74	6.17	2.78	6.04	2.69	-0.13
Rent burden	17,872	0.46	0.26	0.47	0.26	0.44	0.26	-0.03
Score Components and Total Score								
Family size score	41,738	68.03	34.87	81.11	37.42	51.10	21.65	-30.01
Single parenthood score	41,738	19.79	17.35	20.85	17.18	18.41	17.48	-2.44
Number of children under 5 score	41,738	14.38	17.37	18.32	18.51	9.28	14.22	-9.04
Number of children 6 to 18 score	41,738	14.73	15.60	18.48	17.09	9.87	11.77	-8.61
Social vulnerability score	41,738	158.42	38.19	170.86	23.55	142.31	46.57	-28.55
Housing vulnerability score	41,738	36.50	54.16	55.48	61.94	11.92	26.34	-43.55
Application score	41,738	324.46	100.62	375.41	91.90	258.47	67.69	-116.95

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.4: Summary Statistics Voucher Recipients and Non Recipients Elderly Rounds

	All Applicants			Recipients		Non-Recipients		Difference
	N	Mean	SD	Mean	SD	Mean	SD	(7)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tenant in baseline	23,462	0.61	0.49	0.54	0.50	0.64	0.48	0.10
Family income (UF)	23,462	6.74	3.82	6.26	3.17	6.90	4.01	0.63
High density county	23,462	0.49	0.50	0.46	0.50	0.51	0.50	0.04
County above national poverty	23,462	0.39	0.49	0.40	0.49	0.38	0.49	-0.02
PHA in county of residence	23,462	0.49	0.50	0.51	0.50	0.49	0.50	-0.02
Previous app. to ownership subsidy	23,462	0.07	0.25	0.06	0.23	0.07	0.25	0.01
Age at application	23,462	70.46	6.65	75.29	6.89	68.85	5.72	-6.44
Below family adjusted PL	23,462	0.60	0.49	0.60	0.49	0.60	0.49	-0.00
KM to closest PHA	22,175	13.53	19.42	14.04	20.16	13.35	19.16	-0.69
Valid email address	23,462	0.41	0.49	0.34	0.48	0.43	0.50	0.09
Geocoded location	23,462	0.95	0.23	0.94	0.23	0.95	0.23	0.00
Female	23,371	0.61	0.49	0.55	0.50	0.63	0.48	0.08
Spouse/partner	23,462	0.38	0.49	0.37	0.48	0.38	0.49	0.01
Chilean	23,371	0.98	0.15	0.98	0.14	0.98	0.15	-0.00
Santiago MSA	23,462	0.25	0.43	0.22	0.42	0.25	0.44	0.03
Income documents to PHA	23,462	0.95	0.22	0.97	0.17	0.94	0.23	-0.03
Score Components and Total Score								
Family size score	23,462	43.06	14.46	47.42	23.97	41.60	8.84	-5.82
Single parenthood score	23,462	0.18	2.48	0.18	2.53	0.17	2.46	-0.01
Number of children under 5 score	23,462	0.25	2.96	0.76	5.26	0.08	1.52	-0.68
Number of children 6 to 18 score	23,462	0.85	4.90	1.84	7.72	0.51	3.41	-1.33
Social vulnerability score	23,462	165.06	33.71	178.49	9.54	160.56	37.50	-17.93
Housing vulnerability score	23,462	15.99	37.49	40.99	61.77	7.61	17.85	-33.38

This table replicates the analysis in Table A.3 using elderly rounds data. See Table A.3 for details

Table A.5: Balance in Baseline Characteristics in Sample of Randomized Vouchers in Regular Rounds

		Summary Statistics						Balance	
		Pooled		Control		Treated		Test	
	N	Mean	SD	Mean	SD	Mean	SD	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tenant in baseline	539	0.70	0.46	0.70	0.46	0.69	0.46	0.965	0.967
Saving balance on application day (UF)	539	16.03	14.67	16.04	14.37	16.00	15.13	0.425	0.469
Family income (UF)	539	12.31	3.71	12.39	3.58	12.18	3.89	0.227	0.259
Online application	539	0.41	0.49	0.42	0.49	0.41	0.49	0.204	0.225
High density county	539	0.39	0.49	0.40	0.49	0.38	0.49	0.883	0.889
County above national poverty	539	0.60	0.49	0.55	0.50	0.67	0.47	0.328	0.340
PHA in county of residence	539	0.51	0.50	0.53	0.50	0.48	0.50	0.671	0.710
Previous app. to ownership subsidy	539	0.15	0.36	0.14	0.35	0.16	0.37	0.899	0.909
Age 25-35	539	0.67	0.47	0.67	0.47	0.68	0.47	0.337	0.360
Below family adjusted PL	539	0.18	0.38	0.16	0.37	0.20	0.40	0.286	0.327
KM to closest PHA	539	15.74	20.35	14.90	19.02	16.96	22.10	0.415	0.461
Valid email address	539	0.90	0.30	0.90	0.30	0.90	0.30	0.619	0.657
Want to stay same neighborhood	349	0.54	0.50	0.53	0.50	0.54	0.50	0.791	0.818
Satisfaction with housing unit	373	0.65	0.48	0.65	0.48	0.65	0.48	0.601	0.655
Does not know other applicants	341	0.55	0.50	0.50	0.50	0.60	0.49	0.043**	0.110
Access to car	339	0.31	0.46	0.33	0.47	0.27	0.45	0.251	0.321
(Perceived) High social class neighbors	361	0.48	0.50	0.48	0.50	0.49	0.50	0.800	0.803
Baseline Survey response	490	0.86	0.35	0.84	0.37	0.88	0.33	0.517	0.674
Geocoded location	539	0.88	0.33	0.88	0.32	0.88	0.33	0.576	0.602
Female	539	0.97	0.17	0.97	0.18	0.98	0.15	0.577	0.334
Spouse/partner	539	0.04	0.20	0.05	0.21	0.04	0.19	0.933	0.496
Chilean	539	0.95	0.21	0.94	0.23	0.97	0.18	0.144	0.910
Santiago MSA	539	0.13	0.33	0.14	0.34	0.12	0.32	0.888	0.266
Rent	341	5.65	3.08	5.63	3.00	5.70	3.27	0.445	0.633
Rent burden	341	0.50	0.28	0.49	0.28	0.52	0.26	0.057*	0.362
SCREENING FE								Yes	Yes
Joint Significance F-Test (p)								0.749	0.811

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: Balance in Baseline Characteristics in Sample of Randomized Vouchers in Elderly Rounds

		Summary Statistics						Balance	
		Pooled		Control		Treated		Test	
	N	Mean	SD	Mean	SD	Mean	SD	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tenant in baseline	1,187	0.54	0.50	0.55	0.50	0.52	0.50	0.574	0.578
Family income (UF)	1,187	6.03	2.51	6.01	2.46	6.07	2.59	0.622	0.657
PHA in county of residence	1,187	0.47	0.50	0.47	0.50	0.46	0.50	0.264	0.314
County above national poverty	1,187	0.31	0.46	0.33	0.47	0.28	0.45	0.555	0.668
High density county	1,187	0.55	0.50	0.52	0.50	0.59	0.49	0.815	0.835
Female	1,187	0.62	0.49	0.61	0.49	0.64	0.48	0.439	0.450
Age 70-79	1,187	0.57	0.49	0.58	0.49	0.55	0.50	0.428	0.456
Below family adjusted PL	1,187	0.56	0.50	0.57	0.50	0.55	0.50	0.746	0.780
KM to closest PHA	1,187	12.02	16.67	12.66	17.23	11.05	15.75	0.241	0.357
Valid email address	1,187	0.36	0.48	0.38	0.49	0.32	0.47	0.038**	0.072*
Spouse/partner	1,187	0.39	0.49	0.39	0.49	0.39	0.49	0.971	0.984
Chilean	1,187	0.98	0.14	0.98	0.15	0.99	0.12	0.063*	0.373
Previous app. to ownership subsidy	1,187	0.05	0.21	0.05	0.22	0.04	0.19	0.516	0.574
Santiago MSA	1,187	0.27	0.44	0.22	0.41	0.35	0.48	0.905	0.565
Geocoded location	1,187	0.96	0.20	0.95	0.22	0.97	0.18	0.464	0.127
SCREENING FE								Yes	Yes
Joint Significance F-Test (p)								0.094	0.137

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.7: Total Score and Score Components by Group in Sample of Randomized Vouchers

		Pooled		Treated		Controls		Difference
	N	Mean	SD	Mean	SD	Mean	SD	(7)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regular Rounds								
Family size score	539	40.00	0.00	40.00	0.00	40.00	0.00	0.00
Single parenthood score	539	35.00	0.00	35.00	0.00	35.00	0.00	0.00
Number of children under 5 score	539	23.67	13.25	24.00	13.42	23.56	13.21	0.44
Number of children 6 to 18 score	539	5.23	8.68	8.00	10.95	4.29	8.80	3.71
Number of elderly score	539	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of people with disability score	539	1.85	4.13	6.00	13.42	0.44	0.98	5.56
Social vulnerability score	539	180.00	0.00	180.00	0.00	180.00	0.00	0.00
Housing vulnerability score	539	2.64	5.91	2.71	6.07	2.62	5.85	0.10
Application score	539	295.00	28.28	295.00	28.28	295.00	28.28	0.00
Elderly Rounds								
Family size score	1,187	40.00	0.00	40.00	0.00	40.00	0.00	0.00
Single parenthood score	1,187	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of children under 5 score	1,187	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of children 6 to 18 score	1,187	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of elderly score	1,187	58.52	2.26	59.80	0.28	58.04	3.19	1.76
Number of people with disability score	1,187	0.51	0.49	0.20	0.28	0.57	0.57	-0.37
Social vulnerability score	1,187	180.00	0.00	180.00	0.00	180.00	0.00	0.00
Housing vulnerability score	1,187	11.95	7.78	11.21	7.62	13.65	11.45	-2.44
Application score	1,187	385.00	10.00	385.00	10.00	385.00	10.00	0.00

This table replicates the analysis in Table A.3 using elderly rounds data. See Table A.3 for details

Table A.8: Total Score and Score Components by Group in the Evaluation Sample

		Pooled		Treated		Controls		Difference
	N	Mean	SD	Mean	SD	Mean	SD	(7)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regular Rounds								
Family size score	1,131	48.65	6.15	51.27	6.98	47.50	6.88	3.77
Single parenthood score	1,131	27.53	4.84	28.40	2.93	27.03	6.36	1.37
Number of children under 5 score	1,131	12.96	8.74	17.80	11.49	10.20	8.20	7.60
Number of children 6 to 18 score	1,131	11.81	5.22	13.03	7.46	12.05	5.84	0.98
Number of elderly score	1,131	0.85	0.49	0.20	0.33	1.18	0.72	-0.98
Number of people with disability score	1,131	1.63	3.34	6.15	13.33	0.38	0.66	5.77
Social vulnerability score	1,131	173.90	3.42	171.78	4.81	174.51	4.21	-2.73
Housing vulnerability score	1,131	6.60	6.61	5.19	5.25	7.46	7.37	-2.27
Application score	1,131	293.51	28.04	297.37	27.34	291.06	29.05	6.31
Elderly Rounds								
Family size score	1,328	41.19	0.53	42.76	1.74	40.87	1.05	1.89
Single parenthood score	1,328	0.06	0.09	0.19	0.30	0.00	0.00	0.19
Number of children under 5 score	1,328	0.01	0.02	0.00	0.00	0.02	0.04	-0.02
Number of children 6 to 18 score	1,328	0.39	0.23	1.19	1.45	0.08	0.16	1.11
Number of elderly score	1,328	57.35	2.10	57.01	3.39	56.85	2.86	0.16
Number of people with disability score	1,328	1.06	0.76	0.81	0.86	1.31	1.11	-0.49
Social vulnerability score	1,328	177.56	1.11	178.19	2.85	174.46	5.92	3.73
Housing vulnerability score	1,328	13.28	8.11	13.14	6.60	15.87	13.36	-2.73
Application score	1,328	384.65	10.03	385.21	9.87	384.11	9.55	1.09

This table replicates the analysis in Table A.3 using elderly rounds data. See Table A.3 for details

Table A.9: Effect of Regular Voucher Before the Covid-19 Pandemic (2019): Randomization

	N	Control Mean	Control SD	Specification 1 Treatment Effect	Specification 1 OLS p-value	Specification 1 Rand-t p-value	Specification 2 Treatment Effect	Specification 2 OLS p-value	Specification 2 Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Housing Conditions									
Household size Dec 2019	539	2.623	1.127	-0.016	0.884	0.894	-0.021	0.847	0.859
Number of bedrooms	536	1.571	0.764	0.262	0.001***	0.000***	0.266	0.001***	0.000***
Number of people per bedroom	536	1.852	0.740	-0.269	0.000***	0.000***	-0.279	0.000***	0.000***
Overcrowing indicator	536	0.092	0.290	-0.047	0.033**	0.026**	-0.053	0.020**	0.016**
B. Residential Mobility									
Stayed in same unit	472	0.522	0.500	-0.071	0.144	0.147	-0.060	0.214	0.225
Distance (km)	472	6.291	24.988	19.916	0.073*	0.024**	18.607	0.071*	0.021**
Distance (km) (Movers)	233	13.143	34.922	37.667	0.085*	0.042**	33.574	0.071*	0.024**
Stayed 1km or less from application location	233	0.301	0.460	-0.007	0.919	0.940	0.001	0.984	0.981
Moved to another county	233	0.180	0.386	0.042	0.424	0.406	0.055	0.290	0.299
C. Neighborhood Characteristics									
Distance to closest municipality	472	3.491	5.211	0.690	0.229	0.237	0.738	0.215	0.213
Distance to closest school (km)	472	0.953	2.099	0.619	0.097*	0.084*	0.668	0.096*	0.091*
Distance to closest Pre-Shcool (km)	472	1.033	2.580	0.767	0.048**	0.044**	0.784	0.051*	0.048**
Distance to closest Primary Care (km)	440	1.630	2.780	0.623	0.162	0.155	0.632	0.189	0.184
Number of Schools in 1Km	472	5.097	4.496	-0.481	0.293	0.272	-0.449	0.318	0.293
Number of Schools in 2Km	472	15.748	13.032	-1.864	0.117	0.107	-2.030	0.053*	0.048**
Number of Preschool in 1Km	472	3.201	2.615	-0.120	0.650	0.630	-0.103	0.687	0.674
Number of Health Care in 2km	472	5.047	4.359	-0.550	0.146	0.129	-0.632	0.064*	0.065*
Fraction of Public Schools 1Km	386	0.457	0.286	0.021	0.515	0.514	0.032	0.329	0.332
Fraction of Subsidized Schools 1Km	386	0.517	0.277	-0.043	0.177	0.183	-0.053	0.103	0.105
Fraction of Private Schools 1Km	386	0.025	0.087	0.022	0.041**	0.039**	0.021	0.044**	0.039**
Mat. SIMCE, 3 Closest School 2km	412	261.867	17.804	1.630	0.356	0.340	1.119	0.525	0.512
Mat. SIMCE, 3 Closest School 2km	412	248.423	19.461	0.531	0.786	0.755	-0.014	0.994	0.991
Fraction of Low Income Schools 1km	386	0.603	0.335	0.036	0.331	0.327	0.031	0.392	0.409
Fraction of Low Income Schools 2km	415	0.573	0.274	0.031	0.264	0.276	0.025	0.335	0.354
County poverty rate	472	0.105	0.060	0.001	0.786	0.798	-0.001	0.820	0.831
Total crime (County z-score)	472	1.058	1.573	0.052	0.724	0.740	0.040	0.699	0.718
D. Homeownership									
Application to Ownership Programs	539	0.336	0.473	0.061	0.156	0.140	0.028	0.409	0.391
Application to partially funded program (DS1)	539	0.236	0.425	0.044	0.265	0.240	0.018	0.564	0.521
Application to fully funded program (DS49)	539	0.157	0.365	0.013	0.690	0.689	-0.002	0.944	0.941
Active ownership savings account	539	0.918	0.274	0.018	0.486	0.539	0.015	0.568	0.587
Balance in ownership savings account (US)	498	24.278	32.152	4.000	0.167	0.170	2.526	0.316	0.331
SCREENING FE				YES	YES	YES	YES	YES	YES
CONTROLS (Z)				NO	NO	NO	YES	YES	YES
Westfall-Young Multiple Testing (p-value)						0.003***			0.003***

This 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.10: Effect of Elderly Voucher Before the Covid-19 Pandemic (2019): Randomization

		Control		Specification 1			Specification 2		
	N	Mean	SD	Treatment Effect	OLS p-value	Rand-t p-value	Treatment Effect	OLS p-value	Rand-t p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Housing Conditions									
Household size Dec 2019	1,187	1.565	1.059	-0.234	0.000***	0.000***	-0.250	0.000***	0.000***
Number of bedrooms	1,122	1.351	0.736	0.436	0.000***	0.000***	0.445	0.000***	0.000***
Number of people per bedroom	1,118	1.223	0.564	-0.328	0.000***	0.000***	-0.341	0.000***	0.000***
Overcrowing indicator	1,176	0.029	0.169	-0.019	0.041**	0.041**	-0.020	0.030**	0.025**
B. Residential Mobility									
Stayed in same unit	1,073	0.681	0.466	-0.209	0.000***	0.001***	-0.208	0.000***	0.001***
Distance (km)	1,073	16.445	122.110	-5.232	0.378	0.406	-6.618	0.303	0.323
Distance (km) (Movers)	414	51.556	212.333	-32.292	0.065*	0.050**	-34.237	0.071*	0.059*
Stayed 1km or less from application location	414	0.290	0.455	0.014	0.765	0.752	0.012	0.802	0.799
Moved to another county	416	0.268	0.444	-0.041	0.353	0.334	-0.042	0.311	0.303
C. Neighborhood Characteristics									
Distance to closest municipality	1,073	3.905	7.503	-0.832	0.040**	0.036**	-0.701	0.069*	0.061*
Distance to closest school (km)	1,073	1.163	4.251	-0.183	0.374	0.405	-0.138	0.524	0.555
Distance to closest Pre-Shcool (km)	1,073	1.186	4.522	-0.237	0.309	0.327	-0.176	0.468	0.473
Distance to closest Primary Care (km)	1,022	1.663	4.327	-0.174	0.452	0.475	-0.116	0.624	0.636
Number of Schools in 1Km	1,073	7.114	5.818	-0.504	0.159	0.150	-0.727	0.035**	0.026**
Number of Schools in 2Km	1,073	21.450	15.462	-0.468	0.643	0.629	-1.124	0.198	0.196
Number of Preschool in 1Km	1,073	3.755	2.975	-0.227	0.255	0.224	-0.301	0.121	0.109
Number of Health Care in 2km	1,073	6.547	5.699	-0.200	0.584	0.591	-0.413	0.207	0.187
Fraction of Public Schools 1Km	938	0.397	0.237	0.004	0.801	0.825	0.009	0.615	0.625
Fraction of Subsidized Schools 1Km	938	0.534	0.238	-0.004	0.806	0.803	-0.007	0.692	0.674
Fraction of Private Schools 1Km	938	0.069	0.139	-0.000	0.993	0.990	-0.002	0.845	0.856
Mat. SIMCE, 3 Closest School 2km	974	264.493	17.587	-1.203	0.324	0.332	-1.203	0.336	0.347
Mat. SIMCE, 3 Closest School 2km	975	251.926	18.012	-0.451	0.718	0.715	-0.476	0.707	0.700
Fraction of Low Income Schools 1km	938	0.455	0.335	-0.013	0.585	0.573	-0.001	0.953	0.952
Fraction of Low Income Schools 2km	978	0.437	0.267	-0.019	0.313	0.316	-0.008	0.648	0.646
County poverty rate	1,075	0.083	0.047	-0.003	0.408	0.418	0.001	0.542	0.516
Total crime (County z-score)	1,075	1.741	1.970	0.045	0.737	0.734	-0.074	0.496	0.491
D. Homeownership									
Application to Ownership Programs	1,187	0.118	0.323	0.021	0.315	0.303	0.031	0.085*	0.088*
Application to partially funded program (DS1)	1,187	0.067	0.250	0.006	0.694	0.672	0.012	0.404	0.420
Application to fully funded program (DS49)	1,187	0.068	0.252	0.015	0.374	0.388	0.020	0.217	0.241
SCREENING FE				YES	YES	YES	YES	YES	YES
CONTROLS (Z)				NO	NO	NO	YES	YES	YES
Westfall-Young Multiple Testing (p-value)						0.008***			0.008***

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 Follow up Survey Data

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.[81]

[81] 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,

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. The response rate in this group was 57 percent, 60 percent (18,185) in regular rounds and 44 percent (3,023) in elderly rounds.[82] In the evaluation sample, the response rate was 59 percent (779) and 28 percent (171) in regular and elderly rounds, respectively.[83] These rates of response are high for online surveys.

Figures B.1 and **??** 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 5.1 used to analyze balance in Section 5.1. 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 **??** 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 **??** 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.[84]

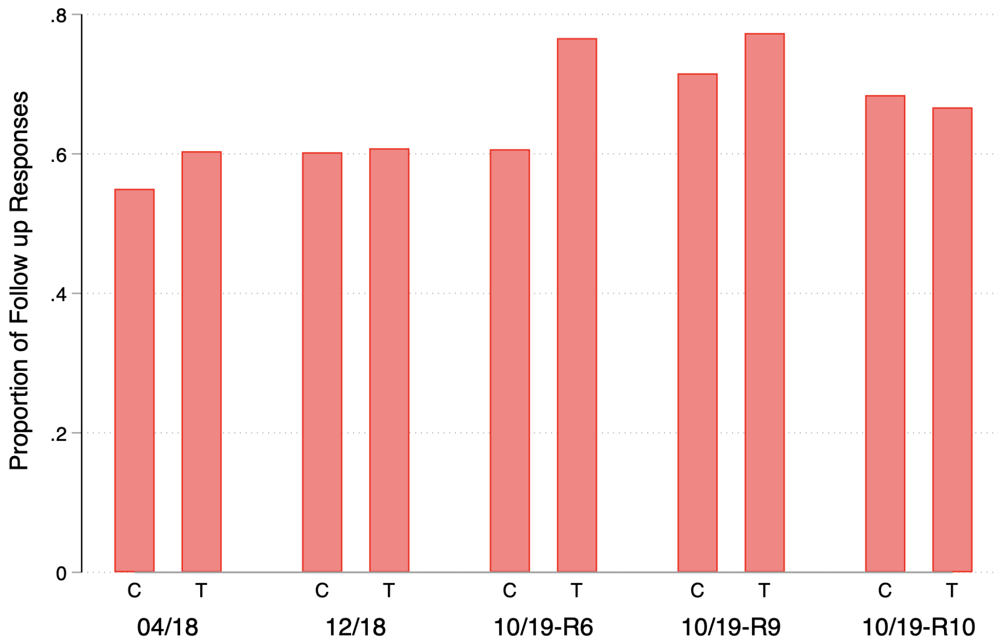
MINVU announced 150k emergency rental subsidies, available also to already voucher recipients of elderly and regular rounds. In this context, we created a blog with short and simple answers to frequently asked questions and provided survey respondents with the opportunity of sending their own questions at the end of the survey, which we responded through the blog. We received more than 10k questions during the data collection period.

[82] 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.

[83] 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.

[84] In the continuity approach, outcomes are analyzed using different bandwidth, therefore, it would be harder

Figure B.1: Follow up Sample Attrition



This Figure shows response rate of the follow up survey per screening of applicants. C is the control group (below the cutoff) and T is the treatment group (above the cutoff).

Table B.2 replicates the balance analysis presented in Section 5.1 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.2 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.
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.

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to study non-linearities caused by attrition in the follow-up survey.

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

Screenings of Applicants	Survey Response (1)	Survey Response (2)
Treat*Assignment April 2018	0.054	0.054
	(0.399)	(0.385)
Treat*Assignment December 2018	-0.048	-0.050
	(0.606)	(0.582)
Treat*Assignment October 2019 (R6)	0.105	0.093
	(0.307)	(0.370)
Treat*Assignment October 2019 (R9)	0.004	0.015
	(0.964)	(0.860)
Treat*Assignment October 2019 (R10)	-0.071	-0.103
	(0.651)	(0.520)
F-Test (p-value)	0.352	0.366
Rand-t Joint Test (p-value)	0.267	0.261
Questionnaires sent	993	993
Responses	634	634
Response rate	0.64	0.64
SCREENING FE	YES	YES
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: Balance in Baseline Characteristics in Regular Rounds-Follow Up Survey

		Summary Statistics						Balance Test	
		Pooled		Control		Treated			
	N	Mean	SD	Mean	SD	Mean	SD	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tenant in baseline	1,328	0.76	0.43	0.76	0.43	0.74	0.44	0.681	0.644
Saving balance on application day (UF)	634	15.77	15.27	15.46	14.05	16.32	17.22	0.716	0.740
Family income (UF)	634	13.55	4.83	13.41	4.76	13.79	4.94	0.213	0.220
Online application	634	0.40	0.49	0.40	0.49	0.40	0.49	0.526	0.474
High density county	634	0.40	0.49	0.43	0.50	0.35	0.48	0.041**	0.035**
County above national poverty	634	0.69	0.46	0.68	0.47	0.71	0.45	0.469	0.455
PHA in county of residence	634	0.50	0.50	0.50	0.50	0.49	0.50	0.709	0.647
Previous app. to ownership subsidy	634	0.16	0.36	0.15	0.36	0.17	0.38	0.520	0.489
Age 25-35	634	0.65	0.48	0.62	0.49	0.69	0.46	0.069*	0.061*
Below family adjusted PL	634	0.20	0.40	0.20	0.40	0.19	0.39	0.841	0.856
KM to closest PHA	634	18.02	23.31	17.58	23.16	18.80	23.59	0.883	0.879
Baseline Survey response	634	0.88	0.33	0.88	0.32	0.88	0.33	0.958	0.940
Want to stay same neighborhood	634	0.59	0.49	0.59	0.49	0.59	0.49	0.652	0.661
Satisfaction with housing unit	478	0.68	0.47	0.67	0.47	0.69	0.46	0.575	0.583
Does not know other applicants	506	0.55	0.50	0.53	0.50	0.58	0.50	0.262	0.285
Access to car	467	0.34	0.47	0.35	0.48	0.33	0.47	0.706	0.700
(Perceived) High social class neighbors	465	0.50	0.50	0.50	0.50	0.50	0.50	0.787	0.792
Geocoded location	495	0.89	0.32	0.89	0.31	0.88	0.33	0.494	0.505
Female	634	0.91	0.28	0.91	0.29	0.91	0.28	0.957	0.973
Spouse/partner	634	0.16	0.36	0.17	0.37	0.14	0.35	0.303	0.332
Chilean	634	0.93	0.25	0.91	0.29	0.97	0.17	0.003***	0.003***
Santiago MSA	634	0.09	0.29	0.10	0.30	0.09	0.28	0.943	0.883
Rent	634	5.91	3.53	5.97	3.69	5.78	3.18	0.735	0.744
Rent burden	442	0.49	0.31	0.49	0.34	0.47	0.26	0.544	0.538
SCREENING FE								Yes	Yes
Joint Significance F-Test (p)								0.159	0.242

This table replicates the analysis in Table 7 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 7 for further details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table B.3: Effect of Regular Voucher Before the Covid-19 Pandemic (2019): Follow Up Sample

				Specification 1			Specification 2		
		Control		Treatment	OLS	Rand-t	Treatment	OLS	Rand-t
	N	Mean	SD	Effect	p-value	p-value	Effect	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Housing Conditions									
Household size Dec 2019	634	3.005	1.256	-0.009	0.936	0.933	0.024	0.821	0.829
Number of bedrooms	632	1.905	0.868	0.144	0.040**	0.049**	0.164	0.019**	0.020**
Number of people per bedroom	632	1.771	0.817	-0.164	0.009***	0.016**	-0.167	0.006***	0.013**
Overcrowing indicator	632	0.119	0.325	-0.043	0.077*	0.087*	-0.039	0.110	0.143
B. Residential Mobility									
Stayed in same unit	560	0.581	0.494	-0.117	0.009***	0.008***	-0.098	0.026**	0.024**
Distance (km)	560	5.060	23.019	13.129	0.053*	0.050*	12.650	0.057*	0.047**
Distance (km) (Movers)	258	12.069	34.416	21.557	0.087*	0.098*	17.526	0.139	0.147
Stayed 1km or less from application location	258	0.333	0.473	-0.060	0.321	0.324	-0.051	0.419	0.407
Moved to another county	258	0.133	0.341	0.069	0.116	0.123	0.051	0.234	0.233
C. Neighborhood Characteristics									
Distance to closest municipality	560	3.201	4.264	0.080	0.850	0.850	0.107	0.805	0.808
Distance to closest school (km)	560	0.905	1.505	0.491	0.111	0.105	0.445	0.157	0.131
Distance to closest Pre-Shcool (km)	560	0.974	2.028	0.398	0.198	0.194	0.358	0.255	0.255
Distance to closest Primary Care (km)	520	1.540	2.155	0.335	0.356	0.372	0.293	0.435	0.448
Number of Schools in 1Km	560	4.696	4.330	-0.418	0.298	0.284	-0.221	0.581	0.571
Number of Schools in 2Km	560	14.559	12.969	-1.356	0.209	0.201	-0.480	0.607	0.591
Number of Preschool in 1Km	560	2.939	2.596	-0.187	0.416	0.433	-0.067	0.761	0.767
Number of Health Care in 2km	560	4.763	4.466	-0.187	0.609	0.604	0.076	0.813	0.813
Fraction of Public Schools 1Km	460	0.445	0.298	-0.012	0.687	0.706	-0.014	0.647	0.656
Fraction of Subsidized Schools 1Km	460	0.512	0.287	0.007	0.804	0.795	0.010	0.731	0.730
Fraction of Private Schools 1Km	460	0.044	0.117	0.005	0.671	0.668	0.004	0.753	0.733
Mat. SIMCE, 3 Closest School 2km	487	265.540	17.532	-1.719	0.317	0.322	-1.986	0.228	0.231
Mat. SIMCE, 3 Closest School 2km	487	250.861	18.475	-1.343	0.463	0.448	-1.175	0.507	0.501
Fraction of Low Income Schools 1km	460	0.605	0.348	0.013	0.705	0.724	0.013	0.698	0.724
Fraction of Low Income Schools 2km	491	0.573	0.268	0.029	0.257	0.292	0.023	0.328	0.350
County poverty rate	560	0.115	0.061	-0.001	0.759	0.743	-0.003	0.437	0.431
Total crime (County z-score)	560	1.151	1.601	0.052	0.733	0.728	0.188	0.062*	0.064*
D. Homeownership									
Application to Ownership Programs	634	0.322	0.468	0.002	0.968	0.927	-0.005	0.870	0.873
Application to partially funded program (DS1)	634	0.240	0.428	-0.014	0.679	0.611	-0.019	0.489	0.496
Application to fully funded program (DS49)	634	0.124	0.330	0.013	0.635	0.633	0.005	0.839	0.830
Active ownership savings account	634	0.911	0.285	0.001	0.981	0.892	-0.003	0.887	0.869
Balance in ownership savings account (US)	578	24.478	34.813	-1.314	0.614	0.586	-1.687	0.438	0.435
SCREENING FE				YES	YES	YES	YES	YES	YES
CONTROLS (Z)				NO	NO	NO	YES	YES	YES
Westfall-Young Multiple Testing (p-value)						0.186			0.290

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