The labor market impacts of unconditional housing out of homelessness

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Abstract

We leverage variation in the timing of unconditional housing recipiency by homeless individuals in Los Angeles County to determine the effects of housing on their employment, earnings, and benefits absorption. We validate our event study approach by demonstrating a parallel trends assumption and that wait times are unrelated to homelessness severity. We find that placement into 2-year Rapid Re-Housing increases extensive-margin employment by 55% from a 20pp baseline. Individuals that made unemployment-to-employment and employment-to-employment transitions exhibited earnings increases of USD 1000 and USD 250 per month, respectively, while exhibiting no change in benefits absorption. Permanent Supportive Housing recipients exhibited no substantial change in labor market outcomes around their placement into housing, but we do observe a decrease in their reported labor-search behavior. We argue that these differences between programmatic outcomes speak to differences in targeting rather than treatment. We perform a simple cost-benefit calculation based on impacts on labor market outcomes and pecuniary benefits, and find that because most recipients are still unemployed or in a low-earning job post-event, the cost-offset through increased earnings alone is near-zero.

Key words: Homelessness; Housing First; Cost Benefit Analysis; Housing Policy; Homelessness, Housing, and Labor Supply
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1 Introduction

What are the labor market impacts of housing homeless people? Housing programs for the homeless are costly, but their positive externalities might offset some or all of them (Gubits et al. (2018); Flaming, Burns, and Matsunaga (2015)).[1] One potential benefit is increased employment, but there exists little evidence as to whether housing programs for the homeless do increase employment and income (B. D. Meyer, Wyse, G. Meyer, et al. (2024); Von Wachter, Schnorr, and Riesch (2020)). We characterize the contribution of labor market impacts of housing homeless individuals and contextualize the externalities of these impacts within the costs of permanent and semi-permanent housing policies.

We use propriety data from the California Policy Lab (CPL) to study how labor market outcomes and services take-up evolve following placement of homeless individuals into Unconditional Housing (UH)-style programs.[2] This data allows us to follow individuals over time and observe the evolution in their earnings, select benefits absorption, and labor market participation. Our central specification estimates a series of event studies around the entry of homeless individuals into two distinct housing programs: Rapid Re-Housing (RRH) and Permanent Supportive Housing (PSH) in Los Angeles County from 2013 to 2019.

This paper is the first to study the employment/earnings impacts of unconditional housing on homeless individuals in a setting simultaneously featuring 1) consistent observation of housing recipients over time, 2) data on employment, earnings, and benefits outcomes that make use of some internal and third-party verification mechanisms beyond pure self-reporting, 3) a sufficiently comprehensive data environment so as to observe a variety of outcomes, as well as characterize causal mechanisms and describe margins of sociodemographic heterogeneity, and 4) credible quasi-random variation in the timing of housing re-

[1] Some examples include 1) increases in income tax collections if homelessness generates labor supply frictions or induces participation in the informal labor market, 2) increase in sales tax collections due to otherwise depressed individual consumption, 3) reduction in public non-housing benefits that the state provides to homeless individuals, 4) the elimination of environmental externalities that reduce property tax collections through base erosion, and 5) the elimination of other costs channeled through activities that are typically thought to positively covary with homeless status, such as healthcare expenses and crime outcomes.

[2] See Evans, Phillips, and Ruffini (2021) for a discussion of the different programs encompassed under "Housing First" (HF) and other similar Unconditional Housing (UH) approaches to homelessness policy. In brief, HF has evolved to refer to an emphasis on immediate, unconditional access to medium- and long-term housing.

cipiency. Prior works have largely focused on public benefits absorption (Cohen (2024); Evans, Phillips, and Ruffini (2021); Augustine and White (2020)). Other prior work has also tended to either rely on overly incomplete or otherwise limited data environments or data based on purely self-reported outcomes, lack of quasi-experimental variation, or exceedingly small sample sizes. Our environment allows us to at least partially address all of these shortcomings in estimating the net costs of UH-style policies. Moreover, our work represents the largest event study focusing on the labor market outcomes and state-level benefits absorption of individuals around placement into UH-style policies and their fiscal implications, with a final treated sample size of roughly 4,000 recipients.

We exploit quasi-random timing in unconditional housing recipiency to estimate the labor market and benefits take-up impacts of receiving unconditional housing. This quasi-random timing of housing recipiency arises from the time elapsed between initial entry into the homelessness service provision system and placement into housing. We provide evidence that conditional on assignment to UH this time elapsed is unrelated to homelessness severity and risk. We find that the employment and earnings effects we observe are driven by housing specifically and not by coincidental treatments related to housing receipt (e.g. connection to other benefits or services). We further explore how these housing effects vary based on ex-post employment transition type and family status. Lastly, we use our estimates to calculate the costs of unconditional housing that are offset by the earnings externalities of these programs.

We find positive effects of RRH on average extensive margin employment probability, labor earnings, and benefits absorption. Most notably, individuals placed into RRH see a nearly 55% increase (+10.8 percentage points) in their probability of finding employment and a USD 200 increase in earnings (relative to a baseline of USD 450) per month. PSH recipients demonstrate no change in their employment or earnings outcomes. We confirm the robustness of our estimates to different censoring restrictions on our sample, different assumptions about the updating process to our employment outcomes, and alternate event study estimation procedures that address concerns about mean reversion and potential heterogeneous treatment effects under staggered event timing across treated units.

We then explore some of the mechanisms that generate our main results. We characterize changes in earnings and benefits absorption based on ex-post employment transition type: conditional on making an unemployment-to-employment (U2E) transition, individual recipients of both RRH and PSH see earnings increases of around USD 1000 and USD 500, respectively, per month. RRH recipients making employment-to-employment (E2E) transitions also see increased earnings post-event by around USD 200-300 per month. This latter finding implies that housing also allows individuals to find either better jobs or work more hours, although we cannot empirically distinguish between these two possibilities. We also document that on average, individuals that are consistently employed in the post-event period report no increase in benefits absorption. Because we observe that individuals finding employment in the post-period see *no* concurrent increase in benefits, we attribute their employment and earnings effects to having stable permanent shelter (as opposed to connection to additional benefits). We also see increases in labor search behavior among RRH recipients and decreases among PSH recipients prior to their move-in events, which we interpret as substantiating our results. We also rule out the possibility that our results are driven by changes in receiving services pertaining to mental health or substance abuse, although mental health or substance abuse problems may improve in response to housing regardless of receiving professional services.

We characterize differential responses by select sociodemographic characteristics. We document no substantial differences in employment outcomes based on race, but a mildly stronger, albeit noisy employment response for women relative to men across both programs. We find that non-veteran recipients of RRH demonstrate a significantly higher employment response. Similarly, parent/guardian recipients of PSH demonstrate a significantly higher employment response.

Finally, we perform a novel cost-benefit analysis of these programs when only considering labor market and earnings effects. We estimate substantial variation in the net fiscal impact of RRH and PSH recipiency based on whether an individual recipient secures employment following housing recipiency. In spite of our large documented employment and earnings effects, individuals employed post-event still earn relatively little income, and an

overwhelming majority of housing recipients do not report employment post-event. As such, the average fiscal offset of these programs attributable solely to labor/earnings externalities amounts to between 1% of the recurring cost during program tenure for RRH and near zero (but non-negative) for PSH recipients. However, our estimates speak to outcomes within two years of housing, which may understate longer-run impacts.

1.1 Related literature

There is substantial precedent for studying homelessness and homelessness housing policy in a cost-benefit framework (Gubits et al. (2018); Gilmer et al. (2010); Spellman (2010)). However, nearly all of the work in this space focuses specifically on the evolution of public benefit/service absorption surrounding placement into UH (either observationally, quasirandomly, or randomly) or even simpler cross sectional analyses of benefits absorption among incumbent homeless populations.[3] Our work contributes uniquely to the existing literature in that 1) we use compelling quasi-experimental variation in the timing of housing recipiency, 2) we observe our sample with frequency both pre- and post-event, 3) we are able to observe both labor market and benefits outcomes frequently over time, and 4) we observe a relatively large number of UH recipients.

Gubits et al. (2018) represents one of the central works in this space, studying a randomized controlled trial in the US that placed 2,200 families into four groups: one receiving long-term rent subsidies, one receiving short-term rent subsidies, one receiving project-based transitional housing, and one acts as a control arm (standard of care). This study measured costs primarily based on homelessness service absorption and found a strong negative impact of permanent housing vouchers on homelessness at a 9% greater overall direct costs (i.e. not considering gross fiscal benefits) than the control group. They also found no significant difference in costs between either short-term subsidies or transitional housing and the control group. This study also documents no change in long-run extensive margin employment

[3]Ly and Latimer (2015) review 12 studies of small-scale housing program evaluation (typically with less than 200 total participants), finding general support for a net reduction in costs of UH policies, but with several studies—both quasi-experimental and randomized experimental—reporting insignificant differences in costs or even increases in costs following placement into HF.

among long- or short-term rent subsidy recipients. However, their employment outcomes are coarsely measured at only two distinct snapshots post-event (roughly 2-3 years). Additionally, Gubits et al. (2018) does not incorporate the fiscal impacts of additional benefit absorption nor indirect fiscal impacts through employment effects. Moreover, a significant portion of the control arm in this study voluntarily took up one of the treatment arms, leading to potential attenuation of their results. Culhane, Metraux, and Hadley (2002) study the evolution in other benefits (namely criminal justice and healthcare utilization) absorption at two discrete snapshots following non-experimental placement into PSH, finding average net 6% cost increases.

Zaretzky and Flatau (2013) represents the only work in our review to also study changes in tax payments, imputed based on reported changes in individual income following placement into UH among a very small sample of individuals ($N \leq 20$) in Australia. Flaming, Burns, and Matsunaga (2015) comprehensively characterize the cost of benefits/service absorption among incumbent homelessness individuals in Santa Clara County, California. The authors find substantial heterogeneity in this cost estimate: while they estimate the average annual public costs of persistently homeless individuals at $\$13,661$ per year, they also find that the highest cost-quintile of persistently homeless individuals generate average annual costs of approximately $\$83,000$. Augustine and White (2020) generate similar estimates for the cost of public benefits absorption by "high-utilizers" in Sonoma County at $\$27,000$ per year.

Another closely-related literature studies the first-order effects of housing, eviction, and homelessness prevention policies on homelessness and sheltered-status. In the realm of intervention-oriented policies, Cohen (2024) is one of the most closely related papers to our work. The author studies the impacts of UH-style program recipiency on sheltered-status in Los Angeles, finding that these programs significantly decrease the probability of individuals' future return to homelessness (as well as the usage of other public benefits). In addition, the author finds finds that rapid placement into these programs has a knock-on effect; that is, placement into (semi-) permanent housing within one month of initial services enrollment significantly decreases homelessness 10 and 20 months post-event. We employ similar data to Cohen (2024); however, as a crucial difference for our study, we observe

employment and earnings outcomes as well as California state-level benefits outside of programmatic exit surveys. Von Wachter et al. (2019) illustrate the importance of targeting at-risk populations prior to their entry into homelessness, but emphasize the intensive data and administrative capacities required by this kind of prediction (and prevention) strategy. Abramson (2023) estimates a spatial-structural model and finds a significant negative impact of receiving rental assistance payments on the probability of exiting housing into homelessness (-45%). Similarly, Evans, Sullivan, and Wallskog (2016) finds that randomly receiving rent relief reduces the probability of entering homelessness by 76%.

Research on the labor market characteristics associated with eviction and housing shocks represents a third closely-related literature to our work. Von Wachter, Schnorr, and Riesch (2020) provides a new baseline for understanding the labor market characteristics of homeless individuals, finding an employment rate of 20% among individuals upon enrollment in homeless service enrollment in Los Angeles County; average annual earnings among employed individuals two years out from homeless service enrollment totals to around $\$13,000$. Desmond and Gershenson (2016) follow a representative survey of low-income renters in Wisconsin over time finding that those subject to eviction exhibit an increased 10-20% likelihood of experiencing an employment separation. Jacob and Ludwig (2012) exploit the wait-list structure of housing voucher lotteries and find that housing voucher recipiency among low-income (not typically homeless) families induces a mild decrease in employment and earnings (-6% and -10% respectively) and a 15% increase in take-up of Temporary Assistance for Needy Families (TANF). Our paper is the first to focus explicitly on the labor market impacts of UH-style programs and the fiscal effects associated with these impacts.

The remainder of this paper proceeds as follows. Section 2 discusses our setting, institutions, and data utilized. Section 3 describes the empirical strategy for identifying average effects of treatment on the treated. Section 4 presents our main results and characterizes heterogeneous responses by ex-post employment transition type and family-status. Section 6 performs a simple cost-benefit analysis utilizing our main results and concludes on a discussion of the higher-level validity and implications of our findings.

2 Background, program administration and data

More than 550,000 people can be classified as homeless on any given night in the Unites States (Council of Economic Advisers (2019)). In most of the United States, homelessness is tracked and managed within local administrative units called Continuums of Care (CoC's) under guidance from the US Department of Housing and Urban Development (HUD). The Los Angeles CoC covers almost the entirety of Los Angeles County, and the Los Angeles Homeless Services Authority (LAHSA) contracts a set of homeless service providers to deliver prevention services to and collect information on homeless and at-risk individuals. Homelessness in Los Angeles is particularly widespread with more than 60,000 people experiencing some form of homelessness each night in 2019 (Von Wachter, Schnorr, and Riesch (2020)). The large homeless population and sizable homelessness housing funding in Los Angeles County lend to a suitable environment for studying the effects of placing homeless individuals into housing programs.

2.1 Program administration

LAHSA and other homelessness services providers administer a variety of UH-style programs. We focus on two of the largest and and most-typical UH-style programs: Rapid Re-Housing (RRH) and Permanent Supporting Housing (PSH). PSH provides recipients with long-term, unconditional housing, whereas RRH provides unconditional housing or housing subsidies to recipients on a time-limited, typically two-year time frame (Evans, Phillips, and Ruffini (2021)).

As outlined in the official scope of services documentation, conditional on homelessness status, neither of the programs features any requirements on employment, additional programmatic involvement (e.g. substance abuse support group attendance), or other behavioral requirements beyond standard tenancy rules typical for market-rate units (e.g. noise ordinances at night, rules about domestic animals, etc.; Los Angeles Homeless Services Authority (2025); Los Angeles Homeless Services Authority (2024)).[4] Their administration

[4]Initial program eligibility typically requires that individuals do not earn more than 50% of area median

documentation explicitly states that participants are not to be screened out on such bases. However, both programs stipulate that recipients meet with program administration staff on a monthly-to-quarterly basis during the duration of their tenancy to address ongoing needs.

Importantly, RRH recipiency in LA sometimes features a shallow rent cost-sharing component. Participants with positive income are asked to contribute a *maximum* of 30% of their income towards the cost of rent. However, participants with verified zero-income, status or other stated financial inability, are not required to participate in rent cost-sharing (Los Angeles Homeless Services Authority (2024)), as determined within the discretion of the RRH case worker.[5] This feature has important implications in our setting: namely that LAHSA has an interest in verifying the income and employment status of its recipients with other state-administered social services programs, which we discuss in greater detail in Section 2.2. To the extent that individuals manage to successfully conceal earnings post-housing recipiency, our estimates will understate the true effect of unconditional housing recipiency on employment/earnings. However, the leniency in the rent cost-sharing component may mitigate such concerns.

Placement into either PSH or RRH is generally predicated by an initial homelessness spell. Homeless or at-risk individuals can receive a wide variety of support services from LAHSA. Their first interaction with one such service provider results in 1) assignment to a case worker that helps direct individuals to services, as well as 2) entry into the Homeless Management Information System (HMIS) in a process referred to as "Coordinated Entry". Individuals can work with their case workers to request housing, and are placed into a queue for some form of direct housing treatment based on their homelessness severity and recommendation of their case worker.

The two programs also differ in their target populations. PSH is broadly targeted toward individuals with greater homelessness severity (chronic homelessness, major health issues, etc.) and RRH is targeted toward individuals with lower homelessness severity. We observe these differences in our data, with PSH recipients, relative to RRH recipients, reporting

income.

[5]From our discussions with RRH administrators, recipients can easily opt out of rent cost-sharing and in practice few RRH recipients pay income toward their rent.

worse employment, earnings, and health outcomes, and greater connection to social services and benefits prior to receiving housing. *A priori*, we anticipate largely different potential outcomes and responses to housing between these two groups.

LAHSA determines each client's position in the housing queue solely based on: (1) verification of homelessness status and broad program eligibility requirements, (2) tenure in the HMIS during current spell, and (3) completeness of their application.[6] An individual's position in this queue does not evolve according to updates to the economic/health/etc. status of that individual (conditional on remaining in the enrollment system), but simply follows the order of the queue as new housing becomes available. In LA County, designated RRH housing and matching managers monitor the private (or non-profit) rental market for suitable units (typically studio or one-bedroom apartments) to house individuals. The housing queue evolves at each moment that a housing supplier indicates to LAHSA that they have new or recently-vacated unit for occupancy. LAHSA matching-managers offer the newly available housing to the next eligible client in the queue. If the client declines this offer, the housing is offered to the next eligible client in the queue without affecting the eligibility or queue position for the initially declining client's housing offers.[7]

2.2 Data

We construct our data from the Los Angeles County HMIS combined with data collected from the State of California and Los Angeles County by the California Policy Lab (CPL).[8] Our data cover the universe of individuals interacting with the HMIS in the LA CoC from

[6]This process of determining queue position for housing step represents a central feature of the case worker assignment IV design in Cohen (2024). We do not observe the queue position in our data.

[7]This process of matching clients to housing in RRH differs slightly in LA County versus in other CoCs. In other CoCs, RRH administration is purely tenant-based, where RRH recipients are granted a time-limited defined subsidy amount to rent any unit they find on their own. However, in LA County, RRH is tenant-based, but depends on available units as determined by LAHSA. RRH recipients in LA County *can* find units on their own, but this alternate process requires RRH administrators to separately determine whether the unit is suitable for subsidy.

[8]CPL refers to this collection of data as part of their "Research Accelerator." These data are intended for CPL-affiliated researchers, bypassing standard proposal processes for accessing individual datasets maintained by separate California state-governmental units.

2013 to February 2020.[9]

Data from the HMIS are maintained according to HUD guidelines. Statutorily, CoCs are required to maintain up-to-date data on a variety of client characteristics as they interact with service providers in the CoC (U.S. Department of Housing and Urban Development (2020)). The HMIS reports information on objects such as income, income sources, non-cash benefits, disabilities, and living situation, and sociodemographic characteristics, *inter alia*. We observe these time-varying characteristics on the HMIS interaction-level. As noted above, RRH and PSH recipients are required to meet with program staff on a monthly-to-quarterly basis during their tenancy. Whenever individuals interact with program case workers or other service providers, we observe an update to their earnings, employment, benefits, and other information. As such, we have estimates for each of these outcomes prior to and during their housing tenure.[10]

2.2.1 Misreporting, measurement error, and censoring

Understanding censoring and measurement error are central for ensuring the validity of our inference procedure and estimation strategy. Sample drop-off may be systematically correlated with outcomes, and changes in misreporting of outcomes may threaten the validity of our estimates. We address both of these concerns below.

Income/employment verification, zero-income verification: Importantly, our income and employment data are **not** purely self-reported. Beyond statutory compliance with official data guidelines, CoCs vary considerably in terms of the extent to which they verify and update their client information. Namely, because employment and income verification is costly and often organizationally difficult, many CoCs simply rely on self-reported information to determine program eligibility. For this reason, as B. D. Meyer, Wyse, G. Meyer, et al. (2024) point out, most work on the employment and earnings characteristics of homeless

[9]Though Cohen (2024) uses similar data in Los Angeles, our data is distinct in several key manners. First, we observe employment, wages, and benefits at each interaction, while Cohen only observes most of these outcomes cross-sectionally upon program exit. Additionally, our data do not report case worker identifiers; this identifier is central to Cohen (2024)'s identification strategy.

[10]We also have data on individuals following their housing recipiency if they continue interacting with services connected to the HMIS.

populations rely on purely self-reported measures of such outcomes.

While systematic, mean-zero mismeasurement of earnings or employment does not necessarily threaten identification in our setting, our main concern deals with systematic *changes* in mismeasurement that occur *around* housing recipiency. In particular, we are concerned about the scenario where both: 1) individuals conceal their earnings prior to housing in order to increase their perceived chances of placement into housing and 2) reduce their income/employment concealment post UH-recipiency as they feel less pressure to do so (although there are no statutory features of RRH or PSH that would incentivize this behavior aside from the income eligibility ceiling, which we view as largely non-binding in our setting).[11] While we do not have external data that speak directly to this concern, several features of HMIS data maintenance specific to LAHSA and the LA CoC alleviate this concern. Additionally, several features of our main results and exploration of mechanisms substantiates our arguments that our findings are not driven by changes in measurement error around housing recipiency (see Section 5.1)

Unlike in other CoCs, LAHSA maintains strict guidelines on income and employment verification in determining program eligibility and in updating their records. RRH and PSH official guidelines explicitly outline the process of income and employment verification and "priority of evidence" (Los Angeles Homeless Services Authority (2025); Los Angeles Homeless Services Authority (2024)). Programs prioritize provision of third party income verification in determining participant eligibility, where other social service providers (namely the LA County Department of Social Services) or employers typically serve as such third parties. *Zero*-income verification would, by definition, result in a null-search from these sources. Of course, such records would not capture earnings from informal labor. In the case that third party income verification cannot be obtained by other service providers, programs then prioritize actual observation of homeless status combined with self-certification. Moreover, in the case that other sources of evidence cannot be obtained, self-declaration of income is accepted against penalty of perjury and requires third-parties to document their attempts at

[11] The opposite scenario could also be of concern, whereby individuals truthfully report income/employment prior to their placement into housing and then increase their income/employment concealment post UH-recipiency. This scenario would negatively bias our estimates, although we do not see any mechanisms that would incentivize this behavior.

independent verification (LAHSA Form 1087). Additionally, RRH and PSH require annual recertification of status; our results show no systematic irregularities that occur on part of employment or earnings outcomes at one-year post-event.

It is fundamentally difficult to verify the income and employment of very low- and zero-income individuals. However, the manner in which LAHSA maintains the data environment indicate that our employment and earnings outcomes are not measured via pure self-reporting. These features of our data environment (in addition to some qualities of our documented results and mechanisms)[12] alleviate our concern regarding asymmetric measurement error in income and employment around housing.

Censoring and sample drop-off: Censoring is typically another key concern in studying homelessness. Permanent exit from sample could be attributable to mortality (which is understandably higher in homeless populations, but also due to ceasing interaction with services covered by the HMIS—either due to geographic mobility or exit from homelessness (considering the limited geographic and administrative scope of the HMIS and the separately-operating CoCs), or simply ceasing interacting with social services.

Because we regularly observe individuals during their housing tenure, we are less concerned with permanent exit from our sample as much as with intermittent censoring or censoring pre-event in the form of few HMIS interactions prior to move-in. These kinds of censoring scenarios could possibly reflect periods of detachment from social services, which may censor true, negatively-selected outcomes.

However, features of the HMIS data administration along with our sample construction decisions address these concerns. As we note above, a key feature of UH recipiency is the requirement to interact with program staff with some regularity—typically on a monthly-to-quarterly basis. Individuals that receive UH are by definition housed and are more accessible to reach and interact with by program staff (Cohen (2024)). We indeed observe that UH

[12]Namely, Section 4 and Section 5.1 document 1) asymmetric employment responses between RRH and PSH recipients, 2) systematic changes in labor search behavior that rationalize our results, and 3) systematic changes in more-accurately-measured benefits receipt and earnings based that align with individuals' respective employment transition type (e.g. benefits earnings decreasing in response to finding employment). We view that these results are unlikely to occur in tandem with a systematic change in measurement error around housing receipt. Such mismeasurement would require additional assumptions in order to generate these observed responses, which we view as implausible.

recipients interact more frequently with the HMIS (Figure A.1). To address concerns regarding pre-event censoring, we restrict our main sample to individuals that are interacting with the HMIS throughout a wide time horizon. This restriction comes with external validity costs in omitting populations that are less-attached to the HMIS, for which reason we relax this restriction in a later robustness check.

2.2.2 Sample construction

We construct our main sample beginning with the universe of individuals that receive either RRH or PSH in LA County between 2013 and February 2020. The data is initially structured on the interaction-level. We aggregate all available information in our data to the individual- by month-level. While this decision obscures some of the precision we have available, the vast majority (93%) of individuals have at most one update per month. The subsequent data is structured as a single panel at the individual-month level.

Our final restriction requires that individuals have at least one interaction with either the HMIS or any state-programmatic benefit case worker in both 1) the 7 months leading up to their housing event and 2) the period between 18 and 24 months post-event. Interactions do not necessarily occur every month. We make this restriction in order to mitigate concerns about differential censoring via sample attrition.

This restriction may have important external validity implications. Individuals with frequent observation seven months prior to housing event may either be positively or negatively selected relative to UH recipients that are excluded from our sample.[13] Section A shows that our main sample actually sees *lower* risk scores than the dropped sample, although our data on risk scores sees incomplete coverage within the population of UH recipients. As with nearly all other works that study homeless populations, our analysis is limited to individuals that interact with social benefits and homelessness outreach programs, which prompts further consideration of the external validity more broadly of using administrative data for

[13]For example, insufficient observation and exclusion from our main sample could be attributable to exit from homelessness and ceasing interaction with the HMIS (producing negative selection into our main sample). On the other hand, other scenarios could introduce positive selection into our main sample. For example, individuals that are only observed shortly before their housing event are by definition placed into housing very quickly and could be characterized by greater homelessness severity.

studying homelessness (B. D. Meyer, Wyse, G. Meyer, et al. (2024)). Section 4.3 presents robustness checks that replicate our main result while alleviating this restriction, instead imposing requirements of interaction closer to the housing date.

Table 1 shows summary statistics among three groups of individuals included in our data. The individuals in the first two columns are treated with a UH intervention and comprise our main sample. For reference, individuals in the third column are "untreated" and are generally characterized as at-risk or contemporaneously experiencing homelessness, but who never receive either PSH or RRH in Los Angeles (between 2013 and 2019). Here, "untreated" does *not* mean that an individual receives no services. By design, everyone in the "untreated" group is still receiving some form of short-term intervention unrelated to semi-permanent housing, such as access to emergency shelter, meetings with case workers, health checkups, etc. Untreated individuals are *excluded* entirely from the main analysis.[14]

Individuals in our sample tend to be around age 45. Men, black people, and US armed forces veterans see outsized representation in both the untreated and treated samples. Individuals are overwhelmingly unemployed at time of their first HMIS interaction (in current spell), although less so for RRH recipients. Interestingly, we observe significantly lower employment among RRH, PSH, and untreated individuals than documented by B. D. Meyer, Wyse, G. Meyer, et al. (2024). However, it is not necessarily the case that our sample of homeless RRH and PSH recipients in LA County should align with national averages. This said, our RRH employment estimates quite closely align with those of Von Wachter, Schnorr, and Riesch (2020), who find an employment rate of 19% among individuals in LA County during the quarter they became homeless (likely aligning more closely with our RRH population). Average total monthly income among those that interact with the HMIS is between USD 300 and 450.[15] Approximately 19% of those receiving Rapid Re-Housing are employed at first interaction, whereas individuals receiving PSH and untreated individuals see even lower employment rates at around 7-8%. Among those employed, average total earnings are

[14] Section 4.3 features a robustness check that uses these untreated individuals as a "never-treated" comparison group in estimating event studies following Borusyak, Jaravel, and Spiess (2024) so as to account for the possibility of heterogeneous treatment effects under staggered event timing across treated units. The results align with our analysis but demonstrate some time-invariant difference in outcomes.

[15] All data denominated in US Dollars are expressed in January 2020 USD.

only around USD 1200-1500 per month upon initial interaction with HMIS, which aligns closely with both B. D. Meyer, Wyse, G. Meyer, et al. (2024) and Von Wachter, Schnorr, and Riesch (2020). Most individuals are homeless for 1-3 years prior to receiving some form of long-term housing intervention, although wait times are shorter for RRH recipients.

3 Empirical framework and estimation strategy

To study the effect of treating homeless individuals with RRH or PSH, we estimate a series of event studies around the placement of individuals into one of these housing programs. Our main outcomes-of-interest include whether an individual is employed, as well as their earnings, and benefits take-up for select programs.

Our main specification estimates regressions with two-way fixed effects on the month- and individual-level of the form:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}, \tag{1}$$

for individual and month effects $\{\alpha_i, \delta_t\}$ and a mapping $q(t)$ of month to quarter of the year.[16]

Leveraging the quasi-random variation in timing of housing recipiency yields coefficients $\{\hat{\beta}_j\}$. These estimates $\hat{\beta}_j$ correspond to an Average Treatment Effect on the Treated (ATT) under the following assumption: 1) $E[y_{ij}|j < 0] = \alpha_i + \delta_j \ \forall \ j < 0, \ i$ (parallel trends and no anticipation). Violations to quasi-random timing would result in observably non-parallel pre-event trends, for instance. We interpret this effect as an ATT specifically for the impact of housing under an additional assumption, 2) that at event time, no other changes occur. We view this second assumption as central in identifying the ATT of housing, as it may be the case that upon connection to housing, individuals may also be connected to other services

[16]The precise date of entry into UH accommodations sees some reporting error. In 15% of our housing events, we observe a client-reported move-in date in addition to the statutory entry date recorded by the case worker; Figure A.3 illustrates important discrepancies between these dates, suggesting the presence of potential measurement error in the event month. To accommodate this problem, we specify our main reduced forms as quarterly averages on the monthly level.

that also affect outcomes. We explicitly investigate this possibility below in Section 5.1; we provide evidence that this assumption indeed holds in our setting.

As discussed in Cohen (2024), individual wait times between entry into the HMIS and eventual placement into housing are often determined based on reasons unrelated to individuals circumstances or outcomes, such as 1) ability of the assigned case worker (which Cohen (2024) demonstrates is assigned seemingly quasi-randomly), 2) supply of available housing units, and 3) demand by other individuals of housing units. Table 2 shows a series of univariate regressions of wait times on individuals demographic and economic characteristics, showing that in our setting, none of these characteristics exert any substantial explanatory power over wait times. From the table, we cannot reject the null-hypothesis of *no* relationship between wait times and economic and homelessness severity-related outcomes, as well as with age, gender, and racial background. However, some sociodemographic characteristics predict slightly shorter or longer wait times. Guardians and veterans wait 5.4 and 2.7 months less on average for RRH, and individuals reporting mental health or substance abuse issues tend to wait 2-4 months longer for both RRH and PSH. Additionally Figure 1 Panel (a) shows explicitly that there is no observable relationship between individuals' risk score assessment the time elapsed between initial intake and placement into housing.[17] Importantly however, our identification assumption does not depend on homogeneous outcomes.

This design also addresses concerns surrounding potential mean reversion. Namely, individuals likely experience a significant negative earnings shock (e.g. disemployment) that induces them into homelessness. In this case, a measurement/attribution problem arises because at this point, individuals interacting with the HMIS are necessarily near the bottom of the income/employment distribution and so, mechanically, their outcomes cannot further deteriorate. Two points alleviate this concern. First, Figure 1 Panel (b) shows the distribution of timing between initial HMIS interaction and housing event for both eventual RRH and PSH recipients. By construction, all individuals in our main sample had at least

[17]The County of Los Angeles assigns individuals this risk score, the Vulnerability Index - Service Prioritization Decision Assistance Tool (VI-SPDAT), based on their personal situation and characteristics in order to prioritize them for these different housing programs. Though VI-SPDAT serves as a tool for UH prioritization, there is no evidence that it determines an individual's position in the housing queue to which they are referred.

6 months elapse between entry into the HMIS and their placement into unconditional housing.[18] The distribution of wait times sees wide support, ranging from 6 months to five years (modally five quarters). Moreover, the distribution presents no discontinuities which could suggest manipulation or incomparability of recipients. We argue that individuals undergoing these wait times would have already experienced a substantial portion of their eventual mean reversion (i.e. individuals are *not* placed into housing at their *lowest* point). Second, we further falsify the presence of general mean reversion by demonstrating parallel pre-event trends. Mean reversion could manifest in the form of a significantly positive trend leading into housing events. Our designs show that this threat does not pose a concern.

We run event studies as specified by Equation (1) on individuals in our sample that receive exclusively either RRH or PSH between January 2014 and February 2018, binning observations that occur more than 13 months prior to or 25 months after placement into housing. assume earnings We treat as the event the earliest instance of housing program recipiency for each individual and require individuals not receive either RRH or PSH prior to January 2014.[19] We estimate our specification separately for RRH recipients and PSH recipients, so that each set of coefficients $\{\hat{\beta}_j\}$ corresponds to estimates of the ATT for each respective program.

We interpret the sequence of $\{\hat{\beta}_j\}$ for each program as the *within*-program ATT of that respective UH-style housing program. On a fundamental level, RRH and PSH represent programs intended for two entirely separate recipient populations. Namely, those receiving PSH are determined to have little-to-no capacity to work and are more negatively selected than those receiving RRH, as evidenced by both the stated program requirements/goals and

[18] We relax this restriction in Section 4.3. Cohen (2024) uses a slightly different primary sample by construction, as he studies individuals that are placed quickly into housing by a "better" case worker (measured by the speed by which they place other clientele into housing). In his case, the treated sample consists primarily of individuals that receive housing within one month of intake. It is unclear whether our sample is negatively or positively selected relative to this benchmark, although Section A shows that both RRH and PSH recipients in our main sample have slightly higher risk scores compared to those that were dropped by the requirement to be interacting with the HMIS sufficiently before and after one's housing event. Figure 1 shows that there is no clear relationship between wait time and homelessness risk score.

[19] We observe that 81.7% of individuals receive UH benefits only once, 14.6% two times, 2.9% three times, and .8% at least four times. In order to avoid positive selection, we refrain from restricting our sample to individuals that receive housing support only once between January 2014 and February 2018.

by the simple differences in observable characteristics as reported in Table 1.[20] We should *a priori* suspect largely different labor responses for these two populations. Second, the treatment itself differs fairly drastically between the two programs. While both programs are intended to provide fully subsidized housing, those receiving PSH are often expected to continue absorbing the subsidy *ad infinitum* and, outside of extreme circumstances, cannot see this subsidization revoked.

By construction, our data do not capture individuals that never interact with LAHSA or the HMIS. Most clients interact with the system via voluntary walk-in to service provision centers, through referral via an interaction with another public service, or through street outreach. We anticipate that the population of homeless individuals that never interact with the system feature even greater negative selection on outcomes than our observed population. We further discuss interpretation issues related to external validity in Section 6.

Our main outcomes of interest include the following: 1) whether an individual reports or is administratively observed as employed; 2) employment earnings; 3) total benefits income, i.e. the amount of pecuniary benefits received in total from the following programs: SSI, SSDI, Unemployment Benefits, TANF, Veteran Affairs assistance, Social Security, and General Assistance from LAHSA entities; 4) "other" category income (the aggregation of worker's compensation, private disability insurance payouts, pension payments, child support, alimony payments received, and unallocable income)[21]; 5) an indicator for whether an individual takes up insurance from any one of Medicaid, SCHIP, Medicare, or Veterans Affairs); and 6) whether an individual takes up any of the following nonpecuniary benefits: SNAP, WIC, TANF Childcare/Transportation, or another unallocated nonpecuniary benefit.

4 Results

We document large positive effects of housing recipiency on labor market outcomes, particularly for recipients of RRH. Figure 2 illustrates the evolution in mean extensive margin

[20] Additionally, because the program offers truly permanent unconditional housing, PSH likely induces substantially larger income effects than does RRH.

[21] For earnings and cash benefits amounts, which we observe less frequently in our data, we impose constant earnings between observations.

employment around placement into UH. The showing a stark increase from 15% to nearly 25% for RRH recipients around move-in, but a more muted response among PSH recipients ranging ranging between 5% and 6.5% with a mild positive trend leading into the event. Section 7 elaborates on this result, plotting estimates from the event studies following Equation 1 following placement into both RRH and PSH. Table 3 - Table 4 summarize these results in relation to pre-period baselines. These tables omit the estimated effect at event time in order to prevent picking up effects due to increased reporting upon move-in.

4.1 The Effects of RRH

Panel (a) Section 7 shows our event study estimates for RRH. These figures show that employment, earnings, and benefits outcomes all exhibit no differential pre-event trends. Therefore, as established in Section 3, the coefficients plotted in the post-period should be interpreted as the ATT of placement into RRH. We test and confirm that these effects are not driven by alternative treatments concurrent with placement into housing in Section 5.1.

RRH substantially improves labor market outcomes of its recipients. Following Table 3, recipients see an average extensive margin employment rate increase of 10.8 percentage points relative to a pre-period baseline of 20% (an increase of 54%). On average, RRH recipients also see increased total incomes of around USD 210 per month—a 47% increase from pre-period levels. Of this average increase in total income, about 65% comes from increases in earned income and most of the remainder from benefits income. On the intensive margin, individuals employed both before and after receiving housing saw their incomes increase by 20% on average. We also observe a relatively small increase in benefits income that decreases in magnitude over time, becoming indistinguishable from a null-effect two years post-event. Importantly, these results obscure heterogeneity by employment transition subpopulation type (explored in Section 5.1, where we also show that benefits do not change among individuals with earnings).

Table 4 displays analogous results for aggregated programmatic benefits (see Table B.1 for results on more disaggregated programmatic benefits). These results show that recipients see substantial increases in their take-up of some of these other benefits. These nonpecuniary

benefits include SNAP and TANF Childcare/Transportation, in addition to programs like medicaid, medicare, and WIC. After receiving RRH, individuals see a near a substantial increase in the probability of receiving pecuniary benefits (+9.1pp from a baseline of 40.2pp), but no increase in the probability of receiving non-cash non-insurance benefits from a prior state of not receiving any of these benefits. RRH recipients see a substantial increase in the probability of receiving insurance benefits (+20.9pp from a baseline of 57.7pp) which drives nearly all of the increase in access to health insurance following placement into housing, with 80% of this response driven by increased connection to Medicaid.

Overall, placement into RRH results in large improvements to labor market outcomes and increased take-up of some social programs. Increased take-up of social benefits is likely driven by two separate effects: the newly attained access to a domicile and permanent address as well as increased interfacing with LAHSA. This latter effect likely manifests as the pre-intervention trends visible in some of the figures, namely as an anticipation effect in which individuals expect to receive housing in the near-future and are being directly connected to programs through their assigned case prior to actual RRH recipiency. We attribute the increase in employment and earnings to housing recipiency (as opposed to connection to benefits), as the changes in benefits or benefits income does not respond to housing among the subsample of individuals finding employment (see Section 5.1).

4.2 The Effects of PSH

Panel (b) of Section 7 shows our event study coefficients for PSH. The results for PSH recipients demonstrate some significant contrast with those for RRH recipients. Namely, earnings and employment responses among this group exhibit substantial noise: the scale of these responses and their proximity to zero fairly confidently suggest a null-result for PSH. However, PSH recipiency is accompanied by a significant increase in benefits income by USD 114 against a baseline of USD 380 (+30%), although we observe a minor pre-event increases in benefits income.

Panel (b) of Table 4 shows that PSH recipients also see a large increase in other programmatic benefits. There are a few key differences in benefits absorption responses for

PSH recipients when compared to RRH recipients. In particular, we observe an increase in non-cash and non-insurance benefits take-up among this group (4pp from a baseline of 54pp) as well as increased take-up of LAHSA-provided health services, such as HIV/AIDS management and prevention services, mental health services, and substance abuse treatment, although evolution in take-up of these services also exhibit substantial increases leading up to housing.[22] We observe an even greater share of increased insurance coverage attributable to Medicaid (94% of the increase in any insurance coverage) for PSH, as well. Overall, PSH seems to have little-to-no effect on employment/earnings, but still has fairly large effects on benefits absorption and connection to other services. We discuss the differences between PSH and RRH in greater detail in Section 6.

4.3 Robustness

We identify three primary extensions to corroborate our main results. First, our sample restriction requires sufficient observation of UH recipients before and after UH recipiency. As we discuss in Section 2, this restriction may introduce external validity concerns. For this reason, we replicate our main results using a broader sample of UH recipients that relaxes this requirement, only requiring their interaction with homelessness service providers within two months prior to and between 7 and 12 months following their UH recipiency. As such, we replicate our main results on this sample within a more narrow timeframe in which recipients are interacting with the HMIS. This weaker sample restriction yields 6,298 RRH recipients and 3,988 PSH recipients and includes individuals that either enter the HMIS closer to their entry date (i.e. receive housing more quickly) or that exit the HMIS more quickly.

Figure B.2 replicates our main results from Section 7 for this alternative sample. The results of this estimation strategy yield estimates that are qualitatively and quantitatively similar, if not giving slightly stronger employment responses. We observe a 10 percentage point increase in employment of RRH recipients as well as a small and initially significant 1pp increase in employment among PSH recipients. We observe similar results for the earned and benefits income of UH recipients, although RRH recipients exhibit a mild pre-event increase

[22] See Table B.1.

in benefits income absorption of 20 USD per month between two quarters prior to housing to their event time, peaking at 40 USD one quarter post-event.

In the second robustness check, we re-estimate extensive margin employment effects using an alternate variable that interpolates extensive margin employment values between HMIS interactions by "carrying forward" employment status. Our main variable for extensive margin employment is mapped to missing during months without an interaction. Given the different scenarios for censoring we describe above, it isn't obvious the kind of bias that our censoring may introduce. As such, implementing this "carry forward" interpolation of employment status between interactions represents an assumption that UH recipients inform their case workers whenever their employment status changes: and absent of any new information their employment status has not changed. We view this scenario to represent a logical and desirable alternative to test, as if our data environment is operating as intended (i.e. picking up changes in employment, benefits, and earnings whenever they occur), this interpolated measure of employment should represent a better measurement of extensive margin employment.

Figure B.3 displays the results for RRH and PSH recipients for this interpolated measure of extensive margin employment. We find qualitatively and quantitatively similar results. For RRH recipients, employment increases by an additional 9 percentage points one quarter post-event. One difference is that with this interpolated measure of employment, this employment increase among RRH recipients demonstrates more stability over time than in our main specification, only beginning to mildly decrease from its peak beginning 1.5 years post-event. Our results for the extensive margin employment outcomes of PSH recipients are nearly identical to as in our main specification: we observe a mild and noisy pre-event increase in employment by around 1pp, but little post-event movement in employment status.

Lastly, we also replicate our main event study estimates using the alternate event study methodology of Borusyak, Jaravel, and Spiess (2024). This methodology uses a never-treated sample of individuals in the HMIS data to provide an alternate estimation baseline group. In addition to addressing concerns about bias introduced by heterogeneous effects using two-way fixed effects estimators, the inclusion of a never-treated group can also address concerns

about mean reversion and counterfactual evolution in outcomes if UH recipients were instead never-treated units. In our main specification (without a never-treated group), our estimates may be susceptible to capturing mean-reversion in the longer-run: we observe individuals likely following negative shocks (e.g. to health or employment) that result in homeless status; in the absence of intervention with a UH policy, individuals may mechanically be more likely to exhibit an increase in their extensive margin employment.[23]

Figure B.4 shows our results. The results of this estimation procedure are largely consistent with our main results, but reveal some differences of note. We observe that RRH recipients see on average 2pp higher pre-event employment than never-treated units. Compared against this baseline, they exhibit a 6pp increase in extensive-margin employment, slightly smaller than in our main results. This employment increase exhibits a mild post-event decline similarly to as in our main results. Earnings of RRH recipients is identical to as in our main results, and the increase in benefits is similar in magnitude but now appears more stable post-event. The results for PSH recipients are largely unchanged, although we observe persistent differences in benefits income received by PSH recipients versus never-treated individuals. We interpret these results to corroborate our main results, alleviating potential concerns 1) about mean reversion and 2) about bias arising from estimation of treatment effects with two-way fixed effect estimators under heterogeneous treatment effects and staggered treatment timing.

[23]Following Borusyak, Jaravel, and Spiess (2024), never-treated and treated individuals are assigned to common cohorts based on their entry in to the HMIS. Potential outcomes for treated individuals are calculated using the outcomes of never-treated individuals within the same cohort group based on calendar time and individual fixed effects. The treatment effect for each individual post-event period is calculated as the difference between their realized outcome and predicted potential outcome at each time period. The average treatment effect at each post-event time period is constructed as an equally-weighted average of individual treatment effects.

5 Mechanisms and heterogeneity

5.1 Mechanisms

We now turn to exploring the mechanisms that drive our results. Our aim is twofold. First, we aim to separate out the distinct channels of housing and connection to additional social benefits that could occur simultaneously upon placement into UH. It may be the case that placement into UH situations generates benefits to its recipients both from shelter and through connection to additional social services at the same time. Although this possibility does not threaten the internal validity of our design or estimation strategy, separating between these channels will help us understand to what extent our observed impacts are attributable to the shelter and domicile value specifically of having housing (e.g. having an address, having a stable domicile, etc.). Second, we want to elaborate on our results by informing how employment-search and service receipt for mental health issues and substance abuse evolves around UH receipt and how earnings respond conditional on different employment-transition types.

We start by stratifying our event studies by ex-post employment transition type: we can further parse mechanisms by studying which kinds of incomes and benefits responded conditional on certain employment transition types. For example, we can rule out the possible social benefits channel of housing reciept if social benefits do not respond to housing receipt conditional on employment. We primarily focus on unemployment-to-employment ("U2E") and employment-to-employment ("E2E") transitions here following treatment with either RRH or PSH, as these are the margins that feature movement into employment.[24][25] Since employment can fluctuate from month-to-month, we define "employed" in the pre-event period as being employed in 80% or more of the pre-period sample and "unemployed" as being employed in 20% or less of the pre-period. This definition introduces important sample size

[24] Section C displays results for other transition types.

[25] Table C.1 displays coefficients of regressions predicting these ex-post employment transition types based on observable characteristics dealing with age, race, health status, and homelessness severity. For instance, non-white recipients of both RRH and PSH were less likely to make transitions into employment (from either unemployment or employment). Other predictors of transition into employment include homelessness severity, age, and controlled substance use.

limits,[26] but indeed help us identify which income/benefits margins respond conditional on employment. Unemployment and employment are defined analogously in the post-period. By construction, there are some individuals that we can say nothing about (e.g. those who were employed for 50% of the pre-period, for instance). We can precisely identify the employment transition type for 69% of the RRH sample. The remaining 31% have employment fluctuations that we cannot decisively categorize into one of these mutually exclusive ex-post employment transition types.

Panel (a) of Section 7 shows outcomes related to U2E transitions following RRH enrollment (we study more stylized disaggregations in Figure C.1). Individuals characterized by U2E transitions secure employment almost immediately in most cases and see their monthly earnings increase by USD 800-1000. Importantly, these figures show *no* measurable concurrent increase in pecuniary or non-pecuniary benefits other than procurement of insurance.

This finding is central in determining our interpretation of the main results documented in Section 7. This result implies that the observed increase in employment and earnings among RRH recipients is attributable to receipt of housing, as opposed to other concurrent treatments benefits. However, this is not to say that receipt of housing does not drive (some) connection to other services and benefits. Figure C.3 and Figure C.8 display the results of U2U transitioners, demonstrating that there *is* some role housing recipiency has in facilitating connection to additional benefits. Table C.2 summarize these responses. Overall, our interpretation of the main RRH result is that RRH causally induces an increase in employment/earnings *and* benefits uptake. Among individuals finding employment, we attribute the underlying mechanism to housing recipiency and not to connection to other services. However, among individuals that do *not* find employment in the post-period, increases in income are driven by connection to services and programmatic benefits upon housing receipt.

We show analogous results for E2E transitions following RRH enrollment in panel (b) of Section 7. Individuals who were previously employed (and remain employed) increased their earnings by an average of around USD 200-300 which accounts for the entire increase in their

[26] E.g. if we observe a 10pp increase in employment for our 1,707 main-sample RRH recipients, a maximum of 170 individuals could be tagged as U2E transitioners. This definition further reduces the true number of individuals that we tag as U2E transitioners.

total monthly income, as they exhibit no change in benefits income nor the probability of receiving any new non-cash- or cash-benefits (see Figure C.2). We are unable to disentangle whether this increase in earned income is the result of individuals taking on more hours, a better job, or both, since hours, employer, job title, etc. are not available in our final data.

Section 7 shows outcomes following U2E and E2E transitions, respectively, for PSH recipients. We can precisely identify the employment transition type for 87% of the PSH sample. The remaining 13% have employment fluctuations that we cannot decisively categorize into one of these ex-post employment transition types, as described at the beginning of the section. Earned income increases by around USD 500 per month whereas benefits income exhibits a more mild increase of around 200 per month. Among U2E transitions, we observe no increase in the probability of receiving new nonpecuniary or pecuniary benefits. For PSH recipients classified ex-post as E2E transitioners, we document no increase in earned income. Among this group, we observe a slight decrease in take-up of any pecuniary or programmatic benefits.

Beyond employment transitions, we can substantiate our results by estimating our designs using "looking-for-work" status. Anticipation of placement into housing may influence job-searching behavior. Moreover, our observed non-response of employment among PSH recipients may seem counterintuitive in light of the negative income effects of truly permanent receipt of housing. We attribute this observation to fact that pre-event employment among PSH recipients is around 5%, so mechanically there may be little scope for decrease. Figure 6 Panels (a) and (d) show estimates of Equation (1) using an indicator for "looking-for-work" status as the dependent variable.[27] Panel (a) indeed shows a substantial pre-event increase in RRH recipients' looking-for-work status on the order of nearly 10pp between about nine-months pre-event and event-time, after which job-search status mildly decreases. For PSH recipients, however, we observe a compelling decrease in job-search pre-event. We interpret this result as a negative income effect of housing receipt: while PSH recipients are largely unemployed pre-event, their labor supply mechanically cannot decrease. But, their labor search does decrease. We emphasize that according to official LAHSA documentation out-

[27]This variable is only populated conditional on being unemployed.

lining the administration of these programs, allocation to these programs does not depend on having a job or job-search behavior (Los Angeles Homeless Services Authority (2025); Los Angeles Homeless Services Authority (2024)), so we do not interpret these results as nominal reporting responses.

Panels (b)-(c) and (e)-(f) show the results for RRH and PSH whether an individual is receiving services for with either mental health issues or substance abuse issues respectively. It is not obvious to what extent receiving services pertaining to these issues indicates status improvements[28], but we view these outcomes as suggestive in ruling out other mechanisms. Namely, for RRH recipients, we observe very minor changes around housing recipiency in the probability they receives services for either mental health or substance abuse issues. The magnitudes of responses vary between +0.5 and and -2 percentage points, indicating that our results are not driven by connection to mental health or substance abuse services. We *do* observe a compelling increase in connection of PSH recipients to these services upon move-in that endure for 3 quarters.

5.2 Heterogeneity by sociodemographic characteristics

Our data also provide information on a variety of socioeconomic characteristics, which we exploit to perform heterogeneity analyses. Our data allow us to identify six margins of heterogeneity of interest: 1) gender, 2) race, 3) family/guardian status, 4) history of mental health issues, 5) history of substance abuse, and 5) veteran status.[29] Prior work has highlighted the sociodemographic heterogeneity in the prevalence and severity of homelessness (e.g. B. D. Meyer, Wyse, and Corinth (2023) Montgomery (2021)). Von Wachter et al. (2019) also emphasize the role of efficiency and targeting in designing optimal homelessness and poverty alleviation policy. By estimating our designs along these margins of heterogeneity, we aim to we speak to both literatures on optimal targeting and on differential

[28] Our data also feature indicators for reporting adverse mental health or reporting substance abuse issues; however, these variables are not systematically measured prior to placement into housing.

[29] Unfortunately, our data do not allow us to identify other potential margins of heterogeneity that have been documented as experiencing outsized homelessness severity, such as individuals of former criminal conviction status or of transgender identity (Glick et al. (2020)). Other such fields either do not exist in our data or see poor population (also education status).

impacts.

We estimate the following equation:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}, \tag{2}$$

where $Status_i$ is an indicator for whether individual i is categorized as belonging to the sociodemographic status of interest. This equation is a simple variation of Equation (1) that includes an interaction term $\sum_{q(j)\neq -1} \psi_{q(j)} Status_i \cdot \mathbb{1}\{EventTime_{q(t(i))} = q(j)\}$ so as to capture the differential employment response based on membership in the given sociodemographic group. We continue to stratify by UH program. Each of these margins except for family/guardian status are directly-reported. For family/guardian status, we distinguishing between heads-of-household with or without families based on satisfying *one* of the following conditions:[30] 1) in individual i's household ID number, there is at least one other distinct individual j that is identified as a minor (capturing both dual- and single-parent households); 2) individual i's household ID number contains at least three distinct individuals (in case we do not directly observe minor status within a household). We tag adult individuals satisfying *either* of these two conditions as "guardians" and adult individuals satisfying neither of these conditions as "non-guardians". Moreover, for studying this margin, in order to ensure proper comparison, we restrict our analysis to individuals that identify as heads-of-household.

Figure 7 and Figure 8 displays the heterogeneous employment responses for RRH and PSH respectively, and Section D presents tables that summarize these responses and additional outcomes. For RRH recipients, we observe a mildly larger employment response for women relative to men. We observe no compelling difference in employment responses to RRH recipiency based on guardian status, racial/ethnic background, or pre-event reported substance abuse problems. We observe a mildly lower employment response for individuals that prior-to-event reported mental health problems, and a substantially lower employment response for individuals with veteran status, which may be by age or health issues.

[30] All individual IDs are also assigned a separate household ID regardless of their family/household status.

For PSH, we observe a significantly larger employment response among guardians relative to non-guardians and, consistent with the results for RRH, a mildly increase in female employment response relative to men. Unlike for RRH, we observe a mildly larger employment response to PSH recipiency for veterans than for non-veterans. We observe no differential response for individuals reporting adverse mental health status prior to move-in. Lastly, we observe a relatively lower employment response of PSH recipients that are non-white and that reported substance abuse problems prior to move-in.

6 Discussion and conclusion

6.1 Net fiscal impacts of unconditional housing

We can apply our findings on the labor market impacts of these UH-style programs to more precisely inform the net fiscal costs and benefits of these programs. We conceptualize the social budgeter's net flow cost/benefit of extending housing to a homeless individual i in a simple manner. First, for an individual i's housing state $\xi \in \{h, s\}$, homeless or sheltered respectively, and "skill-type" θ_i that indexes ability to recover from homelessness and its associated adverse states, we express their fiscal flow as:

$$\tau_i(\xi, z(\xi, \theta_i); \theta_i) - b_i(\xi, z(\xi, \theta_i); \theta_i) - e(\xi),$$

for some level of taxes paid τ_i, state-benefits absorbed b_i (direct programmatic benefits as well as other public service system usage such as medical or criminal justice services), income z, and homogeneous social and environmental externalities e (e.g. crime, environmental impacts, and their associated capitalization into land values and property taxes, etc.). Moving an individual from a homeless to a sheltered housing state at a flow cost c results in the

social budgeter's non-welfare-weighted net flow cost/benefit of

$$\begin{aligned} CB_{\theta_i} &= (\tau_i(s, z(s, \theta_i); \theta_i) - \tau_i(h, z(h, \theta_i); \theta_i)) \\ &\quad - (b_i(s, z(s, \theta_i); \theta_i) - b_i(h, z(h, \theta_i); \theta_i)) \\ &\quad - (e(s) - e(h)) - c \\ &:= \Delta\tau_{\theta_i} - \Delta b_{\theta_i} - \Delta e - c. \end{aligned}$$

We quantify the net cost/benefit as the difference in individual taxes paid, less the change in the value of environmental externalities less the change in benefits absorbed between states. In our framework all heterogeneity across individuals is subsumed by skill-type θ.

Substantial attention has been placed on quantifying the average change in benefits absorption from $\mathbb{E}[\Delta b]$. While no work to our knowledge has identified this parameter in a context that simultaneously features 1) a large sample size, 2) frequent observation over time, 3) comprehensive observation of benefits absorption, and 4) compelling causal identification, extant research suggests significant fiscal benefits through this channel. Culhane, Metraux, and Hadley (2002) find a UH cost-offset of 20% through changes in shelter use, hospitalization, and incarceration during program tenure. Zaretzky and Flatau (2013) estimate cost-offsets through changes in health, criminal justice, and welfare service absorption equal to 35% for men receiving supported accommodations. No works to our knowledge have attempted to estimate $\mathbb{E}[\Delta e]$.[31]

We instead place our focus on $\mathbb{E}[\Delta\tau]$: the extent to which unconditional housing costs are offset through their impacts on labor market outcomes. In doing so, we consider how different labor market transition types impact earnings and therefore federal income tax payments, Earned Income Tax Credit (EITC) payments, sales taxes, and payroll taxes.[32]

We run regressions of the form $y_{it} = \alpha_i + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + u_{it}$ and input the coefficient estimates $\hat{\beta}$ and the p-value of the post-pre difference into Table 5. We assume that individuals reporting employment earn income in the formal labor market in a manner

[31] Additionally, we are not aware of any works that consider the general equilibrium effects of homelessness interventions on the rental market.

[32] We consider 2017 as our year of cost-benefit analysis.

subject to general labor income taxes. As an illustration of how we incorporate changes in tax receipts, consider an RRH recipient categorized ex-post as an E2E transitioner. We estimate that they increase their total formal annualized income from USD 11,615 to USD 16,847. We assume individuals earn no capital income and pay payroll and sales taxes according to imputations in Piketty, Saez, and Zucman (2018). We assume that these individuals pay income taxes as single filers, claim the standard deduction (valued at USD 6300 for single-filers prior to the Tax Cuts and Jobs Act (TCJA) in 2017, although the TCJA subsequently doubled the exclusion limit), and that 75% of filers claim the EITC (as single adults), corresponding with publicly available IRS estimates. EITC claimants in our sample cease receiving EITC benefits at this earnings level in 2017 (a decrease from USD 236) and pay USD 786 more in Federal Income taxes (applying the 2017 standard deduction and income tax rate). According to Piketty, Saez, and Zucman (2018), these individuals pay a combined 5% and 10% of their income on sales and payroll taxes respectively, generating an additional USD 523 in payroll taxes. These individuals pay USD 261 and 274 more in sales tax (for EITC non-claimants and claimants respectively). Therefore, among E2E transitioners, tax payments increase on average by USD 953 per year.[33]

The Los Angeles Housing Authority budgets rental costs on efficiency units (Single Room Occupancy (SRO) or studio units) at USD 18,324 per year. Assuming an outside option of investing these funds at a 4% annual return, this figure rises to USD 19,000. Therefore, during the program's two-year tenure, the labor market impacts among E2E transitioners offset 5% of the recurring programmatic cost of RRH through earnings externalities onto sales taxes, payroll taxes, and federal income taxes. We perform this calculation for other employment transition types for both RRH and PSH and combine the estimates using the proportion of recipients composing each respective ex-post employment transition type to calculate an average cost-offset attributable *solely* to labor market responses on average.

The results of Table 5 illustrate how the fiscal externalities through the labor impacts of unconditional housing weigh against the costs of RRH and PSH during program tenure.[34]

[33]This estimate ignores the interaction of heterogeneity in earnings and the nonlinearity of the income tax schedule, as well as with the nonlinearity of the EITC benefits schedule.

[34]These tables omit confidence bands, as the only uncertainty/heterogeneity comes from the estimated change in income.

The table reveals several novel facts. 1) For both programs, the average cost-offset through earnings effects is very small. During program recipiency, the labor market impacts of RRH and PSH offset the recurring cost by 0.96% and 0.01% respectively. 2) The amount of disemployment induced by receiving unconditional housing is largely outweighed by the positive employment effects (E2E and U2E transitioners outnumber E2U transitioners by a factor of between 2-5); this fact contrasts with previous findings positing net disemployment effects of unconditional housing recipiency via income effects (e.g. Jacob and Ludwig (2012)). 3) While the net labor market fiscal externalities are small, they are not negative. In light of these observations, the contrast in fiscal externalities based on post-event employment perhaps also highlights the role of targeting and homelessness prevention in optimal policy design (Von Wachter et al. (2019)).

Less immediately evident are the fiscal implications following program tenure. Whether the program induces permanent exit from homelessness *and* housing support has key implications for fiscality. Because PSH housing recipiency *is* indeed unconditionally permanent, this concern is less relevant for PSH recipients and we instead focus on RRH. We do not directly observe housing status following program exit. In order to address this issue, we infer recidivism into homelessness based on observation within the HMIS at least two years subsequent to program entry, by which time RRH program tenure will have ended. 10% of U2E and E2E transitioners continue to interact with the HMIS two years post-event. Assuming that 90% of U2E and E2E transitioners both do not recidivate into homelessness and maintain their post-event employment behavior, with no evolution in real earnings, the fiscal externalities of *solely* the labor market impacts of RRH for individuals employed post-event cover the gross cost after approximately 16 years.

Ultimately, we find that the cost-offset of unconditional housing programs through labor market responses are small in comparison to those through public service absorption. Moreover, we find significant heterogeneity in the overall net fiscal impacts both between RRH and PSH, as well as over their recipients. This substantial cost/benefit variation by ex-post employment transition type underscores the relevance of more recent work on targeting homelessness-prevention and assistance (Von Wachter et al. (2019)). Of course, this

discussion entirely foregoes the normative social welfare considerations of moving individuals out of homelessness.

6.2 Discussion

Our main results illustrate substantial, but widely heterogeneous impacts of RRH and PSH on the labor market outcomes of their recipients. We find overall positive effects of RRH on average extensive margin employment probability, labor earnings, and benefits absorption. Most notably, individuals placed into RRH see a nearly 55% increase (10.8 percentage points) in their probability of finding employment. Earnings income increases by 73% (USD 137) and benefits income increase by 30% (USD 74) on average.

Among individuals that find employment post-event, monthly income increases by around USD 800-1000 with no concurrent increase in benefits income; even individuals employed prior to their placement into RRH see increased earnings by nearly USD 200 per month. However, RRH recipients that see stable employment in the post-event period only form about 11% of the treated sample. Individuals that do not see stable employment in the post-event period do not report increased labor earnings, but rather see their benefits income increase by around USD 100 per month.

This result on heterogeneity by ex-post employment transition types reveals important insight into mechanisms that allows us to address the threat of simultaneity. Because we observe that individuals finding employment in the post-period see *no* concurrent increase in benefits, we attribute our observed ATTs on employment and earnings to access to a stable permanent shelter. This said, the observation that benefits receipt increases among individuals unemployed in the post-period indicates the scope for UH programs to play in connecting individuals to benefits.

We document more muted effects for PSH recipients, which we argue highlight the differences in treatment unit selection between the two programs (given that the programs operate essentially identically for the first two years). PSH recipients report no compelling increase or decrease in probability of being employed. Instead, PSH recipients see a larger

increase in their benefits absorption upon connection to unconditional housing relative to their RRH treated counterparts. One might find the lack of a discernible decrease in earnings and employment somewhat striking due to the potential size of the income effects associated with receipt of truly permanent supportive housing.

Overall, we interpret our estimates as Average effects of Treatment on the Treated (ATT), and in this way do not represent the impact of extending program recipiency to the marginally homeless individual. Considering this distinction, our two programs of interest are very different; RRH is designed with the intent of targeting individuals with lower homelessness severity, whereas PSH is more targeted toward individuals with greater homelessness severity and health risk. Moreover, our analysis—as with nearly all other quantitative studies on homelessness—is limited to studying individuals that interact with homelessness service providers, and so likely studies a positively selected population relative to homeless individuals disconnected from social services.

6.3 Conclusion

We exploit the quasi-random timing of unconditional housing recipiency in Los Angeles County to determine the effects of housing the homeless on employment, earnings, and select benefits absorption. We test and confirm the validity of our event study approach by demonstrating that the wait times between entry in the in the HMIS and receipt of UH is unrelated to homelessness severity.

While our analysis sees some limitations common to nearly all other works on homelessness, we view this work as the first to estimate the labor market impacts of UH recipiency leveraging many of the new advances in administrative homelessness data infrastructure. Our work uniquely contributes to the existing literature for the following reasons: 1) We use compelling quasi-experimental variation in the timing of housing recipiency; 2) we observe our sample with frequency both pre- and post-event; 3) we observe both labor market and benefits outcomes frequently over time; 4) our data on employment, earnings, and benefits outcomes feature some internal and third-party verification process beyond pure self-reporting; and 5) we observe a relatively large number of UH recipients.

We find substantial impacts on employment (+10.8pp from a baseline of 20%) and earnings (+USD 200 from a baseline of USD 450) following placement into RRH and no strong effects in either direction following placement into PSH. This contrast likely speaks to differences in selection criteria into each respective program, rather than solely to underlying differences in the actual treatment. We confirm the robustness of our estimates to different censoring restrictions on our sample, different assumptions about the updating process to our employment outcomes, and alternate event study estimation procedures that address concerns about mean reversion and potential heterogeneous treatment effects under staggered event timing across treated units.

We then explore some of the mechanisms that generate our main results. We characterize changes in earnings and benefits absorption based on ex-post employment transition type: conditional on making a U2E transition, individual recipients of both RRH and PSH see earnings increases of around USD 1000 and USD 500, respectively, per month. RRH recipients making E2E transitions also see increased earnings post-event by around USD 200-300 per month. We also document that on average, individuals reporting consistent employment in the post-event period report no increase in benefits absorption. Because we observe that individuals finding employment in the post-period see *no* concurrent increase in benefits, we attribute these effects to having stable permanent shelter. We also see increases in labor search behavior among RRH recipients and decreases among PSH recipients, which we interpret as substantiating our observed results. Finally, we rule out the possibility that our results are driven by changes in receiving services pertaining to mental health or substance abuse, although mental health or substance abuse problems may improve in response to housing regardless of receiving professional services.

We study heterogeneity by sociodemographic characteristics that are observable in our data. We find no evidence of differential employment response for heads-of-families for RRH recipients, but we find a large extensive-margin employment response of heads-of-families in the context of PSH. Similarly, we find no significant differences between men and women, and only a small difference by veteran status.

Based on these results, we estimate that the cost-offset of these programs *solely* through

their effect on recipients' earnings are net positive, but small (around than 1% cost-offset during program recipiency for RRH recipients and near zero for PSH) relative to the existing estimates of the reduction in public service usage. In spite of our large documented employment and earnings effects, individuals employed post event still earn relatively little income, and an overwhelming majority of housing recipients do not report employment post-event. Moreover, our cost-offset estimates are small on average, because while the extensive-margin employment effects of these programs are large in relative terms, individuals employed post-event still earn relatively little income, and an overwhelming majority of housing recipients still do not report consistent employment post-event. However, our estimates speak to outcomes within two years of housing, which may understate longer-run impacts.

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7 Main figures and tables

Table 1: Sample summary statistics

	RRH	PSH	Untreated
Demographics			
Black	0.635	0.556	0.389
Male	0.595	0.606	0.636
Veteran	0.293	0.061	0.067
Age at first interaction (years)	41	47	42
Prior Housing Status			
Months homeless since first spell	16	41	-
Months homeless since prior spell	8	24	-
Most common prior living situation	PNMFH (42%)	PNMFH (39%)	PNMFH (67%)
Second most —	Emergency Shelter (23%)	Emergency Shelter (36%)	Emergency Shelter (19%)
Third most —	Transitional Housing (9%)	Move from Prior HF (7%)	Living with Family (2%)
Employment, earnings and benefits at first interaction			
Employed	.189	.070	.079
Total earned income among employed	1477	1135	1200
Total benefits income among employed	140	66	44
Total monthly income among employed	1635	1213	1255
Total benefits income	232	355	219
Total monthly income	458	420	292
Individuals in sample	1,707	2,265	-

This table displays select demographic, housing, and employment tabulations stratified by final sample subgroups of treatment status. Dollar values are expressed in units USD January 2020. "PNMFH" refers to "Place not meant for habitation". Months Homeless Since First Spell is calculated as the difference between the event month and the earliest stated homelessness spell. Months Homeless Since Prior Spell is calculated as the difference between the event month and the latest stated homelessness spell prior to the housing event. Untreated individuals experience no UH-style housing intervention. Most common pre-event living situations are reported at event time for RRH and PSH recipients and upon earliest interaction for untreated individuals. Monthly earnings, benefits, and employment statistics are reported at earliest interaction within the spell.

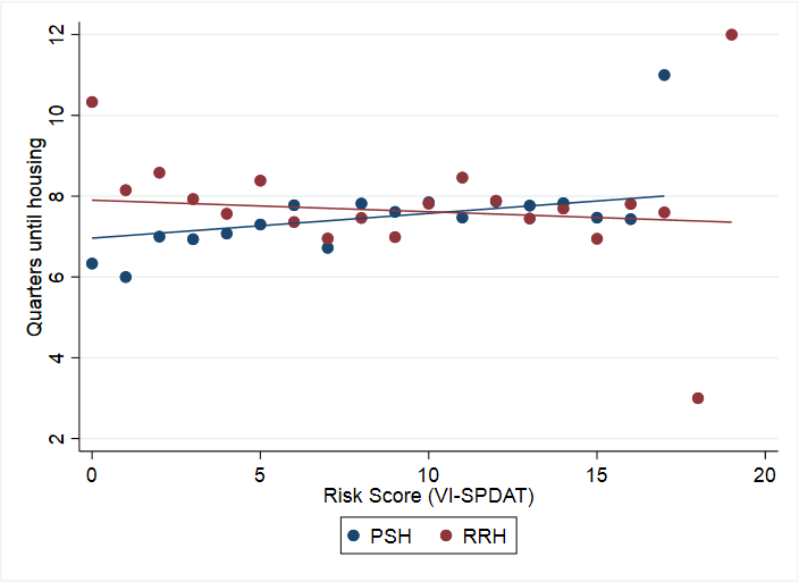
Table 2: Univariate regressions of wait times on recipient observable characteristics

	RRH				PSH			
	Coefficient	SE	N	R^2	Coefficient	SE	N	R^2
Risk score	-.0639	.121	633	.000481	.141	.107	840	.00194
Female	-.146	.624	1339	.0000417	-.319	.466	2070	.000223
Guardian	-5.41	.851	967	.0361	-2.08	1.75	1921	.000763
BIPOC status	.813	.651	1353	.00117	1.15	.454	2100	.00296
Any reported pre-event mental health issues	3.15	.64	1302	.0187	3.5	.532	2047	.0215
Any reported pre-event substance abuse issues	2.81	.744	1353	.011	2.68	.451	2100	.0168
Veteran status	-2.69	.674	1261	.00928	-1.12	.961	2083	.000501
Age at move-in event	.0278	.023	1123	.00128	.0307	.0189	2063	.00127
Employed pre-event relative to all others pre-event	-.332	1.41	1353	.0000492	-1.12	1.68	2100	.000178
Employed pre-event relative to unemployed pre-event	-.81	1.42	917	.000426	-1.45	1.68	1841	.000339

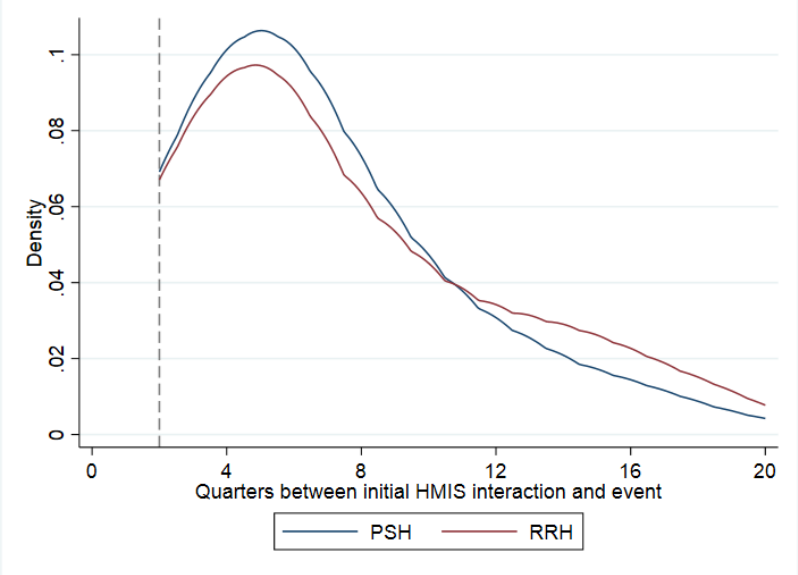
Note: This table displays a series of univariate cross-sectional regressions of wait time on observable characteristics, where each row under each respective RRH or PSH column-set corresponds with a separate regression. The dependent variable, wait time, corresponds with the number of months elapsed between an individual's most recent entry into the HMIS out of homelessness and their placement into any unconditional housing. This variable is our main variable generating quasi-experiment variation in housing recipiency. Pre-event (un)employment is measured as whether an individual is observed as (un)employed in at least 80% of pre-event observations; this definition generates some individuals with missing unemployment status, so the final two variables measure the correlation of wait time and pre-event employment relative to these two different baselines. The regressions are stratified by housing program. Columns "SE" give heteroskedasticity-robust standard errors.

Figure 1: Housing event timing

(a) Risk Score v. time elapsed between intake and housing event



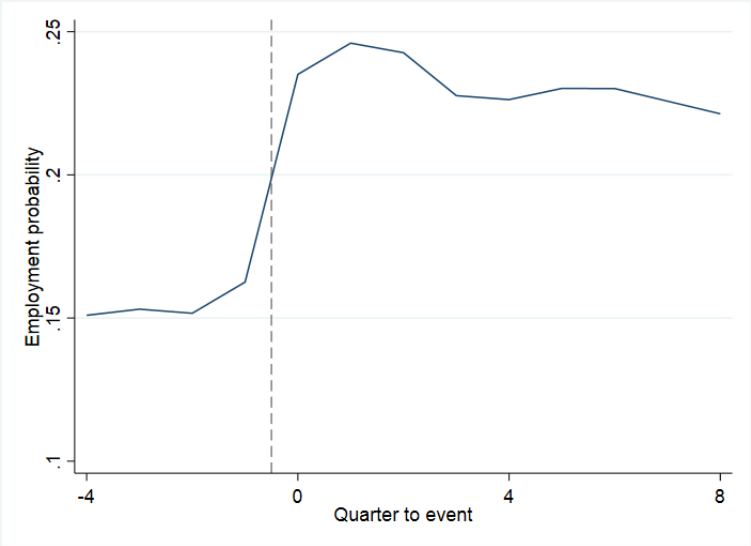
(b) Distibution of time elapsed between HMIS entry and housing event



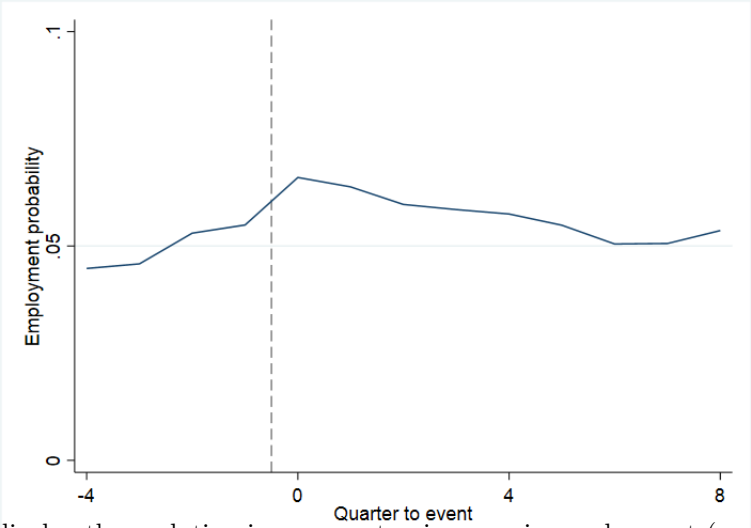
Note: These figures illustrate metadata around the timing of unconditional housing events. Panel (a) displays a binned scatterplot (stratified by housing type) of quarters elapsed between initial client intake and placement into unconditional housing on Risk Score (VI-SPDAT). The estimated slope coefficient is 0.01 (p-value 0.64). Panel (b) displays the distribution of quarters (using an Epanechnikov kernel) elapsed between entry into the HMIS and clients' housing events.

Figure 2: Time series of extensive margin employment probability around event time

Panel (a): RRH



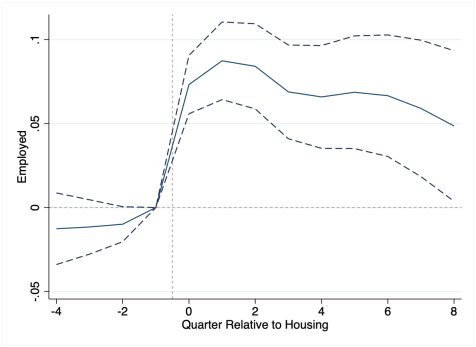
Panel (b): PSH



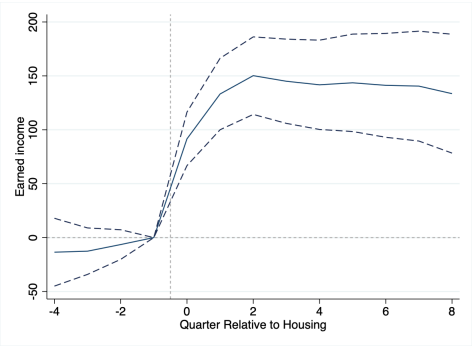
Note: These figures display the evolution in mean extensive margin employment (employment probability) around housing recipiency events. Individuals are identified as employed if they either report employment or positive earnings or if they are observed in any of the administrative data systems with positive earnings or employment. Panel (a) displays the evolution in outcomes for RRH recipients. Panel (b) displays the evolution in outcomes for PSH recipients. Event time zero corresponds with the quarter of move-in-event.

Figure 3: Main event study results

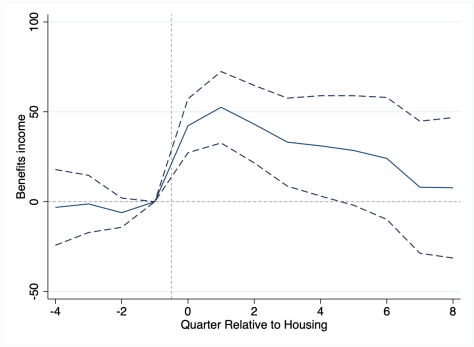
RRH results



Panel (a): Employment

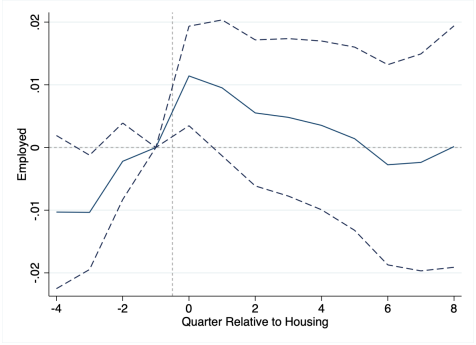


Panel (b): Earned income

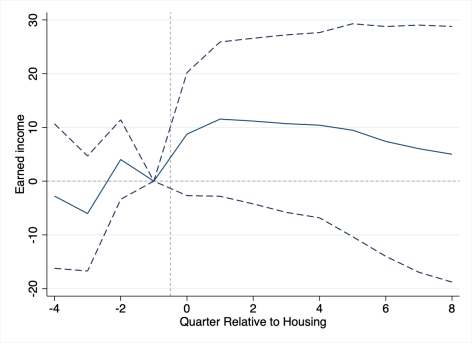


Panel (c): Benefits income

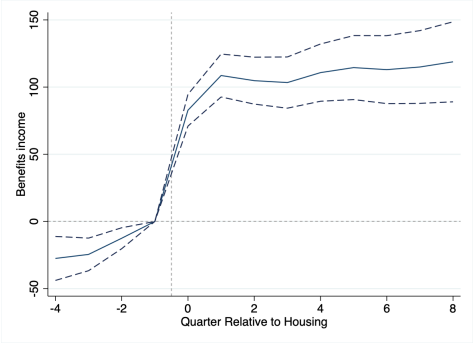
PSH results



Panel (d): Employment



Panel (e): Earned income



Panel (f): Benefits income

Note: These figures display the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Dependent variables are listed on the y-axis. Panels (a)-(c) show the event study estimates for Rapid Re-Housing. Panels (d)-(f) show the results for Permanent Supportive Housing. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Table 3: Event studies (labor market and earnings outcomes)

Panel (a): RRH

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Pre-period ($t \leq -2$)	-0.021	-33.541	-0.018	-17.012	-0.034	-18.239	-0.022	-0.632	-0.015
	(0.007)	(12.122)	(0.017)	(11.203)	(0.024)	(6.351)	(0.013)	(2.521)	(0.020)
Post-period ($t \geq 1$)	0.088	176.026	0.171	120.428	0.173	55.615	0.064	4.578	0.076
	(0.012)	(17.923)	(0.026)	(16.728)	(0.042)	(9.787)	(0.021)	(4.429)	(0.040)
Post-pre difference	0.108	209.567	0.190	137.441	0.207	73.854	0.086	5.210	0.092
	(0.014)	(21.164)	(0.031)	(20.829)	(0.053)	(11.402)	(0.026)	(5.473)	(0.052)
Pre-event average	0.200	446.146	6.441	186.863	6.812	254.508	6.214	14.345	6.361
	[0.400]	[643.714]	[0.808]	[540.247]	[0.833]	[410.454]	[0.706]	[128.033]	[0.852]
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.59	0.65	0.72	0.64	0.78	0.72	0.84	0.56	0.98
N	59728	58813	34402	55546	11174	58813	25610	58813	1346
Number of clusters	1707	1700	1249	1697	601	1700	1061	1700	99

Panel (b): PSH

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Pre-period ($t \leq -2$)	-0.008	-23.113	-0.008	-2.721	0.054	-22.954	-0.010	1.672	0.007
	(0.004)	(6.004)	(0.009)	(3.636)	(0.046)	(5.206)	(0.007)	(1.177)	(0.020)
Post-period ($t \geq 1$)	0.010	103.818	0.091	12.339	0.013	91.175	0.087	2.761	-0.011
	(0.006)	(10.456)	(0.014)	(7.158)	(0.074)	(7.965)	(0.012)	(2.047)	(0.025)
Post-pre difference	0.018	126.931	0.099	15.061	-0.041	114.129	0.096	1.088	-0.018
	(0.007)	(11.649)	(0.016)	(7.956)	(0.078)	(9.030)	(0.014)	(1.986)	(0.038)
Pre-event average	0.071	440.889	6.131	54.633	6.636	377.950	6.066	9.181	6.297
	[0.257]	[468.959]	[0.731]	[271.963]	[0.788]	[417.875]	[0.705]	[102.177]	[0.918]
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.65	0.70	0.81	0.65	0.86	0.75	0.85	0.60	0.98
N	80771	79445	66842	78253	4501	79456	62717	79456	1203
Number of clusters	2264	2248	2124	2248	244	2248	2066	2248	78

This table displays the coefficients from event study regressions with two-way fixed effects of the form $y_{it} = \alpha_i + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 1\} + \theta \cdot \mathbb{1}\{EventTime_{it} = 0\} + u_{it}$ on the sample of unconditional housing recipients entering between January 2014 and February 2018. Panel (a) studies RRH recipients; Panel (b) studies PSH recipients. The pre- and post-period coefficients ($\hat{\gamma}$ and $\hat{\beta}$) are specified relative to the base-period average at one period prior to the housing event. The post-pre difference value subtracts $\hat{\gamma}$ from $\hat{\beta}$; the event-period coefficient $\hat{\theta}$ is omitted in this calculation. The pre-period includes up to 12 months pre-event, and the post-period extends to 24 months post-event. Standard errors are clustered on the individual-level and are reported in parentheses. Standard deviations are reported in hard brackets.

Table 4: Event studies (broad programmatic benefits)

Panel (a): RRH

	(1) Pecuniary ben.	(2) Nonpecuniary ben.	(3) Insurance benefit.	(4) Any insurance	(5) HIV/AIDS services	(6) Mental health services	(7) Substance abuse services
Pre-period ($t \leq -2$)	-0.029	-0.025	-0.056	-0.062	0.000	-0.011	0.004
	(0.008)	(0.008)	(0.008)	(0.009)	(0.001)	(0.006)	(0.004)
Post-period ($t \geq 1$)	0.059	-0.037	0.174	0.199	0.002	0.005	-0.015
	(0.013)	(0.013)	(0.013)	(0.014)	(0.002)	(0.009)	(0.006)
Post-pre difference	0.087	-0.011	0.230	0.261	0.002	0.016	-0.018
	(0.015)	(0.015)	(0.014)	(0.016)	(0.002)	(0.011)	(0.006)
Pre-event average	0.404	0.469	0.560	0.546	0.008	0.117	0.056
	[0.491]	[0.499]	[0.496]	[0.498]	[0.088]	[0.321]	[0.230]
Month fixed effects	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X
Adj. R-squared	0.66	0.68	0.59	0.50	0.70	0.43	0.43
N	58813	58110	58342	58813	56881	56881	56881
Number of clusters	1700	1681	1691	1700	1707	1707	1707

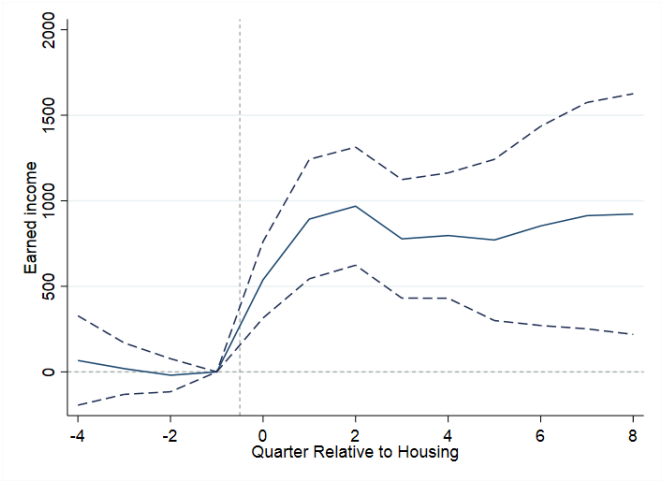
Panel (b): PSH

	(1) Pecuniary ben.	(2) Nonpecuniary ben.	(3) Insurance benefit.	(4) Any insurance	(5) HIV/AIDS services	(6) Mental health services	(7) Substance abuse services
Pre-period ($t \leq -2$)	-0.053	-0.036	-0.057	-0.056	-0.009	-0.086	-0.029
	(0.006)	(0.007)	(0.007)	(0.007)	(0.003)	(0.008)	(0.005)
Post-period ($t \geq 1$)	0.109	0.004	0.178	0.186	0.009	0.107	0.030
	(0.010)	(0.011)	(0.011)	(0.012)	(0.005)	(0.014)	(0.009)
Post-pre difference	0.162	0.040	0.234	0.242	0.018	0.193	0.059
	(0.011)	(0.012)	(0.011)	(0.012)	(0.006)	(0.015)	(0.010)
Pre-event average	0.683	0.537	0.533	0.513	0.054	0.323	0.102
	[0.465]	[0.499]	[0.499]	[0.500]	[0.226]	[0.468]	[0.302]
Month fixed effects	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X
Adj. R-squared	0.59	0.65	0.63	0.58	0.57	0.45	0.41
N	79456	79184	78848	79456	76942	76942	76942
Number of clusters	2248	2243	2238	2248	2264	2264	2264

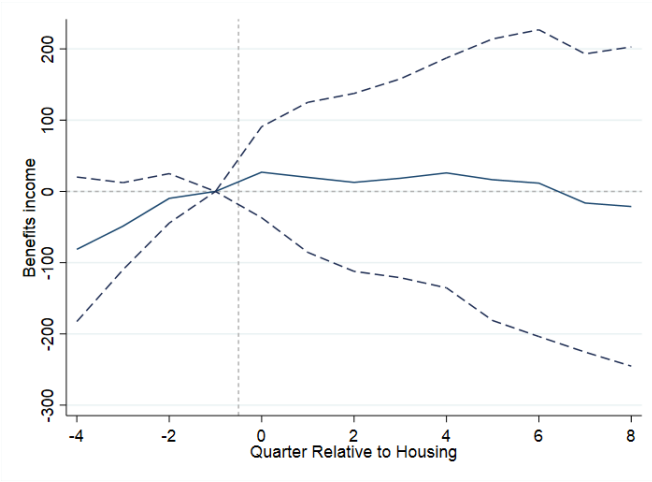
This table displays the coefficients from event study regressions with two-way fixed effects of the form $y_{it} = \alpha_i + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 1\} + \theta \cdot \mathbb{1}\{EventTime_{it} = 0\} + u_{it}$ on the sample of unconditional housing recipients entering between January 2014 and February 2018. Panel (a) studies RRH recipients; Panel (b) studies PSH recipients. The pre- and post-period coefficients ($\hat{\gamma}$ and $\hat{\beta}$) are specified relative to the base-period average at one period prior to the housing event. The post-pre difference value subtracts $\hat{\gamma}$ from $\hat{\beta}$; the event-period coefficient $\hat{\theta}$ is omitted in this calculation. The pre-period includes up to 12 months pre-event, and the post-period extends to 24 months post-event. Standard errors are clustered on the individual-level. Standard deviations are reported in hard brackets.

Figure 4: RRH results by employment transition type

Unemployment-to-employment ("U2E") transitioners

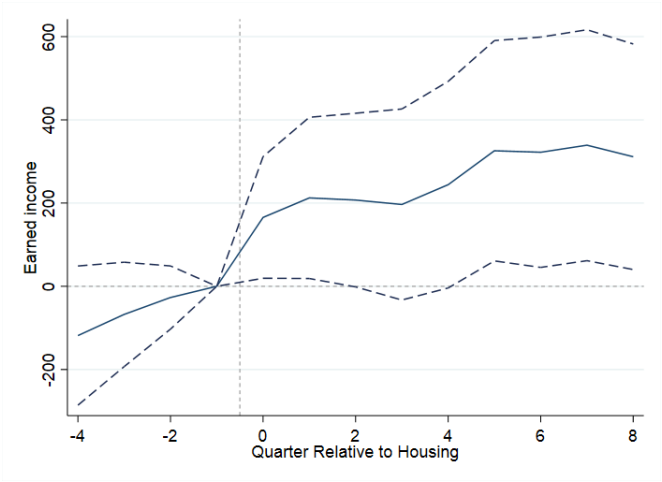


Panel (a): Earned income

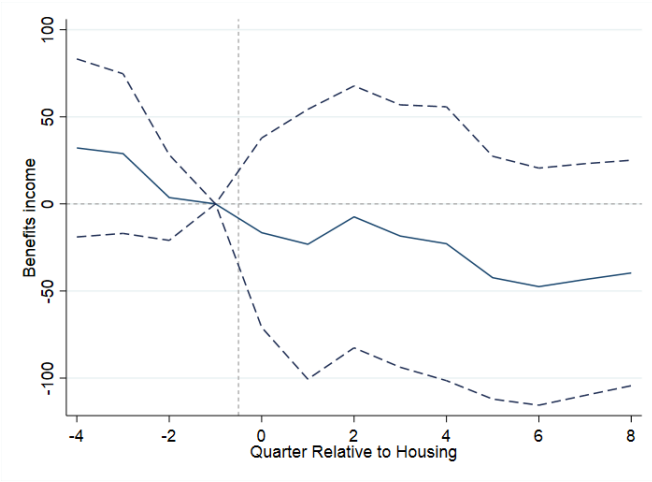


Panel (b): Benefits income

Employment-to-employment ("E2E") transitioners



Panel (c): Earned income



Panel (d): Benefits income

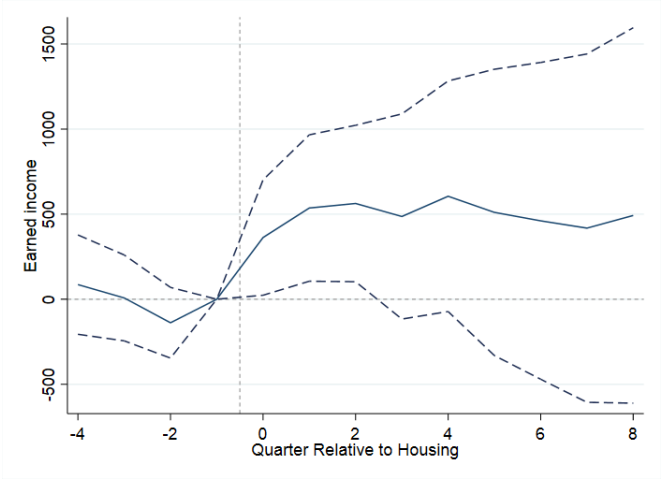
Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$$

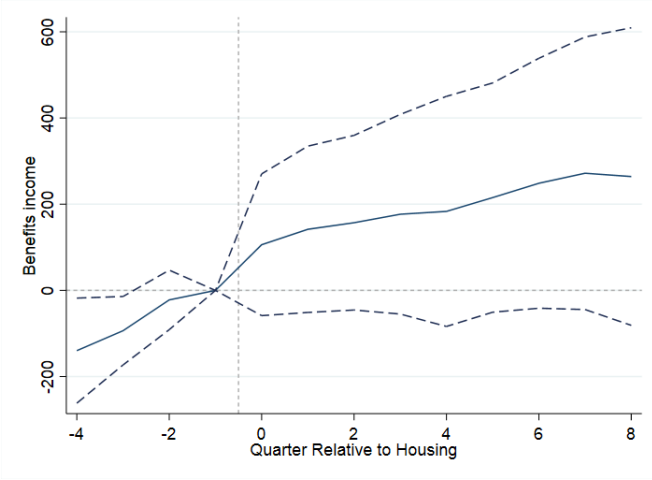
The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition between unemployment in the pre-period and employment in the post-period in panel (a) and retain employment throughout the pre- and post-period in panel (b). Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure 5: PSH results by employment transition type

Unemployment-to-employment ("U2E") transitioners

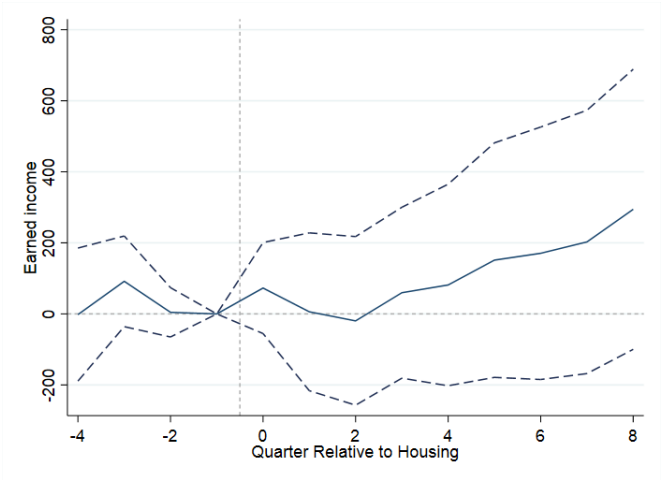


Panel (a): Earned income

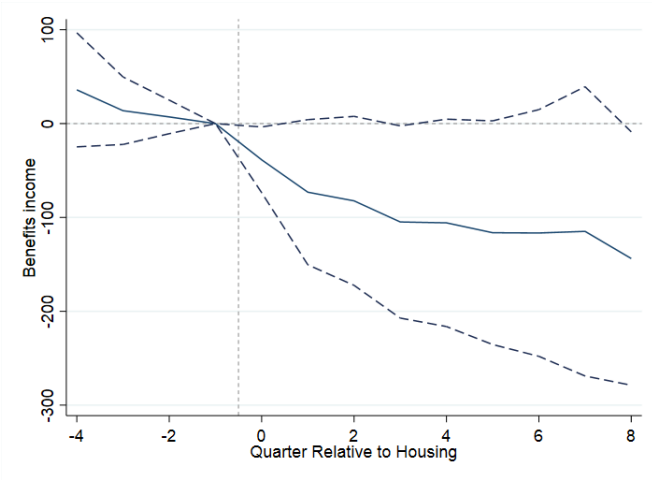


Panel (b): Benefits income

Employment-to-employment ("E2E") transitioners



Panel (c): Earned income



Panel (d): Benefits income

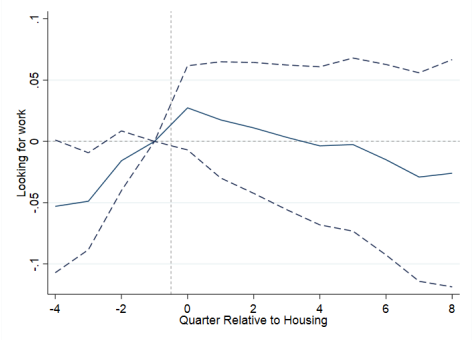
Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$$

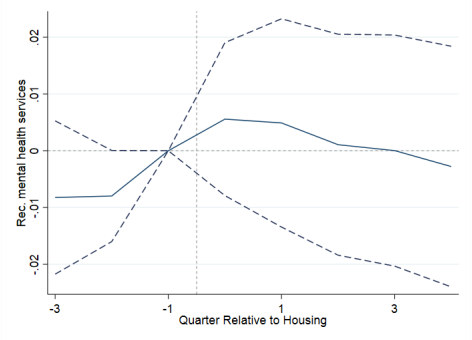
The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition between unemployment in the pre-period and employment in the post-period in panel (a) and retain employment throughout the pre- and post-period in panel (b). Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure 6: Additional mechanisms: job-search, mental health, and substance abuse

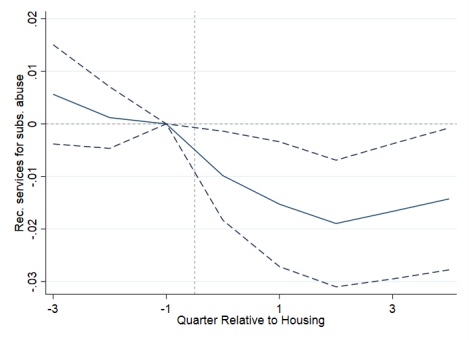
RRH results



Panel (a): "Looking for work"

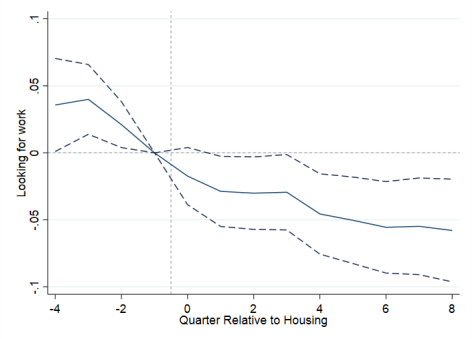


Panel (b): Receiving mental health services

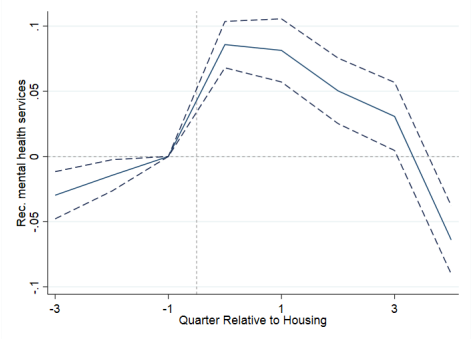


Panel (c): Receiving substance abuse services

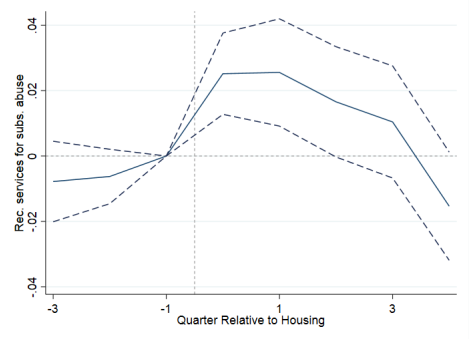
PSH results



Panel (d): "Looking for work"



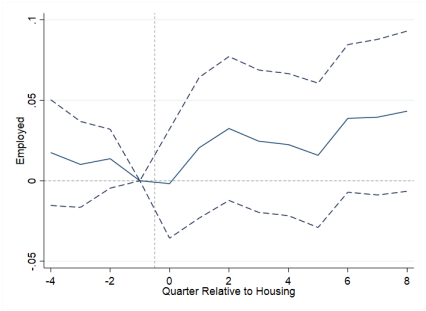
Panel (e): Receiving mental health services



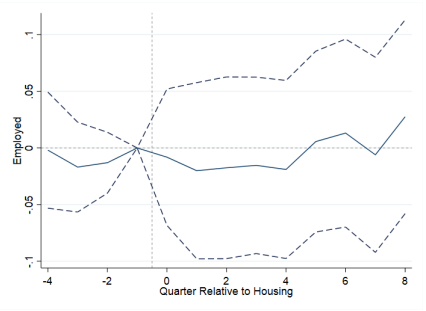
Panel (f): Receiving substance abuse services

Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$ The estimation sample includes individuals receiving RRH or PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Looking for work" corresponds with a dependent variable that indicates is whether an individual states they are "looking for work", conditional on not having employment. Outcomes for receiving mental health or substance abuse services are measured less-systematically beyond one year post-event, at which time we end our estimation horizon. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

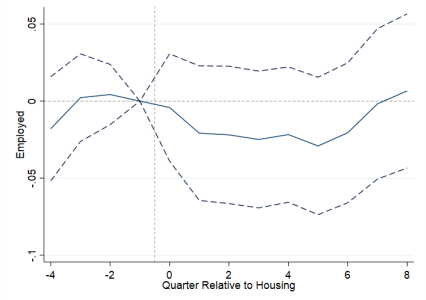
Figure 7: Extensive margin employment around unconditional RRH housing event: By select sociodemographic characteristics



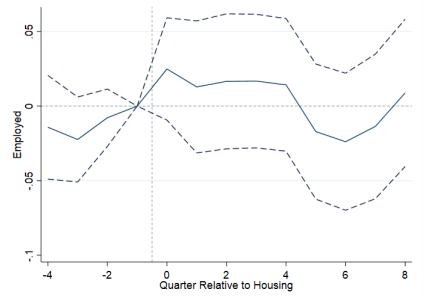
Panel (a):
Women v. men



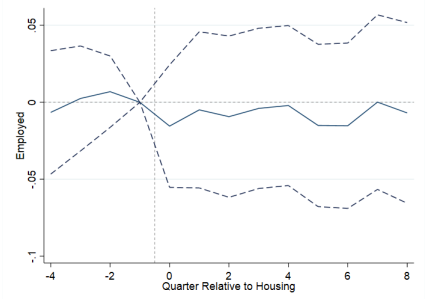
Panel (b):
Guardians v. non-guardians



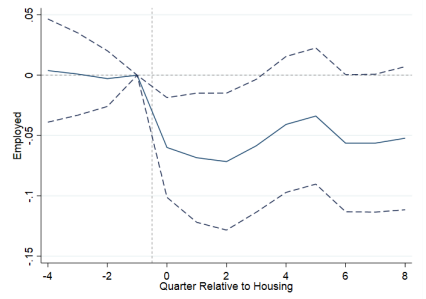
Panel (c):
Pre-event reported
mental health problems



Panel (d):
Non-white



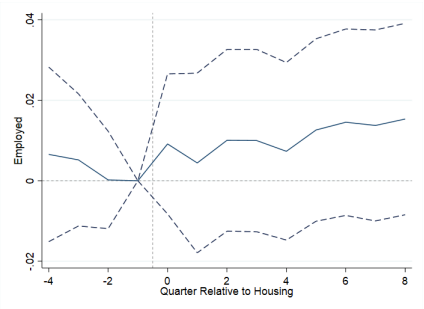
Panel (e):
Pre-event reported
substance abuse problems



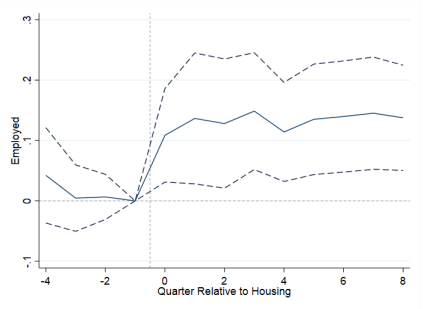
Panel (f):
Veteran status

Note: These figures display coefficients $\{\hat{\psi}_{q(t)}\}$ estimating heterogeneous extensive-margin employment responses to event study coefficients from the two-way fixed effect regression: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$, where $Status_i$ is an indicator for whether individual i is binarily tagged as the respective sociodemographic status as given by the panel title. The population consists of individuals in our sample receiving RRH between 2014 and 2018. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

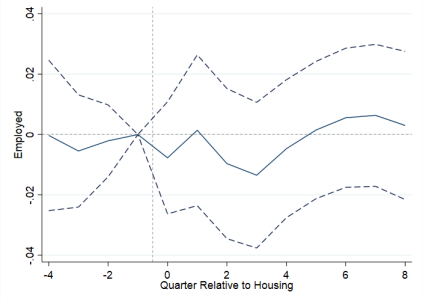
Figure 8: Extensive margin employment around unconditional PSH housing event: By select sociodemographic characteristics



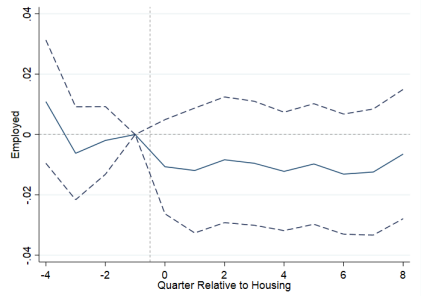
Panel (a):
Women v. men



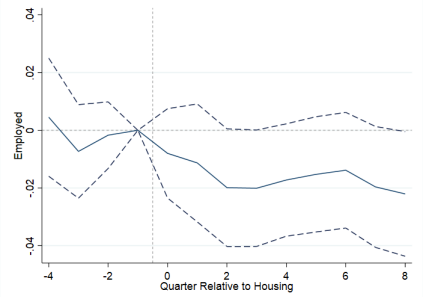
Panel (b):
Guardians v. non-guardians



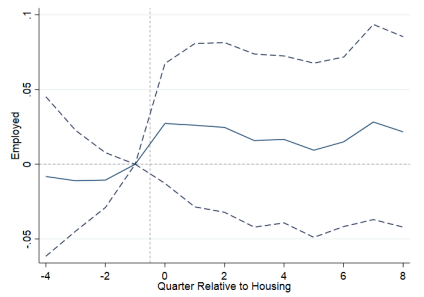
Panel (c):
Pre-event reported
mental health problems



Panel (d):
Non-white



Panel (e):
Pre-event reported
substance abuse problems



Panel (f):
Veteran status

Note: These figures display coefficients $\{\hat{\psi}_{q(t)}\}$ estimating heterogeneous extensive-margin employment responses to event study coefficients from the two-way fixed effect regression: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$, where $Status_i$ is an indicator for whether individual i is binarily tagged as the respective sociodemographic status as given by the panel title. The population consists of individuals receiving PSH between 2014 and 2018. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Table 5: Cost/benefit through labor channel of UH policies during program tenure

Panel (a): RRH

Transition	% of recipients	Annual inc. (pre)	Annual inc. (post)	P-stat (difference)	ΔT	% offset
U2E	2.34%	103.8	15046.86	0	2173.06	11.44%
E2E	6.91%	11610	16846.80	0	953.67	5.02%
U2U	58.00%	98.28	113.65	0.804	-0.54	0.00%
E2U	1.82%	7668	72	0	-796.69	-4.19%
Other	30.93%	4224	6900	0	260.33	1.37%
Total	100%	-	-	-	182.46	0.96%

Panel (b): PSH

Transition	% of recipients	Annual inc. (pre)	Annual inc. (post)	P-stat (difference)	ΔT	% offset
U2E	0.75%	216	12804	0	1737.15	9.14%
E2E	1.41%	10788	11388	0.738	152.70	0.80%
U2U	84.19%	42.36	12	0.095	-4.55	-0.02%
E2U	0.97%	9984	204	0	-1204.8	-6.34%
Other	12.67%	3396	3960	0.271	21.90	0.12%
Total	100%	-	-	-	2.44	0.01%

This table displays tabulations for a cost-benefit calculation of UH policies through the labor market channel during program tenure. Each row corresponds with a different ex-post employment transition type. Employment transition types based on employment status at least 80% of the respective event-period. E.g., "E2U" corresponds with individuals employed at least 80% of the pre-event period and unemployed at least 20% of the post-event period. Individuals categorized as "Other" satisfy none of four definitions. The annual income columns display estimates of the pre- and post-event coefficients from the regression: $y_{it} = \alpha_i + \delta_t + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -1\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 1\} + u_{it}$, and the P-stat column corresponds with the significance of the difference between the estimated coefficients (with standard errors clustered on the individual level). The column ΔT maps the change in income to a change in federal tax collections based on the 2017 federal income tax, Earned Income Tax Credit, and estimates from Piketty, Saez, and Zucman (2018) for payroll and sales tax expenses. Income estimates are omitted for the aggregation of all transition types in order to avoid confusion with regards to the calculation of change in taxes paid and net offset (which are nonlinear functions of pre- and post-event income). Panel (a) performs this calculation for Rapid Re-Housing recipients; Panel (b) performs this calculation for Permanent Supportive Housing recipients. These tables omit confidence bands, as the only uncertainty/heterogeneity comes from the estimated change in income.

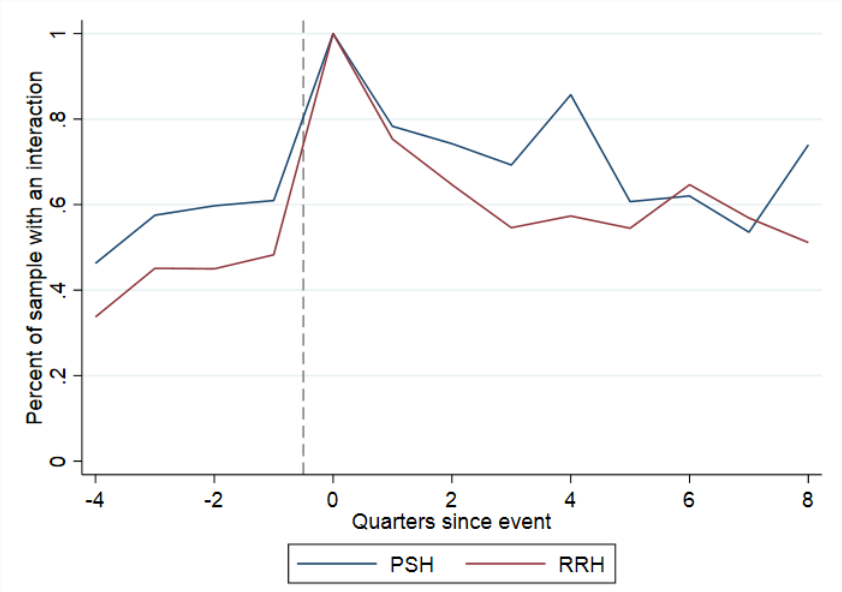
Appendix A Additional metadata

Table A.1: Difference in risk score based on sample inclusion

	PSH	RRH
	(1)	(2)
Inclusion in main-sample (binary)	.829	1.2
	(.153)	(.15)
Constant	9.33	7.33
	(.0931)	(.0487)
N	2453	6186
R^2	.0116	.011

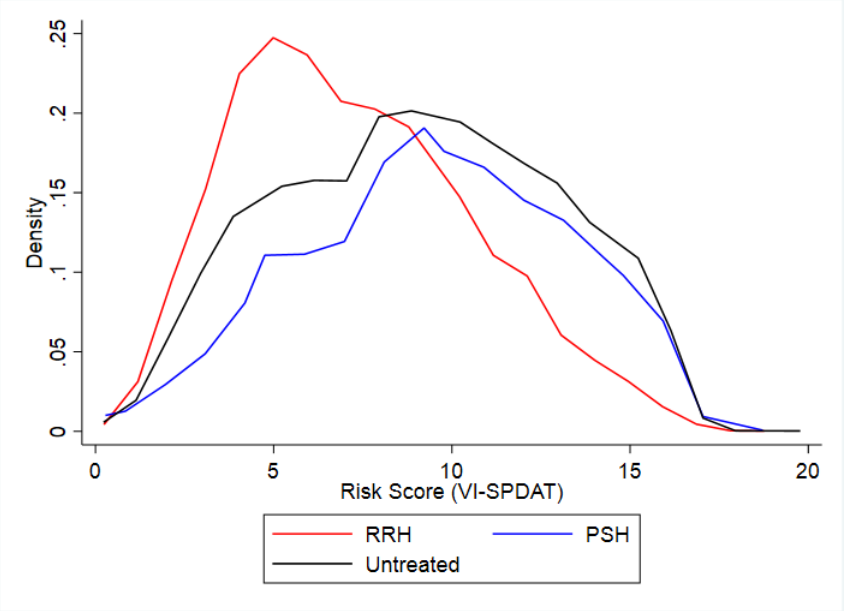
Note: This table displays coefficients from two cross-sectional regressions of risk score on an indicator for inclusion in our main sample. Individuals within each respective pool of housing recipients (during our considered timeframe) are included in our main sample if they are observed at least 7 months prior to their move-in *and* between 18 and 24 months following their move-in. The regressions are stratified by housing program. Parentheses contain heteroskedasticity-robust standard errors.

Figure A.1: Percent of sample interacting by quarter



This figure displays the proportion of our primary treated sample that registers an interaction with either the HMIS or a social service provider in a given quarter around their housing event.

Figure A.2: Distribution of VI-SPDAT (risk) scores



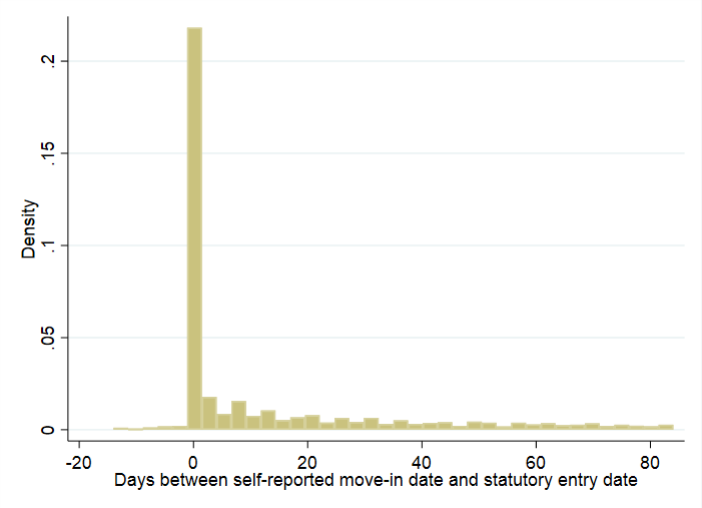
This figure displays the distribution of VI-SPDAT (risk) scores for individuals in the HMIS data based on programmatic treatment status. Risk scores are reported upon the latest solicitation prior to event for treated individuals and as an average across risk assessments for untreated individuals throughout their interactions with the HMIS.

Figure A.3: Housing recipiency discrepancy in entry date

(a) Reported day of month



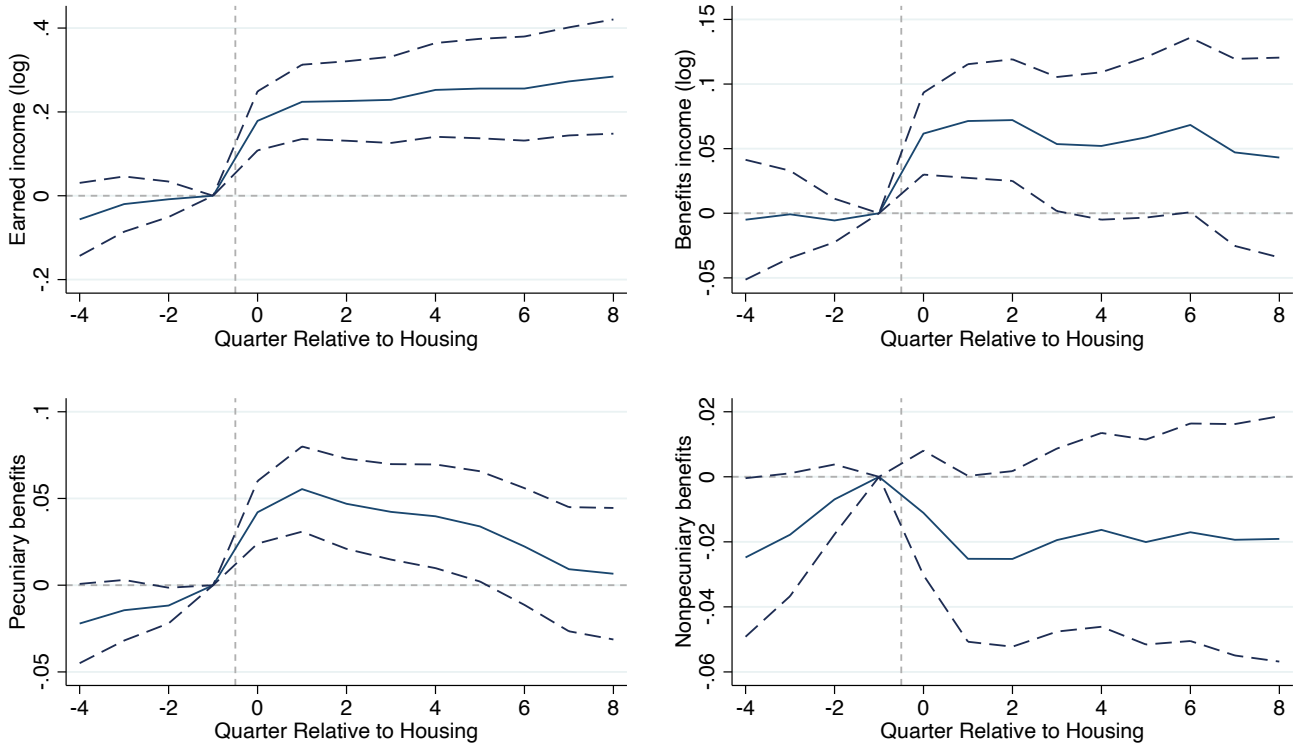
(b) Observed difference between reported entry date and move-in date



These figures demonstrate the discrepancy between the reported date of entry into unconditional housing accommodations and the potentially true date of entry for housing recipiency events in our sample. Panel (a) plots the relative frequency of date of the month of entry as reported by case worker (statutory) versus as reported by client (self-reported), and Panel (b) plots the relative frequency of days difference between statutory and self-reported move-in date. Client-reported move-in dates are only available for around 15% of the sample.

Appendix B Additional central specification results and robustness

Figure B.1: Additional income and broad programmatic outcomes:
Panel (a): RRH

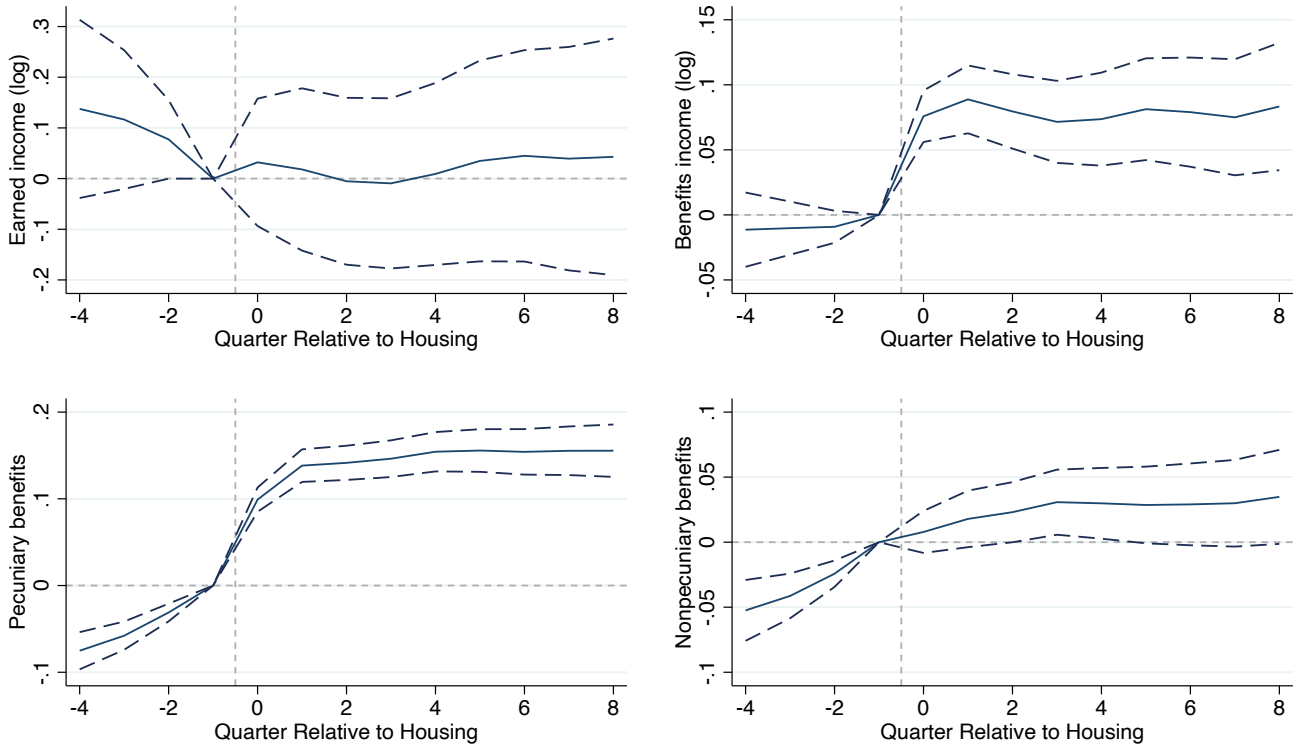


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing. Panel (b) shows the results for Permanent Supportive Housing by month.

Figure B.1: Additional income and broad programmatic outcomes:
Panel (b): PSH



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing. Panel (b) shows the results for Permanent Supportive Housing by month.

Table B.1: Event studies (specific select programmatic benefits)

Panel (a): RRH

	(1) SSI	(2) SSDI	(3) Unemployment ben.	(4) TANF	(5) SNAP	(6) WIC	(7) Medicaid	(8) Other TANF
Pre-period ($t \leq -2$)	-0.006	-0.008	-0.001	-0.010	-0.039	-0.000	-0.058	-0.001
	(0.005)	(0.004)	(0.002)	(0.004)	(0.007)	(0.002)	(0.009)	(0.001)
Post-period ($t \geq 1$)	0.021	0.004	0.005	0.028	0.058	-0.003	0.137	-0.004
	(0.007)	(0.004)	(0.003)	(0.007)	(0.010)	(0.004)	(0.013)	(0.002)
Post-pre difference	0.027	0.013	0.006	0.037	0.097	-0.003	0.195	-0.004
	(0.008)	(0.006)	(0.004)	(0.009)	(0.012)	(0.005)	(0.015)	(0.003)
Pre-event average	0.103	0.039	0.013	0.101	0.326	0.024	0.406	0.006
	[0.304]	[0.194]	[0.114]	[0.302]	[0.469]	[0.152]	[0.491]	[0.078]
Month fixed effects	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X
Adj. R-squared	0.74	0.66	0.51	0.77	0.79	0.70	0.69	0.53
N	58809	58812	58813	58784	57269	57269	58342	57269
Number of clusters	1700	1700	1700	1700	1656	1656	1691	1656

ID-clustered standard errors in parentheses

Panel (b): PSH

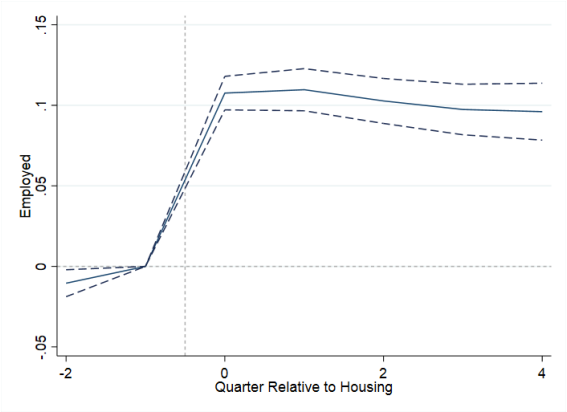
	(1) SSI	(2) SSDI	(3) Unemployment ben.	(4) TANF	(5) SNAP	(6) WIC	(7) Medicaid	(8) Other TANF
Pre-period ($t \leq -2$)	-0.019	-0.004	0.000	-0.002	-0.036	-0.000	-0.055	0.000
	(0.005)	(0.003)	(0.002)	(0.001)	(0.006)	(0.001)	(0.007)	(0.001)
Post-period ($t \geq 1$)	0.070	0.010	-0.001	0.004	0.059	0.002	0.172	0.001
	(0.008)	(0.005)	(0.002)	(0.003)	(0.008)	(0.002)	(0.011)	(0.001)
Post-pre difference	0.088	0.014	-0.002	0.007	0.095	0.002	0.227	0.001
	(0.009)	(0.006)	(0.003)	(0.003)	(0.010)	(0.002)	(0.012)	(0.001)
Pre-event average	0.218	0.056	0.008	0.022	0.435	0.005	0.461	0.001
	[0.413]	[0.231]	[0.090]	[0.147]	[0.496]	[0.069]	[0.499]	[0.037]
Month fixed effects	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X
Adj. R-squared	0.77	0.70	0.37	0.81	0.79	0.74	0.65	0.67
N	79455	79432	79456	79456	77542	77542	78848	77542
Number of clusters	2248	2248	2248	2248	2196	2196	2238	2196

ID-clustered standard errors in parentheses

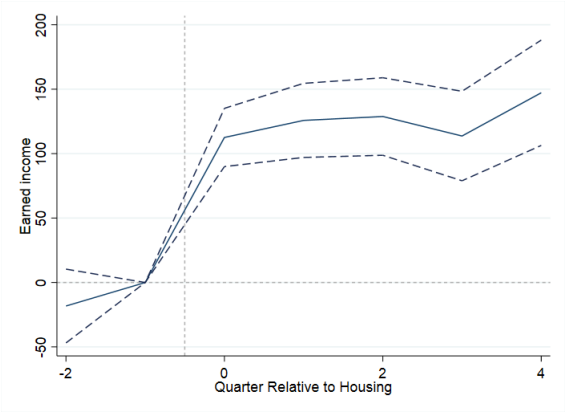
This table displays the coefficients from event study regressions with two-way fixed effects of the form $y_{it} = \alpha_i + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 1\} + \theta \cdot \mathbb{1}\{EventTime_{it} = 0\} + u_{it}$ on the sample of unconditional housing recipients entering between January 2014 and February 2018. Panel (a) studies RRH recipients; Panel (b) studies PSH recipients. The pre- and post-period coefficients ($\hat{\gamma}$ and $\hat{\beta}$) are specified relative to the base-period average at one period prior to the housing event. The post-pre difference value subtracts $\hat{\gamma}$ from $\hat{\beta}$; the event-period coefficient $\hat{\theta}$ is omitted in this calculation. The pre-period includes up to 12 months pre-event, and the post-period extends to 24 months post-event. Standard errors are clustered on the individual-level. Standard deviations are reported in hard brackets.

Figure B.2: Robustnesss: Main event study results on alternative sample

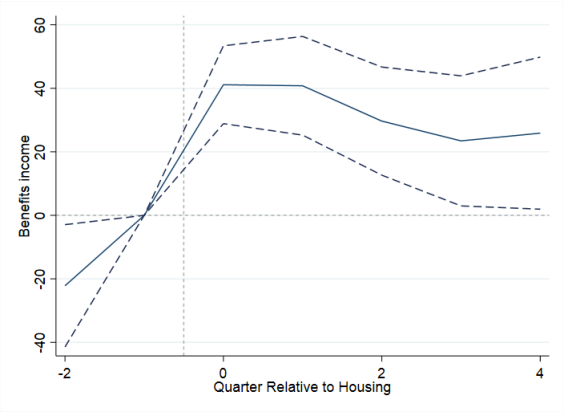
RRH results



Panel (a): Employment

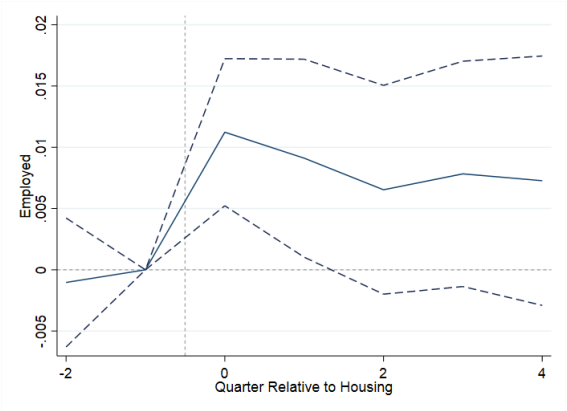


Panel (b): Earned income

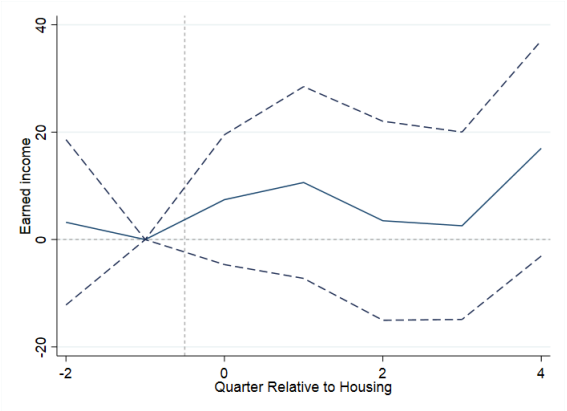


Panel (c): Benefits income

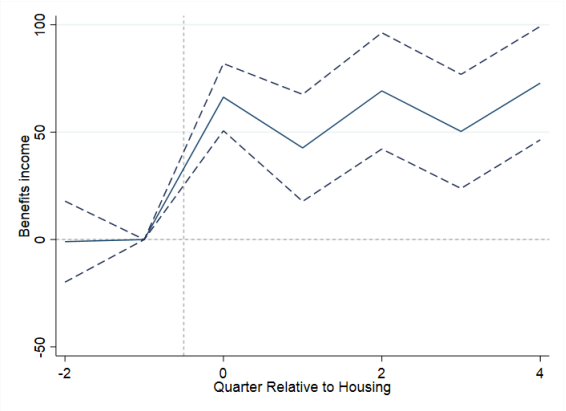
PSH results



Panel (d): Employment



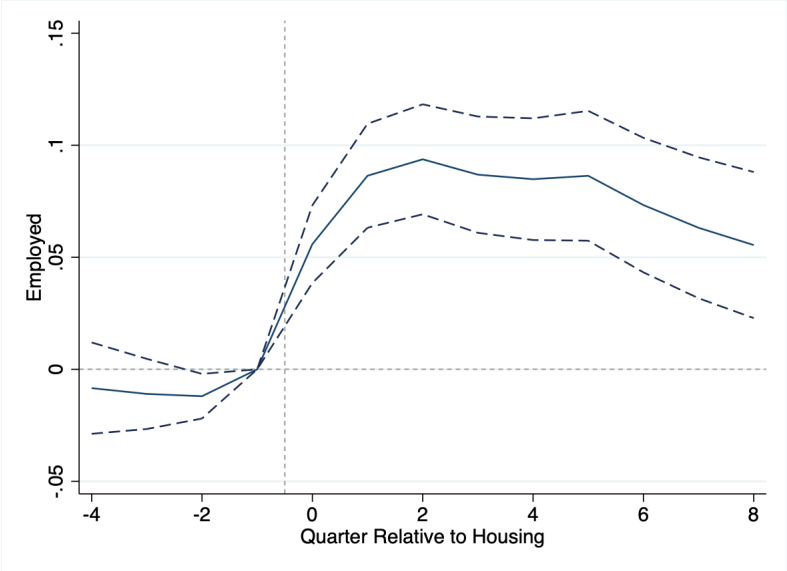
Panel (e): Earned income



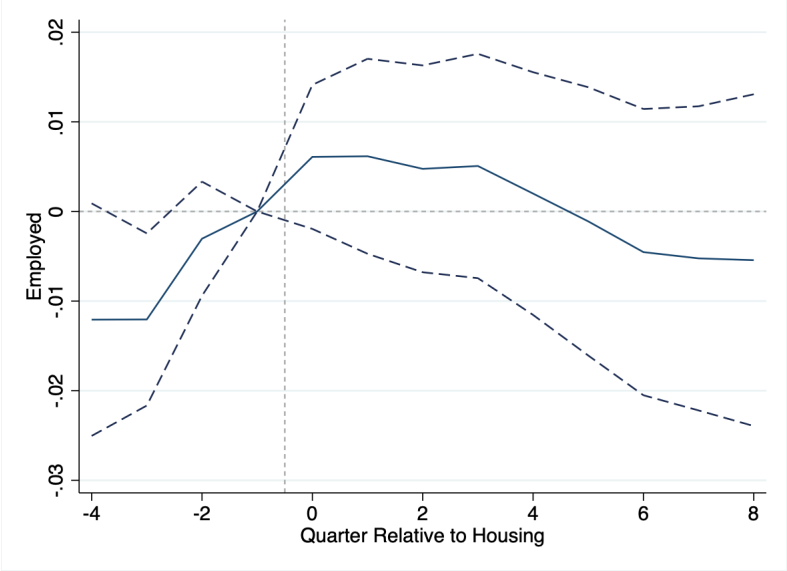
Panel (f): Benefits income

Note: These figures display the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$. The estimation sample includes individuals receiving housing benefits between 2014 and 2018 that are observed at least two months prior to between 7 and 12 months following housing receipt. the sample time frame spans from January 2013 to February 2020. Timing is binned up to 3 quarters prior to and 5 quarters since each individual's housing event. Dependent variables are listed on the y-axis. Panels (a)-(c) show the event study estimates for Rapid Re-Housing; Panels (d)-(f) show the results for Permanent Supportive Housing. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure B.3: Robustnesss of employment result:
Interpolating employment between interactions



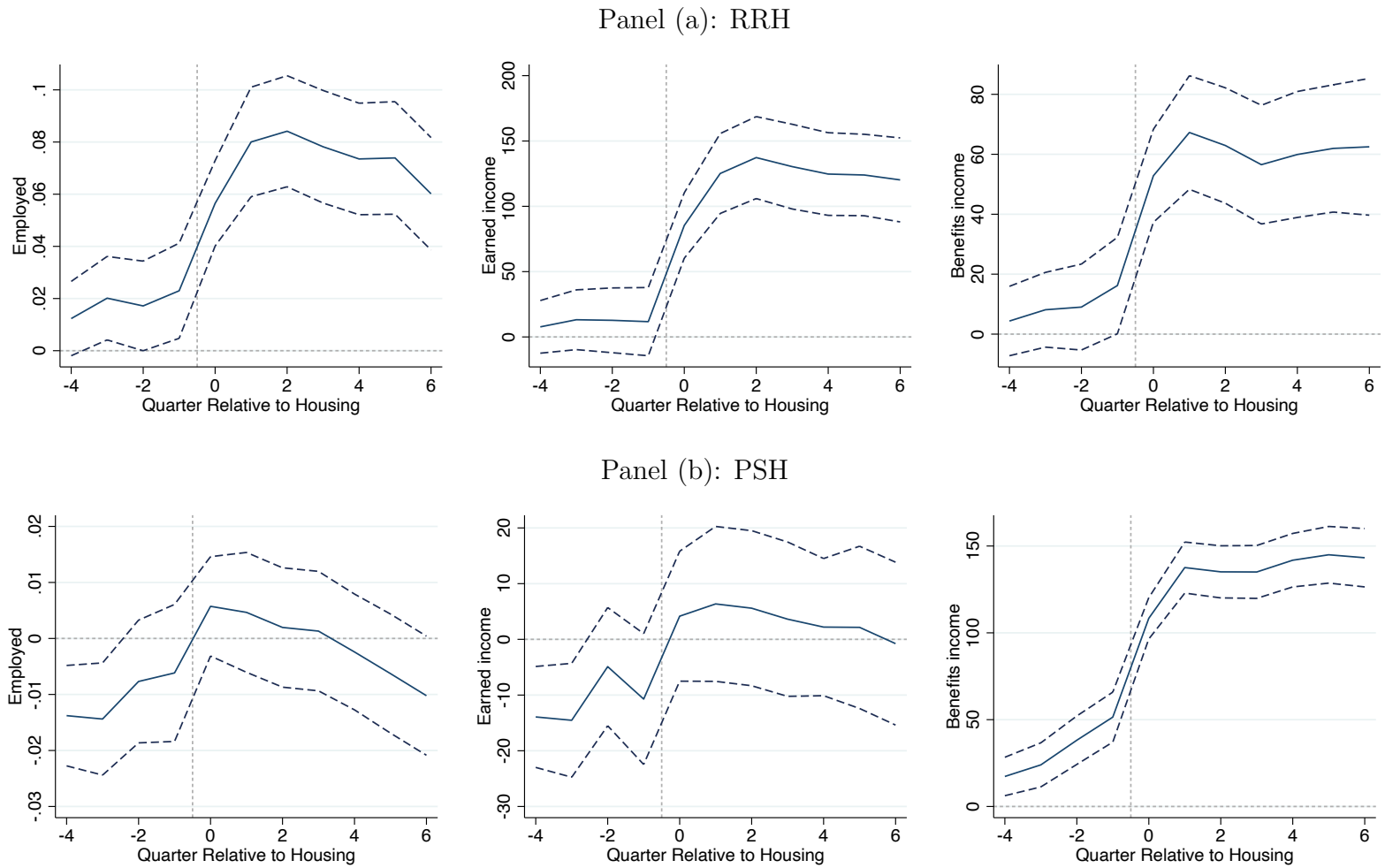
Panel (a): RRH



Panel (b): PSH

Note: These figures display the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$. The dependent variable is extensive margin employment; however, unlike for the main results on extensive margin employment, the variable for extensive margin employment is "carried forward" between interactions instead of recorded as missing. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

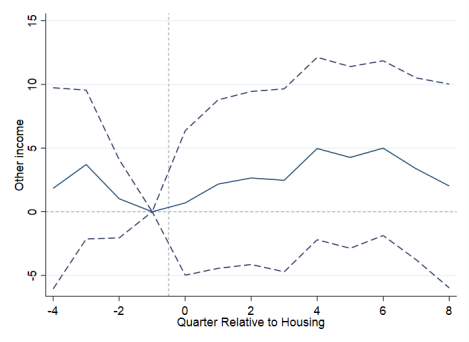
Figure B.4: Event studies following Borusyak, Jaravel, and Spiess (2024)



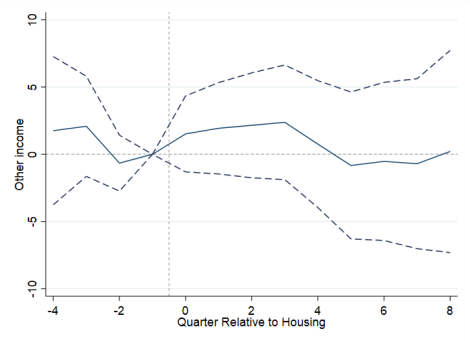
These figures display the coefficients $\{\hat{\beta}_{q(t)}\}$ following the two-step estimation procedure outlined in Borusyak, Jaravel, and Spiess (2024). The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Dependent variables are listed on the y-axis. Panel (a) shows the event study estimates for Rapid Re-Housing. Panel (b) shows the results for Permanent Supportive Housing. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure B.5: Event study results: other income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The category of "other income" comprises income generated from worker's compensation, private disability insurance payouts, pension payments, child support, alimony payments received, and unallocated income. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing. Panel (b) shows the results for Permanent Supportive Housing by month. This specification does not interpolate dependent variables between housing recipients' interactions with the HMIS and related systems.

Appendix C Additional results on heterogeneity by ex-post employment transition type

Table C.1: Predictors of ex-post employment transition type

Panel (a): RRH

	(1) U2U	(2) U2E	(3) E2E	(4) E2U	(5) None
Male	0.011	-0.0059	-0.020	0.012	0.0032
	(0.028)	(0.013)	(0.017)	(0.0077)	(0.028)
Black	-0.068	-0.0041	0.011	0.024	0.038
	(0.033)	(0.012)	(0.020)	(0.0079)	(0.031)
Hispanic	-0.061	0.021	-0.0069	0.017	0.029
	(0.038)	(0.017)	(0.024)	(0.0085)	(0.037)
Native Am.	0.12	-0.026	-0.053	-0.017	-0.020
	(0.074)	(0.024)	(0.033)	(0.0046)	(0.072)
Asian	-0.078	-0.041	0.16	0.058	-0.095
	(0.10)	(0.013)	(0.11)	(0.062)	(0.098)
Pacific Islander	0.18	-0.048	0.050	-0.010	-0.18
	(0.084)	(0.0085)	(0.065)	(0.0044)	(0.067)
Age at event	0.0066	-0.0012	-0.00074	0.000030	-0.0047
	(0.00097)	(0.00036)	(0.00052)	(0.00025)	(0.00093)
Veteran	0.11	-0.019	-0.023	0.014	-0.084
	(0.036)	(0.0099)	(0.019)	(0.014)	(0.033)
Mental health disorder	0.13	0.0017	-0.042	-0.0074	-0.087
	(0.029)	(0.012)	(0.016)	(0.0091)	(0.027)
Alcohol abuse	0.043	0.055	-0.065	0.056	-0.089
	(0.088)	(0.053)	(0.013)	(0.053)	(0.075)
Drug abuse	0.11	-0.030	0.017	-0.015	-0.087
	(0.080)	(0.011)	(0.057)	(0.0080)	(0.072)
Drug & alcohol abuse	-0.0060	-0.015	-0.039	0.0038	0.056
	(0.063)	(0.0094)	(0.014)	(0.024)	(0.062)
Months in homelessness spell	0.00073	-0.000013	-0.00030	-0.000057	-0.00036
	(0.00037)	(0.000098)	(0.00014)	(0.000099)	(0.00035)
Months since earliest spell	0.00040	-0.00014	0.0000081	-0.000051	-0.00022
	(0.00023)	(0.000049)	(0.000089)	(0.000043)	(0.00024)
Times homeless	-0.0012	-0.0011	-0.013	0.00061	0.015
	(0.0053)	(0.0016)	(0.0026)	(0.0013)	(0.0052)
Constant	0.21	0.11	0.20	-0.0053	0.48
	(0.052)	(0.022)	(0.033)	(0.013)	(0.050)
Adj. R-squared	0.10	0.01	0.03	0.00	0.05
N	1446	1446	1446	1446	1446

Heteroskedasticity-robust standard errors in parentheses

This table displays the coefficients from cross-sectional multivariate regressions of the form $y_{it} = \beta_0 + \Gamma X_{it} + e_{it}$ on the sample of Rapid Re-Housing (RRH) recipients entering between January 2014 and February 2018. Each of dependent variables in each corresponds with a different ex-post employment transition type, with "U" referring to unemployment and "E" referring to employment (defined as having the respective status in at least 80% of the relevant period relative to event). E.g. "U2E" refers to the binary outcome of whether an individual was observed as unemployed in at least 80% of pre-event observations and observed as employed in at least 80% of post-event observations. Parentheses report heteroskedasticity-robust standard errors.

Table C.1: Predictors of ex-post employment transition type

Panel (b): PSH

	(1) U2U	(2) U2E	(3) E2E	(4) E2U	(5) None
Male	0.030	-0.0046	-0.0026	-0.0068	-0.016
	(0.015)	(0.0044)	(0.0051)	(0.0045)	(0.014)
Black	-0.025	0.0029	0.0018	0.00028	0.020
	(0.016)	(0.0043)	(0.0051)	(0.0041)	(0.014)
Hispanic	-0.039	-0.0034	-0.0021	-0.0019	0.046
	(0.021)	(0.0044)	(0.0064)	(0.0050)	(0.020)
Native Am.	0.039	-0.0064	-0.012	0.0082	-0.029
	(0.040)	(0.0023)	(0.0031)	(0.015)	(0.037)
Asian	-0.0014	-0.0073	-0.013	0.015	0.0065
	(0.050)	(0.0029)	(0.0043)	(0.022)	(0.046)
Pacific Islander	0.0089	-0.010	0.029	-0.013	-0.015
	(0.081)	(0.0037)	(0.046)	(0.0047)	(0.073)
Age at event	0.0057	-0.00019	-0.00073	-0.00023	-0.0045
	(0.00067)	(0.00015)	(0.00025)	(0.00016)	(0.00063)
Veteran	-0.031	0.0040	0.025	-0.0050	0.0072
	(0.034)	(0.010)	(0.017)	(0.0023)	(0.029)
Mental health disorder	0.057	-0.0049	-0.0055	-0.0068	-0.040
	(0.017)	(0.0047)	(0.0059)	(0.0046)	(0.016)
Alcohol abuse	0.030	0.0016	-0.011	0.013	-0.033
	(0.027)	(0.0090)	(0.0030)	(0.013)	(0.024)
Drug abuse	0.051	-0.0083	-0.0067	0.0015	-0.038
	(0.027)	(0.0022)	(0.0084)	(0.0079)	(0.025)
Drug & alcohol abuse	0.059	-0.0010	-0.014	0.0012	-0.046
	(0.024)	(0.0066)	(0.0031)	(0.0067)	(0.022)
Months in homelessness spell	0.00011	-0.000035	-0.000089	-0.0000066	0.000017
	(0.00018)	(0.000034)	(0.000043)	(0.000019)	(0.00017)
Months since earliest spell	0.00020	0.0000067	0.000019	-0.000032	-0.00020
	(0.00014)	(0.000027)	(0.000042)	(0.000014)	(0.00013)
Times homeless	-0.0010	-0.0015	-0.0025	-0.00037	0.0054
	(0.0027)	(0.00051)	(0.00068)	(0.00058)	(0.0025)
Constant	0.52	0.030	0.066	0.031	0.35
	(0.044)	(0.011)	(0.017)	(0.011)	(0.040)
Adj. R-squared	0.05	-0.00	0.01	-0.00	0.04
N	2324	2324	2324	2324	2324

Heteroskedasticity-robust standard errors in parentheses

This table displays the coefficients from cross-sectional multivariate regressions of the form $y_{it} = \beta_0 + \Gamma X_{it} + e_{it}$ on the sample of Permanent Supportive Housing (PSH) recipients entering between January 2014 and February 2018. Each of dependent variables in each corresponds with a different ex-post employment transition type, with "U" referring to unemployment and "E" referring to employment (defined as having the respective status in at least 80% of the relevant period relative to event). E.g. "U2E" refers to the binary outcome of whether an individual was observed as unemployed in at least 80% of pre-event observations and observed as employed in at least 80% of post-event observations. Parentheses report heteroskedasticity-robust standard errors.

Table C.2: Labor market, earnings, and benefits outcomes:
Conditional on finding employment post-event

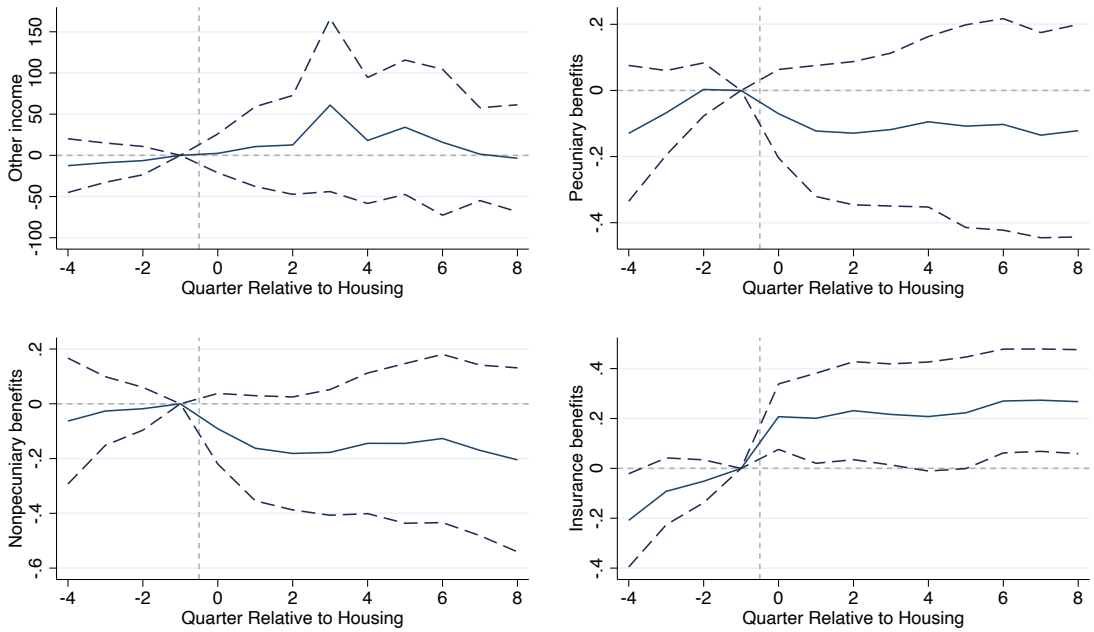
Panel RRH recipients

	Unemployment-to-employment transition				Employment-to-employment transition			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Earned inc.	Benefits inc.	Other inc.	Employed	Earned inc.	Benefits inc.	Other inc.
Pre-period ($t \leq -2$)	-0.030	-80.857	18.833	9.987	0.065	-3.770	-54.270	96.229
	(0.024)	(298.763)	(84.283)	(11.897)	(0.032)	(270.418)	(64.330)	(79.924)
Post-period ($t \geq 1$)	0.897	1141.534	-41.335	-9.010	0.030	311.986	-69.061	77.193
	(0.030)	(339.245)	(73.993)	(14.314)	(0.042)	(264.105)	(54.482)	(71.741)
Post-pre difference	0.927	1222.391	-60.168	-18.998	-0.034	315.756	-14.791	-19.035
	(0.031)	(220.247)	(58.926)	(19.597)	(0.029)	(198.119)	(56.274)	(38.917)
Pre-event average	0.017	103.027	217.896	3.787	0.980	1151.386	73.495	25.302
	[0.128]	[369.524]	[290.541]	[36.911]	[0.141]	[697.478]	[217.680]	[97.194]
Month fixed effects	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X
Adj. R-squared	0.85	0.48	0.63	0.28	0.05	0.56	0.66	0.23
N	3417	485	577	577	1953	337	374	374
Number of clusters	101	94	99	99	59	58	59	59

ID-clustered standard errors in parentheses

Note: This table displays the coefficients from event study regressions with two-way fixed effects of the form $y_{it} = \alpha_i + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 1\} + \theta \cdot \mathbb{1}\{EventTime_{it} = 0\} + u_{it}$ on the sample of subsample of RRH recipients in our main sample that we observe as being employed in at least 80% of their post-housing events. The pre- and post-period coefficients ($\hat{\gamma}$ and $\hat{\beta}$) are specified relative to the base-period average at one period prior to the housing event. The post-pre difference value subtracts $\hat{\gamma}$ from $\hat{\beta}$; the event-period coefficient $\hat{\theta}$ is omitted in this calculation. The pre-period includes up to 12 months pre-event, and the post-period extends to 24 months post-event. Standard errors are clustered on the individual-level. Standard deviations are reported in hard brackets.

Figure C.1: RRH U2E Transitions (other main outcomes)

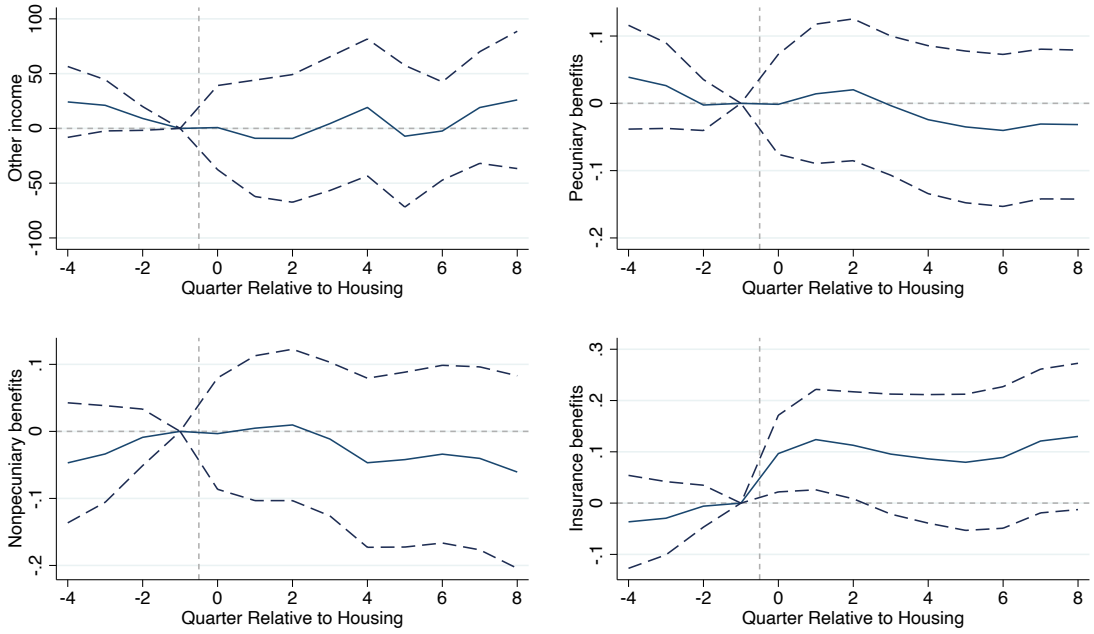


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$$

The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition between unemployment and employment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.2: RRH E2E Transitions (other main outcomes)

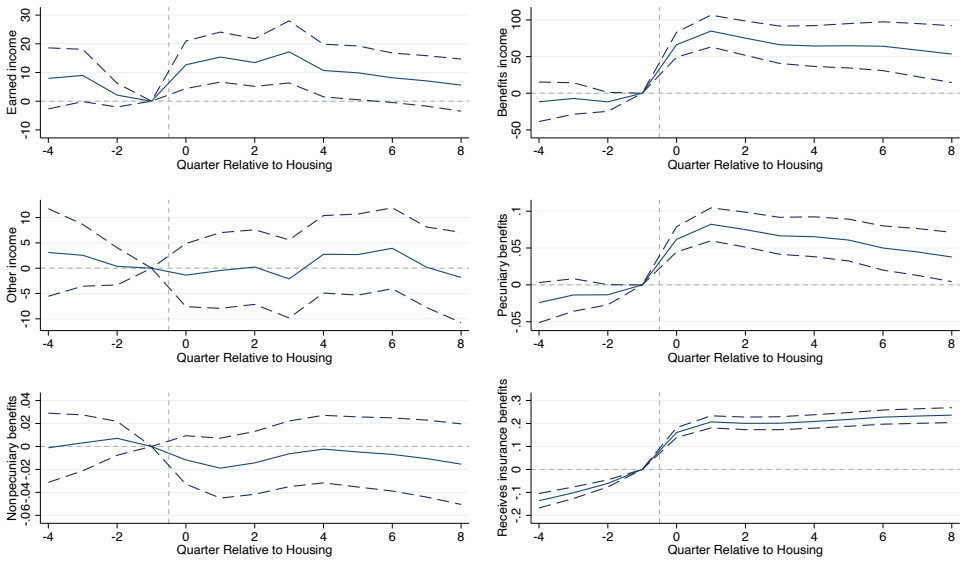


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$$

The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition from employment and employment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.3: RRH U2U Transitions

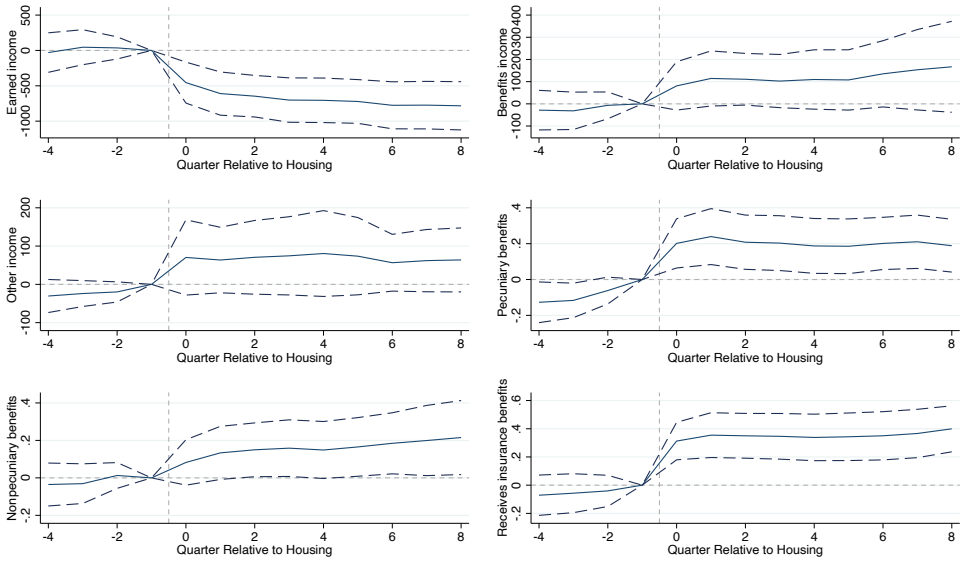


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition from unemployment to unemployment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.4: RRH E2U Transitions

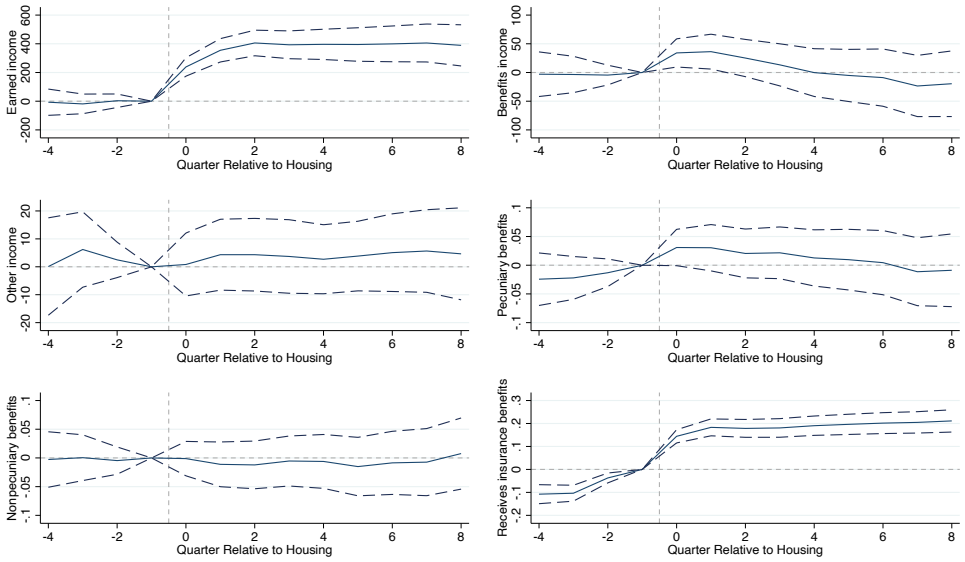


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition from employment to unemployment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.5: RRH "None" Transitioners

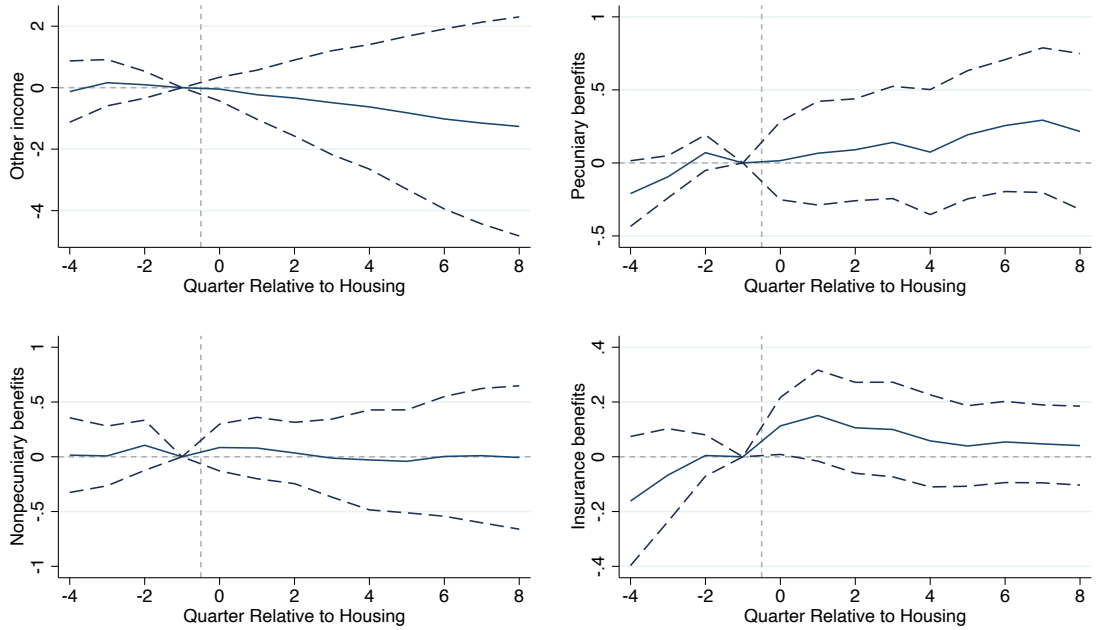


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition report employment between 20 and 80% of months pre- and/or post-event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.6: PSH U2E Transitions (other main outcomes)

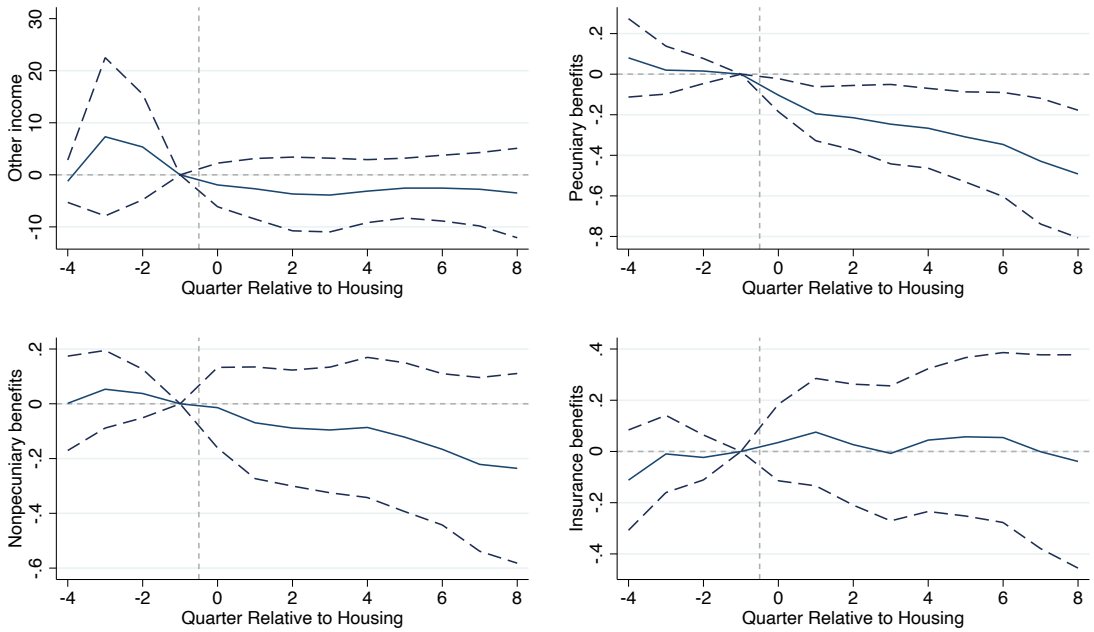


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$$

The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition between unemployment and employment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.7: PSH E2E Transitions (other main outcomes)

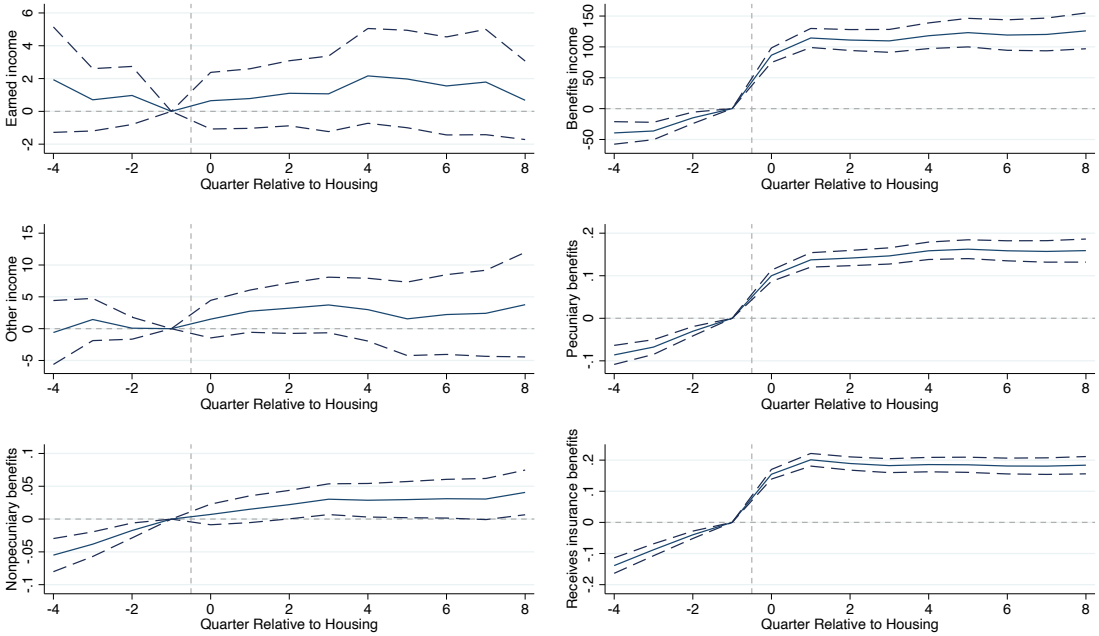


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$$

The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition from employment and employment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.8: PSH U2U Transitions

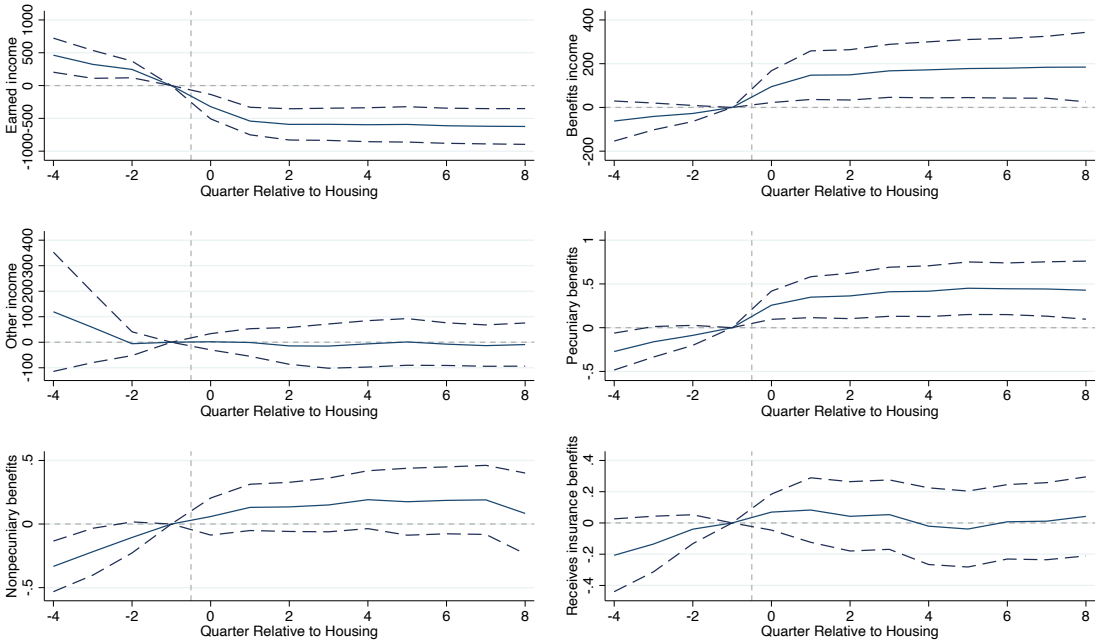


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition from unemployment and unemployment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.9: PSH E2U Transitions

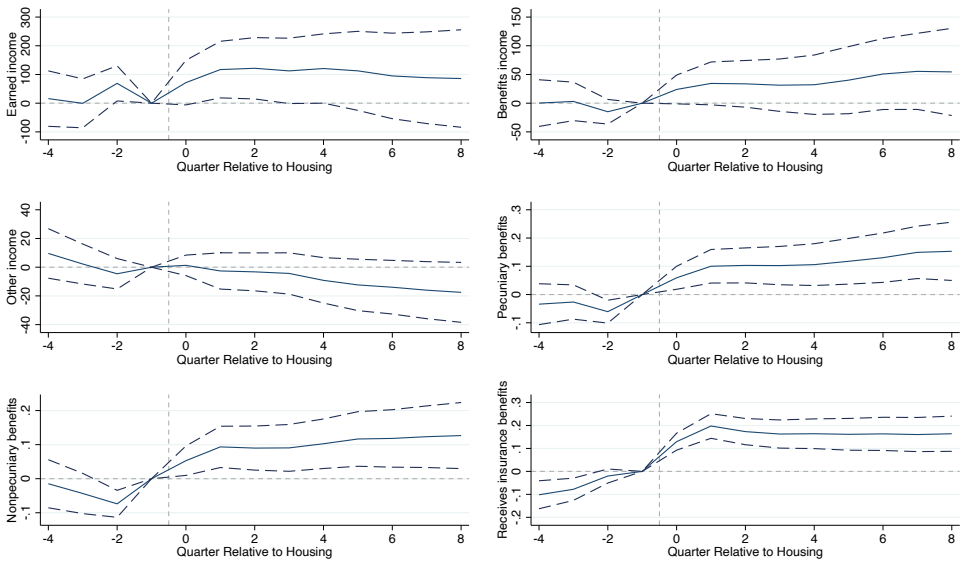


This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition from employment to unemployment after the event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure C.10: PSH "None" Transitioners



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \delta_t + \sum_{q(j) \neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition report employment between 20 and 80% of months both pre- and post-event. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Appendix D Additional results on sociodemographic heterogeneity

Table D.1: Labor market, earnings, and benefits outcomes: Heterogeneity by gender

Panel (a): RRH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Female	0.021	10.987	-0.007	44.316	-0.068	-35.448	-0.041	12.551	0.105
	(0.020)	(33.779)	(0.049)	(29.327)	(0.073)	(21.638)	(0.046)	(9.528)	(0.081)
Post-event	0.094	152.649	0.139	93.544	0.141	61.590	0.071	-2.323	-0.040
	(0.015)	(26.607)	(0.037)	(22.954)	(0.053)	(16.860)	(0.033)	(5.672)	(0.053)
Constant	0.137	472.576	6.475	183.096	6.934	281.018	6.230	16.944	6.315
	(0.009)	(16.372)	(0.022)	(14.916)	(0.036)	(9.702)	(0.018)	(3.859)	(0.023)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.54	0.57	0.65	0.56	0.63	0.69	0.81	0.52	0.98
N	56331	11865	7302	11308	2179	11865	5522	11865	232
Number of clusters	1692	1679	1188	1669	489	1679	955	1679	65

ID-clustered standard errors in parentheses

Panel (b): PSH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Female	0.008	-35.317	-0.035	11.020	-0.173	-48.855	-0.046	6.501	0.075
	(0.010)	(19.382)	(0.027)	(15.395)	(0.164)	(15.591)	(0.024)	(4.991)	(0.106)
Post-event	0.017	108.898	0.079	4.723	-0.027	107.645	0.083	-2.461	-0.102
	(0.007)	(13.919)	(0.018)	(8.099)	(0.106)	(11.632)	(0.017)	(4.535)	(0.092)
Constant	0.042	481.001	6.158	48.938	6.727	420.962	6.101	10.250	6.309
	(0.004)	(7.580)	(0.010)	(4.690)	(0.054)	(6.018)	(0.009)	(2.199)	(0.047)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.55	0.58	0.74	0.50	0.69	0.67	0.80	0.40	0.97
N	77982	12602	10598	12413	593	12604	9970	12604	121
Number of clusters	2233	2170	1994	2159	164	2170	1915	2170	37

ID-clustered standard errors in parentheses

Note: This table display coefficients from event study regressions of the form: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \varepsilon_{it}$. $Status_i$ in the above regressions is an indicator for whether individual i is a woman. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH recipiency and $\{\hat{\psi}_k\}$ identify the relative differential response of individuals of a given sociodemographic status relative to those who are not. The population consists of individuals in our sample receiving RRH (in Panel (a)) or PSH (in Panel (b)) between 2014 and 2018. Standard errors are clustered at the individual-level.

Table D.2: Labor market, earnings, and benefits outcomes: Heterogeneity by guardian status

Panel (a): RRH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Guardian	0.002	40.541	-0.046	94.149	-0.161	-76.248	-0.115	10.632	0.151
	(0.036)	(62.353)	(0.061)	(53.204)	(0.089)	(35.951)	(0.053)	(14.757)	(0.128)
Post-event	0.128	162.150	0.141	97.213	0.128	63.781	0.085	4.590	-0.025
	(0.017)	(28.913)	(0.033)	(26.041)	(0.047)	(17.788)	(0.030)	(5.979)	(0.053)
Constant	0.168	621.506	6.479	254.644	6.933	365.336	6.231	17.082	6.257
	(0.011)	(21.069)	(0.024)	(18.711)	(0.035)	(12.838)	(0.021)	(4.605)	(0.028)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.53	0.51	0.64	0.55	0.64	0.66	0.80	0.54	0.99
N	38308	8402	6555	8001	2005	8402	4944	8402	183
Number of clusters	1132	1122	1041	1116	437	1122	843	1122	50

ID-clustered standard errors in parentheses

Panel (b): PSH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Guardian	0.125	-80.425	-0.071	-27.614	-0.864	-84.065	-0.064	36.519	0.156
	(0.048)	(51.664)	(0.060)	(17.541)	(0.449)	(42.893)	(0.049)	(28.921)	(0.146)
Post-event	0.015	87.199	0.063	7.105	-0.100	84.423	0.063	-1.754	-0.120
	(0.006)	(11.690)	(0.015)	(7.533)	(0.090)	(9.087)	(0.013)	(3.408)	(0.117)
Constant	0.045	499.175	6.153	52.696	6.740	435.137	6.095	10.415	6.294
	(0.004)	(7.637)	(0.010)	(4.922)	(0.058)	(5.923)	(0.009)	(2.235)	(0.074)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.55	0.58	0.74	0.49	0.69	0.68	0.80	0.39	0.98
N	71657	11706	10119	11527	576	11708	9531	11708	98
Number of clusters	2040	1984	1884	1973	158	1984	1811	1984	32

ID-clustered standard errors in parentheses

Note: This table display coefficients from event study regressions of the form: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \varepsilon_{it}$. $Status_i$ in the above regressions is an indicator for whether individual i is a parent or guardian of children. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH recipiency and $\{\hat{\psi}_k\}$ identify the relative differential response of individuals of a given sociodemographic status relative to those who are not. The population consists of individuals in our sample receiving RRH (in Panel (a)) or PSH (in Panel (b)) between 2014 and 2018. Standard errors are clustered at the individual-level.

Table D.3: Labor market, earnings, and benefits outcomes:
Heterogeneity by pre-event mental health status

Panel (a): RRH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Reported mental health issues	-0.020	-17.510	-0.074	-48.838	0.005	31.706	-0.032	1.230	-0.089
	(0.019)	(32.671)	(0.049)	(27.662)	(0.071)	(22.298)	(0.050)	(9.120)	(0.114)
Post-event	0.107	163.850	0.170	133.110	0.131	31.251	0.068	3.397	0.086
	(0.015)	(27.279)	(0.042)	(24.479)	(0.048)	(16.534)	(0.046)	(4.590)	(0.079)
Constant	0.144	477.744	6.469	188.339	6.908	282.794	6.223	15.824	6.264
	(0.009)	(16.590)	(0.023)	(15.000)	(0.035)	(9.906)	(0.020)	(3.480)	(0.028)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.54	0.56	0.65	0.55	0.63	0.68	0.81	0.51	0.98
N	54866	11539	7192	10996	2174	11539	5407	11539	217
Number of clusters	1621	1610	1161	1600	483	1610	930	1610	61

ID-clustered standard errors in parentheses

Panel (b): PSH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Reported mental health issues	-0.020	-17.510	-0.074	-48.838	0.005	31.706	-0.032	1.230	-0.089
	(0.019)	(32.671)	(0.049)	(27.662)	(0.071)	(22.298)	(0.050)	(9.120)	(0.114)
Post-event	0.107	163.850	0.170	133.110	0.131	31.251	0.068	3.397	0.086
	(0.015)	(27.279)	(0.042)	(24.479)	(0.048)	(16.534)	(0.046)	(4.590)	(0.079)
Constant	0.144	477.744	6.469	188.339	6.908	282.794	6.223	15.824	6.264
	(0.009)	(16.590)	(0.023)	(15.000)	(0.035)	(9.906)	(0.020)	(3.480)	(0.028)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.54	0.56	0.65	0.55	0.63	0.68	0.81	0.51	0.98
N	54866	11539	7192	10996	2174	11539	5407	11539	217
Number of clusters	1621	1610	1161	1600	483	1610	930	1610	61

ID-clustered standard errors in parentheses

Note: This table display coefficients from event study regressions of the form: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \varepsilon_{it}$. $Status_i$ in the above regressions is an indicator for whether individual i reported adverse mental health status prior to housing recipiency. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH recipiency and $\{\hat{\psi}_k\}$ identify the relative differential response of individuals of a given sociodemographic status relative to those who are not. The population consists of individuals in our sample receiving RRH (in Panel (a)) or PSH (in Panel (b)) between 2014 and 2018. Standard errors are clustered at the individual-level.

Table D.4: Labor market, earnings, and benefits outcomes: Heterogeneity by non-white racial status

Panel (a): RRH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Non-white	0.014	4.173	0.005	-0.680	-0.035	-11.667	-0.018	8.869	0.134
	(0.020)	(35.151)	(0.052)	(28.985)	(0.070)	(25.067)	(0.051)	(8.354)	(0.126)
Post-event	0.097	156.546	0.137	115.337	0.153	53.908	0.067	-2.694	-0.097
	(0.017)	(30.632)	(0.047)	(25.234)	(0.062)	(23.152)	(0.044)	(6.452)	(0.121)
Constant	0.137	471.728	6.472	185.223	6.914	277.911	6.226	17.106	6.349
	(0.009)	(16.278)	(0.022)	(14.736)	(0.035)	(9.759)	(0.019)	(3.751)	(0.037)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.54	0.57	0.65	0.56	0.63	0.69	0.81	0.52	0.98
N	56829	11961	7360	11401	2229	11961	5532	11961	232
Number of clusters	1707	1694	1197	1684	497	1694	958	1694	65

ID-clustered standard errors in parentheses

Panel (b): PSH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Non-white	-0.012	-21.967	-0.021	5.810	0.006	-16.751	-0.026	-11.886	-0.102
	(0.009)	(19.069)	(0.027)	(12.249)	(0.171)	(16.157)	(0.025)	(5.544)	(0.083)
Post-event	0.026	105.822	0.075	4.180	-0.111	97.431	0.079	7.142	-0.043
	(0.008)	(14.644)	(0.022)	(8.315)	(0.143)	(12.586)	(0.020)	(3.836)	(0.060)
Constant	0.042	482.830	6.160	49.594	6.729	422.422	6.103	10.010	6.318
	(0.004)	(7.539)	(0.010)	(4.592)	(0.054)	(6.027)	(0.009)	(2.152)	(0.048)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.56	0.58	0.75	0.50	0.69	0.67	0.80	0.40	0.97
N	79081	12815	10779	12624	601	12817	10142	12817	121
Number of clusters	2264	2201	2024	2190	166	2201	1943	2201	37

ID-clustered standard errors in parentheses

Note: This table display coefficients from event study regressions of the form: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \varepsilon_{it}$. $Status_i$ in the above regressions is an indicator for whether individual i is a non-white. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH recipiency and $\{\hat{\psi}_k\}$ identify the relative differential response of individuals of a given sociodemographic status relative to those who are not. The population consists of individuals in our sample receiving RRH (in Panel (a)) or PSH (in Panel (b)) between 2014 and 2018. Standard errors are clustered at the individual-level.

Table D.5: Labor market, earnings, and benefits outcomes:
Heterogeneity by pre-event substance abuse status

Panel (a): RRH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Reported substance abuse	-0.007	-30.178	-0.047	-68.268	0.134	35.260	-0.013	0.305	-0.064
	(0.023)	(36.962)	(0.054)	(29.742)	(0.100)	(28.161)	(0.051)	(10.858)	(0.064)
Post-event	0.108	167.678	0.156	134.045	0.101	36.468	0.059	3.009	0.048
	(0.014)	(22.871)	(0.031)	(20.981)	(0.043)	(13.180)	(0.027)	(5.341)	(0.042)
Constant	0.137	470.243	6.470	181.815	6.918	279.660	6.226	17.103	6.310
	(0.009)	(16.294)	(0.022)	(14.849)	(0.034)	(9.514)	(0.018)	(3.724)	(0.028)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.54	0.57	0.65	0.56	0.63	0.69	0.81	0.52	0.98
N	56829	11961	7360	11401	2229	11961	5532	11961	232
Number of clusters	1707	1694	1197	1684	497	1694	958	1694	65

ID-clustered standard errors in parentheses

Panel (b): PSH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Reported substance abuse	-0.007	-30.178	-0.047	-68.268	0.134	35.260	-0.013	0.305	-0.064
	(0.023)	(36.962)	(0.054)	(29.742)	(0.100)	(28.161)	(0.051)	(10.858)	(0.064)
Post-event	0.108	167.678	0.156	134.045	0.101	36.468	0.059	3.009	0.048
	(0.014)	(22.871)	(0.031)	(20.981)	(0.043)	(13.180)	(0.027)	(5.341)	(0.042)
Constant	0.137	470.243	6.470	181.815	6.918	279.660	6.226	17.103	6.310
	(0.009)	(16.294)	(0.022)	(14.849)	(0.034)	(9.514)	(0.018)	(3.724)	(0.028)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.54	0.57	0.65	0.56	0.63	0.69	0.81	0.52	0.98
N	56829	11961	7360	11401	2229	11961	5532	11961	232
Number of clusters	1707	1694	1197	1684	497	1694	958	1694	65

ID-clustered standard errors in parentheses

Note: This table display coefficients from event study regressions of the form: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \varepsilon_{it}$. $Status_i$ in the above regressions is an indicator for whether individual i reported substance abuse problems prior to UH recipiency. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH recipiency and $\{\hat{\psi}_k\}$ identify the relative differential response of individuals of a given sociodemographic status relative to those who are not. The population consists of individuals in our sample receiving RRH (in Panel (a)) or PSH (in Panel (b)) between 2014 and 2018. Standard errors are clustered at the individual-level.

Table D.6: Labor market, earnings, and benefits outcomes: Heterogeneity by veteran status

Panel (a): RRH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Veteran	-0.053	42.800	-0.121	-72.785	0.023	105.503	0.039	-22.595	-0.064
	(0.024)	(45.438)	(0.051)	(35.241)	(0.067)	(35.111)	(0.050)	(13.058)	(0.071)
Post-event	0.126	161.908	0.166	134.942	0.122	30.558	0.046	7.790	0.046
	(0.015)	(23.611)	(0.032)	(21.579)	(0.047)	(13.670)	(0.027)	(5.405)	(0.041)
Constant	0.147	514.427	6.475	205.210	6.917	300.886	6.225	18.606	6.312
	(0.010)	(17.374)	(0.022)	(15.747)	(0.035)	(10.351)	(0.019)	(4.050)	(0.026)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.53	0.55	0.65	0.55	0.62	0.68	0.81	0.52	0.98
N	51190	11013	7356	10477	2227	11013	5530	11013	232
Number of clusters	1536	1524	1195	1515	496	1524	957	1524	65

ID-clustered standard errors in parentheses

Panel (b): PSH recipients

	(1) Employed	(2) Income	(3) Log inc.	(4) Earned inc.	(5) Log earned inc.	(6) Benefits inc.	(7) Log benefits inc.	(8) Other inc.	(9) Log other inc.
Post-event × Veteran	0.024	133.571	-0.016	-3.811	-0.528	165.516	0.025	-29.365	-0.049
	(0.024)	(76.280)	(0.056)	(23.129)	(0.361)	(68.313)	(0.046)	(24.939)	(0.106)
Post-event	0.018	87.106	0.063	7.937	-0.091	80.454	0.062	1.218	-0.058
	(0.006)	(11.283)	(0.015)	(7.224)	(0.084)	(8.788)	(0.013)	(3.398)	(0.067)
Constant	0.043	485.658	6.161	49.970	6.749	424.576	6.104	10.310	6.309
	(0.004)	(7.617)	(0.010)	(4.655)	(0.057)	(6.070)	(0.009)	(2.191)	(0.045)
Month fixed effects	X	X	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X	X	X
Adj. R-squared	0.56	0.58	0.75	0.50	0.70	0.67	0.80	0.40	0.97
N	78116	12708	10748	12517	601	12710	10111	12710	121
Number of clusters	2236	2173	2018	2162	166	2173	1937	2173	37

ID-clustered standard errors in parentheses

Note: This table display coefficients from event study regressions of the form: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \sum_{q(j)\neq -1} \psi_{q(j)} \cdot Status_i \cdot \mathbb{1}\{EventTime_{q(t(j))} \geq 0\} + \varepsilon_{it}$. $Status_i$ in the above regressions is an indicator for whether individual i is an US armed forces veteran. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH recipiency and $\{\hat{\psi}_k\}$ identify the relative differential response of individuals of a given sociodemographic status relative to those who are not. The population consists of individuals in our sample receiving RRH (in Panel (a)) or PSH (in Panel (b)) between 2014 and 2018. Standard errors are clustered at the individual-level.

Appendix E Data Construction

Our data originates entirely from the Los Angeles Homelessness Management Information System (HMIS). HMIS data is collected at the continuum-of-care-level, which comprises the majority of Los Angeles County. Here, we elaborate on the construction of the panel that we use in our analysis.

Data is initially broken up into a number of files available for use by researchers. Among those files, we use files denoted (internally) as Client, Disabilities, Education and Employment, Enrollment, Income and Benefits, and Services. Each of these files is unique at either the individual-level, individual- by program-level, or at the individual- by interaction-level. A brief description of each of the datasets follows.

"Client": Data is unique at the individual-level. Primarily contains demographic information that is collected at intake into the system (and is time-invariant). Little-to-no manipulation of the file is necessary for it to conform.

"Disabilities": Data is unique at the individual- by date-level. Data recorded here are primarily indicators for 6 broad categories of disabilities: physical disabilities, developmental disabilities, chronic conditions, HIV/AIDS, mental health, substance abuse. In cases with duplicate entries within a given date, we replace disability information with the maximum of the reported information on that date (i.e. indicator for an issue would take value 1 within a date if one of the entries indicated it).

"Education and Employment": Data is unique at the individual-by date-level. Data recorded are primarily updates on information regarding employment and earnings.

"Enrollment": Data is unique at the individual-by enrollment-level. An "enrollment", in this case, is a specific type of interaction with the HMIS. Any interaction that meets this criteria is then recorded, along with what type of interaction it was. In general, one should think of these as enrollments into programs; i.e. employment training programs, housing referrals, etc.

"Income and Benefits": Data is unique at the individual-by interaction-level. Information, such as earned income, employment status, benefits enrollments, etc. are recorded here. Information for income and benefits are not recorded for every type of enrollment and so is not available at every HMIS interaction.

"Services": Data is unique at the individual-by service interaction-level. In this way, each individual can have zero to dozens of services rendered (and recorded) on any given day. Every service recorded is administered by LAHSA or a LAHSA affiliate. Each time a service is rendered, it is *not* necessarily the case that an update is made to one of the other datasets; in fact, updates to other sets made as a result of a service interaction are the exception. We collapse relevant service information to the individual-by month-level and retain the number of services rendered (in a given month), as well as the total estimated value of these services. These are the variables utilized in the main text.

In interactions with the systems that record the data, a consistent ID is maintained so that individuals can be tracked. Therefore, merging the files is simple and the only choice

available to the researcher is whether (and how) to collapse information into a panel. Our primary panel is at the month-year by individual-level. As such, in instances where multiple interactions take place in the same month, for the same person, we take either the mean or the max of the recorded value. In general, we take the mean for numerical entries (income in a month, for instance) and we take the max for an interaction (an indicator for whether someone was receiving TANF, for instance). In this way, each person has at most one unique value for each variable in each month-year of our panel.