A Safety Net for the Tenuously Housed: Experimental Evidence from a Large Government Program

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Abstract

Rent affordability is one of the most pressing issues facing American cities, and households who default on rent risk eviction, which generates downstream consequences including homelessness. But there is limited evidence on the benefits of a safety net for those who are currently housed but at risk of losing housing. Partnering with a large-scale government program, I randomize a set of structured interventions for those with an eviction notice—those who are currently housed and have 1-3 months of rental arrears. One treatment pays tenants' arrears, their current month of rent, and up to 3 months of future rent; a second treatment adds 6 months of case management. Using linked administrative data, and fielding novel baseline and follow-up surveys, I study the impact of assistance on eviction, homelessness, and other socioeconomic outcomes. The results show that it is feasible to reach households prior to eviction court and that these households face multiple sources of uninsurable risk to income and expenses. Assistance is highly effective at preventing eviction filings and judgments, and at preventing homelessness within 6 months. At 1 year, the impact on shelter entry persists but impacts on broader measures of homelessness and eviction attenuate. I develop a Baily-Chetty framework for optimal housing assistance showing that the standard social insurance formula requires an extensive-margin correction—strictly positive because households at the eviction margin prefer housing but cannot afford it without assistance—and use the experimental estimates to inform the sufficient statistics characterizing the optimal level and targeting of assistance.

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1 Introduction

Rising housing costs is one of the most pressing issues in American cities, and rent affordability is a challenge faced acutely by low-income households. When households fail to pay their rent they risk eviction, which affects approximately 2.7 million households in the United States per year (Gromis et al., 2022), and generates downstream consequences including homelessness (Collinson et al., 2024b). Yet there is limited evidence on the effectiveness of a safety net aimed at stabilizing housing for the currently housed but at risk of losing housing.

Two practical challenges arise in designing a safety net for housing. A first concern is *timing*, and reaching households prior to the legal system, which typically occurs after 1-2 years of increasing distress (Collinson et al., 2024b). A second challenge is *targeting*, and reaching those households who would otherwise become homeless. Given the high costs of paying households' rent, narrowing the scope of services to those at imminent risk of losing housing is a critical design feature affecting the costs and benefits of homelessness prevention policy (Evans et al., 2021).

In this paper, I partner with a large urban county (population 928,000) to develop and randomize a set of structured interventions to study the safety net for the tenuously housed. To my knowledge, it is the first study of a large-scale government program to target assistance for those with an eviction notice. Eligible households are currently housed but 1-3 months in arrears, and have not yet had an eviction court filing. An important feature of the program is its broad eligibility criteria: All households with an eviction notice are eligible, lending itself to an analysis of targeting.

Timing the assistance at the eviction notice stage is appealing because it is the first step in the legal process of eviction. An official notice is legally required before a landlord is permitted to file an eviction case, but unlike the court filing, the notice is not in the public record. It is crucial to intervene prior to the court filing, because the act of filing is public and may negatively impact tenants' ability to find future housing. Yet it is also challenging to provide services at the notice stage precisely because eviction notices are not in public records, and it is hard to identify this population at risk of eviction. I overcome this challenge by partnering with a large-scale government program that elicits applications from those with a notice. The broad eligibility criteria of the program is also a critical advantage of this study, because eligibility is not conditional on the tenant's ability or inability to pay future rent, allowing me to study impacts across the distribution of tenant disadvantage.

I field an original baseline survey as part of the program intake form. The survey allows me to characterize the sources of economic distress that precede households defaulting on rents. The survey also elicits expectations about households' ability to pay their future rent without assistance, and asks households their preference for using assistance in their current residence or in an alternative residence. The survey also asks households about their counterfactual living situation if they were required to vacate their current housing. Respondents are truthfully informed that their survey responses do not affect their likelihood of receiving services, and I verify that responses correspond reasonably well with follow-up realized outcomes, consistent with tenants reporting truthfully.

Working with the government agency, I randomize approximately $13 million dollars in rental assistance and case management services. The first treatment pays households' arrears, their current month, and up to three months of future rent. The second treatment arm adds an additional 6 months of case management. The control group receives information about other assistance programs in the area but receives no direct assistance and are ineligible to reapply for 6 months, meaning they effectively must resolve their housing crisis on their own, reflecting the status quo in most American cities that do not have assistance for households who

default on rent. The program is a monthly lottery, with all applicant households included in the study. Over 4,000 households have enrolled during the 20-month enrollment period of February 2024 through December 2025.

The primary outcomes are the household having an eviction filing or judgment at the application address, and an indicator for staying in an overnight homeless shelter or having any contact with the homelessness system. To supplement these outcomes, I field a survey for each cohort at 10 months from randomization, to more fully characterize households' housing and living arrangements. The survey includes a measure of unsheltered homelessness, which is not available in administrative homelessness data. In addition to these outcomes, data use agreements are in progress to obtain administrative records on quarterly earnings, including hours worked, public assistance receipt including SNAP and TANF, health records including emergency department use and mental health claims. The earnings and health records will further allow me to characterize the nature of shocks that precede the eviction notice.

Those facing transitory distress may be more likely to benefit from emergency rental assistance, which offers insurance against the risk of losing housing, compared to those experiencing persistent distress, for whom the assistance may only delay homelessness rather than prevent it. Households may also differ in their counterfactual living situation and whether they can stay with friends and family. Those without informal support are likely to be at greater risk of homelessness if they are required to vacate. I pre-specified these two subgroups to connect the experiment to two economic channels that govern the design of housing assistance as a social insurance program. The application data and baseline survey, as well as the linked administrative data, lend themselves to classifying households into these subgroups. The rich baseline data further permit using the methodology of Chernozhukov et al. (2025) for characterizing treatment effect heterogeneity.

Households who apply to the program are 70% female-headed, and approximately 60% have children, a similar demographic profile to those facing eviction cases in Chicago and New York that I study in earlier work. Households are highly rent burdened, and over 60% report a job loss or reduced hours as the reason for falling behind on rent. Approximately 14% report a death in the family, the same proportion as the share reporting a loss of benefits. Notably, only 25% of households report that they anticipate the distress to persist beyond 3 months, and 72% expect to pay their rent in full without assistance within 3 months. While these results show that a large share of households believe they are facing a transitory period of distress, and are optimistic about their future ability to pay rent, over 60% of households say they do not know where they will stay if they have to vacate. Approximately 12% report they will share in a homeless shelter, and a strikingly high share—17% —report they will stay outdoors or in a place not meant for habitation such as a car or a tent. Only 5% report that they would stay with friends or family. When households are asked if they would prefer to use assistance funds at their current residence or at an alternative residence, 86% of households prefer to remain in place.

Assistance is highly effective at reducing both eviction filings and judgments. Assignment to treatment reduces eviction filings within six months by 8.0 percentage points (pp), or approximately 52% relative to the control group mean, and reduces eviction judgments by 4.5 pp, a reduction of roughly 82%. Effects on homelessness are similarly large: treatment reduces emergency shelter entry by 1.1 pp (92%) at six months and reduces any contact with the homelessness management system by 7.0 pp (66%) over the same horizon. Evidence from the 10-month follow-up survey corroborates the administrative findings, with treated households reporting 19.2 pp lower rates of unsheltered homelessness and nearly 1 fewer place stayed since application relative to the control group. Approximately 6% of the control group are paying zero rent, which is consistent with the low baseline share who report being able to move in with friends or family.

At 1-year, while the impact on the extreme outcome of shelter entry persists (1.7 pp, or 89%), the broader homelessness and eviction impacts attenuate. The treatment effect on any homelessness contact falls from 7.0 pp at six months to 3.0 pp at one year. Event studies confirm that while treatment and control groups diverge sharply in the quarters immediately following the lottery, with the treatment group benefiting from the program, after 2 quarters the treatment group has homelessness flows similar to the control group, meaning the cumulative effects attenuate to approximately 20%. I find evidence that effects on homelessness fade more quickly for households facing the most severe and persistent financial strain—consistent with the interpretation that the assistance successfully stabilizes tenants experiencing transitory distress but may only delay rather than prevent homelessness for those facing persistent challenges. These findings point to the potential for improved targeting and the design of tiered assistance, which I discuss at the end of this paper.

To interpret these findings and characterize the optimal level and design of housing assistance, I develop a sufficient statistics approach analogous to but distinct from unemployment insurance (Baily, 1978; Chetty, 2006). The optimal benefit equates the standard fiscal externality to three marginal welfare gains: an inframarginal consumption smoothing gain, present in any transfer program; an extensive-margin housing gain from keeping budget-constrained households housed who would otherwise be evicted; and a housing continuity gain from avoiding the fixed costs of re-entering housing in a subsequent period. The extensive margin gain is absent from the UI setting, since marginal households are indifferent between unemployment and employment and the envelope theorem applies. The housing continuity gain also does not appear in the UI setting but is analogous to a job-separation cost in the context of short-term work subsidies (Giupponi and Landais, 2023).

The framework illustrates two distortions that emerge under a multi-period benefit. A *windfall distortion* arises because households with transitory shocks recover above the rent threshold in the second period remain in the recipient pool, diluting the average value of assistance and pushing the optimal benefit below what a planner reaching only persistent-shock households would set. A *retention distortion* arises because households with persistent shocks who have access to an affordable outside housing option are subsidized to remain in units they cannot sustain without ongoing assistance. I show that the latter distortion can be resolved by a menu policy that offers a moving transfer alongside the standard benefit by inducing self-selection. A menu also reduces the windfall distortion by separating the recipient pool into movers, who face no windfall, and stayers, among whom the distortion persists. In ongoing work, I aim to use the experimental estimates to identify these sufficient statistics and evaluate the optimality conditions quantitatively.

This paper contributes to several literatures. A growing literature on eviction, including my prior work, shows the negative impacts of eviction on adults and children (Desmond, 2012; Collinson et al., 2024b, 2025). Recent interventions have largely focused on courtroom interventions, including legal aid and nudges to appear in court (Cassidy and Currie, 2023; Caspi and Rafkin, 2023; Abramson, 2024; Abramson and Nieuwerburgh, 2024). Intervening at the courtroom stage is practical since it is the first public record of the household being at risk of losing housing, but the timing may be too late, since the landlord-tenant relationship has deteriorated by the courtroom (Rafkin and Soltas, 2025) and the filing itself may affect downstream outcomes. This paper is the first to study a large-scale government program for those with an eviction notice, a population that is challenging to observe. Using original surveys and linked administrative data, I am able to characterize the sources of distress that precede eviction, which are critical for designing a safety net for the tenuously housed.

This paper also contributes to the literature on optimal social insurance. The sufficient statistics approach

has been applied to a range of social insurance programs including unemployment insurance (Baily, 1978; Gruber, 1997; Chetty, 2006; Kolsrud et al., 2018), disability insurance (Diamond and Sheshinski, 1995; Haller et al., 2024), health insurance (Finkelstein et al., 2019), food assistance (Finkelstein and Notowidigdo, 2019; Anders and Rafkin, 2026), legal aid (Collinson et al., 2024a), and bankruptcy (Dávila, 2020; Gross et al., 2021), but not to housing assistance. Perhaps the closest parallel is short-time work, which subsidizes firms to reduce hours rather than lay off workers during a downturn (Giupponi et al., 2022; Giupponi and Landais, 2023). In the housing context, the assistance program subsidizes the currently housed to retain housing, which generates a standard consumption smoothing benefit, but it has additional gains because it prevents costly transitions and downstream effects such as homelessness. As in the short-time work setting, housing assistance faces the challenge that the underlying shock may be transitory or persistent, with different implications for whether the program prevents or merely delays the negative outcome. What is distinct in the housing setting is that households on the margin of eviction are not indifferent between states and strictly prefer housing, which generates a positive extensive-margin welfare gain absent from both the UI and short-term work frameworks.

A growing literature studies homelessness, which is rare and difficult to predict (Wachter et al., 2019; Meyer et al., 2024). The literature has largely focused on homelessness remediation rather than prevention, including the Family Options Study (Gubits et al., 2016). Recent work finds that one-time assistance is effective at reducing shelter entry (Evans et al., 2016; Phillips and Sullivan, 2023), although the programs they study impose somewhat restrictive eligibility criteria, such as demonstrating an ability to pay future rent, or demonstrating an inability to pay future rent and being classified as intermediate risk of homelessness. One contribution of this paper is to study a broader population with an eviction notice, which allows me to characterize the impact of assistance across the range of disadvantage and to provide new evidence on the design of the safety net to prevent rather than delay homelessness. An additional strength of this study is to incorporate an original survey to measure unsheltered homelessness, which is rarely observed by researchers. I also contribute by connecting the homelessness prevention and remediation literature to the social insurance framework.

This paper also contributes to the broader literature on the growing crisis of housing affordability. Economists have pointed to housing supply constraints as a source of rising housing costs and have largely recommended policies that ease supply-side barriers, such as zoning and regulatory constraints, as effective tools to address the challenge (Glaeser and Gyourko, 2018; Abramson and Landvoigt, 2025). But housing construction takes time, and it is critical to study the safety net for those at risk of losing housing in the face of rising rents, which is the focus here. A related literature documents the benefits, particularly for children, of moving to high-opportunity neighborhoods (Chetty et al., 2016; Bergman et al., 2024) and the benefits of neighborhood revitalization (Chetty et al., 2026). Despite the promise of these programs, they are challenging to scale. This paper complements this literature by considering the design of a safety net that prevents downward moves—from housing into homelessness.

2 Economic Framework

This section presents a simple model for analyzing the effects of policies to provide a safety net for the housing unstable. The model is useful for studying the welfare consequences of alternative policies and for providing sufficient statistics for the welfare effects of policy changes.

2.1 Environment

The economy has two periods. Market rent is $\bar{r}$ and is assumed constant, so the analysis is partial equilibrium. Each household draws period 1 income y from a distribution F on support $[\underline{y}, \infty)$ and is privately informed about its type along two dimensions. First, income draws are either *transitory* or *persistent*: transitory types (share p) draw period 2 income y_2 from F truncated to $[\bar{r}, \infty)$, while persistent types receive the same y in period 2. Second, a fraction π of households have access to an *outside housing option* at rent $r_0 = \underline{y}$, the income-support lower bound, which they can relocate to at transition cost $m > 0$; outside-option type is assumed independent of whether the household's shock is transitory or persistent, reflecting that some households have friends or family they could move in with. The four types are TN (transitory, no option), TO (transitory, option), PN (persistent, no option), and PO (persistent, option), with shares $p(1-\pi)$, $p\pi$, $(1-p)(1-\pi)$, and $(1-p)\pi$, respectively. Population shares are known to the planner, while individual types are private information. Households with income $y < \bar{r}$ lose housing without assistance, since they cannot afford market rent on their own.

There is a single consumption good and households have period utility $v(c)$, which is strictly increasing and concave. Households are hand-to-mouth, so consumption in each period equals the income that remains after paying rent net of taxes and benefits. Losing housing imposes a consumption-equivalent cost $d > 0$, which reflects the downstream consequences such as homelessness; an evicted household consumes $y - d$. I assume there is a fixed transition cost m, which is incurred when households voluntarily change units, or when an evicted household transitions back to housing.

Timing and decisions. The model timing is that in each period households first receive income, pay taxes, and receive transfers, and then their housing and consumption outcomes are realized. Consumption is income net of payments at the end of each period, or if they are evicted, consume $y - d$. The only household decision is whether to move or not, which occurs once at the beginning of period 2 after net income is received. I assume $m > \bar{r} - \underline{y}$ so that no household voluntarily moves without assistance, reflecting strong attachment to one's housing or fixed costs from relocating.[1]

Government. The government decides two policy parameters: an income eligibility threshold $\hat{y}$ and a per-period benefit b_t to households who have income below the threshold. The government finances the benefit with a head tax on households with housing who have income above the threshold. The government sets τ_t to balance the budget in each period,

$$\tau_t\big[1 - F(\hat{y})\big] = b_t\big[F(\hat{y}) - F(\tilde{y}(b_t))\big], \tag{1}$$

where $\tilde{y}(b_t) \equiv \bar{r} - b_t$ is the *affordability threshold* below which a household is evicted. The government is also subject to a feasibility constraint $\hat{y} \geq \bar{r} + \tau_t$, which means no taxpayer is taxed into negative consumption after paying rent. A higher benefit lowers $\tilde{y}$, expanding coverage to households that would otherwise lose housing—an *extensive margin* without a direct analog in the standard unemployment insurance setting. In an extension discussed below, I allow the government to transfer an amount μ as a moving subsidy under verifiable relocation.

[1] Note that there is no moral hazard in the framework above, in order to emphasize the new welfare terms in the housing setting relative to UI, but housing search could be added, in which more generous benefits would introduce moral hazard in housing search.

2.2 Planner Problem: Single-Period Benefit

As a benchmark, consider the optimal single-period benefit, so the planner chooses $(b_1, \hat{y})$ to maximize two-period household welfare and let $b_2 = \mu = 0$. The following equation equating the marginal welfare gain to the fiscal externality (Appendix):

$$\underbrace{1}_{\substack{\text{(i) inframarginal}\\ \text{consumption smoothing}}} + \underbrace{\frac{[v(0) - v(\tilde{y} - d)]\, f(\tilde{y})}{[F(\hat{y}) - F(\tilde{y})]\, [v'(c_H^-)]}}_{\substack{\text{(ii) extensive-margin}\\ \text{housing gain}}} + \underbrace{\frac{p\, f(\tilde{y})\, [v(y_2 - \bar{r}) - v(y_2 - \bar{r} - m)]}{[F(\hat{y}) - F(\tilde{y})]\, [v'(c_H^-)]}}_{\substack{\text{(iii) housing}\\ \text{continuity gain}}} = \underbrace{\frac{d\tau_1}{db_1} \cdot \frac{[v'(c_H^+)]}{[v'(c_H^-)]}}_{\substack{\text{(iv) fiscal externality}}} . \tag{2}$$

Term (i) is the normalized inframarginal gain from raising consumption of existing recipients, present in any transfer program and equivalent to the consumption-smoothing term in the Baily–Chetty sufficient-statistics formula for UI. Terms (ii) and (iii) are specific to housing and have no analog in UI.

Term (ii) is the *extensive-margin housing gain*: at the margin $\tilde{y}$, a higher benefit keeps $f(\tilde{y})$ additional households housed who would otherwise be evicted, producing a discrete welfare jump $v(0) - v(\tilde{y} - d) > 0$. In UI the insured event is a reduction in a flow income, not the loss of a durable good, and no cost d analogous to losing housing. The extensive-margin term is absent from the Baily–Chetty formula because the marginal worker is assumed indifferent between being unemployed and receiving benefits, and being employed. In the housing context, the marginally unhoused are not indifferent between being unhoused and being housed.[2]

Term (iii) is the *housing continuity gain*: households kept housed in period 1 avoid paying transition cost m to re-enter housing in period 2, saving $[v(y_2 - \bar{r}) - v(y_2 - \bar{r} - m)] > 0$ in expectation over recovered income for transitory types. This term reflects the fixed cost of moving—analogous to the job-separation cost in Giupponi and Landais (2023).

Term (iv) is the *fiscal externality*: raising b_1 requires raising τ_1, reducing consumption of all $1 - F(\hat{y})$ taxpayers. The term $d\tau_1/db_1$ captures the mechanical fiscal cost—the per-recipient cost to the existing caseload plus a pool-expansion cost from newly housed households—and the second term converts it into welfare units relative to recipients. This term parallels the fiscal externality in Finkelstein and Notowidigdo (2019), where expanding program take-up imposes a cost on the government from paying benefits to marginal enrollees. By concavity of the utility function, taxpayers have higher income than recipients—so the fiscal externality is always smaller in welfare terms than the gains on the left-hand side, confirming that all four components have a strictly positive net welfare gain from the first dollar of assistance.

Proposition 1 *The first dollar of housing assistance strictly raises welfare:* $dW/db_1|_{b_1=0} > 0$.

The result follows the standard argument that at $b_1 = 0$ the marginal recipient has strictly higher marginal utility than the marginal taxpayer, so the inframarginal redistribution gain exceeds the tax burden. The terms new to the housing context, (ii) and (iii), add further welfare gains. The optimal eligibility threshold $\hat{y}^*$ equates the welfare gain to the marginal household from entering the recipient pool against the per-taxpayer cost of the required tax increase, with the feasibility constraint $\hat{y} \geq \bar{r} + \tau_1$ ensuring $\hat{y}^* > \bar{r}$.

[2] Another distinction is that the benefit is paid conditional on retaining housing — only households who remain housed below the eligibility threshold receive assistance, whereas UI compensates workers after job loss has already occurred. The housing setting is in this respect closer to the short-time work programs studied by Giupponi and Landais (2023), where subsidies are paid to firms conditional on retaining workers rather than to workers after separation. This conditionality is also what generates the extensive-margin term in the decomposition: the relevant margin is *homelessness prevention* rather than *homelessness remediation*, and the benefit creates an affordability threshold $\tilde{y}$ below which households lose housing rather than a replacement-rate schedule above which households accept being unhoused. An alternative design would subsidize the unhoused directly, targeting the re-entry margin instead; I consider this in the Appendix.

Note that the program with a one-period benefit has fade-out in that some participants who are spared eviction in period 1 are evicted in period 2: when $b_2 = 0$ (i.e., the benefit period is short relative to the length of the shock for the persistent group), the share of period 1 participants who face eviction by period 2 equals the combined share of PN types and PO types who do not self-finance a move. For these subgroups of households, the benefit *delays* rather than *prevents* homelessness. I discuss below how to empirically estimate the persistence share $1 - p$ based on the fade-out effect of the program, and in the next subsection I discuss how two-period policies mitigate fade out of program effects.

2.3 Two-Period Benefits and Move Distortions

Suppose the planner chooses $(b_1, b_2, \hat{y})$ freely with no moving subsidy (i.e., $\mu = 0$). By choosing $b_2 > 0$, the government has the possibility of keeping both transitory and persistent types housed in both periods. But I show the two-period policy also introduces two distortions: first, providing transitory households extended support after they are able to pay their rent without assistance; and second, inefficiently reducing moves by the persistent group with an outside option. I show that it is optimal to set $b_2^* > 0$, but I also show these two distortions arise (Appendix) through the composition of the period 2 recipient pool.

I call the first distortion a *windfall distortion*. Transitory types recover to $y_2 \geq \bar{r}$ in period 2 and are housed regardless of b_2; those who draw $y_2 \in [\bar{r}, \hat{y})$ remain in the recipient pool and receive b_2 as a pure consumption transfer. Their presence dilutes the average shadow value of the benefit—the pool mixes households for whom b_2 determines housing status with households for whom it provides only consumption insurance—pushing b_2^* below the level a planner targeting only persistent types would set.

The second is a *retention distortion*. Among persistent types, b_2 keeps both PN and PO types housed. But for PO types, affordable housing exists outside their current unit; they remain in units they cannot sustain without ongoing assistance when a one-time moving transfer would instead place them in affordable housing at lower long-run fiscal cost. The benefit must be provided in two periods to achieve the same housing outcome a single relocation transfer would have achieved. This distortion does not appear in the FOC for b_2, but it does, however, inflate the fiscal cost on the right-hand side of the FOC for $\hat{y}$: every PO type below $\hat{y}$ draws b_2 each period when a one-time moving transfer would have sufficed, so the marginal cost of expanding eligibility is higher than it would be if PO types were provided a moving subsidy.

The windfall distortion operates through the composition of the period 2 recipient pool. In period 1, every recipient has income $y < \bar{r}$ and high marginal utility. In period 2, the recipient pool mixes two groups: persistent types who still have the same low income $y < \bar{r}$ (marginal utility unchanged relative to period 1) and recovered transitory types who draw $y_2 \in [\bar{r}, \hat{y})$ and have strictly lower marginal utility since $y_2 \geq \bar{r} > y_1$. This compositional shift reduces the consumption-smoothing gain per dollar on the left-hand side of the period 2 FOC relative to period 1, pushing $b_2^* < b_1^*$. The magnitude of this decline is increasing in the transitory share p: a larger share of recovered transitory types in the period 2 pool means a larger compositional gap in average marginal utility, and a larger fall in the optimal benefit. The share p is identified from the experimental fade-out rate. The retention distortion, by contrast, does not directly shift the optimal benefit level, but it increases the marginal cost of expanding eligibility and motivates the menu policy of Section 2.4.

2.4 Menu Policy

The retention distortion motivates a menu policy that induces PO types to self-select into relocating to the available outside option. The planner offers a common period 1 benefit b_1, and a choice between a *stay* option paying benefit b_S in period 2, and a *move* option paying mover benefit b_M plus a one-time moving transfer μ, paid conditional on verified relocation.[3] Verification is the key enforcement mechanism: PO and TO types can satisfy the relocation condition because an affordable outside option exists and a physical move is observable; PN and TN types cannot satisfy it because no affordable alternative exists, making mimicry infeasible.

The planner's goal is to have PO types choose move and all other types choose stay. It is crucial that the planner does not oversubsidize relocation among the other types, which would introduce a new distortion. TN and PN cannot move and always choose to stay. TO types recover to $y_2 \geq \bar{r}$ regardless and prefer stay so long as μ is not too generous relative to b_S. PO types prefer move when the net transfer $b_M+\mu-m$ sufficiently exceeds the consumption value of staying. These two incentive constraints—PO types choose move, TO types do not—impose identical bounds on μ: Given (b_S, b_M), the incentive compatibility constraints jointly pin the moving transfer to a unique value $\mu^* = m - (\bar{r} - \underline{y}) - (b_M - b_S)$. The planner's effective instrument is the total mover transfer $TM \equiv b_M + \mu^* = m - (\bar{r} - \underline{y}) + b_S$.

The result, proved in the Appendix, is that b_M and μ are perfect substitutes: what matters is the total transfer to movers TM and the stayer benefit b_S. Raising b_S relaxes the TO no-move constraint, creating room for a proportionally larger mover transfer while keeping TO types in place.

2.5 Connecting to the Experimental Evidence

The welfare decomposition in equation (2) organizes what the experiment identifies and what requires calibration. Each of the three benefit terms maps onto a distinct empirical object. The *extensive-margin housing gain* (term ii) is the most directly identified. The six-month treatment effect on housing loss measures the share of assisted households who would have been evicted without the program but are kept housed with it—precisely the $f(\tilde{y})$ households at the affordability margin whose welfare jumps by $v(0) - v(\tilde{y} - d)$. This is the primary object of interest in the empirical analysis and the term that distinguishes housing assistance from a pure consumption transfer.

The *persistence share* $1 - p$ is identified from the fade-out of treatment effects: the ratio of one-year to six-month treatment effects on housing loss provides a lower bound on $1-p$, since households with transitory shocks recover to $y_2 \geq \bar{r}$ and no longer need assistance by period 2. The persistence share governs both the magnitude of the windfall distortion—a larger p means more recovered transitory types in the period-2 recipient pool—and the size of the optimal benefit decline $b_2^* < b_1^*$.

The *housing continuity gain* (term iii) is identified from the difference between the extensive-margin effect and the fade-out. Households kept housed in period 1 who have transitory shocks avoid paying transition cost m to re-enter housing in period 2; the treatment effect decay between six months and one year captures the share of households for whom the period-1 benefit provided continuity value beyond the immediate housing spell.

The *consumption smoothing gain* (term i) and the *fiscal externality* (term iv) require calibration from the income distribution F and the benefit and tax levels (b_t, τ_t). The ratio $E^+[v']/E^-[v']$ can be approximated

[3]The period 1 benefit b_1 is common across contracts because the move or stay decision occurs after b_1 is paid, and all four types have the same observable income $y < \bar{r}$ in period 1, meaning contracting on future choice is unlikely to be enforceable.

from the income gap between recipients and taxpayers near $\hat{y}$, combined with an assumed curvature of v.

Together, these objects allow the sufficient-statistics formula (2) to be evaluated empirically: whether the first dollar of housing assistance raises welfare is confirmed by Proposition 1, but the quantitative magnitude of each term and the optimal benefit level b_1^* depend on the empirical values of the EM gain, the persistence share, the continuity gain, and the fiscal externality.

3 Background

3.1 The Eviction Process

The eviction process begins when the landlord serves their tenant with a formal eviction notice. The notice initiates either a 10-day, 14-day, or 30-day period where the tenant must pay their arrears or vacate the unit. This period varies across jurisdictions but systematic reviews have found it ranges between a few days and a month (Legal Services Council). An official notice is required by law but is not recorded in public records, which creates a challenge for policymakers seeking to provide assistance prior to the legal system's involvement: there is no systematic way to identify these at-risk households.

If the tenant and landlord do not resolve the dispute, the landlord may proceed to file an eviction case with the court, at which point tenant receives a summons to appear in court. After a hearing, the court may issue an eviction order granting the landlord possession of the unit and a money judgment for unpaid rent, or the court may dismiss the case. If the case ends in an eviction judgment—which is also referred to as an "order for possession" or a "writ of restitution"—the landlord may file the order with the Sheriff's Office, which enforces the eviction by executing the lockout.

A distinguishing feature of eviction relative to foreclosure—the analogous legal process for homeowners—is the speed of evictions. The period between notice and enforcement can be as brief as a few weeks, leaving households with little time to arrange alternative housing or apply for assistance, while the foreclosure process is typically upwards of one year and sometimes two years (Herkenhoff and Ohanian, 2019). Once a case is filed, the eviction record becomes public. Since these records are collected by credit bureaus and screening companies, they may affect a tenant's ability to obtain housing in the future. Intervening prior to the court filing stage is therefore important both for preventing the immediate loss of housing and for avoiding the downstream consequences of having an eviction filing or judgment.

3.2 Government Program

This study is a partnership with a government program that is operated by the Department of Human Services (DHS) in a large urban county with a population of approximately 928,000. The county has high housing costs and rent burden: as of the 2021–2024 American Community Survey, approximately 20% of renters pay more than 50% of household income on rent, a rate slightly below high-cost urban counties such as Manhattan (23%) or Los Angeles County (27%). Approximately 2.5–2.9% of renter households in the county have an eviction filing per year, which is below the national estimate of close to 7% of renter households by (Gromis et al., 2022). The county operates in a relatively tenant-friendly legislative environment.

By partnering with a government program, this study is able to reach renter households with an eviction notice. The program is well-known and is advertised through the DHS's website, and the DHS has relationships with legal aid providers, housing coalitions, and other social service organizations that publicize the program. To provide a sense of the program's coverage and reach, Figure 1 depicts the number of monthly applications

and the number of eviction filings. Like other studies in this setting, I do not observe the total number of households with an eviction notice. Nevertheless, Figure 1 shows that the program reaches a high percentage of households who will eventually have an eviction filing. The average census tract poverty rate of application addresses is 13.1, whereas the tract poverty rate of eviction case addresses is 13.8. The slightly lower poverty rate of applications relative to cases may reflect that more disadvantaged tenants are less likely to know about or apply to the program. It may also reflect that conditional on an eviction notice, more disadvantaged tenants are less likely to pay or vacate, and are subsequently more likely to receive a court filing.

As part of the Covid-19 pandemic federal relief effort, the DHS was in charge of making emergency rental assistance payments totaling $69.8 million. After the pandemic, the county council allocated a significantly scaled-down budget for rental assistance and case management services and asked me to develop a formal evaluation to improve program design and services given their limited funds. During the study enrollment period of February 2024 to December 2025, the safety net program had $13 million for emergency rental assistance to renter households facing imminent eviction.

4 Study Design

4.1 Enrollment and eligibility

The program accepts applications through an online portal and conducts a monthly, randomized lottery for services. The online portal clearly states that assistance is conducted via a fair lottery and lists the date of the next lottery. At application, households consent to their data being studied for research purposes in the program terms and conditions.

To be eligible, households must reside in the county, hold a lease in their own name, be between one and three months behind on rent, have received a formal eviction notice, and have household income below 80% of the Area Median Income. Households in subsidized units such as Section 8 or Permanent Supportive Housing are not eligible. Ineligible households receive no assistance and are excluded from the lottery and the analysis.

Eligible applicants are classified into one of six priority groups based on criteria established by the program administrator, including the immediacy of the eviction risk, the presence of children or elderly household members, disability status, domestic violence or sexual assault survivor status, prior eviction history, and the number of units owned by the landlord. Individuals from all priority groups are randomized into treatment and control, and higher priority groups are randomized to treatment at higher rates.[4] All analyses include application month-by-priority group fixed effects. See Appendix C for additional details on the criteria.[5] In practice, the vast majority of applicants are in priority group 6, which are households with a 14-day or 30-day eviction notice A1. A strength of this study is the broad eligibility criteria, which departs from prior work on emergency assistance, and being able to identify treatment effects across the distribution of households with an eviction notice.

[4]The government agency targets were to assign 20% of total service seats each month to priority groups 1, 2, 3, and 5 and 10% of total service seats assigned to priority groups 4 and 6. When priority groups received fewer than a threshold number of applicants (10), we randomly assigned a fixed proportion to treatment.

[5]Prior to this study, eligibility for program services was based on the same criteria, and the priority groups were also identical. One difference is offers for services were first allocated to households from higher priority groups, which were considered more vulnerable, before lower priority groups.

4.2 Intervention and Randomization

Eligible households are randomized into either of two treatment arms or a control group. One treatment arm is rental assistance-only, which pays up to 3 months of arrears, the current month's rent, and up to 3 months of future rent. When households are assigned to treatment the landlord is paid rent directly by the government agency. A second treatment adds an additional 6 months of case management. Case managers assist households with budgeting, job search, and navigating available public assistance programs. See the Appendix for the full scope of case management services. The control group does not receive any direct assistance and is not eligible to reapply for assistance at the same address for six months, effectively meaning they must resolve their housing crisis on their own. The program administrator provides information to those who are denied services about available alternative services in the area, and our control group receives this information. Control households are randomly assigned a within-priority group waitlist rank. The government agency uses the waitlist rank to replace treatment households who do not take up services with another household from the same priority group and randomization month, to exhaust all available capacity.

The number of households served each month is based on service provider capacity constraints and on the aim of spending down funds smoothly over the budget period determined by the county government. During the enrollment period of February 2024 to December 2025, 4,381 households have been enrolled, with 27% assigned to rental assistance only, 11% assigned to rental assistance plus case management, and the remainder to control group. Randomization occurs monthly, at the household level, and is stratified by priority group.[6]

5 Data Sources

Program Data

Household demographic data is collected during the application process. The application form collects information on household structure, demographics, rental unit characteristics, and income. Personally identifying information (PII) is recorded for each individual residing in the unit 50 percent of more of the year, enabling each member of the household to be linked to other datasets by name, date of birth, social security number, race, gender, residential address. Additionally, property owner name, email, telephone number, and address are reported by the tenant.

As part of the research study, the government agency has allowed me to add additional questions to the intake form. These questions are aimed at understanding the typical sources of distress that precede the eviction notice, and to elicit information about household expectations about the duration of the distress period and being able to pay their rent in full without assistance in future months. Additional questions ask about where the tenant would likely move if required to vacate, and whether they would prefer to use assistance funds at their current unit or at an alternative unit. See Appendix D for these added intake questions.

Treatment assignment take-up and duration is measured through government agency records, which include household case number, application date, priority group, case status, amount of rental assistance paid,

[6]The power calculations for adult tenants in the household specified enrolling households for 20 months, or until we have enrolled 3,600 households, with approximately 510 assigned to each treatment arm and the remaining households to the control group. For the analysis of children's outcomes, a separate pre-analysis plan specifies a 30-month enrollment period and we are continuing to enroll and randomize households for this analysis.

amount of utility assistance paid, and payment recipient information. Payment records provide additional landlord PII, including legal property owner name and EIN which can be used to link to landlord outcomes.

Eviction

Court records are public records maintained by the county in which the program is located. Electronic case journal entries and PDF documents include defendant name(s), plaintiff name(s), residential address of the unit in question, date of case, the amount being sought by the landlord, eviction filings, and eviction judgments. We link program and random assignment information to eviction records by name and address.[7] The primary outcomes constructed using this data are (1) an eviction case filing and (2) an eviction judgment (or writ of restitution).

Homelessness

The program county's Homelessness Management Information System (HMIS) collects administrative data on use of the county's continuum of care. This data includes stays in overnight emergency shelters and other homelessness services, including diversion, transitional housing, permanent supportive housing, rapid re-housing, warming centers, etc. I follow **?** and construct two outcomes: (i) emergency shelter stay and (ii) any use of homelessness services.

Household Follow-up Survey

In addition to the data sources described above, I develop a follow-up survey to measure tenant's housing situation since applying to the program, their current housing situation, and their mental health. The program county electronically distributes this survey to the primary applicant in each household using the same email address used at application, and we also send notifications about the survey via text message. The survey is conducted monthly, for each enrollment cohort, 10 months after randomization. The survey is therefore 7 months after the assistance period is over for the treatment group. We compensate tenants for completing the survey.

The survey includes questions assessing housing stability, persistence of financial distress, food insecurity, investment in children, social ties, and mental health. See Appendix E for the complete survey questionnaire and logic.

Survey Incentive Randomization. Because of concerns about potential bias from selective non-response, we randomly assign half of households to be compensated with a high incentive amount and the other half with a low incentive amount. Households receive information about the incentive amount in the survey invitation, which specifies the delivery of a gift card upon completion. The survey randomization follows Dutz et al. (2025) and the goal is to generate variation in response rates that allow us to detect potential selection bias and to implement a correction when analyzing survey outcomes.

[7]Note that because tenant name and address are the only PII available in the eviction court records, I am unable to study evictions at addresses other than the one at application.

5.1 Summary Statistics

Table 1 presents summary statistics of the analysis sample at application. Applicants are predominantly female (71%) and 58% of them have children in the household. The modal household at risk of losing housing is a single mother with children, and the demographic profile is quite similar to those facing eviction in Chicago and New York (Collinson et al., 2025). A high share—26%—report having experienced domestic violence or being a sexual assault survivor.

A novel finding, observed here because rent and income are collected by the county at application, is that these households are highly rent burdened: average monthly rent is $1,712 and average monthly household income is $1,945, meaning that at intake—a single point in time—households are on average spending 88% of income on rent and leaving little room for any other expense. The household income measure is verified by the program to meet the income eligibility criterion using pay stubs or an attestation to having no income. Because many households also report in the intake survey, described below, having lost a job or experiencing reduced hours, the rent burden at application is likely higher than their rent burden if it were averaged over a longer period.[8] Households are an average of 2.07 months behind on rent at the time of application. Only 10% self-report having any prior eviction history. Using the administrative HMIS data, which goes back to 2015—8 years prior to the study—26% have had an interaction with the homelessness system and 8% have ever used an emergency shelter. Hence, for the majority of households, when they apply for assistance it is likely their first time at risk of losing housing or having contact with the homelessness system.

Table 1 reports results from the baseline survey on the nature of the distress precipitating the eviction notice. The most common reasons for falling behind on rent are job loss or reduced hours (65%) and financial hardship due to unexpected expenses, which include auto, health, or childcare expenses (68%). A strikingly high share report a death in the family (14%), a similar percentage to partner separation (16%), rent increase (15%), or loss of benefits (14%).

Perhaps surprisingly, only 25% of households at application expect the distress to persist 3 months or more. Similarly, when asked if the household expects to pay rent in full without assistance in 1 month, 3 months, 6 months, or 1 year, and respondents can only select one, 30% report 1 month, and 42% select 3 months. These findings suggest that the duration of assistance of the program is well-suited for the majority of households. But it may also reflect that households are overly optimistic about their ability to rebound from the negative shock.

Lastly, respondents are asked where they would stay if required to vacate their unit, and they may select only one response. A high percentage (62%) say they do not know, a high share (12%) say they would stay in a homeless shelter, and a strikingly high share (17%) say they would stay outdoors, or in a place not meant for habitation, including a car, a tent, etc. A surprisingly low share (5%) say they would stay with friends or family, which echoes the low percentage who respond affirmatively to an earlier question of being able to rely on friends or family for a temporary place to stay. The broad takeaway is that households at risk of eviction have limited informal support and a high risk of homelessness, despite having limited prior contact with the homelessness system.

[8]The linked administrative earnings data will supplement this income measure with earnings and hours measured longitudinally.

6 Empirical Analysis

The empirical analysis includes one member per household: the household head or the primary applicant on the application form. I report the intent-to-treat (ITT), the causal effect of a tenant i in household h being offered rental assistance, or rental assistance plus case management, relative to the control group that receives no assistance, using the following regression:

$$y_{ih} = \alpha + \delta T_h + \gamma X_{ih} + \theta_{gt} + \epsilon_{ih} \tag{3}$$

where y_{ih} is the outcome of interest, T_h is an indicator for the household receiving either the rental assistance only or the rental assistance plus case management treatment, X_{ih} represents baseline demographics, and θ_{gh} represents priority group-by-calendar month fixed effects. The controls include demographics measured at intake: race, gender, age, marital status, number of adults in household, number of children in household, and lagged dependent variable, e.g. average quarterly earnings or HMIS system contact in the two years prior to application. I also estimate a modified version of equation 3 with separate indicators for being assigned to the rental assistance only treatment, T_{1h}, and being assigned to rental assistance plus case management, T_{2h}. Since the program uses a randomized waitlist, we will drop the last applicant getting an offer (de Chaisemartin and Behaghel, 2020).

I will also estimate the local average treatment effect (LATE) of receiving assistance using the following two-stage least squares regression where the first stage is:

$$S_{ih} = \pi_0 + \pi_1 T_h + \pi_2 X_{ih} + \theta_{gt} + \nu_{ih} \tag{4}$$

and the second stage is:

$$y_{ih} = \rho_0 + \rho_1 \hat{S}_{ih} + \rho_2 X_{ih} + \theta_{gt} + \eta_{ih} \tag{5}$$

where S_{ih} is an indicator for individual i in household h being randomized into either treatment and also taking up the housing stabilization services.

Subgroups I pre-specify two key subgroups for heterogeneity analysis, constructed using application and intake survey data. The first divides the sample into tenants experiencing transitory versus persistent strain, and the second divides the sample into tenants who are able to rely on friends or family members for a place to stay, and those who are not.

One hypothesis that I aim to evaluate is whether tenants experiencing transitory distress are more likely to have homelessness averted by the program, while tenants experiencing more permanent distress may experience homelessness delayed by the program. In an update of this draft, I will leverage the linked administrative earnings data and application survey to classify tenants into transitory v. persistent strain, following the pre-analysis plan. As of this writing, I use the baseline survey questions asking tenants about the sources of distress precipitating the eviction notice, including unanticipated losses of income and unexpected costs, and whether they expect these circumstances to continue (see Appendix D for the intake question language). I classify tenants as experiencing transitory distress based on the following questions about beliefs:

- "Yes" to "Is the loss of income or increase in expenses that caused you to fall behind on rent likely to continue"

- On the question "Do you expect to be able to pay your current monthly rent without additional assistance in", an indicator for the respondent not selecting 1 month, 3 months, or 6 months.

The second key subgroup is motivated by my prior research suggesting that family ties are important for mitigating the effects of eviction, at least for children (Collinson et al., 2025). The hypothesis is that tenants who have access to friends and family for support will be less likely to become homeless if they do not receive assistance, and they may benefit more from the program. I construct an index of family support based on the following intake survey question items:

- Selecting "Friend's place" or "Family member's place" as an alternative place to stay if required to immediately vacate
- Answering "yes" to "Do you have any family members or friends in [program county] or [neighboring county] you could rely on for a temporary place to stay, if necessary?"
- The total of "How many family members have you been in contact with in the last six months that live in [program county] or [neighboring county]?" and "How many friends have you been in contact with in the last six months that live in [program county] or [neighboring county]?"

For each of the three items above, I standardize each item, take the sum, and re-standardize the index. I then classify tenants as having above v. below median access to family support. I validate the family and friend network support classification using control group data, and study whether those with below median support networks have lower earnings and a higher likelihood of experiencing homelessness in the 1-2 years after application compared to those with above median support networks.

I explore alternative approaches to classifying tenants into these subgroups, including a machine learning approach (Wager and Athey, 2018; Chernozhukov et al., 2024) to use the intake questionnaire to predict treatment effect heterogeneity, which may reveal additional insights about future targeting of subgroups. Haushofer et al. (2022) also provides guidance on classification for targeting of interventions, and the tradeoff between targeting for deprivation v. impacts.

The subgroup analysis will test two hypotheses: (1) whether the impact of the intervention differs significantly for the subgroup experiencing transitory strain compared to the impact of the intervention on the subgroup experiencing persistent strain; (2) whether the impact of the intervention differs significantly for the subgroup with access to friends or family for support compared to those without access to such support.

7 The Effects of the Safety Net on Tenants

7.1 First Stage

Table A1 presents the first stage. Assignment to treatment increases the probability of receiving any service by 83.5 percentage points, and the control group take-up rate is 6%, reflecting a strong and highly significant first stage. That the control group is not zero likely reflects reapplications that result in assignment to treatment, or those on the waitlist who are later assigned to treatment.[9]

[9]In the housing context, a first stage of 83.5 percent is notably high. Jacob and Ludwig's (2012) study of Chicago's housing vouchers, for example—a continuing subsidy that is more generous than the one studied here—has a first stage of approximately 50%. This difference likely reflects the relative speed in this setting between application, random assignment, and payments made by the program, which limits attrition due to relocation or households finding alternative housing.

7.2 Eviction

Table 2 presents estimates of the impact of emergency rental assistance on eviction filings and judgments. The results show the program is highly effective at preventing eviction filings: assignment to treatment reduces the probability of an eviction filing within six months by 9.2 percentage points, or approximately 48% relative to the control group mean of 0.19, and significant at the 1 percent level. Receiving services reduces the likelihood of an eviction filing by 11.8 percentage points (column 3), a 62% reduction relative to control. Column (4) shows that rental assistance alone reduces filings by 12.2 percentage points, while rental assistance plus case management reduces filings by 9.8 percentage points, with confidence intervals that do not allow us to reject equality of the two treatment effects.

Turning to eviction judgments, Table 2 shows that assignment to treatment reduces eviction judgments by 6.1 percentage points, a 76 percentage point reduction. Column 3 shows that the impact of receiving services is 8.2 percentage points, in statistical terms a 100% reduction relative to the control group. It is worth emphasizing that the program's negative impact on eviction filings and judgments is precisely its goal: to reach households prior to the legal system, and to reduce their likelihood of having public eviction records, the latter of which has well-established negative consequences.

The event study in Figure 2 shows the validity of the parallel trends assumption in the pre-treatment period and shows that the treatment and control groups diverge sharply after the lottery. The divergence is particularly pronounced for eviction filings, with the control group trajectory continuing to rise in the post-lottery period while the treatment group trajectory flattens considerably. The eviction filing and judgment variables naturally taper off as there is a natural upper bound of being evicted at most one time from their application address.

7.3 Homelessness

Tables A2 present estimates of the impact of emergency rental assistance on shelter entry and any contact with the homelessness system at three-, six-, and twelve-month horizons. At three months, treatment reduces shelter entry by 1.5 percentage points, representing an 83% reduction relative to the control mean of 0.018, and statistically significant at the 1 percent level. The effect is 1.9 percentage points at six months (a 61% reduction), and is no longer statistically significant at one year, although the point estimate represents a 18 percent reduction.

A similar pattern emerges for the variable indicating any contact with the homelessness system. The treatment reduces homelessness contact by 5.1 percentage points at three months (an 81 percent reduction), and 7.0 percentage points at six months (a 66% reduction), both of which are significant at the 1 percent level. The point estimate reduces to 3.0 percentage points at one year (significant at the 10 percent level), representing a 21 percent cumulative reduction in homelessness by year 1.

Note the growing homelessness rates for the control group over the study period: 1.8 percent use a shelter within 3 months, 3.1 percent use a shelter within 6 months, and 5.1 percent use a shelter within 1 year. This estimate contrasts with the 8 percent who use a shelter at any point in the 8 years prior to the study. The overall shares using homeless shelters are of comparable magnitudes to those with eviction filings in Chicago and New York (Collinson et al., 2024b). The broader homelessness contact measure shows similar patterns for the control group. One clear takeaway is that this program is reaching households at a time that is precisely before a period of distress and before many experience homelessness for the first time.

The event study in Figure 3 corroborates these patterns. Pre-treatment trends in shelter entry and

any homelessness contact are parallel across treatment and control groups (p-values of 0.721 and 0.952, respectively). Post-lottery, the treatment group exhibits substantially lower rates of both shelter entry and homelessness contact in the near term, with the gap narrowing at longer horizons. A somewhat troubling finding is that the treatment group has elevated rates of homelessness after the assistance period ends. This finding suggests that for some, the program assistance is delaying homelessness rather than preventing it. While the data has not arrived for the more recently enrolled cohorts, and these results may still change, this finding motivates the analysis that was laid out in the pre-analysis plan, of understanding better the nature of the distress being transitory or persistent, and understanding the program effects after the assistance period.

7.4 Follow-up Survey

Housing outcomes are difficult to fully characterize with administrative datasets, and therefore I conduct a follow-up survey at 10 months from randomization. The survey was sent to one member per household, the primary applicant. I use the survey to study the household's housing environment since randomization, to study the current living situation, and to measure mental health.

Panel A of Table 4 focuses on housing stability since applying for assistance. The survey asks if the tenant has stayed at a homeless shelter at any point since applying for assistance. The control group mean is 6.2 percent, and the intent-to-treat is -6.0 percentage points (significant at the 10 percent level). The control mean and the intent-to-treat are highly similar to those estimates using administrative data, which lends support to households answering the survey accurately and to the homelessness result. Moreover, while the administrative HMIS data is for the county, the survey is unrestricted geographically, lending further support to the negative impact on sheltered homelessness. Turning next to unsheltered homelessness, which is not captured in administrative data sources, I ask respondents if they have spent any nights, since application, in a place not meant for habitation, including sleeping in a car, a tent, outdoors, etc. The control mean of any nights unsheltered is 27.1 percent. The program reduces unsheltered homelessness by 18.2 percentage points, a 67 percent effect and significant effect at the 5 percent level. The number of places stayed for as little as one night is 2.2 for the control group, and the program reduces this by 0.93, an estimate that is statistically significant at the 5 percent level.

Turning next to the household's current housing situation, I ask tenants if they are living at the same address as application. The control group mean is 57.1 percent, and the treatment effect is 10.2 percent, although not statistically significant. This result shows that a substantial share of the control group is able to remain in the same housing as they were living at application, and suggestive evidence that receiving assistance increases the likelihood of households continuing to live at that residence. When asked whether they are currently unhoused—meaning that they report having no address, are staying in a motel/hotel, are staying outdoors, or are staying in a homeless shelter—2 percent of the control group replies affirmatively. This result suggests that the control group faces some homelessness, but the degree of homelessness has subsided somewhat in the months since peak distress, echoing the findings using the administrative data. I construct a measure of doubling up using the household roster that is part of the survey, and is equal to one if there is another adult in the household that is not a cohabiting partner or adult child. I further set this measure equal to 0 if the respondent reports, in response to a separate question, that they have a private space. Approximately 12.5 percent of control households are doubled up, and the treatment has no statistically significant effect.

The current rent of the control group is $1470 per month, $241 lower than the average at baseline of

$1712 per month. Conditional on paying a positive amount of rent, the control group pays $1583 per month, which is also lower than the baseline, reflecting to some extent that some share of control group households have moved into lower-rent units. The effect of being assigned to receive assistance is $278, and significant at the 5% level, suggesting that the program is keeping tenants housed in their original, higher-rent units. Note also that 6.2 percent of the control group reports paying 0 current rent—a number quite similar to the baseline share who report that they would move in with friends or family if required to vacate—and being assigned to treatment reduces this likelihood by 5.8 percentage points (significant at 10%), which is consistent with treated households being more likely to remain in formal rental housing.

Lastly, I include the PHQ-2 screening for depression and the GAD-2 screening for anxiety. The control group means are high for both of these measures: 44.9 percent screen positive for depression, and 55.5 percent screen positive for anxiety. Being assigned to treatment has a negative effect on both measures—2.1 pp for depression, and 8.4 pp for anxiety—but neither estimate is statistically significant. This analysis will have increased power as additional waves of the survey continue to be deployed.

Survey non-response. The survey has a 15% response rate, which is low but not uncommon among surveys in low-income populations. Survey non-response has become a challenge in social science research broadly (Meyer and Mittag, 2019). In anticipation of incomplete response rates, I worked with the government agency to randomize incentives and provide notice of the incentive amount in the email invitation to complete the survey. Specifically, I randomly assign half of households to be compensated with a high incentive amount ($40) and the other half with a low incentive amount ($20).[10]

I investigate the impact of being assigned to receive services on response rates, finding no statistically significant relationship. In ongoing work, I will show the relationship between survey incentive amount at response rates, and implement the correction of Dutz et al. (2025).

7.5 Treatment Effect Heterogeneity

Figure 4 examines treatment effect heterogeneity for two key outcomes at 6 months: any eviction filing, and any homeless contact. Because the administrative earnings data are not available as of this writing, I construct two measures of persistent distress: (i) those having a high rent burden, and (ii) those who report at baseline not expecting to be able to pay their rent in full within either 1 month or 3 months. Those not classified as experiencing persistent distress are classified as having transitory distress. I classify households as having access to friend or family support using the intake data if the respondent says yes to being able to rely on a friend or family member for a place to stay, or responds "friend or family member's place" when asked where they would stay if required to vacate their unit. Only a small share of households have access to friends or family for support and hence this subgroup is small. As a second measure of support, I classify households by whether they expect to stay in a shelter or outdoors if required to vacate.

For the outcome of eviction filing, treatment effects are broadly similar across households classified as high versus low rent burden and across those who do versus do not expect to pay rent in full within one to three months, suggesting that assistance is effective across the distribution of financial strain. For any homelessness contact, there are similarly large negative effects of the program on homelessness across the range of subgroups. The most notable findings of this analysis are the control group means: those who report having not being able to rely on friends or family have homelessness contact rates of 10.9 percent,

[10]These incentive amounts were increased from $5 and $20 after receiving updates from the county on response rates from the initial survey waves, and after revisiting the study budget for incentives and our interest in increasing survey response rates.

compared to 6.8 percent of those who report being able to rely on friends or family. Similarly, those who report expecting to stay in a shelter or outdoors have homelessness rates of 13.3 percent, compared to 9.4 percent of those who do not expect to stay in a shelter or outdoors. These findings suggest that predicting homelessness likelihood may be feasible, even if treatment effect heterogeneity is relatively muted. Lastly, I implement the (Chernozhukov et al., 2025) method of detecting heterogeneity treatment effects and cannot rule out a null hypothesis of no heterogeneous effects. Note that this is a split-sample method and the analysis lacks power for splitting the sample and then testing for heterogeneity among further subgroups.

7.6 Homelessness Delayed vs. Averted: Heterogeneity in Long-Run Program Impacts

The average treatment effects reported in Section 7.6 mask substantial heterogeneity in how households respond to emergency rental assistance over time. While the program generates large near-term reductions in sheltered and unsheltered homelessness for most recipients, the evidence of fade-out by nine to twelve months suggests that for a meaningful subset of households, the program *delays* rather than *averts* housing instability. This section characterizes the baseline predictors of these two response types, with the goal of informing the design of a more targeted assistance model.

To distinguish delayers from averters, we adapt the generic machine learning framework of Chernozhukov et al. (2025) (hereafter CDDF) to the panel structure of our outcomes. Our approach proceeds in two steps. In the first step, we construct a household-level *fade-out outcome* defined as the difference between a household's average homelessness in the late window (quarters three and four post-randomization) and their average homelessness in the early window (quarters one and two):

$$\Delta_i \;=\; \bar{y}_{i,\,\text{late}} \;-\; \bar{y}_{i,\,\text{early}}. \tag{6}$$

A positive value of Δ_i for a treated household indicates that the program's protective effect is waning—the household is a candidate delayer. A negative or zero value indicates a sustained benefit—the household is a candidate averter.

In the second step, we apply the CDDF sample-splitting procedure to estimate a proxy predictor of the conditional average treatment effect (CATE) on Δ_i. We randomly partition the sample into an auxiliary half and a main half. On the auxiliary sample, we use lasso separately on the treatment and control groups to predict Δ_i from a vector of pre-randomization household characteristics X_i, including monthly rent, household income, rent burden, rental tenure, domestic violence history, disability status, gender, race and ethnicity, and household composition. The difference between the treatment-group and control-group predictions,

$$\hat{S}(X_i) \;=\; \hat{f}_T(X_i) \;-\; \hat{f}_C(X_i), \tag{7}$$

serves as a proxy for the CATE on the fade-out outcome. We repeat this procedure 100 times with independent random splits and classify each household according to its average predicted rank of $\hat{S}(X_i)$ across repetitions, following the aggregation approach recommended by CDDF. Households in the top half of the average rank distribution are classified as predicted delayers; those in the bottom half are classified as predicted averters.

We summarize heterogeneity in the fade-out treatment effect using the Best Linear Predictor (BLP) of

the CATE on the proxy predictor. The BLP regression takes the form

$$\Delta_i \;=\; \alpha \;+\; \beta_1\big(T_i - \hat{p}_i\big) \;+\; \beta_2\big(T_i - \hat{p}_i\big)\big(\hat{S}_i - \bar{\hat{S}}\big) \;+\; \theta\,\hat{f}_C(X_i) \;+\; \varepsilon_i, \tag{8}$$

where T_i is the treatment indicator, $\hat{p}_i$ is the propensity score, $\hat{S}_i - \bar{\hat{S}}$ is the centered proxy predictor, and $\hat{f}_C(X_i)$ is the control-group prediction included to improve precision. Observations are weighted by $1/[\hat{p}_i(1-\hat{p}_i)]$ and strata fixed effects are included in all specifications. The coefficient β_1 estimates the average treatment effect on the fade-out outcome—that is, the average degree to which treatment shifts a household's homelessness trajectory—while β_2 captures the degree to which $\hat{S}(X_i)$ explains heterogeneity in that effect. Rejecting the null hypothesis that $\beta_2 = 0$ constitutes evidence that observable baseline characteristics predict meaningful variation in long-run program impacts.

Table 5 reports the Classification Analysis (CLAN), comparing mean baseline characteristics of predicted averters and predicted delayers. The differences are large and statistically significant along the dimensions most directly tied to long-run affordability. Predicted delayers earn substantially less than predicted averters—\$1,237 per month compared to \$2,677—and face higher monthly rents (\$1,845 versus \$1,561), resulting in extreme rent burdens. They also have shorter rental tenures (19.6 versus 25.1 months), suggesting weaker attachment to their current housing situation prior to program entry. These patterns are consistent with the interpretation that the program successfully stabilizes housing in the near term for all recipients, but that households facing persistent affordability constraints—those for whom the underlying financial gap between income and rent is too large to bridge with a one-time payment—return to housing instability once the assistance is exhausted.

Domestic violence history is also a strong predictor of delay: 33 percent of predicted delayers report a domestic violence history, compared to 13 percent of averters. This finding points to a distinct mechanism for the delay group—ongoing safety concerns that limit the durability of housing stability regardless of financial assistance. Disability status, gender, household composition, and age are not meaningfully different across the two groups, suggesting that the delay-versus-avert distinction is primarily a story about financial precarity and, in a subset of cases, ongoing safety risk rather than about household demographics more broadly.

There are significant racial and ethnic differences across the two groups. Black households are substantially over-represented among predicted delayers (53 percent versus 8 percent), while Hispanic, Native American, and White households are over-represented among averters. We interpret these differences as reflecting structural inequalities in housing market access and labor market outcomes—the same forces that generate the income and rent burden gaps documented above—rather than as independent predictors of program response.

Taken together, these results motivate a tiered assistance design. The characteristics that most reliably predict delay—low income, high rent burden, short prior tenure, and domestic violence history—are all observable at program entry and could therefore serve as targeting criteria for a more intensive intervention arm. Households with these characteristics may benefit more from a combination of rental assistance and housing-search support designed to help them transition to units they can sustain without ongoing subsidies, rather than from a repeat of the same one-time assistance that proved insufficient in the current program.

8 Conclusion

Rising housing costs and rent affordability represent one of the most important challenges facing American cities. Low-income households who fall behind on their rent face eviction, affecting millions of households annually and having negative consequences for adults and children. Yet there is limited causal evidence on the effectiveness of policies to stabilize housing for those at imminent risk of eviction, and no studies that intervene prior to the legal system. This paper addresses that gap by partnering with a large government program to randomize $13 million of assistance among households who have received an eviction notice and are one to three months behind on rent. Using linked administrative records on eviction and homelessness and a novel baseline and follow-up survey, the paper provides experimental evidence on the short- and medium-run impacts of assistance on eviction, homelessness, and housing stability.

The program is highly successful at reaching households prior to the eviction court process at a moment of acute distress, and at preventing eviction filings and judgments. The program is also highly successful at reducing sheltered and unsheltered homelessness in the near term. These short-run effects are extremely large in magnitude and robust across specifications and subpopulations. The survey shows alarming rates of unsheltered homelessness among the control group, which is significantly reduced by the program. By measuring unsheltered homelessness, this study points to the limits of administrative data in capturing the full extent of the homeless population (Meyer et al., 2023), and the full benefits of homelessness prevention programs. I find that the impacts on homelessness fade out at 1 year, although the measure of any contact with the homeless system remains statistically and economically significantly. Nevertheless the findings point to characterizing the population experiencing transitory distress, for whom the program is likely to avert homelessness, and designing an alternative policy to serve those for whom the program is delaying homelessness.

This paper will be updated with earnings, public assistance, health claims, and credit records data to produce a comprehensive assessment of the costs and benefits of the program, and the longer-run effects.

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Tables

Table 1: Summary Statistics: Households at Application

	Mean
Demographics	
Female	0.71
Household size	2.62
Number of other adults	0.52
Any children	0.58
Housing characteristics	
Tenure in unit (months)	21.96
Monthly rent	1711.69
Months past due	2.07
Monthly household income	1945.48
Respondent characteristics	
Domestic violence or sexual assault survivor	0.26
Have a disability	0.15
Receiving SSI or SSDI	0.13
Eviction history (self-report)	0.10
Any homelessness history (since 2015)	0.26
Shelter use history (since 2015)	0.08
Reason for falling behind on rent	
Lost job or reduced hours	0.65
Financial hardship or expenses (e.g., car, health, childcare)	0.68
Death in family	0.14
Rent increase	0.15
Partner separation	0.16
Loss of benefits (e.g., SNAP, VA, disability)	0.14
Experiencing temporary vs. persistent distress	
Distress likely to persist >3 mo.	0.25
Expect to pay rent without assistance in 1 mo.	0.30
Expect to pay rent without assistance in 3 mo.	0.42
Where to stay if required to vacate	
Don't know	0.62
Homeless shelter	0.12
Outdoors	0.17
Friends or family	0.05
Prefer to use assistance at current unit	0.86
Observations	3830

The table above presents sample averages of variables from the program intake data and survey, from February 2024 through September 2025.

Table 2: Eviction Filings and Judgments

	3 Months		6 Months		1 Year	
	OLS	IV	OLS	IV	OLS	IV
Panel A: Eviction Filing						
Assigned to Treatment	-0.054***		-0.080***		-0.040*	
	(0.011)		(0.014)		(0.021)	
Received Services (IV)		-0.071***		-0.098***		-0.045**
		(0.015)		(0.017)		(0.023)
Control group mean	0.108		0.153		0.200	
R^2	0.046	0.013	0.053	0.016	0.040	0.007
Panel B: Eviction Judgment						
Assigned to Treatment	-0.030***		-0.045***		-0.026**	
	(0.006)		(0.008)		(0.013)	
Received Services (IV)		-0.040***		-0.055***		-0.029**
		(0.008)		(0.010)		(0.014)
Control group mean	0.036		0.055		0.069	
R^2	0.053	0.014	0.059	0.016	0.044	0.007
Observations	3,830	3,830	3,319	3,319	2,274	2,274

The regression above is at the household-level: $y_i = \beta_0 + \beta_1 T_i + \gamma X_i + \theta_{gt} + \epsilon_i$. Dependent variable in panel A is an indicator for having an eviction filing, and in panel B is an indicator for having an eviction judgment. All regressions include priority group $\times$ application month f.e. Sample includes those enrolled between February 2024 and September 2025.

Table 3: Homelessness

	3 Months		6 Months		1 Year	
	OLS	IV	OLS	IV	OLS	IV
Panel A: Homeless Shelter Entry						
Assigned to Treatment	-0.005*		-0.009**		-0.017**	
	(0.003)		(0.004)		(0.007)	
Received Services (IV)		-0.006*		-0.011**		-0.019**
		(0.003)		(0.005)		(0.007)
Control group mean	0.005		0.012		0.021	
R^2	0.054	0.003	0.057	0.009	0.046	0.011
Panel B: Any Homelessness Contact						
Assigned to Treatment	-0.051***		-0.070***		-0.030*	
	(0.008)		(0.012)		(0.018)	
Received Services (IV)		-0.068***		-0.087***		-0.033*
		(0.011)		(0.014)		(0.020)
Control group mean	0.063		0.106		0.145	
R^2	0.056	0.018	0.057	0.018	0.053	0.015
Observations	3,830	3,830	3,319	3,319	2,274	2,274

Regression estimates the ITT at the household-level: $y_i = \beta_0 + \beta_1 T_i + \gamma X_i + \theta_{gt} + \epsilon_i$. Dependent variable in panel A is an indicator for having any shelter entry, and in panel B is an indicator for any interaction with the homelessness system. All regressions include priority group $\times$ application month f.e. Sample includes those who apply between Feb. 2024 and Sept. 2025, and is restricted further to applications enrolled early enough to observe HMIS data for full outcome period.

Table 4: Follow-up Survey

	Control Mean	Treatment Effect
A. Housing since application		
Any homeless shelter stay	0.067	-0.066*
		(0.035)
Any unsheltered homelessness (i.e., not for habitation)	0.275	-0.186***
		(0.061)
Number of places stayed	2.165	-0.952***
		(0.361)
Number of applications to rental housing	1.981	-0.456
		(0.405)
Difficulty finding place to rent	0.417	-0.109
		(0.073)
B. Current housing		
Same address as application	0.567	0.106
		(0.073)
Currently unhoused	0.025	-0.023
		(0.024)
Doubling up	0.125	0.043
		(0.048)
Stable place to live	0.283	-0.034
		(0.067)
Current rent	1471.669	278.300**
		(118.361)
Current rent | >0	1584.874	207.609*
		(108.294)
Zero current rent	0.062	-0.059*
		(0.033)
Dollars behind on rent	2094.014	-265.499
		(537.270)
Expect to pay rent in 1 month	0.393	-0.006
		(0.073)
Expect to pay rent in 3 months	0.466	0.072
		(0.077)
C. Mental health		
Screens positive for depression (PHQ-2)	0.449	-0.027
		(0.078)
Screens positive for anxiety (GAD-2)	0.551	-0.080
		(0.079)
Social isolation index (standardized)	-0.155	0.084
		(0.153)
Observations		337

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The regression above is at the household-level: $y_i = \beta_0 + \beta_1 T_i + \gamma X_i + \theta_{gt} + \epsilon_i$, with outcomes measured in the follow-up survey. The dependent variable is listed in the row. All regressions include priority group $\times$ application month f.e. Sample includes those enrolled between February 2024 and September 2025.

Table 5: Characteristics of Predicted Homelessness Averted vs. Delayed

	Averters		Delayers		
Variable	Mean	(SE)	Mean	(SE)	p-value
Financial characteristics					
Monthly rent ()	1607.837	(16.375)	1773.111	(18.190)	<0.001***
Monthly HH income ()	2351.122	(50.330)	1536.252	(45.380)	<0.001***
Rental tenure (months)	25.573	(1.087)	18.773	(0.822)	<0.001***
Rent burden (rent/income)	5.512	(3.265)	8.234	(3.492)	0.570
Demographics					
Domestic violence	0.198	(0.013)	0.316	(0.015)	<0.001***
Disability	0.186	(0.012)	0.120	(0.010)	<0.001***
Female	0.704	(0.015)	0.716	(0.014)	0.549
Any children	0.533	(0.016)	0.644	(0.015)	<0.001***
Any member age 65+	0.064	(0.008)	0.004	(0.002)	<0.001***
Race and ethnicity					
Black	0.029	(0.005)	0.606	(0.016)	<0.001***
Hispanic	0.074	(0.008)	0.065	(0.008)	0.422
Native American	0.090	(0.009)	0.067	(0.008)	0.063*
Asian	0.030	(0.005)	0.017	(0.004)	0.073*
White	0.551	(0.016)	0.065	(0.008)	<0.001***

Averted = bottom 50% of average predicted fade-out score; Delayed = top 50%. The predicted fade-out score is the average rank of $\hat{S}(X_i) = \hat{f}_T(X_i) - \hat{f}_C(X_i)$ across 100 independent sample splits using lasso, following Chernozhukov et al. (2025). The fade-out outcome is average homelessness in quarters 3–4 minus quarters 1–2. p-values from two-sided tests of equality of means.

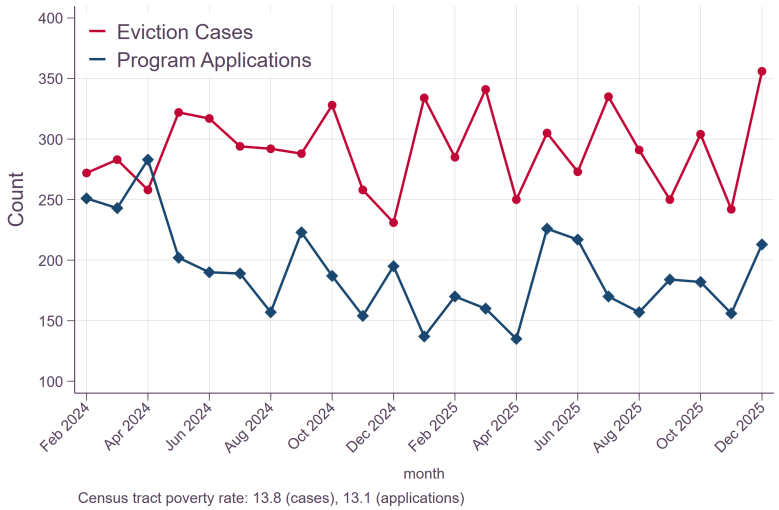
Table 6: Best Linear Predictor and Median Split Results

	Best Linear Predictor			Median Split		
	$\hat{\beta}_1$	$\hat{\beta}_2$	$p(\hat{\beta}_2=0)$	Averters	Delayers	p(diff)
Early effect (Q1–Q2)	-0.0101 [-0.0383, 0.0188]	-0.0060 [-0.0933, 0.0909]	0.509	0.0000 [-0.0422, 0.0244]	-0.0149 [-0.0459, 0.0155]	0.417
Fade-out (Q3–Q4 minus Q1–Q2)	0.0373 [-0.0231, 0.1008]	0.0000 [-1.0202, 1.0456]	0.367	0.0000 [-0.0022, 0.0294]	0.0329 [-0.0309, 0.0979]	0.250

Each row reports median estimates across 100 independent sample splits following Chernozhukov et al. (2025). The early effect outcome is average homelessness in quarters 1–2; the fade-out outcome is average homelessness in quarters 3–4 minus quarters 1–2. $\hat{\beta}_1$ is the average treatment effect; $\hat{\beta}_2$ tests whether the proxy predictor $\hat{S}(X_i)$ explains heterogeneity. The median split divides households into those predicted to have homelessness averted (bottom 50% of $\hat{S}$) and those predicted to have homelessness delayed (top 50%). Confidence intervals (95%) in brackets are medians of repetition-specific intervals.

Figures

Figure 1: Monthly Applications and Eviction Filings

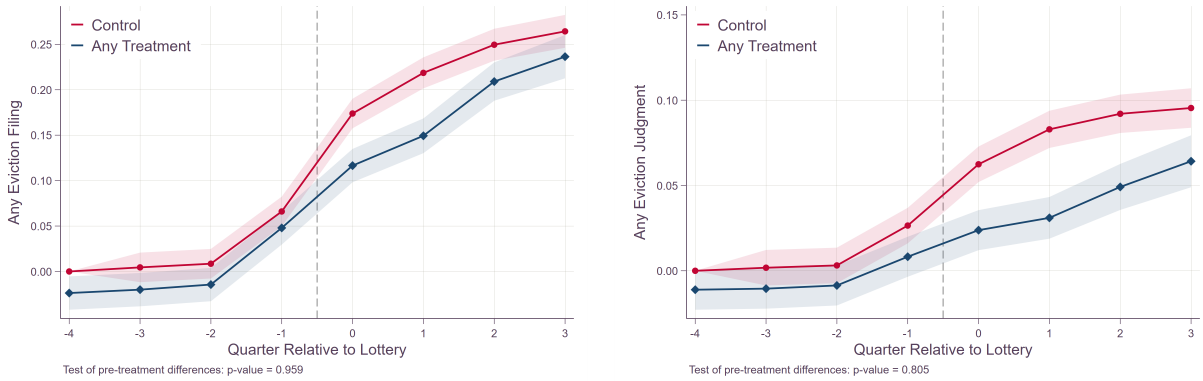


Notes: The figure shows monthly applications to the program and monthly eviction filings in the county, between February 2024 and December 2025.

Figure 2: Eviction Filings and Judgments

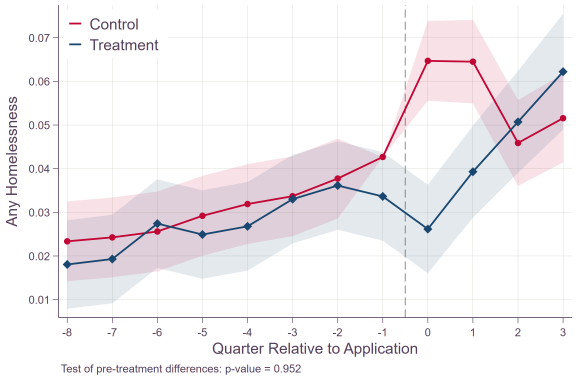
(a) Any Eviction Filing

(b) Any Eviction Judgment



Notes: The regression above is at the household-level: $y_{it} = \alpha + \gamma T_i + \sum_{t \neq -1} \beta_t + \sum_{t \neq -1} \delta_t T_i + \theta_{gt} + \epsilon_{it}$, where applicant name and address is linked to eviction court records. All regressions include priority group $\times$ application month f.e. Sample includes those enrolled between Feb. 2024 and Sept. 2024. 90% confidence intervals shown.

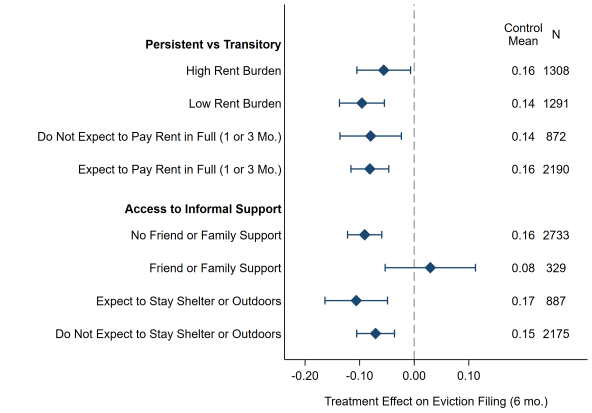
Figure 3: Homelessness



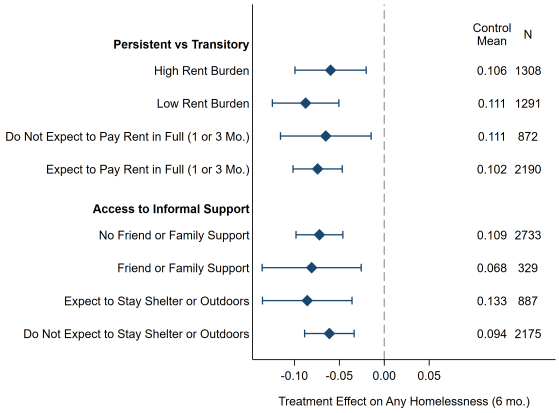
Notes: The regression above is at the household-level: $y_{it} = \alpha + \gamma T_i + \sum_{t \neq -1} \beta_t + \sum_{t \neq -1} \delta_t T_i + \theta_{gt} + \epsilon_{it}$. All regressions include priority group × application month f.e. Sample includes those enrolled between Feb. 2024 and Sept. 2025. 90% confidence intervals shown.

Figure 4: Heterogeneity: Eviction Filings and Homelessness at 6 Months

(a) Any Eviction Filing



(b) Any Homelessness Contact



Notes: The regression above is at the household-level: $y_{it} = \alpha + \gamma T_i + \sum_{t \neq -1} \beta_t + \sum_{t \neq -1} \delta_t T_i + \theta_{gt} + \epsilon_{it}$. All regressions include priority group × application month f.e. Sample includes those enrolled between Feb. 2024 and Sept. 2025. 90% confidence intervals shown.

A Appendix Tables and Figures

Table A1: First Stage

	Any Service			Total Amount Paid		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.835***	0.835***		6838.825***	6830.006***	
	(0.009)	(0.009)		(112.225)	(111.866)	
Rental Assistance			0.817***			6902.009***
			(0.010)			(130.070)
Case Management			0.869***			6699.407***
			(0.013)			(164.329)
Baseline controls	No	Yes	Yes	No	Yes	Yes
Control group mean	0.06			423.22		
Observations	3140	3140	3140	3140	3140	3140
R^2	0.839	0.840	0.840	0.688	0.692	0.692

The regression above is at the household-level: $y_i = \beta_0 + \beta_1 T_i + \gamma X_i + \theta_{gt} + \epsilon_i$. Dependent variable is an indicator for receiving any service. All regressions include priority group × application month f.e. Sample includes those enrolled between February 2024 and September 2025.

Table A2: Homelessness by Rental Assistance and Case Management

	3 Months		6 Months		1 Year	
Panel A: Homeless Shelter Entry						
Assigned to Treatment	-0.005*		-0.009**		-0.017**	
	(0.003)		(0.004)		(0.007)	
Rental Assistance		-0.004		-0.009*		-0.022***
		(0.003)		(0.005)		(0.008)
Case Management		-0.005		-0.010*		-0.012
		(0.004)		(0.006)		(0.008)
Control group mean	0.005		0.012		0.021	
R^2	0.054	0.054	0.057	0.057	0.046	0.047
Panel B: Any Homelessness Contact						
Assigned to Treatment	-0.051***		-0.070***		-0.030*	
	(0.008)		(0.012)		(0.018)	
Rental Assistance		-0.049***		-0.071***		-0.024
		(0.009)		(0.013)		(0.022)
Case Management		-0.054***		-0.069***		-0.036
		(0.012)		(0.017)		(0.022)
Control group mean	0.063		0.106		0.145	
R^2	0.056	0.056	0.057	0.057	0.053	0.053
Observations	3,830	3,830	3,319	3,319	2,274	2,274

Regression estimates the ITT at the household-level: $y_i = \beta_0 + \beta_1 T_i + \gamma X_i + \theta_{gt} + \epsilon_i$. Dependent variable is an indicator for having any shelter entry within 3- months, 6-months, and 1-year. All regressions include priority group × application month f.e. Sample includes those who apply between Feb. 2024 and Sept. 2025, and is restricted further to applications enrolled early enough to observe HMIS data for full outcome period.

B Program Eligibility Criteria

1. Household must live in the program county
2. Household must agree to Case Management Services, if so assigned
3. Household must be a renter
4. Household must currently reside in unit
5. Households must have an eviction notice, 30-day notice, 14-day pay or vacate from their landlord
6. Household must have past due rent (program can not serve utilities only)
7. Household is behind on rent up to 3 months
8. Household is under 80% Area Median Income
9. Household must have a lease in their own name
10. Household cannot be in subsidized unit (Section 8, Permanent Supportive Housing, Voucher Program, etc) and must be fully responsible for 100% of rent[11]
11. Unit size must be 'reasonable' for the household size
12. Household must have the following documents:
    - Photo IDs for all household members over age 18
    - Proof of income or self-attestation to no income
    - Lease in their own name
    - Ledger from landlord
    - 14-day, 30-day, or eviction notice
    - Proof of residency (utility bill in their name with address, recent mail received)
    - Utility bills in their name (must have past due rents to be eligible for this program)

[11] The program administrator dropped this criterion in May of 2025.

C Priority Groups

1: Eviction notice, households with children under age 18 or adult children with disability over age 18, seniors over age 65, household member with severe or life-threatening medical condition, owner with less than 5 units, survivor of domestic violence or sexual assault, history of evictions, felony likely to impact housing, faced employment or housing discrimination.

2: 10-day notice, 14-day notice, 30-day notice, households with children under 18 or children with disability over age 18, seniors over 65, one of the following: household member with severe or life-threatening medical condition, survivor of domestic violence or sexual assault, history of evictions OR felony likely to impact housing, faced employment or housing discrimination.

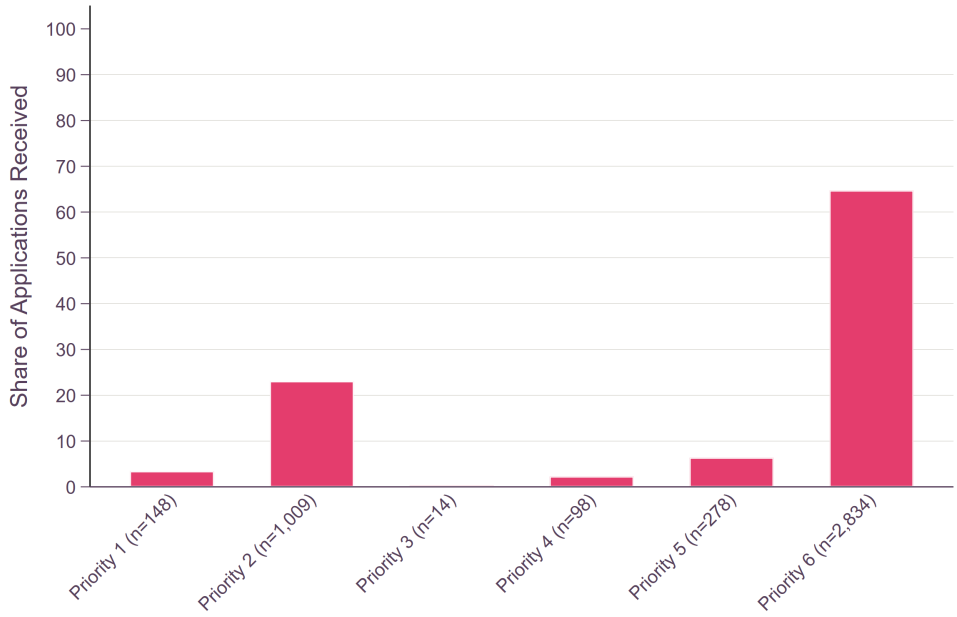
3: Eviction notice, faced employment or housing discrimination.

4: 10-day notice, 14-day notice, 30-day notice, faced employment or housing discrimination.

5: Eviction notice.

6: 14-day notice, 30-day notice.

Figure A1: Applications by Priority Group



Notes: The figure shows applications to the program in each priority group (February 2024-December 2025).

D Program Intake

In addition to the program's usual application form, which includes tenant and household member identifiers, landlord identifiers, monthly rent, amount behind on rent, and other eligibility questions, the research team has added the questions listed below.

Before asking these questions, applicants are informed that these questions are for research and evaluation purposes only and will not be used to determine eligibility for assistance.

1. What is the primary reason you have fallen behind on rent? (Select all that apply)
    (a) Lost job
    (b) Reduced hours
    (c) Lost benefits (SNAP, VA, disability, etc.)
    (d) Reduced child support income or family support
    (e) Rent was increased
    (f) Separation with spouse or partner
    (g) Departure of cohabitating adult(s) not on lease whom expenses were shared with
    (h) Health expenses
    (i) Childcare expenses
    (j) Elder care expenses
    (k) Other expenses (car broke down, etc.)
2. Is the loss of income or increase in expenses that caused you to fall behind on rent likely to continue? If yes, for how long?
    (a) Yes [Enter number of months]
    (b) No
    (c) Don't know
3. Do you expect to be able to pay your current monthly rent without additional assistance in:
    (a) 1 month: Yes/No/Don't know
    (b) 3 months: Yes/No/Don't know
    (c) 6 months: Yes/No/Don't know
    (d) 1 year: Yes/No/Don't know
4. If you were required to immediately vacate (move out of) your residence, where would you stay?
    (a) A friend's place
        i. Shared space
        ii. Private space
    (b) A family member's place
        i. Shared space
        ii. Private space
    (c) Another apartment/rental home
    (d) A hotel/motel
    (e) A homeless shelter
    (f) Outdoors
    (g) Don't know
    (h) Other (please specify)
5. How many adults (ages 18+) live with you at your residence, excluding your own children and excluding any spouse, boyfriend, or girlfriend? [Enter number]
6. How many school-age children live with you at your residence more than 50% of the time, excluding children you are the parent or guardian of? [Enter number]

7. How many people live with you in total (excluding yourself and including any children living there more than 50% of the time)? [Enter number]
8. If you were eligible for rental assistance, would you rather use assistance for your current residence's rent, or move to a different residence and use the funds there?
    (a) Prefer to stay at current residence
    (b) Prefer to move to different residence
    (c) Don't know
9. How many family members have you been in contact with in the last six months that live in [the county or neighboring county]? [Enter number]
10. How many friends have you been in contact with in the last six months that live in [the county or neighboring county]? [Enter number]
11. Do you have any family members or friends in [the county or neighboring county] you could rely on for a temporary place to stay, if necessary?
    (a) Yes
    (b) No

E Follow-Up Survey

The questions below comprise the 10-month survey.

1. What is your name and date of birth? [First, Middle, and Last Name] [DOB in MM/DD/YYYY format]
2. What is the physical address you were living at 9 months ago (when you applied for Eviction Prevention assistance from [this county])? [Enter street address line 1] [address line 2] [city] [state] [ZIP code]
3. Are you still living at the same address?
    (a) Yes
    (b) No
4. [IF NO to Q3] What is your current address?
    (a) [Street address line 1] [address line 2] [city][state] [ZIP code]
    (b) I don't have an address
5. [IF NO to Q3] Is your current address:
    (a) A friend's place
        i. Shared space (couch, shared bedroom, etc.)
        ii. Private space (own room)
    (b) A family member's place
        i. Shared space (couch, shared bedroom, etc.)
        ii. Private space (own room)
    (c) Another apartment/home my household owns or rents
    (d) A hotel/motel
    (e) A homeless shelter
    (f) Outdoors

(g) A car/RV not connected to utilities (e.g. sewer, electricity)

(h) Other [Enter type of place]

6. [IF NO to Q3] Why did you move from the address you were living at when you applied for Eviction Prevention assistance? Select all that apply.

   (a) I could no longer afford to pay rent

   (b) I was forced to leave by someone else (e.g., I was evicted, or the property was condemned)

   (c) The place was getting run down or the neighborhood was dangerous

   (d) I chose to form my own household

   (e) I found a better place, or one that was closer to a job, friends, or family

   (f) Some other reason [Enter reason for move]

7. How many people live in your current household in total, including yourself? [Numeric entry]

8. Please enter the initials of all the people living in your household, the age of all the people living in your household, and your relationship with all the people living in your household.

   (a) [Enter initials of all people living in your household][column for initials]

   (b) [Enter age of all people living in your household][column for age]

   (c) [drop down menu including self, partner, child, other relative, roommate, friend, other][relation column]

9. In your current living situation, do you ever have concerns about your personal or physical safety?

   (a) Never/Rarely/Sometimes/Fairly often/Frequently

10. How much do you worry about crime occurring close to where you live, either in the building or in the immediate area of where you stay?

    (a) Never/Rarely/Sometimes/Fairly often/Frequently

11. What is your current monthly rent? [Numeric entry]

12. [IF YES to Q3 OR A, B, C, or D to Q5] In the past six months, have you made every rent payment on time and in full?

    (a) Yes

    (b) No

13. [IF NO to Q12] How many months behind are you on rent at your current place? [Enter number of months]

14. [IF NO to Q12] What is the primary reason you have fallen behind on rent? (Select all that apply)

    (a) Lost job

    (b) Reduced hours

    (c) Lost benefits (SNAP, VA, disability, etc.)

    (d) Reduced child support income or family support

    (e) Rent was increased

    (f) Separation with spouse or partner

    (g) Departure of roommate whom expenses were shared with

    (h) Health expenses

    (i) Childcare expenses

(j) Elder care expenses

(k) Other expenses (car broke down, etc.)

15. Do you expect to be able to pay your current monthly rent in:

    (a) 1 month: Y/N/Don't know/Not applicable

    (b) 3 months: Y/N/Don't know/Not applicable

    (c) 6 months: Y/N/Don't know/Not applicable

    (d) 12 months: Y/N/Don't know/Not applicable

16. Stable housing means you are certain about your housing situation and have a choice over when you move. How would you describe your current level of housing stability?

    (a) I have a stable place to live

    (b) I have a place to live today, but I am worried about losing it in the future

    (c) I do not have a stable place to live

    The following questions will ask about your experiences in the past 9 months (since you applied for Eviction Prevention assistance).

17. Since you applied for Eviction Prevention assistance, how many different places have you stayed the night, even for as little as one night? [numeric entry box]

18. Since you applied for Eviction Prevention assistance, have you spent any nights in a place not usually meant for people to live (for example, in a car, in a tent, on a park bench, under an overpass)?

    (a) Yes

    (b) No

19. Since you applied for Eviction Prevention assistance, have you spent any nights in a homeless shelter?

    (a) Yes

    (b) No

20. Since you applied for Eviction Prevention assistance, how many applications to rent or buy an apartment, house, or mobile home have you submitted? [Enter number]

21. [IF Q20 $>$ 0] Since you applied for Eviction Prevention assistance, have any of these applications for housing been rejected? How many? [Enter number]

22. Since you applied for Eviction Prevention assistance, have you had difficulties finding a landlord willing to rent to you, even though you could afford the unit?

    (a) Yes

    (b) No

    Please select how often the following statements have been true about your food situation.

23. Within the past 9 months we worried whether our food would run out before we got money to buy more.

    (a) Often true

    (b) Sometimes true

    (c) Never true

    (d) Don't know

24. Within the past 9 months the food we bought just didn't last and we didn't have money to get more.

(a) Often true

(b) Sometimes true

(c) Never true

(d) Don't know

25. When you applied for Eviction Prevention assistance, did any children under age 18 live with you?

    (a) Yes [Enter age of youngest child]

    (b) No

26. [IF YES to Q29 & age ≤ 5] In the past eight months, how many times have you had to switch childcare or preschool providers? [Enter number]

27. [IF YES to Q29] In the past eight months, have any of your children accessed any of the following? Select all that apply.

    (a) Attended preschool

    (b) Attended Head Start or Early Head Start

    (c) Attended childcare

        i. Licensed child care center

        ii. In home childcare (child care located in some else's home)

        iii. A relative or friend

    (d) Enrolled in or used Medicaid or the Children's Health Insurance Program

    (e) Received any annual vaccination

28. In a typical week, how many times do you talk on the telephone with family, friends, or neighbor? [0, 1,...,5, 6+]

29. In a typical week, how often do you get together with friends or relatives (for example, going out together or visiting in each other's homes)? [0,1,...,5, 6+]

30. How often do you attend church or religious services?

    (a) 0 times per year

    (b) 1-3 times per year

    (c) 4 or more times per year

31. How often do you attend meetings of the clubs or organizations you belong to?

    (a) 0 times per year

    (b) 1-3 times per year

    (c) 4 or more times per year

The following section asks questions about your mental health.

32. Over the last two weeks how often have you been bothered by the following problems?

    (a) Feeling nervous, anxious, or on edge: Not at all/Several days/More than half the days/Nearly every day

    (b) Not being able to stop or control worrying: Not at all/Several days/More than half the days/Nearly every day

    (c) Worrying too much about different things: Not at all/Several days/More than half the days/Nearly every day

    (d) Trouble relaxing: Not at all/Several days/More than half the days/Nearly every day

(e) Being so restless that it is hard to sit still: Not at all/Several days/More than half the days/Nearly every day

(f) Becoming easily annoyed or irritable: Not at all/Several days/More than half the days/Nearly every day

(g) Feeling afraid, as if something awful might happen: Not at all/Several days/More than half the days/Nearly every day

33. Over the last two weeks, how often have you been bothered by any of the following problems?

(a) Little interest or pleasure in doing things: Not at all/Several days/More than half the days/Nearly every day

(b) Feeling down, depressed, or hopeless: Not at all/Several days/More than half the days/Nearly every day

(c) Trouble falling or staying asleep, or sleeping too much: Not at all/Several days/More than half the days/Nearly every day

(d) Feeling tired or having little energy: Not at all/Several days/More than half the days/Nearly every day

(e) Poor appetite or overeating: Not at all/Several days/More than half the days/Nearly every day

(f) Feeling bad about yourself — or that you are a failure or have let yourself or your family down: Not at all/Several days/More than half the days/Nearly every day

(g) Trouble concentrating on things, such as reading the newspaper or watching television: Not at all/Several days/More than half the days/Nearly every day

(h) Moving or speaking so slowly that other people could have noticed? Or the opposite — being so fidgety or restless that you have been moving around a lot more than usual: Not at all/Several days/More than half the days/Nearly every day

(i) Thoughts that you would be better off dead or of hurting yourself in some way: Not at all/Several days/More than half the days/Nearly every day

34. Do you have any suggestions for [this county's] Eviction Prevention Program to improve its services? [text entry box]

35. What is a good email address to send a $XX.XX gift card to as a thank you for completing this survey? [text entry box with email validation]

Thank you for completing our survey! Your participation will inform research to improve the eviction prevention program to better serve tenants in the future.

- If you or someone you know may be considering suicide, text or call 9-8-8 to reach the Suicide & Crisis Lifeline.
- To find a mental health provider in or around [this county], follow the instructions in this interactive map.

F Estimating Heterogeneous Treatment Effects

This appendix describes the method we use to identify which households experience a durable reduction in homelessness following emergency rental assistance ("averters") and which households experience only a temporary reduction ("delayers"). Our approach adapts the generic machine learning framework of Chernozhukov et al. (2025) (hereafter CDDF) to the panel structure of our outcome data.

The central challenge is that the distinction between delaying and averting homelessness is not directly observable. A household that is homeless-free twelve months after receiving assistance might have permanently resolved its housing instability, or it might simply not yet have experienced the return to homelessness that will eventually occur. To address this, we construct a composite outcome—the *fade-out outcome*—that captures the trajectory of treatment effects over time, and we use machine learning to identify which pre-randomization baseline characteristics predict this trajectory. We then use the predicted trajectory type to classify households and validate the classification using an independent event study.

F.1 CDDF Framework

CDDF develop a method for estimating heterogeneous treatment effects that is valid even when the number of potential predictors is large relative to the sample size. We briefly describe the method before explaining how we adapt it to our setting.

Let Y be the outcome of interest and X be a vector of pre-randomization covariates. Units are randomly assigned to treatment ($T = 1$) or control ($T = 0$) with known propensity score $p(X)$. The conditional average treatment effect (CATE) function is:

$$\tau_0(X) \;=\; \mathbb{E}[Y \mid T = 1,\, X] - \mathbb{E}[Y \mid T = 0,\, X]. \tag{9}$$

CDDF propose estimating properties of $\tau_0(X)$ via the following steps:

1. Split the sample into two approximately equal parts: a *main sample* and an *auxiliary sample*.
2. Use the auxiliary sample to train a machine learning algorithm to predict Y from X. This prediction is performed twice: once using only treated observations and once using only control observations, yielding fitted models $\hat{f}_T(X)$ and $\hat{f}_C(X)$, respectively.
3. For observations in the main sample, form the proxy predictor

   $$\hat{S}(X) \;=\; \hat{f}_T(X) - \hat{f}_C(X),$$

   which serves as a potentially biased but informative estimate of $\tau_0(X)$.
4. Use $\hat{S}(X)$ to extract properties of the CATE distribution via the regressions described below.

Although $\hat{S}(X)$ is a biased and inconsistent estimator of $\tau_0(X)$ in general, CDDF show that it can still be used to identify the best linear predictor (BLP) of $\tau_0(X)$ on $\hat{S}(X)$, sorted group average treatment effects (GATES), and the characteristics of high- and low-CATE groups (the Classification Analysis, or CLAN).

Best Linear Predictor The BLP of $\tau_0(X)$ on $\hat{S}(X)$ is identified by estimating the following weighted regression on the main sample:

$$Y \;=\; \beta_1\big(T - p(X)\big) + \beta_2\big(T - p(X)\big)\big(\hat{S}(X) - \bar{S}\big) + \theta\, \hat{f}_C(X) + \varepsilon, \tag{10}$$

where observations are weighted by $w(X) = 1/[p(X)(1-p(X))]$ and $\bar{S}$ denotes the sample mean of the proxy predictor. The coefficient β_1 estimates the average treatment effect, and β_2 captures the degree to which $\hat{S}(X)$ explains heterogeneity in treatment effects. A test of $\beta_2 = 0$ is a joint test of no treatment effect heterogeneity and irrelevance of the proxy predictor. The control $\hat{f}_C(X)$ is included to reduce residual variance, following CDDF's recommended specification. All regressions include strata fixed effects (*priorityXdate*) to account for the stratified randomization design.

Sorted Group Average Treatment Effects CDDF also propose partitioning the main sample into K groups based on non-overlapping quantiles of $\hat{S}(X)$ and estimating average treatment effects within each group. Define groups $\mathcal{G}_1, \mathcal{G}_2, \ldots, \mathcal{G}_K$ by the quantiles of $\hat{S}(X)$. The group average treatment effects (GATES) are identified by estimating:

$$Y \;=\; \sum_{k=1}^{K} \gamma_k\big(T - p(X)\big) \cdot \mathbf{1}(\mathcal{G}_k) + \theta\, \hat{f}_C(X) + \varepsilon, \tag{11}$$

where the weights are the same as in equation (10). The coefficient γ_k estimates $\mathbb{E}[\tau_0(X) \mid \mathcal{G}_k]$, the average treatment effect for units in group k. In our application, we use $K = 2$ groups (a median split), labeling the high-$\hat{S}$ group as predicted delayers and the low-$\hat{S}$ group as predicted averters.

Classification Analysis CDDF show that the average characteristics of units in each GATES group can be estimated by simply computing group means, and that differences across groups can be tested using standard t-tests of equality of means. This Classification Analysis (CLAN) characterizes the observable baseline attributes of households predicted to experience delay versus aversion, and motivates the tiered assistance design discussed in Section 6 of the paper.

Aggregation Across Sample Splits Because the proxy predictor $\hat{S}(X)$ depends on which observations are assigned to the main versus auxiliary sample, all estimates are subject to sample-splitting uncertainty in addition to standard estimation uncertainty. Following CDDF, we address this by repeating steps (1)–(4) one hundred times using independent random splits, re-estimating equations (10) and (11) for each split, and reporting the median of the one hundred resulting estimates. We report the median of the one hundred repetition-specific 95 percent confidence intervals as our summary uncertainty measure. Simulation evidence in CDDF shows that one hundred repetitions is sufficient to eliminate most sample-splitting uncertainty, and our own sensitivity checks confirm that results are stable across a range of repetition counts.

For the Classification Analysis, we require a stable household-level classification rather than a per-split classification. We therefore aggregate across splits by computing the average rank of $\hat{S}(X)$ for each household across all one hundred repetitions:

$$\bar{r}_i \;=\; \frac{1}{R}\sum_{r=1}^{R} \text{rank}\Big(\hat{S}_r(X_i)\Big)\,, \tag{12}$$

where $\hat{S}_r(X_i)$ is the proxy predictor for household i in repetition r and $R = 100$. Households with $\bar{r}_i$ above the median are classified as predicted delayers; those below the median are classified as predicted averters. Averaging ranks across repetitions provides a more stable classification than relying on any single split, since classification noise averages out at rate $1/\sqrt{R}$.

F.2 Adapting CDDF to Identify Delay Versus Aversion

Defining the Fade-Out Outcome The standard CDDF framework estimates heterogeneity in a single scalar outcome. Our objective is different: we want to identify households for whom the treatment effect is sustained over time (averters) versus those for whom it attenuates or reverses (delayers). We accomplish this by constructing a composite outcome that captures trajectory shape.

Let Y_{iq} denote an indicator for any homelessness for household i in quarter q following randomization, where $q \in \{1, 2, 3\}$. We define two composite outcomes:

$$Y_i^{\text{early}} = \frac{Y_{i1} + Y_{i2}}{2}, \tag{13}$$

$$Y_i^{\text{fadeout}} = Y_{i3} - Y_i^{\text{early}}. \tag{14}$$

The early outcome Y_i^{early} measures average homelessness in the first two post-randomization quarters. The fade-out outcome Y_i^{fadeout} measures whether homelessness in the later window (quarter 3) is higher or lower than in the early window. A positive fade-out treatment effect—that is, a larger treatment effect on Y^{fadeout} for treated versus control households—indicates that the program's protective effect is waning over time, consistent with a delay mechanism. A negative or zero fade-out treatment effect indicates that the program's early benefit is sustained or growing, consistent with aversion.

We apply CDDF to Y^{fadeout} as the outcome of interest. The resulting proxy predictor $\hat{S}(X)$ captures which baseline characteristics predict a larger fade-out treatment effect. Households in the top half of the average $\hat{S}(X)$ rank distribution are predicted delayers; those in the bottom half are predicted averters.

We note two differences from the standard binary outcome application. First, Y^{fadeout} is not bounded in $[0, 1]$, so we do not clip the lasso predictions to the unit interval as we do for Y^{early}. Second, Y^{fadeout} takes both positive and negative values under treatment, which means the interpretation of the median split differs from the standard application: a high $\hat{S}(X)$ value indicates predicted fade-out (delay), not simply predicted high treatment benefit.

Machine Learning Algorithm We use lasso as the machine learning algorithm in step (2) of the CDDF procedure. We select the lasso penalty parameter via ten-fold cross-validation within the auxiliary sample, with a fallback to the plugin selector of **?** in the event that cross-validation selects no predictors. Lasso is trained separately on treated and control units in the auxiliary sample. The covariate vector X includes monthly rent, monthly household income, rental tenure, rent burden (rent-to-income ratio), indicators for domestic violence history, disability, female head of household, Black, Hispanic, Native American, Asian, and White race/ethnicity, any children in household, and any household member aged 65 or older.

We use lasso rather than a richer algorithm such as gradient-boosted trees for two reasons. First, our sample size (approximately 1,200 households per split half) is modest by the standards of machine learning applications, making regularized linear methods more appropriate than highly flexible nonparametric methods. Second, lasso coefficients are directly interpretable and can be compared across repetitions to assess the stability of predictor selection, which we report in a sensitivity check below.

Propensity Score Our experimental design stratifies randomization by application wave and priority tier (the priority group-by-date strata). Within each stratum, the propensity score is known by design and equals the fraction of applicants assigned to treatment. We compute $p(X)$ empirically as the within-stratum treatment rate. Because the propensity score is known, we do not need to estimate it, which eliminates one source of estimation uncertainty that arises in observational applications of CDDF.

Implementation Details We implement the CDDF procedure in Stata using the built-in `lasso` command. For each of the one hundred repetitions, we:

1. Draw a uniform random variable for each household and assign those with values below 0.5 to the auxiliary sample and the remainder to the main sample.
2. Estimate separate lasso models for Y^{fadeout} on the treated and control subsets of the auxiliary sample, using ten-fold cross-validation to select the penalty. If cross-validation selects zero predictors, we fall back to the plugin selector.
3. Generate predictions $\hat{f}_T(X)$ and $\hat{f}_C(X)$ for the main sample from the auxiliary-sample lasso fits, and form the proxy predictor $\hat{S}(X) = \hat{f}_T(X) - \hat{f}_C(X)$.
4. Estimate the BLP regression (10) on the main sample using IPW weights and strata fixed effects, storing $\hat{\beta}_1$, $\hat{\beta}_2$, their standard errors, and the p-value on $\hat{\beta}_2$.
5. Compute the median split of $\hat{S}(X)$ within the main sample, assign households to high and low groups, and estimate the GATES regression (11), storing group-specific treatment effects and their standard errors.
6. Record the rank of each household's $\hat{S}(X)$ within the main sample's rank distribution for use in the aggregated CLAN classification.

After all one hundred repetitions, we compute medians of the stored coefficients and construct the CLAN classification using equation (12). The CLAN table (Table 5 in the main text) reports two-sided t-tests of equality of means for each baseline characteristic between the predicted delayer and averter groups, where the group means and standard errors are themselves medians over the one hundred splits.

F.3 Validation: Event Study by Predicted Type

To validate the CDDF classification, we estimate a separate event study for predicted averters and predicted delayers. We estimate:

$$Y_{iqt} = \sum_{t \neq -1} \alpha_t \cdot \mathbf{1}(t) \cdot T_i + \sum_{t \neq -1} \delta_t \cdot \mathbf{1}(t) \cdot T_i \cdot D_i + \lambda D_i + \mu_s + \varepsilon_{iqt}, \tag{15}$$

where D_i is the predicted delayer indicator from equation (12), $\mathbf{1}(t)$ are period indicators for quarters $t \in \{-4, -3, -2, 0, 1, 2, 3\}$ relative to application (the reference period is $t = -1$), T_i is the treatment

indicator, and μ_s are strata fixed effects. The coefficient α_t traces out the treatment effect trajectory for predicted averters at period t. The coefficient δ_t captures the differential trajectory for predicted delayers relative to averters at the same period. Under the delay hypothesis, we expect $\delta_t \approx 0$ for pre-treatment periods and $\delta_t > 0$ for later post-treatment periods, as the delayer group's protective effect fades.

Figure [X] presents the estimated α_t and $\alpha_t+\delta_t$ sequences. Consistent with the delay hypothesis, predicted delayers exhibit a near-zero initial treatment effect at $t = 0$ that converges back toward and slightly above zero by $t = 2$–3, while predicted averters maintain a sustained negative treatment effect throughout the observation window. The joint test of no post-treatment differential ($\delta_t = 0$ for all $t > 0$) yields $p = 0.270$, which does not reach conventional significance but is directionally consistent with the classification. The pre-trend tests yield $p = 0.502$ for the averter group and $p = 0.460$ for the differential (triple interaction), both consistent with parallel pre-trends.

We note that the event study is a descriptive validation exercise rather than an independent test of the delay mechanism. Because the CLAN classification is derived from post-randomization outcomes in the same dataset, it is expected—by construction—to be correlated with post-treatment trajectories. The event study's value lies in confirming that the classification captures trajectory *dynamics* rather than simply sorting households by overall treatment effect magnitude, and in verifying that the pre-treatment period is not anomalous across groups.

F.4 Sensitivity Checks

We conduct several sensitivity checks to assess the robustness of the delay versus avert classification.

Number of repetitions. We re-run the analysis using 20, 50, and 200 repetitions. The CLAN table results are qualitatively identical across all specifications, reflecting the fact that averaging ranks across repetitions converges at rate $1/\sqrt{R}$ and the dominant source of uncertainty for the CLAN table is sampling variance in the group means rather than classification noise from sample splitting.

Alternative split fractions. We re-run the analysis using 33/67 and 40/60 auxiliary/main splits in place of the default 50/50 split. Results are stable. The 50/50 split maximizes the precision of the main-sample estimates while providing adequate training data for lasso; alternative splits trade these off in predictable ways.

Late window definition. Our baseline fade-out outcome uses quarter 3 as the late window. We re-run the analysis defining the late window as quarters 2 and 3 averaged. The CLAN characteristics and the direction of the BLP coefficients are unchanged, though the magnitude of the fade-out treatment effect is somewhat attenuated when quarter 2 is included in the late window, consistent with the event study evidence that $t = 2$ shows partial but incomplete convergence.

Alternative classification threshold. We re-run the CLAN analysis using tercile splits (top third versus bottom third of the average rank distribution) in place of the median split. This sharpens the contrast between groups—the income gap between the top and bottom terciles is larger than between the top and bottom halves—but at the cost of discarding the middle third of the sample. Results for financial characteristics and domestic violence history are robust to this change, as are the racial composition results.

Machine learning algorithm. As a robustness check, we repeat the analysis using ridge regression in place of lasso. The CLAN characteristics are qualitatively similar, though the fade-out BLP $\hat{\beta}_2$ coefficient is slightly larger under ridge, reflecting ridge's tendency to distribute predictive weight more broadly across correlated predictors rather than selecting a sparse set as lasso does.