The labor market impacts of unconditional housing out of homelessness

Jakob Brounstein   John Wieseltheir *

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Abstract

We leverage variation in the timing of unconditional housing recipiency by homeless individuals in Los Angeles County to determine the effects of housing the homeless on their employment, earnings, and benefits absorption. Placement into 2-year Rapid Re-Housing (RRH) increases extensive-margin labor market participation by nearly 60% from a baseline of 19pp, while Permanent Supportive Housing (PSH) recipients exhibit a 25% increase in extensive-margin employment from a baseline of 7pp. We find little evidence of heterogeneous response based on family-status for RRH recipients, but we do find a mildly positive employment effects for heads-of-households in PSH. We characterize earnings and benefits responses based on ex-post employment transition type and find that both U2E and E2E transitioners report income increases of approximately USD 800-1000 and USD 200 per month respectively while exhibiting little-to-no change in benefits absorption. These groups outnumber E2U transitioners by a factor of between 2.5-5. Finally, we estimate the program cost-offset specifically through earnings effects during program tenure (ignoring other externalities). We estimate the cost-offset of these policies during program tenure specifically attributable to earnings effects at 1% for RRH and 0 but nonnegative for PSH (5-10% and 1-9% for RRH and PSH recipients respectively employed post-event).

Key words: Homelessness; Housing First; Cost Benefit Analysis; Housing Policy; Homelessness, Housing, and Labor Supply
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*UC Berkeley Department of Economics, 530 Evans Hall, Berkeley, CA 94720 (Jakob Brounstein e-mail: jakob.brounstein@gmail.com; John Wieselthier e-mail: johnwieselthier@berkeley.edu). We thank Hilary Hoynes, Patrick Kline, Jesse Rothstein, and Emmanuel Saez for their generous feedback on this work. We are highly grateful to the California Policy Lab for providing and processing the HMIS data for our use. We would also like to thank Dean Obermark and Janey Rountree for their guidance in navigating the HMIS and its component datasets. This paper previously circulated as "The fiscality of housing the homeless: Evidence from housing programs in Los Angeles". Our results do not represent the views of the California Policy Lab or the Los Angeles Homelessness Services Authority; all mistakes are our own.

1 Introduction

What are the labor market impacts of housing homeless people and how do these impacts affect our understanding of the overall pecuniary costs and benefits of homelessness housing programs? The answer to this question has important implications for how policymakers approach solutions to homelessness. While homelessness housing policies are typically associated with high rental and construction costs, homeless status is also associated with a variety of negative externalities borne by the public.[1] We seek to characterize the contribution of labor market impacts of housing homeless individuals and contextualize the externalities of these impacts within the costs of permanent and semi-permanent housing policies.

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

This paper is the first to study the impacts of unconditional housing to homeless individuals in a setting simultaneously featuring 1) consistent observation of housing recipients over time, 2) a sufficiently comprehensive data environment so as to observe a variety of outcomes allowing us to characterize different employment, earnings, and benefits responses

[1] Some examples include 1) reductions in income tax collections if homeless status creates labor supply frictions or induces participation in the informal labor market, 2) reductions in sales tax collections due to depressed individual consumption, 3) direct costs in the form of non-housing benefits that the state provides to homeless individuals, 4) environmental externalities that reduce property tax collections through base erosion, and 5) 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. There exist some disagreement over the specific programs included within HF, but within this group of policies, we focus *exclusively* on Rapid Re-Housing (RRH) and Permanent Supportive Housing (PSH).

as well as different margins of heterogeneity, and 3) credible quasi-experimental variation. Prior works nearly entirely focus on public benefits absorption, and tend to either rely on overly incomplete data environments, lack of quasi-experimental variation, or exceedingly small sample sizes. Our environment allows us to at least partially address all of these shortcomings in estimating the net costs of UH-style policies. Moreover, our work represents the largest event study focusing on the labor market outcomes and state-level benefits absorption of individuals around placement into UH-style policies and their fiscal implications, with a final treated sample size of roughly 6,000 recipients.

We exploit quasi-random timing in unconditional housing recipiency to estimate the labor market and benefits uptake impacts of receiving unconditional housing. We characterize how this response varies based on 1) family status and 2) ex-post employment transition type. Lastly, we use our estimates to calculate the costs of unconditional housing offset by the earnings externalities of these programs.

There is substantial precedent for studying homelessness and homelessness housing policy in a cost-benefit framework (Gubits et al. (2018); Gilmer et al. (2010); Spellman (2010)). However, nearly all of the work in this space focuses specifically on the evolution of public benefit/service absorption surrounding placement into UH (either observationally, quasirandomly, or randomly) or even simpler cross sectional analyses of benefits absorption among incumbent homeless populations. We place our focus instead on the fiscal externalities associated with the employment effects of UH recipiency. Ly and Latimer (2015) reviews 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.

Gubits et al. (2018) (the Family Options Study) represents one of the central works in this space, studying an RCT in the US that allocated 2,200 families between PSH, RRH, transitional housing, or a control arm (standard of care). This study measured costs primarily based on homelessness service absorption and found a strong negative impact of PSH on homelessness at a 9% greater cost than the control group, and no significant difference between either RRH or transitional housing and the control group at a 9% lower cost.[3] However, this study 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 and bias of their results. Culhane, Metraux, and Hadley (2002) study the evolution in other benefits (namely criminal justice and healthcare utilization) absorption at two discrete snapshots following non-experimental placement into PSH, finding average net 6% cost increases. Zaretzky and Flatau (2013) represents the only work in our review to also study changes in tax payments, imputed based on reported changes in individual income following placement into UH among a very small sample of individuals ($N \leq 20$) in Australia. This study reports an increase of annual income tax receipts of USD 1600 among single men, corresponding with a 6 percentage point increase in employment probability and a USD 3000 per-person-year increase in income tax payments within the employed group. Flaming, Burns, and Matsunaga (2015) is another important study in this space. The authors comprehensively characterize the cost of benefits/service absorption among incumbent homelessness individuals in Santa Clara County, California. Importantly, the authors find substantial heterogeneity in this cost estimate: while they estimate the average annual public costs of persistently homeless individuals at $\$13,661$ per year, they also find that the highest cost-quintile of persistently homeless individuals generate average annual costs of approximately $\$83,000$. Augustine and White (2020) generate similar estimates for the cost of public benefits absorption by “high-utilizers” in Sonoma County at $\$27,000$ per year.

Insofar as we focus on the fiscal impacts of homelessness housing policy via employment, research on the labor market characteristics associated of eviction and housing shocks represents a second closely-related literature space for 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

[3] This study also finds a mild decrease in extensive margin employment among PSH recipients.

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 waitlist structure of housing voucher lotteries and find that housing voucher recipiency among low-income (not typically homeless) families induces a mild decrease in employment and earnings (-6% and -10% respectively) and a 15% increase in take-up of Temporary Assistance for Needy Families (TANF). Our paper is the first to focus explicitly on the labor market impacts of UH-style program recipiency and the fiscal effects associated with these impacts.

Another closely-related literature studies the first-order effects of homelessness housing programs and prevention policies on homelessness and sheltered-status. Von Wachter, Bertrand, et al. (2019) illustrate that 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%. In the realm of intervention-oriented policies, Cohen (Forthcoming) is one of the more closely related papers to our work. The author studies the impacts of UH-style program recipiency on sheltered-status in Los Angeles, finding that these programs significantly decrease the probability of individuals' future return to homelessness (as well as the usage of other public benefits). In addition, the author finds finds that rapid placement into these programs has a knock-on effect; that is, placement into (semi-) permanent housing within one month of initial services enrollment drastically improves sheltered outcomes 10 and 20 months later. We employ similar data to Cohen (Forthcoming); 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.

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 60% increase (10.9 percentage points) in their probability of finding employment. PSH recipients report a smaller increase in probability of finding employment by only 25% (1.8 percentage points). We also explore heterogeneity along family status. We find little evidence of differential employment response for heads-of-families for RRH recipients, but we find a mildly outsized extensive-margin employment response of heads-of-families in the context of PSH.

We then turn to characterizing earnings and benefits responses among UH recipients based on ex-post employment transition type. Among individuals making a U2E transition, individual recipients of both RRH and PSH see earnings increases upwards of USD 1000 per month. RRH recipients making E2E transitions also see increased earnings post-event by around USD 200 per month, suggesting housing allows individuals to find either better jobs or work more hours. Additionally, we find that within each program, the share of individuals reporting employment (making either U2E or E2E transitions around housing) exceeds the share of individuals making E2U transitions by a factor of 2.5-5. We also document that on average, individuals reporting consistent employment in the post-event period report no increase in benefits absorption.

Finally, we perform a novel cost-benefit analysis of these programs when only considering the labor market and earnings externalities. We estimate substantial variation in the net fiscal impact of RRH and PSH recipiency based on whether an individual recipient secures employment following housing recipiency. Nonetheless, in spite of our large documented employment and earnings effects, an overwhelming majority of housing recipients do not report employment post-event so that the average fiscal offset of these programs attributable to labor/earnings externalities amounts to between 1% of the recurring cost during program tenure for RRH on average and near zero (but nonnegative) for PSH recipients.

2 Data and Setting

More than 550,000 people can be classified as homeless on any given night in the Unites States (Council of Economic Advisers (2019)). In most of the United States, homelessness is tracked and managed by local branches of the HMIS called Continuums of Care (CoC). 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 those who are at risk of becoming homeless. 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 sizeable homelessness housing funding in Los Angeles County generate a considerable sample size for studying the effects of placing homeless individuals into housing programs.

Our data is constructed entirely from the Los Angeles County Homelessness Management Information System (HMIS) combined with data collected from the State of California and Los Angeles County by the California Policy Lab (CPL).[4] These data allow us to follow individuals over time and observe the evolution in their employment status, earnings, and benefits uptake, *inter alia*, between 2013 and 2019. Importantly, we observe individuals' employment, housing, and benefits uptake outcomes upon each interaction with one of these aforementioned systems. As such, we have estimates for each of these outcomes prior to, during, and after housing recipiency (if exit occurred). Methods of linking across datasets, construction of variables, and imputation/interpolation strategies are described in more detail in Section C.

Individuals in our data are uniquely identified by a masked ID that is common across a number of Los Angeles County Departments. Each time an individual interacts with the

[4]Specifically, CPL refers to this collection of data as being part of their "Research Accelerator." These data are intended for CPL-affiliated researchers, but bypasses standard proposal processes for accessing individual datasets maintained by separate governmental units.

Los Angeles system, an update is made to their file.[5] These file updates include the reason for the update (e.g. services rendered, if applicable), as well as updates to a number of outcomes of interest: earnings, employment status, health status, housing status, etc. Everyone in our sample, in particular, has "touched" the HMIS in Los Angeles at some point between 2010 and 2020. This feature of the data should indicate to the reader that everyone observed in our data, including those that we consider "untreated" by long-term intervention programs, have been characterized by serious risk of homelessness (Von Wachter, Bertrand, et al. (2019)). Though Cohen (Forthcoming) uses similar data, our data is distinct in two key manners. First, we observe employment, wages, and benefits at each interaction, while Cohen only observes most of these outcomes upon program entry and exit.[6] Second, we can observe outcomes following exit from housing recipiency for as long as individuals continue interacting with other linked services outside of HMIS.

We collapse all available information to the individual- by month-level. While this decision obscures some of the precision we have available, the vast majority (93%) of individuals have at most one update per month. We further restrict our sample to individuals that receive some form of housing benefits out of homelessness between 2013 and 2019. The cleaned data is structured as a single panel at the individual-month level. Our final restriction requires that individuals report at least one HMIS interaction in both 1) 6 months leading up to their housing event (exclusive of event month) and 2) the period between 18 and 24 months post-event. Because interactions do not necessarily occur every month, our main specification interpolates information during missing periods. This process consists simply of projecting information forward to the next interaction; we do not extend our interpolations beyond an individuals' last observation in our data.[7] We provide non-interpolated results that are consistent with our findings in Section B. Data denominated in Dollars are expressed in January 2020 USD, accounting for inflation.

[5]An "interaction" represents any service provision or client meeting. The designation of interaction ranges from items such as referrals from a case coordinator, rent arrears, or outreach, *inter alia*.

[6]There are several complementary aspects of Cohen's data that are central to his empirical strategy. Notably, we cannot observe case worker identifiers, which precludes us from employing a similar evaluator instrument.

[7]For more detail on the projections and missing information, see Section B.

We focus on the two generally largest 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 to recipients on a time-limited, typically two-year time frame (Evans, Phillips, and Ruffini (2021)). Neither of the programs features any requirements on work/employment, additional programmatic involvement (e.g. substance abuse support group attendance), or other behavioral requirements beyond standard tenancy rules typical for market-rate units (e.g. noise ordinances at night, rules about domestic animals, etc.). Importantly, neither program features a mandated reduction in housing subsidy generosity in response to the recipient(s) finding employment.

Placement into either of these programs is generally predicated by an initial homelessness spell, wherein individuals are placed into a queue for some form of direct housing treatment. LAHSA determines each client's position in this queue solely based on: (1) verification of homelessness status and broad program eligibility requirements,[8] (2) tenure in the HMIS during current spell, and (3) completeness of their application.[9] 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 become available. The housing queue evolves at each moment that a housing supplier (typically a nonprofit organization) indicates to LAHSA that they have new or recently-vacated unit for occupancy. LAHSA matching managers offer the newly available housing to the next eligible client in the queue. If the client declines this housing offer, the housing is offered to the next eligible client in the queue without adversely affecting

[8]Clients are also assigned a continuous risk score that, *de jure*, coarsely corresponds with a housing recommendation. The County of Los Angeles, along with most other counties, 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 different housing programs. However, in practice, these scores are assigned with substantial noise and they fail to predict placement into housing programs. The data demonstrates that RRH recipients, for instance, have significantly lower housing-priority scores than individuals never receiving any permanent housing benefits, and that the risk-score only explains 2% of the variation between individuals in whether and what type of housing they receive. Figure A.8 shows that risk score has little bearing on both assignment and timing of assignment.

[9]This step represents the focal point of the case worker assignment instrumental variables design in Cohen (Forthcoming).

the eligibility or queue position for the initially declining client's housing offers. For these reasons, we argue that the timing of UH recipiency conditional on eligibility is sufficiently random.[10]

Table 1 shows summary stats among three groups of individuals included in our data. We construe individuals in the first two columns as treated with a semi-permanent housing intervention. 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). Individuals in our sample tend to be around age 40 and are overwhelmingly unemployed. Average total monthly income among those that interact with the HMIS is between USD 300 and 450. Approximately 20% of those receiving Rapid Re-Housing are employed at first interaction, whereas individuals receiving PSH and untreatd 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. Most individuals are homeless for 1-3 years prior to receiving some form of long-term housing intervention and are either living in a place not meant for habitation (PNMFH), or at an emergency shelter prior to treatment.

Importantly, the majority of those interacting with the HMIS never receive long-term treatment in the form of either PSH or RRH. "Untreated" does *not*, however, mean that they receive 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. Finally, untreated individuals are *excluded* entirely from the analysis. We do not utilize these individuals as a matched control or as any sort of comparison group in the main specification of our event studies.

Lastly, note the substantial reduction in sample size between our baseline number of UH recipients and our final sample. This sample drop-off is due to our requirement that we

[10] If individuals experience no wait in being matched to housing following initial service enrollment, the event time of housing recipiency may be systematically correlated with pre-event outcomes. Figure A.4 shows the distribution of wait times across treated units.

observe individuals interacting with the HMIS with sufficient frequency pre- and post-event. Ex-ante, it is unclear how requiring housing recipients interact with the HMIS a sufficient number of times prior and subsequent to housing may impact results relative to a perfect-information baseline. Individuals with more frequent HMIS interactions may demonstrate greater responsibility or represent stronger institutional connection than those interacting less frequently, which may induce positive selection in our sample. Individuals dropping off may do so due to death, which may induce further positive selection, or may do so due to successful exit from homelessness, which may induce negative selection. Censoring via sample attrition remains a crucial challenge in studying homeless populations. As a demonstration of the robustness of our results, Section B.3 presents our main results without imposing the requirement of observing recipients at least once between 18 and 24 months post-event (maintaining the requirement that we observe the individual in the 6 months leading up to their housing event); this change increases our RRH sample to 11,215 individuals (from 3,028) and our PSH sample to 5,056 (from 3,006). The results from this specification are both quantitatively and qualitatively similar to our main results.[11]

3 Empirical framework

To study the effect of treating homeless individuals with RRH or PSH on their labor market and benefits uptake outcomes, we estimate a series of simple event studies around the placement of said individuals into one of these housing programs. Our main outcomes-of-interest include whether an individual reports holding employment, earnings, and benefits uptake for select programs. We also observe several other outcomes dealing with absorption of a variety of nonpecuniary benefits.

Our main outcomes of interest include the following: 1) whether an individal reports or is administratively observed as employed; 2) total benefits income, i.e. the Dollar amount of benefits received in total from the following programs: SSI, SSDI, Unemployment Bene-

[11] We view the largest differences from this robustness check as arising in our heterogeneity analysis by guardian-status (analogous to Section 4.3); in this alternative sample specification, we document more compelling evidence of outsized employment and earnings responses to housing among guardians.

fits, TANF Veteran Affairs assistance, Social Security, and General Assistance from LAHSA organisms; 3) "other" category income (the aggregation of worker's compensation, private disability insurance payouts, pension payments, child support, alimony payments received, and unallocated income); 4) an indicator for whether an individual takes up insurance from any one of Medicaid, SCHIP, Medicare, or Veterans Affairs); and 5) whether an individual takes up any of the following nonpecuniary benefits: SNAP, WIC, TANF Childcare, TANF Transportation, or another unallocated nonpecuniary benefit. We also present selected results on disaggregated subprograms and logged Dollar amounts.

Individuals have fairly infrequent interactions with the HMIS. Additionally, the precise date of move into UH accommodations sees substantial reporting error. In 15% 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.5 illustrates important discrepancies between these dates, suggesting the presence of potential measurement error in the event month. To accommodate these issues, we specify our main reduced forms as quarterly averages on the monthly level. 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 effects α_i, month effects $\delta(t)$ and a mapping $q(t)$ of month t to quarter q of the year.

We run event studies as specified in Equation (1) on individuals in our dataset 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 recipiency for each individual and require individuals had not received either RRH or PSH prior to January 2014.[12] Additionally, we require that individuals are observed at least once at least two

[12]We observe that 81.7% of individuals receive UH benefits only once, 14.6% two times, 2.9% three times,

months prior to their housing placement event. This restriction helps ensure that individuals are placed into housing off of the queue rather than placed immediately into housing in absence of a wait time (i.e. in a manner that might pick up negative pre-event trends that drive individuals to enroll in homelessness services).

Leveraging the quasi-random variation in timing of housing recipiency yields coefficients $\{\hat{\beta}_j\}$. Central to this design, we argue that the timing of individuals' placement into UH is orthogonal to $\{\varepsilon_{ik}\}_{k\leq 0}$. An individual's position in the housing offer queue is unrelated to the evolution in their own economic/health situation conditional on remaining in the queue (as opposed to voluntarily withdrawing from the queue). As such, we construe the *timing* of housing recipiency as unrelated to evolution in pre-event outcomes. Under this assumption, we interpret $\{\hat{\beta}_j\}$ as estimating an average treatment effect of housing on treated (ATT) individuals j periods since the housing event. The validity of these estimates $\{\hat{\beta}_j\}$ for the ATT relies on assumptions of non-anticipatory responses to housing events and that post-event counterfactual outcomes would evolve in line with pre-event outcomes. 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.

Given this design, we interpret of the sequence of $\{\hat{\beta}_j\}$ for each group (RRH and PSH separately) as the *within*-group treatment effects of that respective UH-style housing program on that group. As such, because we do not address the role of selection between or into these two programs, we do not interpret these ATT estimates as externally valid for understanding the impact of extending a specific UH subprogram to the representative *marginally-eligible* homeless individual (i.e. we do not interpret these these impacts as Average Treatment Effects). Additionally, 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

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.

never interact with the system feature even greater negative selection on outcomes than our observed population. We further discuss interpretation issues related to external validity in Section 5.

Within this setup, we stratify our estimation and our entire analysis based on whether an individual received PSH or RRH (estimating two separate sequences of effects $\{\beta_j^{PSH}\}$ and $\{\beta_j^{RRH}\}$). On a fundamental level, RRH and PSH represent programs intended for two entirely separate recipient populations. Namely, those receiving PSH are determined to have no-to-little 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. Thus, we can understand PSH as more closely akin to a disability relief program, and 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.

Figure 1 illustrates the timing of housing events within our sample. This figure demonstrates that RRH events occur an order of magnitude more frequently than PSH events, which matches the grave nature of PSH as a treatment. Additionally, both types of events trail off significantly in frequency by the end of the sample timeframe, with the drop in PSH frequency occurring about three years prior to the decrease for RRH events. It's unclear in the data whether this is due to lack of housing availability in later years, or otherwise.

Figure A.4-Figure A.7 display more information on how individuals interact with the HMIS and related service providers. Figure A.4 and Figure A.6 plot the frequency of the time between individual's housing events and their earliest and latest, respectively, interactions with the HMIS and related service providers.[13] Given the role of data censoring in studying

[13] Time-horizon censoring is particularly problematic for studying homelessness issues, as non-observation beyond a certain time frame can indicate a variety of likely, but drastically different outcomes—such as death (Meyer, Wyse, and Logani (2023)), recidivism into homelessness without interaction with public services,

homelessness issues, understanding these interactions is important for our setting. New information on housing recipient outcomes is only generated upon interaction with the services covered in our data, so the frequency and timespan of individual interactions around housing events are key for the robustness of our design as well as for the fidelity of our dependent variable interpolation method.

For this reason, our main specification features both the use of interpolated dependent variables as well as two way fixed effects on the individual-time level. These two way fixed effect ensure that our estimates do not compare outcomes between units that see censoring in a manner that is correlated with their outcomes, but rather represent *within*-unit effects. The use of interpolated dependent variables helps prevent picking up selective censoring on positively- or negatively-selected units (e.g. positively-selected units may interact with our data less frequently than do negatively-selected units, which would introduce negative bias in standard event study estimates). This decision requires an assumption that the interpolated values estimate their true values in an unbiased manner. As a matter of demonstrating robustness, we also present in Section B.1 estimates of our central results that do not make use of an interpolation technique; these estimates are generally consistent with our preferred specification, but are somewhat noisier. Section B.2 present results on the monthly-level, largely consistent with our main results, albeit sometimes demonstrating pre-event trends and outsized responses at event time.

4 Results

We document generally large positive effects of housing program recipiency on labor market outcomes that demonstrate substantial heterogeneity by program type. Figure 2 - Figure 4 plot the various event study coefficients for placement into each type of housing program. Table 2 - Table 4 summarize these results in relation to pre-period baselines. These tables

or successful transition out of homelessness. The HMIS gathers additional data on housing recipients (in addition to from their non-housing HMIS interactions) from post-housing "exit" interviews with housing recipients at 6, 12, and 24 months post-housing recipiency when possible. However, we still observe individuals in our data upon interaction with services covered in the HMIS and related service providers.

omit the estimated effect at event time in order to prevent picking up effects due to increased reporting upon move-in.

4.1 The Effects of RRH

We find that RRH substantially improves labor market outcomes of its recipients. Recipients see an average extensive margin employment rate increase of 10.9 percentage points relative to a pre-period baseline of 19.4% (an increase of 56%). On average, RRH recipients also see increased total incomes of around USD 200 per month—a 47% increase from pre-period levels. Of this average increase in total income, about 60% comes from increases in earned income and most of the remainder from benefits income. On the intensive margin, individuals reporting non-zero monthly income in both the pre- and post-event periods saw their income increase by 20% on average. Interestingly, we also observe a decrease in overall cash benefits received over time, decreasing to about half the value of the post-event peak by two-years post-event. Importantly, these figures' on average changes in earnings and relative earnings obscure the heterogeneous changes by employment transition subpopulation type (explored in Section 4.4).

Table 3 and Table 4 displays analogous results for aggregated and specific programmatic benefits and shows that recipients see substantial increases in their uptake of some of these other benefits on average. These general nonpecuniary benefits include SNAP and TANF, in addition to programs like medicaid, medicare, WIC, etc. However, as Figure A.2 and Figure A.3 illustrate, the evolution of individual take-up of these specific programs around UH recipiency, more frequently exhibit differential pre-event trends. After receiving RRH, individuals see a near a substantial increase in the probability of receiving pecuniary benefits (+9.1pp from a baseline of 40.2pp), but no increase in the probability of receiving non-cash non-insurance benefits from a prior state of not receiving any of these benefits (Figure A.2 actually illustrates a noisy decrease). Panel (7) of Figure A.2 also indicates a decrease in the probability RRH recipients receive substance abuse services. Given the increased connection to social services following placement into housing, we view that this observation more likely reflects a decrease in substance abuse rather than a decrease in connection to treatment services.

Housing recipients see a substantial increase in the probability of receiving insurance benefits (+20.9pp from a baseline of 57.7pp) which drives nearly all of the increase in access to health insurance following placement into housing. 80% of this response is driven specifically by increased connection to Medicaid. Looking at specific programs, we see the greatest responses coming from increased connections to SSI, SSDI, TANF, increasing in takeup by a factor of 3-4. Individuals also see increased probability of receiving unemployment benefits and SNAP.

Overall, placement into RRH results in large improvements to labor market outcomes and increased take-up of social programs. This induction into social programs is likely driven by two separate effects: the newly attained access to a domicile and permanent address as well as increased interfacing with LAHSA. This latter effect likely manifests as the pre-intervention trends visible in some of the figures, namely as an anticipation effect in which individuals expect to receive housing in the near-future and are being directly connected to programs through the case worker that they are assigned prior to actual RRH recipiency. We cannot precisely disentangle these two effects, though the spikes in employment, earned income, and benefits income seen in the month of treatment are unlikely to be driven predominantly by timing of case worker assignment or connectedness to LAHSA, as we discuss further in Section 5. Moreover, we attribute the increase in employment and earnings to housing recipiency as (as opposed to connection to benefits), as the changes in benefits or benefits income does not respond to housing among the subsample of individuals finding employment (Section 4.4).

4.2 The Effects of PSH

Panels (b) of Figure 2 - Figure 4 and Table 2 - Table 4 show the effects of PSH recipiency on employment outcomes, earnings, unearned income, and general programmatic benefits. PSH recipients demonstrate substantially less benefit to their labor market outcomes following their placement into permanent housing, though these changes represent large, albeit statistically noisier, relative effects. Individuals that receive PSH see an increase in average extensive margin employment of approximately two percentage points (a 26% increase), although this effect doesn't appear to persist beyond one year on average. This employment increase is accompanied by noisy average increase in earned income and a significant increase in benefits income (and overall monthly income) by USD 110 against a baseline of USD 380 (+29%), although PSH recipients exhibit mild pre-event increases in benefits income. Similar to as for RRH recipients, earns on the intensive margin also increase (here by around 9%), suggesting that after receiving stable housing, individuals are either able to work more hours or work at jobs with greater pay.

Similarly to RRH recipients, PSH recipients see a large increase in other programmatic benefits. There are a few key difference in benefits absorption responses for PSH recpents. In particular, we observe an increase in non-cash non-insurance benefits uptake among this group (around 8% from a baseline of 54pp) as well as increased uptake of LAHSA-provided health services, such as HIV/AIDS management and prevention services, mental health services, and substance abuse treatment, although evolution in uptake of these services also exhibit substantial increases leading up to housing. Specific program uptake is also similar for PSH recipients as for RRH. However, we see an even greater share of increased insurance coverage attributable to Medicaid (94% of the increase in any insurance coverage).

4.3 Heterogeneity by family/guardian status

We now turn to decomposing our results based on family/guardian status. Von Wachter, Bertrand, et al. (2019) emphasizes the role of efficiency and targeting in designing optimal homelessness and poverty alleviation policy. It may be the case that heads of households with children demonstrate greater labor market participation responses to housing recipiency than do individuals without families. We investigate this possibility, distinguishing between heads-of-household with or without families based on satisfying either of the following conditions:[14] 1) in individual i's household ID number, there is at least one other distinct individual j that is identified as a minor; 2) individual i's household ID number contains at least three distinct individuals. We tag individuals satisfying either of these two conditions

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

as “guardians” and individuals satisfying neither of these conditions as “non-guardians”. In order to ensure proper comparison, we restrict our analysis that identify as heads of their household unit.

We estimate regressions according to Equation (1), stratifying based on guardian status and by UH program. Figure 5 displays the results separately for RRH and PSH. The results reveal no stark evidence of differential impact based on guardian status for RRH recipients; we observe a mild, yet persistent difference in employment and earnings appears in favor of guardians; however this difference is not statistically significant. Figure A.9 displays additional results on benefits to reveal the only large difference between guardians and non-guardians in the context of RRH recipiency arises in the form of increased absorption of nonpecuniary benefits (like SNAP, WIC, and TANF Transportion and TANF Childcare) among guardians. We do not interpret this general symmetry between the two groups as striking, as we find that RRH recipients demonstrate greater capacity to work. The figure reveals slightly greater insurance takeup responses among non-guardians. However, we observe non-guardians with insurance coverage at two-thirds the rate as guardians in the pre-event period.

For PSH, some difference emerges in based on guardian status. Importantly, guardians exhibit a similar employment impact to the RRH population, albeit far less statistically significant, in response to housing recipiency, while non-guardians exhibit no employment response. This asymmetry in response among PSH recipients by guardian status is somewhat unexpected: receiving permanent unconditional housing out of homelessness induces an increase in extensive-margin employment status for individuals with children, but not among individuals without children. We also observe an outsized increase in benefits income received among non-guardians relative to guardians. In fact, guardians exhibit no increase in their recipiency of cash-benefits. We interpret this effect as an increased connection to benefits among non-guardians, who we observe receiving 30% less in cash benefits prior to event. As or RRH recipients, non-guardians demonstrate a greater increase in insurance coverage than do guardians (which we interpret as a similar equalizing effect).

4.4 Heterogeneity by ex-post employment transition

We are also interested in how the average changes in earnings and benefits uptake documented in Section 4.1 and Section 4.2 decompose between different subpopulations by employment and unemployment transition types following placement into housing. We primarily focus on unemployment-to-employment (U2E) and employment-to-employment (E2E) transitions following treatment with either RRH or PSH, while Section A.4 displays results for other transition types. Clearly, finding employment or improving one's employment outcome is not random, so we are only using these to understand the differences in earnings outcomes in cases where employment is found; an outcome which is obscured by the population-average results in the previous subsections. In this way, we intend to recover something close to an average treatment effect across the (ex-post) marginally treated subpopulation. Table A.1 displays the 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 positive predictors of transition into employment include lower homelessness severity, being younger, and not using drugs or controlled substances.

Since employment can fluctuate from month-to-month, we define "employed" in the pre-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-sample period. Unemployment and employment are defined analogously in the post-period. In this way, there are some individuals that we can say nothing about (e.g. those who were employed for 50% of the pre-period, for instance). Among the 3,000 RRH recipients in our final sample data for whom we can precisely estimate pre-period employment, 58.5% make U2U transitions, 3.3% make U2E transitions, 2.3% make E2U transitions, and 7.9% make E2E transitions. The remaining 28% of individuals have employment fluctuations that are we cannot cleanly categorize in one of these ex-post employment transition types.

First, we show outcomes related to U2E transitions following RRH enrollment in Figure 6. Individuals characterized by U2E transitions secure employment almost immediately in most cases and see their earnings increase by an average of USD 800-1000 per month. This result accounts for the vast majority of the increase in their total monthly income. Benefits income remains sees a mild, statistically insignificant increase. The figure also shows no increase in the probability of absorbing new cash-benefits or non-cash benefits. These observations provides a validation that our for individuals that find employment are driven by simultaneity via connection to addition services; as these individuals find employment, they see no change in their connection to benefits. This said, Figure A.10 and Figure A.13 display the results of U2U transitioners, demonstrating that there is some role housing recipiency perform in facilitating connection to additional benefits.

We show analogous results for E2E transitions following RRH enrollment in Figure 7. Individuals that were previously employed (and remain employed) increased their earnings by an average of around USD 200 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. 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 reported.

Figure 8 and Figure 9 show outcomes following U2E and E2E transitions, respectively, for PSH recipients. Among the 3,000 PSH recipients in our final sample for whom we can precisely estimate pre-period employment, 84.9% make U2U transitions, 0.9% make U2E transitions, 0.9% make E2U transitions, and 1.6% make E2E transitions. 11.8% of individuals are not assigned to any transition group due to their substantial employment fluctuation, as described as the beginning of this section. The outcomes for U2E transitions look very similar to those of RRH transitioners. Earned income increases by around USD 600-800 per month whereas benefits income exhibit only a mild increase, (although this increase is not statistically significant, and this response exhibits some pre-intervention increase). We observe no increase among U2E transitions in the probability of receiving new nonpecuniary or pecuniary benefits. However, for PSH recipients classified ex-post as E2E transitions, we document no increase in earned income. Among these group, we document no increase in uptake of any pecuniary or programmatic benefits.

5 Discussion: mechanisms, external validity, fiscality

Our 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 60% increase (10.9 percentage points) in their probability of finding employment and increase their total benefits income by around 70% on average. Among individuals that find employment, monthly income increases by around USD 800-1000 with little-to-no 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.

We document much more heavily muted effects for PSH recipients, which we interpret to highlight the differences in treatment unit selection between the two programs. PSH recipients report a smaller increase in probability of finding employment by only 25% (2 percentage point), although this increase is more noisy and less persistent over time. Instead, PSH recipients see a larger increase in their benefits absorption upon connection to unconditional housing relative to their RRH treated counterparts. Among the few individuals that find employment upon placement into PSH, we find large increases in earned income unaccompanied by increases in benefits income, but these individuals comprise an increasingly small proportion of the sample of PSH recipients—only around 2.4% of individuals.

In understanding the validity of and mechanisms driving our results, we emphasize two separate points about simultaneity and external validity. First, a natural concern arises regarding simultaneity: housing recipiency may affect earnings and labor market activity both through access to a shelter and through increased access to other programmatic benefits (stable location of domicile, access to a permanent address). In this way, to what extent can we attribute the labor market impacts we detect to these two forces? Section 4.4 and Section A.4 clearly reveal a salient difference in benefits recipiency response along ex-post employment transition type. E2E and U2E transitioners saw increases in both employment and earnings with no accompanied increase in benefits income, whereas U2U transitions saw no change in employment and earnings, but an increase in benefits income. This result indicates that employment responses in this setting operate primarily through access to a stable permanent shelter; having one's own residence improves labor market outcomes. However, it is unclear whether this difference is driven mechanically through use of means testing and whether we would detect a similar difference in the complete absence of means-tested access to benefits.

Second, we view our estimates to be interpreted as average effects of treatment on the treated (ATT), and in this way do not represent the impact of extending program recipiency to the marginal individual (along whichever margin of housing recipiency). 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. With respect to understanding the external validity of our results, we interpret our coefficients as the effects of extending program recipiency to the average respective program recipient.

As an additional caveat to these results, several of our designs—primarily those involving programmatic benefits recipiency—are marked by pre-event trends leading up to placement into housing.[15] We attribute these pre-trends to two sources. First, these pre-event trends may represent a quasi-mechanical result of the process of progressively connecting individuals to services following their initial services enrollment with LAHSA, but prior to their

[15] See for example **??** Panels (a) and (b).

placement into housing. These services provide auxiliary data on our main outcome variables dealing with labor market activity (through survey administration and means testing), but enrollment in these services are observed endogenously as outcomes themselves. Second, individuals that are aware of their impending UH recipiency may improve their outcomes as a causal, but anticipatory response to receiving housing and exiting homelessness. Given the process of constructing of the data within the HMIS, we view the first case to be more likely.

5.1 What are the net fiscal impacts of unconditional housing?

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

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

for some level of taxes paid τ_i, state-benefits absorbed b_i (direct programmatic benefits as well as other public service system usage such as medical or criminal justice services), and homogeneous social and environmental externalities e (e.g. crime, environmental impacts, and their associated capitalization into land values and property taxes, etc.)—all given income z contingent on skill type θ_i and housing state ξ. Moving an individual from a homeless to a sheltered housing state at a flow cost c results in the social budgeters's non-welfare-weighted net flow cost/benefit of

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

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

Substantial attention has been placed on quantifying the average change in benefits absorption from $\mathbb{E}[\Delta b]$. While no work to our knowledge has identified this parameter in a context that simultaneously features 1) a large sample size, 2) frequent observation over time, 3) comprehensive observation of benefits absorption, and 4) compelling causal identification, extant research suggests significant fiscal benefits through this channel. Culhane, Metraux, and Hadley (2002) find a UH cost-offset of 20% through changes in shelter use, hospitalization, and incarceration during program tenure. Zaretzky and Flatau (2013) estimate cost-offsets through changes in health, criminal justice, and welfare service absorption equal to 35% for men receiving supported accommodations. No works to our knowledge have attempted to estimate $\mathbb{E}[\Delta e]$.[16]

We instead place our focus on $\mathbb{E}[\Delta\tau]$: specifically how the labor market impacts of unconditional housing weigh against the costs of housing in the context of the tax system. 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.[17]

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

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

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 $\hat{\gamma}$ and $\hat{\beta}$ and the p-value of the post-pre difference into Table 5. We start by assuming 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. According to our estimates, they increase their total formal annualized income from USD 11,905 to USD 17,409. We assume individuals earn no capital income, and that they pay payroll and sales taxes according to imputations in Piketty, Saez, and Zucman (2018). We assume that all individual income tax filers have no dependents and 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. Eligible EITC claimants in our sample cease receiving EITC benefits at this earnings level in 2017 (a decrease from USD 236) and pay USD 826 more in Federal Income taxes (applying the 2017 standard deduction and income tax rate). According to Piketty, Saez, and Zucman (2018), these individuals pay a combined 5% and 10% of their income on sales and payroll taxes respectively, generating an additional USD 550 in payroll taxes. Individuals pay an additional USD 263 and 275 in sales tax (for EITC non-claimants and claimants respectively). Therefore, among the group of individuals finding stable employment after placement into RRH, tax payments increase on average by USD 994 per year.[18]

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,

[18]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.

during the program's two-year tenure, the labor market impacts among E2E transitioners offset 5.2% of the recurring programmatic cost of RRH through earnings externalties onto sales taxes, payroll taxes, and federal income taxes. We perform this calculation for other employment transition types for both RRH and PSH and combine the estimates using the proportion of recipients composing each respective ex-post employment transition type to calculate an average cost-offset attributable *solely* to labor market responses on average.
The results of Table 5 illustrate the how the fiscal externalties through the labor impacts of unconditional housing weigh against the costs of RRH and PSH during program tenure. The table reveals several novel facts. 1) For both programs, in net, the fiscal externalties associated with the average recipient are very small. During program recipiency, the labor market impacts of RRH and PSH offset the recurring cost by 1.04% and 0.09% 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 2.5-5); this fact contrasts with previous theoretical and empirical findings positing net disemployment effects of unconditional housing recipiency (e.g. Jacob and Ludwig (2012)). 3) Relatedly, while the net labor market fiscal externalities are small, they are not negative. 4) Fiscal externalities and eearnings patterns are similar between RRH and PSH conditional on employment transition type. Lastly, in light of these observations, the contrast in fiscal externalties based on post-event employment perhaps also highlight the role of targeting and homelessness prevention in optimal policy design (Von Wachter, Bertrand, et al. (2019)).

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

Our net fiscal impact estimates should be not be interpreted as the net cost/benefit of UH, but rather a lower bound, due the omission of other fiscal externalities, most notably those from including from direct public service usage[19] as well as environmental externalities. Ultimately, we find that the fiscal externalities 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, Bertrand, et al. (2019)). Of course, this discussion entirely foregoes the normative social welfare considerations of moving individuals out of homelessness. Finally, our data suffers from attrition in the long-run so that we are unable to speak to dynamic effects beyond our two-year time horizon. We also assume no long-run earning growth for U2E and E2E transitioners. As such, we interpret these fiscal impacts as lower-bound estimates.

6 Conclusion

We use the timing of unconditional housing treatments in Los Angeles county to determine the effects of housing the homeless on employment, earnings, and select benefits absorption. We stratify our analysis by the two primary programs of available to homeless individuals in Los Angeles: RRH and PSH. We find substantial labor market benefits following placement into RRH and relatively attenuated effects for placement into PSH. This contrast likely speaks to differences in selection criteria into each respective program, rather than underly-

[19] We are not aware of any work that estimates recidivism and public service usage following exit from unconditional housing settings.

ing differences in the actual treatment.

We explore other margins of heterogeneity. In particular, we explore whether recipients exhibit differential employment response based on their observed family status; we find no evidence of differential employment response for heads-of-families for RRH recipients, but we find a large extensive-margin employment response of heads-of-families in the context of PSH. We also characterize changes in earnings and benefits absorption based on ex-post employment transition type: conditional on making a U2E transition, individual recipients of both RRH and PSH see earnings increases of USD 800-100 per month. RRH recipients making E2E transitions also see increased earnings post-event by around USD 200 per month. We also document that on average, individuals reporting consistent employment in the post-event period report little-to-no increase in benefits absorption.

Based on these results, we estimate that the fiscal impact of these programs—specifically through their impacts on securing and maintaining employment—net positive, but small (around than 1% cost-offset during program recipiency for RRH recipients and near zero for PSH) relative to the existing estimates of the reduction in public service usage. Moreover, the fiscal externalities of these programs are small on average, because while the extensive-margin employment effects of these programs are large in relative terms (+10.9pp from a baseline of 19.4% for RRH and +1.8pp from a baseline of 6.9% for PSH), baseline employment is very low and the overwhelming majority of recipients are not observed as consistently employed in the post-event period.

Our findings suffer from several of shortcomings in our data: 1) inability to observe individuals both outside of this integrated data system and following their exit from the data, 2) sparsity of updates on some outcomes, 3) as well as lack of high-quality individual-level health and crime outcomes. Moreover, the precision of our estimates is likely local to homelessness in Los Angeles County. Although this limitation compromises the external validity of our estimates, our results likely hold for homeless populations in similar urban centers.

Future researchers could strictly improve on our estimates by combining this data with other data that could address these gaps. More precise and comprehensive data would also allow researchers to allocate greater focus to the heterogeneity of costs/benefits by recipient characteristics. Along with the other costs and benefits of homelessness assistance, more precisely estimating *who* would benefit from unconditional housing treatment remains a central question in informing our understanding of the overall impacts of these policies.

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7 Main figures and tables

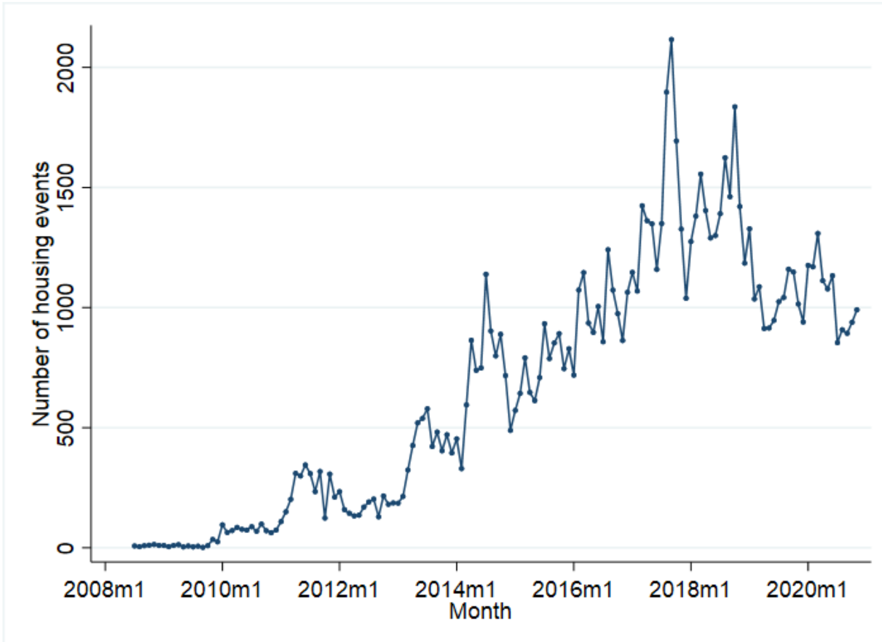
Table 1: Summary statistics on final sample

	RRH	PSH	Untreated
Demographics			
Black	0.600	0.551	0.371
Male	0.536	0.580	0.643
Veteran	0.196	0.060	0.073
Age at first interaction (years)	40	46	42
Prior Housing Status			
Months homeless since first spell	16	54	-
Months homeless since prior spell	8	43	-
Most common prior living situation	PNMFH (34%)	PNMFM (38%)	PNMFH (72%)
Second most —	Emergency Shelter (32%)	Emergency Shelter (35%)	Emergency Shelter (19%)
Third most —	Transitional Housing (12%)	Move from Prior HF (7%)	Living with Family (1%)
Employment, earnings and benefits at first interaction			
Employed	.195	.070	.083
Total earned income among employed	1484	1159	1160
Total benefits income among employed	141	60	41
Total monthly income among employed	1644	1234	1208
Total benefits income	230	347	207
Total monthly income	464	414	278
Individuals	36,090	7,886	59,227
Individuals in final sample	3,028	3,006	-

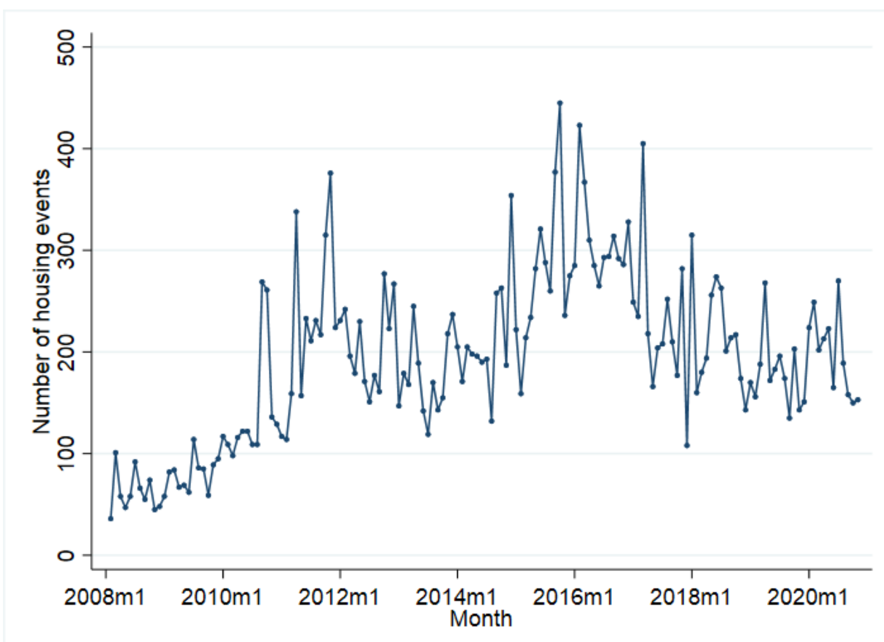
This table displays select demographic, housing, and employment tabulations stratified by final sample subgroups of treatment status. Dollar values are expressed in units USD January 2020. "PNMFH" refers to "Place not meant for habitation". Months Homeless Since First Spell is calculated as the difference 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 recorded at event time for RRH and PSH recipients and upon earliest interaction for untreated individuals.

Figure 1: Timing of housing events

(a) Rapid Re-Housing



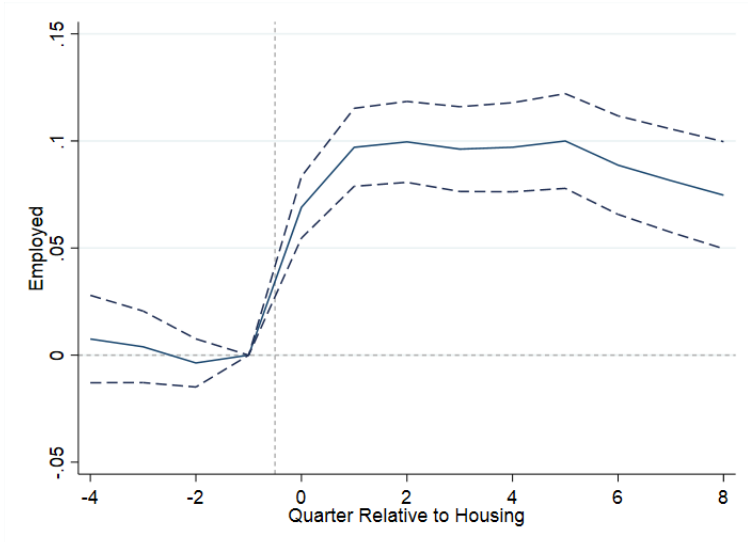
(b) Permanent Supportive Housing



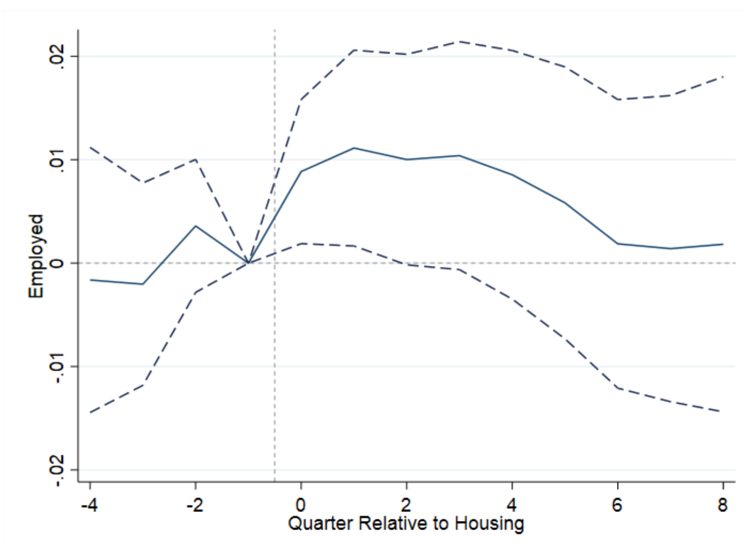
These figures plot the number of housing events per month in Los Angeles County between 2008 and 2020. Panel (a) depicts the number of first-recipiency events for Rapid Re-Housing by month. Panel (b) depicts the number of first-recipiency events for for Permanent Supportive Housing by month. Our primary analysis sample excludes individuals who have ever received both Rapid Re-Housing and Permanent Supportive Housing and excludes recipients entering a housing program prior to January 2014.

Figure 2: Event study results: employment (extensive margin)

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



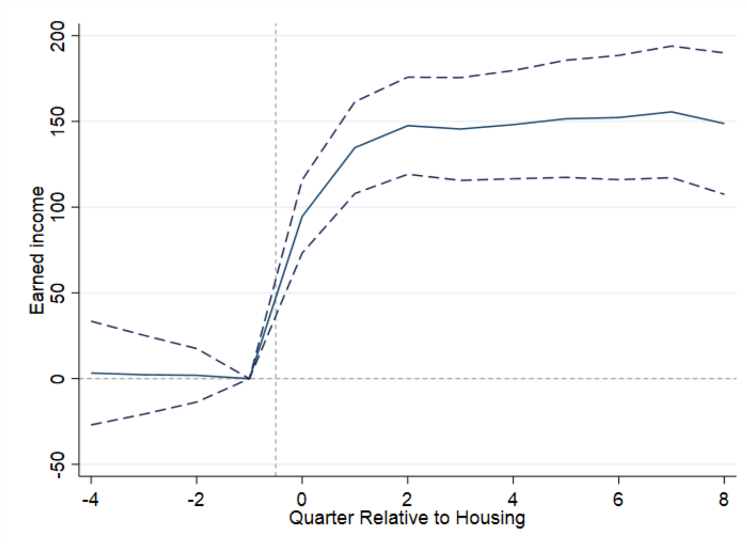
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

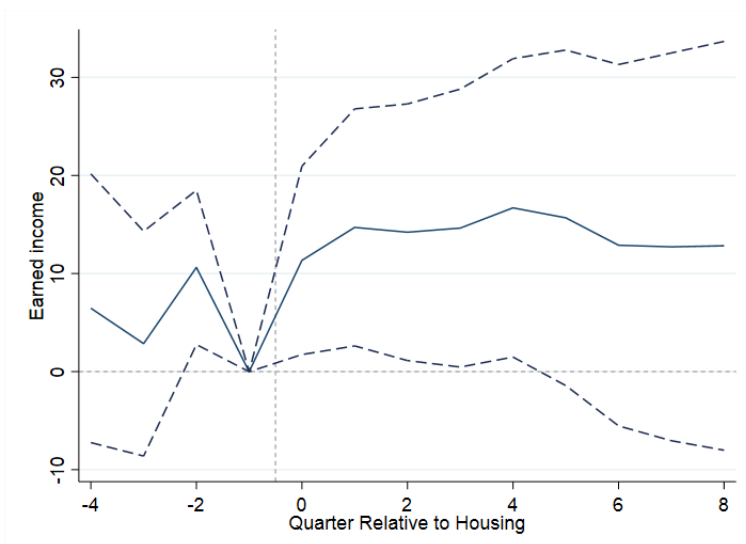
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display.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

Figure 3: Event study results: earned income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



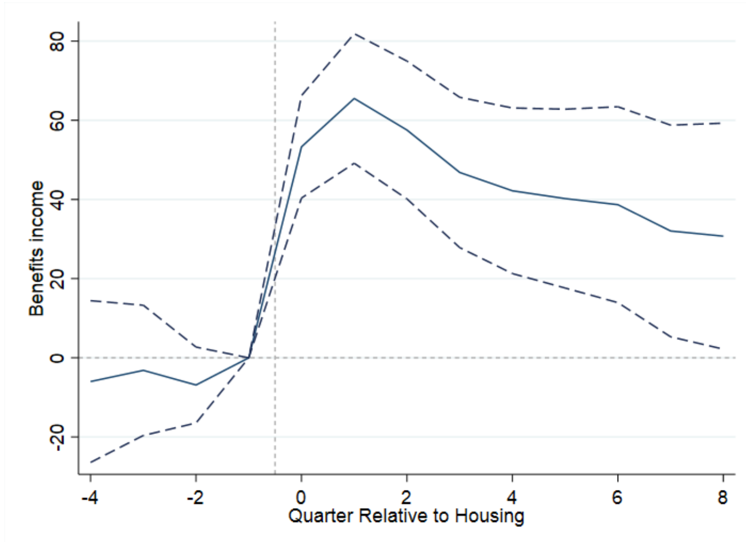
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

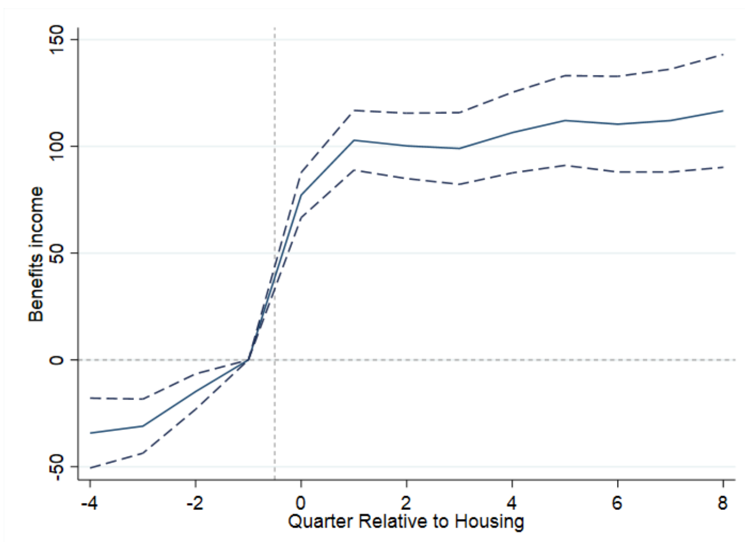
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display.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

Figure 4: Event study results: benefits income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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. "Benefits income" refers to pecuniary support received on part of SSI, SSDI, unemployment benefits, TANF, Veteran Affairs assistance, Social Security, and General Assistance from LAHSA organisms. 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 by month. Panel (b) shows the results for Permanent Supportive Housing by month. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Table 2: 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 ($t \leq -2$)	-0.017	-23.767	-0.028	-13.017	-0.051	-14.558	-0.022	0.320	-0.037
	(0.006)	(10.725)	(0.015)	(9.899)	(0.023)	(5.598)	(0.011)	(2.043)	(0.020)
Post-period ($t \geq 1$)	0.092	183.619	0.164	117.096	0.136	63.651	0.064	3.488	0.071
	(0.009)	(14.486)	(0.020)	(13.082)	(0.033)	(7.757)	(0.016)	(3.244)	(0.035)
Post-pre difference	0.109	207.386	0.192	130.114	0.187	78.210	0.086	3.168	0.108
	(0.011)	(17.475)	(0.026)	(17.041)	(0.044)	(9.387)	(0.021)	(4.169)	(0.049)
Pre-event average	0.194	440.717	6.453	178.227	6.820	257.474	6.230	14.181	6.311
	[0.395]	[639.413]	[0.800]	[527.288]	[0.821]	[418.851]	[0.709]	[126.018]	[0.884]
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	0.62	0.69	0.73	0.66	0.76	0.76	0.85	0.68	0.96
N	96112	94688	53678	89029	18072	94688	40130	94688	2553
Number of clusters	3028	3018	2083	3007	1012	3018	1739	3018	175

ID-clustered standard errors in parentheses

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 ($t \leq -2$)	-0.003	-17.734	-0.005	1.863	0.053	-20.802	-0.010	0.892	0.002
	(0.003)	(5.648)	(0.008)	(3.546)	(0.041)	(4.769)	(0.007)	(1.060)	(0.013)
Post-period ($t \geq 1$)	0.015	102.171	0.084	14.612	0.057	87.960	0.079	2.704	-0.011
	(0.005)	(8.875)	(0.012)	(6.000)	(0.063)	(6.804)	(0.011)	(1.766)	(0.019)
Post-pre difference	0.018	119.905	0.089	12.749	0.004	108.762	0.089	1.811	-0.013
	(0.006)	(10.211)	(0.015)	(6.986)	(0.071)	(7.893)	(0.013)	(1.834)	(0.028)
Pre-event average	0.069	440.767	6.130	52.746	6.630	379.852	6.068	8.991	6.285
	[0.253]	[468.034]	[0.734]	[266.643]	[0.803]	[419.674]	[0.709]	[99.730]	[0.887]
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	0.68	0.72	0.82	0.67	0.87	0.77	0.86	0.62	0.99
N	102085	100396	84209	98759	5757	100407	78963	100407	1548
Number of clusters	3005	2984	2784	2979	313	2984	2704	2984	96

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 and are reported in parentheses. Standard deviations are reported in hard brackets.

Table 3: Event studies (broad programmatic benefits)

Panel (a): RRH

	(1) Pecuniary ben.	(2) Nonpecuniary ben.	(3) Insurance ben.	(4) Any insurance	(5) HIV/AIDS services	(6) Mental health services	(7) Substance abuse services
Pre-period ($t \leq -2$)	-0.025	-0.010	-0.078	-0.073	-0.000	-0.006	0.007
	(0.007)	(0.007)	(0.007)	(0.008)	(0.001)	(0.005)	(0.003)
Post-period ($t \geq 1$)	0.066	-0.009	0.131	0.154	0.000	0.002	-0.013
	(0.009)	(0.010)	(0.010)	(0.010)	(0.002)	(0.007)	(0.004)
Post-pre difference	0.091	0.000	0.209	0.227	0.000	0.008	-0.020
	(0.012)	(0.012)	(0.012)	(0.013)	(0.002)	(0.008)	(0.005)
Pre-event average	0.402	0.458	0.577	0.560	0.009	0.112	0.054
	[0.490]	[0.498]	[0.494]	[0.496]	[0.093]	[0.315]	[0.225]
Month fixed effects	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X
Adj. R-squared	0.71	0.71	0.65	0.57	0.64	0.44	0.44
N	94688	90567	93040	94688	90801	90801	90801
Number of clusters	3018	2878	2979	3018	3028	3028	3028

ID-clustered standard errors in parentheses

Panel (b): PSH

	(1) Pecuniary ben.	(2) Nonpecuniary ben.	(3) Insurance ben.	(4) Any insurance	(5) HIV/AIDS services	(6) Mental health services	(7) Substance abuse services
Pre-period ($t \leq -2$)	-0.047	-0.029	-0.061	-0.058	-0.015	-0.085	-0.025
	(0.006)	(0.006)	(0.006)	(0.007)	(0.003)	(0.007)	(0.005)
Post-period ($t \geq 1$)	0.106	0.012	0.164	0.173	0.009	0.109	0.037
	(0.008)	(0.009)	(0.009)	(0.010)	(0.004)	(0.012)	(0.008)
Post-pre difference	0.153	0.042	0.225	0.231	0.024	0.194	0.062
	(0.010)	(0.011)	(0.010)	(0.011)	(0.005)	(0.013)	(0.009)
Pre-event average	0.685	0.537	0.545	0.526	0.057	0.328	0.102
	[0.465]	[0.499]	[0.498]	[0.499]	[0.232]	[0.470]	[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	0.63	0.68	0.65	0.59	0.58	0.47	0.44
N	100407	100023	99726	100407	96143	96143	96143
Number of clusters	2984	2976	2974	2984	3005	3005	3005

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.

Table 4: 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 ($t \leq -2$)	-0.004	-0.005	0.000	-0.011	-0.039	-0.001	-0.071	-0.001
	(0.004)	(0.003)	(0.002)	(0.004)	(0.007)	(0.002)	(0.008)	(0.001)
Post-period ($t \geq 1$)	0.025	0.005	0.005	0.030	0.055	-0.002	0.101	-0.002
	(0.005)	(0.003)	(0.002)	(0.006)	(0.008)	(0.003)	(0.010)	(0.002)
Post-pre difference	0.030	0.010	0.005	0.040	0.094	-0.001	0.173	-0.001
	(0.006)	(0.004)	(0.003)	(0.007)	(0.010)	(0.004)	(0.012)	(0.002)
Pre-event average	0.103	0.039	0.012	0.106	0.333	0.023	0.421	0.006
	[0.304]	[0.193]	[0.110]	[0.307]	[0.471]	[0.151]	[0.494]	[0.080]
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	0.69	0.58	0.80	0.83	0.75	0.74	0.60
N	94680	94684	94688	94658	87120	87120	93040	87120
Number of clusters	3018	3018	3018	3018	2767	2767	2979	2767

ID-clustered standard errors in parentheses

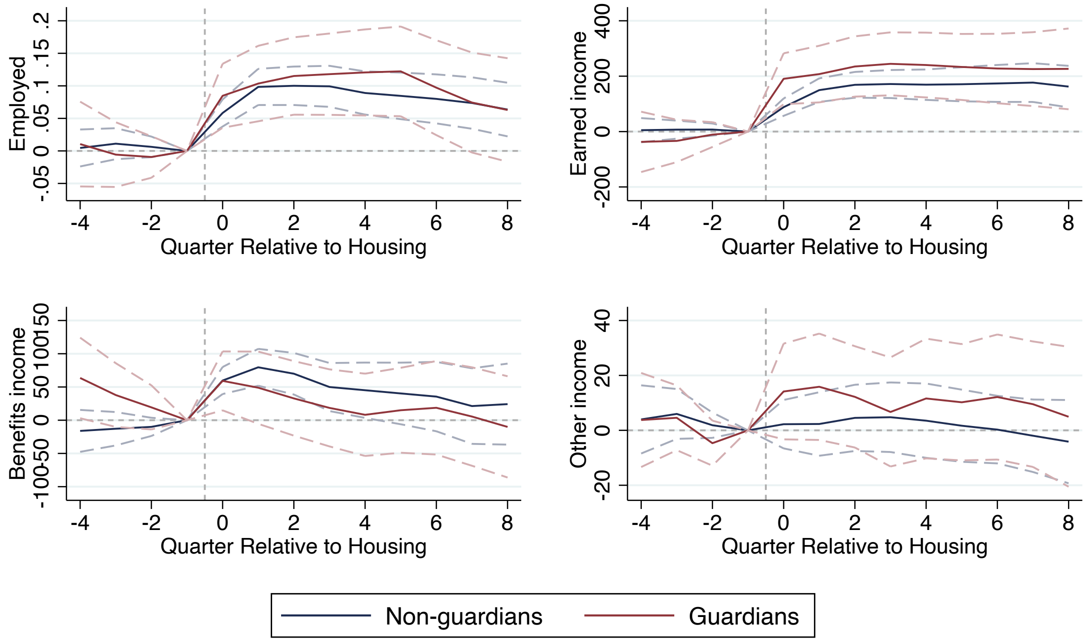
Panel (b): PSH

	(1) SSI	(2) SSDI	(3) Unemployment ben.	(4) TANF	(5) SNAP	(6) WIC	(7) Medicaid	(8) Other TANF
Pre-period ($t \leq -2$)	-0.016	-0.004	0.000	-0.001	-0.033	0.000	-0.060	-0.000
	(0.004)	(0.003)	(0.002)	(0.001)	(0.006)	(0.001)	(0.007)	(0.000)
Post-period ($t \geq 1$)	0.065	0.010	-0.002	0.004	0.057	0.002	0.157	0.001
	(0.007)	(0.004)	(0.002)	(0.002)	(0.007)	(0.001)	(0.009)	(0.001)
Post-pre difference	0.082	0.013	-0.003	0.006	0.091	0.002	0.217	0.001
	(0.008)	(0.005)	(0.002)	(0.003)	(0.009)	(0.002)	(0.011)	(0.001)
Pre-event average	0.218	0.058	0.009	0.022	0.437	0.005	0.471	0.001
	[0.413]	[0.233]	[0.094]	[0.146]	[0.496]	[0.068]	[0.499]	[0.035]
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	0.79	0.72	0.47	0.82	0.80	0.76	0.67	0.64
N	100404	100382	100407	100395	97400	97400	99726	97400
Number of clusters	2984	2984	2984	2984	2899	2899	2974	2899

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 5: Labor and earnings responses by guardian-status
Panel (a): RRH

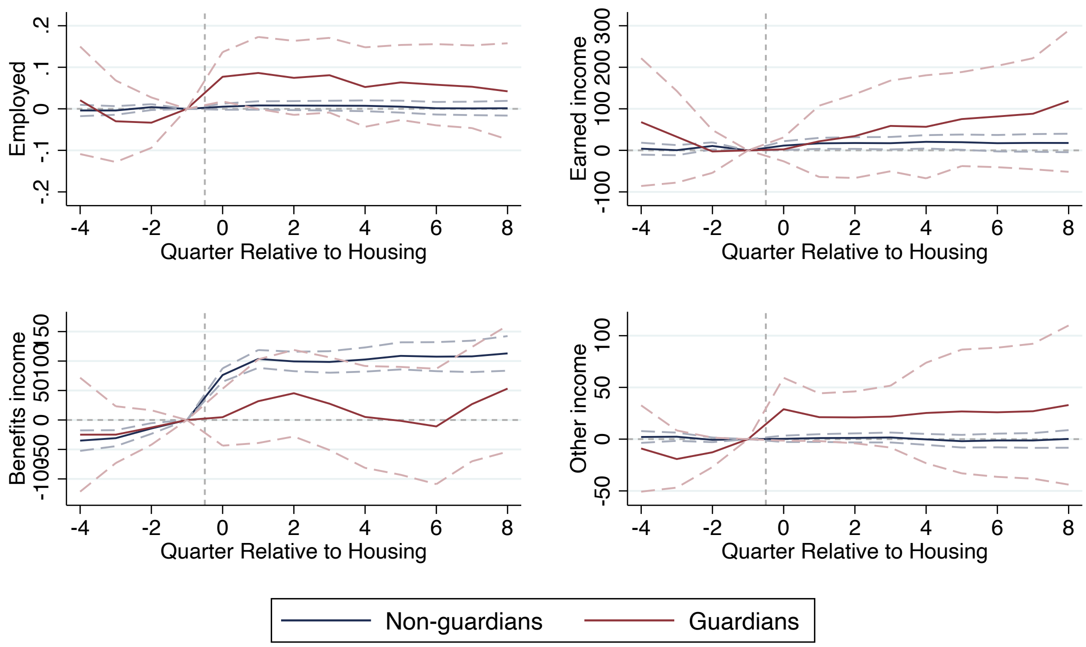


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. “Guardian” status refers to whether or not the individual has children. Timing is binned up to 5 quarters prior to and 9 quarters since each individual’s housing event; these bins are omitted from the coefficient display. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure 5: Labor and earnings responses by guardian-status
Panel (b): PSH

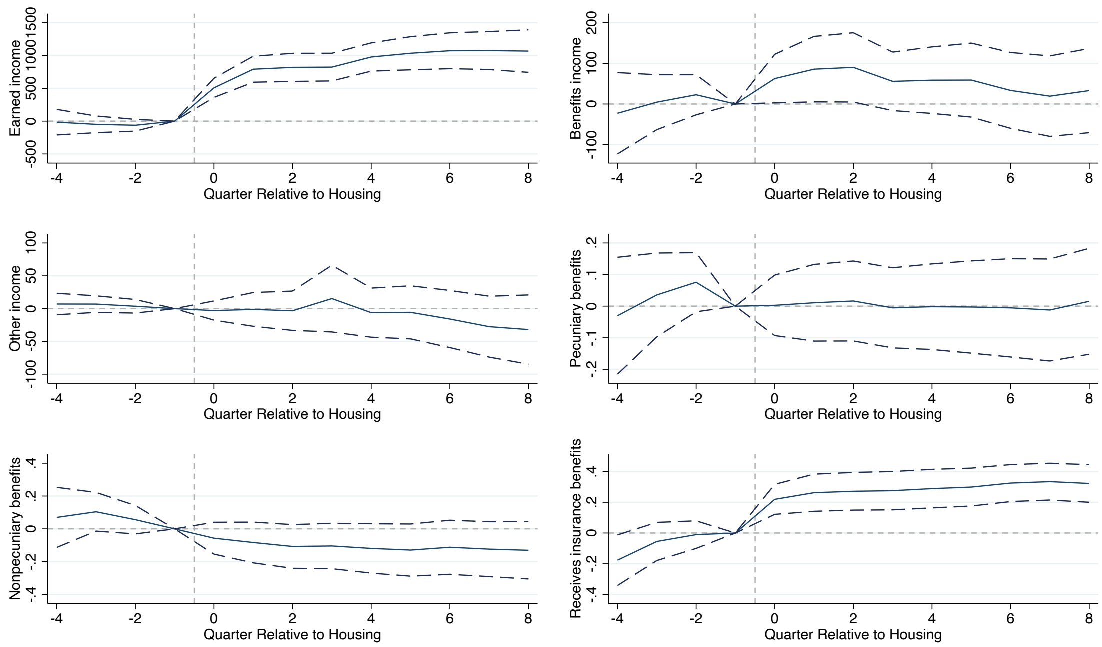


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Guardian" status refers to whether or not the individual has children. 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: RRH U2E Transitions

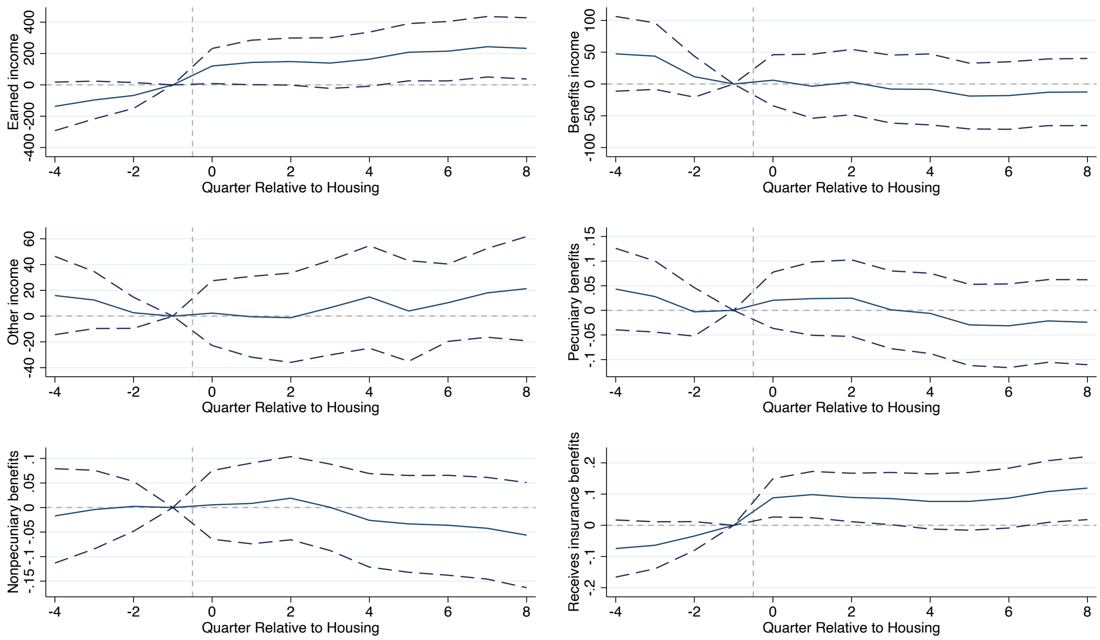


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 7: RRH E2E Transitions

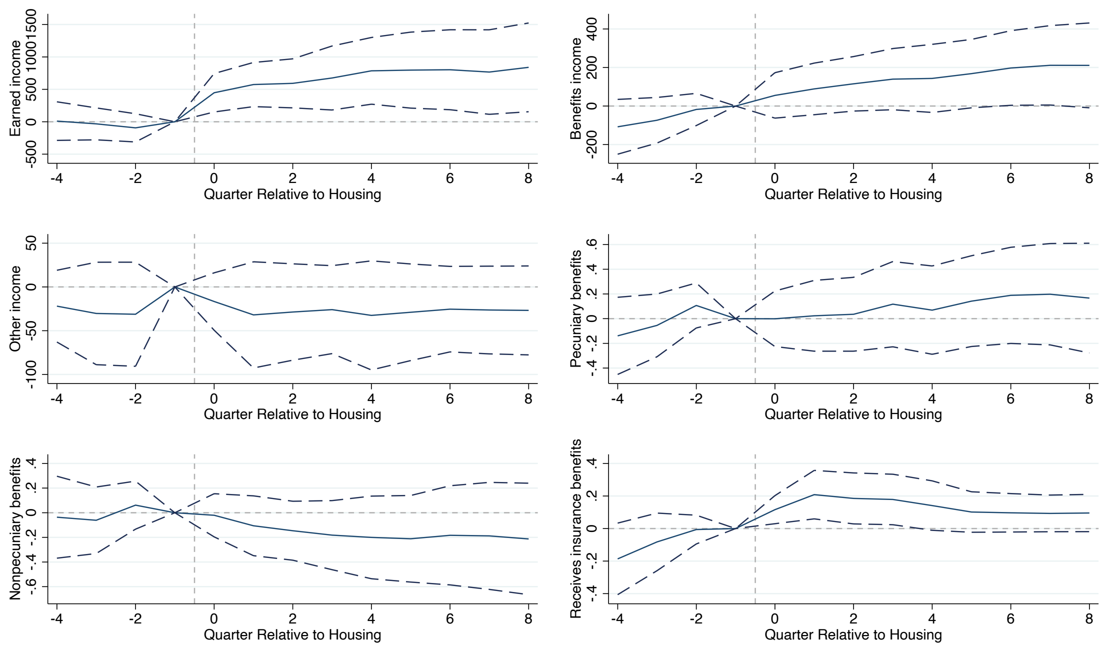


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 8: PSH U2E Transitions

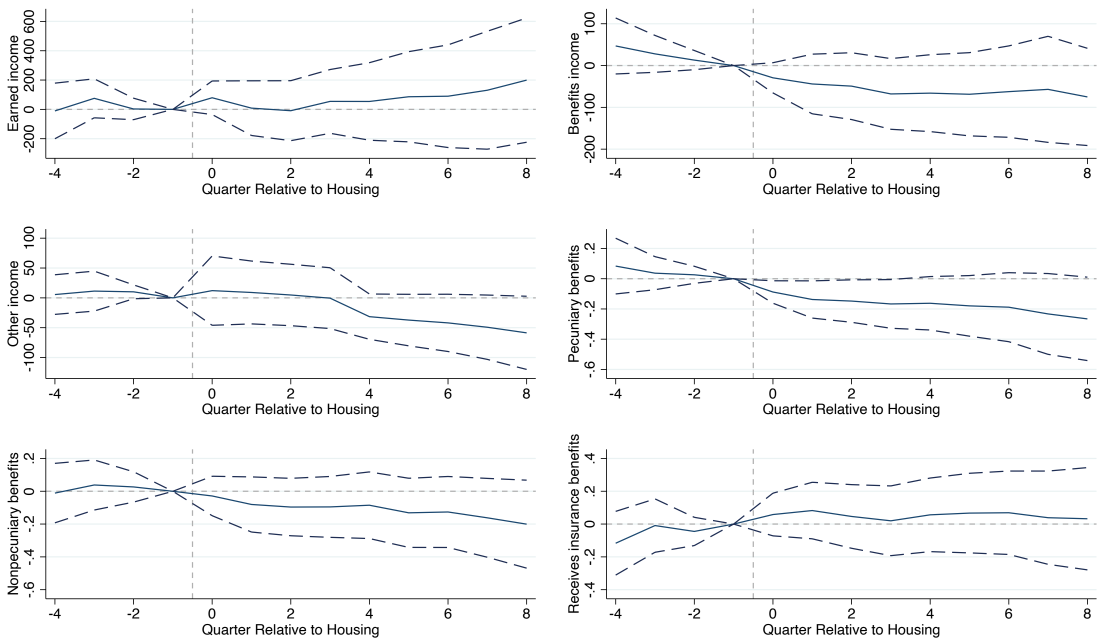


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 9: PSH E2E Transitions



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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.

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

Panel (a): RRH

Transition	% of recipients	Annual inc. (pre)	Annual inc. (post)	P-stat (difference)	ΔT	% offset
U2E	3.34	82.67	13672.5	0	1970.07	10.37
E2E	7.93	11905.09	17409.41	0	993.8	5.23
U2U	58.55	91.48	106.84	0.78	-0.55	0
E2U	2.34	8155.69	389.04	0	-822.29	-4.33
Other	27.84	4098.1	6763.21	0	258.69	1.36
Total	100	-	-	-	197.06	1.04

Panel (b): PSH

Transition	% of recipients	Annual inc. (pre)	Annual inc. (post)	P-stat (difference)	ΔT	% offset
U2E	0.86	187.02	12063.14	0	1630.37	8.58
E2E	1.56	11036.78	12176.31	0.496	233.63	1.23
U2U	84.9	39.87	19.13	0.164	-3.11	-0.02
E2U	0.86	10031.39	181.92	0	-1215.22	-6.4
Other	11.81	3344.06	4471.52	0.022	106.42	0.56
Total	100	-	-	-	17.14	0.09

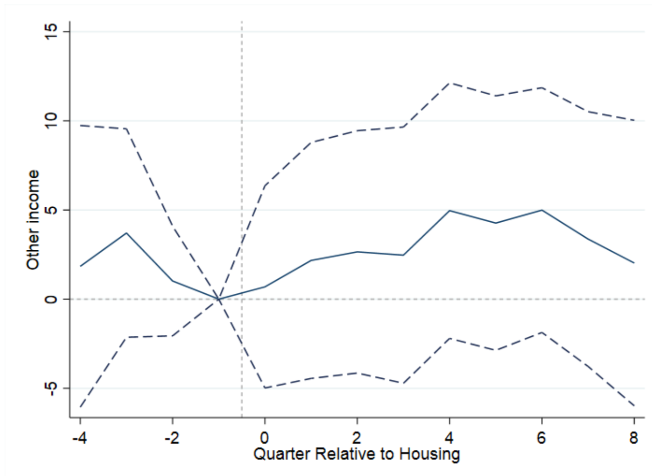
This table displays tabulations for a cost-benefit calculation of UH policies through the labor market channel during program tenure. Each row corresponds with a different ex-post employment transition type. Employment transition types based on employment status at least 80% of the respective event-period. 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.

Appendix A Additional figures and tables

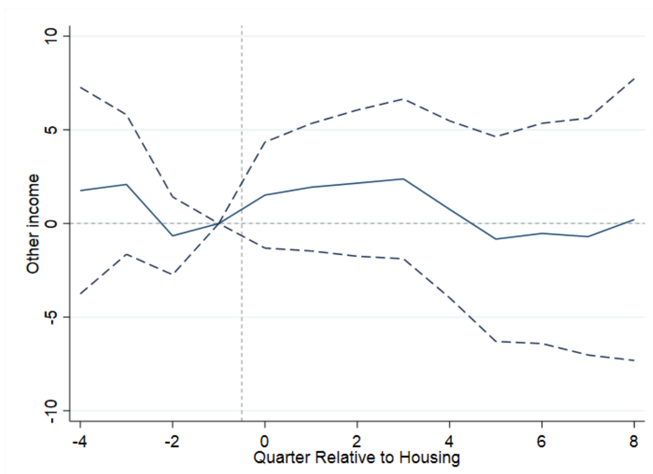
A.1 Additional central specification results

Figure A.1: Event study results: other income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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 by month. 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 A.2: Additional income and broad programmatic outcomes:
Panel (a): RRH

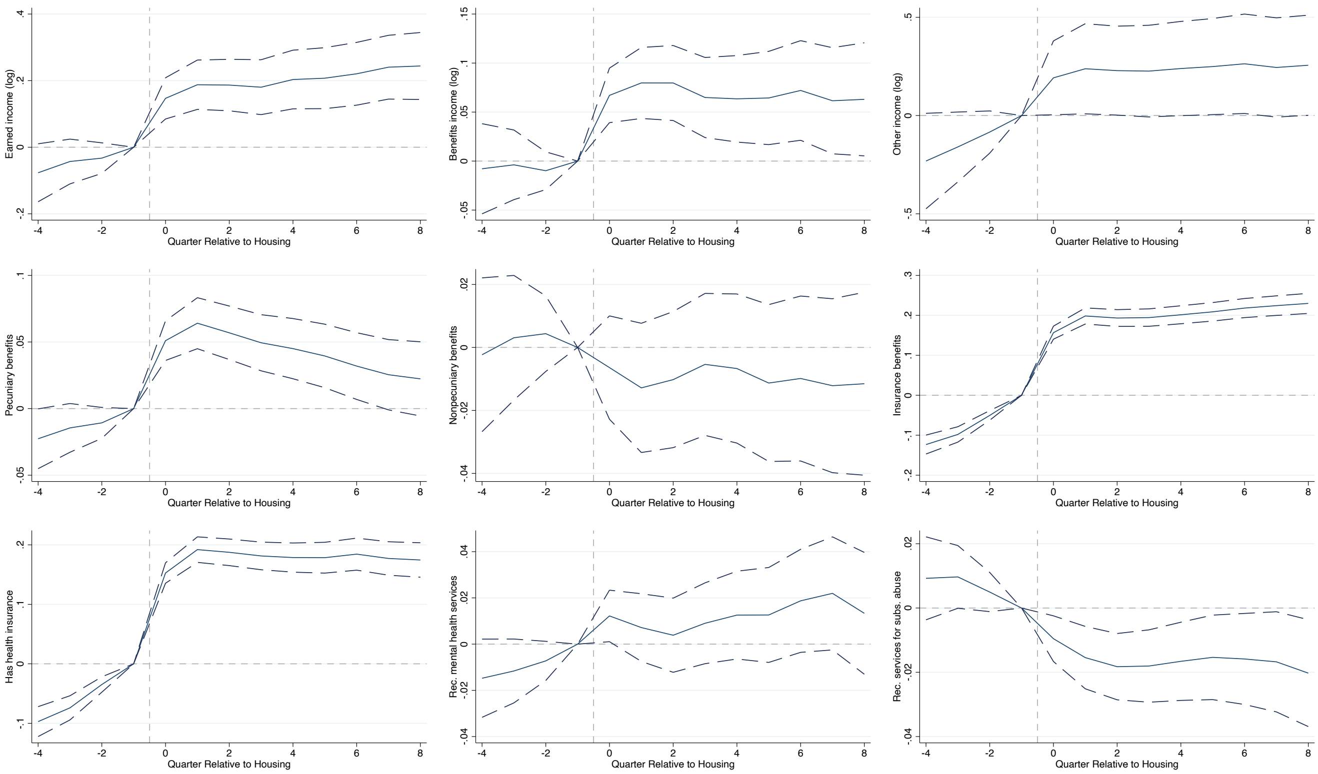
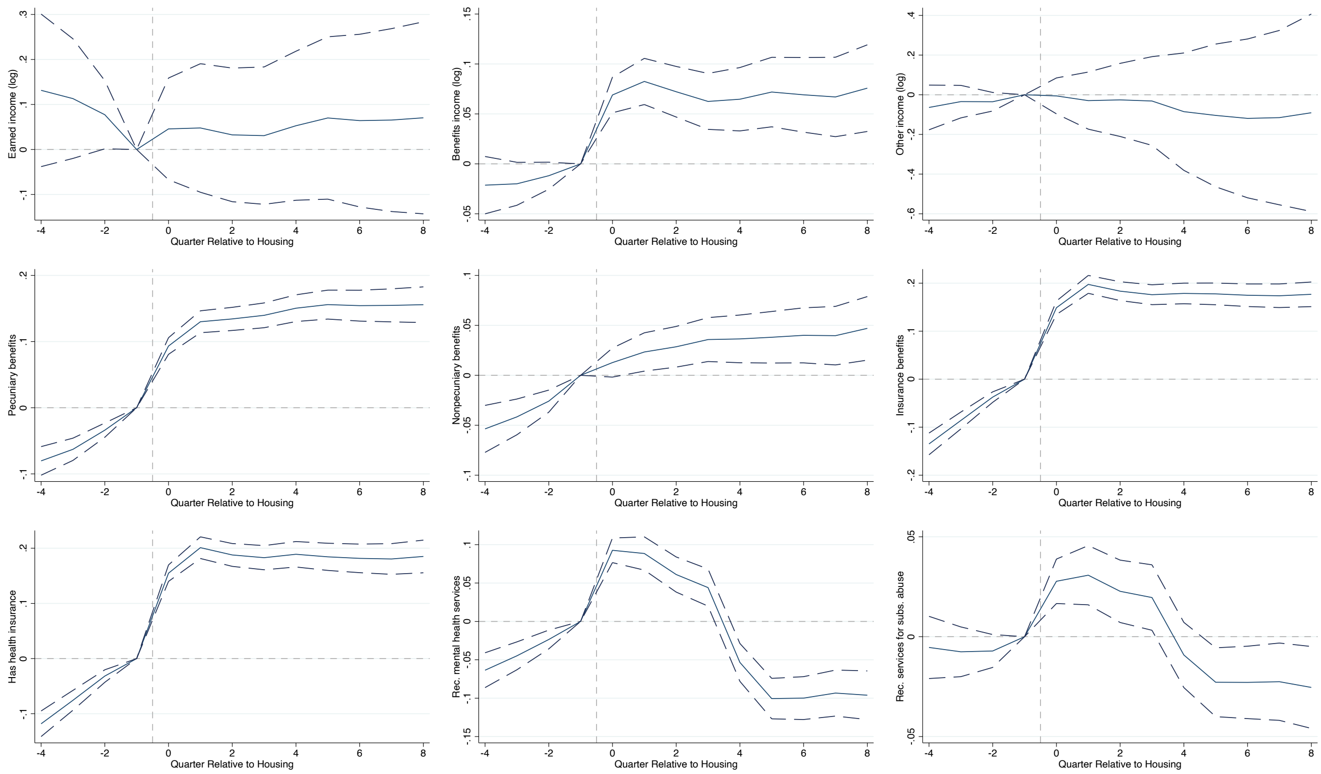


Figure A.2: Additional income and broad programmatic outcomes:
Panel (b): PSH



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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 by month. Panel (b) shows the results for Permanent Supportive Housing by month.

Figure A.3: Additional specific programmatic outcomes:
Panel (a): RRH

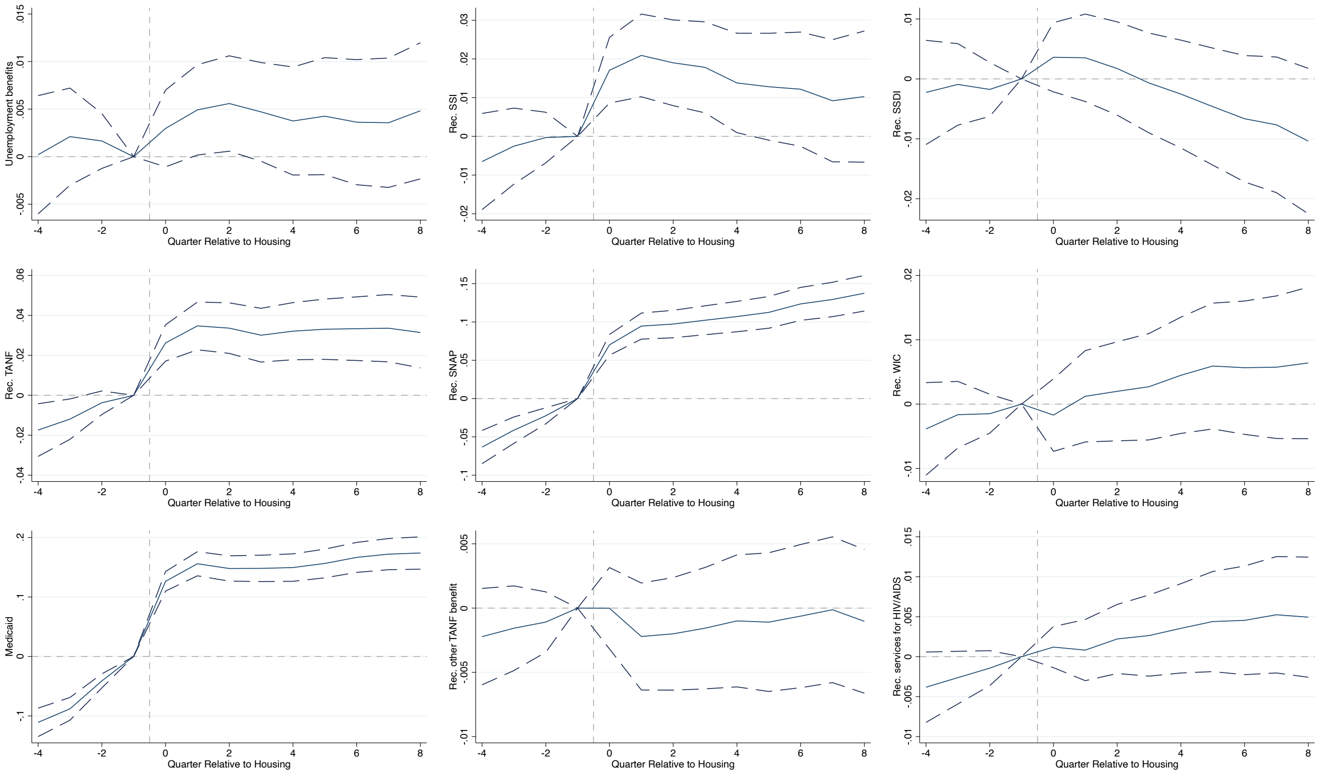
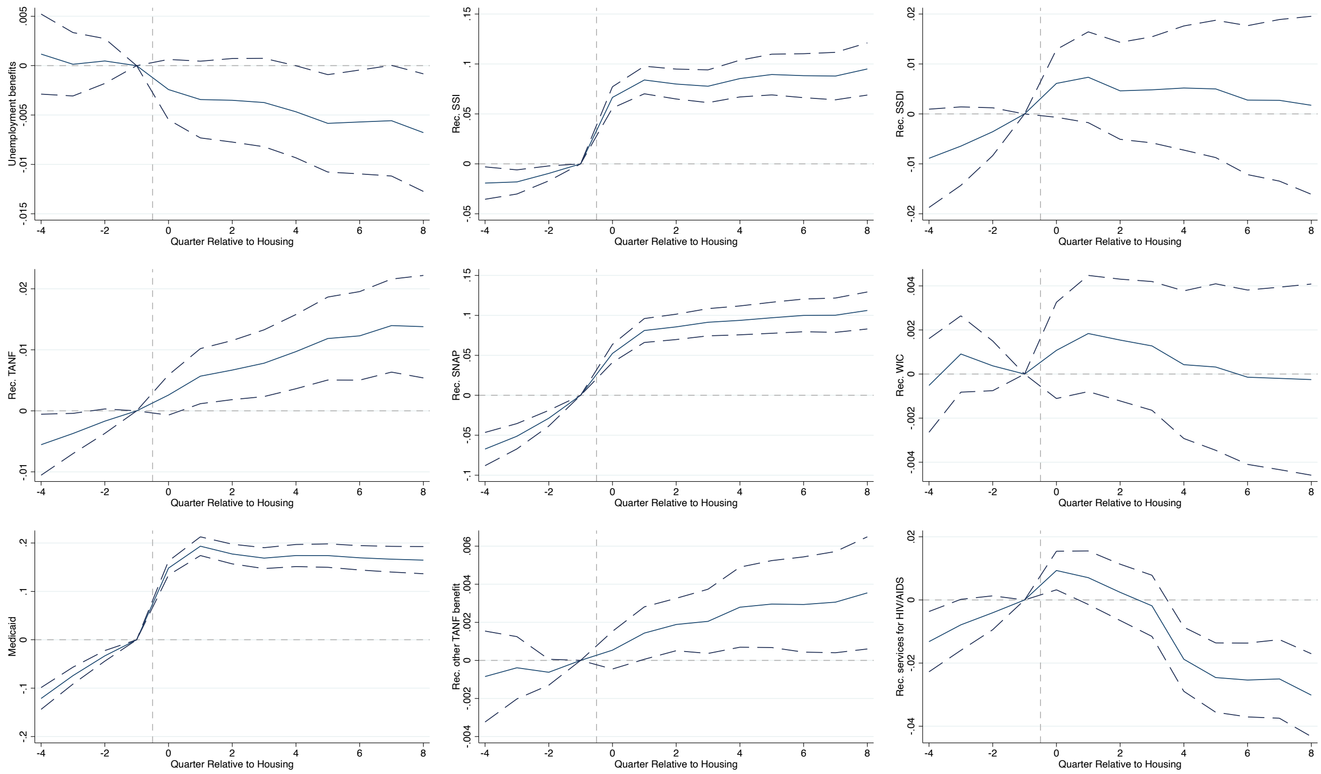


Figure A.3: Additional specific programmatic outcomes:
Panel (b): PSH



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

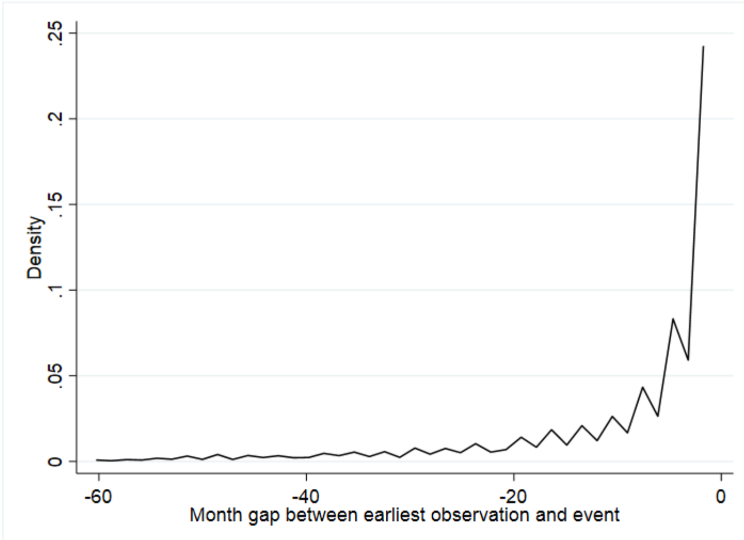
$$y_{it} = \alpha_i + \delta_t + \sum_{q(j)\neq -1} \beta_{q(j)} \mathbb{1}\{EventTime_{q(t(i))} = q(j)\} + \varepsilon_{it}.$$

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 by month. Panel (b) shows the results for Permanent Supportive Housing by month.

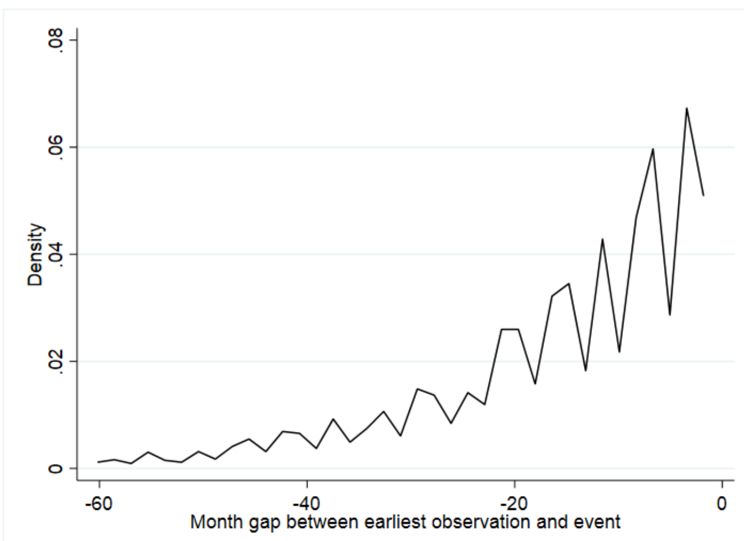
A.2 Additional metadata

Figure A.4: Frequency of gap between housing event and earliest observation

(a) Rapid Re-Housing



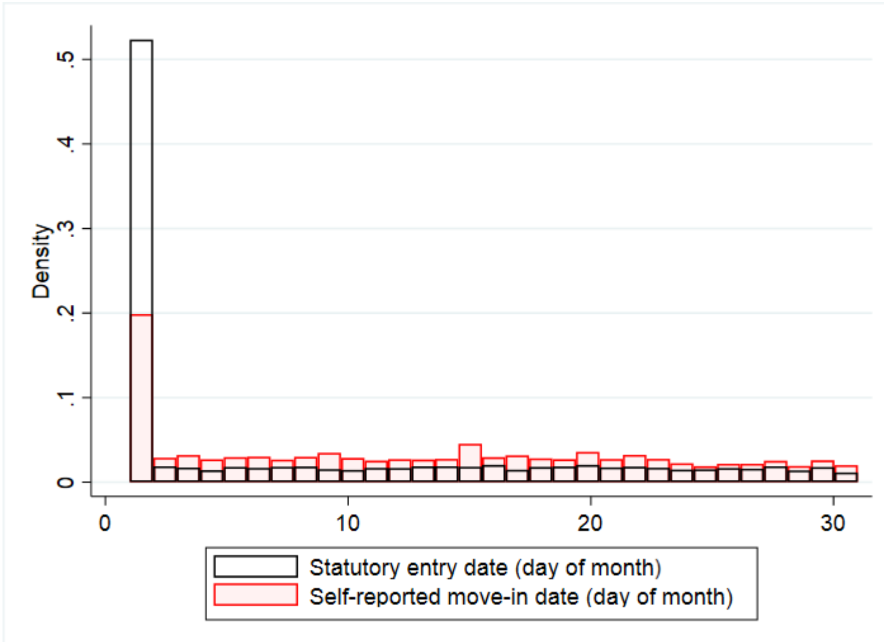
(b) Permanent Supportive Housing



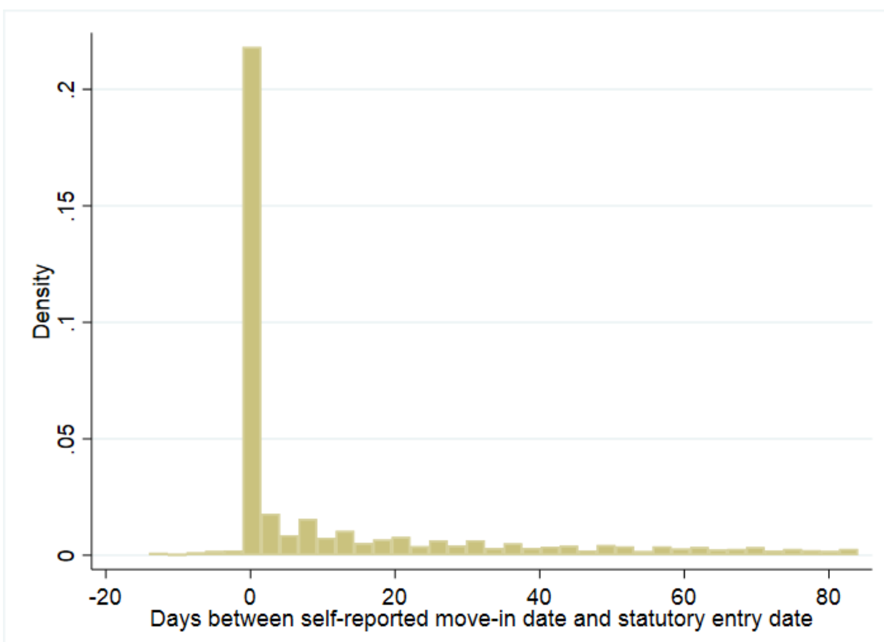
These histograms plot the relative frequency of the time between an individual's housing event and their earliest observation in the HMIS data. For each individual housing recipient, this gap is calculated as $Housing\ event\ month_i - Earliest\ observation\ month_i$. Panel (a) displays this relationship for Rapid Re-Housing recipients, and Panel (b) studies PSH recipients.

Figure A.5: Housing recipiency discrepancy in entry date

(a) Reported day of month



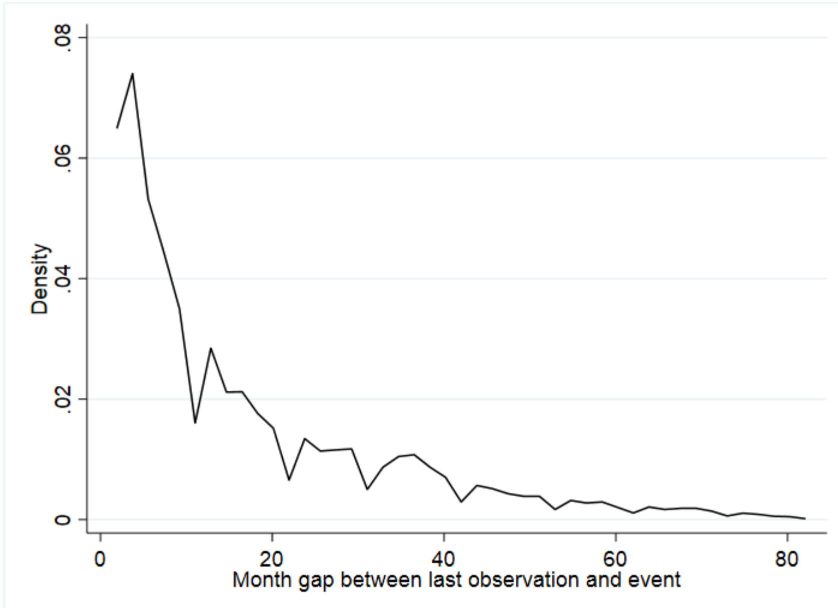
(b) Observed difference between reported entry date and move-in date



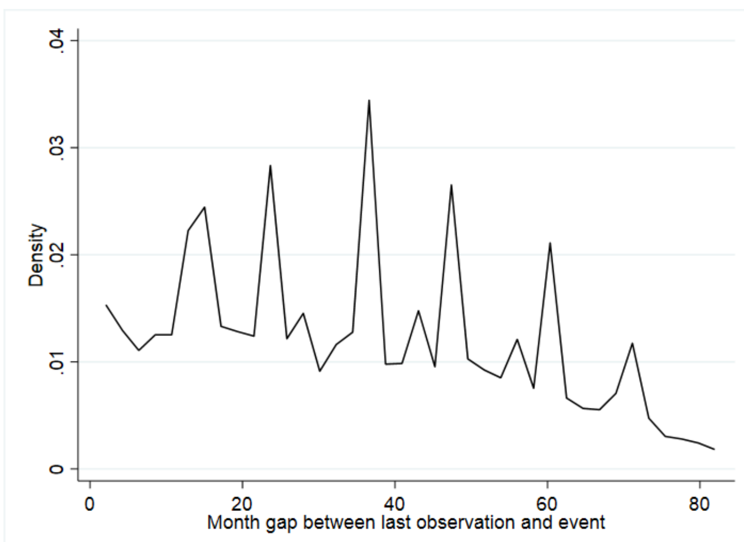
These figures demonstrate the discrepancy between the reported date of entry into unconditional housing accommodations and the potentially true date of entry for housing recipiency events in our sample. Panel (a) plots the relative frequency of date of the month of entry as reported by case worker (statutory)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

Figure A.6: Frequency of gap between housing event and latest observation

(a) Rapid Re-Housing



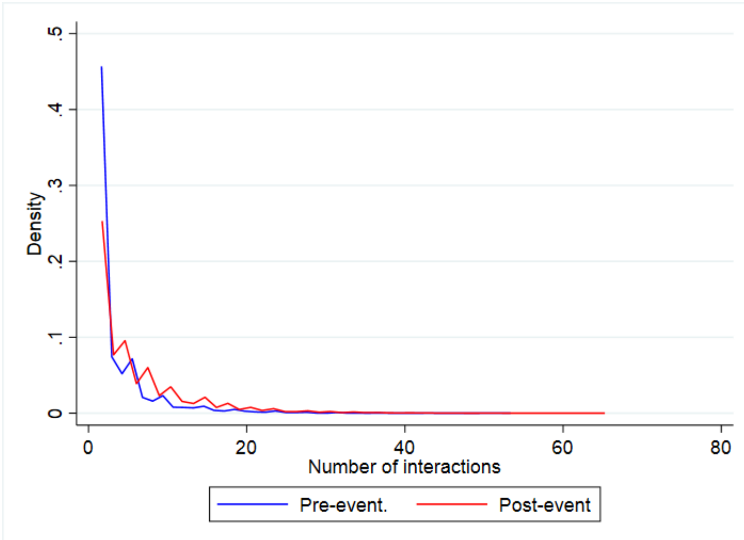
(b) Permanent Supportive Housing



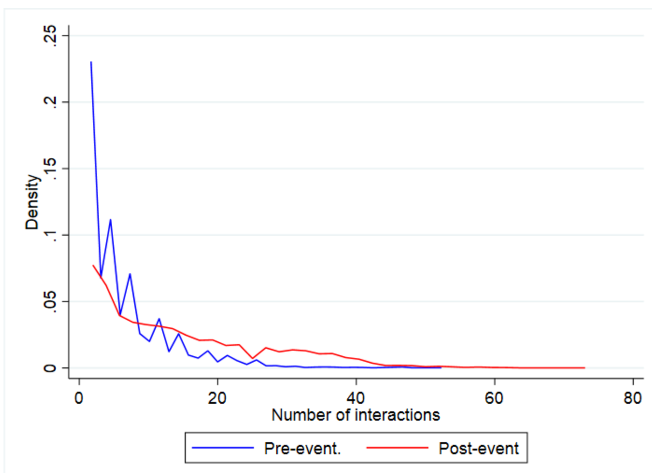
These histograms plot the relative frequency of the time between an individual's housing event and their final observation in the HMIS data. For each individual housing recipient, this gap is calculated as $Latest\ observation\ month_i - Housing\ event\ month_i$. Panel (a) displays this relationship Rapid Re-Housing, and Panel (b) studies PSH recipients.

Figure A.7: Frequency of number of observations pre- and post-event

(a) Rapid Re-Housing

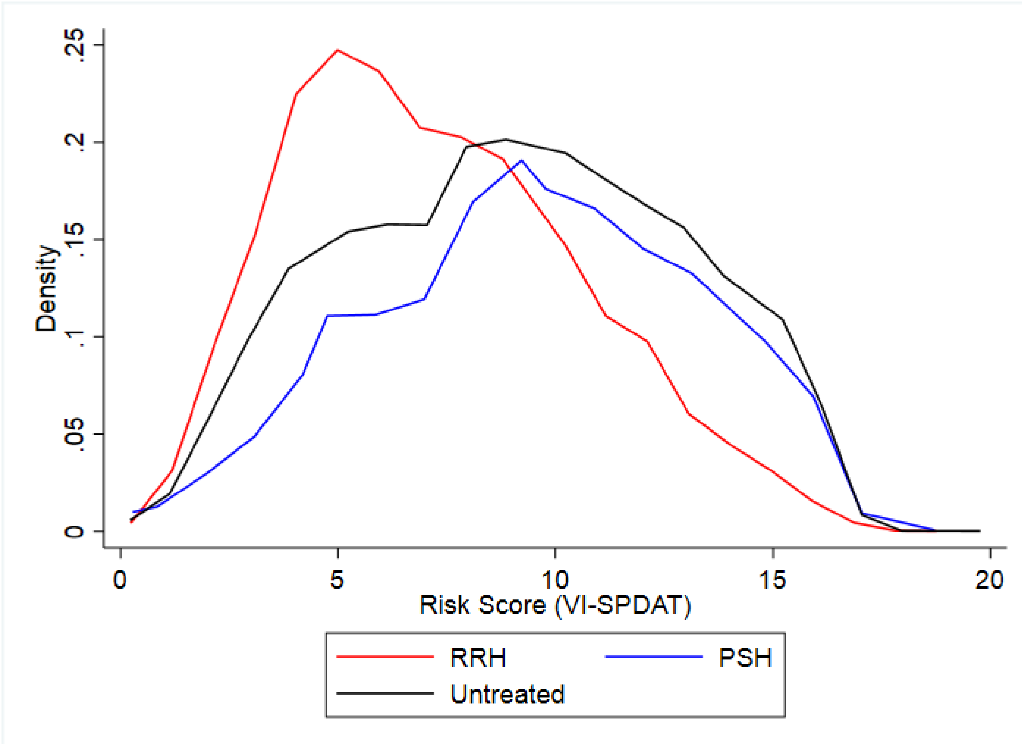


(b) Permanent Supportive Housing



These histograms plot the relative frequency of the number of interactions for each individual, stratifying by pre- and post-event interactions. Panel (a) displays this relationship for Rapid Re-Housing recipients, and Panel (b) studies PSH recipients. Event time is measured in months relative to placement into Housing First.

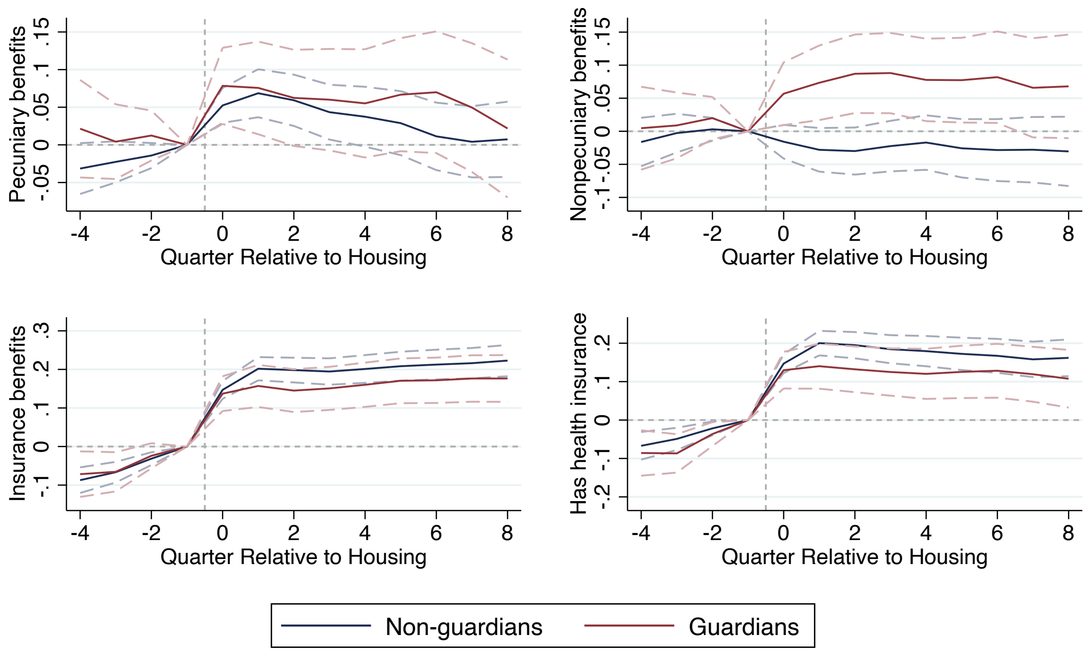
Figure A.8: Distribution of VI-SPDAT (risk) scores



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

A.3 Additional graphs on heterogeneity by guardian status

Figure A.9: Benefits & programmatic responses by guardian-status
Panel (a): RRH

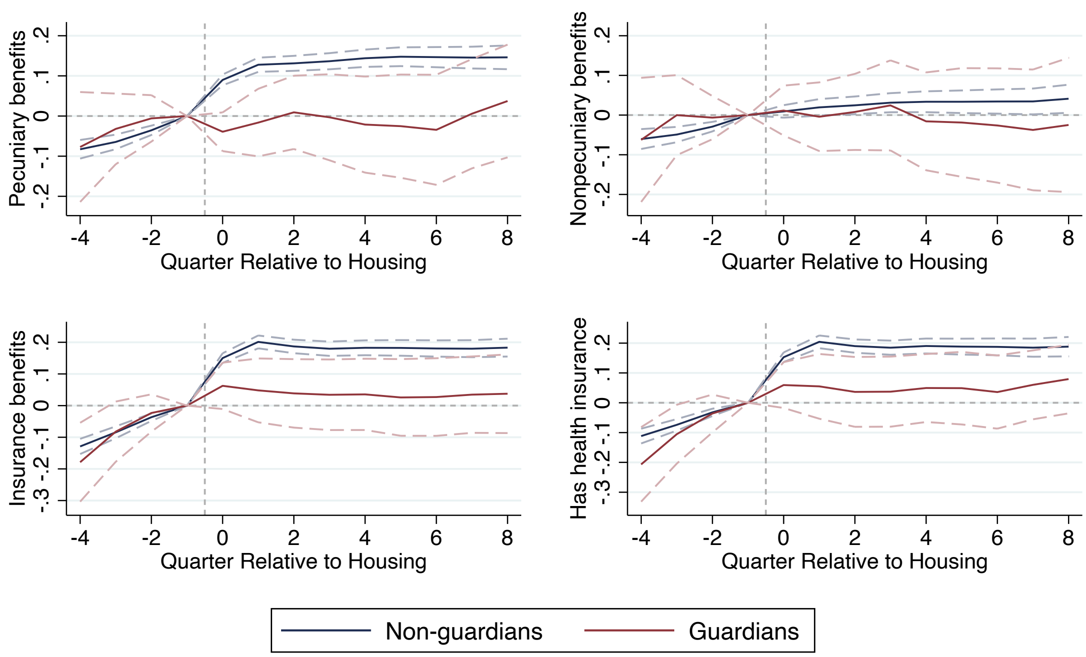


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Guardian" status refers to whether or not the individual has children. 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 A.9: Benefits & programmatic responses by guardian-status
Panel (b): PSH



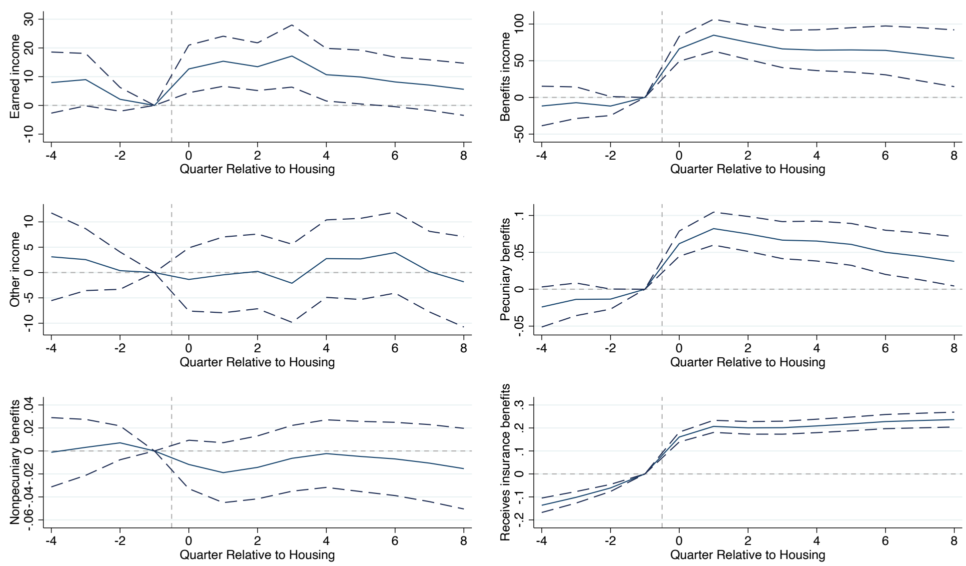
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Guardian" status refers to whether or not the individual has children. 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.

A.4 Additional results for U2U, E2U, and unassigned transitions

Figure A.10: RRH U2U Transitions

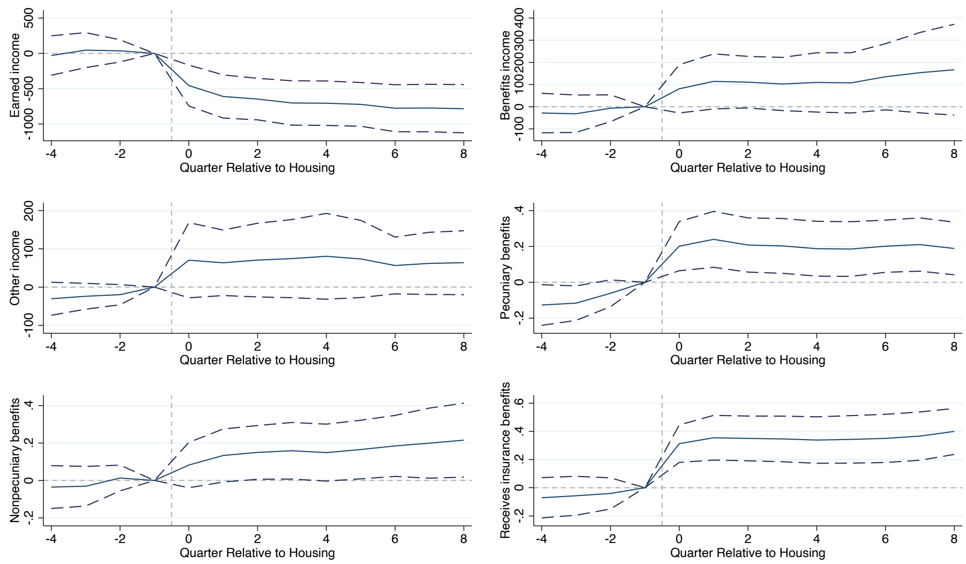


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 A.11: RRH E2U Transitions

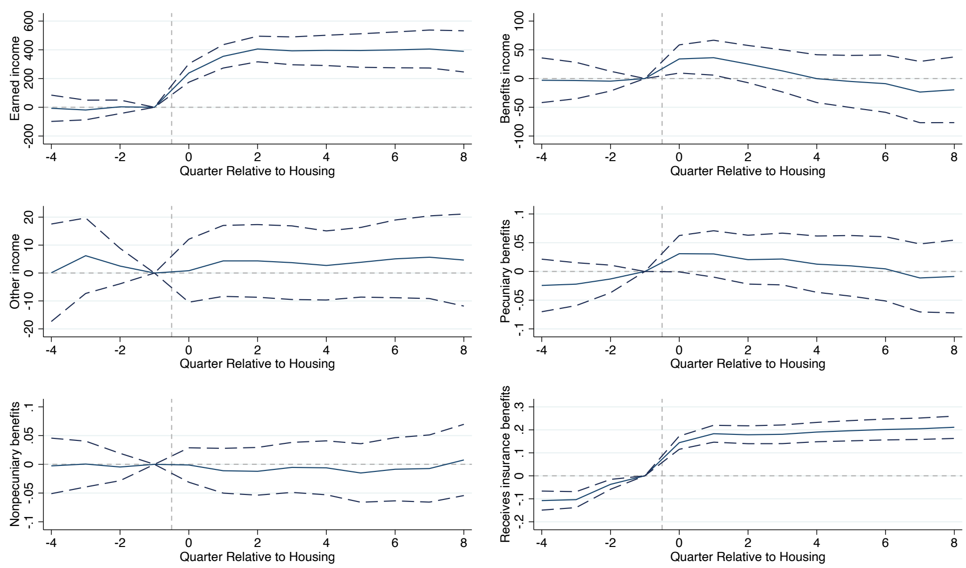


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 A.12: RRH "None" Transitioners

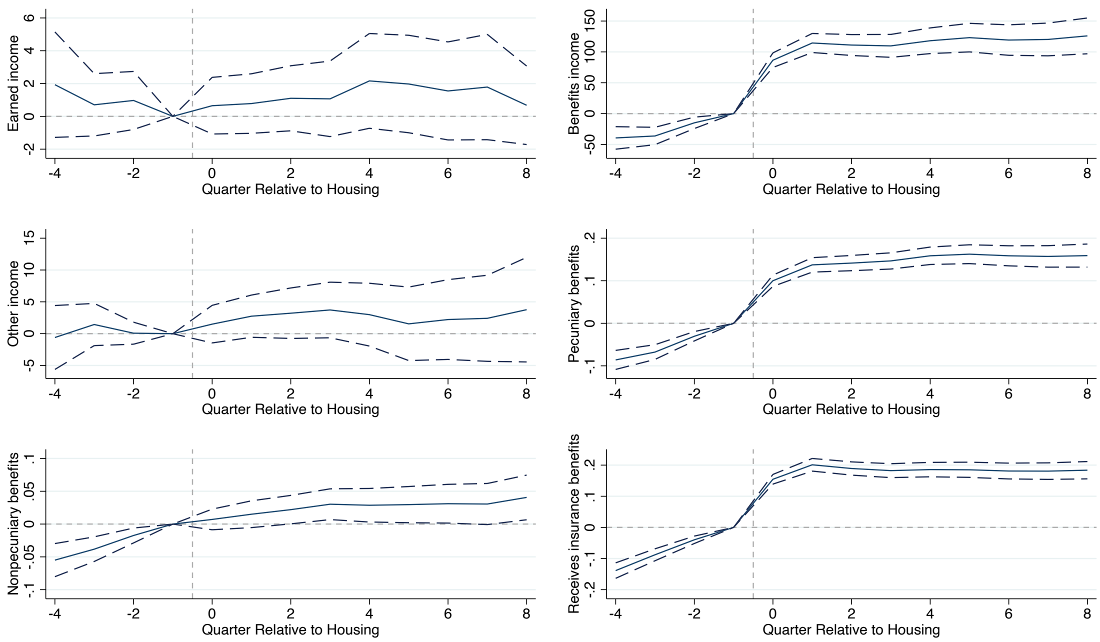


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 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.

Figure A.13: PSH U2U Transitions

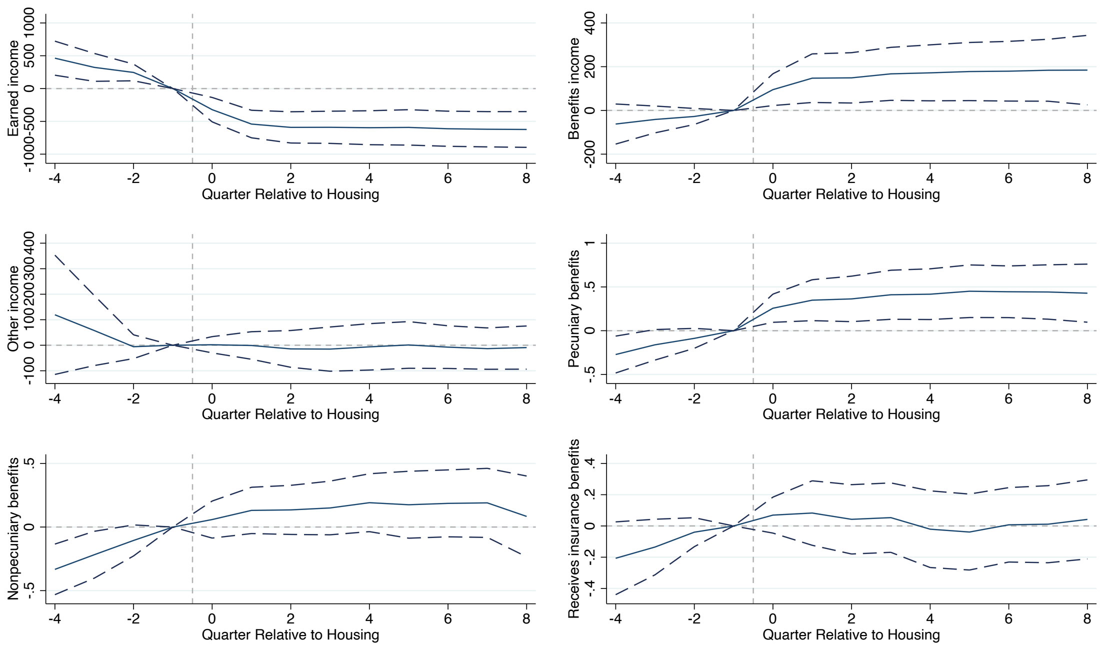


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 A.14: PSH E2U Transitions

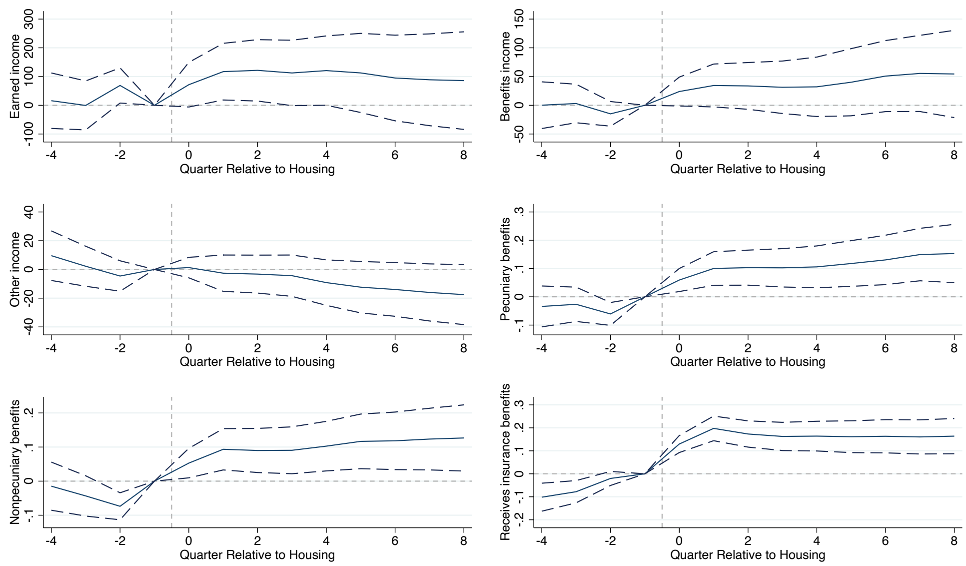


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 A.15: PSH “None” Transitioners



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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.

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

Panel (a): 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

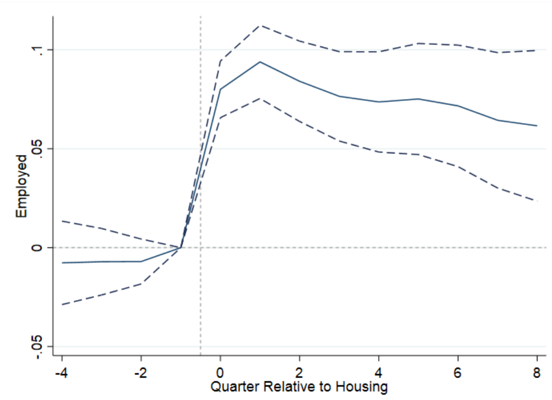
This table displays the coefficients from cross-sectional regressions of the form $y_{it} = \beta_0 + \Gamma X_{it} + e_{it}$ on the sample of Permanent Supportive Housing (PSH) recipients entering between January 2014 and February 2018. Each of dependent variables in each corresponds with a different 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.

Appendix B Robustness: figures and tables

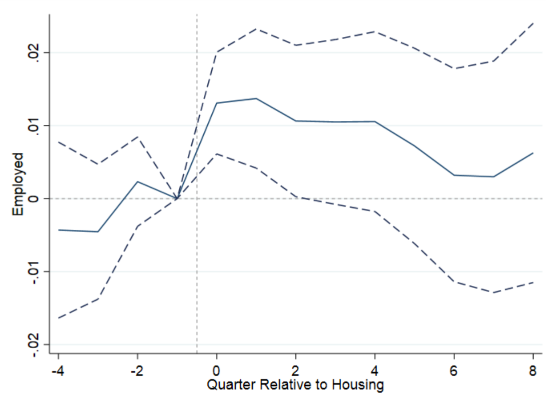
B.1 Main specification results without interpolation

Figure B.1: Event study results: employment (extensive margin)

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



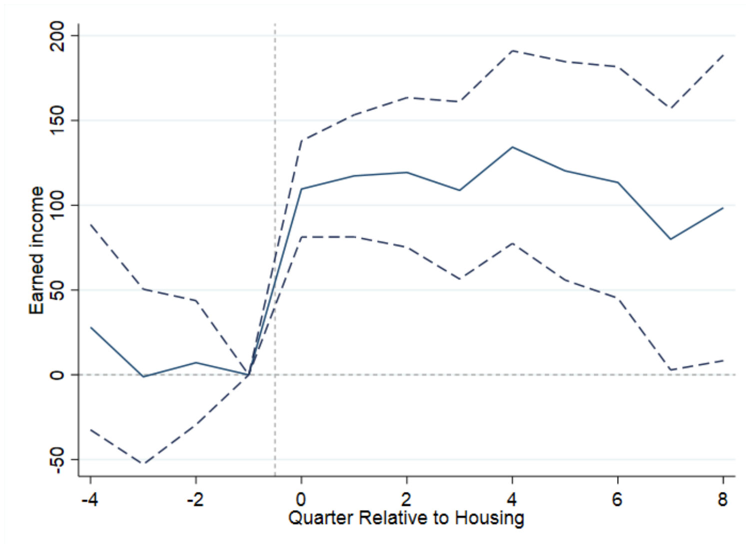
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

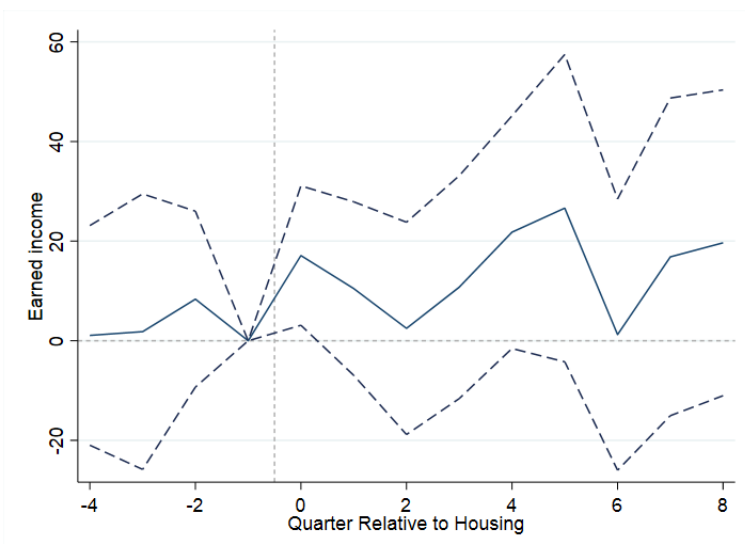
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing by month. 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.2: Event study results: earned income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



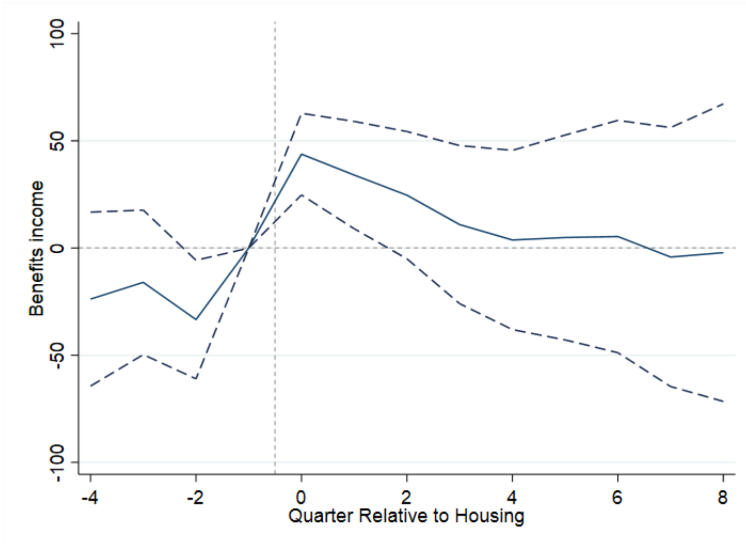
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

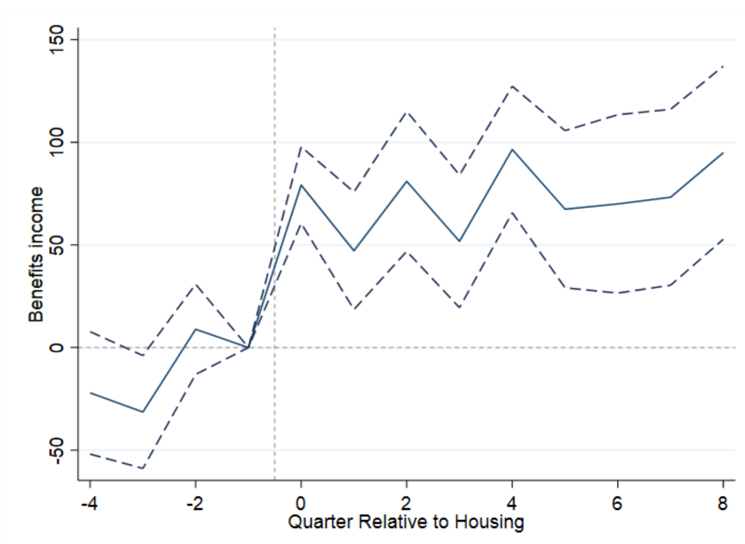
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 5 quarters prior to and 9 quarters since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing by month. 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.3: Event study results: benefits income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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. "Benefits income" refers to pecuniary support received on part of SSI, SSDI, unemployment benefits, TANF, Veteran Affairs assistance, Social Security, and General Assistance from LAHSA organisms. 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 by month. 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.

Table B.1: 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	-74.806	-0.109	-73.373	-0.109	-4.745	-0.034	0.704	-0.089
	(0.007)	(24.361)	(0.032)	(21.050)	(0.048)	(13.574)	(0.021)	(3.778)	(0.042)
Post-period ($t \geq 1$)	0.083	115.843	0.068	61.210	0.043	40.870	0.008	5.538	-0.068
	(0.009)	(25.752)	(0.031)	(22.406)	(0.046)	(13.395)	(0.021)	(3.587)	(0.053)
Post-pre difference	0.104	190.648	0.177	134.583	0.152	45.615	0.043	4.834	0.021
	(0.011)	(19.211)	(0.026)	(17.010)	(0.041)	(10.898)	(0.023)	(3.879)	(0.047)
Pre-event average	0.156	471.604	6.455	177.771	6.853	286.565	6.230	15.573	6.243
	[0.363]	[620.307]	[0.780]	[505.999]	[0.718]	[431.778]	[0.713]	[136.575]	[0.906]
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	0.57	0.59	0.65	0.56	0.58	0.71	0.82	0.62	0.94
N	90815	20473	12021	19408	3838	20473	9050	20473	501
Number of clusters	3028	3010	2003	2982	858	3010	1603	3010	124

ID-clustered standard errors in parentheses

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 ($t \leq -2$)	-0.001	-15.316	0.003	-22.557	-0.106	-0.386	0.021	1.503	0.045
	(0.003)	(15.097)	(0.020)	(8.681)	(0.105)	(12.892)	(0.018)	(2.587)	(0.079)
Post-period ($t \geq 1$)	0.017	36.802	0.013	-15.021	-0.244	48.985	0.033	-0.335	-0.071
	(0.005)	(16.360)	(0.023)	(9.793)	(0.130)	(13.972)	(0.020)	(2.842)	(0.075)
Post-pre difference	0.018	52.118	0.011	7.537	-0.138	49.370	0.012	-1.838	-0.116
	(0.006)	(12.690)	(0.018)	(7.227)	(0.122)	(10.739)	(0.016)	(3.516)	(0.091)
Pre-event average	0.050	459.505	6.111	52.415	6.609	398.903	6.050	9.155	6.264
	[0.218]	[471.127]	[0.744]	[263.497]	[0.783]	[424.576]	[0.720]	[104.444]	[1.010]
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	0.60	0.59	0.75	0.51	0.71	0.68	0.80	0.41	0.98
N	99680	16451	13668	16203	788	16453	12839	16453	161
Number of clusters	3005	2923	2648	2906	217	2923	2535	2923	51

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 and are reported in parentheses. Standard deviations are reported in hard brackets. These specifications do not interpolate dependent variables between housing recipients' interactions with the HMIS and related systems.

Table B.2: 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 ($t \leq -2$)	-0.010	-0.029	-0.043	-0.021	-0.001	-0.006	0.007
	(0.016)	(0.016)	(0.014)	(0.016)	(0.001)	(0.005)	(0.003)
Post-period ($t \geq 1$)	0.045	-0.012	0.031	0.068	0.001	0.004	-0.012
	(0.016)	(0.016)	(0.014)	(0.016)	(0.002)	(0.007)	(0.004)
Post-pre difference	0.054	0.017	0.075	0.089	0.001	0.010	-0.019
	(0.012)	(0.013)	(0.011)	(0.012)	(0.002)	(0.008)	(0.005)
Pre-event average	0.448	0.456	0.735	0.689	0.009	0.112	0.053
	[0.497]	[0.498]	[0.442]	[0.463]	[0.093]	[0.315]	[0.225]
Month fixed effects	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X
Adj. R-squared	0.64	0.64	0.53	0.45	0.60	0.44	0.43
N	20473	17459	18134	20473	96671	96671	96671
Number of clusters	3010	2674	2890	3010	3028	3028	3028

ID-clustered standard errors in parentheses

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 ($t \leq -2$)	-0.037	-0.027	-0.045	-0.013	-0.013	-0.078	-0.022
	(0.016)	(0.017)	(0.016)	(0.017)	(0.003)	(0.007)	(0.005)
Post-period ($t \geq 1$)	0.062	0.001	0.050	0.078	0.009	0.105	0.037
	(0.017)	(0.018)	(0.017)	(0.019)	(0.004)	(0.012)	(0.008)
Post-pre difference	0.099	0.028	0.095	0.092	0.023	0.183	0.059
	(0.012)	(0.014)	(0.012)	(0.013)	(0.005)	(0.013)	(0.009)
Pre-event average	0.727	0.558	0.624	0.595	0.057	0.328	0.102
	[0.446]	[0.497]	[0.484]	[0.491]	[0.232]	[0.470]	[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	0.47	0.57	0.52	0.44	0.59	0.48	0.45
N	16453	15657	15170	16453	102206	102206	102206
Number of clusters	2923	2882	2863	2923	3005	3005	3005

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. These specifications do not interpolate dependent variables between housing recipients' interactions with the HMIS and related systems.

Table B.3: 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 ($t \leq -2$)	0.005	-0.005	0.000	-0.007	-0.054	0.000	-0.035	-0.005
	(0.008)	(0.006)	(0.004)	(0.007)	(0.015)	(0.004)	(0.015)	(0.004)
Post-period ($t \geq 1$)	0.023	0.002	0.004	0.023	-0.001	-0.004	0.007	-0.007
	(0.008)	(0.006)	(0.004)	(0.008)	(0.014)	(0.004)	(0.015)	(0.005)
Post-pre difference	0.019	0.007	0.003	0.030	0.052	-0.004	0.042	-0.002
	(0.007)	(0.005)	(0.003)	(0.007)	(0.013)	(0.005)	(0.011)	(0.004)
Pre-event average	0.118	0.043	0.010	0.109	0.426	0.022	0.551	0.010
	[0.323]	[0.202]	[0.099]	[0.312]	[0.495]	[0.148]	[0.497]	[0.099]
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	0.73	0.61	0.49	0.79	0.78	0.74	0.72	0.51
N	20439	20450	20466	20453	13003	13003	18134	13003
Number of clusters	3010	3010	3010	3009	2296	2296	2890	2296

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 ($t \leq -2$)	0.001	0.003	-0.003	-0.004	-0.043	0.004	-0.057	-0.002
	(0.012)	(0.008)	(0.004)	(0.003)	(0.016)	(0.003)	(0.017)	(0.001)
Post-period ($t \geq 1$)	0.038	-0.001	-0.006	-0.002	0.021	0.003	0.048	0.000
	(0.014)	(0.009)	(0.004)	(0.004)	(0.016)	(0.004)	(0.018)	(0.002)
Post-pre difference	0.036	-0.005	-0.003	0.002	0.064	-0.001	0.105	0.002
	(0.010)	(0.006)	(0.002)	(0.003)	(0.013)	(0.002)	(0.013)	(0.002)
Pre-event average	0.227	0.060	0.009	0.019	0.508	0.004	0.548	0.001
	[0.419]	[0.237]	[0.095]	[0.135]	[0.500]	[0.067]	[0.498]	[0.033]
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	0.72	0.61	0.24	0.77	0.69	0.67	0.56	0.27
N	16413	16400	16445	16450	12226	12226	15170	12226
Number of clusters	2923	2922	2922	2923	2522	2522	2863	2522

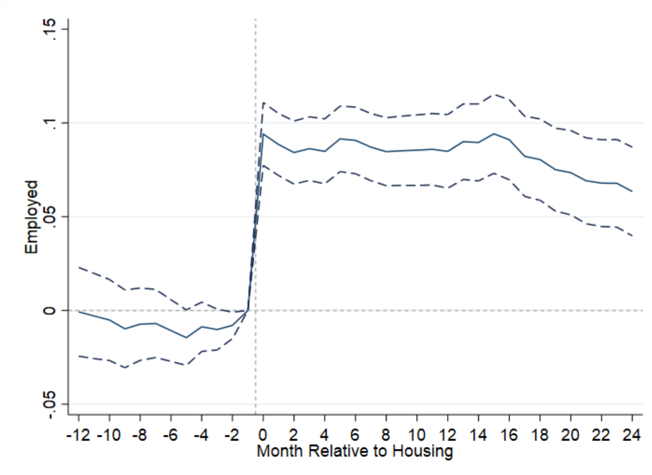
ID-clustered standard errors in parentheses

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. These specifications do not interpolate dependent variables between housing recipients' interactions with the HMIS and related systems.

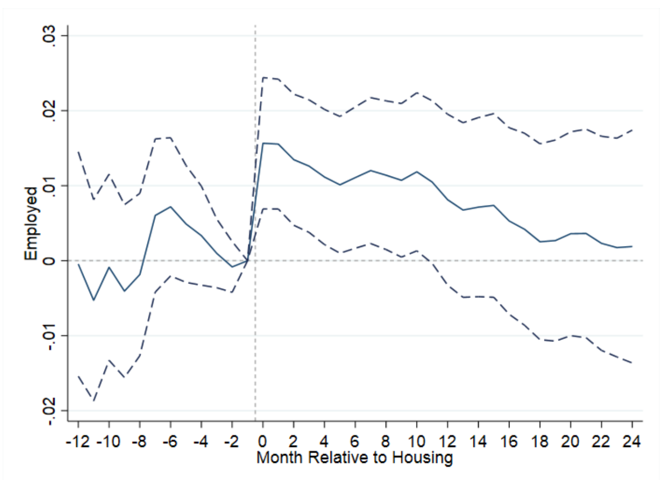
B.2 Main specification results on the monthly level

Figure B.4: Event study results: employment (extensive margin)

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



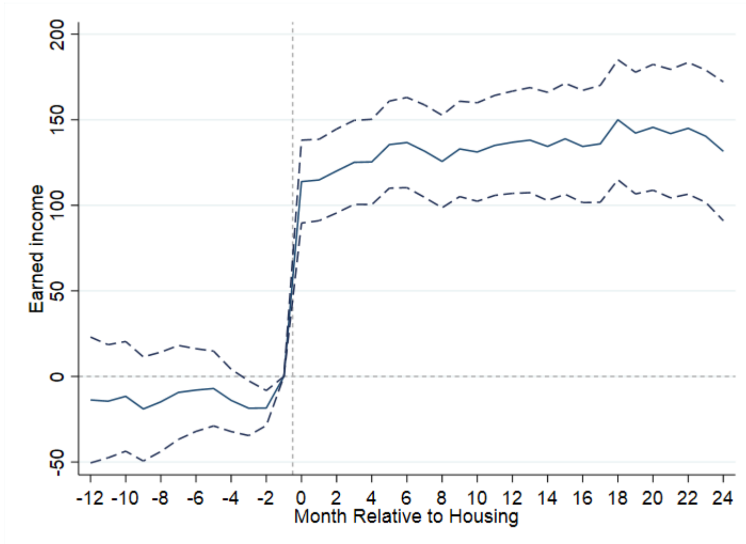
This figure displays the month-level coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m2} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

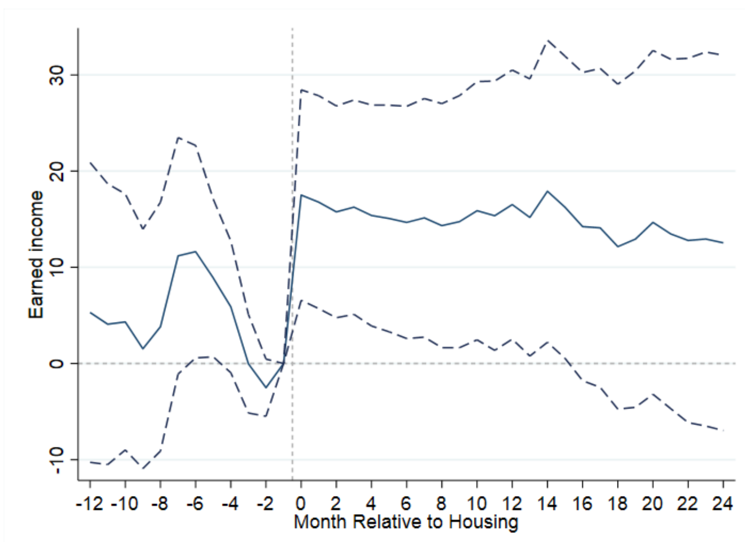
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 13 months prior to and 25 months since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure B.5: Event study results: earned income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



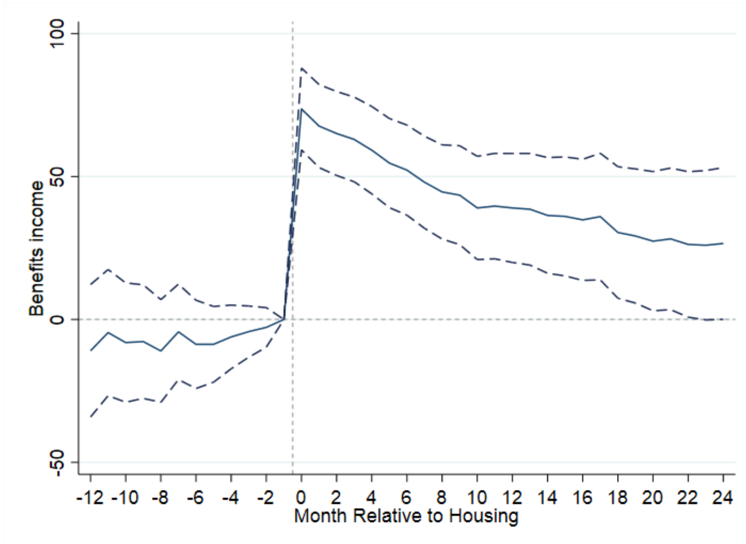
This figure displays the month-level coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m2} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

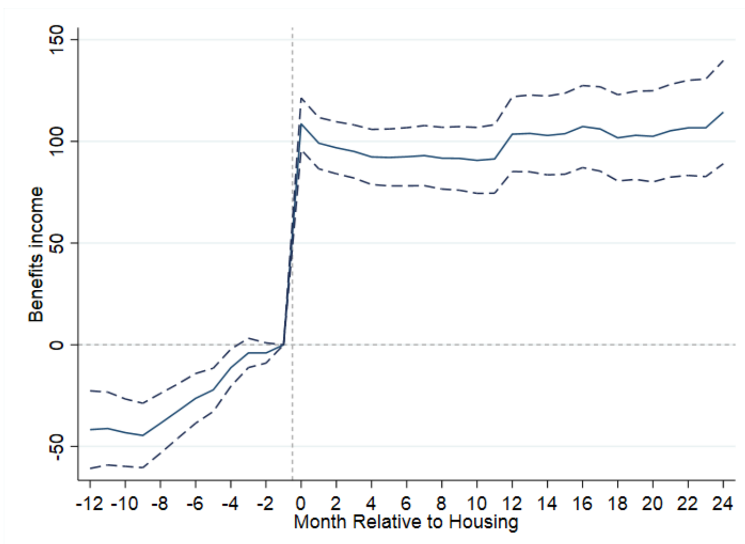
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. Timing is binned up to 13 months prior to and 25 months since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure B.6: Event study results: benefits income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



This figure displays the month-level coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m2} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Benefits income" refers to pecuniary support received on part of SSI, SSDI, unemployment benefits, TANF, Veteran Affairs assistance, Social Security, and General Assistance from LAHSA organisms. Timing is binned up to 13 months prior to and 25 months since each individual's housing events; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

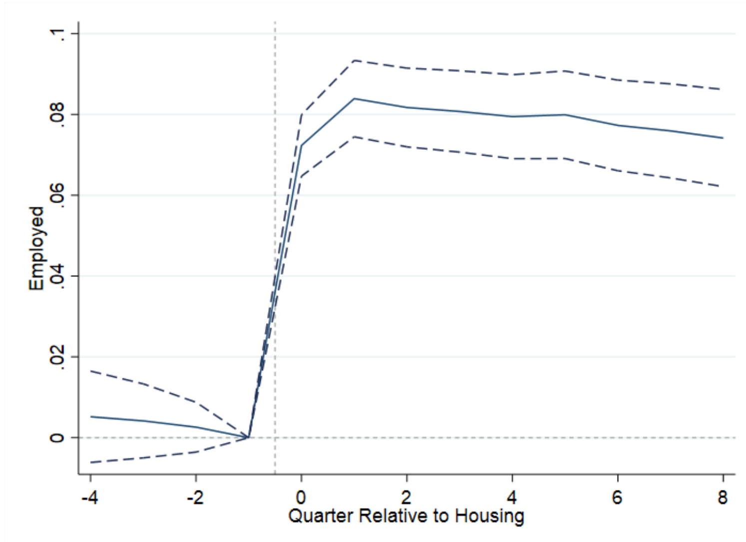
B.3 Main specification results without requirement of observation between months 18 and 24 post-event

This section presents our main specification results on the sample of UH recipients receiving housing between 2014 and 2018, without the sample restriction of our central specification that the individual reports at least one HMIS interaction between 18 and 24 months post-event. Removing this restriction (while maintaining the restriction that we observe each individual at least once 6 months pre-event (exclusive) increases our sample size substantially; this change increases our RRH sample to 11,215 individuals (from 3,028) and our PSH sample to 5,056 (from 3,006).

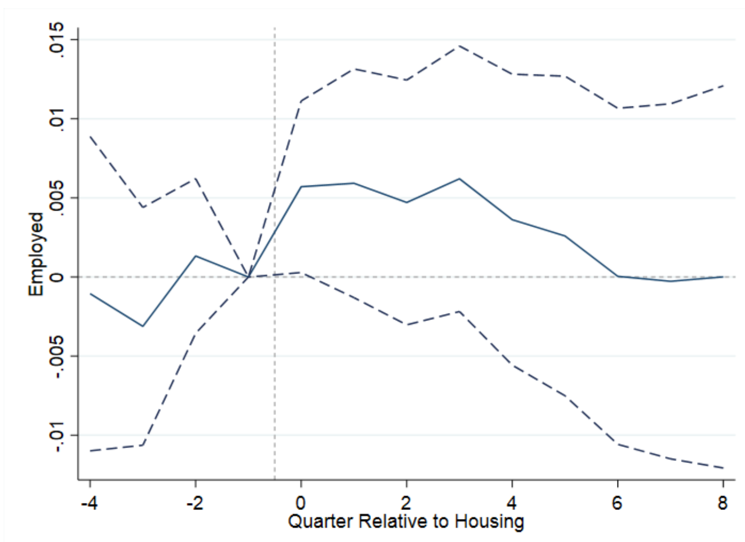
B.3.1 Replication of central results

Figure B.7: Event study results: employment (extensive margin)

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



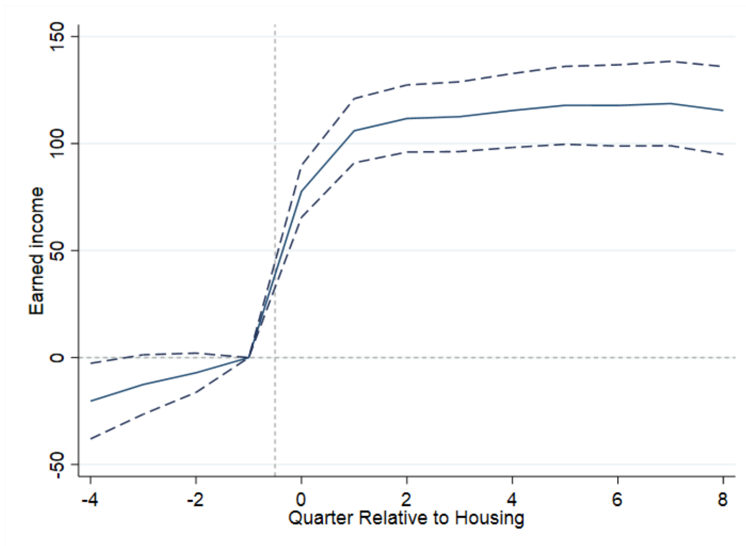
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

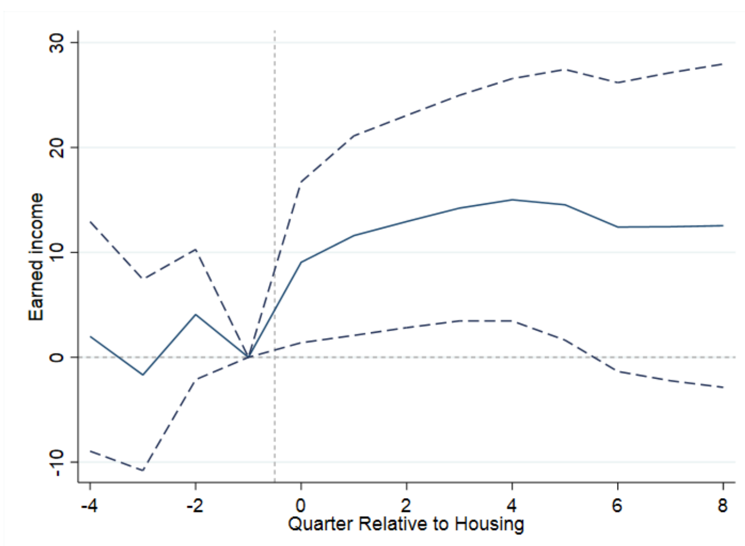
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure B.8: Event study results: earned income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



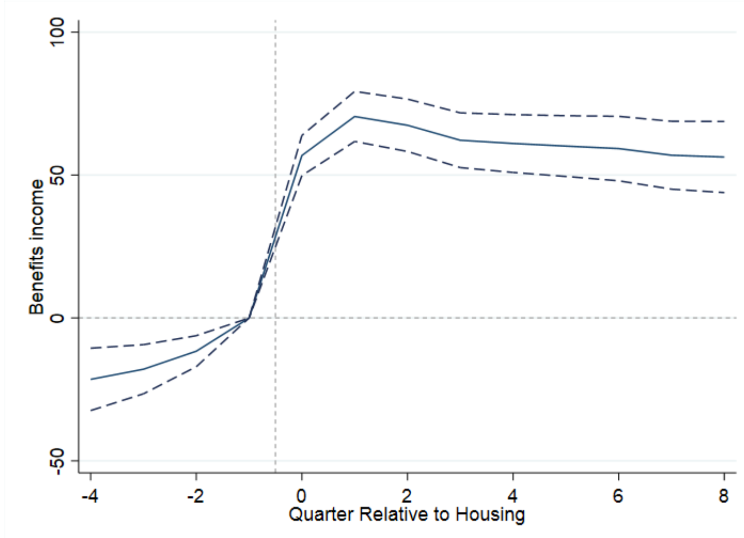
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

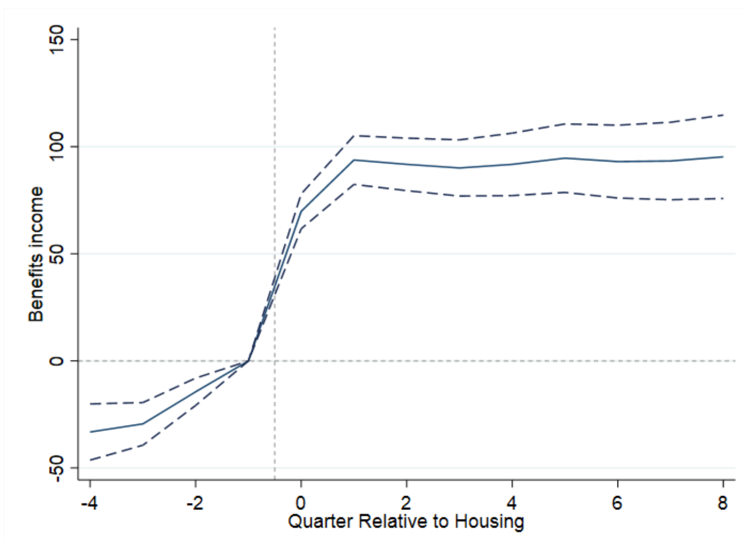
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure B.9: Event study results: benefits income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



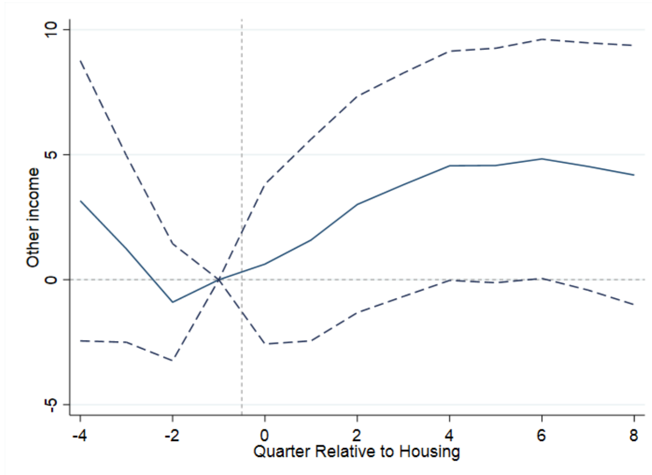
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

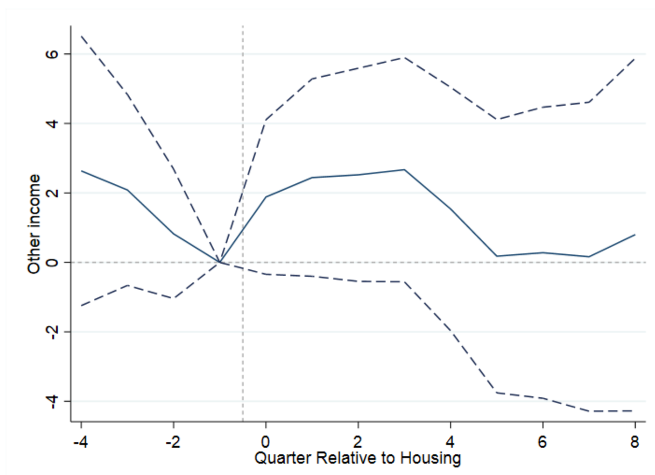
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Benefits income" refers to pecuniary support received on part of SSI, SSDI, unemployment benefits, TANF, Veteran Affairs assistance, Social Security, and General Assistance from LAHSA organisms. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month. Standard errors are clustered at the individual-level and 95% confidence intervals are displayed as dashed lines.

Figure B.10: Event study results: other income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing

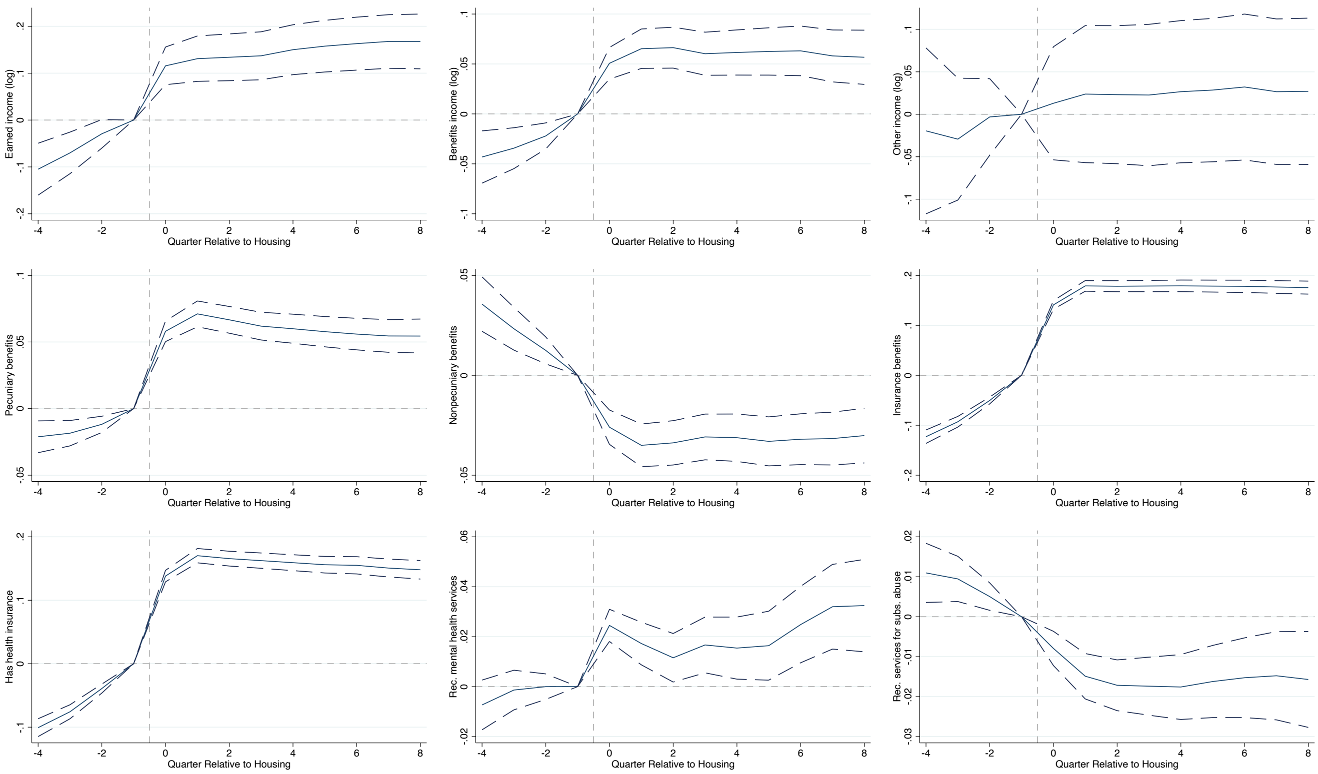


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. 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.11: Additional income and broad programmatic outcomes:
Panel (a): RRH

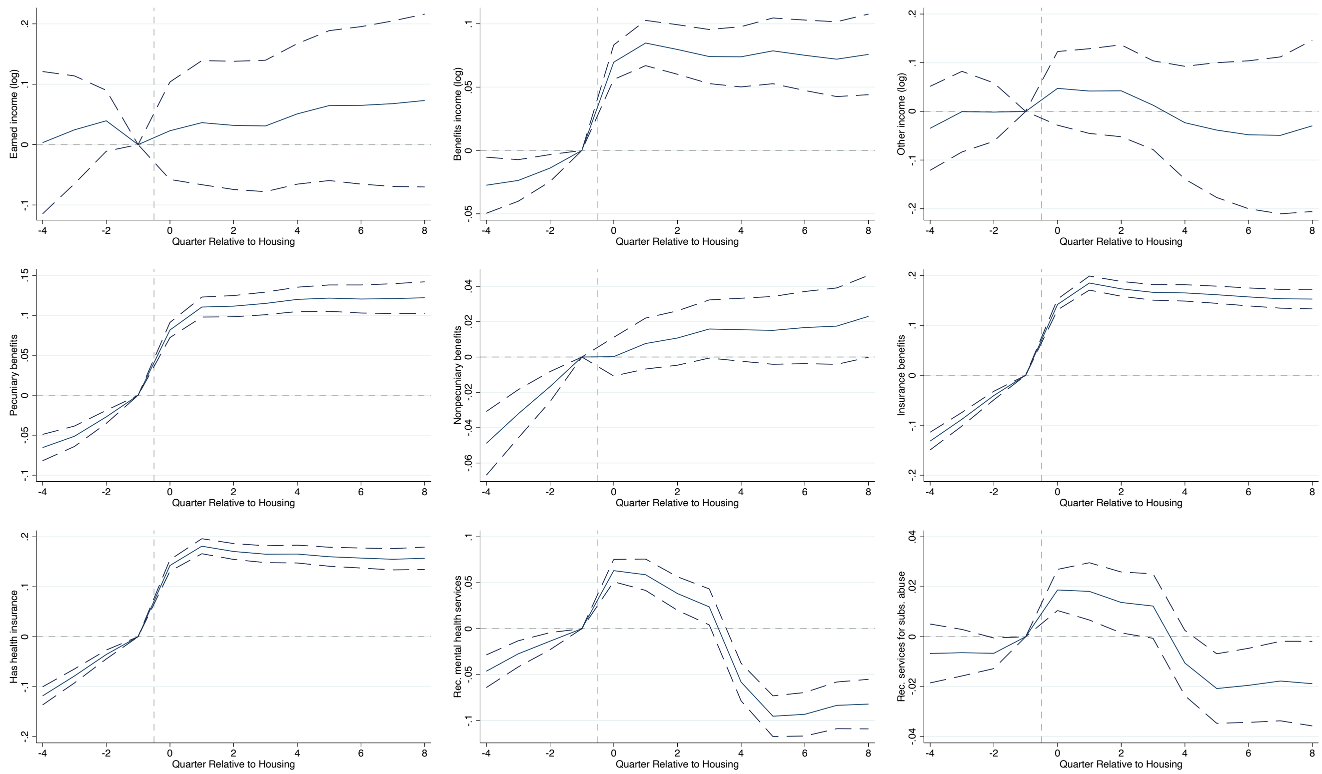


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month.

Figure B.11: Additional income and broad programmatic outcomes:
Panel (b): PSH

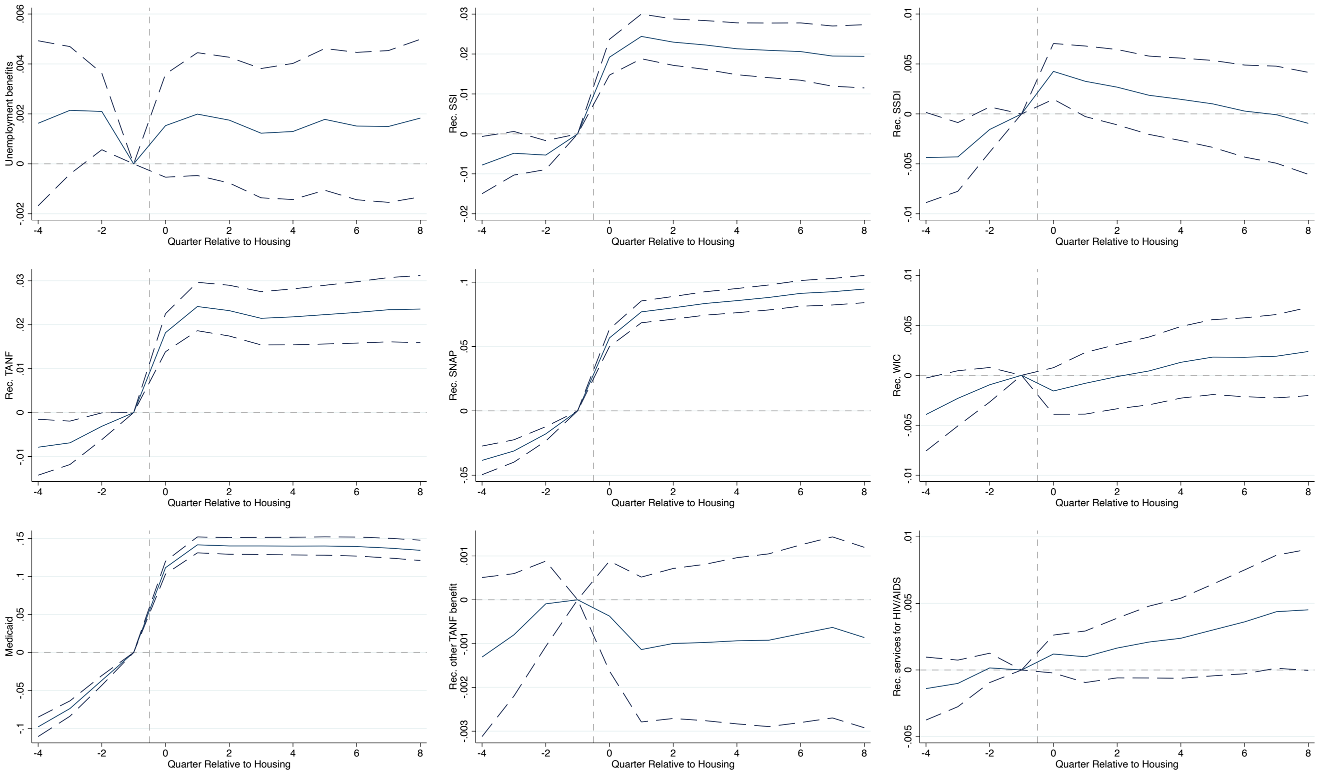


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month.

Figure B.12: Additional specific programmatic outcomes:
Panel (a): RRH

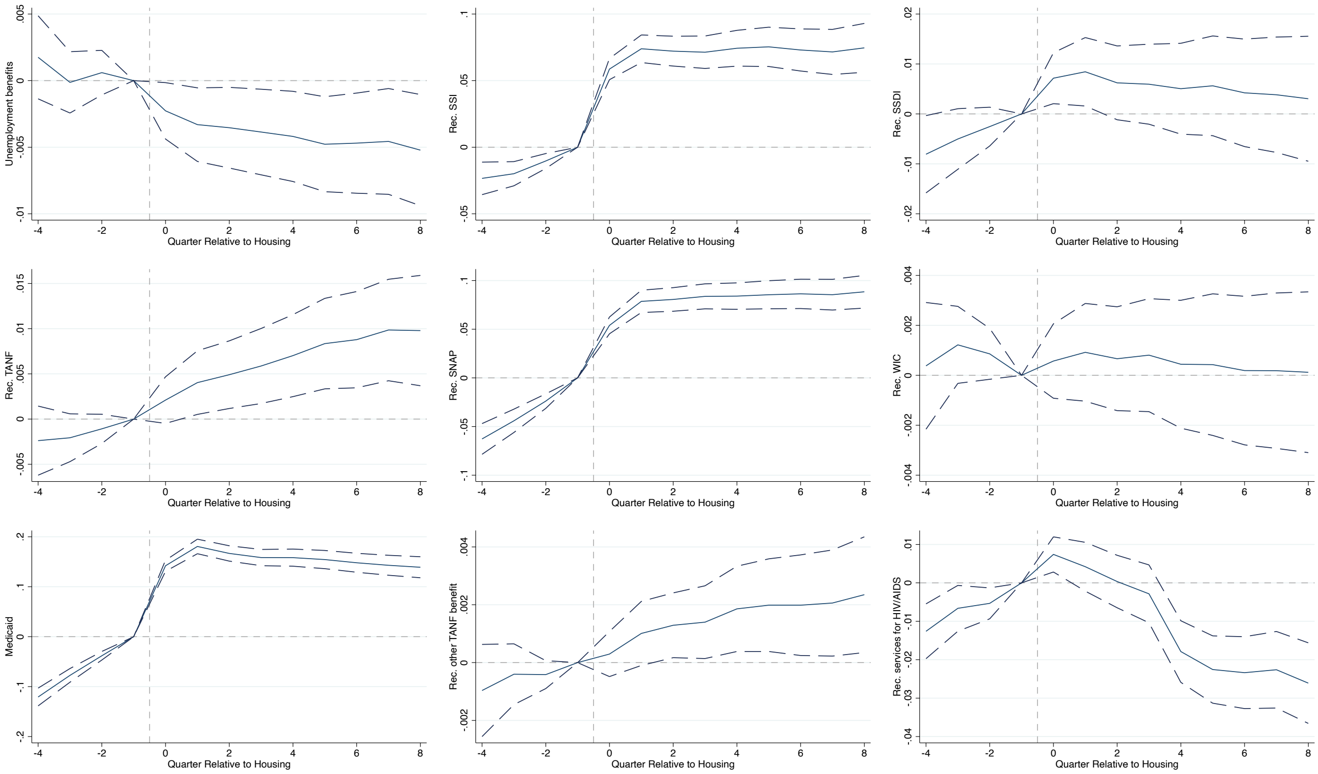


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month.

Figure B.12: Additional specific programmatic outcomes:
Panel (b): PSH



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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 estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month.

Table B.4: Event studies (labor market and earnings outcomes)

Panel (a): RRH

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Employed	Income	Log inc.	Earned inc.	Log earned inc.	Benefits inc.	Log benefits inc.	Other inc.	Log other inc.
Pre-period ($t \leq -2$)	-0.014	-40.020	-0.042	-20.516	-0.056	-21.996	-0.031	-0.012	0.004
	(0.003)	(5.491)	(0.009)	(5.011)	(0.015)	(2.965)	(0.007)	(1.366)	(0.022)
Post-period ($t \geq 1$)	0.077	154.228	0.124	92.101	0.094	61.071	0.055	2.740	0.002
	(0.004)	(7.359)	(0.010)	(6.846)	(0.020)	(3.966)	(0.008)	(1.840)	(0.031)
Post-pre difference	0.091	194.249	0.167	112.617	0.149	83.067	0.086	2.752	-0.002
	(0.005)	(9.163)	(0.014)	(8.895)	(0.028)	(4.990)	(0.012)	(2.413)	(0.042)
Pre-event average	0.183	449.114	6.527	183.597	6.933	254.141	6.276	20.952	6.451
	[0.387]	[695.038]	[0.808]	[581.118]	[0.780]	[440.997]	[0.737]	[162.656]	[0.906]
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	0.71	0.78	0.80	0.75	0.81	0.84	0.90	0.72	0.98
N	346855	339254	179132	312747	59400	339254	134190	339254	9597
Number of clusters	11120	11038	6667	10841	2854	11038	5388	11038	537

ID-clustered standard errors in parentheses

Panel (b): PSH

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Employed	Income	Log inc.	Earned inc.	Log earned inc.	Benefits inc.	Log benefits inc.	Other inc.	Log other inc.
Pre-period ($t \leq -2$)	-0.002	-22.558	-0.010	-0.545	0.026	-23.535	-0.016	1.330	-0.012
	(0.003)	(4.574)	(0.007)	(2.970)	(0.034)	(3.643)	(0.005)	(0.902)	(0.011)
Post-period ($t \geq 1$)	0.008	93.220	0.085	10.487	0.040	80.269	0.077	3.216	0.031
	(0.004)	(6.753)	(0.009)	(4.419)	(0.043)	(5.335)	(0.008)	(1.388)	(0.024)
Post-pre difference	0.010	115.778	0.095	11.033	0.014	103.804	0.093	1.886	0.043
	(0.004)	(8.043)	(0.011)	(5.209)	(0.055)	(6.475)	(0.010)	(1.597)	(0.031)
Pre-event average	0.075	435.199	6.140	58.828	6.659	368.252	6.071	9.239	6.276
	[0.263]	[479.150]	[0.742]	[291.870]	[0.816]	[419.922]	[0.712]	[97.790]	[0.833]
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	0.71	0.75	0.84	0.74	0.88	0.79	0.88	0.65	0.99
N	170953	167312	133504	164248	9980	167323	124422	167323	2584
Number of clusters	5052	4996	4438	4985	516	4996	4267	4996	152

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months post-event. 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 B.5: 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 ($t \leq -2$)	-0.023	0.011	-0.079	-0.070	0.001	-0.003	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)	(0.003)	(0.002)
Post-period ($t \geq 1$)	0.063	-0.028	0.140	0.144	0.001	0.013	-0.010
	(0.004)	(0.005)	(0.005)	(0.005)	(0.001)	(0.004)	(0.003)
Post-pre difference	0.087	-0.038	0.219	0.214	0.000	0.016	-0.016
	(0.006)	(0.006)	(0.006)	(0.006)	(0.001)	(0.005)	(0.003)
Pre-event average	0.373	0.453	0.586	0.576	0.007	0.119	0.059
	[0.484]	[0.498]	[0.493]	[0.494]	[0.084]	[0.324]	[0.235]
Month fixed effects	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X
Adj. R-squared	0.81	0.78	0.74	0.69	0.64	0.52	0.50
N	339254	326714	333928	339254	225002	225002	225002
Number of clusters	11038	10602	10890	11038	11054	11054	11054

ID-clustered standard errors in parentheses

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 ($t \leq -2$)	-0.042	-0.021	-0.067	-0.062	-0.013	-0.058	-0.020
	(0.004)	(0.005)	(0.005)	(0.005)	(0.002)	(0.005)	(0.003)
Post-period ($t \geq 1$)	0.089	0.001	0.155	0.154	0.005	0.075	0.020
	(0.006)	(0.007)	(0.007)	(0.007)	(0.003)	(0.009)	(0.006)
Post-pre difference	0.131	0.022	0.222	0.216	0.018	0.132	0.040
	(0.007)	(0.008)	(0.008)	(0.008)	(0.004)	(0.010)	(0.007)
Pre-event average	0.661	0.529	0.542	0.525	0.049	0.312	0.095
	[0.473]	[0.499]	[0.498]	[0.499]	[0.215]	[0.463]	[0.293]
Month fixed effects	X	X	X	X	X	X	X
ID fixed effects	X	X	X	X	X	X	X
Adj. R-squared	0.70	0.72	0.69	0.64	0.60	0.48	0.46
N	167323	166456	165941	167323	142284	142284	142284
Number of clusters	4996	4978	4972	4996	5042	5042	5042

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months post-event. 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 B.6: 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 ($t \leq -2$)	-0.008	-0.004	0.001	-0.007	-0.030	-0.000	-0.066	-0.001
	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.004)	(0.001)
Post-period ($t \geq 1$)	0.022	0.002	0.002	0.020	0.055	-0.001	0.110	-0.001
	(0.003)	(0.002)	(0.001)	(0.003)	(0.004)	(0.001)	(0.005)	(0.001)
Post-pre difference	0.030	0.007	0.001	0.028	0.086	-0.001	0.176	-0.000
	(0.003)	(0.002)	(0.002)	(0.003)	(0.005)	(0.002)	(0.006)	(0.001)
Pre-event average	0.096	0.034	0.011	0.094	0.300	0.023	0.368	0.005
	[0.295]	[0.182]	[0.103]	[0.292]	[0.458]	[0.151]	[0.482]	[0.073]
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	0.84	0.79	0.68	0.87	0.87	0.83	0.82	0.75
N	339239	339242	339252	339212	315654	315654	333928	315654
Number of clusters	11038	11038	11038	11038	10229	10229	10890	10229

ID-clustered standard errors in parentheses

Panel (b): PSH

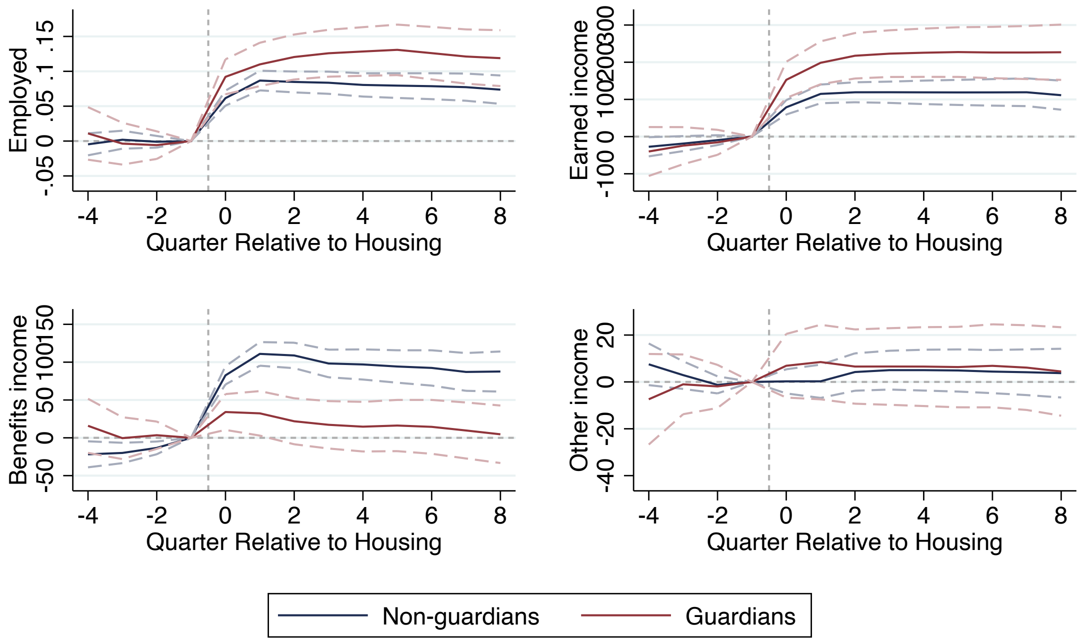
	(1) SSI	(2) SSDI	(3) Unemployment ben.	(4) TANF	(5) SNAP	(6) WIC	(7) Medicaid	(8) Other TANF
Pre-period ($t \leq -2$)	-0.019	-0.003	0.001	-0.000	-0.033	0.001	-0.068	-0.000
	(0.003)	(0.002)	(0.001)	(0.001)	(0.004)	(0.001)	(0.005)	(0.000)
Post-period ($t \geq 1$)	0.061	0.009	-0.002	0.003	0.059	0.001	0.149	0.001
	(0.005)	(0.003)	(0.001)	(0.002)	(0.005)	(0.001)	(0.007)	(0.001)
Post-pre difference	0.079	0.012	-0.003	0.004	0.092	0.000	0.217	0.001
	(0.006)	(0.004)	(0.002)	(0.002)	(0.007)	(0.001)	(0.008)	(0.001)
Pre-event average	0.212	0.057	0.008	0.022	0.421	0.007	0.467	0.001
	[0.409]	[0.231]	[0.091]	[0.147]	[0.494]	[0.082]	[0.499]	[0.034]
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	0.81	0.75	0.56	0.84	0.82	0.81	0.71	0.71
N	167311	167286	167323	167303	162209	162209	165941	162209
Number of clusters	4996	4996	4996	4996	4848	4848	4972	4848

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months post-event. 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.

B.3.2 Heterogeneity by guardian-status

Figure B.13: Labor and earnings responses by guardian-status
Panel (a): RRH

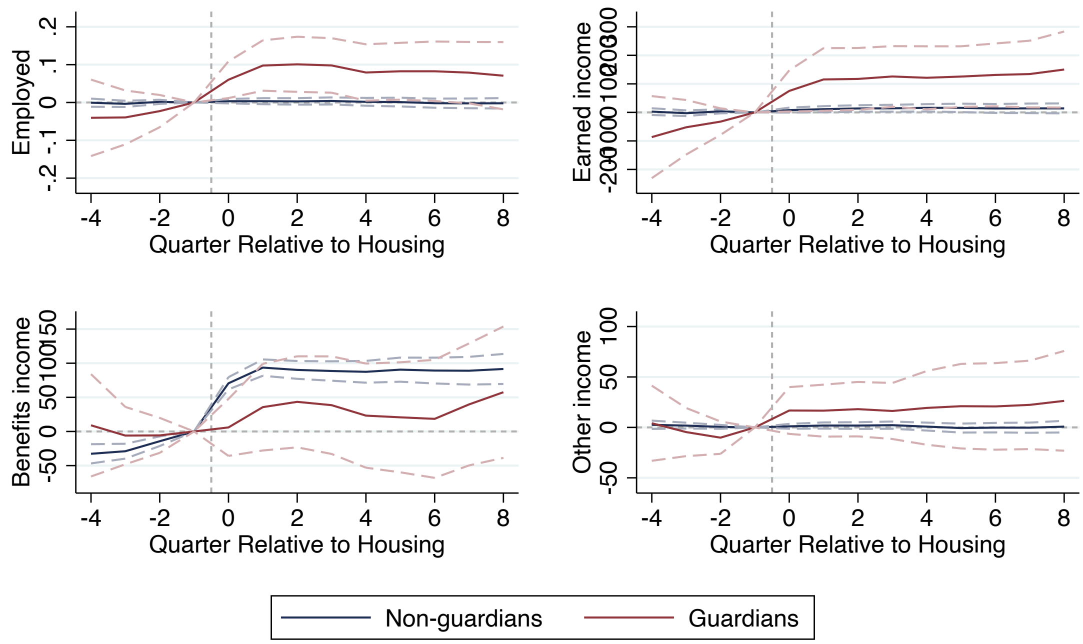


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Guardian" status refers to whether or not the individual has children. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.13: Labor and earnings responses by guardian-status
Panel (b): PSH

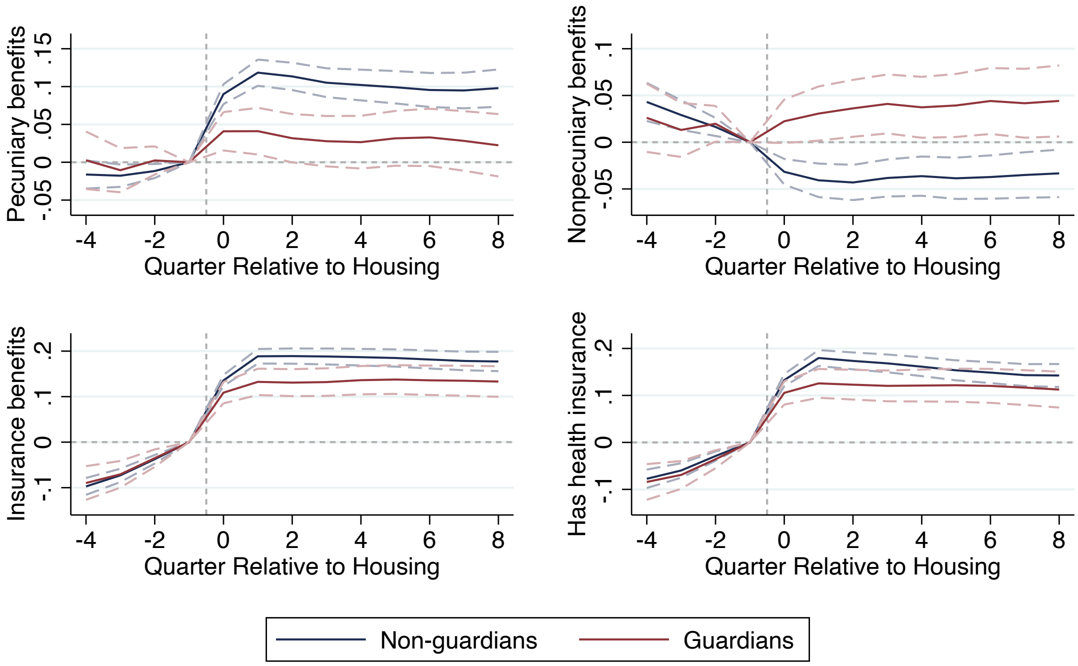


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Guardian" status refers to whether or not the individual has children. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.14: Benefits & programmatic responses by guardian-status
Panel (a): RRH

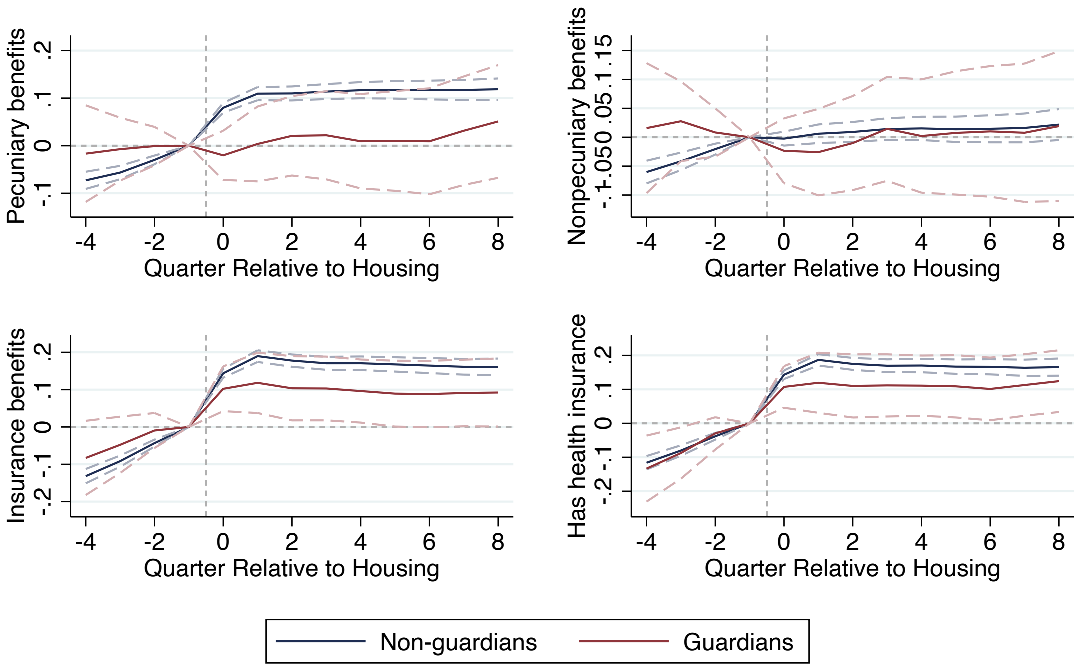


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving RRH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Guardian" status refers to whether or not the individual has children. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.14: Benefits & programmatic responses by guardian-status
Panel (b): PSH



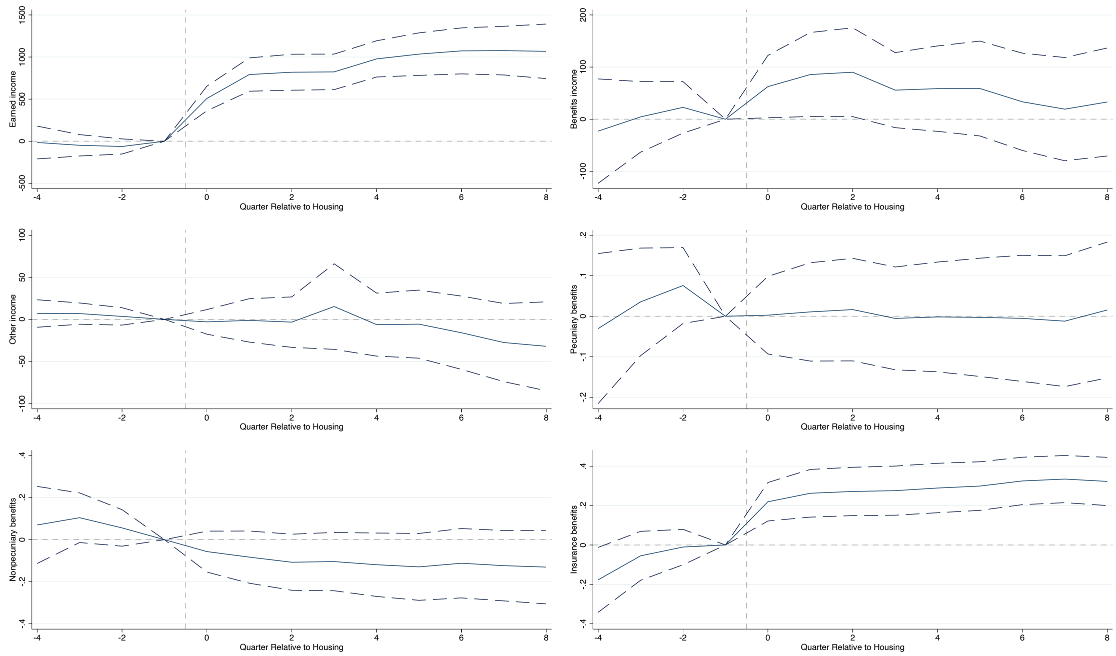
This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

The estimation sample includes heads-of-household receiving PSH between 2014 and 2018; the sample time frame spans from January 2013 to February 2020. "Guardian" status refers to whether or not the individual has children. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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.

B.3.3 Heterogeneity by ex-post employment transition type

Figure B.15: RRH U2E Transitions

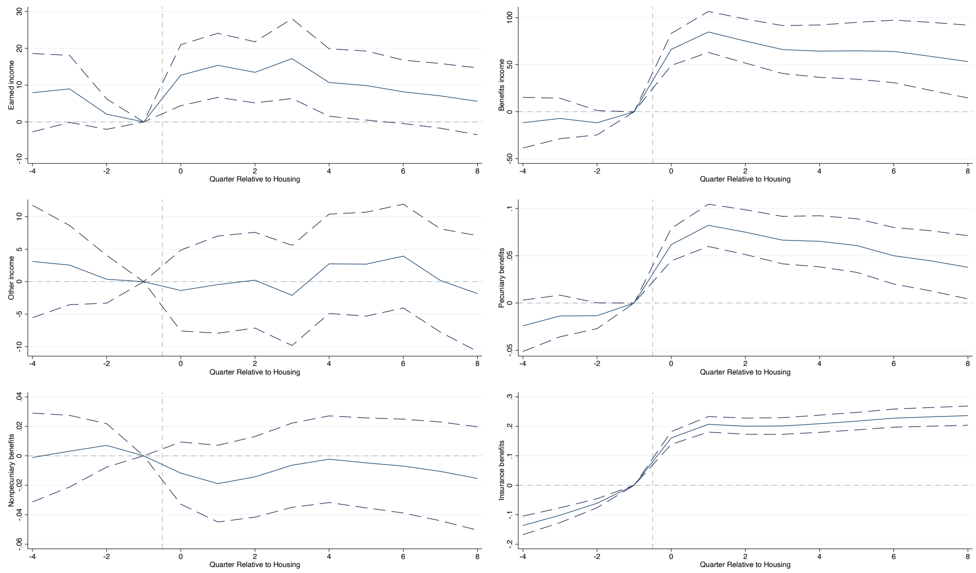


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.16: RRH U2U Transitions

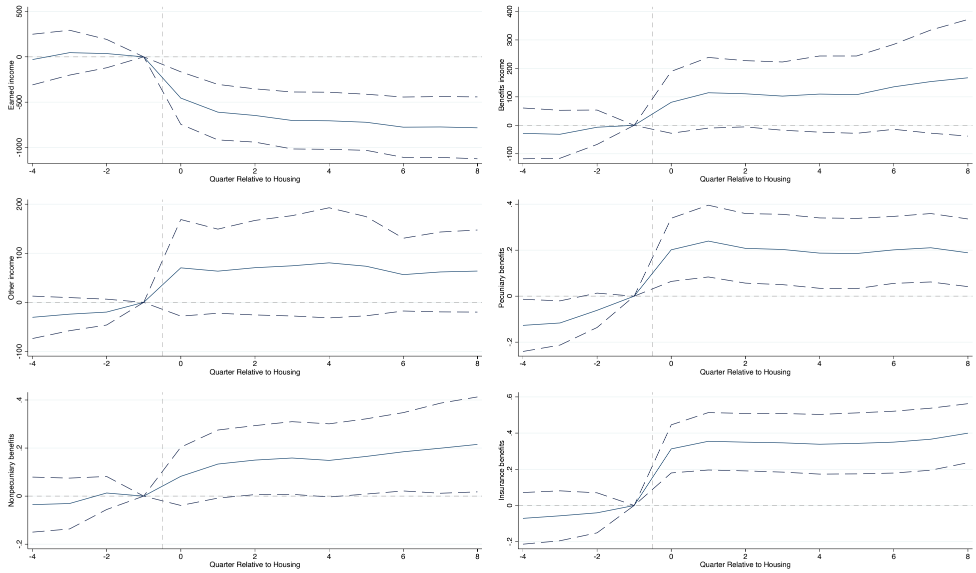


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months post-event. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.17: RRH E2U Transitions

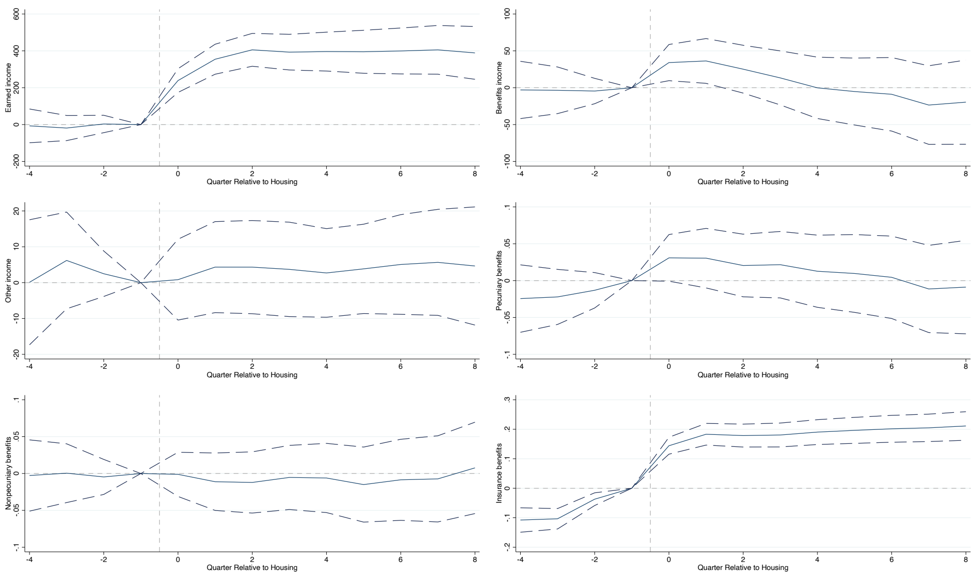


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.18: RRH “None” Transitioners

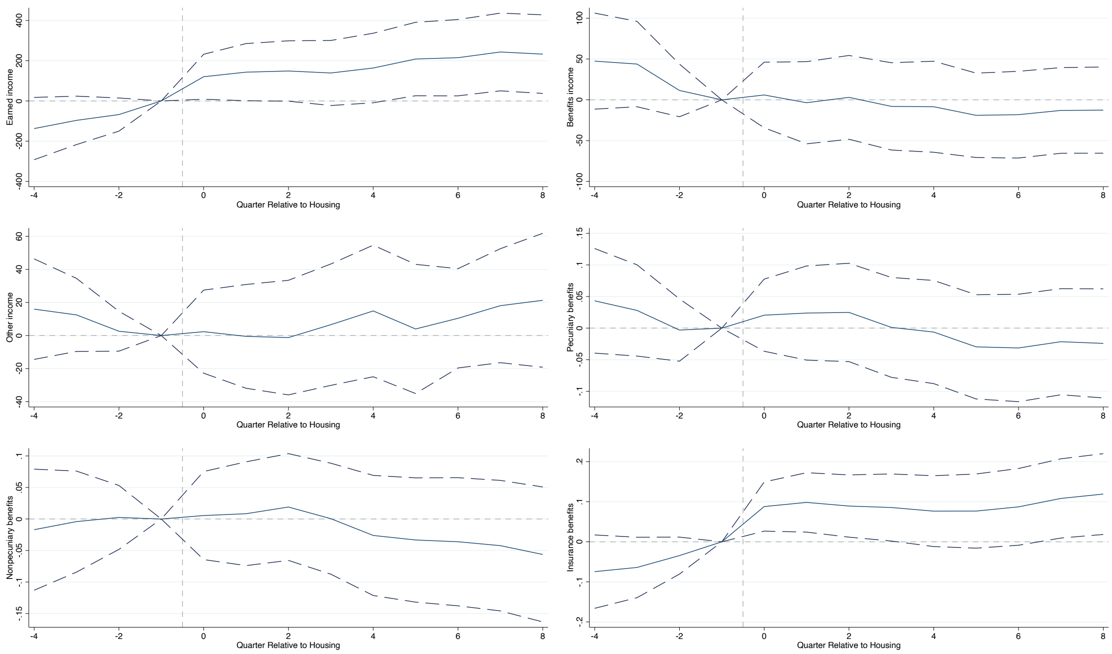


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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 both pre- and post-event. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.19: RRH E2E Transitions

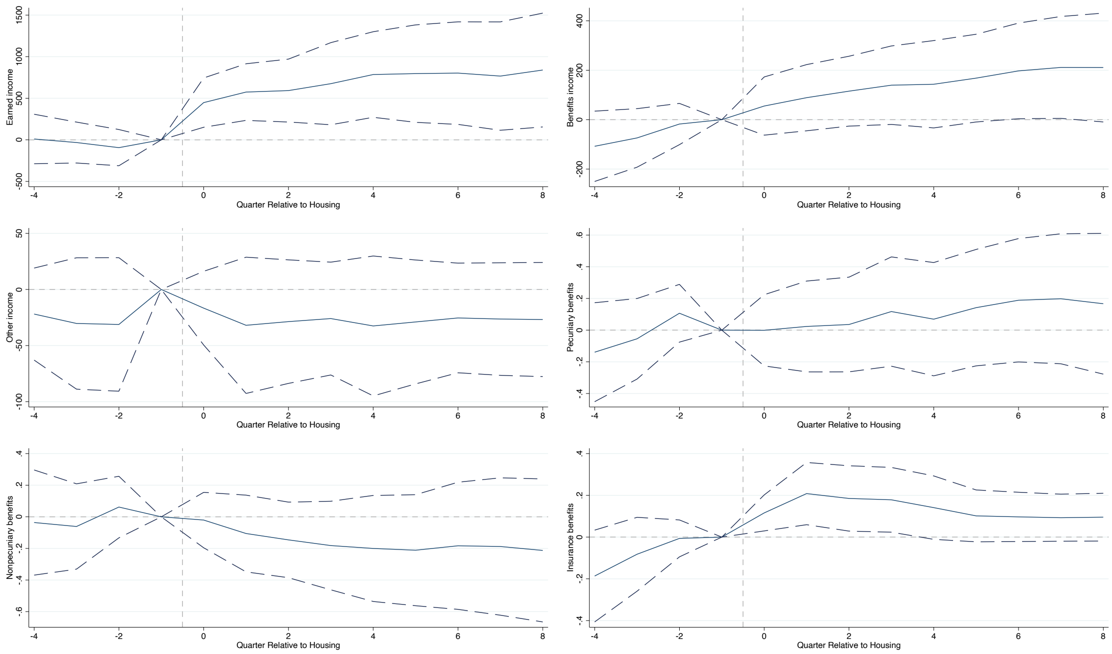


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.20: PSH U2E Transitions

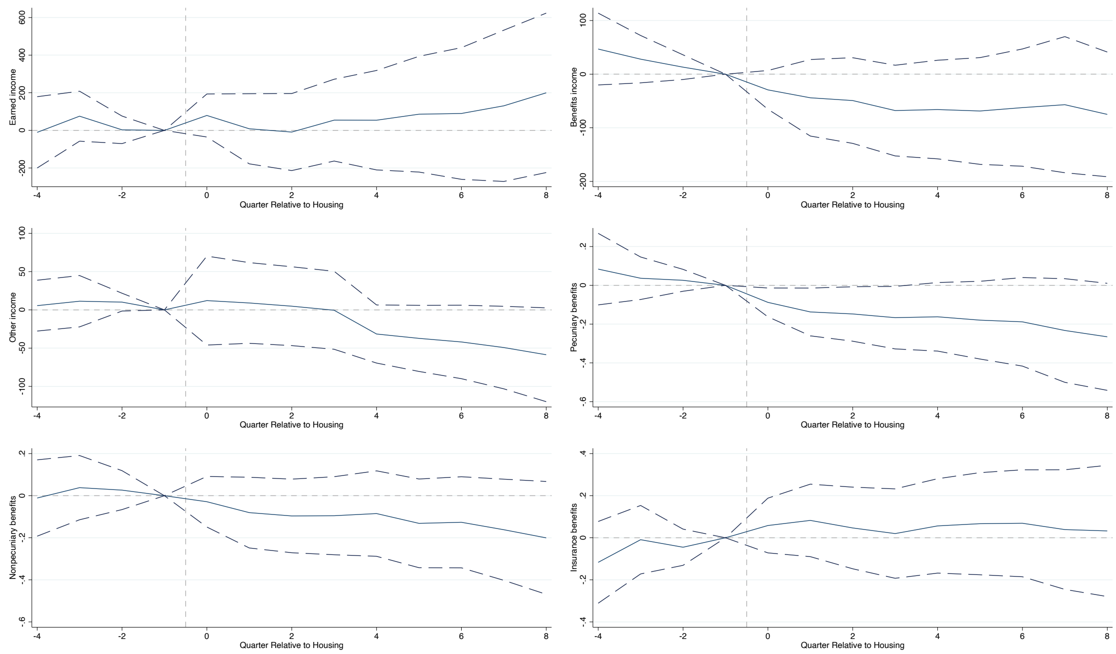


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.21: PSH E2E Transitions

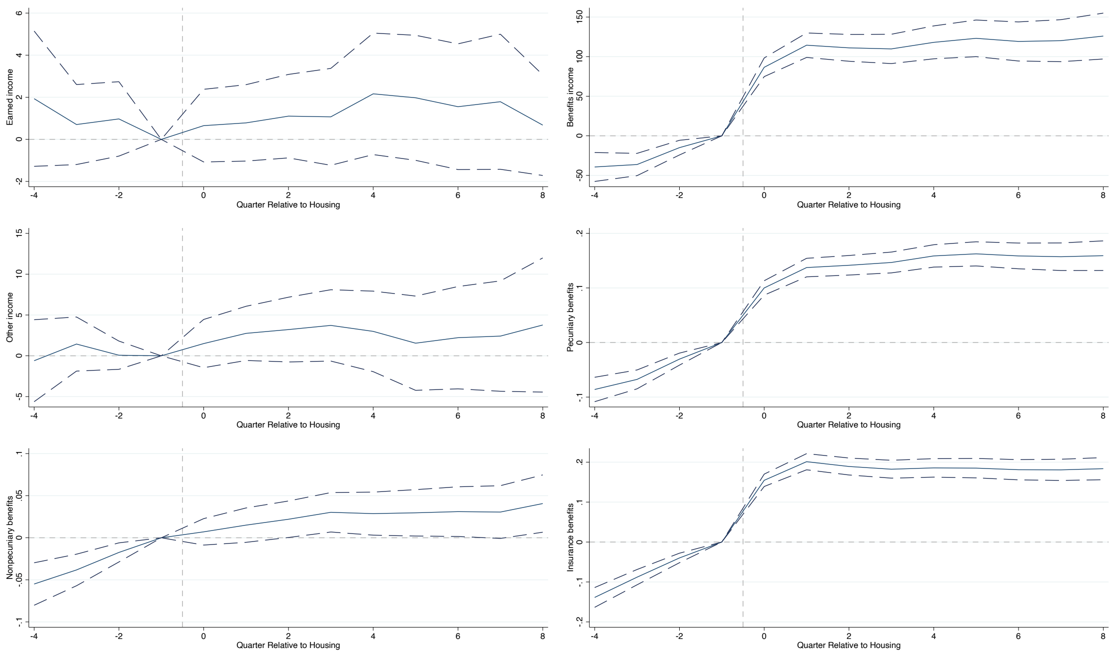


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.22: PSH U2U Transitions

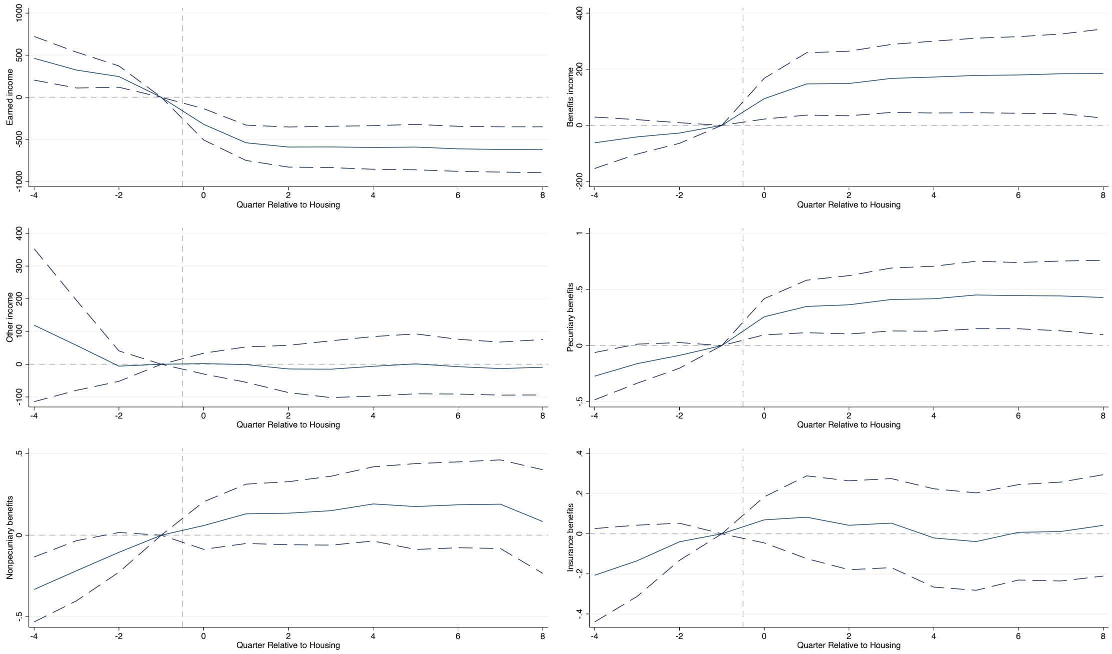


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.23: PSH E2U Transitions

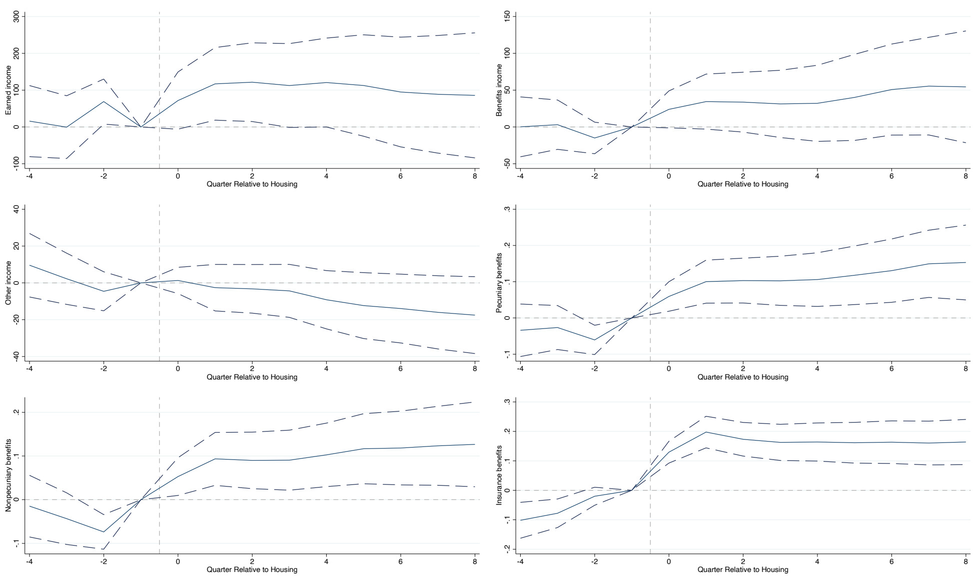


This figure displays the coefficients $\{\beta_{q(t)}\}$ from the event study specification with two-way fixed effects:

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

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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 B.24: PSH “None” Transitioners



This figure displays the coefficients $\{\beta_{q(t)}\}$ 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. The estimation sample differs from the sample in the main text: this sample does not include the restriction that individuals be observed at least once between 18 and 24 months 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 C Data Construction

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

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

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

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

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

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

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

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

In interactions with the systems that record the data, a consistent ID is maintained so that individuals can be tracked. Therefore, merging the files is simple and the only choice available to the researcher is whether (and how) to collapse information into a panel. Our primary panel is at the month-year by individual-level. As such, in instances where multiple interactions take place in the same month, for the same person, we take either the mean or the max of the recorded value. In general, we take the mean for numerical entries (income in a month, for instance) and we take the max for an interaction (an indicator for whether someone was receiving TANF, for instance). In this way, each person has at most one unique value for each variable in each month-year of our panel.

Since updates to most of our data only occur when an individual interacts with the appropriate portion of the system, we don't have consistent estimates for income, benefits, etc. in each month. To resolve this, we interpolate the missing values using a fairly simple forwarding projection. Specifically, for each variable, we adopt the following procedures, in order:

1. If a value is present in a given month-year, do nothing.

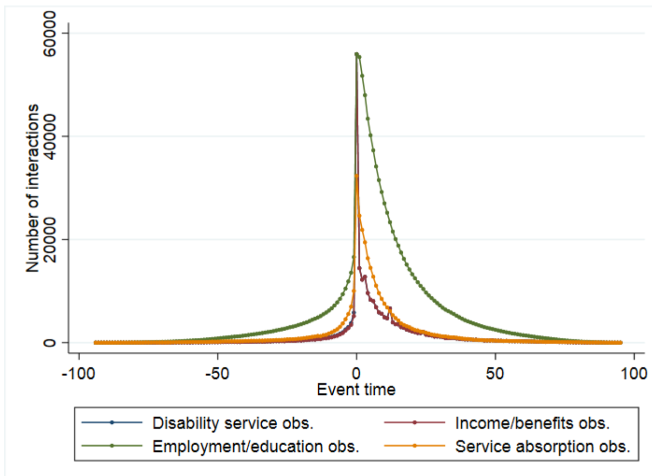
2. If a value is missing in a given month-year, we take the most recently updated value from a dataset interaction.

3. If there are no prior values, no projection is made; in this way, we make no assumptions about these values before an initial interaction with one of these systems.

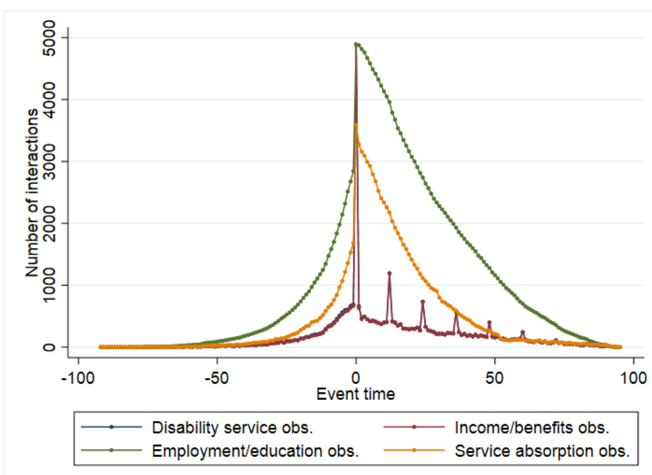
4. We *do* project values forward past an individual's final observed interaction.

Figure C.1: Interaction types around housing event

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



These histograms plot the frequency interactions around individuals' housing events, stratified by the type of interaction (i.e. which HMIS dataset records their interaction). Panel (a) displays this relationship for Rapid Re-Housing recipients, and Panel (b) studies PSH recipients. Event time is measured in months relative to placement into Housing First.