The fiscality of housing the homeless:

Evidence from housing programs in Los Angeles

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Abstract

We leverage variation in the timing of permanent 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 increases extensive-margin labor market participation by 60% from a baseline of 15pp, while Permanent Supportive Housing recipients exhibit no change. We perform a cost-benefit analysis of these programs and estimate the net public cost of placing an individual into Rapid Re-Housing at approximately zero Dollars, whereas placing an individual into Permanent Supportive Housing costs approximately USD 150,000 per 10 years per recipient.

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JEL codes: J22, I38, H31, H43, H53, R58

1 Introduction

What are the overall pecuniary costs and benefits of housing homeless people? 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 study these various costs in quantifying the net fiscal impact of policies aimed at housing homeless individuals.

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 permanent housing programs (PH). 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 types of PH programs: Rapid Re-Housing (RRH) and Permanent Supportive Housing (PSH) in Los Angeles County from 2013 to 2020.

This research space is broadly characterized by a lack of individual-time panel microdata. This limitation has prevented researchers from more sharply quantifying the fiscal impacts of PH programs. Moreover, recent data innovations in this space

[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.

are still limited by censoring issues around individuals exiting the data sources (either through migration, death, or exiting homelessness/homelessness services), as well as relying on individuals' regular interaction with data collection points (e.g. upon benefits transferal).

There is substantial precedent for studying homelessness housing policy in a cost-benefit framework. Many cost-benefit studies in this area broadly rely on cross-sectional comparisons of individuals that are homeless and those located in supportive housing—generally reporting very large reductions in public benefits absorption by between USD 600 and 2,000 annually (Flaming, Burns, and Matsunaga (2009)[2] Flaming, Burns, and Matsunaga (2015)). Flaming, Burns, and Matsunaga (2015) reports that the highest cost-decile of homeless individuals in Santa Clara County absorbed more than USD 83,000 per year. Cohen (2021) is the only large study as of yet to make use of individual-level homelessness data, finding that 80% of the costs associated with PH are offset by reductions in public benefits.

Other works in the cost-benefit space make use of quasi-experimental methods in typically stylized small-sample settings. 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 PH policies, but with several studies—both quasi-experimental and randomized experimental—reporting insignificant differences in costs or even increases in costs following placement into PH. Typically, researchers find that administrative costs increase in the short run alongside decreases in benefits uptake related to adverse events (e.g. medical or criminal justice system usage; Gilmer, Manning, and Ettne (2009)). As a first or-

[2] Flaming, Burns, and Matsunaga (2009) also employs small-sample event studies around placement into nonprofit-provided PH and propensity score matching.

der, most evidence suggests that PH and similar programs are effective in mitigating short run homelessness (Evans, Philips, and Ruffini (2019) reviews this literature), but Corinth (2017) finds a minimal impact of PH on reducing homelessness—that a 10-unit increase in PH spaces reduces local-level homelessness by only 1 individual; the author attributes this finding to poor targeting of PH policies.

However, nearly all of these works claiming to estimate the costs and benefits of PH and similar policies focus on public administrative costs—entirely forgoing benefits pertaining to changes in the tax base. 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 PH among a very small sample of individuals ($N \leq 20$). 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 increase in income tax payments within the employed group.

Much work has focused on establishing a positive relationship between local rental costs and homelessness rates (Economic Advisers (2019); Hanratty (2017);Fargo et al. (2013); Quigley and Raphael (2001); Honig and Filer (1993)). These studies tend to use city- or community-level panels and consensus tends toward the finding that rental costs are significant positive predictors of homelessness rates. Corinth (2017) reports a descriptive elasticity of the rate of homelessness with respect to the median rental cost of one. Relatedly, Abramson (2021) estimates a spatial structural model and finds a significant positive impact of right-to-counsel laws on homelessness (15%) and a negative impact of receiving rental assistance payments on the probability of exiting housing into homelessness (-45%).

This paper adds to the existing literature as follows. Our work represents the largest event study focusing on the employment and benefits uptake outcomes of individuals around placement into PH, with a treated sample size of roughly 60,000 recipients. Prior works 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 PH policies.

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 (8.6 percentage points) in their probability of finding employment. Among individuals that find employment, monthly income increases by around USD 800 with no increase in benefits income. PSH recipients report a smaller increase in probability of finding employment by only 16% (1 percentage point). Instead, PSH recipients see a substantial increase in their benefits absorption upon connection to permanent housing by about 33% from a baseline of USD 360 per month. Among the few individuals ($\sim$1%) that find employment upon placement into PSH, we find large increases in earned income unaccompanied by increases in benefits income.

Finally, we perform a cost-benefit analysis of these programs. We estimate substantial variation in the net fiscal impact of RRH recipiency based on whether an RRH recipient secures employment following housing and whether they recidivate into homelessness in the future. For instance, individuals securing employment following housing generate, in net, around USD 10,000 in public funds per 10 years per individual, while those not reporting employment generate an additional net fiscal cost of around USD 4,000 per 10 years per individual. We estimate the expected net public cost of placing an individual into RRH at approximately zero Dollars per 10 years per recipient. We find much more unambiguously negative fiscal impacts of placement into PSH. We document that PSH recipients do not on average see improved labor market outcomes, at least in the short run. We estimate the cost of placing an individual into PSH at around USD 150,000 per 10 years per recipient. Due to the exclusion of certain costs and benefits from our analysis (e.g. criminal justice system usage, early childhood educational impacts for families, etc.), our results likely underestimate the fiscal benefits of housing homeless individuals.

2 Data and Setting

Our data is constructed entirely from the Los Angeles County Homelessness Management Information System (HMIS) with the help of the California Policy Lab (CPL). This data allows us to follow individuals over time and observe the evolution in their employment status, earnings, and benefits uptake, *inter alia*, between 2013 and 2020. Methods of linking across datasets, construction of variables, and imputation strategies are described in more detail in Appendix B.

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. More than 141,000 people experienced some form of homelessness in 2020.[3] In 2016, Los Angeles designed a USD 1.2B program with the goal of expanding emergency shelter and permanent solutions to combat homelessness. The corresponding emergency shelter capacity was raised to 25,000 by 2020, a 57% increase from the 2017 estimate, and the number of permanent housing units increased by around 1200. 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.

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 Los Angeles system, an update is made to their file.[4] These file updates include the reason for the update (i.e. 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 (Wachter et al. (2019)).

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

[3]LAHSA classified nearly 60,000 people as unsheltered homeless in their January 2020 point-in-time count.

[4]An "interaction" is any service provision or client meeting. The designation of interaction ranges from items like referrals from a case coordinator, to rent arrears, to outreach.

2013 and 2020. The cleaned data is structured as a single panel at the individual-month level. Because interactions do not necessarily occur every month, we also interpolate information in missing periods. In most cases, this process consists simply of projecting information forward to the next interaction, with some limitations on the projections.[5] Data denominated in Dollars are unadjusted for inflation, but over the course of our three year event studies, can be deflated by approximately 6%.[6]

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. 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 only around USD 300-400. Even when looking only at those who are employed, average total earnings are only around USD 1000 per month upon initial interaction with HMIS.[7] 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

[5] For more detail on the projections and missing information, see Appendix B.

[6] The period in the US from 2013 to 2020 is characterized by a relatively stable annual inflation rate of 2%.

[7] 18.3% of those receiving Rapid Re-Housing are employed at first interaction, for instance, and the average earned income (among all RRH recipients) is only USD 191.

housing, such as access to emergency shelter, meetings with case-workers, health checkups, etc.

Table 1: Summary statistics

	RRH	PSH	Untreated
Demographics			
Black	0.509	0.538	0.402
Male	0.483	0.619	0.608
HS Graduate	0.948	0.727	0.851
Veteran	0.188	0.058	0.077
Age (years)	38	46	42
Prior Housing Status			
Months Homeless Since First Spell	17	35	.
Months Homeless Since Prior Spell	10	18	.
Most Common Living Situation	PNMFH (44%)	Emergency Shelter (37%)	PNMFH (57%)
Second Most —	Emergency Shelter (22%)	PNMFH (36%)	Emergency Shelter (18%)
Third Most —	Transitional Housing (9%)	Transitional Housing (9%)	Jail/Prison (2%)
Employment at First Interaction			
Employed	.183	.068	.087
Total Earned Income ($)	191	48	76
Total Monthly Income ($)	434	409	269
Individuals	55,950	4,892	192,084

This table displays select demographic, housing, and employment tabulations stratified by sample subgroups of treatment status. 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.

3 Empirical framework

There are two broad types of PH programs recorded in our data that vary somewhat in scope. The first is Permanent Supportive Housing (PSH; itself comprising three separate subprograms)—encompassing permanent housing through vouchers, permanent cost subsidization, and housing in specific permanent housing unit. This program accounts for 9% of events in our sample. The second, Rapid Re-Housing (RRH), encompasses time-limited housing and housing-assistance programs typically lasting between one- and two years. In principle, recipients of RRH are more positively selected than permanent supportive housing recipients in that they do not express need for permanent housing and have lower risk-scores.[8] However, while RRH recipients do exhibit lower scores on average than permanent supportive housing recipients, in practice there is substantial discretion in which individuals are granted certain housing solutions. Throughout the span of both types of housing programs, there is some variation in the generosity of benefits.

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. Also, 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.

Figure A.1-Figure A.4 display more information on how individuals interact with the HMIS. Given the role of data censoring in studying homelessness issues, understanding these interactions is important for our setting. New information on housing recipient outcomes is only generated upon interaction with the HMIS, so the frequency and timespan of individual interactions with the HMIS around housing events are key for the robustness of our design as well as for the fidelity of our dependent variable imputation method. Figure A.1 and Figure A.2 plot the frequency among individual housing recipients of the timing difference between their housing events and their earliest and latest, respectively, interactions with the HMIS. These

[8] The County of Los Angeles, along with most other counties, assigns individuals a priority 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, 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.

figures demonstrate significant fall-off in observation both before and after housing events, although this issue appears more severe for pre-event interactions.[9]

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. We estimate regressions with two-way fixed effects on the month- and individual-level of the form

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t=k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

Our main outcomes of interest y_{it} 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.

We run this specification on the sample of individuals in our dataset that ever receive a PH program treatment between January 2014 and December 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.[10] In order to avoid positive selection, we refrain from restricting our sample

[9]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, recidivism into homelessness without interaction with public services, 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 individual housing recipients 6, 12, and 24 months post-housing recipiency if possible. However, it is still possible to observe individuals in our data contingent on their interactions with services covered in the HMIS.

[10]We observe that 78% of individuals receive PH benefits only once, 16.5% two times, 3.7% three times, and .3% at least four times (irrespective of whether the multiple treatments over time reflect

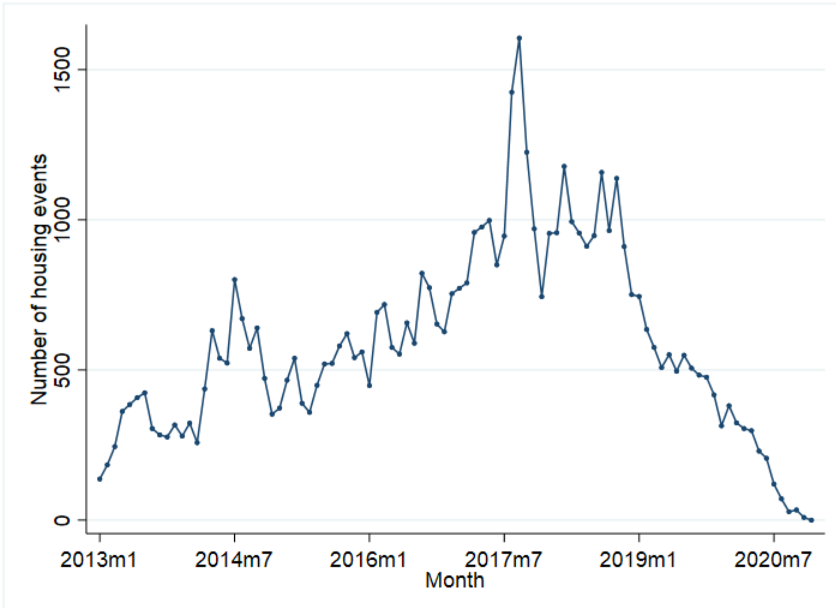
to individuals that receive housing support only once.

Leveraging the quasi-random variation in timing of housing recipiency yields coefficients $\{\hat{\beta}_j\}$. These $\{\hat{\beta}_j\}$ estimate an average treatment effect of housing on treated individuals j periods since the housing event. The validity of these estimators for the average treatment effect on the treated (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.

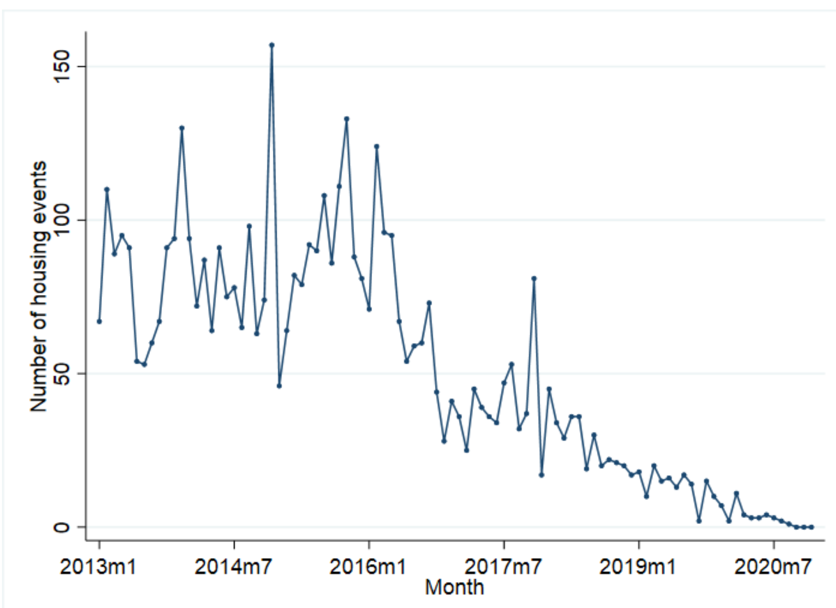
instances of miscoding).

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 among our sample. 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 sample excludes individuals who have ever received both Rapid Re-Housing and Permanent Supportive Housing.

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 5 plot the various event study coefficients for placement into each type of housing program. Table 2 and Table 3 summarize these results in relation to pre-period baselines.

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 8.6 percentage points relative to a pre-period baseline of 15% (an increase of 57%). On average, RRH recipients also see increased total incomes of around USD 190 per month—a 53% increase from pre-period levels. On the intensive margin, individuals reporting non-zero monthly income in both the pre- and post-event periods saw their income increase by 17% on average.

These changes in overall monthly income are driven both by changes in labor earnings as well as from changes in benefits uptake. Placement into permanent housing likely induced additional ties with social workers that can more easily facilitate connections with programmatic benefits. Additionally, having a stable home address often makes it easier for individuals to receive social benefits such as SSI and TANF. Columns (4) and (6) of Table 2 illustrate that average earnings income and benefits income increased by approximately the same amount, and column (8) shows that, overall, the share of individuals' earned labor income out of their total monthly income increased by about 3 percentage points (a 10% increase relative a pre-period baseline of 30%). Importantly, these figures on average changes in earnings obscure the heterogeneous changes by employment transition subpopulation type (explored in subsection 4.3).

We also study the impact of RRH recipiency on the uptake of nonpecuniary and programmatic benefits. Figure 5 illustrates the trajectory of non-permanent-housing-related benefits from the HMIS (e.g. meals absorbed, doctors visits, etc.) around placement into Rapid Re-Housing. We observe that individuals benefits uptake increases sharply following housing, declining smoothly to below pre-housing levels a year-and-half post-event.

Recipients also see substantial increase in their uptake of other benefits. Post-housing event, individuals see a near 50% increase in SSI recipiency, and 40% increase in SSDI recipiency, and a 30% increase in TANF recipiency. Individuals also see substantially increased probability of receiving SNAP (+17%) and a 6% increase in reporting having health insurance.[11]

Overall, the main event study specifications depict large, positive impacts of RRH on labor market outcomes, and increased connections of individuals to social programs. To the extent that benefit absorption wanes over time, RRH may induce net positive fiscal spillovers, which we explore more completely later.

4.2 The Effects of PSH

PSH recipients demonstrate less benefit to their labor market outcomes following their placement into permanent housing. Those that receive PSH see a modest, only marginally significant increase in average extensive margin employment of approx-

[11]The impacts on individual take-up of specific programs are illustrated in Figure A.6.

imately one percentage point (a 16% increase). This mild employment increase is accompanied by a significant increase in benefits income (and overall monthly income) by USD 120 against a baseline of USD 365, a 30% increase in benefits income, without any commensurate increase in labor earnings on either extensive or intensive margins. This change induces a decrease in their average earned share of overall monthly income of 1.2 percentage points (from a baseline of 7.6 percent)

Similarly to RRH recipients, PSH recipients see a large increase in their non-permanent-housing-related benefits from the HMIS, albeit with a substantial leading pre-trend. However, PSH recipients appear to decrease their non-housing HMIS benefits much more rapidly, reverting to pre-transition levels in only six months.

PSH recipients also see some increase in connection with both pecuniary and non-pecuniary benefits following placement into housing. Recipients see mild increases in the probability of receiving SSDI or TANF, but a large increase in the probability of receiving SSI (a 9 percentage point increase against a baseline of 24 percent), although the trajectory of SSI recipiency sees a substantial pre-trend leading into permanent housing recipiency. They also see similar increases in the probability of taking up other nonpecuniary benefits and having health insurance—of similar orders of absolute and relative magnitude as RRH recipients.

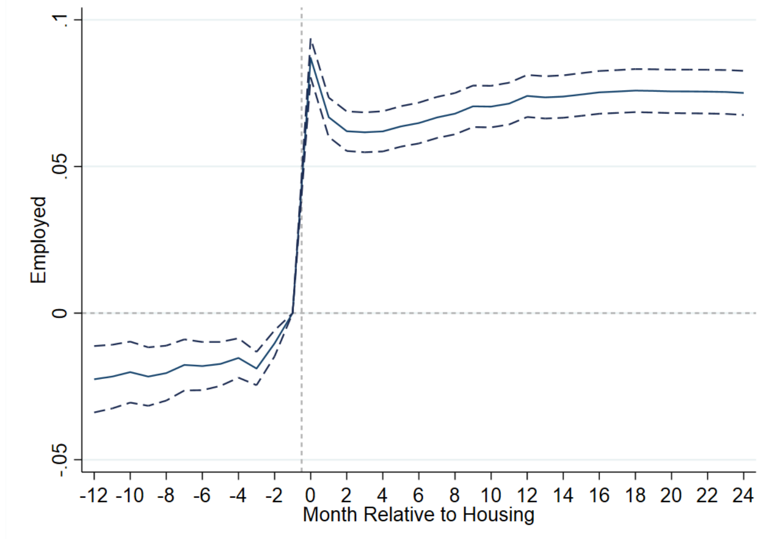
Overall, in the combined presence of significant uptake of social program benefits and absence of improvement in labor market outcomes, PSH appears less likely to generate net positive fiscal spillovers when only considering the activity of individual recipients.

Several of our designs are marked by slight pre-trends leading up to placement into housing. We attribute these pre-trends to a variety of sources. First, individuals that are aware of their impending PH recipiency may improve their outcomes as a purely anticipatory response to receiving housing. Second, individuals not yet selected for PH may endogenously improve their labor market outcomes as a means of influencing their program recipiency. This possibility would also violate the assumptions necessary for interpreting our results as causal. However, the presence of some measurement error in the observed timing of the actual housing events can also replicate the observed pre-trends consistent with the observed ATTs.[12]

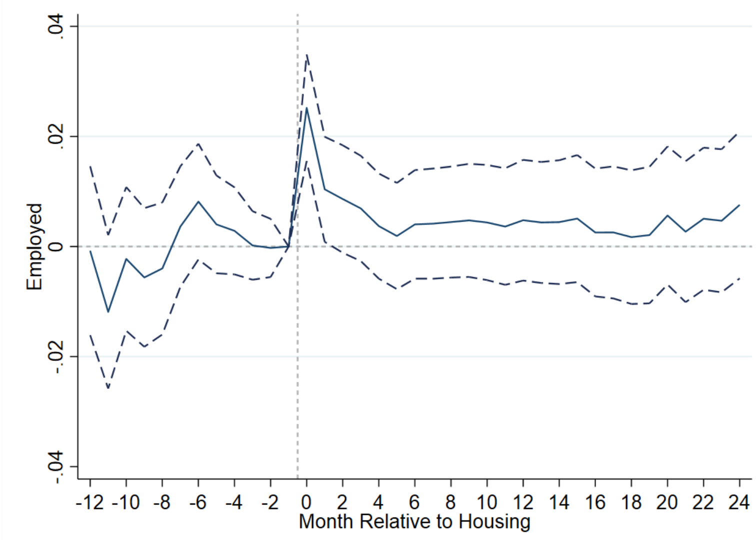
[12] In the data, we observe two variables for the date of housing recipiency. The first is called the "entry date" an the second is called the "move-in date". Our approach assigns the minimum of these two dates as the our observed treatment date (zero event time); however, for the majority observations, one of these two variables is unpopulated.

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

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



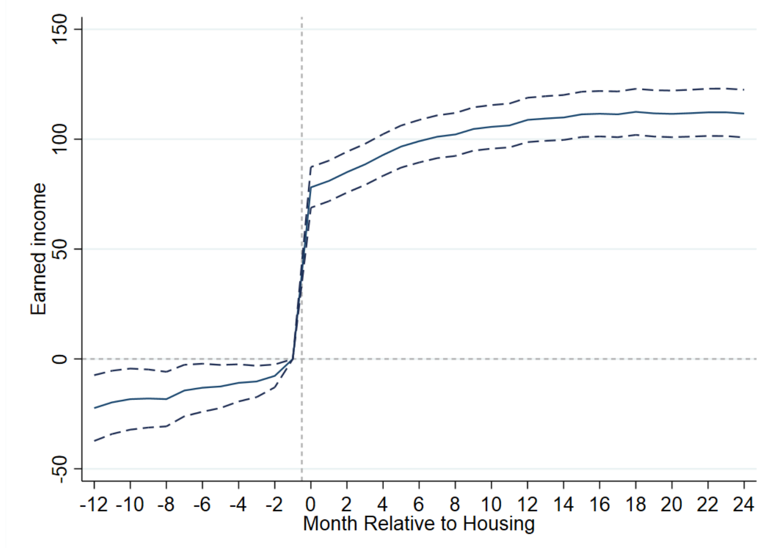
This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

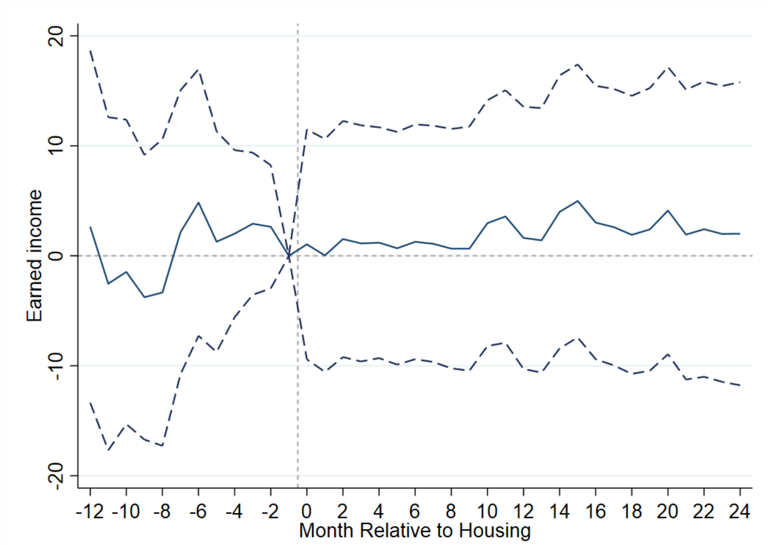
The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from 2013 to 2020. Timing is binned up to 13 months prior to and starting 25 months since each individual's housing event; these bins are omitted from the coefficient display. Panel (a) shows the event study estimates for Rapid Re-Housing by month. Panel (b) shows the results for Permanent Supportive Housing by month.

Figure 3: Event study results: earned income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



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

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

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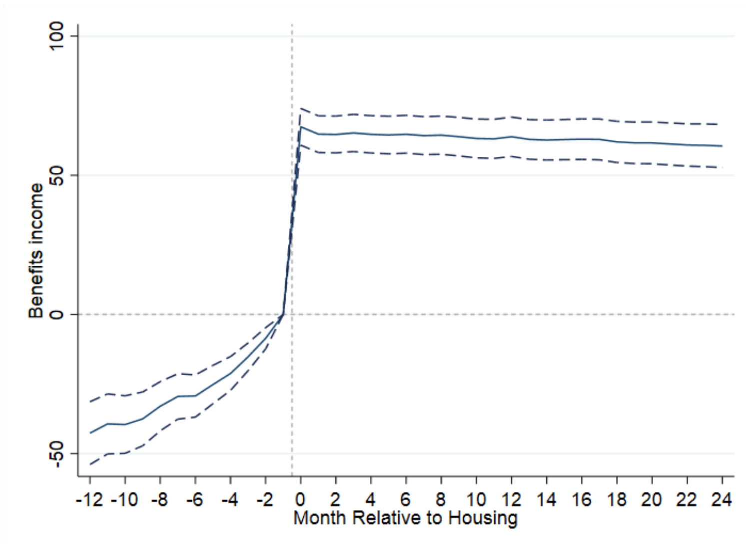
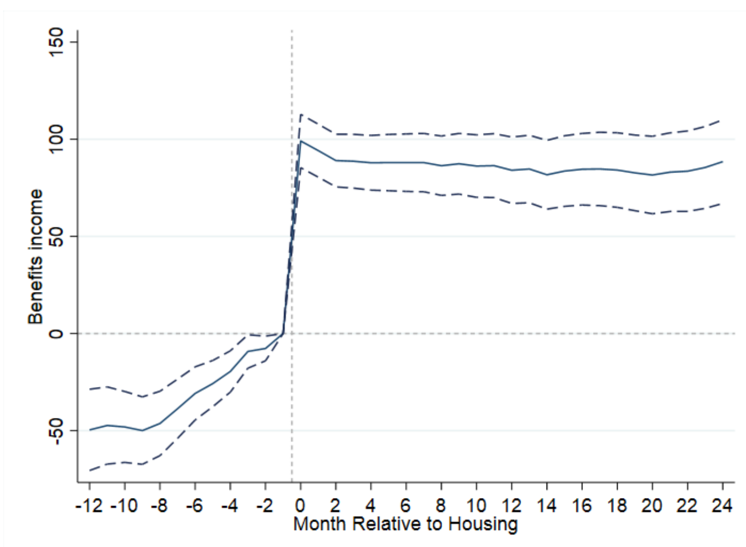
Figure 4: Event study results: benefits income

(a) Rapid Re-Housing



(b) Permanent Supportive Housing



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

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

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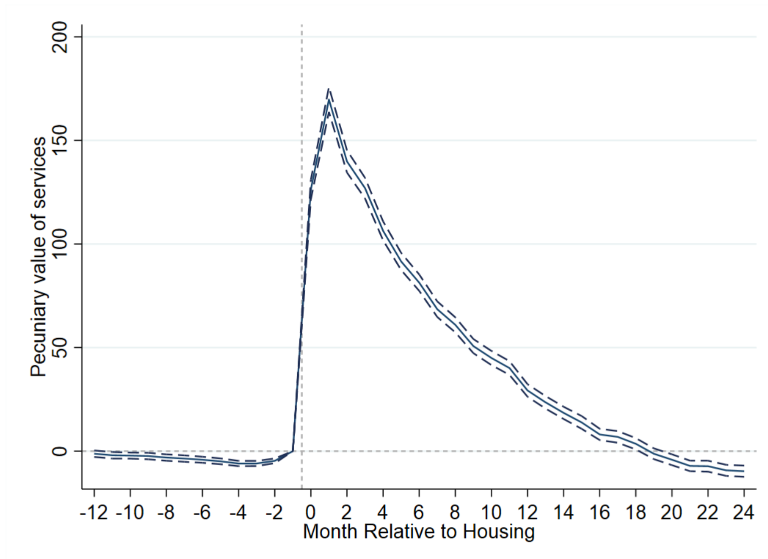
Figure 5: Event study results: services absorbed

(a) Rapid Re-Housing



(b) Permanent Supportive Housing

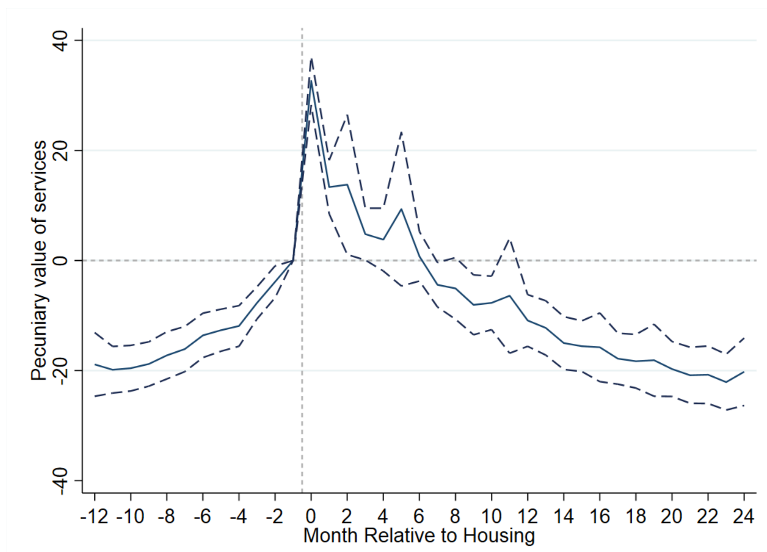


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Table 2: Event studies (labor market and earnings outcomes)

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) Percent earned
Pre-period ($t \leq -2$)	-0.017	-36.746	-0.046	-10.839**	-0.046	-26.069	-0.035	-0.003
	(0.003)	(4..910)	(0.008)	(4.252)	(0.015)	(3.018)	(0.007)	(0.031)
Post-period ($t \geq -0$)	0.069	155.501	0.129	88.543	0.095	66.985	0.057	0.031
	(0.003)	(5.389)	(0.009)	(4.653)	(0.015)	(3.625)	(0.007)	(0.004)
Pre-post difference	0.086	192.239	0.176	99.382	0.141	93.054	0.093	0.035
	(.004)	(7.302)	(0.012)	(6.429)	(0.023)	(4.546)	(0.010)	(0.005)
Base period average	0.150	358.606	6.638	158.979	7.005	199.770	6.369	0.305
	(0.003)	(5.021)	(0.008)	(4.333)	(0.014)	(3.304)	(0.007)	(0.003)
Month FE	X	X	X	X	X	X	X	X
ID FE	X	X	X	X	X	X	X	X
Adj. R-squared	0.79	0.87	0.86	0.84	0.87	0.89	0.92	0.90
N	1188093	1182756	532390	1182490	198213	1182490	395299	576509
No. clusters	44081	44081	21664	44079	9579	44079	16782	21794

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) Percent earned
Pre-period ($t \leq -2$)	-0.003	-29.877	-0.025	-0.090	0.014	-30.303	-0.036	0.004
	(0.003)	(6.619)	(0.011)	(4.335)	(0.056)	(5.356)	(0.009)	(0.004)
Post-period ($t \geq -0$)	0.007	90.952	0.071	0.296	-0.022	90.734	0.081	-0.008
	(0.005)	(8.400)	(0.013)	(5.428)	(0.071)	(6.933)	(0.017)	(0.004)
Pre-post difference	0.010	120.829	0.096	0.387	-0.0350	121.037	0.116	-0.012
	(.005)	(10.568)	(0.017)	(6.994)	(0.095)	(8.605)	(0.015)	(0.006)
Base period average	0.058	428.234	6.142	62.596	6.745	365.893	6.076	0.076
	(0.004)	(7.014)	(0.011)	(4.509)	(0.058)	(5.780).	(0.009)	(0.005)
Month FE	X	X	X	X	X	X	X	X
ID FE	X	X	X	X	X	X	X	X
Adj. R-squared	0.67	0.78	0.85	0.76	0.90	0.81	0.89	0.81
N	113102	112454	86509	112328	6340	112328	81748	92728
No. clusters	3701	3701	3116	3669	344	3699	2991	3128

This table displays the coefficients from event study regressions with two-way fixed effects of the form $y_{it} = \alpha_i + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + \varepsilon_{it}$ on the sample of RRH/PSH recipients from January 2013 to December 2020. The pre- and post-period coefficients are specified relative to the base-period average at one period prior to the housing event. The pre-post difference coefficient evaluates the difference between the post- and pre-period coefficients. The pre-period includes up to 12 months pre-event, and the post-period extends to 24 months post-events; the sample thus consists of individuals placed into PSH between January 2014 and December 2018. ID-clustered standard errors are shown in parentheses.

Table 3: Event studies (programmatic benefits)

Panel (a): RRH

	(1) SSI	(2) SSDI	(3) TANF	(4) Nonpecuniary benefits	(5) SNAP	(6) Health insurance
Pre-period ($t \leq -2$)	-0.010	-0.005	-0.009	-0.033	-0.025	-0.016
	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)
Post-period ($t \geq -0$)	0.022	0.004	0.024	0.056	0.042	0.037
	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)
Pre-post difference	0.032	0.009	0.033	0.089	0.066	0.053
	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)
Base period average	0.065	0.023	0.108	0.819	0.390	0.849
	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)
Month FE	X	X	X	X	X	X
ID FE	X	X	X	X	X	X
Adj. R-squared	0.90	0.86	0.91	0.91	0.88	0.92
N	1182756	1182756	1182756	1169639	802561	1113778
No. clusters	44081	44081	44081	43609	29357	41758

Panel (b): PSH

	(1) SSI	(2) SSDI	(3) TANF	(4) Nonpecuniary benefits	(5) SNAP	(6) Health insurance
Pre-period ($t \leq -2$)	-0.024	0.0004	0.0005	-0.041	-0.018	-0.029
	(0.005)	(0.003)	(0.001)	(0.006)	(0.006)	(0.005)
Post-period ($t \geq -0$)	0.065	0.009	0.004	0.074	0.037	0.045
	(0.007)	(0.005)	(0.002)	(0.007)	(0.007)	(0.006)
Pre-post difference	0.089	0.008	0.003	0.115	0.055	0.074
	(0.008)	(0.005)	(0.002)	(0.008)	(0.008)	(0.007)
Base period average	0.242	0.067	0.025	0.817	0.459	0.866
	(0.006)	(0.004)	(0.002)	(0.006)	(0.006)	(0.005)
Month FE	X	X	X	X	X	X
ID FE	X	X	X	X	X	X
Adj. R-squared	0.84	0.79	0.90	0.76	0.88	0.80
N	112454	112454	112454	111120	103367	101509
No. clusters	3701	3701	3701	3652	3377	3415

This table displays the coefficients from event study regressions with two-way fixed effects of the form $y_{it} = \alpha_i + \gamma \cdot \mathbb{1}\{EventTime_{it} \leq -2\} + \beta \cdot \mathbb{1}\{EventTime_{it} \geq 0\} + \varepsilon_{it}$ on the sample of RRH/PSH recipients from January 2013 to December 2020. The pre- and post-period coefficients are specified relative to the base-period average at one period prior to the housing event. The pre-post difference coefficient evaluates the difference between the post- and pre-period coefficients. The pre-period includes up to 12 months pre-event, and the post-period extends to 24 months post-events; the sample thus consists of individuals placed into PSH between January 2014 and December 2018. ID-clustered standard errors are shown in parentheses.

4.3 Employment Transitions

We are also interested in how the average changes in earnings and benefits uptake documented in subsection 4.1 and subsection 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. 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.

Since employment fluctuates 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. (Un)employment is defined analogously in the post-period. In this way, there are some individuals that we can say nothing about (i.e. those who were employed for 50% of the pre-period, for instance). Among 12,160 RRH recipients in our data for whom we can precisely estimate pre-period employment, 8474 make U2U transitions, 1175 make U2E transitions, 427 make E2U transitions, and 1047 make E2E transitions. The remaining 1037 have employment fluctuations that are too noisy to say anything about, as noted above. There are no time-invariant demographics characteristics (race, gender, age, education, etc.) that consistently predict transition rates in our sample.[13]

First, we show outcomes related to U2E transitions following RRH enrollment in

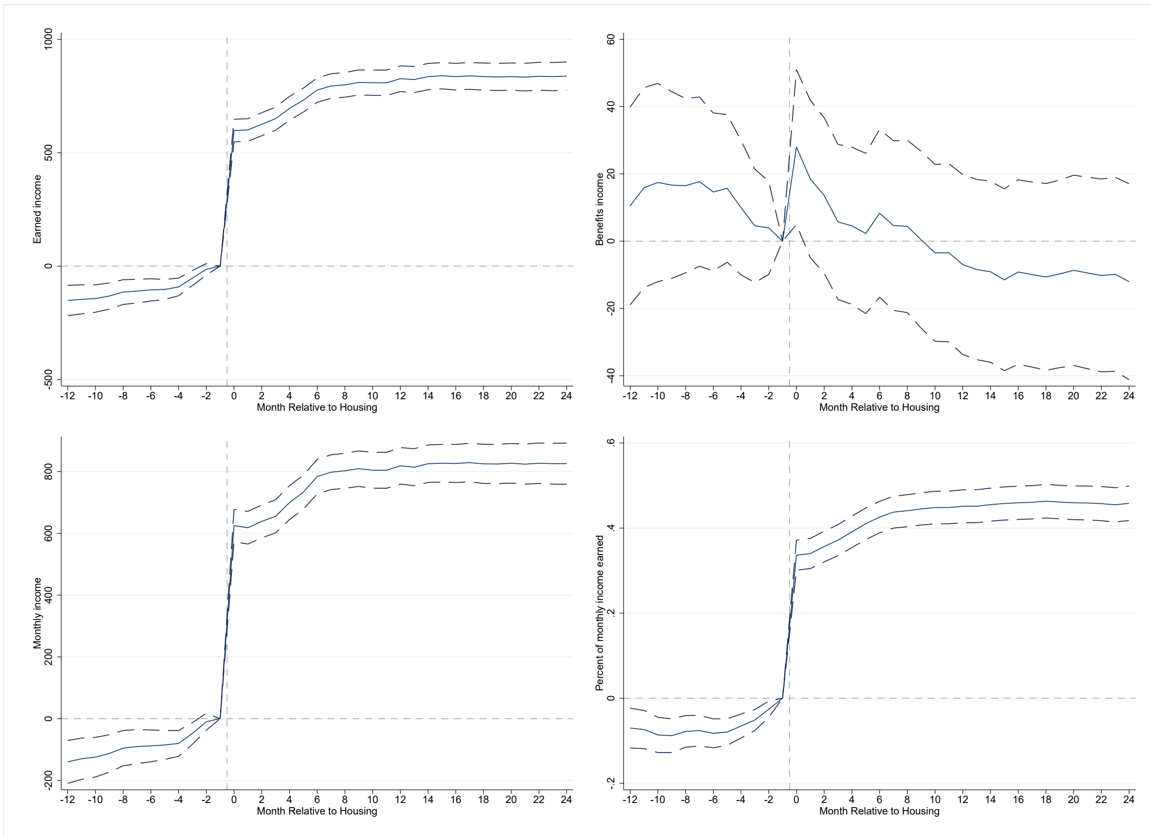
[13] We treat education as time-invariant. We assign each individual their maximum reported education across observations as their "time-invariant" education level.

Figure 6. Individuals characterized by U2E transitions secure employment almost immediately in most cases and see their earnings increase by an average of USD 600-800 per month. This result accounts for the vast majority of the increase in their total monthly income and is nearly three times their average pre-period total monthly income. Benefits income remains nearly constant, which contrasts sharply with the result from subsection 4.1.

Since we cannot disentangle unemployment insurance (UI) income from benefits income, this finding could be the result of two separate sources. One possibility is that, if UI income is a large part of the increase in benefits income documented in subsection 4.1, then the increased benefits income absorbed on average by RRH recipients is likely driven by increased connection to social programming resources. We can rule out the hypothesis that U2E transitioners are more connected to services by the time of RRH recipiency than are U2U transitioners. We observe that individuals making U2U transitions actually report 60 USD more per month (30% more) in benefits income than U2E transitioners. There appear important differences in outcomes between U2U and U2E transitioners.

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 nearly the entire increase in their total monthly income. We are unable to disentangle whether this increase is the result of individuals taking on more hours, a better job, or both, since hours, employer, job title, etc. are not reported. As with U2E transitions, benefits income also remains constant.

Figure 6: RRH U2E Transitions

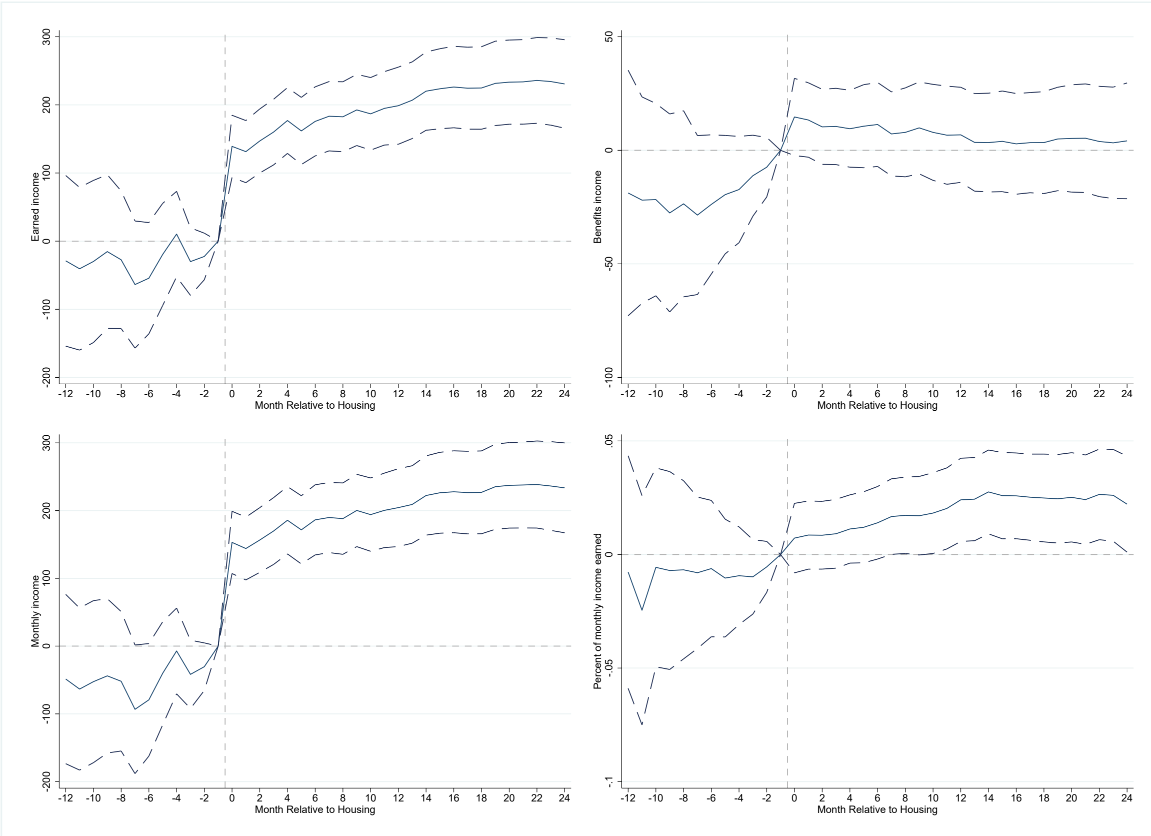


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from 2013 to 2020. The sample is additionally restricted to those who transition between unemployment and employment after the event. Timing is binned up to 13 months prior to and starting 25 months since each individual's housing event; these bins are omitted from the coefficient display

Figure 7: RRH E2E Transitions



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

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

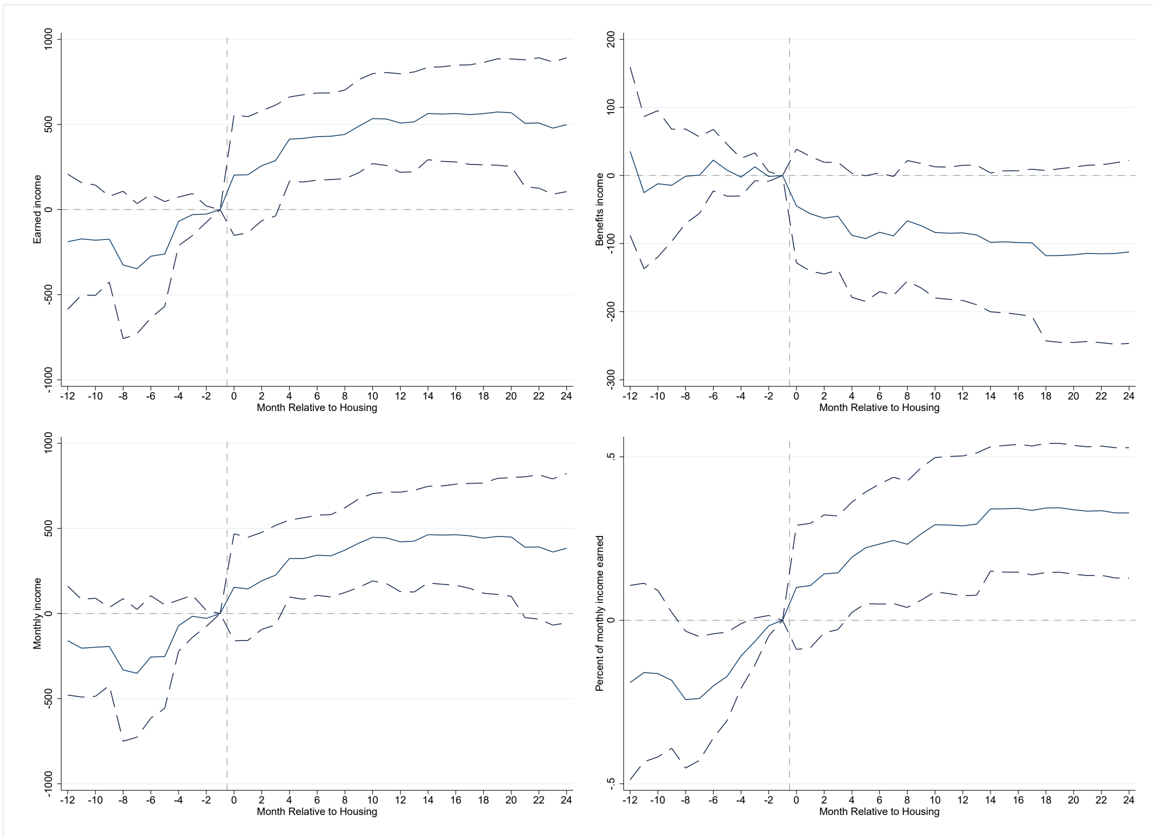
The estimation sample includes individuals receiving RRH between 2014 and 2018; the sample time frame spans from 2013 to 2020. The sample is additionally restricted to those who transition between unemployment and employment after the event. Timing is binned up to 13 months prior to and starting 25 months since each individual's housing event; these bins are omitted from the coefficient display.

Figure 8 and Figure 9 show outcomes following U2E and E2E transitions, respectively, for PSH recipients. Among 2221 PSH recipients in our data for whom we can precisely estimate pre-period employment, 2030 make U2U transitions, 35 make

U2E transitions, 27 make E2U transitions, and 20 make E2E transitions.[14] The outcomes for U2E transitions look like generally dampened versions of the results following RRH interactions, which is unsurprising given the negative selection into PSH. E2E transitions, on the other hand, appear too noisy to draw inference on, which is an artefact of the very low number of initially employed PSH recipients (see Table 1).

[14] 109 individuals are not assigned to any transition group due to their substantial employment fluctuation, as described as the beginning of this section.

Figure 8: PSH U2E Transitions

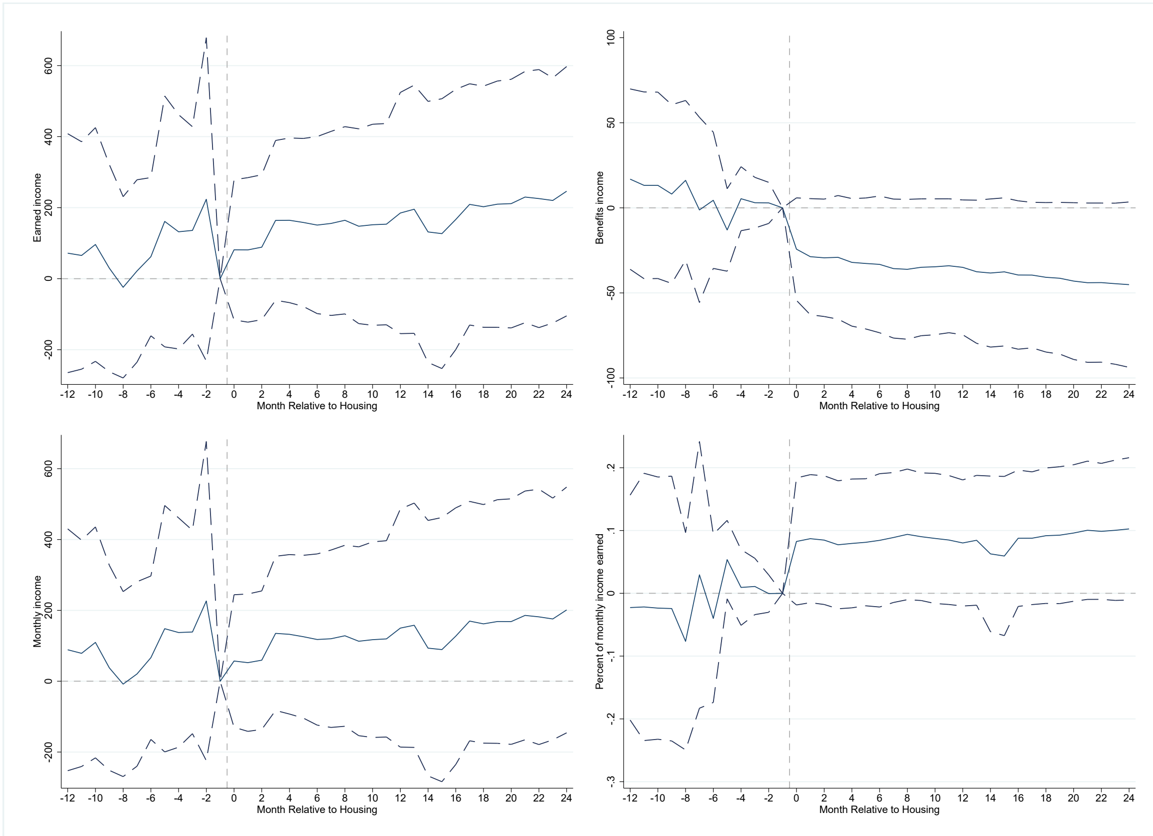


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from 2013 to 2020. The sample is additionally restricted to those who transition between unemployment and employment after the event. Timing is binned up to 13 months prior to and starting 25 months since each individual's housing event; these bins are omitted from the coefficient display.

Figure 9: PSH E2E Transitions



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

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving PSH between 2014 and 2018; the sample time frame spans from 2013 to 2020. The sample is additionally restricted to those who transition between unemployment and employment after the event. Timing is binned up to 13 months prior to and starting 25 months since each individual's housing event; these bins are omitted from the coefficient display.

5 Discussion and Conclusion

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 (8.6 percentage points) in their probability of finding employment. Among individuals that find employment, monthly income increases by around USD 800 with 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 18% 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 from USD 200 to around USD 260 per month.

We document much more heavily muted effects for PSH recipients, likely signaling the differences in treatment unit selection between the two programs. PSH recipients report a smaller increase in probability of finding employment by only 16% (1 percentage point). Instead, PSH recipients see a substantial increase in their benefits absorption upon connection to permanent housing by about 33% from a baseline of USD 360 per month. 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 2.5% of individuals.

5.1 What are the net fiscal impacts of RRH and PSH?

We can apply our novel findings on the labor market impacts of PH to more precisely inform the net fiscal costs and benefits of these programs. We conceptualize the social planner's flow willingness to pay for a homelessness housing program for 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 their program type and employment transition propensity, 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 public medical system usage), and homogeneous social and environmental externalities e (e.g. crime, environmental impacts capitalized into 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 planner's non-welfare-weighted flow willingness-to-pay of

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

i.e. the sum of the differences 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.

While we primarily focus on individual labor market and earnings outcomes, a considerable volume of work has focused on estimating the changes in benefits absorption and social system usage following individual placement into housing. We employ estimates from these prior works to inform our overall cost-benefit calculation, although no estimates yet exist for understanding the environmental externalities of homelessness capitalized into property values (and thereby collected through property taxes).[15]

We begin with RRH recipients that transition from unemployment to employment and proceed by breaking down each term.

We start by assuming that individuals reporting employment earn income in the formal labor market that is subject to general labor income taxes. According to our estimates, individuals finding stable employment increase their annualized earnings from USD 3600 to USD 13200. We assume individuals earn no capital income, and that they pay payroll and sales taxes according to imputations in Piketty, Saez, and Zucman (2017). 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 12500 for single-filers following the Tax Cuts and Jobs Act in 2017), and that 75% of filers claim the EITC, corresponding with publicly available IRS estimates. Eligible EITC claimants in our sample would receive USD 500 on average and pay USD 70 (approximately 10% of 13200 less the standard deduction). Ac-

[15] Additionally, this framework does not consider the general equilibrium effects of homelessness interventions on the rental market. We preliminarily justify this exclusion based on the small size of the homeless population relative to the housed population (less than 0.5%).

cording to Piketty, Saez, and Zucman (2017), these individuals pay a combined 5% and 10% of their income on sales and payroll taxes respectively, generating an additional USD 960 in payroll taxes. Individuals pay an additional USD 480 and 505 in sales tax (for EITC non-claimants and claimants respectively). Therefore, among the group of individuals finding stable employment after placement into RRH, the personal tax payments increase on average by USD 1154 per year.[16]

We project the changes in benefits absorption and social system usage estimated in prior literature homogenously over RRH recipients. We find that average benefits absorption does not change for this group. Estimates from Flaming, Burns, and Matsunaga (2009) report that following placement into housing, medical benefits absorption and jailing costs decrease on average by USD 2280 and USD 2748 per individual-year, respectively, in Los Angeles County.[17]

We assume that housing in Los Angeles County induces a flow rental cost of USD 18,000 per year. We assume that RRH recipiency lasts two years and that 80% of individuals do not recidivate into homelessness again. Finally, we assume that recidivism into homelessness is associated with a complete reversal of observed changes in taxes and benefits absorption and that individuals that do not recidivate into homelessness again see no dynamic earnings or benefits absorption effects (i.e. a non-recidivating individual will earn real USD 13,200 per year for the rest of their working life).[18]

[16]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.

[17]The criminal justice costs do not include the social costs of crimes committed or criminal court costs.

[18]We also assume that homeless individuals do not exit homelessness during this 10-year time horizon.

According to these calculations, we can express the stock willingness-to-pay over a 10-year time horizon of a social planner to allocate RRH to a homeless individual that will gain employment out of an unemployed state as

$$\underbrace{0.8 \cdot \Big(10 \cdot (1154 + 5028) - 2 \cdot (18000)\Big)}_{\text{Does not recidivate after two years}} + \underbrace{0.2 \cdot \Big(2 \cdot (1154 + 5028) - 2 \cdot (18000)\Big)}_{\text{Recidivates after two years}}$$
$$= 15,929,$$

or an average lower bound fiscal benefit of USD 1,600 per person-year (recalling that this figure excludes environmental externalities capitalized into property tax collections as well as the pecuniary value of crime and court procedures).

Following this same procedure for RRH recipients characterized by E2E and U2U transitions[19] we estimate the net cost-benefit of RRH recipiency for these other two groups over a 10-year time horizon at USD 10,855 and USD -3,845 per 10 years per individual respectively.

Combining all of the employment transition type subpopulations of RRH recipients, we estimate the net overall individual-basis fiscal impact of a single unit of RRH recipiency as USD -315 per 10 years per individual. This number should be interpreted as a lower bound cost, given our exclusion of other pecuniary costs and benefits, such as changes in crime and court costs and changes in property tax collections.[20]

[19]We omit E2U transitioners due to their infrequency of observation and suspected likelihood that their transition does not reflect a causal impact of housing, but rather a latent transition probability.

[20]This calculation also assumes that the proportion of U2U, U2E, and E2E transitioners estimated is representative of all RRH recipients in our full sample.

We calculate the net fiscal costs and benefits for PSH recipiency analogously, however assuming PSH tenure lasts indefinitely. We estimate the overall net fiscal impacts of PSH recipiency by subpopulation at USD -104,900, -126,270, and -148,080 for U2E, E2E, and U2U transitioners respectively per 10 years. Overall, the 10-year average individual-basis cost of PSH amounts to USD 147,000.

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 subpopulation underscores the relevance of more recent work on targeting homelessness-prevention and assistance (Wachter et al. (2019)).

Our net fiscal impact estimates above should be interpreted as a lower bound benefit/cost due to our exclusion of some pecuniary costs and benefits—most notably of crime and environmental externalities capitalized into property taxes. Additionally, our estimates mask heterogeneity in health impacts documented in prior work (Flaming, Burns, and Matsunaga (2015)). 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.[21] We also assume no long-run earning growth for U2E and E2E transitioners. As such, we interpret these fiscal impacts as lower-bound estimates.

5.2 Conclusion

We use timing of permanent housing treatments in Los Angeles county to determine the effects of housing the homeless on employment, earnings, benefits absorption,

[21] Specifically, we no longer observe individuals upon their exit from the HMIS. Individuals typically exit the HMIS either upon voluntarily ceasing HMIS benefit absorption or death.

etc. We stratify our analysis by the two primary programs of analysis: RRH and PSH. We find substantial labor market benefits following placement into RRH and relatively null effects for placement into PSH. This contrast likely speaks to differences in selection criteria into each respective program.

Based on these results, we estimate substantial variation in the net fiscal impact of RRH recipiency based on whether an RRH recipient secures employment following housing and whether they recidivate into homelessness in the future. For instance, individuals securing employment following housing generate, in net, around USD 10,000 in public funds per 10 years per individual. Averaged over all of the relevant subpopulations, we estimate the net public cost of placing an individual into RRH at approximately zero Dollars, although this result ignores other pecuniary benefits that would further improve the estimated fiscality of this program.

We find much more unambiguously negative fiscal impacts of placement into PSH. We document that recipients do not on average see improved labor market outcomes, at least in the short run. Combined with our assumption that PSH tenure is permanent, this result implies significantly negative fiscal impacts of placing an individual into PSH of around USD 150,000 per 10 years per recipient, albeit with similar caveats as for our RRH estimates.

Our findings suffer from a number of shortcomings in our data: inability to observe individuals both outside of HMIS and following their exit from the HMIS, sparsity of updates on some outcomes, as well as lack of high-quality individual-level health and crime outcomes. We ultimately interpret our estimates as understating the fiscal benefits of these housing programs. 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 allot greater focus on 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 permanent housing treatment remains a central question in informing our understanding of the overall impacts of these policies.

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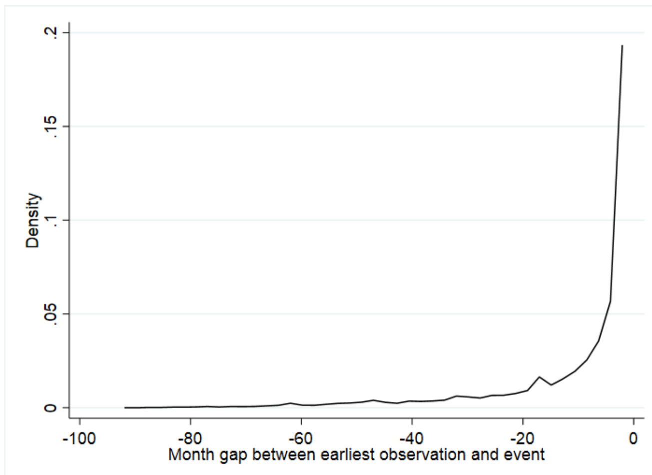
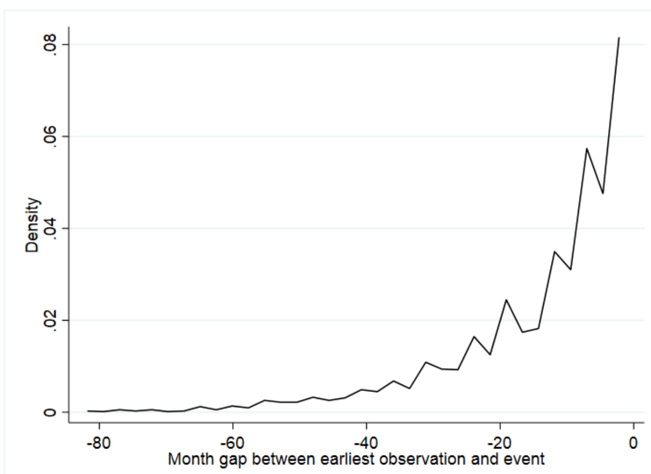
Appendix A Additional figures and tables

Figure A.1: Frequency of gap between housing event and earliest observation

(a) Rapid Re-Housing



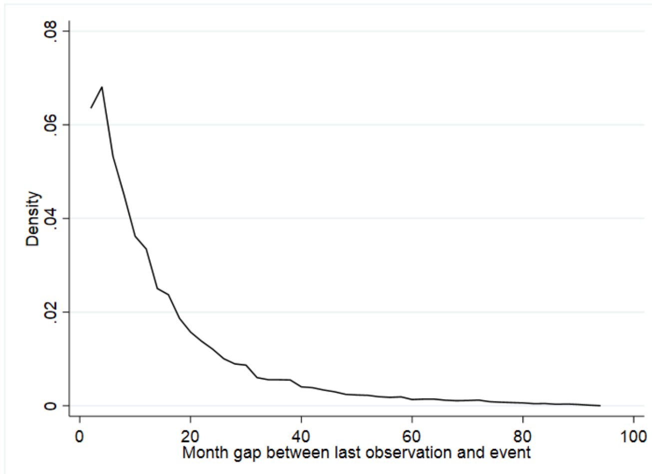
(b) Permanent Supportive Housing



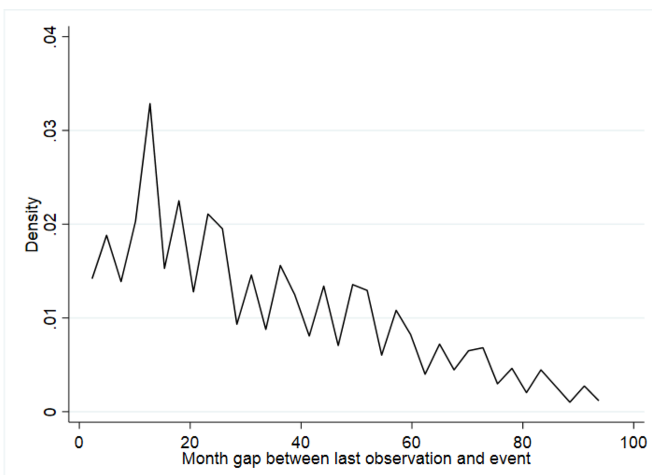
These histograms plot the relative frequency of the time between an individual's housing event and their earliest observation in the HMIS data. For each individual housing recipient, this gap is calculated as $Housing\ event\ month_i - Earliest\ observation\ month_i$. Panel (a) displays this relationship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

Figure A.2: Frequency of gap between housing event and latest observation

(a) Rapid Re-Housing



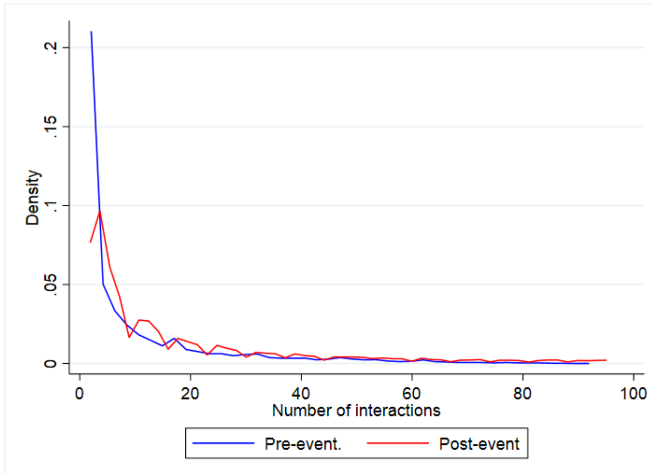
(b) Permanent Supportive Housing



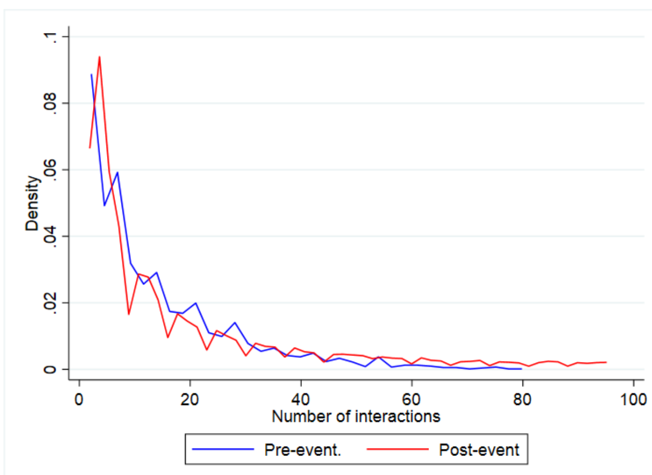
These histograms plot the relative frequency of the time between an individual's housing event and their final observation in the HMIS data. For each individual housing recipient, this gap is calculated as $Latest\ observation\ month_i - Housing\ event\ month_i$. Panel (a) displays this relationship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

Figure A.3: Frequency of number of observations pre- and post-event

(a) Rapid Re-Housing



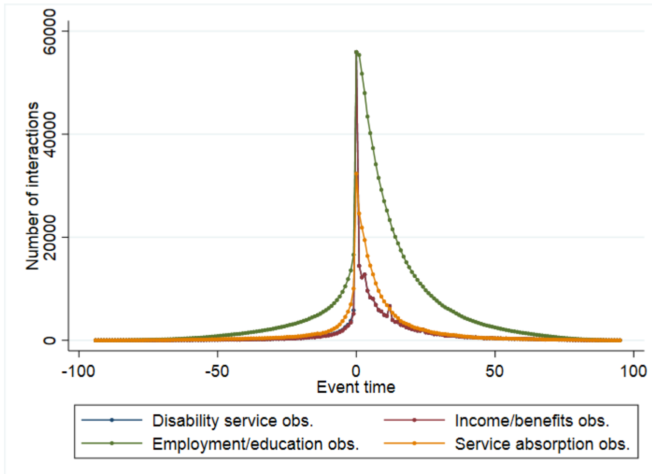
(b) Permanent Supportive Housing



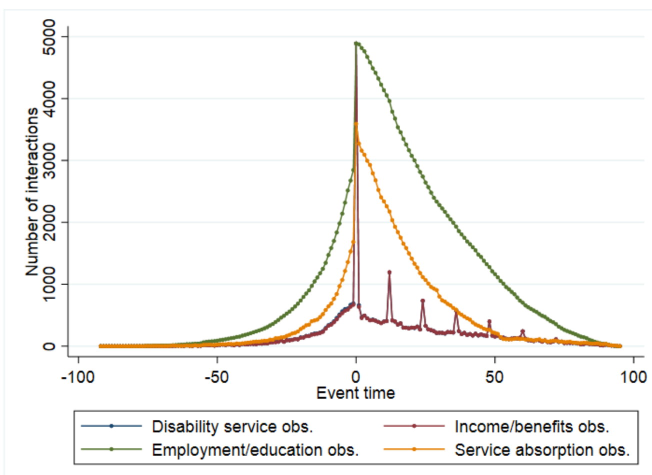
These histograms plot the relative frequency of the number of interactions for each individual, stratifying by pre- and post-event interactions. Panel (a) displays this relationship for Rapid Re-Housing recipients, and Panel (b) studies PSH recipients.

Figure A.4: Interaction types around housing event

(a) Rapid Re-Housing



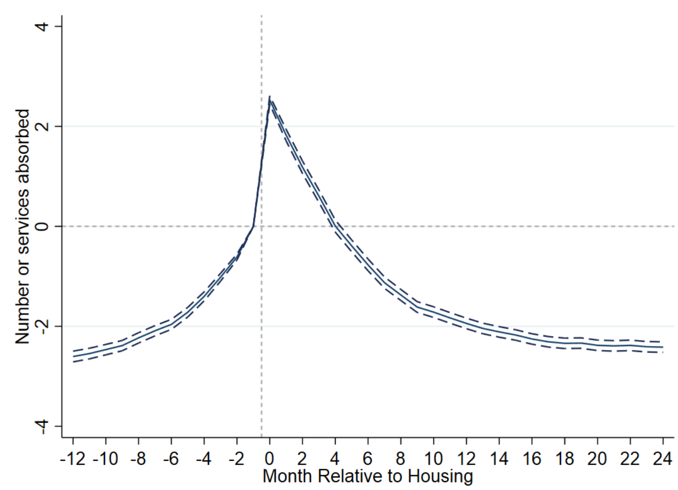
(b) Permanent Supportive Housing



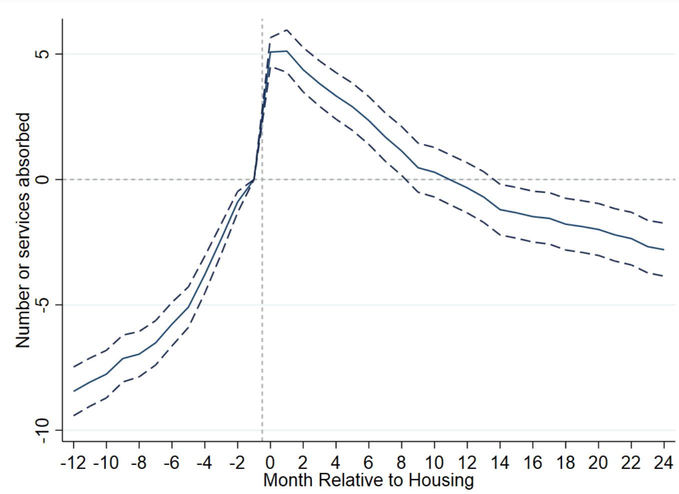
These histograms plot the frequency interactions around individuals' housing events, stratified by the type of interaction (i.e. which HMIS dataset records their interaction). Panel (a) displays this relationship for Rapid Re-Housing recipients, and Panel (b) studies PSH recipients.

Figure A.5: Event study results: number of services absorbed

(a) Rapid Re-Housing



(b) Permanent Supportive Housing

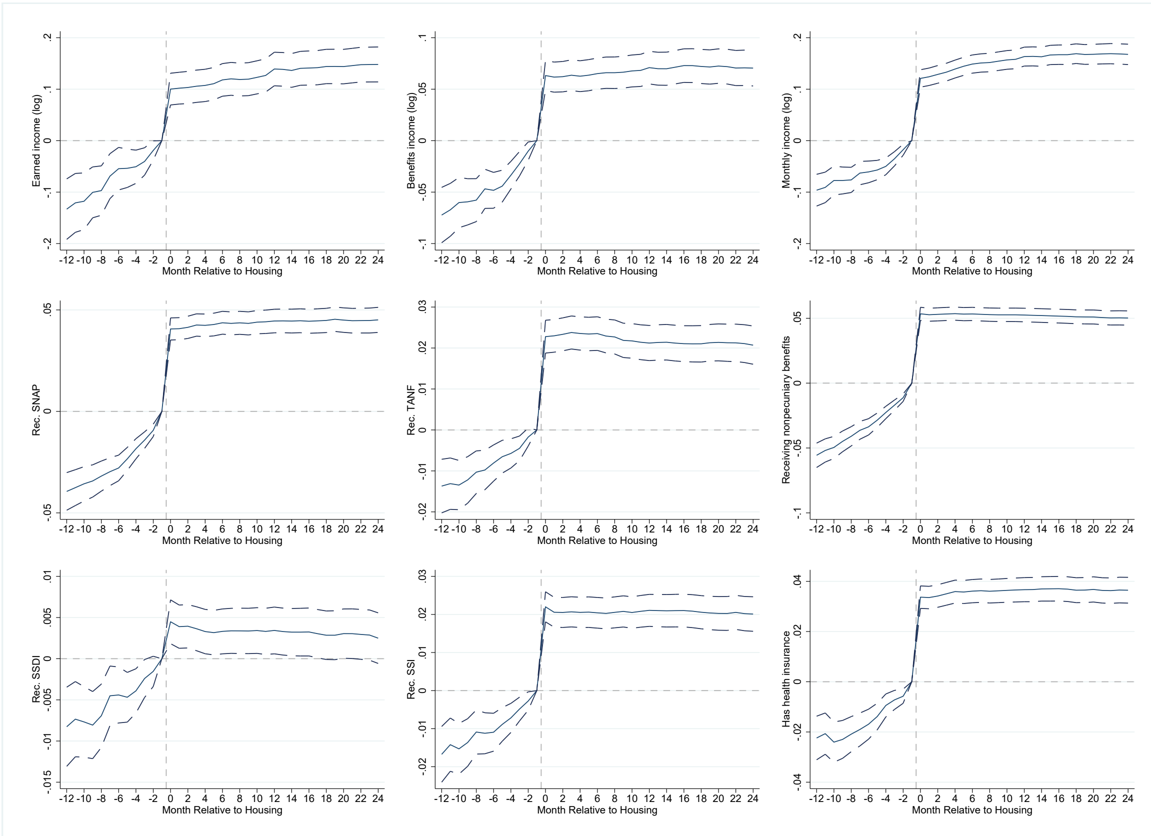


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from 2013 to 2020. Timing is binned up to 13 months prior to and starting 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.

Figure A.6: Other outcomes:
Panel (a): RRH

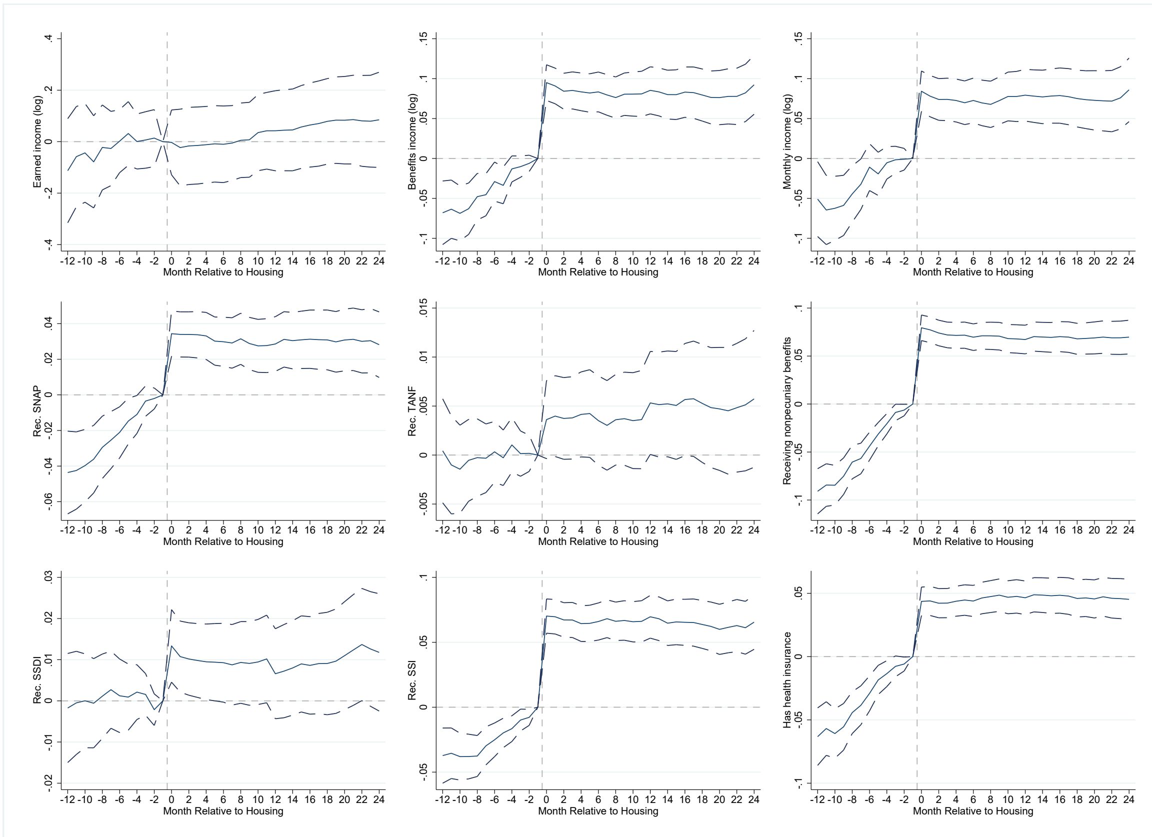


This figure displays the coefficients $\{\beta_j\}$ from the event study specification with two-way fixed effects:

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from 2013 to 2020. Timing is binned up to 13 months prior to and starting 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.

Figure A.6: Other outcomes
Panel (b): PSH



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

$$y_{it} = \alpha_i + \sum_{k=2013m1}^{2020m12} \delta_k \mathbb{1}\{t = k\} + \sum_{j \neq -1} \beta_j \mathbb{1}\{EventTime_{it} = j\} + \varepsilon_{it}.$$

The estimation sample includes individuals receiving housing benefits between 2014 and 2018; the sample time frame spans from 2013 to 2020. Timing is binned up to 13 months prior to and starting 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.

Appendix B 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 impute the missing values using a fairly simple forward projection, with some limitations. 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.