Do homelessness prevention programs prevent homelessness? Evidence from a randomized controlled trial

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Abstract

This paper provides the first evidence from a randomized controlled trial isolating the impact of financial assistance to prevent homelessness. In this study individuals and families at imminent risk of homelessness were offered temporary financial assistance, averaging nearly $2,000 for those assigned to treatment. Our results show that this assistance significantly reduces homelessness by 3.8 percentage points from a base rate of 4.1 percent. The effects are larger for people with a history of homelessness and no children. Despite concerns about cost-effectiveness due to difficulty targeting, our estimates suggest that the benefits to homelessness prevention exceed costs.

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1 Introduction

Housing instability, eviction, and homelessness affect many households. In the 2017 American Housing Survey, the heads of 2.8 million housing units reported being behind on rent, and 0.3 million thought it 'very likely' that they would be evicted in the next two months (U.S. Census Bureau, 2022). Eviction is costly to tenants because evictions can lead to homelessness and collateral consequences, like reduced earnings and credit access (Collinson et al., 2022).

To address the costly nature of becoming homeless, policymakers have supported homelessness prevention programs. These programs provide temporary financial assistance, usually in the form of a payment to landlords, utility companies, etc. on behalf of individuals and families at risk of being evicted or becoming homeless. Prior to the pandemic, 93% of communities had a help line to call for such help (211.org, 2015), and these programs have grown dramatically due to the economic disruptions caused by the pandemic. The federal government appropriated $70 billion for various homelessness prevention programs across CARES, the Consolidated Appropriations Act, and ARPA, including more than $46 billion for direct rent and utility assistance.

Even organizations dedicated to addressing homelessness debate whether homelessness prevention programs are effective. The primary concern is that most individuals and families at risk of losing their housing do not end up homeless, even if they do not receive financial assistance–a study in Chicago, for example, found that among eligible people who sought financial assistance to prevent homelessness but were denied, only 2 percent entered an emergency shelter within 6 months (Evans et al., 2016). Targeting assistance towards those who would otherwise become homeless is difficult, suggesting that funds to prevent homelessness may just crowd out other resources like help from family and friends. The National Alliance to End Homelessness, for example, has argued that "...homelessness prevention programs are notably ineffective at preventing homelessness because of the difficulty of predicting who will actually become homeless" (NAEH, 2020). Quasi-experimental evidence indicating that financial assistance substantially reduces homelessness contradicts this claim (Evans et al., 2016), but to-date there is no direct experimental evidence of the impact of such financial assistance.

In this paper, we test whether providing temporary financial assistance to those at risk of losing their housing can successfully prevent homelessness. We focus on a homelessness prevention program in Santa Clara County, CA, a county with very high levels of both rent and homelessness. The program we study provides financial assistance with rent, security deposits, and other household expenses (e.g. utilities). It pays rent to landlords of tenants who are still housed, behind on rent, and at imminent risk of being removed. Our study focuses on tenants who barely meet the program's criteria for vulnerability and who cannot demonstrate an ability to pay rent after the temporary payment ends, because for this group there was not sufficient funding to provide assistance to everyone and those denied assistance were typically not eligible for other programs providing rental assistance. Among this group, we randomly offer some tenants scarce financial assistance and others not. Treatment compliance is partial because some members of the treatment group do not follow through with the application process necessary to receive the benefit and others in the control group find alternative assistance, but the random offer increases the proportion of people receiving assistance from 12% to 68%.

Our results indicate that financial assistance significantly reduces homelessness. Using administrative records on shelter use and receipt of other homelessness services, we find that the treatment group is 3.8 percentage points less likely to be homeless within six months, compared to a counterfactual rate of 4.1 percent. Emergency shelter use, which declines by 2.5 percentage points from from a base rate of 3.0, drives most of this effect. These effects are only evident for those enrolled prior to March 2020; for people who enroll during the pandemic, rates of entry into homelessness are very low even for the control group, likely due to complementary interventions like the eviction moratorium. The effects are stronger for people with a history of homelessness and households without children. Because our main outcome measure will miss some of those who lose their housing (for example, those who move in with family or friends) we also examine the impact of assistance on the likelihood of switching addresses using consumer reference data. These estimates are less precise, in part because of a smaller sample due to the need to match across data systems. We can reject neither that the effect on address changes is zero nor that it is equal to the effect on our primary measure of homelessness, though these results could also indicate that financial assistance reduces the most extreme forms of housing instability, such as literal homelessness, while leaving milder forms, such as doubling up, unchanged.

This paper provides the first evidence from a randomized controlled trial of the impact of temporary financial assistance on homelessness. Our results complement those from related studies. A prior RCT shows similar effects comparing a comprehensive package of financial assistance, customized case management, and other services to a pure control (Rolston et al., 2013), but no prior study has measured the effects of financial assistance alone through random assignment. The closest existing paper is Evans et al. (2016), which uses naturally occurring variation in referrals from a call center to financial assistance in Chicago. They find that emergency shelter entry drops by 76% compared to a control group rate of 2.1 percentage points within 6 months. Similar to our results, they find that most homelessness prevention clients do not become homeless in the absence of assistance but that financial assistance still generates large reductions in homelessness.

Our results indicate that financial assistance can effectively prevent homelessness in a wide variety of contexts. Evans et al. (2016) examine a traditional prevention program that screens for clients experiencing only temporary shocks and operates in a housing market with moderate rents. The present study examines clients who are on the margin of being eligible for assistance and who cannot demonstrate an ability pay future rent in a county with high and rapidly rising rents. The striking similarity in results despite vastly different contexts suggests that homelessness prevention programs reduce homelessness for a broad set of people in a broad set of market contexts. This breadth of treatment effects is perhaps surprising given that temporary financial assistance programs typically require that applicants have future income sources.

More generally, our results suggest a role for policies that insure households against income shocks. That temporary assistance reduces homelessness indicates that households are not fully insured against income shocks and that financial assistance does not simply crowd out other resources. This evidence may be particularly important as crisis-oriented policies such as eviction moratoria fade.

2 Context

2.1 Income shocks, homelessness, and temporary financial assistance

A large literature has shown that households with low levels of wealth cannot fully insure against income shocks. Consequently, consumption may vary with these shocks (Gruber, 1997), and temporary intervention at crisis moments can help households manage risk. Our paper considers the impact of insurance in the form of temporary assistance for those at risk of becoming homeless. We specifically focus on financial assistance, which along with legal representation and case management, is one of the most common interventions that attempts to sustainably stabilize housing with only a temporary intervention. Despite concerns about being able to target the marginal person who stands to benefit from such an intervention, recent research indicates that homelessness prevention efforts can reduce homelessness (Rolston et al., 2013; Evans et al., 2016).[1]

The current literature leaves three important gaps. First, no randomized controlled trial of temporary financial assistance to prevent homelessness exists. Although strong quasi-

[1]A more extensive literature also studies how to prevent homelessness with non-financial interventions, such as legal counsel (Seron et al., 2001; Greiner et al., 2012; Cassidy and Currie, 2022) and case management upon exiting the hospital (Herman et al., 2011; Basu et al., 2012; Samuels et al., 2015; Shinn et al., 2015); see Evans et al. (2021) for a review of this literature.

experimental evidence exists, random assignment allows one to relax assumptions about the relationship between unobserved characteristics and referral to financial assistance. Second, rigorous evaluations of prevention through financial assistance have considered only a couple specific contexts, leaving questions about external validity. Third, which people benefit most from homelessness prevention efforts, and thus should be targeted, is unclear. High risk households may benefit the most as the group most likely to become homeless, but assistance also might be insufficient to address a dire situation.

2.2 Homelessness prevention in Santa Clara County, CA

Preventing homelessness is a critical policy issue in Santa Clara County, where 45% of households pay more than 30% of their income for rent. According to HUD's 2019 Point-in-Time counts, the county has the 4th largest homeless population in the country, behind only New York, Los Angeles, and Seattle. It also has a very high rate of unsheltered homelessness; of the 9,706 people who were homeless on a night in January 2019, 82% were unsheltered.

Santa Clara County and a network of non-profit organizations collaborate to provide temporary financial assistance to address this issue of homelessness. Destination:Home is a non-profit organization that operates the county's Continuum of Care, or local planning entity that coordinates homelessness services. For prevention programs, they gather funding from the federal, county, and city governments, as well as private foundations. Destination:Home delegates the operation of the temporary financial assistance program to several non-profit organizations. Each agency covers part of the geography of Santa Clara County.[2] Potential clients typically learn of the program through a broad set of social service providers in the community, particularly those that provide housing services.

This study examines the impact of Destination:Home's homelessness prevention program, which began in 2017. The program pays temporary financial assistance. Most commonly,

[2]Sacred Heart Community Service and Salvation Army serve San Jose, where the bulk of homelessness prevention clients live. A smaller number of clients are served by agencies in other jurisdictions within the county: Amigos de Guadalupe, Community Services Agency, LifeMoves, Saint Joseph's Family Center, Sunnyvale Community Services, West Valley Community Services, and Family Supportive Housing.

the program pays rent for the current and past months to the tenant's landlord, though it also assists with security deposits, utilities, and other temporary household expenses that contribute to housing stability (e.g. vehicle repairs). For these needs, the program pays the gap between the overall debt and what the household can pay on their own. While households can request multiple months of rent or return for additional assistance, the modal assistance is a one-time payment for one month of back rent, and cumulative lifetime assistance above $5,000 requires special approval from program management. In fiscal year 2019-2020, the average assistance amount was $4,442, which is approximately two months of rent in Santa Clara County. The program also provides legal services, case management, financial services (e.g., credit counseling) and/or other services (e.g., landlord dispute resolution) to families who, but for such assistance, would become homeless. These non-financial services were available to both the treatment and control groups in our study and are typically available in communities across the country, so our experiment isolates the causal impact of the offer of temporary financial assistance.

This new program complements other existing support for homelessness prevention. Most prominently, the Santa Clara County Office of Supportive Housing funds its own homelessness prevention program and implements it through the same network of non-profit organizations. The administrative data we use for our analyses allows us to measure the fraction of the control group that receives financial assistance through this other program. Several smaller private organizations also operate similar programs.

When requesting assistance from the Destination:Home program, the client must meet program eligibility criteria. To participate, they must be currently housed but at risk of homelessness. The program assesses risk using a questionnaire, called the Prevention–Vulnerability Index–Service Prioritization Decision Assistance Tool (PR-VI-SPDAT), which asks a series of questions about family structure, housing history, financial situation, healthcare history, etc. Based on the answers to these questions, applicants are assigned a risk score ranging from 0 (lowest risk) to 29 (highest risk). The program screens using this tool, unlike other programs in Santa Clara County (and elsewhere, e.g. that in Evans et al. (2016)) that only accept clients who can document future expected income to sustain rent payments after the assistance ends. We will focus on clients who prior to randomization appear eligible for the Destination:Home program but not other programs. Because the eligibility criteria only partially overlap, we can identify a key subgroup that is expected to not receive assistance if assigned to the control condition. It also focuses our study on clients who cannot demonstrate that they have sufficient future resources to cover their living expenses.

3 Empirical strategy

Since the Destination:Home homelessness prevention program is oversubscribed, we worked with the agencies operating the program to setup a system that allocates assistance through a lottery, which operated from July 2019 to December 2020.[3] Clients who have experienced a crisis, like job loss, typically hear from other social service organizations or by word-of-mouth that assistance may be available. Potential clients then visit the agencies in the prevention program to request assistance. If the client has already exited their housing, the agencies direct them to emergency shelter or other services for people who are homeless. If the client has not yet lost their housing, they are a potential fit for prevention services and the study.

We limit the sample to people for whom random assignment occurs and is relevant. First, only mid-risk clients go through random assignment. Clients scoring above 13 on the program's risk score (PR-VI-SPDAT) are all eligible for financial assistance, while those scoring below 8 are all ineligible. We randomly assign clients scoring between 8 and 13 and limit the sample to them. Second, we exclude clients who are expected to receive assistance if assigned to control. Prior to random assignment, the case worker records if they expect that, if denied by the lottery, the client will be eligible for other similar services. Case worker expectations tend to be accurate because they typically learn about the client's income and expenses while talking the client through the intake process. Finally, we exclude clients

[3] Appendix Figure A.1 summarizes the flow of people into the experiment.

who arrive later in the month when the quota of resources for mid-risk clients has been exhausted.[4] In total 6,794 people sought homelessness prevention services from July 2019 to December 2020; 3,039 scored between 8 and 13 on the PR-VI-SPDAT. Of these, 1,776 are excluded from the study because they are potentially eligible for other prevention services, arrive too late in the month, or decline to participate. This leaves 1,263 people who were randomized as part of the study.

The computer conducts an immediate lottery to determine if the case worker should offer assistance. The lottery happens at the individual level but is stratified with a probability of treatment that varies by agency-month. Finally, clients are referred to services. Clients assigned to treatment are offered the Destination:Home program. Those assigned to control receive usual care, which includes case management and information about other ways to find housing in Santa Clara County. Thus, the primary contrast between treatment and control is the financial assistance.

We estimate the intention-to-treat (ITT) effects using the following regression:

$$Y_{iam} = \alpha_0 + \beta_0 Z_{iam} + \psi^0_{am} + \epsilon^0_{iam}, \tag{1}$$

where Y_{iam} is the outcome for person i enrolling at agency a in month m, and Z_{iam} is a dummy indicating whether person i is assigned to treatment based on the lottery. The agency-month fixed effect ψ^0_{am} accounts for the fact that the probability of treatment varies over time and across agencies. ϵ^0_{iam} is an error term. The estimated coefficient on the treatment dummy, $\hat{\beta}_0$, will give us the regression adjusted difference in means between the treatment and control groups, the intent-to-treat estimate of program impact. Since random assignment is at the individual level, we use heteroskedasticity robust standard errors.

Because of imperfect take-up of the treatment and the presence of alternative programs, intent-to-treat effects differ from the effect of receiving assistance, which could be measured in the following equation:

[4]For more details on the design of the experiment and program eligibility, see the appendix.

$$Y_{iam} = \alpha_1 + \beta_1 T_{iam} + \psi^1_{am} + \epsilon^1_{iam}, \tag{2}$$

T_{iam} is an indicator for receiving temporary financial assistance. We are interested in an estimate of the local average treatment effect of receiving temporary financial assistance on the outcome, $\hat{\beta}_1$, but an OLS estimation will measure this treatment effect with bias due to endogenous take-up of financial assistance. So, we estimate the local average treatment effect of temporary financial assistance by two-stage least squares using random assignment as an instrument for receipt of assistance.

For our baseline and primary outcome data, we largely rely on the county's Homeless Management Information System (HMIS), which includes client-level data from all publicly contracted homeless services in Santa Clara County such as homeless shelters, housing subsidies, street outreach, and financial assistance. Our primary outcome is an indicator for whether an individual is homeless. While we do not observe all forms of homelessness, through HMIS we do observe receipt of non-prevention homeless services that are only provided to those who are defined as literally homeless, most commonly emergency shelter.[5] We also use the program entry and exit dates to calculate days homeless since random assignment. Our primary outcome will miss some forms of housing stability, including living unsheltered, moving in with family or friends, or migrating to other counties. Still, entry into emergency shelter provides a useful indicator for homelessness. Shelter entry responds to evictions, increasing by 300% after an eviction order (Collinson et al., 2022); it declines in response to long-term housing subsidies in the same manner as broader survey-based measures of homelessness (Gubits et al., 2018); and it has been the primary outcome in other studies of homelessness prevention (Rolston et al., 2013; Evans et al., 2016). Nonetheless, we also examine the impact of assistance on the likelihood of switching addresses using consumer reference data. The appendix provides more information on how we define the sample

[5]HUD defines someone as literally homeless if they sleep in place not meant for human habitation or a temporary shelter (or are exiting an institution after being homeless).

and these outcomes.

Table 1 shows mean baseline characteristics for different samples of people seeking homelessness prevention services in Santa Clara County. Column (1) shows characteristics for all people seeking homelessness prevention services from July 2019 to December 2020. Clients tend to be young, female, people of color with children. Mean age is 44 years; 60% have children; only 12% are non-Hispanic White; 71% are female. These values are close to the Chicago group studied by Evans et al. (2016), which averages 39 years old, 7% non-Hispanic White, and 87% female. The questions used in the risk scoring process indicate that they arrive in vulnerable situations: 19% have been homeless recently; they average less than $2,000 of assets on hand, 80% owe someone money, 49% have bad credit, and most went to the emergency room in the past year. Even so, past contact with the homeless services is limited. Only 1.7% have received homelessness prevention services in the past year and 5.4% services for people already homeless. The final row combines all of the prior characteristics into a predicted probability of using non-prevention homeless services within 6 months after random assignment.[6] As with other prevention programs, this risk is low, 2.0%, even for a very vulnerable group of people.

Columns (2) to (5) of Table 1 help situate the sample for the experiment within this broader set of people seeking services. Columns (2) and (3) compare people who score as high versus moderate risk on the screening tool. Compared to high risk clients that the program automatically enrolls in services, people eligible for random assignment have similar demographic characteristics but are otherwise less vulnerable in many ways. For example, they are less than half as likely to have a history of homelessness. The predicted risk of homelessness is 2.2% for people scoring in the study range, compared to 3.9% for those who

[6] We compute this index using an OLS regression where the outcome is receipt of non-prevention homeless services within 6 months and the predictors are all other characteristics in Table 1. The sample for this regression includes only people who requested assistance in the two years prior to the start of the experiment but were ineligible for assistance from Destination:Home because they had a risk score lower than 12. We estimate the prediction coefficients in this group because it is a large sample of untreated individuals, but it is a lower risk group than our main sample, which makes the predictions conservative estimates of homelessness risk.

always receive services. In Column (4) we narrow the sample to those in the study, excluding people who are eligible for other prevention services, arrive after an agency's monthly quota has been exhausted, or decline to participate. Baseline characteristics vary little with these restrictions of the sample. Finally, because homelessness services changed dramatically with the onset of the COVID-19 pandemic, our main analysis will focus on people who enrolled between July 2019 and February 2020. While the external environment changed, column (5) shows that baseline characteristics of individuals were similar.

Columns (6) to (8) of Table 1 show that baseline characteristics balance across our treatment and control groups. We focus on the sample of participants who enroll prior to the pandemic.[7] Columns (6) and (7) show mean characteristics for those assigned to treatment versus control. Column (8) displays a regression-adjusted difference in means that is estimated from a regression of the characteristic on the treatment assignment indicator and agency-month fixed effects. The measured differences tend to be small both statistically and practically. For example, risk scores average 10.0 for both groups. Of 19 baseline characteristics, none are statistically different at the 5% level. If we aggregate across all characteristics to get a predicted risk of homelessness, we get a point estimate of 0.6 percentage points lower for the treatment group, which is not statistically significant and is much smaller in magnitude than the treatment effects we observe. Similarly, a joint F-test of balance from a regression of treatment assignment on all baseline characteristics does not reject the null of balance (p=0.41). Finally, we show below that treatment effects change little when controlling for baseline covariates.

[7] Appendix Tables A.1, A.2, and A.3 show similar balance for those matching to consumer address histories, enrolling after the pandemic, and pooling pre-pandemic with post-pandemic, respectively.

4 Results

4.1 Receipt of financial assistance

Random assignment determines offers of financial assistance, which may differ from actually receiving assistance. Not all study participants in the treatment group will receive financial assistance because receiving assistance requires some follow-through by the tenant and landlord. Clients must document their monthly rent and amount of rental debt, and landlords must work with program staff to receive payment; these steps sometimes fail e.g. when details differ from what was originally reported. Some study participants in the control group may receive similar assistance from other programs. We mitigate this issue by focusing on a sample of people that the case manager identifies prior to the lottery as not being eligible for other assistance (almost always because they do not have documentable income going forward), but some treatment of the comparison group will occur when the client's eligibility for other programs is mis-specified during intake.

In the HMIS data we can observe receipt of financial assistance for both groups. In the top panel of Table 2 we report receipt rates by treatment status. Within 3 months, 67% of people assigned to treatment do in fact eventually enroll in the the Destination:Home homelessness prevention program compared to only 1% of those assigned to control.[8] If we include the county's program run by the Office of Supportive Housing and all other homelessness prevention programs entered into HMIS, the treatment group enrolls 68% of the time compared to 12% for the control group. Overall, random assignment generates a meaningful 56 percentage point difference in the likelihood of receiving assistance. Later on, we will use these first stage results to estimate local average treatment effects. Also, Appendix Table A.4 shows that the compliers are similar to the full sample on all baseline characteristics.

[8]Nearly all households who will ever receive financial assistance do so within 3 months; see Appendix Figure A.2. Some households do return for repeat payments. In the 12 months after random assignment, 32% of the treatment group never receives a payment; 34% have one payment; 12% have two different months with a payment, and 22% have 3+ months with payments.

Actual payments show a similar contrast. The treatment group is 55 percentage points more likely to receive a payment within 3 months and receives an average of $2,253 within 3 months, or $1,898 more than the control group. Compared to average monthly rent of $2,201 (2017 American Housing Survey inflated to 2019 dollars) the average assistance amount provides roughly one month of rent, which is similar to other temporary financial assistance programs.[9] Payments mostly go to landlords as back rent ($1,832) but also to security deposits ($324) and other expenses like utilities and transportation ($97). Conditional on receiving any payment, the treatment group also receives a slightly larger average payment of $3,527, compared to $2,598 for the control group.[10] Payment amounts vary some within the treatment group: 36% receive no payment and the average payment among the others is $3,520. As shown in Appendix Figure A.2.b, most payments are below $3,000, but a small right tail increases the mean. Larger payments typically occur when households owe multiple months of rent.

4.2 Treatment effects for homelessness

Our treatment effect estimates (bottom panel of Table 2) show that temporary financial assistance dramatically reduced homelessness. Among the control group, 4.1% of people become homeless within 6 months, compared to 0.9% in the treatment group. The raw difference of 3.2 percentage points increases to 3.8 percentage points controlling for month-agency strata, as in equation (1). At 12 months, rates of homelessness decrease by 5.1 percentage points from a base of 7.2%. Most of this effect accumulates more than 3 months after random assignment, i.e. after payments have been made. See Figure 1.a for results at various time horizons. Appendix Table A.6 shows similar results if we control for baseline covariates. Appendix Section A.4 measures effects on address changes. All of these differences represent the intent-to-treat effect of offering financial assistance.

[9] Data from Chicago in Evans et al. (2016) suggest a lower average payment of $752, but this lower cash value still buys 0.71 months of housing in Chicago, where average rent is only $1,059.

[10] See Appendix Table A.5 for results on conditional means and more detailed payment categories.

Nearly all of the decline in our measure of homelessness results from reduced use of emergency shelters and street outreach. Financial assistance reduces shelter use by 2.5 percentage points from a base rate of 2.5 percent, which can account for about two-thirds of the overall change in program use. Street outreach, which denotes services provided to people living unsheltered, accounts for the remainder, dropping by 1.3 percentage points. Of note, both emergency shelter and street outreach decline, indicating that assistance has similar effects on both sheltered and unsheltered homelessness. Use of longer-term subsidized housing for homeless individuals (rapid re-housing, permanent supportive housing, and transitional housing) also show a negative point estimate, but this decline is not statistically significant.

These decreases in the incidence of homelessness lead to 2.5 fewer days of homelessness, as measured by program entry and exit dates, at 6 months and 7.5 fewer days at 12 months. Among those who become homeless, the average duration is 58 days,[11] which implies that the treatment primarily affects the incidence of homelessness rather reducing duration among people who become homeless.

As noted above, take-up is imperfect and some members of the control group receive financial assistance. The final column measures a local average treatment effect of actually receiving assistance using instrumental variables, as in equations (2) and (3). We define treatment as receiving any assistance within 3 months, so the second row of the top panel of Table 2 is the first stage. The second stage estimate indicates that receiving financial assistance reduces homelessness at 6 months by 6.8 percentage points and 4.5 days.

Financial assistance has less effect on homelessness after the onset of the pandemic. Figure 1.b and Appendix Table A.8 show the same outcomes for people enrolling from March to December 2020.[12]. These results show that entry into homelessness was much less common during the pandemic; only 1.2% of those in the control group subsequently became

[11] Average spell length is similar across the different program types.

[12] The follow-up period for our main sample overlaps somewhat with the pandemic. Results are similar but with larger point estimates and a much smaller sample if we limit the main sample to 180 days prior to the start of the pandemic (Appendix Table A.7). Similarly, treating the data as a panel of person-months and examining treatment effects based on time of observation rather than time of study entry shows that treatment effects are concentrated prior to the pandemic (Appendix Table A.9.

homeless. This very low rate for the control group makes it difficult to detect any effect of assistance.

Low homelessness rates for the control group likely reflect the effects of complementary interventions. First, several COVID-specific temporary financial assistance programs made similar assistance available to the control group through other sources. Second, eviction moratoria prevented landlords from evicting tenants during the post-pandemic period. See the appendix for more details on the relevant policies.

4.3 Heterogeneous effects

Because it can be difficult to target financial assistance to the marginal individuals who would become homeless but for the assistance, it is important to understand which groups benefit most from this intervention. Figure 2 shows ITT effects and confidence intervals for different subgroups. The top bar replicates our full sample results. The circle shows that the treatment group is 3.8 percentage points less likely to access homeless services within 6 months, and the shaded blue bar shows a 95% confidence interval that does not intersect with zero. Each subsequent bar limits the sample to a subgroup. For example, the second bar shows the treatment effect for those with above median risk screening scores, which is slightly larger in magnitude but not statistically different from that for those with lower scores. Although the treatment effects are similar across many of the different groups, they are larger for households without children and for those with a past history of homelessness and are smaller for overcrowded households and those who have trouble with English. See Appendix Table A.10 for precise quantities and statistical tests.

In general, we do not find strong evidence for heterogeneous effects, though power is limited. Treatment effects are larger for characteristics associated with high control group risk of homelessness. However, when we split the sample in half according to risk level implied by all available covariates, treatment effects are not statistically different. Similarly, if we use simple linear controls for the program's risk score to extrapolate to households beyond the reach of the experiment, we find similar treatment effects (Appendix Table A.11 and Appendix Figure A.5).

4.4 Cost Benefit

We estimate a rough cost-benefit using our estimated treatment effects and evidence from the literature. See the Appendix section A.6 for detailed calculations. We use a 'marginal value of public funds' framework (Hendren and Sprung-Keyser, 2020).

The direct costs of offering temporary financial assistance include both the payment itself and staffing costs. Based on our data, average assistance paid amounts to $1,898 per person assigned to treatment. We estimate that case managers spend approximately $240 (6 hours) per client administering the program. The program indirectly saves costs for other public services. We calculate effects on the cost of housing services directly and approximate effects on the health and criminal justice systems by extrapolating from existing studies on homelessness prevention (Evans et al., 2016; Palmer et al., 2019; Downes et al., 2022). We estimate $316 in savings due to reduced use of these public services. Because savings on public services are greater than the program's administrative costs, the net cost of the program, $1,822, is slightly less than the cost of the financial assistance itself.

Benefits accrue both to the recipient and to the broader community. We estimate the private value of the assistance to the recipient at its cash value, $1,898. Cash value is likely conservative because tenants are credit-constrained, and eviction-induced moves have many additional costs: loss of possessions, difficulty finding future housing, and disruptions for children (Desmond, 2016). The landlord also benefits by avoiding vacancies and damages to units that occur after evictions. Using rough approximations drawn from the qualitative literature (Garboden and Rosen, 2019), we estimate benefits to landlords at $219. Finally, the existing literature (Palmer et al., 2019) suggests that homelessness prevention programs generate large public benefits through the reduction of violent crime. While we cannot measure effects on violence directly, we extrapolate that the treatment effects on homelessness we observe would be associated with about \$2,386 in benefits to victims of crime. Altogether, we estimate the program has \$1,898 of direct benefits to recipients and \$2,605 of benefits to non-recipients

5 Conclusion

This study is the first to use a randomized controlled trial to measure the impact of temporary financial assistance for people at imminent risk of homelessness. Randomly offering assistance to tenants increases uptake of temporary financial assistance by 56 percentage points and reduces homelessness by 3.8 percentage points from a base of 4.1 percentage points, largely due to decreases in emergency shelter entry.

Whether homelessness prevention programs are effective is controversial, even among organizations dedicated to addressing homelessness. Targeting assistance towards those who would otherwise become homeless is difficult, and funds to prevent homelessness may just crowd out other resources (like help from family or friends). These concerns lead organizations like the Alliance to End Homelessness to emphasize first providing support to people who are already homeless (NAEH, 2020).

Our study suggests the effect of temporary financial assistance on homelessness is large enough to make it a cost-effective option. The net public cost is \$1,822 per person. We roughly estimate \$4,503 of benefits to recipients through income transfers, to landlords who do not have to turnover vacant units, and to members of the public from defusing violence (Palmer et al., 2019). These values imply \$2.47 of benefits per net dollar spent. A benefit ratio greater than 1 can still be obtained if one ignores either private benefits to recipients or public benefits from reducing violence, but not both.

The cost-effectiveness of other homelessness prevention programs may differ from the findings of this study. Program effectiveness can vary with client characteristics, such as housing history and family composition (Shinn et al., 2013; Greer et al., 2016; Von Wachter et al., 2019). In our context, prevention has larger effects for people with a history of homelessness and households without children. Also, we focus on marginal clients who are at lower risk of homelessness than the program's regular clients (Table 1), and prevention may be more cost effective when targeted toward them. On the other hand, a scaled-up prevention programs could change behavior, leading landlords to strategically file for eviction (Garboden and Rosen, 2019) or incentivize tenants to take on more debt. Prior work suggests that prevention programs have similar individual-level and community-level effects (Rolston et al., 2013; Goodman et al., 2016), but equilibrium effects remain an area for future research.

We find large effects of homelessness prevention despite focusing on tenants who differ from other homelessness prevention programs. Like other prevention programs, the program we study focuses on tenants who are housed, behind on rent, and at imminent risk of losing their housing. However, we focus on people who are only marginally eligible based on the program's assessment of their risk of homelessness, who live in a very high rent county, and who are ineligible for other programs because they cannot demonstrate enough income to pay their rent in the future. Compared to tenants in a moderate rent city who can prove they have sufficient income to pay rent in the future, as in Evans et al. (2016), these tenants likely pose a more difficult test for the effect of temporary financial assistance, yet we still find that financial assistance leads to large decreases in homelessness.

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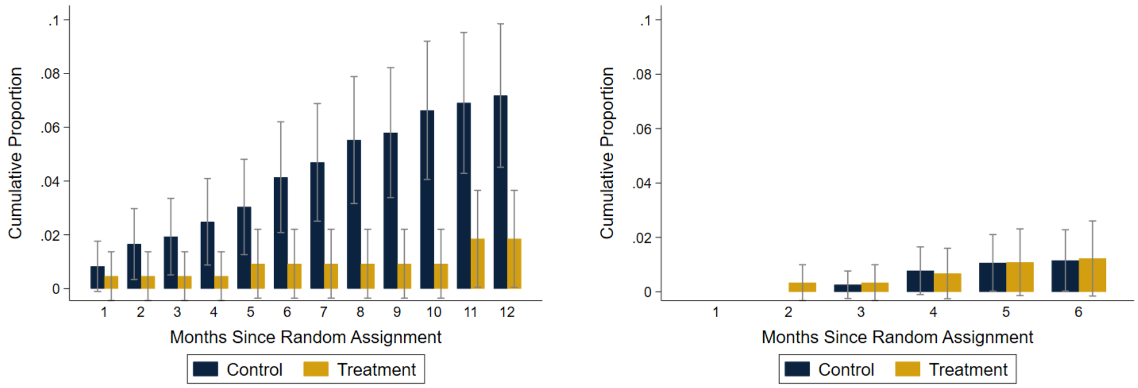


(a) Homeless, Pre-March 2020

(b) Homeless, Post-March 2020

Figure 1: Housing Outcomes over Time

Notes: Each bar show the cumulative probability of an event happening between random assignment and the listed number of months later. The event is enrolling in homelessness services covered by HMIS other than prevention. We split outcomes by random group assignment; the treatment group is offered temporary financial assistance. Error bars show a 95% confidence interval using heteroskedasticity-robust standard errors. The sample for all figures is limited to people who seek homelessness prevention services, go through random assignment, and are not eligible for other services. Panel (a) includes only people arriving before March 1, 2020. Panel (b) includes only those arriving later and limit to a shorter follow-up length because the final participants enrolling in December 2020 did not have 12 months of outcome data at the time of our data extract.

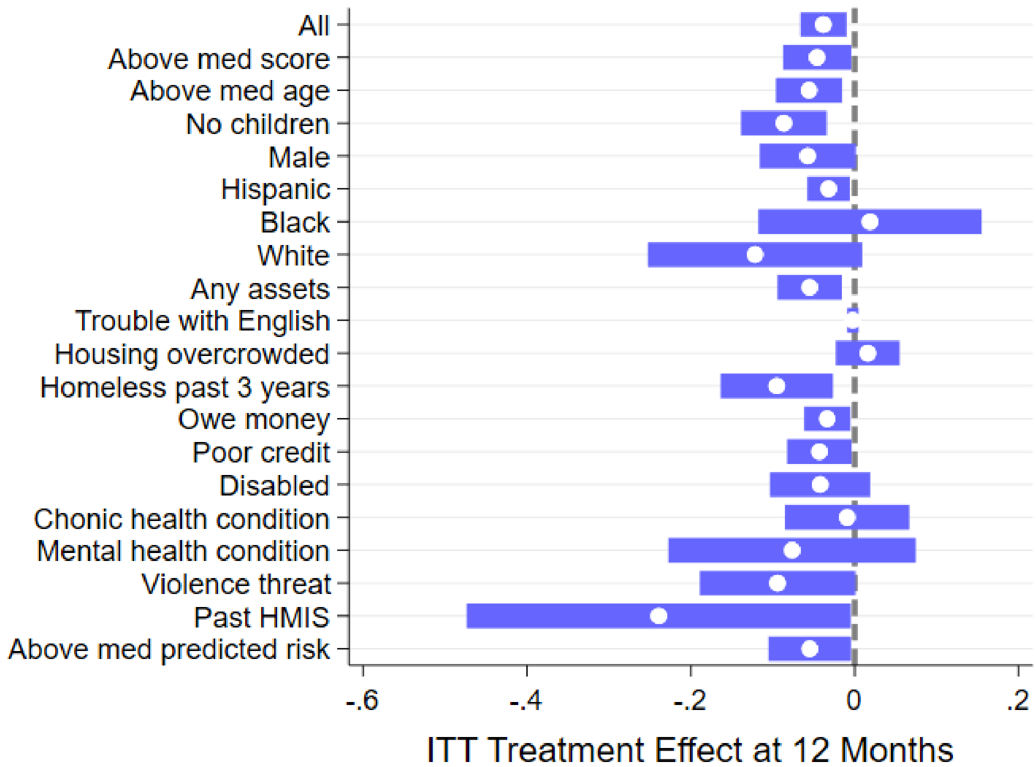


Figure 2: Effects on Homelessness within 6 Months, by Subgroup

Notes: The top row replicates the full sample result from Table 2. 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 use of non-prevention homelessness services within 6 months on treatment assignment and agency-month fixed effects. The bar shows 95% confidence intervals based on heteroskedasticity-robust standard errors.

Table 1: Baseline characteristics, sample selection and baseline balance

	(1) All	(2) Always Treat Score > 13	(3) RCT Eligible $8 \leq S \leq 13$	(4) In RCT	(5) In RCT Early	(6) In RCT Treat	(7) In RCT Control	(8) In RCT Dif.
Assessment score	8.5	16.1	9.9	9.9	10.0	10.0	10.0	-0.0
Age	44	43	44	43	44	44	45	-1
No children	0.40	0.26	0.34	0.36	0.44	0.46	0.43	0.05
Hispanic	0.67	0.69	0.70	0.71	0.64	0.65	0.63	0.01
Non-Hispanic Black	0.076	0.056	0.075	0.078	0.123	0.139	0.113	0.036
Non-Hispanic White	0.12	0.14	0.11	0.11	0.14	0.12	0.16	-0.03
Male	0.29	0.24	0.28	0.26	0.29	0.28	0.29	-0.01
Homeless, past 3 years	0.19	0.50	0.22	0.22	0.26	0.24	0.28	-0.04
Assets on hand	1718	274	362	288	301	265	322	-83
Owe money	0.80	0.96	0.92	0.93	0.94	0.93	0.95	-0.02
Poor credit	0.49	0.79	0.58	0.60	0.65	0.68	0.63	0.04
Violence threat, 6 mos	0.15	0.50	0.16	0.16	0.18	0.19	0.18	-0.03
Chronic health condition	0.17	0.41	0.21	0.21	0.19	0.22	0.17	0.05
Legal problems	0.15	0.49	0.17	0.16	0.22	0.19	0.23	-0.02
Num ER, 6 mos	0.83	1.60	0.97	1.04	1.18	1.16	1.20	0.01
Household changed, 6 mos	0.13	0.38	0.15	0.15	0.16	0.17	0.15	0.01
Any prevention services, 12 mos	0.017	0.016	0.026	0.030	0.031	0.037	0.028	0.015
Any homeless services, 12 mos	0.054	0.093	0.063	0.044	0.066	0.056	0.072	-0.044*
Address change, 12 mos	0.16	0.16	0.16	0.14	0.15	0.19	0.13	0.04
Predicted risk of homelessness	0.020	0.039	0.022	0.020	0.026	0.024	0.028	-0.006
Joint test of balance (p-value)								0.41
N	6794	799	3039	1263	578	216	362	578

Notes: Variables in the first 16 rows ('Assessment score' through 'Household changed, 6 mos') are measured in Santa Clara County HMIS data at assessment. The next three rows are measured as lagged outcomes in the HMIS and Infutor data. The final row shows the fitted values based on coefficients from a regression where the dependent variable is using homelessness services within 6 months after random assignment, the other variables in the table are the independent variables, and the sample includes only people arriving in the two years before the RCT (July 2017-June 2019) who score below the 12 point risk score threshold for program eligibility during that time. Columns (1)-(7) show means for the sample listed in the column header. Column (1) includes all people requesting prevention services from July 1, 2019 to December 31, 2020. Columns (2) and (3) narrow this group to those scoring above 13 and 8-13 on the PR-VI-SPDAT assessment, respectively. Column (4) limits to people who go through random assignment and are not expected to be eligible for other service; column (5) restricts this same sample to those doing random assignment prior to March 1, 2020. Columns (6) and (7) splits column (5) among those assigned to treatment versus control. Column (8) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, based on heteroskedasticity-robust standard errors. 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 effects are zero.

Table 2: Housing outcomes

	(1) Control	(2) Treatment	(3) Dif (OLS)	(4) Dif (IV)
D:H Homelessness Prevention (3 mos)	0.01	0.67	0.67***	
			(0.04)	
Any Homelessness Prevention (3 mos)	0.12	0.68	0.56***	
			(0.04)	
Any Payment (3 mos)	0.10	0.64	0.55***	
			(0.04)	
Total Payments (3 mos)	273	2253	1898***	
			(188)	
–Rent (3 mos)	216	1832	1551***	
			(181)	
–Security Deposit (3 mos)	32	324	319***	
			(81)	
–Other (3 mos)	25	97	29	
			(32)	
N	362	216	578	578
Homeless (6 mos)	0.041	0.009	-0.038***	-0.068***
			(0.014)	(0.026)
–Shelter (6 mos)	0.025	0.005	-0.025**	-0.044**
			(0.012)	(0.022)
–Outreach (6 mos)	0.0110	0.0000	-0.0126*	-0.0223*
			(0.0066)	(0.0119)
–Other Homeless Services (6 mos)	0.0110	0.0046	-0.0093	-0.0164
			(0.0073)	(0.0130)
Homeless (12 mos)	0.072	0.019	-0.051**	-0.090**
			(0.020)	(0.035)
Homeless (3 to 12 mos)	0.064	0.014	-0.046**	-0.081**
			(0.018)	(0.032)
Homeless Days (6 mos)	2.5	0.3	-2.5**	-4.5**
			(1.2)	(2.2)
Homeless Days (12 mos)	7.5	0.5	-7.5***	-13.2***
			(2.5)	(4.5)
N	362	216	578	578

Notes: We measure all outcome variables cumulatively between random assignment and the listed duration afterward using HMIS data. The sample consists of people who went through random assignment between July 2019 and February 2020 and were not eligible for other prevention services. Columns (1) and (2) show raw means for the control and treatment groups, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Column (4) shows the coefficient on receipt of assistance in an instrumental variables regression where the first stage is the row for 'Any HP Payment Made' within 3 months. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

A Appendix

A.1 Program eligibility

Clients visit participating non-profit service providers seeking help to prevent imminent homelessness. Participants must be residents of Santa Clara County with income below 80% of the area median ($106,461 in 2019). Clients meet with a case manager who determines if they are housed; clients who are already homeless are directed to non-prevention programming, such as emergency shelter or long-term rental subsidies. Clients much also be at at imminent risk of homelessness, which is defined as having an eviction notice, having a notice to vacate, or being within 14 days of losing housing or missing a rent payment that could cause the household to lose housing. Clients not at imminent risk of losing housing qualify if their housing is unsafe (e.g. due to domestic violence).

Applicants then complete an eligibility screen called the PR-VI-SPDAT. The standard VI-SPDAT is the most common screening tool used to determine eligibility for programs serving people who are already homeless. PR-VI-SPDAT is a similar tool that is designed to identify people who are both at risk of homelessness and at risk of serious harm (i.e. having experienced violence or threats of violence in the last six months) if they become homeless. This screen tool, asks a series of questions about family structure, housing history, financial situation, healthcare history, etc. Based on the answers to these questions, applicants are assigned a risk score ranging from 0 (lowest risk) to 29 (highest risk). Our study focuses only on people who score in the middle range of the PR-VI-SPDAT–those with scores between 8 and 13. Clients scoring above 13 are automatically eligible for financial assistance, while those scoring below 8 are ineligible. So, we are not able to randomize access to treatment for these groups. The mid-range group on which we focus is eligible for assistance, but under normal operating circumstances, this group is offered assistance only if it is available (instead of being guaranteed an offer). So, for those in this group who enroll in the study, we allocate access to assistance randomly.

Notably, this homelessness prevention program does not screen on whether the client can sustain rent payments after the assistance ends. Many homelessness prevention programs, including the one studied in Evans et al. (2016), deny assistance to people with an income loss of unknown duration and focus attention on people who can demonstrate that they will resolve their situation soon, with a new job or restored public benefits, for example. The lack of a screen for future income has two main implications:

First, eligibility for our program of interest differs from other prevention programs in Santa Clara County, which allows us to identify a comparison group of people who do not have other available sources of assistance. The primary alternative program, funded by the County's Office of Supportive Housing, does not use the PR-VI-SPDAT, but instead requires sustainable income going forward. Since that alternative program is not oversubscribed, anyone denied assistance by the Destination:Home program who can document sufficient future income will get assistance anyway; those who cannot provide such documentation will not. Thus, we limit our sample to the group of people who do not have access to other programs.

Second, homelessness prevention programs may have different effects for people who can document a future ability to cover their rent, as compared to those who cannot. Screening on future income is designed to target funds towards those facing a transitory rather than permanent shock, which presumes that temporary assistance is better suited to address the former. However, this restriction may also screen out the very people who need assistance the most, particularly in a city with high and rapidly rising rents. For example, Evans et al. (2016) study a program that has such a screen and find that the program reduces homelessness, but they also find that, among eligible people, those with the lowest incomes benefit the most. Thus, our study is unique in that it tests whether temporary financial assistance works even when clients cannot demonstrate that they have sufficient future resources to cover their living expenses.

A.2 Random assignment

Since the Destination:Home homelessness prevention program is oversubscribed, we worked with the agencies operating the program to setup a system that allocates assistance through a lottery, which operated from July 2019 to December 2020.[13] A case worker helps the client complete a brief intake process that includes informed consent, a very short baseline survey, and a lottery on the spot to determine if that person will receive assistance.

After being identified as scoring in the appropriate PR-VI-SPDAT range, the case worker informs clients about the study. The case worker briefly explains the purpose of the study and the way the lottery works. All clients already sign a release of information. The study slightly modifies that form to acknowledge the introduction of the lottery. Following the lottery, study participants are not contacted again in the context of the research study. Any prospective clients who are eligible for the study, but do not wish to participate, cannot receive assistance from this particular program.

The case worker enters participant information in a web-based form. This form includes the Homeless Management Information System (HMIS) ID for the person, which allows the study enrollment record to be linked to administrative records for both baseline and outcome data. Importantly, prior to randomization, the case worker records if they expect that, if denied by the lottery, the client will be eligible for other similar services. We exclude from our main sample people who are expected to be eligible for similar assistance through other programs.

The computer conducts an immediate lottery to determine if the case worker should offer assistance. The lottery is stratified with a probability of treatment that varies by agency-month. In particular, each agency has a different quota of treatment and control slots for the month.[14] The order of treatment and control slots is randomly sorted, and then, for any given client, the case worker presses a button and is told the client's treatment status based on the next slot. Random assignment continues until the treatment and control slots are

[13] Appendix Figure A.1 summarizes the flow of people into the experiment

[14] The quotas for each agency-month are set to account for several factors. The total budget of the program is not sufficient to serve all eligible and interested households, which makes random assignment feasible and ethical, but demand at any one point in time and any one agency varies considerably. The quotas are set in response to expected demand, staff capacity, and funding restrictions. But due to the uncertainty of demand, they also have to balance a desire to exhaust funding against spreading it out evenly over an extended period of time. We also randomly perturb the quota each month to make it difficult to predict when the lottery will end.

exhausted. On the last study assignment for the month, the case worker is told that all future applicants for the month will be rejected, and the staff no longer enters client information in the lottery. The result is random assignment of assistance within an agency-month for those eligible clients who apply while the lottery is active.

Not all study participants in the treatment group will receive financial assistance because receiving assistance requires some follow-through by the tenant and landlord. After being approved by lottery for the program, clients must provide a lease or otherwise get their landlord to confirm their monthly rent and amount of rental debt. The landlord must also work with program staff to receive payment. These steps sometimes fail to occur because the client does not follow through or the relevant details differ from what was originally reported. Some study participants in the control group may receive similar assistance from the homelessness prevention program operated by the Office Supportive Housing or other private programs. We mitigate this issue by focusing on a sample of people that the case manager identifies prior to the lottery as not being eligible for other assistance (almost always because they do not have documentable income going forward). Thus, the vast majority of the control group will receive only non-financial assistance, but some treatment of the comparison group will occur to the extent that the client's eligibility for other programs is mis-specified during intake.

Finally, clients are referred to services. Clients assigned to treatment are offered the Destination:Home program. Those assigned to control receive usual care. For our main sample of people who are not eligible for other similar financial assistance, these alternative services include case management and information about other ways to find housing in Santa Clara County. Thus, the primary contrast between treatment and control is the financial assistance.

A.3 Data

As discussed in Section 3, the sample that was randomized through a lottery consists of 1,263 people who sought homelessness prevention services from July 2019 to December 2020, scored between 8 and 13 on the PR-VI-SPDAT, were not potentially eligible for other prevention services, arrived before an agency's monthly quota had been exhausted, and consented to participate. We construct our primary outcome for this sample using Homeless Management Information System data from Santa Clara County. HMIS is a common tool for coordinating homelessness care at the county level. Each record includes the person's HMIS ID, which allows for a nearly perfect match across records.

The data we use starts with people who complete assessments for homelessness prevention services. All agencies in the network use the PR-VI-SPDAT assessment and screen for the Destination:Home program before other programs, so the set of people taking an assessment is the full set of people seeking homelessness prevention services at these agencies. We exclude minors and people who are de-identified in administrative records (e.g. domestic violence survivors). The assessment provides baseline characteristics for the analysis. These data include both the final risk score as well as responses to all questions on the tool, which cover family structure, housing history, financial situation, healthcare history, and so on. The data extract also includes basic demographics that the agencies ask of anyone seeking services. For people who go through study enrollment, we have a couple of additional pieces of information. The case worker records whether the person is eligible for other homelessness prevention programs, and the computer randomly assigns treatment status.

We define an individual as being homeless if, at some point after random assignment, they are recorded in the HMIS system as having stayed at a homeless shelter or received other services only made available to those experiencing homelessness including longer-term subsidized housing (Rapid Re-Housing, Permanent Supportive Housing, Transitional Housing) and contact with the coordinated entry system or street outreach.[15] Thus, our measure of homelessness indicates that the person was homeless, sought services, and was able to access them. Note that no one in our study is homeless at the time of random assignment, because if they were they would not be eligible for financial assistance. With date-specific service records, we can track this measure of homelessness for both treatment and control group participants, as well as use of programs prior to the study.

A.4 Address changes

To measure address changes, we match study participants to data from Infutor Data Solutions, which aggregates consumer information (e.g. cell phone bills, credit records) into an address history that lists exact addresses with start and end dates for most residents in the United States. Staff at Santa Clara County match this data to HMIS with a fuzzy matching algorithm using name, date of birth, and last 4 digits of Social Security Number.[16] We only match 53% of people in the RCT to an Infutor record and 62% of those who enroll prior to March 2020; we limit our sample to these matched observations when analyzing address changes.

We define this secondary outcome as an indicator for whether any address starts or ends during a period of time. Not all address changes imply housing instability–for example, some individuals and families in our study may move to live in a better neighborhood or closer to an employer–but, in general, frequent moves imply a lack of stability in the sample of unstably housed people that we examine. In particular, Phillips (2020) shows that this measure of address changes spikes for the comparison group from Evans et al. (2016) when people in Chicago seek homelessness prevention services but are not served. Address changes are more common than use of homeless services and so can provide a complementary outcome that may detect unsheltered homelessness that does not result in contact with street outreach services and less extreme forms of housing instability, such as when individuals and families informally move in with friends and family in crowded living situations.

Figure A.3.a displays the likelihood of changing addresses for those offered versus not offered financial assistance prior to March 2020. There is some evidence that the control group is more likely to move for the first 8 months after random assignment, but these

[15]Street outreach refers to building relationships with and providing services in unsheltered settings to people who are living unsheltered. For example, an agency might provide case management and health services at an encampment.

[16]Santa Clara County staff implement this match using an algorithm we designed. Because data in Infutor for SSNs is partial, we allow for a couple different types of matches. We require all observations to match exactly on year of birth. We then match either (i) exactly on last 4 digits of SSN and at least one name or (ii) a better fuzzy match on both first and last name (bigram match better than 50%). We allow for multiple matches as Infutor often does not connect records that meet these criteria.

differences are not statistically significant, and by 9 months the moves rates for the two groups are indistinguishable from each other. To be precise, among people who ever match to the consumer reference data, 11% move in the 6 months after requesting assistance. This value is 1 percentage point lower in the treatment group, but the 95% confidence interval ranges from -8 to +7 percentage points. The treatment effect on address changes does not statistically differ from either zero or the treatment effect on homeless program use. Results are similar for the post-pandemic period, as shown in Figure A.3.b.

The contrast between the results for homeless programs and address changes has at least three possible explanations. First, the address change results are more statistically uncertain. Because the matched sample is considerably smaller, confidence intervals are larger, and we cannot reject that the effects on homelessness and address changes are equal. Second, changes in program use could imply changes in take-up–i.e. assistance affects the likelihood of taking up services, but not homelessness. We view this interpretation as unlikely since entry into programs is typically used to measure homelessness (Rolston et al., 2013; Evans et al., 2016), and financial assistance seems less likely than other interventions, like case management, to change take-up of services conditional on being homeless. Third, address changes may measure milder forms of housing instability, such as doubling up with family and friends, which are more common than entering emergency shelter. See, for example, Gubits et al. (2018). In our data, the two measures are largely uncorrelated: 5% of people who change addresses appear in homelessness programs, compared to 4% of those who do not change addresses. Thus, our results may indicate that financial assistance sharply reduces the most extreme forms of housing stability without affecting less extreme situations. This interpretation is consistent with similar results in Chicago (Evans et al., 2016; Phillips, 2020).

The address history data also allow us to test for differential attrition from other administrative records. Our main outcomes on homeless program use come from data that covers only Santa Clara County, so our main results could be biased if financial assistance allows some households to migrate away from the county but not homelessness. Figure A.3.c shows rates at which members of the treatment and control group appear at addresses outside the county. In the control group, 4% of the control group does changes addresses outside the county within 6 months; however, the rate for the treatment group is nearly identical. As with overall move rates, estimates of treatment effects on migration will be noisy in this small sample, but the low base rate of migration and similar estimates across treatment and control suggest migration is a relatively minor issue.

A.5 COVID-Era Housing Policies in Santa Clara County

Several emergency financial assistance programs similar to the one we study appeared in Santa Clara County at the onset of the COVID-19 pandemic. These include local programs such as the COVID Family Assistance Program (Destination:Home), COVID-19 Emergency Homelessness Prevention Program (Destination:Home), and Tenant Based Rental Assistance (City of San Jose). A large state program, the Emergency Rental Assistance Program (State of California), also appeared. Such programs were close substitutes. For example, the Destination:Home program we study received a large spike in households served in the first quarter of 2021, when other local and state programs were absent (Appendix Figure A.4). Though, we unfortunately cannot directly measure take-up of financial assistance in the control group after the onset of COVID with the available HMIS data (Appendix Table A.8), because many of these COVID-specific programs do not appear in HMIS.

Eviction moratoria prevented landlords from evicting tenants in Santa Clara County from March 2020 through the end of our sample period. Federal moratoria from the CARES Act and the CDC covered March 27, 2020 through August 26, 2021, except for a gap in August 2020. California's state moratorium also prevented evictions for rent owed from March 1, 2020 to September 30, 2021 and only allowed them thereafter if the tenant had paid less than 25% of back rent and had not applied for rental assistance. These moratoria very likely reduced the prevalence of evictions and homelessness for families not receiving assistance.

A.6 Cost Benefit Calculations

We estimate a rough cost-benefit for offering temporary financial assistance by combining our estimated treatment effects with evidence from the literature. We divide costs and benefits into four categories: direct cost of the program, indirect effects on public finances, direct benefits to recipients, and spillovers onto non-recipients. We bring these estimates together in a 'marginal value of public funds' framework (Hendren and Sprung-Keyser, 2020). Ultimately, we estimate a net public cost of $1,822 per person, $1,898 of direct benefits to recipients, and $2,605 of benefits to non-recipients, which together imply an MVPF ratio of 2.5.

The direct costs of offering temporary financial assistance include both the payment itself and staffing costs. Based on our data, average assistance paid amounts to $1,898 per person assigned to treatment. Beyond this cost, case managers must spend time assessing clients for the program, verifying documentation, and making payment to the landlord. From another study with time use data (Phillips and Sullivan, 2021) we estimate that these activities take approximately 6 hours, or $240 at a cost of $40 per hour that includes wages and other benefits.

Financial assistance could affect other public expenditures on housing, criminal justice and healthcare systems. Flaming et al. (2015) shows that the vast majority of public finance costs of serving people who are homeless result from housing, criminal justice, and healthcare costs. For housing, we directly observe a decrease of 0.051 homeless spells per person assigned to treatment. Khadduri et al. (2010) estimate that a homeless episode costs $2,400 in 2012 dollars to housing programs, giving an expected savings of $122 in 2012 dollars, which we inflate to $139 in 2020 dollars. We estimate savings to the criminal justice and healthcare systems by extrapolating based on research from Chicago. We assume no change in healthcare costs because of null results in Downes et al. (2022). For criminal justice, Palmer et al. (2019) estimate 0.86 percentage points fewer arrests for violent crimes in response to a 1.6 percentage point drop in shelter entry after 6 months. Results from Flaming et al. (2015) imply that the Santa Clara County criminal justice system spends $11,237 on average when they arrest a homeless person, in 2012 dollars. In our data, shelter entry drops by 2.5 percentage points within 6 months for people assigned to treatment. If the relationship with arrests is proportional to that in Chicago, assignment to financial assistance would avert 0.013 arrests and obtain an expected savings of $171. Housing court costs are likely much smaller than criminal court. Housing courts assign average fees of $109 to tenants (Leung et al., 2021), which tenants frequently do not pay. If this amount provides a rough estimate of the cost of court administration, court costs averted only amount to about \$6. Finally, while there is some evidence that being evicted affects employment (Collinson et al., 2022), tax rates are sufficiently low for this population that any changes in tax revenue would be negligible. Altogether, we estimate \$316 in indirect savings to public finance.

We can easily infer a lower bound for the direct benefits to recipients because the treatment is similar to cash. For some tenants, temporary financial assistance makes a rent payment that would otherwise go unpaid. This payment secures housing services that we value at the amount of the rent. For others, the assistance is inframarginal and frees up funds for the tenant that we value as income. In either of these cases, the value to the tenant is the amount of the payment, averaging \$1,898. Tenants are credit-constrained, and eviction-induced moves have many costs beyond the loss of housing including loss of possessions, increased difficulty finding future housing, and disruptions for children (Desmond, 2016). Because many of these effects are difficult to quantify and value, we use the direct payment amount to value private benefits, but it is clearly an underestimate.

Assistance also affects people other than the tenant. The landlord of the tenant receives some benefits. Garboden and Rosen (2019) report that an eviction typically costs a landlord \$1,000 in repairs and at least 1 month of foregone rent while finding a new tenant, which we value conservatively at 1.5 months of rent at \$2,200 per month. Landlords may also benefit directly from the assistance payment if the incidence of payment does not go entirely to the tenant, but we ignore this effect to get a conservative estimate of external benefits. Overall, if financial assistance averts 0.051 evictions, then landlords save an average of \$219.30 per person assigned to treatment. Beyond the landlord, the previous literature shows the financial assistance benefits the public by reducing violence. We follow Palmer et al. (2019) in how we value victim costs per assault and for how we inflate effects on arrests to account for crime incidents that do not lead to an arrest. These same imply that a decrease of 0.013 arrests implies 0.075 fewer assaults, valued at \$2,386 in benefits to victims.

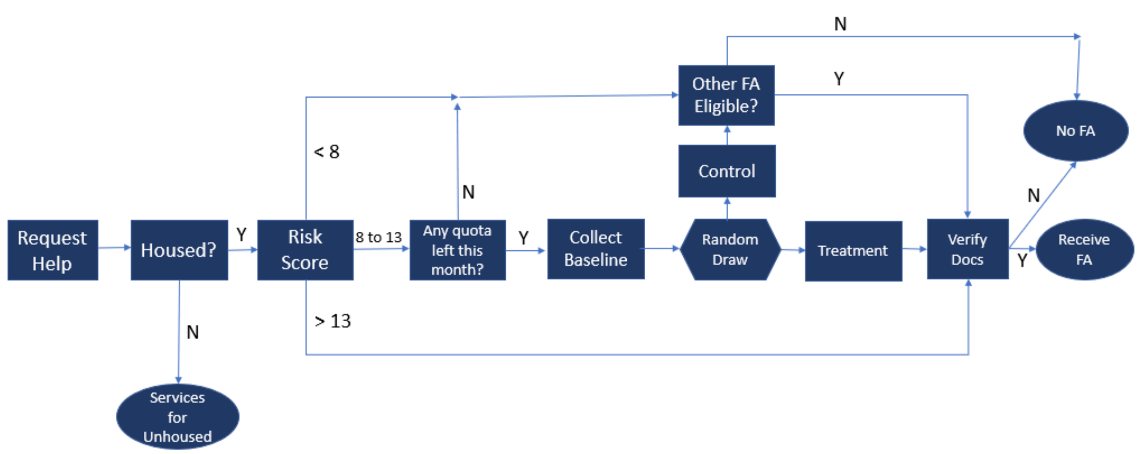
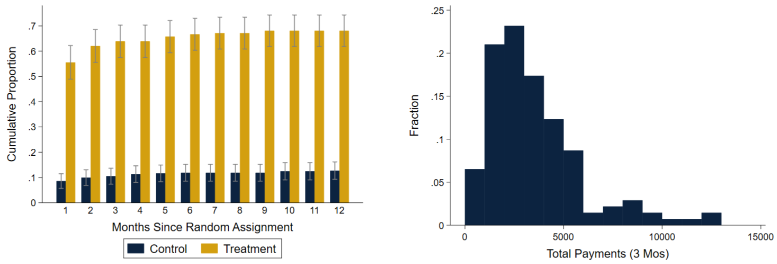


Figure A.1: Experimental Design

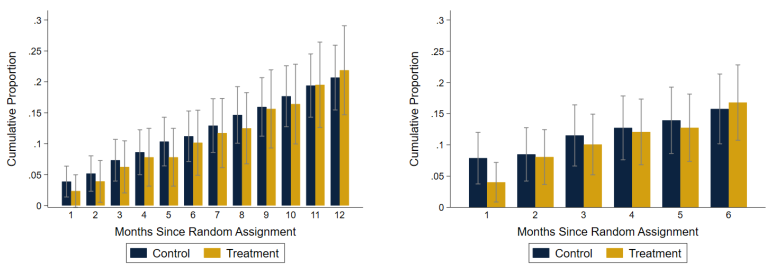
Notes: 'FA' refers to temporary financial assistance.



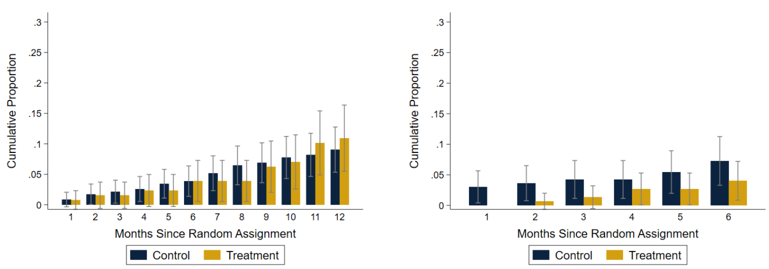
(a) Any Assistance Received (b) Distribution of Assistance (if positive)

Figure A.2: Financial Assistance Received

Notes: For each person, we calculate the amount of financial assistance received between the time of random assignment and later dates. Panel (a) shows the proportion of people in each treatment group receiving any payment by different time horizons. Panel (b) shows the distribution of assistance received within 3 months for people who are not eligible for other assistance, are randomly assigned to treatment, and actually receive assistance within 3 months. Both panels limit the sample to people who enroll before March 1, 2020.



(a) Any Address Change, Pre-March 2020 (b) Any Address Change, Post-March 2020



(c) Address Outside County, Pre-March 2020 (d) Address Outside County, Post-March 2020

Figure A.3: Housing Outcomes over Time

Notes: Each bar show the cumulative probability of an event happening between random assignment and the listed number of months later. The event is having a new address start or an existing address end. We split outcomes by random group assignment; the treatment group is offered temporary financial assistance. Error bars show a 95% confidence interval using heteroskedasticity-robust standard errors. The sample for all figures is limited to people who seek homelessness prevention services, go through random assignment, and are not eligible for other services. Panels (a) and (b) show an outcome covering all moves while panels (c) and (d) indicate only moves outside Santa Clara County. Panels (a) and (c) include people arriving before March 1, 2020. Panels (b) and (d) include those arriving later and limit to a shorter follow-up length because the final participants enrolling in December 2020 did not have 12 months of outcome data at the time of our data extract.

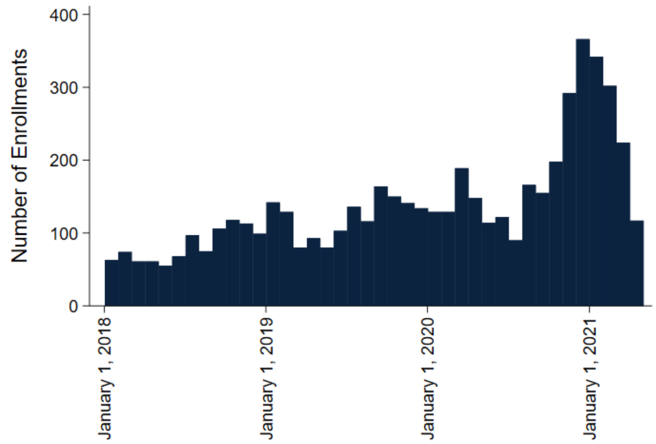


Figure A.4: Number of Homelessness Prevention Program Enrollments in HMIS, by Month

Notes: Each bar shows the total number of households enrolling in homelessness prevention programs covered by the Santa Clara County HMIS data, by month. The data covers the Destination:Home program, the Office of Supportive Housing program, and others. It does not include homelessness prevention programs operated by the State of California and some local programs developed in response to the COVID-19 pandemic.

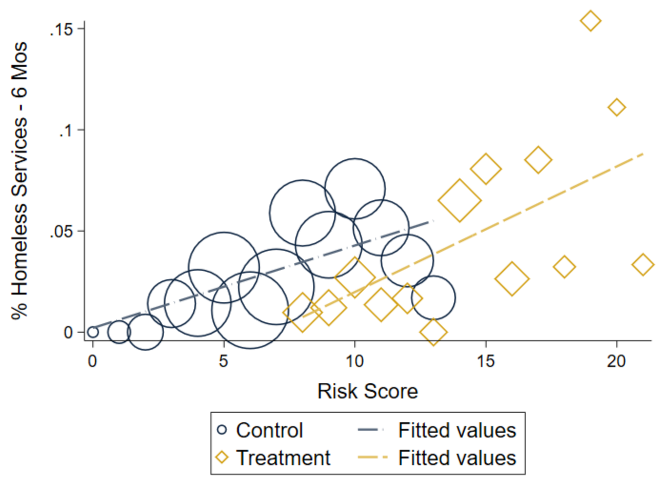


Figure A.5: Homelessness Rates at 6 Months for All Households, by Risk Score

Notes: The graph all people who are assessed for prevention for the first time between July 1, 2019 and March 1, 2020, both those in the main RCT sample and those not. Each circle plots the rate of homelessness at 6 months for a group of clients with the same PR-VI-SPDAT risk score and treatment assignment. The size of the circle is proportional to the number of clients with that risk score. Within the main study sample, assignment to treatment is identical to random assignment so that column (1) replicates the main study results with an added control for risk score. Outside the main RCT sample, we code treatment as follows. In accordance with program rules, households with risk scores below 8 are considered as assigned to control, and those above 13 are considered assigned to treatment. Households scoring between 8 and 13 who do not complete study intake (e.g. because they arrived too late in the month to get assistance) are considered assigned to control. Households who score between 8 and 13 and complete study intake but are excluded from the main sample (e.g. because the case worker expects them to receive assistance elsewhere) are considered assigned to treatment.

Table A.1: Balance table, match to Infutor

	(1) Control	(2) Treat	(3) Adj Diff
Assessment score	9.99	9.96	-0.10
Age	48	47	-1
No children	0.47	0.50	0.05
Hispanic	0.60	0.60	-0.01
Non-Hispanic Black	0.11	0.18	0.09**
Non-Hispanic White	0.18	0.15	-0.04
Male	0.28	0.29	0.01
Homeless, past 3 years	0.28	0.30	0.02
Assets on hand	280	265	-44
Owe money	0.95	0.92	-0.03
Poor credit	0.63	0.70	0.07
Violence threat, 6 mos	0.19	0.20	-0.03
Chronic health condition	0.21	0.21	0.01
Legal problems	0.24	0.16	-0.04
Num ER, 6 mos	1.1	1.1	0.1
Household changed, 6 mos	0.13	0.12	-0.02
Any prevention services, 12 mos	0.013	0.023	0.008
Any homeless services, 12 mos	0.073	0.070	-0.040
Address change, 12 mos	0.13	0.19	0.04
Predicted risk of homelessness	0.030	0.031	-0.002
N	232	128	360

Notes: The sample consists of people who go through random assignment between July 2019 and February 2020, are ineligible for other prevention services, and match to an Infutor address history based on name, date of birth, and last four digits of Social Security Number. Variables in the first 16 rows ('Assessment score' through 'Household changed, 6 mos') are measured in Santa Clara County HMIS data at assessment. The next three rows are measured as lagged outcomes in the HMIS and Infutor data. The final row is the fitted values of a regression where the dependent variable is using homelessness services within 6 months after random assignment, the other variables in the table are the independent variables, and the sample includes people arriving prior to March 2020 who are in the RCT control group and or score below the threshold for program eligibility. Columns (1) and (2) show means for those assigned to control versus treatment, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, based on heteroskedasticity-robust standard errors.

Table A.2: Balance table, post-March 2020

	(1) Control	(2) Treat	(3) Adj Diff
Assessment score	9.76	9.79	-0.01
Age	43	43	0
No children	0.28	0.29	0.02
Hispanic	0.74	0.79	0.04
Non-Hispanic Black	0.036	0.047	0.006
Non-Hispanic White	0.098	0.054	-0.051**
Male	0.24	0.22	-0.01
Homeless, past 3 years	0.18	0.17	-0.03
Assets on hand	303	242	-130
Owe money	0.94	0.89	-0.03
Poor credit	0.52	0.59	0.04
Violence threat, 6 mos	0.11	0.17	0.05*
Chronic health condition	0.23	0.23	-0.03
Legal problems	0.12	0.11	-0.02
Num ER, 6 mos	0.92	0.92	-0.07
Household changed, 6 mos	0.13	0.15	0.02
Any prevention services, 12 mos	0.021	0.040	0.008
Any homeless services, 12 mos	0.021	0.034	0.003
Address change, 12 mos	0.13	0.12	-0.01
Predicted risk of homelessness	0.014	0.015	-0.000
N	387	298	685

Notes: The sample consists of people who go through random assignment between July 2019 and February 2020 and are ineligible for other prevention services. Variables in the first 16 rows ('Assessment score' through 'Household changed, 6 mos') are measured in Santa Clara County HMIS data at assessment. The next three rows are measured as lagged outcomes in the HMIS and Infutor data. The final row is the fitted values of a regression where the dependent variable is using homelessness services within 6 months after random assignment, the other variables in the table are the independent variables, and the sample includes people arriving prior to March 2020 who are in the RCT control group and or score below the threshold for program eligibility. Columns (1) and (2) show means for those assigned to control versus treatment, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, based on heteroskedasticity-robust standard errors

Table A.3: Balance table, all

	(1) Control	(2) Treat	(3) Adj Diff
Assessment score	9.89	9.89	-0.02
Age	44	43	-0
No children	0.35	0.36	0.03
Hispanic	0.69	0.73	0.03
Non-Hispanic Black	0.073	0.086	0.019
Non-Hispanic White	0.13	0.08	-0.04**
Male	0.26	0.25	-0.01
Homeless, past 3 years	0.23	0.20	-0.03
Assets on hand	312	252	-110
Owe money	0.94	0.90	-0.03*
Poor credit	0.57	0.63	0.04
Violence threat, 6 mos	0.14	0.18	0.02
Chronic health condition	0.20	0.23	0.00
Legal problems	0.18	0.15	-0.02
Num ER, 6 mos	1.1	1.0	-0.0
Household changed, 6 mos	0.14	0.16	0.02
Any prevention services, 12 mos	0.024	0.039	0.011
Any homeless services, 12 mos	0.045	0.043	-0.018
Address change, 12 mos	0.13	0.15	0.02
Predicted risk of homelessness	0.020	0.019	-0.003
N	749	514	1263

Notes: The sample consists of people who go through random assignment at any point and are ineligible for other prevention services. Variables in the first 16 rows ('Assessment score' through 'Household changed, 6 mos') are measured in Santa Clara County HMIS data at assessment. The next three rows are measured as lagged outcomes in the HMIS and Infutor data. The final row is the fitted values of a regression where the dependent variable is using homelessness services within 6 months after random assignment, the other variables in the table are the independent variables, and the sample includes people arriving prior to March 2020 who are in the RCT control group and or score below the threshold for program eligibility. Columns (1) and (2) show means for those assigned to control versus treatment, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, based on heteroskedasticity-robust standard errors

Table A.4: Comparison of compliers to other groups

	(1) All Mean	(2) Compliers Mean	(3) Always Mean	(4) Never Mean	(5) (2) - (1) Dif.
Assessment score	10.03	10.09	9.95	9.96	0.06
					(0.11)
Age	44.23	43.81	49.74	42.90	-0.40
					(0.93)
No children	0.44	0.39	0.63	0.46	-0.05
					(0.03)
Hispanic	0.64	0.68	0.37	0.67	0.04
					(0.03)
Non-Hispanic Black	0.12	0.12	0.19	0.10	-0.00
					(0.02)
Non-Hispanic White	0.14	0.11	0.30	0.13	-0.03
					(0.02)
Male	0.29	0.32	0.35	0.21	0.03
					(0.03)
Homeless, past 3 years	0.26	0.23	0.28	0.30	-0.03
					(0.03)
Assets on hand	300.90	345.89	294.37	224.62	45.76
					(45.14)
Owe money	0.94	0.95	0.95	0.91	0.01
					(0.02)
Poor credit	0.65	0.63	0.63	0.70	-0.03
					(0.03)
Violence threat, 6 mos	0.18	0.17	0.21	0.19	-0.01
					(0.03)
Chronic health condition	0.19	0.18	0.19	0.21	-0.01
					(0.03)
Legal problems	0.22	0.26	0.16	0.17	0.04
					(0.03)
Num ER, 6 mos	1.18	1.25	1.21	1.04	0.07
					(0.10)
Household changed, 6 mos	0.16	0.15	0.12	0.19	-0.01
					(0.03)
Any prevention services, 12 mos	0.03	0.02	0.07	0.03	-0.01
					(0.01)
Any homeless services, 12 mos	0.07	0.08	0.12	0.03	0.01
					(0.01)
Address change, 12 mos	0.15	0.12	0.00	0.26	-0.03
					(0.04)
Predicted risk of homelessness	0.03	0.03	0.04	0.02	-0.00
					(0.00)
Fraction of Sample	1.00	0.56	0.12	0.32	

Notes: Column (1) shows means for the sample from Table 2. Columns (3) and (4) limit the sample, respectively, to always takers (assigned control but get prevention) and never takers (assigned treatment but not getting prevention). Column (2) computes complier means based on the other three columns as in Marbach and Hangartner (2020); this calculation ignores strata fixed effects. Heteroskedasticity-robust standard errors are from seemingly unrelated regressions. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***.

Table A.5: Assistance payments, in detail

	(1) Control	(2) Treatment	(3) Dif (OLS)
Total Payments (3 mos)	273	2253	1898***
			(188)
–Rent (3 mos)	216	1832	1551***
			(181)
–Security Deposit (3 mos)	32	324	319***
			(81)
–Other (3 mos)	25	97	29
			(32)
Total Payment, Conditional on Positive (3 mos)	2598	3527	574
			(436)
–Rent Payment, Conditional on Positive (3 mos)	2439	3382	521
			(459)
–SD Payment, Conditional on Positive (3 mos)	1916	2501	122
			(501)
–Other Payment, Conditional on Positive (3 mos)	2297	2995	-995
			(1245)
Any Payment (3 mos)	0.10	0.64	0.55***
			(0.04)
–Any Rent (3 mos)	0.09	0.54	0.47***
			(0.04)
–Any Security Deposit (3 mos)	0.017	0.130	0.122***
			(0.028)
–Any Other (3 mos)	0.011	0.032	0.016
			(0.012)
—-Any Utilities (3 mos)	0.0028	0.0093	0.0099
			(0.0087)
—-Any Other Housing (3 mos)	0.0028	0.0000	-0.0019
			(0.0020)
—-Any Transportation (3 mos)	0.0055	0.0000	-0.0032
			(0.0024)
N	362	216	578

Notes: We measure all outcome variables cumulatively between random assignment and the listed duration afterward using HMIS data. The sample consists of people who went through random assignment prior to March 1, 2020 and were not eligible for other prevention services. Columns (1) and (2) show raw means for the control and treatment groups, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table A.6: Housing outcomes, with controls

	(1) Control	(2) Treatment	(3) Dif (OLS)	(4) Dif (IV)
D:H Homelessness Prevention (3 mos)	0.01	0.67	0.67***	
			(0.04)	
Any Homelessness Prevention (3 mos)	0.12	0.68	0.56***	
			(0.04)	
Any Payment (3 mos)	0.10	0.64	0.55***	
			(0.04)	
Total Payments (3 mos)	273	2253	1899***	
			(185)	
–Rent (3 mos)	216	1832	1559***	
			(179)	
–Security Deposit (3 mos)	32	324	306***	
			(77)	
–Other (3 mos)	25	97	34	
			(35)	
N	362	216	578	578
Homeless (6 mos)	0.041	0.009	-0.032**	-0.056**
			(0.014)	(0.024)
–Shelter (6 mos)	0.025	0.005	-0.020*	-0.035*
			(0.011)	(0.019)
–Outreach (6 mos)	0.0110	0.0000	-0.0115*	-0.0205*
			(0.0062)	(0.0111)
–Other Homeless Services (6 mos)	0.0110	0.0046	-0.0042	-0.0075
			(0.0072)	(0.0127)
Homeless (12 mos)	0.072	0.019	-0.039**	-0.070**
			(0.019)	(0.034)
Homeless (3 to 12 mos)	0.064	0.014	-0.034*	-0.059*
			(0.018)	(0.032)
Homeless Days (6 mos)	2.5	0.3	-1.9**	-3.3**
			(0.9)	(1.6)
Homeless Days (12 mos)	7.5	0.5	-5.7***	-10.2***
			(2.0)	(3.6)
N	362	216	578	578

Notes: We measure all outcome variables cumulatively between random assignment and the listed duration afterward using HMIS data. The sample consists of people who went through random assignment between July 2019 and February 2020 and were not eligible for other prevention services. Columns (1) and (2) show raw means for the control and treatment groups, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Column (4) shows the coefficient on receipt of assistance in an instrumental variables regression where the first stage is the row for 'Any HP Payment Made' within 3 months. In columns (3) and (4) we also control for all variables listed in Table 1 except for predicted risk of homelessness. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table A.7: Housing outcomes, enrolled at least 6 months prior to March 2020

	(1) Control	(2) Treatment	(3) Dif (OLS)	(4) Dif (IV)
D:H Homelessness Prevention (3 mos)	0.00	0.69	0.65***	
			(0.06)	
Any Homelessness Prevention (3 mos)	0.11	0.70	0.55***	
			(0.07)	
Any Payment (3 mos)	0.11	0.65	0.51***	
			(0.08)	
Total Payments (3 mos)	233	2345	1969***	
			(325)	
–Rent (3 mos)	172	1998	1729***	
			(308)	
–Security Deposit (3 mos)	62	320	227*	
			(131)	
–Other (3 mos)	0	27	12	
			(11)	
N	63	71	134	134
Homeless (6 mos)	0.063	0.000	-0.065**	-0.118*
			(0.032)	(0.060)
–Shelter (6 mos)	0.048	0.000	-0.047*	-0.085*
			(0.028)	(0.051)
–Outreach (6 mos)	0.0159	0.0000	-0.0147	-0.0265
			(0.0154)	(0.0279)
–Other Homeless Services (6 mos)	0.032	0.000	-0.033	-0.059
			(0.024)	(0.043)
Homeless Days (6 mos)	5.7	0.0	-5.5	-9.9
			(4.2)	(7.7)
N	63	71	134	134

Notes: We measure all outcome variables cumulatively between random assignment and the listed duration afterward using HMIS data. The sample consists of people who went through random assignment at least 6 months prior to March 1, 2020 but after June 2019 and were not eligible for other prevention services. Columns (1) and (2) show raw means for the control and treatment groups, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Column (4) shows the coefficient on receipt of assistance in an instrumental variables regression where the first stage is the row for "Any HP Payment Made" within 3 months. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table A.8: Housing outcomes, post March-2020

	(1) Control	(2) Treatment	(3) Dif (OLS)	(4) Dif (IV)
D:H Homelessness Prevention (3 mos)	0.02	0.54	0.51***	
			(0.03)	
Any Homelessness Prevention (3 mos)	0.11	0.58	0.45***	
			(0.04)	
Any Payment (3 mos)	0.09	0.52	0.40***	
			(0.04)	
Total Payments (3 mos)	590	2679	1801***	
			(251)	
–Rent (3 mos)	498	2493	1702***	
			(240)	
–Security Deposit (3 mos)	24	89	85**	
			(39)	
–Other (3 mos)	68	97	14	
			(34)	
N	387	298	685	685
Homeless (6 mos)	0.012	0.012	0.004	0.008
			(0.011)	(0.022)
–Shelter (6 mos)	0.0086	0.0082	0.0038	0.0080
			(0.0089)	(0.0184)
–Outreach (6 mos)	0.0029	0.0000	-0.0041	-0.0085
			(0.0042)	(0.0088)
–Other Homeless Services (6 mos)	0.0000	0.0041	0.0065	0.0135
			(0.0066)	(0.0136)
Homeless Days (6 mos)	0.58	0.78	0.75	1.55
			(0.83)	(1.73)
N	347	245	592	592

Notes: We measure all outcome variables cumulatively between random assignment and the listed duration afterward using HMIS data. The sample consists of people who went through random assignment between March and December of 2020 and were not eligible for other prevention services. We limit the follow-up period to 6 months because the outcome data only run through mid-2021. Columns (1) and (2) show raw means for the control and treatment groups, respectively. Column (3) shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and agency-month fixed effects. Column (4) shows the coefficient on receipt of assistance in an instrumental variables regression where the first stage is the row for "Any HP Payment Made" within 3 months. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table A.9: Panel data effects on the flow probability of homelessness, before and after March-2020

	(1) Homeless	(2) Homeless	(3) Homeless	(4) Homeless
Assigned to Treatment	-0.015***	-0.017***	-0.012**	-0.014**
	(0.0053)	(0.0060)	(0.0055)	(0.0066)
Randomized After March 2020	-0.014***	-0.017***		
	(0.0053)	(0.0060)		
Randomized After X Treatment	0.016***	0.020***		
	(0.0062)	(0.0070)		
Observed After March 2020			-0.0045	0.0043
			(0.0056)	(0.0049)
Observed After X Treatment			0.0082	0.012*
			(0.0062)	(0.0072)
Randomization Strata FE	No	Yes	No	Yes
Pre-March Control Mean	0.017	0.017	0.013	0.013
R^2	0.0053	0.022	0.0018	0.020
N	7452	7452	7452	7452

Notes: The sample consists of a balanced panel of people who went through random assignment during the entire duration of the experiment. Each observation is a person-month, covering months 1 to 6 after random assignment. We measure all outcome variables as flow indicators as having a non-prevention HMIS enrollment in the given month. Each column shows a separate regression estimated by OLS. 'Randomized After' is an indicator that mimics or main sample split, dividing people who enrolled in the study before vs. after March 2020. 'Observed After' indicates whether the particular person-month outcome was realized before or after March 2020. Strata are indicators for agency-months. Standard errors clustered by person are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table A.10: Homelessness within 6 months, by sub-group

	Control Has Char	Treatment Has Char	Control Not	Treatment Not	Adj. RF Dif-in-Dif
Above Median Score	0.037	0.000	0.044	0.015	-0.008
					(0.029)
Above Median Age	0.051	0.000	0.032	0.017	-0.033
					(0.030)
No Children	0.077	0.000	0.015	0.017	-0.090***
					(0.032)
Male	0.048	0.017	0.039	0.006	-0.017
					(0.035)
Hispanic	0.026	0.000	0.067	0.027	0.016
					(0.034)
Non-Hispanic Black	0.049	0.067	0.040	0.000	0.065
					(0.063)
Non-Hispanic White	0.088	0.000	0.033	0.010	-0.095
					(0.061)
Any Assets	0.051	0.000	0.032	0.019	-0.036
					(0.028)
Trouble with English	0.010	0.000	0.053	0.013	0.046**
					(0.020)
Housing overcrowded	0.019	0.023	0.045	0.006	0.065***
					(0.024)
Homeless, past 3 years	0.090	0.000	0.023	0.012	-0.077**
					(0.037)
Owe money	0.038	0.010	0.105	0.000	0.137
					(0.095)
Poor credit	0.048	0.014	0.030	0.000	-0.025
					(0.024)
Disabled	0.058	0.000	0.038	0.012	-0.007
					(0.033)
Chronic health condition	0.048	0.021	0.040	0.006	0.038
					(0.039)
Mental health condition	0.051	0.000	0.040	0.010	-0.039
					(0.069)
Violence threat, 6 mos	0.094	0.000	0.030	0.011	-0.066
					(0.047)
Any HMIS, 12 mos	0.156	0.050	0.030	0.005	-0.215**
					(0.093)
Above Median Predicted Risk	0.064	0.020	0.017	0.000	-0.022
					(0.031)

Notes: Each cell shows the proportion of people enrolling in non-prevention homelessness programs within 6 months of random assignment. Except for the top row, the first four columns shows this proportion for sub-groups based on random treatment assignment and the listed baseline characteristic. The final column shows the coefficient on the interaction of treatment assignment and the baseline characteristic in a regression of enrollment of non-prevention homelessness programs on treatment assignment, the baseline characteristic, their interaction, and all interactions of agency-month with the baseline characteristic. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively. The sample consists of people who went through random assignment between July 2019 and December 2020, were not eligible for other prevention services, and arrived prior to March 1, 2020.

Table A.11: Treatment effects on homelessness within 6 months, extrapolating beyond the experiment

	(1) RCT	(2) RCT	(3) Not RCT	(4) All
Assigned to Treatment	-0.038***	-0.032***	-0.019	-0.021**
	(0.014)	(0.012)	(0.013)	(0.0092)
Risk Score	0.00046	-0.0016	0.0050***	0.0048***
	(0.0046)	(0.0041)	(0.0014)	(0.0011)
Randomization Strata FE	Yes	No	No	No
Control Mean	0.041	0.041	0.029	0.032
Treatment Mean	0.0093	0.0093	0.046	0.038
R^2	0.055	0.0087	0.0095	0.0078
N	578	578	1791	2369

Notes: Each column displays results from a regression estimated by OLS where the outcome is homelessness within 6 months. The sample varies across columns. Columns (1) and (2) use the main study sample. Column (3) displays results for all people who are not in the main study sample but are assessed for prevention for the first time between July 1, 2019 and March 1, 2020. These people have risk scores that are too high or low, arrive too late in the month, or are judged by case workers to be likely to receive assistance from other sources. Column (4) combines the samples of the other columns. Each regression includes a control for the program's assessment score and an indicator for being assigned to treatment. Within the main study sample, assignment to treatment is identical to random assignment so that column (1) replicates the main study results with an added control for risk score. Outside the main RCT sample, we code treatment as follows. In accordance with program rules, households with risk scores below 8 are considered as assigned to control, and those above 13 are considered assigned to treatment. Households scoring between 8 and 13 who do not complete study intake (e.g. because they arrived too late in the month to get assistance) are considered assigned to control. Households who score between 8 and 13 and complete study intake but are excluded from the main sample (e.g. because the case worker expects them to receive assistance elsewhere) are considered assigned to treatment. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.