HOUSING THE HOMELESS:

THE EFFECT OF HOMELESS HOUSING PROGRAMS ON FUTURE HOMELESSNESS AND SOCIOECONOMIC OUTCOMES

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Abstract: Funding for housing programs serving the homeless has more than doubled in the past decade, with only scant evidence regarding the causal effect of such programs on future homelessness and socioeconomic outcomes such as crime, employment, and health. Using a random case worker assignment design and a novel dataset constructed by linking administrative records from multiple public agencies in Los Angeles County, I estimate that housing assistance for single adults experiencing homelessness reduces the likelihood of future return to the homeless system by 20 percentage points over an 18-month period, compared to a baseline mean of 40 percent. The decline is driven by housing programs that provide long-term housing solutions and by individuals with physical disabilities and/or severe mental illness. Moreover, my findings show that housing programs reduce crime, increase employment, and improve health, while not increasing reliance on social benefits. A simple cost-benefit analysis implies that up to 80 percent of housing and program costs are offset by these potential benefits in the first 18 months alone. Taken together, these findings demonstrate that well-targeted housing assistance for the homeless with a focus on long-term housing solutions can be rehabilitative for a large segment of the homeless population.

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1 Introduction

Homelessness is an extreme outcome of poverty that is growing rapidly in US cities. There are approximately 550,000 individuals who are homeless on a given night, and more than 1.4 million Americans who use some homeless services at least once a year (Henry et al., 2018). Homelessness is associated with multiple adverse outcomes (e.g., increased mortality and morbidity, increased involvement in criminal activity, and reduced probability of finding housing and employment) which impose a heavy administrative and financial burden on public agencies and local governments, with some estimates showing that the average cost of direct public services alone is $83,000 per homeless person per year (Flaming et al., 2015).

The Housing First approach to homelessness has been the popular treatment approach for homelessness in recent years, with funding for housing assistance programs serving individuals experiencing homelessness more than doubled in the past decade, reaching more than $18 billion nationally in 2019 (United States Interagency Council on Homelessness, 2020; Johnson and Levin, 2018).[1] Yet, there is only scant evidence about its effectiveness in preventing future homelessness and improving welfare due to lack of comprehensive longitudinal data on individuals experiencing homelessness, non-random selection of participants into housing assistance programs, and challenges in conducting randomized controlled trials (Evans et al., 2019; National Academies of Sciences et al., 2018; O'Flaherty, 2019). Moreover, while little is known about future returns to homelessness and housing assistance receipt, recent studies show that a significant share of participants in homeless housing programs return to homelessness while or after receiving housing assistance (Cusack and Ann Montgomery, 2017; Levitt et al., 2013).

This paper studies the effect of homeless housing programs on future homelessness and other socioeconomic outcomes such as crime, employment, and health. I construct a novel and comprehensive panel dataset which allows me to compare outcomes of individuals experiencing homelessness who receive housing assistance to those who do not. I do that by linking administrative records across multiple public service agencies in Los Angeles County, which has the nation's second largest homeless population, including the homeless response system, health services, and the sheriff's department, among others. I then use these links to create a panel dataset at the case-month level containing public service histories of all

[1] There are two contested approaches regarding the role of housing assistance as a treatment policy for homelessness. One approach, called Housing First, is that housing assistance stabilizes a person's life and serves as a platform for rehabilitation (Burt et al., 2017). In contrast, the Treatment First approach holds that individuals experiencing homelessness would not be able to maintain housing without first addressing the problems that caused them to be homeless (Katz, 1990; Husock, 2003). This paper does not compare between the two approaches but shows that the Housing First approach can cause a permanent reduction in homelessness.

single individuals experiencing homelessness in Los Angeles County who sought assistance between 2016 and 2017. This comprises data on homeless services received, including housing assistance, and a series of economic and social outcomes, including involvement in criminal activity, employment, and health care utilization.

I address potential non-random assignments into housing programs using a random case-worker assignment design (an approach that is very similar to the "Judge Fixed Effects" design) to construct an instrumental variable for housing assistance receipt. A naive comparison of individuals who receive housing assistance versus those who do not could lead to wrong conclusions that result from selection into housing programs based on observed and unobserved characteristics of clients. I overcome this potential selection problem by exploiting a quasi-experiment where individuals are randomly assigned into housing assistance programs with different probabilities based on their case worker assignment. This quasi-experiment is the result of as-good-as-random assignment of clients' cases to case workers combined with considerable variation between case workers in their propensity to place individuals in housing programs, even after conditioning on service site, time, and case characteristics.

This paper provides four main results. First, housing assistance discourages future returns to homelessness, which I measure using future interactions with the homeless support system. Using the instrument of case worker housing placement propensity, I estimate that participation in housing programs lowers the probability of returning to the homeless support system within 18 months by 20 percentage points compared to a baseline mean of 40 percent. This finding is especially important since I show that without controlling for selection into housing programs, standard OLS estimates suggest that participation in housing programs increases the likelihood of returning to homelessness. Moreover, these results are not driven only by the ability of clients to remain housed while actively receiving assistance. I find considerable decreases in future return probabilities even after housing assistance has ended for a large portion of clients.

Second, the reduction in future homelessness is larger among individuals who are more likely to receive housing assistance based on their observed characteristics. That is, the estimated reduction in future homelessness among individuals who are more likely to receive housing assistance because of the acuity of their situation (because they have been homeless for a long time or they suffer from substantial disabilities, for instance) is estimated to be two to four times larger compared to the estimated reduction in future homelessness among low-acuity individuals. These heterogeneous effects suggest that (i) providing direct housing assistance to the most vulnerable individuals is highly beneficial, while alternative types of assistance (for example, direct cash assistance) can be more beneficial for low-acuity individuals and (ii) there is room for better targeting of housing program types and services among low-acuity individuals.

Third, the effect of housing programs on future homelessness is larger in programs that provide long-term housing solutions and when clients receive assistance for a longer duration. In particular, I find that the estimated reduction in future homelessness is driven by individuals in permanent housing programs (who also have longer duration), while the estimated impact among individuals in temporary housing programs (e.g., emergency shelters) is not different from receiving no housing assistance at all. Consistent with these findings, the results suggest that reduction in future homelessness is driven almost exclusively by intensive margin responses, that is, by individuals receiving housing assistance for a longer duration (i.e., enrolling in a 6-month housing assistance program versus spending a week in an emergency shelter), while the extensive margin response (i.e., receiving an emergency shelter placement for a couple of nights versus none at all) is small and insignificant.

Fourth, housing programs have a substantial positive effect on a wide range of of socioeconomic outcomes. The findings suggest that housing programs improve health, reduce crime, and increase employment. Specifically, housing assistance lowers the number of emergency department visits within 18 months by 80 percent (compared to baseline mean), reduces the number of jail days within 18 months by 130 percent, the probability of committing a crime by 80 percent (compared to baseline mean), and increases the probability of reporting employment by 24 percentage points within 18 months. Moreover, I find no significant relationship between housing assistance and receipt of various types of social benefits, ruling out potential increases in public spending that result from housing assistance.

These findings have important implications for policy debates over eligibility, duration and targeting of housing assistance types to individuals experiencing homelessness. One important policy question is whether the positive effects from housing are cost-effective. Back-of-the-envelope calculations presented at the end of the paper show that up to 80 percent of program costs are offset by direct savings to public agencies within the first 18 months alone, which I compute as savings from reduced use of homeless and other public services and from increased employment. The overall benefits from housing assistance are likely to be larger due to indirect benefits from potential reduction in street homelessness and its associated burden on public agencies, health and law enforcement in particular, and the fact that the benefits are expected to grow over time as individuals spend more time off the streets. Consistent with that, I find that although the cost of permanent housing programs is on average more than double that of temporary housing programs, the majority of cost savings arises from them, supporting a policy which increases eligibility and resources of housing assistance programs aimed at finding long-term housing solutions.

This paper advances the literature on homelessness in three dimensions. First, it continues the growing trend of using administrative data to study homelessness, which was pioneered by Culhane et al. (2002) and Byrne et al. (2013), with recent work demonstrating the use administrative of data to study homelessness at the national level (Meyer et al., 2021). Second, this study is the first to establish that participation in housing assistance programs has a beneficial causal effect on a wide range of socioeconomic outcomes for individuals experiencing homelessness using large-scale administrative data and the random assignment of screener design (also known as "Judge Fixed Effects").[2] Recent literature reviews by Evans et al. (2019), O'Flaherty (2019), and Kertesz and Johnson (2017) show that while there is an extensive literature on homelessness, few papers have been able to come up with credible causal estimates of the effect of housing assistance policies on subsequent homelessness and additional outcomes of interest. This fact is driven in particular because of the numerous limitations of conducting randomized control trials (e.g., high costs, treatment assignment spillovers, attrition) and having access to high quality data on a large population of individuals experiencing homelessness. Third, this study focuses on single adults experiencing homelessness, an understudied yet important population, that represents more than two thirds of the homeless population. Much of the existing literature focuses on families who experience homelessness or on specific subgroups within the homeless population. For example, Evans et al. (2019) study the effect of housing vouchers for homeless veterans; Aubry et al. (2016) study the effect of Housing First programs in Canada on homeless individuals with serious mental illness; and Gubits et al. (2018) evaluate the effects of the Family Options study on homeless families.

This paper also relates to the growing literature on the effect of housing assistance on family and individual outcomes by focusing on a population group that has not received attention in the past due to data limitations. This literature has mainly focused on specific populations such as people who apply for housing vouchers, like in the Moving to Opportunity studies (Bergman et al., 2019; Chetty et al., 2016; Kling et al., 2007; Pinto, 2018), or who are forced to move after public housing demolitions, like Jacob (2004) and Chyn (2018). However, there are no studies in this literature that examine the impact of housing assistance for individuals experiencing homelessness, who are presumably those who need it the most, and potentially have the largest benefits from receiving housing assistance. Other studies, like Jacob and Ludwig (2012) and van Dijk (2019), study broader populations of low-income families. However, these studies cannot usually identify homeless participants due to the lack

[2] The number of studies that use the random screener design to identify a causal relationship has grown rapidly in recent years, and has been used in the context of incarceration (Aizer and Doyle, 2015; Bhuller et al., 2020; Kling, 2006; Mueller-Smith, 2015), disability insurance (Autor et al., 2019; Dahl et al., 2014; Maestas et al., 2013), foster case (Bald et al., 2019; Doyle, 2007; Doyle Jr, 2008); bankruptcy protection (Dobbie and Song, 2015); and foreclosures (Diamond et al., 2020).

of available data on participants. Finally, a few studies have examined the effect of housing evictions on homelessness, finding that they cause a large and persistent increase in risk of homelessness (Collinson and Reed, 2018; Desmond and Gershenson, 2016; Fetzer et al., 2019; Humphries et al., 2019).[3]

The remainder of the paper proceeds as follows. Section 2 provides background on homelessness in Los Angeles County and briefly describes the different housing program types available to the homeless. Section 3 describes the data. Section 4 describes the research design and verifies its validity. Section 5 presents the main results on subsequent homelessness. Section 6 presents further results on additional economic and social outcomes. Section 7 presents a cost-benefit analysis, and Section 8 concludes.

2 Background

Three features of the homeless response system in Los Angeles county make it an ideal setting to study homelessness and housing. Los Angeles County has a large and growing homeless population, low availability of housing assistance for the homeless, and a universal record-keeping system that records all initial intakes and housing assistance provided by homeless service agencies. Housing assistance for the homeless in this setting is defined as enrollment in at least one program out of a continuum of housing programs (ranging from basic crisis housing to intensive supportive housing) that vary in duration, non-housing services provided, and ability to provide a permanent housing solution.

2.1 Homelessness in Los Angeles County

Los Angeles County has a large and growing number of individuals experiencing homelessness. Figure A.1 graphs Los Angeles County's homeless rate over time. As of 2019, Los Angeles County has the nation's second largest homeless population, with approximately 60,000 individuals experiencing homelessness on a given night, with 45,000 of them living in places not meant for human habitation (Henry et al., 2020). The county's homeless rates reached these unprecedented levels after experiencing rapid growth over the past decade. Specifically, the county's homeless rate increased from 360 to 608 homeless individuals per 100,000 residents between 2010 and 2019, a 70 percent increase.

The demand for housing assistance to serve individuals experiencing homelessness is far greater than the supply of available housing in Los Angeles County. As of 2019, there was a total of 45,116 beds in 764 housing assistance programs that served the homeless or previously homeless population (Henry et al., 2018). This number is roughly half of what is needed to

[3]See Ellen et al. (2016) for an overview of empirical research on housing assistance policies in the U.S.

address the county's needs (Los Angeles Homelessness Services Authority, 2017). In addition, individuals currently being served are expected to occupy their units for a long period of time, implying considerably low vacancy rates. Specifically, the vacancy rate for these beds and units was 8 percent in 2019 (Henry et al., 2020).

2.2 Housing Programs for the Homeless in Los Angeles County

Housing programs for the homeless in Los Angeles County vary along three major dimensions: duration, availability and type of non-housing services, and the ability to provide a permanent housing solution.[4] Based on these dimensions, housing programs that serve the homeless population in Los Angeles County can be broadly categorized into two types: temporary and permanent. Temporary housing programs, commonly known as emergency shelters, provide short-term housing assistance, and are meant to provide crisis or bridge housing for clients while they seek permanent housing solutions. Permanent housing programs provide medium- or long-term housing assistance with the intention of locating a permanent housing solution that can be used by clients after program participation and housing subsidy are completed.

Housing assistance programs also differ in the availability and amount of non-housing services they provide to their clients. Some of the most common non-housing services include case management, basic hygiene services (e.g., meals and showers, basic health care), substance abuse treatments, mental health treatments, life skills courses, and employment readiness workshops, among others. Permanent housing programs tend to provide more health care services, while temporary housing programs mostly offer basic hygiene services. However, there is a large degree of customization and hence variation in the amount or types of non-housing services provided, even among housing programs within the same category. These differences between programs are based both on clients' needs and providers' treatment philosophy. Moreover, many service providers in the county also offer separate non-housing programs that are meant to complement housing programs.

The last important difference between housing programs is their ability to provide long-term housing solutions for clients. Permanent housing programs are based on the Housing First strategy for addressing homelessness. This strategy is based on quickly finding long-term housing solutions in order to minimize the trauma caused by homelessness and to better serve additional problems an individual experiencing homelessness is facing (Burt et al., 2017). These programs locate housing units for clients which they are supposed to occupy even after the housing subsidy period has ended. On the contrary, temporary housing programs are based on a continuum model for homelessness that emphasizes addressing clients' problems and getting them ready for housing prior to finding permanent housing.

[4] A more detailed description of these programs is available in Appendix A.2.

2.3 Los Angeles County's Homeless Coordinated Entry System

The Los Angeles Continuum of Care (CoC), headed by the Los Angeles Homeless Services Authority (LAHSA), is the regional planning body that coordinates housing and services for homeless families and individuals in Los Angeles County. It includes hundreds of service providers who provide a variety of services, ranging from meals and hygiene services, health care, transportation, legal assistance, general case management, and temporary or permanent housing services, among others. Historically, the homeless response system of Los Angeles County was highly decentralized, with its service providers operating independently from one another and having little or no communication with one another.

In 2014, Los Angeles County's homeless service providers adopted and set up the Coordinated Entry System (CES) in response to the county's growing homeless crisis. The CES is a countywide system that brings together all service providers in order to quickly connect individuals to the most appropriate treatment for them. This system was designed to facilitate coordination and resource management for the multiple service providers that comprise the county's crisis response system by combining their information into one system.

The most important feature of the CES for the purposes of this study is the standardization and recording of all clients' intakes across all service providers. Beginning in 2016, as part of the adoption of the CES, all homeless individuals seeking assistance go through the same process when applying for assistance. Single adults experiencing homelessness who are seeking assistance can connect with the county's homeless service providers in one of three ways. First, clients can arrive independently to service providers through a "walk-in" option. Second, clients can be referred to service providers via other public agencies (e.g., health clinics, hospitals, social welfare programs). Third, many service providers operate street outreach teams that scan the streets of the county in order to assist unsheltered homeless individuals.

After clients have engaged with service providers, they are assigned to case workers who assess their acuity level and needs using a standardized assessment tool known as the VI-SPDAT (Vulnerability Index - Service Prioritization Decision Assistance Tool).[5] Their information is entered into the CES to determine their acuity and needs and to provide them with the appropriate care as quickly as possible.[6] After the intake stage is completed, case

[5]The standardized VI-SPDAT assessment for single adults experiencing homelessness in Los Angeles County can be accessed through: `https://www.lahsa.org/documents?id=1306-form-1306-ces-survey-for-individuals-survey-packet.pdf`.

[6]In practice, the CES is still being developed and is not yet fully operational. To date, it serves as a system which prioritizes clients only for Permanent Supportive Housing (PSH) programs. LAHSA plans to expand the system in the future to encompass other services as well. It is important to emphasize that the standardized assessment tool serves as one of several tools the case worker has when deciding what types of services (if any) to provide the client, and does not determine whether the client is eligible for housing assistance. In my context, what matters is that all homeless single adults seeking assistance are required to

workers work with their clients to build an action plan. As part of this plan, clients can receive a variety of different housing and non-housing services from various service providers across the county, according to their needs and availability.

Two features of the Los Angeles County homeless system are important for my analysis. First, when a client engages with a service provider in the system, they are assessed by the first available case worker, so conditional on service provider and time, the assignment to a case worker is as-good-as-random.[7] Second, case workers differ in their propensity to place individuals in housing programs. In my baseline specification, I measure the propensity of a case worker to place a client in a housing program based on the share of cases that ended up receiving housing assistance among the other cases they have handled. When using this measure, I always condition on fully interacted service site by month of intake fixed effects to account for the fact that randomization occurs within the pool of available case workers. This controls for any differences over time and/or across service providers in the availability of resources and the placement rates of case workers.[8]

3 Data and Descriptive Statistics

I create a case-level panel dataset containing information on homeless services received, housing assistance, and additional socioeconomic outcomes for the universe of cases for single individuals experiencing homelessness in Los Angeles County. I then limit the data such that only cases that were as-good-as-randomly assigned to a case worker are considered. I verify that these cases are representative of the overall sample of cases. I then present the distribution of housing assistance treatments in my sample and show that participating in a housing program is positively correlated with future homelessness, bearing out the potential selection into housing programs concerns that motivated my research design.

3.1 Data Sources

I link data recording intakes of single individuals experiencing homelessness with homeless service providers to data sets containing administrative records from multiple public agencies

enter the CES, which allows me to capture the universe of this population in Los Angeles County.

[7]The random assignment of clients to case workers has been confirmed in multiple interviews I conducted with service providers and with representatives from the Los Angeles Homeless Services Authority (LAHSA). They have emphasized that this assignment is based on availability of case workers alone. This is true for all types of initial engagement of clients with providers (walk-ins, referrals, and outreach). I provide empirical evidence that assignments are as-good-as-random in Section 4.3.

[8]In Section 5.4, I show robustness of the results to alternative measures of the case worker housing placement rate.

in Los Angeles County.[9] I then use these linked records to construct a panel dataset containing information on homeless services received, housing assistance, and additional socioeconomic outcomes, such as crime, employment, and health.[10]

The first data source consists of administrative records for individual intakes conducted by homeless service providers throughout Los Angeles County from 2016 to 2018. This data set, commonly known as the VI-SPDAT (Vulnerability Index - Service Prioritization Decision Assistance Tool), is a pre-screening tool that guides case workers when assessing the acuity level and needs of a particular individual. Each record includes a unique individual identifier, intake date, assessment details, and demographic characteristics (e.g., age, race, gender, disabilities, and veteran status). Additionally, each record provides information on the case worker conducting the intake process, including their name, organizational affiliation, and the location where the intake was conducted.

The second data source I use, called the Homeless Management Information System (HMIS), includes information on all homeless services provided (both housing and non-housing services) by homeless service providers in the Los Angeles CoC from January 2010 to June 2019. Additionally, it includes information on the type of service and/or housing program, and the enrollment and exit date (if relevant). For a sub-sample of the records in the HMIS, I observe information on reported income, employment, and social benefits.

The third data source I use, called the Enterprise Linkages Project (ELP), includes information across a spectrum of publicly funded health, mental health, social and corrections services in Los Angeles County, as well as the costs associated with those services and utilization. The ELP started in 2007 with the goal of providing comprehensive information on the multi-system service utilization patterns of persons participating in social welfare programs. It integrates records from the Departments of Health Services (DHS), Mental Health (DMH), Public Health (DPH), Public and Social Services (DPSS), as well as the Probation and Sheriff Departments.

I link the intakes data to the HMIS and ELP data using the unique individual identifiers recorded in them to construct homeless and public service histories of all homeless cases. I use the HMIS data to define my main measure of housing assistance treatment, which is an indicator for whether an individual was enrolled at least once in a housing program within the first 18-months after intake.[11] I use the ELP data to construct economic and social outcomes

[9] Table B.1 provides a summary of the various data sources used in this study, the information contained in them, and the time period they cover.

[10] Appendix B provides detailed information on how the various data sources were cleaned and prepared for analysis.

[11] In practice, approximately 60 percent (90 percent) of housing assistance program enrollments occur within the first six-month (year) after intake, and my results are robust to using different time horizons to define treatment.

for the cases in my data. These include, among others, emergency department admissions, mental health services received, and jail bookings and days.[12]

3.2 Construction of Instrument and Estimation Samples

I construct two samples of homeless cases to implement the case-worker random assignment design. The instrument sample contains all intakes handled by case workers. I construct it for the purpose of measuring a case worker's share of cases handled that ended up receiving housing assistance, which serves as the instrument for housing assistance receipt. I then impose restrictions on the instrument sample to create the estimation sample which contains all intakes that were as-good-as-randomly assigned to case workers.

I impose the following restrictions on the intakes data to construct the instrument sample. First, I focus my attention on intakes conducted in 2016-2017, to be able to follow all cases for a period of up to 18 months after intake. Next, I restrict my attention to individuals age 25-65, since individuals who are not in this age group are not considered single adults (under 25 years old) or might have different needs compared to seniors (individuals older than 65 years old). Next, I remove individuals with missing information on case worker, organizational affiliation, or intake location. Following that, I remove duplicates or assessments for the same individual that were conducted on the same day by different case workers. Finally, I remove veteran cases from my sample since homeless veterans are redirected to the United States Veterans Administration Homeless System for further treatment, and hence their case worker assignment is not relevant to whether they receive housing assistance.[13]

Next, I impose two additional restrictions to set up the estimation sample. These restrictions ensure that I consider cases that are as-good-as-randomly assigned to case workers and that the instrument I use in my research design, case workers' housing placement rate, is informative of case workers' propensity to place individuals in housing programs. Specifically, I restrict my attention to service sites that had at least two case workers working in each month and case workers who handled at least 15 cases in 2016-2017.[14] Appendix B.3 describes the steps above in more detail, and Table B.2 shows how the various restrictions affect the number of cases, clients, case workers and service sites in my sample.

[12] Each agency has somewhat different time periods coverage, affecting my sample sizes when considering different outcomes. See Appendix B for more details.

[13] This fact was also verified in multiple interviews with service providers and representatives from the Los Angeles Homeless Services Authority (LAHSA).

[14] In Section 5.4, I show that my results are robust when excluding case workers with a relatively small number of cases. I chose the threshold of 15 cases in order to increase sample size and given that case workers handle 25 cases on average at any point in time, with the average duration of a case more than one year, which makes 15 cases a reasonable number in this setting.

3.3 Descriptive Statistics

I first verify that the observed characteristics of cases in the estimation sample are representative of the overall sample of cases. I then investigate the typical patterns of housing assistance and future returns to homelessness of the individuals in my data. I find that individuals who receive housing assistance are more likely to return to homelessness in the future compared to individuals who do not, consistent with potential negative selection into housing programs.

The cases in the estimation sample generally have similar characteristics to those of the overall sample of non-veteran cases. Table B.3 documents the key characteristics of the sample of cases I use in the estimation sample (column 1), non-veteran cases that were handled by case workers in 2016-2017 (column 2), and the cases that were excluded from the estimation sample but are included in the instrument sample (column 3). The typical case in the estimation sample represents an individual with an average age of 45 years old, less likely to be a woman (34 percent of overall sample), more likely to be black (51 percent of overall sample), followed by Hispanic and white, with 23 and 20 percent of the overall sample, respectively. Moreover, 72 percent of cases represent individuals who experienced homelessness in the past. Additionally, 61 percent of cases report chronic homelessness (defined as having a long history of homelessness and a physical disability or serious mental illness), and only 35 percent have used homeless services in the year before assessment. Additionally, the average acuity score, which is the result of the standardized assessment conducted by case workers during intake and indicates the level of needs an individual requires, is 7.3 (out of 17), with a score above 8 indicating high acuity. Finally, as can be seen in the last panel of Table B.3, only 10 to 35 percent of cases have reported using homeless or public services in the past year.

Figure I shows the distribution of treatments received for homeless cases in my data. I consider a treatment as enrollment in any housing or non-housing program that occurred in the 18-month period after intake.[15] For simplicity, I show the most intensive service received by the individual. Among the 39,119 non-veteran assessments conducted in 2016-2017, approximately 65 percent of cases received some form of assistance, with about fifty percent of cases receiving housing assistance. In particular, among the cases that received housing assistance, 60 percent received only temporary housing assistance, and the other 40 percent

[15] I define treatment in that way for two reasons. First, waiting times for housing programs are usually very long, implying that the time passed from intake to housing placement can be long as well. Second, I do not observe whether a housing placement is linked directly to the case worker handling the individual during intake, and I take the relaxed assumption that any observed housing placement post-intake is due to case worker involvement to some extent. I have tried limiting the treatment time window to 1-month, 3-months, 6-months, and 12-months after intake, and my results do not materially change. I do not count multiple treatments, but my analysis accounts for the number of days the client received housing assistance and the type of housing program (temporary or permanent) in which the client enrolled in Section 5.3.

received some type of permanent housing assistance. Less than 5 percent of all cases received permanent supportive housing, the most intensive housing assistance treatment available.

Figure II documents the typical return to homelessness patterns for individuals in the instrument sample.[16] For the purpose of my analysis, I define return to homelessness as an enrollment in a street outreach program, implying the individual is currently residing in a place not meant for human habitation, or a new intake, indicating that the individual has returned to seek assistance from the homeless response system.[17] The figure plots the probability an individual returns to the homeless support system at least one time per month in each of the 36-months surrounding the assessment date.[18] There are separate lines for cases that received any housing assistance in the 18 months following intake and those that did not.

Figure II is consistent with the idea that there is potential negative selection into housing programs. It shows that individuals who receive housing assistance are more likely to interact with the homeless support system prior to their intake. It reveals that both type of individuals start with a low probability of interacting with the homeless support system (approximately 1 percent), and that these probabilities increase and diverge as the intake date approaches, reaching 13 percent for individuals receiving housing assistance and 10 percent for individuals who did not receive housing assistance in the month prior to intake.

The most striking feature of Figure II, however, is that individuals who receive housing assistance are more likely to return to homelessness in the post-intake period compared to those who do not, although this gap becomes smaller over time.[19] The probability of returning to the homeless support system decreases over time for both groups, starting from a high of 12.6 percent and 4.5 percent for individuals receiving housing assistance and those who do not in the first month after intake, respectively, to a low of 2.7 percent and 1.7 percent

[16] Individuals in the estimation sample show similar patterns to those in the instrument sample.

[17] This measure of homelessness depends to some extent on the behaviors of the homeless individual. One potential story that could lead to an over-estimate is if people who are housed and subsequently return to homelessness feel reluctant to go back to seek assistance because they became discouraged after not receiving the assistance they desired in previous cases. However, in Section 6, I show that individuals who receive housing assistance see improvements in other outcomes such as crime, employment, and health, making this story unlikely to be the case. Alternatively, a person who is denied housing could be more likely to frequently return to seek assistance because they are hoping to get assistance that they did not receive yet. I discuss this possibility in Section 5.1 and show that there is no increase in the probability of housing assistance receipt conditional on returning to the homeless system. Additionally, in Figure B.1 and in Table C.16, I examine alternative definitions of interactions with the homeless response system. All of them are consistent with my main outcome variable.

[18] Month 0 values are capped at 0.15 for visual purposes since all individuals have a 100 percent probability of returning to the homeless support system in this month by definition.

[19] In Figure B.1, I also show that individuals who receive housing assistance are less likely to report finding a housing solution and are more likely to report going back to the streets or to temporary housing.

for these two groups after 18 months, respectively.[20] Overall, 47 percent of individuals who receive housing assistance would return to the homeless support system within 18 months from intake, compared to only 23 percent among individuals who do not receive housing assistance.

Figure II and Figure B.1 motivate my research design. They suggest that using an OLS or an event-study design to estimate the effect of housing assistance on future returns to the homeless support system can lead to wrong conclusions, because the group of individuals who receive housing assistance is not comparable to the group of individuals who do not in their pre-intake trends. Moreover, the figures suggest that housing assistance does not prevent future returns to homelessness. These patterns in the data motivate me to use an instrumental variable research design to address unobserved selection to treatment, which I implement using the random assignment of cases to case workers quasi-experimental approach to identify the causal effect of housing programs on future to homelessness.

4 Research Design

I exploit the fact that assignment of homeless cases to case workers is as-good-as-random and that case workers differ in their propensity to place clients in housing programs to generate exogenous variation in the probability of receiving housing assistance. I leverage this variation using a leniency ("judge fixed effects") design, which identifies the causal effect of housing assistance on future homelessness and a large set of socioeconomic outcomes.

I validate this research design by performing multiple tests for the four required assumptions of the instrumental variable model (exogeneity, relevance, monotonicity, and exclusion) and show that the instrument is consistent with them all. I also document that the average complier is representative of the average case in the sample, although slightly less likely to have physical disabilities or serious mental illness, or to experience chronic homelessness.

4.1 IV Model

I model the relationship between housing assistance and outcomes using an instrumental variable design. The first stage uses the case worker share of housing placements in other cases as an instrument for housing assistance receipt in the current case. Specifically, a

[20] There are two main reasons for why future homelessness rates are higher in months following intake. First, case outcomes are measured relative to intake date, not relative to housing assistance receipt date, creating a time gap where individuals are not housed and might return to seek assistance. Second, individuals can return to the homeless support system even after receiving housing assistance if they fail to comply with eligibility conditions of the housing program and leave before it has ended, or if their housing program has ended and they are back on the streets or seeking assistance from the system again.

case worker with a high housing placement rate is more likely to get the client into housing regardless of their situation.

I am interested in the causal effect of housing assistance on subsequent homelessness and a wide array of socioeconomic outcomes. This can be captured by the regression model:

$$Y_{it} = \beta_t H_i + X_i^{'}\theta_t + \delta_{sm} + \nu_{it} \tag{1}$$

where β_t is the parameter of interest, H_i is an indicator variable equal to 1 if individual i received any type of housing assistance in the 18-month period after intake, δ_{sm} is a set of fully interacted service site by month of intake fixed effects, the level at which random assignment to case workers happens, X_i is a vector of individual-level covariates, and Y_{it} is the dependent variable of interest measured at month t after individual i's assessment (e.g., cumulative number of returns to the homeless support system 18 months after intake).

As shown in Figure II, the treated versus non-treated groups are not comparable, which raises concerns about selection bias in the OLS estimation of β_t. My research design addresses this concern by exploiting the quasi-random assignment of cases to case workers (conditional on service site and month of assessment) and the fact that some case workers are systematically more likely to place individuals in housing programs. Taken together, this leads to quasi-random variation in the probability an individual will receive housing assistance depending on which case worker they are assigned to. I use this exogenous variation in H_i to draw inference about the causal effect of housing assistance for the homeless.

My main analysis is based on 2SLS estimation of β_t with Equation (1) as the second stage equation and a first stage equation specified as:

$$H_i = \gamma Z_{j(i)} + \rho_{sm} + X_i^{'}\psi + \varepsilon_i \tag{2}$$

where the scalar variable $Z_{j(i)}$ denotes the housing placement rate of case worker j assigned to individual i's case. Formally, it is defined as:

$$Z_{j(i)} = \frac{\sum_{k \neq i} H_{jk}}{N_j - 1} \tag{3}$$

where H_{jk} equals to 1 if individual k who was assigned to case worker j received housing assistance, and 0 otherwise, and N_j is the number of intakes conducted by case worker j in 2016-2017. Under the assumption of instrument exogeneity and monotonicity, the 2SLS estimand can be interpreted as a positive weighted average of the causal effect of housing assistance among the subgroup of individuals who could have received a different housing assistance treatment had their case been assigned to a different case worker.

One might be worried about exactly how to measure the case worker housing placement rate $Z_{j(i)}$ and perform statistical inference. For my main specification, I measure $Z_{j(i)}$ as the leave-out mean housing assistance rate which omits case i, that is, the average housing assistance rate in other cases the case worker has handled. In Section 5.4, I show robustness to alternative measures of $Z_{j(i)}$, including a veterans-included placement rate, a split sample approach, and a residualized placement rate. I also verify the conclusions do not change if I exclude case workers with relatively few cases, change the level of fixed effects, or change the definition of treatment.

In most of my analysis, I perform 2SLS estimation of equations (1) and (2) using the entire sample of all individuals in quasi-randomly assigned cases. However, due to data limitations, and in order to interpret the results and inform policy, I estimate the effect of housing assistance for different subsamples and explore the heterogeneous effects of housing assistance along a variety of dimensions. When exploring outcomes using my administrative records, I can only use early assessments since the end date of many of these records covers less than 18 months after assessment.[21] Additionally, I explore heterogeneous treatment effects by estimating the 2SLS model separately by subgroups.

4.2 First Stage

Case worker's housing placement rate in other cases handled is a strong predictor of housing program enrollment in the current case, satisfying the relevance (first stage) assumption of the IV model. Specifically, being assigned to a case worker with a 10-percentage point higher housing placement rate increases the probability of housing program placement by 6.4 percentage points.

Figure III shows the identifying variation in the data by providing a graphical representation of the first stage. The histogram in the background of the figure shows the distribution of the instrument (controlling for fully interacted service site by month of intake fixed effects and individual-level covariates). The mean of the instrument is 0.51 with a standard deviation of 0.09. The histogram reveals a large variation in a case worker's tendency to place individuals in housing programs. For example, a case worker at the 90th percentile places about 61 percent of cases in housing programs compared to approximately 41 percent for a case worker at the 10th percentile.

Figure III also plots the probability that clients receive housing assistance as a function of whether they are assigned to a case worker with a high or low housing placement rate. The graph is a flexible analog to the first stage equation in Equation (2), plotting estimates

[21] Table B.1 provides a summary of the various data sources used in this study, the information contained in them, and the time period they cover.

from a local linear regression. The likelihood of receiving housing assistance is monotonically increasing in the case worker housing placement rate instrument and is close to linear.

Finally, Figure III also shows a validation exercise I perform which provides evidence that differences in case worker housing placement rates, and not differences in clients' characteristics, are driving the variation in housing assistance receipt. Specifically, the grey line shows estimates from local linear regressions of the probability that clients receive housing assistance as a function of their covariates (i.e., their propensity score) as a function of their case worker housing placement rate. There is no significant relationship between the predicted likelihood of receiving housing assistance and the case worker housing placement rate instrument, indicating that the variation between case workers in their housing placement rate is not driven by the type of clients they serve.

Table I reports first stage estimates where I regress a dummy for whether an individual received housing assistance in the current case on the case worker housing placement rate instrument. In column 4, I include fully interacted service site by month of intake fixed effects and a large set of case-level characteristics. The estimate is highly significant, suggesting that being assigned to a case worker with a 10-percentage point higher overall housing placement rate increases the probability of receiving housing assistance by roughly 6.4 percentage points, compared to a baseline mean of 54 percent.

I found no statistically significant relationship between observable case worker characteristics and their housing placement rates. First, I did not find any statistically significant difference in placement rates based on the case worker's gender or ethnicity. Following that, I examined whether tenure or experience might be connected to different placement rates. Figure B.2 shows that there is no systematic relationship between case worker housing placement rate and the number of intakes the case worker conducted or a proxy for the case worker's tenure, respectively.

I continued my investigation regarding the variation in case workers' housing placement propensities by conducting multiple interviews with homeless service providers in Los Angeles County. All of them emphasized that several case worker unobserved personality traits and skills might be important determinants of housing placement rates. First and foremost, case workers are required to build trust and motivate their clients. This task is challenging because many clients do not trust public institutions and have given up hope that their situation can be improved. Moreover, case workers serve as their clients' point of contact and advocates, assisting them in applying to programs and services, following up on their situation, and intervening if there are any problems or modifications to their case plan. The second important characteristic of case workers is their ability to find the relevant services and funding that the client could get in the shortest time possible. This skill requires extensive knowledge of the homeless support system and good networking skills with other service providers and landlords, which could get their clients to the "front of the line" for services that are in short supply, especially housing.[22]

Bearing in mind that there could be many reasons for why some case workers are more likely to place clients in housing programs compared to others, as long as case workers' assignment to clients is random, these underlying reasons should not matter for the causal interpretation of my analysis.

4.3 Instrument Validity

For the instrument to be valid and interpreted as a local average treatment effect, it needs to satisfy the exogeneity, exclusion, and monotonicity assumptions, in addition to the relevance (first stage) assumption. I perform multiple tests for the four assumptions required for the instrument to be valid.

Instrument Exogeneity. Table II presents evidence that case worker assignment is as-good-as-random. Columns 1-2 show results from a regression of any housing assistance receipt in the 18 months following assessment on a variety of individual level covariates measured before intake. It reveals that demographics, homeless history, and past receipt of housing assistance are highly predictive of whether a client will receive housing assistance in their current case, even after conditioning on service provider and date. In columns 3-4, I examine whether the measure of the case worker housing placement rate can be predicted by this same set of covariates. This is equivalent to the type of test that would be done to verify random assignment in a randomized controlled trial. I find no statistically significant relationship at the 5 percent level between the case worker's placement rate and the various individual level covariates, either individually or jointly. Moreover, the magnitude of the estimates is an order of magnitude smaller compared to their size in Columns 1-2.[23]

As a second test for instrument exogeneity, columns 1-4 of Table I explore what happens if a large set of control variables are added to the first stage regression. If case workers are randomly assigned, pre-determined variables should not significantly change the estimates, as they should be uncorrelated with the instrument. As expected, the coefficient does not change appreciably when demographics, case characteristics, and lagged dependent variables

[22]For example, if a client is eligible for a permanent housing unit but there are no available units, case workers can use their knowledge and skills to find alternative solutions, such as emergency shelter placement, until a permanent housing unit can be found.

[23]The indicator variable for black is the only statistically significant coefficient at the 10 percent significance level. However, the size of this coefficient is 20 times smaller than the size of the same coefficient when housing assistance receipt is used as the dependent variable, implying that the economic significance of this variable on case worker housing placement rate is practically zero.

capturing an individual's prior involvement with the homeless support system and other public agencies are included.

Exclusion Restriction. Interpreting the IV estimates as measuring the causal effect of housing assistance requires an exclusion restriction. That is, the housing placement rate of the case worker should affect the individual's outcomes only through the housing program channel, and not directly in any other way. The key challenge here is that case workers' decisions are multidimensional, with the case worker influencing receipt of both housing and non-housing services. I present empirical evidence that the exclusion restriction assumption holds in Section 5.4. In particular, I show that my estimates do not change appreciably when I augment my baseline model to either control for case worker placement rates in non-housing services or include an instrument for receipt of non-housing services.

Monotonicity. If the causal effect of housing assistance is constant across individuals, then the instrument only needs to satisfy the exogeneity and the exclusion assumptions. With heterogeneous effects, however, monotonicity must also be assumed. In my setting, the monotonicity assumption requires that individuals who were assigned to a case worker with a low housing placement rate and received housing assistance would also receive housing assistance if they were assigned to a case worker with a high housing placement rate. This assumption ensures that the 2SLS estimand can be given a local average treatment effect interpretation, i.e. it is an average causal effect among the subgroup of individuals who could have received a different housing assistance treatment had their case been assigned to a different case worker.

One testable implication of the monotonicity assumption is that the first stage estimates should be non-negative for any subsample. For this test, I estimate the first stage on various subsamples, using the same instrument as before. Results are reported in column 1 of Table C.1. In panel A, I construct a composite index of the characteristics included in Table II, namely predicted probability of receiving housing assistance, using the coefficients from an OLS regression of the probability of receiving housing assistance on these variables. I then estimate separate first stage estimates for the four quartiles of predicted probability of housing assistance receipt. Panel B breaks the data into three case characteristics, based on their acuity scores (low, medium, and high). Panels C, D, E and F split the sample by homeless history, mental health history, emergency health services history and crime history. Panels G, H, I and J split the sample by age, gender, race, and ethnicity. For all these subsamples, the first stage estimates are positive and statistically different from zero, consistent with the monotonicity assumption.

A second implication of monotonicity is that case workers should have a high housing placement rate for a specific case (e.g., history of mental health) if they have a high housing placement rate in other case types (e.g., no history of mental health). To test this implication, I break the data into the same subsamples as I did for the first test but redefine the instrument for each subsample to be the case worker's housing placement rate for cases outside of the subsample. For example, for the history of mental health subsample, I use a case worker's housing placement rate constructed from all cases except history of mental health cases. Column 2 of Table C.1 lists the first stage estimates using this "reverse-sample instrument" which excludes own-type cases. The first stage estimates are all positive and statistically different from zero, suggesting that case workers who have a high housing placement rate for one type of cases also have a high housing placement rate for other types of cases.

4.4 Characteristics of Compliers

The compliers in the sample are defined as those individuals who would receive a different housing assistance treatment if they were assigned to a different case worker. They constitute about 27 percent of all cases in the sample.[24] While the average complier in the sample is generally representative of the average case, they are less likely to have interacted with the homeless system in the past compared to the always- and never-takers in the sample.

I examine the characteristics of the compliers in the sample relative to the always- and never-takers of treatment. I define always-takers as those who would receive housing assistance even when assigned to the case worker with the lowest housing placement rate. Never-takers are defined as those who do not receive housing assistance even when assigned to the case worker with the highest housing placement rate.[25] Compliers are those whose housing assistance receipt is affected by the random assignment to case workers in the sample.

Table III shows summary statistics for the three groups within the estimation sample. The share of compliers in the estimation sample is 27%, the share of always-takers is 26%, and the share of never-takers is 47%. Compliers appear to have similar characteristics to the representative case in the estimation sample, although they are slightly less likely to suffer from disabilities or to interact with the homeless system in the past. In particular, compliers

[24] I follow Dahl et al., 2014 in calculating the share of compliers. I begin by regressing case worker housing placement rate (the instrument) on service site x month of intake fixed effects and all individual controls. Using the residuals from this regression, I define the highest (lowest) housing placement propensity case workers as those in the top (bottom) 2.5 percentile of the residuals' distribution. I then run the first-stage regression on the entire sample (i.e., regressing housing assistance receipt on case worker placement rate), and then compute the share of compliers as the product of the first-stage coefficient of the instrument and the difference between the high and low residual case worker housing placement rate.

[25] Since case worker housing placement rate is a continuous variable, I define the 2.5 percentile and the 97.5 percentile of the case worker housing placement distribution as the threshold of the strictest and most lenient case worker, respectively.

are less likely to have a disability (physical and/or mental), and to be chronic homeless (57% compared to 61% in full sample). Moreover, compliers are less likely to use homeless services (27% compared to 35% in the estimation sample) or to have received housing assistance in the year prior to intake (23% compared to 28% in the estimation sample).

Always-takers and never-takers have higher overall acuity and are more likely to be chronically homeless, have a serious disability, be involved in criminal activity, and use homeless services in the year prior to intake. Interestingly, never-takers are considerably less likely to be black (37% compared to 51% in the estimation sample), while always-takers are considerably more likely to be females (44% compared to 34% in the estimation sample).

Overall, the complier analysis of cases suggests that compliers are slightly more likely to be individuals experiencing homelessness who have not been receiving services from the homeless system in the past, and therefore might be more able to take advantage of housing assistance programs, compared to individuals with higher acuity or a long history of homelessness who interact with the homeless system more frequently.

4.5 Reduced Form

Conditional random assignment is sufficient for the reduced form estimates to be interpreted as the causal effect of being assigned to a case worker with a higher housing placement rate. I show that being assigned to a case worker with a higher housing placement rates has beneficial effects on future homelessness, health, crime, and employment outcomes.

Figure IV presents a graphical representation of the reduced form relationships between the case worker housing placement rate (the instrument) and the main outcomes of interest in the study. In the background of each of the graphs is a histogram for the distribution of case worker housing placement rate, identical to the histogram presented in Figure III.

Panels (a)-(f) of Figure IV plot the reduced-form effects of a case worker's housing placement rate against the following outcomes, using a local linear regression (by order of appearance): any return to the homeless support system, any emergency department visit, any emergency mental health treatment, any criminal charges, number of jail bookings, and any employment reported. All outcomes are measured at 18-months after intake.

The reduced-form estimates are all related to case worker housing placement rate in a monotonic fashion, with varying degrees of precision. First, the likelihood of returning to the homeless system at least once during the 18-months period after intake is monotonically decreasing in the case worker housing placement rate (panel a). Approximately 52 percent of individuals whose cases who are assigned to a case worker with a low housing placement rate (housing placement rate = 0.6, the 10th percentile) are expected to return at least once to seek assistance from the homeless system, contrasted with approximately 48 percent of individuals whose cases are assigned to a case worker with a relatively high housing placement rate (housing placement rate = 0.6, the 90th percentile).

Second, I turn to look at health service utilization outcomes. I show that the likelihood of visiting the emergency department (panel b) or receiving an emergency mental health treatment (panel c) are monotonically decreasing with case worker housing placement rate, with sizable effects relative to the baseline mean. For example, the difference between the 10th and 90th percentile of case worker housing in the probability of visiting the emergency department is around 1 percentage point fewer visits for those individuals whose cases are assigned to case workers with a higher housing placement rate, relative to a baseline mean of 6 percent for the full sample, implying an approximately 16 percent decrease in the probability of visiting the emergency department.

Third, I turn to look at crime outcomes. I show that the likelihood of having any criminal charges (panel d) or the number of jail bookings (panel e) are monotonically decreasing with case worker housing placement rate. For example, the difference between the 10th and 90th percentile of case worker housing in the number of jail bookings in the 18 months after intake is around 0.2 fewer jail bookings for those individuals whose cases are assigned to case workers with a higher housing placement rate, relative to a baseline mean of 1.05 jail bookings in the full sample.

Finally, I look at employment outcomes. I show that the likelihood of reporting employment at least once in the 18-months after intake (panel f) is monotonically increasing with case worker housing placement rate. The difference between the 10th and 90th percentile of case worker housing in the probability of reporting employment at least once in the 18 months after intake is around 6 percentage points higher for those individuals whose cases are assigned to case workers with a higher housing placement rate.

5 Main Outcome: Future Homelessness

I provide evidence that housing assistance prevents and reduces future homelessness, with a strong impact detected both while and after being enrolled in a housing program. I investigate and conclude that the positive correlation between housing assistance and future homelessness is a result of non-random assignment into treatment based on unobservables.

Following that, I proceed to document heterogeneous effects by individual and program characteristics. First, I show that individuals with physical disabilities and/or severe mental illness see larger reductions in return to homelessness rates. Second, I find that the effect of housing assistance on future homelessness is driven by placements in permanent housing programs and that the effect of housing assistance on future homelessness increases in magnitude as the duration of the housing program increases.

5.1 Main Results

Housing assistance significantly discourages future returns to the homeless support system. There is a large post-treatment effect, suggesting that the effect is not driven solely by the ability to maintain housing while actively receiving assistance. Furthermore, the difference between OLS and IV estimates is driven by selection into treatment based on unobserved characteristics that increase the likelihood of future return to homelessness.

Return to Homeless System Probabilities. Figure V graphically presents IV estimates of the effect of housing assistance receipt on the probability of returning to the homeless support system.[26] The graph presents a series of cumulative monthly estimates from 1 month to 18 months after assessment. For example, the estimate at month 6 uses the probability an individual has returned to seek services from the homeless support system at least once by 6 months after intake as the dependent variable in the second stage of the IV model. All of the IV estimates are negative and statistically significant. As expected, the coefficients increase in magnitude over time, since there is more time to return to the homeless support system as time after assessment increases. The estimates suggest that at around 18 months after intake there is a large and statistically significant reduction of over 20 percentage points in future homelessness for those receiving housing assistance.

Comparison to OLS. In Table IV, I present OLS estimates of Equation (1) with and without a rich set of controls. The first specification regresses whether an individual has returned to the homeless support system on whether the individual received housing assistance, but includes no other control variables. The OLS estimates are all positive and significant; for example, individuals receiving housing assistance are 24 percentage points more likely to return at least once over the next 18 months. In the next specification I add all of the individual-level controls and the fully interacted set of service site by month of intake fixed effects. These controls affect the estimates only slightly.

[26] It is important to emphasize that I do not observe whether a client is homeless at any given point in time, only whether the client has returned to the homeless system. My future homelessness measure addresses this measurement issue by including new enrollments in street outreach programs in addition to new intakes. Since street outreach workers actively seek homeless individuals on the streets, implying that the homelessness measure includes both individuals who actively return to the homeless system and individuals who were tracked by the homeless system. However, some individuals may refuse to get services or may not be located by street outreach workers, but may still return to homelessness. My analysis implicitly assumes that case worker assignment is not correlated with these possibilities.

The divergence between the OLS estimates and the IV estimates is stark. The OLS estimates are always positive, while the IV estimates are negative and large. One possible explanation for this difference is that the average causal effect for compliers differ in sign compared to the mean impact for the entire population. To explore this possibility, I follow Bhuller et al. (2020) and characterize compliers by their observable characteristics. I begin by splitting my sample into eight mutually exclusive subgroups based on acuity score (above and below median) and the predicted probability of receiving housing assistance (see Table C.2). The predicted probability of receiving housing assistance is a composite index of all of the observable characteristics, while acuity score is a potentially key source of heterogeneity in effects. Next, I estimate the first stage equation (2) separately for each subsample and calculate the proportion of compliers by subgroup. I then reweight the estimation sample so that the proportion of compliers in a given subgroup matches the share of the estimation sample for the subgroup. The third row of Table IV presents OLS estimates based on this reweighted sample. The results suggest that the differences between the IV and OLS estimates cannot be explained by heterogeneous effects, at least due to case-level observables.

Given that, the only remaining explanation is that the OLS estimates suffer from selection bias due to correlated unobservables. If this is the case, I can conclude that the positive rates of subsequent returns to the homeless support system among homeless individuals receiving housing assistance is due to selection, and not a consequence of housing assistance receipt in itself.

Treatment versus post-treatment effect. The recidivism effect in Figure V can be decomposed into two components, the ability to maintain housing while actively receiving housing assistance and the ability to maintain housing after housing assistance has ended.[27]

In Table C.3, I present quarter-by-quarter estimates for returns to the homeless support system in a particular quarter. In Table IV, I group the first and last 9 months together for increased precision. Both tables reveal sizable reductions in future homelessness, across all periods considered, consistent with a reduction in future homelessness that is not driven solely by the effect of maintaining housing while actively receiving housing assistance.

In panel (a) of Figure VI, I plot a series of IV estimates for the probability of receiving housing assistance, 1 to 18 months after intake. Additionally, I plot the share of individuals actively receiving housing assistance in a given month among the individuals receiving housing assistance in the 18-month period after intake. The figure is similar to a survival function, in that if all treated individuals started receiving housing assistance in month 1, the estimates

[27]Individuals may return to homelessness while actively receiving housing assistance, as they can fail to comply with eligibility requirements of housing programs or have difficulties in adjusting to being housed.

would map out 1 minus the probability of exiting housing programs.[28] As expected, the probability of receiving housing assistance for those who received housing assistance within 18 months after assessment starts out high. This probability falls over time, and becomes somewhat flat around 10 months with about 20 percent of treated individuals enrolled in a housing program.

The main takeaway from panel (a) of Figure VI is that the effect of housing assistance on future homelessness that is driven by maintaining housing while actively receiving housing assistance goes down over time as fewer and fewer treated individuals receive housing assistance. Using this insight, I now graph the probability of ever returning to the homeless support system between months 10 and 18 in panel (b) of Figure VI. By ignoring returns that happened in the first 9 months after intake, I am estimating housing assistance effects that are less likely to be attributed to the ability to maintain housing while actively receiving housing assistance. I find that the effect is statistically significant and increases in magnitude as time from intake increases, such that there is a 20-percentage point reduction in returning at least once to the system between months 10 and 18 after intake.[29]

One concern regarding whether the suggested post-treatment effect is real is the possibility is that prior receipt of housing assistance impacts the probability of receiving housing assistance in the future if the case is assigned to another case worker upon completion of the first housing program or if the individual returns to seek assistance from the homeless support system in the hope of getting additional housing assistance. To explore this possibility, in Table C.4, I examine whether case worker housing placement rate in the current case affects housing assistance receipt for new cases of the same individual. I first estimate how housing assistance in the current case affects the probability of receiving housing assistance in another case in the future. I find a small and insignificant effect of 1.3 percentage points. The insignificant effect on future housing assistance helps interpret the mechanisms behind the main estimates. In particular, they suggest that a mechanical effect from receiving housing assistance in future cases does not explain the large and persistent reduction in future homelessness.

Number of returns to homeless support system. A comparison of Figure V and panel (b) in Figure VI suggests that housing assistance not only prevents an individual from returning to the homeless support system (the extensive margin), but it also prevents individuals from returning multiple times to seek support from the homeless support system (the intensive

[28] It is not exactly a survival function because not all individuals receiving housing assistance begin receiving it in month 1 due to waiting times for an open space.

[29] I cannot rule out completely the possibility that the effect I find is driven by those 20 percent of individuals who are still housed even 18 months after assessment.

margin). To further explore the intensive margin response, panel (a) of Figure VII plots IV estimates for the cumulative number of returns to the homeless support system in the months after intake. The estimated effects become more negative over time. After 18 months, the estimated effect of housing assistance is around 0.56 fewer returns, compared to a baseline mean of 0.72 returns.

Potential returns to the homeless system. The IV estimates represent the average causal effects for compliers who could have received a different housing assistance treatment had their case been assigned to a different case worker. To better understand this LATE, I follow Imbens and Rubin (1997), Dahl et al. (2014) and Bhuller et al. (2020) in decomposing the IV estimates into the average potential outcomes if the compliers would have received housing assistance and if they would not have received housing assistance. The top line in panel (b) of Figure VII is the number of potential returns to the homeless support system if the compliers would not have received housing assistance. The line trends upward in a close to linear fashion, with approximately 0.6 returns on average after 18 months. In sharp contrast, the compliers would have returned fewer times to the homeless support system if they would have received housing assistance; by month 18, they would only have returned less than 0.2 times to the homeless support system.

Panel (c) of Figure VII plots the distribution functions for cumulative potential returns to the homeless support system as of 18 months after intake for compliers if they would have received housing assistance in this time period and if they would not have received housing assistance. The difference between the two CDFs when the number of returns is one is around 10 percentage points, which is approximately half the size of the IV estimate graphed in Figure V at 18 months. Comparing the CDFs farther to the right (i.e., for a larger number of returns) makes clear that housing assistance is not simply preventing low-risk individuals from returning to homelessness. To see this, suppose that housing assistance caused individuals who would have returned once to not return at all, but that high-risk individuals (those who would return more than once to the homeless support system) were unaffected. In this case, the two lines in panel (c) would lie on top of each other starting at 2 returns. But, in fact, the two lines diverge at one return and lie on top of each other only after 8 returns. For example, approximately 15 percent of compliers would return to the homeless support system more than 2 times if they did not receive housing assistance, whereas only slightly more than 5 percent of compliers would have this many returns if they received housing assistance. Taken together, the results suggest that housing assistance must be preventing some individuals from returning many times to seek assistance from the homeless support system and stopping some individuals from returning to the homeless support system altogether.[30]

5.2 Heterogeneous Effects: Individual Characteristics

I document heterogeneous effects of housing assistance receipt on future homelessness by individual characteristics. I begin by showing that the estimated effect for individuals experiencing homelessness for the first time is similar to the estimated effect for the overall sample of cases. I then show that individuals with higher acuity, i.e., with physical disabilities and/or severe mental illness, see larger reductions in return to homelessness rates compared to individuals without these disabilities.

First Time Homeless. It is possible that first-time homeless are much more likely to benefit from housing assistance compared to individuals who have been homeless for a long time, since the former group is more likely to have the required skills to maintain housing. To explore this possibility, I limit the sample to first time homeless, defined as individuals who have not been previously assessed by a case worker and have not received services from the homeless support system in the past. Table C.5 reports results analogous to Table IV for this subsample. The 18-months cumulative estimates in column 3 are smaller for first time users of the system, with the estimated reduction in the probability of future homelessness lower by 5 percentage points compared to the full sample result.

Looking at first time users is useful not only for exploring heterogeneous effects, but also for ease of interpretation. In the estimation sample, individuals can appear more than once if they have multiple intakes over time. These individuals can be in the housing assistance group in one case and the no-housing assistance group in another. With first-time users of the homeless support system, each individual appears only once in the sample. The cost of looking only at an individual's first interaction with the homeless support system is that the sample drops by 44 percent, from 26,752 to 15,146. Given the results are qualitatively similar but with less precision for the smaller sample, I focus on results using the more comprehensive dataset which contains all cases with random assignment.

Heterogeneous effects by observed case characteristics. Table C.6 presents OLS and 2SLS estimates of the effect of housing assistance programs on future homelessness, stratified by observable individual characteristics. Differences in IV results are suggestive of differential

[30] From the graph, one cannot infer whether an individual with 3 returns reduces their returns to 0 versus whether an individual with 3 returns reduces their returns to 1 while the individual with 1 return reduces their returns to 0. But the shapes of the CDFs do imply that high-risk individuals (in terms of risk of returning to the homeless support system) must reduce their number of returns.

impacts of housing assistance on the propensity to return in the future to the homeless support system.

The first result implies that individuals who are more likely to receive housing assistance based on their observed characteristics seem to benefit more from it. In panel A, I split the sample by the predicted probability of receiving housing assistance.[31] I split the sample by being above or below the median of this composite index based on all observables. The OLS estimates suggest that individuals below median propensity of receiving housing assistance are similarly likely to return to the homeless support system compared to those with above median propensity of receiving housing assistance. However, the 2SLS estimates show a different picture, with a reduction of 22 percentage points in future return probability for individuals with above median propensity for receiving housing assistance, compared to a reduction of 17 percentage points in future return probability for individuals with below median propensity for receiving housing assistance.

Consistent with the findings in panel A, I find that the effect of housing assistance on future homelessness is larger in magnitude for those who have higher acuity score, have a physical or mental disability, and are older. In particular, I find that individuals who belong to one or more of these groups (i.e., high-acuity individuals) have approximately twice as large an effect in terms of the reduction in probability of returning to the homeless support system. These characteristics are highly predictive of whether an individual receives housing assistance, suggesting that individuals who are generally prioritized for housing assistance are more likely to benefit from it.

5.3 Heterogeneous Effects: Program Characteristics

I document heterogeneous effects of housing assistance receipt on future homelessness by program characteristics. I find that the effect of housing assistance on future homelessness is driven solely by placements in permanent housing programs. Consistent with this finding, I show that the effect of housing assistance on future homelessness increases in magnitude with the duration of housing assistance, and that this result is driven by intensive margin responses (e.g., moving from a 6-days temporary housing program to a 6-months permanent housing program).

Permanent versus Temporary Housing. As a reminder, there are two main types of housing assistance programs for individuals experiencing homelessness in Los Angeles County: per-

[31] I compute the predicted probability of housing assistance receipt using a probit model where the dependent variable is whether an individual received housing assistance or not on all individual-level characteristics and fixed effects I include in the baseline specification.

manent and temporary. As described in Section 2.2 and in Appendix A, permanent housing programs connect individuals to permanent housing units which they are expected to keep after housing assistance has ended, while temporary housing programs provide temporary shelter for individuals until they can solve their homelessness problem or until space in a permanent housing program becomes available. Whether an individual receives temporary or permanent housing assistance depends to some extent on the acuity of their situation and the availability of beds/units, and among many other factors, including case workers' discretion.

Case workers are able to influence the type of housing assistance an individual receives, and indeed some case workers place more individuals in permanent housing programs compared to others. I examine whether the case worker housing placement rate is also capturing differences in the quality of housing placements, where I consider permanent housing assistance to be of higher quality compared to temporary housing assistance. To explore this possibility, I run a multinomial regression with three outcomes (received permanent housing assistance, did not receive permanent housing assistance but received temporary housing assistance, did not receive housing assistance), and I find that being assigned to a case worker with a higher housing placement rate increases the probability of receiving permanent housing assistance.[32] In addition, in Table C.7, I run first-stage-like regressions where I regress permanent (temporary) housing receipt on case worker housing placement rate, and find that the first-stage coefficients are positive and statistically significant. However, I cannot reject the hypothesis that they are equal.

To explore whether individuals receiving temporary versus permanent housing assistance experience different outcomes, I construct two instruments for temporary and permanent housing assistance receipt in a similar fashion to the original instrument. Specifically, I construct two housing placement rates for each case worker, one for permanent housing placements and the other for temporary housing placements. The sum of these two instruments gives the original housing placement rate instrument.[33]

In Table V, I re-estimate my main IV specification, but with the two separate endogenous variables and instruments described above. I find that individuals who received permanent housing assistance treatment are 31 percentage points less likely to return to the homeless support system within 18 months compared to individuals who received no housing assistance, while individuals who received temporary housing assistance treatment are only 2.3 percentage points less likely to return to the homeless support system within 18 months compared to individuals who did not receive housing assistance, and that this effect is statistically

[32]In a multinomial logit regression, case worker housing placement rate has an average marginal effect of .317 (s.e. .028) for permanent housing assistance versus .192 (s.e. .038) for temporary housing assistance, with no housing assistance being the omitted category.

[33]Table C.8 presents the corresponding balancing tests for these instruments.

insignificant. This result suggests that housing programs that help connect an individual to permanent housing, essentially exiting them from homelessness by securing a long-term housing solution, are more effective in preventing future returns to the homeless support system. However, these programs are more costly, and I address the question of whether they are cost effective in Section 7.

Duration of Housing Assistance It is possible that case workers with a higher propensity to place individuals in housing programs are also more likely to place their clients in programs with a longer duration. If this is the case, the baseline estimates capture a linear combination of the extensive margin effect of enrolling in a housing program and the intensive margin effect of housing assistance duration. As shown in Figure C.1, the median duration of housing assistance is about 100 days in my sample, with roughly 85% of housing assistance duration being less than one year. Empirically, there is significant variation in duration of housing assistance across case workers, even when holding housing placement rates fixed. This is consistent with the hypothesis that case workers' influence is mostly through connecting individuals to housing programs and only slightly influence the duration of assistance.

I explore various models which use duration of housing assistance. To provide context, panel (a) of Figure C.2 graphs housing assistance duration in days (including zeros) as a function of the case worker housing placement rate. Panel (b) illustrates how duration of housing assistance is affected by the instrument. It plots estimates of the probability that the duration of housing assistance will exceed a given number of days (including zeros) as a function of the case worker housing placement rate instrument, and reveals that a case worker's placement rate effect on the number of days is larger for shorter duration spells and decreases as duration of housing assistance increases, also consistent with case workers having more influence on placement rather duration.

A complementary analysis is to replace the endogenous variable of housing assistance receipt with duration of housing assistance, but still use the case worker housing placement rate as the instrument. As shown by Angrist and Imbens (1995), 2SLS applied to an IV model with variable treatment intensity (such as duration of housing assistance in days) captures a weighted average of causal responses to a unit change in treatment, for those whose treatment status is affected by the instrument. The weight attached to the jth unit of treatment is proportional to the number of people who, because of the instrument, change their treatment from less than j to j or more. In my setting, this means that defining the endogenous regressor as duration of housing assistance in days permits identification of a weighted average of the effect of another day of housing assistance. Thus, this parameter captures a convex combination of the extensive margin effect of enrollment in a housing program and the intensive margin effect of longer program duration. When estimating this model with days of housing assistance as the endogenous regressor, the results are consistent with those using the binary housing assistance measure. The effect of increasing the duration of housing assistance by 250 days (the average housing assistance duration implied by the instrument for individuals receiving housing assistance), yields estimates which are similar in size to the estimates based on the binary endogenous variable of housing assistance (see Table C.9).

Finally, I consider models which include both housing program enrollment and housing program duration simultaneously. My first exploration is what happens if I control for a case worker's housing assistance duration rate, defined as the average duration of housing assistance in other cases the case worker has handled. In Table C.10, Panel C, when I add in controls for housing assistance duration rate, the first stage estimate is slightly reduced but the IV estimates are reduced by about half and are no longer statistically significant. This result is due to the high correlation between the case worker housing placement rate and the case worker housing assistance duration rate. In Table C.11, I treat both housing assistance receipt and duration as endogenous variables and use the case worker housing placement and housing assistance duration rates as the two instruments. I find that all of the effect on future homelessness can be attributed to the duration of housing assistance received (intensive margin) and that there is no effect on future homelessness for the extensive margin, suggesting that longer housing assistance spells are driving reductions in future returns to homelessness, consistent with the result that the effect of housing assistance on future homelessness is driven by permanent housing programs, which are usually also longer in duration.

5.4 Robustness

Intakes Per Case Worker. Table C.12 examines the sensitivity of the results to alternative minimum case worker intakes required for inclusion in my estimation sample. Column 1 presents the baseline results, which include any cases whose case worker handled at least 15 cases in 2016-2017. In the next four specifications, I instead require case workers to handle at least 10, 20, 30, or 40 cases, respectively. These changes do not materially affect the estimated effects. This is reassuring, as one might be worried the statistical inference becomes unreliable if the number of cases per case worker is too small.

Fixed Effects Selection. Table C.13 examines the sensitivity of the results by allowing the fixed effects within which time period and site are compared to vary. Column 1 presents the baseline results, where case worker assignment is random conditional on service site by month of intake, for comparison. In this specification, I include cases from service sites that had at least two case workers working in a given month. In the next two specifications, I instead require at least two case workers working in the same site in a given quarter and year, respectively. In columns 4 and 5, I change the sample criteria and require that at least two case workers working in the same month for the same service provider (who might operate several service sites) and in the same Service Planning Area of Los Angeles County (which have different service providers operating in them), respectively.[34] These different selections of the level at which cases are compared are not different from the estimated baseline effects. This is reassuring, as one might be worried the cell sizes used in the estimation sample might be too small, or that some service providers or sites are driving the results, and thus sensitive to changes in specification.

Treatment Definition. Table C.14 examines the sensitivity of the results to the definition of treatment. Column 1 presents the baseline results, where housing assistance treatment is defined as being enrolled in any housing assistance program within 18 months after intake date. In the next four specifications, I instead require that enrollment to housing assistance programs occurs within 1 month, 3 months, 6 months, and 12 months after intake to be considered as treated, respectively. One limitation of the data is that I cannot observe if a placement in a housing program is directly linked to the case worker. As a result, I face a trade-off when deciding what the relevant time period is to consider whether the case worker's involvement was relevant for the housing placement. The closer the housing placement is to enrollment, the more likely it is that the case worker is directly responsible for it. This fact is verified by observing the first-stage coefficients, which range from 0.86 when treatment window is defined as one month after intake to 0.64 when treatment window is 18 months after intake. However, due to the short supply of housing units in Los Angeles County, waiting times for housing assistance, especially for permanent housing programs, can be exceptionally long, reaching more than a year in some cases. As a result, I could count individuals as untreated due to long waiting times. My estimates suggest that the size of the effect of housing assistance on future returns to the homeless support system is larger the longer the treatment window is, consistent with longer waiting time for permanent housing placements and larger effects for these type of programs compared to temporary housing programs (see Section 5.3). Yet reassuringly, all treatment definitions suggest that housing assistance receipt reduces future homelessness.

[34] There are eight service planning areas (SPAs) in the county of Los Angeles.

Instrument Specification. Table C.15 examines sensitivity to changing how the instrument is constructed. In column 2, I check whether the results are sensitive to outliers by winsorizing the top and bottom 5 percent values of the baseline instrument. In column 3, I randomly split the sample in half and use one half of the sample to calculate the average housing placement rate for each case worker. I next use these measures of case worker housing placement rate as an instrument for housing assistance in the other half of the sample. In column 4, I construct the instrument using all available cases, including veteran cases. I construct the measure in this way in order to verify that veterans' housing placements are indeed orthogonal to case worker assignment. Finally, in column 5, I construct the instrument using a residualized, leave-out case worker housing placement rate that accounts for service site by month of intake fixed effects. Specifically, I regress housing assistance receipt on fully interacted service site by month of intake fixed effects and construct a case worker housing placement rate using the residuals obtained from this regression. I construct the measure in this way to address the possibility that there are differences across service sites and over time in availability and policy of providing housing assistance. Across all these different instrument definitions, the resulting estimates (and standard errors) do not materially change.

Alternative Outcomes. Table C.16 examines the robustness of the results to different definitions of future homelessness using the available data in order to alleviate concerns that the results in this study are sensitive to the way future homelessness is defined. In panel A, the explanatory variable of interest (treatment definition) is the standard definition used in this study, that is, an indicator for whether the individual was enrolled in at least one housing program that serves the homeless population during the 18 month period after intake. Column 1 presents the baseline results using the original definition of future homelessness. In columns 2 and 3, I decompose the original future homelessness outcome into its two components: enrollment in a street outreach program (column 2) and new homelessness case intake (column 3). The results in column 2 show that individuals who receive housing assistance are less likely to enroll in a street outreach program during any time from intake. The results in column 3 show that individuals who receive housing are not more likely to return to seek assistance from the homeless support system (the coefficients are statistically insignificant and close to zero) than those who do not receive housing assistance, in sharp contrast to the OLS coefficient that are positive, statistically significant, and large in magnitude.

The outcome variable in column 4 of Table C.16 is an indicator for whether the individual reported finding a permanent housing solution at least once in the 18 month period after intake and in months 10-18 after intake in the top and bottom parts of panel A, respectively. This is a self-reported survey question that individuals engaging with Homelessness Management

Information System (HMIS) are asked as part of their homelessness services process. I note that there are two main caveats that require caution when interpreting the results when using this outcome. First, this data is self-reported, as opposed to all other outcomes so far which were based on administrative records. Second, only individuals who are enrolled in a program that is being operated by a service provider in the homeless support system and provide information on employment and income are included in the sample. With that in mind, I find that individuals who receive housing assistance are 67 percentage point more likely to report finding a permanent housing solution at least once in the 18 month period after intake. The positive likelihood of reporting finding permanent housing remains even when examining only the 10-18 months after intake, where many individuals that received housing assistance are not receiving it anymore, implying that there is also a long-term positive impact of housing assistance receipt on stable housing situation.

In columns 5 and 6 of Table C.16, I examine what happens when considering the temporary housing assistance (i.e., emergency shelter stays) as a negative outcome, in the same way many other studies in the literature have used (Aubry et al., 2016; Gubits et al., 2018; Collinson and Reed, 2018). In column 5, the outcome variable is any enrollment in a temporary housing program in the 18 months after intake or the 10-18 months period after intake. As expected, we find a positive effect for enrolling in temporary housing programs during this period, since it is defined as housing treatment in our baseline treatment definition. However, when considering the 10-18 months period after intake, we see that individuals who received housing assistance are 5.7 percentage points less likely to enroll in a temporary housing program. Although this effect is not statistically significant at the 10 percent level, this indicates that fewer individuals who received housing assistance are still enrolled in temporary housing programs. Finally, in column 6, I add enrollment in temporary housing program to the baseline future homelessness measure (street outreach enrollment and new intake) and find similar patterns.

Finally, In panel B of Table C.16, we repeat the same specifications of panel A but using enrollment in any permanent housing program as the treatment variable instead of enrollment in any housing assistance program. The purpose of this exercise is to include enrollment in a temporary housing program as an indicator for return to homelessness and not as a treatment. The main difference is, as expected, in columns 5 and 6, where enrollment in a temporary housing program in considered as an event that defined return to homelessness. I find that in all periods, individuals who enroll in a permanent housing program are less likely to enroll in a temporary housing program in the future and to return to homeless support system in the 18 month period after intake.

Overall Table C.16 shows that individuals who receive housing assistance are less likely to engage with the homeless system in the future and are more likely to report finding a permanent housing solution, and this effect remains even after many individuals are not receiving housing assistance anymore. The analysis is robust to including temporary housing programs as a negative outcome rather than treatment. However, in the context of Los Angeles, I feel that it is more appropriate to include temporary housing programs as housing assistance since most homeless individuals will not even receive those services as there is no right-to-shelter mandate in Los Angeles County.

Exclusion Restriction As discussed in Section 4.3, interpreting the IV estimates as the average causal effect of housing assistance requires the case worker housing placement rate to affect an individual's outcomes only through the housing assistance channel. A potential issue is that case workers may also affect an individual's receipt of non-housing services that are intended to support the individual's transition out of homelessness. These supportive services include providing meals and showers, health care and mental health treatment, substance abuse treatment, employment, life skills classes and education, and general case management.

To examine the potential impact on individuals' outcomes via non-housing services, I extend my baseline IV model to distinguish between housing assistance and non-housing assistance:

$$H_i = \alpha Z^H_{(j)i} + \gamma Z^S_{j(i)} + \chi_{sm} + \nu_i \tag{4}$$

$$S_i = \tau Z^H_{j(i)} + \psi Z^S_{j(i)} + \lambda_{sm} + u_i \tag{5}$$

$$Y_{it} = \beta_t H_i + \theta_t S_i + \delta_{sm} + X_i^{'} \omega_t + \rho_{it} \tag{6}$$

where j denotes the case worker who handles individual i's case, H_i is an indicator variable equal to 1 if individual i received any housing assistance in the 18 months following intake, S_i is an indicator variable equal to 1 if individual i received any non-housing assistance in the 18 months following intake, $Z^H_{j(i)}$ denotes the case worker housing placement rate, $Z^S_{j(i)}$ denotes the case worker non-housing services placement rate, and X_i is a vector of control variables. All specifications include a full set of service site by month fixed effects. The omitted reference category is no assistance received at all. As in the baseline model, I measure $Z^H_{j(i)}$ and $Z^S_{j(i)}$ as leave-out means.

There are two cases in which the baseline IV estimates are biased because they abstract from the case worker's in providing other types of assistance. In the first case, $Z^H_{j(i)}$ correlates with $Z^S_{j(i)}$, and $Z^S_{j(i)}$ directly affects Y_{it} (conditional on fixed effects and individual level covariates). This would violate the exclusion restriction in the baseline IV model because $Z^H_{j(i)}$ not only affects Y_{it} through H_i but also through its correlation with $Z^S_{j(i)}$. However, controlling for $Z^S_{j(i)}$ in both (1) and (2) eliminates this source of bias. In the second case, $Z^H_{j(i)}$ correlates with S_i conditional on $Z^S_{j(i)}$, and S_i affects Y_{it} holding H_i fixed (conditional on fixed effects and individual level covariates). In the baseline IV model, this would violate the exclusion restriction because $Z^H_{j(i)}$ affects Y_{it} not only through H_i but also through its influence on S_i. The augmented IV model (3)-(5) addresses this issue by including S_i as an additional endogenous variable and $Z^S_{j(i)}$ as an extra instrument.

I examine these two cases and find support for the exclusion restriction. The top panel of Table C.10 repeats the baseline specification for comparison. In panel B, I add the case worker non-housing services placement rate as an additional control in both the first and second stages. The IV estimates for both future homelessness outcomes are similar to my baseline.

I next estimate the augmented IV model given by (3)-(5). Table C.17 presents the first stage, reduced form, and IV estimates. For the housing assistance first stage, the case worker housing placement rate has a coefficient similar to that in the baseline model. For the other first stage, the case worker housing placement rate has a negative impact on receiving non-housing services, but the other instrument has a large positive effect. Looking at the reduced form estimates, the coefficients on the case worker housing placement rate are virtually unchanged relative to the baseline IV model. Likewise, the IV estimates for housing assistance are similar to those from the baseline model which does not include the instrument for the non-housing services placement.

A useful byproduct of examining the threats to exclusion from case worker effects other than housing placement is that it helps with interpretation. The baseline IV model compares potential outcomes if the individual received housing assistance to the outcomes that would have been realized if they did not. The augmented IV model further distinguishes between no assistance at all and non-housing assistance. The IV estimates show significant effects of receiving housing assistance compared to not receiving any assistance, whereas receiving non-housing services has no effect on future homelessness.

6 Additional Socioeconomic Outcomes

In this section, I present my findings on the effect of housing assistance on a large set of socioeconomic outcomes. Table VI presents my main findings. I show that (i) housing assistance causes a reduction in the number of emergency department visits, (ii) a reduction in mental health services received, (iii) a reduction in the number of jail days and the probability of committing a crime, (iv) an increase in the probability of reporting employment, and (v) no effect on receipt of social benefits.[35]

Department of Health Services. In Table D.2, I present OLS and IV estimates of Equation (1) for various outcomes related to Los Angeles County's Department of Health Services (DHS) service utilization. In Panel A, the dependent variable is an indicator equal to 1 if the individual received treatment within 18 months after intake, and in Panel B the dependent variable is the number of treatments (days) the individual received in the same time period. Column 1 combines all treatment types, while columns 2-4 break treatments into inpatient, outpatient and emergency services, respectively. The IV estimates are negative and significant for overall DHS treatments and for emergency department visits, indicating that participation in housing programs lead to a reduction in the number of health services received and of emergency department visits in particular. Specifically, there is a 5.4 percentage points drop in the probability of visiting the emergency department and 0.14 reduction in the number of emergency department visits, although the latter is not statistically significant. Overall, the observed reduction in overall DHS services and emergency department visits suggests that housing assistance helps stabilize an individual's health and also prevents them from being exposed to dangerous and extreme situations which might increase the possibility of physical harm.

Department of Mental Health Services. In Table D.3, I present OLS and IV estimates of Equation (1) for various outcomes related to Los Angeles County's Department of Mental Health (DMH) service utilization. In Panel A, the dependent variable is an indicator equal to 1 if the individual received treatment within 18 months after intake, and in Panel B the dependent variable is the number of treatments (days) the individual received in the same time period. Column 1 combines all treatment types, while columns 2-4 break treatments into acute inpatient, residential and outpatient services. The IV estimates suggest that housing assistance reduces the probability of receiving mental health services in the 18-month period after intake by 4.6 percentage points, relative to a baseline mean of 7 percentage points. Moreover, the estimates suggest that individuals who receive housing assistance spend 3 days fewer in inpatient or skilled nursing facilities treating mental health, compared to a baseline mean of 3.5 days. This suggests that housing programs divert individuals from skilled nursing facilities, which are far more expensive compared to supportive housing programs. In addition, I find that individuals who receive housing assistance see a reduction in outpatient mental health treatments, although this effect is statistically insignificant. Overall, the results

[35] In this section, I use subsamples of my baseline estimation sample because of data limitations. Table D.1 verifies that the first stage and recidivism findings I document in the previous sections are valid across all the subsamples I use to explore additional economic and social outcomes.

suggest that housing assistance receipt leads to a reduction in the probability and number of mental health treatments received, indicating increased stabilization of mental health among housing assistance recipients. Moreover, the decrease in inpatient and residential days in skilled nursing facilities suggest that housing assistance can be a good solution for some individuals with serious mental illnesses who can live on their own but do not have the resources or are facing barriers to housing.

Department of Public Health. In Table D.4, I present OLS and IV estimates of Equation (1) for various outcomes related to the Los Angeles County's Department of Public Health (DPH) service utilization. The Department of Public Health mostly provides substance abuse treatments. In Panel A, the dependent variable is an indicator equal to 1 if the individual received treatment within 18 months after intake, and in Panel B the dependent variable is the number of treatments (days) the individual received in the same time period. Column 1 combines all treatment types, while columns 2-4 break treatments into detox, residential and outpatient services. The IV estimates suggest that housing assistance reduces DPH outpatient services by 0.11 over an 18-month period, compared to a baseline mean of 0.08. Moreover, there seems to be no relationship between housing assistance receipt and participation in detox or residential programs that assist with substance abuse problems.

Criminal Activity. In Table D.5, I present OLS and IV estimates of Equation (1) for various outcomes related to crime from the Los Angeles County Sheriff's Department (LASD) and the Los Angeles County Probation Department.[36] In column 1, the dependent variable is the number of jail bookings an individual had in the 18-month period after intake. The OLS coefficient shows that individuals who received housing assistance are more likely to have been in jail during this period. The IV estimates, however, show that there is a significant reduction in the number of jail bookings, with individuals who received housing assistance having 1.5 fewer jail bookings on average compared to individuals who did not receive housing assistance. Column 2 shows that there is a corresponding decline in the number of jail days for individuals who received housing assistance. In columns 3 and 4, the dependent variables are an indicator for whether the individual was charged for a crime at least once and the number of charges during the 18-month period after intake, respectively. Consistent with the jail results, I find that individuals who received housing assistance were 7.9 percentage points less likely to be charged with at least one crime and were charged with .4 fewer crimes during

[36]The Los Angeles County Sheriff is a major law enforcement agency in Los Angeles County, but it is not the only one. Specifically, many cities operate their own police departments, with the largest one being the Los Angeles Police Department (LAPD). This implies that the records I have doe not cover the universe of law enforcement activity in Los Angeles County, but only a part of it.

this period, compared to baseline means of 0.1 and 0.22, respectively. In columns 5 and 6, the dependent variables are an indicator for whether the individual was under probation at least once during the 18 months after intake and the number of days under probation, respectively. The IV estimates are negative, suggesting that there is a drop in the probability of being under probation; however, this effect is not statistically significant. Taken together, the results on jail bookings, crimes, and probation suggest that housing assistance leads to a reduction in criminal activity, which is translated into fewer jail bookings and days and reduced probability of being under probation.

Employment and Income. The Homeless Management Information System (HMIS) contains self-reported information on income and employment. I use these responses to examine the effects of housing assistance on these outcomes. However, I note that there are two main caveats that require caution when interpreting these results. First, this data is self-reported, as opposed to all other outcomes so far which were based on administrative records. Second, only individuals who are enrolled in a program that is being operated by a service provider in the homeless support system and provide information on employment and income are included in the sample. With that in mind, Table D.6 presents OLS and IV estimates of Equation (1) for employment, income, and social benefits outcomes. In columns 1-2, the dependent variables are an indicator equal to 1 if the individual reported having non-zero income and the individual's reported average monthly income, respectively. The OLS coefficients show that individuals who received housing assistance are also more likely to report non-zero income and also more likely to report a higher monthly income, suggesting that there might be selection on reporting income and employment. The IV estimates show that there is a 26-percentage point increase in the probability of reporting non-zero income and a $442 dollars increase in mean monthly income reported in the 18-month period after intake for individuals who received housing assistance. In columns 3-4, I find similar results for reporting employment and mean monthly wage. In particular, I find a 24-percentage point increase in the probability of reporting employment and a $430 dollars increase in mean monthly wage for individuals who received housing assistance in the 18-month period after intake. In columns 5-6, I show that there is no relationship between housing assistance receipt and social benefits receipt. Taken together, the results suggest that housing assistance leads to increased probability of finding employment, and that this increase in income is driven entirely by employment.[37]

[37] One concern is that preexisting employment and income might be influencing housing assistance receipt and the future homelessness result I find in the previous section. To explore this probability, I have attempted a version of my baseline model where I treat all future outcomes related to health, crime, employment, income, and social benefits, as controls in a specification where the dependent variable is future homelessness. I find that the IV estimates are not changed by the inclusion of these controls, suggesting that the effect I find is indeed driven by the housing assistance channel and not other channels.

Social Benefits. The Homeless Management Information System (HMIS) also contains self-reported information on receipt of various social benefits. I use these responses, in addition to administrative records on receipt of emergency cash assistance from the Department of Public and Social Services (DPSS) to examine the effects of housing assistance on social benefits. For self-reported outcomes, the same caveats and caution outlined for the employment and income data should be taken. In Table D.7, I present OLS and IV estimates of Equation (1) for receipt of different social benefits. In columns 1-4, the dependent variable is an indicator equal to 1 if the individual reported ever receiving emergency cash assistance (General Relief), supplemental security income (SSI), social security disability income (SSDI), and food stamps in the 18-month period after intake. The OLS coefficients show positive correlation between receiving housing assistance and reporting receipt of these social benefits. On the contrary, the IV estimates show no relationship between housing assistance and social benefits receipt. However, the estimates suggest that there is a reduction in receipt of emergency cash assistance and an increase in reporting of SSI, SSDI, and food stamps receipt, although these are not statistically significant. The reduction in emergency cash assistance combined with increase in other social benefits is consistent with increased housing and income stability. Overall, the results suggest that housing assistance does not seem to affect social benefits receipt, and if anything, reduces it.

7 Cost-Benefit Analysis

The most relevant policy implication is whether the positive effects from housing assistance for the homeless I find in this study are cost effective and is there a difference in the cost-effectiveness of different housing program types. It is difficult to estimate the benefits of reductions in homelessness and costs of housing assistance, with the few studies attempting to do so imposing strong assumptions and extrapolations to their computations (Culhane et al., 2002; Evans et al., 2016; Khadduri et al., 2010). I attempt to conduct a simple cost-benefit calculation of housing programs for the homeless. My calculations suggest that up to 80 percent of program costs are offset by corresponding benefits in the first 18 months following intake, and that the benefits tend to be larger in permanent housing programs.

To calculate the costs of housing programs reported in Table VII, I multiply the number of housing assistance days received for each individual in the sample during the 18-month period after initial intake by the average cost per day of each program type, such that direct housing costs are set at $35 per day for temporary housing, $40 per day for rapid re-housing, and $50 per day for permanent supportive housing (LAHSA,2017). The IV estimate which uses this outcome measures a cost of $10,366 per housing program enrollment. This measure captures the average cost of housing programs and not the marginal cost, which I would ideally estimate. In Panel B, I break housing programs by type (temporary and permanent) and estimate the cost of each using the two instruments I used when estimating the impact of permanent versus temporary housing programs on future homelessness in Section 5.3. The IV estimates measure an average cost of $5,095 per temporary housing program enrollment and an average cost of $12,402 per permanent housing program enrollment.

On the benefits side, I measure four broad categories. First, there is a reduction in homeless support system spending on future housing assistance due to fewer returns to the homeless support system. I compute the savings in housing costs per homeless system return avoided as the average housing assistance cost of an assessment in the sample. Homeless support system average savings in housing assistance costs are estimated to be $4,000 per intake. I then create an outcome variable which takes the total number of returns to homeless support system in the 18-month period after intake multiplied by $4,000. Using this measure, I estimate savings of $2,102 per housing program enrollment. In panel B, I estimate savings of $2,885 per permanent housing program enrollment and only an insignificant $558 per temporary housing program enrollment.

The second and third categories of benefits I compute are due to improved health and reduced crime, which are translated to reduction in use of public resources. I use estimates of Los Angeles County on the costs of the various treatments and services I explore in the ELP data. For example, the estimate for a day in jail is $200 per day. I then define public health costs as the sum of DHS and DMH costs, and law enforcement costs as the sum of jail days and probation months, where I use county estimates multiplied by the number of treatments or occurrences of each type of service. The IV estimates of these savings are $2,796 for health costs and $1,724 for law enforcement costs. In panel B, the IV estimates of these savings from temporary housing program enrollment are $3,214 and $1,089 for health and law enforcement, respectively, while the estimated savings from permanent housing program enrollment are $2,085 for health and $1,746 for law enforcement.

The third category of benefits is due to increased employment and no effect on social benefits receipt that I find in Section 6. I estimate the increase in taxes minus social benefits to be $1,146 per housing program enrollment. When looking at different housing program types, I estimate savings of $1,862 per permanent housing program enrollment and $353 in savings per temporary housing program enrollment. I define net transfers as all social benefits received minus all income taxes paid over the 18-month period after intake.

Overall, I find that a substantial portion of housing program costs are offset by the savings to public agencies in the first 18 months following intake. I note that these savings are likely to be even larger, as I ignore the indirect benefits from the reduction in street homelessness.

Moreover, these benefits are likely to accumulate over time and become larger, since the cost of homelessness increases exponentially with time (Flaming et al., 2015). Finally, I note that these savings tend to be larger in permanent housing programs, consistent with my findings regarding the effect of these programs on future homelessness.

8 Conclusions

The ongoing crisis of homelessness has generated a shift towards the Housing First approach, which aims to quickly provide individuals experiencing homelessness with housing assistance without preconditions (Burt et al., 2017). In recent years, researchers and policy makers have questioned whether housing assistance is sufficient to treat homelessness and whether the Housing First approach is cost effective. However, despite the widespread adoption of this policy, the existing literature did not provide robust evidence regarding these questions.

My study fills this gap in the literature using administrative data and exogenous variation in housing assistance receipt to confirm that housing assistance programs for the homeless can indeed reduce future homelessness, in addition to improving other socioeconomic outcomes that contribute to improved likelihood of successful rehabilitation and reintegration to society. The Los Angeles County Homeless Support System, despite its lack of resources, is successful in preventing future homelessness and improving important well-being measures when it provides housing assistance to individuals experiencing homelessness.

While this paper establishes these fundamental results, several important questions remain for future research. My results do not imply that housing assistance alone is cost effective for all individuals experiencing homelessness. Exploring additional research designs that will manipulate housing assistance receipt for the always- and never- takers in my sample is important for understanding how to treat this segment of the population with the highest level of needs. Additionally, while I provide some evidence that housing assistance has a beneficial effect on many socioeconomic outcomes, additional evidence would be useful to assess the external validity of my findings. Finally, the cost-benefit analysis I conducted ignores the most expensive part of housing assistance: acquisition and construction costs. Evidence taking these costs into account, either in a partial- or a general-equilibrium setting would be of great value.

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9 Figures

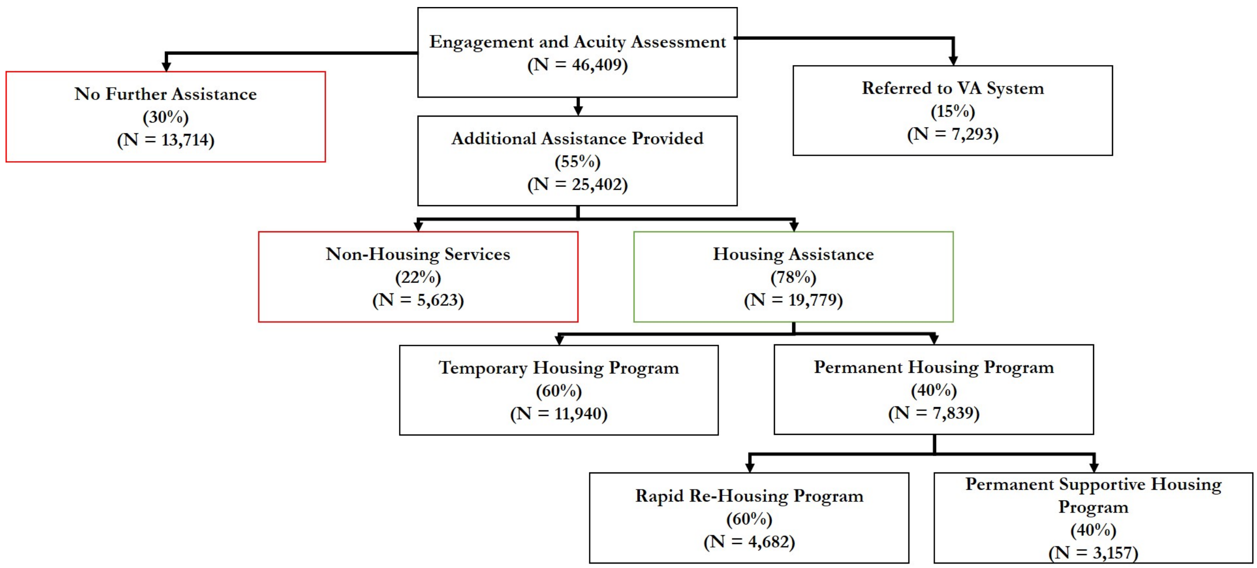


Figure I. CES Process and Best Treatment Distribution.

Note: The following chart displays homeless case outcomes by best treatment received. The sample consists of all intakes conducted in 2016-2017 for single adults experiencing homelessness by the homeless service providers in Los Angeles County. Treatments received are not mutually exclusive and best treatment received is presented for simplicity. The green and red colored boxes represent the treated non-treated cases in my estimation sample, respectively.

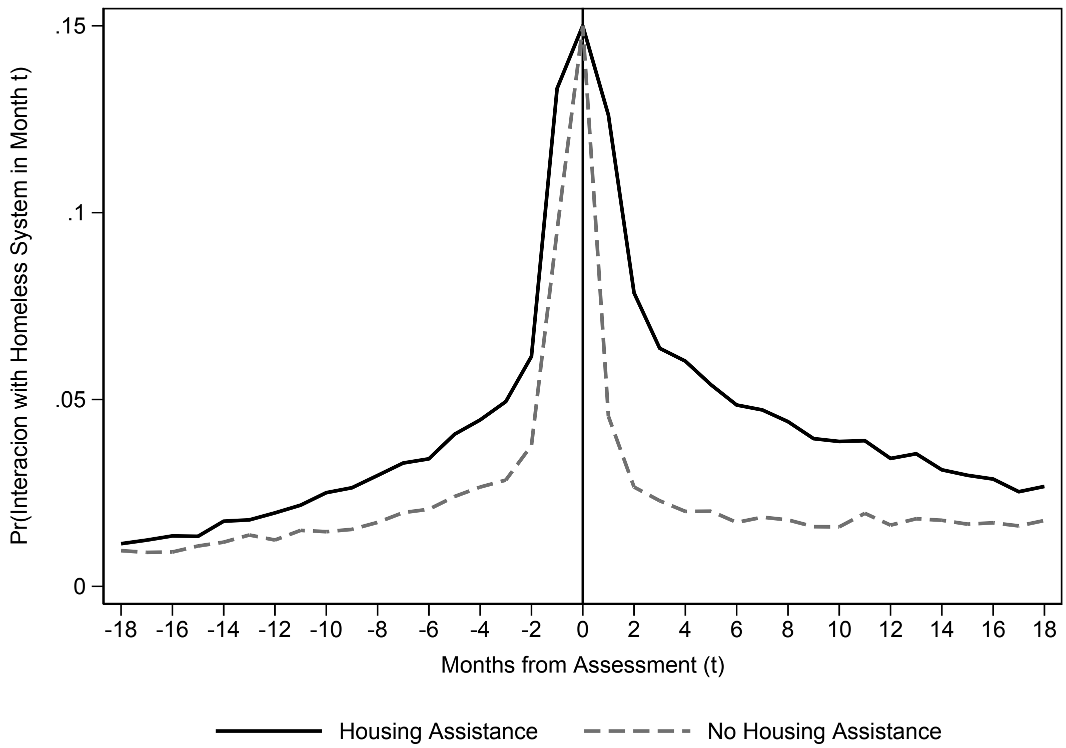


Figure II. Assistance Seeking from the Homeless System Before and After Month of Intake.

Note: Instrument sample consisting of 39,119 non-veteran single adult intakes in 2016-2017. Cases are categorized in two groups, those receiving housing assistance within 18 months from intake date, as shown in solid black, or those not receiving housing assistance within this period, as shown in the dashed grey line. Assistance seeking from the homeless system is defined as enrolling in a street outreach program or being assessed by a case worker at least once in each month. Month 0 outcome is capped at 0.15 for visual purposes (both groups have a probability of 1 in this month by definition).

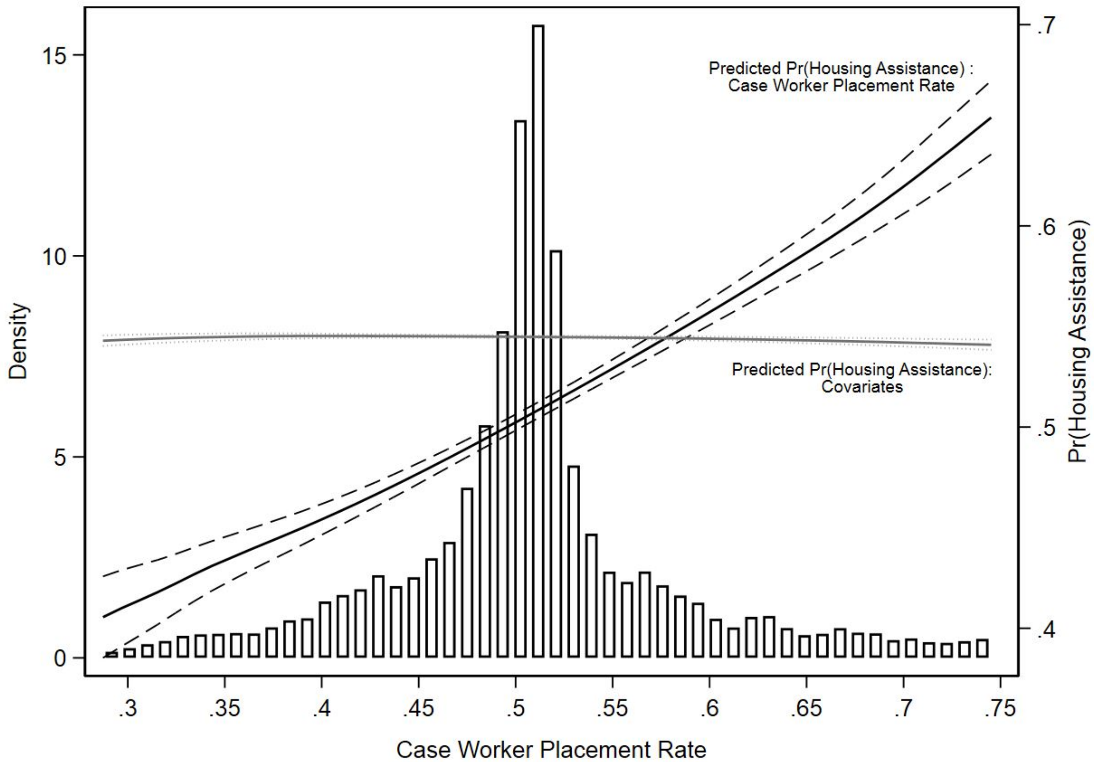
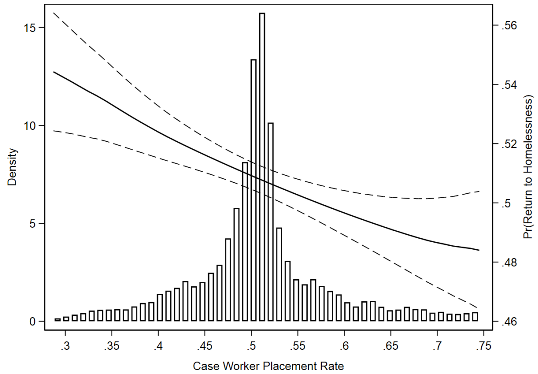
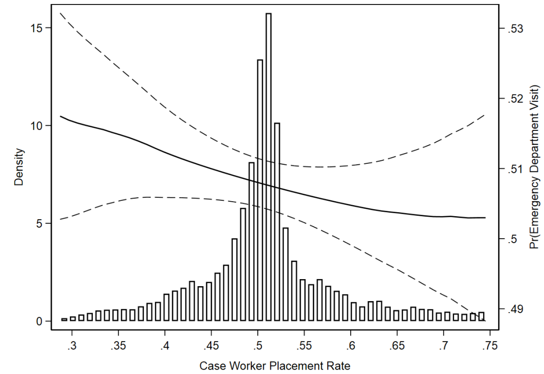


Figure III. First Stage Graph of Housing Assistance Receipt on Case Worker Housing Placement Rate.

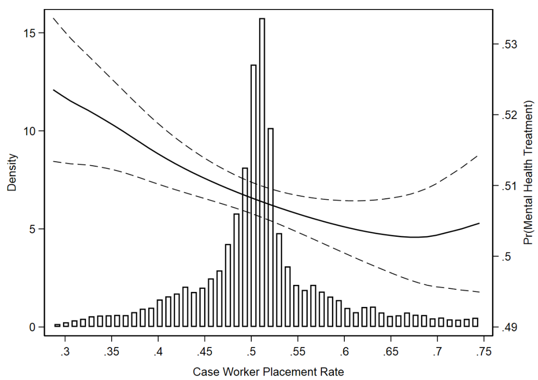
Note: Estimation sample consisting of 26,752 assessments processed in 2016-2017. Probability of housing assistance receipt is plotted on the right y-axis against leave-out mean case worker housing placement rate of the assigned case worker shown along the x-axis. The plotted values are mean-standardized residuals from regressions on site x assessment month fixed effects and all variables listed in Table II. The solid line shows a local linear regression of housing assistance receipt on case worker housing placement rate. Dashed lines show 95% confidence intervals. The histogram shows the density of case worker placement rates along the left y-axis (top and bottom 2% excluded).



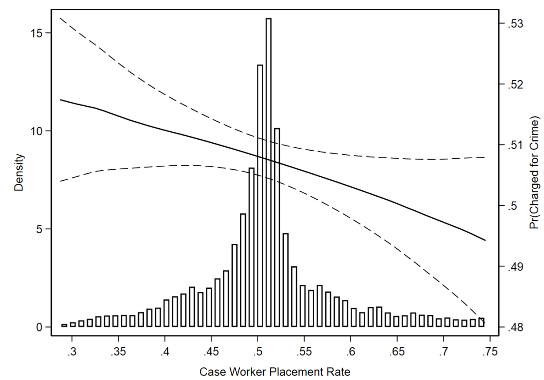
(a) Any Return to Homeless System



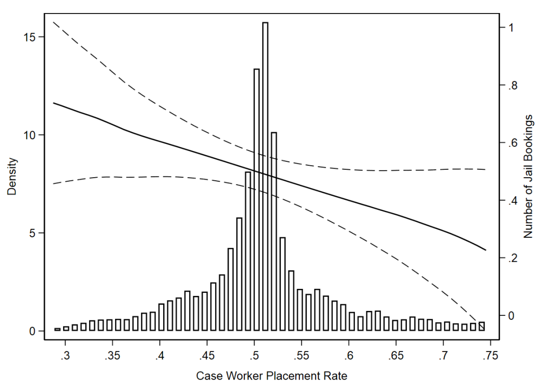
(b) Any Emergency Department Visit



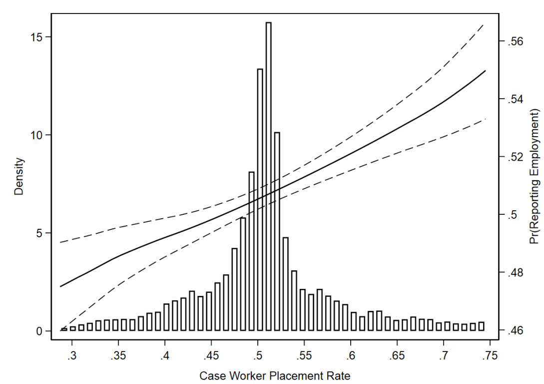
(c) Any Emergency Mental Health Treatment



(d) Any Criminal Charges



(e) Number of Jail Bookings



(f) Any Employment Reported

Figure IV. Reduced Form Graphs of Socioeconomic Outcomes on Case Worker Housing Placement Rate.

Note: Estimation sample consisting of 26,752 assessments processed in 2016-2017. Outcomes of interest (all measured at 18-months after intake) are plotted on the right y-axis against leave-out mean case worker housing placement rate of the assigned case worker shown along the x-axis. The plotted values are mean-standardized residuals from regressions on site x month fixed effects and all variables listed in Table II. The solid line shows a local linear regression of the outcome of interest on case worker housing placement rate. Dashed lines show 95% confidence intervals. The histogram shows the density of case worker placement rates along the left y-axis (top and bottom 2% excluded).

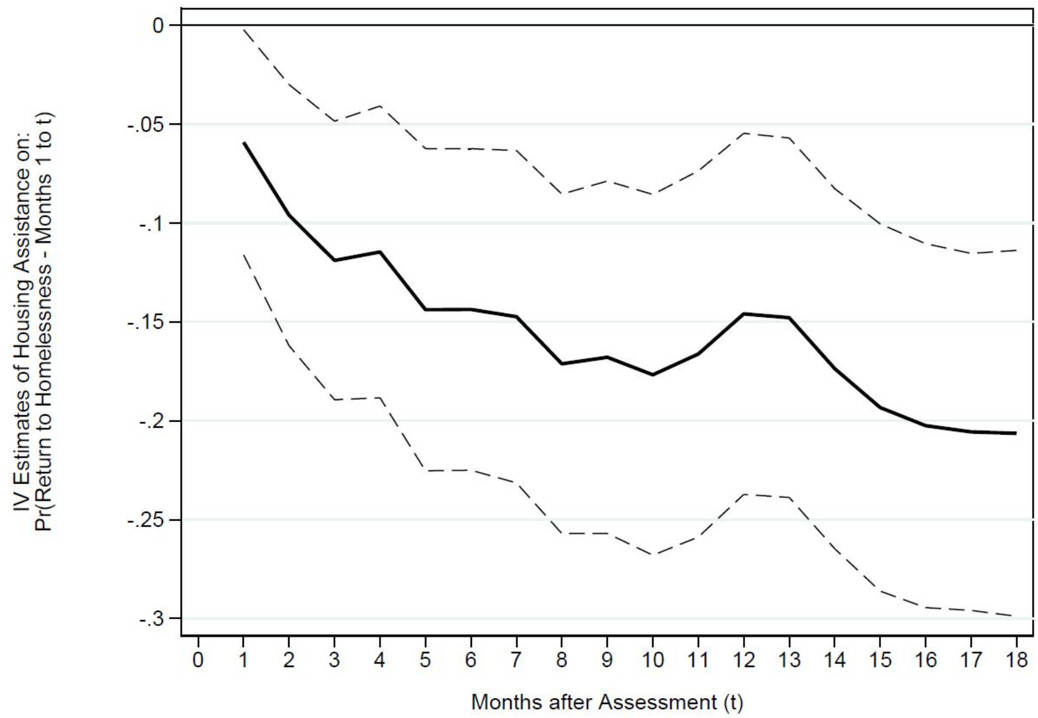
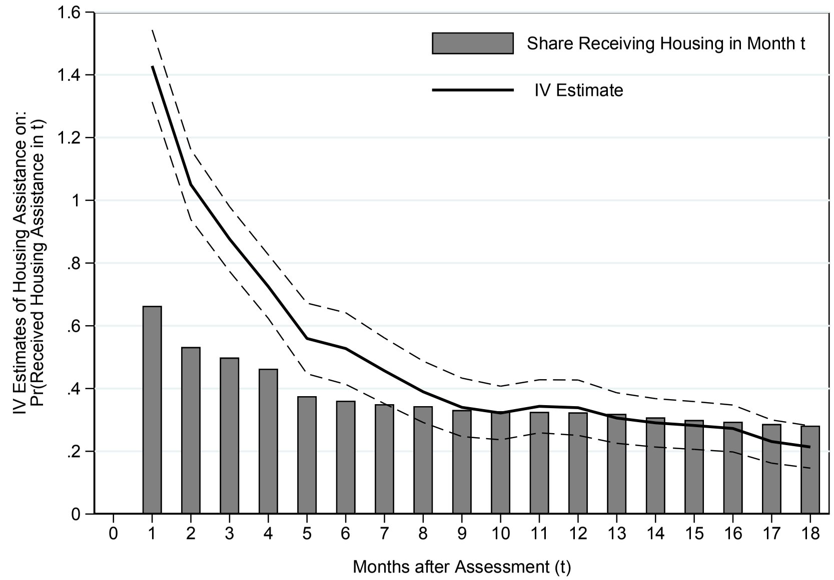
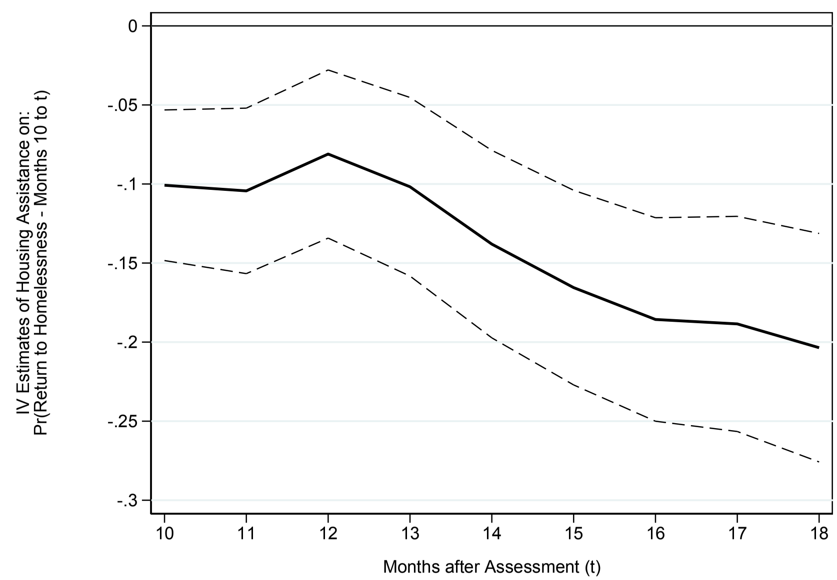


Figure V. The Effect of Housing Assistance on Returning to the Homeless Support System.

Note: Estimation sample consisting of 26,752 assessments processed in 2016-2017. Returns to the homeless support system include a new enrollment in a street outreach program or a new intake. Dashed lines show 90% confidence intervals.



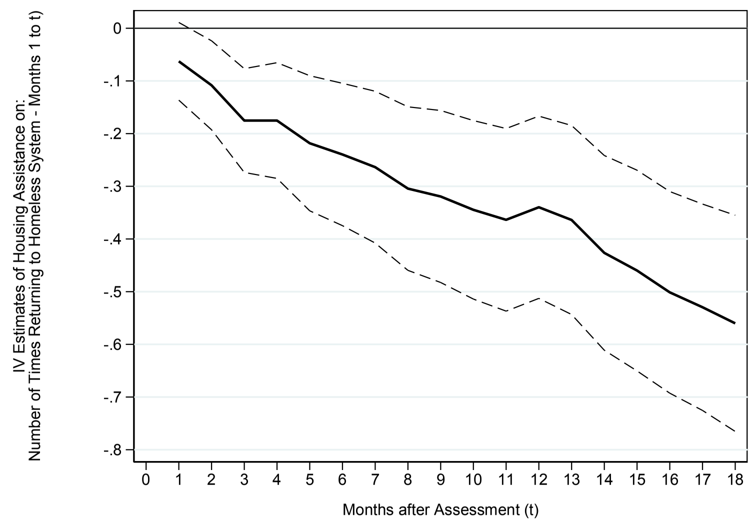
(a) IV Estimates: Pr(Receiving Housing Assistance - Month t)



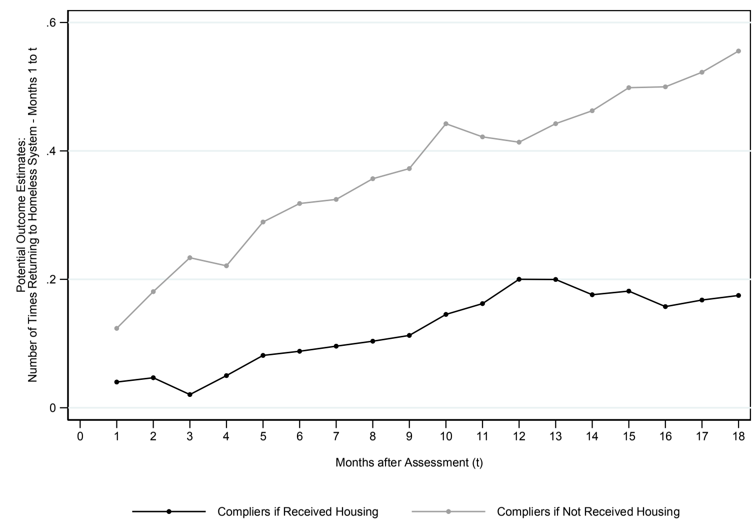
(b) IV Estimates: Pr(Returned to Homeless Support System - Months *10* to t)

Figure VI. Post-Treatment Effect of Housing Assistance on Returning to Homeless Support System.

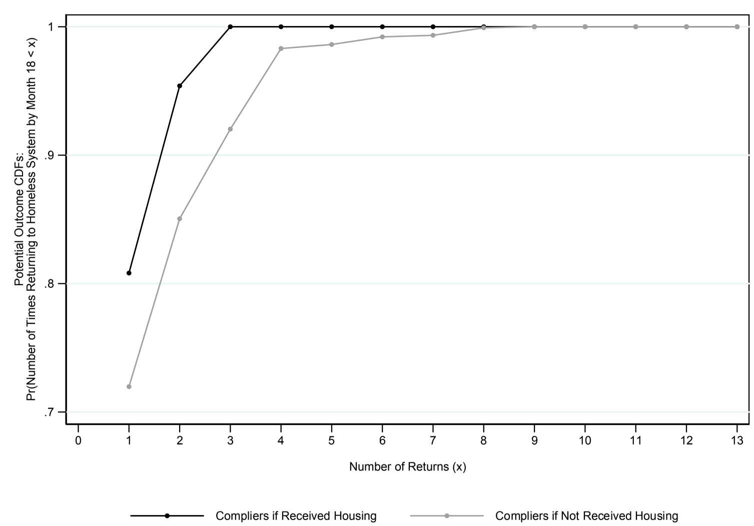
Note: Estimation sample consisting of 26,752 assessments processed in 2016-2017. In panel (a), any active enrollment in a housing program is considered. Grey bars show the share of individuals enrolled in a housing program in month t after intake. In panel (b), returns to the homeless support system include a new enrollment in a street outreach program or a new intake. Dashed lines show 90% confidence intervals.



(a) IV Estimates: No. of Returns to System – Months 1 to t



(b) Potential Outcomes: No. of Returns in Months 1 to t



(c) Potential Outcome CDFs: No. of Returns by Month 18

Figure VII. The Effect of Housing Assistance on Number of Returns to the Homeless System.

Note: Estimation sample consisting of 26,752 assessments processed in 2016-2017. In Panel (a), returns to the homeless support system include a new enrollment in a street outreach program or a new acuity assessment. Dashed lines show 90% confidence intervals. In Panel (b), potential number of returns to the homeless support system for compliers if they receive housing assistance or not are plotted. In Panel (c), the potential number of returns to the homeless system by 18 months after intake for compliers in the case they receive housing assistance and in the case they do not are plotted.

10 Tables

Table I. First Stage Estimates of Housing Assistance on Case Worker Placement Rate.

	(1)	(2)	(3)	(4)
Controls:	Site X Month FEs	Add Demographics	Add Acuity Measures	Add History of Interaction with Public Agencies
Dependent Variable:	**Pr(Received Housing Assistance)**			
Case Worker Housing Placement Rate	0.661***	0.652***	0.652***	0.644***
	(0.0381)	(0.0380)	(0.0382)	(0.0377)
F-statistic (Instrument)	300.13	294.89	291.38	292.22
Dependent Mean	0.545	0.545	0.545	0.545
Number of Assessments	26,752	26,752	26,752	26,752

Note: Columns 1-4 show first stage estimates of different specifications on the estimation sample of assessments conducted in 2016-2017. Column 1 includes site x month of assessment fixed effects. Column 2 adds the individual demographics listed in Table II. Column 3 adds acuity measures described in Table II. Column 4 adds lagged outcomes variables described in Table II. Standard errors are two-way clustered at the case worker and client level. *p<0.1, **p<0.05, ***p<0.01.

Table II. Testing for Random Assignment of Homeless Cases to Case Workers.

	Dependent Variables:				*Explanatory Variables:*	
	Pr(Received Housing Assistance)		**Case Worker Housing Placement Rate**		**Mean**	**Standard Deviation**
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error		
Demographics:						
Age	0.000507*	(0.000273)	0.000	(0.000)	45.12	(11.23)
Female	0.0166**	(0.00654)	0.00246	(0.00212)	0.342	(0.474)
Black	0.142***	(0.0159)	0.00735*	(0.00401)	0.509	(0.500)
Hispanic	0.102***	(0.0161)	0.00638	(0.00417)	0.231	(0.421)
White	0.0949***	(0.0163)	0.00501	(0.00445)	0.195	(0.396)
Acuity Assessment:						
Acuity Score (0-17)	0.00116	(0.00149)	-0.00110	(0.000893)	7.267	(3.710)
Homeless History	-0.0275***	(0.00937)	-0.00212	(0.00262)	0.717	(0.450)
Chronic Homeless	-0.000266	(0.00968)	0.000	(0.00240)	0.613	(0.487)
Physical Disability	-0.00404	(0.00657)	0.00170	(0.00210)	0.697	(0.459)
Serious Mental Illness	-0.000262	(0.00789)	0.000480	(0.00251)	0.576	(0.494)
Self Care Problems	-0.0131	(0.00805)	-0.00603	(0.00440)	0.291	(0.454)
Used Crisis Service in Past 6 Months	-0.0170	(0.0162)	0.00421	(0.00481)	0.0425	(0.202)
Health, Criminal, Housing History (Past 12 Months):						
Any Department of Health Services (DHS) Treatment	0.0102	(0.00848)	0.00135	(0.00160)	0.172	(0.378)
Any Department of Mental Health (DMH) Treatment	-0.000210	(0.0103)	-0.000301	(0.00179)	0.116	(0.321)
Any Substance Abuse Treatment	-0.00106	(0.0108)	0.00322	(0.00206)	0.0846	(0.278)
Involvement with Law Enforcement Agencies	-0.0132	(0.00916)	-0.00106	(0.00188)	0.137	(0.343)
Received Emergency Cash Assistance	0.00306	(0.00864)	0.000453	(0.00176)	0.192	(0.394)
Any Interaction with Homeless System	0.0194	(0.0118)	0.000653	(0.00267)	0.351	(0.477)
Any Housing Assistance Recieved	0.0676***	(0.0148)	0.00433	(0.00336)	0.282	(0.450)
F-statistic for joint significance test	9.174		1.117			
p-value	0.000		0.329			
Number of Cases	26,752				26,752	

Note: Columns 1-4 show estimates for estimation sample of individuals assessed in 2016-2017. Columns 5-6 show descriptive statistics of cases in the estimation sample. All estimations include controls for site x month of assessment FEs. Reported F-statistic refers to a joint test of the null hypothesis for all variables. The omitted category for race is missing/multiple/other race. Standard errors are two-way clustered at the case worker and client level. *p<0.1, **p<0.05, ***p<0.01.

Table III. Summary Statistics by Complier Type.

	Estimation Sample	Compliers (27%)	Always Takers (26%)	Never Takers (47%)
	(1)	(2)	(3)	(4)
Demographics:				
Age Above Median (47)	0.50	0.52	0.49	0.57
	(0.01)	(0.02)	(0.03)	(0.05)
Female	0.34	0.32	0.44	0.37
	(0.01)	(0.02)	(0.03)	(0.04)
Black	0.51	0.52	0.56	0.37
	(0.01)	(0.03)	(0.03)	(0.04)
Hispanic	0.23	0.19	0.18	0.26
	(0.01)	(0.03)	(0.03)	(0.04)
White	0.20	0.20	0.21	0.22
	(0.01)	(0.02)	(0.02)	(0.04)
Acuity Assessment:				
Homeless History	0.72	0.71	0.78	0.86
	(0.01)	(0.02)	(0.03)	(0.04)
Chronic Homeless	0.61	0.57	0.68	0.82
	(0.01)	(0.02)	(0.03)	(0.04)
Physical Disability	0.70	0.64	0.71	0.91
	(0.01)	(0.02)	(0.03)	(0.02)
Mental Disability	0.58	0.51	0.65	0.79
	(0.01)	(0.02)	(0.03)	(0.04)
Self Care Problems	0.29	0.20	0.32	0.34
	(0.01)	(0.03)	(0.04)	(0.04)
Past Health, Criminal, Housing History:				
Any DHS Treatment in Past 12 Months	0.17	0.17	0.14	0.14
	(0.003)	(0.02)	(0.02)	(0.03)
Any DMH Treatment in Past 12 Months	0.12	0.10	0.09	0.14
	(0.002)	(0.02)	(0.02)	(0.03)
Any Substance Abuse Treatment in Past 12 Months	0.08	0.08	0.09	0.07
	(0.002)	(0.02)	(0.02)	(0.02)
Involvement with Law Enforcement Agencies in Past 12 Months	0.14	0.13	0.14	0.18
	(0.002)	(0.02)	(0.02)	(0.04)
Received Emergency Cash Assistance in Past 12 Months	0.19	0.16	0.18	0.18
	(0.002)	(0.02)	(0.02)	(0.03)
Any Interaction with Homeless Support System in Past 12 Months	0.35	0.27	0.42	0.45
	(0.01)	(0.02)	(0.03)	(0.05)
Any Housing Assistance Recieved in Past 12 Months	0.28	0.23	0.34	0.27
	(0.01)	(0.02)	(0.03)	(0.04)

Note: The table shows summary statistics for compliers, always takers, and never takers of housing assistance within my estimation sample. Standard errors are computed using 100 clustered bootstrap replications.

Table IV. The Effect of Housing Assistance on Future Homelessness.

Dependent Variable:	**Pr(Ever Returned to Homeless System)**			**Number of Returns**
Time Period:	Months 1-9 after Assessment (1)	Months 10-18 after Assessment (2)	Months 1-18 after Assessment (3)	Months 1-18 after Assessment (4)
OLS: Housing Assistance	0.228***	0.0867***	0.243***	0.524***
No Controls	(0.0124)	(0.00902)	(0.0150)	(0.0322)
OLS: Housing Assistance	0.245***	0.106***	0.270***	0.563***
All Controls	(0.0120)	(0.00892)	(0.0130)	(0.0383)
OLS: Housing Assistance	0.248***	0.106***	0.274***	0.566***
Complier Re-weighted	(0.0122)	(0.00895)	(0.0132)	(0.0388)
RF: Housing Placement Rate	-0.108***	-0.131***	-0.133***	-0.361***
All Controls	(0.0325)	(0.0266)	(0.0336)	(0.0712)
2SLS: Housing Assistance	-0.168***	-0.204***	-0.206***	-0.560***
All Controls	(0.0543)	(0.0441)	(0.0564)	(0.125)
Dependent Mean	0.28	0.18	0.36	0.64
Complier Mean if No Housing Assistance	0.35	0.18	0.38	0.72
Number of Assessments	26,752	26,752	26,752	26,752

Note: All specifications include site x month of assessment FEs and all the controls listed in Table II. Standard errors are two-way clustered at the case worker and individual level. *p<0.1, **p<0.05, ***p<0.01.

Table V. IV Model with Three Treatment Options: 'Permanent Housing', 'Temporary Housing', and 'No Housing Treatment'.

	First Stages		Reduced Form		IV
	(1)	(2)	(3)		(4)
	Outcome: **Pr(Permanent Housing Placement)**	Outcome: **Pr(Temporary Housing Placement)**	Months 1-18 after Assessment **Pr(Returned to Homeless System)**		Months 1-18 after Assessment **Pr(Returned to Homeless System)**
A. Baseline Specification					
Instrument:				Outcome:	
Housing Placement Rate	0.644***		-0.133***	Housing Assistance	-0.206***
	(0.0377)		(0.0336)		(0.0564)
F-stat (Instrument)	292.22				
Dependent Mean	0.5449		0.3623		0.3623
B. Multiple Treatments Specification					
Instruments:				Outcomes:	
Permanent Housing Placement Rate	0.697***	-0.0338	-0.217***	Permanent Housing	-0.313***
	(0.0382)	(0.0313)	(0.0370)		(0.0547)
Temporary Housing Placement Rate	0.0119	0.605***	-0.0178	Temporary Housing	-0.0232
	(0.0244)	(0.0595)	(0.0380)		(0.0643)
SW F-stat (Instrument)	423.13	113.43			
Dependent Mean	0.1931	0.3518	0.3623		0.3623
Number of Assessments	26,752	26,752	26,752		26,752

Note: All specifications include service site x month of assessment FEs and all the controls listed in Table II. Standard errors are two-way clustered at the case worker and individual level. *p<0.1, **p<0.05, ***p<0.01.

Table VI. The Effect of Housing Assistance on Socioeconomic Outcomes - Main Findings.

	Health		
Dependent Variable (1-18 Months after Assessment):	**Any Emergency Department Visit**	**Any Mental Health Treatment**	**Any Substance Abuse Treatment**
	(1)	(2)	(3)
OLS: Housing Assistance	0.00159	-0.00539	0.00753
All Controls	(0.00619)	(0.00380)	(0.0116)
RF: Housing Placement Rate	-0.0323*	-0.0292**	-0.0723
All Controls	(0.0178)	(0.0136)	(0.0473)
2SLS: Housing Assistance	-0.0541*	-0.0460**	-0.134
All Controls	(0.0302)	(0.0218)	(0.0878)
Dependent Mean	0.06	0.03	0.04
Number of Assessments	11,339	15,510	5,314
	Criminal Activity		
Dependent Variable (1-18 Months after Assessment:	**Jail Bookings**	**Number of Crimes**	**Any Probation**
OLS: Housing Assistance	0.217*	0.0332	0.00329
All Controls	(0.111)	(0.0348)	(0.00362)
RF: Housing Placement Rate	-0.955**	-0.247**	-0.0230
All Controls	(0.389)	(0.115)	(0.0166)
2SLS: Housing Assistance	-1.507**	-0.389**	-0.0363
All Controls	(0.621)	(0.182)	(0.0261)
Dependent Mean	1.05	0.31	0.033
Number of Assessments	15,510	15,510	15,510
	Employment and Income (Any Report)		
Dependent Variable (1-18 Months after Assessment):	**Any Income**	**Employed**	**Social Benefits**
OLS: Housing Assistance	0.146***	0.0834***	0.130***
All Controls	(0.0109)	(0.00794)	(0.0107)
RF: Housing Placement Rate	0.162***	0.152***	0.0566
All Controls	(0.0366)	(0.0447)	(0.0397)
2SLS: Housing Assistance	0.264***	0.242***	0.0923
All Controls	(0.0609)	(0.0724)	(0.0646)
Dependent Mean	0.76	0.14	0.67
Number of Assessments	23,054	23,387	23,054

Note: All specifications include service site x month of assessment FEs and all the controls listed in Table II. Standard errors are two-way clustered at the case worker and individual level. *p<0.1, **p<0.05, ***p<0.01.

Table VII. The Costs and Benefits of Housing Assistance for the Homeless.

	Costs	Benefits (Savings) of Public Agencies Expenditures				
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable (Months 1-18 After Assessment):	Days Spent in Housing Programs	Overall	Future Returns to Homelessness	Health	Law Enforcement	Employment
A. Housing Assistance - All Types						
IV: Housing Assistance	10,366***	-8,044***	-2,102***	-2,796*	-1,724***	-1,146***
	(1,020)	(1,713)	(469.5)	(1,583)	(549.6)	(388.2)
Dependent mean	3,752	5,723	2,413	1,264	941	-138
Number of Assessments	26,752	10,305	26,752	11,339	15,510	23,054
B. Housing Assistance - By Type						
IV: Permanent Housing Assistance	12,402***	-8,053***	-2,885***	-2,085	-1,746***	-1,862***
	(831.6)	(1,642)	(420.6)	(1,753)	(552.9)	(340.0)
IV: Temporary Housing Assistance	5,095***	-4,757**	-557.6	-3,214*	-1,089*	353.7
	(654.7)	(2,048)	(452.4)	(1,742)	(573.5)	(250.9)
Dependent mean	3,752	5,723	2,413	1,263	941	-138
Number of Assessments	26,752	10,305	26,752	11,339	15,510	23,054

Note: Estimation sample and specification with all controls. Standard errors are two-way clustered at the case worker and individual level. Direct housing costs are set to $35 per day for temporary housing, $40 per day for rapid rehousing, and $50 per day for permanent supportive housing, according to the 2017 Los Angeles Housing Gap Analysis. Future returns costs are estimated based on an average housing cost of $4,000 per return, based on direct housing costs computed in column (1). Health costs are the sum of DHS and DMH costs. Law enforcement costs are the costs of jail days and probation months. Cost estimates are taken as described in the text. Net transfers are computed as the total cash transfers, computed as the difference between total income and wage, and taxes received are set at 15% of wages. Overall costs are the sum of columns 3-6. All costs and benefits are estimated for an 18-month period. *p<0.1, **p<0.05, ***p<0.01.