Does Rapid Re-Housing Reduce Homelessness?*

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Abstract

Rapid Re-Housing (RRH) programs provide short-term rental subsidies to help people experiencing homelessness secure market-rate housing. Using linked administrative data from Los Angeles County, we examine the causal effects of RRH on subsequent homelessness, as well as health, crime, public assistance, and labor market outcomes. We compare the outcomes of individuals and families who enrolled in an RRH program and received the subsidy (that is, leased up) to otherwise similar people who enrolled in the same RRH program in the same month but did not lease up. Participants who leased up received an average subsidy of $1,500 per month for seven months, with families receiving more generous subsidies than individuals. Leasing up reduced homeless service use by 28% over four years, including reductions after the end of the subsidy period for both individuals and families. Effects persisted for families, but faded out for individuals after three years. Likewise, leasing up improved health and criminal justice outcomes and increased income from public assistance programs for families, but not individuals. We find no evidence that leasing up reduced employment or earnings for either group based on statewide administrative data. The results indicate that short-term rental subsidies can meaningfully reduce homelessness, especially for families.

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I Introduction

More than 750,000 people in the United States experienced homelessness on any given night in 2024, the highest number on record (de Sousa and Henry, 2024). People who have experienced homelessness have especially poor life outcomes, including very low earnings and even early death (Meyer et al., 2024, 2023). Homelessness is also expensive, with costs up to $80,000 per person annually, including the costs of homeless services, health services, and criminal justice system involvement (Flaming et al., 2015).

The prevailing model to address homelessness is called "Housing First," an approach in which people experiencing homelessness receive permanent housing without requirements such as drug treatment or employment. The Housing First strategy includes two main types of programs: permanent supportive housing and rapid re-housing (RRH). Permanent supportive housing consists of long-term housing subsidies with voluntary supportive services and is typically for people with complex needs. Many randomized controlled trials demonstrate its effectiveness in reducing homelessness (Evans et al. (2021) provide a review). In contrast, RRH provides short-term housing subsidies and is designed for a broader population of people experiencing homelessness. By stabilizing housing in the short run, RRH is intended to provide a foundation that allows participants to improve their longer-term circumstances and ultimately afford housing without a subsidy (Desmond and Gershenson, 2016; Burton et al., 2018).

Despite its increasing use to address homelessness, there is relatively little evidence on the effects of RRH, especially after the end of the subsidy period. The number of RRH beds nationally increased by more than 100,000 between 2014 and 2024 and its share of all permanent housing beds doubled (HUD, 2024, 2014). Because subsidies through RRH are time-limited, typically no more than 24 months, RRH is considered a less expensive and more scalable policy option compared with permanent supportive housing. Particularly in light of potentially dramatic federal changes to funding for Housing First programs, it is critical to understand the effectiveness of potentially lower cost homeless services (NPR, 2025).

This paper provides new evidence on the causal relationship between RRH and homelessness for both individuals and families. We use detailed administrative data from Los Angeles (LA) County on more than 3,500 RRH enrollments in 2019 linked to other county and state agencies, including public assistance, health, crime, and employment records.[1] Our research design compares the outcomes of people who enrolled in an RRH program and received the housing subsidy–a process called "leasing up"–to otherwise similar people who enrolled

[1]The RRH program in LA County was renamed by grant administrators to the time-limited subsidy program. For simplicity, we henceforth refer to the program as RRH.

in RRH with the same provider and in the same month but did not lease up. Close to two-thirds of those who enrolled in RRH in our sample leased up (63%). Importantly for our identification strategy, those who did and did not lease up were similar along more than 30 pre-program characteristics, conditional on provider-by-month fixed effects and indicators for the segments of the homeless services system in LA County (single adults, transition-aged youth, families, and veterans). Plausibly exogenous market factors likely affected whether a participant leased up. For example, the tight housing market and lack of affordable rental units in LA limits the overall supply of appropriate rental units. We document such capacity constraints in the data; there is a strong negative correlation between a participant's probability of leasing up and the number of other participants simultaneously searching for housing with the same RRH provider.

Leasing up with a rental subsidy reduced homelessness over a four year period. Those who leased up received an average of $1,500 per month in rental assistance for 7 months. Our primary homelessness outcome focuses on enrollments in homeless services that indicate a return to homelessness: interim housing, such as emergency shelters or transitional housing, and street outreach, or services for those experiencing sheltered homelessness. Those who leased up were 12.3 percentage points less likely to enroll in an interim housing or street outreach project within four years relative to a comparison mean of 43.9%, a decrease of 28%. The reduction in homeless service use was similar for individuals and families (30% and 25%, respectively). Two supplemental analyses bound the reduction in homelessness between 4 and 20 percentage points. As a lower bound, we compare the outcomes of those who leased up to a positively-selected subset of the comparison group: those who did not lease up and also did not go on to enroll in an interim housing or street outreach project in the year of RRH enrollment. At the other extreme, we employ an examiner assignment design leveraging the quasi-random assignment of RRH caseworkers whose caseloads vary in lease-up rates. Because it is exceedingly likely that caseworkers who are more skilled at getting participants to lease up also improve participant's outcomes in other ways such as providing better case management, this design produces an upper bound. We also show that the reduction in homelessness is unlikely to be driven by differential out-of-county migration and is robust to other alternative designs and samples.

Although RRH was originally designed for people who may only need temporary financial help we find reductions for groups with different baseline levels of needs. Leasing up reduced homelessness for groups with and without prior employment and prior criminal justice involvement, as well as those with lower levels of health needs (no serious mental illness, substance use disorder, or mortality-related diagnosis). We also find suggestive evidence that leasing up reduced homelessness for people with higher levels of health needs, but this

estimate was less precise. Taken together, RRH may be an effective tool for many people experiencing homelessness, including potentially those with higher needs who are believed to need more intensive interventions.

We also find that leasing up decreased broader housing instability by examining effects on a supplemental measure of homelessness from public assistance records. Studies that rely solely on service enrollments undercount true homelessness because they exclude people temporarily living with another person (known as "doubling up"). Some people experiencing homelessness may also not enroll in formal homeless services. In the administrative public benefits data, we observe people's responses to questions about their living situation and mailing addresses from benefits applications and re-certification materials. People can report experiencing homelessness according to federal definitions which include literal homelessness and temporarily doubling up. People experiencing homelessness often also list the benefits office as their mailing address. We combine these indicators to capture housing instability. Leasing up with a rental subsidy reduced housing instability by 8.7 percentage points or 13% overall, with statistically significant reductions again for both individuals and families. We show that these effects are unlikely to be driven by differential benefits receipt; close to 90% of the sample received benefits within four years of enrollment and lease up had only a small and marginally statistically significant impact on benefits receipt.

Particularly for families, leasing up also improved other non-housing outcomes, such as use of costly health services and criminal justice involvement. Families who leased up were less likely to use emergency rooms and crisis stabilization mental health services in the four years after RRH enrollment. They were also less likely to be booked in jail for a misdemeanor offense or have a probation record. We find estimates in the favorable direction along health and crime outcomes for individuals too, but they were generally smaller in magnitude than those for families and statistically insignificant. There was no evidence that lease up changed employment or earnings for individuals or families.

The reduction in homelessness persisted after the end of the subsidy period. As expected given the short-term nature of the intervention, the share of the lease-up group who received rental assistance decreased dramatically over time. Only 11% of the lease-up group still received assistance in the third year after the year of enrollment. Despite the drop-off in subsidy receipt over time, we find a statistically significant decrease in homeless service use in each of the three years after the year of RRH enrollment. Indeed, the decrease in the year of RRH enrollment (32%) was similar in magnitude to the decrease three years later (30%).

There were key differences in persistence for individuals and families, however. Families experienced a sustained decrease in homeless service use even three years after the enrollment year. In contrast, leasing up reduced homeless service use for individuals up to two years

after the year of RRH enrollment–long after the end of the subsidy period–but these effects faded out in the third year after enrollment. One possible explanation for the more persistent effects for families is that families received more generous rental assistance than individuals. Families received about one additional month of assistance and nearly $1,500 more per month than individuals, on average. Consistent with this hypothesis, we find a negative association between subsidy generosity–both in duration and amount–and subsequent homelessness. Another explanation could be that other aspects of people's lives need to change in order to experience long-term reductions in homelessness, such as income or health outcomes. Indeed, leasing up increased income from public assistance programs for families by roughly $1,000 per year over four years, but not for individuals. Leasing up also improved health and reduced criminal justice contact for families only. Taken together, the combination of more generous rental assistance, increased income from public benefits, and potential reinforcing effects between housing stability, improved health, and reduced criminal justice involvement may explain the sustained decrease in homelessness for families. In contrast, for individuals, who received less generous rental assistance and for whom we do not observe improvements in non-housing outcomes, the effects of leasing up on homelessness faded out.

Because existing studies of rapid re-housing (RRH) typically focus on either individuals or families, but not both, the literature has produced a puzzling contrast: RRH appears highly effective for single adults (Cohen, 2024; Ortuzar et al., 2025; Taylor, 2014; Towe et al., 2019; Rodriguez and Eidelman, 2017) yet shows little impact for families (Gubits et al., 2018).[2] This paper contributes a unified analysis that spans both populations, providing a comprehensive view of RRH's effectiveness and helping to reconcile apparent differences in prior findings.

Our study is most similar to Cohen (2024) and we build on its analysis of RRH for single adults in LA County in four ways. First, we expand the analysis beyond single adults to include families and transition-aged youth ages 18 to 24, populations that account for roughly half of RRH enrollments in LA County. Second, we extend the follow-up period, tracking outcomes up to four years after enrollment compared to 20 months in Cohen (2024). This allows us to assess the persistence of effects after rental assistance ends; indeed, we find that effects fade out for individuals in the third year after enrollment. Third, we provide new evidence on mechanisms by linking homeless services data to California employment and earnings records, enabling a direct test of whether RRH improves labor market outcomes and addressing concerns that rental assistance may discourage work. Fourth, the main analysis in Cohen (2024) combines the effects of RRH and permanent supportive housing, whereas

[2]See Byrne et al. (2023) for a review of the literature.

we isolate the effects of RRH.[3] This distinction is critical for policy given differences in cost, duration, and intended populations. These extensions provide a more comprehensive picture of RRH's impacts and underlying mechanisms across a broader population.

Our findings also help to clarify the effectiveness of RRH for families, building on foundational evidence from the Family Options Study. That study randomly assigned more than 2,000 families experiencing homelessness priority access to either a business-as-usual control group or to one of three interventions: long-term rent subsidies, short-term rent subsidies, or transitional housing (Gubits et al., 2018). Long-term rent subsidies were found to be effective in reducing homelessness, but short-term subsidies like RRH and transitional housing were not. However, this apparent null for RRH result may be largely driven by non-compliance: only 60% of families assigned priority to short-term subsidies used them, while 24% of the control group also received such assistance.[4] Adjusting the estimated 14% decrease in shelter entries over 37 months (the only housing stability outcome based on administrative data) for these compliance patterns implies a treatment-on-the-treated estimate of 39% ($= \frac{0.14}{0.60-0.24}$). This is close to the 49% reduction in homeless service use we estimate for families two years after enrollment. This analysis demonstrates that RRH can be effective for families and that differences in prior findings likely reflect study design and implementation rather than fundamentally different program impacts.

More broadly, this study contributes to the growing economics literature on policies to prevent and address homelessness. Prior work shows that evictions have large negative consequences for housing stability, health, and long-term financial outcomes (Collinson et al., 2024), while emergency financial assistance can help prevent homelessness (Evans et al., 2016; Phillips and Evans, 2025). For those already experiencing homelessness, our findings complement rigorous evaluations of permanent supportive housing by demonstrating that RRH can also be an effective and potentially more scalable intervention across a range of populations. The favorable effects we document for families across housing, health, and benefits receipt also suggest the potential for longer-term gains for children, consistent with evidence from other housing interventions and safety net programs (Chetty et al., 2016; Chyn, 2018; Collinson et al., 2025; Hoynes et al., 2015, 2016). Together, these results provide new evidence on how to target and scale housing assistance programs to address homelessness.

[3] Cohen (2024) conducts a supplemental analysis to separately estimate the effects of each program by constructing program-specific measures of caseworker tendencies as instruments. However, the study notes that estimates from IV models with more than one treatment do not have a straightforward interpretation (Kirkoboen et al., 2016; Kline and Walters, 2019).

[4] Non-compliance was less of an issue in the long-term subsidy arm: 83% of those assigned priority received a long-term subsidy compared with 13% of those assigned to the control group.

II Overview of Rapid Re-Housing

RRH programs were created based on the belief that homelessness is primarily driven by high housing costs, and that providing subsidies can effectively address this problem. They consist of three core components: case management, housing navigation services, and time-limited financial assistance. RRH is expected to stabilize housing in the short run, providing people the foundation needed to improve their situations in the longer term so they can afford housing on their own without the subsidy, such as improving their health or employment (Desmond and Gershenson, 2016; Burton et al., 2018). This section summarizes the program in LA County, including the enrollment process and program components. It then describes how the LA model is generally similar to others across the country.

II.A Rapid Re-Housing in Los Angeles County

People are referred to RRH through a variety of pathways depending on the relevant segment of the LA County homeless services system (Wagner et al., 2020). Single adults (that is, adult heads of household without a minor child, including two-adult households) and transition-aged youth between 18 and 24 years old are often referred through the Coordinated Entry System (CES). Community partners, outreach workers, and hospitals also make referrals and some participants self-refer. In contrast, families are referred through a centralized point of intake called Family Solution Centers. Following a referral or entry into the CES, people may be assessed for their vulnerability using a standardized tool: the Vulnerability Index - Service Prioritization Decision Assistance Tool (VI-SPDAT) for single adults, the Next Step Tool for Youth for transition-aged youth, or the Family VI-SPDAT for families. The assessment is intended to occur before RRH enrollment, yet in practice, some people are assessed after enrollment or are not assessed.[5]

To be eligible for RRH, individuals must be literally homeless, fleeing or attempting to flee domestic violence, and earn less than 50% of the median income in the area. Some RRH projects and caseworkers may also apply other eligibility criteria that are not formal requirements, such as having income, a job, or a rental unit ready to move into (Wagner et al., 2020). Program eligibility is regularly re-evaluated with monthly updates to establish that participants continue to have income below the threshold and that they lack sufficient resources and support networks to retain housing without the program.

RRH participants receive case management and housing navigation services upon enrollment. They typically meet with a caseworker at least monthly. Caseworkers connect participants

[5]The assessments are long, can be difficult to administer, and providers may have enforced them differently. The assessments are no longer required in LA County.

to services available in the community, such as public assistance programs, child care, and mental health services. Other aspects of the case management approach vary across providers, such as caseload size and duration. For example, Wagner et al. (2020) found that providers serving transition-aged youth had 20:1 caseloads, compared with up to 60:1 for those serving families. The scope of housing navigation services also varies across providers. In some cases, the caseworker is also the person who provides housing navigation services. But other providers employ distinct staff to identify rental units and develop relationships with landlords. Housing navigation services also range from light support, such as providing participants with a list of potentially available units, to finding available units for participants and accompanying them on visits (Wagner et al., 2020).

Those who leased up received time-limited rental assistance and some also received other financial assistance. Most people can receive rental assistance for up to 24 months, yet those between the ages of 18 and 24 years old can receive it for up to 36 months. Providers have discretion to provide rental assistance for a longer period of time. In practice, however, most people in LA County receive rental assistance for relatively short periods. For example, Wagner et al. (2020) found that 75% of participants were enrolled in the program for less than a year, including nearly half who were enrolled for 6 months or less. Providers apply what is called a progressive engagement strategy to determine both the duration and amount of financial assistance offered to each person. There is no fixed amount or length of rental assistance offered to every participant. For instance, individuals with relatively higher income might be offered less rental assistance than those with lower incomes. In addition to rental assistance, participants can also receive financial assistance for a variety of other housing-related expenses, including security deposits, move-in costs, eviction prevention, and essential furniture. Both rent and other financial assistance is generally provided to a third party rather than directly to the individual.

Importantly for our identification strategy, many RRH enrollees do not ultimately lease-up. Although all enrollees receive case management and housing navigation services, only those who successfully secure a unit receive rental assistance and other financial assistance. In our analytic sample, 63% of enrollees leased up within one year, with lease-up rates ranging from 17% to 100% across providers (Figure A1). Several factors contribute to this variation. A major determinant is the availability of an appropriate rental unit at the time of enrollment, which is shaped by LA's tight rental market and the limited supply of affordable units that meet households' specific needs. Competition among RRH providers for the same pool of units and differences in caseworker effectiveness also influence whether an enrollee ultimately leases up. Individual preferences, such as desired neighborhoods or amenities, or characteristics, such as eviction or credit history, play a role as well, though they operate

alongside these broader market constraints and programmatic features.

II.B Comparison to other Rapid Re-Housing Programs Across the United States

RRH programs are designed locally and vary across the country (Burt et al., 2016), yet the program in LA County is generally similar to others. Dunton and Brown (2019) conducted a survey of all RRH programs in the United States and found that, like LA, most programs participate in a local CES (93%) and serve both individuals and families (68%). All programs include the same three core components of case management, housing navigation services, and time-limited financial assistance. Like LA, nearly all provide rental assistance (97%) and most provide move-in assistance (77%). About three-quarters of programs use a progressive engagement strategy like LA to tailor the amount and duration of financial assistance (73%).

A key distinction between RRH in LA County and nationwide is that the program in LA can provide more generous rental assistance. Only 8% of programs across the country reported providing rental assistance for more than a year; in contrast, RRH in LA provides up to 24 months of assistance for most participants. Even so, as we show in Section III, most people do not participate for the full two years (the average duration for our sample is 7 months). The amount of rental assistance may also be higher in LA County than in other settings, likely because of the high cost of housing in the area. Although the nationwide survey in Dunton and Brown (2019) did not document rental assistance amounts, the typical amount in LA County was considerably larger than in the 12 Family Options Study sites.[6] We may be more likely to find favorable effects of RRH in LA County than in settings with less generous assistance.

At the same time, LA County's tight housing market may make it harder to detect favorable effects than in less constrained settings. LA County has the second largest population of people experiencing homelessness in the country after only New York City (National Homeless Information Project, 2025), though rates of per capita homelessness are in line with other large west coast cities like San Francisco and Seattle. Rental housing costs in LA are also among the highest in the country and rental vacancy rates are among the lowest (Jones, 2024a,b). Taken together, it is perhaps more likely that the average participant in LA would return to homelessness after the end of the program than an average participant elsewhere. In this sense, the favorable effects of RRH in our study could be a lower bound

[6]Monthly program costs across the twelve Family Options Study sites ranged between $563 and $1,388 and rental assistance made up about 70% of program costs, on average (Gubits et al., 2015). In contrast, the average monthly rental assistance for families in our study sample was $2,420.

for the possible effects of the program in other settings.

III Data and Sample

This study benefits from access to detailed administrative data from LA County and California that allow us to connect RRH enrollments to a range of participant outcomes, including returns to homelessness, public assistance receipt, employment and earnings, use of health services, and criminal justice involvement. This section describes the data sources and measures that we use, and describes our analysis sample.

III.A Data Sources and Measures

We use seven sources of administrative data to measure participants' outcomes and background characteristics: (1) homeless services, (2) public assistance, (3) jail bookings, (4) probation, (5) mental health services, (6) medical services, and (7) employment and earnings. The first six sources come from the LA County Chief Information Office's Information Hub, which contains de-identified, individual-level homeless services data linked to records from other county agencies. To build the Information Hub, individuals are matched across agency data using a fuzzy matching algorithm based on personally identifying information, such as full name, date of birth, and social security number (SSN). In addition to encounters with each county agency, the Information Hub also contains demographic information for each person harmonized across the agency files, including age, gender, race, and ethnicity. The seventh data source, employment and earnings records, were available for the entire state of California via a separate linkage between homeless services in LA County and the California Employment Development Department (EDD). EDD conducted the linkage using exact matching based on SSN.[7]

Homeless Services. Data on homeless services are from the Los Angeles Homeless Services Authority which maintains the county's Homeless Management Information System (HMIS). The HMIS includes publicly funded homeless services records between January 2010 and December 2023. It includes basic demographic characteristics, including disability status, and enrollments in five broad types of programs: prevention, street outreach, interim housing, permanent supportive housing, and RRH.[8] Importantly for our study, the HMIS data records

[7] As described later in this section, most RRH participants had available SSNs and we exclude those without available SSNs from our analysis sample.

[8] Prevention services support people at-risk of homelessness through assistance such as help with rent or security deposits, legal aid, and credit repair. Street outreach serves people experiencing unsheltered homelessness. Multidisciplinary teams connect people to housing, health care, and other services. Interim

include detailed information about RRH, including the specific project that participants enrolled into, enrollment and exit dates, whether and when RRH participants moved into a subsidized rental unit, and the amount of rental and other financial assistance participants received through the program.[9]

Our primary outcome is whether RRH participants experienced homelessness based on the HMIS data. We code people as experiencing homelessness if they enrolled in an interim housing or street outreach project in any month after the month of RRH enrollment. Enrolling in one of these homeless services indicates that a person has fallen back into homelessness. We exclude other homeless service enrollments that do not indicate a return to homelessness, like prevention services, or often represent a continuance of services instead of a new experience of homelessness, like enrollment in permanent supportive housing. We examine use of these homeless services at any point within the four years after the month of RRH enrollment, and homeless service use in each year.[10]

Public Assistance. The Information Hub contains public benefits records from the LA Department of Public Social Services (DPSS) between November 2017 and March 2025. It includes monthly indicators for enrollment in safety net programs such as CalWORKs (TANF), CalFresh (SNAP), Medi-Cal (Medicaid), and General Relief. For each program except for Medi-Cal, the data also document the monthly benefit amount. We use this data to measure enrollment in public assistance programs and the dollar amount of benefits received.

We also use public assistance data to create a supplemental measure of housing instability for people enrolled in benefit programs. People can report their living arrangements when they apply for benefit programs by indicating that they are living in an emergency shelter or a place not usually meant as a place to sleep, or staying for fewer than 90 consecutive days with another person or family. People are also required to list their mailing address on benefits applications, with the option of listing a DPSS office address. From conversations with DPSS staff, listing a DPSS office address is a strong signal that the person is experiencing homelessness or an otherwise unstable housing situation. People can technically update their living arrangements or address at any time, but generally update their responses at least annually during a re-certification process. People who enroll in more than one

housing provides temporary housing through emergency shelters, transitional housing programs, and safe havens. There are a relatively small number of interim housing projects in LA County that are not recorded in the county's HMIS data, discussed further in Section VII. Permanent supportive housing offers long-term housing subsidies paired with voluntary supportive services.

[9]Some specific details about RRH enrollments, such as amounts of financial assistance, come from an HMIS data extract that we received separately from LAHSA rather than via the Information Hub.

[10]Section VII discusses the potential for measurement error in this and our supplemental homelessness outcome from public assistance data described below.

benefits program may undergo re-certification at different points throughout the year and may update their information more frequently; for example, in one month for CalWORKs and another month for CalFresh if they applied for each program in different months. Using this information, we construct a supplemental measure of housing instability based on whether people receiving benefits self-reported as experiencing homelessness or listed a DPSS office address as their mailing address. Our housing instability outcome is broader than our primary measure of homelessness based on homeless service use because it includes unstable housing situations beyond literal homelessness (for example, doubling up).

Jail Bookings and Probation. From the LA County Sheriff's Department, the Information Hub includes the universe of bookings in the county jail between January 2010 and April 2025. These data also include the charge codes associated with bookings that distinguish between felonies and misdemeanors. We use these fields to measure criminal justice involvement: whether someone was booked in jail and crime type (felony versus misdemeanor). The Information Hub also includes the universe of individuals under probation supervision in LA County between January 2015 and January 2025 from the Los Angeles County Probation Department. Having a probation record is another indicator of criminal justice involvement.

Mental Health Services. The Information Hub includes records of all publicly funded mental health services between July 2014 and January 2025 from the LA County Department of Mental Health (DMH). We use these data to measure whether someone received an outpatient service or a crisis stabilization service, such as non-law enforcement responses to mental health emergencies and walk-in centers for short-term behavioral health crisis intervention. The data also include billable diagnosis and procedure codes that we use to measure diagnoses related to serious mental illness and substance use.[11]

Medical Services. The Information Hub also contains records of all publicly funded medical services from the Los Angeles County Department of Health Services (DHS) between January 2015 and January 2025. Publicly funded services include those from more than twenty health centers and four major hospitals in LA: Harbor-UCLA Medical Center, Los Angeles General Medical Center, Olive View-UCLA Medical Center, and Rancho Los Amigos National Rehabilitation Center. Although we do not observe privately funded medical services, those that are publicly funded are likely most relevant for the study population of people experiencing homelessness. Based on these data, we measure whether someone received a publicly funded inpatient, outpatient, or emergency department service, and whether someone received a diagnosis related to mortality.[12]

[11] We code serious mental illness and substance use diagnoses based on ICD9/10 diagnosis codes provided by the UCLA Medical School, DMH, and the LA County Department of Health Services.

[12] We code diagnoses related to mortality based on the Elixhauser Comorbidity Index and the Charlson

Employment and Earnings. Quarterly earnings records from EDD allow us to observe the universe of earnings covered by unemployment insurance in each quarter between 1995 and 2025. We measure whether someone was employed after RRH enrollment (coded as whether they had positive earnings) and how much they earned. A key limitation of these data is that we only observe earnings from jobs covered by unemployment insurance. We do not observe earnings from self-employment, under-the-table work, gig work, and other informal employment not covered by unemployment insurance. Although we cannot be certain how important work in the informal sector is for RRH participants, a survey from California suggests that it is a major source of income for all people experiencing homelessness (Kushel et al., 2023). Even so, prior research in LA County confirms that formal sector employment is also relevant; for example, 56% of RRH participants had formal employment in the two years before enrollment, higher than those enrolled in other homeless service types (Von Wachter et al., 2020).

III.B Analysis sample

Our analysis sample consists of heads of households aged 18 and older who enrolled in RRH in LA County in 2019. Focusing on enrollments in 2019 allows us to observe at least one year of pre-enrollment data in all data sources and at least four years of outcome data.[13] In total, there were 5,381 people who enrolled in RRH during this period. If a person enrolled in RRH more than once, we focus on their first enrollment.

We impose three additional restrictions to construct the analytic sample. First, we exclude 1,325 people who were enrolled in permanent supportive housing, RRH, or received housing navigation services before their focal RRH enrollment. Prior receipt of these homeless services likely indicates that the person was transferred into the RRH program administratively rather than a genuine new engagement with the program after a period of experiencing homelessness.[14] Second, we drop 366 people without an available SSN, which was needed for us to link homeless services data with employment and earnings records. Lastly, we exclude 70 people who had an invalid move-in date because we cannot be certain whether

Comorbidity Index. Both indices are well-established measures of a patient's risk of one-year mortality (Charlson et al., 1987; Elixhauser et al., 1998).

[13]The study period overlaps with the COVID-19 pandemic which could affect the generalizability of the results. Section VII includes a check to confirm that our main estimates of lease up on homeless service use are similar for participants who enrolled in 2017.

[14]Because this is our largest sample restriction, we test sensitivity to including people who previously received permanent supportive housing, RRH, or housing navigation services in Section VII and find similar results. We do not restrict the sample based on homeless service enrollments after the focal RRH enrollment; for example, people who do not lease up may go on to enroll in permanent supportive housing or any other homeless service.

they leased up. After applying these restrictions, the analytic sample consists of 3,620 RRH participants.

Table 1 describes the sample. Column 1 consists of all heads of household who used homeless services in LA County in 2019 and Column 2 shows those in our analysis sample who enrolled in RRH. RRH serves a different mix of households than homeless services overall. Those enrolled in RRH were more likely to be in families with a child under age 18 (42% versus 7%) and less likely to be single adults ages 25+ (52% versus 84%) or transition-aged youth (7% versus 9%).[15,16] Those enrolled in RRH were more likely to be Black, non-Hispanic (48% versus 41%) and female (56% versus 41%), less likely to have a physical disability (38% versus 49%), and more likely to be veterans (26% versus 8%). The average age of both groups was about 40 years old, but those enrolled in RRH were about 2.5 years younger, on average. As expected given the typical pathway to RRH, participants were slightly more likely to have previously enrolled in interim housing or street outreach (38% versus 34%). In addition, people enrolled in RRH were far more likely to have received public assistance (83% versus 61%). Consequently, they also received more income from benefit programs, on average, and were more likely to have an unstable housing situation.

Column 3 describes the 63% of people in our analysis sample who leased up. The characteristics of those who leased up were remarkably similar to the full sample of RRH participants in Column 2, with a few exceptions. Those who leased up were slightly less likely to be in families (40% versus 42%), more likely to be Black, non-Hispanic (51% versus 48%), and less likely to be White, non-Hispanic (19% versus 16%). Despite similar rates of public assistance receipt, those who leased up received about $250 less in public assistance in the year prior to RRH enrollment, possibly since a smaller share were members of families. Those who leased up were also somewhat less likely to have unstable housing (60% versus 63%) based on public benefits data.

[15]RRH programs in LA are funded to serve these three distinct groups (single adults, single transition-aged youth, and families). A higher or lower share of these groups may have enrolled in RRH if program funding were allocated differently.

[16]Because of the small number of transition-aged youth in our analysis sample, for the rest of the paper, we combine single adults and single transition-aged youth into one category for those enrolled in the program as individuals, as opposed to families.

Table 1: Summary Statistics

		Analysis Sample	
	LA Homeless Services	Full Sample	Leased-Up
Demographics			
Single Adult (ages 25+)	0.84	0.52	0.53
Single Transition-Aged Youth (ages 18-24)	0.09	0.07	0.08
Family	0.07	0.42	0.40
Hispanic	0.31	0.32	0.32
Black, non-Hispanic	0.41	0.48	0.51
White, non-Hispanic	0.24	0.19	0.16
Female	0.41	0.56	0.55
Age (years)	43.59	41.11	41.39
Disability	0.49	0.38	0.37
Veteran	0.08	0.26	0.27
Homeless Services Before Enrollment			
Interim Housing or Street Outreach	0.34	0.38	0.37
Public Assistance Receipt Before Enrollment			
Any Public Assistance	0.61	0.83	0.82
Amount of Public Assistance ($)	2,173	4,530	4,266
Unstable Housing on Application	0.46	0.63	0.60
Observations	58,672	3,620	2,266

Notes. Column 1 reports the characteristics of all heads of household enrolled in a homeless service in LA County in 2019. Column 2 includes all people in the analysis sample and Column 3 includes the subset of people who leased-up with the RRH program. Single adults include all heads of household ages 25+ without a minor child, including multi-adult households. Single transition-aged youth includes all heads of households between 18 and 24 years old without a minor child. Receipt of interim housing and street outreach services is equivalent to a lagged outcome variable and are measured in the four years before the focal RRH enrollment. Public assistance receipt is measured in the year before the focal enrollment. Percentages may not add to 100 percent due to rounding.

For those who leased up, Table 2 describes participant's experiences with RRH. It took 74 days (2.5 months), on average, between enrollment and moving into a rental unit with the subsidy, though individuals moved in about a month earlier than families. Participants received the subsidy for close to 7 months for a total amount of rental assistance of $10,026, on average, or $1,463 per month. Participants also received about $665 for other expenses, such as move-in costs or essential furniture.[17] The subsidy duration and amount varied

[17]About one-quarter of the lease-up group (27%) leased up again in a subsequent RRH enrollments during the study's four-year outcome period. Many of these are likely to have been administrative transfers from one RRH program to another. Across all RRH enrollments, the average duration of rental assistance was 10.3 months and the average amount of monthly rental assistance was $1,415.

considerably for individuals and families. On average, families received the subsidy for about one month longer than individuals (7.3 versus 6.5 months) and received more than double the amount of monthly rental assistance ($2,420 versus $1,044); see Figure A2 for the full distribution of the duration and amount of rental assistance for each sample. Given their need for more bedrooms, it makes sense that families receive a higher amount of monthly rental assistance than individuals. The amount of monthly rental assistance aligns with HUD's estimates of fair market rent in LA (estimates of 40th percentile rents for units of standard quality) of $1,384 for a 1-bedroom unit, $1,791 for a 2-bedroom unit, and $2,401 for a 3-bedroom unit during the middle of our study period in 2019 (HUD, 2025).

Table 2: Description of Rapid Re-Housing for the Lease-Up Group

	Full Sample	Individuals	Families
Days from Enrollment to Lease-Up	74	62	91
Months of Rental Assistance	6.9	6.5	7.3
Total Rental Assistance ($)	10,026	6,822	17,732
Monthly Rental Assistance ($)	1,463	1,044	2,420
Total Other Financial Assistance ($)	665	665	631
Observations	2,266	1,360	906

Notes. This table describes the experiences of the Rapid Re-Housing program for those in the full sample and the samples of individuals and families who leased-up. Amounts of rental and other financial assistance are missing for 14% of the full sample, including 14% of individuals and 13% of families. To address outliers, the amount of rental and other financial assistance are trimmed at the 5th and 95th percentiles.

IV Empirical Strategy

The central challenge for identifying the causal effects of RRH is that people who participate in the program are different than those who do not participate. Whether someone enrolls in an RRH program is not random. For instance, compared with all people enrolled in homeless services in LA, those enrolled in RRH are more likely to be members of families and receive public assistance (Table 1). Therefore, our empirical strategy focuses on a comparison of more similar groups: those who enrolled in RRH and leased up versus those who enrolled with the same RRH provider around the same time but did not lease up. Members of the comparison group may have received case management and housing navigation services, but did not receive a rental subsidy through the program.

Crucially for our identification strategy, variation in lease-up status can arise from factors

that are plausibly exogenous to a person's housing outcomes. As described in Section II, the probability of lease-up depends heavily on market conditions at the time of enrollment, especially given LA's tight rental market and competition for the same pool of rental units within and across RRH providers. Additionally, quasi-random assignment of RRH caseworkers who vary in their effectiveness at securing units for their clients introduces further idiosyncratic variation in a person's lease-up status. Together, these features generate an element of quasi-randomness in one's lease-up status. Individual preferences (for example, desired neighborhoods or amenities) and characteristics (for example, eviction or credit history) likely also influence lease-up status. To the extent that such preferences or unobserved characteristics are correlated with observable characteristics, we aim to account for these individual-factors through regression adjustment. For example, although we do not observe prior eviction history, it is likely correlated with prior homeless service use. Likewise, we do not observe credit history, but we control for prior public assistance receipt which is presumably related.

To isolate plausibly exogenous variation in lease-up status, our main analysis includes fixed effects to compare the outcomes of RRH participants who enrolled with the same homeless services project around the same time and were served within the same segment of the homeless services system. We include project-by-month fixed effects to compare those enrolled in the same project (that is, an RRH program offered by a provider organization) in the same month. Most people in the analysis sample are in project-by-month cells that include at least one person who leased up and one person who did not lease up (81%).[18] We also include indicators for household type (families with minor children, single adults, and single transition-aged youth) and veteran status because these populations are served by different segments of the homeless service system in LA County and may be prioritized differently, even if served by the same provider.

To identify an unbiased estimate of lease up, our analyses require satisfying a conditional independence assumption (CIA). Conditional on the fixed effects described above, lease-up status must be unrelated to a participant's potential outcomes. The CIA would be violated in our setting if unobserved characteristics affect both whether a person leases up and whether they experience homelessness in the future. For example, an individual's interpersonal skills may help them in the negotiation process with landlords to successfully lease up, and may also help them maintain housing. Another violation of the CIA would arise if a poor credit history or history of eviction both makes them less likely to find a rental unit with the RRH program and puts them at increased risk of future homelessness.

[18]Panel B of Table A12 shows the results are robust to excluding project-month cells without variation in lease up status.

We probe the CIA by conducting balance tests of whether lease-up status is independent of a rich set of observable, pre-program characteristics. As expected, the first column of Table 3 establishes that lease-up status (without the fixed effects) was related to a vector of 30 pre-program characteristics, including demographics, homeless service histories, prior receipt of public assistance programs, prior health services and diagnoses, and criminal justice system involvement. The F-statistic from a joint test is 3.999. The second column shows that lease-up status is no longer jointly related to the covariates after adjusting for the fixed effects (F-statistic = 1.020). The third and fourth columns replicate the same findings, but using the smaller set of covariates available for analysis with the study's administrative employment records (demographics, homeless service histories, and prior employment and earnings). Table A1 lists the covariates included in the joint test as well as results from individual balance tests for each covariate. Out of 32 covariates, only one was statistically significant at the five percent level.[19] The samples of individuals and families were similarly balanced along observable characteristics (Table A2). Therefore, the balance tests show that our design is promising for satisfying the CIA and isolating plausibly exogenous variation in lease-up status.

Table 3: Balance Tests

	All Covariates Except Employment		HMIS and Employment Covariates	
	Unadjusted	Adjusted	Unadjusted	Adjusted
F-Statistic from Joint Test	3.999	1.020	5.405	1.488
p-value from Joint Test	0.000	0.436	0.000	0.107
Observations	3,620	3,620	3,620	3,620

Notes. The unadjusted columns in this table report the results from separate regressions of lease-up on two sets of covariates: (1) all covariates except employment and (2) HMIS and employment covariates. The first two columns include the covariates listed in Table A1, which come from the Homeless Management Information System (HMIS) and other data sources from the Information Hub. The first two columns do not include employment and earnings-related covariates because those records are linked to the HMIS but not other sources in the Information Hub. Therefore, the second two columns include only the covariates listed in Table A1 that come from homeless services records and employment and earnings data from California's Employment Development Department. The adjusted columns include the same covariates as the corresponding unadjusted model and additionally control for project-by-month fixed effects, household type, and veteran status.

We also directly test the notion that plausibly exogenous market factors influence a person's chances of leasing up. If the limited availability of rental units affects a person's

[19]Five other covariates were statistically significant at the 10% level, including a lagged version of our primary homeless service use outcome. As described below, we employ regression adjustment to account for any potential imbalance and implement supplemental research designs to bound the estimated effects on homelessness.

likelihood of leasing up, we would expect a negative relationship between the number of other people enrolled in the same RRH project competing for rental units around the same time as a person and that person's chances of successfully leasing up. We construct a proxy measure of each homeless service project's capacity by following two steps. First, for each observation in the dataset, we count the number of other people searching for a housing unit on the focal person's RRH enrollment date.[20] We then define the capacity of each project at any given time as the maximum count from step 1.[21] As expected, by regressing a person's lease-up status on their project's capacity proxy, there was a negative and statistically significant relationship between a project's capacity and their chances of leasing up (the point estimate is -0.21 with a standard error of 0.07). Furthermore, the project's capacity at the time when someone enrolled was unrelated to their background characteristics using the same joint balance test from column 2 of Table 3 but with the capacity proxy as the dependent variable instead of lease-up status (F-statistic = 0.862 and p-value = 0.683).

Our main specification takes the following form:

$$Y_i = \beta_0 + \beta_1 LeaseUp_i + \beta_2 X_i + \delta_{pm} + \theta_i + \epsilon_i \tag{1}$$

where Y_i is the outcome for person i and $LeaseUp_i$ is an indicator equal to one if the person leased up with an RRH rental subsidy within one year of enrolling in the program. X_i is a vector of individual-level covariates that we include to account for the small imbalance in observed, pre-program characteristics between the lease-up and comparison groups. For most specifications, X_i includes the demographic characteristics and prior service history variables listed in Table A1, except for prior employment and earnings that are not available in the Information Hub. $\delta_p m$ and θ_i represent the project-by-month fixed effects and fixed effects for household type and veteran status; and ϵ_i is an error term.

V Results

This section presents new evidence on the causal effects of leasing up with a short-term rental subsidy through the RRH program on homelessness, including sources of heterogeneity, and effects on health and crime outcomes.

[20] We define 'searching for a housing unit' as the period between the RRH enrollment date and the lease-up date.

[21] The mean of the proxy is 0.70, suggesting that the average homeless services project operates at about 70 percent capacity at any given time.

V.A Main Findings on Homelessness

Table 4 shows the effects of lease up on experiencing homelessness at any point within the four years after RRH enrollment for the full sample (Panel A), and the samples of individuals (Panel B) and families (Panel C). Standard errors are shown in parentheses and the adjusted comparison group means are reported in curly brackets. The first column reports the effects on our main homelessness outcome of subsequent enrollments in an interim housing or street outreach project. The second column shows effects on our supplemental measure of unstable housing based on public benefits applications.

Leasing up through the RRH program reduced homelessness. Those who leased up were 12.3 percentage points less likely to enroll in an interim housing or street outreach project within four years, a 28% reduction relative to a comparison mean of 43.9% (Table 4). The estimate is statistically significant at the 1% level.[22] The estimates for individuals and families were both statistically significant and similar in magnitude: 11.8 and 12.1 percentage point reductions, respectively. However, because individuals in the comparison group were less likely to enroll in interim housing or street outreach projects than families in the comparison group, the percentage reduction in homelessness was larger for individuals than families (30% versus 25%).

In terms of the number of days that people experienced homelessness, leasing up reduced the amount of time that people were enrolled in interim housing or street outreach projects by 97 days, a 60% decrease compared with a comparison mean of 163 days (Table A4). The decrease was larger for interim housing than street outreach projects, at 76 and 21 days, respectively.[23] Families experienced a larger drop in the number of days enrolled in homeless services (116 days versus 77 days). However, the comparison mean for individuals was lower than for families and the percentage decrease was nearly identical for both groups (57% for individuals versus 59% for families).

Our findings on leasing up with a short-term rental subsidy are smaller, but within the range of what has been found in prior studies of RRH for families and single adults. The Family Options Study, which randomly assigned more than 2,000 families to various housing interventions, found that priority for a short-term rental subsidy reduced shelter entries (the only primary housing outcome measured in administrative data) by 14% between months 21 to 32 after random assignment. However, this ITT estimate was imprecise in part because of noncompliance. There was only a 36 percentage point difference in take-up of the short-term

[22]Column 3 of Table A3 presents the same impact estimate along with the regression coefficients and standard errors for all covariates.

[23]The amount of time enrolled in a street outreach project may understate the length of unsheltered homelessness because people may not enroll in such services immediately upon experiencing unsheltered homelessness.

subsidy between families assigned priority for it and control group families. A back of the envelope scaling implies a treatment-on-the-treated estimate of 39% (= 0.14/0.36), a bit higher than the 25% decrease that we estimate for families over the four-year follow-up period. Likewise, in heterogeneity analyses by program type, Cohen (2024) found that enrolling in the RRH program reduced homelessness by 54% at 20 months for single adults at the margin of enrolling in RRH, again higher than the 30% reduction that we find for individuals. Our results could be smaller because we identify the effects of leasing up with a rental subsidy, net of any potentially favorable effects of other aspects of RRH like case management.

We also find that lease up reduced our supplemental measure of unstable housing based on applications for public benefit programs. Leasing up reduced unstable housing by 8.7 percentage points for the full sample, or a 13% reduction relative to a comparison mean of 68.4%. Both individuals and families experienced statistically significant decreases in unstable housing equal to 7.9 percentage points (or 13%) and 9.4 percentage points (or 12%) respectively. As we will show in Section VI, lease up led to a small increase in benefits receipt which could bias findings on unstable housing based on benefits data. To guard against the possibility that selection drives the estimated reduction in unstable housing, we take the very conservative approach of coding those who do not receive benefits as experiencing stable housing (that is, we code those who did not receive public benefits as zeroes for this outcome). Section VII provides further discussion of the potential for measurement error in this outcome.

Table 4: Effects of Lease-Up on Homelessness and Unstable Housing Within Four Years

	Enrolled in Interim Housing or Street Outreach	Unstable Housing on Benefits Application
Panel A: Full Sample (N = 3,620)		
Leased Up	-0.123***	-0.087***
	(0.017)	(0.014)
	{0.439}	{0.684}
Panel B: Individuals (N = 2,116)		
Leased Up	-0.118***	-0.079***
	(0.025)	(0.021)
	{0.396}	{0.612}
Panel C: Families (N = 1,504)		
Leased Up	-0.121***	-0.094***
	(0.026)	(0.021)
	{0.492}	{0.781}

Notes. Each panel and column reports estimates from a separate regression of the outcome on lease-up. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

There are several possible reasons why the effects of lease up on housing instability are smaller than the effects on homeless service use. The definition of housing instability on public benefits applications is broader than enrolling in an interim housing or street outreach project. Housing instability includes doubling-up. Therefore, leasing up with a rental subsidy may have shifted some people away from experiencing literal homelessness and into situations of doubling up, while still reducing housing instability overall. Institutional factors in the benefits application process could also play a role. Changes to the unstable housing outcome require that people update their housing status and address information with DPSS. While these changes are prompted by a regular recertification process, there is no strict requirement for people to respond truthfully, which means there could be people whose changes in housing status are not observed in the data. In addition, the unstable housing

indicator is not just a measure of housing status, but also a measure of people's subjective confidence in that status. People must feel secure enough in their housing in order to record it with the government agency supplying important benefits. If lease up successfully keeps someone housed, but they still feel subjectively uncertain about their long-term stability, then they may not feel confident enough to update their address information with DPSS. Finally, because we take the conservative approach of counting people who did not apply for a benefits program as having stable housing situations, we may be understating the true reduction in housing instability.[24]

V.B Subgroup Analyses

We test whether the effects of leasing up through the RRH program on our main homelessness outcome differed for people expected to be at higher and lower risk of homelessness. RRH has historically been thought to work best for people experiencing homelessness with relatively lower needs (Burt et al., 2016). Consistent with this notion, we find that leasing up reduced homelessness for those without known risk factors for homelessness, including complex health needs, unemployment, and prior criminal justice contact (Nilsson et al., 2019). For example, Table 5 shows that for those with lower levels of health needs (defined as those without a diagnosis related to serious mental illness, substance use, or mortality before the program), lease up reduced homeless service use by 13.9 percentage points (33%). There were similar reductions for those with prior employment and no prior criminal justice contact. Counter to this notion, however, we find evidence that RRH can also be effective in reducing homelessness for those with higher levels of needs, including those without formal employment or with criminal justice contact prior to RRH enrollment. We also find suggestive evidence that RRH reduces homelessness for people with higher levels of health needs. The point estimate for this group indicates a reduction in homelessness, but it is statistically significant only at the 10% level and about half the magnitude as the lower health needs group after considering the different base rates.

There is also policy interest in understanding whether the effects of lease up vary by race and ethnicity given well-documented disproportionalities in homelessness (Evans et al., 2021). We find that leasing up reduced homelessness within four years for Black, Hispanic, and White people (Table A5). The magnitude of the reduction varied, with the largest decrease for White people (42%) compared with decreases of 26% and 29% for Black and Hispanic people, respectively.

[24]The remainder of the study focuses on our primary measure of homelessness based on interim housing and street outreach enrollments because of the potential for selection into benefit programs in the supplemental housing instability outcome.

Table 5: Effects of Lease-Up on Homeless Service Use, by Level of Health Needs, Prior Employment, and Criminal Justice Contact

	Health Needs		Prior Employment		Prior Criminal Justice	
	Lower Needs	Higher Needs	Employed	Unemployed	No Contact	Contact
Leased Up	-0.139***	-0.081*	-0.115***	-0.141***	-0.104***	-0.139***
	(0.020)	(0.044)	(0.022)	(0.033)	(0.020)	(0.042)
	{0.420}	{0.513}	{0.415}	{0.483}	{0.407}	{0.502}
Observations	2,794	826	2,312	1,308	2,656	964

Notes. Each column reports estimates from a separate regression of whether the person enrolled in a street outreach or interim housing project on lease-up within four years. Higher health needs indicates that the person had a diagnosis relates to a serious mental illness, substance use, or mortality in the four years before RRH enrollment based on Department of Mental Health records. Prior employment status is based on having positive earnings in the four years prior to enrollment based on the administrative earnings records. Prior criminal justice contact indicates whether the person was booked in jail or had a probation record in the four years before enrollment in the program. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

V.C Findings on Health and Crime

In addition to effects on homelessness, we examined the effects of leasing up with a short-term rental subsidy on health services utilization and criminal justice system involvement. Panel A of Table 6 shows that lease up reduced the use of health services within four years. Lease up reduced the use of emergency rooms in publicly funded hospitals by 3.5 percentage points (22%), without changing the use of inpatient or outpatient hospital services. It also reduced the use of crisis stabilization mental health services. Together, this pattern of findings suggests improved health outcomes for families. Comparing the effects of lease up for individuals in Panel B to those for families in Panel C shows that the improvement in health was driven by families. For families, lease up led to a statistically significant reduction in the use of emergency rooms and crisis stabilization mental health services, whereas the estimates for individuals were smaller and less precise.

We found the same pattern of improved non-housing outcomes for families, but not individuals, when focusing on criminal justice involvement. Table 7 shows that families who leased up were 61% less likely to have a probation record within four years than those who did not lease up. Lease up also may have reduced jail bookings for families, especially for misdemeanor offenses. Like the findings for health services, the estimates for individuals are generally also in the favorable direction, but are not statistically significant.

Table 6: Effects of Lease-Up on Use of Health Services Within Four Years

	Department of Health Services			Department of Mental Health		
	Emergency Room	Inpatient	Outpatient	Any Service	Outpatient	Crisis Stabilization
	Panel A: Full Sample (N = 3,620)					
Leased Up	-0.035**	-0.012	-0.001	-0.031*	-0.031*	-0.022**
	(0.014)	(0.008)	(0.012)	(0.016)	(0.016)	(0.009)
	{0.161}	{0.050}	{0.121}	{0.328}	{0.316}	{0.061}
	Panel B: Individuals (N = 2,116)					
Leased Up	-0.027	-0.005	-0.008	-0.028	-0.025	-0.018
	(0.020)	(0.011)	(0.016)	(0.020)	(0.020)	(0.012)
	{0.164}	{0.042}	{0.117}	{0.283}	{0.264}	{0.067}
	Panel C: Families (N = 1,504)					
Leased Up	-0.048**	-0.021*	0.007	-0.028	-0.030	-0.028**
	(0.021)	(0.014)	(0.020)	(0.028)	(0.028)	(0.013)
	{0.158}	{0.061}	{0.127}	{0.388}	{0.385}	{0.051}

Notes. Each panel and column reports estimates from a separate regression of the outcome on lease-up. Panel A includes the full sample while Panels B and C report results for individuals and families, respectively. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effects of Lease-Up on Use of Criminal Justice Involvement Within Four Years

		Jail Booking Type		
	Jail Booking	Felony	Misdemeanor	Probation
	Panel A: Full Sample (N = 3,620)			
Leased Up	-0.033**	-0.017	-0.023*	-0.007
	(0.015)	(0.012)	(0.013)	(0.006)
	{0.222}	{0.127}	{0.169}	{0.027}
	Panel B: Individuals (N = 2,116)			
Leased Up	-0.024	-0.021	-0.004	-0.001
	(0.020)	(0.017)	(0.018)	(0.008)
	{0.220}	{0.136}	{0.163}	{0.025}
	Panel C: Families (N = 1,504)			
Leased Up	-0.046*	-0.017	-0.046**	-0.019**
	(0.024)	(0.019)	(0.021)	(0.008)
	{0.226}	{0.119}	{0.175}	{0.031}

Notes. Each panel and column reports estimates from a separate regression of the outcome on lease-up. Panel A includes the full sample while Panels B and C report results for individuals and families, respectively. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VI Mechanisms

The results presented so far show that leasing up through the RRH program reduced homelessness. For families, it also led to improvements in health and lower rates of criminal justice involvement. This section explores potential mechanisms through which these effects operate. We examine effects on homelessness before and after the end of the subsidy, whether subsidy amount and duration play a role, and how lease up up affected employment, earnings, and income public benefit programs.

VI.A Effects During and After the Subsidy

Because receipt of the rental subsidy is time-limited, it is important to assess the extent to which the effects on homelessness persist after the end of the subsidy period. For this analysis, we first document trends in subsidy receipt to establish that most people receive the subsidy only in the year they enrolled in the program. We then estimate the effects of lease up separately in the four years after enrollment.

Table A6 shows that all people in the lease-up group received the subsidy in their year of enrollment, by construction, but subsidy receipt decreased dramatically thereafter. In the first year after the enrollment year, only 31% received the subsidy, including 26% of individuals and 39% of families. Although the subsidy is intended to last up to 24 months for most people, 11% of people in the lease-up group received the subsidy in the third year after the enrollment year, including 9% of individuals and 15% of families. Some of these people may have re-enrolled in RRH after their initial enrollment ended.

Because members of the comparison group could eventually go on to lease up through the RRH program after the enrollment year, we next test the effects of lease up (in the year of enrollment) on receipt of an RRH rental subsidy by year. The findings in Table A7 confirm the steep drop off in subsidy receipt over time. Panel A shows that about 16% of people in the comparison group went on to the receive the RRH rental subsidy in the first post-enrollment year. Even so, those in the lease-up group were twice as likely to receive the subsidy that year. By the third year post-enrollment, those who initially leased up were only 67% more likely to receive the subsidy relative to a comparison mean of 7%.[25] Receipt of the subsidy in the third year post-enrollment was driven entirely by families. Individuals who initially leased up were no more likely to receive the subsidy three years after the enrollment year than those who did not initially lease up.

Having established the drop off in subsidy receipt after the year of enrollment, we now turn to the question of whether the reduction in homelessness persisted after the end of the subsidy period. Table 8 shows that there was a statistically significant reduction in interim housing and street outreach enrollments in each of the four outcome years. The percentage-point effect was largest in the year of enrollment, an 11 percentage point reduction, and smallest three years after enrollment year, a 3 percentage point decline. Yet those who did not lease up were also less likely to enroll in homeless services over time and the percentage reduction in homelessness was somewhat similar in the year of enrollment (32%) and three years later (29%). Overall, lease up led to a persistent reduction in the use of interim housing and street outreach services, even in the later years after the end of the

[25] Those in the lease-up group were also no more or less likely than the comparison group to have enrolled in a permanent supportive housing project within the four year outcome period (Table A8).

subsidy period. There were key differences in the persistence of the effects for individuals and families. For individuals, the effects of lease up faded out, with no difference in homeless service use between individuals who did and did not lease up in the third year after the year of enrollment. In contrast, families who leased up experienced a sustained reduction in homelessness. In fact, the magnitude of the reduction in homeless service use for families was larger in the third year after the enrollment year than in the year of enrollment when all families received the rental subsidy (49% versus 27%).[26]

[26]Although we cannot entirely rule out that the longer term effects for families are driven by those 15 percent of families who were still receiving the subsidy in the third year after enrollment, we conduct a supplemental analysis to confirm the persistence of effects for families after the end of the subsidy period. For families, we re-estimate impacts on both subsidy receipt and homeless service use by year using the 2017 cohort of RRH participants instead of our main 2019 sample. With the 2017 cohort, families who leased up were no more or less likely to continue receiving rental assistance after the first year post-RRH enrollment (Panel A of Table A9). The timing dynamics may have been different for the earlier cohort because RRH around that time was typically offered for shorter durations (Wagner et al., 2020). Despite no impacts on subsidy receipt in the second and third years post-enrollment, families who leased up were still less likely to enroll in interim housing or street outreach projects (Panel B of Table A9). Therefore, we expect the reduction in homelessness to persist long after the end of the subsidy period for families.

Table 8: Effects of Lease-Up on Homeless Service Use Over Time

	Enrolled in Interim Housing or Street Outreach			
	Year of Enrollment	One Year After	Two Years After	Three Years After
	Panel A: Full Sample (N = 3,620)			
Leased Up	-0.109***	-0.077***	-0.046***	-0.026**
	(0.016)	(0.011)	(0.010)	(0.010)
	{0.343}	{0.124}	{0.097}	{0.089}
	Panel B: Individuals (N = 2,116)			
Leased Up	-0.097***	-0.059***	-0.042***	-0.017
	(0.022)	(0.016)	(0.015)	(0.014)
	{0.277}	{0.131}	{0.105}	{0.089}
	Panel C: Families (N = 1,504)			
Leased Up	-0.115***	-0.091***	-0.038**	-0.045***
	(0.024)	(0.016)	(0.015)	(0.017)
	{0.430}	{0.107}	{0.078}	{0.092}

Notes. Each panel and column reports estimates from a separate regression of enrollment in an interim housing or street outreach project in the year on lease-up. The year of enrollment indicates the year of RRH enrollment. Panel A includes the full sample while Panels B and C report results for individuals and families, respectively. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VI.B Effects of Subsidy Amount and Duration

One reason why leasing up may have led to a more persistent reduction in homelessness for families than individuals could be because families received more generous rental assistance. Families received about one more month of the subsidy and more than twice the amount of rental assistance each month than individuals (Table 2). To test this hypothesis, we conducted separate analyses replacing the binary treatment indicator of lease-up status in our regression models with: (1) the amount of rental assistance received and (2) the number

of months of rental assistance.[27] Table A10 shows that participants who received larger or longer rental subsidies were less likely to enroll in interim housing or street outreach projects within four years. For example, an additional $1,000 of total rental assistance was associated with a 0.2 percentage point decrease in the likelihood of using homeless services. These findings suggest that receipt of a more generous rental subsidy may play a role in explaining why families experience more persistent, favorable effects than individuals, with the caveat that subsidy generosity (conditional on subsidy receipt) may be endogenous.

VI.C Effects on Employment, Earnings, and Income

Receiving an RRH rental subsidy might reduce the chances of subsequent homelessness by addressing barriers to affording housing costs. For example, stable housing could provide the foundation needed to focus on finding or keeping a job. In turn, improved labor market outcomes can help people afford housing on their own without the subsidy. On the other hand, as with any public benefits program, there may be a concern that offering people a rental subsidy reduces the incentive to work. Indeed, the Family Options Study found that priority access to long-term housing subsidies slightly reduced employment for families (Gubits et al., 2018).

To investigate these channels, we first test the effects of leasing up on labor market outcomes. The first two columns in Table 9 show that lease up did not affect either employment within four years or average quarterly earnings over the four years post-enrollment. The point estimates on employment and quarterly earnings are statistically insignificant for the full sample and for the samples of individuals and families. This analysis includes formal employment only; we cannot speak to potential changes in outcomes from self-employment, gig work, or other informal work that may be important to people enrolled in RRH.

Despite no change in earnings from formal employment, lease up may have increased the likelihood of receiving public benefits and had a statistically significant effect on the dollar amount of benefits received (Table 9). The overall effect on benefits receipt within four years was small (1.7 percentage points or a 2% increase) and statistically significant only at the 10 percent level. However, over four years, those who leased up received an additional $2,048 from benefit programs, on average, or a 9% increase relative to a comparison mean of $23,113. The increase in benefits issuances was driven by families, who received an average of more than $4,000 in benefits over the four years. Table A11 shows this was mostly from

[27] This analysis focuses on each participant's initial RRH enrollment during the sample period to allow for a more straightforward comparison of these results to the study's main findings. All members of the lease-up group have positive values for the new continuous treatment variables. Some comparison group members who took more than a year to lease up in their focal RRH enrollment also have positive values. The analysis excludes the 14% participants who leased up but were missing financial assistance data.

CalFresh. The effect was smaller and statistically significant at the 10% level for individuals. That leasing up caused families to see increased total income more so than individuals may contribute to why the reduction in homelessness persisted for families and faded out for individuals.

Table 9: Effects of Lease-Up on Employment, Earnings, and Benefits Within Four Years

	Employed	Quarterly Earnings ($)	Benefits Receipt	Benefits Amount
	Panel A: Full Sample (N = 3,620)			
Leased Up	0.015	122	0.017*	2,048***
	(0.016)	(122)	(0.010)	(759)
	{0.615}	{2,156}	{0.893}	{23,113}
	Panel B: Individuals (N = 2,116)			
Leased Up	0.005	15	0.026	1,100*
	(0.023)	(173)	(0.016)	(596)
	{0.549}	{2,030}	{0.845}	{11,347}
	Panel C: Families (N = 1,504)			
Leased Up	0.013	121	0.009	4,143***
	(0.025)	(170)	(0.008)	(1,595)
	{0.718}	{2,430}	{0.956}	{39,262}

Notes. Each panel and column reports estimates from a separate regression of the outcome on lease-up. Panel A includes the full sample while Panels B and C report results for individuals and families, respectively. All regressions control for project-by-month fixed effects, household type, and veteran status. Analyses of employment and quarterly earnings also control for the covariates listed in Table A1 except for public assistance receipt, health services and diagnoses, and crime. Vice versa, analyses of benefits receipt and amounts control for all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets.

VII Robustness Checks and Measurement Error

This section describes findings from several alternative analytic strategies and samples we used to assess the sensitivity of our results and discusses the potential for measurement

error in our homelessness outcomes.

VII.A Supplemental Designs to Bound the Reduction in Homelessness

First, we conducted an exercise designed to produce a lower bound estimate of the reduction in homelessness by using a positively-selected comparison group. This choice of comparison group aims to address the possibility that unobserved characteristics like interpersonal skills or credit history may relate both to lease-up status and future experiences of homelessness. We compared our main lease-up group to members of the original comparison group who did not enroll in an interim housing or street project in the year of RRH enrollment. That is, we selected the comparison group for this analysis based on their initial outcomes. These comparison group members were arguably more likely than others who did not lease up to have resolved their homelessness on their own, without receiving the rental subsidy.[28] Panel A of Table A12 shows that, by construction, people who leased up were more likely to have used homeless services in the year of enrollment. However, the statistically significant point estimate indicates a 4.2 percentage point reduction in enrollments in interim housing or street outreach projects during years one to three after the RRH enrollment year. In our main analysis (not using this positively-selected comparison group), we estimated a statistically significant 10.1 percentage point reduction in interim housing and street outreach enrollments in years one to three after the year of enrollment.[29] Therefore, the 4.2 percentage point reduction represents a lower bound of the possible reduction in homelessness, especially because this analysis excludes effects during the subsidy period in the RRH enrollment year.

Second, we employed an examiner assignment design similar to Cohen (2024) to produce an upper bound for the reduction in homelessness. In our setting, all RRH participants are quasi-randomly assigned to caseworkers who provide case management and housing navigation services on a "first come, first served" basis. Caseworkers vary in the proportion of their caseloads who lease up, perhaps because they differ in their ability to help their participants complete the lease-up process. These features lend themselves to an examiner assignment design; Appendix B provides more information about how we implement this supplemental design, including evidence for the identification assumptions of relevance, exogeneity, and average monotonicity. However, in our setting, the exclusion restriction

[28] They may instead have been more likely to leave LA County and go on to experience homelessness elsewhere. If that were the case, we would incorrectly code this comparison group as not experiencing homelessness based on our LA County data. Therefore, even if these comparison group members were not actually "positively selected" and truly did experience homelessness, this exercise is still designed to produce a lower bound estimate of the possible reduction in homelessness due to lease up.

[29] This estimate is equal to the pooled impact estimate across the three years after the RRH enrollment year from Table 8.

is unlikely to hold and we do not interpret estimates from this approach as the causal effects of leasing up. It is highly likely that RRH caseworkers influence participant's outcomes through channels other than leasing up. RRH caseworkers who are more likely to get their participants to lease up may also provide better case management or be better at identifying higher quality housing situations for their clients.[30] Still, the correlation between caseworker tendencies and other favorable channels like better case management is useful for bounding the reduction in homelessness. Indeed, Panel A of Table A12 shows that the two-stage least squares estimate of lease up on homeless service use within four years is 20.2 percentage points – a larger reduction than our main finding of a 12 percentage point reduction. Taken together with the results employing a positively-selected comparison group, we can bound the effect of lease up on the use of homeless services between a 4- and 20-percentage reduction.

VII.B Sensitivity to Other Alternative Analytic Decisions

We also assess sensitivity to our main approach for addressing the conditional independence assumption and to alternative samples. Our main approach employs regression adjustment using the study's rich administrative data to address the small imbalance in a few pre-program characteristics shown in Table A1. The reduction in homelessness is larger without adjusting for covariates than in models that include the project*month and priority group fixed effects, confirming the expected selection bias in who leased up. In contrast, estimates from models with the fixed effects only versus fixed effects plus regression adjustment for the full covariate set are very similar (Table A3). Panel B of Table A12 also shows that our results were similar when using a variety of alternative samples, including (a) restricting the lease-up group to those with values of total rental assistance between the 10th and 90th percentiles; (b) including participants who had enrolled in a permanent housing project prior to the focal RRH enrollment, (c) including only RRH enrollments from 2017, prior to the COVID-19 pandemic, and (d) excluding project-month cells with only people who leased up or only people who did not lease up. In all cases, the estimates are statistically significant and similar in magnitude to our main results. Panel C further shows the results remain statistically significant with different approaches to clustering standard errors to account for the potential correlation between the outcomes of people assigned to the same project and those assigned to the same project around the same time.

[30]RRH caseworkers are not necessarily the same workers in Cohen (2024). That study focuses on workers who make the initial decision of whether to place someone in a housing assistance program, who may be less likely to influence people's outcomes in ways other than program placement.

VII.C Potential Measurement Error in Homelessness Outcomes

Both the study's main homelessness outcome from HMIS and its supplemental outcome from DPSS are subject to measurement error and it is important to consider how measurement may affect our findings.

Homeless Services Enrollments. Our primary outcome is whether a person enrolled in an interim housing or street outreach project after enrolling in RRH. A key strength of using homeless service enrollments as a proxy for literal homelessness (that is, excluding doubling up situations) is that people are generally verified as truly experiencing homelessness by caseworkers as part of the eligibility process. This lends credibility that the measure is capturing people who are actually experiencing homelessness (as opposed to self-reported measures that may be less credible). The main drawback is that people may experience literal homelessness without formally enrolling in homeless services. There are two types of literal homelessness to consider: sheltered and unsheltered. By definition, people experiencing sheltered homelessness must have enrolled in an interim housing project. Although people could enroll in interim housing projects in LA County that are not recorded in the study's HMIS data (for example, faith-based shelters), these are a small share of enrollments and there is little theoretical reason to suspect a correlation with lease-up status.[31] The bigger issue in our setting is that the number of people experiencing unsheltered homelessness is likely larger than the number of street outreach enrollments. Undercounting unsheltered homelessness may lead us to under- or over-state the extent to which lease up reduced homelessness. On one hand, people in the lease-up group could return to unsheltered homelessness without enrolling in a street outreach project if, for example, they had a negative experience with RRH. This would lead us to overstate the reduction in homelessness. At the same time, those who did not lease up could also have subsequently experienced unsheltered homelessness without enrolling in street outreach services because of their experiences with RRH. This would lead us to underestimate the reduction in homelessness. We are more concerned about the former scenario since the latter issue would not change the study's underlying finding that lease up reduced homelessness.

We have several reasons to believe that the number of RRH participants who leased up but then subsequently return to unsheltered homelessness without enrolling in street outreach services was too small to meaningfully affect our conclusion that lease-up reduced homelessness. First, detailed qualitative data confirms that people tend to have positive experiences with RRH programs, suggesting that there is little theoretical reason to suspect

[31] We are not aware of any systematic data on the number of interim housing enrollments excluded from HMIS but conversations with our county partners indicate it is not likely to be a large share.

there to be a strong "discouragement" channel from subsequently enrolling in homeless services (Jefferson et al., 2020). Second, it is extremely unlikely that street outreach workers systematically enroll people based on their lease-up status from a former RRH enrollment; there is little basis to think workers would access or consider such information during the enrollment process. Third, because we also see reductions in self-reported homelessness based on public benefits data, it would have to be the case that those in the lease-up group were not only discouraged from enrolling in homeless services if they returned to homelessness, but also discouraged from reporting their true living situation on public benefits applications. Lastly, we conducted an empirical exercise to assess the extent of differential misclassification needed to overturn our main estimate on homeless service use. If we assume that there was no measurement error for the comparison group–a very conservative assumption–there would need to be substantial "discouragement" effects among the lease-up group in order for the estimated reduction in homelessness to become statistically insignificant.[32]

Housing Instability from Benefits Applications. Our supplemental measure of homelessness from public benefits applications is also subject to measurement error both because of selection into benefits receipt and the potential that self-reported living arrangements are not accurate. We take the conservative approach of coding people as experiencing unstable housing only if they both received benefits and their benefits records indicated housing instability (based on self-reported homelessness or listing the DPSS office as their mailing address). This choice biases our estimates toward finding no effect of lease up on housing instability. Table 9 showed that leasing up may increase benefits receipt within four years by about 2% for the full sample, a result statistically significant at the 10% level. The estimate was three times larger for individuals than for families, likely because 96% of families who did not lease up received benefits compared with 85% of individuals. As a result, for individuals in particular, we may be less likely to have accurately observed whether they experienced housing instability. Yet our approach of coding these individuals as experiencing stable housing–even if they may have in fact experienced housing instability–biases us against a finding that lease up reduced housing instability. The findings are nearly identical when

[32]In Figure A3, we simulate scenarios in which we vary the probability that those in the lease-up group who we did not observe as enrolling in an interim housing or street outreach project within four years actually experienced homelessness. Instead of a 0 for the outcome, we vary the value of the outcome for these people between 0.01 and 0.20 and re-estimate impacts using our main analytic approach. We do not change the value of the outcome for those who did not lease up or those who leased up and subsequently enrolled in an interim housing or street outreach project. The point estimate for the effects of lease up on homelessness becomes statistically insignificant at a value of 10 percentage points, meaning that 10 percent of the lease-up group who we did not observe as experiencing homelessness would have needed to be misclassified, with no misclassification for the comparison group. Relative to the adjusted lease-up group mean of 31.6% from Table 4, a 10 percentage point misclassification is equivalent to a 32% increase in true homelessness for the lease-up group.

we instead take the alternative, less conservative, approach of estimating effects for housing instability only for those who received benefits during the outcome period (Table A13).[33]

Another concern with the supplemental housing instability outcome is that it may not accurately capture a person's true living situation, even for those who received benefits. The outcome is constructed by combining multiple indicators from benefits records, including initial benefits applications and changes to a person's living arrangements or address as part of a re-certification process. It is possible that people did not always accurately report their living situation and that government workers did not fully verify self-reported information. Such issues would introduce bias in our analysis only if the accuracy of our housing instability measure was correlated with lease up. However, there is little reason to suspect people who leased up received special attention when they applied for or re-certified benefits because DPSS workers do not typically know whether someone participated in RRH, let alone whether they successfully leased up with a rental subsidy. LAHSA, not DPSS, operates RRH.

Although we cannot assess the accuracy of the housing instability outcome using the study data, we can test sensitivity to how we define housing instability. To cast a wide net, we counted those who reported a DPSS office as their mailing address as experiencing housing instability. DPSS indicated this is a common practice for those without a stable, permanent address. But there may be other reasons why someone would prefer to list the DPSS office address, such as victims fleeing domestic violence or those who otherwise prefer privacy. When we instead define housing instability only based on self-reported homelessness, without incorporating address data, the results on housing instability are smaller in magnitude, but still indicate a reduction in housing instability (Table A13). Overall, the findings for housing instability are generally robust across samples and outcome definitions.

VII.D Out-of-County Migration

A remaining concern is that lease up caused people to leave LA County and experience homelessness in other places, which we do not observe in the study's LA County administrative data. This would mean the observed reduction in homelessness could be driven by increased

[33]We re-estimated impacts on our primary measure of homeless service use and the supplemental DPSS housing stability outcome for the 2,508 people in our sample (69% of the full sample) who continuously received benefits in all 16 quarters after the quarter of RRH enrollment. As expected, lease up increased the chances of continuously receiving benefits by 2.9 percentage points (standard error = 0.015) relative to a comparison mean of 70.3%. Even so, among those who continuously received benefits, those who leased up were similar along pre-program characteristics to those who did not lease up (Table A14). The differences between the groups were mpt statistically significant at the 5% level for any of the 30 pre-program characteristics and the F-statistic and p-value from a joint test as in the second column of Table 3 were equal to 0.934 and 0.570, respectively. For the sample of continuous benefits recipients, the estimated reduction in homelessness from both outcomes were nearly identical to our main estimates (Table A13).

out-of-county migration rather than an actual decrease in homelessness. This is unlikely to be the case, however, because RRH rental subsidies are typically used within the county, based on conversations with homeless services providers. In addition, as described above, lease up increased the chances of receiving benefits from the county in each of the 16 quarters after program enrollment, which we take as an indicator that those who leased up were more likely to remain in the county.[34] Estimates of lease up on the study's homelessness measures for the sample of people who we know did not leave the county because they received public benefits from LA county in all 16 quarters after program enrollment were very similar to our main findings (Table A13).

VIII Conclusion

This study provides new evidence of the causal effects of RRH on homelessness for individuals and families. Using detailed administrative data from LA County, we compare the outcomes of people who successfully lease up with an RRH rental subsidy to otherwise similar people enrolled in the same RRH program in the same month but who did not lease up, at least in part due to plausibly exogenous market factors. On average, those who leased up received about $1,500 per month of rental assistance for 7 months from their focal RRH enrollment.

We find that leasing up with a rental subsidy reduced homelessness. People who leased up were 28% less likely to enroll in interim housing or street outreach projects within four years. They were also less likely to report housing instability in public benefits applications. Although we find similar reductions in homelessness within four years for both individuals and families, the effects were more persistent for families. The effects for individuals faded out two years after the end of the subsidy period. The sustained reduction for families only may be a result of the larger and longer subsidies typically given to families compared with individuals. We also find that leasing up improved health outcomes, reduced crime, and increased income (from benefit programs) for families, but not for individuals. There could be reinforcing effects between stable housing, increased income, and improved health and well-being that manifested for families only.

To help interpret the findings, it is useful to consider the cost required to avert a day of homelessness. Table A4 indicates that leasing up decreased the amount of time enrolled in homeless services by 97 days over the four year follow-up period, including 76 days spent in

[34] Lease up also increased the chances that we observed an encounter with any public agency in LA County in the study's administrative data. The coefficient on lease up from a regression like in our main analysis where the dependent variable was having any county record within four years is 0.051 with a standard error of 0.014.

interim housing and 21 days of street outreach services. Over the same four years, people received an average of $10,026 in rental assistance and $665 in other financial assistance from their focal RRH enrollment for a total of $10,691. Some people who leased up eventually went on to re-enroll in RRH such that the total amount of financial assistance across the four year post-enrollment period was $15,516, on average.[35] We therefore estimate that averting a day of homelessness costs $160 ($= \frac{\$15{,}516}{97}$). The cost of averting a day of homelessness would likely decrease with a longer follow-up period because the subsidy is time-limited, but the reduction in homelessness persists for families. As a comparison, drawing on cost estimates from two large-scale RCTs of permanent supportive housing programs (the HUD-VASH and At Home/Chez Soi programs), (Ortuzar et al., 2025) summarizes that the cost of averting a day of homelessness for permanent supportive housing programs ranges between $265 and $344. Because rental assistance through permanent supportive housing is not time-limited, their cost-effectiveness is unlikely to decrease with a longer follow-up period. In sum, the cost-effectiveness of RRH is at least on par with permanent supportive housing, and may even be more cost-effective.

Our findings highlight that RRH can be an effective tool to address homelessness, especially among families. For families, future research could examine the extent to which lease up among families may have improved their children's outcomes. For individuals, it is worth considering whether more generous subsidies might prevent fade out. In addition, for all groups, research should study the extent to which the lease-up rate within RRH programs might be improved. The 63% lease up rate in our study is perhaps unsurprising given the documented challenges with landlords accepting housing vouchers (Aliprantis et al., 2022). More work could draw from the voucher literature by testing strategies for increasing RRH lease-up rates (Bergman et al., 2024), as well as examine the general equilibrium consequences of increasing lease up rates (Eriksen and Ross, 2015).

[35] We exclude program administrative costs from this estimate, such as the costs of case management and housing navigation services, because the lease-up and comparison groups likely incurred similar administrative costs in our setting.

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Does Rapid Re-Housing Reduce Homelessness?

Brian Blackwell and Max Gross

Online Appendix

A. Supplemental Online Figures and Tables

A1. Supplemental Online Tables

Table A1: Balance Tests for the Full Sample

	Comparison Mean	Leased-Up	Standard Error
Demographics			
More than One Adult	0.162	0.022	(0.015)
Number of Children	0.805	0.059*	(0.033)
Hispanic	0.327	-0.004	(0.018)
Black, Non-Hispanic	0.459	0.032*	(0.019)
White, Non-Hispanic	0.206	-0.030*	(0.016)
Female	0.554	0.009	(0.015)
Age (Years)	40.95	0.03	(0.49)
Had Disability	0.384	-0.002	(0.019)
Triage Tool Score at Homeless Services Intake			
Lower Risk (Scores 0-7)	0.243	0.028*	(0.016)
Higher Risk (Scores 8 and Above)	0.305	-0.014	(0.018)
Homeless Services (Four Years Before Enrollment)			
Interim Housing or Street Outreach	0.400	-0.033*	(0.018)
Other Non-Permanent Homeless Services	0.140	0.007	(0.014)
Public Assistance Receipt (Year Before Enrollment)			
Any Public Assistance	0.822	0.006	(0.015)
Amount of Public Assistance ($)	4,589	-94	(190)
Received CalFresh	0.741	-0.011	(0.017)
Received CalWorks	0.360	-0.001	(0.013)
Received Medi-Cal	0.670	-0.009	(0.019)
Received General Relief	0.213	-0.010	(0.016)
Unstable Housing on Application	0.632	-0.007	(0.018)
Health Services and Diagnoses (Four Years Before Enrollment)			
DHS Outpatient	0.127	0.009	(0.014)
DHS Emergency Room	0.139	-0.007	(0.014)
DHS Inpatient	0.038	-0.002	(0.008)
DMH Outpatient	0.268	-0.016	(0.017)
DMH Crisis Stabilization	0.060	-0.005	(0.009)
Serious Mental Illness	0.206	-0.034**	(0.015)
Substance Use	0.070	-0.006	(0.010)
Mortality-Related Diagnosis	0.032	0.005	(0.007)
Crime (Four Years Before Enrollment)			
Felony Booking	0.146	-0.005	(0.014)
Misdemeanor Booking	0.231	-0.023	(0.017)
Probation	0.034	0.000	(0.008)
Employment and Earnings (Four Years Before Enrollment)			
Ever Employed	0.631	0.013	(0.020)
Average Quarterly Earnings ($)	1,375	155	(107)
Observations	1,354	2,266	

Notes. This table reports the results from separate regressions of the covariate on lease-up. All regressions control for project-by-month fixed effects, household type, and veteran status. The first column reports the adjusted comparison group mean. The second and third columns report the coefficient and standard error on lease-up status, respectively. We use a four year look back period for most covariates, except those from the Department of Social Services (DPSS) on public assistance receipt. The DPSS data begins in 2017, later than the other data sources, so we use a one-year look back period for public assistance-related covariates. Receipt of interim housing or street outreach is equivalent to a lagged outcome variable. The CES Triage Tools (VI-SPDAT, VI-FSPDAT, and the Next Step Tool) were not assessed for all people at homeless services intake and therefore about half of the analytic sample was missing triage tool score data.

Table A2: Balance Tests for Individuals and Families

	All Covariates Except Employment		HMIS and Employment Covariates	
	Unadjusted	Adjusted	Unadjusted	Adjusted
	Panel A: Individuals (N = 2,116)			
F-Statistic from Joint Test	3.097	1.104	4.044	0.896
p-value from Joint Test	0.000	0.322	0.000	0.557
	Panel B: Families (N = 1,504)			
F-Statistic from Joint Test	3.332	0.884	4.017	0.875
p-value from Joint Test	0.000	0.647	0.000	0.586

Notes. This table reports the results from regressions of lease-up on two sets of covariates for the sample of individuals (Panel A) and families (Panel B). The first two columns include the covariates listed in Table A1, which come from the Homeless Management Information System (HMIS) and other data sources from the Information Hub. The first two columns do not include employment and earnings-related covariates because those records are linked to the HMIS but not other sources in the Information Hub. Therefore, the second two columns include only the covariates listed in Table A1 that come from homeless services records and employment and earnings data from California's Employment Development Department. Results from adjusted models additionally control for project-by-month fixed effects, household type, and veteran status.

Table A3: Effects of Lease-Up on Homeless Service Use Within Four Years With Alternative Covariates

	Enrolled in Interim Housing or Street Outreach	Enrolled in Interim Housing or Street Outreach	Enrolled in Interim Housing or Street Outreach
Lease-Up	-0.215***	-0.129***	-0.123***
	(0.017)	(0.018)	(0.017)
Demographics			
More than One Adult			0.031
			(0.021)
Number of Children			0.003
			(0.010)
Hispanic			-0.001
			(0.030)
Black, non-Hispanic			0.045
			(0.030)
White, non-Hispanic			0.065**
			(0.032)
Female			-0.006
			(0.021)
Age (Years)			0.002***
			(0.001)
Had Disability			0.020
			(0.018)
Triage Tool Score at Homeless Services Intake			
Lower Risk (Scores 0-7)			-0.072***
			(0.022)
Higher Risk (Scores 8 and Above)			-0.049**
			(0.021)
Homeless Services (Four Years Before Enrollment)			
Interim Housing or Street Outreach			0.091***
			(0.020)
Other Non-Permenent Homeless Services			0.064***
			(0.025)
Public Assistance Receipt (Year Before Enrollment)			
Any Public Assistance			0.013
			(0.033)
Amount of Public Assistance			0.000
			(0.000)
Received CalFresh			0.008
			(0.029)
Received CalWorks			0.027
			(0.028)
Received Medi-Cal			-0.046
			(0.022)
Received General Relief			0.037
			(0.024)
Unstable Housing on Application			0.076***
			(0.023)
Health Services and Diagnoses (Four Years Before Enrollment)			
DHS Outpatient			-0.015
			(0.026)
DHS Emergency Room			0.074***
			(0.028)
DHS Inpatient			-0.056
			(0.050)
DMH Outpatient			0.002
			(0.027)
DMH Crisis Stabilization			0.001
			(0.041)
Serious Mental Illness			0.020
			(0.032)
Substance Use			-0.014
			(0.041)
Mortality-Related Diagnosis			0.065
			(0.047)
Crime (Four Years Before Enrollment)			
Felony Booking			0.071**
			(0.028)
Misdemeanor Booking			0.015
			(0.022)
Probation			-0.089*
			(0.047)
Observations	3,620	3,620	3,620
Includes Fixed Effects		✓	✓

Notes. This table reports the results from separate regressions of homeless service use on lease-up. The first column does not include covariates. The second and third columns control for project-by-month fixed effects, household type, and veteran status. The third column additionally controls for the full set of covariates in the main analytic approach.

Table A4: Effects of Lease-Up on Days in Interim Housing and Street Outreach Projects Within Four Years

	Days in Interim Housing or Street Outreach	Days in Interim Housing	Days in Street Outreach
	Panel A: Full Sample (N = 3,620)		
Leased Up	-97***	-76***	-21***
	(8)	(7)	(4)
	{163}	{128}	{35}
	Panel B: Individuals (N = 2,116)		
Leased Up	-77***	-49***	-28***
	(11)	(8)	(6)
	{136}	{91}	{45}
	Panel C: Families (N = 1,504)		
Leased Up	-116***	-105***	-11**
	(13)	(12)	(4)
	{195}	{174}	{21}

Notes. Each column reports estimates from a separate regression of the number of days a person was enrolled in interim housing and street outreach projects within four years on lease-up. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effects of Lease-Up on Homeless Service Use Within Four Years, by Race and Ethnicity

	Black, non-Hispanic	Hispanic	White, non-Hispanic
Leased Up	-0.105***	-0.129***	-0.225***
	(0.025)	(0.033)	(0.047)
	{0.406}	{0.448}	{0.537}
Observations	1,735	1,174	680

Notes. Each column reports estimates from a separate regression of whether the person enrolled in a street outreach or interim housing project on lease-up. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Share of the Lease-Up Group that Received a Rental Subsidy through RRH, by Year

	Full Sample	Individuals	Families
Year of Enrollment	1.000	1.000	1.000
One Year After	0.310	0.257	0.390
Two Years After	0.184	0.135	0.258
Three Years After	0.113	0.090	0.148
Observations	2,266	1,360	906

Notes. This table reports the unadjusted share of the lease-up group that received a rental subsidy through the RRH program in each year after program enrollment. By construction, all members of the lease-up group received the subsidy in the year of enrollment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effects of Lease-Up on Receipt of Rental Subsidy Through RRH, by Year

	Received Rental Subsidy in			
	Year of Enrollment	One Year After	Two Years After	Three Years After
	Panel A: Full Sample (N = 3,620)			
Leased Up	1.000***	0.162***	0.100***	0.048***
	(0.000)	(0.017)	(0.015)	(0.013)
	{0.000}	{0.164}	{0.097}	{0.072}
	Panel B: Individuals (N = 2,116)			
Leased Up	1.000***	0.129***	0.063***	0.019
	(0.000)	(0.022)	(0.018)	(0.016)
	{0.000}	{0.133}	{0.081}	{0.074}
	Panel C: Families (N = 1,504)			
Leased Up	1.000***	0.204***	0.141***	0.079***
	(0.000)	(0.029)	(0.026)	(0.022)
	{0.000}	{0.211}	{0.128}	{0.076}

Notes. Each panel and column reports estimates from a separate regression of receiving the rental subsidy through RRH in a given year on lease-up in the focal year of enrollment. The year of enrollment indicates the year of RRH enrollment. Panel A includes the full sample while Panels B and C report results for individuals and families, respectively. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Effects of Lease-Up on Enrollment in Permanent Supportive Housing Within Four Years

	Full Sample	Individuals	Families
Leased Up	0.006	0.019	-0.012
	(0.012)	(0.017)	(0.018)
	{0.073}	{0.069}	{0.077}
Observations	3,620	2,116	1,504

Notes. Each column reports estimates from a separate regression of whether the person enrolled in a permanent supportive housing project within four years on lease-up. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Effects of Lease-Up on Receipt of Rental Subsidy Through RRH and Homeless Service Use for Families Enrolled in 2017, by Year

	Year of Enrollment	One Year After	Two Years After	Three Years After
	Panel A: Received Rental Subsidy			
Leased Up	1.000***	0.199***	-0.019	-0.022*
	(0.000)	(0.021)	(0.015)	(0.013)
	{0.000}	{0.116}	{0.115}	{0.083}
	Panel B: Enrolled in Interim Housing or Street Outreach			
Leased Up	-0.087***	-0.057***	-0.035***	-0.024**
	(0.023)	(0.014)	(0.012)	(0.010)
	{0.394}	{0.113}	{0.082}	{0.054}
Observations	2,246	2,246	2,246	2,246

Notes. This table uses an earlier sample of families who enrolled in RRH in 2017 and otherwise applies the same sample restrictions as our main 2019 sample. Each panel and column reports estimates from a separate regression of receiving the rental subsidy through RRH (Panel A) or enrolling in interim housing or street outreach projects (Panel B) in a given year on lease-up in the focal year of enrollment. The year of enrollment indicates the year of enrollment in the RRH program. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effects of Amount and Duration of the Rental Subsidy on Homeless Service Use Within Four Years

	Enrolled in Interim Housing or Street Outreach
Subsidy Amount ($1,000)	-0.002**
	(0.001)
Months of Subsidy	-0.007***
	(0.002)
Observations	2,974

Notes. Instead of a binary indicator for lease-up status, the treatment variables in this table are (1) the dollar amount (in thousands of dollars) and (2) the number of months of the rental subsidy that a person received from their focal RRH enrollment. The lease-up group has positive values for the treatment variables. A small number of comparison group members also have positive values, if they leased-up more than one year after they enrolled in the program. The analysis excludes participants who leased-up but were missing financial assistance data. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Effects of Lease-Up on Amount of Benefits Within Four Years, by Program

	CalFresh	CalWorks	General Relief
Panel A: Full Sample (N = 3,620)			
Leased Up	1,615***	535	-39
	(351)	(459)	(71)
	{11,893}	{9,528}	{918}
Panel B: Individuals (N = 2,116)			
Leased Up	765**	322	-20
	(327)	(328)	(119)
	{7,153}	{2,489}	{1,343}
Panel C: Families (N = 1,504)			
Leased Up	2,915***	1,454	-114*
	(693)	(978)	(59)
	{18,548}	{18,822}	{347}

Notes. Each panel and column reports estimates from a separate regression of the outcome on lease-up. Panel A includes the full sample while Panels B and C report results for individuals and families, respectively. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses and the adjusted comparison means are shown in curly brackets.

Table A12: Robustness to Alternative Analytic Approaches, Samples, and Clustering of Standard Errors

	Enrolled in Interim Housing or Street Outreach
Panel A: Alternative Analytic Approaches	
Positively-Selected Comparison Group (Year of Enrollment)	0.154***
N= 3,075	(0.012)
Positively-Selected Comparison Group (Years 1-3 After Enrollment)	-0.042**
N= 3,075	(0.016)
Examiner Assignment Design	-0.202***
N = 3,448	(0.040)
Panel B: Alternative Samples	
Trimmed Lease-Up Group Based on Subsidy Amount	-0.117***
N = 3,393	(0.018)
Include Participants with Prior Permanent Housing	-0.140***
N = 5,276	(0.015)
Enrolled in RRH in 2017	-0.124***
N = 5,520	(0.016)
Exclude Project-Month Cells Without Variation in Lease Up Status	-0.125***
N = 2,914	(0.017)
Panel C: Alternative Clustering of Standard Errors	
By Project	-0.123***
N = 3,620	(0.018)
By Project and Month	-0.123***
N = 3,620	(0.020)

Notes. Panel A reports the results from regressions using alternative analytic approaches. For the positively-selected comparison group design, the lease-up group is the same as in our main analyses and the comparison group includes those who enrolled in the RRH program, did not lease-up in the year of enrollment, and also did not enroll in an interim housing or street outreach program in the year of RRH enrollment. By construction, the lease-up group had higher rates of homeless service use than the positively-selected comparison group in the year they enrolled in RRH. Appendix B provides more information about the examiner assignment design. Panel B shows results based on alternative samples. The trimmed lease-up group sample based on subsidy amount includes all participants in the original lease-up group with financial assistance amounts between the 10th and 90th percentiles. The fixed-effects variation sample excludes observations where project*month fixed-effects cells without variation in lease-up status are excluded. Panel C shows results from our main analytic approach, but with standard errors clustered at the project and project*month levels. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Sensitivity Tests for Supplemental Measure of Unstable Housing on Benefits Applications

	Continuous Benefits Receipt Sample		Full Sample
	Enrolled in Interim Housing or Street Outreach	Unstable Housing on Benefits Application	Reported Homelessness on Benefits Application
Leased Up	-0.117***	-0.093***	-0.056***
	(0.022)	(0.018)	(0.015)
	{0.472}	{0.797}	{0.674}
Observations	2,508	2,508	3,620

Notes. The first two columns of this table show the effects of lease-up on the study's main homelessness outcome and the supplemental measure of housing instability based on public benefits data. The sample for the first two columns consists of people who were continuously enrolled in a public benefits program in each of the 16 quarters after the quarter of RRH enrollment. The last column includes the full sample and shows the effects of lease-up on whether someone self-reported experiencing homelessness on their benefits application. The outcome in the last column does not consider whether someone listed a DPSS office as their mailing address. Each column reports estimates from a separate regression of the outcome on lease-up. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

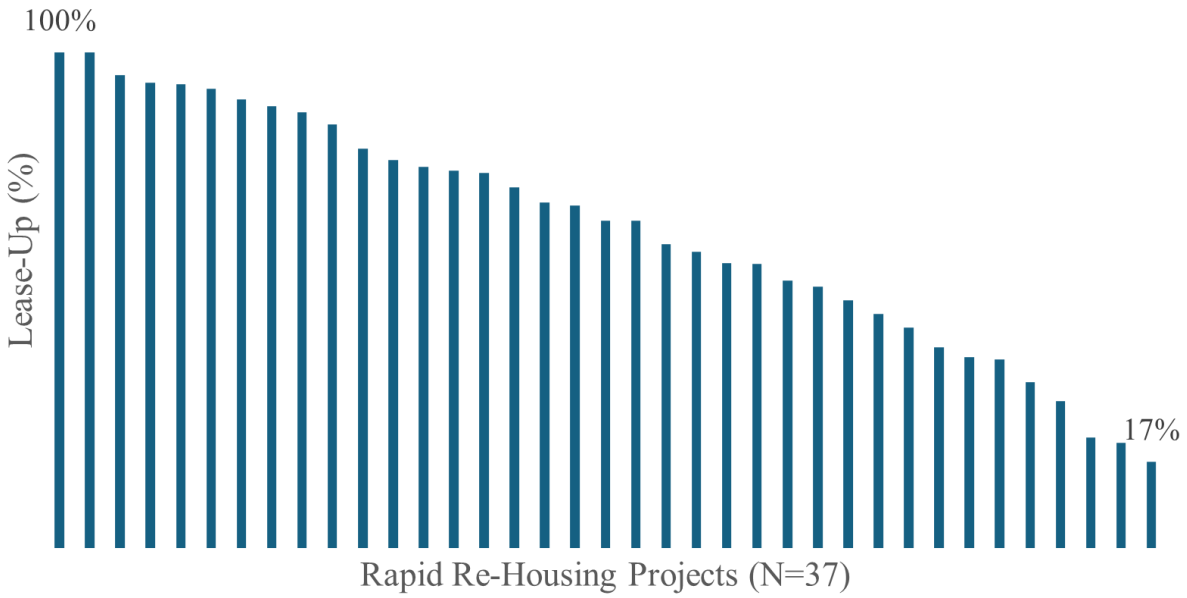
Table A14: Balance Tests for the Sample of People who were Continuously Enrolled in Public Benefits Programs

	Comparison Mean	Leased-Up	Standard Error
Demographics			
More than One Adult	0.196	0.019	(0.021)
Number of Children	1.005	0.078*	(0.047)
Hispanic	0.360	-0.013*	(0.023)
Black, non-Hispanic	0.460	0.043*	(0.023)
White, non-Hispanic	0.173	-0.026	(0.018)
Female	0.653	0.006	(0.018)
Age (years)	38.52	0.35	(0.54)
Had Disability	0.374	-0.026	(0.024)
Triage Tool Score at Homeless Services Intake			
Lower Risk (Scores 0-7)	0.238	0.033*	(0.020)
Higher Risk (Scores 8 and Above)	0.291	-0.022	(0.021)
Homeless Services (Four Years Before Enrollment)			
Interim Housing or Street Outreach	0.394	-0.024	(0.023)
Other Non-Permanent Homeless Services	0.145	0.009	(0.017)
Public Assistance Receipt (Year Before Enrollment)			
Any Public Assistance	0.978	-0.009	(0.008)
Amount of Public Assistance ($)	6,175	-237	(266)
Received CalFresh	0.887	-0.012	(0.016)
Received CalWorks	0.485	-0.009	(0.019)
Received Medi-Cal	0.838	-0.038*	(0.020)
Received General Relief	0.242	-0.007	(0.019)
Unstable Housing on Application	0.763	-0.016	(0.021)
Health Services and Diagnoses (Four Years Before Enrollment)			
DHS Outpatient	0.152	0.007	(0.019)
DHS Emergency Room	0.154	-0.016	(0.018)
DHS Inpatient	0.036	0.003	(0.010)
DMH Outpatient	0.323	-0.025	(0.024)
DMH Crisis Stabilization	0.057	0.001	(0.013)
Serious Mental Illness	0.236	-0.039*	(0.021)
Substance Use	0.072	-0.009	(0.013)
Mortality-Related Diagnosis	0.035	0.002	(0.009)
Crime (Four Years Before Enrollment)			
Felony Booking	0.168	-0.003	(0.019)
Misdemeanor Booking	0.255	-0.021	(0.022)
Probation	0.037	0.008	(0.011)
Observations	937	1,571	

Notes. This table reports the results from separate regressions of the covariate on lease-up for the sample of RRH participants who were continuously enrolled in a public benefits program in each of the 16 quarters after the quarter of RRH enrollment. All regressions control for project-by-month fixed effects, household type, and veteran status. The first column reports the adjusted comparison group mean. The second and third columns report the coefficient and standard error on lease-up status, respectively. We use a four year look back period for most covariates, except those from the Department of Social Services (DPSS) on public assistance receipt. The DPSS data begins in 2017, later than the other data sources, so we use a one-year look back period for public assistance-related covariates. Receipt of interim housing or street outreach is equivalent to a lagged outcome variable. The CES Triage Tools (VI-SPDAT, VI-FSPDAT, and the Next Step Tool) were not assessed for all people at homeless services intake and therefore about half of the analytic sample was missing triage tool score data.

A2. Supplemental Online Figures

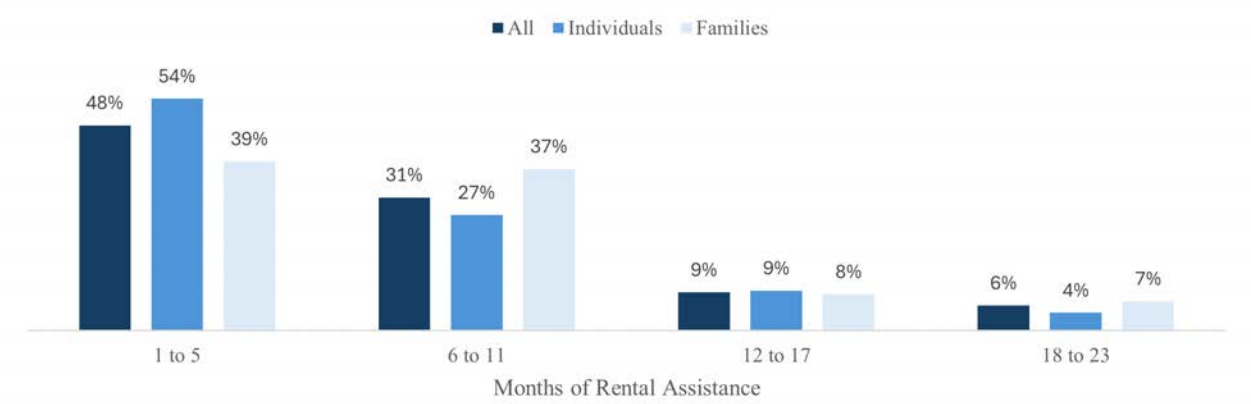
Figure A1: Distribution of Lease-Up Rates Across Rapid Re-Housing Projects



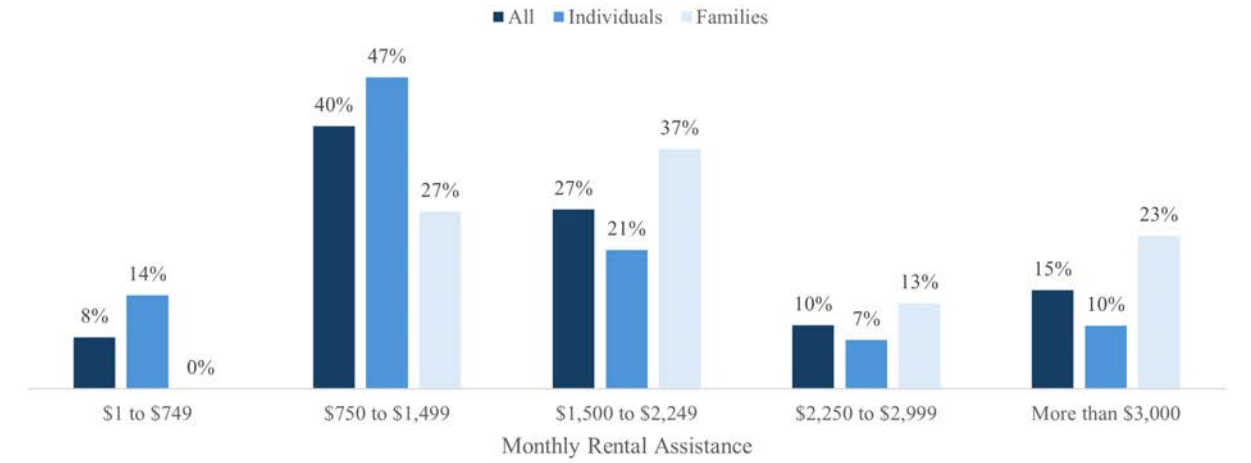
Notes. This graph plots the percentage of participants who leased-up in each of the 37 Rapid Re-Housing projects in the sample.

Figure A2: Distribution of Duration and Amount of Rental Assistance for the Lease-Up Group

(a) Duration

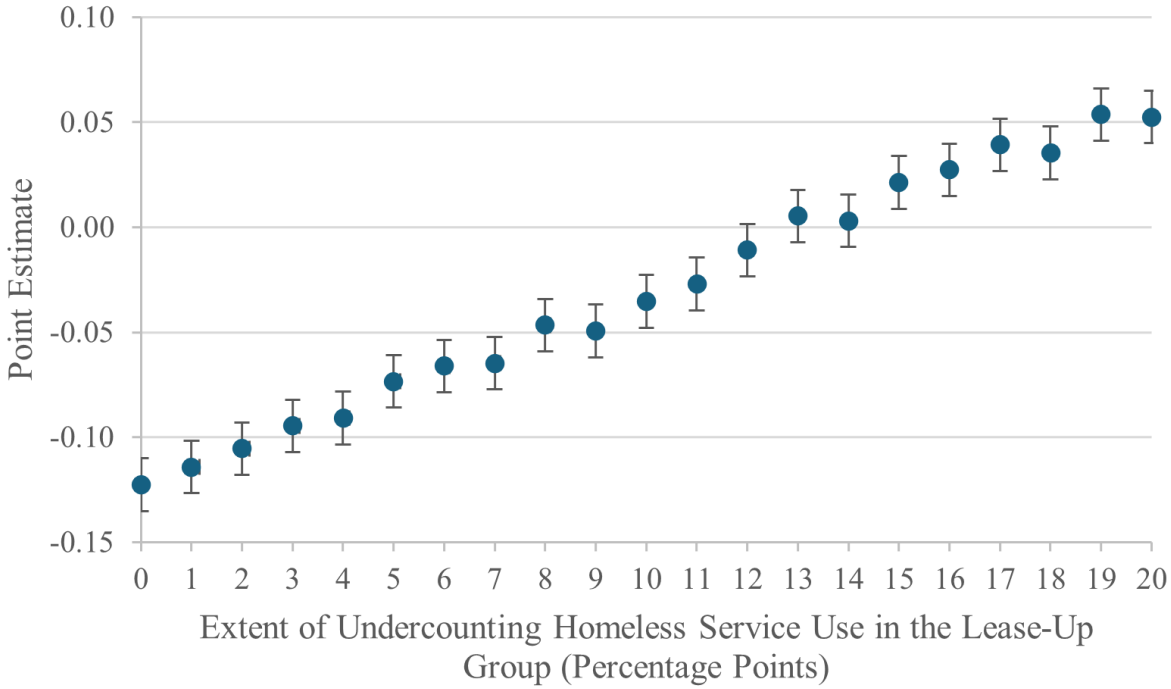


(b) Amount



Notes. This figure shows the distribution of the duration and amount of rental assistance from each member of the lease-up group's focal RRH enrollment. To address outliers, the amount of rental assistance is trimmed at the 5th and 95th percentiles. There were a total of 2,266 heads of households in the lease-up group, including 1,360 individuals and 906 members of families.

Figure A3: Effects of Lease-Up on Homeless Service Use, With Varied Rates of Undercounting Enrollments Among the Lease-Up Group



Notes. This graph plots the point estimates and 95% confidence intervals from 21 separate regression of enrollments in interim housing and street outreach within four years on lease-up. The leftmost estimate is from our main analysis. Each subsequent estimate to the right comes from varying the extent of undercounting homeless service enrollments in the lease-up group. The "1 percentage point" estimate replaces values of the outcome for people in the lease-up group who we do not observe enrolling in interim housing or street outreach within four years from 0 to 0.01. The rightmost "20 percentage point" estimate replaces those outcome values from 0 to 0.20. We do not change outcome values for the comparison group or for those in the lease-up group who we do observe enrolling in interim housing or street outreach. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings.

B. Details of Supplemental Examiner Assignment Research Design

To assess the sensitivity of the study's findings, we employ a supplemental research design leveraging the quasi-random assignment of RRH participants to caseworkers. In our setting, all participants in the RRH program are assigned to caseworkers who provide case management and housing navigation services on a "first come, first served" basis. Caseworkers vary in the proportion of their caseloads who lease up, perhaps because they differ in their ability to help their participants complete the lease-up process. These features lend themselves to an examiner assignment design, similar to Cohen (2024).[36] This appendix provides details about the supplemental examiner assignment research design.

We construct a leave-one out instrument for each program participant equal to the percentage of other participants assigned to the same caseworker who leased up. To create more accurate measures of caseworker lease-up tendencies, we restrict the sample to participants assigned to a caseworker who served a minimum of five total RRH participants. There is considerable variation in caseworker lease-up tendencies. The average of the instrument was 0.63 and the standard deviation was 0.32.

Interpreting results from the examiner assignment design as the causal effects of lease up requires four identification assumptions. First, the instrument must predict lease-up status. The first column of Table B1 shows the strong first-stage regression of lease-up status on the instrument for the full sample. The F-statistic of 520 indicates that the instrument is strong. Second, the unobserved determinants of homelessness must be independent of the caseworker's lease-up tendencies. We test this assumption by examining the extent to which observable characteristics are correlated with the instrument. As expected due to the quasi-random assignment of caseworkers, the rich set of pre-program characteristics shown to be predictive of lease up in Table 3 are not jointly predictive of the instrument. The F-statistic and p-value from a joint test of significance are 1.3 and 0.13, respectively. Third, average monotonicity must hold, whereby the covariance between each participant's caseworker-specific lease-up status and caseworker tendencies should be positively correlated for all subgroups. The remaining columns from Table B1 shows the first-stage is positive and statistically significant for individuals, families, and different race and ethnicity groups.

The examiner assignment design also requires an exclusion restriction for the estimates to be interpreted as local average treatment effects; however, in our setting, the exclusion

[36]The RRH caseworkers are not necessarily the same workers in Cohen (2024), which focuses on the workers who make the initial decision of whether to place someone in a housing assistance program and may be less likely to influence people's outcomes in ways other than program placement.

restriction is unlikely to hold. For the exclusion restriction to hold, RRH caseworkers must influence participant's outcomes only through lease up. But it is exceedingly likely that caseworkers influence participant's outcomes through channels other than leasing up. For instance, caseworkers who are more likely to get their participant's to lease up likely also provide better case management or identify higher quality housing situations for their clients. Therefore, we do not interpret estimates from the examiner assignment design as the causal effects of lease up. Still, the correlation between caseworker tendencies and other favorable channels like better case management is useful for bounding the reduction in homelessness from lease up. Indeed, Panel A of Table A12 shows that the two-stage least squares estimate of lease up on enrollments in interim housing or street outreach projects within four years is 20.2 percentage points – a larger reduction than our main estimate in Table 4.

Table B1: First Stage Relationship Between Caseworker Lease-Up Tendencies and Lease-Up

	Full Sample	Individuals	Families	Black Non-Hispanic	Hispanic	White Non-Hispanic
Leased Up	0.673***	0.657***	0.731***	0.607***	0.739***	0.725***
	(0.030)	(0.043)	(0.041)	(0.045)	(0.053)	(0.081)
Observations	3,448	2,017	1,431	1,661	1,108	649
F-Statistic	520	234	311	179	192	80

Notes. Each column reports estimates from a separate regression of lease-up status on the caseworker lease-up tendency instrument. All regressions control for project-by-month fixed effects, household type, veteran status, and all covariates listed in Table A1 except for employment status and earnings. Standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.