The Impact of Temporary Rental Subsidies on Homelessness: A Randomized Controlled Trial

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Abstract

Unconditional, indefinite housing subsidies have been shown to reduce homelessness, but such programs are massively over-subscribed. Policymakers have expanded less expensive, time-limited Rapid Re-Housing (RRH) subsidies to serve more people, with little empirical backing. Time-limited rental subsidies are predicated on the idea that homelessness results from financial constraints that can be addressed with temporary assistance. We conduct a randomized controlled trial that provides temporary rental subsidies to single, homeless adults. During the 30 months after random assignment, the treatment group receives an unconditional average of about $10,000 in financial assistance, even though only about half of those assigned to the treatment group successfully lease a unit with assistance. Preliminary results indicate that this assistance leads to a reduction in homelessness of one third and a reduction in the number of shelter days by two-thirds while the subsidy is active. Preliminary sub-sample results indicate that a substantial portion of these effects persist after the subsidy ends. Long-term follow-up will be available for three-quarters of the sample by summer 2025.

JEL Codes: R28, I38
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1 Introduction

On a given night in 2024, more than seven hundred thousand people slept in emergency shelters, transitional housing, or unsheltered places like sidewalks, bus stops, or cars (De Sousa & Henry 2024). Being homeless has dramatic costs, both for people experiencing homelessness and for the communities addressing the public costs of homelessness. Those experiencing homelessness have worse employment outcomes, more encounters with the criminal justice system, and worse health (Evans et al. 2021). Homelessness increases mortality risk, aging a person as much as 20 years (Meyer et al. 2023). The cost of all public services for someone experiencing homelessness, including welfare services and hospital and criminal justice costs, averages $83,0000 per person per year (Flaming et al. 2015). While national counts of homelessness declined from the start of national counts in 2007 through 2016, they have increased 40 percent since then, with the years 2023 and 2024 setting consecutive records.

At the same time, unconditional rental subsidies for people experiencing homelessness have steadily increased. 'Housing First' models that prioritize immediate rental assistance have expanded and displaced transitional housing programs that condition housing assistance on receipt of mental health or substance use treatment (Evans et al. 2021). The primary option, permanent supportive housing, combines indefinite rental subsidies with voluntary treatment services for people who are chronically homeless. These long-term, unconditional subsidies have proven effective in reducing homelessness (Tsemberis & Eisenberg 2000, Rosenheck et al. 2003) but are expensive and massively oversubscribed. As a result, policymakers have rapidly expanded less expensive, time-limited rental subsidies that can serve more households with the same budget.[1] Rapid Re-Housing (RRH) programs provide rental housing vouchers to rent from a private landlord with a limited term of support, typically between 6 months and 2 years. Local and national RRH programs have added more than 100,000 beds since 2014, almost quadrupling the 37,000 beds allocated to such

[1] See Evans et al. (2021) for a summary of long-term trends in the organization and content of homelessness services.

programs ten years ago (De Sousa & Henry 2024). Despite this sharp growth in the use of time-limited rental subsidies, very little rigorous evidence tests whether they are effective in addressing rising levels of homelessness.

In this paper, we implement a randomized controlled trial to determine whether time-limited rental subsidies improve housing stability for single, homeless adults. The study takes place in Santa Clara County, CA, a county that in 2022 had the 4th largest homeless population in the US. The subsidy program is operated by the County's Office of Supportive Housing (OSH) and a local non-profit service provider. Between July 2018 and November 2023, we randomly offered about half of 733 eligible individuals a spot in the program, which includes rental assistance and case management. We examine outcomes during three distinct periods: 1) the enrollment period, or the initial 6 months after being offered assistance, when most participants are still finding housing to lease; 2) the subsidy period, or the window from 6 to 18 months after study enrollment, when the rental subsidy is typically active; and 3) the post-subsidy period, or the window from 18 to 30 months when subsidies from the program have ceased for most participants.

The randomized controlled trial (RCT) design results in a sharp difference in treatment intensity between the treatment and control groups. When offered temporary subsidies, the treatment group is 86 percentage points more likely to enroll in a housing subsidy program and 46 percentage points more likely to successfully pay for an apartment with rental assistance within six months. Within eighteen months of random assignment, the treatment group receives an (unconditional) mean of $8,501 in temporary rental assistance, or $8,166 more than the control group. This gap narrows dramatically in months 18-30, when subsidies have largely ended and the treatment group adds only another $1,313 beyond the control group.

Our estimated treatment effects show that temporary rental subsidies sharply reduce homelessness during the subsidy period. We quantify homelessness using Homelessness Management System (HMIS) data on enrollment in programs throughout the county that admit

people who are already homeless. Those offered rental subsidies are 12 percentage points less likely to enter emergency shelter, permanent supportive housing, and street outreach programs, about a one-third decline from a 38% rate in the control group, and this difference is statistically significant. Most of this decline results from a drop in emergency shelter use and street outreach. For example, 27% of the control group enters an emergency shelter between 6 and 18 months after study enrollment, compared to only 19% of the treatment group.

To understand the merits of a temporary subsidy program relative to alternative interventions that aim to reduce homelessness, it is important to determine whether the sharp reduction in homelessness that we find during the subsidy period persists after the subsidies stop. While we do not yet observe outcomes for the full sample for the post-subsidy period, a sub-sample of early enrollees provides a preliminary test of whether gains in housing stability persist.[2] For participants where we observe outcomes for at least 30 months after random assignment (about 60% of the full sample), we find that those offered temporary subsidies are ten percentage points less likely to be homeless in the post-subsidy period (statistically significant at the 5 percent level), an almost 30 percent decrease relative to the control group mean.

This paper provides some of the first experimental evidence on the effect of temporary housing subsidies for people already experiencing homelessness. Several experiments show that indefinite subsidies can reduce homelessness; see Evans et al. (2021) for a summary. A smaller but growing literature finds that temporary financial assistance for people who are not yet homeless can also reduce emergency shelter entry (Evans et al. 2016, Phillips & Sullivan 2022). But rigorous evidence testing whether temporary rental subsidies are sufficient to reverse the situations of people already experiencing homelessness is very limited. Most of the best available studies find positive effects but require selection-on-observables assumptions (Rodriguez & Eidelman 2017, Blackwell & Santillano 2023). One exception is

[2]As shown in A.2b in the appendix, we will have long-term outcomes for the full sample in May of 2026.

the 'Community-Based Rapid Re-housing' arm of the seminal Family Options Study (Gubits et al. 2018), which randomly assigned temporary subsidies versus usual care for families living in emergency shelter and found no lasting statistical differences in housing outcomes. We find different results, but the intervention we examine differs from the one considered in Family Options in two important ways that could explain these different findings. First, we study people without children, rather than families. The average participant is a 48 year-old man with no children who has spent 75% of the past three years homeless; 47% have previously interacted with street outreach, and 78% have been to jail. The effectiveness of temporary subsidies may be different for this group and the families enrolled in Family Options. Second, our study and Family Options differ in the programming available to the control group. In the Family Options usual care group, about 22% of participants accessed RRH services anyway. By contrast, single individuals in Santa Clara County during our study period had few available alternatives, making such crossover very rare. Despite similar sample sizes and because of differences in the available usual care, our study has greater statistical power to detect persistent effects of temporary subsidies.

Our work also contributes to a much larger literature in economics on the effects of housing subsidies on individual behavior. In addition to being expensive, long-term housing subsidies have been shown to decrease labor supply among beneficiaries (Jacob & Ludwig 2012, Gubits et al. 2018), and more generous subsidies have been shown to increase the rents of landlords with minimal impact on neighborhood and unit quality for the voucher holders (Collinson & Ganong 2018). Aside from being less expensive, temporary subsidies are less likely to induce these negative side effects (Zhang 2023). Our results indicate the extent to which the known benefits of long-term subsidies can be retained while reducing potential downsides of generosity.

Most broadly, our results shed light on a key contemporary question in public policy: why do people remain homeless? Both popular opinion and policymakers debate the extent to which homelessness is driven primarily by financial constraints or non-financial considera-

tions, such as mental health issues and substance use. If homelessness results from permanent problems, either non-financial considerations or a persistent mismatch between income and the cost of living, then temporary assistance is not likely to have a sustained effect. That we find some evidence that temporary subsidies reduce homelessness even after subsidies end indicates that a significant fraction of homelessness results from financial constraints and particularly from temporary constraints that can be overcome with time-limited support.

2 Rapid Re-Housing in Santa Clara County, CA

2.1 Santa Clara County, CA

Santa Clara County is perhaps best known as the heart of Silicon Valley, home to numerous technology giants such as Google and Facebook. It is the most populous county in the San Francisco Bay area region and is one of the most affluent counties in the nation. Despite its affluence, according to HUD's 2022 Point-in-Time counts, Santa Clara County has the 4th largest homeless population in the country and the highest percentage of unsheltered homeless persons (De Sousa et al. 2022). While people experiencing homelessness nationwide face numerous challenges in obtaining and maintaining stable housing, the individuals experiencing homelessness in Santa Clara County face particularly difficult obstacles. At the start of the study in July of 2018, Santa Clara County had the 5th highest median rent in the country and remained among the top five highest rental rates through 2023.[3] Furthermore, Santa Clara's vacancy rates remain historically low, averaging only 5% over the past decade.[4]

[3]HUD rent estimates at the 50th percentile for all Fair Market Rent areas can be found at https://www.huduser.gov/portal/datasets/50per.html

[4]Estimate from the Census' Housing Vacancies and Homeownership (CPS/HVS) at http://www.census.gov/housing/hvs/data/rates.html

2.2 The Rapid Re-Housing Intervention

While the structure of Rapid Re-Housing programs can differ across communities, the primary component of these programs is a temporary rent subsidy, typically lasting 12-18 months. They may provide some non-financial support as well, but unlike traditional 'Transitional Housing' programs, they focus primarily on rent payment and do not condition rent payment on compliance with receiving other services, like mental health or substance use treatment. Unlike longer-term programs, like housing vouchers or permanent supportive housing, RRH programs limit the duration of subsidies. Given the less intensive financial support, RRH programs are often designed to serve clients with less chronic needs, but the scarcity of long-term subsidies often creates significant overlap between the set of clients served by RRH and more intensive programs.

Rapid Re-Housing has become an increasingly popular option among local governments, including in Santa Clara County. Figure 1 displays federal Housing Inventory Count data covering all homelessness response programs in the United States. RRH programs have increased dramatically from 37,000 beds to 145,000 beds over the past decade. This massive increase in temporary subsidies for market rentals has come alongside increases in long-term subsidies, like permanent supportive housing, and at the expense of traditional transitional housing programs.

The intervention we study is funded by local government and operated by a local non-profit organization. The Office of Supportive Housing in Santa Clara County funds the program via tax revenue. HomeFirst, a long-time provider of housing services in Santa Clara County, implements the Rapid Re-Housing services. The program serves people who are literally homeless by providing them with a temporary rental assistance subsidy that is meant to ease homeless persons' transition back into stable housing. In addition to the rental assistance, participants receive case management services that include assistance in finding a housing unit and can also include assistance generating additional income through employment and public benefit receipt. The program only serves households without chil-

dren, which is a unique feature of the program. While the Office of Supportive Housing funded other Rapid Re-Housing programs during out study period, these other programs tend to prioritize other groups (families, veterans, etc.) who have little overlap with the study sample.

Upon enrolling in the program, staff help a client identify a housing unit with a landlord willing to receive the program subsidy. As shown in Appendix Table A.1, participants who find a unit do so in a median of 2.5 months from enrollment to move-in, but there is considerable variation with a mean of 4 months and a standard deviation of 4 months. While searching, RRH participants are often placed in transitional or interim housing subsidized by the county and are guaranteed a bed in emergency shelters. Finding a unit quickly depends on finding a willing landlord with a unit the tenant would like to rent. Prior research has shown that many landlords are hesitant to rent even to recipients of longer term subsidy programs that do not target those who are homeless(Phillips 2017, Cunningham et al. 2018, Faber & Mercier 2022, Aliprantis et al. 2022), and homeless individuals likely face even greater skepticism.

Because it can be difficult to find landlords who will accept RRH subsidies, the program assists participants with housing search. HomeFirst housing specialists actively identify receptive landlords and locate vacant units. Staff work with landlords to show clients up to three potential market rental units. If the client does not move forward with the third option, the case manager will exit the client from RRH and, depending on the needs of the individual, refer the person to another program. Upon finding a suitable unit with a willing landlord, HomeFirst staff facilitate the process of lease signing and move-in. For example, a housing specialist may sit with a client during a lease signing to explain the legal process and then after the signing connect them to a social service agency that delivers basic furniture.

After signing a lease, the program provides its core element: temporary rental subsidies. Rent payments are made directly to the tenant's landlord by either HomeFirst or a separate county vendor. The monthly subsidy can be the full value of rent but can also be a partial

payment, depending on the income of each client. HomeFirst designed the duration of rental assistance to be 12 months. Appendix Table A.1 confirms that both the median and mean duration from move-in to program exit is 12 months. The duration of assistance can in some cases last up to two years but typically stops after a much shorter period. If a client has not exited the program after one year, HomeFirst has a case conference to assess the situation and decide whether to extend services for a longer period. A key consideration is the client's level of program engagement and the potential to become stably housed. If this is unlikely, the client may be referred to a different program. Extensions were much more likely during the height of the COVID pandemic, when RRH services were extended to account for additional challenges in meeting with clients and prospective landlords.

Though the primary focus and expenses are rental subsidies, this program also provides some limited case management to participants. During the study period, HomeFirst employed one to four RRH case managers, each with a capacity to serve 20-25 clients at any given time. The program required participants to meet every three months with their case managers and an employment specialist. These meetings primarily focused on generating income, discussing employment opportunities and, when applicable, helping participants sign up for or renew public benefits (e.g. social security, disability).

3 Empirical Strategy

3.1 Study Enrollment and Random Assignment

As in most communities, Santa Clara County uses a coordinated entry system to match people experiencing homelessness to the community resources that could best fit their needs. Under this system, individuals experiencing homelessness seek services at various public and private agencies throughout the county. They first complete the standard assessment tool, Vulnerability Index – Service Prioritization Decision Assistance Tool (VI-SPDAT), which assigns each person a score and recommends services and housing options based on that score.

The VI-SPDAT orders people within a waitlist of people eligible for Permanent Supportive Housing and Rapid Re-Housing in the County's Homeless Management Information System (HMIS).

Figure 2 outlines how the study enrollment process operated within the county's existing prioritization system. The study maintained the county's existing eligibility criteria for a RRH program serving single adults. First, the client must already be homeless, according to HUD category 1 or 4 definition of being homeless.[5] Most participants were living in shelters or encampments at study enrollment. Second, the client must not have custodial rights for children (either currently living with them or not) at the time of enrollment. Households may include more than one adult. Third, participants must have a VI-SPDAT score between 4 and 7, the county's typical range for Rapid Re-Housing programs. The most vulnerable clients with scores above 7 are instead eligible for permanent subsidies.

Whenever HomeFirst has capacity to enroll new clients, Santa Clara County uses this waitlist to identify potentially eligible clients and refer groups of them to HomeFirst. Prior to the start of the study, the county prioritized individuals within the eligible range (4-7) by VI-SPDAT score, referring first those who scored a 7, then those who scored a 6, and so on. Furthermore, they prioritized within VI-SPDAT score according to the risk sub-score (one of four VI-SPDAT sub-scores). As a final tiebreaker, the County referred those with the most days homeless among clients with otherwise equal scores.

The evaluation changed how clients were chosen among this set of eligible people, intentionally recruiting the full range of eligible VI-SPDAT scores. When HomeFirst had capacity, the county would refer lists of eligible clients. Each list would have an equal number of clients who scored a 7, 6, 5, and 4 on the VI-SPDAT. Within a VI-SPDAT score, the lists created by the county continued to prioritize those with higher "risk scores" and days homeless. When HomeFirst exhausted the list of potential participants, the county would share a new list.

[5]HUD category 1 homelessness covers people who lack a fixed, regular, and adequate nighttime residence, i.e. people who slept in places not mean for human habitation or emergency shelters (or institutions like jails or hospitals immediately after having been homeless). HUD category 4 homelessness covers people fleeing domestic violence who have neither another residence nor sufficient support to obtain other housing.

Program staff contacted individuals referred by the county and enrolled them in the study. Case managers and outreach workers would make three attempts to connect with a potential client and remove them from the list if unsuccessful.[6] Upon locating individuals, they would determine interest in participating in the study. Among those interested, they would setup an in-person meeting[7] and complete an intake form on SurveyCTO, which caseworkers accessed through a mobile app on a computer tablet. The intake process confirmed program eligibility criteria, documented informed consent to research, and collected baseline information on the person's housing history and access to government assistance.

At the end of this intake process, study participants were immediately randomly assigned to the program or usual care. The tablet computer assigned clients randomly on the spot to either a treatment group offered housing subsidies or a control group referred to usual care. Stratification and the probability of treatment varied over time. Initially, we used independent draws with probability one-half. We quickly shifted to random assignment stratified by study entry time with alternating strata of six and four consecutive participants.[8] Alternating strata sizes prevented long strings of assignments to one group but also made treatment assignment difficult to predict. As of March 26 2020, during the COVID-19 crisis, there was no longer excess demand for the RRH program, likely because of newly available housing supports made available during the emergency. As a result, we suspended the randomization process, and all eligible participants were assigned to receive RRH services. We do not include these participants in the analysis below. When random assignment resumed in August 31, 2020, we randomized within alternating strata of six and three consecutive par-

[6] A reasonable attempt usually means calling someone if they have a contact number or visiting the places frequented by a known client. Staff would also check HMIS records and contact homeless shelters or colleagues at other organizations to make contact with a potential client who had recently received services, as recorded in HMIS.

[7] For a few months during the height of the COVID-19 pandemic, with the approval of both the SCC IRB and Notre Dame's IRB, case managers completed the intake form and informed consent process over the phone.

[8] During the first months of the study, caseworkers enrolling study participants encountered a string of control assignments. Even though treatment assignments were likely to balance in the future, this became a concern for the team of caseworkers tracking eligible clients. The change to randomize within strata ensured that study group assignments would balance out more quickly for each caseworker. This change did not alter the 50/50 chance of a study participant to get RRH services.

ticipants, applying an altered ratio of two participants placed into the treatment group for each client placed in the control group. This ratio allowed us to align RRH enrollments with HomeFirst's capacity to take on new clients. We returned to a 1:1 randomization ratio on October 1, 2021. In summary, all people in our final study sample were randomly assigned, but stratification and the probability of treatment changed over time.

While the treatment group was immediately enrolled in the Rapid Re-Housing program described above, usual care options for control group participants were relatively limited. Individuals could only be enrolled in the study and randomized once; thus, existing control group participants were not eligible to be referred to this RRH program for a second time during the study period. At the close of the study, HomeFirst's RRH program for singles stopped enrolling new clients. Options with other programs were also rare. While several other county programs focus on families with children, no other Rapid Re-Housing programs dedicated to singles were available during the study period, though some narrower programs, e.g. for veterans, that partially overlap with the study sample were available. This situation changed somewhat after the end of our study treatment but before the end of our long-run follow-up period, at which time local Rapid Re-Housing programs appeared that focused on enrolling people displaced after the removal of street encampments. These latter programs are relevant for our study population but, because they come after the end of treatment subsidies, are better categorized as long-run outcomes (indicating that the person was living in an encampment) than substitutable treatments.

3.2 Data

Our primary data source is Santa Clara County's Homeless Management Information System (HMIS). HMIS provides baseline characteristics and program enrollment outcomes. In HMIS we can observe demographic characteristics, VI-SPDAT scores, shelter entry, and access to other temporary housing and homelessness prevention services. HMIS collects client-level data from all publicly contracted homeless service providers in Santa Clara County, allowing

us to observe date-specific service records for both treatment and control group participants. We use these service records to measure homelessness, as indicated by using services for people who are literally homeless. In future work, we will also use HMIS as the primary database to merge with other County records.[9]

We measure our primary, pre-specified homelessness outcome using program enrollment from HMIS. HMIS tracks enrollment in a variety of programs, including some for people who are housed but at risk of homelessness and others for people who are already homeless. See Appendix Figure A.1 for a taxonomy of homelessness programs. Our outcome measures include only programs for people who are already homeless. In particular, we include enrollment in emergency shelter, street outreach, permanent supportive housing, transitional housing, and safe haven programs. We exclude programs that do not imply homelessness, such as prevention programs and non-housing services other than street outreach. To keep a distinction between the treatment and outcomes, we always exclude the study Rapid Re-Housing program at HomeFirst from the outcome measure. We sometimes include non-study Rapid Re-Housing programs, which for our sample largely appear during long-run follow-up when local programs appeared for people being displaced after street encampment removals. Because we did not anticipate the appearance of these programs in our pre-analysis plan, we show robustness of the results to including versus excluding non-study Rapid Re-Housing programs. For all types of homelessness programs, we use program enrollment dates to measure the fraction of people initiating any such services during a particular time period.

We add baseline survey data from study enrollment. Study participants go through an intake form with their caseworker immediately before randomization. The survey asks for demographic information, length of homelessness, income, and access to health insurance and public benefits. As an additional check, we compare HMIS outcomes at baseline with the self-reported information from the survey.

[9]The research team does not access PII directly. We keep track of enrollment and treatment status via the HMIS ID. OSH data specialists will use HMIS ID, name, SSN, date of birth, and other characteristics to link HMIS data with other County records.

3.3 Regression Specification

Because we assign treatment randomly, we can compute treatment effects as simple intent-to-treat comparisons of means. We also control for strata fixed effects to account for the experimental design and pre-specified control variables to potentially improve precision by removing residual variance. For our main specification, we estimate:

$$Y_i = \alpha + \beta T_i + \mathbf{X_i}\delta + \epsilon_\mathbf{i}, \tag{1}$$

where Y_i is an outcome, such as an indicator for using homelessness programs within 1 year of random assignment; T_i is an intent-to-treat indicator of treatment group assignment (assigned to Rapid Re-Housing); X_i is a vector of controls; and ϵ_i is a random error term. The vector of controls measured at baseline includes the outcome in the year prior to study enrollment (if available), years since first becoming homeless (winsorized at the 95th percentile), total months homeless in the past three years, total earned income (winsorized at 95th percentile), total income (winsorized at 95th percentile), age, age squared, and indicators for VI-SPDAT scores, female, and month of study enrollment. These controls were pre-specified in our pre-analysis plan. We also include a set of indicators for strata of random assignment, which was omitted from our original pre-analysis plan but is necessary for proper inference. For our main results, we show in Appendix Table A.5 that accounting for stratification instead using randomization inference does not affect our results. We estimate our main specification by ordinary least squares and report heteroskedasticity-robust standard errors with no clustering.

3.4 Study Sample

We enrolled 733 participants in the study between July 2018 and November 2023. Of these, 373 were assigned to the treatment group receiving RRH services and 360 to the control group receiving usual care. The COVID-19 pandemic and other administrative issues led

to no or very slow study enrollment during 2020 and the first half of 2021. See Appendix Figure A.2a for cumulative enrollment trends. Overall, we enrolled 290 study participants before the COVID shutdown, 71 from the time we resumed the lottery through the end of 2021, and 372 from 2022 onward. As noted above, everyone in this sample must be eligible for RRH, currently experiencing homelessness, not living with dependent children, and must consent to participate in the study.

In Table 1, we report the means for observable characteristics for both the treatment and control groups. In the third column, we report the difference in means across these groups. These differences are regression-adjusted differences to account for the randomization strata (but not other pre-specified controls), so they may not exactly match the raw difference between the first two columns. If random assignment was successful, we would expect to see only small mean differences that are statistically indistinguishable from zero. Prior overall use of homelessness services, VI-SPDAT score, housing history, and most demographics are balanced. Only differences in one race/ethnicity indicator and ever enrolling in street outreach prior to random assignment are statistically indistinguishable from zero. These two observed imbalances are not a major source of concern. Given a large list of variables, it is not surprising to find at random at least one variable with a significant difference. Also, any differences in baseline characteristics do not point to likely differences in follow-up outcomes; the first row of Table 1 shows the rate of entry into homelessness services we would predict based on baseline characteristics, and it is similar across the two groups.[10]

Table 1 also shows that this program serves individuals with significant barriers to stable housing. By definition, everyone is experiencing homelessness at baseline and has a VI-SPDAT score in the RRH range of 4 to 7. This group is intended to be a middle range,

[10]To compute this value, we subset the sample to our control group and run a linear regression of our primary outcome of admission to homelessness services (within 6 to 18 months of random assignment), which includes admission to shelter, street outreach, permanent supportive housing, transitional housing, or safe haven, on income, VI-SPDAT scores at baseline, dummies for female, Hispanic, black, strata fixed effect and pre-randomization shelter admission, homelessness prevention, rapid rehousing, permanent supportive housing, services only, street outreach, and transitional housing. We use the coefficients from that regression to predict fitted values for the full sample.

while people scoring over 7 with greater barriers are directed to permanent subsidies. Still, two-thirds of study people assigned to control have previously resided in emergency shelter, and almost half have recorded contact with street outreach workers. On average, they have been homeless for most of the last three years, three-fourths have been in jail, more than ten percent suffer from mental health or cognitive issues, and about one-fifth say they have been removed from an apartment or program because of substance abuse. Other demographics are typical of single individuals who have experienced homelessness in Santa Clara County. The average age is almost 50 years old, and 70% are male. The group is racially diverse: 45% Hispanic, almost 20% non-Hispanic Black, and about 5% Asian. The study participants monthly income, which includes earned income as well as income from government benefits, is about $750.

4 Results

4.1 Receipt of Rental Assistance

The fraction of those in the treatment group who receive rental subsidies rises in the six months after random assignment, as more participants find a unit to rent, but then falls in subsequent months as some exit the program. Figure 3a shows the cumulative share of participants who have ever received rental assistance from any program covered by HMIS, by treatment status and months since random assignment. In the first month, few rent payments have yet been made as households search for an appropriate housing unit and a landlord willing to accept payment from the program. By 6 months after random assignment, 86% of people assigned to treatment have administratively enrolled in Rapid Re-Housing and 48% have received some amount of rental assistance, compared to less than 5% of those assigned to the control group. The treatment share only increases marginally as we extend to 30 months after random assignment. Figure 3b shows the dynamics of rent payment as characterized by the unconditional mean of the flow of rental payments over time. Rental

payments begin at low levels but rise sharply over the next six months as households begin leases. By a year and a half after random assignment, payments have fallen dramatically from their peak as households reach program time limits.

We use this information derived from the HMIS data on the typical timing of rental assistance to define study outcome periods. We split the main outcomes into three distinct periods: within six months of random assignment (short-run outcomes), from six to 18 months after random assignment (medium-run outcomes), and from 18 to 30 months after random assignment (long-run outcomes). The three distinct periods correspond to different stages of the process: short-run outcomes show the take-up of programs right after random assignment but before many in the treatment group have found a unit to rent with the subsidy; medium-run outcomes measure housing stability during the period when many in the treatment group are receiving Rapid Re-Housing payments, and long-run outcomes measure housing stability after Rapid Re-Housing payments have largely ended. We choose these splits consistent with our pre-analysis plan based on the average six-month lag between random assignment and the first rent payment and the typical one-year time span between the first and the last rent payment among those who enroll in RRH (see Appendix Table A.2).

Table 2 quantifies the flow of rental assistance more precisely during these three periods. Within the first six months, 86% of the treatment group has enrolled in a RRH program, nearly always the study program, compared to only 8% of the control group, mostly due to other programs.[11] During this time, 48% of the treatment group has begun receiving financial assistance from Rapid Re-Housing. Incomplete take-up of assistance typically reflects inability to locate a housing unit and willing landlord, though some participants also fail to follow through on their initial interest in the program.

The gap in assistance amounts widens in the medium run and, as expected, shrinks in the 18 to 30 months after random assignment, when RRH payments expire. Specifically, between

[11]See appendix A.4 for discussion of the few cases of control group enrollment in the study RRH program.

6 to 18 months after random assignment, people in the treatment group receive $3,577 more in rental assistance and $4,826 more in financial assistance overall. After 18 months, these differences shrink to $890 and $1,313, respectively. Similarly, the rate at which the treatment group receives any assistance at all is similar in the short (49%) and medium (51%) run, but much lower in the long run (18%). By about 18 months after random assignment, the temporary rental subsidies have largely run their course.

4.2 Treatment Effects for Homelessness

We measure homelessness using enrollment records for housing services that are only available to people who are homeless. Our primary outcome includes enrollment in emergency shelters, permanent supportive housing, street outreach, and transitional housing. For any given time frame, we measure indicators for whether a person began enrollment in the relevant set of programs during that time period. In these preliminary results, for each period, we restrict the sample to the individuals we can observe for at least that many months since random assignment.

Figures 4a and 4b preview the main result, showing that RRH reduces homelessness. They show the average number of new shelter enrollments, first cumulatively and then the flow, by months since random assignment. In the first month, people assigned to treatment briefly enter emergency shelter at higher rates, likely because they have priority access to shelter during housing search. After this initial period, people assigned to treatment have lower rates of enrollment in other homelessness services. This gap persists over time, though attenuating slightly.

Table 3 shows short-run outcomes in the six months after randomization, when many treatment households are still leasing up. Even within the first 6 months, placement in RRH also has an effect on the use of some other services. In the top row, we notice that the treatment group is 7.6 percentage points less likely to be homeless, as measured by enrollment in other programs. Most of this decrease results from less use of emergency shelters and street

outreach. This decline in emergency services is partially offset as people assigned to RRH are more likely to receive transitional housing, consistent with the structure of the RRH program–people approved for RRH have priority access to some transitional services while looking for a landlord to accept their RRH subsidy.

Homelessness also declines when subsidies are available to the treatment group. Table 4 shows results for months 6 to 18. Those who are assigned to RRH have an 8 pp lower rate of entry into emergency shelter, a 3 pp lower rate of entry into permanent supportive housing, and about a 10 pp lower rate of contact with street outreach, but continue to have (statistically insignificant) greater entry into transitional housing programs. Our overall measures indicate that RRH leads to a 12 to 13 pp decrease in homelessness. These effects are large, particularly when considering that only half of the treatment group successfully accesses financial payments. For example, shelter entry declines from 27% in the control group to 19% in the treatment group.

Table 5 provides a preliminary test of whether gains in housing stability persist after housing subsidy payments end. Because we restrict the sample to people we can observe for at least 30 months since random assignment, the sample is much smaller (just under 60 percent of the full study sample), and statistical power is more constrained. However, even with this smaller sample, we find evidence of reductions in homelessness in the long run. Homelessness falls by either a statistically insignificant 6 percentage points or a statistically significant 10 percentage points, depending on whether we include non-study Rapid Re-Housing programs in our outcome measure. As discussed above, non-study RRH programs are best thought of as substitutes for the study program in the short run but as an outcome in the long-run. Non-study RRH programs at long-run follow-up directly enroll participants from cleared street encampments, indicating homelessness at that time. Thus, our preferred estimates include them and indicate a 10 percentage point decline in homelessness in the long-run. However, this result uses data on only about half of the study sample and should be interpreted with caution.

4.3 Heterogeneous effects

We also examine whether the treatment effects differ across various groups. Because many communities use a VI-SPDAT score to target access to RRH, we consider whether there are heterogeneous treatment effects across different scores. Figure 5a shows treatment effects on homelessness for people with different VI-SPDAT scores; the effects do not vary noticeably or systematically with the VI-SPDAT score. This fact, though, is perhaps not surprising because the score does not appear to be very predictive of subsequent homelessness; the control group mean for people scoring a 7 is 0.388, only 4.6 percentage points greater than that for those scoring a 4. A scoring system that instead predicts risk of homelessness based on HMIS data may do somewhat better at identifying people who are both at risk and have larger treatment effects. We define quartiles of a data-driven index of the risk of homelessness based on HMIS history.[12] Figure 5b shows treatment effects for sub-groups defined by quartiles of that predicted risk. From the control group means, we see that the risk of homelessness is half as large in the bottom quartile as compared to the top quartile, but similar among the top three quartiles. Treatment effects for the top quartile of risk are about twice as large as those for the bottom quartile, which are both smaller and statistically indistinguishable from zero. Treatment effects are also sizable for the 2nd and 3rd quartiles. These results provide some suggestive evidence that there could be benefits from targeting the program to the higher risk groups, but we do not have sufficient precision to reject the hypothesis that the treatment effects are the same across these groups.

We also include preliminary results that examine variation in treatment effects for more detailed subgroups. Figure 6a suggests that there are no significant differences in the receipt of rental assistance by gender, race, VI-SPDAT score, age, behavioral problems, or contact

[12]We predict the outcome - use of non-homelessness prevention services - in the control group using a regression of the baseline value of all primary outcomes, SPDAT score dummies, gender, race dummies, ethnicity dummies, month of interview dummies, and months between baseline and follow-up dummies. We then estimate fitted values for all observations using observed characteristics and the coefficients from that regression. Following Abadie et al. (2018), we avoid endogenous stratification on an estimated index by using a repeated split sample procedure that, repeatedly, defines quartiles using one half of the control group and estimates treatment effects on the other half.

with the criminal justice system. Understanding the well-documented racial disparities and stigma associated with criminal records, this result suggests that the case management aspect of the program and/or the guaranteed rent from the County to the landlord offset any challenges to finding and successfully leasing a unit for Rapid Re-Housing enrollees. Similarly, in Figure 6b we do not observe noticeably different treatment effects across demographic subgroups for the use of homelessness services 6 to 18 months after random assignment, when many in the treatment group are still receiving rental subsidies.

5 Conclusion

This study examines the effect of temporary rental subsidies on homelessness. Over 2.5 years, people offered subsides receives an average of $9,479 more assistance than a comparable control group, with nearly all of these subsidies paid in the first 1.5 years. These subsidies flow to the half of the treatment group that successfully leases a unit that accepts program payments. Preliminary results indicate that these subsidies lead to large differences between the treatment and the control groups in homelessness in the short- and long-run. Treatment group participants were 13 percentage points less likely to be homeless between 6 and 18 months after random assignment, a 33% decline. We do not yet have long-term follow-up for the full study sample, but in the sub-sample for which we observe outcomes after the subsidies expire, we still see that the program leads to a 30% decline in homelessness.

These results suggest that temporary subsidies can be a cost-effective response to homelessness. To consider cost-effectiveness, it is useful to compute the program cost of averting a day of homelessness. While this calculation ignores many potential costs and benefits, it provides one useful metric. Tables 3, 4, and 5 together imply that the treatment group experienced 45 fewer days in emergency shelters. Not all days homeless are spent in shelters; Table 4 implies that 64% of averted homeless spells are from shelters. So, we estimate that the treatment averts about 70 days of homelessness. Table 2 indicates that the treatment

group receives $9,479 of additional financial assistance over the course of the study. Thus, we estimate that during the 2.5 year study period, averting a day of homelessness cost about $129. Because the cost of the temporary subsidy does not extend beyond the study follow-up period, this value only falls if benefits extend to later years. For example, if we assume that the observed effects in months 18 to 30 extend through year 15 when the average sample member is age 65, the cost per homeless day averted falls to $65.

The cost effectiveness of temporary subsidies compares well to the longer-term subsidies that are best supported by existing evidence. For example, a randomized trial (Rosenheck et al. 2003) implies that the prominent HUD-VASH permanent supportive housing program for veterans reduces homeless days by 90 during the 3-year study period at a program cost of $12,390,[13] or $137 per homeless day averted. Similar calculations for the large-scale At Home/Chez Soi permanent supportive housing trial (Goering et al. 2014) and the long-term voucher arm of the Family Options study for homeless families (Gubits et al. 2016) imply costs of $315 and $342 per day, respectively.[14] Cost-effectiveness of permanent subsidies likely does not improve with a longer study window. Treatment effects of permanent subsidies decline over time (Goering et al. 2014), though cost declines proportionally if fading effects results from lower take-up. Overall, cost effectiveness of Rapid Re-Housing for single adults compares favorably to other programs even over moderate time horizons and likely to a greater extent over longer time horizons.

[13]The HUD-VASH study has more detailed cost data available. To be comparable to the present study, we focus on voucher and healthcare costs only, calculating program cost using the reported cost to the government, subtracting out shelter and incarceration costs.

[14]For At Home/Chez Soi, we use treatment effects on shelter use and street homelessness alongside stated program costs. For Family Options, we consider the long-term subsidy arm, averaging the observed treatment effects on days homeless at 18 months and 37 months. For that study, we limit costs to an approximate $25,000 added by long-term vouchers, ignoring savings from other housing programs for comparability.

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Figures and Tables

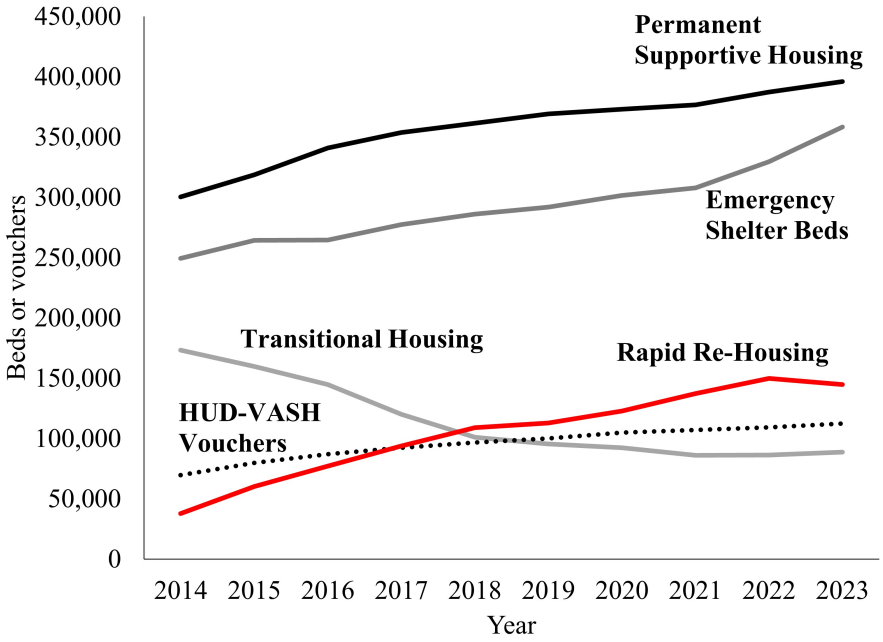


Figure 1: Resources Devoted to Homelessness

Note: PSH, Emergency Shelter, Transitional Housing, and Rapid Re-Housing are taken from HUD's Housing Inventory Count. The number of HUD-VASH vouchers is found in `https://www.hud.gov/sites/dfiles/PIH/documents/HUD%20VASH%20Awards%202008-2023.pdf`

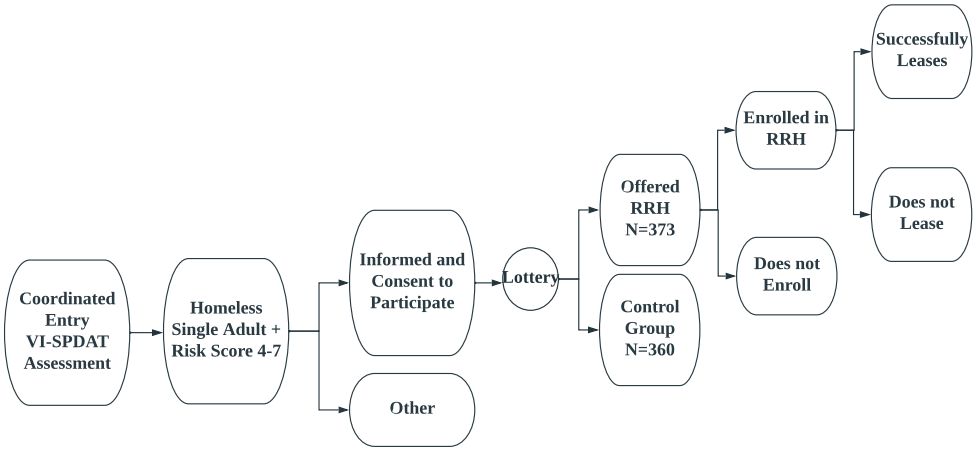
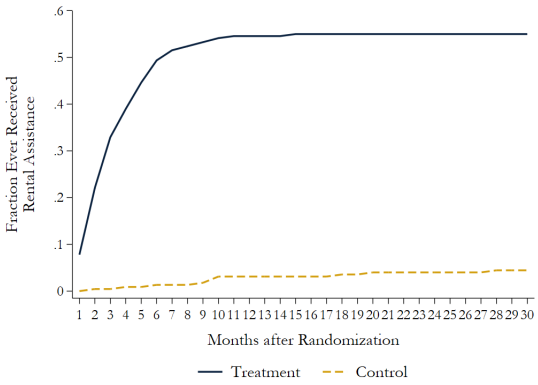
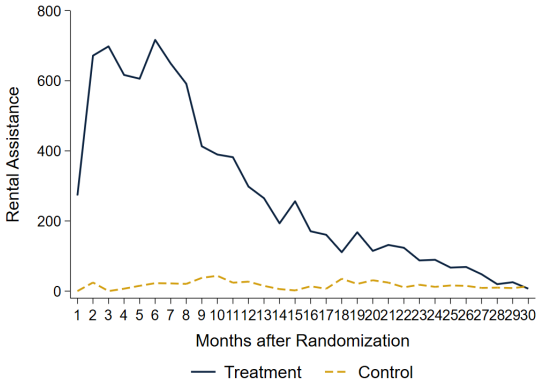


Figure 2: Study Enrollment Process



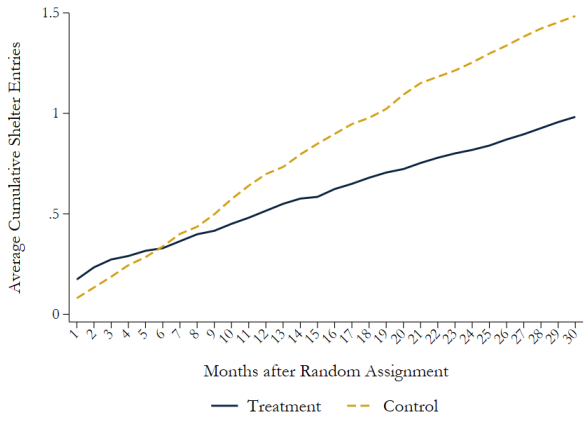
(a) Fraction Ever Received Rental Assistance



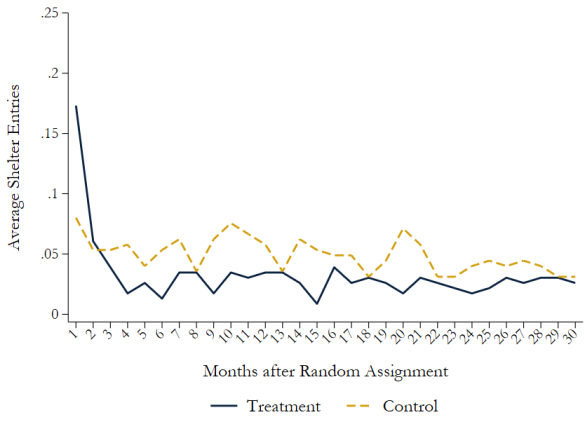
(b) Unconditional Mean Monthly Rental Assistance

Figure 3: Rental Assistance Patterns

Note: data cover financial payments by all programs in Santa Clara County HMIs. Sample limited to participants at least 30 months of follow-up. Study participants with no payments are also included.



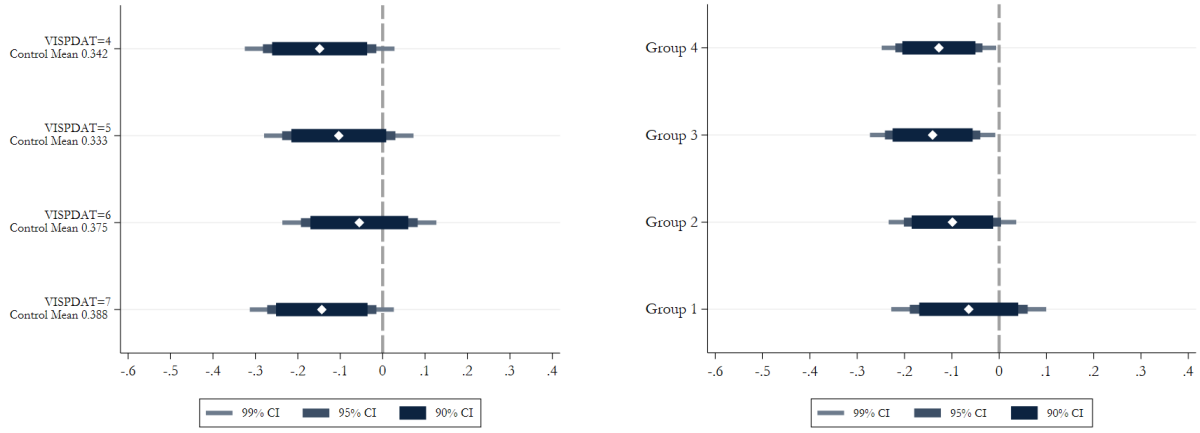
(a) Cumulative Times In a Shelter



(b) Mean Shelter Entries

Figure 4: Entry into Emergency Shelter, by Treatment Assignment

Note: Outcomes are measured using shelter entry dates for all emergency shelters in Santa Clara County HMIS. The sample is limited to participants with 30 months of follow-up in the data.

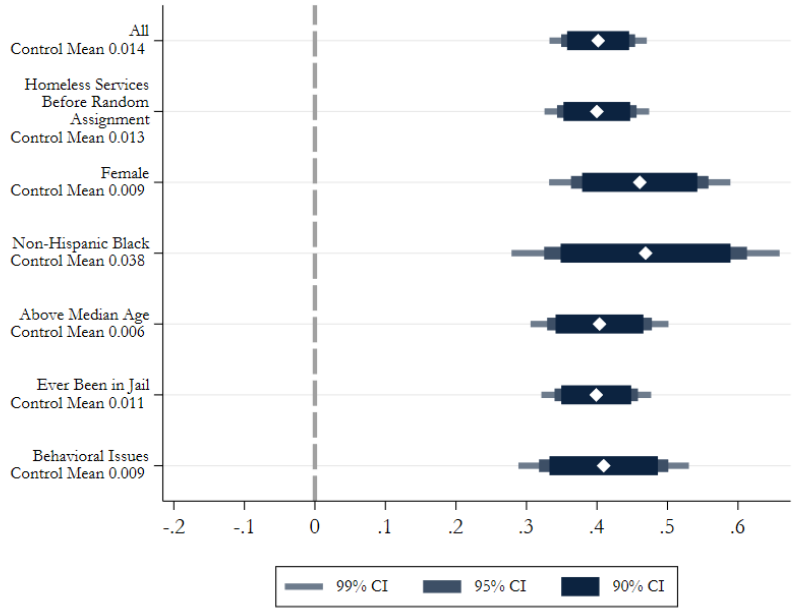


(a) By VI-SPDAT Score

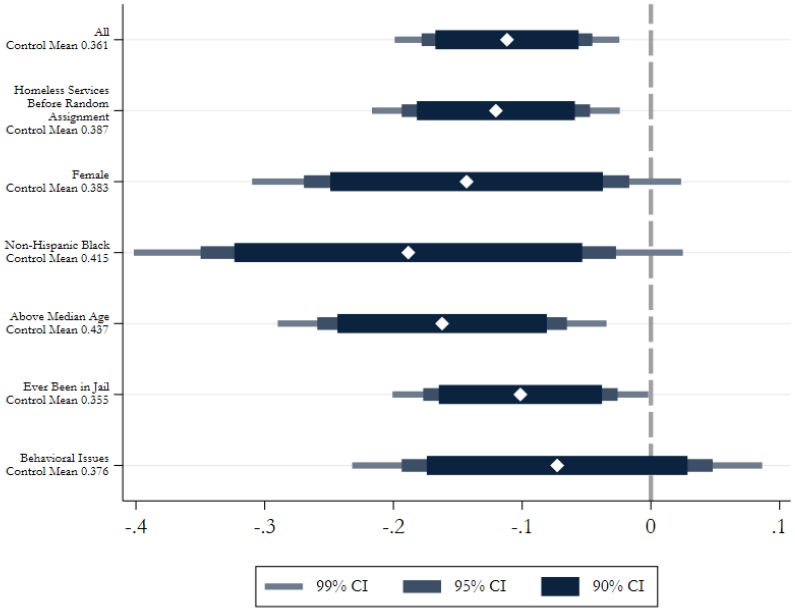
(b) By Predicted Risk of Homelessness

Figure 5: Treatment Effect on Use of Homelessness Services (6 to 18 months) by VI-SPDAT and Predicted Risk Group

Notes: Each plotted point shows the coefficient on a treatment assignment dummy in a regression of our primary outcome of admission to homelessness services, which includes admission to shelter, street outreach, permanent supportive housing, transitional housing, or safe haven, within 6 to 18 months of random assignment, subsetting the sample to the category listed on the y-axis. Figure 5a plots treatment effects for sub-samples of each VI-SPDAT score. Figure 5b plots the treatment effects for sub-samples defined by quartiles of a data-driven predicted risk of homelessness. The predicted values come from a linear regression estimated within the control group of our primary outcome of admission to homelessness services on income, VI-SPDAT scores at baseline, dummies for female, Hispanic, black, strata fixed effect and pre-randomization shelter admission, homelessness prevention, rapid rehousing, permanent supportive housing, services only, street outreach, and transitional housing. We use the coefficients from that regression to predict fitted values for the full sample. We follow Abadie et al. (2018) using a repeated split sample estimation procedure.



(a) Received Any Rental Assistance



(b) Homeless (6 to 18 Months)

Figure 6: Treatment Effects, by Sub-Group

Notes: The top row in each panel replicates the full sample result. Each subsequent row limits the sample to the category listed. Each plotted point shows the coefficient on a treatment assignment dummy in a regression of receipt of rental assistance on treatment assignment with bars showing confidence intervals. The top panel shows treatment effects on whether the participant received any financial assistance and the bottom panel for our primary measure of homelessness within 6-18 months.

Table 1: Baseline Characteristics

	Treatment	Control	Adj. Diff
Predicted Risk of Homelessness	0.37	0.36	-0.00025
			(0.011)
Homelessness Services			
Any Homeless Services	0.84	0.86	-0.020
			(0.027)
Ever in Shelter	0.69	0.65	0.030
			(0.036)
Ever in Street Outreach	0.39	0.47	-0.078**
			(0.038)
VI-SPDAT Score	5.71	5.79	-0.043
			(0.13)
Years Since Housing Instability Began	7.56	7.34	0.44
			(0.57)
Months Homeless in Last Three Years	27.1	27.0	0.30
			(0.81)
Demographics			
Age	47.9	47.7	0.30
			(1.02)
Monthly Income	757.7	746.7	-9.58
			(95.5)
Female	0.27	0.30	-0.026
			(0.034)
Hispanic	0.43	0.48	-0.048
			(0.038)
Non-Hispanic White	0.30	0.29	0.020
			(0.036)
Non-Hispanic Black	0.20	0.15	0.047
			(0.029)
Asian or Asian American	0.048	0.047	0.00065
			(0.016)
Belongs to Native or Indigenous Group	0.094	0.18	-0.090***
			(0.026)
Ever Been in Jail	0.78	0.78	-0.0015
			(0.032)
Mental Health or Cognitive Issues Affect Access to Housing	0.13	0.12	0.0040
			(0.025)
Drinking or Drug Use Caused Housing Instability	0.22	0.21	0.019
			(0.032)
Joint test of balance (p-value)			.075
N	373	360	733

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. The regression-adjusted difference includes fixed effects for randomization strata. The VI-SPDAT reported on HMIS uses the score from the assessment closest to random assignment. If that assessment does not list a score, we use the next available score as long as the assessment is within two years of random assignment. This is based on the process OSH has specified for caseworkers. Homelessness Services is defined as admissions to shelter, street outreach, perm. supp housing, transitional housing, and safe haven. The predicted use of homelessness services comes from limiting the sample to control participants and regressing the use of homelessness services after random assignment on income and VI-SPDAT scores at baseline, dummies for female, Hispanic, black, and pre-randomization shelter admission, homelessness prevention, rapid rehousing, permanent supportive housing, services only, street outreach, and transitional housing. The joint test of balance comes from a regression of treatment assignment on all listed baseline characteristics (except predicted risk) and strata fixed effects, the p-value corresponds to an F-test that all coefficients except the strata fixed effect are zero.

Table 2: Financial and Rental Assistance After Random Assignment

	Treatment	Control	Adj. Diff
6 months			
Enrolled in Study RRH	0.85	0.011	0.86***
			(0.020)
Enrolled in Any RRH	0.86	0.083	0.78***
			(0.025)
Ever Received Financial Assistance from Any RRH	0.48	0.025	0.46***
			(0.030)
Any Type of Financial Assistance Amount from Any RRH	3432.6	166.4	3339.9***
			(288.2)
Rental Assistance Amount from Any RRH	3075.9	81.0	3149.2***
			(254.4)
N	373	360	733
6-18 months			
Enrolled in Study RRH	0.039	0.0061	0.034***
			(0.013)
Enrolled in Any RRH	0.063	0.037	0.028
			(0.019)
Ever Received Financial Assistance from Any RRH	0.51	0.055	0.47***
			(0.034)
Any Type of Financial Assistance Amount from Any RRH	5068.2	430.6	4826.3***
			(410.8)
Rental Assistance Amount from Any RRH	3843.1	298.7	3577.4***
			(308.7)
N	335	328	663
18-30 months			
Enrolled in Study RRH	0	0	0
			(.)
Enrolled in Any RRH	0.018	0.066	-0.075***
			(0.025)
Ever Received Financial Assistance from Any RRH	0.18	0.042	0.14***
			(0.034)
Any Type of Financial Assistance Amount from Any RRH	1445.2	216.7	1312.9***
			(280.4)
Rental Assistance Amount from Any RRH	1009.8	202.1	889.7***
			(213.1)
N	217	213	430

Notes: Enrollment outcomes use program enrollment dates from HMIS. Rental and financial assistance outcomes are measured using payment dates and amounts for all Rapid Re-Housing programs in HMIS. Adjusted difference comes from a regression of the listed variable on a treatment indicator, strata fixed effects, and pre-specified controls (see text). Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. Standard errors in parentheses.

Table 3: Use of Housing Services within 6 Months of Random Assignment

	Treatment	Control	Adj. Diff
Any Homeless Services (Not Incl. any RRH)	0.26	0.32	-0.076**
			(0.036)
Any Homeless Services (Not Incl. the RRH study)	0.26	0.36	-0.11***
			(0.036)
- Entered Shelter	0.21	0.23	-0.042
			(0.032)
- Entered Perm. Supp. Housing	0.0027	0.011	-0.0074
			(0.0067)
- Entered Street Outreach	0.038	0.094	-0.066***
			(0.020)
- Entered Transitional Housing	0.035	0.0083	0.029**
			(0.012)
- Entered Safe Haven	0.0027	0.0028	-0.00021
			(0.0047)
Entered Homeless Prevention	0.0080	0.017	-0.0089
			(0.0087)
Entered Services Only	0.35	0.21	0.11***
			(0.037)
Ever in RRH Targeted to Encampments	0.0027	0.014	-0.013**
			(0.0068)
Days in Shelter	12.7	18.8	-7.82**
			(3.63)
N	373	360	733

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. Standard errors in parentheses. Adjusted difference comes from a regression of the listed variable on a treatment indicator, strata fixed effects, and pre-specified controls (see text). Outcomes are measured using enrollment dates from HMIS. In the top row, homelessness services include enrollments in shelter, street outreach, permanent supportive housing, transitional housing, and safe haven. The category "Services Only" covers a wide range of programs. These include transportation programs like UPLIFT, employment training, legal services, mobile showers, and case management without housing services. We restrict the sample to participants we can observe for at least 6 months after random assignment.

Table 4: Use of Housing Services 6 to 18 Months After Random Assignment

	Treatment	Control	Adj. Diff
Any Homeless Services (Not Incl. Any RRH)	0.26	0.38	-0.12***
			(0.039)
Any Homeless Services (Not Incl. the RRH Study)	0.27	0.40	-0.13***
			(0.040)
- Entered Shelter	0.19	0.27	-0.083**
			(0.033)
- Entered Perm. Supp. Housing	0.015	0.046	-0.029*
			(0.015)
- Entered Street Outreach	0.090	0.17	-0.097***
			(0.028)
- Entered Transitional Housing	0.024	0.021	0.010
			(0.013)
- Entered Safe Haven	0	0	0
			(.)
Entered Homeless Prevention	0.015	0.021	-0.0022
			(0.010)
Enrolled in Services Only	0.38	0.27	0.12***
			(0.040)
Ever in RRH Targeted to Encampments	0.0030	0.0091	-0.0041
			(0.0060)
Days in Shelter	20.2	48.0	-31.5***
			(7.68)
N	335	328	663

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. Standard errors in parentheses. Adjusted difference comes from a regression of the listed variable on a treatment indicator, strata fixed effects, and pre-specified controls (see text). Outcomes are measured using enrollment dates from HMIS. In the top row, homelessness services include enrollments in shelter, street outreach, permanent supportive housing, transitional housing, and safe haven. The category "Services Only" covers a wide range of programs. These include transportation programs like UPLIFT, employment training, legal services, mobile showers, and case management without housing services. We restrict the sample to participants we can observe for at least 18 months after random assignment.

Table 5: Use of Housing Services 18 to 30 Months After Random Assignment

	Treatment	Control	Adj. Diff
Any Homeless Services (Not Incl. Any RRH)	0.24	0.31	-0.064
			(0.049)
Any Homeless Services (Not Incl. the RRH Study)	0.25	0.34	-0.10**
			(0.050)
- Entered Shelter	0.17	0.23	-0.037
			(0.045)
- Entered Perm. Supp. Housing	0.0046	0.0094	-0.0041
			(0.0092)
- Entered Street Outreach	0.12	0.12	0.0038
			(0.034)
- Entered Transitional Housing	0.0092	0.0094	0.0030
			(0.0092)
- Entered Safe Haven	0	0	0
			(.)
Entered Homeless Prevention	0.028	0.019	0.010
			(0.018)
Enrolled in Services Only	0.19	0.26	-0.072
			(0.046)
Ever in RRH Targeted to Encampments	0	0.019	-0.023*
			(0.012)
Days in Shelter	30.1	38.7	-5.73
			(10.9)
N	217	213	430

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. Standard errors in parentheses. Adjusted difference comes from a regression of the listed variable on a treatment indicator, strata fixed effects, and pre-specified controls (see text). Outcomes are measured using enrollment dates from HMIS. In the top row, homelessness services include enrollments in shelter, street outreach, permanent supportive housing, transitional housing, and safe haven. The category "Services Only" covers a wide range of programs. These include transportation programs like UPLIFT, employment training, legal services, mobile showers, and case management without housing services. We restrict the sample to participants we can observe for at least 30 months after random assignment.

A Appendix

A.1 Homelessness Services Taxonomy

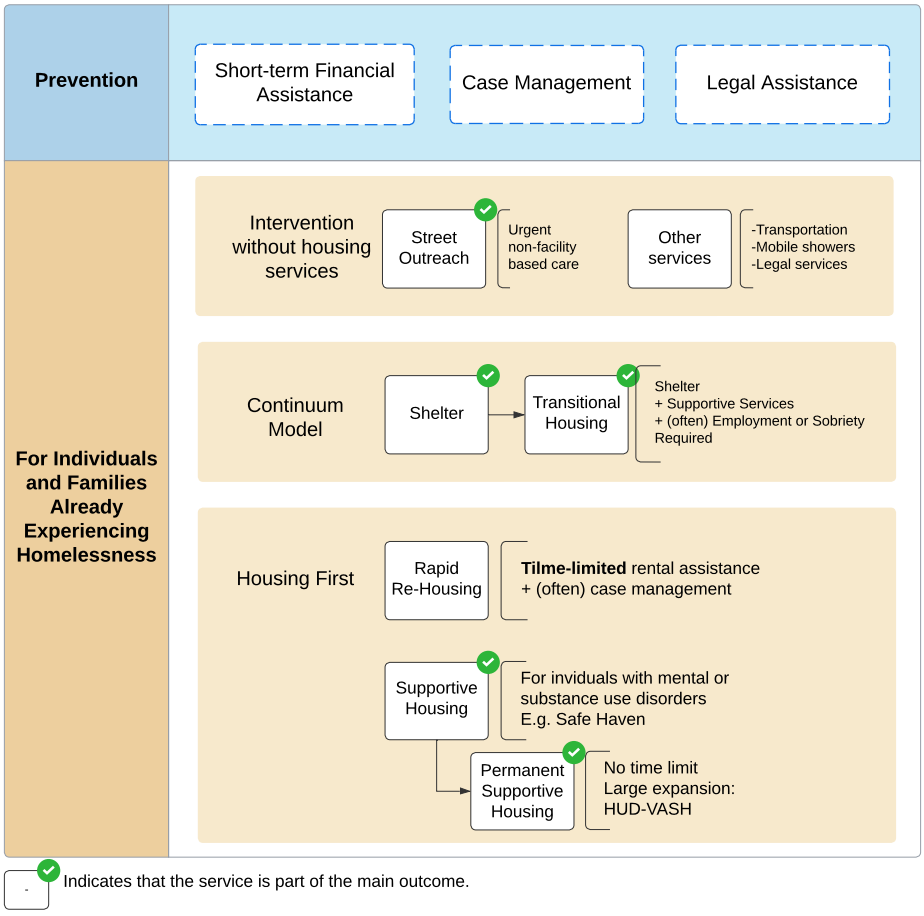
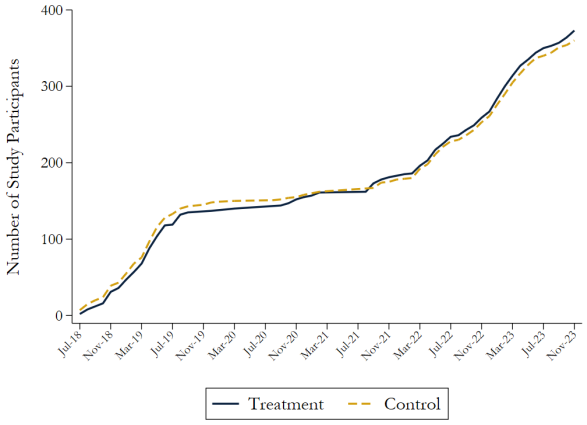
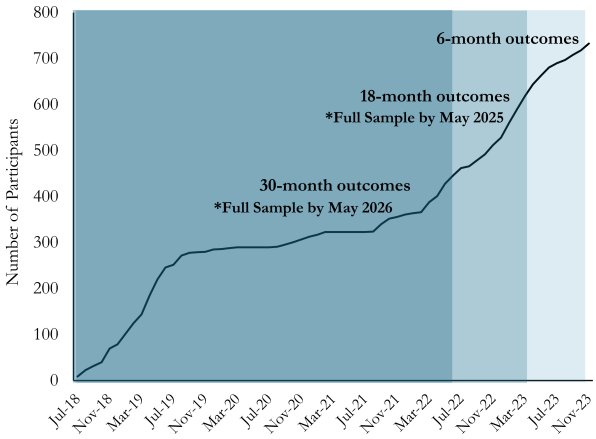


Figure A.1: Approaches to Reduce or Prevent Homelessness

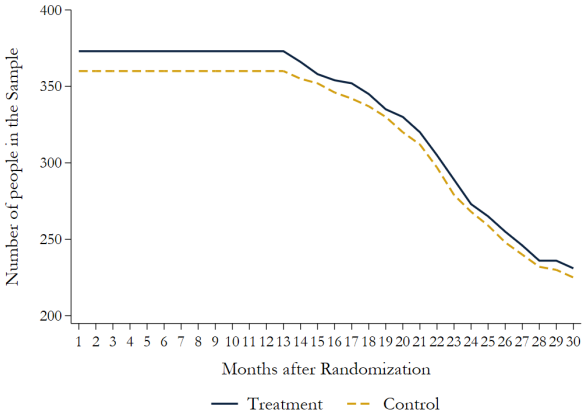
A.2 Enrollment and Outcomes Timeline



(a) Cumulative Study Enrollment (Jul 2018 - Nov 2023)



(b) Outcomes Timeline



(c) Sample size included by Outcomes Lag

Figure A.2: Study Enrollment and Outcomes Timeline

A.3 Rapid Re-Housing Summary Statistics (Conditional on Enrollment)

Table A.1: Length of Rapid Re-Housing Stages

	Mean	Median	SD	Min	Max
- Days from Enrollment to Move-In	119.0	69	127.6	0	770
- Days from Enrollment to Exit from RRH	381.2	375	259.0	7	1086
- Days from Move-In to Exit from RRH	356.9	365.5	227.7	0	829
- Days from Randomization to RRH Enrollment	157.7	3	339.8	0	1928
- Days from Randomization to Move-In	238.9	100	322.5	0	1953
- Days from Randomziation to Exit from RRH	520.9	469	400.6	7	2139
Observations	230				

Notes: We restrict the sample to participants we can observe for at least 30 months after random assignment and who enrolled in RRH

Table A.2: Payment Descriptives

	Mean	Median	SD	Min	Max
- Days from Random Assignment to First Rent Payment	172	82	279	13	1,948
- Days from Random Assignment to First Other Fin. Assit. Payment	178	67	288	2	1,948
- Days from First to Last Rent Payment	362	324	241	0	1,272
- Days from First to Last Other Fin. Assist. Payment	271	237	271	0	1,521
- Cumulative Rent Payments	14,962	12,969	9,280	0	42,029
- Cumulative Other Fin. Assist. Payments	5,136	2,339	5,575	15	23,245
- Average Monthly Rent Payment	1,629	1,437	860	308	4,742
- Average Monthly Other Fin. Assist. Payment	681	532	668	4	4,007
Observations	160				

Notes: We restrict the sample to participants we can observe for at least 30 months after random assignment and who enrolled in RRH.

A.4 Review of Special Non-Compliance Cases and Team Response

For a very small number of study participants, the actual study group to which the individual was assigned, does not match the original group assigned to them. We recorded 9 cases of non-compliance due to human error and technical issues with the software used to enroll and randomize participants. These cases include incorrect assignments to both the treatment group and the control group. These cases of non-compliance were identified by comparing HomeFirst's study tracker to the randomization result documented on SurveyCTO's server (This is different from the noncompliance we can observe in the HMIS data).

After documenting these cases of non-compliance, we introduced additional checks and safeguards in the intake form software (SurveyCTO). Furthermore, to ensure a reliable internet connection for each of the tables used during study enrollment, HomeFirst had the tablets connected to a 3G service to improve the internet connection when the caseworkers meet with clients outside of the office. In response to these changes, such cases of non-compliance were rare. The number of cases of noncompliance is also quite small relative to the projected sample size of the study, so we expect their impact on the overall study to be negligible.

The Santa Clara County IRB and the University of Notre Dame IRB were notified of these events.

A.5 Main Tables with Randomization Inference

Table A.3: Baseline Characteristics

	Treatment	Control	Adj.Diff	Adj. Diff (reghdfe)	Adj. Diff (areg)	Adj. Diff (ritest)
Homelessness Services						
Predicted Risk of Homelessness	0.35	0.36	-0.0089	-0.0089	-0.0089	-0.0089
			(0.011)	(0.011)	(0.011)	(0.016)
			[0.42]	[0.41]	[0.42]	[0.44]
Any Homeless Services (Not Including RRH)	0.84	0.86	-0.020	-0.020	-0.020	-0.020
			(0.027)	(0.027)	(0.027)	(0.016)
			[0.47]	[0.47]	[0.47]	[0.49]
- Ever in Shelter	0.69	0.65	0.030	0.030	0.030	0.030
			(0.036)	(0.035)	(0.036)	(0.016)
			[0.41]	[0.40]	[0.41]	[0.42]
- Ever in Street Outreach	0.39	0.47	-0.078**	-0.078**	-0.078**	-0.078**
			(0.038)	(0.037)	(0.038)	(0.0060)
			[0.04]	[0.03]	[0.04]	[0.038]
- Ever in Perm. Supp. Housing	0.029	0.022	0.0074	0.0074	0.0074	0.0074
			(0.012)	(0.012)	(0.012)	(0.015)
			[0.55]	[0.54]	[0.55]	[0.62]
- Ever in Transitonal Housing	0.13	0.16	-0.018	-0.018	-0.018	-0.018
			(0.027)	(0.026)	(0.027)	(0.016)
			[0.50]	[0.49]	[0.50]	[0.49]
- Ever in Safe Haven	0.0054	0.0056	-0.00096	-0.00096	-0.00096	-0.00096
			(0.0060)	(0.0056)	(0.0060)	(0)
			[0.87]	[0.86]	[0.87]	[1]
Ever in RRH	0.10	0.086	0.027	0.027	0.027	0.027
			(0.022)	(0.022)	(0.022)	(0.014)
			[0.22]	[0.23]	[0.22]	[0.24]
Ever in Homelessness Prevention	0.024	0.028	-0.0061	-0.0061	-0.0061	-0.0061
			(0.012)	(0.012)	(0.012)	(0.015)
			[0.62]	[0.61]	[0.62]	[0.65]
Ever in Services Only	0.74	0.73	0.017	0.017	0.017	0.017
			(0.033)	(0.032)	(0.033)	(0.015)
			[0.61]	[0.60]	[0.61]	[0.65]
VI-SPDAT Score	5.56	5.60	-0.044	-0.044	-0.044	-0.044
			(0.079)	(0.078)	(0.079)	(0.016)
			[0.58]	[0.57]	[0.58]	[0.57]
Number of Days Homeless at Random Assignment	2572.9	2580.9	68.0	68.0	68.0	68.0
			(171.1)	(171.3)	(171.1)	(0.015)
			[0.69]	[0.69]	[0.69]	[0.69]
Age	47.9	47.7	0.30	0.30	0.30	0.30
			(1.02)	(1.00)	(1.02)	
			[0.77]	[0.77]	[0.77]	[0.77]
Income	527.6	551.5	-19.7	-19.7	-19.7	-19.7
			(52.7)	(52.4)	(52.7)	
			[0.71]	[0.71]	[0.71]	[0.70]
Female	0.27	0.30	-0.026	-0.026	-0.026	-0.026
			(0.034)	(0.034)	(0.034)	
			[0.44]	[0.44]	[0.44]	[0.49]
Hispanic	0.43	0.48	-0.048	-0.048	-0.048	-0.048
			(0.038)	(0.038)	(0.038)	
			[0.21]	[0.21]	[0.21]	[0.21]
White	0.62	0.58	0.062*	0.062*	0.062*	0.062*
			(0.037)	(0.037)	(0.037)	
			[0.10]	[0.09]	[0.10]	[0.10]
Black	0.22	0.16	0.050*	0.050*	0.050*	0.050*
			(0.030)	(0.029)	(0.030)	
			[0.09]	[0.09]	[0.09]	[0.090]
Asian or Asian American	0.048	0.047	0.00065	0.00065	0.00065	0.00065
			(0.016)	(0.016)	(0.016)	
			[0.97]	[0.97]	[0.97]	[1]
Belongs to Native or Indigenous Group	0.094	0.18	-0.090***	-0.090***	-0.090***	-0.090***
			(0.026)	(0.026)	(0.026)	
			[0.00]	[0.00]	[0.00]	[0]
Joint test of balance (p-value)			.075			
N	373	360	733			

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. Ritest does randomization inference within the randomization strata and is set to use 1000 replications or draws. The VI-SPDAT reported on HMIS uses the score from the assessment closest to random assignment. If that assessment does not list a score, we use the next available score as long as the assessment is within two years of random assignment. This is based on the process OSH has specified for caseworkers. The predicted shelter entry rate comes from limiting the sample to control participants and regressing shelter admissions after random assignment on income, VI-SPDAT scores at baseline, dummies for female, Hispanic, black, and pre-randomization shelter admission, homelessness prevention, rapid rehousing, permanent supportive housing, services only, street outreach, and transitional housing. Standard errors in parentheses and p-values in brackets.

Table A.4: Baseline Characteristics

	Treatment	Control	Adj. Diff	Adj. Diff (reghdfe)	Adj. Diff (areg)	Adj. Diff (ritest)
Demographics						
Age (Average of CTO and HMIS values)	47.9	47.7	0.30	0.30	0.30	0.30
			(1.02)	(1.00)	(1.02)	
			[0.77]	[0.77]	[0.77]	[0.77]
Income (Average of CTO and HMIS values -winsorized at 95th pctile)	527.6	545.7	-19.7	-19.7	-19.7	-19.7
			(52.7)	(52.4)	(52.7)	
			[0.71]	[0.71]	[0.71]	[0.72]
Female (HMIS)	0.27	0.30	-0.026	-0.026	-0.026	-0.026
			(0.034)	(0.034)	(0.034)	
			[0.44]	[0.44]	[0.44]	[0.48]
Hispanic	0.43	0.48	-0.048	-0.048	-0.048	-0.048
			(0.038)	(0.038)	(0.038)	
			[0.21]	[0.21]	[0.21]	[0.23]
White	0.62	0.58	0.062*	0.062*	0.062*	0.062*
			(0.037)	(0.037)	(0.037)	
			[0.10]	[0.09]	[0.10]	[0.11]
Black	0.22	0.16	0.050*	0.050*	0.050*	0.050*
			(0.030)	(0.029)	(0.030)	
			[0.09]	[0.09]	[0.09]	[0.11]
Asian or Asian American	0.048	0.047	0.00065	0.00065	0.00065	0.00065
			(0.016)	(0.016)	(0.016)	
			[0.97]	[0.97]	[0.97]	[1]
American Indian/Alaska Native/Indigenous/Native Hawaiian/Pacific Islander	0.094	0.18	-0.090***	-0.090***	-0.090***	-0.090***
			(0.026)	(0.026)	(0.026)	
			[0.00]	[0.00]	[0.00]	[0.0010]
Joint test of balance (p-value)			.075			
N	373	360	733			

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. Ritest does randomization inference within the randomization strata and is set to use 1000 replications or draws. The VI-SPDAT reported on HMIS uses the score from the assessment closest to random assignment. If that assessment does not list a score, we use the next available score as long as the assessment is within two years of random assignment. This is based on the process OSH has specified for caseworkers. The predicted shelter entry rate comes from limiting the sample to control participants and regressing shelter admissions after random assignment on income, VI-SPDAT scores at baseline, dummies for female, Hispanic, black, and pre-randomization shelter admission, homelessness prevention, rapid rehousing, permanent supportive housing, services only, street outreach, and transitional housing. Standard errors in parentheses and p-values in brackets.

Table A.5: Financial and Rental Assistance After Random Assignment

	Treatment	Control	Adj. Diff	Adj. Diff (reghdfe)	Adj. Diff (areg)	Adj. Diff (ritest)
6 months						
Enrolled in study RRH	0.85	0.011	0.86***	0.85***	0.85***	0.85***
			(0.020)	(0.020)	(0.021)	(0)
			[0.00]	[0.00]	[0.00]	[0]
Enrolled in any RRH	0.86	0.083	0.78***	0.78***	0.78***	0.78***
			(0.025)	(0.024)	(0.025)	(0)
			[0.00]	[0.00]	[0.00]	[0]
Ever Fin Assist from any RRH	0.49	0.036	0.46***	0.46***	0.46***	0.46***
			(0.031)	(0.030)	(0.031)	(0)
			[0.00]	[0.00]	[0.00]	[0]
Any Type of Financial Assistance Amount from any RRH	3553.6	338.6	3249.9***	3249.9***	3249.9***	3249.9***
			(318.7)	(308.2)	(318.7)	(0)
			[0.00]	[0.00]	[0.00]	[0]
Rental Assistance Amount from any RRH	3116.6	253.2	2989.6***	2989.6***	2989.6***	2989.6***
			(283.4)	(272.7)	(283.4)	(0)
			[0.00]	[0.00]	[0.00]	[0]
N	373	360	733	713	733	733
6-18 months						
Entered study RRH	0.039	0.0061	0.034***	0.034***	0.034***	0.034***
			(0.013)	(0.012)	(0.013)	(0.014)
Entered any RRH	0.063	0.037	0.028	0.027	0.027	0.027
			(0.018)	(0.018)	(0.019)	(0.040)
Ever Fin Assist from any RRH	0.51	0.055	0.47***	0.47***	0.47***	0.47***
			(0.034)	(0.032)	(0.034)	(0)
Any Type of Financial Assistance Amount from any RRH	5068.2	430.6	4824.1***	4826.3***	4826.3***	4826.3***
			(410.9)	(402.2)	(411.6)	(0)
Rental Assistance Amount from any RRH	3843.1	298.7	3574.2***	3579.8***	3579.8***	3579.8***
			(308.7)	(307.0)	(308.5)	(0)
N	335	328	663	647	663	663
18-30 months						
Entered study RRH	0	0	0	0	0	0
			(.)	(.)	(.)	
			[.]	[.]	[.]	[1]
Entered any RRH	0.018	0.066	-0.075***	-0.074***	-0.074***	-0.074***
			(0.025)	(0.022)	(0.025)	
			[0.00]	[0.00]	[0.00]	[0]
Ever Fin Assist from any RRH	0.18	0.042	0.14***	0.14***	0.14***	0.14***
			(0.034)	(0.032)	(0.034)	
			[0.00]	[0.00]	[0.00]	[0]
Any Type of Financial Assistance Amount from any RRH	1445.2	216.7	1312.9***	1328.0***	1328.0***	1328.0***
			(280.4)	(289.0)	(281.2)	
			[0.00]	[0.00]	[0.00]	[0]
Rental Assistance Amount from any RRH	1009.8	202.1	889.7***	901.8***	901.8***	901.8***
			(213.1)	(218.0)	(213.5)	
			[0.00]	[0.00]	[0.00]	[0]
N	217	213	430	417	430	430

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. Ritest does randomization inference within the randomization strata and is set to use 1000 replications or draws. Standard errors in parentheses and p-values in brackets.

Table A.6: Use of Housing Services within 6 Months of Random Assignment

	Treatment	Control	Adj. Diff	Adj. Diff (reghdfe)	Adj. Diff (areg)	Adj. Diff (ritest)
Used homelessness services (not incl. any RRH)	0.26	0.32	-0.076**	-0.076**	-0.076**	-0.076**
			(0.036)	(0.035)	(0.036)	(0.020)
			[0.04]	[0.03]	[0.04]	[0.040]
Used homelessness services (not incl. the RRH study)	0.26	0.36	-0.11***	-0.11***	-0.11***	-0.11***
			(0.036)	(0.035)	(0.036)	(0)
			[0.00]	[0.00]	[0.00]	[0]
- Entered Shelter	0.21	0.23	-0.042	-0.042	-0.042	-0.042
			(0.032)	(0.031)	(0.032)	(0.041)
			[0.19]	[0.18]	[0.19]	[0.21]
- Entered Street Outreach	0.038	0.094	-0.066***	-0.066***	-0.066***	-0.066***
			(0.020)	(0.020)	(0.020)	(0)
			[0.00]	[0.00]	[0.00]	[0]
- Entered Perm. Supp. Housing	0.0027	0.011	-0.0073	-0.0073	-0.0073	-0.0073
			(0.0067)	(0.0063)	(0.0067)	(0.027)
			[0.28]	[0.24]	[0.28]	[0.080]
- Entered Transitional Housing	0.035	0.0083	0.029**	0.029**	0.029**	0.029**
			(0.012)	(0.012)	(0.012)	(0.0099)
			[0.02]	[0.01]	[0.02]	[0.010]
- Entered Safe Haven	0.0027	0.0028	-0.00016	-0.00016	-0.00016	-0.00016
			(0.0046)	(0.0044)	(0.0046)	(0.022)
			[0.97]	[0.97]	[0.97]	[0.95]
Entered Homeless Prevention	0.0080	0.017	-0.0089	-0.0089	-0.0089	-0.0089
			(0.0087)	(0.0085)	(0.0087)	(0.048)
			[0.31]	[0.29]	[0.31]	[0.36]
Entered Services Only	0.35	0.21	0.11***	0.11***	0.11***	0.11***
			(0.037)	(0.035)	(0.037)	(0)
			[0.00]	[0.00]	[0.00]	[0]
Ever in RRH targeted to encampments	0.0027	0.014	-0.013**	-0.013**	-0.013**	-0.013**
			(0.0068)	(0.0064)	(0.0068)	(0.022)
			[0.05]	[0.04]	[0.05]	[0.050]
Days in shelter	12.7	18.8	-7.78**	-7.78**	-7.78**	-7.78**
			(3.63)	(3.51)	(3.63)	(0.014)
			[0.03]	[0.03]	[0.03]	[0.020]
N	373	360	733	713	733	733

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.
Standard errors in parentheses and p-values in brackets. Ritest does randomization inference within the randomization strata and is set to use 1000 replications or draws. The category "Services Only" covers a wide range of programs. These include transportation programs like UPLIFT, employment training, legal services, mobile showers, and case management without housing services.

Table A.7: Use of Housing Services 6 to 18 Months After Random Assignment

	Treatment	Control	Adj. Diff	Adj. Diff (reghdfe)	Adj. Diff (areg)	Adj. Diff (ritest)
Used homelessness services (not incl. any RRH)	0.26	0.38	-0.12***	-0.12***	-0.12***	-0.12***
			(0.039)	(0.037)	(0.039)	
			[0.00]	[0.00]	[0.00]	[0]
Used homelessness services (not incl. the RRH study)	0.27	0.40	-0.13***	-0.13***	-0.13***	-0.13***
			(0.040)	(0.038)	(0.040)	
			[0.00]	[0.00]	[0.00]	[0]
- Entered Shelter	0.19	0.27	-0.083**	-0.083**	-0.083**	-0.083**
			(0.033)	(0.032)	(0.033)	
			[0.01]	[0.01]	[0.01]	[0.020]
- Entered Street Outreach	0.090	0.17	-0.098***	-0.098***	-0.098***	-0.098***
			(0.028)	(0.027)	(0.028)	
			[0.00]	[0.00]	[0.00]	[0.010]
- Entered Perm. Supp. Housing	0.015	0.046	-0.029*	-0.029**	-0.029*	-0.029*
			(0.015)	(0.014)	(0.015)	
			[0.05]	[0.05]	[0.05]	[0.080]
- Entered Transitional Housing	0.024	0.021	0.010	0.010	0.010	0.010
			(0.013)	(0.012)	(0.013)	
			[0.43]	[0.40]	[0.43]	[0.39]
- Entered Safe Haven	0	0	0	0	0	0
			(.)	(.)	(.)	
			[.]	[.]	[.]	[1]
Entered Homeless Prevention	0.015	0.021	-0.0022	-0.0022	-0.0022	-0.0022
			(0.010)	(0.011)	(0.010)	
			[0.83]	[0.84]	[0.83]	[0.91]
Enrolled in Services Only	0.38	0.27	0.12***	0.12***	0.12***	0.12***
			(0.040)	(0.039)	(0.040)	
			[0.00]	[0.00]	[0.00]	[0.010]
Ever in RRH targeted to encampments	0.0030	0.0091	-0.0041	-0.0041	-0.0041	-0.0041
			(0.0060)	(0.0057)	(0.0060)	
			[0.49]	[0.48]	[0.49]	[0.47]
Days in shelter	20.2	48.0	-31.4***	-31.4***	-31.4***	-31.4***
			(7.69)	(7.28)	(7.69)	
			[0.00]	[0.00]	[0.00]	[0]
N	335	328	663	647	663	663

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.
Standard errors in parentheses and p-values in brackets. Ritest does randomization inference within the randomization strata and is set to use 1000 replications or draws. The category "Services Only" covers a wide range of programs. These include transportation programs like UPLIFT, employment training, legal services, mobile showers, and case management without housing services.

Table A.8: Use of Housing Services 18 to 30 Months After Random Assignment

	Treatment	Control	Adj. Diff	Adj. Diff (reghdfe)	Adj. Diff (areg)	Adj. Diff (ritest)
Used homelessness services (not incl. any RRH)	0.24	0.31	-0.064	-0.064	-0.064	-0.064
			(0.049)	(0.047)	(0.049)	
			[0.19]	[0.18]	[0.19]	[0.18]
Used homelessness services (not incl. the RRH study)	0.25	0.34	-0.10**	-0.10**	-0.10**	-0.10**
			(0.050)	(0.048)	(0.050)	
			[0.04]	[0.03]	[0.04]	[0.040]
- Entered Shelter	0.17	0.23	-0.037	-0.037	-0.037	-0.037
			(0.045)	(0.042)	(0.045)	
			[0.41]	[0.38]	[0.41]	[0.45]
- Entered Street Outreach	0.12	0.12	0.0038	0.0038	0.0038	0.0038
			(0.034)	(0.034)	(0.034)	
			[0.91]	[0.91]	[0.91]	[0.94]
- Entered Perm. Supp. Housing	0.0046	0.0094	-0.0041	-0.0041	-0.0041	-0.0041
			(0.0092)	(0.0094)	(0.0092)	
			[0.65]	[0.66]	[0.65]	[0.72]
- Entered Transitional Housing	0.0092	0.0094	0.0030	0.0030	0.0030	0.0030
			(0.0092)	(0.0095)	(0.0092)	
			[0.75]	[0.75]	[0.75]	[0.73]
- Entered Safe Haven	0	0	0	0	0	0
			(.)	(.)	(.)	
			[.]	[.]	[.]	[1]
Entered Homeless Prevention	0.028	0.019	0.010	0.010	0.010	0.010
			(0.018)	(0.016)	(0.018)	
			[0.55]	[0.51]	[0.55]	[0.58]
Enrolled in Services Only	0.19	0.26	-0.072	-0.072	-0.072	-0.072
			(0.046)	(0.044)	(0.046)	
			[0.12]	[0.10]	[0.12]	[0.10]
Ever in RRH targeted to encampments	0	0.019	-0.023*	-0.023**	-0.023*	-0.023*
			(0.012)	(0.011)	(0.012)	
			[0.06]	[0.04]	[0.06]	[0.030]
Days in shelter	30.1	38.7	-5.70	-5.70	-5.70	-5.70
			(10.9)	(10.4)	(10.9)	
			[0.60]	[0.58]	[0.60]	[0.56]
N	217	213	430	417	430	430

Notes: Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.
Standard errors in parentheses and p-values in brackets. Ritest does randomization inference within the randomization strata and is set to use 1000 replications or draws. The category "Services Only" covers a wide range of programs. These include transportation programs like UPLIFT, employment training, legal services, mobile showers, and case management without housing services.