FACULTAD ECONOMÍA Y NEGOCIOS UNIVERSIDAD DE CHILE DEPARTAMENTO DE ECONOMÍA

SERIE DE DOCUMENTOS DE TRABAJO

SDT 563

RENTAL ASSISTANCE IN MIDDLE-INCOME COUNTRIES: QUASI-EXPERIMENTAL EVIDENCE FROM CHILE

Autores:
Javiera Selman

Santiago, Mayo de 2025

sdt@econ.uchile.cl
econ.uchile.cl/publicaciones

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

Javiera Selman *

April 18, 2025

Click *here* for most recent version

Abstract

This paper presents the first evaluation of a rental voucher program in a middle-income country: Chile. I estimate treatment effects before and after the COVID-19 outbreak using a local randomization regression discontinuity design and administrative and survey data. Pre-pandemic results mirror U.S. evidence: voucher receipt improved housing conditions and increased mobility but did not lead to relocation to higher-quality neighborhoods. Post-pandemic results (November 2020) show that vouchers helped low-income families cope with the aggregate shock by reducing reliance on debt and enhancing housing stability. Findings highlight a previously underappreciated insurance role of rental subsidies during periods of economic distress. ***JEL Codes:*** I38, O18, R23

*Departamento de Economía, Facultad de Economía y Negocios, Universidad de Chile. This research was made possible by a data-use agreement with the Chilean Ministry of Housing and Urban Planning (MINVU). Special thanks to Rajeev Dehejia, Ingrid Gould Ellen, Tatiana Homonoff, Daniel Waldinger, Kathy O'Regan and Patrick Button for valuable comments; to conference participants at the 2024 AREUEA National, 2023 LACEA LAMES Meeting, 2022 First LACEA Urban Annual Meeting, 2022 Summer Institute of the National Bureau of Economic Research, 2021 APPAM Fall Research Conference, and the UEA 11th EU Meeting. This research was supported by NYU-Wagner. All mistakes are my own. Email: jselman@fen.uchile.cl

1 Introduction

Rising rents and stagnant wages have left low-income households increasingly vulnerable to homelessness, overcrowding, and deprived neighborhoods—conditions linked to adverse long-term outcomes for children (Chetty, Hendren and Katz, 2016; Chyn, 2018; Chyn and Katz, 2021; Collinson et al., 2024). The COVID-19 pandemic further strained household finances and intensified the risk of eviction (Ellen, O'Regan and Ganz, 2020). In response, several countries introduced or expanded direct rental assistance–most notably, voucher programs–to mitigate housing insecurity, while improving neighborhood access for disadvantaged families (OECD, 2024). However, little is known about their effectiveness outside the U.S. context and their role in helping families cope with unexpected economic shocks.

This research presents the first rigorous evaluation of a rental voucher program in a middle-income country. Using a unique dataset combining administrative and survey data collected before and during the COVID-19 pandemic, I address two central questions: (i) What is the effect of Chile's rental voucher program on overcrowding, residential mobility, neighborhood characteristics, and demand for subsidized homeownership? and (ii) How does access to rental assistance influence the ways in which families cope with major economic shocks, such as those experienced during the COVID-19 pandemic?

Rental voucher programs have expanded across Latin America in recent years. The *Subsidio de Arriendo* (Rental Subsidy), implemented by the Chilean Ministry of Housing and Urban Planning (MINVU) in December 2013, was the first such policy in the region. It marked a major shift away from long-standing reliance on large-scale, demand-side subsidies for homeownership, and constituted the first national effort to support low-income families in the rental market.

Voucher impacts on housing consumption depend on initial housing quality, local rental market characteristics, and how program rent caps align with market rents; families initially paying above the cap may reduce housing consumption (Olsen, 2003). Empirical evidence on rental voucher programs, primarily focused on the U.S. Housing Choice Voucher Program (HCVP), finds that while vouchers effectively reduce rent burden, crowding, and homelessness, they have had limited success in improving neighborhood quality (Ellen, 2020; Wood, Turnham and Mills, 2008).

Modeled after the HCVP, the Chilean program subsidizes private-market rents for units that meet minimum quality standards and fall below a maximum allowable rent threshold. Unlike the U.S. program–which covers the gap between the maximum allowable rent and 30% of household income and maintains assistance conditional on income eligibility (Collinson, Ellen and Ludwig, 2015)–Chile provides a fixed subsidy through two shorter-term voucher schemes: the regular voucher for young families and the elderly voucher, a more generous subsidy for a shorter period for individuals aged 60 and older. In both schemes, MINVU offers a form of partial rent insurance: recipients may defer up to three consecutive monthly payments without losing their benefit, while landlords continue to receive the subsidy. Compared to the U.S. program, these design features may help broaden coverage among eligible households (Zhang, 2022), mitigate labor supply disincentives (Jacob and Ludwig, 2012), and reduce housing instability in response to aggregate shocks (Abramson and Van Nieuwerburgh, 2024). In this paper, I evaluate the two voucher schemes separately.

Institutional disparities between high- and middle-income countries may further affect voucher effectiveness (Colburn, 2021; Ross and Pelletiere, 2014). In Chile, the low-income rental market is relatively small and highly informal. In 2013, only 12% of households in the bottom income quintile rented, and nearly half lacked formal lease agreements. By 2017, this share had risen modestly to 16%, but informality remained around 50% (National Socioeconomic Survey , CASEN). These conditions likely exacerbate the barriers voucher holders face during the housing search, contributing to Chile's lower voucher utilization rate (45%) compared to the U.S. (60%) (Ellen, O'Regan and Strochak, 2024). In the U.S., evidence suggests that beyond affordable housing shortages, barriers include landlord refusal (Aliprantis, Martin and Tauber, 2020; Chan and Fan, 2023; Phillips, 2017) and short search windows (Ellen, O'Regan and Strochak, 2024). Also, some landlords may inflate rents to match the program's rent cap (Collinson and Ganong, 2018).

I exploit Chile's rental voucher assignment mechanism using a Regression Discontinuity Design (RDD) to estimate causal effects of the program. During the study period, MINVU assigned available vouchers multiple times based on a discrete application score, granting them to families above specific cutoffs. In cases of score ties at the cutoff, MINVU implemented a three-step tie-breaking protocol that included randomization. Given that the discrete application score is supported on a small number of mass points, I use the Local Randomization approach to RDD developed by Cattaneo, Idrobo and Titiunik (2019), as traditional local polynomial methods are known to yield inconsistent treatment effect estimates, invalid inference, and problematic bandwidth selection under these conditions (Branson and Mealli, 2018; Cattaneo, Idrobo and Titiunik, 2019; Díaz and Zubizarreta, 2023; Kolesár and Rothe, 2018).

The evaluation sample comprises 926 and 1,717 applicants immediately above and below the eligibility cutoff in regular and elderly rounds, respectively, between 2017 and 2019. Randomized voucher assignments account for 58% of the sample in regular rounds and 97% in elderly rounds. Pre-pandemic outcome data (December 2019) is obtained from different administrative and public data sources. Follow-up outcomes were collected via a survey implemented with MINVU in November 2020–eight months after the COVID-19 outbreak.

Pre-pandemic intent-to-treat (ITT) estimates show that voucher receipt significantly reduced overcrowding by 4.4 percentage points (pp) among regular voucher recipients–a 37% reduction relative to the control group–and by 1.4 pp (47%) among elderly recipients. The voucher also increased residential mobility, affecting both the extensive margin (whether households moved) and the intensive margin (how far they moved). Regular and elderly voucher holders were 7.1 pp (17%) and 25.7 pp (76%) more likely to relocate, respectively, and both groups were 5.5 pp more likely to move across county boundaries relative to their control groups. This increased mobility did not lead to overall improvements in neighborhood quality, although the voucher did affect specific neighborhood attributes: elderly recipients appeared to relocate closer to denser areas with more services, while young families in regular rounds tended to move farther from schools. Notably, rather than substituting away from homeownership, elderly voucher recipients increased applications for homeownership subsidies, while no effect was observed among regular recipients.

Post-pandemic outcomes–available only for regular voucher recipients–show that the subsidy significantly helped young low-income families cope with the COVID-19 shock. Specifically, voucher holders were 15.3 pp (23%) less likely to increase debt and, despite having the option to defer up to three consecutive rent payments, were 10.9 pp (50%) less likely to miss rent. They were also 13.1 pp (18%) more likely to hold a formal lease, potentially offering greater protection against eviction during the pandemic. These findings underscore a previously underappreciated social insurance role of rental vouchers during periods of economic distress.

This paper provides the first quasi-experimental evaluation of a rental voucher program in a middle-income country, marking a significant departure from existing studies, which have focused primarily on the United States (Chetty, Hendren and Katz, 2016; Collinson, Ellen and Ludwig, 2019; Ellen, 2020; Jacob and Ludwig, 2012; Kling, Liebman and Katz, 2007; Pollakowski et al., 2022; Wood, Turnham and Mills, 2008). While some research has examined rental assistance in other developed countries (Brewer et al., 2019; Eerola and Lyytikäinen, 2021; Gibbons, Sanchez-Vidal and Silva, 2020; Hyslop and Rea, 2019) and in lower-income settings–for example, Barnhardt, Field and Pande (2017) evaluated a project-based rental housing program in India and found no improvements in socioeconomic outcomes or tenure security, alongside evidence of social isolation and reduced informal insurance–programs differ substantially in design and administration. To my knowledge, this is also the first evaluation of rental vouchers for elderly households, a population that has received limited attention despite representing a growing share of housing subsidy beneficiaries (Collinson, Ellen and Ludwig, 2015; Reina and Aiken, 2022).

This research also contributes to the small but growing literature on the role of rental assistance in promoting housing stability–defined as access to safe, stable, and affordable living conditions (Abramson, 2023; Abramson and Van Nieuwerburgh, 2024; DeLuca and Rosen, 2022; Ellen, O'Regan and Ganz, 2020). Prior empirical work, largely from pre-pandemic settings and focused on highly vulnerable populations (homeless shelter residents, TANF recipients, and public housing tenants), yields mixed results (Gubits et al., 2016; Sanbonmatsu et al., 2011; Wood, Turnham and Mills, 2008). In contrast, this study examines the behavioral responses of families without prior housing assistance–the most typical voucher applicant–during a period of severe economic distress.

The remainder of the paper is organized as follows. Section 2 details the design of the program, while Section 3 describes the data. Section 4 outlines the research design, and Section 5 explains the implementation of the Local Randomization approach to Regression Discontinuity Designs. Section 6 presents the empirical findings, and Section 7 concludes.

2 Policy Design

The *Subsidio de Arriendo* (Rental Subsidy), launched in 2013 by the Chilean Ministry of Housing and Urban Planning (MINVU), provides a fixed, time-limited subsidy for use in the private market, aiming to reduce overcrowding and improve access to better locations. Chile administers two voucher schemes: regular rounds targeting young families and elderly rounds for adults aged 60 or older. Between 2017 and 2019–the analyzed period–MINVU received approximately 40,000 applications and awarded 23,000 vouchers, with 80% allocated through regular rounds.

Eligibility is based on a national vulnerability index–the National Household Social Registry or RSH[1]– with the program targeting households in the bottom 70% of the distribution who are either renting or doubled up. Regular rounds target 18 or older-headed families with monthly income between 7 UF[2] (US$270) and 25 UF (US$900), with at least 4 UF (US$155) in private savings to buy a house. Elderly rounds of vouchers target individuals aged 60 or older, with monthly income above 3.8 UF (US$145), and require no savings.

Families may apply online or in person at any of the 52 local housing authorities (Housing and Urban Planning Service, or SERVIU).[3] To allocate vouchers to the most vulnerable families, MINVU calculates an application score using a complex formula. As the assignment mechanism is central to identifying causal effects, a detailed description is provided in the next subsection.

Voucher holders in regular rounds receive a total subsidy of 170 UF (US$6,500), paid in fixed monthly installments to cover rents up to the maximum allowable amount, over approximately four and a half years. The subsidy cannot exceed 80% of the monthly rent. Elderly recipients receive higher monthly benefits for a shorter period: 213 UF (US$8,170), paid in fixed monthly installments, covering up to 95% of the rent below the maximum allowable amount, for two years.[4] Recipients have 24 months to begin using the voucher–four to twelve times longer than the lease-up period in the U.S. program (Collinson, Ellen and Ludwig, 2015)–and both schemes allow families to flexibly spread their monthly payments over an eight-year window.

During the period of analysis, the regular monthly subsidy increased from 3 UF (US$114) to 4.2 UF (US$161), and the maximum allowable rent amount–applied to both voucher schemes–rose from 8.6 UF (US$330) to 11 UF (US$422). Voucher amount and rent caps are set nationally, except in 30 designated high-cost counties (out of 346 total), located in the extreme north and south, where thresholds were slightly higher. In these areas, the monthly subsidy increased from 3.5 UF (US$134) to 4.9 UF (US$188), and the maximum allowable rent rose from 10 UF (US$384) to 13 UF

[1]Administered by the Ministry of Social Development, the index is derived from survey and administrative data on educational attainment, income, expenses, health, food security, and living conditions. Families are categorized into 7 groups based on their position in the score distribution: below the 40th, between the 41st and 50th, 51st-60th, 61st-70th, 71st-80th, 81st-90th, and 91st-100th percentiles. The RSH is used for the provision of most social assistance in Chile.

[2]The inflation-indexed unit of account used in Chile, adjusted daily based on changes in the Consumer Price Index.

[3]MINVU oversees administrative decisions, assigns vouchers, and pays rent to landlords. SERVIUs provide information, assist with application and lease validation, and coordinate housing inspections. Municipalities may also support application processes.

[4]In elderly rounds, total subsidy and voucher amounts vary slightly across RSH groups. The benefits described are those received by the most vulnerable group, which represents 99% of elderly applicants in the evaluation sample.

(US$499). While the number of high-cost counties has increased, the core structure of the program remains unchanged, making the findings in this paper directly relevant to its current design.

Minimum unit standards and documentation requirements are consistent across voucher types. Families already renting an eligible unit may stay in place, while those who are doubled up ought to move. Eligible units must have three separated spaces, a residential use certificate issued by the municipality, and a Chilean tax registration number; additionally, the landlord must not be a member of the beneficiary's extended family. Recipients generate a lease through an online platform, and if the landlord provides the necessary documentation, the process is typically quick. Unlike the U.S. program, there is no rent negotiation with government officials, and unit inspections are rare. The voucher is activated once the tenant makes their first co-payment, after which MINVU transfers the full rent to the landlord.

The change in housing consumption following voucher receipt depends on local market conditions and how the rent cap compares to actual rents; families initially paying more than the maximum allowable rent may reduce housing consumption.[5] Online Appendix Figure B.1 illustrates how the Chilean and U.S. rental voucher programs shift the budget set of low-income families. After the second year in the U.S., the rent ceiling becomes non-binding, and the voucher unambiguously increases housing consumption (Collinson, Ellen and Ludwig, 2015; Olsen, 2003). However, as in the first year of voucher receipt in the U.S., the predicted effects of the Chilean program on housing consumption are ambiguous due to a binding maximum allowable rent threshold.

The design of the Chilean program incorporates several features of interest to policymakers (Ross and Pelletiere, 2014). Unlike the U.S. program, it delivers a fixed subsidy over a shorter benefit window, allowing broader coverage and potentially mitigating labor-supply disincentives inherent in income-based benefits (Jacob and Ludwig, 2012; Zhang, 2022). The program also includes a partial rent-guarantee insurance mechanism: if a tenant fails to pay their required co-payment, MINVU steps in as guarantor, continuing voucher payments to the landlord for up to three consecutive months—and for any number of additional single-month periods. This feature may help families cope with unexpected temporary shocks–such as the COVID-19 pandemic–by enhancing housing stability during times of economic uncertainty (Abramson and Van Nieuwerburgh, 2024).

In response to the COVID-19 pandemic, MINVU introduced two temporary extensions to prevent

[5] A 2016 internal report by MINVU showed that the maximum allowable rent averaged 74% of the median rent in regional rental markets, ranging from 52% to 128%.

benefit expiration in 2020. Specifically, unused vouchers set to expire in 2020 were granted an additional 12 months, and subsidies that had been fully utilized by 2020 were extended by six months. These changes are unlikely to affect the main analysis, as they applied to earlier rounds than those included in this study.

2.1 Voucher Assignment Mechanism

Rounds remain open for two to nine months. During each round, MINVU assigns 1,000 to 3,000 rental vouchers every one or two months by screening applicants using an application score based on multiple data sources (Table I). In each applicant screening, vouchers are awarded to those with the highest scores.

MINVU uses a rolling application system. Applicants who are not selected are re-screened along with new applicants in the following assignment period. This process continues until a family receives a voucher or the round closes. To be reconsidered in a future round, applicants must reapply, although few do so.

The number of available vouchers and assignment periods are set by decree before each round opens, although both can change due to administrative or political decisions outside the control of the rental policy team at MINVU. Notably, these adjustments are not publicly announced.

In 2017, a reform to the National Vulnerability index (RSH) altered the social vulnerability score component in the application score computed by MINVU, replacing a continuous index with a categorical one. As a result, total application scores became discrete, including multiples of five. This introduced frequent ties at the score cutoff, leading MINVU to implement a three-step tie-breaking protocol: (1) re-rank tied applicants using their family size score; (2) if needed, re-rank again using the vulnerability score; and (3) if ties persist, randomly assign remaining vouchers among applicants with identical family size and social vulnerability scores. I do not rely exclusively on the randomized sample—given its small size in regular rounds—but I report results for this subsample as a robustness check in Section 6.3.

3 Data

This paper assembles a unique data set by linking administrative, survey and public data from three different time periods: baseline (at application), December 2019 (pre-pandemic), and September-

November 2020 (six to eight months after the COVID-19 outbreak).

Baseline data. Administrative and self-reported information collected by MINVU to assess applicants' eligibility and calculate their application score. These data include socioeconomic, demographic, and housing characteristics–along with geographic location. To replicate voucher assignments, I linked this data to public records to verify individual scores, assignment dates and cutoffs.[6] I complement the baseline data with an online survey, administered in partnership with MINVU, to all applicants in regular rounds between March 2017 and October 2019. The survey, conducted prior to the announcement of voucher assignment results, elicited information on residential mobility, housing and neighborhood experiences, as well as preferences and beliefs. The average response rate was 78%.

Also, I assembled a unique geocoded dataset combining location from multiple data sources provided by MINVU with baseline survey information. This data was linked to public geocoded records on municipalities, local housing authorities (SERVIUs), and county-level information–including poverty rates from CASEN 2017, and density from the 2012 Census data. Finally, MINVU supplied administrative data on household applications to the two largest homeownership programs, *Fondo Solidario de Vivienda (DS49)* and *Subsidio Clase Media (DS1)*.[7]

Pre-pandemic outcomes data. Administrative records for December 2019 includes unit characteristics, household composition, and location data from the National Household Social Registry, as well as application to the two homeownership programs and the status of private savings accounts (used for homeownership applications). I also build neighborhood variables by linking applicant location in 2019 to detailed geocoded data on schools, healthcare centers, and county-level assault, robbery, theft, and poverty rates.

Pandemic outcomes data. A follow up survey was administered between September and November 2020 in partnership with MINVU. This survey collected information on housing and neighborhood characteristics and satisfaction levels, income, employment, and behavioral responses during the first eight months following the COVID-19 outbreak. It also included retrospective questions regarding residential mobility, which I used to complement and asses the quality of the existing administrative data from the pre-pandemic period.

[6] Any inconsistencies were resolved through consultations with voucher program policymakers.

[7] DS49 provides fully funded housing for the most vulnerable families, while DS1 offers partial funding to less vulnerable families, with a down payment that decreases with house price and household income.

4 Identification Strategy

I use the exogenous variation from surpassing the score cutoff $X_i > c$ and the actual randomization at the cutoff to estimate causal treatment effects of the rental voucher program using a sharp multi-cutoff Regression Discontinuity Design (RDD). Figure I shows the sharp discontinuity in treatment status at the cutoff. Following standard practice in multi-cutoffs designs, I pool all applicant screenings $s_t \in S$ and normalize the score in each screening to have a cutoff $c_{s_t} = 0$.

In the Chilean rental voucher program, the running variable is discrete, with only 131 mass points in regular and 109 in elderly rounds (Figure II). When the running variable has few mass points, continuity-based methods yield inconsistent estimates, invalid inference and bandwidth selection (Branson and Mealli, 2018; Cattaneo, Idrobo and Titiunik, 2019; Díaz and Zubizarreta, 2023; Kolesár and Rothe, 2018).[8] I therefore adopt the Local Randomization Approach to Regression Discontinuity Design (LRRD), developed by Cattaneo, Idrobo and Titiunik (2019).[9]

The LRRD assumes that within a narrow window $W = [c - e, c + e]$, treatment is as good as randomized. Within W, the distribution of the forcing variable is assumed to be known and the same across units, satisfying score ignorability: $Y_{i \in W}(X_{i \in W}, D_{i \in W}) = Y_{i \in W}(D_{i \in W})$. The design also requires that potential outcomes depend on the score only through treatment and that there is no interference across units (SUTVA). While interference could occur through neighborhood spillovers or market interactions, this seems unlikely given the small program size, low lease-up rates, the assignment mechanism, and identification from a narrow window W.

Under LRRD, causal treatment effects (τ_{LRRD}) can be identified without parametric modeling assumptions–as in an experimental setting–as:

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

[8]Let $Y_i(1)$ and $Y_i(0)$ be the potential outcomes under treatment and control, and let $D_i = D_i(X_i) = I(X_i \geq c^*)$ $\in \{0, 1\}$ be the treatment indicator. The observed outcome for individual is $Y_i = D_i Y_i(1) + (1 - D_i) Y_i(0)$. The continuity assumption implies that regression functions $\mathbb{E}\left\{Y_i(1)|X_i = 0\right\}$ and $\mathbb{E}\left\{Y_i(0)|X_i = 0\right\}$ at the cutoff ($X_i = 0$) can be used to approximate the average outcome that units just above the threshold would have experienced in the absence of treatment. The average treatment effect at the cutoff is $\tau_{Cont} = \mathbb{E}\left\{Y_i(1) - Y_i(0)|X_i = 0\right\} = \lim_{x \downarrow c} \mathbb{E}\left\{Y_i(1)|X_i = 0\right\} - \lim_{x \uparrow c} \mathbb{E}\left\{Y_i(0)|X_i = 0\right\}$ (Lee and Lemieux, 2010). However, when the running variable is discrete, the specification bias in the average treatment effect ($\mathbb{E}\left\{Y_i(0)|X_i = c\right\} - \mathbb{E}\left\{Y_i(0)|X_i = c_k\right\}$) is no longer negligible at the cutoff.

[9]See Branson and Mealli (2018) for a review of alternative estimation methods in Regression Discontinuity Designs.

4.1 Window (Bandwidth) Selection

Similar to the standard continuity approach, the key step in LRRD is selecting a valid window W where treatment and control groups are balanced on pre-treatment covariates and where treatment is plausibly randomized i.e. there is no manipulation of the score.

Manipulation in the LRRD is tested using a binomial test of the treatment assignment probability in a narrow window around the cutoff (Cattaneo, Idrobo and Titiunik, 2019). If applicants cannot manipulate their score precisely, treatment probability q should be consistent with the assumed assignment mechanism (complete randomization) within this window. Table II shows that the observed treatment probability does not differ significantly from $q = 0.5$, consistent with non-manipulation. Manipulating the score would require anticipating voucher availability, one's own score, and the entire score distribution—highly unlikely in this setting.[10]

For window selection, I implemented the data-driven procedure developed by Cattaneo, Idrobo and Titiunik (2019), which searches for the largest window around each cutoff where LRRD conditions hold. The procedure tests for balance in pre-treatment covariates across treated and control applicants in progressively larger windows to identify the largest window where the minimum p-value from balance tests remains above a pre-determined significance threshold (α).

I apply the window selection procedure separately to each applicant screening s_t, yielding screening-specific windows W_{s_t}. These are then pooled to create the evaluation sample W_0. I use pre-treatment covariates that did not enter the application score formula directly, which were created using administrative data from other government agencies or divisions inside MINVU, or were obtained from survey and geocoded data, not observed by policy makers at MINVU.

If ties occur at the cutoff, I adjust the running variable based on the program's three-step tie-breaking protocol (see Section 2.1). Specifically, I assign $X_i = -1$ to applicants randomized out of treatment and $X_i = 1$ to those randomized into treatment; if tie-breaking occurred via non-random score components, I assign $X_i = \pm 2$. This transformation ensures treated and control units lie on opposite sides of the cutoff. Any transformation that preserves this order yields equivalent results under LRRD (Cattaneo, Idrobo and Titiunik, 2019).

[10]Online Appendix Figure B.2 presents normalized score distribution by screening in the evaluation sample.

4.2 Estimation

In the evaluation sample W_0, causal treatment effects are estimated using the following equation for outcome Y of applicant i in screening s_t:

$$Y_{i,s_t} = \alpha + \tau_{LRRD} D_{i,s_t} + Z_{i,s_t}\beta + \gamma_{s_t} + \epsilon_{i,s_t} \tag{2}$$

Where D_{i,s_t} is a treatment indicator ($X_{i,s_t} > 0$), γ_{s_t} are dummies for each applicant screenings, and Z_{i,s_t} is a subset of baseline covariates used for window selection. The parameter of interest, τ_{LRRD}, is the Intent-to-Treat (ITT) effect. It is the weighted average of screening s_t-specific treatment effects of voucher assignment. Because each screening s_t occurs at a different point in time, the available data cannot disentangle variation across different cutoffs from variation in the number of months households have access to the voucher.

Although I focus on ITT estimates, I present Local Average Treatment Effects (LATE) using the discontinuity at the score cutoff as an instrument for voucher utilization in Section 6. Since compliance is one-sided–individuals in the control group cannot receive the voucher–LATE corresponds to ITT estimates scaled by the estimated compliance rate (Angrist and Pischke, 2008).

The next section explains the implementation of the LRRD approach to select windows W_{s_t}, evaluates covariate balance, and describes the characteristics of the evaluation sample W_0.

5 Sample Construction

5.1 Implementation of Data-Driven Window Selection Procedure

I replicate the voucher assignment process for each applicant screening s_t conducted between March 2017 and September 2019. Because the program uses a rolling application system, some individuals are screened multiple times within the same round (Section 2). The initial dataset includes 95,553 observations corresponding to 56,704 unique applicants across 82 screenings in 21 assignment periods, spanning eight rounds. Appendix Table A.1 summarizes the number of participants, score ranges, available vouchers, and cutoffs per assignment period by round type.

Following Cattaneo, Idrobo and Titiunik (2019), I keep only screenings with at least 10 observations on each side of the cutoff c_{s_t} in the smallest possible window to ensure sufficient statistical power for balance tests.[11] This criterion removes 30,301 observations (24,246 applicants) across 61 screenings, 60% of which correspond to 2019, after regional screenings were introduced.

The rolling application system within rounds introduces two types of control units: later-treated (applicants who received a voucher in a subsequent assignment) and never-treated (applicants who never received a voucher). Because few screenings include later-treated units close to the threshold, I drop them to compare recipients exclusively to those who were never treated. This restriction excludes 26,773 observations (7,071 applicants) across nine screenings.[12]

The final sample for window selection comprises 38,847 observations (32,789 applicants) across 12 screenings (seven in regular and five in elderly rounds), spanning nine assignment periods from 2017 to 2019. Within these s_t, I test for covariate balance within four progressively larger windows around the cutoff, selecting windows W_{s_t} based on a significance threshold of $\alpha = 0.1$.[13]

Following (Cattaneo, Idrobo and Titiunik, 2019), I split pre-treatment covariates into two sets and use the first set to select W_{s_t} using the *rdwinselect* Stata package. This set includes pre-treatment variables that vary within screenings and are observed for the full sample: family income, gender, tenure type, household poverty status (based on income per capita)[14], prior application to homeownership programs, and an indicator for non-missing geolocation. Additional round-specific variables include savings, online application, and SERVIU presence in the county (regular rounds), and marital status (elderly rounds).[15]

The second set, used for additional falsification tests within s_t in the evaluation sample W_0, includes: age range indicators (25-35 in regular rounds; 70-79 in elderly rounds), Chilean nationality, prior applicants within 500 meters, housing quality (crowding indicator and shelter type–formal or informal), county-level poverty, density, macro-region dummies, and distance to nearest SERVIU office. Round-specific variables include marital status, rent burden (available after

[11] Assuming a discrete outcome, a minimum detectable effect of one standard deviation, and significance levels between 0.05 and 0.15, this yields 60-80% power in the smallest window.

[12] Regular screenings from April–September 2017, September and November 2018, and August 2019, and the June 2018 elderly screening were excluded. In all but two cases, control groups included only later treated units.

[13] Balance tests use finite-sample exact randomization inference methods. P-values are not adjusted for multiple hypotheses testing to remain conservative.

[14] According to the 2017 CASEN, the 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%.

[15] In the small samples around the cutoff in each s_t, some covariates are constant. Overcrowding, marital status, low-quality housing in the April 2018 regular screening; nationality, baseline survey response, and overcrowding in the October 2019 O'Higgins screening; and nationality, overcrowding, and low-quality housing in the Los Lagos screening.

September 2018), baseline survey response, and survey-based measures of housing satisfaction and preference to stay in place (regular rounds), as well as SERVIU presence (elderly rounds).

Eight windows W_{s_t} were selected and aggregated into the evaluation sample W_0.[16] Tables III and IV summarize the selected windows by voucher type, reporting the assignment period, region, cutoff, sample size, number of treated and control units, minimum p-value from balance tests, and the normalized score range in each W_{s_t}.

The evaluation sample W_0 includes 2,643 observations from 2,622 unique applicants: 926 observations (910 applicants) from five regular screenings and 1,717 observations (1,712 applicants) from three elderly screenings. The maximum window size is $[-15, 15]$ in regular rounds and $[-5, 5]$ in elderly rounds. While the 2017–2018 sample includes national screenings, 2019 screenings are concentrated in southern regions for regular rounds and in central regions for elderly rounds. Randomized assignments account for 58% of the regular sample and 97% of the elderly sample.

5.2 Balance of the Evaluation Sample (W_0)

Given limited within-screening variation in some pre-treatment covariates, I assess overall balance between treatment and control groups in the evaluation sample using two regression models corresponding to distinct hypothesis tests: (1) a weaker test of the null hypothesis that covariate differences average to zero across screenings, and (2) a stricter test of the null that covariate differences are zero within screenings (Young, 2019). These tests are estimated as follows:

$$Z_{i,s_t} = \alpha + \sum_{s} \gamma_{s_t} \times \mathbf{1}\{s_t = s\} + \delta D_{i,s_t} + \epsilon_{i,s_t} \tag{3}$$

$$Z_{i,s_t} = \alpha + \sum_{s} \gamma_{s_t} \times \mathbf{1}\{s_t = s\} + \sum_{s} \delta_{s_t} \times D_{i,s_t} \cdot \mathbf{1}\{s_t = s\} + \epsilon_{i,s_t} \tag{4}$$

Where Z_{i,s_t} includes both sets of baseline covariates, D_{i,s_t} is the treatment indicator, γ_{s_t} denotes a set of screening indicators, δ denotes the pooled treatment effect, and δ_{s_t} the screening-specific treatment effect. Tables V and VI present the results. Columns 7 and 8 report the first test–whether δ in equation 3 is zero–and Columns 9 and 10 report the second test–whether all δ_{s_t} in equation 4 are jointly zero. The bottom panel presents joint significance tests from a regression of

[16] Nine windows were initially selected using the first set of covariates. Additional balance tests within s_t using the second set led to two adjustments: (1) narrower windows for the April 2018 and October 2019 regular screenings in O'Higgins–originally $[-5, 5]$ and $[-15, 10]$, respectively–and (2) the exclusion of the July 2019 elderly screening in Santiago. In total, 312 observations were dropped from the sample.

treatment on pre-treatment covariates available for the full sample, using both large-sample and randomization-based inference.

Treated and control applicants are balanced on key pre-treatment characteristics not used in voucher assignment by MINVU. Differences are small and generally not statistically significant–only two covariates are significant under both tests–and joint tests fail to reject the null of balance. Additionally, treatment effects in Section 6 remain stable after controlling for these covariates, which is consistent with balanced treated and control groups.

To further assess identification, Appendix Table A.2 presents differences in score components and total score between treatment and control groups within W_0. Differences are small, especially in elderly rounds where randomization is more common. Moreover, based on the score formula in Table I, these differences are not meaningful for program eligibility.

5.3 Sample Descriptive Statistics and External Validity

In regular rounds, the evaluation sample comprises applicants who are predominantly Chilean (97%), female (91%), and single (88%), with 65% aged 25-35 (Column 4 in Table V). Most are already renting (77%), and the average family income is 13.19 UF (US$516). About 15% have previously applied for a homeownership subsidy. Applicants live in high-poverty counties but report relatively low housing vulnerability. A majority express housing satisfaction (66%) and a desire to remain in place (56%).

In elderly rounds, the sample is more evenly split by gender (61% female), marital status (43% with a spouse), and age (41% aged 60-75) (Column 4 in Table VI). Average family income is lower than among regular applicants at 5.43 UF (US$212), and only 5% have previously applied for homeownership subsidies. These applicants are not more crowded than regular applicants, but they live in worse housing conditions, as they are more likely to reside in informal shelters. They also tend to live in less poor counties and in closer proximity to a local housing authority (SERVIU).

Multiple cutoffs can mitigate the local nature of RDD estimates by averaging treatment effects across different points of the score distribution (Cattaneo et al., 2016). To assess the external validity of this evaluation, I compare all recipients in screenings s_t included in the sample to those within the narrow windows W_{s_t} used to build W_0 ($s_t \in W_0$). Although W_0 includes less vulnerable recipients–those closest to the cutoff–both groups are similar across income, homeownership-subsidy applications, tenure type, demographics, SERVIU county presence, and prior applicants within 500 meters. In regular rounds, they also match on baseline survey response rate, online application status, rent burden, housing satisfaction, and preference to remain in place.[17]

The main exception concerns overcrowding: recipients in W_0 have smaller household sizes and, consequently, lower overcrowding rates, especially in regular rounds (3% vs. 41% among regular voucher recipients and 1% vs. 11% among elderly recipients). Because initial crowding may influence voucher use and, in turn, housing and neighborhood choices, this imbalance may reduce external validity. However, within the full sample of recipients in $s_t \in W_0$, initially overcrowded households have remarkably similar voucher utilization rates to less crowded households.

6 Results

Tables VII and VIII report intent-to-treat (ITT) effects–τ_{LRRD} in equation 2–estimated before the pandemic (December 2019), separately for regular and elderly rounds. Table IX presents corresponding ITT effects during the COVID-19 pandemic (November 2020).

For December 2019, outcomes include housing conditions, residential mobility, neighborhood access, and application to homeownership programs. For November 2020, the analysis focuses on broader socioeconomic impacts, including income, employment, and how families coped with the shock induced by the pandemic.

Each table reports the counterfactual mean and standard deviation (Column 2); estimates of τ_{LRRD} with standard errors clustered at the individual level (Column 3); p-values from OLS estimation (Column 4); randomization-based p-values (Column 5); and Romano-Wolf p-values adjusting for multiple hypothesis testing (Column 6).[18] Column 3 corresponds to my preferred specification, which includes baseline covariates $Z_{i,s}$ used in balance tests. Appendix Tables A.3, A.4, and A.5 present estimates excluding these covariates (Column 4) and show that including them yields efficiency gains and has minimal impact on the estimated coefficients.

I focus primarily on the ITT estimates throughout the analysis. However, Appendix Tables A.3, A.4, and A.5 also include Local Average Treatment Effects (LATE) using two-stage least squares

[17] Online Appendix Tables B.1 and B.2 compare voucher holders in W_0 to all recipients in $s_t \in W_0$.

[18] The randomization-t exact test developed by Young (2019) is implemented using the Stata package *randcmd*, and Romano-Wolf p-values are computed using the Stata package *rwolf* by Clarke, Romano and Wolf (2020). In both cases, I use 1,000 iterations and re-randomize the data by screening of applicants, as in a stratified experimental design.

estimates (2SLS), employing the discontinuity at the cutoff as an instrument for voucher utilization (Column 6). As expected, they are larger in magnitude than the ITT estimates, reflecting partial compliance with voucher assignment. On average, voucher utilization in regular rounds was 29% by December 2019 and rose to 49% by November 2020. In elderly rounds, the average lease-up rate remained stable at 54% through November 2020.

The next subsections present the main findings grouped by outcome categories, beginning with housing conditions and residential mobility before the pandemic.

6.1 Results Before the COVID-19 Pandemic (December 2019)

6.1.1 ***Housing conditions and residential mobility***

Voucher receipt significantly reduced overcrowding–defined as having three or more household members sleeping in the same bedroom (Tables VII and VIII). In regular rounds, overcrowding decreased by 4.4 percentage points (pp), representing a 37% decline relative to the control group. Notably, among untreated regular voucher applicants, overcrowding increased from 7% at application (Column 2 in Table V) to 12% by December 2019 (Column 2 in Table VII). In contrast, crowding among elderly households in the control group remained low, in the 1-3% range. Still, the voucher reduced crowding among elderly recipients by 0.3 persons per bedroom (24%) and overcrowding by 1.4 pp (Rand-t p-value=0.127; Romano-Wolf p-value=0.088).

The reduction in overcrowding among elderly recipients was driven by both an increase in the number of bedrooms and a reduction in household size. In contrast, for regular voucher holders the effect was primarily due to an increase in the number of bedrooms, as indicated by the not statistically significant (positive) coefficient on household size.

Regular voucher receipt increased residential mobility[19] by 7.1 pp (17%) and led to relocation farther from applicants' homes. Voucher holders were 7 pp (10%) less likely to remain within one kilometer (km) of their original location, 5.5 pp (48%) more likely to move more than ten km, and 5.5 pp (79%) more likely to relocate across county boundaries. Among elderly voucher holders, these effects were even more pronounced: they were 25.7 pp (76%) more likely to relocate to a different housing unit, 19.4 pp (26%) less likely to stay within one km, 8.4 pp (76%) more likely to move more than ten km, and 5.5 pp (61%) more likely to move to a different county.

[19] Defined as an indicator variable for a non-zero distance between baseline location and December 2019.

These effects are substantial, particularly when compared to national residential mobility rates of low-income families. According to the 2018 National Survey on Urban Quality of Life, only 9% of individuals in the lowest socioeconomic groups in urban areas aged 30-59, and 2.5% of those aged 60 or older, moved over the previous two years. In contrast, 43% of untreated regular applicants and 34% of untreated elderly applicants moved between application and December 2019, suggesting that applicants to the Chilean rental voucher program face high housing mobility pressures (see Column 2 in Tables VII and VIII).

*6.1.2 **Neighborhood characteristics***

The increased residential mobility generated by the voucher did not translate into improved access to higher-quality locations. Tables VII and VIII report treatment effects on seven neighborhood quality outcomes measured as of December 2019: (i) access to early childhood educational institutions and (ii) access to schools, both measured by the distance (km) to the closest institutions; (iii) school quality, proxied by the average math and language standardized test scores of nearby schools[20]; (iv) access to health care, measured by the distance to primary care centers and hospitals; (v) distance to the closest municipality, which tend to be in denser areas with more commercial activity; and (vi) county-level poverty; and (vii) safety, measured as the county-level share of individuals 18 years or older who reported being victims of assault, robbery, or theft in police records. All outcomes are expressed as standardized z-scores.

The voucher did not affect overall neighborhood quality but did influence specific neighborhood characteristics, with important differences across rounds. In regular rounds, voucher receipt reduced school access, increasing the distance to early childhood education centers by 0.193 standard deviations and to schools by 0.230 standard deviations, with no significant change in school quality. In both cases, these effects correspond to an average increase of approximately 0.4 kilometers relative to the counterfactual. Among elderly recipients, vouchers reduced the distance to a municipality by 0.101 standard deviations–an average reduction of 0.8 kilometers–suggesting increased relocation to denser areas with greater commercial activity. However, this effect is not statistically significant after adjusting for multiple hypothesis testing. Effects on poverty, crime, and healthcare access were not statistically significant for either voucher type.

[20] Based on the three closest (pre)schools within a one-kilometer radius, or the closest available if no (pre)schools exist within this distance.

6.1.3 *Homeownership*

The voucher did not reduce applications to homeownership subsidies. In regular rounds, voucher assignment had no significant effect on applications to any major homeownership programs or on the likelihood of maintaining an active savings account for homeownership, which is required to apply to these programs. In contrast, among the elderly, voucher assignment significantly increased applications by 4.7 pp (39%), mainly driven by a significant rise in applications to the fully subsidized program targeting the most vulnerable households–*Fondo Solidario (DS49)*–which provides housing typically located on the urban periphery (Blanco, Cibils and Miranda, 2014).

6.2 Results During the COVID-19 Pandemic (November 2020)

This section presents treatment effects (τ_{LRRD} in equation 2) using the subset of individuals who responded the online survey implemented between September and November 2020. The survey was sent to 716 unique applicants to regular rounds in the evaluation sample (W_0) with valid email, 65% of whom responded. The final sample includes 465 unique applicants (corresponding to 496 observations): 282 in the control group and 183 in the treatment group. This section focuses on regular vouchers due to the small number of elderly applicants who responded to the survey; only 37% of those in W_0 had a valid email, and the response rate among them was 38.4%.

I find no evidence of selective attrition in the follow-up survey data. Appendix Tables A.6 and A.7 show that voucher assignment did not affect response rates and that treatment and control groups remained balanced on baseline covariates. This supports the validity of the local randomization assumptions in the survey sample. Appendix Table A.8 presents pre-pandemic treatment effects using survey respondents. Compared to the full sample, the counterfactual mean and estimated treatment effects are similar, although some point estimates are not statistically significant in the smaller sample. Estimates also remain stable with and without baseline controls, further suggesting no selective attrition.

I use the follow-up survey to analyze household behavioral responses during the COVID-19 pandemic, then turn to housing and neighborhood characteristics to examine whether voucher receipt led to improvements in residential conditions. Differences between pre- and post-pandemic treatment effects may reflect either longer-term impacts of the voucher or broader effects of the pandemic. However, given the available data, I cannot distinguish between these channels.

6.2.1 *Household behavioral responses during the COVID-19 pandemic*

The survey elicited information on the magnitude of the shock and the strategies that families employed to cope. The data reveal a substantial negative impact of the pandemic on low-income households: 80% of non-voucher holders in the sample experienced partial or total household income loss, 18% were temporarily unemployed due to COVID-19, and 95% resorted to extraordinary measures to adapt to the new economic circumstances. Common strategies included reducing food expenses (57%), relying on emergency assistance (57%),[21] using household savings (49%), and missing monthly bill payments (44%)–including rent payments (22%).

To examine how families coped with the negative shock, I categorized behavioral responses into four groups, creating indicator variables equal to one if respondents engage in any strategy within each subgroup. Specifically, the indicators capture: (i) new income generation (cash from sales, new remunerated activity, or emergency relief benefits), (ii) increased debt (via formal and informal loans or missed monthly bill payments), and (iii) reduced expenses (on food, healthcare, or utilities). The fourth group, residential adjustments, includes three distinct strategies–moving out, others moving in, and missed rent payments. These are reported separately to further understand the voucher's impact on housing instability. Table IX presents the results.

Voucher receipt changed how families coped with the negative shock. Specifically, voucher holders were less likely to face financial distress and housing instability: they were 15.3 pp (23%) less likely to increase debt–12.4 pp (18%) less likely to report being debt overloaded–and were 10.9 pp (50%) less likely to miss rent payments. Although the effects on the likelihood of generating new income (-7.7 pp–11%–; Rand-t p-value=0.172), moving out of their homes (-3.1 pp–44%–, Rand-t p-value=0.221), or others moving in (4.9 pp–82%–, Rand-t p-value=0.118), were not statistically significant at conventional levels, these estimates are consistent with beneficial coping mechanisms from the voucher. Voucher holders were also 13.1 pp (18%) more likely to have a formal lease, potentially offering additional protection against eviction during the pandemic.

These results are consistent with a broader impact of the voucher on housing affordability. Specifically, voucher receipt reduced rent burden by 10.1 pp (21%), despite no significant changes in

[21] An emergency family income allowance for low-income families (US$80 per household member) was provided in May 2020. Two additional allowances were provided later, after survey data collection. In addition, an exceptional regulation allowed Chilean families to withdraw private savings for retirement three times during the pandemic. The first was in August 2020, although it is unlikely that voucher holders had sufficient funds to access. The remaining two withdrawals occurred after survey data collection.

household income or total rent. This reduction likely increased disposable income and contributed to fewer missed rent payments. In addition, the voucher may have shifted spending preferences toward greater housing stability and risk aversion during a period of heightened uncertainty. The observed reduction in missed rent payments relative to the control group suggests that voucher holders did not rely on the partial rent-guarantee insurance mechanism during this period (Section 2). Consistent with this interpretation, administrative data on rent co-payments–available for the subset of recipients who used their vouchers–indicate that overall rent co-payment behavior remained stable during this period.[22] Taken together, the evidence suggests both affordability and behavioral responses contributed to improved housing stability during this period.

These findings highlight a previously underappreciated social insurance role of rental vouchers during economic downturns–limiting the need to take on additional debt and helping stabilize housing conditions for low-income families.

6.2.2 ***Housing and neighborhood characteristics***

Table IX examines housing and neighborhood characteristics during the pandemic period, including satisfaction measures and housing-related consumption indicators such as heating systems, Wi-Fi, and cable TV. While the voucher did not affect total rent levels–suggesting that overall housing quality remained unchanged–it did improve several specific housing features. Specifically, voucher holders were 14.4 pp (19%) more likely to have an independent kitchen and 9.2 pp (12%) more likely to have some form of heating in their homes, while access to cable TV and WiFi remained unaffected. These improvements in housing conditions may have contributed to higher housing satisfaction: voucher recipients were 6.4 pp (8%) more likely to be satisfied with their homes (Rand-t p-value=0.112), although the effect is not statistically significant after adjusting for multiple hypothesis testing.

The effect on overcrowding remains negative but smaller than in the pre-pandemic period and not statistically significant in the smaller sample.[23] This may reflect the observed increase in bedrooms among the control group by November 2020, as well as the impact of the voucher on housing composition–specifically, a 9 pp (28%) increase in the likelihood of living with a partner (Rand-t p-value=0.086) and a 4.9 pp increase in the likelihood of others moving in (Rand-t p-value=0.118).

[22]Online Appendix Figure B.3 shows the likelihood of skipping rent payments, timing, and co-payment method in regular rounds before and after the pandemic. Payment delays in July 2020 appear to reflect a change in method.

[23]See Table A.8 for pre-pandemic outcomes in survey sample and Table VII or Table A.3 for the full sample.

However, neither effect is statistically significant after adjusting for multiple hypothesis testing.

To measure treatment effects on neighborhood outcomes, I constructed two neighborhood quality indices: one capturing the number of accessible amenities within a four-block radius from home and another reflecting disamenities in their neighborhoods. Amenities include childcare, schools, public transportation, and primary care, whereas disamenities encompass drug-related commercial activity, destroyed properties, graffiti, gang fights, armed-related activity, public alcohol consumption, and prostitution. Both indices are expressed as standardized z-scores.

The voucher did not improve overall neighborhood characteristics or neighborhood satisfaction. Moreover, recipients were 10.1 pp (35%) less likely to report that they could ask their neighbors for help with childcare. Although pre- and post pandemic outcomes are not directly comparable, these findings are consistent with prior evidence suggesting that rental vouchers alone do not improve access to better neighborhoods for low-income families (Ellen, 2020), and that rental policy in low-income settings may increase social isolation (Barnhardt, Field and Pande, 2017).

The absence of changes in rents or neighborhood quality–combined with improvements in specific housing features–suggests that, unlike the evidence from the U.S. (Collinson and Ganong, 2018), landlords in Chile did not respond to the voucher by matching rents to program rent caps during the early implementation period, despite the nationally fixed rent payment standard.

6.3 Robustness Checks

I test whether the results are robust to the choice of the window or bandwith used to define the evaluation sample. Specifically, I estimate treatment effects for the pre-pandemic period using the smallest window around the cutoff ($W = [-1, 1]$), including only randomized voucher holders.

The randomized sample includes 539 regular applicants and 1,672 elderly applicants. Online Appendix Tables B.3 and B.4 assess balance between treatment and control groups within this subset of the evaluation sample. The sample remains balanced on baseline covariates used in Section 5. Voucher utilization–both overall and by applicant screening–also remains consistent. Among regular recipients, utilization was 32% by December 2019 and 47% by November 2020. Among elderly recipients, utilization remained stable at 54% through November 2020.

Tables X and XI present treatment effects in the period before the pandemic (December 2019) in $W = [-1, 1]$ in regular and elderly rounds. Specifically, these tables present the OLS, intent-to-treat (ITT) effects with and without baseline controls, and Local Average Treatment Effects (LATE), using the discontinuity at the cutoff as an instrument for voucher utilization. Compared to the full sample (Tables A.3 and A.4), results are robust to alternative bandwidth or window selection. Coefficients remained stable, although some standard errors increased using the smaller sample.

7 Discussion

This research coincided with the COVID-19 pandemic, offering a unique opportunity to examine not only the effects of rental voucher programs in a middle-income country but also their performance during a period of significant economic disruption. Before the pandemic, results align with U.S.-based evidence: voucher receipt reduced overcrowding and increased residential mobility–effects that were more pronounced among elderly recipients, who received a much larger monthly subsidy–but did not improve overall neighborhood quality. If anything, elderly recipients appeared to relocate closer to denser areas, while young families with children in regular rounds moved farther from schools.

The larger elderly voucher appears better aligned with applicants' residential mobility needs than the regular voucher for younger households. However, the observed differences in outcomes are modest relative to the size of the benefit gap between the two voucher types. This discrepancy may reflect the greater vulnerability of elderly recipients or substantial barriers low-income families face in using their vouchers, especially in higher-quality neighborhoods. As such, these results do not speak directly to the elasticity of housing outcomes with respect to subsidy size.

Additionally, voucher receipt did not reduce applications to homeownership programs; on the contrary, it increased applications among elderly recipients. This result is consistent with either strong preferences for homeownership or a more immediate need for housing assistance, given that the elderly received the voucher for a shorter period.

During the pandemic, rental vouchers demonstrated a broader impact on families in regular rounds. The program helped low-income households cope with the economic shock by reducing the need to take on additional debt and stabilizing housing conditions. This effect appears to be driven, in part, by a reduction in rent burden and shifts in spending preferences toward greater housing stability during uncertain times. These findings highlight a previously underappreciated social insurance role of rental subsidies during economic downturns–a role made all the more critical by the housing vulnerability exposed during the pandemic: in Chile, the number of households living in slums rose by 74% in the first year of the pandemic (Techo, 2021).

Positive impacts on housing outcomes, alongside null effects on overall neighborhood quality, persisted during the pandemic. However, due to data limitations, I cannot disentangle the direct long-term effects of voucher receipt from broader pandemic-related disruptions, or differences in benefit durations. Future research using longer follow-up periods could help isolate these effects and identify subgroups of recipients who may benefit most from the policy.

This research provides valuable insights for the ongoing housing policy debate on rental voucher design (Ellen, O'Regan and Ganz, 2020). The findings underscore the limits of financial support alone in improving access to better environments for low-income families. However, a fixed, modest, short-term voucher can extend coverage to a broader share of eligible families while still improving housing-related outcomes without affecting employment. In addition, partial rent-guarantee insurance mechanisms that cover missed co-payments for a limited period may support housing stability during economic shocks without necessarily creating moral hazard.

References

Abramson, Boaz. 2023. "The equilibrium effects of eviction and homelessness policies." Working Paper.

Abramson, Boaz, and Stijn Van Nieuwerburgh. 2024. "Rent guarantee insurance."

Aliprantis, Dionissi, Hal Martin, and Kristen Tauber. 2020. "What Determines the Success of Housing Mobility Programs?" *Available at SSRN 3737041*.

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

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

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

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

Brewer, Mike, James Browne, Carl Emmerson, Andrew Hood, and Robert Joyce. 2019. "The curious incidence of rent subsidies: Evidence of heterogeneity from administrative data." *Journal of Urban Economics*, 114: 103198.

Cattaneo, Matias D, Nicolás Idrobo, and Rocío Titiunik. 2019. *A practical introduction to regression discontinuity designs: Volume II.* Cambridge University Press.

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

Chan, Eric W, and Yulian Fan. 2023. "Housing discrimination in the low-income context: Evidence from a correspondence experiment." *Journal of Housing Economics*, 59: 101889.

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

Chyn, Eric. 2018. "Moved to opportunity: The long-run effects of public housing demolition on children." *American Economic Review*, 108(10): 3028–3056.

Chyn, Eric, and Lawrence F Katz. 2021. "Neighborhoods Matter: Assessing the Evidence for Place Effects." *Journal of Economic Perspectives*, 35(4): 197–222.

Clarke, Damian, Joseph P. Romano, and Michael Wolf. 2020. "The Romano-Wolf multiple-hypothesis correction in Stata." *The Stata Journal*, 20(4): 843–859.

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

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

Collinson, Robert, Ingrid Gould Ellen, and Jens Ludwig. 2015. "Low-income housing policy." In *Economics of Means-Tested Transfer Programs in the United States, Volume 2*. 59–126. University of Chicago Press.

Collinson, Robert, Ingrid Gould Ellen, and Jens Ludwig. 2019. "Reforming housing assistance." *The ANNALS of the American Academy of Political and Social Science*, 686(1): 250–285.

Collinson, Robert, John Eric Humphries, Nicholas Mader, Davin Reed, Daniel Tannenbaum, and Winnie Van Dijk. 2024. "Eviction and poverty in American cities." *The Quarterly Journal of Economics*, 139(1): 57–120.

DeLuca, Stefanie, and Eva Rosen. 2022. "Housing Insecurity Among the Poor Today." *Annual Review of Sociology*, 48: 343–371.

Díaz, Juan D, and José R Zubizarreta. 2023. "Complex discontinuity designs using covariates: Impact of school grade retention on later life outcomes in Chile." *The Annals of Applied Statistics*, 17(1): 67–88.

Eerola, Essi, and Teemu Lyytikäinen. 2021. "Housing allowance and rents: Evidence from a stepwise subsidy scheme." *The Scandinavian Journal of Economics*, 123(1): 84–109.

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

Ellen, Ingrid Gould, Katherine M O'Regan, and Amy Ganz. 2020. "A renter safety net: A call for federal emergency rental assistance." *Securing Our Economic Future, ed. Melissa S. Kearney and Amy Ganz (Washington DC: Aspen Institute Press, 2020)*.

Ellen, Ingrid Gould, Katherine O'Regan, and Sarah Strochak. 2024. "Race, Space, and Take Up: Explaining housing voucher lease-up rates." *Journal of Housing Economics*, 63: 101980.

Gibbons, Stephen, Maria Sanchez-Vidal, and Olmo Silva. 2020. "The bedroom tax." *Regional Science and Urban Economics*, 82: 103418.

Gubits, Daniel, Marybeth Shinn, Michelle Wood, Stephen Bell, Samuel Dastrup, Claudia Solari, Scott Brown, Debi McInnis, Tom McCall, and Utsav Kattel. 2016. "Family options study: 3-year impacts of housing and services interventions for homeless families." *Available at SSRN 3055295*.

Hyslop, Dean R, and David Rea. 2019. "Do housing allowances increase rents? Evidence from a discrete policy change." *Journal of housing economics*, 46: 101657.

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

Kling, Jeffrey R, Jeffrey B Liebman, and Lawrence F. Katz. 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica*, 75(1): 83–119.

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

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

National Socioeconomic Survey (CASEN). 2013, 2017. "Encuesta de Caracterización Socioeconómica Nacional (CASEN)." Ministerio de Desarrollo Social, Gobierno de Chile.

OECD. 2024. "OECD Affordable Housing Database: PH3.1 Public Spending on Housing Allowances." *OECD Webfile Service*, OECD Directorate of Employment, Labour and Social Affairs – Social Policy Division. Last updated June 26, 2024.

Olsen, Edgar O. 2003. "Housing programs for low-income households." In *Means-tested transfer programs in the United States*. 365–442. University of Chicago Press.

Phillips, David C. 2017. "Landlords avoid tenants who pay with vouchers." *Economic Letters*, 151: 48–52.

Pollakowski, Henry O, Daniel H Weinberg, Fredrik Andersson, John C Haltiwanger, Giordano Palloni, and Mark J Kutzbach. 2022. "Childhood housing and adult outcomes: a between-siblings analysis of housing vouchers and public housing." *American Economic Journal: Economic Policy*, 14(3): 235–272.

Reina, Vincent J, and Claudia Aiken. 2022. "Moving to opportunity, or aging in place? The changing profile of low income and subsidized households and where they live." *Urban Affairs Review*, 58(2): 454–492.

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

Sanbonmatsu, Lisa, Lawrence F Katz, Jens Ludwig, Lisa A Gennetian, Greg J Duncan, Ronald C Kessler, Emma K Adam, Thomas McDade, and Stacy T Lindau. 2011. "Moving to opportunity for fair housing demonstration program: Final impacts evaluation."

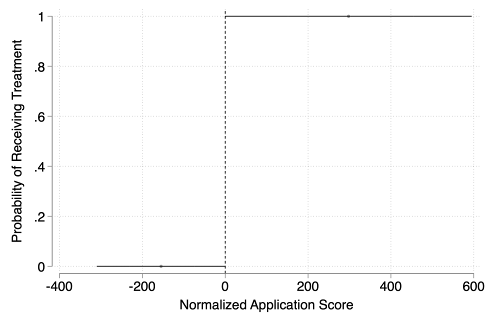
Wood, Michelle, Jennifer Turnham, and Gregory Mills. 2008. "Housing affordability and family well-being: Results from the housing voucher evaluation." *Housing Policy Debate*, 19: 367–412.

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

Zhang, Ning. 2022. "In-kind housing transfers and labor supply: a structural approach."

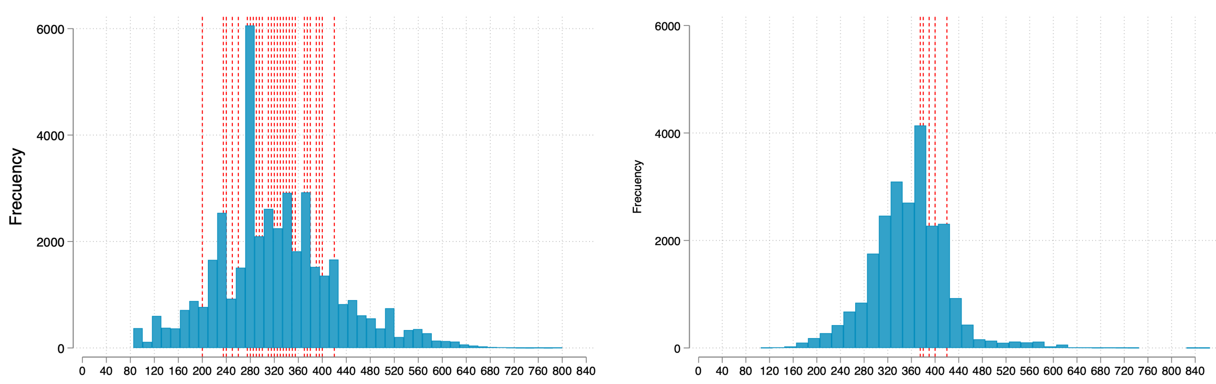
Figures

FIGURE I. Sharp RD Design



This figure presents the treatment probability for all values of the normalized score c.

FIGURE II. Multiple Cutoff Regression Discontinuity Design



(a) Regular Rounds **(b)** Elderly Rounds

This figure shows the distribution of the application score and cutoffs in regular (Panel a) and elderly (Panel b) rounds in the pooled data.

Tables

TABLE I. 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 (up to 3)	20 per prev application	=
11	Social Vulnerability (RSH Index)	0 (81-100th), 45 (71-80th), 90 (61-70th) 135 (51-60th), 180 (40-50th)	=
12	Housing Vulnerability[3]	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

Notes: (1) Applicant not counted in household size in regular rounds; (2) Age is measured as of December of the application year; (3) Sum of pre-defined scores for crowding, housing quality, and access to reliable water and basic sanitation.

TABLE II. Density Test

	Binomial Test (q=0.5)				
Sample	N	Observed T	Expected T	Observed q	p-value
	(1)	(2)	(3)	(4)	(5)
All screenings	4,099	2,023	2,050	0.49	0.417
Screenings in Wo	2,679	1,318	1,340	0.49	0.417

This table reports binomial tests for manipulation in the running variable using data within the small window $W = [-5, 5]$ around the cutoff. It presents the results for all screenings considered in window selection, followed by those in the evaluation sample. The null hypothesis assumes a probability of success $q = 0.5$. See Section 4 for more details.

TABLE III. Window Selection Results Regular Rounds

Assignment Date	Region (1)	Cutoff (2)	Total (3)	Controls (4)	Treated (5)	Min pvalue (6)	Left (7)	Right (8)
11apr2018	All	285	144	66	78	0.114	-2	2
28dec2018	All	345	375	295	80	0.208	-5	2
10oct2019	O'Higgins	285	65	47	18	0.114	-5	2
10oct2019	Araucania	285	275	153	122	0.400	-15	15
10oct2019	Los Lagos	275	67	47	18	0.262	-1	1

This table describes each applicant screenings in regular rounds of the program within the evaluation sample. Column 1 indicate whether the screening was national or region-based. Column 2 presents the cutoff. Columns 3 to 5 report the total sample size, and the number of individuals below (control) and above (treated) the cutoff. Columns 6 to 8 summarize the selected window: the minimum p-value from all balance tests using covariates explained in Section 5, and the minimum and maximum value of the running variable within the window.

TABLE IV. Window Selection Results Elderly Rounds

Assignment Date	Region (1)	Cutoff (2)	Total (3)	Controls (4)	Treated (5)	Min pvalue (6)	Left (7)	Right (8)
04sep2017	All	380	1,173	337	836	0.284	-5	2
11apr2018	All	380	355	248	107	0.144	-2	2
05jul2019	Valparaiso	380	189	159	30	0.188	-5	5

This table replicates Table III for applicant screenings in elderly rounds within the evaluation sample.

TABLE V. Balance in Baseline Characteristics in Regular Rounds

		Control		Treated			Balance Test 1		Balance Test 2	
	N	Mean	SD	Mean	SD	Diff	F-test (p)	Rand-t (p)	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Female	926	0.89	0.31	0.91	0.29	0.01	0.628	0.693	0.197	0.226
Poor	926	0.20	0.40	0.22	0.42	0.02	0.518	0.518	0.966	0.970
Tenant	926	0.82	0.39	0.77	0.42	-0.04	0.896	0.849	0.929	0.944
Family income (UF)	926	13.10	4.57	13.19	4.98	0.09	0.286	0.272	0.178	0.195
Previous app. to ownership subsidy	926	0.14	0.34	0.15	0.36	0.01	0.568	0.487	0.863	0.479
Geocoded location	926	0.91	0.28	0.92	0.26	0.01	0.609	0.545	0.147	0.865
Nearby SERVIU (county)	926	0.49	0.50	0.45	0.50	-0.04	0.467	0.661	0.563	0.228
Saving balance (UF)	926	17.15	37.42	15.77	15.82	-1.38	0.409	0.474	0.430	0.571
Online Application	926	0.36	0.48	0.35	0.48	-0.01	0.825	0.800	0.402	0.413
Age 25-35	926	0.59	0.49	0.65	0.48	0.06	0.192	0.179	0.132	0.148
Chilean	926	0.93	0.25	0.97	0.18	0.04	0.072*	0.081*	0.003***	0.015**
Spouse/partner	926	0.14	0.35	0.12	0.33	-0.02	0.892	0.871	0.601	0.634
Previous app. in neighborhood (500mts)	926	0.45	0.50	0.47	0.50	0.02	0.144	0.632	0.565	0.312
Number of bedrooms	926	1.61	0.76	1.66	0.82	0.05	0.634	0.990	0.265	0.250
Low-quality housing	926	0.10	0.29	0.06	0.23	-0.04	0.915	0.104	0.037**	0.535
Overcrowding indicator	926	0.07	0.26	0.03	0.18	-0.04	0.111	0.170	0.235	0.599
County poverty rate	926	0.12	0.07	0.13	0.07	0.01	0.589	0.581	0.321	0.336
Santiago	926	0.13	0.34	0.11	0.31	-0.02	0.585	0.541	0.687	0.620
North	926	0.06	0.24	0.03	0.18	-0.02	0.466	0.435	0.745	0.239
Valparaiso	926	0.08	0.27	0.06	0.24	-0.02	0.527	0.511	0.403	0.378
Center South	926	0.26	0.44	0.23	0.42	-0.03	0.186	0.178	0.393	0.634
South	926	0.47	0.50	0.56	0.50	0.09	0.924	0.913	0.973	0.350
High density county	926	0.39	0.49	0.35	0.48	-0.04	0.370	0.392	0.875	0.882
KM to closest SERVIU	926	18.67	22.85	21.39	26.19	2.72	0.421	0.423	0.647	0.677
Answered Baseline Survey	926	0.71	0.45	0.72	0.45	0.01	0.893	0.855	0.026**	0.041**
Rent burden	696	0.48	0.27	0.51	0.22	0.02	0.822	0.811	0.555	0.423
Rent (UF)	860	5.32	3.15	5.24	2.70	-0.08	0.935	0.953	0.869	0.137
Desire to stay in place	558	0.57	0.50	0.56	0.50	-0.02	0.477	0.489	0.679	0.698
Satisfied with housing	602	0.66	0.47	0.66	0.48	-0.00	0.833	0.822	0.144	0.212
SCREENING INDICATORS							Yes	Yes	Yes	Yes
SCREENING INDICATORSxTREAT							No	No	Yes	Yes
Joint Significance (p)									0.890	0.842

This table presents summary statistics and balance tests between treatment and control groups in the evaluation sample in regular rounds. Columns 1-5 report baseline characteristics. Columns 7-8 show results from the first balance test using the weaker null hypothesis from 3, and Columns 9-10 report results from the second test including interaction terms from 4. April 2018 is the omitted category. Columns 7 and 9 use large-sample inference (F-test); Columns 8 and 10 report Fisherian randomization inference p-values (Randomization-t exact test), computed using 1,000 iterations in the Stata package randcmd (Young, 2019). The bottom panel reports joint significance tests regressing treatment on baseline covariates using both inference methods. See Section 5 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE VI. Balance in Baseline Characteristics in Elderly Rounds

		Control		Treated			Balance Test 1		Balance Test 2	
	N	Mean	SD	Mean	SD	Diff	F-test (p)	Rand-t (p)	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Female	1,717	0.60	0.49	0.61	0.49	0.01	0.368	0.342	0.644	0.656
Poor	1,717	0.56	0.50	0.52	0.50	-0.04	0.159	0.169	0.551	0.550
Tenant	1,717	0.55	0.50	0.52	0.50	-0.03	0.937	0.946	0.285	0.290
Family income per capita (UF)	1,717	5.43	2.08	5.53	2.21	0.10	0.309	0.198	0.700	0.472
Previous app. to ownership subsidy	1,717	0.06	0.23	0.05	0.23	-0.00	0.777	0.776	0.808	0.823
Geocoded location	1,717	0.92	0.27	0.89	0.32	-0.03	0.963	0.989	0.795	0.846
Spouse/partner	1,717	0.39	0.49	0.43	0.50	0.04	0.918	0.942	0.999	0.999
Nearby SERVIU (county)	1,717	0.51	0.50	0.56	0.50	0.04	0.029**	0.033**	0.055*	0.084*
Age 60-75	1,717	0.57	0.50	0.41	0.49	-0.16	0.885	0.848	0.331	0.322
Chilean	1,717	0.98	0.13	0.99	0.12	0.00	0.662	0.683	0.723	0.879
Previous app. in neighborhood (500mts)	1,717	0.60	0.49	0.71	0.45	0.11	0.234	0.223	0.608	0.597
Number of bedrooms	1,717	1.34	0.66	1.30	0.61	-0.04	0.039**	0.037**	0.209	0.235
Low-quality housing	1,717	0.18	0.38	0.19	0.39	0.01	0.927	0.901	0.439	0.441
Overcrowding indicator	1,717	0.01	0.12	0.01	0.11	-0.00	0.993	1.000	0.324	0.491
County poverty rate	1,717	0.09	0.05	0.09	0.05	0.00	0.548	0.559	0.535	0.537
Santiago	1,717	0.22	0.42	0.26	0.44	0.03	0.146	0.129	0.211	0.052*
North	1,717	0.09	0.29	0.13	0.34	0.04	0.396	0.418	0.400	0.591
Valparaiso	1,717	0.36	0.48	0.23	0.42	-0.13	0.671	0.671	0.751	0.893
Center South	1,717	0.20	0.40	0.21	0.41	0.01	0.721	0.738	0.750	0.893
South	1,717	0.12	0.33	0.17	0.38	0.05	0.320	0.309	0.024**	0.847
High density county	1,717	0.51	0.50	0.50	0.50	-0.01	0.727	0.717	0.833	0.840
KM to closest SERVIU	1,717	12.73	17.02	12.25	17.93	-0.48	0.259	0.267	0.199	0.209
SCREENING INDICATORS							Yes	Yes	Yes	Yes
SCREENING INDICATORSxTREAT							No	No	Yes	Yes
Joint Significance F-Test (p)									0.580	0.602

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

TABLE VII. Effect of Regular Voucher Before the COVID-19 Pandemic (2019)

Outcome Variable	N (1)	Control Mean [SD] (2)	ITT (3)	OLS (4)	Rand-t (5)	RWolf (6)
Household size	925	2.85	0.115	0.175	0.187	0.666
		[1.21]	(0.084)			
Number of bedrooms	921	1.76	0.243***	0.000	0.001	0.001
		[0.83]	(0.055)			
Number of people per bedroom	921	1.80	-0.165***	0.000	0.001	0.001
		[0.69]	(0.042)			
Overcrowing indicator	921	0.12	-0.044**	0.036	0.044	0.013
		[0.33]	(0.021)			
Moved to diff. unit	849	0.43	0.071*	0.057	0.047	0.118
		[0.50]	(0.037)			
Distance (km)	849	14.07	5.483	0.714	0.768	0.362
		[164.01]	(14.931)			
Stayed in 1km radius	849	0.71	-0.070**	0.050	0.053	0.118
		[0.46]	(0.035)			
Moved +10km away	849	0.12	0.055**	0.043	0.043	0.118
		[0.33]	(0.027)			
Moved to another county	849	0.07	0.055***	0.009	0.014	0.103
		[0.26]	(0.021)			
County level poverty	856	-0.05	-0.043	0.474	0.423	0.963
		[1.00]	(0.060)			
County crime victims	856	0.02	0.061	0.175	0.190	0.615
		[0.83]	(0.045)			
School Quality	816	0.02	-0.025	0.721	0.698	0.963
		[0.94]	(0.071)			
School access	849	-0.08	0.230**	0.022	0.017	0.039
		[0.69]	(0.101)			
Distance to early childhood educ.	849	-0.07	0.193**	0.031	0.032	0.051
		[0.76]	(0.090)			
Distance to health care	849	-0.05	0.143	0.136	0.130	0.250
		[0.78]	(0.096)			
Kms to closest municipality	921	0.00	-0.003	0.964	0.961	0.963
		[1.01]	(0.064)			
Application to Ownership Programs	926	0.31	0.017	0.579	0.583	0.763
		[0.46]	(0.031)			
Application DS1	926	0.23	0.009	0.762	0.737	0.904
		[0.42]	(0.029)			
Application DS49	926	0.12	0.005	0.804	0.807	0.904
		[0.32]	(0.022)			
Active ownership savings account	926	0.92	0.012	0.516	0.487	0.904
		[0.27]	(0.018)			

This table presents estimates of equation 2 using outcomes measured in December 2019. Column 2 shows the control group mean with the standard deviation in square brackets. Column 3 reports Intent-to-Treat (ITT) estimates, including applicant screenings fixed-effects and baseline covariates explained in Section 6. OLS standard errors are in parenthesis. Column 4 shows OLS p-values, Column 5 presents Fisherian randomization inference, and Column 6 exhibits Romano-Wolf adjusted for multiple hypothesis testing p-values. Significance levels: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

TABLE VIII. Effect of Elderly Voucher Before the COVID-19 Pandemic (2019)

Outcome Variable	N (1)	Control Mean [SD] (2)	ITT (3)	OLS (4)	Rand-t (5)	RWolf (6)
Household size	1,717	1.60	-0.157***	0.002	0.004	0.003
		[1.11]	(0.051)			
Number of bedrooms	1,604	1.35	0.439***	0.000	0.000	0.001
		[0.74]	(0.045)			
Number of people per bedroom	1,604	1.24	-0.300***	0.000	0.000	0.001
		[0.59]	(0.027)			
Overcrowing indicator	1,604	0.03	-0.014	0.128	0.127	0.088
		[0.17]	(0.009)			
Moved to diff. unit	1,549	0.34	0.257***	0.000	0.000	0.001
		[0.47]	(0.028)			
Distance (km)	1,549	22.37	2.661	0.764	0.775	0.478
		[150.24]	(8.845)			
Stayed in 1km radius	1,549	0.76	-0.194***	0.000	0.001	0.001
		[0.43]	(0.026)			
Moved +10km away	1,549	0.11	0.084***	0.000	0.000	0.001
		[0.31]	(0.020)			
Moved to another county	1,549	0.09	0.055***	0.002	0.002	0.003
		[0.29]	(0.018)			
County level poverty	1,577	-0.03	-0.050	0.289	0.283	0.943
		[0.98]	(0.047)			
County crime victims	1,575	-0.01	-0.007	0.848	0.865	0.929
		[0.80]	(0.039)			
School Quality	1,509	-0.02	0.000	0.994	0.997	0.929
		[0.99]	(0.055)			
School access	1,549	0.04	-0.066	0.200	0.192	0.827
		[1.27]	(0.051)			
Distance to early childhood educ.	1,549	0.04	-0.066	0.193	0.184	0.850
		[1.24]	(0.051)			
Distance to health care	1,549	0.04	-0.067	0.209	0.207	0.829
		[1.24]	(0.053)			
Kms to closest municipality	1,624	0.04	-0.101*	0.052	0.063	0.312
		[1.11]	(0.052)			
Application to Ownership Programs	1,717	0.12	0.047***	0.009	0.011	0.025
		[0.33]	(0.018)			
Application DS1	1,717	0.07	0.023	0.108	0.089	0.289
		[0.26]	(0.014)			
Application DS49	1,717	0.07	0.034**	0.017	0.016	0.017
		[0.25]	(0.014)			

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

TABLE IX. Effect of Regular Voucher During COVID-19 (2020)

Outcome Variable	N (1)	Control Mean [SD] (2)	ITT (3)	OLS (4)	Rand-t (5)	RWolf (6)
Formal Lease	330	0.73	0.131***	0.009	0.490	0.044
		[0.44]	(0.050)			
Total rent (unit)	326	6.80	-0.018	0.947	0.013	0.933
		[2.81]	(0.274)			
Rent burden (rent paid)	284	0.48	-0.101***	0.001	0.952	0.006
		[0.27]	(0.031)			
Employed	344	0.69	-0.021	0.713	0.004	0.933
		[0.47]	(0.056)			
Family Income	340	13.42	0.326	0.592	0.703	0.933
		[5.71]	(0.608)			
Debt overload	342	0.69	-0.124**	0.032	0.612	0.075
		[0.46]	(0.058)			
Spouse/Partner	339	0.32	0.090	0.102	0.086	0.342
		[0.47]	(0.055)			
Household Size	428	3.28	-0.058	0.675	0.665	0.699
		[1.40]	(0.139)			
Number of people per bedroom	417	1.58	-0.099	0.157	0.159	0.649
		[0.69]	(0.070)			
Overcrowding indicator	418	0.11	-0.033	0.283	0.303	0.649
		[0.31]	(0.031)			
Number of bedrooms	417	2.24	0.022	0.784	0.792	0.848
		[0.88]	(0.081)			
Laundry Room	415	0.36	0.031	0.526	0.502	0.848
		[0.48]	(0.049)			
Kitchen Room	415	0.75	0.144***	0.001	0.001	0.022
		[0.43]	(0.043)			
Heat system	415	0.80	0.092***	0.003	0.003	0.033
		[0.40]	(0.030)			
Cable, Wifi	412	0.59	-0.024	0.531	0.543	0.848
		[0.35]	(0.037)			
Satisfaction current housing unit	459	0.77	0.064	0.101	0.112	0.547
		[0.42]	(0.039)			
Household income loss after COVID-19	343	0.80	-0.071	0.185	0.198	0.266
		[0.40]	(0.054)			
COVID-19: Expenses	340	0.69	-0.025	0.658	0.652	0.770
		[0.46]	(0.056)			
COVID-19: Debt	340	0.67	-0.153**	0.010	0.013	0.013
		[0.47]	(0.059)			
COVID-19: New Income Source	340	0.71	-0.077	0.176	0.172	0.266
		[0.46]	(0.057)			
COVID-19: Moved out	340	0.07	-0.031	0.220	0.229	0.246
		[0.25]	(0.025)			
COVID-19: Others moved in	340	0.06	0.049	0.131	0.118	0.151
		[0.23]	(0.032)			
COVID-19: Delayed rent payments	340	0.22	-0.109**	0.011	0.011	0.085
		[0.42]	(0.043)			
Satisfaction current neighborhood	443	0.82	-0.043	0.276	0.274	0.754
		[0.38]	(0.039)			
Positive Ammenities	450	-0.02	-0.030	0.773	0.764	0.998
		[1.02]	(0.105)			
Negative Ammenities	328	-0.00	-0.012	0.919	0.914	0.998
		[1.01]	(0.115)			
Would ask neighbors for childcare	427	0.29	-0.101**	0.026	0.021	0.090
		[0.45]	(0.045)			

This table replicates the analysis in Table VII using survey data for regular rounds collected between September and November 2020. See Table VII for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE X. Pre-pandemic Outcomes in $W = [-1, 1]$ (Regular Rounds)

Outcome Variable	N (1)	Control Mean [SD] (2)	Voucher Use OLS (3)	Reduced Form ITT (4)	Reduced Form ITT (5)	IV LATE (6)
Household size	538	2.65	-0.177	0.067	0.055	0.186
		[1.13]	(0.123)	(0.119)	(0.115)	(0.363)
Number of bedrooms	536	1.58	0.484***	0.297***	0.311***	1.044***
		[0.77]	(0.097)	(0.080)	(0.075)	(0.235)
Number of people per bedroom	536	1.84	-0.528***	-0.247***	-0.265***	-0.890***
		[0.65]	(0.069)	(0.058)	(0.053)	(0.167)
Overcrowing indicator	536	0.10	-0.107***	-0.046*	-0.053**	-0.178**
		[0.29]	(0.023)	(0.025)	(0.026)	(0.085)
Moved to diff. unit	480	0.48	0.214***	0.096*	0.073	0.240
		[0.50]	(0.060)	(0.050)	(0.050)	(0.155)
Distance (km)	480	21.13	13.802	-2.893	-3.586	-11.830
		[226.92]	(24.635)	(23.643)	(25.253)	(81.131)
Stayed in 1km radius	480	0.67	-0.165***	-0.084*	-0.066	-0.216
		[0.47]	(0.060)	(0.047)	(0.048)	(0.152)
Moved +10km away	480	0.14	0.096**	0.043	0.037	0.121
		[0.34]	(0.047)	(0.035)	(0.035)	(0.109)
Moved to another county	480	0.09	0.058	0.039	0.043	0.142*
		[0.29]	(0.037)	(0.028)	(0.028)	(0.086)
County level poverty	485	-0.26	-0.075	0.013	-0.076	-0.250
		[0.90]	(0.101)	(0.090)	(0.079)	(0.217)
County crime victims	485	0.06	0.038	-0.010	0.078	0.257
		[0.82]	(0.073)	(0.077)	(0.059)	(0.189)
School Quality	467	0.00	-0.024	-0.028	-0.039	-0.132
		[0.93]	(0.123)	(0.091)	(0.089)	(0.282)
School access	480	-0.07	0.393	0.348**	0.331*	1.093*
		[0.80]	(0.260)	(0.165)	(0.173)	(0.576)
Distance to early childhood educ.	480	-0.08	0.430*	0.368**	0.314**	1.036**
		[0.81]	(0.222)	(0.144)	(0.147)	(0.490)
Distance to health care	480	-0.04	0.349	0.311**	0.259	0.854
		[0.88]	(0.236)	(0.156)	(0.163)	(0.539)
Kms to closest municipality	536	0.08	0.083	0.049	-0.019	-0.064
		[1.17]	(0.134)	(0.102)	(0.098)	(0.318)
Application to Ownership Programs	539	0.34	0.045	0.061	0.040	0.135
		[0.47]	(0.052)	(0.044)	(0.041)	(0.129)
Application DS1	539	0.24	0.029	0.044	0.027	0.090
		[0.43]	(0.048)	(0.041)	(0.037)	(0.116)
Application DS49	539	0.16	-0.005	0.013	0.003	0.009
		[0.36]	(0.038)	(0.033)	(0.033)	(0.103)
Active ownership savings account	539	0.92	-0.004	0.018	0.015	0.049
		[0.27]	(0.033)	(0.025)	(0.026)	(0.084)
COVARIATES			YES	NO	YES	YES
SCREENING FE			YES	YES	YES	YES

This table replicates the analysis in Table VII for the sample of randomized vouchers within the narrower window $W = [-1, 1]$ from regular rounds. See Table VII for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE XI. Pre-pandemic Outcomes in $W = [-1, 1]$ (Elderly Rounds)

Outcome Variable	N	Control Mean [SD] (2)	Voucher Use OLS (2)	Reduced Form ITT (3)	Reduced Form ITT (4)	IV LATE (5)
Household size	1,672	1.59	-0.115***	-0.207***	-0.195***	-0.359***
		[1.10]	(0.043)	(0.055)	(0.051)	(0.093)
Number of bedrooms	1,562	1.35	0.889***	0.433***	0.432***	0.767***
		[0.74]	(0.045)	(0.048)	(0.047)	(0.077)
Number of people per bedroom	1,562	1.24	-0.492***	-0.317***	-0.312***	-0.554***
		[0.59]	(0.025)	(0.032)	(0.027)	(0.046)
Overcrowing indicator	1,562	0.03	-0.017**	-0.017*	-0.016*	-0.028*
		[0.17]	(0.007)	(0.009)	(0.009)	(0.016)
Moved to diff. unit	1,509	0.33	0.355***	0.269***	0.266***	0.462***
		[0.47]	(0.026)	(0.028)	(0.028)	(0.047)
Distance (km)	1,509	22.50	18.864**	2.151	0.902	1.568
		[151.97]	(9.409)	(9.175)	(8.927)	(15.427)
Stayed in 1km radius	1,509	0.76	-0.286***	-0.201***	-0.201***	-0.350***
		[0.42]	(0.027)	(0.027)	(0.027)	(0.046)
Moved +10km away	1,509	0.10	0.105***	0.073***	0.077***	0.134***
		[0.31]	(0.022)	(0.020)	(0.020)	(0.035)
Moved to another county	1,509	0.09	0.119***	0.044**	0.053***	0.092***
		[0.28]	(0.020)	(0.018)	(0.018)	(0.031)
County level poverty	1,537	-0.03	-0.016	-0.024	-0.051	-0.088
		[0.99]	(0.047)	(0.060)	(0.048)	(0.083)
County crime victims	1,535	-0.00	-0.058	-0.011	-0.009	-0.016
		[0.80]	(0.040)	(0.048)	(0.040)	(0.068)
School Quality	1,471	-0.01	0.003	-0.015	-0.010	-0.017
		[0.98]	(0.054)	(0.058)	(0.057)	(0.098)
School access	1,509	0.04	-0.033	-0.066	-0.066	-0.114
		[1.28]	(0.046)	(0.052)	(0.053)	(0.091)
Distance to early childhood educ.	1,509	0.04	-0.043	-0.072	-0.070	-0.121
		[1.25]	(0.047)	(0.052)	(0.053)	(0.091)
Distance to health care	1,509	0.04	-0.030	-0.065	-0.064	-0.111
		[1.24]	(0.048)	(0.055)	(0.055)	(0.094)
Kms to closest municipality	1,582	0.03	-0.113**	-0.074	-0.096*	-0.167*
		[1.10]	(0.048)	(0.057)	(0.053)	(0.091)
Application to Ownership Programs	1,672	0.12	0.061***	0.045**	0.047**	0.086**
		[0.33]	(0.020)	(0.018)	(0.018)	(0.034)
Application DS1	1,672	0.07	0.016	0.018	0.020	0.036
		[0.26]	(0.015)	(0.015)	(0.015)	(0.027)
Application DS49	1,672	0.07	0.052***	0.035**	0.035**	0.064**
		[0.25]	(0.016)	(0.014)	(0.014)	(0.026)
COVARIATES			YES	NO	YES	YES
SCREENING FE			YES	YES	YES	YES

This table replicates the analysis in Table VIII for the sample of randomized vouchers within the narrower window $W = [-1, 1]$ from elderly rounds. See Table VIII for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

A Appendix: Additional Tables

TABLE A.1. Assignments in Regular and Elderly Rounds

Assignment Date	N (1)	Min Xi (2)	Max Xi (3)	Vouchers (4)	Cutoff (5)
Regular Rounds					
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
Elderly Rounds					
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 summarizes assignment periods. Panel A covers regular rounds, Panel B elderly rounds. For June–October 2019, Columns 1–4 aggregate 16 regional screenings, and Column 5 shows average cutoff.

TABLE A.2. Balance in Score Components in the Evaluation Sample

		Control		Treated		Difference
	N	Mean	SD	Mean	SD	(5)-(3)
	(1)	(2)	(3)	(4)	(5)	(6)
Regular Rounds						
Family size score	926	45.16	5.87	47.80	8.89	2.64
Single parenthood score	926	29.16	5.44	30.71	3.77	1.55
Number of children under 5 score	926	14.63	12.03	21.89	13.06	7.26
Number of children 6 to 18 score	926	9.24	8.09	10.07	10.31	0.83
Number of elderly score	926	0.91	0.87	0.05	0.11	-0.86
Number of people with disability score	926	0.40	0.66	6.05	13.39	5.65
Social vulnerability score	926	175.97	2.62	174.24	6.01	-1.73
Housing vulnerability score	926	13.82	22.60	10.68	20.25	-3.14
Application score	926	292.45	28.46	296.04	27.92	3.59
Elderly Rounds						
Family size score	1,717	40.24	0.41	42.62	2.42	2.39
Single parenthood score	1,717	0.03	0.06	0.00	0.00	-0.03
Number of children under 5 score	1,717	0.00	0.00	0.00	0.00	0.00
Number of children 6 to 18 score	1,717	0.04	0.07	1.14	1.90	1.10
Number of elderly score	1,717	57.31	3.56	56.89	4.26	-0.42
Number of people with disability score	1,717	0.92	0.50	1.02	0.91	0.10
Social vulnerability score	1,717	178.64	0.87	177.72	3.25	-0.92
Housing vulnerability score	1,717	20.77	16.60	22.69	17.98	1.93
Application score	1,717	379.88	0.11	380.22	0.38	0.34

This table reports summary statistics of total application scores and components in the evaluation sample for regular and elderly rounds. Columns 6 shows the difference in means between treatment and control group.

TABLE A.3. Pre-pandemic Outcomes (OLS, ITT, and LATE estimates) for Regular Rounds

Outcome Variable	N (1)	Control Mean [SD] (2)	Voucher Use OLS (3)	Reduced Form ITT (4)	Reduced Form ITT (5)	IV LATE (6)
Household size	925	2.85	0.006	0.112	0.115	0.124
		[1.21]	(0.098)	(0.089)	(0.084)	(0.287)
Number of bedrooms	921	1.76	0.420***	0.215***	0.243***	0.711***
		[0.83]	(0.072)	(0.060)	(0.055)	(0.181)
Number of people per bedroom	921	1.80	-0.370***	-0.134***	-0.165***	-0.591***
		[0.69]	(0.054)	(0.047)	(0.042)	(0.136)
Overcrowing indicator	921	0.12	-0.082***	-0.035*	-0.044**	-0.183***
		[0.33]	(0.021)	(0.021)	(0.021)	(0.070)
Moved to diff. unit	849	0.43	0.208***	0.080**	0.071*	0.263**
		[0.50]	(0.046)	(0.037)	(0.037)	(0.119)
Distance (km)	849	14.07	19.627	6.125	5.483	41.642
		[164.01]	(19.242)	(14.663)	(14.931)	(40.131)
Stayed in 1km radius	849	0.71	-0.157***	-0.074**	-0.070**	-0.241**
		[0.46]	(0.047)	(0.035)	(0.035)	(0.117)
Moved +10km away	849	0.12	0.099***	0.062**	0.055**	0.180**
		[0.33]	(0.038)	(0.027)	(0.027)	(0.090)
Moved to another county	849	0.07	0.048*	0.055**	0.055***	0.158**
		[0.26]	(0.028)	(0.022)	(0.021)	(0.072)
County level poverty	856	-0.05	-0.040	0.021	-0.043	-0.010
		[1.00]	(0.081)	(0.070)	(0.060)	(0.188)
County crime victims	856	0.02	-0.014	-0.001	0.061	0.191
		[0.83]	(0.058)	(0.060)	(0.045)	(0.150)
School Quality	816	0.02	-0.049	-0.027	-0.025	-0.098
		[0.94]	(0.098)	(0.073)	(0.071)	(0.240)
School access	849	-0.08	0.320*	0.249**	0.230**	0.804**
		[0.69]	(0.168)	(0.102)	(0.101)	(0.335)
Distance to early childhood educ.	849	-0.07	0.333**	0.227**	0.193**	0.710**
		[0.76]	(0.149)	(0.093)	(0.090)	(0.301)
Distance to health care	849	-0.05	0.270*	0.179*	0.143	0.499
		[0.78]	(0.153)	(0.097)	(0.096)	(0.316)
Kms to closest municipality	921	0.00	0.056	0.044	-0.003	0.073
		[1.01]	(0.094)	(0.067)	(0.064)	(0.222)
Application to Ownership Programs	926	0.31	0.013	0.016	0.017	0.101
		[0.46]	(0.041)	(0.033)	(0.031)	(0.105)
Application DS1	926	0.23	-0.008	0.007	0.009	0.062
		[0.42]	(0.038)	(0.030)	(0.029)	(0.096)
Application DS49	926	0.12	0.004	0.006	0.005	0.021
		[0.32]	(0.027)	(0.022)	(0.022)	(0.076)
Active ownership savings account	926	0.92	0.008	0.014	0.012	0.032
		[0.27]	(0.024)	(0.019)	(0.018)	(0.062)
COVARIATES			YES	NO	YES	YES
SCREENING FE			YES	YES	YES	YES

This table presents different estimators for the effect of the rental voucher on outcomes measured in December 2019, included in Table VII. Columns 2 reports the control mean and standard deviation (in brackets). Column 3 presents OLS estimates of the effect of voucher use. Columns 4 and 5 report ITT estimates from the RDD research design with and without covariates used in balance tests. Column 6 shows LATE estimates from a two-stage least squares model using the score discontinuity as an instrument for voucher use. All specifications include applicant screenings fixed effects. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE A.4. Pre-pandemic Outcomes (OLS, ITT, and LATE estimates) for Elderly Rounds

Outcome Variable	N	Control Mean [SD] (2)	Voucher Use OLS (2)	Reduced Form ITT (3)	Reduced Form ITT (4)	IV LATE (5)
Household size	1,717	1.60	-0.111**	-0.151***	-0.157***	-0.259***
		[1.11]	(0.043)	(0.057)	(0.051)	(0.084)
Number of bedrooms	1,604	1.35	0.880***	0.443***	0.439***	0.800***
		[0.74]	(0.044)	(0.046)	(0.045)	(0.068)
Number of people per bedroom	1,604	1.24	-0.489***	-0.296***	-0.300***	-0.538***
		[0.59]	(0.024)	(0.032)	(0.027)	(0.041)
Overcrowing indicator	1,604	0.03	-0.020***	-0.014	-0.014	-0.025*
		[0.17]	(0.007)	(0.010)	(0.009)	(0.015)
Moved to diff. unit	1,549	0.34	0.351***	0.261***	0.257***	0.482***
		[0.47]	(0.026)	(0.028)	(0.028)	(0.043)
Distance (km)	1,549	22.37	17.885*	3.795	2.661	10.770
		[150.24]	(9.256)	(8.909)	(8.845)	(14.917)
Stayed in 1km radius	1,549	0.76	-0.283***	-0.195***	-0.194***	-0.371***
		[0.43]	(0.027)	(0.027)	(0.026)	(0.042)
Moved +10km away	1,549	0.11	0.112***	0.081***	0.084***	0.167***
		[0.31]	(0.022)	(0.020)	(0.020)	(0.032)
Moved to another county	1,549	0.09	0.121***	0.048***	0.055***	0.100***
		[0.29]	(0.020)	(0.018)	(0.018)	(0.029)
County level poverty	1,577	-0.03	-0.008	-0.028	-0.050	-0.006
		[0.98]	(0.047)	(0.058)	(0.047)	(0.073)
County crime victims	1,575	-0.01	-0.057	-0.004	-0.007	-0.020
		[0.80]	(0.040)	(0.047)	(0.039)	(0.062)
School Quality	1,509	-0.02	0.015	-0.006	0.000	-0.046
		[0.99]	(0.053)	(0.056)	(0.055)	(0.090)
School access	1,549	0.04	-0.030	-0.070	-0.066	-0.087
		[1.27]	(0.045)	(0.051)	(0.051)	(0.089)
Distance to early childhood educ.	1,549	0.04	-0.039	-0.073	-0.066	-0.079
		[1.24]	(0.046)	(0.051)	(0.051)	(0.088)
Distance to health care	1,549	0.04	-0.025	-0.073	-0.067	-0.086
		[1.24]	(0.047)	(0.053)	(0.053)	(0.091)
Kms to closest municipality	1,624	0.04	-0.113**	-0.083	-0.101*	-0.157*
		[1.11]	(0.048)	(0.056)	(0.052)	(0.085)
Application to Ownership Programs	1,717	0.12	0.060***	0.047***	0.047***	0.077**
		[0.33]	(0.020)	(0.018)	(0.018)	(0.032)
Application DS1	1,717	0.07	0.019	0.022	0.023	0.027
		[0.26]	(0.015)	(0.014)	(0.014)	(0.025)
Application DS49	1,717	0.07	0.049***	0.035**	0.034**	0.069***
		[0.25]	(0.016)	(0.014)	(0.014)	(0.025)
COVARIATES			YES	NO	YES	YES
SCREENING FE			YES	YES	YES	YES

This table replicates the analysis in Table A.3 using data from elderly rounds and outcomes from December 2019 in Table VIII. See Tables A.3 and VIII for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE A.5. Post-pandemic Outcomes (OLS, ITT, and LATE estimates) for Regular Rounds

Outcome Variable	N (1)	Control Mean [SD] (2)	Voucher Use OLS (3)	Reduced Form ITT (4)	Reduced Form ITT (5)	IV LATE (6)
Formal Lease	330	0.73	0.304***	0.127***	0.131***	0.195***
		[0.44]	(0.040)	(0.048)	(0.050)	(0.064)
Total rent (unit)	326	6.80	0.025	-0.094	-0.018	-0.230
		[2.81]	(0.284)	(0.289)	(0.274)	(0.398)
Rent burden (rent paid)	284	0.48	-0.145***	-0.116***	-0.101***	-0.152***
		[0.27]	(0.033)	(0.030)	(0.031)	(0.042)
Employed	344	0.69	-0.008	-0.027	-0.021	-0.034
		[0.47]	(0.064)	(0.054)	(0.056)	(0.088)
Family Income	340	13.42	-0.285	0.388	0.326	0.285
		[5.71]	(0.652)	(0.593)	(0.608)	(0.948)
Debt overload	342	0.69	-0.135*	-0.120**	-0.124**	-0.221**
		[0.46]	(0.069)	(0.057)	(0.058)	(0.090)
Spouse/Partner	339	0.32	0.001	0.098*	0.090	0.164*
		[0.47]	(0.064)	(0.057)	(0.055)	(0.087)
Household Size	428	3.28	-0.310**	-0.075	-0.058	-0.235
		[1.40]	(0.134)	(0.137)	(0.139)	(0.216)
Number of people per bedroom	417	1.58	-0.264***	-0.102	-0.099	-0.148
		[0.69]	(0.072)	(0.068)	(0.070)	(0.106)
Overcrowding indicator	418	0.11	-0.056*	-0.034	-0.033	-0.061
		[0.31]	(0.031)	(0.029)	(0.031)	(0.048)
Number of bedrooms	417	2.24	0.067	0.019	0.022	-0.067
		[0.88]	(0.085)	(0.080)	(0.081)	(0.127)
Laundry Room	415	0.36	0.039	0.019	0.031	0.027
		[0.48]	(0.058)	(0.050)	(0.049)	(0.077)
Kitchen Room	415	0.75	0.130***	0.138***	0.144***	0.207***
		[0.43]	(0.047)	(0.042)	(0.043)	(0.066)
Heat system	415	0.80	0.074**	0.103***	0.092***	0.149***
		[0.40]	(0.031)	(0.033)	(0.030)	(0.049)
Cable, Wifi	412	0.59	0.025	-0.004	-0.024	-0.041
		[0.35]	(0.044)	(0.037)	(0.037)	(0.058)
Satisfaction current housing unit	459	0.77	0.198***	0.066*	0.064	0.100*
		[0.42]	(0.035)	(0.039)	(0.039)	(0.060)
Household income loss after COVID-19	343	0.80	-0.074	-0.067	-0.071	-0.126
		[0.40]	(0.063)	(0.051)	(0.054)	(0.081)
COVID-19: Expenses	340	0.69	-0.040	-0.015	-0.025	-0.024
		[0.46]	(0.066)	(0.055)	(0.056)	(0.087)
COVID-19: Debt	340	0.67	-0.071	-0.139**	-0.153**	-0.256***
		[0.47]	(0.069)	(0.059)	(0.059)	(0.091)
COVID-19: New Income Source	340	0.71	-0.179***	-0.065	-0.077	-0.148*
		[0.46]	(0.066)	(0.056)	(0.057)	(0.086)
COVID-19: Moved out	340	0.07	-0.012	-0.037	-0.031	-0.048
		[0.25]	(0.030)	(0.023)	(0.025)	(0.039)
COVID-19: Others moved in	340	0.06	-0.000	0.036	0.049	0.089*
		[0.23]	(0.035)	(0.033)	(0.032)	(0.052)
COVID-19: Delayed rent payments	340	0.22	-0.171***	-0.108**	-0.109**	-0.160**
		[0.42]	(0.040)	(0.042)	(0.043)	(0.063)
Satisfaction current neighborhood	443	0.82	0.079*	-0.042	-0.043	-0.055
		[0.38]	(0.041)	(0.039)	(0.039)	(0.062)
Positive Ammenities	450	-0.02	-0.128	-0.014	-0.030	0.013
		[1.02]	(0.123)	(0.105)	(0.105)	(0.159)
Negative Ammenities	328	-0.00	-0.205*	0.046	-0.012	-0.004
		[1.01]	(0.123)	(0.115)	(0.115)	(0.177)
Would ask neighbors for childcare	427	0.29	-0.059	-0.100**	-0.101**	-0.160**
		[0.45]	(0.049)	(0.044)	(0.045)	(0.071)
COVARIATES			YES	NO	YES	YES
SCREENING FE			YES	YES	YES	YES

This table replicates the analysis in Table A.3 using survey data from regular rounds and November 2020 outcomes from Table IX. See Table A.3 for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE A.6. Follow Up Sample Attrition in Regular Rounds

	Response Prob. (1)	Rand-t (2)	Response Prob. (3)	Rand-t (4)
Treat	0.089	0.321	0.093	0.300
	(0.309)		(0.284)	
Treat*December 2018	-0.064	0.002	-0.057	0.677
	(0.576)		(0.612)	
Treat*October 2019 (O'Higgins)	-0.025	0.866	-0.043	0.769
	(0.865)		(0.777)	
Treat*October 2019 (Araucania)	-0.032	0.742	-0.021	0.827
	(0.764)		(0.842)	
Treat*October 2019 (Los Lagos)	-0.098	0.595	-0.136	0.438
	(0.563)		(0.420)	
F-Test (p-value)	0.789		0.658	
Rand-t Joint Test (p-value)	0.716		0.597	
Observations	776		776	
COVARIATES	NO		YES	

This table shows the effect of voucher assignment on survey response (R_{i,s_t}), replacing Z_{i,s_t} with R_{i,s_t} in equation 4. Columns 1 and 3 report OLS estimates of δ_{s_t} and standard errors, with and without baseline covariates. Columns 2 and 4 show Fisherian randomization inference p-values (Randomization-t exact test), based on 1,000 iterations using the Stata package randcmd (Young, 2019). The bottom panel presents joint significance test for each $\delta_{s_t} = 0$ using both inference methods. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE A.7. Balance in Baseline Characteristics in Follow-Up Survey Sample

	N (1)	Control Mean (2)	Control SD (3)	Treated Mean (4)	Treated SD (5)	Diff (6)	Balance Test F-test (p) (7)	Balance Test Rand-t (p) (8)
Female	496	0.92	0.28	0.93	0.26	0.01	0.836	0.743
Poor	496	0.20	0.40	0.19	0.39	-0.01	0.729	0.765
Tenant	496	0.85	0.35	0.80	0.40	-0.06	0.683	0.602
Family income (UF)	496	13.47	4.85	13.40	4.77	-0.07	0.693	0.705
Previous app. to ownership subsidy	496	0.13	0.34	0.14	0.34	0.00	0.878	0.869
Geocoded location	496	0.91	0.28	0.93	0.25	0.02	0.818	0.791
Nearby SERVIU (county)	496	0.53	0.50	0.48	0.50	-0.05	0.335	0.782
Saving balance (UF)	496	15.03	14.11	15.70	16.46	0.67	0.869	0.339
Online Application	496	0.40	0.49	0.39	0.49	-0.01	0.799	0.760
Age 25-35	496	0.62	0.49	0.70	0.46	0.08	0.086*	0.082*
Chilean	496	0.92	0.27	0.98	0.15	0.05	0.017**	0.018**
Spouse/partner	496	0.13	0.34	0.11	0.31	-0.02	0.851	0.848
Previous app. in neighborhood (500mts)	496	0.46	0.50	0.49	0.50	0.03	0.541	0.828
Number of bedrooms	496	1.65	0.78	1.63	0.85	-0.02	0.847	0.582
Low-quality housing	496	0.07	0.26	0.04	0.19	-0.04	0.532	0.346
Overcrowding indicator	496	0.05	0.23	0.03	0.16	-0.03	0.332	0.499
County poverty rate	496	0.12	0.06	0.13	0.07	0.01	0.739	0.720
Santiago	496	0.10	0.30	0.08	0.28	-0.02	0.572	0.512
North	496	0.05	0.22	0.04	0.19	-0.01	0.882	0.922
Valparaiso	496	0.09	0.28	0.06	0.24	-0.03	0.623	0.639
Center South	496	0.28	0.45	0.27	0.45	-0.01	0.108	0.105
South	496	0.48	0.50	0.55	0.50	0.07	0.272	0.286
High density county	496	0.42	0.49	0.33	0.47	-0.09	0.060*	0.076*
KM to closest SERVIU	496	18.88	24.04	21.27	25.30	2.39	0.666	0.653
Answered Baseline Survey	496	0.88	0.32	0.86	0.34	-0.02	0.326	0.330
Rent burden	305	0.48	0.32	0.49	0.28	0.01	0.842	0.870
Rent (UF)	394	5.58	3.89	5.39	3.50	-0.19	0.615	0.617
Desire to stay in place	377	0.60	0.49	0.58	0.49	-0.02	0.568	0.588
Satisfied with housing	403	0.67	0.47	0.70	0.46	0.03	0.440	0.443
SCREENING INDICATORS							Yes	Yes
SCREENING INDICATORSxTREAT							No	No
Joint Significance (p)							0.835	0.776

This table replicates the analysis in Table V using the subset of regular applicants who responded the follow up survey. It only presents the first balance test, excluding interaction terms from 3. See Table V for further details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

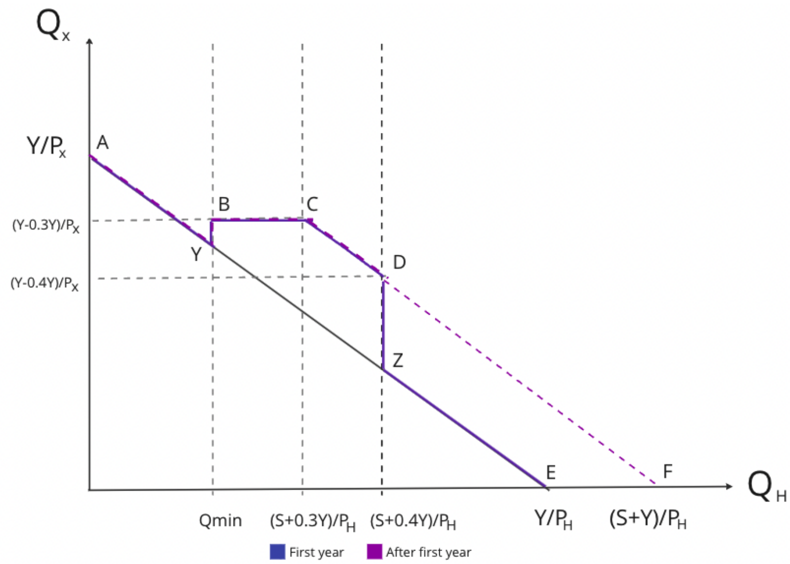
TABLE A.8. Pre-pandemic Outcomes (OLS, ITT, and LATE estimates) in Survey Data

Outcome Variable	N (1)	Control Mean [SD] (2)	Voucher Use OLS (3)	Reduced Form ITT (4)	Reduced Form ITT (5)	IV LATE (6)
Household size	496	3.04	-0.203	0.042	0.076	0.236
		[1.24]	(0.140)	(0.124)	(0.124)	(0.376)
Number of bedrooms	495	1.96	0.535***	0.161*	0.189**	0.584**
		[0.88]	(0.114)	(0.082)	(0.081)	(0.239)
Number of people per bedroom	495	1.72	-0.515***	-0.119*	-0.134**	-0.413**
		[0.70]	(0.076)	(0.066)	(0.063)	(0.181)
Overcrowing indicator	495	0.11	-0.081***	-0.019	-0.020	-0.062
		[0.32]	(0.024)	(0.028)	(0.028)	(0.084)
Moved to diff. unit	457	0.44	0.255***	0.072	0.071	0.222
		[0.50]	(0.071)	(0.050)	(0.051)	(0.150)
Distance (km)	457	7.91	11.205	9.166	10.190	31.875
		[36.58]	(10.507)	(7.588)	(7.569)	(23.340)
Stayed in 1km radius	457	0.69	-0.132*	-0.070	-0.070	-0.220
		[0.46]	(0.074)	(0.047)	(0.047)	(0.145)
Moved +10km away	457	0.12	0.038	0.060*	0.058	0.183*
		[0.32]	(0.054)	(0.036)	(0.035)	(0.107)
Moved to another county	457	0.08	0.017	0.043	0.049*	0.152*
		[0.27]	(0.041)	(0.029)	(0.028)	(0.085)
County level poverty	462	-0.05	-0.020	0.010	-0.063	-0.197
		[0.95]	(0.141)	(0.092)	(0.079)	(0.220)
County crime victims	462	0.04	0.053	-0.009	0.080	0.250
		[0.82]	(0.094)	(0.079)	(0.060)	(0.186)
School Quality	438	0.06	-0.082	-0.089	-0.080	-0.248
		[0.94]	(0.137)	(0.100)	(0.098)	(0.292)
School access	457	-0.03	-0.130	0.232	0.199	0.623
		[0.84]	(0.130)	(0.165)	(0.139)	(0.435)
Distance to early childhood educ.	457	-0.03	-0.131	0.162	0.129	0.403
		[0.86]	(0.113)	(0.137)	(0.118)	(0.367)
Distance to health care	457	-0.00	-0.200*	0.161	0.120	0.374
		[0.89]	(0.115)	(0.153)	(0.131)	(0.408)
Kms to closest municipality	495	0.01	-0.181	0.017	-0.012	-0.038
		[1.08]	(0.112)	(0.095)	(0.085)	(0.255)
Application to Ownership Programs	496	0.30	0.076	0.006	0.000	0.001
		[0.46]	(0.062)	(0.044)	(0.041)	(0.122)
Application DS1	496	0.24	0.037	-0.030	-0.039	-0.119
		[0.43]	(0.056)	(0.040)	(0.037)	(0.110)
Application DS49	496	0.11	0.044	0.024	0.023	0.071
		[0.31]	(0.050)	(0.030)	(0.030)	(0.089)
Active ownership savings account	496	0.91	0.037	0.012	0.008	0.024
		[0.28]	(0.034)	(0.025)	(0.025)	(0.074)
COVARIATES			YES	NO	YES	YES
SCREENING FE			YES	YES	YES	YES

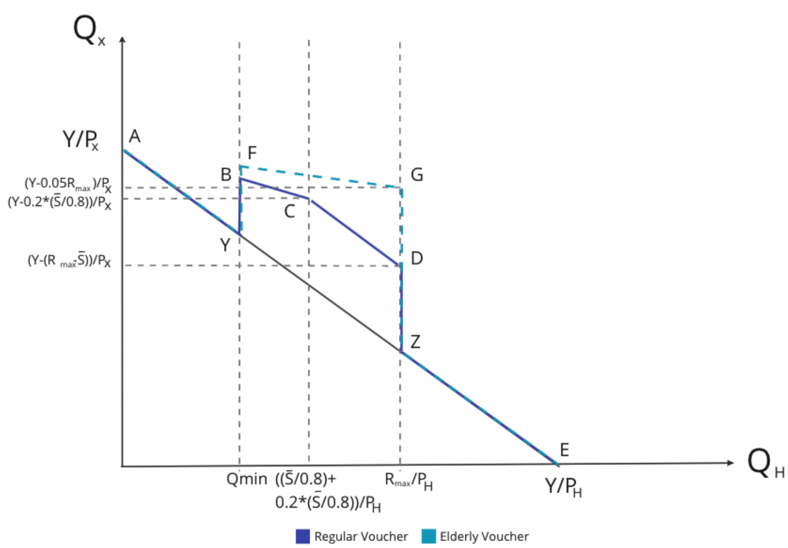
This table replicates the analysis in Table A.3 using the subset of follow up survey respondents. See Table A.3 for details.

B Supplemental Appendix

FIGURE B.1. Budget Set in the Rental Voucher Program



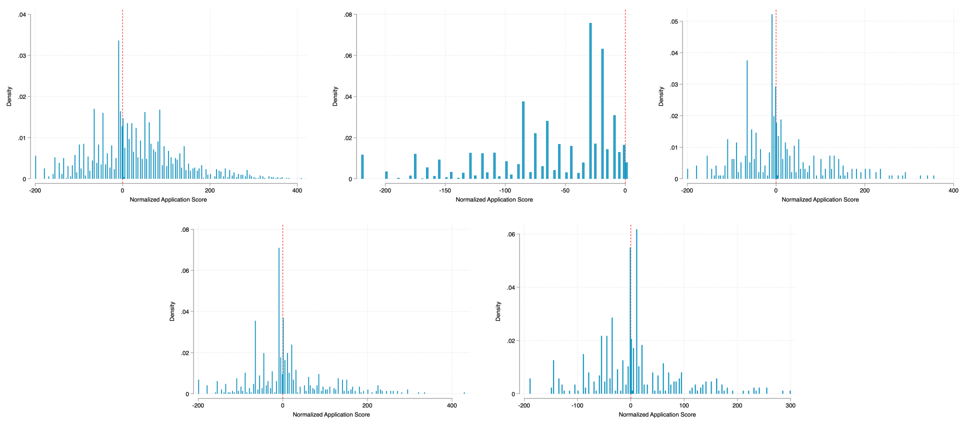
(a) US Housing Choice Voucher Program



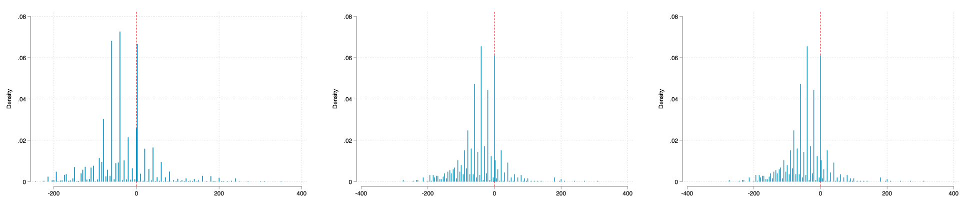
(b) Chilean Rental Voucher

The figure illustrates how the introduction of the rental voucher changes the budget set in the U.S. (a) and Chile (b). In the U.S., recipients initially pay 30% of income (Y) toward rent in units meeting a minimum quality threshold ($Q_{\min}$). The government covers the gap between Fair Market Rent (FMR) and 30% of family income, denoted by S. In the first year, households face the budget constraint AYBCDZE, as rent payments cannot exceed 40% of income. In subsequent years, this restriction is lifted, expanding the budget set to AYBCDF. In Chile, the regular voucher provides a fixed subsidy, capped at 80% of rent, and can only be used in units below the maximum allowable rent ($R_{\max}$), generating the budget set AYBCDZE. In elderly rounds, recipients pay 5% of rent in qualifying units. The resulting budget set is AYFGZE. In both voucher types, doubled-up households are assumed to start below $Q_{\min}$.

FIGURE B.2. Normalized Score Distribution of Applicant Screenings in W_0



(a) Regular Applicant Screenings



(b) Elderly Applicants Screenings

This figure presents the normalized score distribution by applicant screening in the evaluation sample W_0 in regular rounds (a) and elderly rounds (b).

TABLE B.1. Regular recipients in W_0 vs. All Regular Recipients in Selected Screenings s_t

	All			Sample			Difference
	N	Mean	SD	N	Mean	SD	(5)-(2)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	2,354	0.85	0.35	348	0.90	0.30	0.05
Poor	2,354	0.31	0.46	348	0.23	0.42	-0.09
Tenant	2,354	0.68	0.46	348	0.78	0.41	0.10
Family income (UF)	2,354	14.13	5.28	348	13.22	4.95	-0.90
Previous app. to ownership subsidy	2,354	0.18	0.38	348	0.16	0.36	-0.02
Geocoded location	2,354	0.91	0.29	348	0.92	0.28	0.01
Nearby SERVIU (county)	2,354	0.49	0.50	348	0.46	0.50	-0.03
Saving balance (UF)	2,354	15.04	14.18	348	15.91	15.74	0.87
Online Application	2,354	0.34	0.47	348	0.36	0.48	0.02
Age 25-35	2,354	0.58	0.49	348	0.65	0.48	0.08
Chilean	2,354	0.94	0.24	348	0.97	0.17	0.03
Spouse/partner	2,354	0.23	0.42	348	0.12	0.33	-0.10
Previous app. in neighborhood (500mts)	2,354	0.51	0.50	348	0.44	0.50	-0.07
Low-quality housing	2,354	0.17	0.37	348	0.06	0.24	-0.10
Overcrowding indicator	2,348	0.41	0.49	348	0.03	0.18	-0.38
County poverty rate	2,354	0.11	0.06	348	0.13	0.07	0.01
Santiago	2,354	0.16	0.37	348	0.10	0.30	-0.06
North	2,354	0.07	0.25	348	0.03	0.18	-0.04
Valparaiso	2,354	0.09	0.28	348	0.06	0.23	-0.03
Center South	2,354	0.31	0.46	348	0.30	0.46	-0.01
South	2,354	0.38	0.48	348	0.51	0.50	0.13
High density county	2,354	0.43	0.50	348	0.35	0.48	-0.08
KM to closest SERVIU	2,354	16.58	20.84	348	20.37	25.42	3.78
Answered Baseline Survey	2,354	0.69	0.46	348	0.73	0.44	0.04
Rent burden	2,091	0.54	0.13	263	0.50	0.22	-0.04
Rent (UF)	2,285	5.43	1.61	312	5.34	2.71	-0.10
Desire to stay in place	1,472	0.55	0.50	214	0.56	0.50	0.01
Satisfied with housing	1,515	0.59	0.49	232	0.66	0.47	0.08
Score Components and Total Score							
Family size score	2,354	76.52	35.40	348	50.80	22.58	-25.71
Single parenthood score	2,354	22.20	16.86	348	29.07	13.15	6.87
Number of children under 5 score	2,354	19.87	17.43	348	22.50	13.01	2.63
Number of children 6 to 18 score	2,354	17.21	16.32	348	10.63	11.40	-6.58
Number of people with disability score	2,354	1.75	7.13	348	6.98	12.70	5.24
Number of elderly score	2,354	0.61	4.99	348	0.09	1.61	-0.53
Social vulnerability score	2,354	173.54	18.77	348	172.37	19.17	-1.17
Housing vulnerability score	2,354	49.86	57.03	348	13.05	22.85	-36.81
Previous application score	2,354	4.67	8.99	348	5.80	9.70	1.13
Application score	2,354	364.99	75.78	348	300.78	24.73	-64.21

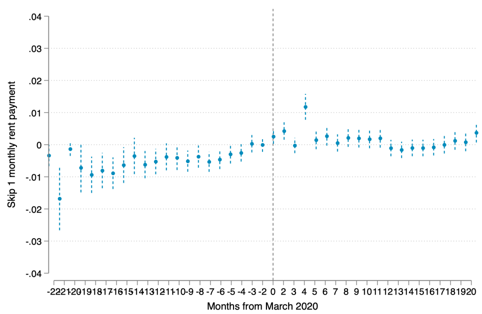
This table shows summary statistics for all regular recipients in screenings s_t selected into the evaluation sample (Columns 1-3) and for those included in the evaluation sample (Columns 4-6). Column 7 reports the difference in means between the two groups.

TABLE B.2. Elderly recipients in W_0 vs. All Elderly Recipients in Selected Screenings s_t

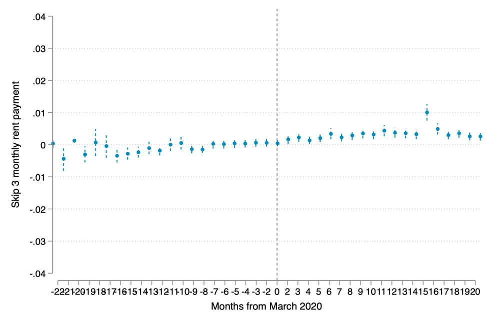
	All			Sample			Difference
	N	Mean	SD	N	Mean	SD	(2)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	3,035	0.55	0.50	973	0.61	0.49	-0.06
Poor	3,035	0.59	0.49	973	0.52	0.50	0.07
Tenant	3,035	0.48	0.50	973	0.52	0.50	-0.03
Family income (UF)	3,035	6.38	3.31	973	6.35	2.88	0.04
Previous app. to ownership subsidy	3,035	0.06	0.24	973	0.05	0.23	0.01
Geocoded location	3,035	0.90	0.31	973	0.89	0.32	0.01
Spouse/partner	3,035	0.41	0.49	973	0.43	0.50	-0.02
Nearby SERVIU (county)	3,035	0.55	0.50	973	0.56	0.50	-0.01
Age 60-75	3,035	0.52	0.50	973	0.41	0.49	0.12
Chilean	3,035	0.98	0.13	973	0.99	0.12	-0.00
Previous app. in neighborhood (500mts)	3,035	0.60	0.49	973	0.71	0.45	-0.11
Low-quality housing	3,035	0.33	0.47	973	0.19	0.39	0.14
Overcrowding indicator	2,872	0.11	0.32	973	0.01	0.11	0.10
County poverty rate	3,035	0.09	0.05	973	0.09	0.05	0.00
Santiago	3,035	0.22	0.42	973	0.26	0.44	-0.03
North	3,035	0.14	0.34	973	0.13	0.34	0.00
Valparaiso	3,035	0.24	0.43	973	0.23	0.42	0.01
Center South	3,035	0.23	0.42	973	0.21	0.41	0.01
South	3,035	0.18	0.38	973	0.17	0.38	0.01
High density county	3,035	0.46	0.50	973	0.50	0.50	-0.03
KM to closest SERVIU	3,035	12.65	17.53	973	12.37	17.93	0.28
Score Components and Total Score							
Family size score	3,035	45.71	20.14	973	40.95	6.60	4.76
Single parenthood score	3,035	0.18	2.53	973	0.00	0.00	0.18
Number of children under 5 score	3,035	0.46	4.09	973	0.00	0.00	0.46
Number of children 6 to 18 score	3,035	1.46	6.59	973	0.16	2.02	1.30
Number of people with disability score	3,035	3.64	10.50	973	0.34	3.17	3.30
Number of elderly score	3,035	57.27	9.66	973	59.45	4.68	-2.17
Social vulnerability score	3,035	178.37	9.94	973	179.72	4.07	-1.35
Housing vulnerability score	3,035	51.13	56.86	973	14.41	22.99	36.72
Previous application score	3,035	3.28	8.35	973	0.82	4.27	2.46
Application score	3,035	419.39	50.76	973	380.02	0.32	39.37

This table replicates the analysis in Table B.1 using elderly rounds data. See Table B.1 for details.

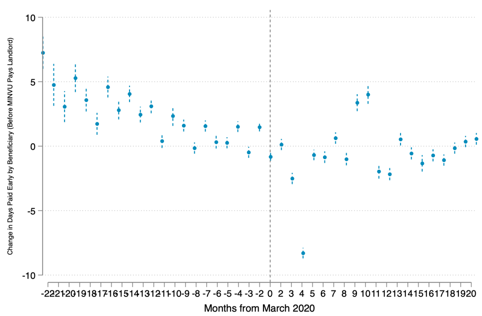
FIGURE B.3. Rent Co-Payment Behavior (Administrative Data)



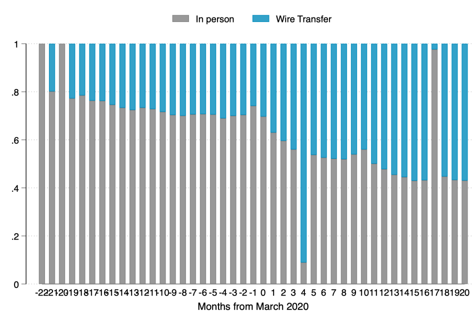
(a) One month gap



(b) Three months gap



(c) Payment Timing



(d) Payment Method

This Figure analyzes payment behavior during the period before and after the onset of the COVID-19 pandemic ($t = 0$ corresponds to March 20th 2020). Panel (a) reports the probability of skipping one monthly payment; Panel (b), the probability of skipping three consecutive payments; Panel (c) shows overall delays in rent co-payment, and Panel (d) presents the distribution of payment methods used by voucher holders.

TABLE B.3. Balance in Baseline Characteristics in $W = [-1, 1]$ (Regular Rounds)

		Control		Treated		Balance Test		
	N	Mean	SD	Mean	SD	Diff	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	539	0.97	0.18	0.98	0.15	0.01	0.577	0.592
Poor	539	0.16	0.37	0.20	0.40	0.04	0.283	0.292
Tenant	539	0.76	0.43	0.72	0.45	-0.04	0.592	0.607
Family income (UF)	539	12.39	3.58	12.18	3.89	-0.21	0.321	0.322
Previous app. to ownership subsidy	539	0.14	0.35	0.16	0.37	0.01	0.373	0.588
Geocoded location	539	0.88	0.33	0.91	0.29	0.03	0.907	0.367
Nearby SERVIU (county)	539	0.54	0.50	0.49	0.50	-0.05	0.748	0.981
Saving balance (UF)	539	16.04	14.37	16.00	15.13	-0.04	0.611	0.731
Online Application	539	0.42	0.49	0.41	0.49	-0.00	0.519	0.497
Age 25-35	539	0.67	0.47	0.68	0.47	0.01	0.610	0.569
Chilean	539	0.94	0.23	0.97	0.18	0.02	0.144	0.138
Spouse/partner	539	0.05	0.21	0.04	0.19	-0.01	0.933	0.845
Previous app. in neighborhood (500mts)	539	0.42	0.49	0.50	0.50	0.08	0.211	0.814
Number of Bedrooms	539	1.42	0.71	1.51	0.77	0.09	0.826	0.864
Low-quality housing	539	0.13	0.34	0.07	0.25	-0.06	0.777	0.216
County poverty rate	539	0.11	0.06	0.12	0.06	0.01	0.969	0.972
Santiago	539	0.17	0.37	0.13	0.33	-0.04	0.443	0.421
North	539	0.09	0.29	0.04	0.19	-0.05	0.093*	0.073*
Valparaiso	539	0.10	0.30	0.08	0.27	-0.02	0.615	0.618
Center South	539	0.30	0.46	0.29	0.45	-0.01	0.109	0.127
South	539	0.35	0.48	0.47	0.50	0.12	0.582	0.594
High density county	539	0.39	0.49	0.38	0.49	-0.01	0.928	0.923
KM to closest SERVIU	539	15.75	19.10	17.62	22.44	1.87	0.516	0.502
Answered Baseline Survey	539	0.75	0.44	0.81	0.40	0.06	0.130	0.113
Rent burden	257	0.48	0.26	0.53	0.28	0.04	0.097*	0.099*
Rent (UF)	355	5.35	3.18	5.38	3.53	0.03	0.952	0.947
Desire to stay in place	355	0.54	0.50	0.54	0.50	0.00	0.869	0.867
Satisfied with housing	380	0.64	0.48	0.65	0.48	0.01	0.835	0.824
SCREENING INDICATORS							Yes	Yes
SCREENING INDICATORSxTREAT							No	No
Joint Significance F-Test (p)							0.765	0.844

This table presents summary statistics and balance tests between treatment and control groups in the sample of randomized vouchers within the narrower window $W = [-1, 1]$ from regular rounds. Columns 1-5 report baseline characteristics. Columns 7-8 show results from the first balance from equation 3 using, respectively, large-sample inference (F-test) and Fisherian randomization inference p-values (Randomization-t exact test), computed using 1,000 iterations in the Stata package randcmd (Young, 2019). The bottom panel reports joint significance tests from regressing treatment on baseline covariates using both inference methods. See Section 5 and Table VII for details. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

TABLE B.4. Balance in Baseline Characteristics in $W = [-1, 1]$ (Elderly Rounds)

		Control		Treated			Balance Test	
	N	Mean	SD	Mean	SD	Diff	F-test (p)	Rand-t (p)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	1,672	0.61	0.49	0.61	0.49	-0.00	0.540	0.557
Poor	1,672	0.57	0.50	0.52	0.50	-0.05	0.114	0.114
Tenant	1,672	0.55	0.50	0.51	0.50	-0.04	0.885	0.898
Family income per capita (UF)	1,672	5.40	2.02	5.57	2.15	0.16	0.165	0.427
Previous app. to ownership subsidy	1,672	0.06	0.23	0.05	0.22	-0.00	0.779	0.775
Geocoded location	1,672	0.92	0.27	0.89	0.32	-0.04	0.949	0.966
Spouse/partner	1,672	0.39	0.49	0.42	0.49	0.04	0.907	0.892
Nearby SERVIU (county)	1,672	0.51	0.50	0.56	0.50	0.05	0.027**	0.030**
Age 60-75	1,672	0.57	0.50	0.40	0.49	-0.18	0.853	0.876
Chilean	1,672	0.98	0.13	0.99	0.12	0.00	0.666	0.685
Previous app. in neighborhood (500mts)	1,672	0.60	0.49	0.72	0.45	0.11	0.170	0.151
Number of Bedrooms	1,672	1.33	0.65	1.28	0.60	-0.05	0.018**	0.026**
Low-quality housing	1,672	0.16	0.37	0.19	0.39	0.03	0.320	0.335
Overcrowding indicator	1,672	0.01	0.11	0.01	0.09	-0.01	0.301	0.331
County poverty rate	1,672	0.08	0.05	0.09	0.05	0.00	0.762	0.751
Santiago	1,672	0.23	0.42	0.26	0.44	0.03	0.087*	0.086*
North	1,672	0.09	0.29	0.13	0.34	0.04	0.208	0.225
Valparaiso	1,672	0.36	0.48	0.22	0.42	-0.14	0.656	0.611
Center South	1,672	0.20	0.40	0.21	0.41	0.01	0.622	0.619
South	1,672	0.12	0.33	0.17	0.38	0.05	0.266	0.260
High density county	1,672	0.51	0.50	0.49	0.50	-0.02	0.442	0.475
KM to closest SERVIU	1,672	12.74	17.13	12.06	17.76	-0.68	0.193	0.171
SCREENING INDICATORS							Yes	Yes
SCREENING INDICATORSxTREAT							No	No
Joint Significance F-Test (p)							0.289	0.356

This table replicates the analysis in Table B.3 using the sample of randomized vouchers within the narrower window $W = [-1, 1]$ from elderly rounds. See Table B.3 and VIII for details.