# The labor market impacts of unconditional housing out of homelessness

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**Abstract**

We leverage variation in the timing of unconditional housing receipt by homeless individuals in Los Angeles County to determine the effects of housing on their employment, earnings, and benefit receipt. We validate our event study approach by demonstrating that outcomes follow parallel trends prior to housing receipt, showing that wait times are unrelated to homelessness severity, and ruling out the possibility of other relevant changes occurring simultaneously to housing receipt. 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 reflect 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; Homelessness 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 insofar as lack of housing represents a labor market friction. But, there exists little evidence as to whether housing programs for the homeless do increase employment and income (Meyer, Wyse, Grunwaldt, et al. (2021), Von Wachter, Schnorr, and Riesch (2020), and Jacob and Ludwig (2012)). Moreover, public spending on homeless individuals represents a first-order welfarist concern from the social planner's perspective, as such individuals could reasonably constitute those with the highest marginal social welfare weights (Saez and Stantcheva (2016)).

We use panel 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 analysis centers on 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. There is precedent in the homelessness literature, as well as conceptual differences in the goals and implementation of the two programs, that motivate our focus on RRH and PSH, as well as our approach in studying them as distinct. For one, PSH and RRH are typically the most widely implemented Housing First programs (Evans, Phillips, and Ruffini (2021)). Additionally, a growing literature emphasizes the importance of targeting homelessness program placement based on potential benefit

[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.

(Von Wachter et al. (2019)). Yet, RRH and PSH serve two very different populations. RRH tends to target homeless and at-risk individuals and families with moderate needs who can quickly transition to independence. Conversely, PSH tends to target those with chronic, severe service needs, particularly with disabilities, who require long-term, indefinite support. Moreover, housing receipt under RRH is typically time-limited for a duration of two years, whereas receipt of housing under PSH is indefinite. In this respect, one might anticipate larger income effects associated with PSH.

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, characterize causal mechanisms, and describe margins of sociodemographic heterogeneity, and 4) credible quasi-random variation in the timing of housing receipt. 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 (Zaretzky and Flatau (2013)). 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. Importantly however, our results only apply to individuals that 1) interact with homelessness services providers and 2) are willing to enroll in UH programs; this subpopulation of homeless and at-risk individuals likely differs substantially from homeless individuals that do not meet these criteria.

We exploit quasi-random timing in receipt of unconditional housing to estimate the labor market and benefit take-up impacts of receiving unconditional housing. This quasi-random

timing of housing receipt arises from the time elapsed between initial entry into the homelessness services 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, as determined by the housing authorities. Using event studies that leverage this variation in time until housing receipt, our main specifications estimate the employment and earnings impacts of unconditional housing receipt for individuals receiving RRH and PSH. We turn to characterizing the mechanisms that drive our observed responses (e.g. homelessness severity, income effects associated with housing receipt, looking-for-work status) as well as heterogeneous impacts across different sociodemographic backgrounds. Lastly, we use our estimates to calculate the costs of unconditional housing that are offset specifically 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 argue that the difference in employment response between RRH and PSH recipients is attributable to differences in selection between these programs rather than solely treatment effects of these programs themselves.[3] We demonstrate the robustness of our main results to 1) different censoring restrictions on our sample, 2) different assumptions about the updating process to the employment outcomes in our data, 3) estimating alternate event study specifications that control for homelessness services entry-year cohort specific effects and the threat of mechanical mean reversion to greater levels of employment, and 4) alternate event study estimation procedures that address concerns about mean reversion and potential heterogeneous treatment effects under staggered event timing across treated units.

A natural question that arises relates to the mechanisms that drive our main results. We first characterize changes in earnings and benefits absorption based on ex-post em-

[3]We examine this hypothesis of different underlying potential outcomes between PSH and RRH recipients explicitly in Section 5. One can see evidence of this difference in selection by looking at Figure A.2 and Table 1.

ployment transition type. Conditional on making an unemployment-to-employment (U2E) transition following housing receipt, 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 in our data environment. We observe that individuals with frequent employment in the post-period see *no* concurrent increase in benefits uptake. For this reason, we attribute the employment and earnings effects of U2E and E2E transitioners primarily to having stable permanent shelter rather than to connection to additional benefits coinciding with placement into housing.

We also find 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 further 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.

Following these aforementioned differences between RRH and PSH treatment effects, we hypothesize that this difference is largely attributable to heterogeneous potential outcomes: RRH recipients are less disadvantaged than PSH recipients. To further explore this mechanism, we estimate treatment effect heterogeneity by measures of predicted baseline advantage. We generate measures of ex-ante predicted advantage by regressing pre-period earnings on sociodemographic characteristics and projecting predicted values onto individuals. We find that this measure of "predicted baseline advantage" strongly predicts the treatment effect in our baseline specification for RRH, with the "most advantaged" individuals exhibiting employment effects three times as large the "least advantaged". However, for PSH, we document no gradient of treatment effect in this advantage index. We attribute this null result to an inherent lack of dispersion in the underlying distribution of pre-period earnings among PSH recipients: PSH recipients are overwhelmingly classified as *very* disadvantaged,

with little heterogeneity. Therefore, while we do observe some evidence of income effects in the form of a decrease in looking-for-work status among PSH recipients, we attribute most of the difference in outcomes between these two programs as driven by heterogeneous potential outcomes.

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 cost-benefit analysis of these programs when only considering labor market and earnings effects (i.e., excluding other channels for fiscal spillovers, such as effects on criminal justice system and public health system usage). We estimate substantial variation in the net fiscal impact of RRH and PSH receipt based on whether an individual recipient secures employment following housing receipt. 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 program costs for RRH and near zero (but non-negative) for PSH recipients. However, our estimates only speak to outcomes within two years of housing, which may understate longer-run impacts on earnings and recidivism into homelessness.

## 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 cross-sectional analyses of benefits absorption among incumbent

homeless populations.[4]

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 that 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 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 cost increases of 6%.

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

[4]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.

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 receipt 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 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 receipt 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 demonstrates their robustness to changes. Section 5 explores mechanisms and characterizes heterogeneous responses by ex-post employment transition type and different demographic characteristics. 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 United States (Council of Economic Advisers (2019)). In most of the US, 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 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 pets, etc.; Los Angeles Homeless Services Authority (2025); Los Angeles Homeless Services Authority (2024)).[5] Their administrative 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 receipt 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.[6] 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 receipt, our estimates will *understate* the true effect of unconditional housing receipt on employ-

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

[6] 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.

ment/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 or shelter-access treatment based on their homelessness severity and recommendation of their case worker. Individuals can be placed into multiple queues for receipt of different housing/shelter programs, but our data only report the housing/shelter option taken upon offer.

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.) while RRH is targeted toward individuals with lower homelessness severity. We observe these differences in our data, with PSH recipients, relative to RRH recipients, reporting 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 their current spell, and (3) completeness of their application.[7] 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

[7]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 directly observe the queue position in our data.

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.[8]

## 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).[9] Our data cover the universe of individuals interacting with the HMIS in the LA CoC from 2013 to February 2020.[10]

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, 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

[8]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 also 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.

[9]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. These data are maintained separately from and cannot be merged with other existing Los Angeles county administrative datasets, such as on credit usage and unemployment insurance rolls.

[10]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.

their housing tenure.[11]

Importantly, we only observe changes in individuals' employment or earnings if these outcomes are explicitly reported in an interaction. In our main specification, we assume that outcomes are constant between interactions. Section 4.3 relaxes this assumption using a series of alternate specifications, including mapping non-interaction periods to missing or mapping them uniformly to zero in a "worst-case scenario".

### 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 Meyer, Wyse, Grunwaldt, et al. (2021) point out, most work on the employment and earnings characteristics of homeless 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 receipt. 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-receipt as they feel less pressure to do so (although there are no statutory features of RRH or PSH that would incentivize this behavior aside

[11] We also have data on individuals following the end of their housing receipt if they continue interacting with services connected to the HMIS.

from the income eligibility ceiling, which we view as largely non-binding in our setting).[12] While we do not have external data that speak directly to this concern, several features of HMIS data maintenance protocol specific to LAHSA and the Los Angeles CoC alleviate this concern. Additionally, several features of our main results and exploration of mechanisms support our argument that our findings are not driven by changes in measurement error around housing receipt (see Section 5)

Unlike in most 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 independent verification.[13] Following housing enrollment, while LAHSA faces no explicit pecuniary incentives to verify the income and employment status of PSH recipients, LAHSA does rely on employment and earnings outcomes in order to determine rent cost-sharing levels of RRH recipients. For this reason, LAHSA sees incentive to continue to seek out accurate reporting of these outcomes post-enrollment. Additionally, both RRH and PSH require annual re-certification of status; our results show no systematic irregularities that occur on part of employment or earnings outcomes at the one-year post-event mark.

[12]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-receipt. This scenario would negatively bias our estimates, although we do not see any mechanisms that would incentivize this behavior.

[13]See, for instance, LAHSA Form 1087 (link available as of March 2026).

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[14]) 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 receipt 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 recipients interact relatively frequently with the HMIS (Figure A.1). Moreover, Table A.2 confirms that the share of individuals' non-missing observations are largely uncorrelated

[14]Namely, Section 4 and Section 5 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 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.

with observable characteristics, including pre- and post-event employment probability.[15] 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 January 2013 and February 2020. The data is initially structured on the interaction-level. We aggregate all available information in our data to the individual $\times$ month level. While this decision potentially 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. We construct an additional sample that consists of individuals that interact with the HMIS but do not receive UH between 2013 and February 2020 for the purpose of demonstrating robustness and exploring external validity.

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.[16] Table A.1 shows that our main sample actually sees *lower* risk scores than the dropped sample, although our data

[15] We do observe some positive correlation between missing observations and wait times, which makes sense given the mechanical relationship between wait time and the possibility of non-reporting.

[16] 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.

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 studying homelessness (Meyer, Wyse, Grunwaldt, et al. (2021)). Section 4.3 presents robustness checks that replicate our main result while removing 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.[17]

Individuals in our sample tend to be around age 45 at time of their earliest interaction with the HMIS. Men, non-white people, and US armed forces veterans see greater representation in both the untreated and treated samples relative to the US population. Individuals are overwhelmingly unemployed at the time of their first HMIS interaction, although less so for RRH recipients. Interestingly, we observe significantly lower employment among RRH, PSH, and untreated individuals than documented by Meyer, Wyse, Grunwaldt, et al. (2021). 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). The table also demon-

[17] 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.

strates that PSH recipients and untreated individuals have a higher risk score (as judged by HMIS case workers) than do RRH recipients.[18] Average total monthly income among those that interact with the HMIS is between USD 300 and 450.[19] 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 only around USD 1200-1500 per month upon initial interaction with HMIS, which aligns closely with both Meyer, Wyse, Grunwaldt, et al. (2021) and Von Wachter, Schnorr, and Riesch (2020). Finally, most individuals are homeless for 1-3 years prior to receiving some form of long-term housing intervention, although this result is partly mechanically determined by our main sample restriction requiring individual to have waited more than six months to housing receipt. We also observe that wait times are shorter for RRH recipients relative to PSH 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.[20]

[18] "Risk score" is determined by VI-SPDAT and/or VI-F-SPDAT. Figure A.2 plots a histogram of risk scores among these three populations.

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

[20] The precise date of entry into UH accommodations contains 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

Leveraging the quasi-random variation in timing of housing receipt 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 specifically, as it may be the case that upon connection to housing, individuals may also be connected to other services that also affect outcomes. We explicitly investigate this possibility below in Section 5; we provide evidence that this assumption indeed holds in our setting, although other unobserved confounders may persist (such as increased caseworker assistance following placement into housing).

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 to individuals in a seemingly quasi-random manner), 2) supply of available housing units, and 3) demand by other individuals of housing units.

Our identification assumption does not depend on an orthogonality condition between wait times and individual pre-event characteristics. This said, we are still interested in further validating our use of wait time by demonstrating that wait time is unrelated to characteristics that are ostensibly indicative of potential outcomes, such as risk score or labor market attachment. 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 *zero* 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,

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.

respectively, and individuals reporting mental health or substance abuse issues tend to wait 2-4 months longer for both RRH and PSH. Note that sample sizes vary substantially between these univariate regressions, reflecting the variation in information available pertaining to certain demographic characteristics. For this reason, we do not attempt to control for these covariates in our main specification. Additionally Figure 1 Panel (a) shows explicitly that there is no observable relationship between individuals' risk score assessment and the time elapsed between initial intake and placement into housing.[21]

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. Upon their first interaction with the HMIS within a homelessness spell, individuals 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 6 months elapse between entry into the HMIS and their placement into unconditional housing.[22] 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

[21]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. Also note that families are assigned a similarly designed score, referred to as VI-FSPDAT on the same scale. We refer to both of these measures as Risk Score.

[22]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 this 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 Table A.1 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.

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. We treat as the event the earliest instance of housing program receipt for each individual and require individuals not receive either RRH or PSH prior to January 2014.[23] 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 by the simple differences in observable characteristics as reported in Table 1.[24] 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.

In terms of contextualizing the external validity of this design, 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

[23] 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.

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

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); 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. Our main specification assumes that values of these variables are constant between interactions, while Section 4.3 presents several robustness checks of our results to this assumption, including dropping between-interaction observations or assuming "worst-case scenario" outcomes between interactions (e.g. non-observation corresponding to unemployment, zero earnings, etc.).

# 4 Results

We document large positive effects of housing receipt on labor market outcomes, particularly for recipients of RRH. Figure 2 illustrates the evolution in mean extensive margin employment around placement into UH. The figure shows a sharp increase in employment from 15% to nearly 25% for RRH recipients around move-in, but a more muted response among PSH recipients ranging between 5% and 6.5% with a mild positive trend leading into the event. Figure 3 elaborates on this result, plotting estimates from the event studies following Equation 1 following placement into both RRH and PSH. Table 3 and 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, although our estimates demonstrate stability in the post-event period.

## 4.1 The Effects of RRH

Panels (a)-(c) of Figure 3 show 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.

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 sub-population type (explored in Section 5, where we also show that benefits do not change among individuals with earnings). For completeness, we also estimate specifications using the natural logarithm of the outcome variables throughout this paper. However, the administrative data used in this setting contain a substantial mass of zero values (e.g., individuals with no earnings or employment in a given period), which is unsurprising given our setting. These specifications necessarily exclude a large share of observations and therefore materially change the composition of the estimation sample. The resulting estimates from these logarithmic transformations should therefore be interpreted as not directly comparable with our the baseline specifications.

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 substantial average increase in the probability of receiving pecuniary benefits (+8.7pp from a baseline of 40.4pp), but no increase in the probability of receiving non-cash non-insurance benefits. RRH recipients see a substantial increase in the probability of receiving insurance benefits (+23pp from a baseline of 56pp) 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 could be driven by two distinct effects: the newly attained access to a domicile and permanent address, and/or to 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 receipt. However, movement into housing may also be accompanied by a discontinuous change in connection to benefits, which we explore in Section 5.

## 4.2 The Effects of PSH

Panels (d)-(f) of Figure 3 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 receipt 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.[25] 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 implement several robustness checks and extensions to corroborate our main results. Figure 4 Panel (a) estimates employment effects following our main specification while mapping all observations between client interactions with case workers to missing. Replacing these observations with missing values may reduce precision of our estimates and represent a more conservative estimation approach if interaction-status is positively correlated with true outcomes. Panel (b) represents an alternate scenario where outcomes between interactions are mapped to zero in a "worst case scenario" (i.e. unemployment is assumed if there is no update to employment status in a given period). Panel (c) drops individuals' earliest observation in their homelessness spell. Values are frequently reported upon program entry and so, if we exclusively use (and project forward) these values then we don't necessarily know whether the program led to employment or vice-versa. To address this possibility, this panel excludes program entry responses to see if the main employment results still hold. Panel (d) uses an alternate event study procedure proposed by 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 spec-

[25] See Table B.1.

ification (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.[26]

All of these specifications yield nearly identical results on qualitative and quantitative bases. One notable difference is that in Panel (d) using the procedure from Borusyak, Jaravel, and Spiess (2024), 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 our main results.[27] We interpret these results to corroborate our main results, alleviating potential concerns regarding 1) biased censoring, 2) biased reporting upon entry into the HMIS, 3) mean reversion, and 4) bias arising from estimation of treatment effects with two-way fixed effect estimators under heterogeneous treatment effects and staggered treatment timing.

As an additional robustness check, we also estimate an alternate specification that more directly compares individuals who enter the homelessness services system at the same time but receive housing at different points due to variation in the underlying wait list. This approach allows us to more directly control for variation in wait times since HMIS entry as well as address concerns about mean reversion. To this end, Table 5 replicates our main estimates for RRH and PSH, however including calendar month fixed effects interacted with HMIS entry-year cohort indicators. Including this alternate fixed effect structure allows us to

[26]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 these individual treatment effects.

[27]See also Figure B.4 for additional results using the procedure from Borusyak, Jaravel, and Spiess (2024). The results here show that earnings impacts of RRH recipients are nearly identical to those 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.

make comparisons of outcomes between individuals, controlling for wait time and the possible mechanical effects associated with longer wait times. For example, this approach allows us to compare the evolution in outcomes for two individuals that both entered into the HMIS in 2015 and assign them the same calendar fixed effect for March 2018. The table yields similar, albeit mildly attenuated estimates relative to those in our main specification, suggesting that our main results are not driven by differential trends across entry cohorts or by mean reversion dynamics following entry into the homelessness services system. Specifically, this approach estimates an employment and earnings effect for RRH of 8.9pp and 115 USD per month, respectively.

One additional concern could pertain to our main sample restriction that requires sufficient observation of UH recipients before and after UH receipt. 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 receipt. 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.3 replicates our main results from Figure 3 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 10pp 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 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.

# 5 Mechanisms and heterogeneity

## 5.1 Mechanisms

We now turn to exploring the mechanisms that drive our results. Our aim is threefold. 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. Lastly, we want to explore treatment effect heterogeneity along measures of homelessness severity in order to further precise the role of heterogeneous potential outcomes.

**Differential responses by employment transition type.** We start by stratifying our event studies by ex-post employment transition type. We can further parse mechanisms by estimating disaggregated income and benefits responses conditional on certain employment transition types. For example, we can rule out the possible social benefits channel of housing receipt if social benefits do not respond to housing receipt conditional on employment. This approach is *not* intended to produce causal estimates. 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.[28] Since employment can fluctuate from month-to-month, we define "employed"

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

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 limits,[29] but indeed help us identify which income/benefits margins respond conditional on employment (or unemployment). Unemployment and employment are defined analogously in the post-period. By construction, there are some individuals that we cannot assign to one of these categories (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.[30]

Panel (a) of Figure 5 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 Figure 5. 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 receipt 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 receipt and not to connection to other services. How-

of transition into employment include homelessness severity, age, and controlled substance use.

[29] 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.

[30] We do not perform a version of this exercise for specifically ex-ante employment status, e.g. that compares stably employed versus unemployed individuals in the pre-event period, as such an approach would risk capturing pure mechanical mean reversion effects. Namely, individuals that are frequently unemployed in the pre-period are mechanically more likely to find employment post-housing; individuals that are stably employed in the pre-period are mechanically more likely to lose employment.

ever, 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 Figure 5. 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 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.

Figure 6 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.

**Job Search and connection to mental health and substance abuse services.** We can substantiate our results by estimating our designs for variables related to work status and ability to work. First, we study changes in "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 7 Panels (a) and (d) show estimates of Equation (1) using an indicator for "looking-for-work" status as the dependent variable.[31] Panel

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

(a) indeed shows a substantial pre-event increase in RRH recipients' looking-for-work status of between 5pp and 10pp from about nine-months pre-event to 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 indicative of a negative income effect of housing receipt: because 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 outlining the administration of these programs, placement into 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, but also as indicative of a real change in job search behavior.

Panels (b)-(c) and (e)-(f) show the results for RRH and PSH for 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[32], but we view these outcomes as suggestive in speaking to other mechanisms. Namely, for RRH recipients, we observe very minor changes around housing receipt in the probability they receive services for either mental health or substance abuse issues. The magnitudes of responses vary between +0.5 and -2 percentage points, indicating that our effect sizes are not driven by reported 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 endures for 3 quarters.

**Treatment effect heterogeneity by predicted baseline advantage.** We hypothesize that the difference in observed effects for RRH and PSH is largely attributable to heterogeneous potential outcomes: RRH recipients are less disadvantaged than PSH recipients. One way to explore this mechanism is to estimate heterogeneous treatment effects of UH along a measure of advantage, determined by predicted pre-housing earnings. We start by cross-sectionally regressing individuals' average pre-housing event earnings on observable covariates pertaining to race, gender, veteran status, HMIS risk score, and homelessness

[32]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.

severity (where we include no covariates that are mechanically related to earnings). From this regression we generate values of predicted pre-housing event earnings. We further assign housing recipients to terciles of predicted earnings and estimate variations of Equation (1) with event-time interacted with predicted pre-housing earnings tercile.[33] We perform this procedure separately for RRH and PSH recipients.

Table 6 displays the results of the estimation procedure. For RRH, our index of pre-housing event advantage demonstrates a strong positive correlation with the effect size associated with housing event.[34] Relative to individuals in the bottom-tercile of predicted pre-housing event earnings, top-tercile individuals see a 2.8 times larger impact on employment probability and a 4.2 times larger impact on monthly earnings. Middle-tercile individuals see a 2 times larger impact on earnings and a 1.5 times larger (albeit insignificant) impact on employment probability. However, note that we do not observe any compelling change in intensive margin earnings, as measured by log earnings, suggesting that this difference is largely driven by differential extensive-margin employment effects (although we cannot distinguish effects of working higher-paying jobs versus longer hours conditional on employment).[35] Importantly, we observe little meaningful heterogeneity in employment or earnings effects for PSH recipients along predicted pre-housing event earnings. This arises because mechanically there is little actual variation in earnings among PSH recipients. This observation leads to a lack of meaningful dispersion in predicted earnings among PSH recipients who overwhelmingly receive low values of predicted advantage. Figure B.6 shows the underlying distribution of these predicted values for PSH and RRH recipients. Compared to the distribution of predicted earnings among RRH recipients, predicted pre-event earnings for PSH recipients are both highly concentrated and very low. Using quantiles of predicted earnings among this group obscures a lack of true difference between the "most"

[33] We adopt this approach, as estimating these regressions along the distribution *observed* pre-housing event averages as opposed to *predicted* values risks introducing mechanical mean reversion. For instance, when we study heterogeneous treatment effects for individuals frequently employed in the pre-event period, through this conditioning process, individuals mechanically exhibit outsized probability of losing employment.

[34] For clarity, we refer to individuals with higher predicted pre-housing event earnings as "more advantaged".

[35] Additionally note that the composition of the sample changes dramatically when we measure outcomes transformed by the natural logarithm. As noted in Section 4, this sample difference is due to the widespread presence of zero earnings among homeless and at-risk individuals we observe.

and "least advantaged" groups.[36][37] From this exercise, we conclude that differences in underlying homelessness severity and advantage are important drivers of treatment effect heterogeneity, although this approach does not explicitly rule out the possibility that differential income effects associated with RRH and PSH are also driving our estimated treatment effect differences.

## 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 6) veteran status.[38] Prior work has highlighted the sociodemographic heterogeneity in the prevalence and severity of homelessness (e.g. 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 impacts.

We estimate the following equation:

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

where $Status_i$ is an indicator for whether individual $i$ is categorized as belonging to the sociodemographic group of interest. This equation is a simple variation of Equation (1)

[36] Indeed, using more granular quantiles, such as deciles of predicted earnings produces similarly null results of heterogeneous impacts for PSH recipients.

[37] Table B.2 and Table B.3 present results for analogous procedures that use, instead of predicted pre-housing event earnings, predicted HMIS risk score and pre-housing event employment propensity. The results are qualitatively identical to our results using predicted pre-housing event earnings.

[38] 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 (e.g. education status).

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:[39] 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 8 and Figure 9 displays the heterogeneous employment responses for RRH and PSH respectively, and Appendix 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 receipt 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.

For PSH, we observe a significantly larger employment response among guardians relative to non-guardians and, consistent with the results for RRH, a mild increase in the employment response for women relative to men. Unlike for RRH, we observe a mildly larger employment response to PSH receipt 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.

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

# 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" $\theta_i$ 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 $\tau_i$, 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 $\theta$.

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 $\mathbb{E}[\Delta e]$.[40]

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.[41]

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 7. We assume that individuals reporting employment earn income in the formal labor market in a manner 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

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

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

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.[42]

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 7 illustrate how the fiscal externalities through the labor impacts of unconditional housing weigh against the costs of RRH and PSH during program tenure.[43] The table reveals several novel facts. 1) For both programs, the average cost-offset through earnings effects is very small. During program receipt, 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 receipt via income effects (e.g. Jacob and Ludwig (2012)). 3) While the net labor market fiscal externalities are small, they are not negative, which may be surprising considering the hypothetical income effects of housing receipt. 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)).

[42]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.

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

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 receipt *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. 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 surprising due to the potential size of the income effects associated with receipt of truly permanent supportive housing. However, this lack of decrease may in part be mechanical due to the already very-low levels of employment among PSH recipients pre-event (see Table 1). Corroborating this possibility, we indeed observe a decrease in labor search on part of PSH recipients as they move into their PSH accommodations. We hypothesize that the differences in responses to PSH and RRH arise due to differences in selection between individuals placed into each program, rather than differences in the treatment effects of the programs themselves. Although we cannot rigorously test this possibility with a causal design, we observe negative selection of PSH recipients relative to RRH recipients on pre-event employment, wait times, health status, and risk scores, which ostensibly signifies worse potential outcomes. Moreover, we observe very little heterogeneity in (adverse) predicted outcomes for PSH recipients relative to RRH recipients. Although we

do observe some evidence of income effects among PSH recipients (in the form of a decrease in looking-for-work status around placement into housing), these findings reinforce the argument that there exist meaningful differences in underlying potential outcomes between the two groups that drive their divergent results.

Overall, we interpret our estimates as Average effects of Treatment on the Treated (ATT), and in this way they do not represent the impact of extending program receipt to the "marginally treated" 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

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 receipt 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 receipt; 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 in a setting where homelessness is a salient issue.

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 further explore the mechanisms underlying these responses. Because we observe that individuals finding employment in the post-period see *no* concurrent increase in benefits, we interpret our main results to suggest that these employment effects are likely attributable to having stable permanent shelter (although other confounding factors may persist, such as statutorily mandated interactions with ones' caseworker (Los Angeles Homeless Services Authority (2024))). We also document increases in labor search behavior among RRH recipients and decreases among PSH recipients, which we interpret as substantiating our observed results.

Based on our results, we estimate that the cost-offset of these programs *solely* through their effect on recipients' earnings are net positive, but very small (around than 1% cost-offset during program receipt 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 consistent employment post-event. However, our estimates only speak to outcomes within two years of housing, which may understate longer-run impacts on earnings and recidivism into homelessness.

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# 7 Main figures and tables

Table 1: Sample summary statistics

| | **RRH** | **PSH** | **Untreated** (excluded from main panel) |
|---|---|---|---|
| **Demographics** | | | |
| Non-white | 0.635 | 0.556 | 0.389 |
| Female | 0.405 | 0.394 | 0.364 |
| Veteran | 0.293 | 0.061 | 0.067 |
| Age at first interaction (years) | 41 | 47 | 42 |
| Risk score | 7.467 | 9.622 | 8.868 |
| **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 | - |

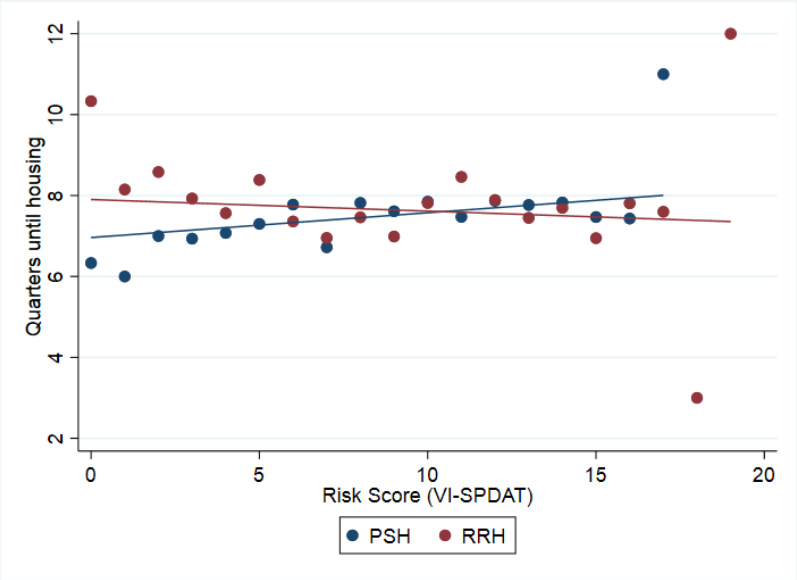
This table displays select demographic, housing, and employment tabulations stratified by final sample subgroups of treatment status. Dollar values are expressed in units USD January 2020. "PNMFH" refers to "Place not meant for habitation". Months Homeless Since First Spell is calculated as the difference 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

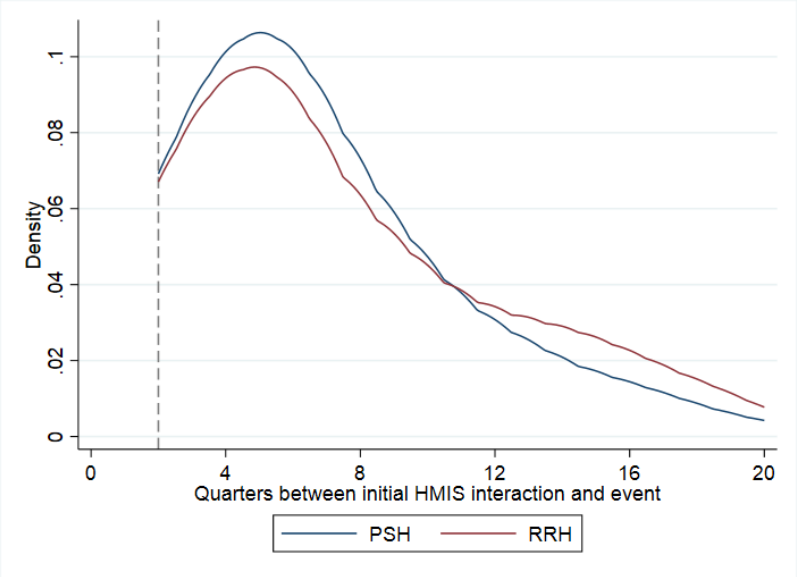
| | RRH | | | | PSH | | | |
|---|---|---|---|---|---|---|---|---|
| | Coefficient | SE | N | $R^2$ | Coefficient | SE | N | $R^2$ |
| Risk score | -.0639 | .121 | 633 | .000481 | .141 | .107 | 840 | .00194 |
| Female | -.146 | .624 | 1339 | .0000417 | -.319 | .466 | 2070 | .000223 |
| Guardian | -5.41 | .851 | 967 | .0361 | -2.08 | 1.75 | 1921 | .000763 |
| Non-white | .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 |
| F-statistic on multivariate regression | 3.87 | | | | 5.27 | | | |
| Adjusted $R^2$ on multivariate regression | .081 | | | | .061 | | | |

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 receipt. 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: Wait time until placement into unconditional housing




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

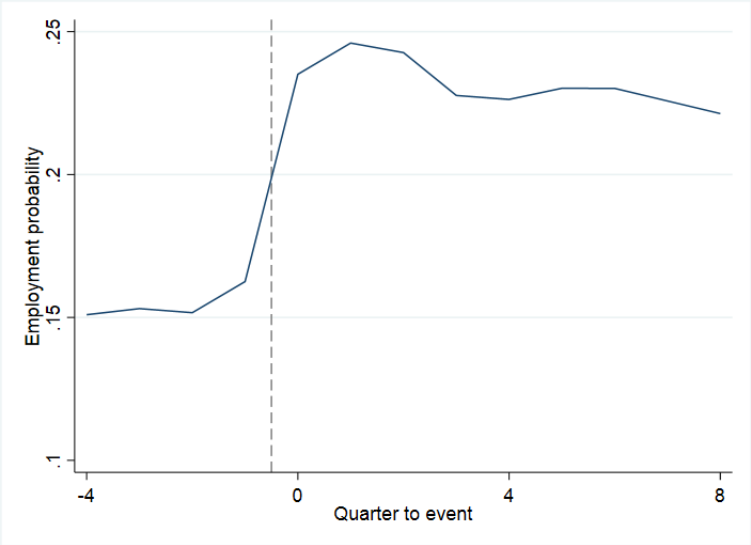



Panel (b): Distribution of time elapsed between HMIS entry and housing event

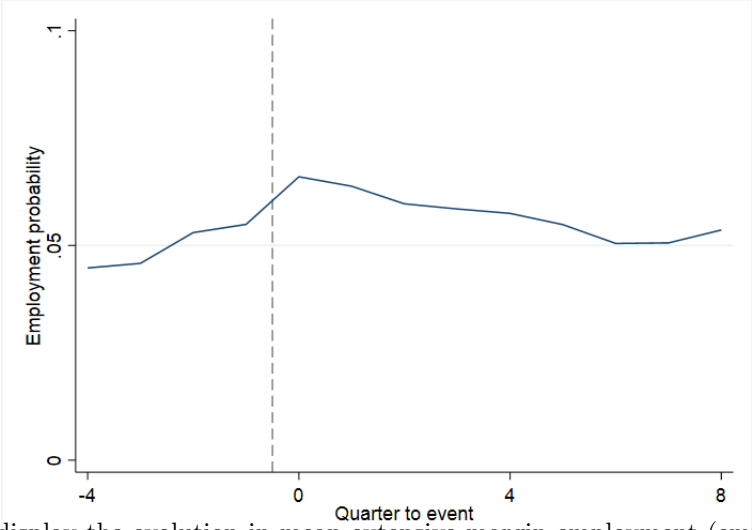
Note: These figures illustrate metadata around the timing of unconditional housing events. Panel (a) displays a binned scatterplot (stratified by housing type) of quarters elapsed between initial client intake and placement into unconditional housing on Risk Score (SPDAT). SPDAT takes the VI-SPDAT value for individuals and the VI-FSPDAT for families. 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



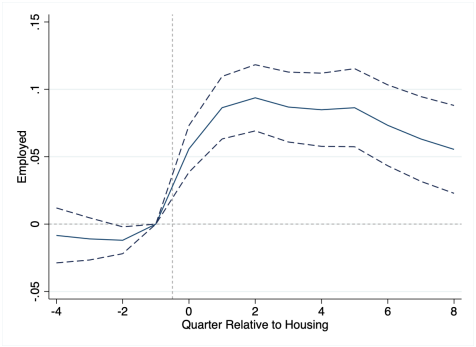

Panel (b): PSH



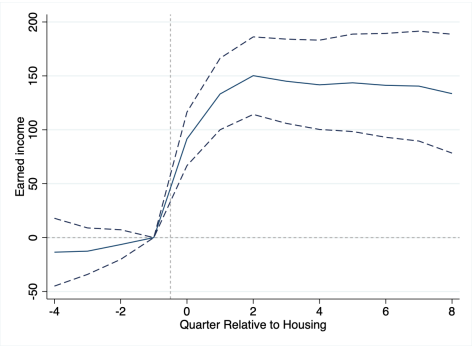

Note: These figures display the evolution in mean extensive margin employment (employment probability) around housing receipt events. Individuals are identified as employed if they either report employment or positive earnings or if they are observed in any of the administrative data systems with positive 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

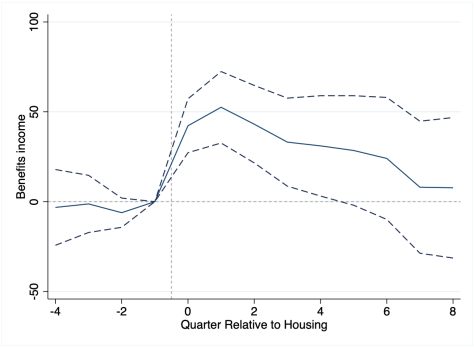
Rapid Re-Housing results




Panel (a): Employment

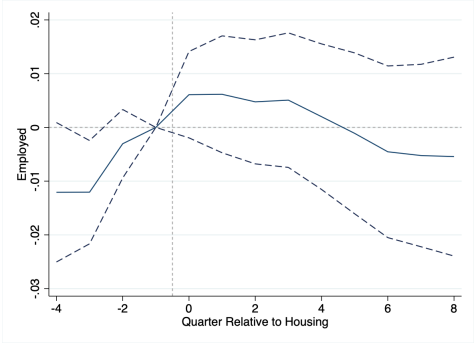



Panel (b): Earned income

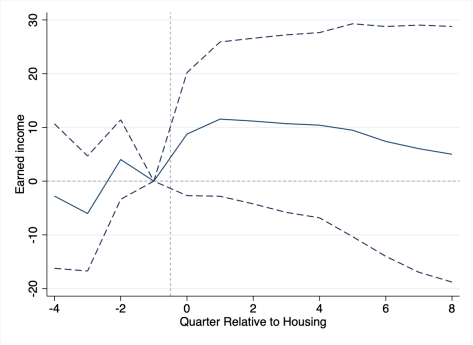



Panel (c): Benefits income

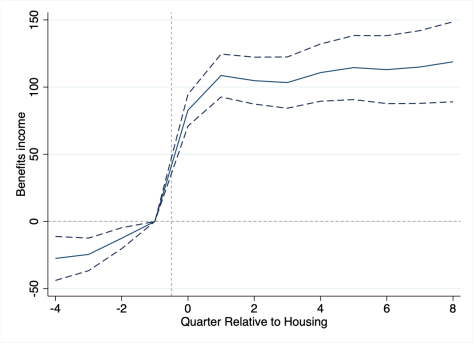
Permanent Supportive Housing results




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 $y_{it} = \alpha_i + \delta_t + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 1\} + \theta \cdot \mathbb{1}\{EventTime_{it} = 0\} + u_{it}$ 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 |

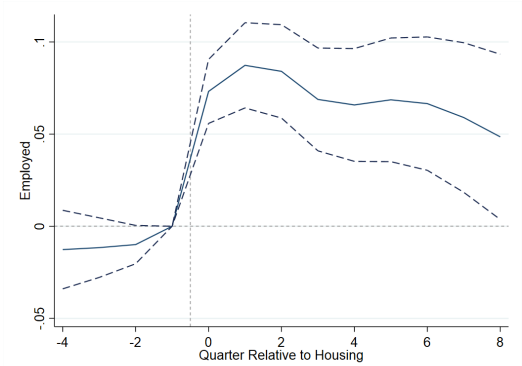
This table displays the coefficients from event study regressions with two-way fixed effects of the form $y_{it} = \alpha_i + \delta_t + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 1\} + \theta \cdot \mathbb{1}\{EventTime_{it} = 0\} + u_{it}$ on the 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.

Table 5: Robustness of main event study estimates
Including HMIS entry-year cohort × month fixed effects

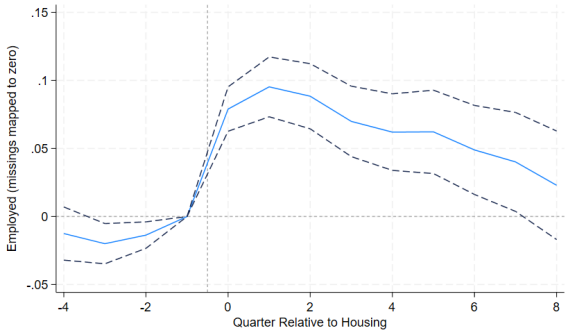
| | RRH | | | | PSH | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Employed | Earned inc. | Benefits inc. | Other inc. | Employed | Earned inc. | Benefits inc. | Other inc. |
| Post-event ($t \geq 0$) | 0.089 | 115.315 | 75.232 | 6.800 | 0.018 | 11.481 | 114.389 | 0.253 |
| | (0.014) | (20.087) | (10.653) | (5.154) | (0.006) | (7.380) | (8.529) | (2.029) |
| Constant | 0.182 | 190.360 | 238.253 | 13.813 | 0.060 | 50.987 | 396.533 | 10.386 |
| | (0.010) | (14.484) | (7.696) | (3.723) | (0.004) | (5.221) | (6.030) | (1.434) |
| **Pre-event average** | 0.200 | 186.875 | 254.361 | 14.346 | 0.071 | 54.633 | 377.950 | 9.181 |
| | [0.400] | [540.263] | [410.039] | [128.037] | [0.257] | [271.963] | [417.875] | [102.177] |
| HMIS-cohort × 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 | 0.59 | 0.64 | 0.72 | 0.56 | 0.65 | 0.65 | 0.75 | 0.60 |
| N | 59727 | 55545 | 58812 | 58812 | 80771 | 78253 | 79456 | 79456 |
| Number of clusters | 1707 | 1697 | 1700 | 1700 | 2264 | 2248 | 2248 | 2248 |

Note: This table displays the coefficients from event study regressions of the form $y_{it} = \alpha_i + \gamma_{t,k(i)} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + u_{it}$ on the sample of unconditional housing recipients entering between January 2014 and February 2018. The regression includes fixed effects $\alpha_i$ on the individual level and fixed effects $\gamma_{t,k(i)}$ on the HMIS entry-year cohort × month level (e.g. identical March 2018 effects for all UH recipients that entered the HMIS in 2015). Columns (1)-(4) and Columns (5)-(8) stratify between RRH and PSH recipients respectively. 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.

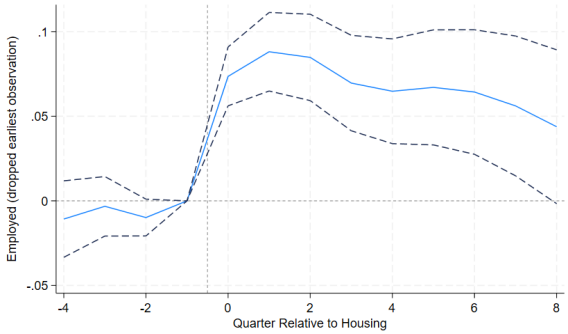
Figure 4: Robustness of employment response for Rapid Re-Housing



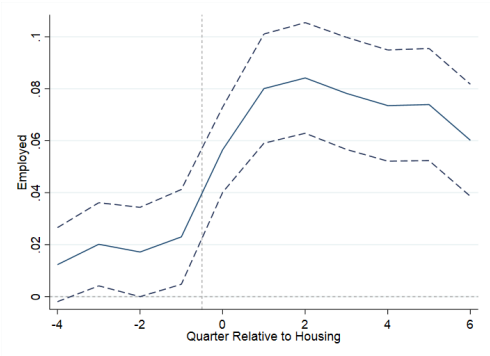

Panel (a): Dropping observations between interactions




Panel (b): All missings mapped to zero




Panel (c): Dropping earliest observation within spell




Panel (d): BJS Imputation

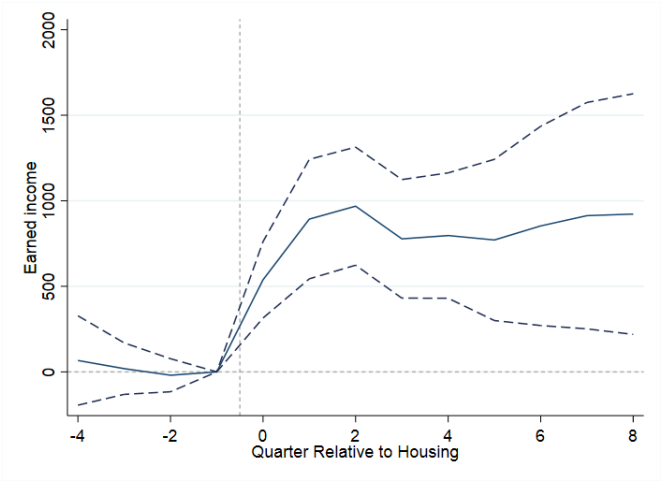
Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from different versions of the event study specification with two-way fixed effects:

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

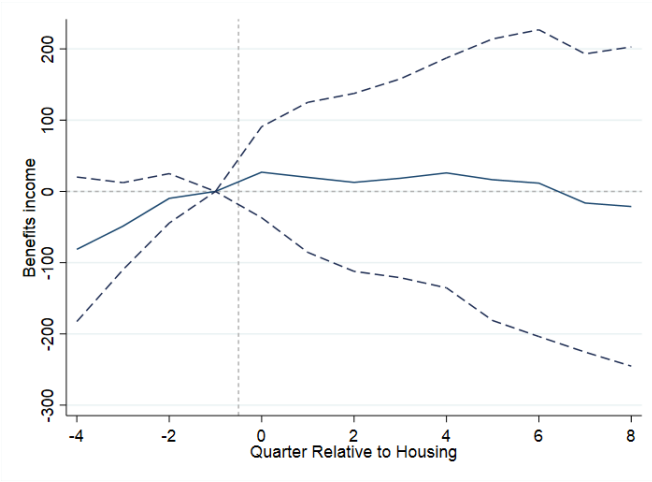
Panel (a) maps all observations between interactions to missing. Panel (c) maps all missing observations and observations between individual interactions with caseworkers to zero. Panel (c) drops individuals' earliest observation within their spell. Panel (d) estimates our event study using the procedure from Borusyak, Jaravel, and Spiess (2024). Timing is binned up to 5 quarters prior to and 9 quarters after 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: RRH results by employment transition type

Unemployment-to-employment ("U2E") transitioners

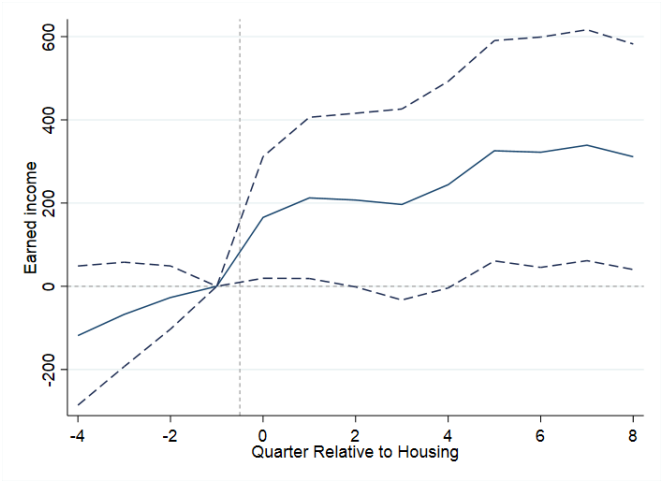



Panel (a): Earned income

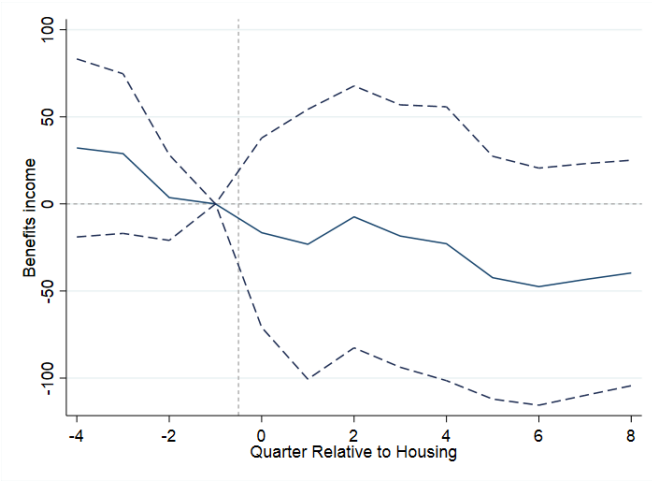



Panel (b): Benefits income

Employment-to-employment ("E2E") transitioners




Panel (c): Earned income




Panel (d): Benefits income

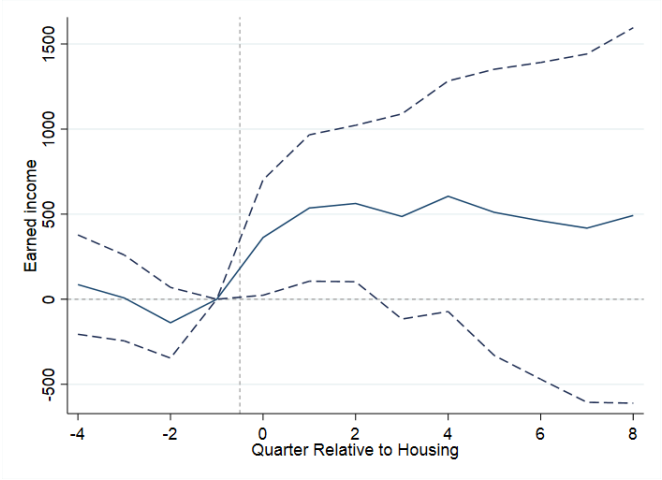
Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

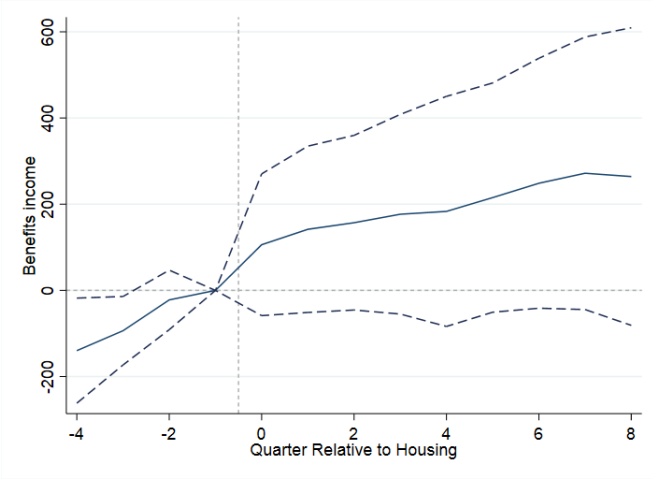
The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition between unemployment in the pre-period and employment in the post-period in panel (a) and retain 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 6: PSH results by employment transition type

Unemployment-to-employment ("U2E") transitioners

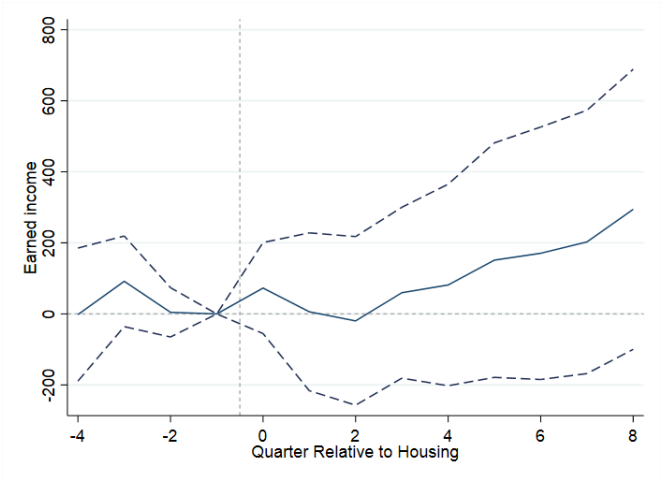



Panel (a): Earned income

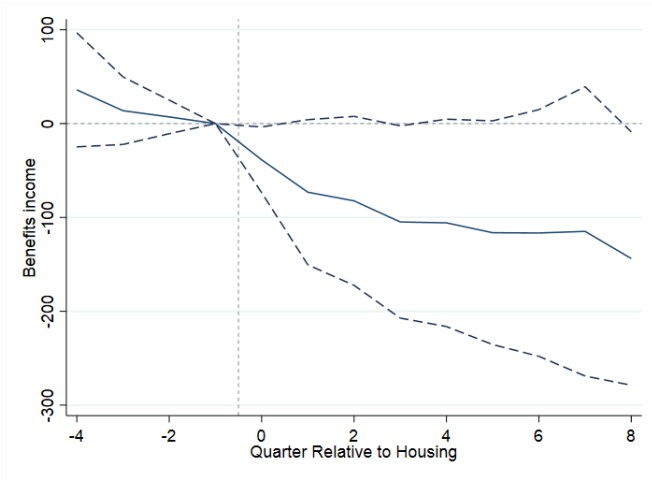



Panel (b): Benefits income

Employment-to-employment ("E2E") transitioners




Panel (c): Earned income




Panel (d): Benefits income

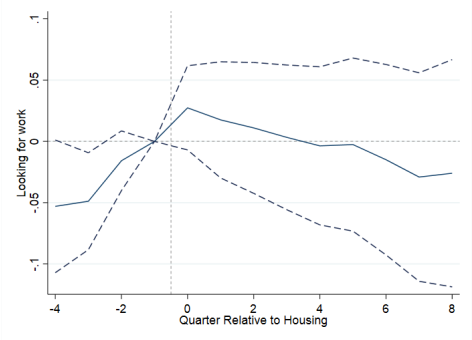
Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

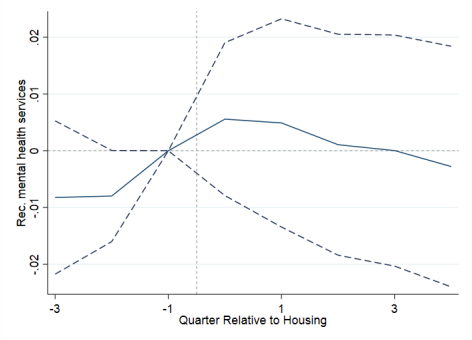
The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The sample is additionally restricted to those who transition between unemployment in the pre-period and employment in the post-period in panel (a) and retain 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 7: Additional mechanisms: job-search, mental health, and substance abuse

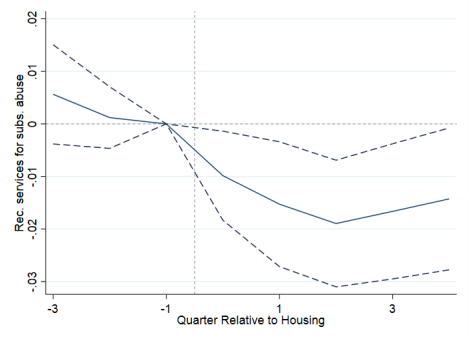
RRH results




Panel (a): "Looking for work"

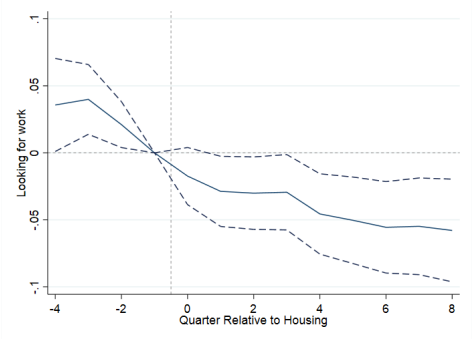



Panel (b): Receiving mental health services

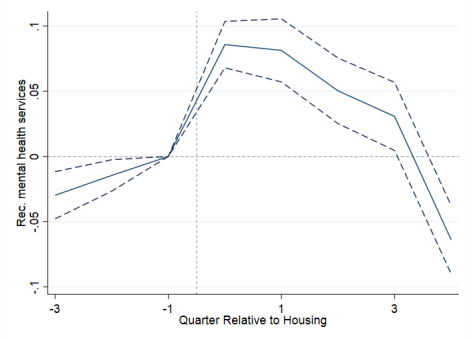



Panel (c): Receiving substance abuse services

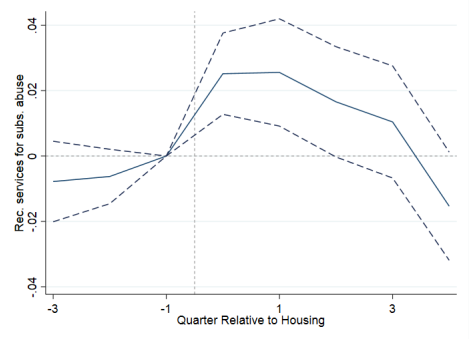
PSH results




Panel (d): "Looking for work"




Panel (e): Receiving mental health services




Panel (f): Receiving substance abuse services

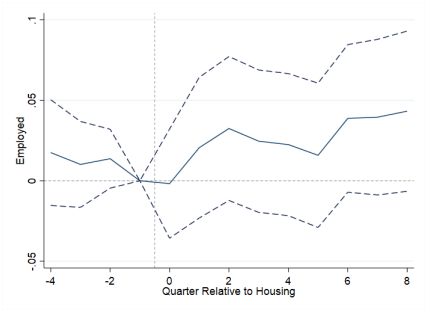
Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$ The estimation sample includes individuals receiving 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.

Table 6: Main event study estimates
By terciles of predicted pre-event earnings

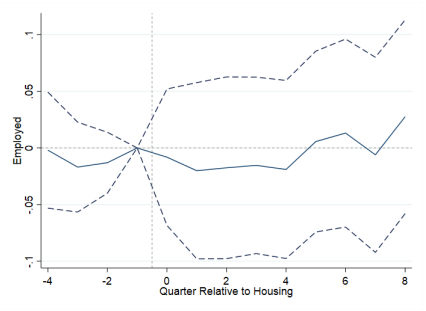
| | RRH | | | PSH | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Employed | Earned inc. | Log earned inc. | Employed | Earned inc. | Log earned inc. |
| Middle predicted earnings tercile × Post | 0.03 | 51.13 | -0.07 | -0.00 | -2.13 | -0.00 |
| | (0.02) | (23.14) | (0.10) | (0.01) | (9.86) | (0.30) |
| Top predicted earnings tercile × Post | 0.11 | 179.45 | -0.06 | 0.01 | 8.61 | 0.08 |
| | (0.02) | (38.90) | (0.10) | (0.01) | (18.05) | (0.25) |
| Post | 0.06 | 54.81 | 0.26 | 0.02 | 12.57 | -0.07 |
| | (0.02) | (17.61) | (0.09) | (0.01) | (6.60) | (0.24) |
| Month fixed effects | X | X | X | X | X | X |
| ID fixed effects | X | X | X | X | X | X |
| Adj. R-squared | 0.59 | 0.64 | 0.78 | 0.65 | 0.65 | 0.86 |
| N | 59728 | 55546 | 11174 | 80771 | 78253 | 4501 |
| Number of clusters | 1707 | 1697 | 601 | 2264 | 2248 | 244 |

Note: This table displays the coefficients from event study regressions of the form $y_{it} = \alpha_i + \delta_t + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + \sum_{l=1}^{3} \gamma_l \ Tercile_{l(i)} \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + u_{it}$ on the sample of unconditional housing recipients entering between January 2014 and February 2018, stratifying by UH type. $Tercile_{l(i)}$ corresponds with the individuals' cross-sectional tercile of predicted pre-event monthly earnings, where this value is predicted using another regression of average pre-event earnings score on pre-event characteristics, including age, HMIS risk score, veteran status, gender, race, health status, and indicators of homelessness severity (number of times and months homeless in the prior three years). Coefficients on Tercile × Post terms are estimated relative to the effect for the bottom tercile of predicted employment predicted earnings score. Standard errors are clustered on the individual-level and are reported in parentheses.

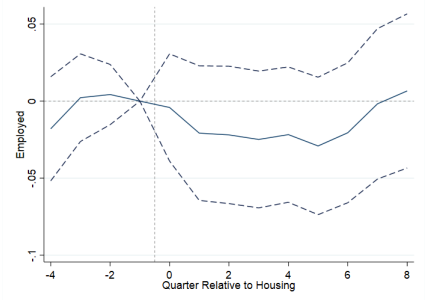
Figure 8: Extensive margin employment around unconditional RRH housing event: By select sociodemographic characteristics



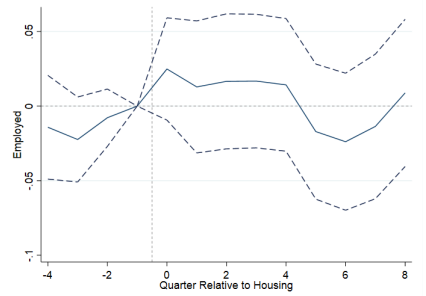

Panel (a):
Women v. men



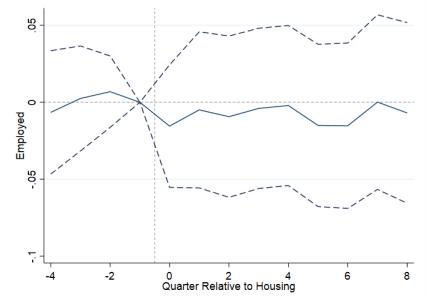

Panel (b):
Guardians v. non-guardians



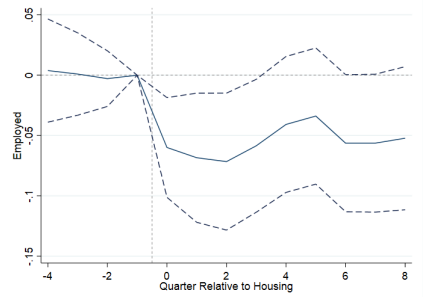

Panel (c):
Pre-event reported
mental health problems




Panel (d):
Non-white



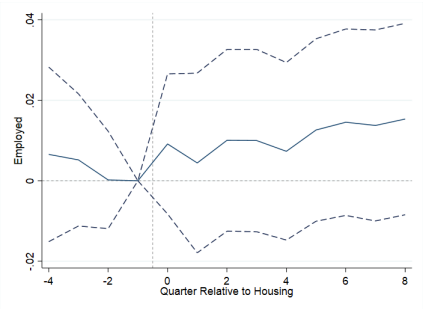

Panel (e):
Pre-event reported
substance abuse problems



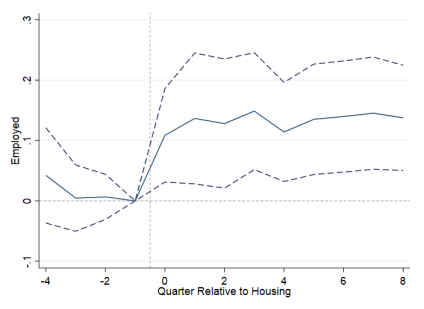

Panel (f):
Veteran status

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 tagged as belonging to 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.

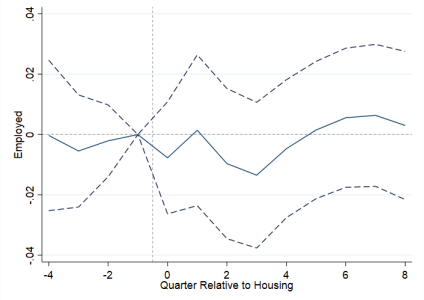
Figure 9: Extensive margin employment around unconditional PSH housing event: By select sociodemographic characteristics



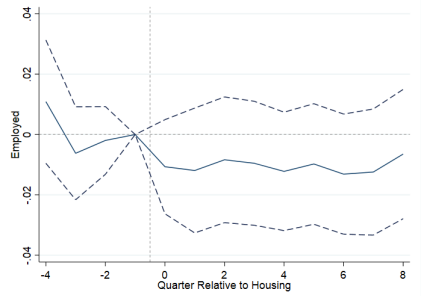

Panel (a):
Women v. men



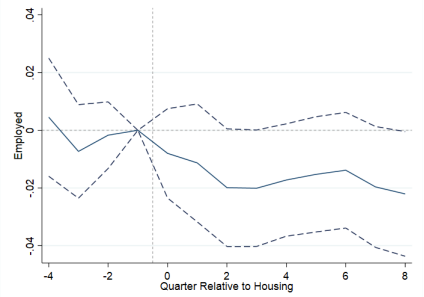

Panel (b):
Guardians v. non-guardians



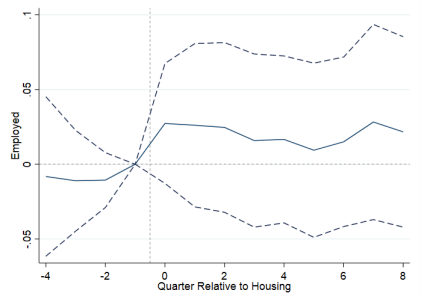

Panel (c):
Pre-event reported
mental health problems




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 tagged as belonging to 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 7: 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.

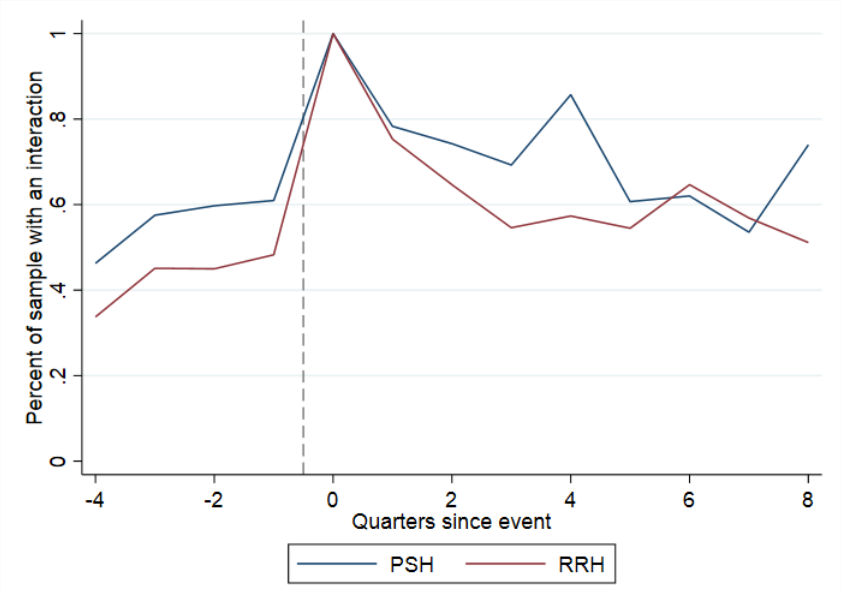
# Appendix A Additional metadata

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 |

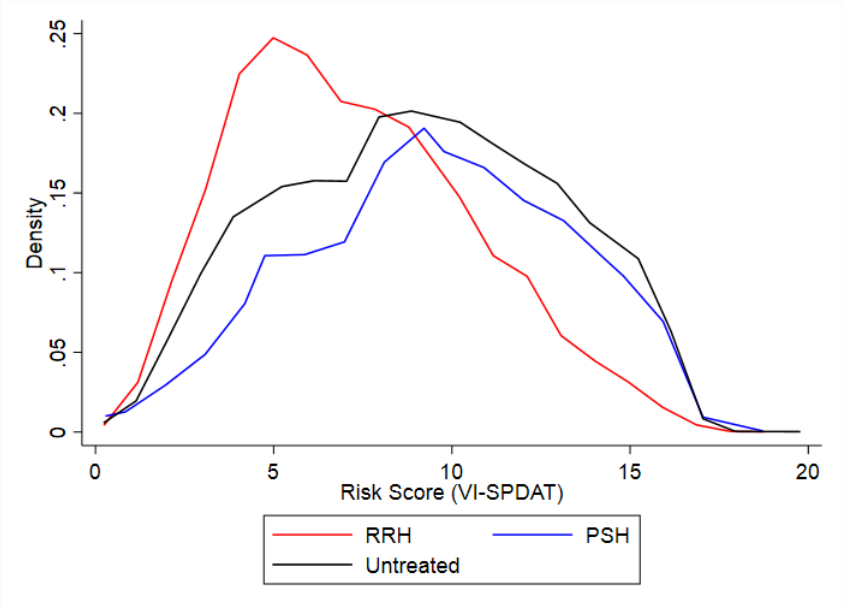
Note: This table displays coefficients from two cross-sectional regressions of risk score on an indicator for inclusion in our main sample. Individuals within each respective pool of housing recipients (during our considered timeframe) are included in our main sample if they are observed at least 7 months 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 main 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 SPDAT (risk) scores




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

Table A.2: Univariate regressions of share of non-missing pre-event observations on recipient observable characteristics

Panel (a): RRH

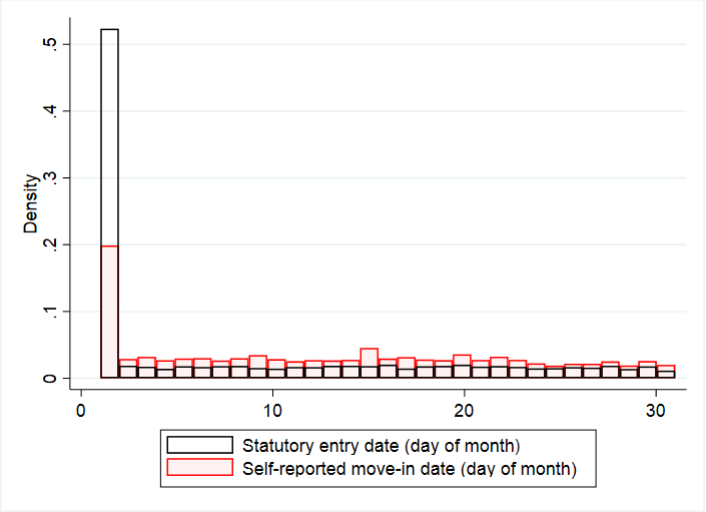
| | Coefficient | SE | N | $R^2$ |
|---|---|---|---|---|
| Non-white | .00512 | .0154 | 1,707 | .0000652 |
| Female | -.0288 | .0147 | 1,692 | .00227 |
| Veteran | .0383 | .0192 | 1,536 | .00271 |
| Parent/guardian | -.0265 | .0224 | 1,132 | .00123 |
| Age at first interaction (years) | .00166 | .000383 | 1,701 | .0108 |
| Substance abuse pre-event | .0818 | .0178 | 1,707 | .0118 |
| Mental health issues pre-event | .098 | .0156 | 1,621 | .0245 |
| Risk score | -.00265 | .00284 | 699 | .00123 |
| Wait time (quarters) | -.00535 | .000459 | 1,707 | .0587 |
| Log wait time | -.113 | .00991 | 1,707 | .0531 |
| Employed pre-event | -.0155 | .0317 | 1,707 | .000147 |
| Employed post-event | -.0208 | .0263 | 1,707 | .000405 |

Panel (b): PSH

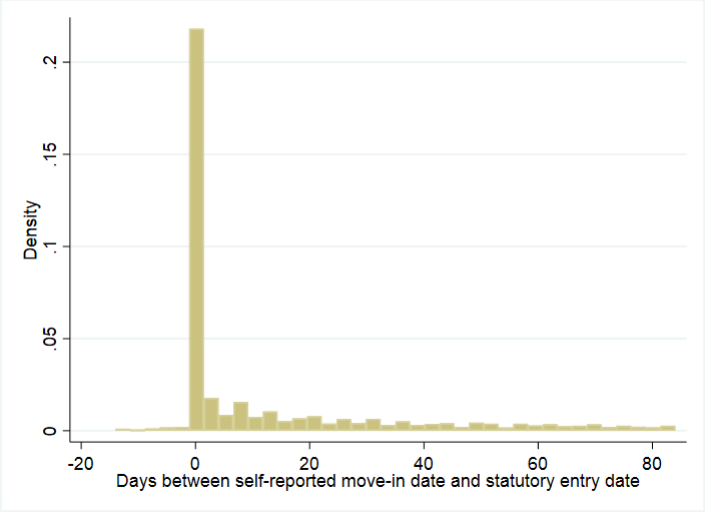
| | Coefficient | SE | N | $R^2$ |
|---|---|---|---|---|
| Non-white | -.0483 | .0144 | 2,265 | .00505 |
| Female | .00957 | .0146 | 2,234 | .000194 |
| Veteran | .0313 | .0353 | 2,237 | .000401 |
| Parent/guardian | -.0143 | .05 | 2,041 | .0000443 |
| Age at first interaction (years) | .00196 | .000501 | 2,265 | .0064 |
| Substance abuse pre-event | .0904 | .0139 | 2,265 | .0181 |
| Mental health issues pre-event | .0856 | .0164 | 2,202 | .013 |
| Risk score | -.00275 | .0029 | 873 | .00105 |
| Wait time (quarters) | -.00364 | .000538 | 2,265 | .0164 |
| Log wait time | -.061 | .0112 | 2,265 | .0102 |
| Employed pre-event | -.0954 | .0544 | 2,265 | .00146 |
| Employed post-event | -.0504 | .0558 | 2,265 | .000465 |

This table displays a series of univariate cross-sectional regressions of share of missing pre-event observations on observable characteristics, where each row under each respective RRH or PSH column-set corresponds with a separate regression. The dependent variable corresponds with the share individuals' quarterly observations between their entry into the HMIS and their housing event that are missing. The regressions are stratified by housing program. Columns "SE" give heteroskedasticity-robust standard errors.

Figure A.3: Housing receipt discrepancy in entry date




Panel (a) Reported day of month

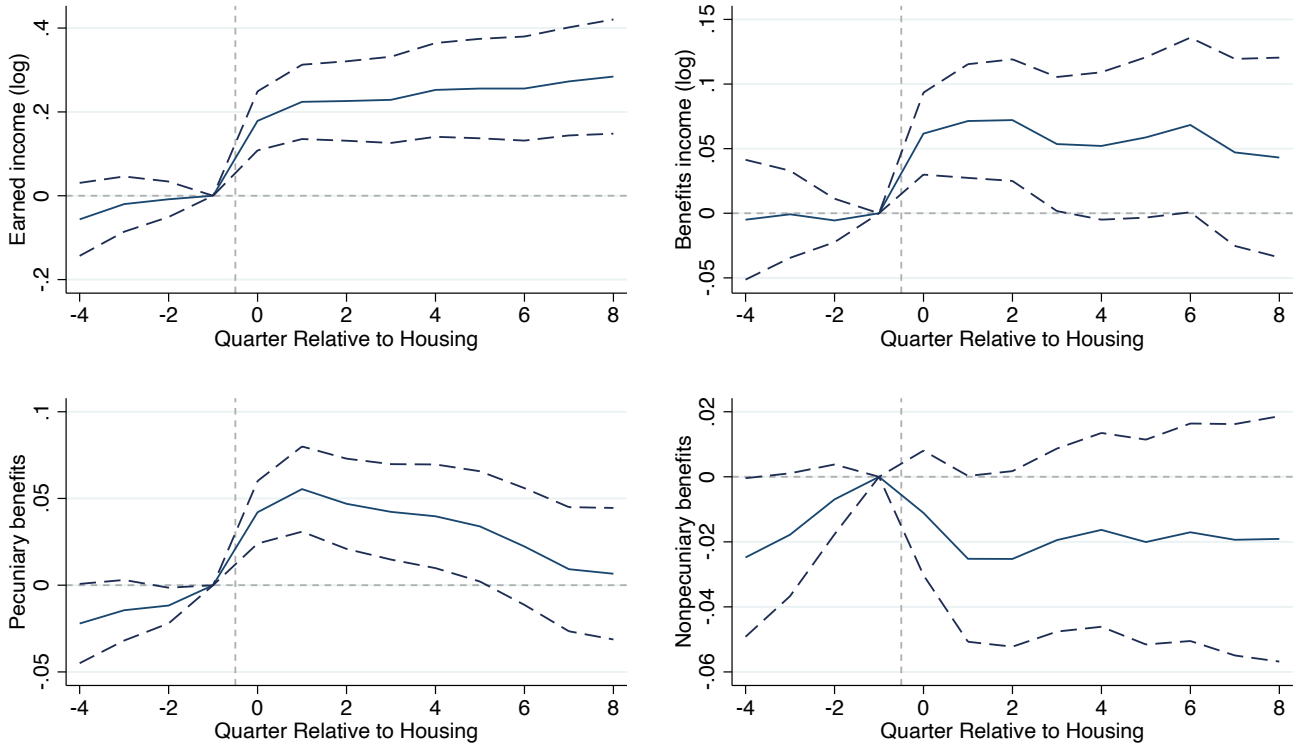



Panel (b) Observed difference between reported entry date and move-in date

These figures demonstrate the discrepancy between the reported date of entry into unconditional housing accommodations and the potentially true date of entry for housing receipt 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

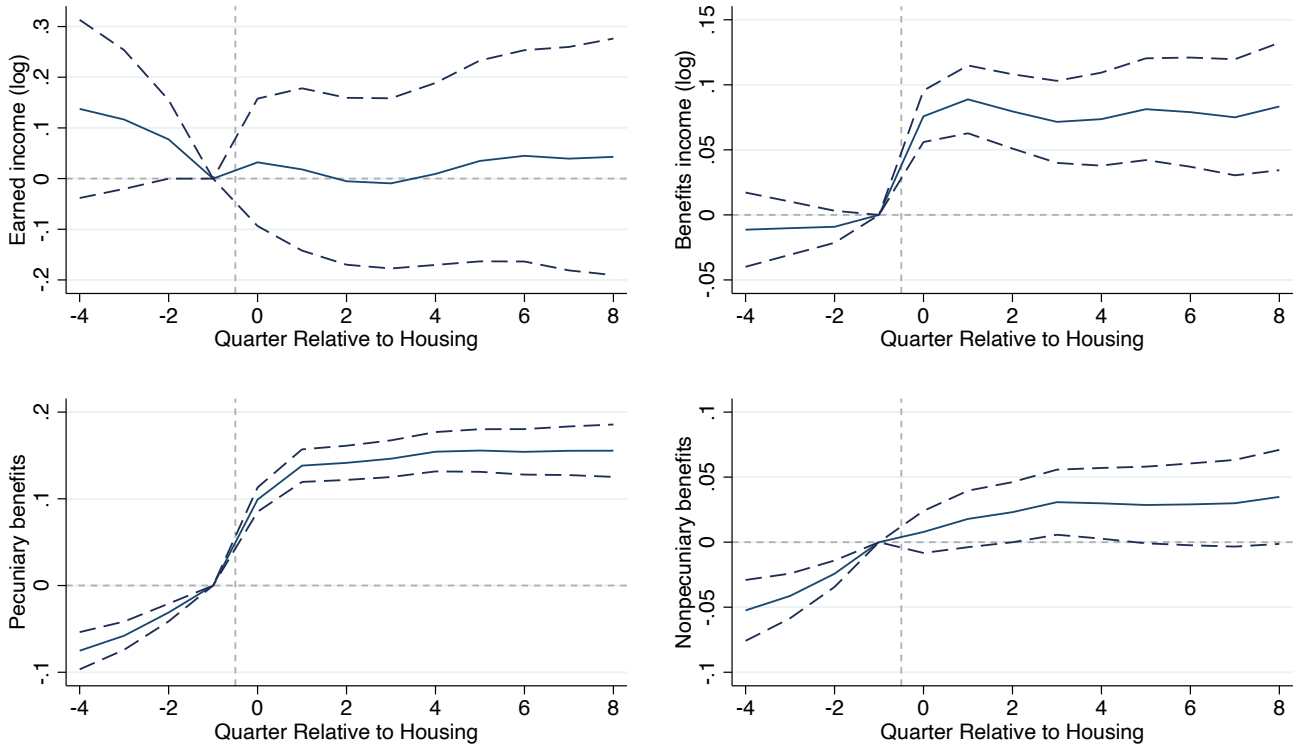



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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

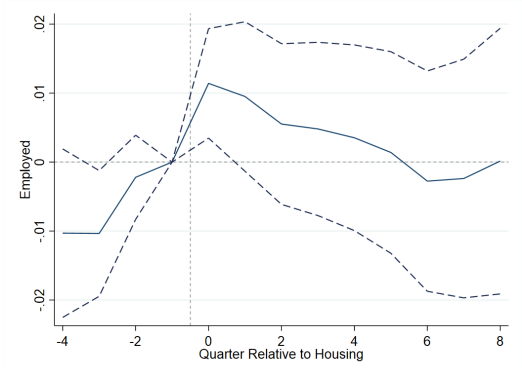
Panel (b): PSH

| | (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 |

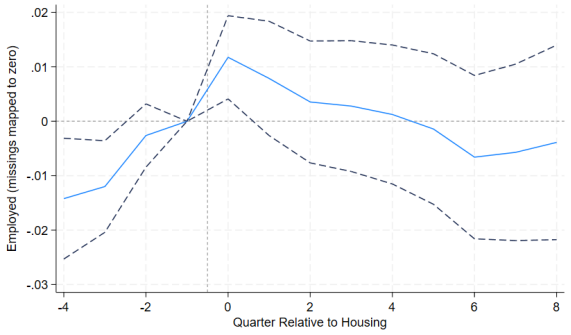
ID-clustered standard errors in parentheses

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.

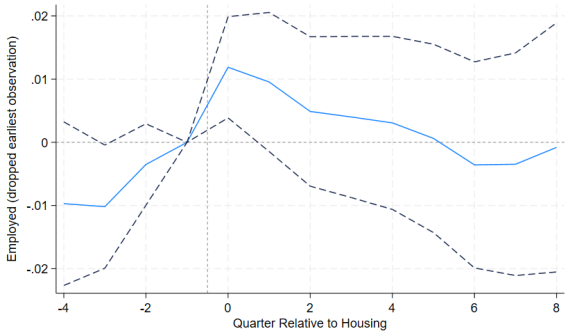
Figure B.2: Robustness of (lack of) employment response for Permanent Supportive Housing



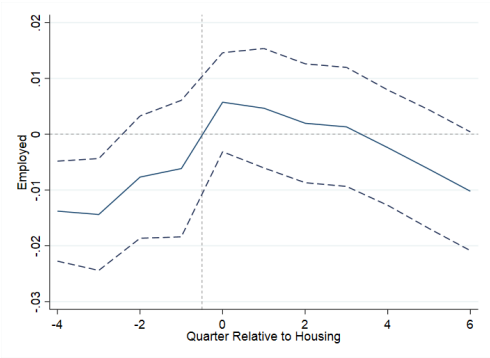

Panel (a): Dropping observations between interactions




Panel (b): All missings mapped to zero




Panel (c): Dropping earliest observation within spell




Panel (d): BJS Imputation

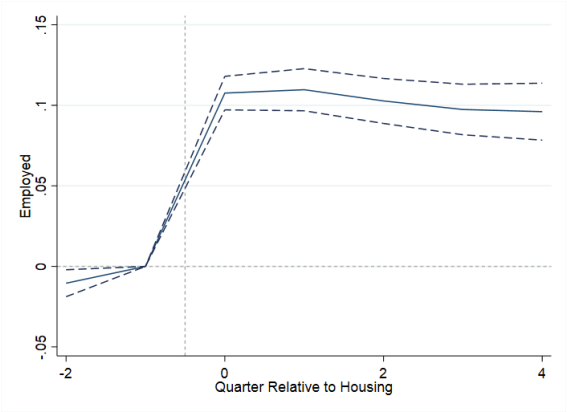
Note: This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from different versions of the event study specification with two-way fixed effects:

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

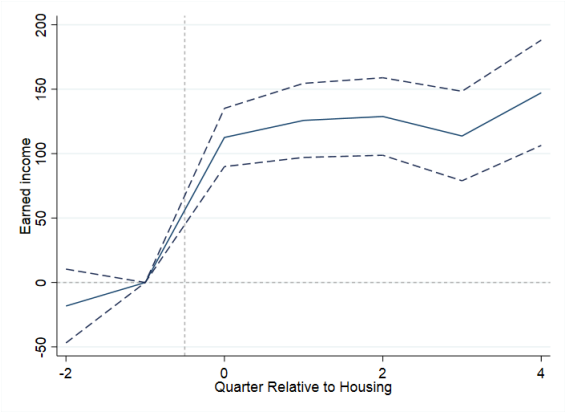
Panel (a) maps all observations between interactions to missing. Panel (b) drops individuals' earliest observation within their spell. Panel (c) maps all missing observations and observations between individual interactions with caseworkers to zero. Panel (d) estimates our event study using the procedure from Borusyak, Jaravel, and Spiess (2024). Timing is binned up to 5 quarters prior to and 9 quarters after 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 B.3: Robustness: Main event study results on alternative sample

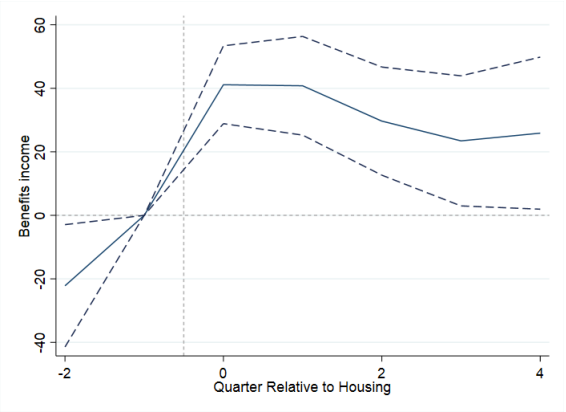
RRH results




Panel (a): Employment

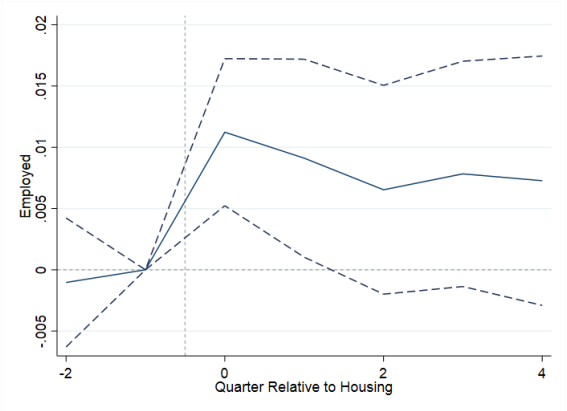



Panel (b): Earned income

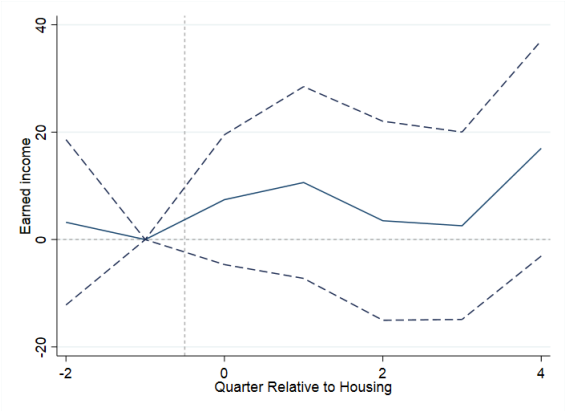



Panel (c): Benefits income

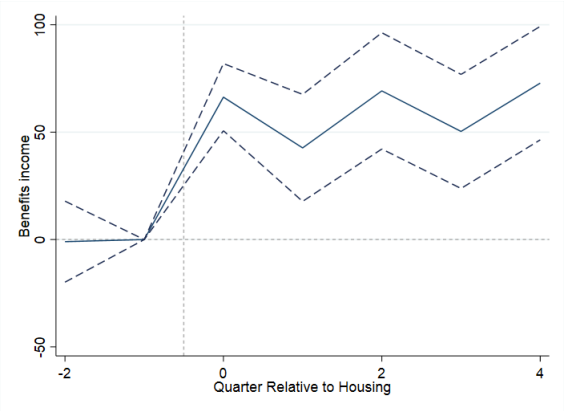
PSH results




Panel (d): Employment




Panel (e): Earned income

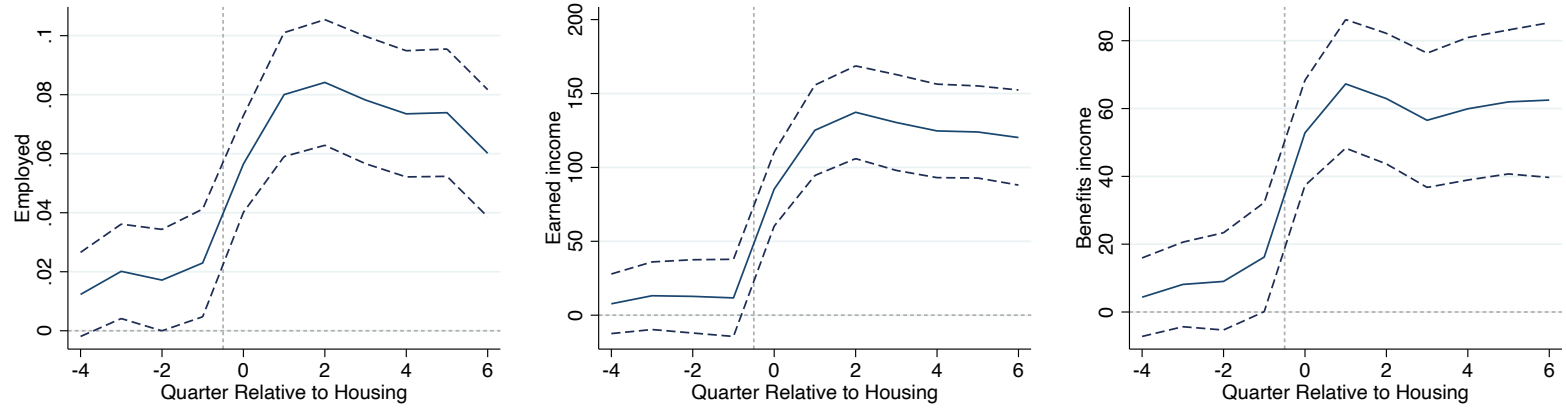



Panel (f): Benefits income

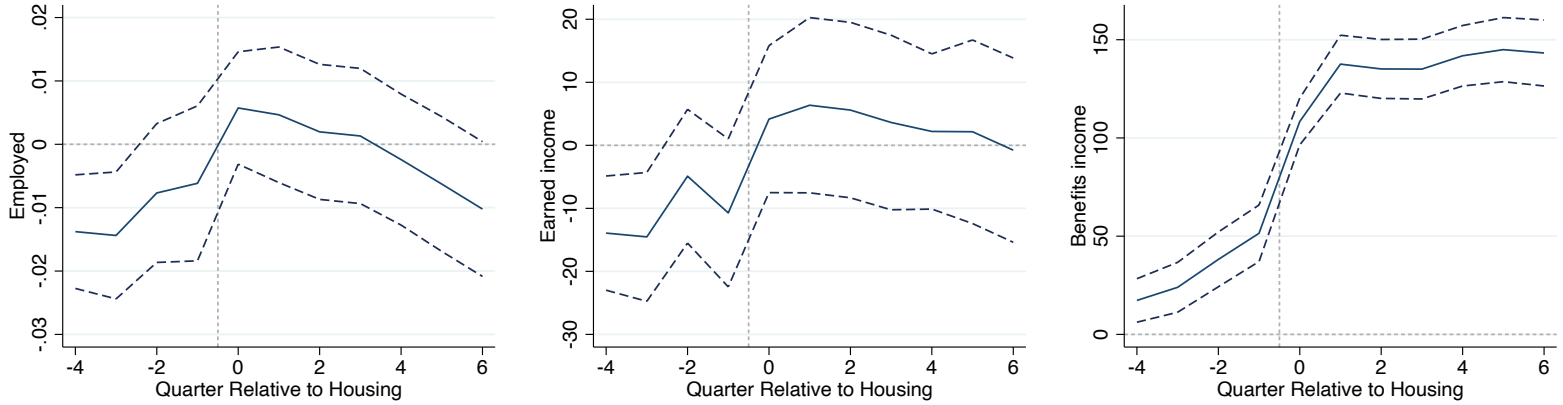
Note: These figures display the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects: $y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}$. The estimation sample includes individuals 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.4: Event studies following Borusyak, Jaravel, and Spiess (2024)

Panel (a): RRH




Panel (b): PSH




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.

## Table B.2: Main event study estimates
## By terciles of predicted risk score

| | RRH | | | PSH | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Employed | Earned inc. | Log earned inc. | Employed | Earned inc. | Log earned inc. |
| Middle predicted risk score tercile × Post | 0.01 | -52.99 | -0.12 | 0.00 | 2.29 | 0.42 |
| | (0.03) | (39.42) | (0.10) | (0.01) | (17.47) | (0.17) |
| Top predicted risk score tercile × Post | -0.07 | -139.69 | -0.03 | 0.01 | -1.33 | -0.11 |
| | (0.02) | (34.02) | (0.09) | (0.01) | (16.19) | (0.20) |
| Post | 0.12 | 198.30 | 0.25 | 0.01 | 14.15 | -0.13 |
| | (0.02) | (29.71) | (0.08) | (0.01) | (15.16) | (0.12) |
| Month fixed effects | X | X | X | X | X | X |
| ID fixed effects | X | X | X | X | X | X |
| Adj. R-squared | 0.59 | 0.64 | 0.78 | 0.65 | 0.65 | 0.86 |
| N | 59728 | 55546 | 11174 | 80771 | 78253 | 4501 |
| Number of clusters | 1707 | 1697 | 601 | 2264 | 2248 | 244 |

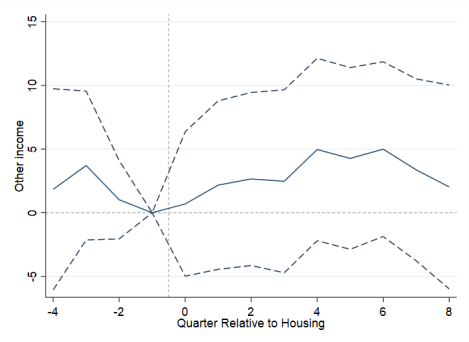
Note: This table displays the coefficients from event study regressions of the form $y_{it} = \alpha_i + \delta_t + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + \sum_{l=1}^{3} \gamma_l \ Tercile_{l(i)} \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + u_{it}$ on the sample of unconditional housing recipients entering between January 2014 and February 2018, stratifying by UH type. $Tercile_{l(i)}$ corresponds with the individuals' cross-sectional tercile of predicted risk score, where HMIS risk score is predicted using another regression of risk score on pre-event characteristics, including age, veteran status, gender, race, health status, and indicators of homelessness severity (number of times and months homeless in the prior three years). Coefficients on Tercile × Post terms are estimated relative to the effect for the bottom tercile of predicted risk score. Standard errors are clustered on the individual-level and are reported in parentheses.

## Table B.3: Main event study estimates
## By terciles of predicted employment propensity

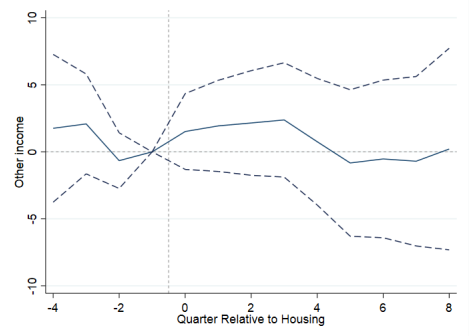
| | RRH | | | PSH | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Employed | Earned inc. | Log earned inc. | Employed | Earned inc. | Log earned inc. |
| Middle predicted employment tercile × Post | 0.03 | 48.39 | -0.00 | 0.00 | 2.86 | -0.10 |
| | (0.02) | (27.29) | (0.09) | (0.01) | (10.52) | (0.29) |
| Top predicted employment tercile × Post | 0.04 | 115.91 | 0.04 | 0.00 | 13.75 | -0.01 |
| | (0.02) | (38.29) | (0.10) | (0.01) | (17.81) | (0.21) |
| Post | 0.08 | 78.12 | 0.18 | 0.02 | 9.23 | 0.01 |
| | (0.02) | (19.87) | (0.08) | (0.01) | (7.01) | (0.20) |
| Month fixed effects | X | X | X | X | X | X |
| ID fixed effects | X | X | X | X | X | X |
| Adj. R-squared | 0.59 | 0.64 | 0.78 | 0.65 | 0.65 | 0.86 |
| N | 59728 | 55546 | 11174 | 80771 | 78253 | 4501 |
| Number of clusters | 1707 | 1697 | 601 | 2264 | 2248 | 244 |

Note: This table displays the coefficients from event study regressions of the form $y_{it} = \alpha_i + \delta_t + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + \sum_{l=1}^{3} \gamma_l \ Tercile_{l(i)} \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + u_{it}$ on the sample of unconditional housing recipients entering between January 2014 and February 2018, stratifying by UH type. $Tercile_{l(i)}$ corresponds with the individuals' cross-sectional tercile of predicted pre-event employment propensity, where this propensity is predicted using another regression of risk score on pre-event characteristics, including age, HMIS risk score, veteran status, gender, race, health status, and indicators of homelessness severity (number of times and months homeless in the prior three years). Coefficients on Tercile × Post terms are estimated relative to the effect for the bottom tercile of predicted employment propensity score. Standard errors are clustered on the individual-level and are reported in parentheses.

Figure B.5: Event study results: other income




Panel (a) Rapid Re-Housing



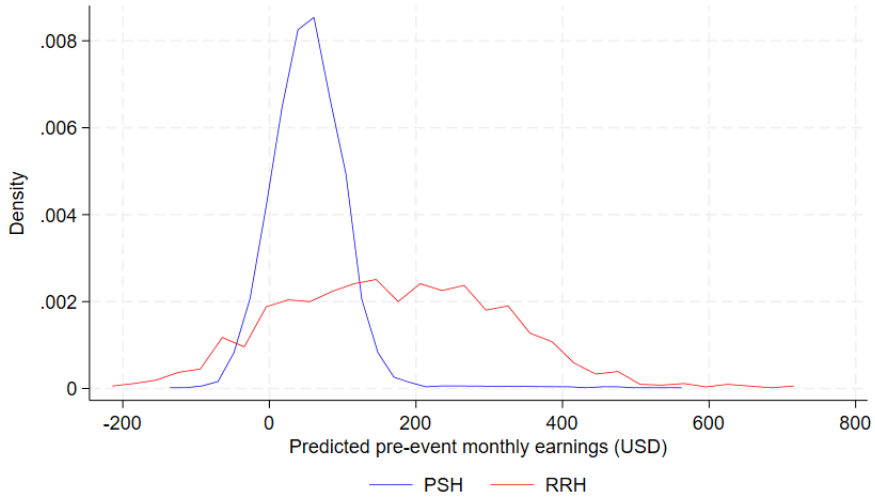

Panel (b) Permanent Supportive Housing

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.

Figure B.6: Distribution of predicted pre-event earnings




Note: This figure plots the distribution of predicted pre-event monthly earnings for the population of RRH and PSH recipients comprising our sample. This value is predicted using a regression of average pre-event earnings on pre-event characteristics, including age, HMIS risk score, veteran status, gender, race, health status, and indicators of homelessness severity (number of times and months homeless in the prior three years). Predicted values are generated via this procedure separately for RRH and PSH.

# 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

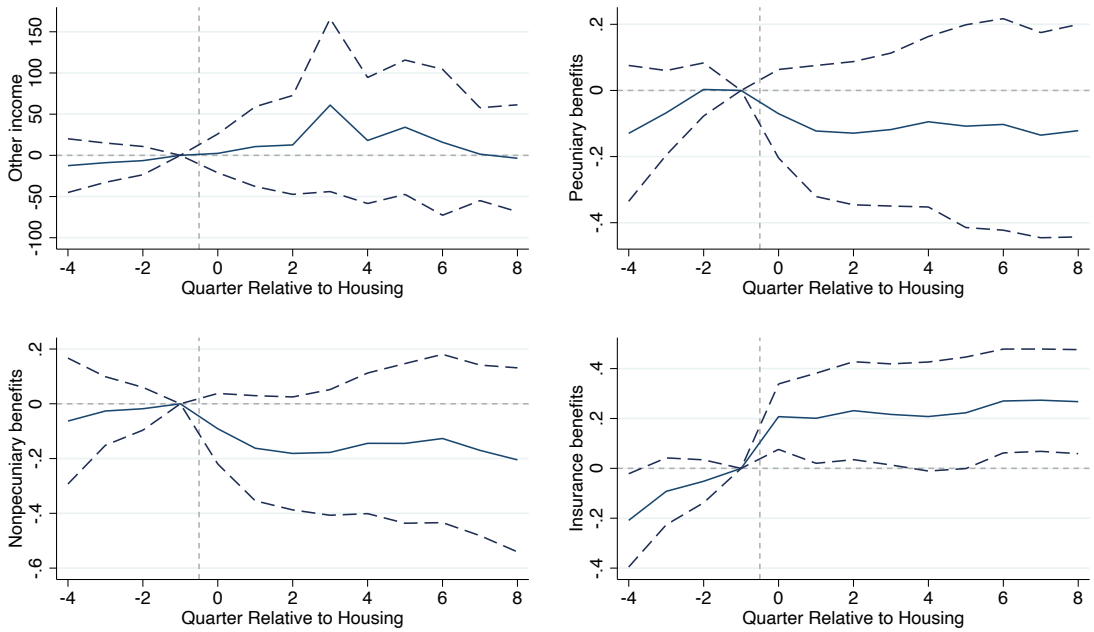
Panel RRH recipients

| | 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)

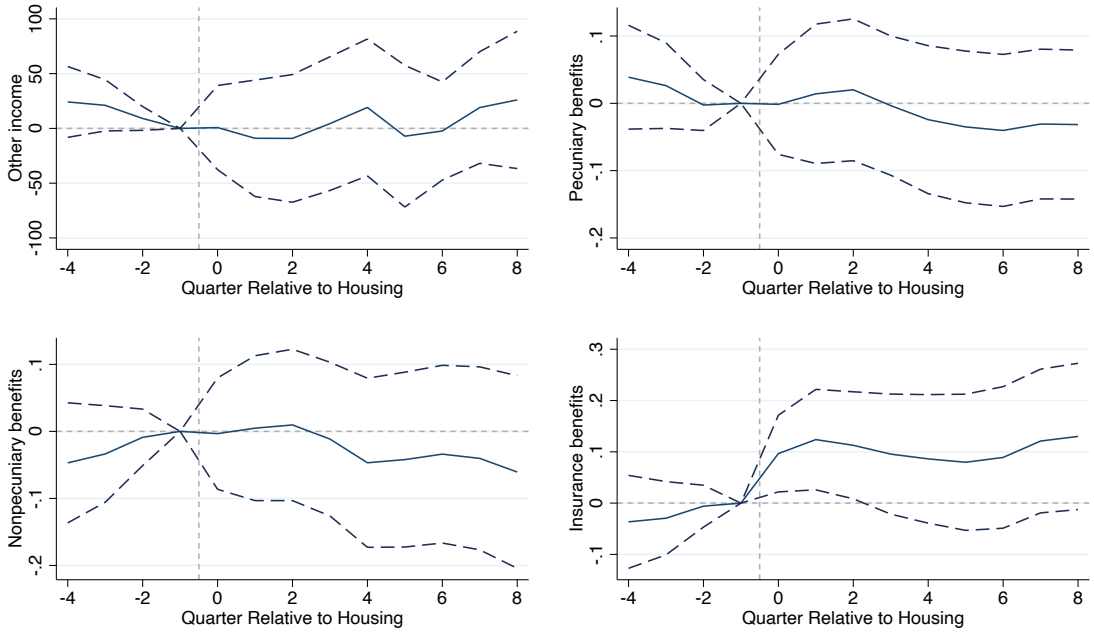



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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)

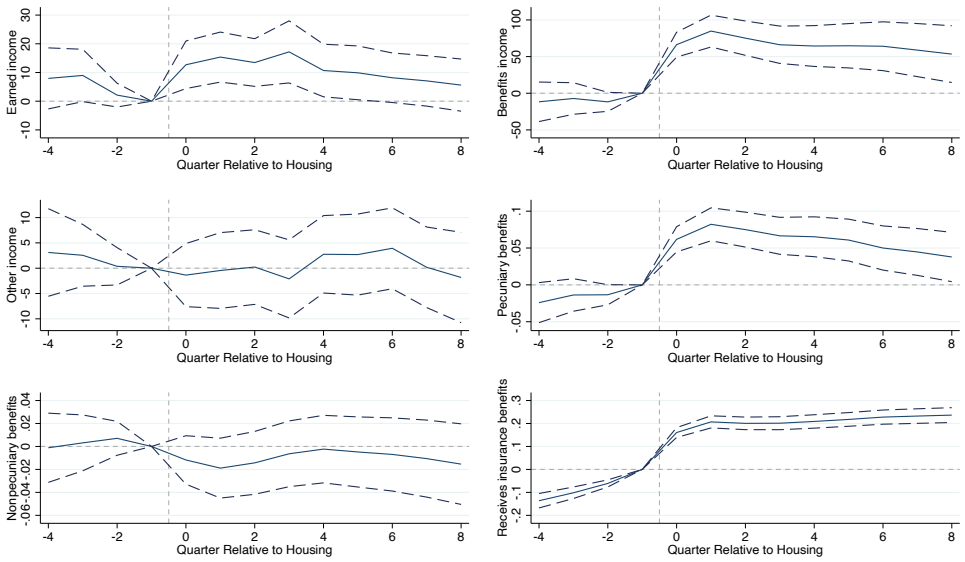



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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

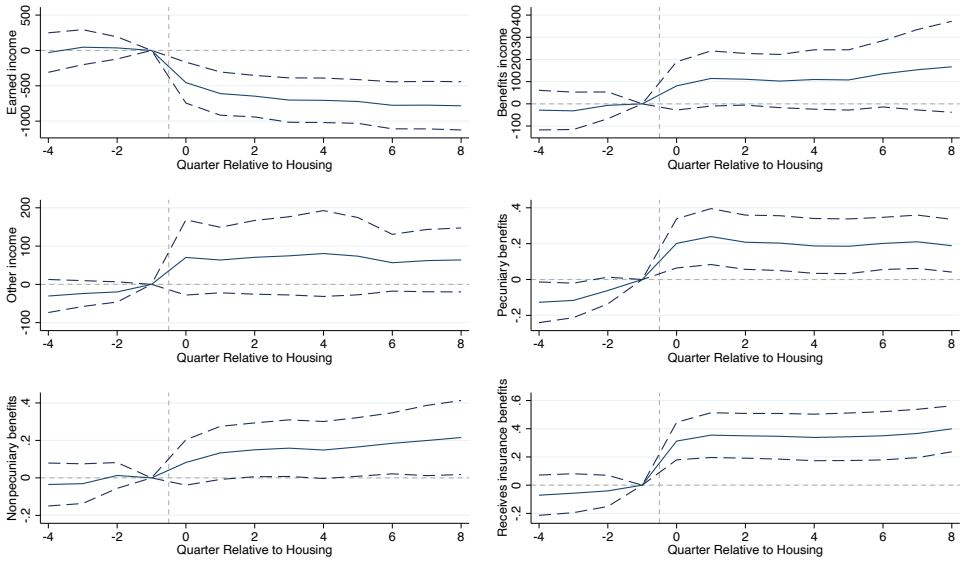



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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

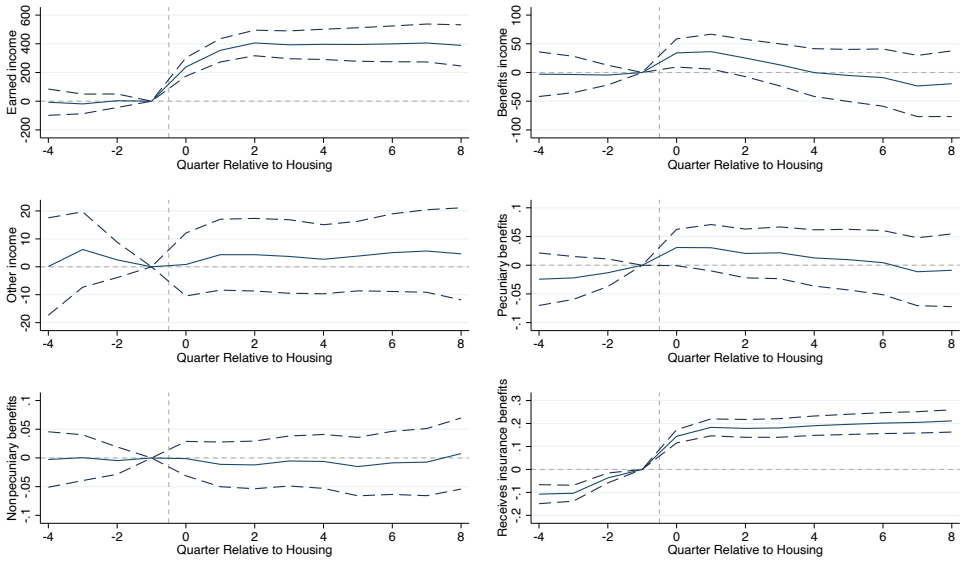



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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

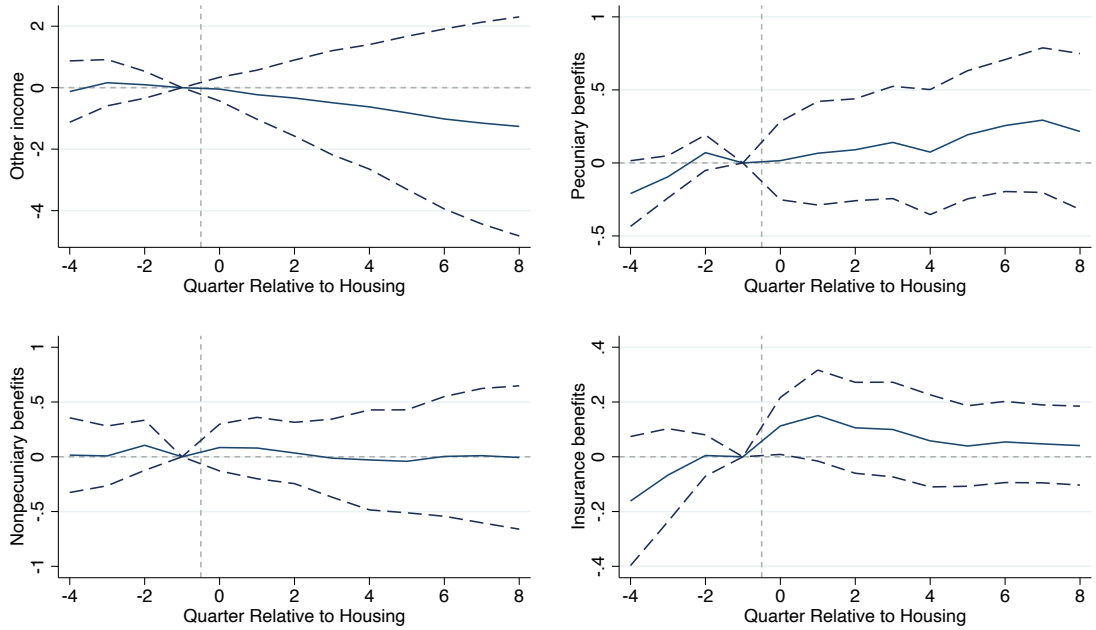



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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)

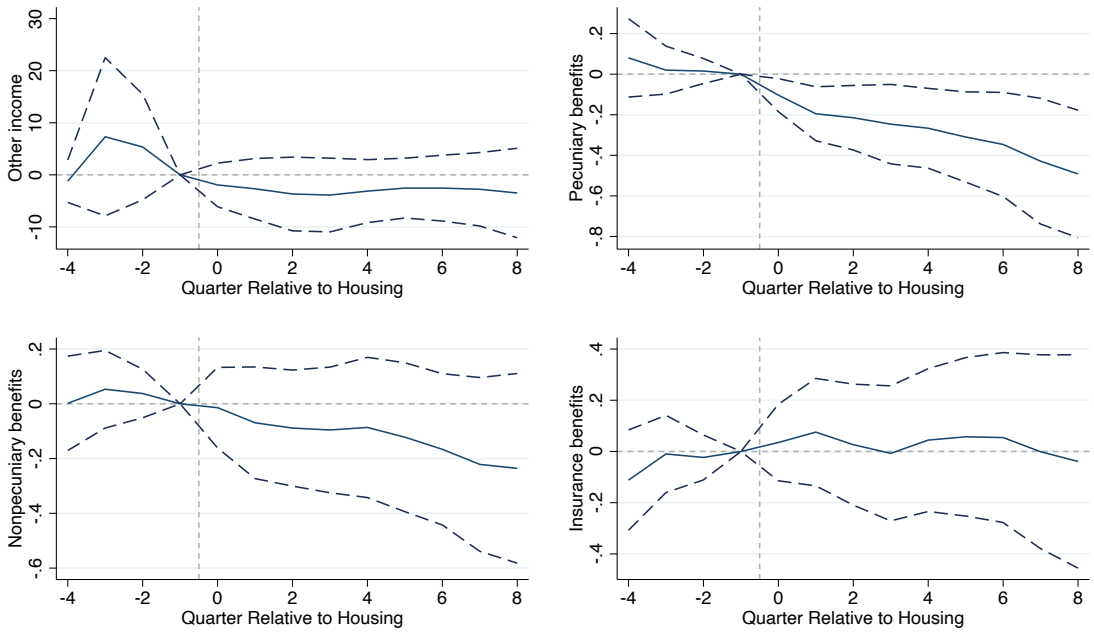



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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)

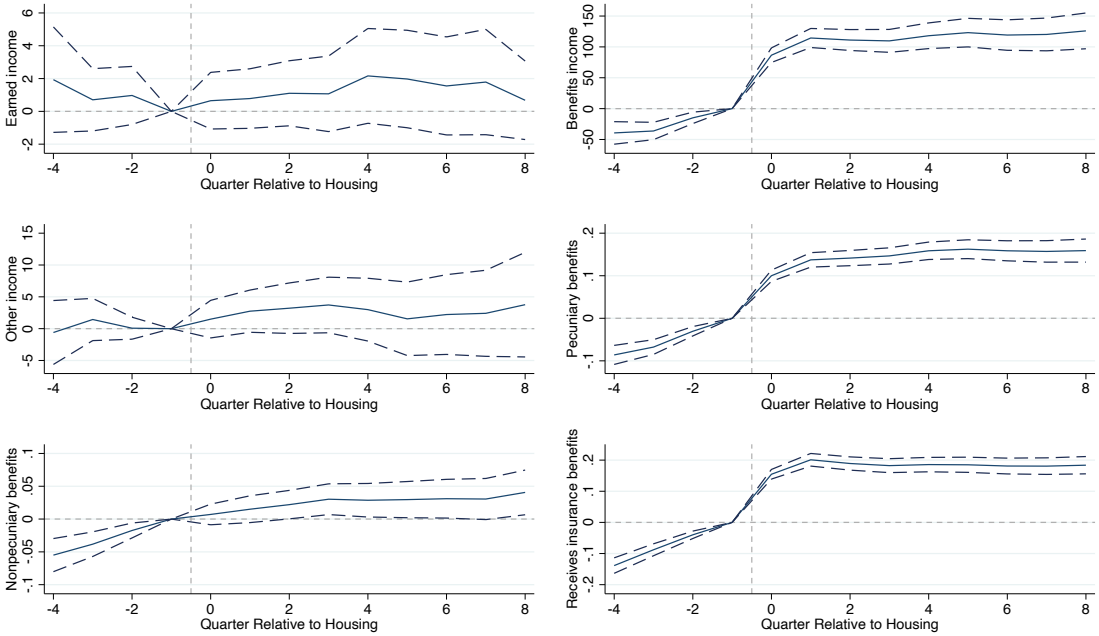



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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

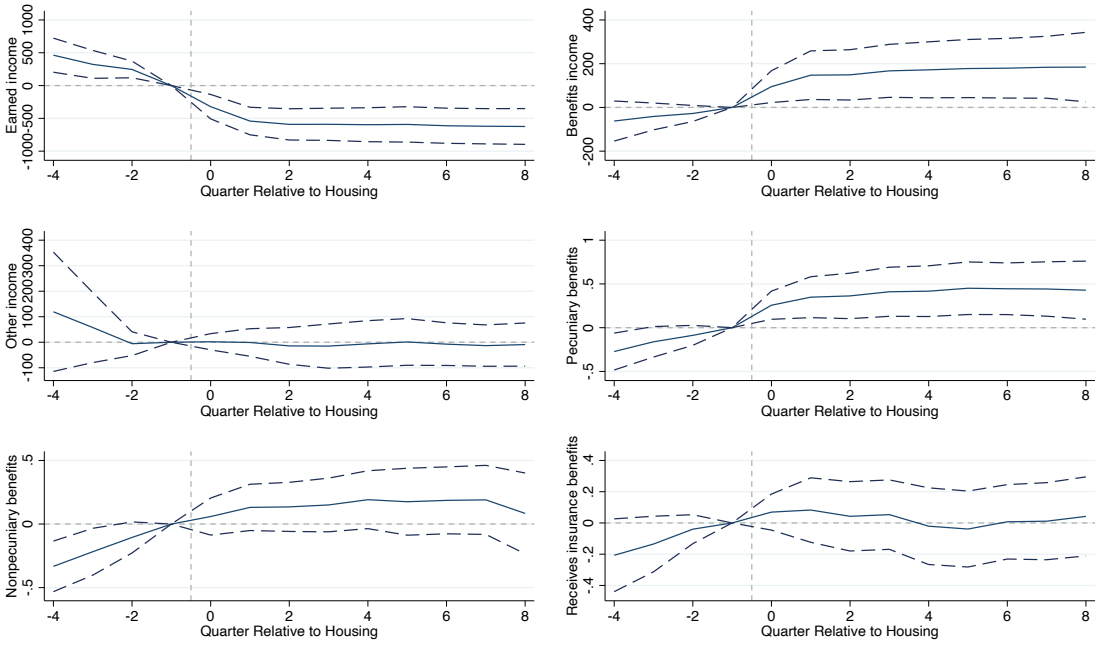



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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

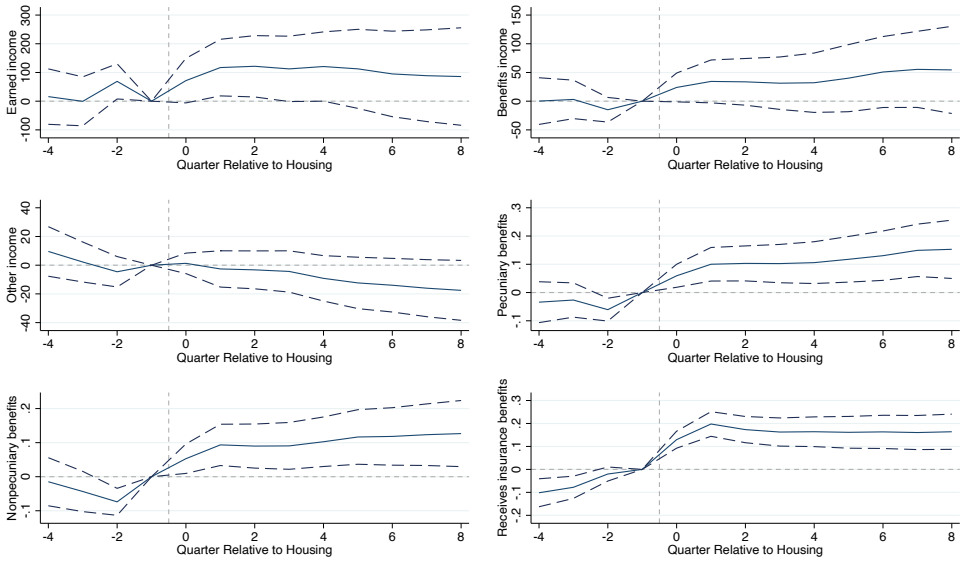



This figure displays the coefficients $\{\hat{\beta}_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 receipt 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 receipt 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) |
| **Pre-event difference** | -0.046 | 58.268 | -0.140 | -126.598 | -0.278 | 164.799 | 0.091 | 15.402 | 0.335 |
| | (0.016) | (40.039) | (0.064) | (33.470) | (0.095) | (29.693) | (0.069) | (11.879) | (0.525) |
| 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 | 0.000 | -18.065 | -0.021 | -8.694 | -0.230 | -2.505 | -0.022 | -6.038 | 0.066 |
| | (0.011) | (21.621) | (0.035) | (14.397) | (0.164) | (17.600) | (0.030) | (7.115) | (0.084) |
| Post-event | 0.019 | 108.025 | 0.081 | 14.293 | 0.060 | 90.476 | 0.083 | 4.953 | -0.134 |
| | (0.011) | (19.806) | (0.034) | (12.723) | (0.140) | (15.555) | (0.028) | (6.941) | (0.077) |
| Constant | 0.042 | 481.797 | 6.159 | 49.997 | 6.718 | 421.319 | 6.102 | 9.765 | 6.326 |
| | (0.004) | (7.481) | (0.010) | (4.530) | (0.055) | (5.911) | (0.009) | (2.207) | (0.043) |
| **Pre-event difference** | -0.019 | 100.215 | -0.012 | -9.860 | 0.270 | 111.349 | 0.017 | -2.863 | -0.385 |
| | (0.010) | (28.678) | (0.056) | (16.177) | (0.130) | (25.080) | (0.057) | (5.790) | (0.597) |
| 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 | 0.75 | 0.50 | 0.70 | 0.67 | 0.80 | 0.40 | 0.97 |
| N | 77580 | 12635 | 10615 | 12445 | 598 | 12637 | 9986 | 12637 | 121 |
| Number of clusters | 2202 | 2148 | 1974 | 2137 | 165 | 2148 | 1895 | 2148 | 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 $i$ reported adverse mental health status prior to housing receipt. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH receipt 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 receipt 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) |
| **Pre-event difference** | -0.044 | 33.680 | -0.204 | -58.890 | -0.103 | 76.750 | -0.095 | 11.705 | 0.445 |
| | (0.018) | (42.988) | (0.069) | (35.296) | (0.124) | (33.516) | (0.075) | (16.053) | (0.438) |
| 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 | -0.013 | -16.204 | -0.009 | -18.621 | 0.253 | 1.681 | 0.016 | -4.260 | 0.084 |
| | (0.009) | (19.390) | (0.026) | (13.522) | (0.165) | (16.039) | (0.024) | (5.792) | (0.139) |
| Post-event | 0.025 | 101.071 | 0.067 | 17.428 | -0.216 | 86.461 | 0.054 | 2.210 | -0.136 |
| | (0.008) | (15.877) | (0.020) | (11.536) | (0.111) | (11.972) | (0.018) | (5.209) | (0.109) |
| Constant | 0.042 | 482.310 | 6.160 | 48.754 | 6.735 | 422.635 | 6.105 | 9.926 | 6.325 |
| | (0.004) | (7.684) | (0.010) | (4.858) | (0.055) | (6.035) | (0.009) | (2.273) | (0.047) |
| **Pre-event difference** | -0.006 | 19.341 | -0.030 | -3.758 | 0.297 | 22.424 | -0.044 | -0.406 | -0.123 |
| | (0.008) | (23.599) | (0.042) | (13.216) | (0.113) | (21.492) | (0.043) | (6.473) | (0.492) |
| 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 | 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 $i$ reported substance abuse problems prior to UH receipt. Coefficients $\{\hat{\beta}_k\}$ represent ATT estimates of UH receipt 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 receipt 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 main 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.