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Reducing Incomplete Take-Up Through Digital Innovation in Latin America

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Abstract

I develop a welfare-based framework that links a data-driven incremental innovation cycle to a take-up model and use it to study how digital tools can raise participation and welfare in rental-voucher programs. The welfare effects of digital interventions hinge on targeting and on the distribution of frictions and fiscal externalities across applicant types. I apply the framework to Chile's rental-voucher program using a large randomized evaluation (N=11,149) of a digital counseling platform to increase voucher utilization. The evaluation randomized (i) basic program information, (ii) motivational neighborhood messaging, and (iii) comprehensive neighborhood search tools. Using administrative and survey data with machine learning, I show that information frictions (assignment timing, administrative access), supply constraints (landlords, geography, tenure), and demand factors (savings, income, family composition) predict utilization; yet unassisted utilization is only moderately predictable, indicating important post-assignment frictions. Causal forests reveal substantial heterogeneity: basic information helps economically vulnerable households with lower supply barriers; motivational messages mainly aid already-engaged renters; comprehensive tools have the broadest—and growing—reach. Counseling increased utilization without raising rents and modestly improved neighborhood amenities, at the cost of longer search. Results support pairing universal basic information with widely available search tools and complementary supports.

JEL classificattions: I38, H53, R38, O33, O54

Keywords: Social assistance take-up; Housing voucher programs; Online counseling; Information frictions; Latin America and the Caribbean.

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1 Introduction

Many families fail to use benefits available to them, forgoing substantial resources. Incomplete take-up limits the welfare gains from social assistance by inducing misallocation and underserving the most vulnerable (Ko and Moffitt, 2024; Hoynes, Joyce and Waters, 2024; Bhargava and Manoli, 2015). In high-income settings, evidence points to multiple barriers: transaction costs, limited information about benefits and procedures, stigma, and other behavioral frictions such as inattention or misperceived costs or benefits (Finkelstein and Notowidigdo, 2019).

Reducing incomplete take-up in middle-income countries poses additional challenges (Ko and Moffitt, 2024). Many countries lack reliable socioeconomic data for targeting; administrative capacity is weaker; and large agricultural and informal sectors complicate income-based eligibility. Governments often rely on geographic targeting and local implementers to identify and enroll eligible households.[1] Addressing these infrastructural gaps is essential for scalable, inclusive interventions that can enhance participation.

I examine how artificial intelligence (AI) and digital tools can reduce incomplete take-up in social assistance while improving welfare in Latin America. Digital tools can directly lower participation frictions—by simplifying applications or providing supportive environments that mitigate psychological barriers—or indirectly improve design and targeting (Bennett, 2024; Athey, Keleher and Spiess, 2025; Haushofer et al., 2022; Banerjee et al., 2024). In high-income countries, such tools have improved individual decisions, market functioning, and public service delivery (Heller and Kessler, 2024; Chan and Fan, 2023; Giannella et al., 2024; Goldin et al., 2022). Yet limited connectivity and digital literacy in low-income and rural populations constrain effectiveness and can exacerbate inequalities if not addressed (Cristia and Vlaicu, 2022; Nicolás and Sampaio, 2024; Palacio Ludeña, 2021; Zuger and Asghari, 2024), highlighting the importance of combining these tools with human-centered approaches (Ajzenman et al., 2024; Castell et al., 2025; Bearson and Sunstein, 2025).

I develop a conceptual framework that integrates the data-driven incremental innovation cycle of Athey (2025) with the take-up model of Finkelstein and Notowidigdo (2019), adapted to Latin America's fiscal and digital constraints. The framework guides the design of digital innovations—

[1] A 2019 multi-country survey in Latin America found that roughly half of households below the poverty line received some cash transfer, while about 40% of beneficiaries were above the poverty line (Stampini, Medellín and Ibarrarán, 2023).

for example, personalized outreach, simplified applications, and automated reminders—aimed at raising take-up and welfare. With behavioral biases, the welfare impact of interventions that increase participation depends on three components: (i) the distribution of frictions across types, (ii) the selection of who is induced to participate (targeting), and (iii) type-specific fiscal externalities. Interventions that shift participation toward high-gain, low-fiscal-cost types raise welfare; those that primarily enroll high-friction, low-benefit types may not.

I apply the framework to rental-voucher programs. Unlike standard transfers, vouchers entail a two-stage take-up process: households must (i) apply and be awarded a voucher and (ii) utilize it (or lease up) by navigating private rental markets, securing a landlord willing to accept the subsidy, and signing a formal lease. This second stage introduces supply-side constraints and magnifies behavioral frictions, particularly in tight markets (Phillips, 2020; Aliprantis, Martin and Tauber, 2022). I focus on the utilization margin, using Chile's national voucher program as a case study.

I leverage rich administrative, survey, and experimental data from a large-scale field experiment of an online counseling platform, *Aprendo y Arriendo* ($A\&A$), that provided digital assistance to 11,149 voucher recipients via a control group and three treatment arms: (1) basic information about program rules and procedures, (2) motivational information encouraging consideration of neighborhood characteristics, and (3) comprehensive neighborhood search tools (including an interactive amenity map). The control group received business-as-usual vouchers without digital assistance.

I extend the original impact evaluation of *Aprendo y Arriendo* by Celhay and Selman (2023) in two directions aligned with the welfare framework. First, I study barriers to utilization among the control group using machine learning to identify baseline predictors of lease attempts (successful and unsuccessful) and successful lease-ups four and twenty-four months post-treatment. Information frictions (assignment timing, administrative access), supply-side frictions (landlord relationships, tenure, geography, unit quality), and demand-side barriers (savings, income, family composition) all predict utilization—although demand-side barriers operate through complex interactions. Despite rich baseline data, random-forest models achieve only moderate predictive power (AUC 58–62%), indicating that post-assignment factors not observable at baseline are consequential. Second, I estimate heterogeneous treatment effects using causal machine learning to identify which families benefited most from each counseling type. In the short term, assign-

ment timing is the strongest common predictor of responsiveness—consistent with an information/awareness channel—while benefit profiles diverge across arms and over time: basic information helps economically vulnerable families with lower mobility costs and supply constraints; motivational messaging has weaker, selective effects (primarily among already-engaged renters); and comprehensive search tools reach broader groups, albeit with longer searches.

The results suggest that universal basic information, complemented by broadly available comprehensive search tools, can increase utilization and generate welfare gains, while binding resource and supply constraints require complementary policies.

This research contributes to the literature on incomplete take-up in social programs (Arbogast, Chorniy and Currie, 2022; Finkelstein and Notowidigdo, 2019; Banerjee et al., 2024; Moffitt, 1983; Bhargava and Manoli, 2015; Chareyron, Domingues and Lieno-Gaillardon, 2021; Currie, 2006; Deshpande and Li, 2019; Mani et al., 2013; Bertrand, Mullainathan and Shafir, 2004; Carneiro, Galasso and Ginja, 2019; Bearson and Sunstein, 2025; Giannella et al., 2024; Dahl, 2020; Carneiro et al., 2016). I extend the framework in Finkelstein and Notowidigdo (2019) to a Latin American setting and apply it to a distinct policy domain—rental-voucher programs. The paper also contributes to rental-voucher policy design, a literature largely centered on the U.S. Housing Choice Voucher Program (HCVP) and mobility programs targeting opportunity moves (Schwartz, Mihaly and Gala, 2017; DeLuca and Rosenblatt, 2017; Bergman, Denning and Manoli, 2019; Kling, Liebman and Katz, 2007; Collinson and Ganong, 2018; Ellen, O'Regan and Harwood, 2023; Galiani, Murphy and Pantano, 2015; Geyer, Dastrup and Finkel, 2019; Reina and Aiken, 2022; Bergman, Chan and Kapor, 2020; Bergman et al., 2019; Aliprantis, Martin and Tauber, 2022; Shroder, 2002), and to the small body of work on rental vouchers in middle-income countries (Selman, 2025). By documenting barriers to voucher utilization and heterogeneity in the impacts of digital assistance, I provide new evidence to inform the design of unconditional rental-voucher programs.

The remainder of the paper proceeds as follows. Section 2 develops the conceptual framework integrating incremental innovation with welfare analysis of take-up behavior. Section 3 applies the framework to Chile's rental-voucher program and describes the *Aprendo y Arriendo* digital platform. Section 4 presents the data. Section 5 outlines the empirical strategy. Section 6 reports results in three parts: barriers to utilization in the control group, impacts and mechanisms of digital assistance, and treatment-effect heterogeneity. Section 7 discusses implications for the design and targeting of digital interventions in Latin America.

2 Conceptual Framework

This section develops a conceptual framework to analyze the welfare implications of digital interventions aimed at increasing participation in social programs. The approach combines the data-driven incremental innovation cycle proposed by Athey (2025) with the economic model of take-up behavior developed by Finkelstein and Notowidigdo (2019). The goal is to guide the design of digital innovations—such as personalized outreach, simplified applications, or automated reminders—that reduce incomplete take-up and generate welfare gains. I adapt this combined framework to the context of Latin America, emphasizing low fiscal capacity, behavioral frictions, and digital inequalities.

The data-driven incremental innovation cycle characterizes an iterative process through which digital tools and data are used to refine product design over time. Each stage is informed by economic theory and supported by modern causal inference and econometric techniques using machine learning. The approach facilitates scalable, low-cost experimentation and promotes rapid learning and adaptation. This is key to track winners and losers from digital advances and implement policies that reduce, not exacerbate, digital inequalities.

The model developed by Finkelstein and Notowidigdo (2019) demonstrates that behavioral frictions can distort application decisions. When this occurs, the private welfare gains from efforts to increase take-up depend on the magnitude of individuals' behavioral biases. These biases may differ in size and sign across different types of individuals. Moreover, holding the induced change in participation fixed, the welfare implications of an intervention's targeting depend on the relative behavioral biases and fiscal externalities across types. While the model yields ambiguous theoretical predictions regarding the welfare impact of any particular intervention, these can be assessed empirically.

2.1 The Innovation Cycle

The cycle can be divided into two phases, each involving multiple steps (Figure 1). The initial design phase begins with the identification of a specific problem, accompanied by a detailed analysis of the institutional context and the constraints, incentives, and behavioral biases faced by key stakeholders. This phase entails diagnosing underlying frictions, market failures, and inefficiencies. It also involves formulating a theory of change that articulates the potential direct and

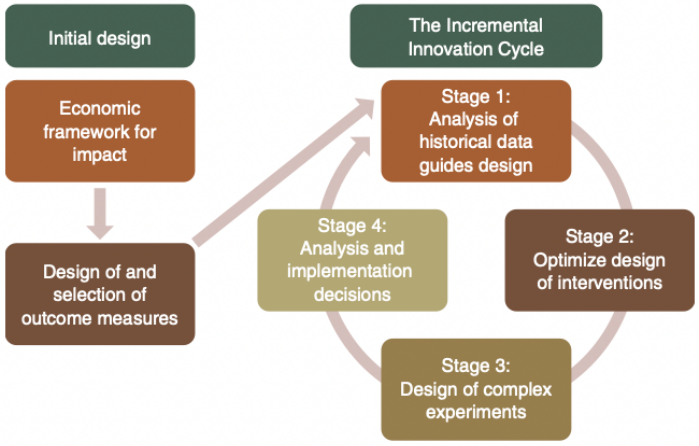


Figure 1. Data-Driven Incremental Innovation Cycle

This figure presents the data-driven incremental innovation cycle. Source: Athey (2025).

indirect effects of the initial product and subsequent modifications. At this stage, high-level goals are translated into measurable outcomes, and considerations of data availability, quality, and collection costs are explicitly incorporated into the design.

The output of this phase is an initial digital tool or intervention, designed to address the identified problem, and the establishment of an organizational infrastructure capable of implementing, evaluating, and collecting relevant data on outcomes. Once this initial product has been deployed and evaluated, the process moves into the second phase: the incremental innovation cycle.

This second phase follows a four-step iterative procedure. It begins by re-analyzing data from the initial intervention—now treated as historical data—to generate hypotheses about which features can be improved. At this stage, economic theory is used to model expected effects of potential changes, and recent advances in machine learning and causal inference offer tools to analyze treatment-effect heterogeneity (e.g., conditional average treatment effects, or CATEs), optimal treatment assignment rules (e.g., targeting optimality criteria), and trade-offs in the presence of competing objectives (e.g., via policy possibility frontiers).

Based on these insights, a new set of features or interventions is developed and tested. The cycle proceeds with experimental evaluation, followed by empirical analysis of the new data, which informs the next round of iterative improvement. This feedback loop enables continuous refinement and scaling of interventions, grounded in both empirical evidence and theoretical rigor.

Combining a data-driven incremental innovation cycle with the welfare model of Finkelstein and Notowidigdo (2019) provides a roadmap for designing digital interventions that reduce incomplete take-up without leaving the most in need behind. The idea is to iterate—pilot small design-and-delivery changes, measure who benefits (and who does not), and update accordingly—while tracking welfare effects across groups over time. This keeps welfare outcomes at the core of the innovation process and helps ensure that gains do not accrue only to easier-to-reach households.

2.2 Model

There are two types of taxpayers, high- and low-skill, $j \in \{L, H\}$. Each type has an unobserved wage θ_j, with $\theta_H > \theta_L$. In the original model, individuals choose hours of work ($\theta_j h_j$) and whether to apply to a transfer program that provides benefits B if income is below an earnings cutoff r^*. In middle-income countries, low-income families do not pay taxes and have very low labor supply elasticity (Banerjee et al., 2024). In this context, hours worked for those who apply (A_j) and do not apply ($\neg A$) are the same ($h_j^A = h_j^{\neg A}$); therefore, the net-of-tax earnings is $y_j \equiv \theta_j h_j - \tau(\theta_j h_j) = \theta_j h_j$, with $y_j^A = y_j^{\neg A}$.[2]

Individuals derive utility from consumption $u(y_j)$, and disutility from the hours worked ($v(h_j)$) and from applying to the program. In this setting, work effort is unaffected by application status, so $v(h_j^A) = v(h_j^{\neg A})$. Application disutility captures time and effort spent compiling documents, filling out forms, attending interviews, and experiencing stigma. This disutility is represented by $\bar{\Lambda}\kappa_j + c_j + \delta_j$, where:

- $\bar{\Lambda}$ is a common "ordeal" parameter set by the planner;
- κ_j is the variation in utility cost with $\bar{\Lambda}$ for each type j (type j's sensitivity to the ordeal);
- $c \sim f_j(c)$ is an individual-specific utility cost of applying and is distributed according to a type-specific distribution $f_j(c)$;
- δ_j captures misperception of utility costs.

Individuals may also misperceive program benefits, captured as $(1 + \varepsilon_j)B$, where $\varepsilon_j < 0$ (with $\varepsilon_j = \delta_j = 0$ in the neoclassical benchmark).

[2]These assumptions follow the empirical application in Finkelstein and Notowidigdo (2019), who apply the model to study incomplete take-up of the Supplemental Nutrition Assistance Program (SNAP) program among elderly individuals. Their experimental intervention reduced private application costs via information alone or information plus assistance for eligible applicants. In their empirical adaptation, they assume the intervention does not affect earnings, and fiscal externalities arise from application/administrative costs rather than endogenous earnings responses.

Digital tools can lower $\bar{\Lambda}$ (e.g., streamlining applications, automating document verification, or providing real-time assistance), shift ε_j toward zero by providing tailored information about program benefits or eligibility, and reduce δ_j by addressing stigma, procrastination, or cognitive overload via personalized nudges or supportive messaging.

In this setting, an individual type j chooses to apply if the expected utility of applying (A) is greater than the utility of not applying $(\neg A)$:

$$u(y_j^A + (1+\varepsilon_j)B) - (\bar{\Lambda}\kappa_j + c_j + \delta_j) > u(y_j^{\neg A}) \tag{2.1}$$

The number of applicants from type j individuals, or take-up $A_j = F_j(c^*)$, depend on c_j^*: the threshold level of c^* such that j decides to apply if $c < c^*$.

$$c_j^* = u(y_j^A + (1+\varepsilon_j)B) - \bar{\Lambda}\kappa_j - \delta_j - u(y_j^{\neg A}) \tag{2.2}$$

Equation 2.2 implies that providing information about benefits or costs, or reducing transaction costs directly—that is, increasing ε_j or decreasing δ_j or $\bar{\Lambda}$—will, respectively, increase c^* (and A_j) more for types with larger benefit underestimation, cost overestimation, or higher utility costs of application (i.e., larger κ_j).

The social planner chooses the transfer program, including the ordeal parameter $\bar{\Lambda}$, to maximize social welfare. Social welfare is defined in equation 2.3 and includes the mechanical costs of the program and the fiscal externalities from individual application choices on the government budget. The low supply elasticity and zero tax rate to low-income families in middle-income countries imply that fiscal externalities (G_j^A and $G_j^{\neg} A$) will be determined by public costs of reviewing an application to determine eligibility (Kleven and Kopczuk, 2011), as well as the unintended consequences of enrollment on government budget. Automation can reduce administrative review costs (G_j^A). For rental-voucher programs, automation could reduce lease-creation costs. Unintended spillovers might include reduced applications to more expensive homeownership programs that provide housing in poorer neighborhoods or farther away from employment opportu-

nities (Navarrete and Navarrete, 2016).

$$W = \underbrace{V_L + V_H}_{\text{Private Welfare}} - \underbrace{[B(A_L + A_H)]}_{\text{Mechanical Program Costs}} + \underbrace{\left[A_L G_L^A + (1 - A_L) G_L^{\neg A} + A_H G_H^{\neg A} + (1 - A_H) G_H^A\right]}_{\text{Fiscal Externality from Program}} \tag{2.3}$$

Where $V_j = \int_0^{c_j^*} \left[u(y_j^A + B) - (\bar{\Lambda}\kappa_j + c)\right] dF_j(c) + \int_{c_j^*}^{\infty} \left[u(y_j^{\neg A})\right] dF_j(c)$ is the private benefit for individual j.[3] Note that ε_j influences application behavior but not realized utility, since it shifts perceived, not actual, benefits.

If an intervention T has zero marginal costs and impacts program participation by reducing misperceived cost and benefits and lowering transaction costs ($dT = d\varepsilon, -d\bar{\Lambda}, -d\delta$), then welfare changes from its implementation are:

$$\begin{aligned}\frac{dW}{dT} = \underbrace{\mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT} + \kappa_L A_L + \kappa_H A_H}_{\text{Change in Private Welfare}} - \underbrace{\left[B\left(\frac{dA_L}{dT} + \frac{dA_H}{dT}\right)\right]}_{\text{Change in Program Costs}} \\ + \underbrace{\left[G_L^A - G_L^{\neg A}\right]\frac{dA_L}{dT} + \left[G_H^A - G_H^{\neg A}\right]\frac{dA_H}{dT}}_{\text{Change in Fiscal Externality}}\end{aligned} \tag{2.4}$$

Where $\mu_j \equiv u(y_j^A + B) - u(y_j^A + (1 + \varepsilon_j)B)$ is the difference between the actual and perceived utility when applying.

Equation 2.4 implies that any belief updating or correction of behavioral biases will change private welfare through changes in μ_j and that changes in actual transaction costs will change private welfare through κ_j. In addition, welfare changes depend on the change in the mechanical cost of the program (the size of the transfer) and the change on fiscal externalities from a larger enrollment. This represents an important departure from the neoclassical model in which welfare implications of increasing take-up depend only on fiscal externalities through labor supply responses: $\varepsilon_j = 0$, then $\mu_H = \mu_L = 0$, and ΔPrivate welfare $= 0$.

Equation 2.4 also suggests that the effect on welfare depends on the composition of those who apply because of the intervention (T), that is, the marginal applicant.

In this model, with exogenous income and a fixed benefit amount, the effect on social welfare in

[3] $Pr(\text{apply}) \cdot E[u()|\text{apply}] + Pr(\neg\text{apply}) \cdot E[u()|\neg\text{apply}]$

response to a change in targeting (the share of enrollees who are type L) depends on the relative size of frictions across types. For a given enrollment response—holding $\frac{dA}{dT}$ constant—this effect is

$$\frac{\partial}{\partial\left(\frac{de}{dT}\right)}\left(\frac{dW}{dT}\right)\Bigg|_{dA/dT} = \left[(\mu_L - \mu_H) + (G_L^A - G_L^{\neg A}) - (G_H^A - G_H^{\neg A})\right](E_H + E_L), \tag{2.5}$$

where $e = \frac{E_L}{E_L + E_H}$. If the intervention changes targeting, then the change in W is a function of the difference in private welfare $(\mu_L - \mu_H)$ and the difference in the fiscal externality $(G_L^A - G_L^{\neg A}) - (G_H^A - G_H^{\neg A})$ from enrolling an L vs an H type. In contrast to the neoclassical model, increasing the share of L type is not necessarily better for social welfare when there are behavioral biases. The effect on welfare depends on the effect of changing targeting on both private welfare and fiscal externalities. In Latin America, machine learning algorithms can improve this targeting composition ($\frac{de}{dT}$) by using alternative data sources (satellite imagery, transaction records) to reduce inclusion/exclusion errors.

This model shows that, in the presence of behavioral biases, the key parameters for welfare analysis of interventions that aim to increase take-up rates are the relative size of frictions across types, the effects of the interventions on targeting, and the differential effect on fiscal externalities across types. Interventions that raise participation among individuals with high utility gains and low fiscal costs will increase welfare, while expanding take-up among high-friction, low-benefit types (need expensive support but gain little from it) could do the opposite. Thus, the design and targeting of digital innovations must be aligned with underlying heterogeneity in barriers to maximize both participation and welfare. Specifically, the parameters $\varepsilon_j, \delta_j, \mu_j, G_j^A, \frac{de}{dT}$ should guide outcome selection, experimentation, and data analysis in the incremental innovation cycle.

Direct observation of δ_j and ε_j is not possible, but they can be inferred from randomized interventions that shift perceived costs or benefits and affect application behavior. For instance, ε_j can be proxied using information experiments that increase awareness of benefits, while δ_j may be captured via interventions aimed at correcting misperceptions or reducing stigma. Structural estimation using revealed preferences from discrete choice models can also be employed. Measuring fiscal externalities $(G_j^A, G_j^{\neg A})$ requires linking participation to administrative cost data and downstream outcomes—such as homeownership subsidy expenditures—captured through administrative records. To capture μ_j, one can compare expected versus realized utility for marginal

applicants using follow-up survey data or subjective well-being measures in randomized trials. While specific measurement strategies must be context-specific, this framework can be generalized across settings where behavioral biases and digital disparities drive incomplete take-up.

2.3 Key Implications for Digital Innovation Design

The welfare model reveals that digital interventions' effects are theoretically ambiguous even when they successfully reduce frictions or improve targeting. This section synthesizes the key design considerations for policymakers implementing the incremental innovation cycle.

First, welfare ambiguity necessitates empirical evaluation. Interventions that raise participation among high-need types ($\uparrow E_L$) do not necessarily improve welfare if these groups impose larger fiscal burdens or if private gains are constrained by persistent misperceptions. Similarly, interventions reducing transaction costs ($\downarrow \bar{\Lambda}$) disproportionately benefit types with high κ_j (high sensitivity to ordeal costs), potentially shifting enrollment composition in ways that affect net welfare depending on relative fiscal externalities. Personalized interventions may not outperform uniform policies when within-treatment heterogeneity is low or cross-treatment correlations are high (Shchetkina and Berman, 2024).

Second, beyond technical efficiency, digital innovations raise critical equity concerns. Machine learning can enhance targeting precision (increasing $\frac{de}{dT}$) through algorithmic methods, alternative data sources (e.g., satellite imagery, transaction records), and decision-support platforms (Noriega-Campero et al., 2020).[4] Digital tools can also facilitate communication and improve outreach strategies, ensuring eligible individuals are aware of benefits. As equation 2.2 shows, better targeting raises welfare only if it shifts enrollment toward types with higher $(\mu_L - \mu_H) + (G_L^A - G_L^{\neg A}) - (G_H^A - G_H^{\neg A})$.

However, algorithmic recommendations may encode pre-existing inequalities if data quality varies across groups or if optimization prioritizes treatment effects over addressing initial barriers. Moreover, individuals with limited connectivity, digital literacy, or institutional trust may be excluded even when formally eligible, deepening structural disparities. The framework addresses these concerns by treating digital exclusion and trust as measurable outcomes alongside take-up and welfare, using AI to identify at-risk populations for targeted outreach via trusted intermediaries,

[4]The use of satellite data can be particularly useful for top-down policy implementation in centralized systems, often used in Latin America (Ko and Moffitt, 2024).

and deploying decision-support tools that help policymakers visualize trade-offs across fairness, efficiency, and coverage (Noriega-Campero et al., 2020; Björkegren, Blumenstock and Knight, 2022; Athey, 2025). By embedding these considerations into the incremental innovation cycle, this framework provides a roadmap for designing digital interventions that genuinely improve welfare—a particularly pressing challenge in middle-income countries where fiscal constraints and digital divides raise fundamental questions about the distributional impacts of digital policy tools.

3 Case Study: Take-up in Rental-Voucher Programs

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; Collinson and Reed, 2018; Chyn and Katz, 2021). 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, rental-voucher programs—to mitigate housing insecurity by reducing rent burdens and, potentially, improving neighborhood access (OECD, 2024).

The United States and the United Kingdom operate two of the world's largest rental-assistance programs. These programs differ fundamentally in design, administration, and reach. The U.S. Housing Choice Voucher Program (HCVP, formerly Section 8), introduced in 1974, is a means-tested but rationed subsidy that covers the difference between 30 percent of a household's income and local market rents up to a local standard. It provides generous assistance to roughly one in four eligible families due to capped funding and long wait lists. Once awarded a voucher, households must locate a private rental unit that meets federal housing quality standards and falls within local rent limits; participants typically have 60–120 days to lease a qualifying unit. Public Housing Authorities (PHAs) inspect and approve the unit before authorizing payment directly to landlords (Collinson, Ellen and Ludwig, 2016; Ellen, O'Regan and Ganz, 2020). While these safeguards ensure quality and proper use of funds, they also create utilization frictions: about 40% of issued vouchers remain unused (Ellen, O'Regan and Harwood, 2023).

In contrast, the U.K.'s Local Housing Allowance (LHA), launched in 1982 and now part of Universal Credit, functions as a broad entitlement with simplified administration. Benefits depend

on household size and local rents (set at the 30th percentile of market rents). All qualifying low-income renters are eligible, there are no program-specific inspections, and housing standards are enforced through general rental regulation rather than through the benefit system. LHA is typically paid directly to tenants (not landlords); authorities verify tenancy at entry and may redirect payments to landlords in cases of arrears or vulnerability. This structure speeds access but weakens assurance that funds are used for housing. Coverage is extensive, but real benefit values have eroded due to freezes and rent growth (Hickman et al., 2017).

In Latin America, Chile's *Subsidio de Arriendo* (Rental Subsidy)—implemented by the Ministry of Housing and Urban Planning (MINVU) in December 2013—was the region's first rental-assistance program, marking a shift away from long-standing reliance on large demand-side homeownership subsidies (Blanco, Cibils and Miranda, 2014). In recent years, Argentina, Brazil, Colombia, Peru, and Uruguay have adopted smaller-scale variants,[5] with differences in targeting, duration, eligibility for existing renters, and the option to purchase the rented unit. Despite this diversity, programs follow the U.S. model—that is, targeted, means-tested, and budget-limited voucher-style programs—rather than the U.K.'s entitlement-based allowance.

As in the early years of U.S. Section 8, Latin American vouchers primarily aimed to improve housing conditions and affordability—not neighborhood access (Collinson, Ellen and Ludwig, 2016). The U.S. emphasis on neighborhood opportunity emerged later, following empirical evidence on neighborhood effects on children. In Chile, the move toward rental assistance reflected both international advice and domestic housing market pressures. When Chile joined the OECD in 2010, the organization recommended reducing reliance on homeownership subsidies, arguing that they had pushed low-income families to the urban periphery, constrained the formal rental market, and reinforced spatial inequality (OECD, 2012). Because Latin American programs closely follow the U.S. voucher model, their potential impacts can be interpreted through the U.S. theoretical and empirical lens.

3.1 Barriers to Voucher Utilization

Rental-voucher programs differ from other social benefits in several ways, including in how take-up is defined. Eligible individuals face a dual take-up decision: families must first apply to the program and then search for a unit meeting specific minimum quality standards and a landlord

[5] Mexico and Paraguay have recently announced similar initiatives.

willing to accept the voucher. The second step is the lease-up or utilization stage. To map this to the theoretical model presented above, I assume that these decisions are independent and focus on voucher utilization.

Barriers to application are expected to be similar to traditional welfare programs, including lack of information about the program, misperceptions about eligibility or success probabilities (δ_j),[6] misperceptions about benefits (ε_j), and procedural burdens associated with compiling documents or visiting local housing authorities ($\bar{\Lambda}\kappa_j$) (Bhargava and Manoli, 2015). Neighborhood spillovers may also impact program participation, both through the direct interactions between neighbors—as in traditional welfare programs—and through indirect interactions in the private rental market (Manski, 2000).

Among voucher recipients, utilization may be constrained by transaction costs from administrative complexity during the contract-signing process; high housing-search costs, exacerbated by the limited availability of affordable housing and difficulty of finding a landlord willing to accept the voucher (Phillips, 2020; Christensen and Timmins, 2018); and preferences to remain in the same unit or neighborhood, exacerbated by the lack of information regarding other neighborhoods' characteristics or about the benefits for children from residing in better neighborhoods (Chetty, 2015; Schwartz, Mihaly and Gala, 2017; Bergman et al., 2019).

These barriers increase the likelihood that total disutility from participation exceeds the threshold ($c_j > c_j^*$), leading households to forgo the benefit. In the model, such barriers operate via higher misperceived utility costs (δ_j) and greater procedural burden ($\bar{\Lambda}\kappa_j$)—including the difficulty of coordinating with landlords, navigating bureaucratic requirements, or securing documentation. These barriers may also lower realized utility (μ_j) where eligible housing is scarce or landlord refusal is common, implying heterogeneous take-up and welfare effects across individuals in different geographic and institutional contexts.

At each stage, incomplete take-up can be tackled with digital tools and AI. In both cases, the effect on welfare will depend on the effects on targeting and the differences in private welfare and fiscal externalities across groups facing different barriers.

[6]The model could also be extended to include a share π_j of acceptance. The uncertainty about acceptance comes from uncertainty about eligibility rules or implementation errors. In the original paper, Finkelstein and Notowidigdo (2019) incorporate these additional parameters. While the mathematical expressions changed, the key parameters and empirical implications are the same.

3.2 Prior Evidence on Rental-Voucher Programs

Theory yields ambiguous predictions for how rental vouchers affect housing consumption and mobility. Vouchers expand households' budget sets, but realized gains depend on how tightly rent caps track market rents, landlords' pricing responses to payment standards, and the enforcement of minimum quality standards; households initially paying above the cap may even reduce consumption. On mobility, vouchers can lower instability by buffering income shocks, or they can raise mobility if benefits facilitate upgrading. (Collinson, Ellen and Ludwig, 2016; Olsen, 2003)

Empirical evidence on rental-voucher programs—primarily focused on the U.S. HCVP—shows that vouchers are a powerful policy tool for addressing housing cost burdens, crowding, and homelessness but have limited success in improving neighborhood quality (Mills et al., 2006; Ellen, 2020; Wood, Turnham and Mills, 2008). Causal evidence shows that rental voucher receipt improves children's educational attainment (Schwartz et al., 2020), while for adults it lowers employment and earnings and increases participation in other programs (Temporary Assistance for Needy Families [TANF], Medicaid, SNAP) (Jacob and Ludwig, 2012).

Incomplete take-up of rental vouchers remains a critical challenge. However, the reasons why households that applied and many times waited years for a voucher ultimately failed to use such a generous benefit are poorly understood. The scarce evidence, however, shows that barriers seem to be larger for the most vulnerable (Ellen, O'Regan and Harwood, 2023). Recent descriptive work shows that African American households and residents of predominantly African American neighborhoods have lower utilization, take longer to lease, and move farther; lease-up rates are lower in tighter, higher-cost markets (Ellen, O'Regan and Strochak, 2024).

The limited improvements in neighborhood quality for those who successfully lease up, combined with evidence of large long-term neighborhood effects on children's outcomes, have motivated a shift in research focus. Rather than studying barriers to voucher utilization—the ability to find any housing—most of the empirical work examines barriers to opportunity moves: relocations to neighborhoods with better schools, lower poverty rates, and stronger economic mobility.

Interventions designed to facilitate opportunity moves include increasing subsidy amounts in high-rent areas through Small Area Fair Market Rents (SAFMRs) (Collinson and Ganong, 2018; Reina and Aiken, 2022; Bergman et al., 2019; Geyer, Dastrup and Finkel, 2019) and implementing

assisted housing mobility programs that combine voucher offers with counseling services.[7] These mobility programs typically provide financial incentives, information about low-poverty areas, and help with engaging landlords in order to move to targeted neighborhoods. Results have been mixed, with substantial variation in effectiveness across sites and program designs.

Standardized counseling or small, short-run financial incentives have not been effective in large cities (Schwartz, Mihaly and Gala, 2017). To date, the most effective model is the Creating Moves to Opportunity (CMTO), in Seattle, King County, Washington, which paired intensive, customized counseling with search assistance, short-term financial aid, and proactive landlord outreach. At a cost of about US$4,700 per move, CMTO increased opportunity moves by 38 percentage points off a 15% control mean (Bergman, Chan and Kapor, 2020). Qualitative evidence indicates success came from sustained, personalized support that motivated families through search, suggesting that barriers extend beyond discrimination, information gaps, or liquidity to deeper psychological and social constraints (Bergman, Chan and Kapor, 2020). These complex demand-side constraints highlight scope for digital and AI tools to deliver personalized counseling at scale.

Mobility programs have been implemented by housing authorities with unusually high lease-up rates and have not impacted voucher utilization. Barriers to opportunity moves and to utilization may overlap but are not identical. In the HCVP, roughly 20% lease up in place and about 40% use vouchers within their original ZIP code (Ellen, O'Regan and Strochak, 2024). Moreover, participants are often better informed—frequently already assisted—or served by especially capable authorities, limiting external validity to typical unassisted voucher settings such as the programs implemented in Chile and other Latin American countries.

3.3 The Chilean Rental Subsidy

Modeled after the U.S. HCVP, Chile's *Subsidio de Arriendo* subsidizes private-market rents for units that meet minimum quality standards and fall below a maximum allowable rent threshold. Implemented by MINVU in December 2013 with technical advice from the U.S. Department of Housing and Urban Development (HUD), the program's primary goals were to expand access to formal rental housing, reduce overcrowding, and delay applications to subsidized homeownership

[7]Major assisted mobility programs include the Moving to Opportunity in Baltimore, Boston, Chicago, Los Angeles, and New York City (Kling, Liebman and Katz, 2007); the Chicago Regional Housing Choice Initiative (CRHCI) (Schwartz, Mihaly and Gala, 2017); the Baltimore Housing Mobility Program (BHMP) (DeLuca and Rosenblatt, 2017); and, more recently, the Creating Moves to Opportunity (CMTO) in King County, Washington (Bergman et al., 2019).

programs—particularly among young families.

The program provides a fixed, time-limited subsidy through two main voucher schemes: regular rounds targeting young families and elderly rounds for adults aged 60 or older.[8] Between 2017 and 2019, MINVU received approximately 40,000 applications and awarded 23,000 vouchers, with 80% allocated through regular rounds. By comparison, in 2020 alone approximately 40,000 homeownership subsidies were delivered, underscoring the much smaller scale of the rental program.

In 2019, the reported annual cost per active beneficiary was US$2,357 (transfers plus administration).[9] Administrative costs represented 5.8% of transfers, below homeownership subsidies (9.7%) (Ministerio de Desarrollo Social, 2019).

Eligibility is determined by the *Registro Social de Hogares* (RSH), a national vulnerability index administered by the Ministry of Social Development.[10] The program targets households in the bottom 70% of the RSH distribution who are renting or living doubled up with other families. Specifically, regular rounds target families headed by adults aged 18 or older with monthly incomes between 7 UF (US$270) and 25 UF (US$900) and at least 4 UF (US$155) in private savings toward future home purchase—homeownership programs require up to 40% higher minimum savings.[11] Elderly rounds target individuals aged 60 or older with monthly incomes above 3.8 UF (US$145) and impose no savings requirement.

The regular voucher for young families offers 170 UF (US$6,500), paid in fixed monthly installments of 4.2 UF (US$161), to cover rents up to a maximum of 11 UF (US$422) for about three and a half years. The voucher cannot cover more than 80% of the monthly rent. The elderly voucher provides roughly 213 UF (US$8,170) over two years, covering up to 95% of the rent ceiling. A recent reform automatically extended the elderly voucher program for two additional years for each recipient. In both schemes, voucher holders may space out the use of their total subsidy over eight years. Families can use the subsidy to pay the security deposit if they need to. Also, MINVU

[8] Additional temporary voucher schemes have included special rounds to curb inflows into informal settlements, respond to natural disasters, and extend rental assistance to less-vulnerable households (those between the 50th and 80th percentiles of the national vulnerability index).

[9] Computed as total expenditures on transfers and administration (US$28 million) divided by the number of beneficiaries actively using their vouchers (11,888).

[10] The RSH is used for most social assistance programs in Chile. The index combines survey and administrative data on education, income, expenses, health, food security, and housing conditions. Families are categorized into seven vulnerability groups based on percentile thresholds: below the 40th, 41st–50th, 51st–60th, 61st–70th, 71st–80th, 81st–90th, and 91st–100th percentiles.

[11] UF (*Unidad de Fomento*) is an inflation-indexed unit of account adjusted daily to the Consumer Price Index.

provides partial rent insurance for landlords to cover short-term nonpayment risk.

The voucher amount and the rent payment standard are set nationally, except for Santiago, and the extreme north and south of country, where families receive 4.9 UF (US$188) to pay monthly rents of up to 13 UF (US$499).

Families may apply online or in person at any of the 52 regional Housing and Urban Planning Services (SERVIU).[12] Each program round remains open for two to nine months and includes multiple assignment periods. Within each assignment period, MINVU ranks applicants by region using an application score that summarizes vulnerability and housing need. Vouchers are awarded within each region until the quota is filled. Unsuccessful applicants are reconsidered in subsequent assignment periods within the same round but must reapply once the round closes; only about 15% do so.

After receiving a voucher, recipients have up to twenty-four months to find a qualifying unit and activate the subsidy—considerably longer than the 60–120 days allowed under the HCVP. Households already renting an eligible unit may lease up in place, while doubled-up families must move. Eligible units must have at least three separate spaces, a municipal residential-use certificate, and a registered tax identification number; landlords cannot be family members.

Once a unit is found, recipients create a lease on MINVU's online platform, submitting property and landlord information. The lease is notarized and validated by SERVIU, and the voucher becomes active after documentation approval and payment of the first copayment. The process is straightforward when landlords possess all required documentation, yet about 20% of leases created on the platform fail to activate due to ineligible units or landlord withdrawal. MINVU collects tenants' copayments and transfers the total rent directly to landlords in a single monthly payment, reducing transaction costs and easing portability for voucher holders who wish to move.

Compared with the HCVP, Chile's fixed, time-limited voucher broadens coverage and minimizes labor supply disincentives (Jacob and Ludwig, 2012; Zhang, 2025). However, the use of national rather than city-level rent caps may encourage landlords to align rents with payment standards, potentially limiting quality gains (Collinson and Ganong, 2018; Hyslop and Rea, 2019; Gibbons and Manning, 2006; Brewer et al., 2019; Phillips, 2017; Chan and Fan, 2021; Aliprantis, Martin

[12]MINVU oversees administrative decisions, assigns vouchers, and pays rent to landlords. SERVIUs provide information, assist with applications and lease validation, and coordinate inspections. Municipalities may also assist applicants.

and Tauber, 2022). By contrast, the inclusion of partial rent insurance and a simpler lease-up process may raise landlord participation and reduce short-term housing instability (Abramson and Van Nieuwerburgh, 2024).

Figure 2 illustrates applications, voucher assignments, and lease-up rates between 2017 and 2023. While the exact number of eligible families is unknown, a recent report by the Chilean Budget Office (DIPRES) estimates that 43.4% of the eligible population has applied to the program (DIPRES, 2022). The average share of applicants that have been awarded a voucher (57.2%) implies that one out of four eligible families have been awarded a voucher. However, in the program's first decade, only 45% of awarded vouchers were successfully utilized.

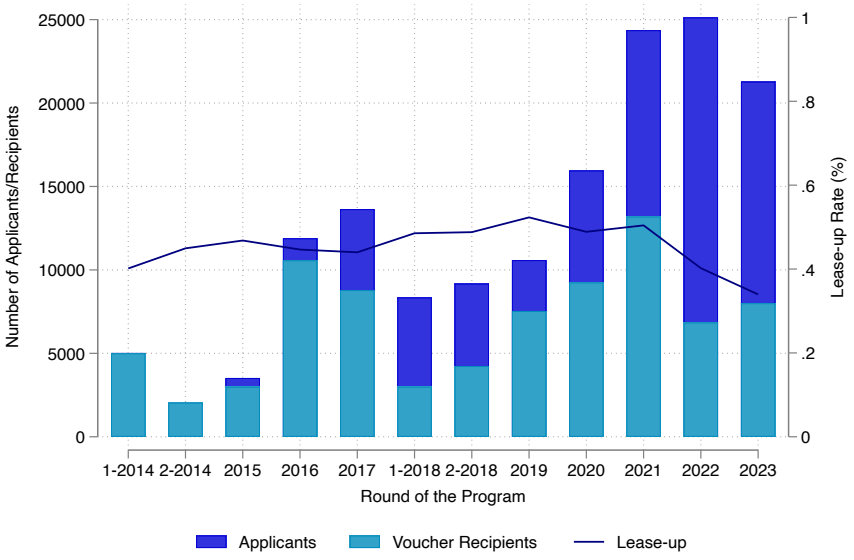


Figure 2. Program Participation

This figure uses administrative data on all applicants to the program and all leases created by voucher holders in the Ministry's platform. Lease-up rate is computed in March 2024. Dashed lines show total lease creation or lease attempts.

Beyond design, institutional context strongly influences effectiveness. Chile's low-income rental market remains small and highly informal: by 2017, only 16% of households in the bottom income quintile rented, and nearly half lacked formal lease agreements (Ministerio de Desarrollo Social, 2017). These structural conditions heighten search and administrative frictions, helping explain Chile's lower voucher utilization rate compared with the United States.

A recent evaluation of the *Subsidio de Arriendo* shows reduced overcrowding and increased residential mobility but no overall improvement in neighborhood quality (Selman, 2025). Elderly

recipients tended to relocate to denser areas, while young families with children often moved farther from schools—patterns consistent with U.S. HCVP evidence. Voucher assignment did not reduce homeownership applications; instead, applications rose among elderly recipients, suggesting complementarity with ownership subsidies. Heterogeneity by tenure and SERVIU access indicates that complementary policies—such as targeted housing-search assistance for doubled-up families and greater SERVIU capacity in underserved areas—may be necessary to translate benefits into better locational outcomes. During COVID-19, vouchers also stabilized households by lowering rent burdens, mitigating reliance on debt, and improving housing stability in the face of income shocks.

Taken together, low lease-up rates and null neighborhood improvements in both middle- and high-income settings point to structural constraints beyond affordability. Generous transfers alone are insufficient: informational, behavioral, and administrative frictions at the lease-up stage remain central—the very margin digital tools such as the online counseling program *Aprendo y Arriendo* aim to address.

3.4 Online Counseling Program *Aprendo y Arriendo* (*A&A*)

The *A&A* platform is an online counseling program designed to help Chilean rental-voucher holders understand and use their vouchers more effectively (Celhay and Selman, 2023). Funded by the Chilean Budget Office, it delivered targeted support along three dimensions, randomly assigned across recipients.

The Basic Information Treatment (T1) offered clear and personalized information on voucher status; step-by-step lease-up guidance; a Q&A blog responding to participant inquiries submitted via WhatsApp, email, or the platform; and regular (weekly or bi-weekly) reminders to encourage use of both the platform and the subsidy. The Reduced Neighborhood Information Treatment (T2) added an informational video summarizing evidence on the long-term benefits of growing up in better neighborhoods—especially for children, who are present in nearly all recipient households. Finally, the Full Neighborhood Information Treatment (T3) included all prior components and gave families access to a user-friendly *Neighborhood Browser*, an interactive search tool that allowed beneficiaries to explore neighborhoods based on characteristics such as demographics, luminosity, school quality, and access to services and amenities such as green spaces, groceries, schools, and healthcare. Although fully digital, the platform was designed to mimic the guidance

of a social worker by combining information, support, and motivation to encourage voucher use and informed neighborhood choice.[13] The experimental design is summarized in Figure 3.

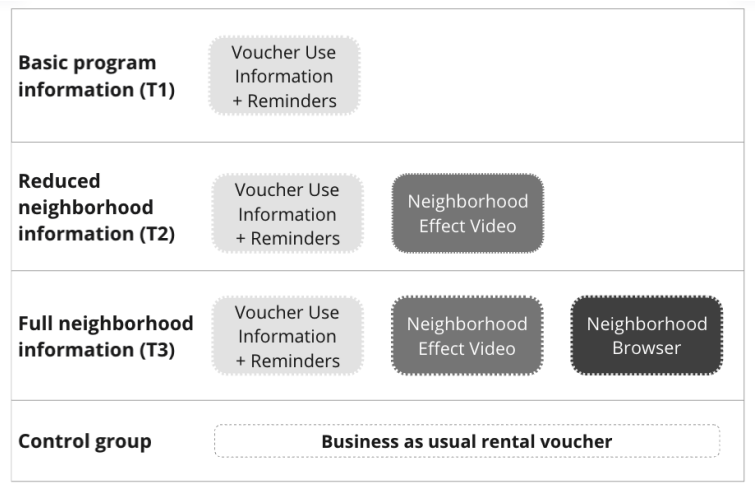


Figure 3. Experimental Design

These treatments can be mapped to the theoretical framework as follows: (i) basic program information (T1) reduces transaction costs, (ii) informing families about neighborhood benefits (T2) alters their perceptions of the value of relocating and the potential benefits of the voucher, and (iii) providing detailed neighborhood characteristics (T3) reduces search costs.

The program was launched on March 9th, 2022, and sent the last reminder on July 26th, 2022. The sample included all 11,149 voucher holders from the 2021 Regular Round who had a valid email address and had not leased up by March 9th. This represented 81% of all assigned vouchers in that round. Specifically, it excluded 2,607 voucher holders: 209 with no valid email information, 300 who participated in the pilot study, 20 who withdrew from the program before treatment, and 2,078 voucher holders that already leased up by the time we sent our first email. The randomization was stratified by region, assignment period (July 29th, Oct 21st, Dec 9th and 30th), tenure, children in school age, and survey response.

To access the platform, participants were first required to open an email containing information about their voucher status to promote voucher utilization and about the intervention to encourage platform use. Compliance with treatment was high: 97% of treated beneficiaries opened the initial email, and 79% of recipients clicked the email link and accessed $A\&A$. The treated spent on

[13]Initially focused on supporting families throughout the leasing process, the program is now being expanded to provide guidance for local housing authorities and landlords, enhancing collaboration between public and private stakeholders.

average seventeen minutes on the platform. Figure 4 shows examples of the platform interface.

(a) Program Information

(b) Neighborhood Browser

Figure 4. Platform Design

4 Data

I use the experimental dataset assembled in Celhay and Selman (2023). That study built a unique dataset that links (i) administrative, survey, and public data measured at application to (ii) administrative records on all leases created by voucher holders between March 2022 and March 2024, (iii) platform activity data, and (iv) a follow-up survey fielded between September and November 2022 (six to eight months after the intervention began).

Baseline data. Administrative and self-reported data is collected by MINVU to assess applicants' eligibility and calculate their application score. It includes socioeconomic, demographic, and housing characteristics—along with geographic location. This was complemented with survey data, administered online to all applicants in regular rounds since March 2017. The survey,

conducted prior to the announcement of voucher assignment results, elicited information on their residential mobility, housing and neighborhood experiences, and their preferences and beliefs. The average response rate was 78%.

In addition, the authors built a unique geocoded dataset combining locations from multiple data sources with the baseline survey and linked these 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.

Outcome data. Monthly administrative records for leases created in MINVU's online platform between March 2022 and March 2024 include housing and landlord characteristics, the date of lease creation, activation status, and whether the lease remains active. In total, the experimental sample created 6,331 leases on the platform after treatment, 20% of which did not activate—mostly due to supply-side barriers.

To measure additional outcomes, a follow-up survey was implemented from September 15 to November 15, 2022, eliciting beliefs, preferences, housing and neighborhood satisfaction, and search experiences, including the number of contacted landlords, time and frequency of search activities, and locations of units considered. Although control-group respondents were somewhat more likely to complete the survey, treatment and control groups remain balanced on observables. Appendix Figure I and Table I report survey attrition and balance, respectively. Finally, complete platform analytics are observed (e.g., email opens, link clicks, time on content, and question clicks).

5 Empirical Strategy

I expand the analysis of the experimental data from the impact evaluation of $A\&A$ in Celhay and Selman (2023) in two steps aligned with the model's key parameters.

I first replicate their intention-to-treatment effects (ITT) on an outcome of interest Y_i using the following equations:

$$Y_i = \beta_0 + \beta_1 T_i + X_i'\delta + \phi_s + \epsilon_i \tag{5.1}$$

where T_i is a binary indicator that equals one if beneficiary i was assigned to any treatment arm,

X_i is a vector of baseline covariates,[14] and ϕ_s are randomization strata fixed effects.[15]

To estimate arm-specific effects relative to the control and to compare arms, I replace T_i with three indicators, where l indicates the treatment arm:

$$Y_i = \alpha_0 + \sum_{l=1}^{3} \alpha_l T_{l,i} + X_i'\delta + \phi_s + \epsilon_i \tag{5.2}$$

Following (Celhay and Selman, 2023), I analyze four outcomes: lease attempt (lease creation) and successful lease-up (voucher activation) measured at four and twenty-four months post-treatment. While searches are not observed directly, lease attempts are less exposed to supply-side constraints than are successful lease-ups.

I then extend the analysis in two ways. First, I examine barriers to voucher utilization among the control group receiving business-as-usual vouchers. To identify which baseline characteristics predict successful utilization, I employ random-forest models with stratified sampling by voucher cohort on the control group (N=2,804). I use a 70-30 train-test split by outcome to ensure representative samples. The random-forest classifier uses 2,000 trees with maximum depth of 10 and class weights balanced to account for low utilization rates. I set minimum samples per split to 20 and minimum samples per leaf to 10 to prevent overfitting.

To identify the most predictive covariates, I use five-fold cross-validation to maximize the area under the ROC curve (AUC), which measures the model's ability to correctly rank participants by their likelihood of voucher utilization. I retain 62 baseline variables from an initial pool of 189 candidate features, excluding variables with excessive missing values ($> 10\%$) and low variance (< 0.01). Selected variables span five categories: program experience (e.g., online application, previous application, assignment timing), available information to recipients (e.g., county SERVIU presence, distance to municipality, knowledge of prior recipients), demographic characteristics (e.g., income, savings, family size, age, migration status, gender, married), unit quality (e.g., tenancy, housing costs, crowding, satisfaction), and perceived supply frictions (e.g., prior landlord engagement, pessimistic beliefs about landlord perceptions). Table 1 provides the complete list.

[14] Control variables include demographic characteristics (female household head, age over 35, migrant status, family size, number of children under 5 years old, number of children aged 6–18, married, presence of elderly household members), socioeconomic indicators (income quintiles, savings below 10 UF, Social Registry of Households score below 40% indicating high vulnerability), housing quality measures (three categories of housing vulnerability, overcrowding), and program-related factors (county has a local SERVIU office, applied in person rather than online).

[15] Each strata is a unique combination of region, assignment period (July 29th, Oct 21st, Dec 9th and 30th), tenure (doubled up or renting), children in school age, and baseline survey response.

Second, I identify who benefits most from digital assistance using the same 62 baseline variables over the entire experimental sample (N=11,149). I estimate conditional average treatment effects (CATEs) using the Causal Forest Double Machine Learning (CausalForestDML) method, which combines causal forests (Wager and Athey, 2018) with the double machine learning framework (Chernozhukov et al., 2018). This approach estimates individual-level treatment effects as a function of baseline characteristics without imposing parametric assumptions about heterogeneity.

The estimation proceeds in two stages. First, the method uses random-forest models to flexibly predict both outcomes $\hat{E}[Y|X]$ and treatment assignment $\hat{P}[T|X]$ from baseline covariates. Second, it estimates treatment effects using a causal forest on the residualized outcomes and treatments, partitioning the covariate space to identify subgroups with similar responses. The first-stage models use default random-forest parameters (100 trees), while the causal forest uses 2,000 trees to ensure stable treatment-effect estimates. Cross-fitting is stratified by randomization strata to preserve the experimental design. This double machine learning approach provides valid inference even when the first-stage models are complex and high-dimensional (Chernozhukov et al., 2018).

For each participant, I recover counterfactual outcomes under the control and each arm, yielding individual treatment-effect estimates comparable across arms. I then define "positive responders" (individual treatment effect > 0) and "non-responders" (individual treatment effect ≤ 0) and compare their baseline characteristics using t-tests for continuous variables and Fisher's exact tests for binary variables. Effect sizes are reported as standardized mean differences. I conduct this analysis separately for each treatment arm and outcome, focusing on characteristics with effect sizes ≥ 0.15 in at least one treatment arm. This heterogeneity analysis reveals which interventions shift enrollment toward which population types.

Category	Variables
Program experience	Online application, Previous application, Assignment timing (days between application and voucher assignment), Voucher not assigned in the next assignment, Cohort 1 (July 2021), Cohort 2 (October 2021), Cohort 3 (December 9th 2021), Cohort 4 (December 30th 2021), Baseline survey response.
Available information	County has a SERVIU; 1km, 2kms, 3–4kms, 5–10kms, 11kms+ to municipality building; Know prior recipients.
Demographic characteristics	Income, Savings, Age, Family size, Children 6–18, Children 0–5, Migrant, Female, Married, Non-family unit, Doubled up, $<$1 year in current home, $>$5 years in current home, RSH, COVID-19 total income shock.
Unit quality	Number of bedrooms, Housing costs, House satisfaction, Neighborhood satisfaction, Neighborhood perceived safety, Informal shelter, Overcrowding, Missing neighborhood satisfaction, Renting a unit from a family member, Renting from a non-family member, Doubled up, Missing tenancy.
Geography (region)	All sixteen regions in the country.
Market/Supply frictions	Prior landlord engagement to rent with the voucher, Pessimistic belief about landlords' perceptions of the program: government payment delays, tenant payment delays, unknown program/rules.

Table 1. Selected Variables Grouped by Category

6 Results

In the control group, 18% had attempted to lease by July 2022 and 14% had successfully leased up. By March 2024, these rates rose to 40% and 36%, respectively. Online counseling through *Aprendo y Arriendo* significantly increased voucher utilization, with effects that persisted over time (Celhay and Selman, 2023).

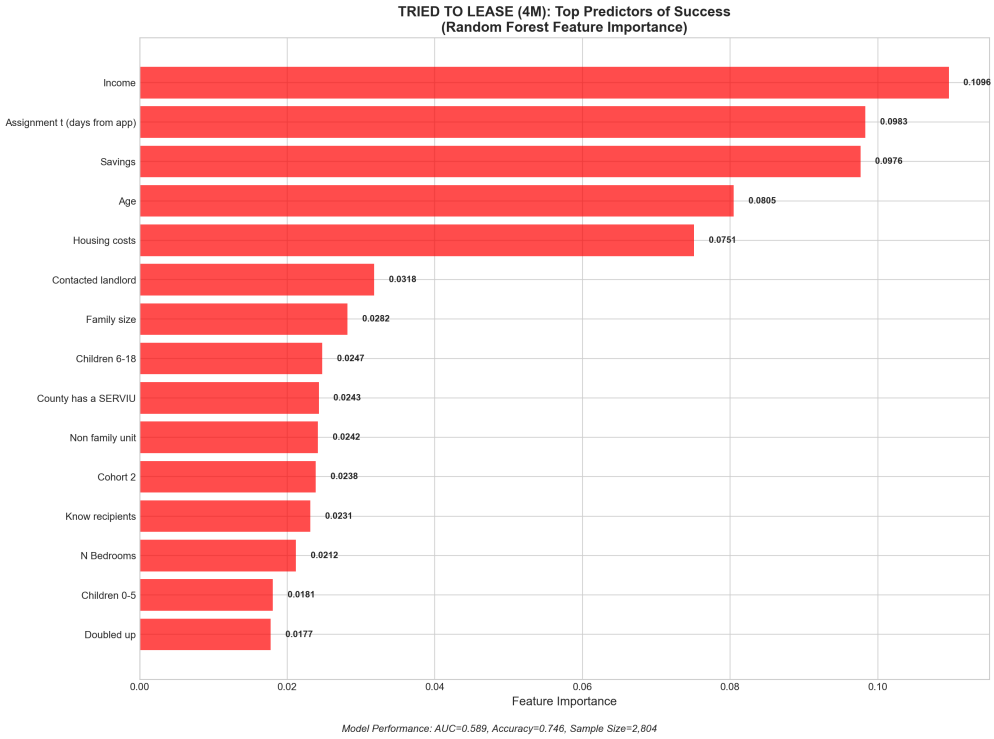
This section follows the empirical strategy in Section 5. First, it documents barriers to voucher utilization among recipients of business-as-usual vouchers. Second, it summarizes the results of the evaluation based on (Celhay and Selman, 2023). Third, it identifies which subgroups benefited most from each counseling arm.

6.1 Voucher Utilization Barriers Among the Control Group

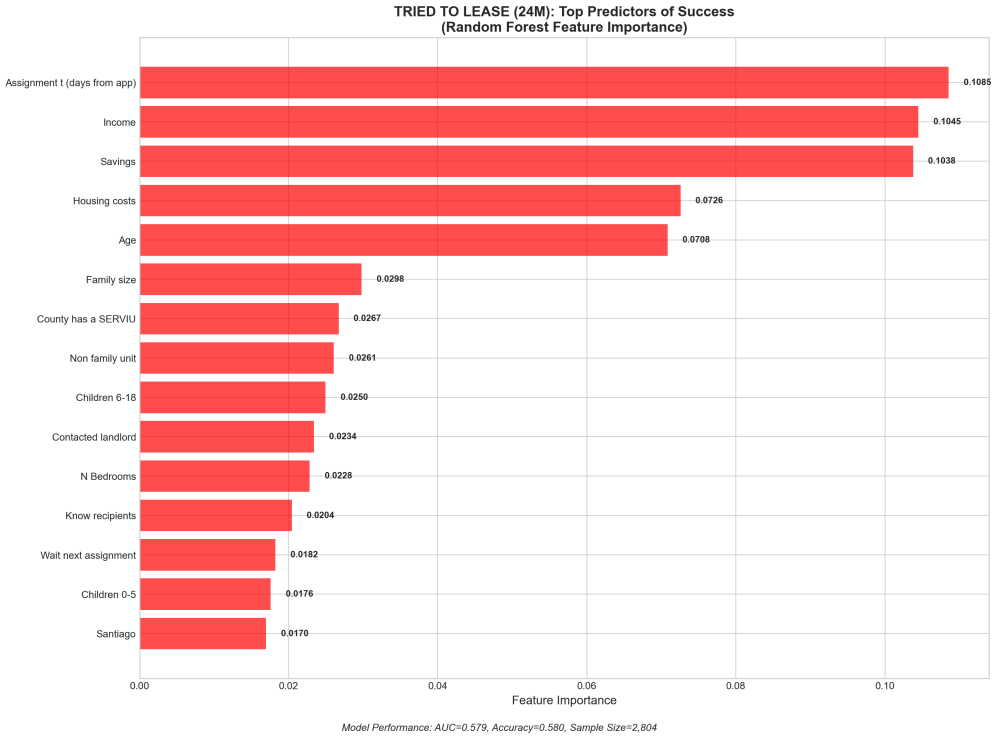
I employ machine learning methods to identify which baseline characteristics predict voucher utilization among voucher recipients. Using random-forest models with stratified sampling by voucher cohort on the control group (N=2,804), I predict the four outcomes of interest: successful lease-up at four months, attempted lease-up at four months, successful lease-up at twenty-four months, and attempted lease-up at twenty-four months. See Section 5 for more details.

The models exhibit moderate predictive power: 58%–62% AUC across outcomes—despite rich baseline covariates—indicating that successful lease-up and lease attempts are driven significantly by factors not observable at baseline. This underscores the role of barriers beyond applicant characteristics and the importance of post-assignment support (and data collection).

Figures 5 and 6 respectively report the most important predictors of lease attempts and successful lease-ups at four and twenty-four months. Several patterns are consistent across outcomes and horizons. The top predictors are largely the same—household savings and income, time elapsed between application and voucher assignment, age, and housing costs (rent for tenants, monthly contributions for doubled-up households)—with relative importance varying by outcome. Unit-quality indicators (e.g., number of bedrooms), tenure status (doubled-up versus renting from a relative or non-relative), contextual factors (residence in Santiago, presence of children—especially school-aged), and behavioral factors (prior landlord contact) also rank highly. Knowing a prior voucher holder and the presence of a local housing authority (SERVIU) in the county are also important predictors of both lease attempts and lease-up in the control group. Taken together, these results confirm the coexistence of demand-side barriers (including information frictions), supply-side constraints, and frictions induced by program design (Schwartz, Mihaly and Gala, 2017).



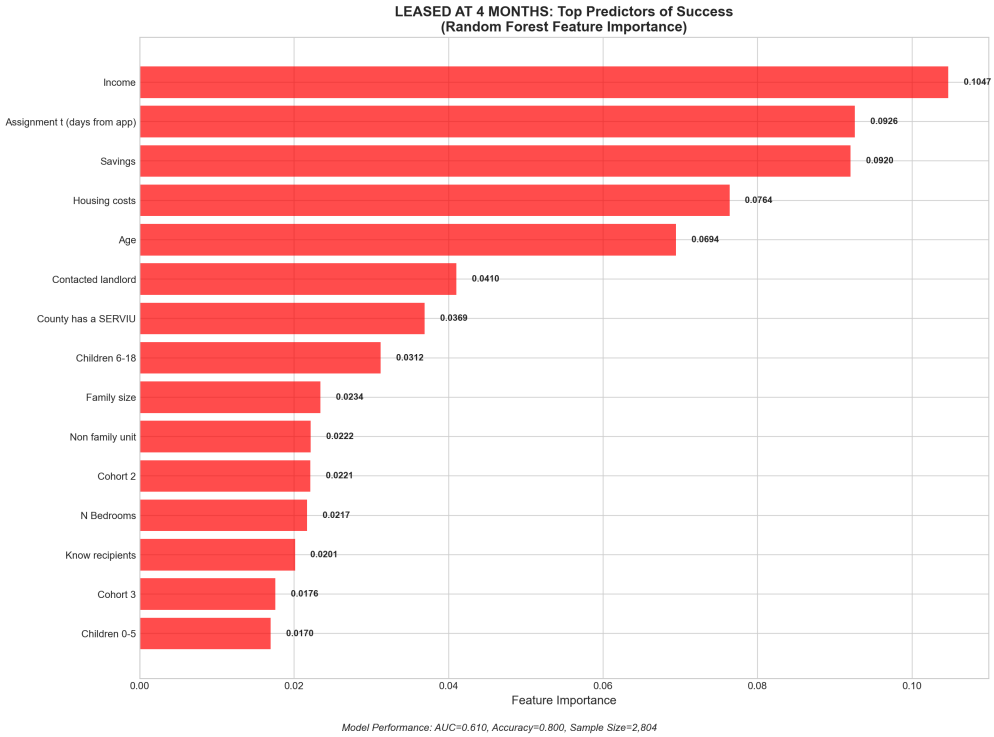
(a) Lease Attempts 4m



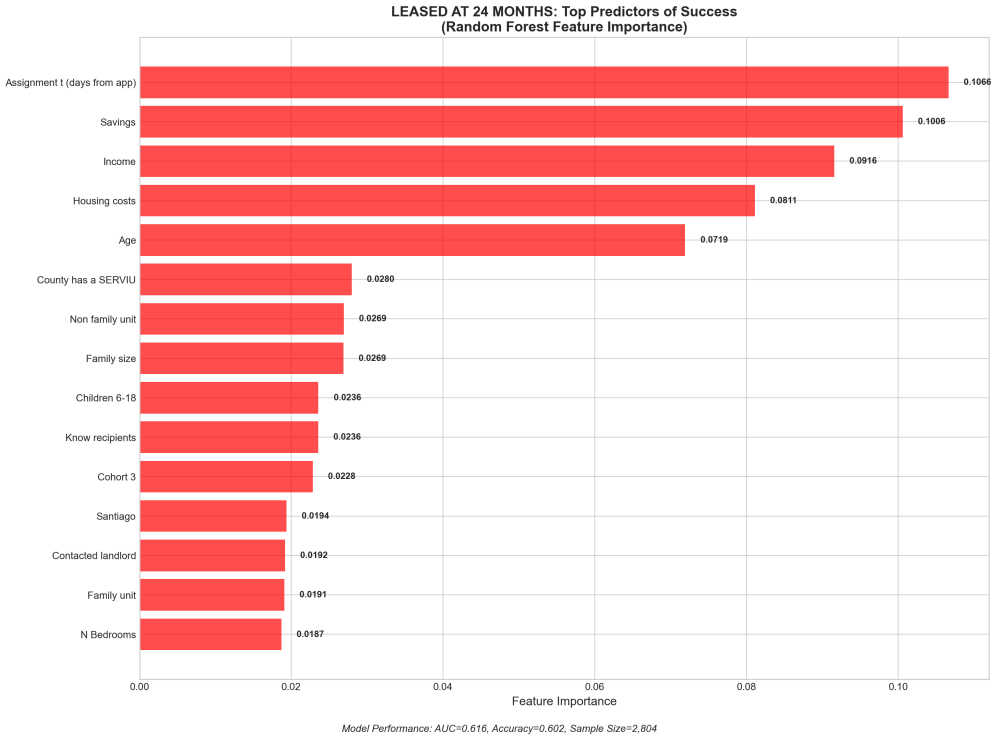
(b) Lease Attempts 24m

Figure 5. Feature Importance for Voucher Utilization Rate (Lease Attempt)

This figure shows feature importance for the first outcome of interest, lease attempt. Bar length indicates each variable's relative contribution to prediction accuracy. The measure (mean decrease in accuracy from random permutation) captures how much predictive power the model loses when that feature is removed. Longer bars indicate characteristics more strongly associated with voucher utilization.



(a) Successful Lease-up 4m



(b) Successful Lease-up 24m

Figure 6. Feature Importance for Voucher Utilization Rate (Successful Lease-up)

This figure replicates the analysis in Figure 5 for the second outcome of interest, successful lease-up. See Figure 5 for details.

Appendix Figure **??** reports the top predictors for the December voucher cohorts—the most recent assignments in the sample. Because these cohorts received vouchers only a few months before the intervention, their patterns are especially informative for designing timely post-assignment support that can be deployed immediately after receipt. Earlier cohorts do not differ systematically in socioeconomic characteristics from later cohorts, but they are less likely to be able to lease up in place (more often renting from a family member or doubled up), more likely to live in Santiago, and more likely to reside in counties without a SERVIU (Celhay and Selman, 2023).

The overall pattern of predictors closely mirrors that of the full sample: income, savings, age, housing costs, and time elapsed between application and assignment remain the strongest predictors across outcomes. If anything, SERVIU presence and prior engagement with landlords to rent using the voucher are stronger predictors for these recently assigned cohorts, whereas knowing a prior voucher holder is a weaker predictor of utilization.

Feature-importance analysis does not, by itself, indicate how individual, contextual, and behavioral factors correlate with voucher utilization—that is, which characteristics act as barriers or facilitators. To explore correlations, Table 2 compares average values of the characteristics identified in the feature-importance analysis between recipients who attempted to use their voucher and those who did not; Table 3 makes the analogous comparison for recipients who successfully leased up versus those who did not. I focus on outcomes twenty-four months post-treatment. Column 1 reports the mean for those who attempted to lease/successfully leased up; Column 2 reports the mean for those who did not; Column 3 reports the difference and its statistical significance. Appendix Tables II and III replicate the analysis for the December cohorts.

Families in the control group who attempted to lease by March 2024 tend to be smaller, less likely to have school-aged children, and more likely to rent from a non-family member—despite paying less in rent. They are also more often located outside Santiago. These patterns suggest that households in the most precarious situations face higher barriers to participation, particularly those with larger families or limited experience in the formal rental market (e.g., doubled up, renting from relatives, or without prior contact with landlords). Information and institutional access also appear critical: living in a county with a SERVIU office and knowing a previous voucher holder are both positively associated with lease attempts, underscoring the importance of informational networks and local administrative support.

Differences in demographic and socioeconomic characteristics (family size, household composi-

Feature	Attempt/Lease-up	No Attempt/Lease-up	Difference
Savings	24.022	23.777	+0.245
Income	13.637	14.027	-0.390
Assignment t (days from app)	90.214	82.825	+7.389***
Age	35.124	34.846	+0.278
Housing costs	241961.842	249195.980	-7234.138*
Family size	2.830	2.961	-0.131***
N Bedrooms	1.606	1.596	+0.011
Children 6-18	0.860	0.930	-0.070*
Children 0-5	0.559	0.596	-0.036
County has a SERVIU	0.514	0.460	+0.054**
Contacted landlord	0.491	0.419	+0.071***
Know recipients	0.336	0.263	+0.073***
Santiago	0.222	0.278	-0.056**
Non-family unit	0.699	0.630	+0.069***
Cohort 3	0.421	0.381	+0.041*
Doubled up	0.213	0.254	-0.040*
Cohort 2	0.185	0.237	-0.052**
Wait next assignment	0.397	0.322	+0.076***
Cohort 1	0.131	0.179	-0.049***
Cohort 4	0.263	0.203	+0.060***
Family unit	0.052	0.080	-0.029**

Table 2. Lease Attempt after 24 Months

This table compares baseline characteristics between control-group members who attempted to lease within 24 months after the intervention and those who did not. Column 1 reports mean values for those who attempted to lease (N=4,910). Column 2 reports mean values for those who did not attempt to lease (N=6,239). Column 3 shows the difference and its statistical significance from a two-sample t-test for continuous variables and chi-square test for binary variables. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Feature	Attempt/Lease-up	No Attempt/Lease-up	Difference
Savings	24.439	23.558	+0.881
Income	13.761	13.932	-0.171
Assignment t (days from app)	89.679	83.603	+6.076**
Age	35.195	34.825	+0.371
Housing costs	241732.125	248857.609	-7125.484*
Family size	2.850	2.942	-0.091*
N Bedrooms	1.613	1.592	+0.021
Children 6-18	0.853	0.930	-0.076*
Children 0-5	0.556	0.595	-0.039
County has a SERVIU	0.526	0.456	+0.070***
Contacted landlord	0.499	0.419	+0.079***
Know recipients	0.342	0.265	+0.077***
Santiago	0.207	0.283	-0.076***
Cohort 3	0.448	0.368	+0.080***
Non-family unit	0.718	0.623	+0.094***
Cohort 2	0.180	0.236	-0.056***
Doubled up	0.203	0.257	-0.054**
Wait next assignment	0.386	0.333	+0.052**
Cohort 1	0.124	0.180	-0.056***
Cohort 4	0.248	0.216	+0.032
Family unit	0.044	0.083	-0.039***

Table 3. Successful Lease-up after 24 Months

This table compares baseline characteristics between control-group members who successfully leased up within 24 months after the intervention and those who did not. Column 1 reports mean values for those who successfully leased up (N=4,336). Column 2 reports mean values for those who did not lease up (N=6,813). Column 3 shows the difference and its statistical significance from a two-sample t-test for continuous variables and chi-square test for binary variables. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

tion) and unit characteristics (housing costs and number of bedrooms) are smaller in magnitude than differences in location, information access, and tenure status. Results also show that, six to nine months post-assignment, earlier voucher cohorts without assistance were less likely to attempt to lease.

Results closely mirror the comparison between recipients who successfully leased up and those who did not. Findings for December's voucher cohorts are also consistent, although average differences in demographic and socioeconomic characteristics (family size, household composition) and unit characteristics (housing costs and number of bedrooms) are no longer statistically significant. In contrast, differences in information access (via social networks or local SERVIU presence), prior contact with landlords, tenure status, and local market conditions persist.

Notably, top predictors (savings, income, age, housing costs, and assignment timing) show small or non-significant mean differences between groups. This divergence occurs because random forest captures non-linear effects, threshold relationships, and higher-order interactions that do not manifest as simple mean differences. For instance, a variable may have similar means across groups but differs substantially in its distribution tails, or its effect may depend critically on values of other variables. The contrast between machine learning importance and traditional statistical tests highlights that barriers to voucher utilization—especially demographic and socioeconomic barriers—operate through complex, conditional mechanisms rather than uniform linear effects.

These findings have direct implications for program design: effective post-assignment support should prioritize families who are doubled up, have larger households, lack prior landlord relationships, reside in tighter rental markets, or live in counties without local SERVIU offices—precisely the populations most likely to struggle with voucher utilization under business-as-usual conditions. However, the evidence of complex, conditional barrier mechanisms suggests that one-size-fits-all interventions are unlikely to be optimal. Instead, different types of assistance may be more or less effective depending on families' specific combinations of constraints. The heterogeneity analysis in Section 6.3 tests this proposition directly.

6.2 Impact Evaluation of *Aprendo y Arriendo*

This section first presents the impact of *Aprendo y Arriendo* on voucher utilization. Using survey data, I then analyze underlying mechanisms driving the results. The analysis is based on Celhay and Selman (2023).

Table 4 shows estimated effects of *Aprendo y Arriendo* four and twenty-four months post-treatment. The platform had significant positive effects on voucher utilization. *A&A* raised lease attempts by 6.0 and 5.5 percentage points (p.p.) four and twenty-four months after the intervention, respectively. Relative to the control group, these effects represent increases of 33% and 14%. The effects of *A&A* on successful lease-ups were smaller but still statistically significant. Appendix Figure IV illustrates the effects of the online counseling program on lease attempts and successful lease-ups over the entire period of analysis, spanning two to twenty-four months after treatment. The effects on voucher utilization increased in the first six months and persisted over time without much variation. By the sixth month, when the survey was implemented, *A&A* had increased the probability of lease attempts by 7.3 p.p. (33%) and the likelihood of successful lease-ups by 5.1 p.p. (28%).

Importantly, the share of unsuccessful lease attempts remained unchanged. After four months, 43% of the attempts by both the control and treatment groups were unsuccessful. This share dropped to 19% by the end of the period. This result suggests that *Aprendo y Arriendo* provided valuable information otherwise unavailable to rental-voucher holders—without increasing the administrative burden on local housing authorities.

	Lease Attempt		Successful Lease-up	
	4 months	24 months	4 months	24 months
	(1)	(2)	(3)	(4)
All Treat	0.060***	0.055***	0.034***	0.043***
	(0.009)	(0.011)	(0.008)	(0.010)
Treat 1	0.075***	0.057***	0.047***	0.045***
	(0.011)	(0.013)	(0.010)	(0.013)
Treat 2	0.051***	0.037***	0.034***	0.032**
	(0.011)	(0.013)	(0.010)	(0.013)
Treat 3	0.055***	0.071***	0.022**	0.052***
	(0.011)	(0.013)	(0.010)	(0.013)
T1=T2=T3	0.085	0.037	0.038	0.272
N	11,149	11,149	11,149	11,149
Control Mean	0.40	0.40	0.14	0.40
Strata	YES	YES	YES	YES
Add. Controls	YES	YES	YES	YES

Table 4. Effects on Voucher Utilization 4 and 24 Months Post-Treatment

This table shows treatment effects on lease attempts and successful lease-ups four and twenty-four months post-treatment. The first row shows treatment effects pooling all treatment arms together. Rows 2 to 4 present the effects of a different regression, comparing each treatment arm to the control group. Row 5 shows the p-value of an F-test for the null that treatment effects were the same across T1, T2, and T3. All regressions include strata fixed effects and baseline covariates described in Section 5.

The evaluation found significant differences across treatment arms, which remain statistically significant after twenty-four months for lease attempts. Appendix Figures V and VI show differences in treatment effects on lease attempts and successful lease-ups across treatment groups over time. Panels (a) and (b) compare the reduced neighborhood information arm (T2) and the complete-information arm (T3), respectively, to the basic counseling arm (T1), which generated the largest effects on both outcomes four months post-treatment. Panel (c) reports differences between T3 and T2 over time.

Encouraging families to consider neighborhood characteristics during search without providing actionable neighborhood-level information (T2) was the least effective treatment. In contrast, basic counseling (T1) produced the largest effects on both lease attempts and successful lease-ups during the first eight months. Over that period, point estimates for the complete neighborhood information arm (T3) rose and eventually exceeded those for T1—although the gap is not statistically significant. Consistent with this, differences between T2 and T3 widen over time. The gap is statistically significant for lease attempts but, while similar in direction, is not statistically significant for successful lease-ups.

The *A&A* platform may have increased voucher utilization through multiple channels. Celhay and Selman (2023) use follow-up survey data to examine different mechanisms.[16]

First, email notifications and regular reminders may increase awareness of voucher status, prompting action. Second, clear information about program rules and lease-up procedures may reduce transaction costs by clarifying administrative requirements. Table V shows that *A&A* significantly increased voucher awareness: treated participants were 15.5 p.p. (20%) more likely to correctly report their assignment status. Also, all treatment arms similarly increased knowledge of lease-up procedures, unit-quality requirements, and landlord eligibility criteria. These results confirm that the platform successfully conveyed basic program information across all treatment groups. However, these results do not explain differences across treatment arms.

Third, neighborhood information may alter search behavior by changing perceived benefits of relocation with the voucher and reducing search costs—though the net effect on utilization is theoretically ambiguous, as better information could either encourage moves to superior neigh-

[16]The survey sample remains balanced across treatment and control groups (Section 4). Appendix Table IV reports treatment effects on successful lease-ups using administrative outcomes and self-reported voucher utilization, restricting to survey respondents. Results are similar.

borhoods or reveal that staying in place is optimal. Table V reveals important differences across treatment arms in search patterns. Recipients of basic information (T1) were no more likely than the control group to conduct active housing searches, though they were more likely to try to stay in place (more likely to search within their current neighborhood and more likely to contact their current landlord). This pattern suggests that clarifying program rules primarily enabled families to attempt to use the voucher without relocating.

In contrast, recipients of neighborhood information (T2 and T3) were more likely to search and conducted broader housing searches—less concentrated in their current location—than T1 recipients did. Notably, T3 recipients—who had access to the comprehensive Neighborhood Browser—were significantly more likely to report ongoing searches at the time of the follow-up survey (6–8 months post-treatment). This suggests that access to detailed neighborhood data produced longer search periods. In addition, T2 and T3, but not T1, increased participants' beliefs that the subsidy provided access to better housing and neighborhoods.

Taken together, these results indicate that *A&A* operated through multiple complementary channels. All treatment arms increased program awareness and knowledge, reducing informational barriers. However, neighborhood information (T2 and T3) additionally shifted search behavior and perceived program value in ways that basic information (T1) did not.

From a policy perspective, it is important to understand whether differences across treatment arms go beyond search and voucher utilization, extending to housing consumption, neighborhood access, or other relevant outcomes for families' well-being. Examining these additional outcomes is also relevant for understanding through which channels *Aprendo y Arriendo* increased voucher utilization: families may have leased up in place or relocated to new eligible units.

I examine residential mobility patterns, rents, crowding, and neighborhood characteristics among voucher holders using follow-up survey data. Appendix Table V shows that the intervention increased voucher utilization through both channels: lease-up in place rose by 4.7 p.p. (18% increase from the control mean of 25.3%), while moving to a different unit increased by 2.1 p.p. (14% increase from the control mean of 14.5%). Both effects are statistically significant and similar across all three treatment arms.

The intervention did not affect monthly rents paid by voucher users, suggesting that housing consumption was unaffected by assistance. However, *A&A* reduced crowding and improved

neighborhood quality along specific dimensions—consistent with the previous evaluation of the voucher program (Selman, 2025) (Section 3.3). Treated families gained better access to amenities—measured by a z-score combining proximity to childcare, schools, healthcare, public transportation, and parks. The point estimate is large and statistically significant only for treatment T2 (Reduced Neighborhood Information); however, differences across coefficients are not statistically significant. Furthermore, T2, not T3, reduced self-reported exposure to disamenities in the neighborhood (violence and drug activity). In contrast, treatment T3 led families to move closer to family members compared to other treatments. The differential effects across treatment arms—particularly T3's impact on family proximity—suggest that more comprehensive information tools enable families to optimize along dimensions they value most.

These results have important implications for program design: while basic information facilitates voucher use, neighborhood tools may be necessary to help families achieve better locations. However, these neighborhood improvements come at the cost of longer search durations.

6.3 Treatment Heterogeneity

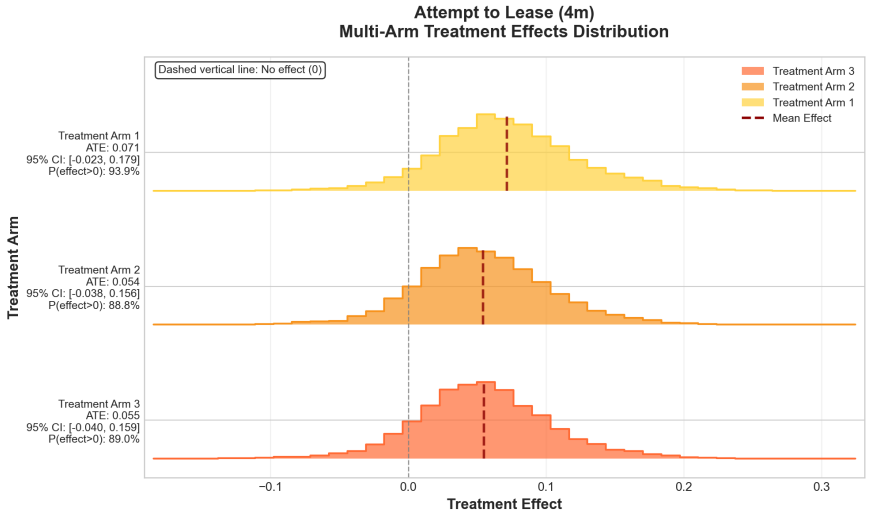
The preceding analyses established two key findings. First, families in the control group face multiple barriers to voucher utilization, with information frictions (awareness, SERVIU access, and knowledge of prior recipients) and supply-side frictions (landlord access, tenure type, and geographic constraints) operating alongside complex demand-side barriers that depend on interactions among demographic, housing, and financial characteristics (Section 6.1). Second, digital assistance significantly increases utilization by increasing awareness and clarifying program procedures, although treatment effects differ by treatment arm. Neighborhood information changed beliefs about voucher benefits and search behavior, delaying voucher utilization.

Given that demand-side barriers operate through complex mechanisms while information and supply-side frictions may be more uniform, different interventions may be more or less effective for different population subgroups. In this section, I use the causal machine learning model explained in Section 5 to identify which families benefited most from each type of assistance.

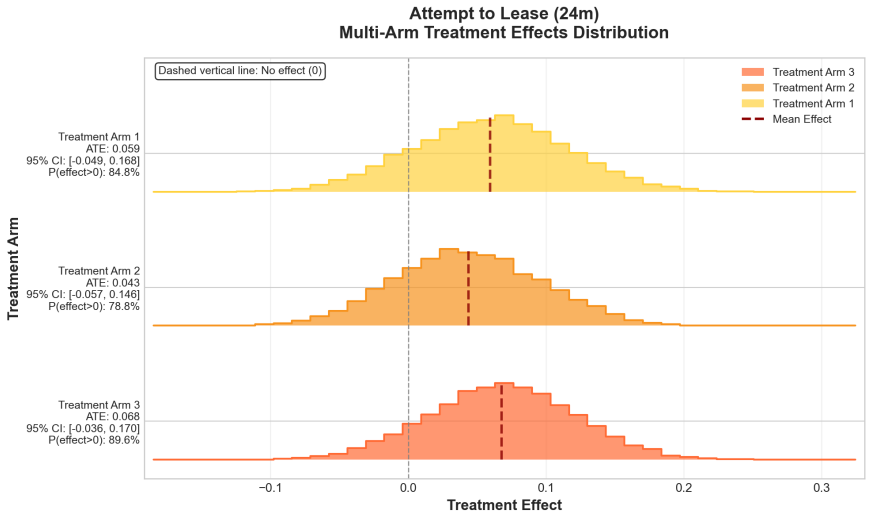
Figures 7 and 8 show CATEs for the entire sample, respectively for each outcome of interest. Appendix Figures VII- VIII and IX-X show results for earlier and later voucher cohorts, respectively. The distributions reveal substantial heterogeneity in treatment responsiveness, suggesting that digital assistance is not one-size-fits-all. Notably, treatment arms help the majority of recipients,

not just a small subset.

Heterogeneity increases over time, raising the share of non-responders. This is consistent with average treatment effects. After twenty-four months, motivational information about neighborhoods (T2) provides a weaker nudge than either basic information (T1) or comprehensive search tools (T3) do. Comparing earlier and later cohorts shows that T3 is much more effective for earlier cohorts, who faced larger barriers.



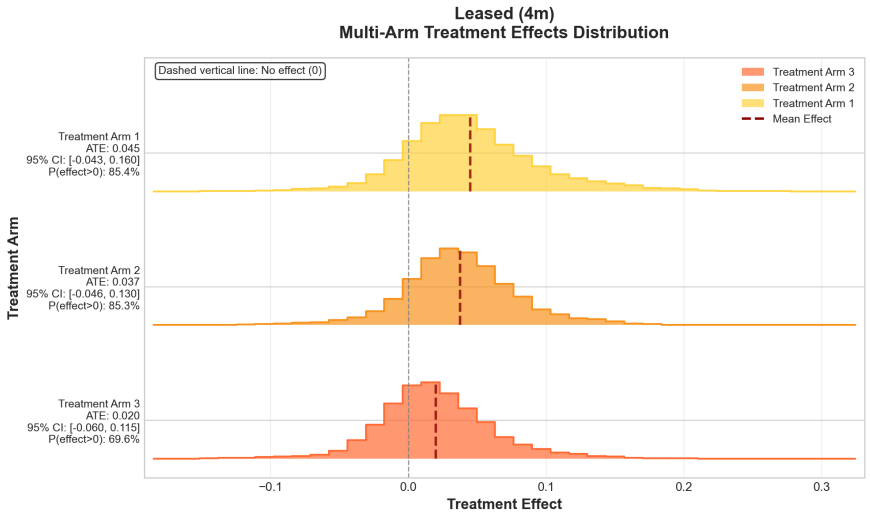
(a) Lease Attempt-4 Months



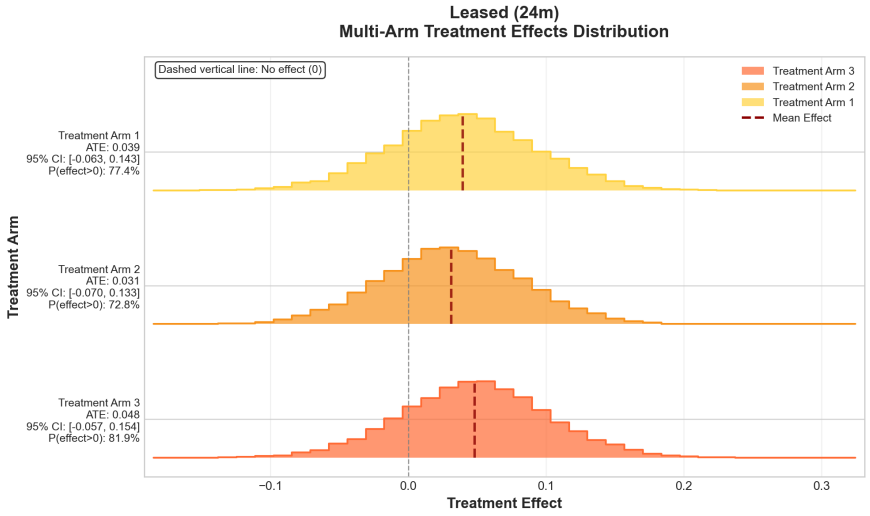
(b) Lease Attempt-24 Months

Figure 7. Distribution of Individual-Level Treatment Effects (Causal Forest Double ML)

This figure presents CATE distributions for the three treatment arms (T1: yellow; T2: orange; T3: red) with vertical dashed red lines indicating mean effects. Panels show effects on (a) attempting to lease within 4 months, and (b) attempting to lease within 24 months. Sample includes all experimental participants (N=11,149). Treatment effects are estimated as individual-level differences in predicted outcomes under treatment versus control, conditional on baseline characteristics. Positive values indicate the treatment increased the probability of the outcome.



(a) Successful Lease-up-4 Months



(b) Successful Lease-up-24 Months

Figure 8. Distribution of Individual-Level Treatment Effects (Causal Forest Double ML)

This figure replicates Figure 7 using successful lease-up as the outcome of interest. Panels show effects on (a) successful lease-up within 4 months, and (b) successful lease-up within 24 months. Sample includes all experimental participants (N=11,149). See Figure 7 for details.

The Reduced Neighborhood Information Treatment (T2) shows consistently smaller effects than T1 and T3 do across all outcomes and cohorts. Whether this reflects that it provides uniformly weaker assistance to all recipients or instead that it helps a different subset of the population cannot be determined from these distributions alone. Therefore, I use the results of the model to create benefit profiles that reveal which types of families each treatment arm helps most—informing potential targeting strategies for future assistance.

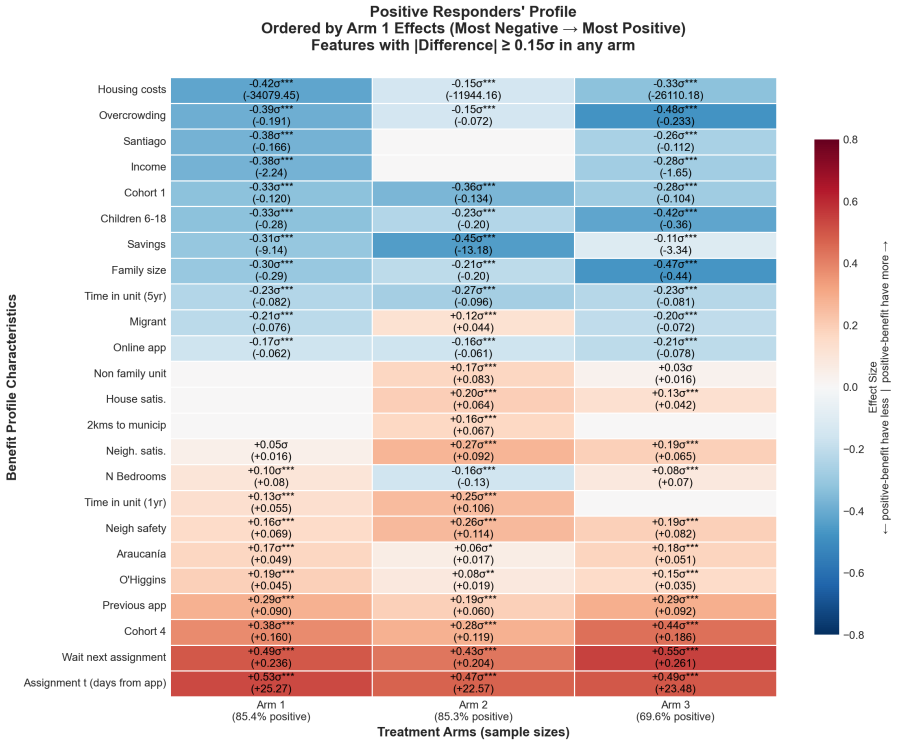
Figure 9 presents heatmaps showing benefit profiles. Each cell shows the difference in means between "positive" responders and "non-responders", measured in standard deviations (raw difference in parentheses). Features are ordered by differences between responders and non-responders under treatment arm 1. Blue cells (on the top) represent characteristics that are lower among responders, and red cells (on the bottom) represent characteristics that are higher among responders. Blank cells represent covariates that do not explain heterogeneity under certain arms.

Four months post-treatment, the single strongest predictor of positive treatment response in terms of successful lease-up across all arms is assignment timing: families who waited longer before voucher assignment—especially those who waited beyond the next scheduled assignment—are disproportionately represented among positive responders (those who successfully leased up). Compared to non-responders, positive responders waited 23–25 days (0.5 standard deviations) longer between application and assignment. The rolling application process may have been confusing, particularly when additional assignments were added before a round closed with little public announcement. Consistent with this interpretation, digital assistance disproportionately helped recipients from the unpublicized December 30th voucher assignment and was less likely to help recipients from earlier voucher cohorts. This indicates that digital assistance's immediate value lies in increasing program awareness—addressing information barriers identified in Section 6.1.

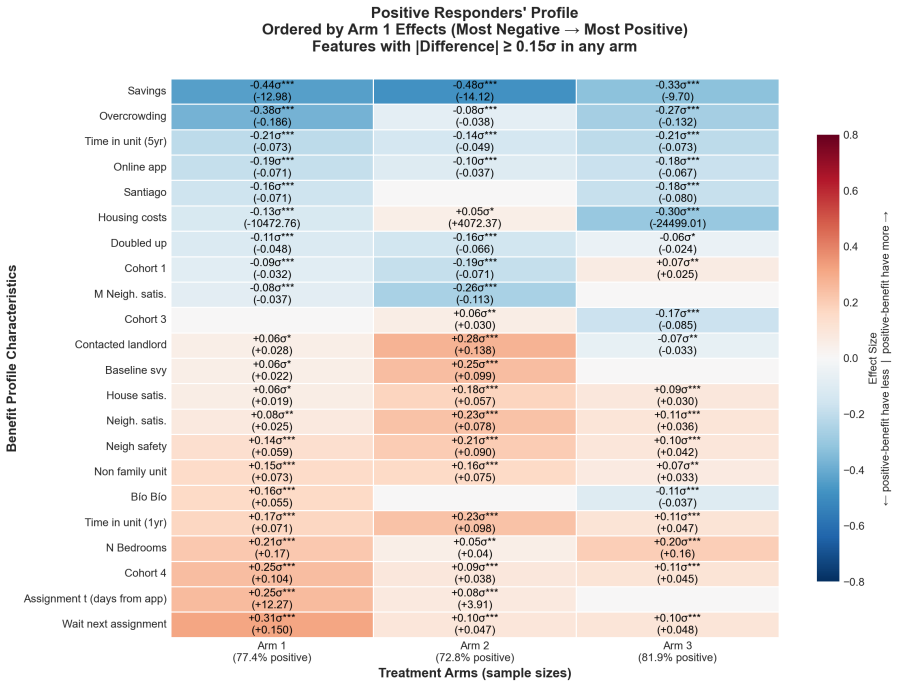
A second recurring pattern four months post-treatment is prior program experience: repeat applicants responded more than first-timers, with positive responders being 9 p.p. more likely to have applied previously. This suggests that even more experienced households extracted actionable new information from online counseling that complemented their prior knowledge. After twenty-four months, differences between positive responders and non-responders are smaller, especially for T1 and T3. Both assignment timing and previous experience in the program are much weaker predictors of positive response, especially for the groups receiving neighborhood information (T2 and T3). Taken together, these patterns suggest that the counseling information- and transaction-cost channel is strongest in the short run; over time, remaining effects operate through other mechanisms.

Results also show that treatment arms—especially T2—enabled different groups of voucher holders to utilize their vouchers four and twenty-four months post-treatment. The most basic information (T1) helped the most economically vulnerable families: positive responders had 9 UF

(US$345) lower savings and 2 UF (US$77) lower monthly income than non-responders did—two characteristics that strongly predicted voucher utilization in the control group (Section 6.1). However, T1 positive responders had lower mobility costs—they lived in smaller families (0.3 fewer members), with fewer (0.3) school-aged children, and in less crowded homes (19 p.p. less likely to be overcrowded)—and faced lower supply-side barriers—they were less likely to be migrants (8 p.p.), lived outside Santiago (17 p.p.), and paid lower housing costs (US$45 less per month). Digital assistance was also more beneficial for those who applied in person rather than online—suggesting that access to local housing authorities complements assistance.



(a) After 4 Months



(b) After 24 Months

Figure 9. Benefited Profile-Successful Lease-up

This figure presents benefit profiles for successful lease-up at 4 months (Panel a) and 24 months (Panel b) post-treatment. Each cell shows the standardized mean difference between positive responders (treatment effect > 0) and non-responders (treatment effect ≤ 0), with raw differences in parentheses. Features are ordered by Treatment Arm 1 differences. Blue cells indicate characteristics lower among responders; red cells indicate characteristics higher among responders. Sample includes all experimental participants (N=11,149). * p<0.1, ** p<0.05, *** p<0.01.

After twenty-four months, the main differences between positive responders and non-responders for T1 were that positive responders were more likely to be renting from a non-family member, suggesting that the basic information further helped those renting a more suitable unit. These patterns reveal an important nuance: while the most basic information (T1) addresses the financial vulnerability barrier (low savings/income), it is less effective at promoting successful lease-up for families with larger supply-side barriers and mobility costs.

Positive responders to T2 differ systematically from T1/T3 responders. For T2, positive responders and non-responders are similar in terms of housing costs, crowding, income, and location, but differ substantially on other dimensions. Positive responders include families with more experience in the rental market (migrants [4 p.p. more likely], those who recently moved [11 p.p.], and those renting from a non-family member [8 p.p.]) who live in better areas (closer to municipal buildings, those who feel safer in their neighborhoods, and those who are more satisfied with their housing [6 p.p.] and neighborhood [11 p.p.]). However, T2 responders have lower savings (13 UF), which may prevent them from applying to homeownership subsidies or restrict their options in the formal rental market. After twenty-four months, positive responders are also more likely to have engaged with a landlord to rent with the voucher prior to voucher assignment, confirming the relevance of prior experience in the rental market for responding to the motivational video. Combined with higher baseline survey response rates among positive responders, the evidence suggests that over the twenty-four month period, T2's motivational information becomes more valuable for families who are already somewhat engaged.

For T3, compared to T1 and T2, positive responders and non-responders do not differ in terms of savings but have even lower mobility costs (smaller, less crowded families with fewer school-aged children). Similar to T2, T3's positive responders also reported higher satisfaction with their housing (4 p.p.) and neighborhood (7 p.p.), although differences from non-responders are smaller. After twenty-four months, positive responders are characterized by paying lower housing costs, living in bigger units, experiencing less crowding, and having lower savings. In contrast to T2, engagement with a landlord prior to voucher assignment and baseline survey response are not higher among positive responders. This pattern suggests that over time T3 helps a broader population of voucher holders, regardless of their prior search experience. However, T3 remains less effective for the most housing-deprived families, indicating that comprehensive neighborhood tools cannot overcome severe resource constraints that limit voucher utilization.

Appendix Figure XI shows characteristics of positive responders in terms of lease attempts. Four months post-treatment, positive responders and non-responders are similar in most characteristics, except for savings. Positive responders to T1 and T2 have lower savings—especially T2—and positive responders to T3 have higher savings. Twenty-four months post-treatment, observable covariates are weaker predictors of lease attempts, but variation in profiles across treatment arms increased, especially for T3, resembling benefit profiles for successful lease-ups twenty-four months post-treatment. Combining the results for lease attempts and successful lease-ups, the evidence suggests that four months post-treatment, similar voucher holders went out to search although they were all not equally likely to succeed.

This research reveals important heterogeneity in how digital assistance addresses barriers to voucher utilization. Welfare implications of the provided assistance depend on whether interventions enroll types with favorable combinations of utility gaps $\mu_L - \mu_H$ and fiscal externalities $(G_L^A - G_L^{\neg A}) - (G_H^A - G_H^{\neg A})$. Mapping these findings to the welfare model yields several insights. First, basic information in all treatment arms generated positive private welfare gains by correcting misperceptions ($\mu_j > 0$) and reducing transaction costs ($\bar{\Lambda}$). Second, the platform did not increase the share of unsuccessful lease attempts, indicating modest fiscal externalities from administrative review costs (G_H^A). Third, neighborhood information improved neighborhood access (amenities, safety, family proximity) without increasing rents, suggesting that welfare gains beyond simple utilization increase—although at the cost of delaying voucher utilization.

Treatment arm effectiveness varied systematically across population subgroups depending on underlying demand-side and supply-side constraints. While definitive welfare rankings require measuring μ_j and (G_L^A), the heterogeneity analysis provided empirical evidence on the targeting parameter $\frac{de}{dT}$. Positive responders in the successful lease-up margin twenty-four months post-treatment are particularly informative, given that dT is fixed across treatment arms.

T1 shifted enrollment toward financially vulnerable families, which could improve welfare if these families have large misperceptions. This seems plausible given that T1 increased utilization by clarifying program rules, suggesting substantial misperceptions among vulnerable families. T3 helped an increasingly broad population over time regardless of savings levels or prior search experience, suggesting that $\frac{de}{dT} \approx 0$—minimal change in enrollment composition. Furthermore, T3 positive responders moved closer to family, indicating that they may have higher utility gains from the program. T3's broad reach combined with higher utility gains could generate posi-

tive welfare effects even without explicitly targeting vulnerable populations. T2 helped already-engaged families likely to have smaller misperceptions, suggesting limited welfare gains despite utility gains from improved neighborhood access.

7 Discussion

This paper develops a welfare-based framework for how digital technologies can reduce incomplete take-up in social assistance and applies it to Chile's rental-voucher program, leveraging experimental evidence from the online counseling program *Aprendo y Arriendo*, which delivered three information treatments targeting distinct utilization barriers. I identify barriers among the control group and estimate heterogeneous treatment effects to map which populations benefit from which intervention. I found that utilization is jointly predicted by information frictions (assignment timing, administrative access), supply-side frictions (landlord relationships, geography, unit quality), and demand-side constraints (savings, income, family composition), with the latter operating through complex interactions.

Digital assistance significantly increased utilization, with assignment timing emerging as the strongest universal predictor: families who waited 23–25 days longer responded more positively across all arms, confirming that the platform's immediate value lies in improving voucher awareness within confusing assignment processes. Beyond timing, benefit profiles diverged sharply. Basic information (T1) helped economically vulnerable families but only those with lower mobility costs and supply-side barriers—not those in the most severe housing distress. Motivational information (T2) showed weak effects among already-engaged families. Comprehensive tools (T3) demonstrated broad reach while enabling neighborhood improvements at the cost of longer searches.

The findings generate actionable policy implications. Universal provision of basic information (T1) appears justified given its broad effectiveness and potential for high μ_L among vulnerable families, though complementary interventions may be needed for those facing higher supply-side barriers and mobility costs. Comprehensive tools (T3) should be broadly available given their ability to generate welfare gains across diverse populations. Motivational information provides limited value and could be deprioritized.

In addition, results reveal a fundamental limitation: digital assistance proved less effective for families in worse housing conditions. Complementary interventions—such as increased subsi-

dies, landlord engagement, and mobility counseling—may be necessary for the most vulnerable. Policymakers must balance efficiency with equity, as algorithmic optimization risks serving easiest-to-help rather than most-in-need families.

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A Appendix

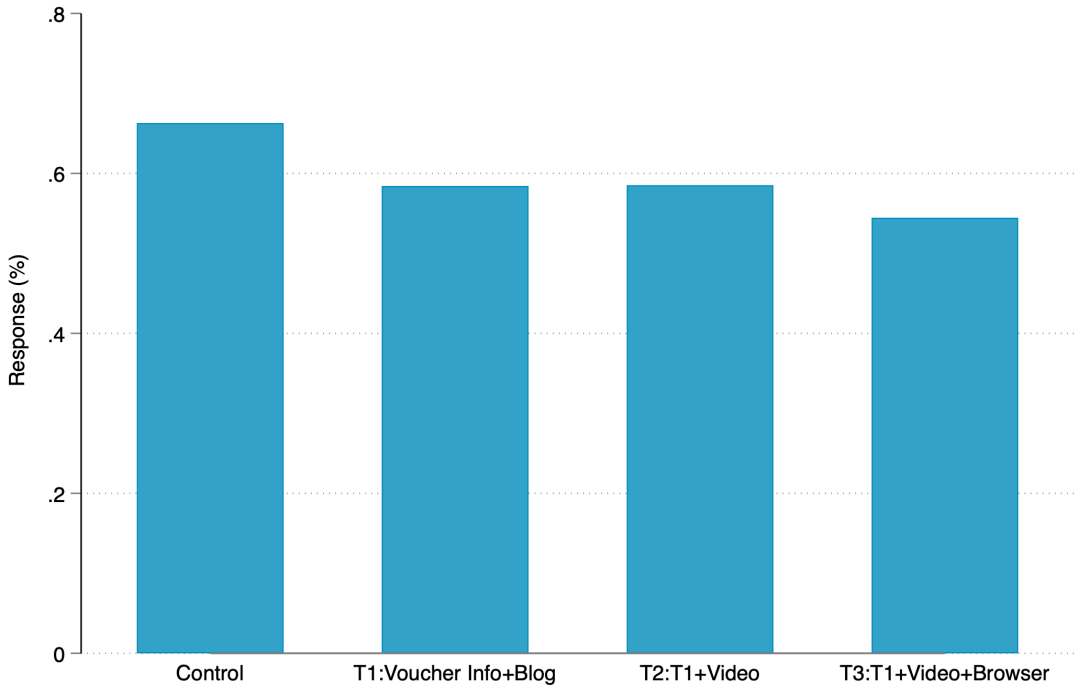


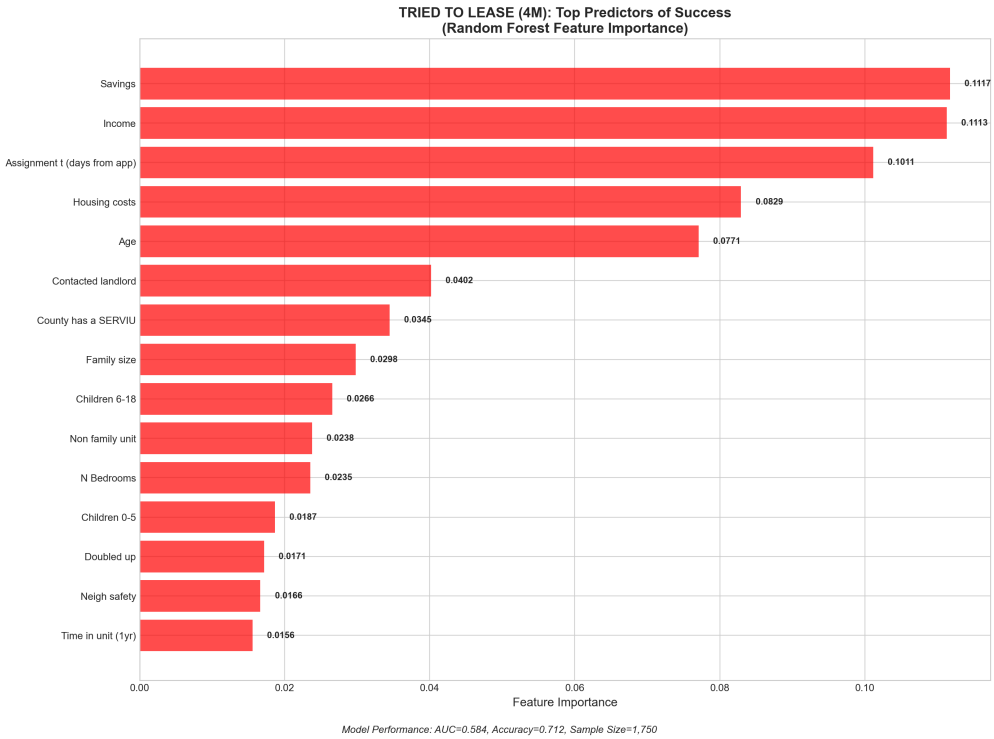
FIGURE I. Follow-up Sample Attrition

This figure shows follow-up survey response rate by treatment group.

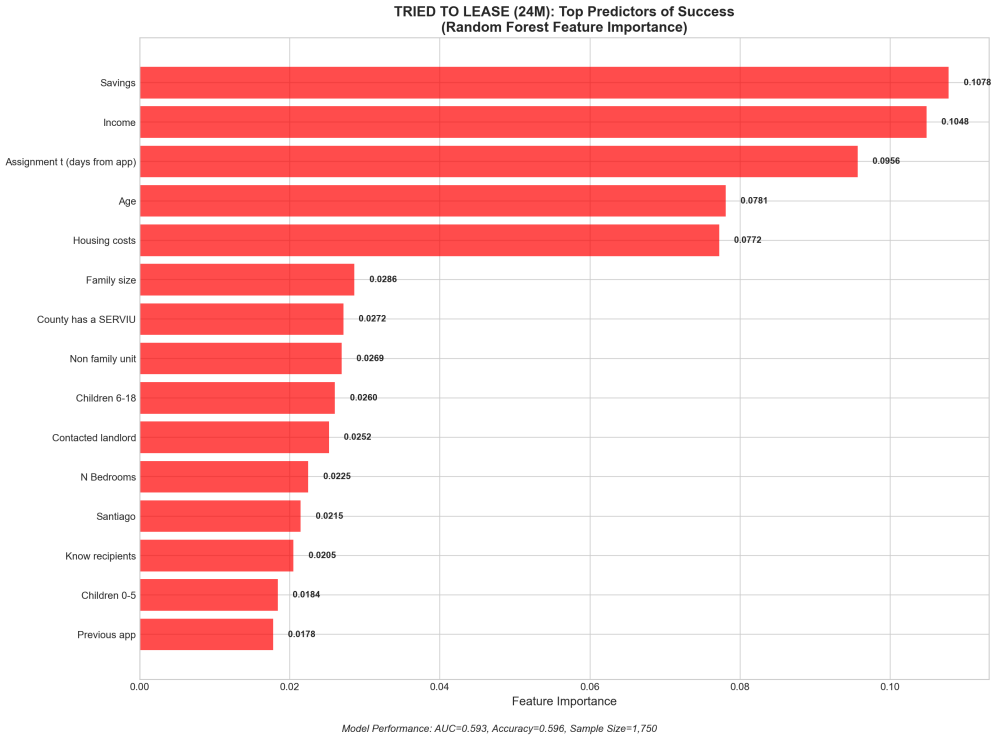
	Mean C (1)	Mean T1 (2)	Mean T2 (3)	Mean T3 (4)	T vs C (5)	T1=T2=T3(p) (6)
Female	0.823	0.841	0.832	0.847	0.014	0.723
	(0.382)	(0.366)	(0.374)	(0.361)	(0.010)	
Age	34.963	34.779	34.536	34.480	-0.374	0.437
	(9.780)	(9.683)	(9.346)	(9.392)	(0.262)	
Married	0.194	0.181	0.195	0.186	-0.006	0.364
	(0.396)	(0.385)	(0.396)	(0.389)	(0.011)	
Migrant	0.161	0.170	0.178	0.189	0.017*	0.553
	(0.368)	(0.376)	(0.383)	(0.392)	(0.010)	
Family size	2.955	2.929	2.944	2.954	-0.025	0.602
	(0.967)	(0.950)	(0.957)	(0.944)	(0.022)	
Child 0-5	0.516	0.527	0.510	0.522	0.007	0.404
	(0.500)	(0.499)	(0.500)	(0.500)	(0.012)	
Elder	0.026	0.023	0.021	0.022	-0.003	0.744
	(0.159)	(0.151)	(0.143)	(0.146)	(0.004)	
Disability	0.033	0.029	0.033	0.025	-0.003	0.287
	(0.178)	(0.169)	(0.180)	(0.156)	(0.005)	
Income (UF)	13.959	14.020	14.007	13.835	-0.081	0.566
	(6.065)	(6.183)	(5.990)	(5.931)	(0.165)	
Savings (UF)	23.302	23.573	22.030	23.417	-0.326	0.142
	(29.290)	(29.645)	(25.241)	(30.876)	(0.794)	
40% RSH	0.925	0.904	0.915	0.913	-0.014*	0.731
	(0.263)	(0.295)	(0.279)	(0.282)	(0.007)	
House	0.643	0.631	0.628	0.628	-0.012	0.830
	(0.479)	(0.483)	(0.484)	(0.483)	(0.013)	
Formal shelter	0.789	0.774	0.786	0.777	-0.007	0.851
	(0.408)	(0.419)	(0.410)	(0.416)	(0.011)	
Basic sanitation	0.895	0.882	0.878	0.892	-0.011	0.473
	(0.307)	(0.322)	(0.327)	(0.311)	(0.009)	
Not crowded	0.605	0.607	0.606	0.582	-0.009	0.284
	(0.489)	(0.489)	(0.489)	(0.493)	(0.012)	
No geocoded location	0.101	0.107	0.119	0.113	0.014*	0.532
	(0.302)	(0.309)	(0.324)	(0.316)	(0.008)	
Days to assignment	85.145	87.947	87.532	86.006	2.217**	0.512
	(46.590)	(48.842)	(49.760)	(49.265)	(0.943)	
Online	0.842	0.851	0.847	0.850	0.007	0.955
	(0.364)	(0.356)	(0.360)	(0.357)	(0.010)	
Created a lease pre-AyA	0.074	0.090	0.083	0.061	0.004	0.001
	(0.261)	(0.286)	(0.276)	(0.240)	(0.007)	
Prior application	0.114	0.101	0.103	0.106	-0.009	0.702
	(0.317)	(0.301)	(0.304)	(0.308)	(0.008)	
Score	368.986	367.621	367.126	370.774	-0.731	0.439
	(83.921)	(83.571)	(80.835)	(82.208)	(1.790)	
PHA in the County	0.477	0.470	0.495	0.453	-0.003	0.093
	(0.500)	(0.499)	(0.500)	(0.498)	(0.013)	
Observations	1,859	1,631	1,618	1,518	6,626	6,626
Test F (p)	0.248					

TABLE I. Balance in Follow-up Sample

This table examines balance across treatment groups in the follow-up survey sample. Columns 1–4 report mean characteristics for each treatment arm (T1–T3) and the control group. Column 5 reports the difference between all treatments pooled and the control group. Column 6 reports the p-value from an F-test of equality of means across groups. Standard errors are in parentheses.



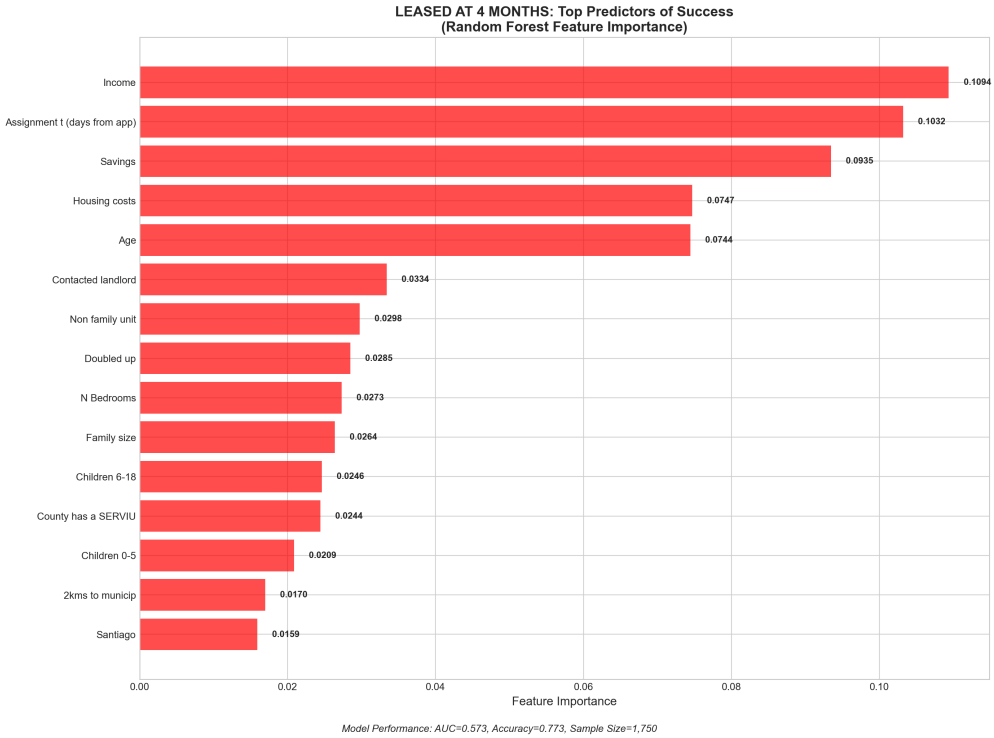
(a) Lease Attempts 4m



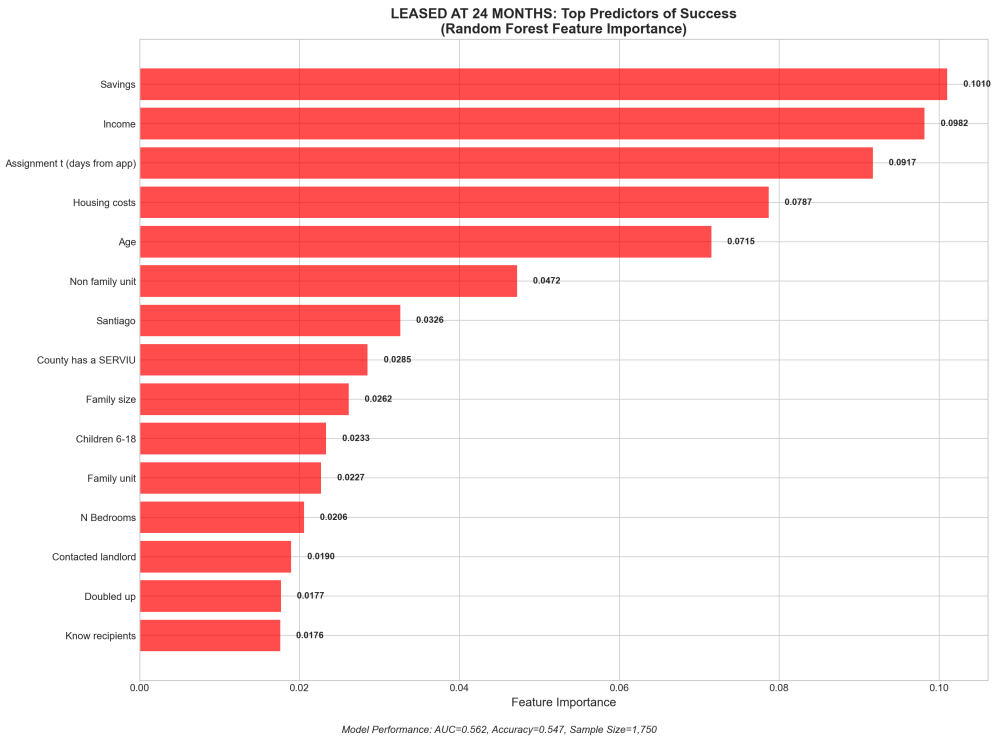
(b) Lease Attempts 24m

FIGURE II. Feature Importance for Voucher Utilization (Lease Attempt)-December's Cohorts

This figure replicates the analysis in Figure 5 using the sample of voucher holders in the control group from December's cohorts. See Figure 5 for details.



(a) Successful Lease-up 4m



(b) Successful Lease-up 24m

FIGURE III. Feature Importance for Voucher Utilization (Successful Lease-up)-December's Cohorts

This figure replicates the analysis in Figure 6 using the sample of voucher holders in the control group from December's cohorts. See Figure 6 for details.

Feature	Attempt/Lease-up	No Attempt/Lease-up	Difference
Savings	24.023	23.175	+0.849
Income	13.351	13.640	-0.288
Assignment t (days from app)	105.426	102.278	+3.148
Age	35.717	35.428	+0.289
Housing costs	244246.209	249155.594	-4909.385
Family size	2.661	2.744	-0.083
N Bedrooms	1.643	1.636	+0.007
Children 6-18	0.765	0.795	-0.030
Children 0-5	0.495	0.534	-0.039
County has a SERVIU	0.516	0.458	+0.057*
Contacted landlord	0.510	0.436	+0.075**
Know recipients	0.343	0.279	+0.064**
Cohort 3	0.616	0.652	-0.036
Santiago	0.229	0.291	-0.062**
Non-family unit	0.729	0.651	+0.078***
Doubled up	0.195	0.244	-0.049*
Wait next assignment	0.578	0.548	+0.030
Cohort 4	0.384	0.348	+0.036
Family unit	0.047	0.070	-0.024*

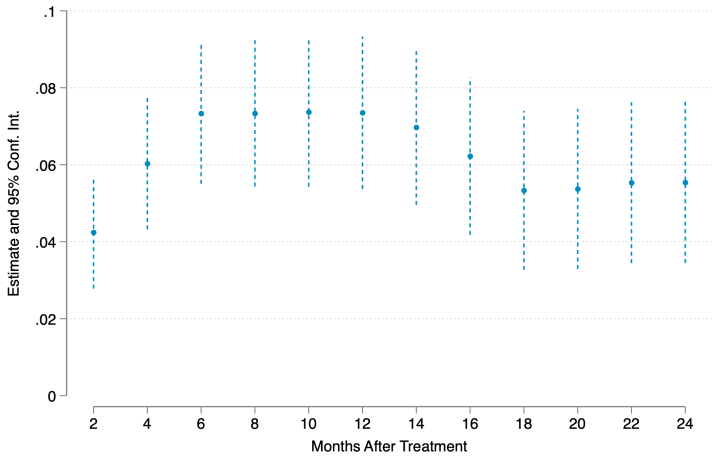
TABLE II. Lease Attempt after 24 Months: December's Voucher Cohorts

This table replicates the analysis in Table 2 using December's voucher cohorts. See Table 2 for details. * p<0.1, ** p<0.05, *** p<0.01.

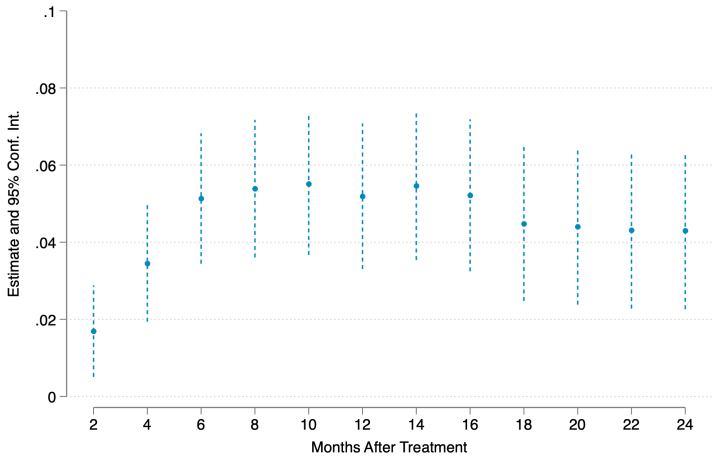
Feature	Attempt/Lease-up	No Attempt/Lease-up	Difference
Income	13.503	13.520	-0.017
Savings	24.613	22.835	+1.778
Assignment t (days from app)	103.752	103.603	+0.149
Age	35.786	35.400	+0.387
Housing costs	242955.829	249701.404	-6745.575
Family size	2.702	2.711	-0.009
N Bedrooms	1.648	1.633	+0.016
Children 6-18	0.772	0.788	-0.016
Children 0-5	0.501	0.527	-0.025
Contacted landlord	0.513	0.439	+0.074**
County has a SERVIU	0.520	0.459	+0.061*
Know recipients	0.349	0.279	+0.070**
Cohort 3	0.644	0.631	+0.013
Santiago	0.214	0.297	-0.083***
Non-family unit	0.749	0.642	+0.107***
Wait next assignment	0.551	0.568	-0.016
Doubled up	0.182	0.249	-0.067**
Cohort 4	0.356	0.369	-0.013
Family unit	0.038	0.074	-0.036**

TABLE III. Successful Lease-up after 24 Months: December's Voucher Cohorts

This table replicates the analysis in Table 3 using December's voucher cohorts. See Table 3 for details. * p<0.1, ** p<0.05, *** p<0.01.



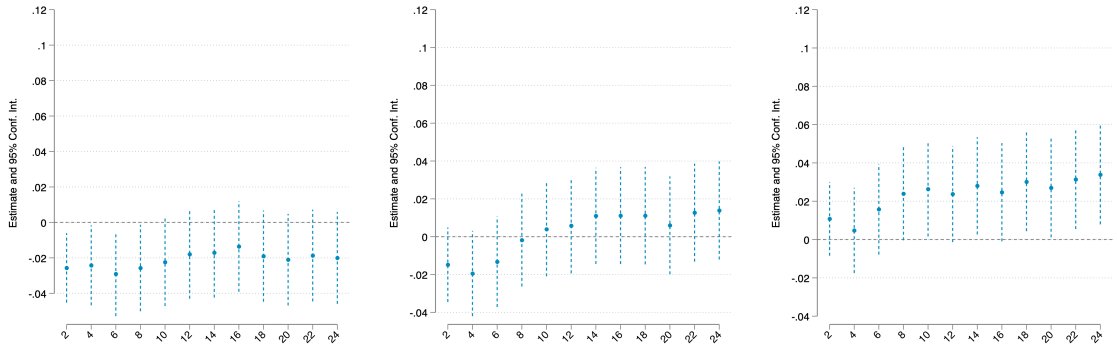
(a) Lease Attempts



(b) Successful Lease-up

FIGURE IV. Event Study

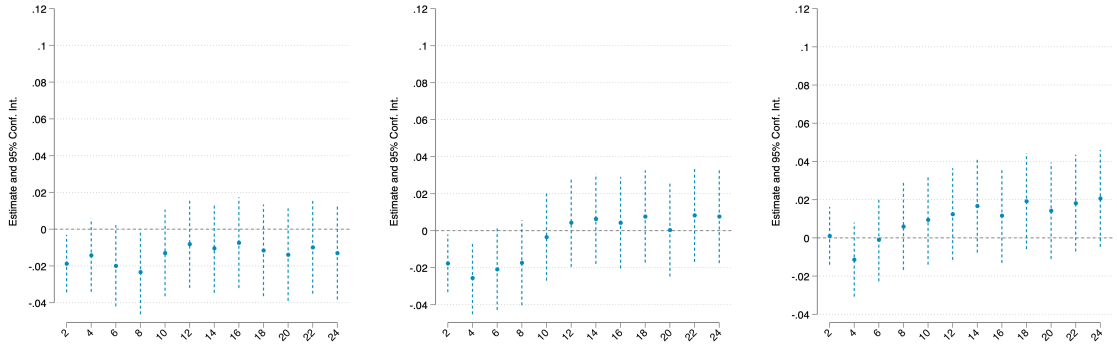
This figure shows treatment effects over the period spanning two to twenty-four months post-treatment. Panel (a) shows the effect of *A*&*A* on lease attempts and Panel (b) the effect on successful lease-ups.



(a) $H_0 : \delta_{T_2} - \delta_{T_1} = 0$ **(b)** $H_0 : \delta_{T_3} - \delta_{T_1} = 0$ **(c)** $H_0 : \delta_{T_3} - \delta_{T_2} = 0$

FIGURE V. Heterogeneity by Treatment Arms - Lease Attempts

This figure compares treatment effects on lease attempts across treatment arms. Panels (a) and (b), respectively, compare the effects of the groups receiving incomplete (T2) and complete (T3) neighborhood information to the most basic information (T1). Panel (c) compares treatment effects between T2 and T3.



(a) $H_0 : \delta_{T_2} - \delta_{T_1} = 0$ **(b)** $H_0 : \delta_{T_3} - \delta_{T_1} = 0$ **(c)** $H_0 : \delta_{T_3} - \delta_{T_2} = 0$

FIGURE VI. Heterogeneity by Treatment Arms - Successful Lease-up

This figure compares treatment effects on successful lease-ups across treatment arms. Panels (a) and (b), respectively, compare the effects of the groups receiving incomplete (T2) and complete (T3) neighborhood information to the most basic information (T1). Panel (c) compares treatment effects between T2 and T3.

	Lease up 4m	Lease up 6m	Lease up 8m	Current Voucher Use
	(1)	(2)	(3)	
All Treat	0.056***	0.097***	0.113***	0.108***
	(0.013)	(0.015)	(0.015)	(0.015)
Treat 1	0.080***	0.131***	0.139***	0.141***
	(0.018)	(0.019)	(0.020)	(0.020)
Treat 2	0.062***	0.096***	0.108***	0.105***
	(0.018)	(0.020)	(0.020)	(0.020)
Treat 3	0.023	0.063***	0.089***	0.076***
	(0.017)	(0.020)	(0.020)	(0.020)
T1=T2=T3	0.019	0.011	0.076	0.016
Control Mean	0.19	0.25	0.29	0.28
Observations	4,284	4,284	4,284	4,284

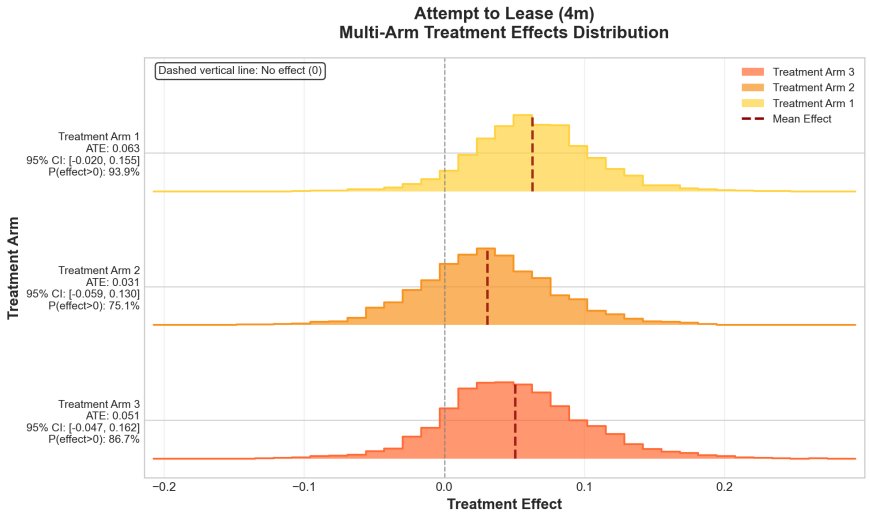
TABLE IV. Effects on Voucher Utilization: Survey Sample

This table reports treatment effects on voucher utilization at the time of the follow-up survey. Columns 1–3 show ITT effects by months 4, 6, and 8 using administrative outcomes, restricting the sample to survey respondents. Column 4 reports the effect on self-reported voucher utilization. The first row pools all treatment arms. Rows 2–4 report separate regressions comparing each arm (T1, T2, T3) to the control group. Row 5 reports the p-value from an F-test of equal effects across T1, T2, and T3. All regressions include strata fixed effects and baseline covariates described in Section 5.

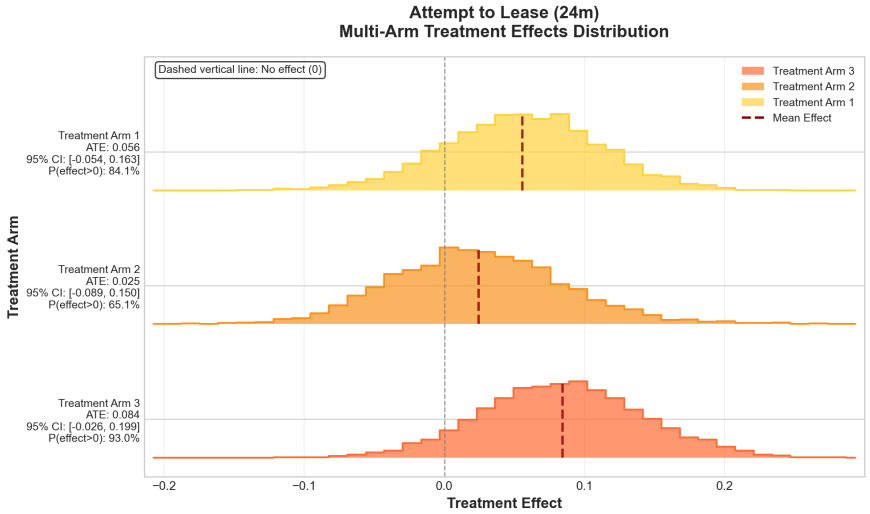
Outcome	N	Control Mean	All Treat	T1	T2	T3	T1=T2=T3	T1=T2	T1=T3	T2=T3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Assignment awareness	6,227	0.787	0.155	0.157	0.149	0.160	0.449	0.374	0.716	0.217
			(0.010)***	(0.011)***	(0.012)***	(0.011)***				
			0.001***	0.001***	0.001***	0.001***				
Voucher Status awareness	4,180	0.681	0.191	0.198	0.198	0.177	0.392	0.968	0.219	0.240
			(0.014)***	(0.017)***	(0.017)***	(0.017)***				
			0.001***	0.001***	0.001***	0.001***				
Unknown housing requirements	4,647	0.260	-0.104	-0.112	-0.099	-0.101	0.685	0.422	0.497	0.916
			(0.014)***	(0.016)***	(0.017)***	(0.017)***				
			0.001***	0.001***	0.001***	0.001***				
Unknown landlord requirements	4,592	0.308	-0.105	-0.108	-0.105	-0.101	0.923	0.857	0.690	0.824
			(0.015)***	(0.018)***	(0.018)***	(0.018)***				
			0.001***	0.001***	0.001***	0.001***				
Reason: Not know how	2,734	0.121	-0.056	-0.047	-0.058	-0.061	0.666	0.517	0.382	0.838
			(0.013)***	(0.016)***	(0.016)***	(0.015)***				
			0.001***	0.010***	0.001***	0.001***				
Subsidy: Better house	5,421	0.439	0.018	-0.009	0.033	0.032	0.056	0.034	0.044	0.955
			(0.015)	(0.019)	(0.019)*	(0.019)				
			0.158	0.329	0.093*	0.134				
Subsidy: Better neighborhood	5,421	0.338	0.038	0.007	0.047	0.061	0.018	0.039	0.007	0.494
			(0.015)***	(0.018)	(0.018)**	(0.019)***				
			0.047**	0.329	0.047**	0.047**				
Leased up in place	5,016	0.253	0.047	0.064	0.041	0.035	0.278	0.225	0.131	0.740
			(0.014)***	(0.018)***	(0.018)**	(0.018)*				
			0.003***	0.003***	0.041**	0.077*				
Move: Diff unit	5,446	0.145	0.020	0.018	0.026	0.017	0.809	0.585	0.960	0.564
			(0.011)*	(0.014)	(0.014)*	(0.014)				
			0.085*	0.157	0.085*	0.161				
Rent (UF)	4,699	9.148	0.089	0.008	0.121	0.146	0.390	0.283	0.211	0.822
			(0.086)	(0.104)	(0.105)	(0.110)				
			0.629	0.814	0.629	0.436				
Number of bedrooms	5,231	2.400	0.023	-0.029	0.073	0.026	0.021	0.006	0.114	0.205
			(0.027)	(0.032)	(0.036)**	(0.034)				
			0.798	0.436	0.425	0.778				
Crowded	5,216	0.189	-0.030	-0.032	-0.036	-0.022	0.674	0.763	0.555	0.380
			(0.012)***	(0.014)**	(0.014)**	(0.015)				
			0.133	0.225	0.137	0.413				
Access to amenities	5,116	-0.048	0.068	0.046	0.098	0.060	0.411	0.198	0.745	0.356
			(0.031)**	(0.039)	(0.039)**	(0.040)				
			0.041**	0.206	0.033**	0.148				
Disamenities (violence/drugs)	4,780	0.043	-0.052	-0.051	-0.088	-0.015	0.223	0.372	0.384	0.083
			(0.032)	(0.039)	(0.040)**	(0.041)				
			0.101	0.166	0.039**	0.319				
Nearby family	4,860	0.549	0.025	0.002	0.021	0.055	0.042	0.353	0.013	0.112
			(0.016)	(0.020)	(0.020)	(0.020)***				
			0.148	0.407	0.283	0.033**				
Ask their landlord	4,827	0.863	0.041	0.055	0.027	0.039	0.065	0.021	0.173	0.378
			(0.011)***	(0.012)***	(0.013)**	(0.013)***				
			0.003***	0.001***	0.079*	0.017**				
Conducted search	5,637	0.597	0.023	-0.006	0.030	0.046	0.017	0.052	0.006	0.401
			(0.014)	(0.018)	(0.018)*	(0.018)**				
			0.138	0.326	0.128	0.041**				
Current search	5,723	0.529	-0.009	-0.041	-0.002	0.019	0.007	0.044	0.002	0.276
			(0.015)	(0.018)**	(0.018)	(0.019)				
			0.102	0.008***	0.163	0.061*				
Landlord will to accept (new)	3,360	0.579	-0.048	-0.034	-0.046	-0.065	0.498	0.641	0.240	0.479
			(0.020)**	(0.025)	(0.025)*	(0.025)**				
			0.045**	0.161	0.077*	0.041**				
Search: Different county	3,425	0.229	-0.011	-0.030	-0.025	0.022	0.031	0.842	0.016	0.027
			(0.016)	(0.020)	(0.020)	(0.021)				
			0.292	0.138	0.185	0.188				
Search: Same neighborhood	3,425	0.600	0.023	0.056	0.002	0.010	0.063	0.029	0.066	0.733
			(0.019)	(0.024)**	(0.024)	(0.024)				
			0.185	0.050*	0.424	0.372				

TABLE V. Additional Survey Outcomes

This table presents the treatment effects on additional outcomes using the follow-up survey. Each row reports results from regression analysis conducted on different outcome variables. Column 1 shows the number of observations and Column 2 the mean outcome of the control group. Column 3 shows the causal effect of pooling all treatment arms together, and Columns 4, 5, and 6 show the results of regressing the outcome on dummies for T1, T2, and T3. Columns 7, 8, and 9 present p-values of F-tests for different nulls regarding the equality of the effects across treatment arms. All regressions include strata fixed effects and baseline covariates described in Section 5. Standard errors included in parentheses. P-values adjusted for multiple hypothesis testing are presented below standard errors.



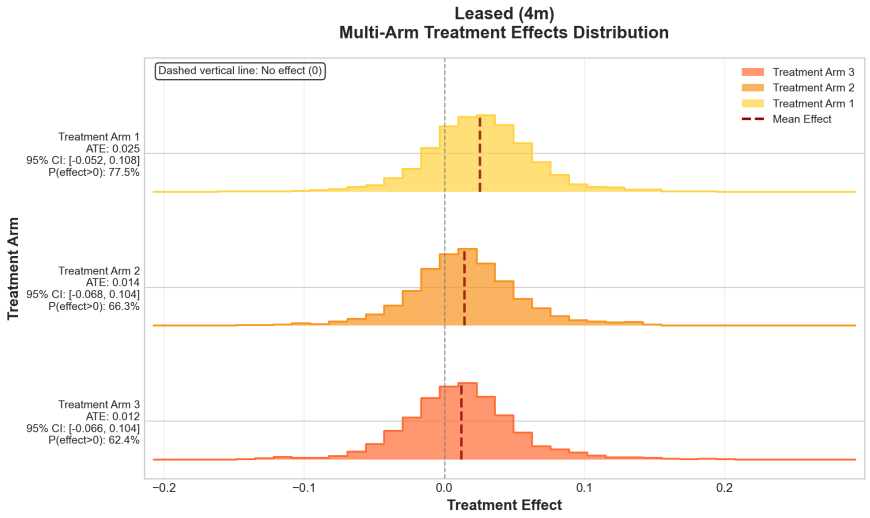
(a) Lease Attempt-4 Months



(b) Lease Attempt-24 Months

FIGURE VII. Distribution of Individual-Level Treatment Effects: July and October Cohorts

This figure replicates the analysis in Figure 7 restricting the sample to participants assigned vouchers in July or October 2022 cohorts (N=6,971). See Figure 7 for details.

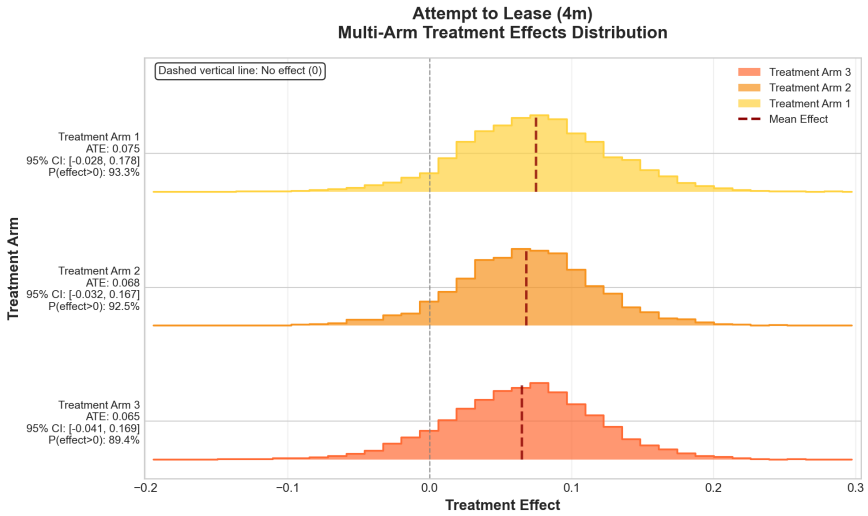


(a) Successful Lease-up-4 Months

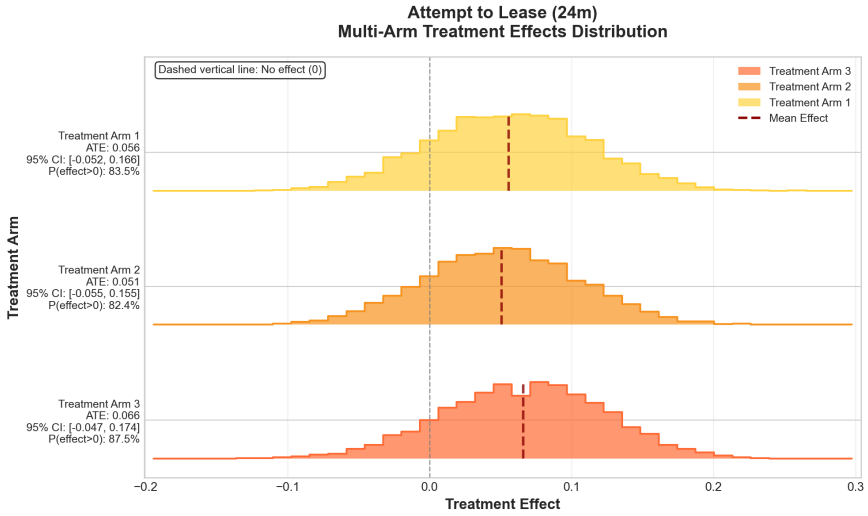
(b) Successful Lease-up-24 Months

FIGURE VIII. Distribution of Individual-Level Treatment Effects: July and October Cohorts

This figure replicates the analysis in Figure 8 restricting the sample to participants assigned vouchers in July or October 2022 cohorts (N=6,971). See Figure 8 for details.



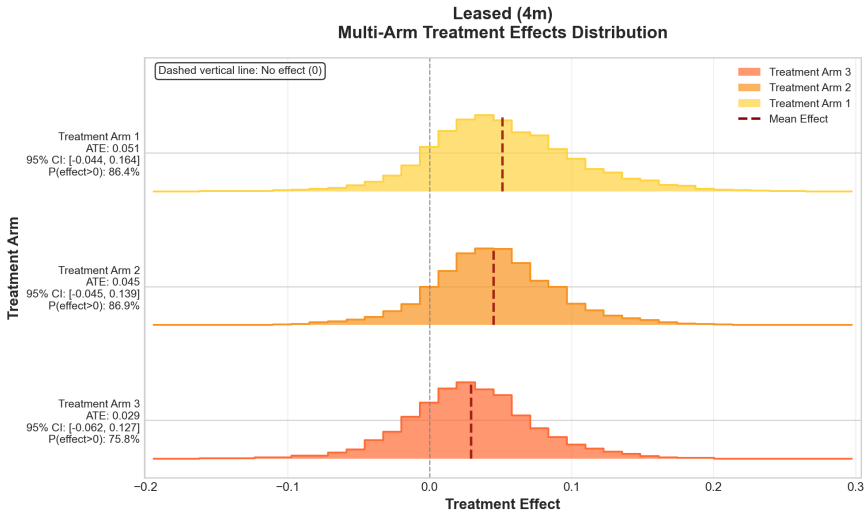
(a) Lease Attempt-4 Months



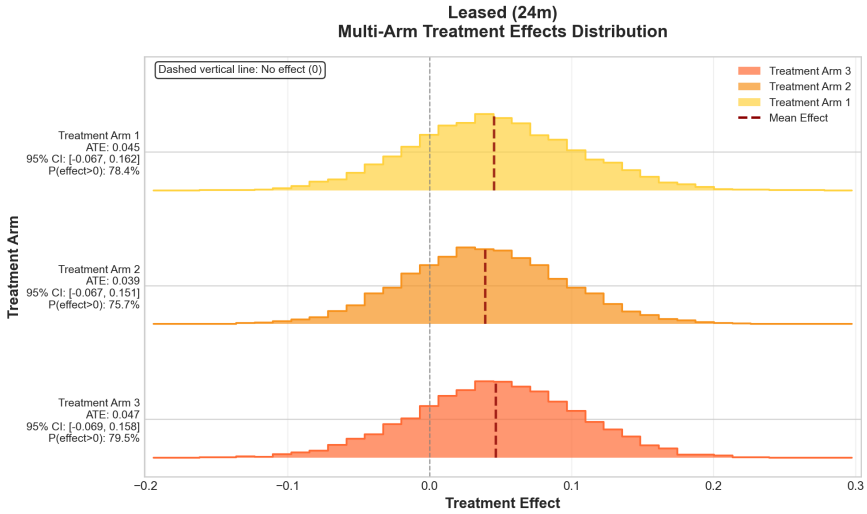
(b) Lease Attempt-24 Months

FIGURE IX. Distribution of Individual-Level Treatment Effects: December's Cohorts

This figure replicates the analysis in Figure 7 restricting the sample to participants assigned vouchers in December 2022 cohorts (N=4,178). See Figure 7 for details.



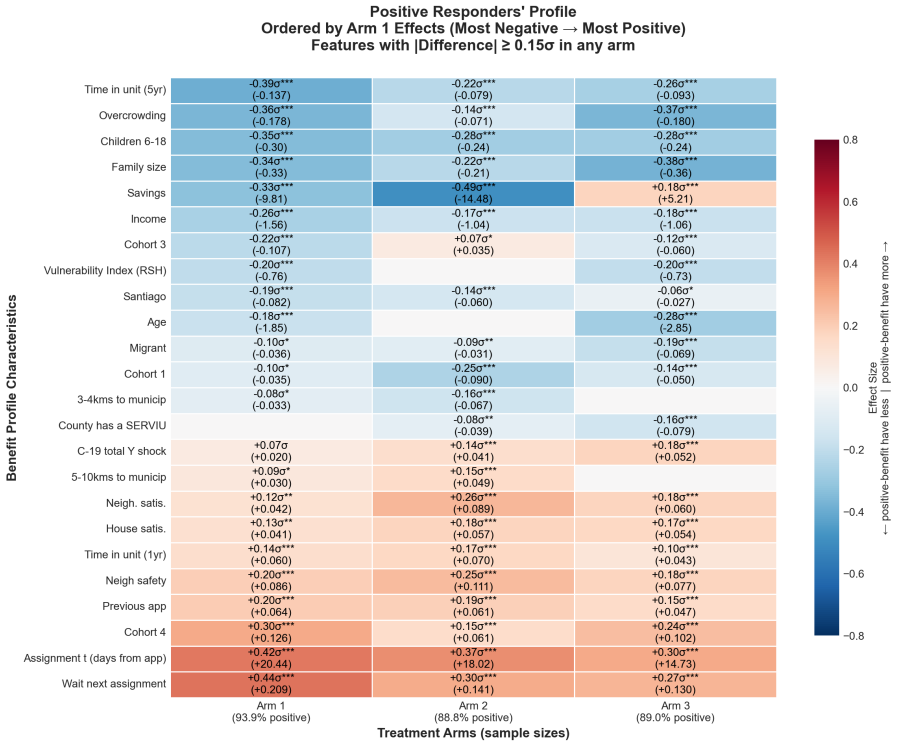
(a) Successful Lease-up-4 Months



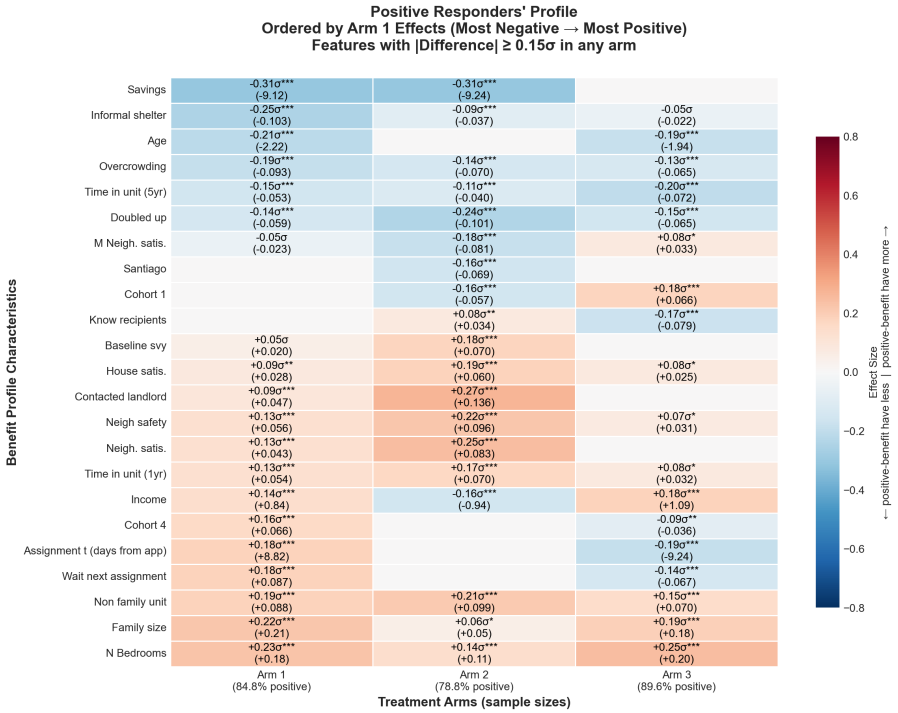
(b) Successful Lease-up-24 Months

FIGURE X. Distribution of Individual-Level Treatment Effects: December's Cohorts

This figure replicates the analysis in Figure 8 restricting the sample to participants assigned vouchers in December 2022 cohorts (N=4,178). See Figure 8 for details.



(a) Lease Attempts After 4 Months



(b) Lease Attempts After 24 Months

FIGURE XI. Benefited Profile-Lease Attempts

This figure replicates the analysis in Figure XI for lease attempts at 4 months (Panel a) and 24 months (Panel b) post-treatment. See Figure XI for details.