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

David C. Phillips, and James X. Sullivan [*]

June 2022

Abstract

This paper provides the first evidence from a randomized controlled trial of the impact of financial assistance to prevent homelessness. In this study individuals and families at imminent risk of homelessness were offered one-time 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.

JEL Classification: I38, H75, R21, R28

Keywords: homelessness prevention, emergency financial assistance

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 and Reed, 2018; Humphries et al., 2019).

To address the costly nature of becoming homeless, policymakers have supported homelessness prevention programs. These programs make one-time payments for unpaid rent to 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 such assistance across CARES, the Consolidated Appropriations Act, and ARPA, including more than $46 billion for direct rent and utility assistance.

Whether homelessness prevention programs are effective is controversial, even among organizations dedicated to addressing homelessness. 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. Early in the pandemic, the National Alliance to End Homelessness urged local agencies to focus on programs for people who were already homeless because, "...homelessness prevention programs are notably ineffective at preventing homelessness because of the difficulty of predicting who will actually become homeless" (NAEH, 2020). Recent quasi-experimental evidence indicates that financial assistance substantially reduces homelessness, but to-date there is no direct experimental evidence of the impact of such cash assistance.

In this paper, we test whether providing emergency 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 pays an average of two months of rent to landlords of tenants who are still housed, behind on rent, and at imminent risk of being removed. We focus on marginal tenants who barely meet the program's criteria for vulnerability and who cannot demonstrate an ability to pay rent after the temporary payment ends. 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 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 much less precise 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 while leaving milder forms unchanged.

This paper provides the first evidence from a randomized controlled trial of the impact of emergency financial assistance on homelessness. Our results complement those from related studies.

A prior RCT shows similar effects for a comprehensive package of financial assistance, customized case management, and other services (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 marginal clients 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.

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 emergency financial assistance

A large literature has shown that households with low levels of wealth do not have sufficient access to alternative resources or credit to fully insure against income shocks. Consequently, consumption varies with these shocks. For example, Gruber (1997) shows that consumption declines during

unemployment, and unemployment insurance only partially eliminates this change. More recently, Ganong et al. (2020) show that workers' consumption varies when their firm makes company-wide changes in pay and that the consumption response is stronger for Black households, Hispanic households, and those with low levels of wealth. Thus, temporary interventions at crisis moments have the potential to help households manage risk in the presence of credit constraints.

Our paper considers the impact of insurance in the form of financial assistance for those at risk of becoming homeless. Despite concerns about being able to target the marginal person through such an intervention, recent research indicates that homelessness prevention efforts can reduce homelessness. The HomeBase Community Prevention program provides a combination of emergency financial assistance and customized case management. A randomized controlled trial showed that this program reduced shelter entry (Rolston et al., 2013). Most similar to the present study, a quasi-experimental study in Chicago showed that financial assistance alone reduced shelter entry by 76% (Evans et al., 2016).

The current literature leaves three important gaps. First, no randomized controlled trial of emergency financial assistance to prevent homelessness exists. Although there is strong quasi-experimental evidence, random assignment relaxes assumptions about the relationship between unobserved characteristics and referral to financial assistance. Second, rigorous evaluations of prevention have considered relatively few contexts, leaving questions about the extent to which existing evidence applies to other situations. Third, little is known about which groups of people benefit most from homelessness prevention efforts and thus should be targeted. High risk households may benefit the most from one-time financial assistance, because they are perhaps the group most likely to become homeless in the absence of such assistance. On the other hand, they may not benefit at all if the assistance only delays the inevitable and is insufficient to address the underlying causes of a dire situation. In this study, we address these questions, examining financial assistance provided by random drawing to a group of people who are housed and at risk of homelessness but face a more lasting loss of income than those examined in previous studies.

2.2 Homelessness prevention in Santa Clara County, CA

Communities all across the country offer homelessness prevention programs that provide assistance to households who are currently housed but at risk of homelessness (typically due to potential eviction) because of an income shock. The most common forms of assistance are one-time financial assistance, legal representation, and case management. All of these interventions attempt to stabilize housing in a lasting way with only a short term intervention.

Preventing homelessness is a critical policy issues in Santa Clara County (SCC), where fully 45% of households pay more than 30% of their income for rent. According to HUD's 2019 Point-in-Time counts, it has the 4th largest homeless population in the country, behind only New York, Los Angeles, and King County, WA (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.

SCC and a network of non-profit organizations collaborate to provide emergency financial assistance to address this issue of homelessness. Destination:Home is a non-profit organization that operates the county's Continuum of Care for homelessness programs. 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 program and the provision of assistance to several non-profit organizations. Each agency covers part of the geography of Santa Clara County.[1]

This study examines the impact of Destination:Home's homelessness prevention program, which began in 2017. The program pays one-time financial assistance to the tenant's landlord. In fiscal year 2019-2020, the average assistance amount was $4,442, which is approximately two months of rent in SCC. 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, so we focus on the financial assistance component of the

---

[1]Sacred Heart Community Service and Salvation Army serve San Jose, where the bulk of homelessness prevention clients live. Smaller groups 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.

intervention.

This new program complements other existing support for homelessness prevention. Most prominently, the SCC Office of Supportive Housing funds its own homelessness prevention program and implements it through the same network of non-profit organizations. Several smaller private organizations also operate similar programs. The administrative data we use for our analyses allows us to account for the fact that a small fraction of the control group will access financial assistance through these other 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 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 instead of on whether the client can sustain rent payments after the assistance ends, unlike many homelessness prevention programs, such as the one studied in Evans et al. (2016) and others in SCC. As a result, those eligible for the program we study are unlikely to be eligible for other programs in SCC that provide emergency financial assistance, which allows us to identify an appropriate comparison group. 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.[2] A case worker completes the program eligibility screen. 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 all eligible for financial assistance, while

---

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

those scoring below 8 are all ineligible. For those in the middle range, a case worker helps the client complete a brief study intake process that includes informed consent. They complete a very short baseline survey that records the person's HMIS ID (for data linking) and if the case worker expects that, if denied by the lottery, the client will be eligible for other similar services. We exclude from our main sample people who are eligible for similar assistance through other programs. Then, 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. 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.[3]

We estimate the intention-to-treat (ITT) effects by regressing our outcomes on an indicator for whether a person is assigned to treatment based on the lottery as well as agency-month fixed effects. These ITT estimates capture the regression-adjusted difference in outcomes between those who are assigned to treatment and those who are assigned to control. We use the following regression:

$$Y_{iam} = \alpha_0 + \beta_0 Z_{iam} + \psi_{am}^0 + \epsilon_{iam}^0, \tag{1}$$

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_{am}^0 accounts for the fact that the probability of treatment varies over time and across agencies. ϵ_{iam}^0 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.

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:

---

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

$$Y_{iam} = \alpha_1 + \beta_1 T_{iam} + \psi_{am}^1 + \epsilon_{iam}^1, \tag{2}$$

T_{iam} is an indicator for receiving emergency financial assistance. We are interested in an estimate of the local average treatment effect of receiving emergency 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 emergency 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.[4] We also use the program entry and exit dates to calculate days homeless since random assignment. Because our primary outcome 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. The appendix provides more information on how we define the sample 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

---

[4]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).

used in the risk scoring process indicate that they arrive in vulnerable situations: 19% have been homeless recently; they average 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.[5] 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 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 prior to March 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 full sample of participants who enroll prior to the pandemic.[6] Columns (6) and (7) show mean characteristics for those assigned to treatment versus control.

---

[5]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 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.

[6]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.

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. Also, we show below that treatment effects change little when controlling for baseline covariates.

4 Results

4.1 Receipt of financial assistance

While random assignment determines whether a study participant is initially offered one particular source of financial assistance, there is non-compliance because not all members of the treatment group will receive assistance, and some members of the control group may receive assistance from other sources. 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 (usually 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.

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. 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. With an F-stat of 206 on treatment, the first stage is strong, and traditional inference will be similar to joint t-F testing (Lee et al., 2021). Also, Appendix Table A.4 shows that the compliers are similar to the full sample on all baseline characteristics.

Actual payments show a similar contrast. The treatment group is 55 percentage points more likely to receive a payment, and receives an average of $2,253 within 3 months, or $1,898 more than the control group. There is some variation 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, most payments are below $3,000, but a small right tail increases the mean.

4.2   Treatment effects for homelessness

Our treatment effect estimates (bottom panel of Table 2) show that emergency 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% increases to 3.8% 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%. See Figure 1.a for results at various time horizons. Appendix Table A.5 shows similar results if we control for baseline covariates. All of these differences represent the intent-to-treat effect of offering financial assistance.

Because our main outcome measure will miss some of those who lose their housing (for example,

11

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. As we discuss in the appendix, these results are much less precise because of a smaller sample due to the need to match across data systems. The estimated treatment effects are not statistically different from either zero or the effect on homelessness, though these results could also indicate that financial assistance reduces the most extreme forms of housing instability while leaving milder forms unchanged.

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 accounts for the remainder, dropping by 1.3 percentage points. 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,[7] which implies that the treatment primarily affects the incidence of homelessness rather reducing duration among people who become homeless.

Financial assistance has less effect on homelessness after the onset of the pandemic. Figure 1.b shows the same outcomes for people enrolling from March to December 2020.[8] 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 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, including eviction moratoria and financial assistance provided outside the homelessness system, which made traditional homelessness prevention efforts

---

[7]Average spell length is similar across the different program types.
[8]We limit the follow-up period to 6 months since the outcome data only run through mid-2021.

less relevant for stabilizing housing during the early stages of the pandemic.[9]

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.

4.3   Subgroup 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 three shaded blue bars show 90%, 95%, and 99% confidence intervals that do 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.8 for precise quantities and statistical tests.

In all of these cases, 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.

---

[9]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.

5   Conclusion

This study is the first to examine the impact of emergency financial assistance for people at imminent risk of homelessness in the context of a randomized controlled trial. Randomly offering assistance to tenants increases uptake of emergency financial assistance by 56 percentage points. This increased financial assistance reduces homelessness by 3.8 percentage points from a base of 4.1 percentage points, largely due to decreases in emergency shelter entry. Households with no children and a prior history of homelessness have larger effects.

Whether homelessness prevention programs are effective is controversial, even among organizations dedicated to addressing homelessness. The primary concern is that targeting assistance towards those who would otherwise become homeless is difficult, suggesting that funds to prevent homelessness may just crowd out other resources (such as 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). The results from our study, however, suggest the effect of emergency financial assistance on homelessness is large enough to make it a cost-effective option. Emergency financial assistance generates benefits to recipients through income transfers, to landlords who do not have to turnover vacant units, to members of the public affected by violence generated by housing instability (Palmer et al., 2019), and to public finances that spend less treating the effects of homelessness.

We bring estimates of the costs and benefits of emergency financial together in a 'marginal value of public funds' framework (Hendren and Sprung-Keyser, 2020). The details of these estimates are provided in the appendix. Direct program costs of $2,138 per person and indirect public savings of $316 imply a net public cost of $1,822 per person. We weigh this against $1,898 of direct benefits to recipients and $2,605 of benefits to non-recipients. These values imply an MVPF ratio of 2.6 overall. An MVPF greater than 1 can still be obtained if one ignores either private benefits to recipients or public benefits from reducing violence, but not both.

Moreover, there is growing evidence that certain observable characteristics can help identify those at greatest risk of homelessness. Two studies in New York show that housing history, family

composition, and other observable characteristics can predict homelessness among people requesting assistance from prevention programs (Shinn et al., 2013; Greer et al., 2016). A recent study from Los Angeles finds similar insights using more recent machine learning tools to analyze administrative records integrated across many public benefit systems (Von Wachter et al., 2019). In our context, prevention has larger affects for people with a history of homelessness and households without children. Also, we focus on marginal clients. As we show in Table 1, the program's regular clients have almost double the risk of homelessness. If prevention is more cost effective for traditional clients, then the program would be more cost effective if it targeted these clients.

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.

References

211.org (2015), 'Find your local 2-1-1 service'.
  URL: *http://ss211us.org*

Collinson, R. and Reed, D. (2018), 'The effects of evictions on low-income households', *Unpublished Manuscript* pp. 1–82.

Desmond, M. (2016), *Evicted: Poverty and profit in the American city*, Crown.

Downes, H., Phillips, D. C. and Sullivan, J. X. (2022), 'The effect of emergency financial assistance on healthcare use', *Journal of Public Economics* **208**, 104626.

Evans, W. N., Sullivan, J. X. and Wallskog, M. (2016), 'The impact of homelessness prevention programs on homelessness', *Science* **353**(6300), 694–699.

Flaming, D., Toros, H. and Burns, P. (2015), 'Home not found: The cost of homelessness in silicon valley'.

Ganong, P., Jones, D., Noel, P., Greig, F., Farrell, D. and Wheat, C. (2020), 'Wealth, race, and consumption smoothing of typical income shocks', *NBER Working Paper* (w27552).
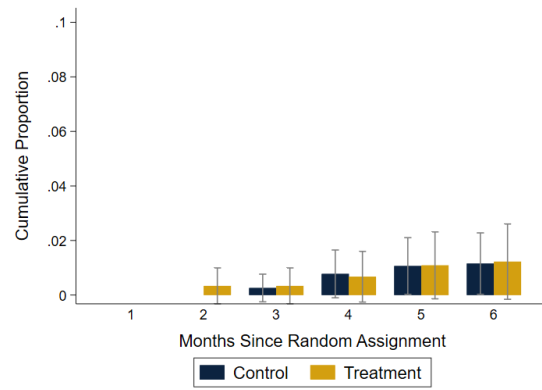
Garboden, P. M. and Rosen, E. (2019), 'Serial filing: How landlords use the threat of eviction', *City & Community* **18**(2), 638–661.

Greer, A. L., Shinn, M., Kwon, J. and Zuiderveen, S. (2016), 'Targeting services to individuals most likely to enter shelter: Evaluating the efficiency of homelessness prevention', *Social Service Review* **90**(1), 130–155.

Gruber, J. (1997), 'The consumption smoothing benefits of unemployment insurance', *The American Economic Review* **87**(1), 192.

Gubits, D., Shinn, M., Wood, M., Brown, S. R., Dastrup, S. R. and Bell, S. H. (2018), 'What interventions work best for families who experience homelessness? impact estimates from the family options study', *Journal of Policy Analysis and Management* **37**(4), 835–866.

Hendren, N. and Sprung-Keyser, B. (2020), 'A unified welfare analysis of government policies', *The Quarterly Journal of Economics* **135**(3), 1209–1318.

Humphries, J. E., Mader, N. S., Tannenbaum, D. I. and Van Dijk, W. L. (2019), Does eviction cause poverty? quasi-experimental evidence from cook county, il, Technical report, National Bureau of Economic Research.

Khadduri, J., Leopold, J., Sokol, B. and Spellman, B. (2010), 'Costs associated with first-time homelessness for families and individuals', *Available at SSRN 1581492* .

Lee, D. S., McCrary, J., Moreira, M. J. and Porter, J. R. (2021), Valid t-ratio inference for iv, Technical report, National Bureau of Economic Research.

Leung, L., Hepburn, P. and Desmond, M. (2021), 'Serial eviction filing: civil courts, property management, and the threat of displacement', *Social Forces* **100**(1), 316–344.

Marbach, M. and Hangartner, D. (2020), 'Profiling compliers and noncompliers for instrumental-variable analysis', *Political Analysis* **28**(3), 435–444.

NAEH (2020), 'Use esg-cv to help those currently experiencing homelessness first'.

Palmer, C., Phillips, D. C. and Sullivan, J. X. (2019), 'Does emergency financial assistance reduce crime?', *Journal of Public Economics* **169**, 34–51.

Phillips, D. C. (2020), 'Measuring housing stability with consumer reference data', *Demography* **57**(4), 1323–1344.

Phillips, D. C. and Sullivan, J. X. (2021), Cash and case management: Evidence from a randomized controlled trial of homelessness prevention, Technical report, Unpublished Working Paper.

Rolston, H., Geyer, J., Locke, G., Metraux, S. and Treglia, D. (2013), 'Evaluation of the homebase community prevention program', *Final Report, Abt Associates Inc, June* **6**, 2013.

Shinn, M., Greer, A. L., Bainbridge, J., Kwon, J. and Zuiderveen, S. (2013), 'Efficient targeting of homelessness prevention services for families', *American journal of public health* **103**(S2), S324–S330.

U.S. Census Bureau (2022), 'American housing survey', `https://www.census.gov/programs-surveys/ahs/data/interactive/ahstablecreator.html?s_areas=00000&s_year=2017&s_tablename=TABLES08&s_bygroup1=1&s_bygroup2=1&s_filtergroup1=3&s_filtergroup2=1`. Accessed: 2022-02-09.

Von Wachter, T., Bertrand, M., Pollack, H., Rountree, J. and Blackwell, B. (2019), 'Predicting and preventing homelessness in los angeles'.
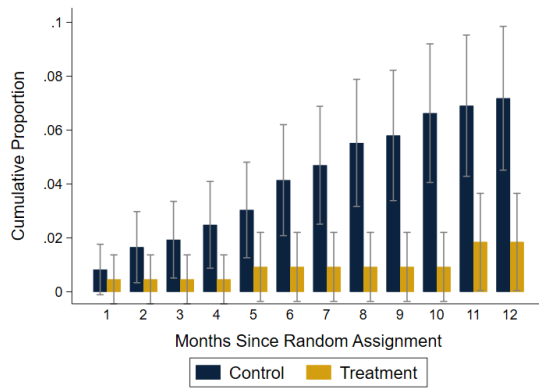
(a) Homeless, Pre-March 2020   (b) Homeless, Post-March 2020

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 people arriving before March 1, 2020. Panel (b) includes 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.
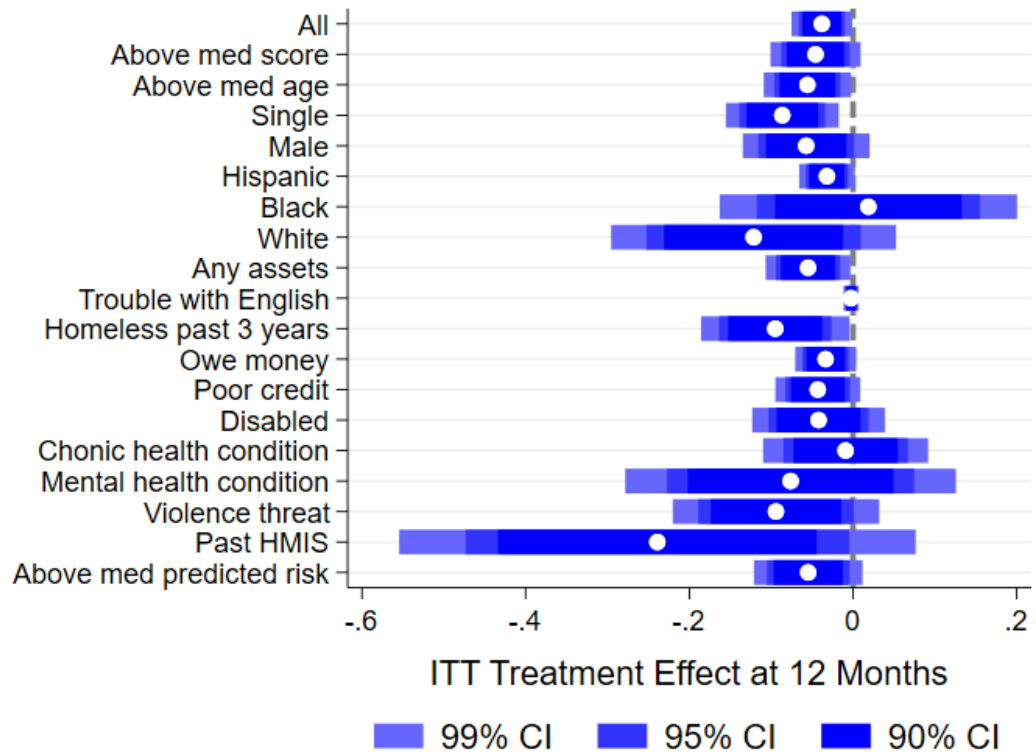
Figure 2: Effects on Homelessness within 6 Months, by Subgroup

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 90, 95, and 99% 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 ≤ S ≤ 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 past year	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
N	6794	799	3039	1263	578	216	362	578

Notes: Variables in the first 16 rows 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 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 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.

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 Payments (3 mos)	216	1832	1551***	
			(181)	
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 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 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. 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 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 there are limited funds available. 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 substitutes in SCC, 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.[10] 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 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.[11] 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 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

---

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

[11]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.

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 (usually 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

We measure our primary outcome 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 D:H 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. 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.[12] 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 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).

---

[12]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.

A.5   Cost Benefit

We estimate a rough cost-benefit for offering emergency financial assistance using a combination of our estimated treatment effects and 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.

The direct costs of offering emergency 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, we would 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 and Reed, 2018), 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, emergency 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 $2,200. 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. Using the same methods as (Palmer et al., 2019) to move from arrests to victim costs, a decrease of 0.025 in shelter episodes within 6 months per person assigned to treatment implies 0.075 fewer assaults valued at $2,386 in benefits to victims.

We bring these estimates together in a 'marginal value of public funds' framework (Hendren and Sprung-Keyser, 2020). Direct program costs of $2,138 per person and indirect public savings of $316 imply a net public cost of $1,822 per person. We weigh this against $1,898 of direct benefits to recipients and $2,605 of benefits to non-recipients. These values imply an MVPF ratio of 2.6 overall. An MVPF greater than 1 can still be obtained if one ignores either private benefits to recipients or public benefits from reducing violence, but not both.
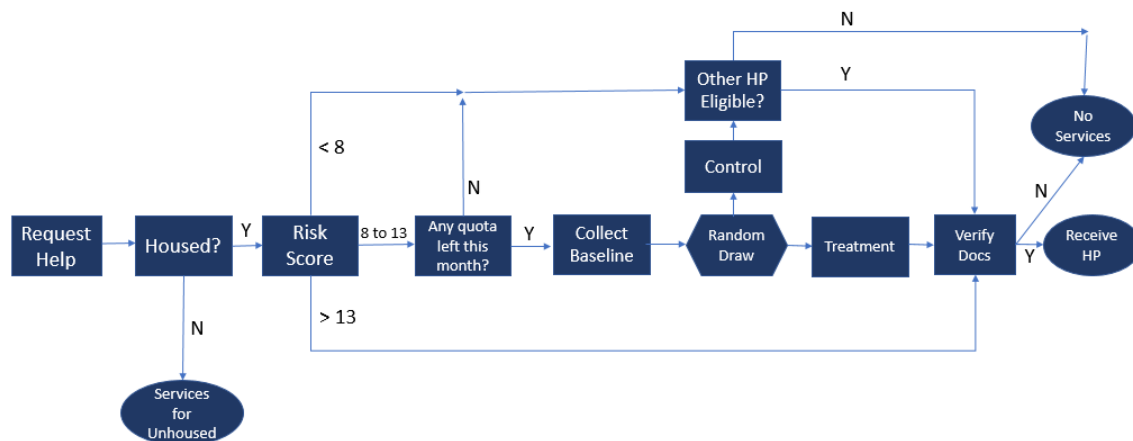
Figure A.1: Experimental Design
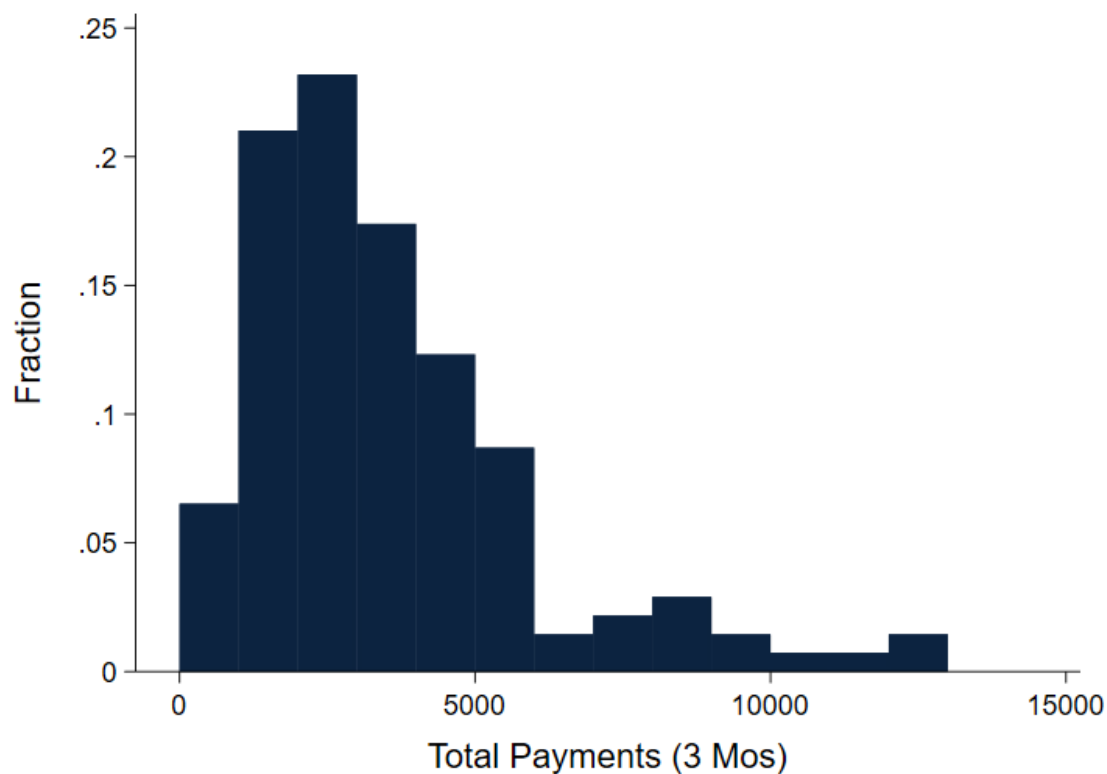
Figure A.2: Distribution of Financial Assistance Received (if positive), Treatment Group Only

Notes: For each person, we calculate the amount of financial assistance received between the time of random assignment and 3 months later. The figure shows the distribution of this variable for people who enroll before March 1, 2020, are not eligible for other assistance, are randomly assigned to treatment, and actually receive assistance within 3 months.
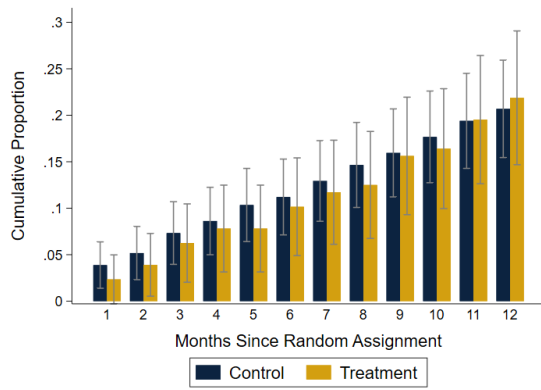
(a) Address Change, Pre-March 2020   (b) Address Change, Post-March 2020
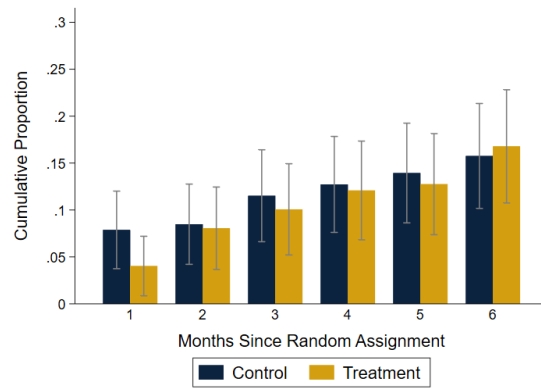
Figure A.3: Housing Outcomes over Time

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. Panel (a) includes people arriving before March 1, 2020. Panel (b) includes 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.

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 past year	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 prior to March 1, 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 13 rows 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 past year	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 after March 1, 2020 and are ineligible for other prevention services. Variables in the first 13 rows 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 past year	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 13 rows 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 past year	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

Table A.5: 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 Payments (3 mos)	216	1832	1559***	
			(179)	
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 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 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. 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.6: 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 Payments (3 mos)	498	2493	1702***	
			(240)	
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 after 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. 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.7: Housing outcomes, all

	(1) Control	(2) Treatment	(3) Dif (OLS)	(4) Dif (IV)
D:H Homelessness Prevention (3 mos)	0.02	0.60	0.58***	
			(0.02)	
Any Homelessness Prevention (3 mos)	0.11	0.62	0.50***	
			(0.03)	
Any Payment (3 mos)	0.10	0.57	0.46***	
			(0.03)	
Total Payments (3 mos)	436	2500	1843***	
			(164)	
Rent Payments (3 mos)	362	2216	1636***	
			(157)	
N	749	514	1263	1263
Homeless (6 mos)	0.027	0.011	-0.016*	-0.031*
			(0.009)	(0.017)
–Shelter (6 mos)	0.017	0.007	-0.010	-0.019
			(0.007)	(0.014)
–Outreach (6 mos)	0.0071	0.0000	-0.0081**	-0.0156**
			(0.0039)	(0.0074)
–Other Homeless Services (6 mos)	0.0056	0.0043	-0.0010	-0.0019
			(0.0049)	(0.0094)
Homeless Days (6 mos)	1.6	0.5	-0.8	-1.6
			(0.7)	(1.4)
N	709	461	1170	1170

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 any time 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: 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)
Single	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)
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 past year	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 at any time, were not eligible for other prevention services, and arrived prior to March 1, 2020.