Personalizing Homelessness Prevention: Evidence from a Randomized Controlled Trial

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Abstract

Through a randomized controlled trial, we test whether providing personalized case management along with emergency financial assistance more effectively prevents homelessness than financial assistance alone. For the study sample of young adults and families with children who are at risk of homelessness, our results indicate that participants assigned to case management and financial assistance are more likely to access other homeless programs and no less likely to be evicted. Downstream outcomes are mostly unchanged, though the probability of being arrested increases. Using non-experimental variation in the nature of the case management provided across staff, we find that case management is associated with better outcomes when it is more intensive and pays financial assistance quickly.

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1 Introduction

About 1.3 million individuals and 300 thousand children enter a homeless shelter each year in the United States; at some point in the year, another 1.1 million students sleep unsheltered, in a motel, or doubled-up with family and friends (Henry et al. 2018). Given the high private and social costs of homelessness, communities all across the country offer homelessness prevention programs to those at risk of losing their housing. The most common type of intervention is emergency financial assistance–payments for rent, typically paid directly to the landlord–for individuals and families at risk of eviction or becoming homeless. In 2015, 93% of communities had a community hotline offering such assistance (211.org 2015), and these programs expanded rapidly in the wake of the COVID-19 pandemic with the federal government appropriating $70 billion for them in three relief bills (CARES Act 2020, Consolidated Appropriations Act 2020, American Rescue Plan Act 2021). Recognizing that many of those on the brink of homelessness face challenges beyond just a shortfall in resources, homelessness prevention programs often provide a variety of other services, including case management that is designed to help the individual or family navigate the many obstacles that put them at risk (Burt et al. 2006).

Cash transfers and personalized case management provide different approaches for promoting housing stability, and which approach is the most cost effective may depend on the nature of the crisis that recipients face. If, for example, individuals are at risk of homelessness because a transitory income shock reduces financial resources, then an intervention that provides financial assistance to address the shortfall may be effective. On the other hand, if those at risk of homelessness face other obstacles to housing stability such as a permanent shock to income, a lack of information about housing opportunities, or the direct effects of an income shock on cognition (Mullainathan & Shafir 2013), then a more comprehensive set of services might be more effective. Previous research has suggested that that comprehensive case management programs offered in addition to financial assistance have been effective in other contexts, such as encouraging degree completion (Evans et al. 2017, Weiss et al. 2019, Brough et al. 2022). However, little is known about whether programs that offer case management services as well as financial assistance are more effective at preventing homelessness than programs that offer only cash.

The need for more comprehensive services is hotly debated in other areas of housing policy. For temporary subsidy programs that require tenants to generate income quickly, case management can range from required weekly meetings to optional meetings more than a month apart (Burt et al. 2016). Led by changes in federal funding, services for chronically homeless individuals have shifted dramatically from transitional housing programs, which make housing contingent on sobriety, employment, and/or participation in other services, toward supportive housing programs, which provide housing unconditionally (Evans et al. 2021). This shift aligns with a large body of evidence showing that housing interventions that prioritize unconditional long-term subsidies reduce homelessness (Tsemberis & Eisenberg 2000, Aubry et al. 2015, Gubits et al. 2018). On the other hand, many housing authorities have added considerable case management to programs that otherwise focus narrowly on housing costs for a broader population. Adding housing mobility supports to the Housing Choice Voucher program, for example, helps tenants lease-up in a much broader range of neighborhoods with greater access to opportunity (DeLuca & Rosenblatt 2017, Bergman et al. 2019).

In this paper, we test whether the combination of case management and financial assistance is more effective at preventing homelessness than financial assistance alone. In partnership with King County, WA and two dozen private social service agencies, we conducted a randomized controlled trial that assigned people to receive either simple financial assistance or a combination of case management and financial assistance. In practice, both groups received about $1,400 of financial assistance on average, though the case management group was less likely to receive any financial assistance at all. Help for the case management group included additional direct housing assistance, such as creating housing plans and communicating with landlords, as well as help generating income and emotional support. We enrolled 631 people across these two study arms and we tracked housing stability measures and downstream outcomes for both groups using administrative data.

We find that assignment to a combination of case management and financial assistance increases use of programs for those who have already become homeless (such as emergency shelter or street outreach). Within one year of random assignment, 3.5 percent of the funds only group enrolls in programs for the homeless, and this rate increases by 4.2 percentage points for the case management plus funds group. One potential explanation for this perhaps counterintuitive result is that our measure of homelessness (use of homelessness programs) could indicate that the group that receives case management plus funds has greater access to homelessness programs if they become homeless. For this reason, we also examine other indicators of housing instability, including eviction court filings and address changes, that are not dependent on use of homeless services. Results for these other measures are not statistically different from zero, though the point estimate for eviction filings is also positive, and its 95% confidence interval can rule out eviction filings decreasing by more than 1.5 percentage points from a 14.2 percentage point base. Overall, we conclude that case management did not, on average, improve housing stability significantly beyond any effect of financial assistance alone.

We also link study participants to downstream outcomes including use of public benefits (SNAP/TANF), arrests, and healthcare use. We observe no consistent evidence that case management raises public benefit use by connecting people to services. Results for healthcare use are mixed, with no changes in headline measures of overall use. However, arrests increase noticeably. Assignment to case management plus financial assistance increases the probability of being arrested within one year by 4.3 percentage points from a 3.7 percentage point base.

To understand these results, we examine how the effects differ across case managers that take different approaches to implementing case management. For example, case managers differ in the intensity and duration of case management services as well as in how quickly they distribute financial assistance. Using this non-experimental variation in treatment approaches across case managers, we find evidence that case management leads to positive outcomes when it is more intensive and worse outcomes when it causes financial assistance to be delayed. On average, the case management group receives more case management, similar average financial assistance, and a lower probability of any payment, particularly within 30 days. But this treatment-control contrast varies widely across case managers, and the variation in funding rates and intensity of service are largely independent. We show that the case management plus financial assistance group has particularly poor housing stability outcomes when assigned to the case managers that frequently delay payment and that this effect is of sufficient magnitude to account for any increased housing stability in the case management plus financial assistance group. At the same time, case managers who devote more hours to the case management plus financial assistance group see increased use of homeless programs but no increase in eviction rates, which suggests they actively connect participants to programming when they become homeless. While these results go beyond pure experimental variation and should be interpreted with caution, we find suggestive evidence that any negative outcomes associated with case management can be accounted for by instances in which case management delays financial payment, while more intensive case management is associated with benign outcomes.

Our results build on a growing literature on the effectiveness of homelessness prevention programs. The early literature focused on the targeting properties of prevention, finding that most people who request assistance but are not treated do not ultimately become homeless (Shinn et al. 2013, Greer et al. 2016), though big data may improve targeting (Von Wachter et al. 2019). Despite the difficulty of targeting homelessness prevention, a randomized controlled trial found that Homebase, an intensive combination of case management services, referrals to other services, and limited financial assistance, reduced the likelihood of shelter entry for participants (Rolston et al. 2013). Financial assistance alone has also been shown to be effective in a quasi-experimental study in Chicago (Evans et al. 2016, Palmer et al.

2019, Downes et al. forthcoming) and a recent RCT in California (Phillips & Sullivan 2022).[1]

We contribute to this literature on homelessness prevention interventions by isolating the effect of case management. We conduct the largest sample randomized controlled trial to date of a homelessness prevention program. Our results suggest that complicated but effective programs like Homebase cannot be decomposed simply into the effects of their constituent parts and added up. Financial assistance and case management interact with each other, and the value of case management will depend on both its design and how it interacts with financial assistance. This interaction suggests one cannot determine the effect of case management plus financial assistance relative to cash alone by comparing results across different studies.

More generally, our results provide some guidance on when personalized anti-poverty interventions will be more effective than cash. Our non-experimental results suggest that more intensive case management interventions can be more effective than light-touch versions. This insight aligns with results from particularly intensive interventions that observe big changes in outcomes (Evans et al. 2017, Weiss et al. 2019, Brough et al. 2022, Evans et al. 2020, Bergman et al. 2019). Our results also suggest that case management can be counter-productive when it unintentionally prevents the provision of financial relief. In a context with limited information and agency problems between case managers and clients, a tradeoff can arise between helping clients manage the complexity of poverty and freeing the client to reach their own goals. This tradeoff bears similarity to results from the job training literature, in which some participants exit benefits to avoid case management (Black et al. 2003) even when others benefit enough to improve outcomes on average (Santillano et al. 2020). Our results suggest that allowing program clients to opt out of case management may avoid some of this tradeoff. Providing more individual choice around when to attach intensive case management to programs that also provide cash-like resources may help minimize the cost and maximize the benefits of personalized, time-intensive interventions.

[1] See Evans et al. (2021) for a more comprehensive review of the evidence on the effectiveness of both prevention programs and programs for people who are already homeless.

2 King County's Youth and Family Homelessness Prevention Initiative (YFHPI)

2.1 King County, WA

King County, Washington is a rapidly-growing community with rising rents and incomes. Centered on Seattle, it is the home of large tech companies like Amazon and Microsoft. Statistics from Decennial Censuses and the 2019 American Community Survey show that King County's population grew by 30% from 2000 to 2020, compared to 18% for the whole United States. Both income and housing costs have risen rapidly with the population, though rent has risen faster. Median household income has grown to $102,594, up 93% since 2000, while median rent has increased by 129% to $1,736 per month.

King County's rapid income growth masks considerable inequality. With rising rents outpacing income, 45% of residents report paying more than 30% of their income in rent. Additionally, King County has a much larger homeless population than other counties its size. HUD's 2020 Point-in-Time count included 11,751 homeless individuals, the 3rd largest total in the United States, behind only Los Angeles and New York, but the 13th largest overall population.

2.2 The YFHPI Program

King County's largest prevention program, the Youth and Family Homelessness Prevention Initiative (YFHPI), spends about $5 million per year to prevent homelessness among households that either have children or are headed by transition-age youth under the age of 25. YFHPI provides services through a network of more than two dozen private, non-profit organizations that are embedded in communities that experience higher risk of homelessness. Rather than focusing on large social service agencies with particular experience in housing, this program focuses on agencies that provide a wide range of services to a particular community and are deeply connected to that community. These communities include groups of immigrants and refugees, communities of color (American Indian and Black), transition-age youth, people experiencing domestic violence, and families including a person with a disability. Each of these agencies then hires one case manager to operate the YFHPI program model. King County provides funding, training, and oversight to ensure consistency of services across the widely varying agencies.

The YFHPI intervention was designed to include a combination of case management and access to flexible funds at the case manager's discretion. The program was modelled on the Washington State Domestic Violence Housing First Program (Mbilinyi 2015). YFHPI began providing services in 2017, about one year prior to the start of our sample.

Case managers can directly provide financial assistance through flexible funds at their discretion. The most common use of funds is paying back rent directly to landlords, but other uses are permitted. Expenses are always paid directly by the case manager rather than given to the client. The county typically caps annual payments at $2,300 per client, though agencies can apply to have the cap waived. Within the given budget, these funds are intentionally flexible. The only restrictions are that the family must be at imminent risk of homelessness and the case manager must determine that the payment will prevent homelessness. Security deposits may be no more than one month's rent, mortgage payments are not allowed, and expenditures on auto repairs and gift cards are limited.

One innovative aspect of the YFHPI program is its focus on case management. Case management was designed based on a model of progressive engagement, meaning that case managers customize the level and extent of case management services to a particular client's situation. Regular case management activities can include housing-specific activities like navigating interactions with landlords and identifying suitable housing as well as broader activities related to budgeting, employment, dependent care, etc., that can support housing stability. Case managers aim to develop a relationship of trust that allows the participant to better navigate a path to housing stability. As described in more detail below, a qualitative team (Fyall & Fowle 2019) observed case management activities consistent with this program design.

The services provided by YFHPI differ from other homelessness programs in the community in a couple of ways. First, unlike other housing programs that provide services to those who are already homeless, YFHPI targets those who are not yet homeless. According to 2020 HUD Housing Inventory Count data, King County has about 16,000 beds in programs for people who were already homeless, mostly in permanent supportive housing and emergency shelters. Second, compared to other programs that focus on preventing homelessness, YFHPI's case management element is unique. Other groups provide legal aid or simple financial assistance, and during the COVID-19 pandemic, financial assistance in particular became more available due to federal funding that supported King County's Eviction Prevention and Rent Assistance Program. But these programs lack the focus on case management of the YFHPI program.

3 Experimental Design

The design of this randomized controlled trial and the analysis of the data were pre-registered (`https://www.socialscienceregistry.org/trials/2854`).

3.1 Study Enrollment and Random Assignment

The study enrollment process includes three elements: program eligibility, study eligibility, and random assignment. Figure 1 summarizes the process.

Study participants must first be eligible for the YFHPI program. YFHPI rules require that participants must be residents of King County, have a dependent minor or a household head below age 25, and be currently housed but at imminent risk of losing housing. Imminent risk is the most difficult criterion to measure. The county trains case managers to focus on households close to losing housing; such households are behind on rent and typically have received notices to pay or vacate. The program also formally screens people using a risk screening tool. Case managers ask potential clients a series of questions about their housing history, income, demographics, and risk factors for homelessness (e.g. criminal justice contact, household composition changes). The tool summarizes these answers as a risk score, ranging from 0 for those with little-to-no risk of homelessness to 61 for those facing an extremely high level of risk. Nine out of ten potential clients score between 15 and 36. Clients scoring above 15 are eligible for the program. In practice, the database records few individuals with risk scores too low to qualify for the program because case managers save time by informally screening low-risk people out prior to taking the assessment.

To participate in the study clients must be eligible for the YFHPI program, score as moderate risk on the program screening tool, and consent to participate. People scoring as low risk (< 15) are ineligible for YFHPI. People scoring as high risk (> 28) were excluded from the study because the implementing organizations had ethical concerns about assigning them to a study arm that did not include case management. Those in the middle range were offered the opportunity to participate in the study. Case managers informed participants about the study, including data collection procedures and the potential to be randomly assigned to options with or without active case management. Then, clients chose whether to opt into or out of the study. Those who opt out of the study received access to both case management and financial assistance. Clients vary in whether they prefer immediate financial assistance or a combination of case management and flexible funds. [2] The active choice of clients means that we enroll a subset of clients who are at moderate risk and have some interest in straight financial assistance.

We then assign households to receive either a combination of case management and financial assistance or a program of simple financial assistance. The combined program reflects the standard YFHPI program described above, as it had been already operating. The funds only intervention operates through the same case managers but is simpler. Participants

[2] In a case review prior to this study, case managers stated that 59% of their clients would prefer to receive just financial assistance but the staff thought that only 20% could manage without case management.

assigned to this group are asked to document their need but are otherwise guaranteed immediate financial assistance. If the need is not verified, they will not receive funds. Funding can be used to cover all of the same expenses as in the standard YFHPI intervention, typically back rent. Case managers provide little case management beyond that required to make a payment.

Assignment to groups happens in the computer database used to enroll clients. After documenting informed consent, the computer system assigns people randomly and independently to the two groups in equal proportion. We observe the original random assignment. The system also asks the case manager to record the client's treatment status. Assigned and actual treatment status match in 93% of cases, so we conduct all analysis as intent-to-treat comparisons based on the original random treatment assignment.[3]

3.2 Data

Baseline data originates in the program databse for the YFHPI program. The database includes the risk scoring assessments for both study participants and the broader set of people who apply for and receive services from YFHPI. We use these risks assessments to determine the study sample and measure baseline characteristics, such as housing history. Program enrollment records also include demographic information. Together, these records provide control variables in our main specification and allow us to test for baseline balance.

We also measure treatment using the YFHPI program database. The database records the original random treatment assignment, the treatment group recorded by the case manager, and records of actual services provided. The County requires that case managers log both financial assistance and case management hours. Since these records assign transactions and hours to particular clients, we can observe actual services received. For financial assistance, the recorded amounts must line up with audits, which makes them precise records of

[3]Of the other 7% of cases, 2% result from clients asking to withdraw from the financial assistance arm of the study. The other 5% result from an immediate difference between the result of random assignment and the program enrollment recorded by the case manager. The latter indicates either a data entry error or non-compliance with random assignment by the case manager, which are identical in the data.

assistance paid. Case management hours are more difficult to verify, and conversations with case managers suggest that they under-report case management hours due to the burden of record-keeping. Thus, we interpret the magnitude of financial assistance literally but interpret measured case management hours as an ordinal measure of case management intensity rather than a precise measure of hours of case management.

We measure outcomes for household heads by connecting program data to other administrative records. The program records the household head's name, date of birth, ZIP code, and social security number.[4] We use these identifiers to match to the household head's records in other databases for at least one year before and after study enrollment.

We measure housing stability outcomes in three different ways. We measure homeless program use through Homeless Management Information System (HMIS) data maintained by the county. HMIS provides a comprehensive database covering enrollment in various public and private services for homeless individuals, and such data is a typical primary outcome for studies of homelessness prevention (Rolston et al. 2013, Evans et al. 2016). We supplement homeless program use with information on evictions and address changes. We access eviction filing and judgment information from housing court records compiled by American Information Research Services and the Housing Justice Project. We measure address changes using data from Infutor Data Solutions. They construct address histories from various sources of consumer data (e.g. cell phone bills), and prior work shows that it can be used to measure housing moves for unstably housed people (Diamond et al. 2019, Phillips 2020).

For outcomes beyond housing stability, we rely on administrative records from the State of Washington. The Research and Data Analysis (RDA) group at the Washington Department of Social and Health Services has linked several state administrative datasets. We connect to this dataset to measure various outcomes. These outcomes include use of public benefits such as SNAP and TANF, arrests aggregated by the Washington State Patrol, and healthcare use

[4] While the program also records some identifiers for dependents, we were unable to gain access to records on child protection or education outcomes for children.

from Medicaid claims. See the data appendix for more details on the administrative data we use, matching procedures, and definitions of outcomes.

3.3 Study Sample

We enrolled 631 participants in the study between May 2018 and March 2020. Of these, 321 were assigned to funds plus case management and 310 to funds only. As noted above, everyone in this sample must be eligible for YFHPI, at moderate risk of homelessness, and consent to participate in the study. In practice, we define the sample using program records as discussed in more detail in the data appendix.

Table 1 describes baseline characteristics of participants in the homelessness prevention program. The first two columns describe the study participants, with the funds only group in the first column and the funds plus case management group in the second column. The first column shows that study participants have been and continue to be at high risk of homelessness: 55% expect to lose housing within 10 days, 60% have been homeless before, and 39% have been evicted before. In contrast to chronically homeless people and similar to participants in other homelessness prevention programs, most participants are women with children. People of color are over-represented with 56% of participants identifying as Black or African American, compared to 7% of all King County residents. Lagged outcome measures also show extensive interactions with services. In the past year, 9% have used homeless programs, 74% have received cash or food benefits, and 49% have used healthcare. Because of random assignment, these characteristics are similar across the two groups; the final column formally tests for baseline balance and shows that the two groups do not differ statistically, including for lagged outcome measures.

Table 1 also reports average characteristics of YFHPI participants who are not in the study. The third column shows middle-risk YFHPI participants who were eligible for the study but did not participate. They appear similar to study participants. The fourth column displays baseline characteristics for people who are excluded from the study because of high risk scores. This group differs noticeably from the study group. Nearly all expect imminent housing loss (84%) and have been homeless (88%) and evicted (71%) before. Some of these differences are mechanical because the baseline characteristics are inputs into the risk score, but high risk YFHPI participants are also somewhat more likely to have used homeless programs (13%) in the past year, which is not a risk score input. Overall, baseline characteristics suggest that people who consent to random assignment and outcome tracking are not selected on observable characteristics, but people who screen as high risk differ in some observable ways.

3.4 Quantitative and Qualitative Evidence of Differences in Services by Treatment Status

To compare services received by the two groups, we rely on both qualitative and quantitative observations. For qualitative data, we simply summarize results reported by Fyall & Fowle (2019). They conducted 13 hour-long interviews with case managers and spent 25 hours directly observing them working. For quantitative data, we analyze logs of financial transactions and case management hours recorded by case managers.

3.4.1 Qualitative Assessment of Services Received

The qualitative study observed several main categories of case management activities provided to the case management plus financial assistance group. Case managers support housing stability directly by communicating with landlords, searching for new housing, and helping clients develop housing plans and budgets. They help clients generate income via both active job search assistance and help accessing public benefits. They provide emotional support via counseling and motivational interviewing. Case managers support these different case management activities by pro-actively contacting clients regularly and meeting with them in-person both in the office and out in the community. When clients have needs beyond the ability of the case manager, they regularly connect clients to other services with referrals. Overall, the case management provided was holistic, including activities both directly and indirectly related to housing, and characterized by repeated interaction over a period of months.

The two groups receive noticeably different case management services, on average. The funds only group tends to have only one interaction with the case manager focused on collecting documents needed to pay the landlord. One case manager stated:

> "I have a form for what kind of documentation I need... I give [the client] the whole list... I tell them as long as I have all this I can write a check..."

After payment, clients typically exit the program. Case managers contrast this quick transaction with the case management plus financial assistance group, to whom they provide all of the services described above. Of that group, one case manager stated:

> "We are able to be much more supportive. We are able to spend more time with them, and we can work with them for longer periods of time."

Case managers tend to describe the services provided to that group with phrases like 'long-term case management.'

Though the two groups differ overall, some crossover happens between the groups. Within the case management plus financial assistance group, clients vary in their interest level. Some case managers report clients who disengage with case management after receiving financial assistance. Within the funds only group, 12 clients withdrew from the treatment arm after random assignment and subsequently received case management.[5] More generally, case managers disagree on whether providing referrals consists of case management and several case managers also provide referrals (but not other case management) to participants in the funds only group. Finally, while most case managers followed random assignment, 2-3 of the 13 case managers in the qualitative study did not make any distinction between the two groups.

[5] We still include these individuals in the intent-to-treat analysis according to their original random assignment.

While both groups have access to the same financial assistance, the case management model also affects the process and timeline of payment for some people. Both groups face similar program rules, e.g. limits on types and amounts of payments, but the process differs somewhat. As noted above, case managers focus primarily on payment for the funds only group. Clients in that group expect payment to be made quickly, and case managers typically deliver within one or two weeks. Case managers describe the funds only intervention with phrases like 'a quick turnaround' and 'entitled to the money.' On the other hand, case managers exercised more discretion for provision of financial assistance to the case management group. Fyall & Fowle (2019) note that financial assistance for that group could take many weeks to disburse, and case managers 'sometimes delayed payment to see if case management might solve the client's financial problems or if other sources of financial support could be obtained.' On the other hand, the longer-term case management could lead to more service provision. As one case manager stated,

> 'I don't mind holding onto [the case management group]. . . I don't want to exit someone too early unless they can't get services anymore. I'd rather provide as much services as possible.

Since the broader program allowed a person to appeal for repeat payments or to exceed typical program spending limits, the presence of a long-term advocate could lead to more financial assistance for some clients.

3.4.2 Quantitative Assessment of Services Received

We can summarize differences in services across the two intervention groups using quantitative data on financial assistance paid and case management hours logged.

The two groups receive similar financial assistance on average, but the case management groups is less likely to receive any payment at all. Figure 2.a plots the distribution of financial assistance received by treatment assignment. The case management group experiences a more dispersed distribution, being more likely to receive both no payment at all and payments above the program's typical $2,300 limit that require special permission from the county. Table 2 quantifies these differences more precisely. The first two columns show mean services received by the funds only group versus the case management group. The final column shows the difference in those means, adjusted for baseline covariates. The case management group is 9 percentage points less likely to receive financial assistance within 30 days and 6 percentage points within 1 year. However, by 1 year from random assignment, mean financial assistance is similar across the two groups, $1,444 for the funds only group and $1,425 for the case management group. Overall, these facts are consistent with expectations from program design and the qualitative work. Case managers process financial assistance according to simple rules for the funds only group but with discretion for the case management plus financial assistance group, making payment rates differ. On the other hand, long-term case management relationships may allow clients in the case management plus financial assistance group to access larger amounts when special circumstances arise.

Recorded case management hours differ considerably across the groups. Case managers are required to record their hours working with each client; however, the qualitative study found strong evidence that these records are incomplete. We use them as an ordinal measure of case management intensity, and the reported difference in the number of hours almost certainly under counts the actual difference.[6] Figure 2.b shows that the distribution of recorded service time shifts to the right for the case management group. Table 2 shows that case managers record roughly double the number of contact hours with the case management plus financial assistance group, 15.7 hours compared to 8.2 hours. The funds only group typically only interacts with the case manager once to complete the program enrollment process and manage payment to the landlord. Matching this expectation, case management hours are similar for the two groups on the day the person enrolls in the program with all differences occurring afterward. While difficult to compare directly, the case management

[6] How much these records undercount case management is difficult to tell, but we observe roughly double the recorded hours at a subset of 4 agencies that King County reported were more likely to have complete records.

services provided by YFPHI appear roughly similar to other successful prevention programs. Rolston et al. (2013) do not report measures of case management intensity in their evaluation of Homebase. However, financial reports imply that Homebase spends about $2,300 per client on non-financial support, which is similar to what we estimate for YFHPI, about $3,900.[7]

Finally, as expected, case managers retain clients in the case management plus financial assistance group longer. Table 2 shows that the case management plus financial assistance group typically exits contact with the case manager 1-2 months later than the funds only group. When exiting, the case manager records their housing status. People in the case management group are less likely to exit when the case manager knows they are housed and more likely to exit to an unknown situation. At least some of this contrast is mechanical. Case managers close the case for the funds only group after paying back rent, when it is known that these clients are housed. Still, we do observe many clients in the case management plus financial assistance group concluding case management without a resolution to their housing situation; half exit to a situation that is either unknown or unhoused.

3.5 Regression Specification

Because we assign treatment randomly and independently, we can compute treatment effects as simple intent-to-treat comparisons of means. In practice, we also control for pre-specified control variables to potentially improve precision by removing residual variance. For our main specification, we estimate:

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

Y_i is an outcome, such as an indicator for using homeless programs within 1 year of

[7] From 2005-2007, Homebase contracted out $24.3 million in services, served 8,294 clients, and paid $1,150 in financial assistance per client (Cheney 2008). These values imply a cost per client of $1,780 in non-financial assistance, or $2,310 in 2020 dollars. That value is also similar to what Rolston et al. (2013) report, which is $1,896, or $2,221 in 2020 dollars. YFHPI enrolled 1,519 clients during our 23-month study period, 310 of whom did not receive case management (Table 1). Netting financial costs of $1,435 per client (Table 2) from an annual budget of $3.6 million yields a cost of $3,867 for non-financial services per case management client.

random assignment; T_i is an intent-to-treat indicator of treatment group assignment; X_i is a vector of controls;[8] and ϵ_i is a random error term. We estimate this specification using OLS and report heteroskedasticity-robust standard errors with no clustering.

4 Main Results

4.1 Housing Stability

4.1.1 Main Effects

Our primary outcome is an indicator for whether an individual uses homelessness services recorded in the HMIS system. We restrict these services to those that are only available for individuals who are already homeless (emergency shelter, street outreach, coordinated entry, or longer-term subsidy). This measure was designated as our primary outcome in our pre-analysis plan as a proxy for becoming homeless. Similar outcomes have been used to measure homelessness in other studies (Rolston et al. 2013, Evans et al. 2016, Phillips & Sullivan 2022).

Our results for this primary outcome indicate that the case management plus financial assistance group enrolls in homeless programs at a significantly higher rate than the funds only group. Figure 3.a shows the proportion of each group that enrolls in homeless programs by months since random assignment. A large gap opens between the groups in the first 6 months months and then persists out to 12 months after random assignment. The top row of Table 3 quantifies this difference. As shown in the first two columns, 3.5% of the funds only group has accessed homeless programming within one year compared to 7.8% of the case management group. The final column measures this difference, adjusted for pre-specified

[8]The vector of controls includes the outcome in the year prior to study enrollment, whether housing loss is expected within 10 days, whether the household has been homeless before, number of children, age, age squared and indicators for female, Black, Hispanic/Latino, having a family member with a disability, and month of study enrollment. These controls were pre-specified in our pre-analysis plan. We also pre-specified household size as a control variable, but we omit this variable because the underlying database does not record it.

baseline covariates; the difference of 4.2 percentage points is statistically significant at the 5% level. The remaining rows of the top pane of Table 3 show the component programs that compose our main measure. While particular components are too rare to have much statistical power, most of the effect we observe appears to be driven by contact with street outreach, coordinated entry (diversion), and longer-term subsidies (permanent supportive housing, rapid re-housing, and transitional housing) rather than emergency shelter.

One potential explanation for this result is that our measure of homelessness (use of homelessness programs) could indicate that the group that receives case management plus financial assistance has greater access to homelessness programs if they become homeless. Case manager referrals might particularly lead to increases in generous long-term subsidies, though the increased contact with street outreach is more difficult to account for by direct referrals. For this reason, we also examine other indicators of housing instability that are not dependent on use of homeless services including eviction court filings and address changes.

The evidence on evictions and address changes is less definitive but rule out large improvements in housing stability due to case management. As shown in Figure 3.b, eviction filings are also somewhat more common for the case management plus financial assistance group. However, as shown in Table 3, this difference is not statistically different: 14% of the funds only group has an eviction filing within 12 months, compared to 17% of the case management plus financial assistance group. The regression-adjusted difference of 4.1 percentage points is similar to the point estimate for homeless program use but not statistically different from zero because the base rate and standard error for eviction filings are greater. However, the 95% confidence interval for eviction filings runs from -1.5 to +9.6 percentage points, meaning that we can rule out large reductions in eviction filings. Point estimates for different aspects of eviction judgments are similarly positive and not statistically different from zero. For address changes, we have a smaller sample due to low match rates between consumer reference data and the underlying data from the experiment. Results for address changes in Figure 3.c and the final panel of Table 3 have negative point estimates but also very wide confidence intervals. Overall, the evidence on evictions and address changes is less definitive than that for homeless program use. However, we can conclude that case management does not lead to large reductions in evictions, and we find little evidence that being assigned to case management plus financial assistance improves housing stability, on average, as compared to having access to only financial assistance.

4.1.2 Subgroup Effects

We have limited power to detect heterogeneity in treatment effects. However, the YFHPI program includes a wide variety of clients with differing reasons for homelessness. In Appendix Tables 6 and 7 we report sub-group results for homeless program use and eviction filing. Overall, we find little statistical evidence of heterogeneity, though there is some evidence that our main results for homeless program use are driven by participants who do not identify as Hispanic/Latino.

4.1.3 External Validity

This study focuses on a subset of homelessness prevention clients. People must score as moderate risk for homelessness and opt into both data collection and a chance at a program with no case management component. As discussed above, the study sample appears to be somewhat lower risk than clients with high scores who are excluded from the study. So, we investigate the extent to which our main results may extrapolate to all YFHPI clients. This exercise is limited in that we do not observe high risk and non-consenting clients receive a funds-only intervention, and we cannot link them to data outside King County, due to a lack of informed consent. However, we can analyze some de-identified data on homeless program use for these clients.

We observe two facts that are consistent with strong external validity of our main results. First, the base rate of homeless program use in our study sample is actually higher than other studies, alleviating worries that we study a low-risk group. For example, in both Evans et al.

(2016) and Phillips & Sullivan (2022), 0.5% of those assigned to financial assistance enter emergency shelter within 6 months. In our study sample, the equivalent value is 1.6% at 6 months (and 2.6% at 12 months).

Second, outcomes for treated individuals appear similar for people in versus out of the study. Figure 4 displays homeless program use rates at 12 months for the two study groups and people not in the study, split out by risk score. Within the study, treatment effects do not appear to vary much with risk score. Beyond the study sample, middle-score people who opt out of the study access homeless programs only slightly more often than the study case management group, 9.5% of the time time versus 7.8%. This result contrasts with what would be expected if people opt out of the study because they expect good outcomes with case management, we might expect the set of middle-score people who opt out of the study and receive case management and financial assistance to do noticeably better than the study participants that are assigned to the case management and financial assistance group, some of whom may have selected into the study desiring to only get financial assistance. Similarly, program use is only slightly higher (10.5%) for people with high scores who are not in the study. Of course, funds-only outcomes for non-participants cannot be observed; if they have particularly bad outcomes only when receiving funds, then the results of the study would be less externally valid. However, the limited variation in outcomes with risk score and the lack of heterogeneity within the study suggest that treatment effects would be similar for individuals outside the study.

4.2 Outcomes beyond Housing

Even if case management does not improve housing outcomes, it may affect downstream outcomes such as use of public benefits (SNAP/TANF), arrests, and healthcare use. To determine whether case management affects other outcomes we link our study participants to administrative data. To measure benefit receipt, we link study participants to data on receipt of food or cash benefits paid by the Washington's Economic Services Administration, including SNAP (called Basic Food in Washington) and TANF. Unfortunately, we cannot observe receipt of federal disability or Social Security benefits. The two study groups use these benefits at similar rates. For example, the top panel of Table 4 shows that 63% of the funds only group and 60% of the case management plus financial assistance group received food benefits from SNAP 12 months after random assignment. While case management might plausibly connect participants to such benefits in an attempt to free up resources to pay for housing, we see no evidence that case management leads to greater take-up of these programs.

We also link our study participants to arrest data from the Washington State Patrol, which aggregates arrest data from local jurisdictions for background checks. Results using these data indicate that the case management plus financial assistance group is significantly more likely to be arrested. As shown in the middle panel of Table 4, 6.9% of the case management plus financial assistance group is arrested within one year compared to 3.7% of the funds only group. The regression-adjusted difference of 4.3 percentage points is statistically significant at the 5% level. We observe increased risk of arrest for all severity of offenses, ranging from misdemeanors to felonies, though most of the change comes from common, mid-severity gross misdemeanors (e.g. petty theft, simple assault). We interpret this result as an unambiguously negative outcome for case management clients. Contact with police does not necessarily imply guilt, so more arrests could reflect an increase in criminal behavior or a greater chance of being caught in a biased criminal justice system. Unstable housing may destabilize inter-personal relationships leading to violence or homelessness may simply increase the likelihood that a law-abiding person comes into contact with police (Palmer et al. 2019). Either way, it indicates that assignment to case management has a negative effect for at least a small proportion of people.

Results for healthcare use are more ambiguous. We observe inpatient, outpatient, and emergency healthcare visits as tracked by Medicaid claims and impute total cost as in Finkelstein et al. (2012). As indicated in the bottom panel of Table 4, the total cost of healthcare is 34% greater in the case management plus financial assistance group, but this large difference is imprecise because of the skewed distribution of healthcare costs. The likelihood of receiving any healthcare is more similar across the two groups, though we do observe increased use of inpatient healthcare that originates in an emergency visit, from 4.5% to 8.3%, which is the main driver of the difference in healthcare costs. Results for behavioral healthcare are similarly ambiguous. We use an existing measure of need for mental health or substance use treatment based on arrest, prescription, diagnosis, and treatment information. We do not detect large differences in need for these behavioral health services. On the other hand, we do observe a large decrease in mental health prescription fills, which is driven by the intensive rather than the extensive margin. Homelessness is sometimes tied to mental health and substance use disorders, and case managers might connect clients to behavioral healthcare. This makes results on healthcare use ambiguous because an increase in use could demonstrate either worse health or greater referral to services by case managers. In addition, we find mixed results, with a relatively wide confidence interval for total costs alongside suggestions of increased inpatient visits but decreased use of pharmaceutical treatment for mental health.

5 Discussion

We find that assignment to case management leads to a surprising increase in homeless program use. This increase could plausibly result from either worse housing stability or greater program use. Using data on eviction filings, we can rule out large improvements in housing stability. Increased rates of arrest indicate that some small group of case management participants have clearly negative outcomes.

We explain this unexpected set of results with two main facts. First, we document that the nature of case management varies across case managers in the YFHPI program. Second, we show that housing stability outcomes vary systematically with both the intensity of case management provided and the likelihood that the presence of case management delays financial assistance. The data suggest that versions of case management that delay financial assistance could drive the rare negative outcomes we observe while versions of case management that are intensive in time but also quickly pay financial assistance have more positive results. Both the intensity of and approach to case management appear to matter.

5.1 The Content of Case Management Varies across Agencies

Qualitative observations indicate that the content of case management varies across agencies and case managers. Fyall & Fowle (2019) find that the scope for intensive investment in each client varies with caseload. Those with smaller caseloads typically met with clients at least weekly for 1-3 months. Those with larger caseloads report being less active in following up with clients. Agencies also vary widely, from small non-profits to large multi-service agencies, leading to differences in case managers' ability to connect clients to internal service referrals. Finally, case managers are individual people with different styles. Some put great emphasis on emotional support activities. Others focus on actively helping clients with particular steps toward a stable housing lease and greater income.

We observe some evidence of heterogeneity across case managers in the data. Figure 5.a shows service characteristics across case managers. Each bubble plots average hours logged per client against the rate at which a given case manager pays out financial assistance within 30 days. We show each case manager twice, once for their funds-only clients and once for case management clients. The size of the bubble reflects the sample size for that group. Two facts are apparent. First, case management hours and funding rates are largely uncorrelated. Second, both of the measures vary widely across case managers, with average recorded hours ranging from close to zero and up to 30 hours per client and funding rates from 30% to 100%. Figure 5.b calculates the within-case manager difference between the two treatment groups. Even with case manager effects absorbed, the difference in time ranges from zero to 15 hours. Many case managers provide financial assistance to both groups at similar rates but others pay the case management group as much as 30 to 50 percentage points less often. While some of these within-case-manager measurements have relatively small samples, the differences in how case managers provide services are not just noise. We estimate separate regressions of each treatment intensity measure on case manager-treatment group indicators and interactions. For both measures, we can reject the joint null that the interaction effects are zero ($p < 0.0001$).

5.2 What Elements of Case Management Matter?

We use this variation in the implementation of case management to estimate which elements of case management matter. Table 5 estimates dose-response regressions. Each cell estimates a separate regression. The italicized outcomes change with the rows, with each regression including an outcome label and two treatment coefficients with standard errors. The specification changes with the column. In the first three columns, we regress the listed outcome on observed case management hours and an indicator for receiving financial assistance within 30 days, estimating by OLS. Controls vary across the columns as indicated at the end of the table. The final three columns estimate IV models that use a full set of treatment group-case manager interactions as instruments for the two treatment characteristics. In spirit, these IV specifications use the variation in program styles across case managers documented in Figures 5.a and 5.b.

Clients appear to do better when financial assistance is paid quickly. For example eviction filing rates are lower for people who receive financial assistance soon after enrollment. As shown in the first row and first column of Table 5, those who receive financial assistance immediately are 6.6 percentage points less likely to have an eviction filing conditional on case management hours, an effect that is significant at the 10% level. This gap is similar if we include our standard set of controls and case manager indicators, as in the second and third columns, although the estimate is less precise when all controls are included. The OLS specification uses both variation in treatment approaches across agencies and differences in take-up across individuals. When we focus only on variation in treatment approaches across agencies in the final three columns, the effects only get larger. Clients at agencies that provide immediate funding 10% more often have a 2.2 percentage point lower likelihood of an eviction filing. This point estimate remains similar as we add the standard controls, though the standard error increases when we control for case manager indicators. IV results for use of homeless services are somewhat more muted, which could indicate that receiving funds quickly both decreases homelessness and increases access to services. Results for the worst outcomes, eviction judgements and accessing street outreach, appear similar to those for eviction filings.

The importance of quickly paying financial assistance could help explain the small increase in the number of people with negative outcomes that we observe in the case management plus financial assistance group. From above, assignment to the case management plus financial assistance group delays payment for 9% of people. Taking the IV estimates from Table 5 at face value, that would imply about a 2 percentage point increase in eviction filings. Subject to sampling error, that estimate is similar in magnitude to the point estimates for eviction filing that we observe in the experiment.

Also, more intensive case management is associated with greater use of homeless programs but no greater risk of eviction. The first column of Table 5 shows that clients who are recorded as receiving 10 more hours of case management are 1.2 percentage points less likely to have an eviction filing. This coefficient is small and statistically insignificant. Including controls and using agency-treatment instruments matters little. Overall, there is very little relationship between eviction filing and the intensity of case management. At the same time, homeless program use correlates positively with case management hours, particularly when we only exploit cross-agency variation. Agencies that provide ten more hours of case management see their clients access homeless programs by 1.8 to 7.4 percentage points more often (columns 4-6). These results suggest that case management, per se, is beneficial rather than causing negative outcomes. If case management were inherently problematic, outcomes should become worse with more case management. Instead, more intensive models of case management seem to correlate with a benign combination of unchanged housing stability and greater ability to access programs when housing instability occurs.

Of course, caution should be taken when interpreting results from these dose response regressions. These analyses go beyond the variation in treatment generated by the experiment. Variation in treatment approaches across case managers could be driven by differences in client characteristics or correlated with other aspects of the case manager, which would bias these estimates. Additionally, the available variation in funding rates and case management hours limits precision. However, these results suggest a possible explanation for the findings of the randomized controlled trial. Some varieties of case management may benefit clients while others do not. The evidence suggests that when case management prevents or delays financial assistance, it can cause negative outcomes for a minority of clients. Meanwhile, more intensive case management shows more positive outcome. The most beneficial variety of case management for clients may be one that combines intense services with urgency in paying financial assistance.

6 Conclusion

We report the results of a randomized controlled trial measuring the effects of personalized case management for people at risk of homelessness. We compare two groups that both have access to financial assistance but receive different levels of case management. In practice, the case management plus financial assistance group receives more staff time to support housing stability and similar mean financial assistance but a lower probability of receiving any financial assistance. The case management plus financial assistance group is more likely to access homeless programs in the year after random assignment. Other housing stability measures are mixed. While most downstream outcomes are not different between the two groups, the case management plus financial assistance group is arrested more frequently.

We provide results on just two treatment arms in one homelessness prevention program, but the results seem useful more broadly. While we only observe two experimental groups, non-experimental variation across case managers in their case management approaches provides suggestive evidence that case management leads to negative outcomes when it delays payment of financial assistance but leads to more positive outcomes when it is more intensive and pays assistance more quickly. Regarding external validity, we provide some evidence that outcomes for the study case management group are similar to outcomes for clients in the same program but not in the study. More generally, we focus on the largest non-financial homelessness prevention program in a county with the 3rd largest homeless population in the United States. Thus, we provide a useful, new piece of evidence. Even so, the experimental literature on homelessness prevention programming remains small and studies of new programs in other locations are needed.

Our results suggest that the ideal homelessness prevention program might give clients more agency around when to access time-intensive, personalized case management. In the context of homelessness prevention, our results suggest that programs will need to actively triage people between larger programs that provide simple financial assistance and smaller programs for people needing more intensive services. This approach might mirror programming for chronically homeless individuals, like permanent supportive housing, that gives clients the option to access or ignore non-financial supports.

This insight around the agency of the individual may be a useful lesson for other policy spheres beyond homelessness prevention. Intensive programming is costly not just because of time but because staff have some influence over the allocation of resources. In situations where case managers have imperfect information about clients, case management may influence the distribution of resources in a way that unintentionally leads to negative outcomes for some cases. On the other hand, the intended purpose of these programs will also appear for some clients, and personalized assistance will help those clients navigate the complexity of poverty and better achieve their own goals. In a world with many different frictions, the optimal policy may be to give space for participants to have agency. Future work should consider whether greater scope for clients to choose between cash-like options or more intensive programming leads to better outcomes.

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Figures and Tables

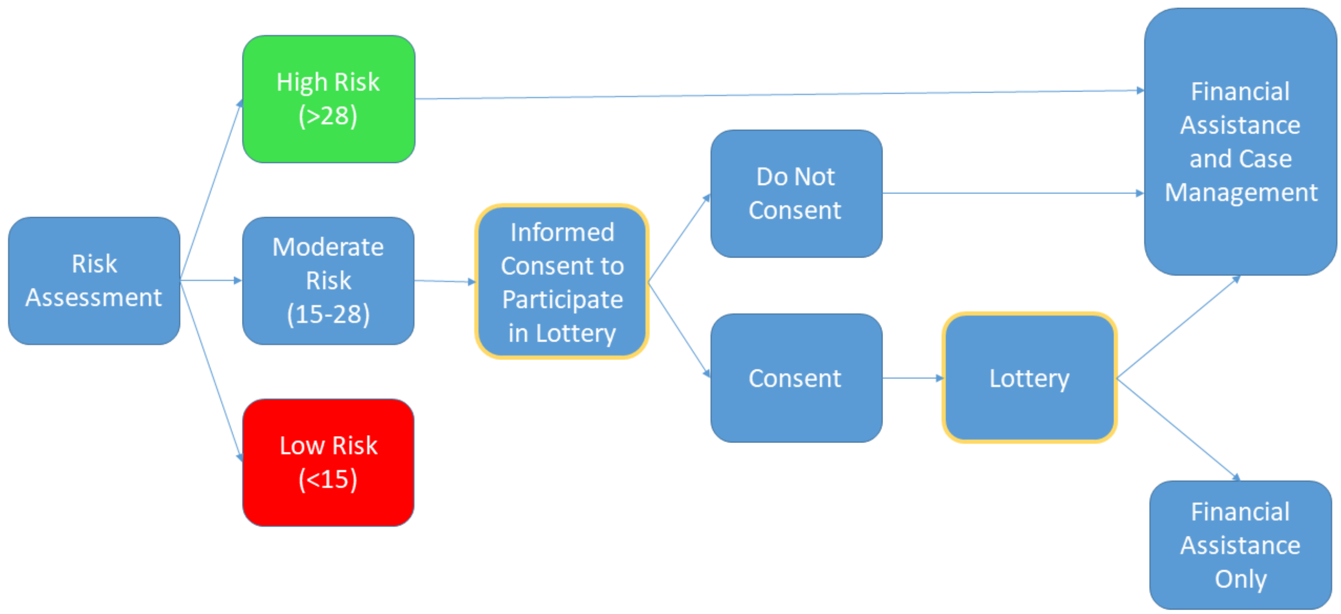
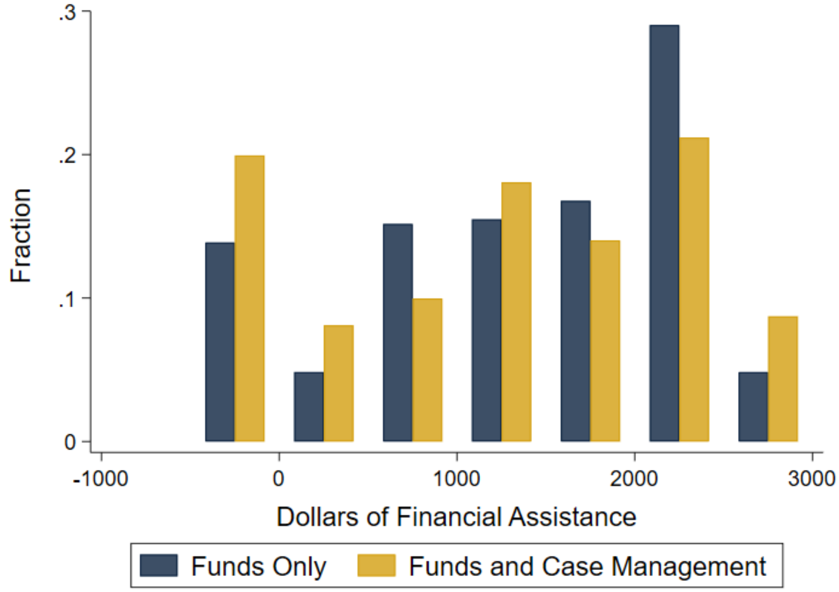
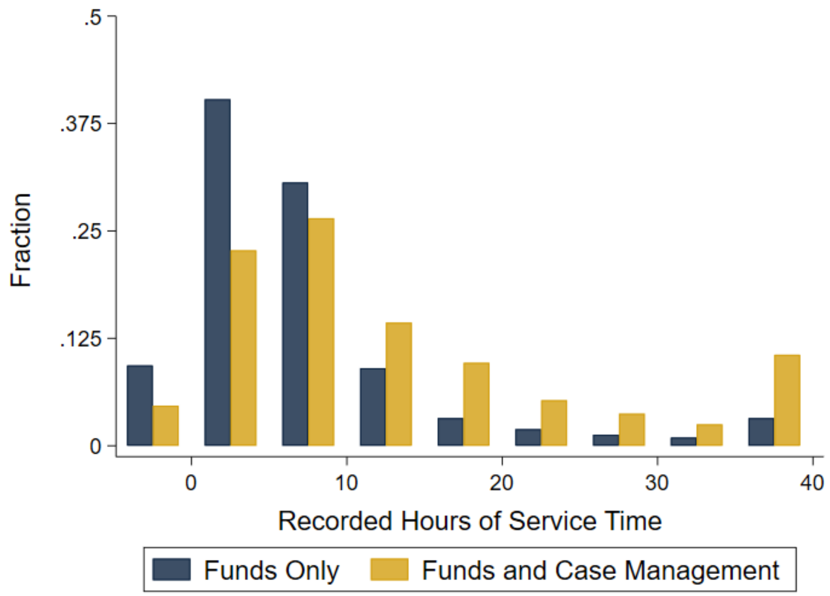


Figure 1: Recruitment Process



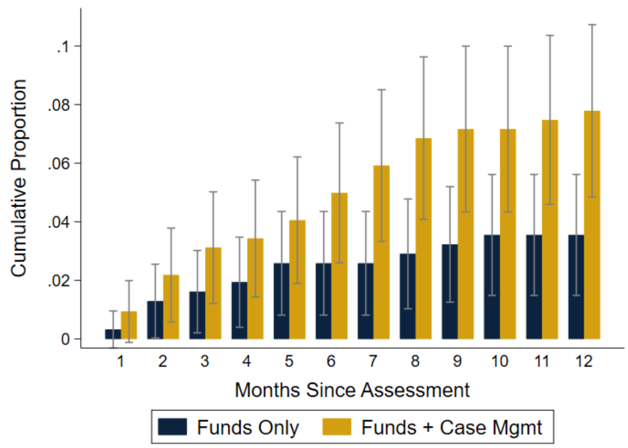
(a) Financial Assistance



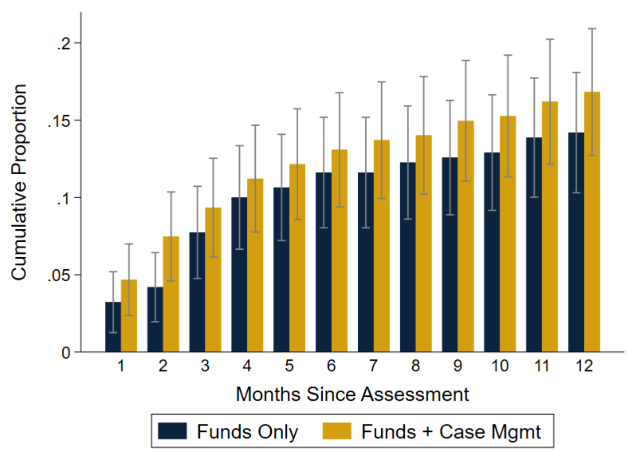
(b) Case Management

Figure 2: Services Received, by Treatment Assignment

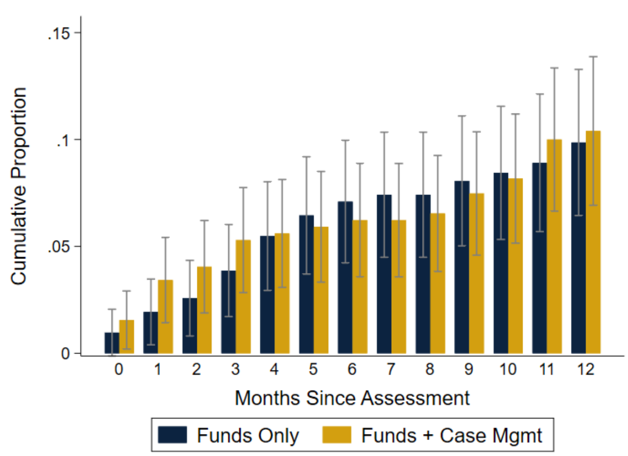
Notes: We measure financial assistance using program records of financial transactions and case management hours using time logs recorded by case managers. The latter likely undercounts case management hours. Both outcomes are measured as cumulative services received between 30 days prior to random assignment and 365 days after random assignment. In the figure, we winsorize financial assistance at $2,500 and service time at 40 hours for readability. We split outcomes by random group assignment, financial assistance only or a combination of financial assistance and case management.



(a) Homeless Services Use



(b) Eviction Filings



(c) Address Changes

Figure 3: Housing Outcomes over Time, by Treatment Assignment

Notes: Each bar show the cumulative probability of an event happening between random assignment and the listed number of months later. In (a) the event is enrolling in homelessness services covered by HMIS other than prevention; in (b) it is having a court eviction filing, and in (c) it is having a new address start or an existing address end. We split outcomes by random group assignment, financial assistance only or a combination of financial assistance and case management. Error bars show a 95% confidence interval using heteroskedasticity-robust standard errors. The sample for all figures is limited to the main sample that goes through random assignment. Panel (c) further limits the sample to people matching to an address history prior to random assignment.

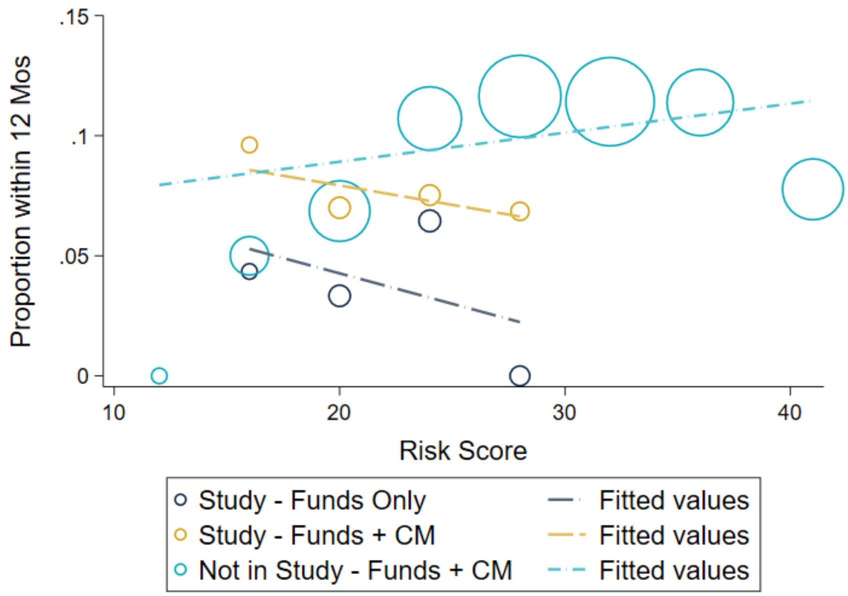
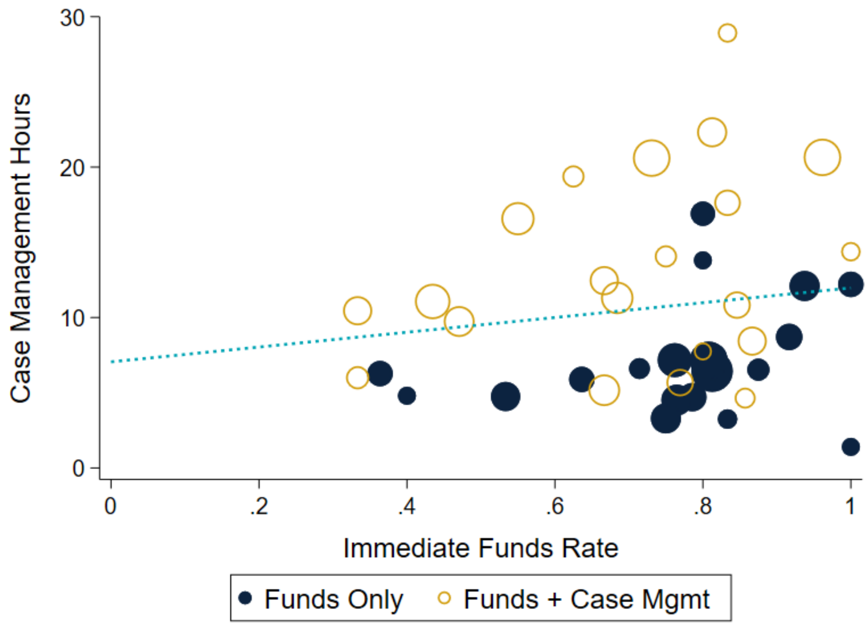
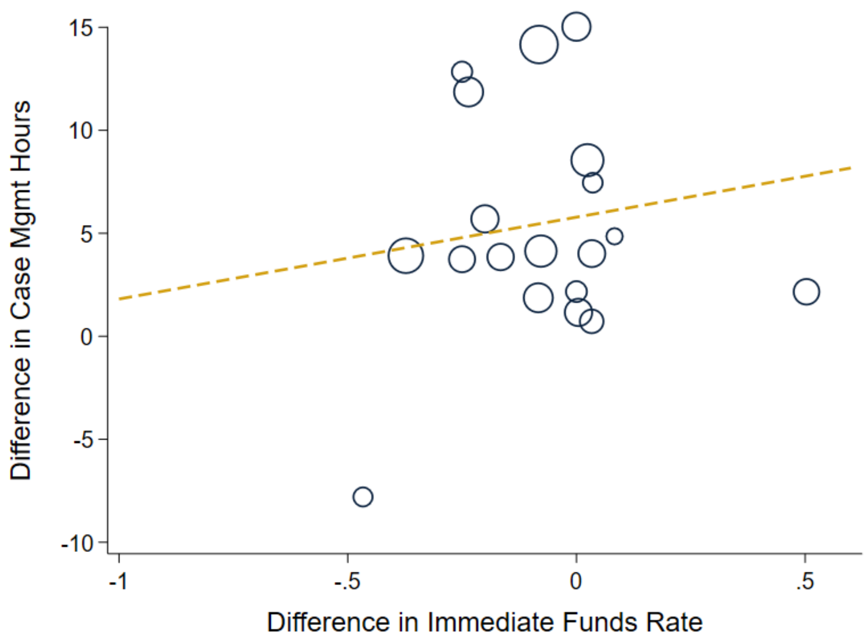


Figure 4: Homeless Program Use for Program Participants, RCT vs. non-RCT Participants, by Risk Score

Notes: The outcome is use of homeless programs between random assignment and 12 months later, as measured by HMIS records. Each bubble shows the mean outcomes for a set of people with the same assessment score and the same study group. The size of the bubble reflects the sample size for that group. Lines are linear fits of the group averages, weighting by group size. Assessment scores are from the YFHPI program risk tool. The sample is the union of all observations in Table 1, plus 6 observations scoring as 'low risk.' Study groups are defined as in Table 1.



(a) Average Services, By Case Manager and Treatment Group



(b) Treatment-Control Difference in Services, by Case Manager

Figure 5: Variation in Services Received

Notes: In (a), each bubble shows outcomes for participants from the the main sample assigned to the same case manager and treatment group. The vertical axis plots average case management hours within 1 year for that group, and the horizontal axis plots the proportion of people in that group who received any financial assistance within 30 days. In (b), each bubble represents one case manager and shows the difference between outcomes for clients assigned to the case management + funds group versus those for the only only group. The size of the bubble reflects the sample size for that group. Lines are linear fits of the group averages, weighting by group size. The sample is the main experiment sample. Case management hours for each client are winsorized at the 95th percentile.

Table 1: Baseline Characteristics

	In Study Funds	In Study Funds + CM	Not Study Mid Score	Not Study High Score	Diff. (1)-(2)
Assessment risk score	22.3	21.9	22.8	34.1	-0.43
					(0.32)
Housing loss in 10 days	0.55	0.55	0.55	0.84	0.0061
					(0.040)
Past episode of homelessness	0.60	0.53	0.61	0.88	-0.074*
					(0.039)
Past eviction	0.39	0.39	0.37	0.71	0.0054
					(0.039)
Number of children	1.90	2.06	1.89	2.10	0.16
					(0.11)
Age	37.4	36.6	40.8	33.3	-0.83
					(0.80)
Female	0.81	0.76	0.77	0.81	-0.050
					(0.033)
Black or African American	0.56	0.53	0.56	0.57	-0.029
					(0.040)
Hispanic/Latino	0.097	0.13	0.17	0.12	0.031
					(0.025)
HH member with disability	0.29	0.30	0.38	0.39	0.012
					(0.036)
N	310	321	373	515	631
Homeless program, past yr	0.090	0.069	0.13	0.13	-0.022
					(0.022)
Eviction filing, past yr	0.13	0.11			-0.017
					(0.026)
N	310	321	373	515	631
Food/cash benefits, past yr	0.74	0.70			-0.036
					(0.037)
Any arrest, past yr	0.035	0.054			0.018
					(0.017)
Any healthcare visit, past yr	0.49	0.48			-0.0026
					(0.042)
N	282	298	373	515	580

Notes: The top panel shows variables measured during assessment and enrollment for the YFHPI program. The middle panel comes from HMIS and eviction court records in the year prior to the date of random assignment. The final panel comes from Washington State linked administrative records, with arrests and healthcare measured cumulatively in the 12 months prior to the month of random assignment and the use of food and cash benefits measured at a point in time 12 months prior. The first two columns show raw means for the two treatment groups, funds only versus funds plus case management. The next two columns shows records for people who complete the risk assessment but do not enroll in the study. The third column shows people who score in the study range (15 to 28) but do not enroll in the study, primarily due to not consenting, and the fourth column shows those who score too high (> 28) and do not enroll in the study. Observations in these columns meet all other criteria for being in the sample. Blank cells indicate situations where we cannot use identifiers to match to data sources outside the County due to lack of consent. The final column shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment, estimated on a sample combining the first two columns. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table 2: Services Received

	Funds	Funds + Case Mgmt	Adj. Diff.
Any funds in 30 days	0.78	0.69	-0.090**
			(0.035)
Any funds in 1 year	0.86	0.80	-0.061**
			(0.030)
Mean funds within 30 days	1107.4	976.5	-143.0**
			(71.1)
Mean funds within 1 year	1444.8	1425.2	-36.3
			(86.0)
Case management hours in 1 year	8.17	15.7	7.62***
			(1.37)
Case management hours on enrollment day	2.25	2.40	0.20
			(0.17)
Case management hours after enrollment day	5.93	13.3	7.42***
			(1.36)
N	310	321	631
Days to Exit	167.5	212.8	38.9***
			(14.9)
Exit to Housed	0.65	0.53	-0.13***
			(0.040)
Exit to Unhoused	0.042	0.053	0.011
			(0.017)
Exit to Unknown	0.31	0.41	0.12***
			(0.039)
N	310	321	631

Notes: For the top two panels, we measure outcomes variables cumulatively between random assignment and the listed duration afterward using HMIS data. In the bottom panel, we use outcomes from the person's first recorded exit from the YFHPI program after random assignment in program records. The first two columns show raw means for the two treatment groups. The third column shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and controls for the outcome in the year prior to study enrollment, whether housing loss is expected within 10 days, whether the household has been homeless before, number of children, age, age squared and indicators for female, Black, Hispanic/Latino, having a family member with a disability, and month of study enrollment. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table 3: Housing Outcomes, 12 Months

	Funds	Funds + Case Mgmt	Adj. Diff.
Any homeless program use	0.035	0.078	0.042**
			(0.018)
–Emergency shelter	0.026	0.016	-0.0071
			(0.011)
–Street outreach	0.013	0.034	0.024*
			(0.013)
–Coordinated entry	0.019	0.031	0.0091
			(0.012)
–Longer-term subsidy	0.0097	0.025	0.015
			(0.010)
N	310	321	631
Eviction filing	0.14	0.17	0.041
			(0.028)
Eviction judgment	0.16	0.18	0.037
			(0.029)
Any monetary judgment	0.11	0.11	0.012
			(0.025)
Amount of judgment	461.2	467.2	55.1
			(127.2)
N	310	321	631
Address change	0.23	0.25	-0.016
			(0.059)
N	127	126	253

Notes: We measure all outcome variables cumulatively between random assignment and 12 months afterward. The top panel comes from HMIS data, the middle panel from eviction court records, and the bottom panel from consumer reference address histories. The top two panels use our main sample; the bottom panel narrows the sample to individuals who match to an address history prior to random assignment. The first two columns show raw means for the two treatment groups. The third column shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and controls for the outcome in the year prior to study enrollment, whether housing loss is expected within 10 days, whether the household has been homeless before, number of children, age, age squared and indicators for female, Black, Hispanic/Latino, having a family member with a disability, and month of study enrollment. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

Table 4: Downstream Outcomes, 12 Months

	Funds	Funds + Case Mgmt	Adj. Diff.
Any food or cash benefits	0.73	0.69	-0.016
			(0.039)
–SNAP	0.63	0.60	-0.0093
			(0.042)
–TANF	0.097	0.087	0.0059
			(0.025)
N	267	277	544
Any arrest, cumulative	0.037	0.069	0.043**
			(0.019)
–Felony	0.0075	0.022	0.015
			(0.0092)
–Gross misd.	0.015	0.040	0.027**
			(0.013)
–Misdemeanor	0.0075	0.011	0.0050
			(0.0064)
–Unknown	0.022	0.032	0.018
			(0.014)
N	267	277	544
Cost to Medicaid, cumulative	1543.2	1873.2	520.3
			(410.6)
Any Medicaid visit, cumulative	0.51	0.51	0.026
			(0.040)
–Emergency outpatient	0.39	0.42	0.048
			(0.040)
–Emergency inpatient	0.045	0.083	0.047**
			(0.023)
–Non-emergency inpatient	0.045	0.051	0.0083
			(0.018)
–Non-emergency outpatient	0.48	0.47	0.017
			(0.040)
Need for mentall illness treatment	0.12	0.097	-0.013
			(0.026)
Need for substance use treatment	0.037	0.032	0.0060
			(0.015)
# mental health prescription fills (wins.)	1.57	1.13	-0.58**
			(0.28)
Any mental health prescription	0.21	0.19	-0.016
			(0.030)
N	267	277	544

Notes: All outcomes come from linked Washington State administrative records on people who have ever used public benefits. We restrict the sample to people who match to some record prior to the month of random assignment. Food and cash benefits includes benefits operated by the Economic Services Administration (SNAP, TANF, HEN, and ABD). Arrests originate from the Washington State Patrol. Healthcare use originates from Medicaid claims. We measure receipt of food and cash benefits at a point in time 12 months after random assignment; all other variables are cumulative between the month of random assignment and 12 months afterward. The first two columns show raw means for the two treatment groups. The third column shows the coefficient on a treatment assignment dummy in a regression of the listed variable on treatment assignment and controls for the outcome in the year prior to study enrollment, whether housing loss is expected within 10 days, whether the household has been homeless before, number of children, age, age squared and indicators for female, Black, Hispanic/Latino, having a family member with a disability, and month of study enrollment. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and *** respectively.

Table 5: Dose Response, Regressions of Housing Outcomes on Services Received

	OLS	OLS	OLS	IV	IV	IV
Eviction filing						
Case Mgmt Hours/10	-0.012	0.00078	0.0045	-0.034	0.00062	0.0085
	(0.012)	(0.013)	(0.014)	(0.021)	(0.022)	(0.035)
Immediate Financial Assistance	-0.066*	-0.072*	-0.050	-0.22***	-0.22**	-0.16
	(0.035)	(0.037)	(0.039)	(0.084)	(0.086)	(0.13)
Eviction judgment						
Case Mgmt Hours/10	-0.022**	-0.0064	-0.00024	-0.049**	-0.012	0.0025
	(0.011)	(0.012)	(0.014)	(0.021)	(0.022)	(0.036)
Immediate Financial Assistance	-0.094**	-0.10***	-0.082**	-0.23***	-0.25***	-0.19
	(0.037)	(0.038)	(0.040)	(0.085)	(0.084)	(0.13)
Any homeless program use						
Case Mgmt Hours/10	0.012	0.014	0.018	0.018	0.029*	0.074***
	(0.0093)	(0.010)	(0.012)	(0.013)	(0.015)	(0.027)
Immediate Financial Assistance	0.0021	-0.0018	0.0050	-0.11*	-0.083	-0.049
	(0.021)	(0.021)	(0.023)	(0.057)	(0.051)	(0.072)
Street outreach						
Case Mgmt Hours/10	0.0046	0.0067	0.0063	0.016	0.026**	0.039**
	(0.0055)	(0.0057)	(0.0063)	(0.010)	(0.012)	(0.020)
Immediate Financial Assistance	0.015	0.013	0.011	-0.063*	-0.050	-0.11**
	(0.011)	(0.012)	(0.013)	(0.036)	(0.034)	(0.055)
Address change						
Case Mgmt Hours/10	-0.0076	-0.0039	0.0035	-0.032*	-0.028	-0.016
	(0.011)	(0.012)	(0.014)	(0.018)	(0.021)	(0.032)
Immediate Financial Assistance	-0.015	0.0015	-0.0052	0.033	0.023	-0.051
	(0.029)	(0.031)	(0.032)	(0.058)	(0.060)	(0.098)
CM Indicators	N	N	Y	N	N	Y
Controls	N	Y	Y	N	Y	Y
Observations	592	592	592	592	592	592

Notes: Each cell shows the results of a separate regression, reporting the coefficients on case management hours received within one year and an indicator for receiving any financial assistance within 30 days. The outcome is the same within rows and listed in italics. The specification varies across columns. The first three columns are estimated by OLS. The final three columns are estimated by 2SLS using as instruments a vector of case worker indicators interacted with a treatment assignment indicator. Other controls vary across columns as reported at the bottom of the table. 'Controls' refers to the same set as Table 3. Case management hours are winsorized at the 95th percentile. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***, respectively.

7 Data Appendix

7.1 Sample Definition

Conceptually, the study consists of people who are eligible for YFHPI, score below 28 on the risk score tool, and consent to participate in the study. In practice, recruitment and random assignment occurs entirely within the YFHPI program database. We construct the analysis sample by applying these conceptual rules to the available program records.

Sample construction starts with the universe of risk score assessment records. If a person takes the assessment, they have already met all other program criteria, including residence and presence of youth or children. We focus on the set of assessments taken by clients between January 2018 and March 2020 for clients who enroll in the program after May 2018. For informed consent reasons, we limit to households with a head over age 18. We also remove 'system' records not attached to an actual person.

For each person, we identify one focal assessment record. For each person, we identify the first assessment that would make the person eligible for YFHPI. In particular, most people have only one assessment record, which we use. If someone's first score is above 15, we use that record. If someone's first score is below 15 and later re-scores above 15, we use the latter record. If households include multiple assessed people we keep only the first study-eligible record.

We use this set of assessment records to identify people who were eligible for the study and gave consent. Case managers track the final group assignment of the person into the database: funds only, funds and case management, did not consent, not eligible for random assignment, or withdrew. We exclude from the main analysis sample those who are listed as not consenting or not eligible for random assignment. Case managers also upload risk scores and consent forms to the database, which almost always align with the recorded group assignment. King County staff also checked group assignment against other internal records, which we used to correct a small number of data entry errors and to update instances of clients withdrawing from their group of random assignment.

We use the selected record to define both initial treatment assignment and actual treatment group. Random assignment is based on internal numbers generated by the database. In particular, before October 2018 the system assigned treatment based on whether the person's randomly assigned user ID was odd or even; after October 2018 it was based on the seconds place of the system time. For actual treatment group, we use the case worker's recorded value in most cases. For households with multiple records, we observe the person who took the focal assessment. If a person withdrew, we updated their final assignment to be in the case management group.

7.2 Linking to Housing Outcomes

7.2.1 Homeless Program Use

We measure homeless program use by matching to Homeless Management Information System (HMIS) data. During the study, King County's Department of Community and Human Services (DCHS) operated HMIS in King County. It is similar to HMIS data in other communities. It provides a common data system for homeless programs, both public and private. It records when individuals enroll in homeless programs, attached to dates and individual identifiers. The data also assigns a program type to each enrollment. We use an extract with records through August 2021. Records start as early as 2005, though in practice HMIS in King County appears to have good coverage for this sample starting in about 2016.

King County DCHS staff match the program data to HMIS using shared identifiers. Both databases include information on name, date of birth, ZIP code, and social security number. DCHS staff already match these two databases for their own internal evaluation purposes. So, we rely on the same fuzzy matching algorithm that they use and analyze only de-identified data. Given the comprehensive nature of HMIS in King County, we interpret non-matching to HMIS as the absence of homeless program use.

We focus on programs that require a person be literally homeless to access services and were thus included in King County's own internal definition of homelessness using this data at the start of the study. These programs include coordinated entry (diversion/assessment), emergency shelter, permanent supporting housing, other permanent housing, rapid re-housing, street outreach, and transitional housing. We exclude day shelter, homeless prevention, and services only, which may be accessed without being literally homeless. Throughout the paper, we also combine permanent housing, rapid re-housing, and transitional housing into a measure of 'longer-term subsidies.' The data is naturally at the person-program enrollment level, so we aggregate it to person-level indicators for whether a person had any program enrollment dates of a certain type between random assignment and x months later.

7.2.2 Eviction

We measure eviction filings and judgments using records from King County Court. As is typical of eviction court records, these records include tenant name and address alongside court information like the filing date, judgment date, and any judgment amount the tenant owes. We access these filings through two sources: American Information Research Services, Inc and the Housing Justice Project. Both of these sources get records from the court itself. In practice, we find that these two sources cover a nearly identical sample of eviction filings. In the main analysis, we report results using AIRS data because it has a longer time period with good coverage ranging from 2006 through April 2021. Evictions become rare after March 2020 due to both local and national eviction moratoria related to the COVID-19 pandemic.

King County DCHS staff match eviction filings to program records. Eviction filings only include name and address, so we rely heavily on fuzzy matching on names. We prioritize matches that also match on ZIP code, but because addresses can change over time, we include records that match only on name. As a result, we likely over-match to eviction filings to some extent. However, we believe the name match is appropriate for three reasons. First, we study a group selected precisely because they are at risk of eviction, making them much more likely than the average person to appear in eviction filing. The 15% annual eviction filing rate we find is not unusually high given that 39% of the sample has been evicted in the past. Second, we observe eviction filings spike around the time of program enrollment (see Appendix Figure 6). Third, any over-matching simply adds noise to both the treatment and control groups, which is accounted for in the standard errors, rather than biasing treatment effects.

We aggregate from individual filings to person-level outcomes. We use the dates of randomization and any eviction filings to compute if each person had a filing within a given time frame. We assume non-matches imply no eviction filing. We compute similar outcomes using judgment dates for indicators of any judgment, any monetary judgment, and the total amount of monetary judgments in the time period.

7.2.3 Address Changes

We measure address changes using consumer reference data. Infutor Data Solutions aggregates various consumer data, e.g. cell phone bills, into contact information for use in commercial marketing and identity verification. This process creates an address history with exact addresses attached to the effective date and last verification date for the person at that address. We treat these dates as the start and end of the person's residence, respectively. This data has been used in economics research to measure housing stability, e.g. in response to rent control (Diamond et al. 2019). Coverage of the data back in time is quite good; it can pick up people exiting public housing when it is demolished in the 1990s (Phillips 2020). For this study, we use an extract of the March 2021 vintage of the data that includes anyone who has ever had an address in King County, WA, regardless of current state of residence

We match program data to the consumer reference address histories using date of birth, first name, last name, and last 4 digits of SSN. King County DCHS 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.

For analysis with the Infutor data, we limit the sample to observations that match to an Infutor address that begins before the person's enrollment in the study. All participants in the study must have a residence in King County to enter the program, so they would all match to a comprehensive list of address histories. However, the Infutor data is not fully comprehensive because it is built from consumer data, and some people have a less substantial paper trail. For example, people who are young or Hispanic/Latino are less likely to match to Infutor (Phillips 2020). In practice we have Infutor data for 42% of the sample. Match rates are similar by treatment status, 43% for the funds only group and 41% for the case management group, which alleviates concerns of differential attrition. However, the low match rate does reduce statistical power.

Using these matches, we measure whether the person changes addresses. We define a household as having an address change in the relevant time period if the person matches to an Infutor record that has an address start date or end date during the time period. For multiple matches, we aggregate across all matches.

7.3 Washington State Administrative Data

7.3.1 Data Linking

For outcomes beyond housing stability, we access outcomes from the Research and Data Analysis group of the Washington State Department of Social and Health Services. They maintain DSHS's own data on all individuals who have ever been clients of DSHS. Since DSHS operates large cash, food, and medical benefits, including SNAP, TANF, and Medicaid. As a result, nearly anyone who has low-income and has lived in the State of Washington appears in their data. They then link DSHS's internal data with data from other state departments.

Staff at RDA link YFHPI program records to the RDA database using name, date of birth, and social security number. The algorith is described in Campbell et al. (2008). Because we have access to detailed identifiers and the RDA database covers nearly all low-income Washington residents, match rates are both high and high quality. We limit the analysis sample for downstream outcomes to people who match to some RDA record prior to random assignment. Overall, the downstream outcome sample includes 92% of the main sample to RDA records. Match rates are similar across treatment arms, 93% for the case management group and 91% for the funds only group.

7.3.2 Measuring Outcomes

Records for food and cash benefits come from DSHS's own records from the Economic Services Administration, which administers these programs. The data covers not only the large SNAP and TANF programs but also the much smaller Housing and Essential Needs cash benefit and the Aged, Blind, or Disabled cash benefit. The State of Washington also supplements TANF and SNAP to include people ineligible for the federal programs (e.g. undocumented/recent immigrants), and for simplicity we consider these programs identical to TANF and SNAP. Since enrollment in these programs is persistent, we measure outcomes as flow indicators for being enrolled in the program at a moment in time. We do not observe benefit amounts.

We measure arrests using data from the Washington State Patrol. The WSP aggregates arrest records from local jurisdictions across the entire state of Washington, including but not limited to King County. The underlying database is the same one used for background checks within the State of Washington. It covers all arrests that require fingerprinting, including all felonies and gross misdemeanors. We receive counts of total arrests within the state as covered by the data. These counts are both the overall total and decomposed by severity of offense (felony, etc.) and type of offense (assault, etc.). Since arrests are rare, we process these variables into cumulative indicators for any arrest between random assignment and the listed follow-up period.

We measure healthcare outcomes using data from the Washington State Health Care Authority. Their Provider One database includes all healthcare claims paid for by Medicaid, including Medicaid managed care and the portion paid by Medicaid for dual-enrolled people, as well as some smaller state programs (e.g. for pregnant women and foster children). It does not include unpaid care, care paid for by private health insurance and healthcare exchange plans,or care paid for by Medicare. Because we study a sample of households with low income and children, Medicaid has good coverage in this context. In the 2019 American Community Survey, 81% of children and 54% of adults in Washington from households with income below the federal poverty line had health insurance from Medicaid. We calculate total expected healthcare cost as in Finkelstein et al. (2012), applying their cost weights to the counts of healthcare visits in our data.

All records received from RDA report outcomes by calendar month. We re-center all variables into months relative to the random assignment so that month zero is the calendar month of random assignment. We consider month zero as the first post-treatment month.

8 Appendix Tables and Figures

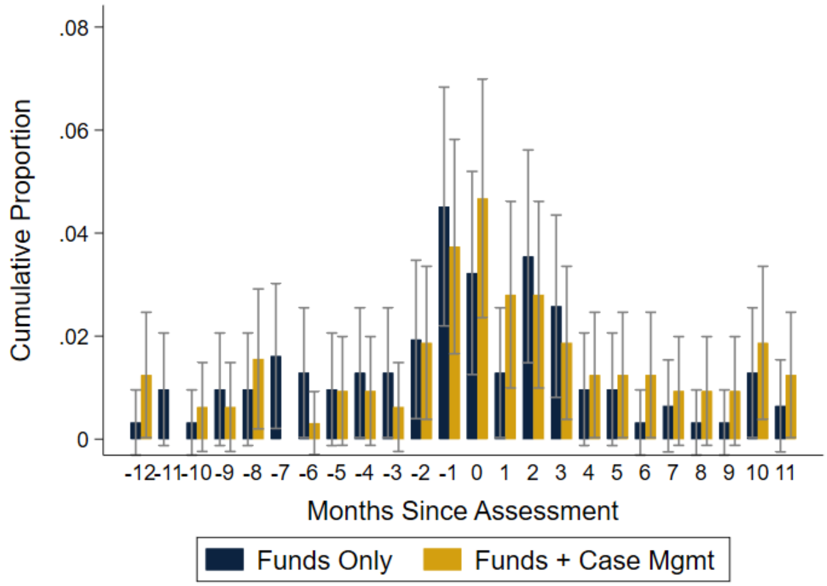


Figure 6: Flow Probability of an Eviction Filing, by Months Since Random Assignment and Treatment Group

Notes: Each bar show the flow probability of an eviction filing in the listed month. Month zero is the first month after random assignment. Error bars show a 95% confidence interval using heteroskedasticity-robust standard errors. The sample for all figures is limited to the main sample that goes through random assignment.

Table 6: Homeless Program Use, by Subgroup

	Funds Has Char	Funds + Case Mgmt Has Char	Funds Not	Funds + Case Mgmt Not	Adj. Dif-in-Dif
Risk score 22+	0.034	0.077	0.037	0.079	0.0054
					(0.038)
Black or African American	0.040	0.094	0.029	0.060	0.017
					(0.038)
Hispanic/Latino	0.033	0.024	0.036	0.086	-0.076*
					(0.045)
White	0	0.050	0.045	0.087	0.011
					(0.037)
Female	0.032	0.082	0.052	0.066	0.039
					(0.047)
Age 36+	0.036	0.043	0.034	0.11	-0.067*
					(0.037)
Household has a member with a disability	0.045	0.063	0.032	0.084	-0.036
					(0.041)
Assessed before March 2019	0.043	0.099	0.024	0.046	0.032
					(0.035)
Housing loss expected within 10 days	0.035	0.062	0.036	0.098	-0.023
					(0.037)
Past episode of homelessness	0.054	0.11	0.0081	0.046	0.010
					(0.034)
Past eviction	0.033	0.064	0.037	0.087	-0.0079
					(0.036)
HMIS - Any - 12 months pre	0.14	0.13	0.025	0.074	-0.048
					(0.097)
High turnover	0.032	0.081	0.041	0.074	0.011
					(0.038)
High caseload	0.050	0.12	0.020	0.036	0.053
					(0.038)

Notes: In the first four columns, each cell shows the proportion of people using homeless programs within 12 months for a different subgroup defined by treatment assignment and a baseline characteristic. Each row describes a particular characteristic. The first column shows means for people assigned to funds only who have the listed characteristic. The second column shows people assigned to case management + funds who have the listed characteristic. The third and fourth columns similarly show funds only and case management + funds means for people without the characteristic. The final column shows the coefficient on the interaction between the characteristic and treatment assignment in a regression of the outcome on the interaction as well as a treatment assignment indicator, an indicator for the characteristic, and all of the controls from Table 3. 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 is identical to Table 3.

Table 7: Eviction Filing, by Subgroup

	Funds Has Char	Funds + Case Mgmt Has Char	Funds Not	Funds + Case Mgmt Not	Adj. Dif-in-Dif
Risk score 22+	0.18	0.18	0.088	0.16	-0.069
					(0.057)
Black or African American	0.13	0.18	0.16	0.16	0.049
					(0.059)
Hispanic/Latino	0.10	0.098	0.15	0.18	-0.053
					(0.082)
White	0.10	0.19	0.15	0.16	0.11
					(0.069)
Female	0.15	0.17	0.086	0.16	-0.054
					(0.068)
Age 36+	0.12	0.13	0.17	0.21	-0.049
					(0.058)
Household has a member with a disability	0.15	0.13	0.14	0.19	-0.069
					(0.064)
Assessed before March 2019	0.15	0.20	0.14	0.12	0.073
					(0.058)
Housing loss expected within 10 days	0.18	0.18	0.093	0.15	-0.079
					(0.058)
Past episode of homelessness	0.16	0.18	0.11	0.15	-0.0029
					(0.059)
Past eviction	0.16	0.18	0.13	0.16	0.010
					(0.063)
HMIS - Any - 12 months pre	0.18	0.26	0.14	0.16	0.049
					(0.12)
High turnover	0.11	0.17	0.20	0.16	0.097
					(0.061)
High caseload	0.14	0.21	0.15	0.13	0.080
					(0.058)

Notes: In the first four columns, each cell shows the proportion of people with an eviction filing within 12 months for a different subgroup defined by treatment assignment and a baseline characteristic. Each row describes a particular characteristic. The first column shows means for people assigned to funds only who have the listed characteristic. The second column shows people assigned to case management + funds who have the listed characteristic. The third and fourth columns similarly show funds only and case management + funds means for people without the characteristic. The final column shows the coefficient on the interaction between the characteristic and treatment assignment in a regression of the outcome on the interaction as well as a treatment assignment indicator, an indicator for the characteristic, and all of the controls from Table 3. 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 is identical to Table 3.