Housing First or Treatment First? Evidence from the VA's homelessness programs

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Abstract

Debates in homelessness policy often pit Housing First programs, in which subsidized housing is offered unconditionally and immediately, against Treatment First interventions, which combine short-term housing with treatment for issues like substance use. I study a national sample of nearly 300,000 unhoused, mentally ill Veterans who are potentially eligible for the VA's Housing First program (known as HUD-VASH or VA Supportive Housing) or for a Treatment First program (offered by a variety of local contractors). Using an instrumental variables design, I find that enrolling in Housing First reduces three-year mortality by 4.6 percentage points relative to a no-program counterfactual. In contrast, Treatment First has no long-term effects on health. Housing First is also cost-saving, as it causes individuals to substitute away from lengthy inpatient stays.

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The number of unhoused individuals in the US reached a record high of 770,000 at the end of 2024 (Porter 2024). The visible presence of homeless people – many seemingly dealing with mental health and/or substance abuse issues – is a source of vehement debate. Some advocates argue that homelessness is, in fact, a housing problem, with permanent housing solutions required to help those most in need. Others argue that those who are chronically unhoused have underlying mental health or substance use issues that must be addressed first in order to make progress in reducing homelessness.

In the 1990s, homelessness advocates developed the concept of Housing First, or that for those most in need, highly-subsidized permanent housing should be given unconditionally and immeditely. The philosophy relies on the idea that requiring pre-conditions to housing merely acts as a barrier to seeking care, and that it is hard to address underlying mental health issues without having a stable home first.

Housing First programs are pitted against more traditional programs that emphasize housing readiness. In these models, individuals generally enter transitional housing, or temporary communal housing in which substance use and mental health treatment are required, sometimes along with sobriety. After a stay in these transitional homes, individuals eventually are deemed ready to enter regular, generally unsubsidized housing. In this paper, I call transitional housing programs "Treatment First" for simplicity.

RCTs analyzing Housing First programs have consistently demonstrated increases in housing stability compared to outcomes for individuals not enrolled in the program (e.g.,Tsemberis and Eisenberg 2000, Tsemberis et al. 2004, and Kertesz et al. 2009). But is housing stability the appropriate goal for homelessness policy, or should homelessness programs focus on larger goals of improved health and general well-being? Advocates of Treatment First or Housing Readiness programs argue the latter, and point out that Housing First's effects on mental and physical health remain unclear (NASEM 2018). A related methodological concern is that Housing First studies typically compare a permanent housing subsidy to an ambiguous counterfactual, which may include a combination of entry to Treatment First programs and no housing intervention at all.

The Department of Veterans (VA) affairs is perhaps the ideal setting to shed light on this debate. Veterans have been historically overrepresented in the population of unhoused individuals, and eliminating Veteran homelessness specifically has been an important federal goal (Tsai and Rosenheck 2015). Since the 1990s, the VA, along with the Department of Housing and Urban Development (HUD), have run a joint Housing First program. Known as HUD-VASH (VA Supportive Housing), the program offers housing vouchers to chronically homeless Veterans in the same style as Section 8. Concurrently, the VA has a large transitional housing program – called the Grant and Per Diem (GPD) program – which contracts out third-party agencies, such as the Salvation Army, to run short-term, Treatment First-style housing, where Veterans can stay up to two years.

In this paper, I evaluate the effects of Housing First versus Treatment First using a national sample of nearly 300,000 unhoused Veterans with mental illness. Beginning with their initial intake assessment, I follow these Veterans for three years, tracking program participation and long-term health outcomes. To disentangle the effects of different program types, I explicitly define three treatments: 1) housing voucher (HUD-VASH); 2) transitional housing (GPD); and 3) no housing intervention.

Program enrollment in this setting is the result of a combination of factors: voucher availability, caseworker recommendations, and Veteran choice. Thus, to address the endogeneity of treatment assignment, I use an instrumental variables framework with two treatment options, relative to the counterfactual of no treatment (Kirkeboen et al. 2016, Heinesen et al. 2022, Mountjoy 2022). I consider arrival to the VA homelessness clinic to be the result of a random shock to housing stability. This allows me to exploit two quasi-random sources of variation based on clinic arrival time. First, for a given area and year, permanent housing vouchers are fixed according to HUD allocations. These vouchers are notoriously in short supply, allowing me to exploit the length of the voucher waiting list.

Specifically, I define my first instrument as the leave-out percent of individuals who receive a housing voucher in three months or less among those who arrive to the same clinic within a 60-day window of one's intake date. I show that this variable is strongly predictive of the probability a Veteran is assigned to HUD-VASH, while also satisfying a wide battery of orthogonality checks, conditional on time and location controls.

Secondly, when a Veteran arrives to the homelessness clinic, based on both staffing availability and patient needs, they may be assessed by a generalist who triages them into different programs, or they may be seen by a specialist who oversees a specific program they are interested in. In particular, GPD liaisons are in charge of managing referrals to transitional housing. Those immediately seen by a GPD liaison are far more likely to enter transitional housing, likely both due to social worker encouragement and accelerated entry into a particular housing unit. As patients are assigned to case-workers based on need, I do not use direct social worker assignment; instead, I exploit the availability of a transitional housing liaison at one's time of arrival as an instrument. Specifically, I define my second instrumental variable as an indicator for whether a transitional housing liaison performs any intake assessment in the week of one's arrival at the VA clinic. I show that that this simple indicator is highly predictive of assignment to a transitional housing program, while also satisfying a parallel set of orthogonality checks.

Using these two instrumental variables and a setup with two endogenous treatments, I find that Housing First decreases three-year mortality by 4.6 percentage points (s.e.=1.7) relative to neither program. In contrast, transitional housing has no statistically significant effect on mortality, with point estimates very close to 0. In fact, I can statistically rule out that Treatment First is as effective as Housing First at reducing mortality.

Prior research has shown that 2SLS estimates with multiple endogenous treatments rely on a no "essential heterogeneity" assumption, or treatment effect heterogeneity that is correlated with selection into treatment (Heckman et al. 2006, Kirkeboen et al. 2016, Heinesen et al. 2022). More specifically, to uncover interpretable and well-defined causal effects, complier groups induced into different treatment statuses cannot have heterogeneous average treatment effects for a particular treatment, ruling out models like selection-on-gains and other forms of heterogeneity that are correlated with behavior. I conduct some simple tests to check whether no essential heterogeneity is a good approximation in my setting. First, I show that complier types all have similar mean predicted mortality, implying that compliers who are induced into the voucher program have similar baseline risk as compliers who are induced into transitional housing. Further, I use overidentification tests to rule out treatment effect heterogeneity with respect to other important individual characteristics. Lastly, I show that heterogeneous treatment effects across locations are uncorrelated with the propensity to be treated, which is inconsistent with a Roy (1951) selection framework.

Why does Housing First have such a large effect on long term mortality, while Treatment First appears to have no impact? To shed some light on mechanisms, I analyze housing stability over time, engagement with VA homelessness and mental health services, and cause of death. First, as echoed by other research, I find that Housing First increases long term housing stability. In contrast, housing stability for transitional housing recipients starts high but declines over time. Secondly, I find that both programs massively increase engagement with VA social workers. These effects are permanent for the voucher program, and fading over time for transitional housing. Next, I evaluate engagement with mental health care. Following the program philosophy of Treatment First, I find that transitional housing increases engagement with mental health care in the short term; however, effects fade to 0 as Veterans exit the program. In contrast, I find that Housing First has no effect on mental health care utilization, ruling out that mortality effects come from mental health treatment. Lastly, I show that voucher recipients are much less likely to die from extreme heat or cold, violence, and infections caused by risky injection drug use behavior, such as needle sharing. Overall, my results suggest that long term, stable housing improves health, with required mental health treatment not compensating for the effects of permanent housing.

Even if housing improves health, critics have argued that it may not be cost effective. In rent alone, HUD pays an estimated $581 per month for every HUD-VASH voucher (National Homeless Information Project 2018). However, I find that Housing First substantially reduces inpatient length of stays related to homelessness and mental health. Lower inpatient costs exceed program costs, making Housing First cost-saving in the long term.

This paper contributes to both the literature on Housing First, as well as HUD-VASH specifically. Prior randomized control trials and other retrospective studies have found that HUD-VASH leads to more stable housing, but with small sample sizes, researchers have generally found imprecise results for health outcomes (Rosenheck et al. 2003, Montgomery et al. 2013, Evans et al. 2019). Other Housing First studies for non-Veteran populations have found that offering permanent housing subsidies to the unhoused reduces inpatient stays, ED visits, and incarceration rates, but has little effect on employment (Basu et al. 2012, Stergiopoulos et al. 2015, Aubry et al. 2016, NASEM 2018, Evans et al. 2019, Brounstein and Wieselthier 2024, Cohen 2024). Using the largest sample to date to study the causal impact of Housing First, this paper is the first to show that housing reduces long-term mortality.

Additionally, this paper contributes to a scarce literature on transitional housing and other residential programs than emphasize mental health treatment. There is some limited quasi-experimental evidence than transitional housing improves living conditions and reduces inpatient length of stays even after program exit, but no evidence that it improves long term substance use outcomes (Siskind et al. 2013). Expanding beyond unhoused individuals to those with substance use disorders more broadly, residential rehabilitation programs also resemble Treatment First interventions in that they bundle short term housing and behavioral therapy. However, there is also limited empirical evidence that rehab has long-lasting health effects (Reif et al. 2014).

Most notably, this paper contributes to the literature on Housing First versus Treatment First. Prior research typically compares Housing First interventions to "standard of care" treatment methods, which often includes a substantial number of individuals who receive no treatment at all. These studies make it difficult to clearly delineate the impact of a Housing First versus Treatment First intervention. A single RCT, the Family Options study, explicitly compares permanent housing subsidies to transitional housing (Gubits et al. 2018). Unfortunately, the authors can only measure intention to treat effects, and there are low rates of program take up, especially for transitional housing. The authors find that permanent housing again increases housing stability and financial wellbeing, but find no statistically significant impacts on health, which they measure using surveys. This study is the first to explicitly compare average treatment effects of Housing First versus Treatment First.

I. Setting

i. VA Homelessness Services

The Department of Veterans Affairs (VA) operates over 1,000 facilities across the US that provide integrated healthcare and homelessness services to US Veterans. Veterans in need of housing services can drop by any VA medical center or Community Resource and Referral Center – outpatient clinics targeted towards unhoused Veterans – to be evaluated for various homelessness interventions, including housing and case management. The VA fully centralized their intake and referral process in April 2011 via the Homeless Operations Management and Evaluation System (HOMES).

Every Veteran who contacts homelessness services is given an initial intake assessment, from which they are referred to programs meeting their needs. Veterans with lower needs or those who are merely at risk of homelessness may be offered interventions like case management or temporary rental assistance. Chronically unhoused and mentally ill Veterans are typically referred to longer-term housing interventions. The two largest programs serving this latter group of Veterans are HUD-VASH and the Grant & Per Diem program. Veterans are assigned to particular programs based on both social worker recommendation, Veteran choice, and program availability. Additionally, Veterans who are eligible for VA healthcare – which excludes those with a dishonorable discharge – can be referred to VA primary care, behavioral health, or other relevant healthcare services.

ii. Housing First: HUD-VASH

HUD-VASH is a national program that distributes vouchers to provide permanent housing to formerly unhoused Veterans, with around 100,000 vouchers currently in use. The program began in 1992 and was massively expanded by the Obama administration, with a goal of ending Veteran homelessness. The program functions similarly to the Department of Housing and Urban Development (HUD)'s Section 8 program (Chetty et al. 2016). Those admitted to HUD-VASH receive a housing voucher and are able to choose their own apartment, subject to landlord approval. The voucher pays all or the majority of rent; Veterans are only responsible for contributing 30% of their income, which in many cases is de minimus.[1]

The budget and allocation of HUD-VASH vouchers is decided at the federal level each year. Vouchers are distributed across cities to local public housing agencies (PHAs), who partner with the VA. To qualify for the program, Veterans must be currently unhoused, with vouchers distributed to those most in need if they are in short supply. More specifically, the VA guidelines prioritize those who are chronically homeless – defined as those who have a mental or physical disability and have been unhoused for over a year or experienced multiple bouts of homelessness. Due to low voucher availability, wait times for the program are high. The median Veteran referred to HUD-

[1] Some Veterans with a service-connected disability, like PTSD, receive disability payments from the VA, which count towards the 30% income payment.

VASH typically waits three months to receive a voucher. While enrolled in the program, Veterans must also agree to case management services, which can vary in frequency. Veterans can keep their vouchers indefinitely, and voucher receipt is not contingent on sobriety or receipt of any mental health services. In some cases, individuals may lose their voucher if they are evicted from the apartment due to "serious lease violations," such as excessive property damage or illegal activity. Nevertheless, VA guidelines recommend that PHAs only consider voucher termination as a last resort (National Housing Law Project 2008).

iii. Treatment First: Grant & Per Diem

The VA's Grant & Per Diem Program (GPD) is the VA's largest transitional housing program, with over 12,000 beds.[2] The program awards grants to community partners, like the Salvation Army, to provide temporary communal housing and supportive services to Veterans, notably mental health and substance use treatment. Transitional housing facilities offer wrap around support services like case management, counseling, and addiction treatment, with a goal of making clients "ready" for permanent housing.

Given that third parties run transitional housing facilities, there is considerable variation across locations – with some housing requiring sobriety in order to remain in the program. While all programs have case workers, only some have licensed therapists on site; other sites may refer individuals to VA clinics for mental health treatment. Veterans who enter GPD can stay up to two years, after which the program helps find them a more permanent housing solution, whether independent or subsidized. GPD targets a similar group of clients as HUD-VASH. Veterans may choose to enroll in transitional housing rather than HUD-VASH due to client preference, social worker recommendation, or low housing voucher availability.

[2]While HUD-VASH vouchers can be permanent, GPD beds have high turnover, which is why bed numbers are much smaller despite high participation in my sample.

II. Data

Data on homelessness program participation comes from the VA's Homelessness Registry, which combines data from HOMES, the Homeless Management Information system (HMIS), and the Corporate Data Warehouse (CDW) to comprehensively document participation in the VA's homelessness programs, including entry and discharge dates. Relevant for my analysis, the registry also tracks initial intake assessments, as well as the primary affiliation of the social worker who completed one's assessment.

I link these data to full clinical records in CDW, the VA's detailed health record system, matching on social security number. CDW contains detailed patient demographic data, as well as documentation of all inpatient and outpatient encounters occurring at any VA hospital or clinic. Moreover, the VA has reliable mortality data, combining data from the Veterans Benefits Administration (VBA), Centers for Medicare and Medicaid Services (CMS), and Social Security Administration (SSA). Validation studies suggest the sensitivity of the available indicator of death is 98% (i.e., the fraction of true deaths that are accurately recorded is 98%). Matching on Social Security Numbers, I also link patient data from the VA with cause of death data, coded by ICD-10 code, from the National Death Index.

Finally, via CDW, I am able to access full social worker and clinical notes for every Veteran that meets with a VA-affiliated provider. To ascertain housing status over time, I process these clinical notes using the Natural Language Processing algorithm ReHouSED, which categorizes each note into "stably housed," "unstably housed," and "unknown" (Chapman et al. 2021). While use of Large Language Models is not authorized for the VA's clinical notes system, this rule-based NLP algorithm far outperforms methods like keyword mentions by understanding sentence context. In particular, the algorithm notes when mentions of housing are historical, hypothetical, or negated.[3] I provide more

[3]Compared to a human reading the notes, ReHouSED has a precision rate ($\frac{\text{True positives}}{\text{True positives+False positives}}$) of 65.3% and a

details in Section IV.vii.

III. Empirical Approach

i. Sample construction

The VA fully centralized their homelessness intake and referral process in April 2011. Veterans who contact homelessness services complete an initial assessment, after which they are referred to various homelessness services, including permanent and transitional housing. Thus, the sample consists of all individuals who had an initial intake assessment with VA homelessness services between April 2011 and February 2017, allowing me to follow all individuals for three years prior to the start of the COVID-19 pandemic. For Veterans with multiple intake assessments during this period, I use the date of their first assessment. To track Veterans' health outcomes in CDW, I restrict the sample to Veterans also enrolled in VA healthcare. Finally, as both programs target unhoused Veterans with mental illness, I restrict the sample to those with a documented behavioral health and/or substance use disorder. More specifically, the Veteran must have had some healthcare encounter in the prior year in which the provider noted that the patient had a mental health condition, as documented via ICD-9 or ICD-10 codes.

The final sample consists of 289,932 unhoused Veterans with mental illness. As shown in Table 1, the sample is 90% male, 36% Black, 7% Hispanic, and has an average age of 50. Comparing these demographic characteristics with the general US population of unhoused individuals (HUD 2024), my sample has a much higher percentage of men, a lower percentage of Hispanic individuals, and excludes homeless children (Appendix Table A1).

Many Veterans in my sample struggle with drug or alcohol use, with around 54% suffering from a substance use issue. Moreover, around 62% of the sample suffers from a chronic physical condition,

recall rate ($\frac{\text{True positives}}{\text{True positives+False negatives}}$) of 65.8% (Chapman et al. 2021).

such as diabetes or heart disease. About 12% of the sample receives a HUD-VASH voucher within three months of intake, and about 18% of the sample enrolls in transitional housing. Characteristics of those enrolling in a housing intervention look similar to mean characteristics, with some differences. Notably, transitional housing recipients are more likely to suffer from substance use and more likely to have received mental health inpatient care, suggesting that transitional housing may be targeted towards those particularly interested in addiction treatment.

ii. Instrumental variables design

I analyze the effects of HUD-VASH and GPD through the lens of a potential outcomes model with three mutually exclusive and exhausive treatment categories for individual i: $D_i \in \{v, t, 0\}$. Let $D_i(z_v, z_t)$ indicate potential treatments if the instruments take on values (z_v, z_t), D_i indicate realized treatment, $Y_{k,i}$ describe potential outcomes given treatment k, and Y_i describe realized outcomes. I define v as receiving a HUD-VASH voucher within three months of one's intake assessment, t as entering transitional housing within three months, and 0 as entering neither program. In the small share of cases where a Veteran enrolls in both HUD-VASH and transitional housing within three months, I define their treatment status based on the program they enroll in first. Veterans assigned one treatment status may eventually enter a different program. For instance, those who initially receive no housing intervention could enter a transitional housing program a year later. However, for simplicity, I define treatment here based on the initial program entered following one's intake assessment.[4]

Those assigned 0 may receive a non-housing intervention like case management services. In rare cases – less than 3% of observations in the sample – Veterans may also receive rapid rehousing or temporary rental assistance services via another VA program, Supportive Services for Veteran Families (SSVF). Given that SSVF is given to a small percentage of the sample and is targeted towards

[4] I consider switching across programs in Section IV.vi.

those with lower needs, I ignore this alternative treatment in the baseline analysis. However, in robustness checks, I drop individuals who immediately enroll in SSVF. Overall, one can think of v, t, and 0 as representing Housing First, Treatment First, and no housing intervention, respectively.

Initial treatment status is clearly non-random in this setting. Veterans may enroll in a particular program as the result of a combination of factors, including voucher availability, caseworker recommendations, and Veteran choice. Thus, to address the endogeneity of treatment assignment, I use an instrumental variables framework. Importantly, in contrast to settings with a single intervention, this setting requires a second instrument to separately identify treatment effects for Housing First and Treatment First margins.

I consider time of arrival to the VA homelessness clinic to be as good as random. Veterans likely seek out homelessness services after a shock to their housing stability, the timing of which may be difficult to predict. This allows me to exploit two quasi-random sources of variation based on clinic arrival time. First, for a given area and year, permanent housing vouchers are fixed according to HUD allocations. These vouchers are coveted and in short supply, often resulting in long wait times to enroll in the program. While one's exact wait time may be correlated with need, voucher availability at the time of clinic arrival may vary for quasi-random reasons. Thus, as my first instrument, I exploit a proxy for the length of the voucher waiting list. In particular, let $Z_{v,i}$ be the (leave-out) percent of individuals who receive a housing voucher in three months or less among those who arrive to the same clinic within a 60-day window of one's intake date.

Secondly, when a Veteran arrives to the homelessness clinic, based on both staffing availability and patient needs, they may be assessed by a generalist who triages them into different programs, or they may be seen by a specialist who oversees a specific program. In particular, GPD liaisons are in charge of managing referrals to transitional housing. If first seen by a generalist, individuals must be referred to the liaison in order to be admitted to a particular transition housing unit. Those immediately seen by a GPD liaison essentially bypass this step, resulting in accelerated entry into the program. Moreover, liaisons likely have wider knowledge of transitional housing availability and may encourage Veterans to enter a particular program. As patients are assigned to caseworkers based on need, I exploit the availability of a transitional housing liaison at one's time of arrival as an instrument. Specifically, let $Z_{t,i} = 1$ if a transitional housing liaison performs any intake assessment in the week one arrives to the clinic. More explicitly, I define the two instruments used in my setting below:

$$Z_{v,i} = \frac{1}{|I_{j(i)}| - 1} \sum_{i' \in I_{j(i)}} \mathbf{1}(i' \neq i) D_{v,i'} \tag{1}$$

$$Z_{t,i} = \mathbf{1}(\text{Tr. liasion available}_{s(i)w(i)}) \tag{2}$$

where $s(i)$ and $w(i)$ refers to the individual's VA station and week of arrival, respectively, $I_{j(i)}$ is the set of patients who arrive within a 60-day time window of one's intake date, and $D_{v,i} = \mathbf{1}(D_i = v)$. Across all regressions, I control for VA station × year and VA station × month, controlling for any non-random variation in patient arrival, voucher availability, and staffing that varies across clinic-years or due to seasonality. Additionally, in some cases, individuals may be referred to homelessness services from an inpatient service related to mental health and/or homelessness, particularly the VA's long-term residential treatment programs for substance use. In these cases, patient arrival time may less random, as VA social workers may coordinate with inpatient staff to manage the patient's discharge plan. Thus, in the baseline analysis, I control for whether the patient had an inpatient stay in the prior year related to mental health or homelessness. In robustness checks, I also drop these patients.

Finally, in addition to standard IV assumptions, prior research has shown that 2SLS with multiple endogenous treatments rely on a no essential heterogeneity assumption (Kirkeboen et al. 2016, Heinesen et al. 2022). In other words, to uncover well-defined treatment effects, complier groups induced by $Z_{v,i}$ and $Z_{t,i}$ into different treatment statuses must have the same average treatment effects. For example, 2SLS estimands may not be interpretable as causal effects if compliers induced into Housing First have a larger treatment effect from voucher receipt compared to other complier types. In Section IV.iv, I discuss this assumption in more detail, as well as argue that no essential heterogeneity is likely a reasonable model of treatment effects in my setting.

iii. Understanding complier types

In order to assess the no essential heterogeneity condition, it is useful to define complier groups in this setting. To do so, I assume treatment assignment adheres to a simple choice model, which follows from program context. Individuals choose between a voucher, transitional housing, and neither program based on a combination of offers and preferences. Based on both program availability and eligibility, social workers may give individuals an offer to enroll in a specific program. After an individual is given offers, they can decide among programs according to their preferences.

Higher voucher availability and transition liaison availability increase the probability of receiving a voucher or transitional housing offer, respectively. Let $O_{v,i}(Z_{v,i}) \in \{0,1\}$ be an indicator for receiving a voucher offer, where $O_{v,i}$ is weakly increasing with $Z_{v,i}$. Similarly, let $O_{t,i}(Z_{t,i}) \in \{0,1\}$ be an indicator for receiving a transitional housing offer, where $O_{t,i}(1) \geq O_{t,i}(0)$. An individual has the following choice set:

$$\mathcal{F}_i(Z_{v,i}, Z_{t,i}) = \begin{cases} \{0\} & \text{if } (O_{v,i}(Z_{v,i}), O_{t,i}(Z_{t,i})) = (0,0) \\ \{0, v\} & \text{if } (O_{v,i}(Z_{v,i}), O_{t,i}(Z_{t,i})) = (1,0) \\ \{0, t\} & \text{if } (O_{v,i}(Z_{v,i}), O_{t,i}(Z_{t,i})) = (0,1) \\ \{0, v, t\} & \text{if } (O_{v,i}(Z_{v,i}), O_{t,i}(Z_{t,i})) = (1,1) \end{cases}$$

After receiving offers, an individual can decide among programs according to his or her preferences. I assume an individual derives utility $U_{0,i}$ from enrolling in no program, $U_{v,i}$ from the voucher program, and $U_{t,i}(Z_{t,i})$ from transitional housing, where $U_{t,i}(1) \geq U_{t,i}(0)$. Notably, I assume higher voucher availability only enters treatment choice by increasing the probability of receiving a voucher offer. Alternatively, transitional liaison availability may affect both offers and preferences for transitional housing, as the transitional liaison may encourage Veterans to enter the program. I assume an individual chooses a program based on their set of offers as follows:

$$D_i(Z_{v,i}, Z_{t,i}) = \arg\max{}_{k \in \mathcal{F}_i(Z_{v,i}, Z_{t,i})} U_{k,i}(Z_{t,i})$$

Following this choice framework, $Z_{v,i}$ induces individuals into the voucher program from a counterfactual of either transitional housing or no housing intervention. Similarly, $Z_{t,i}$ induces individuals into transitional housing from a counterfactual of either the voucher program or no housing intervention. Figure 1 shows a schematic of how $Z_{v,i}$ and $Z_{t,i}$ induce individuals into different treatment statuses. This specific model of treatment choice is not needed to uncover causal effects (Heinesen et al. 2022). However, it allows me to identify concrete complier types based on behavior and will later allow me to identify complier characteristics, which will be useful in assessing the no essential heterogeneity condition (Mountjoy 2022, Kirkeboen et al. 2016; see Section IV.iv.).

Individuals may choose a treatment state based on both the value of $Z_{v,i}$ and $Z_{t,i}$ they receive. For example, someone who prefers vouchers over transitional housing may choose a voucher if they are offered both programs, but transitional housing if they are not offered a voucher. To gain intuition, consider binary versions of $(Z_{v,i}, Z_{t,i}) \in \{0,1\}^2$, where $Z_{v,i} = 1$ indicates high voucher availability. Let $\mathbf{D}_i(z_v, z_t) = (D_i(0,0), D_i(1,0), D_i(0,1), D_i(1,1))$ describe potential treatments when $(z_v, z_t) \in \{0,1\}^2$. In Table 2, I enumerate potential complier groups consistent with this choice framework and potential behavioral responses to $Z_{v,i}$ and $Z_{t,i}$. In principle there are seven such groups, depending on an individual's potential treatment status when facing each of the four possible values of the combination of $Z_{t,i}$ and discretized $Z_{v,i}$. These include four single-instrument compliers who only respond to one instrument (C_0^v, C_0^t, C_v^t and C_t^v) and three joint-instrument compliers whose decisions depend on the joint realization of both instruments ($C_0^{v+,t-}, C_0^{v-,t+}$, and C_t^{v-}).

Given these complier groups, propensity scores can be used to identify complier share mixtures. For instance, $-(\Pr[D_{t,i} = 1|\ Z_{v,i} = 1, Z_{t,i} = 0] - \Pr[D_{t,i} = 1|Z_{v,i} = 0, Z_{t,i} = 0])$ gives the share of individuals who are induced from t to v by $Z_{v,i}$, fixing $Z_{t,i}$ at 0. This group corresponds to the combined share of C_t^v and C_t^{v-} compliers, according to behavioral responses in Table 2. Let $P(c)$ be the population share of c compliers. In Appendix Table A2, I show how propensity scores map to complier groups, and in Appendix Figure A2, I provide more intuition.

Observed propensity score differences generally capture mixtures of complier types, since changes in $Z_{v,i}$ and $Z_{t,i}$ can simultaneously move different complier groups across multiple treatment boundaries. However, if changes in treatment shares are additive with respect to the two instruments, complier shares can be point identified. Specifically, define instrument additivity as:

$$\begin{aligned} &\Pr[D_{k,i} = 1 \mid Z_v = 1, Z_t = 1] - \Pr[D_{k,i} = 1 \mid Z_v = 1, Z_t = 0] \\ &= \Pr[D_{k,i} = 1 \mid Z_v = 0, Z_t = 1] - \Pr[D_{k,i} = 1 \mid Z_v = 0, Z_t = 0] \end{aligned} \tag{3}$$

In words, this condition says the change in voucher and transitional housing enrollment when $Z_{v,i}$ and $Z_{t,i}$ increase equals the sum of their separate effects.[5] Given my monotonic choice structure, where each instrument only increases the probability of enrolling in a particular treatment, this condition implies that the two instruments shift disjoint complier groups. Joint-instrument compliers therefore have zero probability, allowing the remaining complier groups to be point identified. Formally, I show that the following holds:

[5]This condition can be re-written as $\Pr[D_{k,i} = 1|Z_v = 1, Z_t = 1] - \Pr[D_{k,i} = 1|Z_v = 0, Z_t = 0] = (\Pr[D_{k,i} = 1|Z_v = 1, Z_t = 0] - \Pr[D_{k,i} = 1|Z_v = 0, Z_t = 0]) + (\Pr[D_{k,i} = 1|Z_v = 0, Z_t = 1] - \Pr[D_{k,i} = 1|Z_v = 0, Z_t = 0])$, or the change in the share in k from $(z_v, z_t) = (0,0)$ to (1,1) is equal to change from (0,0) to (1,0) plus the change from (0,0) to (0,1).

Proposition 1 (Complier shares). *Under the treatment choice model described above, if* $\Pr[D_{k,i} = 1|Z_v = 1, Z_t = 1] - \Pr[D_{k,i} = 1|Z_v = 1, Z_t = 0] = \Pr[D_{k,i} = 1|Z_v = 0, Z_t = 1] - \Pr[D_{k,i} = 1|Z_v = 0, Z_t = 0]$ $\forall k$*, then:*

$$P(C_0^{v+,t-}) = P(C_0^{v-,t+}) = P(C_t^{v-}) = 0$$

The remaining complier shares can be point identified as follows:

$$P(C_0^v) = -(\Pr[D_0 = 1|Z_v = 1, Z_t = 0] - \Pr[D_0 = 1|Z_v = 0, Z_t = 0])$$

$$P(C_0^t) = -(\Pr[D_0 = 1|Z_v = 0, Z_t = 1] - \Pr[D_0 = 1|Z_v = 0, Z_t = 0])$$

$$P(C_t^v) = -(\Pr[D_t = 1|Z_v = 1, Z_t = 0] - \Pr[D_t = 1|Z_v = 0, Z_t = 0])$$

$$P(C_v^t) = -(\Pr[D_v = 1|Z_v = 0, Z_t = 1] - \Pr[D_v = 1|Z_v = 0, Z_t = 0])$$

Proof. See Appendix Section A1.2. □

Under this additivity condition, the shares of C_0^v, C_0^t, C_t^v, and C_v^t are fully identified. Conceptually, this is because only a single group is induced from treatment status k by $Z_{j,i}$: C_0^v compliers are the only group induced by $Z_{v,i}$ from 0, C_0^t compliers are the only group induced by $Z_{t,i}$ from 0, C_t^v compliers are the only group induced by $Z_{v,i}$ from t, and C_v^t compliers are the only group induced by $Z_{t,i}$ from v.

This additivity condition corresponds empirically to the absence of an interaction between $Z_{v,i}$ and $Z_{t,i}$ in the first stage.[6] To test this empirically, I estimate the following regressions:

$$D_{k,i} = \alpha_{v,k} Z_{v,i} + \alpha_{t,k} Z_{t,i} + \alpha_{vt,k} Z_{v,i} Z_{t,i} + \zeta_{k,s(i)y(i)} + \phi_{k,s(i)m(i)} + X_i \beta_k + \epsilon_{k,i} \quad (4)$$

[6] See Appendix Table A2 for a full mapping of first stage coefficients to complier shares.

where $D_{k,i}$ is an indicator for enrolling in treatment k, $\zeta_{k,h(i)y(i)}$ are VA station × year fixed effects, $\phi_{k,h(i)m(i)}$ are VA station × month fixed effects, and X_i are the baseline controls, which includes whether the patient had a prior inpatient stay related to homelessness or mental health. Across all treatments $D_{k,i}$, I find that the interaction coefficient, $\alpha_{vt,k}$, is close to 0 and statistically insignificant.[7] According to the proposition above, this implies that joint-compliers have 0 share, and the shares of C_0^v, C_0^t, C_v^t and C_t^v are fully identified from first stage regressions. For complier shares estimates, see Appendix Section A1.2. I will return to these complier types when I describe and provide evidence for the no essential heterogeneity assumption in Section IV.iv.

IV. Results

i. First stage: how do $Z_{v,i}$ and $Z_{t,i,}$ impact treatment status?

First, I evalute the relevance of the instruments, or whether $Z_{v,i}$ and $Z_{t,i}$ actually shift people across treatments $0, v$, and t. Returning to Eq. 4, in Table 3, I report coefficients $\alpha_{v,k}$ and $\alpha_{t,k}$ when $D_{k,i} \in \{D_{v,i}, D_{t,i}, D_{0,i}\}$, or indicators for enrolling in the voucher program, transitional housing, and neither, respectively.[8] I find that $Z_{v,i}$ and $Z_{t,i}$ strongly predict enrollment in the voucher program and transitional housing, respectively (t-stat.= 30, 44). Specifically, a 10 percentage point increase in voucher availability increases the probability of enrolling in the voucher program by about 5.1 percentage points, and transitional housing liaison availability increases the probability of enrolling in transitional housing by about 8.9 percentage points.

Given that treatments are mutually exclusive and exhaustive, it follows that $Z_{v,i}$ and $Z_{t,i}$ decrease the probability of enrolling in other treatments. In particular, $Z_{v,i}$ decreases the probability that

[7] These interaction terms are close to 0 and statistically insignificant whether $Z_{v,i}$ is used in its continuous version or converted into a binary indicator (for high versus low values of the leave-out share assigned a HUD-VASH voucher).

[8] Given the findings reported in the previous section, I exclude the $Z_{v,i}Z_{t,i}$ interaction term from first stage and 2SLS regressions in the main text. As noted, all the interaction coefficients are close to 0 and insignificant.

$D_i = \{t, 0\}$, and $Z_{t,i}$ decreases the probability that $D_i = \{v, 0\}$. Furthermore, $Z_{v,i}$ and $Z_{t,i}$ both cause much larger decreases in the probability of receiving no intervention than the probability of switching between interventions. In other words, while there is still significant switching between treatment statuses t and v, a much larger share of compliers would have received no intervention under the counterfactual than another housing intervention. For precise estimated complier shares, see Appendix Section A1.2. Finally, in column 4, I show reduced form estimates for the effect of $Z_{v,i}$ and $Z_{t,i}$ on three-year mortality. I combine these first stage and reduced form estimates to obtain 2SLS coefficients in Section IV.iii.

ii. Balance checks: are $Z_{v,i}$ and $Z_{t,i,}$ correlated with individual characteristics?

In order for $Z_{v,i}$ and $Z_{t,i}$ to be valid instruments, they must be as good as randomly assigned, conditional on the baseline controls. Notably, certain VA stations and time periods may systematically have longer wait times. In particular, HUD adjusts voucher allocations to particular areas yearly based on homelessness counts and unmet Veteran need. Therefore, I control for any endogeneity in wait time using detailed VA station × year and VA station × month fixed effects. The remaining variation in residualized $Z_{v,i}$ represents random shocks to voucher availability not explained by yearly HUD allocations or seasonality effects.

Secondly, when an individual arrives to a VA medical or referral center for homelessness intake, they may be referred to a particular social worker based on their needs and/or preferences. For instance, those particularly interested in transitional housing may immediately meet with a transitional housing liaison upon arrival. In order to adjust for any non-random assignment to particular social worker types, in constructing $Z_{t,i}$, I instead rely on caseworker $availability$. Additionally, as with $Z_{v,i}$, the detailed VA station × time controls account for any potential correlation between patient demographics at particular stations and staffing.

To directly test whether $Z_{v,i}$ and $Z_{t,i}$ appear to be as good as random, conditional on the baseline controls, I run the following set of regressions:

$$C_i = \theta_v Z_{v,i} + \theta_t Z_{t,i} + \zeta_{0,s(i)y(i)} + \phi_{0,s(i)m(i)} + X_i\beta_0 + \epsilon_{0,i} \tag{5}$$

where C_i are relevant Veteran characteristics. In particular, if a given characteristic C_i is predictive of mortality but uncorrelated with $Z_{v,i}$ and $Z_{t,i}$, this suggests that both high and low risk Veterans are equally likely to be assigned high and low values of $Z_{v,i}$ and $Z_{t,i}$, indicating quasi-random assignment. In Figure 2, I show results from Eq. 5 using a wide-range of characteristics, including key demographics such as age, race, and gender; mental health conditions including depression, pychoses, substance use, and alcohol use; and physical health comorbidities. Moreover, in order to create an aggregate measure of patient risk, I construct a predicted mortality for each patient, $\hat{Y}_i$, based on an OLS regression of three year mortality on all patient characteristics (Chan et al. 2023, Costantini 2025).[9] I standardize all characteristics C_i so that coefficients can be interpreted in terms of standard deviations.

I find that coefficients θ_v and θ_t are small and statistically insignificant for 33 of 35 individual C_is for both θ_v and θ_t. For the other two characteristics, coefficients are marginally statistically significant, consistent with this being due to random chance. Perhaps most importantly, for $Z_{v,i}$ and $Z_{t,i}$, the point estimates when $\hat{Y}_i$ is on the left hand side are almost exactly 0, and results are far from statistical significance. On the whole, these results provide compelling evidence that high risk patients are not systematically assigned to particular values of $Z_{v,i}$ or $Z_{t,i}$.

[9] For the mortality prediction, I include age in 4-year buckets as separate fixed effects.

iii. Mortality effects

To determine how participation in the voucher program and transitional housing impact three year mortality, I run the following 2SLS system:

$$D_{v,i} = \pi_v Z_{v,i} + \pi_t Z_{t,i} + \zeta_{1,s(i)y(i)} + \phi_{1,s(i)m(i)} + X_i\beta_1 + \epsilon_{1,i} \tag{6}$$

$$D_{t,i} = \gamma_v Z_{v,i} + \gamma_t Z_{t,i} + \zeta_{2,s(i)y(i)} + \phi_{2,s(i)m(i)} + X_i\beta_2 + \epsilon_{2,i} \tag{7}$$

$$Y_i = \beta_v D_{v,i} + \beta_t D_{t,i} + \zeta_{3,s(i)y(i)} + \phi_{3,s(i)m(i)} + X_i\beta_3 + \epsilon_{3,i} \tag{8}$$

where Eq. 6 and 7 are first stage regressions for the voucher program and transitional housing, respectively, and Eq. 8 is the structural equation for the outcome Y_i representing 3-year mortality, estimated by replacing $D_{v,i}$ and $D_{t,i}$ with their predicted values from the first stage models.

2SLS results imply that enrollment in the voucher program decreases mortality by 4.6 percentage points (s.e.=1.7), relative to a no-program counterfactual (Table 3, column 2). In contrast, the point estimate for the effect of transitional housing on mortality is close to 0. In fact, I can reject that the voucher program and transitional housing have the same effect on mortality at conventional significance levels (F = 4.06; p-value = 0.044). In Appendix Figure A3, I show the mortality effect over time for both programs. The effect of Housing First rises steadily over time, becoming statistically significant around year two. On the other hand, effects for transitional housing consistently hover near 0. On the whole, the voucher program appears to have a large and statistically significant impact on long term mortality, while transitional housing likely has no or a very small, statistically undetectable impact.

Comparing these estimates to the OLS results (Table 3, column 1), 2SLS estimates for β_v and β_t have the same sign but are of larger magnitude. Recall that OLS coefficients are a combination of the average treatment effect plus a selection bias term reflecting non-random enrollment in the voucher and transitional housing programs. Those for whom $D_{v,i} = 1$ are a combination of voucher always takers – or those who always enroll in the voucher program no matter what values of $Z_{v,i}$ and $Z_{t,i}$ they receive – and C_0^v, C_t^v, and C_v^t complier groups. Similarly, $D_{t,i} = 1$ are a combination of transitional housing always takers and C_0^t, C_t^v, and C_v^t complier groups. Finally, the excluded category, $D_{0,i} = 1$, are a combination of never takers – those who never enroll in either program – and C_0^t and C_0^v complier groups. In this setting, voucher and transitional housing always takers are likely higher risk individuals: both programs target chronically unhoused, mental ill Veterans, and higher need individuals in particular are given priority for HUD-VASH. Therefore, OLS results are consistent with the selection bias term attenuating the effect of the voucher program, as always takers likely have higher baseline mortality risk than never takers.[10]

In column 3, I show the IV results when I add all individual X_i control variables (the same variables shown in Figure 2). Results are nearly identical. Next, in the baseline analysis, I consider three treatments: voucher, transitional housing, or neither. Conceptually, 'neither' represents individuals who received no housing intervention. However, around 3% of individuals in my sample receive another light touch housing intervention: Supportive Services for Veteran Families (SSVF), which includes short term rental assistance and rapid rehousing programs. Individuals participating in SSVF are unlikely to be compliers in my setting, as this program is targeting towards Veterans with lower needs – in particular, those who are housed but are having trouble paying rent or were recently evicted. However, to ensure that SSVF receipt does not change the results, I drop these individuals in column 4. Finally, around 24.1% of my sample had an inpatient stay related to homelessness or

[10] In principle, OLS estimates could also reflect selection bias coming from different complier groups enrolling in v, t, and 0. However, I argue in Section IV.iv that these groups look observably similar to each other in terms of mortality risk.

mental health at a VA hospital in the prior year. Social workers often coordinate with VA inpatient staff if Veterans are identified as unhoused, leading to referrals to homelessness intake. In these cases, patient arrival time may less random. In the baseline analysis, I control for whether the patient had a prior inpatient stay. To further ensure that this issue does not bias my results, in column 5, I drop these individuals entirely. Dropping those with prior inpatient stays make the effects of the voucher program look even larger.

iv. Evaluating no essential heterogeneity

Prior research has shown that in order to recover well-defined causal effects, 2SLS with multiple endogenous treatments relies on restrictions on substitution patterns, a no essential heterogeneity assumption, or some combination of both (Kirkeboen et al. 2016, Kline and Walters 2016, Heinesen et al. 2022, Bhuller and Sigstad 2024, Humphries et al. 2025). According to the choice framework described in Section III.iii, I assume that $Z_{v,i}$ only affects treatment choice by increasing the probability that individuals enroll in v(from a counterfactual of either 0 or t), and $Z_{t,i}$ only affects treatment choice by increasing the probability that individuals enroll in t. Under these behavioral restrictions, Heinesen et. al (2022) show that to uncover interpretable and well-defined causal effects, there also must be no essential heterogeneity. In other words, complier groups induced by $Z_{v,i}$ and $Z_{t,i}$ into different treatment statuses also cannot have heterogeneous average treatment effects for a particular treatment. Formally,

$$E[Y_{k,i} - Y_{0,i} | C_i = c] = E[Y_{k,i} - Y_{0,i} | C_i = c'] \tag{9}$$

for all complier groups c and for treatment $k \in \{v, t\}$.[11] $Y_{k,i}$ represents potential mortality for individual i receiving treatment k. This model could be violated if those induced into v from 0 benefit more from vouchers than other complier types, for example. Eq. 8 rules out some, but not all forms of selection-on-gains. Among compliers, individuals cannot select into Housing First or Treatment First in way that is correlated with benefits. However, the condition does not impose restrictions on treatment effects for always takers and never takers, who may have different baseline mortality risk than compliers.

Intuitively, if different complier groups look very different from each other, this assumption is more likely to be violated. For example, in a comparison of no college, community college, and four-year college, Mountjoy (2022) finds that compliers on the margin between community college and no college have much lower prior test scores than those on the margin between community college and four year college, casting doubt on whether these different complier groups would experience similar gains from attending a four year college. However, in this setting, treatments are not "ordered;" instead, social workers assert that transitional housing and the voucher program both target the most at risk individuals.

To more formally evaluate the no essential heterogeneity assumption in my setting, I conduct three additional empirical tests. First, using additional instruments and overidentification tests, I evaluate whether one can reject homogenous treatment effects across key observable characteristics X_i. If additional instruments are added to the model, one can conduct a J-test to reveal whether different instruments yield different IV estimates, which may be suggestive of heterogeneous treatment effects. In particular, by interacting $Z_{v,i}$ and $Z_{t,i}$ with characteristics X_i, I can answer the following question: if compliers were only those that had $X_i = x$, would estimated mortality effects be the same? If the

[11] If there are additional restrictions on substitution patterns, no essential heterogeneity is not needed. In particular, if all compliers are induced from the same counterfactual treatment state, 2SLS has a causal interpretation even with unrestricted heterogeneity (Heinesen et al. 2022, Bhuller and Sigstad 2024).

J-test fails to reject homogenous effects across key observable characteristics, this would reassure the reader that there is no substantial treatment effect heterogeneity in general, mitigating concern over essential heterogeneity.

In Table 4, I show overidentification test results when I interact $Z_{v,i}$ and $Z_{t,i}$ with a given characteristic X_i and add these interactions as additional instruments to the 2SLS model. To ensure instrument validity, in these regressions, I also control for the given characteristic X_i interacted with the baseline station × time fixed effects. I include tests for demographic and mental health characteristics, as well as high predicted mortality. Across all regressions, I fail to reject homogenous treatment effects across observable characteristics.

As an additional test, I examine complier characteristics directly. If different complier groups have much higher baseline mortality risk $\hat{Y}_i$, on average, than other complier groups, this would cast doubt on the assumption that average treatment effects are equal across groups. In the former test, I failed to reject homogenous treatment effects for compliers that have high versus low predicted mortality (see Table 4). Nevertheless, to further reassure the reader that complier groups look similar to each other, I evaluate complier characteristics using a similar approach to that of Abadie (2003). For the case of binary instruments and no control variables, Mountjoy (2022) shows that the following Wald estimator yields mean $\hat{Y}_i$ for compliers who are induced by instrument $Z_{j,i}$ into treatment j from k, holding $Z_{j',i}$ fixed:

$$E[\hat{Y}_i|C_k^j] = \frac{E[\hat{Y}_i D_{k,i}|Z_{j,i}=1, Z_{j',i}] - E[\hat{Y}_i D_{k,i}|Z_{j,i}=0, Z_{j',i}]}{E[D_{k,i}|Z_{j,i}=1, Z_{j',i}] - E[D_{k,i}|Z_{j,i}=0, Z_{j',i}]} \tag{10}$$

This result follows from Proposition 1, where the denominator represents the negative of the complier share, $-P(C_k^j)$, and the numerator is $-E[\hat{Y}_i|C_k^j]P(C_k^j)$. Thus, dividing the two yields mean $\hat{Y}_i$ for C_k^j. Eq. 10 can be used to determine mean characteristics for C_0^v, C_0^t, C_t^v, and C_v^t compliers. In practice, I empirically identify $E[\hat{Y}_i|C_k^j]$ by running 2SLS regressions where $\hat{Y}_i D_{k,i}$ is on the left hand side and

I instrument for $D_{k,i}$ with $Z_{j,i}$.

In Figure 3, I show complier mean predicted mortality across all groups. I cannot reject that all groups have the same mean $\hat{Y}_i$. In other words, all complier groups have similar baseline mortality risk, on average, making it much more likely that they have the same average treatment effects.

Another way of thinking about the no essential heterogeneity assumption is that treatment effect heterogeneity must be unrelated to behavior. In other words, no essential heterogeneity does not rule out all forms of heterogeneity; instead, it says that compliers cannot select into a certain treatment in a way that is correlated with potential benefits. The most obvious model one may have in mind here is that of Roy selection. If those who particularly benefit from the voucher program are voucher compliers, no essential heterogeneity does not hold.

To test for evidence of selection-on-gains, I examine the relationship between estimated local average treatment effects and the propensity to be treated (via the first stage). More specifically, starting with the voucher program, I divide the sample into the VA's Veteran Integrated Service Networks (or VISNs), which are based on geographic area. I run first stage stage regressions separately by VISN for the effect of $Z_{v,i}$ on $D_{v,i}$. Then, I estimate the effect of the voucher program on mortality for each VISN (via 2SLS regressions). Finally, I plot the relationship between the first stage and 2SLS estimates. I repeat this process for transitional housing: by VISN, I estimate both the effect of $Z_{t,i}$ on $D_{t,i}$ and the LATE for the effect of transitional housing on mortality. I show results in Appendix Figure A4. Although 2SLS estimates are noisy due to small sample sizes, for both the voucher program and transitional housing, I find that VISN-specific LATEs are uncorrelated with the first stage. In other words, in areas where treatment effects are larger, individuals are not more likely to select into the program – ruling out Roy selection. On the whole, these results suggest that even if there is unobservable treatment effect heterogeneity, this heterogeneity is not correlated with the propensity to be treated.

Finally, what if there are small violations of no essential heterogeneity that are difficult to detect? To evaluate the degree of potential bias, I use a bounding approach. In particular, Heinesen et al. (2022) decompose the multi-instrument 2SLS coefficient into a LATE term and selection terms that involve the difference in average treatment effects across complier groups. The weight of these selection terms depend on the share of compliers who switch between treatments – in my setting, C_t^v and C_v^t compliers. Bhuller and Sigstad (2024) similarly argue that if these complier shares are 0, 2SLS recovers a well-defined LATE. Following this logic, if both the share of C_t^v and C_v^t compliers are small and there are only small violations to the no essential heterogeneity condition, 2SLS estimates will be close to true LATEs. In my setting, C_t^v and C_v^t compliers represent only 16.1% and 15.7% of all $Z_{v,i}$ and $Z_{t,i}$ compliers, respectively (see Appendix Table A3). More rigorously, I can precisely estimate bounds on the true LATE under the assumption that average treatment effects across complier groups differ by a factor of ϵ,or

$$|E[Y_{k,i} - Y_{0,i}|C_i = c] - E[Y_{k,i} - Y_{0,i}|C_i = c']| < \epsilon \tag{11}$$

In Appendix Section A2, I derive analytical worst-case scenario bounds on $LATE_v = E[Y_{v,i} - Y_{0,i}|C_0^v]$, the LATE of Housing First (compared to no intervention) for C_0^v compliers. The same logic can be applied to derive bounds for $LATE_t = E[Y_{t,i} - Y_{0,i}|C_0^t]$, the LATE of Treatment First for C_0^t compliers. In Table 6, I show bounds on both LATEs under different values of ϵ. I find that even with an ϵ as large as one percentage point, or around 20% of the size of the treatment effect, the bounds remain tight: $LATE_v \in (-0.049, -0.043)$ and $LATE_t \in (-0.010, -0.004)$. In fact, $LATE_v$ and $LATE_t$ have non-overlapping bounds as long as $\epsilon < 0.06$ p.p. These results suggest that β_v and β_t are only far from true LATEs if there are large violations to Eq. 9, which is likely implausible given the tests presented in this section.

v. Evaluating exclusion

Finally, in order for my instruments to be valid, they must influence mortality only by influencing treatment status. In other words, voucher wait time and transitional liaison availability can only have an impact on mortality by shifting individuals between treatments 0, t, and v. Most relevant for this analysis, $Z_{v,i}$ and $Z_{t,i}$ cannot affect mortality by changing how individuals interact with the homelessness system as a whole – for instance, by getting into care faster or increasing engagement in alternative programs, such as case management services.

In Table 7, I test various additional concerns related to exclusion. First, one concern is that homelessness wait times are correlated; in other words, if it takes longer to get a housing voucher, this could be because homelessness services are backed up in general. Therefore, in column 2, I add a control for the mean (leave-out) wait time (in days) to one's follow up appointment with a social worker (regardless of which homelessness program the patient enrolled in). IV results are very similar, suggesting that the results are not driven by more efficient homelessness services in general. Second, $Z_{t,i}$ relies on the availability of a transitional housing liaison, a specialty social worker in VA homelessness clinics. This brings up a related concern: if a homelessness referral center has a specialty staff member available, perhaps they are better staffed in general, which may improve the referral process on the whole. To mitigate this concern, in column 3, I control for the number of unique staff available at patient intake. Lastly, a final concern is that transitional housing liaisons may do more than refer patients to GPD. Importantly, the VA also has emergency housing and case management services. If transitional housing liaisons are more (or less) likely than other social workers to refer individuals to these programs, that could bias mortality results. Thus, in columns 4 and 5, I control for entering emergency housing and case management within three months of intake.[12] Finally, as discussed in Section IV.iii, in Table 3, I also drop individuals who enroll in an

[12]A related concern is that case workers may refer individuals to an inpatient service related to homelessness and/or mental health, notably the VA's mental health residential treatment programs and domiciliary care. About 6.5% of the

alternative housing program, SSVF. Overall, the results are highly robust across specifications.

vi. Switching between treatment statuses

Until now, I have considered a static model of treatment status: individuals are in v, t, or 0 based on the first program they enroll in within the first three months following intake. In practice, individuals can switch between treatment statuses: someone initially enrolled in neither program may receive a housing voucher at month six, or an individual in transitional housing might enroll in HUD-VASH once they exit their transitional housing unit.[13]

Descriptively, only 7.8% of those in 0 – who initially enroll in neither program – receive a housing voucher within a year following intake. In contrast, about 20.8% of those enrolled in transitional housing receive a housing voucher within a year. There are several possible explanations for why rates are much higher for those in tr. First, compared to the broader sample population, those who enroll transitional housing are likely more similar to HUD-VASH recipients, making them more likely to qualify for and enroll in the program. Second, individuals originally interested in HUD-VASH may enroll in transitional housing while they wait for their housing voucher to come through. Finally, this could partially be a treatment effect: part of the goal of transitional housing is to help you secure more permanent housing following program exit, which could include transitional housing social workers helping you sign up for HUD-VASH.

To ensure that my static model of treatment status does not impact the results, I run a series of robustness checks related to switching between treatment statuses in Appendix Table A4. First, I redefine $D_{v,i}$ as an indicator for *ever* receiving a housing voucher within a year of intake, even for those who initially enroll in transitional housing. This helps address two concerns: 1) that the voucher availability instrument may effect availability beyond three months, and 2) that some individuals may

sample enters one of these programs within three months of intake. I also control for referral to an inpatient service, and coefficients are nearly identical to the baseline specification.

[13]Since individuals can keep housing vouchers permanently, this is less of a concern for people who initially enroll in v.

only enroll in transitional housing for a short period while they await their voucher. In Appendix Table A5 column 1, I show the baseline first stage (with $D_{v,i}$ as the dependent variable) for reference. In column 2, I show the first stage when $D_{v,i}$ is re-defined as receiving a housing voucher within a year. Interestingly, the coefficient on $Z_{t,i}$ becomes insignificant in this model. In other words, for those who would have received Housing First under the counterfactual – or C_v^t compliers – transitional housing merely delays their entry into HUD-VASH.

In Table A4 column 3, I show the baseline 2SLS mortality results for reference. In column 4, I show 2SLS results when I replace $D_{v,i}$ with receiving a voucher within a year, which are nearly identical. If anything, in this specification, the effect of transitional housing becomes a more precise 0. Finally, in column 5, I simply drop individuals whose initial treatment status is 0 or *tr* but later enroll in the voucher program. Coefficients are also highly aligned with the baseline results.

vii. Mechanisms

Why does Housing First have such a large impact on long term mortality, but Treatment First has little effect? In this section, I evaluate potential explanations. First, previous research has found that offering unhoused individuals permanent, subsidized housing substantially reduces the number of days spent homeless compared to the status quo (Rosenheck et al. 2003, Montgomery et al. 2013). Thus, one potential explanation is that housing alone decreases mortality. Unsurprisingly, Meyer et al. (2023) find that the unhoused population experiences 3.5 times the mortality risk of those who are housed, controlling for demographic characteristics. Additionally, if individuals stay enrolled in Housing First, they may become more attached to the homelessness support system, especially compared to those enrolled in programs that are temporary in nature. Without monitoring by social workers and other staff, Veterans who disappear from the system may be particularly likely to experience poor outcomes. Finally, an alternative explanation, often espoused by Housing First advocates, is that housing stability is a necessary pre-condition for individuals to seek the care they need – particularly in addressing mental health and substance use. To shed light on this point, I evaluate Veterans' engagement with mental and physical healthcare over time.

Housing status over time

First, I evaluate Veterans' housing status over the three year period following program enrollment. Housing status is a particularly hard outcome to study in a non-RCT setting, as it is typically not recorded systematically in clinical records. Additionally, there are no records of housing status for those who disengage entirely from VA homelessness services, making full ascertainment difficult.

To partially address these issues, using the social worker and clinical notes available within CDW, I analyze Veteran notes using the Natural Language Processing (NLP) algorithm ReHouSED. Described and validated in detail by Chapman et. al (2021), this rule-based NLP algorithm classifies individual clinical notes into "stably housed," "unstably housed," and "unknown" categories. The algorithm outperforms methods like keyword matching by understanding sentence context – in particular, it identifies cases where housing stability is hypothetical (e.g., "he would like to own his own apartment"), negated (e.g., "he is not homeless"), or historical (e.g., the patient's past medical history lists a homelessness ICD code, which is no longer current). I customize the algorithm for my context so that transitional housing is counted as stable housing, while shelter stays, doubling up, and street homelessness count as unstable housing. Appendix Figure A5 provides examples of how RehouSED interprets and classifies notes into stably housed and unstably housed. If the algorithm sees no clear mention of stable or unstable housing, it classifies the note as unknown.

To employ RehouSED, I collect all clinical or social worker notes for Veterans in my sample, identifying over 50 million clinical notes that include a housing keyword.[14] After running RehouSED

[14]Reducing the notes to those that include a housing keyword was necessary to reduce the computational intensity of the algorithm.

on these notes, I generate a month-level stable housing variable, which equals 1 if the Veteran was ever identified as stably housed in that month. Then, I create a quarter level mean, $H_{q,i}$, or the percent of months during that quarter in which the Veteran was identified as stably housed.[15] If the Veteran had no mentions of housing status during that period, I classify them as having unknown housing status during that quarter with an indicator $U_{q,i}$.

Maintaining the category of unknown housing status eliminates concern over attrition bias, as those participating in no housing intervention are much less likely to have their housing status recorded. Importantly, however, the interpretation of regression results is subtle in the setting: stable housing essentially captures the joint outcome between having stable housing and having housing status recorded – e.g., by meeting with a social worker or in some cases, a primary care doctor.

After classifying housing status for all Veterans during the three year follow up period, I run 2SLS regressions where the outcome variables are either $H_{q,i}$ or $U_{q,i}$. I show the results in Figure 4. In quarter 1 (days 0-90), transitional housing recipients are more likely to be housed than HUD-VASH recipients. These results reflect the fact that Treatment First compliers are able to enter transitional housing units almost immediately. In contrast, Housing First recipients have to find an apartment that accepts their voucher, a much slower process. Nevertheless, this trend reverses by quarter 2. Housing First recipients maintain stable housing at a much higher rate throughout the study period, with an effect size reaching its peak around 34 percentage points in quarter 3 (days 180 - 270). In contrast, transitional housing has an attenuating effect on housing stability over time, with an effect size of only 9 p.p. by quarter 12 (days 990-1080). These results are also in line with institutional details: while Housing First recipients can keep their vouchers forever, GPD participants must find their own housing after a 6 month to 2 year stay in a transitional housing unit.

[15] I exclude notes classified as "unknown" from this quarter-level mean if the individual had at least one documented housing status during the quarter. In other words, if the patient was identified as stably housed in months 4 and 6 and had unknown housing status in month 5, I count them as having 100% stable housing during that quarter.

Similarly, both programs make one less likely to have unknown housing status, but transitional housing also has an attenuating effect over time. These results imply that social workers stay in contact with and can much better track the whereabouts of those actively enrolled in HUD-VASH or GPD. I further explore engagement with homelessness services in the next section. For more details on housing status over time, including complier means, see Appendix Section A3.

Engagement with homelessness services

Next, as implied in the prior section, enrollment in Housing First and transitional housing both increase the probability that one engages with VA social workers. In particular, HUD-VASH participants are required to participate in case management. Therefore, one channel for the mortality effect is that VA caseworkers can keep track of where Veterans are and potentially intervene in a crisis. To systematically track engagement with VA homelessness services over time, for each year following intake, I create an indicator, $SW_{i,y}$, for having a visit with a VA social worker during the year.[16] I then run 2SLS regressions with $SW_{i,y}$ for each year as the outcome variable. I show results in Figure 5. Overall, Housing First participants are 40 percentage points more likely to stay in contact with VA social workers, a result that is stable across the study period. Transitional housing participants are also 30 percentage points more likely to stay engaged with homelessness services in year 1, with effects fading over time as Veterans exit GPD housing.

Engagement with mental and physical health services

Lastly, I evaluate the hypothesis that housing stability is a necessary pre-requisite to address other issues, particularly substance use and mental health. This line of reasoning gets at the root of the Housing First versus Treatment First debate: with Housing First, mental health and substance use

[16] I exclude any visits in days 0-90, as Veterans must mechanically meet with a social worker in order to enroll in HUD-VASH or GPD.

treatment is optional. However, proponents hypothesize that individuals are better able to participate and comply with mental health treatment once they have a stable roof over their heads. Alternatively, in Treatment First models, mental health treatment, and sometimes sobriety, are typically required to stay enrolled in the program.

As all individuals in my sample were assigned a prior mental health diagnosis code, the majority previously attended at least one mental health visit. Thus, I construct two *intensive* margin measures of engagement with mental health care over time. First, I construct a simple variable of the count of mental health visits attended in year y. Secondly, as no shows are a big issue in mental health care, I construct a no show rate for each year y, or the percentage of visits one did not attend over the total number scheduled. As everyone in my sample has mental health or substance use condition, I let the no show rate equal 100% if one did not schedule or attend any visits during year y.

I show 2SLS regression results on use of mental health care in Figure 6. I find that transitional housing both increases one's use of mental health care in the first year and decreases the no show rate, consistent with transitional housing programs often requiring mental health treatment.[17] However, these effects are impermanent, fading to 0 by year 2 as Veterans exit GPD housing. In contrast, Housing First appears to have no impact on engagement with mental health care. This evidence suggests that the mortality effect is not driven by mental health treatment. However, I cannot rule out that housing alone improves one's mental health, even though Veterans are not seeking more care once they are housed.

Finally, in Appendix Figure A7, I also show results for engagement with VA primary care over time. I define engagement with primary care as attending any visit in year y, following general guidelines for checkup frequency. I find that neither program has an impact on overall engagement

[17] Effects of transitional housing on use of mental health care are likely undermeasured here, as I can only observe visits at the VA. While in transitional housing units, some Veterans may participate in in-house therapy delivered via third party organizations.

with primary care.

viii. Cause of death

Does housing alone explain the mortality effect, or do Veterans who recieve a housing voucher also benefit from improved mental health and reduced substance use? To further illuminate mechanisms, I link my sample to National Death Index data on cause of death, dividing deaths into those caused primarily by housing alone, those related to mental health or drug use, and those related to other causes. I show 2SLS results on cause of death in Table 8.

First, I investigate potential causes of death primarily explained by housing alone. Individuals who lack housing – particularly those who live on the streets – are more exposed to the elements, making them particularly vulnerable to extreme heat or cold. In Table 8 column 1, I find that voucher recipients much less likely to die from exposure to the elements. Secondly, without a safe place to call home, unhoused individuals are much more likely to subject to violence or assault. In column 2, I also find evidence that voucher recipients are much less likely to die from assault. These two outcomes demonstrate clearly that shelter alone lowers mortality risk.

Next, I investigate causes of death primarily related to mental health or substance use. Most intuitively, if mental health is improving and substance use is declining among Housing First recipients, one may expect suicide or overdose rates to decline. In column 3, I show some suggestive, though underpowered evidence that voucher recipients have lower rates of suicide and overdose. Lastly, I investigate a cause of death closely linked to both substance use and homelessness: HIV/AIDS, Hepatitis B, and Hepatitis C infections. Transmitted via blood, these infections are often spread among people who inject drugs via sharing needles or other injection drug use equipment. According to NASEM (2018), unhoused injection drug users are particularly likely to contract these viruses, as they often lack places to cleanly and safely inject drugs. In column 4, I also find that voucher recipients are at much lower risk of dying from HIV, Hepatitis B (HBV), or Hepatitis C (HCV). Decomposing this coefficient further, I find that HCV deaths explain around 76% of the effect.

The interpretation of this result is more subtle. While individuals often contract these viruses via unsafe injection practices, one can often survive many years with these conditions even if left untreated. However, HIV weakens the immune system, making one much more vulnerable to new infections. Similarly, viral hepatitis weakens the liver over time, leading to increased mortality risk from new infections or alcohol binges. Thus, cause of death here is multifaceted: first, it could suggest decreased drug or alcohol use, as well as safer injection practices. Prior descriptive research on people who inject drugs find a strong correlation between homelessness and risky injection behaviors, such as syringe and equipment sharing (Hotton et al. 2021). This mechanism is closely connected to "harm reduction" philosophy of addiction treatment, which espouses that even if drug use continues, safe injection practices – such as providing individuals sterile needles via needle exchanges – can help reduce drug use's harmful effects. Second, among those with pre-existing infections, it could suggest decreased risk of contracting non-drug related infections. For example, those who lack housing often have poorer hygiene, leading to skin or soft tissue infections. Finally, mortality reductions could be caused by improved adherence to antiviral medications. I find some weak evidence of this phenomenon: Housing First makes one 2.5 p.p. (s.e.=1.5) more likely to fill an antiviral medication for HIV, HCV, or HBV during the study period. Overall, causes of death ostensibly related to homelessness, mental health, or both explain about half of the mortality effect.

ix. Policy implications and cost effectiveness

Are any types of transitional housing effective?

Transitional housing at the VA is administered by third party agencies, who differ in both services provided and program requirements (such as requiring weekly therapy or even sobriety to stay enrolled). In fact, the VA explicitly defines different transitional housing types, which may be more or less service-intensive. This raises the question: are any types of transitional housing effective, and if so, what lessons can be learned from them? In particular, critics argue that transitional housing with strict requirements may merely increase barriers to obtaining a permanent home. In the extreme case, an individual may be kicked out their transitional housing unit and lose both immediate housing and social worker support.

Thus, one potential hypothesis is that transitional housing may be more effective if it: 1) does not have strict program requirements, and 2) primarily emphasizes placing individuals in long term housing. Two types of VA transitional housing fall more closely under this model: "low demand" and "bridge housing." In the "low demand" model, mental health treatment is offered, but explicitly not required, and services are focused on finding permanent housing. Similarly, the "bridge" model emphasizes very short term stays in transitional housing units, with the focus being on securing permanent housing. While I cannot observe the program type that individuals enroll in, I can observe the percent of transitional housing beds in each VISN that are classified as low demand and/or bridge. To examine program heterogeneity, I divide VISNs into those with above and below median shares of low demand/bridge beds. Then, I rerun the mortality IV regressions separately for the above and below median samples. I show results in Table A5. Though treatment effects are noisy, I find suggestive evidence that low demand and bridge housing may be more effective than other transitional housing types.

Is Housing First worth funding?

Even if Housing First programs improve health, are they worth paying for? One way to think about cost-effectiveness in this setting is to consider housing to be a health intervention. In public health research, medical interventions in the US – such as a new cancer treatment – are generally considered cost-effective if they cost less than \$100,000 - \$150,000 per year of life saved. In deciding what programs are worth funding, researchers construct incremental cost-effectiveness ratios (or ICERs), comparing a given intervention against this benchmark. To assess cost-effectiveness, I consider the ICER of HUD-VASH (compared to no intervention) over the three year study period as:

$$\text{ICER} = \frac{\Delta\text{cost}}{\Delta\text{life years}} \tag{12}$$

On the cost side, I first document if/when an individual in my sample received housing through HUD-VASH. I conservatively assume that once an individual obtains housing, they continue to use their HUD-VASH voucher throughout the remainder of the study period. Then, I run 2SLS regressions where the outcome variable is the number of months the individual used a housing voucher. I find that HUD-VASH enrollment causes one to use a housing voucher for 23.0 (s.e.=0.7) additional months over the study period. Excluding the cost of case management, HUD pays an estimated \$581 per month for every HUD-VASH voucher (National Homeless Information Project 2018). Thus, estimated housing costs per treated individual are approximately \$13,380.

Next, I estimate the additional costs of social worker visits compared to those who receive no housing intervention. I approximate the cost of social worker visits using the VA's Health Economics Resource Center (HERC), which estimates costs to the VA using Medicare reimbursement rates. Using the total cost of social worker visits as the left hand side variable in regressions, I estimate an additional cost for those who enroll in HUD-VASH of \$9,667 (s.e.=885).

I also assess changes in healthcare and homelessness services utilization that could lead to cost-savings among HUD-VASH recipients. First, I find that enrolling in the voucher program leads to an average of 7.9 (s.e.=2.98) less days of VA emergency shelter use over the study period. I estimate the average daily cost of a shelter stay to be \$31 (HUD 2010),[18] leading to a cost savings of \$246. Finally,

[18] This estimate averages the daily cost across various shelter types.

and most significantly, the VA operates various inpatient treatment programs related to homelessness and mental health, including residential rehabilitative treatment programs and domiciliary care. I find that enrolling in Housing First substantially decreases the length of stay in one of these programs by 12.57 days (s.e.=4.77) over the study period. According to HERC estimates, this decline in length of stay translates to a cost-saving of $41,734 (s.e.=$16,429).[19,20]

This result is consistent with descriptive studies from the medical literature, which find both very large healthcare spending among homeless individuals, as well as substantial cost differences once these individuals are housed. For chronically homeless individuals with alcohol use – a similar population to that of this paper – a study from Seattle finds that average total healthcare spending prior to housing was $15,072 per year. This number declined to $8,592 in the year following housing, translating to $19,440 in healthcare cost savings over three years (Larimer et al. 2009). A study from Massachussets finds that healthcare costs were around $32,331 per year for unsheltered homeless individuals, compared to only $9,648 for those with shelter, with the primary difference coming from inpatient costs (Koh et al. 2022). This translates to $68,049 in healthcare cost-savings over three years. Relatedly, other descriptive studies have noted that homeless individuals have longer hospital stays than non-homeless individuals. A study from New York City finds that homeless patients stay around 4.1 extra days in the hospital per discharge, with the majority of these stays being related to substance use or mental illness (Salit et al. 1998). Physicians reported delaying patients' discharge due to lack of housing and worry about compliance with follow up care. Considering these massive cost savings from shorter hospitalizations, one could think of Housing First as a diversion program – keeping individuals out of costly psychiatric care.

On the whole, due to a substantial reduction in inpatient costs, I find that Housing First is ac-

[19] Importantly, I replicate the RCT result from Rosenheck et al. (2003), which finds no effect of HUD-VASH on the *number* of inpatient stays. However, cost estimates change substantially once length of stay and visit-specific HERC cost estimates are calculated.

[20] I do not find any other significant effects on healthcare utilization, including outpatient mental health, primary care, ED visits, and hospitalizations related to physical health.

tually *cost-saving* over the three-year study period: compared to those who enrolled in no housing intervention, HUD-VASH saved $18,931 per participant. Thus, even if the intervention incurred no health benefit, it would be worth funding purely from a cost perspective. I summarize these findings in Table 9. This cost analysis should be interpreted as the costs incurred by either the VA or HUD, which is what I can observe. However, I likely underestimate broader cost-savings of HUD-VASH due to reduced use of other public services. For example, Housing First may reduce incarceration rates (Cohen 2024). In addition, I cannot observe non-VA emergency shelter use or Medicaid or Medicare spending.

Given that HUD-VASH incurs lower costs and has higher benefits than no housing intervention, from the ICER perspective, Housing First is a dominant intervention. Thinking about costs and benefits from an ICER perspective is also closely connected to the public finance literature on the Marginal Value of Public Funds (see Hendren and Sprung-Keyser 2020) in cases where fiscal externalities are close to 0. Although I am unable to observe employment for Housing First participants, Brounstein and Wieselthier (2024) estimate minimal labor supply effects from enrollment in permanent housing, likely due to large rates of unemployment among this population. Thus, if participants have any positive willingness to pay for improved quality of life and increased longevity due to permanent housing, the marginal value of public funds in this setting is infinite.

V. Discussion

This study is not only the first to show that Housing First unequivocably reduces mortality, but it is the first to explicitly show that Housing First far outperforms Treatment First type interventions in terms of both long term housing stability and health outcomes. My results add to a growing body of evidence that the primary solution to homelessness is housing, and that psychosocial issues do not need to be addressed prior to addressing issues of housing instability (e.g., Rosenheck et al. 2003, Gubits et al. 2018, Montgomery et al. 2013). Moreover, echoing descriptive results from the medical literature, I find that Housing First is cost-saving due to large reductions in psychiatric hospitalization length of stay.

However, my results cast doubt on the narrative that Housing First models provide the stability needed for individuals with mental health issues to begin to address them. Despite high rates of mental health treatment non-attendance in this population, Housing First does not lead to an increase in mental health treatment nor improve the rate of appointment attendance. In contrast, Treatment First models do improve adherence to mental health treatment in the short term, but effects are short-lasting and do not seem to translate into large health gains. Thus, for the large population of unhoused, mentally ill individuals, other interventions may be necessary to improve long term engagement with mental health care. Despite having little impact on healthcare utilization, Housing First has a large effect on overall health and wellbeing, as indicated by lower mortality rates. In particular, permanent housing reduces deaths from extreme weather, violence, and infections caused by unsafe drug injection. This latter result provides some suggestive evidence that "harm reduction" in addiction treatment – or providing resources to allow individuals to safely inject drugs – could reduce mortality.

Comparing outcomes from Housing First versus Treatment First interventions is of critical policy relevance. In an executive order from July 24, 2025, entitled "Ending Crime and Disorder on America's Streets," the Trump administration officially ended federal support for Housing First policies. Instead, the administration hopes to shift funding towards programs focused on mental health and substance use treatment. Emphasizing high rates of behavioral health disorders and addiction among the homeless, the Trump adminstration stated that Housing First policies "deprioritize accountability and fail to promote treatment, recovery, and self-sufficiency." Despite the adminstration's promise to curb street homelessness, this study suggests that scaling back Housing First initiatives will likely have the opposite of the desired effect.

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Table 1: Veteran characteristics

	Full sample	Voucher recipients	Transitional housing enrollees
Male	0.90	0.84	0.92
Black	0.36	0.37	0.39
Hispanic	0.07	0.07	0.06
Age	50.0	49.0	50.7
Depression	0.66	0.67	0.63
Psychoses	0.13	0.11	0.13
Alcohol use	0.41	0.37	0.49
Drug use	0.40	0.37	0.49
Chronic physical disease	0.62	0.62	0.61
Prior inpatient - homelessness	0.12	0.10	0.15
Prior inpatient - mental health	0.23	0.18	0.31
Voucher receipt	0.12	1.0	0.0
Transitional housing	0.18	0.0	1.0
N	289,933	34,285	52,901

Note: This table shows mean characteristics for the full sample, voucher recipients, and transitional housing recipients. Voucher and transitional housing refers to enrolling in that program within the first 3 months of intake.

Table 2: Complier groups

Complier group	$\mathbf{D}_i(z_v, z_t) =$ $(D_i(0,0), D_i(1,0), D_i(0,1), D_i(1,1))$	Description
C_0^v	$(0, v, 0, v)$	Compliers who only respond to $Z_{v,i}$
C_0^t	$(0, 0, t, t)$	Compliers who only respond to $Z_{t,i}$
$C_0^{v+,t-}$	$(0, v, t, v)$	Compliers who respond to both, but $Z_{v,i}$ is more compelling
$C_0^{v-,t+}$	$(0, v, t, t)$	Compliers who respond to both, but $Z_{t,i}$ is more compelling
C_t^v	(t, v, t, v)	Compliers who always respond to $Z_{v,i}$, with a counterfactual of t
C_t^{v-}	(t, v, t, t)	Compliers who respond to $Z_{v,i}$ only when $Z_{t,i} = 0$, with a counterfactual of t
C_v^t	(v, v, t, t)	Compliers who always respond to $Z_{t,i}$, with a counterfactual of v

Note: This table defines allowed complier groups according to my treatment choice model. I convert both instruments to binary variables for simplicity, but one can think of $z_v = 1$ as high voucher availability. Column 2 shows potential treatments for each complier group when z_v and z_t are both 0, when $z_v = 1$ and $z_t = 0$, when $z_v = 0$ and $z_t = 1$, and when both instruments equal 1. Column 3 provides a conceptual description. Note that under this model, $\mathbf{D}_i(z_v, z_t) = (v, v, t, v)$ is not a possible behavioral response pattern. For these individuals, $O_{v,i}(0) = 1$ (given that they choose v when $z_v = 0$). When $(z_v, z_t) = (0, 1)$, they choose t, which implies that $\arg\max_{k \in \{0,v,t\}} U_{k,i} = t$. Thus, when $(z_v, z_t) = (1, 1)$, they must also choose t, as their choice set and preferences remain unchanged.

Table 3: First stage and reduced form

	(1) Voucher	(2) Transitional	(3) Neither	(4) 3-year mortality
Perc. receiving voucher	0.510	-0.050	-0.460	-0.02316
	(0.017)	(0.016)	(0.020)	(0.00861)
Tr. liaison available	-0.013	0.089	-0.076	-0.00004
	(0.002)	(0.002)	(0.002)	(0.00001)
Dep. var. mean	0.118	0.182	0.699	0.066
Observations	289,932	289,932	289,932	289,932

Note: This figure shows first stage (columns 1-3) and reduced form (column 4) regression coefficients for $Z_{v,i}$ (perc. receiving voucher) and $Z_{t,i}$ (tr. liaison available). In columns 1-3, indicators for enrolling in the voucher program, transitional housing, or neither are the left-hand side variables. In column 4, 3-year mortality is the left-hand side variable. Baseline controls include VA station $\times$ year, VA station $\times$ month, and indicators for having a prior inpatient stay related to homelessness or mental health. Standard errors for all regressions are clustered by VA station $\times$ year $\times$ month.

Table 4: Effect of voucher and transitional housing on mortality

	(1) OLS	(2) IV	(3) All controls	(4) Drop SSVF	(4) Drop prior inpatient
Voucher	-0.013	-0.046	-0.046	-0.045	-0.058
	(0.001)	(0.017)	(0.016)	(0.018)	(0.019)
Trans. housing	-0.004	-0.007	-0.012	-0.012	-0.003
	(0.001)	(0.015)	(0.014)	(0.015)	(0.019)
Dep. var. mean	0.066	0.066	0.066	0.066	0.063
Observations	289,932	289,932	289,932	281,474	219,937

Note: This table shows the main results for the effect of the voucher program and transitional housing on three-year mortality. Column 1 shows OLS results, and column 2 shows 2SLS IV results. Column 3 shows 2SLS regressions when I add all patient characteristics as control variables. Column 4 shows the estimates when I drop patients who enroll in SSVF, or temporary rental assistance services. Finally, column 5 shows results when I drop patients with a prior inpatient mental health or homelessness stay. Baseline controls include VA station $\times$ year, VA station $\times$ month, and indicators for having a prior inpatient stay related to homelessness or mental health. Standard errors for all regressions are clustered by VA station $\times$ year $\times$ month.

Table 5: Overidentification tests

Additional instruments		Voucher	Transitional housing
Male $\times Z_{v,i}$	2SLS	-0.044	-0.006
Male $\times Z_{t,i}$		(0.017)	(0.015)
	First stage F	234	460
	Overid. p-value	0.304	
Black $\times Z_{v,i}$	2SLS	-0.039	-0.007
Black $\times Z_{t,i}$		(0.016)	(0.015)
	First stage F	248	465
	Overid. p-value	0.279	
Hispanic $\times Z_{v,i}$	2SLS	-0.047	-0.007
Hispanic $\times Z_{t,i}$		(0.017)	(0.015)
	First stage F	228	466
	Overid. p-value	0.818	
Depression $\times Z_{v,i}$	2SLS	-0.044	-0.005
Depression $\times Z_{t,i}$		(0.017)	(0.015)
	First stage F	227	462
	Overid. p-value	0.585	
Psychoses $\times Z_{v,i}$	2SLS	-0.049	-0.007
Psychoses $\times Z_{t,i}$		(0.017)	(0.015)
	First stage F	227	462
	Overid. p-value	0.867	
Alcohol use $\times Z_{v,i}$	2SLS	-0.044	-0.011
Alcohol use $\times Z_{t,i}$		(0.017)	(0.015)
	First stage F	227	475
	Overid. p-value	0.463	
Drug use $\times Z_{v,i}$	2SLS	-0.048	-0.011
Drug use $\times Z_{t,i}$		(0.018)	(0.015)
	First stage F	229	461
	Overid. p-value	0.184	
High $\hat{Y}_i \times Z_{v,i}$	2SLS	-0.047	-0.011
High $\hat{Y}_i \times Z_{t,i}$		(0.016)	(0.015)
	First stage F	230	465
	Overid. p-value	0.153	
All interactions	2SLS	-0.039	-0.006
(16 additional		(0.017)	(0.016)
instruments)	First stage F	54	97
	Overid. p-value	0.592	

Note: This table shows 2SLS and overidentification test (J-test) results when I add additional instruments to the model. For a given characteristic X_i, I construct additional instruments as $Z_{v,i}X_i$ and $Z_{t,i}X_i$ and add these to the baseline 2SLS regressions. To maintain instrument validity in these specifications, I also interact X_i with the baseline time $\times$ station controls. High $\hat{Y}_i$ refers to above median predicted mortality.

Table 6: 2SLS bounds under small violations to no essential heterogeneity

	(1)	(2)
Epsilon	Voucher	Transitional housing
0.010	(-0.049, -0.043)	(-0.010, -0.004)
0.005	(-0.047, -0.045)	(-0.008, -0.006)
0.001	(-0.046, -0.046)	(-0.007, -0.007)

Note: This table shows estimated upper and lower bounds on true LATEs if there are small violations to no essential heterogeneity, or under the condition that $|E[Y_{k,i} - Y_{0,i}|C_i = c] - E[Y_{k,i} - Y_{0,i}|C_i = c']| < \epsilon$ for various values of epsilon. In column 1, I show bounds on $LATE_v = E[Y_{v,i} - Y_{0,i}|C_0^v]$, the LATE of Housing First (compared to no intervention) for C_0^v compliers. In column 2, I show bounds on $LATE_t = E[Y_{t,i} - Y_{0,i}|C_0^t]$, the LATE of Treatment First for C_0^t compliers. I derive these bounds in Appendix Section A2.

Table 7: Exclusion restriction tests

	(1) Baseline	(2) Number of staff	(3) Homelessness services speed	(4) Emergency housing use	(5) Case management
Voucher	-0.046	-0.050	-0.049	-0.046	-0.046
	(0.017)	(0.019)	(0.018)	(0.017)	(0.017)
Trans. housing	-0.007	-0.011	-0.008	-0.007	-0.006
	(0.015)	(0.016)	(0.015)	(0.015)	(0.015)
Dep. var. mean	0.066	0.066	0.066	0.066	0.066
Observations	289,932	289,932	289,923	289,932	289,932

Note: This table shows the effect of the voucher program and transitional housing on three-year mortality when I add additional controls related to exclusion. Column 1 shows the baseline result, column 2 controls for the number of staff working at intake, and column 3 controls for the (leave out) average wait time to a follow up appointment with a social worker. Next, column 4 controls for use of emergency housing, and column 5 controls for use of case management. Baseline controls include VA station $\times$ year, VA station $\times$ month, and indicators for having a prior inpatient stay related to homelessness or mental health. Standard errors for all regressions are clustered by VA station $\times$ year $\times$ month.

Table 8: Cause of death

	(1) Exposure to the elements	(2) Violence/assault	(3) Suicide or overdose	(4) HIV or Hepatitis B/C	(5) Other causes of death
Voucher	-0.0016	-0.0051	-0.0088	-0.0059	-0.0251
	(0.0006)	(0.0018)	(0.0069)	(0.0026)	(0.0159)
Trans. housing	-0.0006	-0.0005	0.0029	-0.0002	-0.0087
	(0.0007)	(0.0013)	(0.0059)	(0.0020)	(0.0136)
Dep. var. mean	0.0001	0.0006	0.0098	0.0014	0.0537
Observations	289,932	289,932	289,932	289,932	289,932

Note: This table shows the mortality effect broken up by cause of death. HIV, Hepatitis B, and Hepatitis C are often caused by injection drug users sharing needles. Exposure to the elements refers to extreme heat or cold. Baseline controls include VA station $\times$ year, VA station $\times$ month, and indicators for having a prior inpatient stay related to homelessness or mental health. Standard errors for all regressions are clustered by VA station $\times$ year $\times$ month.

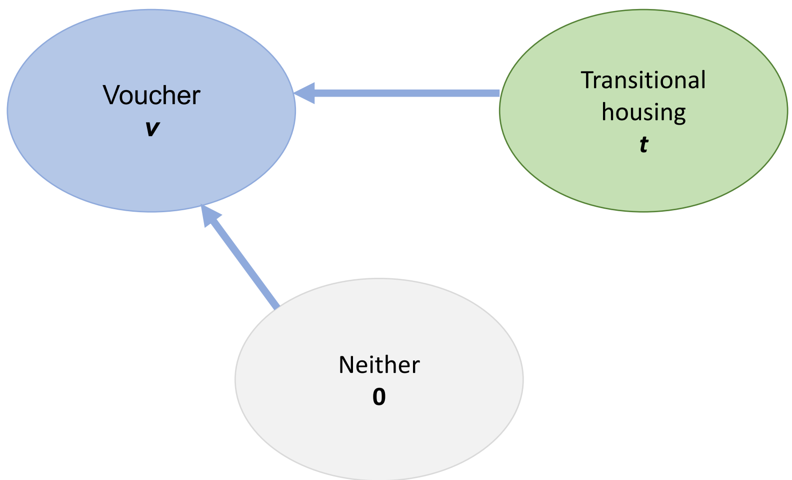
Table 9: Housing First cost-benefit analysis

Program costs		Program savings		Total
Housing costs	Social worker visits	Shelter stays	Inpatient stays	
\$13,380	\$9,667	-\$246	-\$41,734	-\$18,931
(\$407)	(\$885)	(\$92)	(\$16,429)	

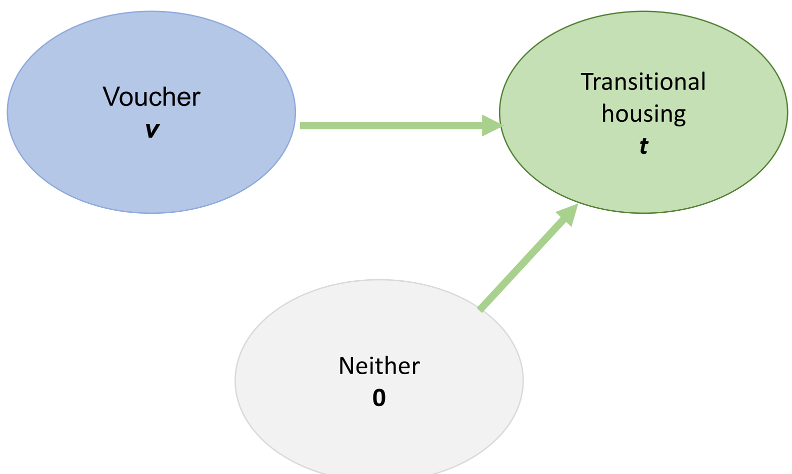
Note: This table shows the costs of funding HUD-VASH over the three year study period, including cost-savings from reduced use of other programs. I include costs incurred by the VA and HUD, which is what I can observe. Housing costs are based on the number of months HUD-VASH participants were actively using a housing voucher, based on social worker documentation (compared to the no-intervention group). Social worker visit costs and the cost of inpatient stays come from estimates from the VA's Health Economics Resource Center (HERC) and are based on Medicare reimbursement rates.

Figure 1: Treatment flows induced by $Z_{v,i}$ and $Z_{t,i}$

A: Flows induced by $Z_{v,i}$

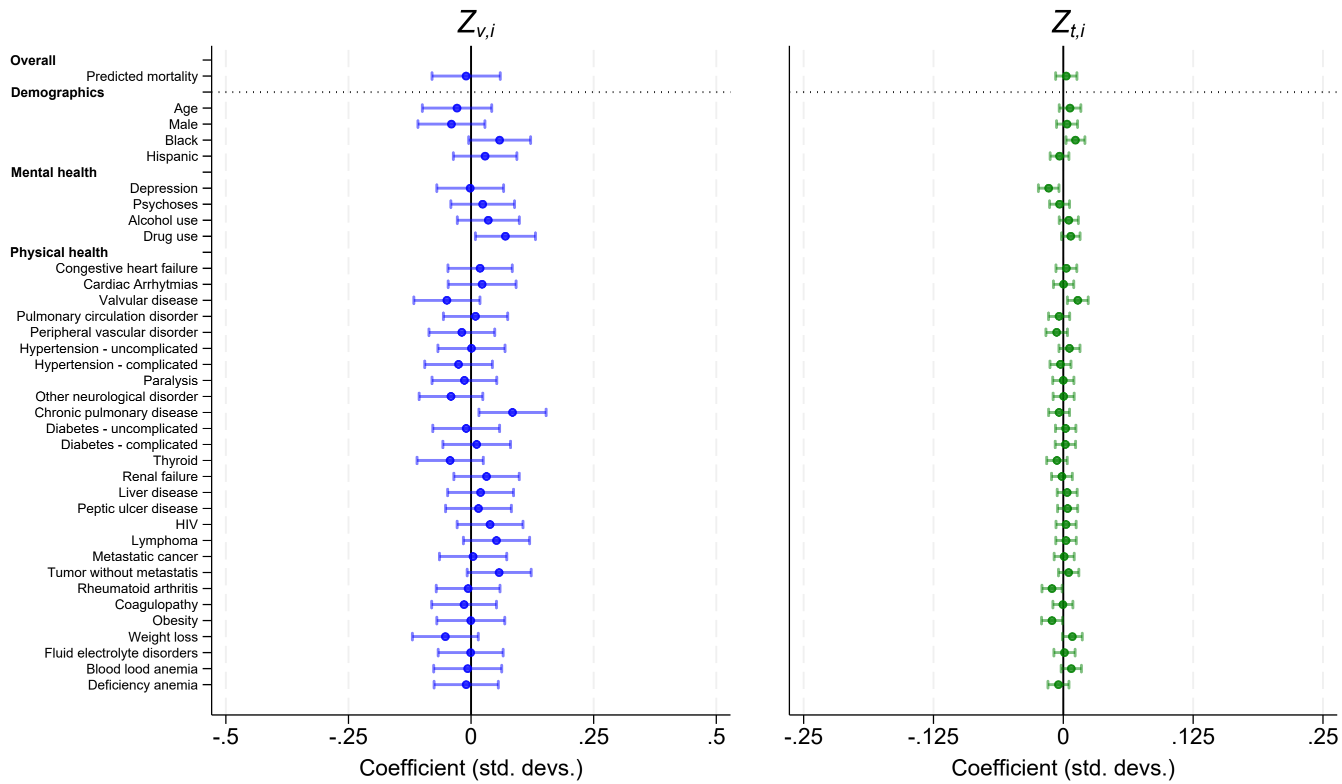


B: Flows induced by $Z_{t,i}$



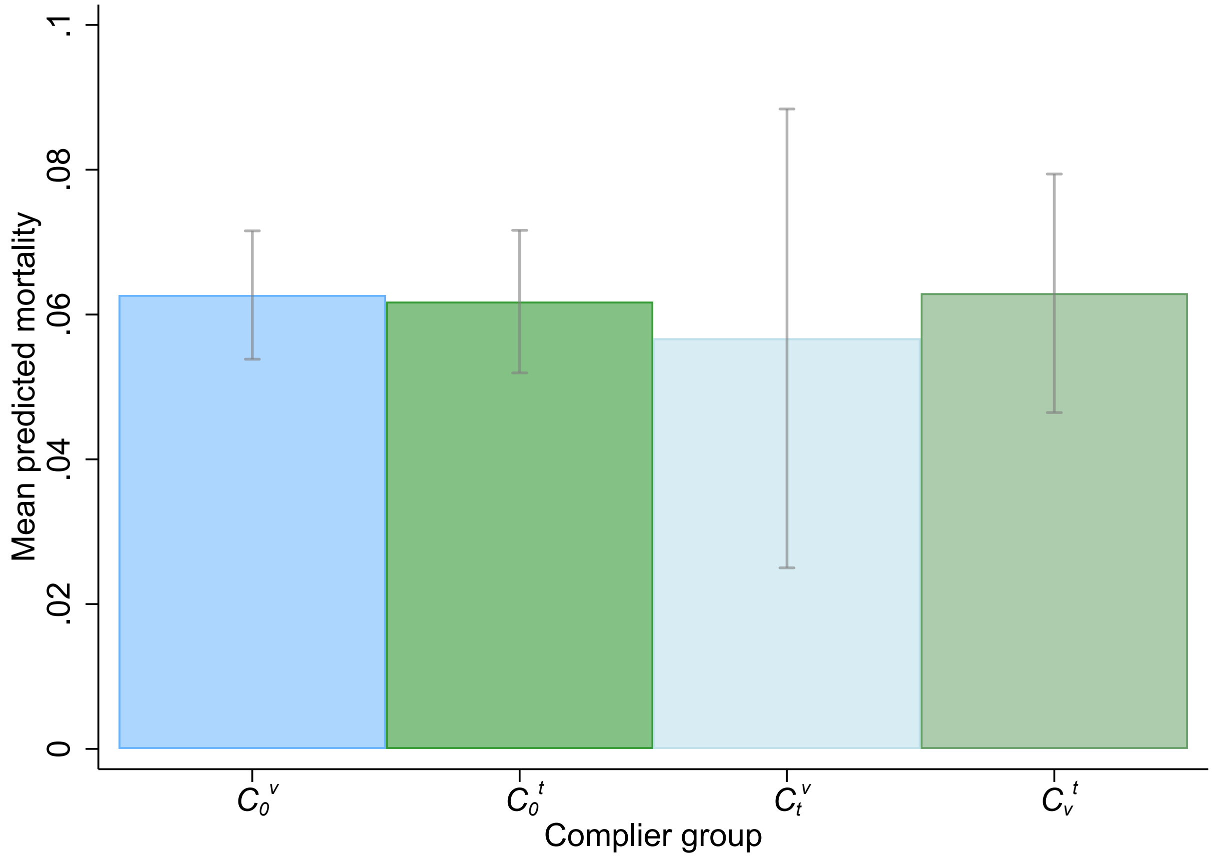
Note: This figure shows allowed flows between treatment states induced by $Z_{v,i}$ and $Z_{t,i}$, according to my treatment choice model.

Figure 2: Balance tests



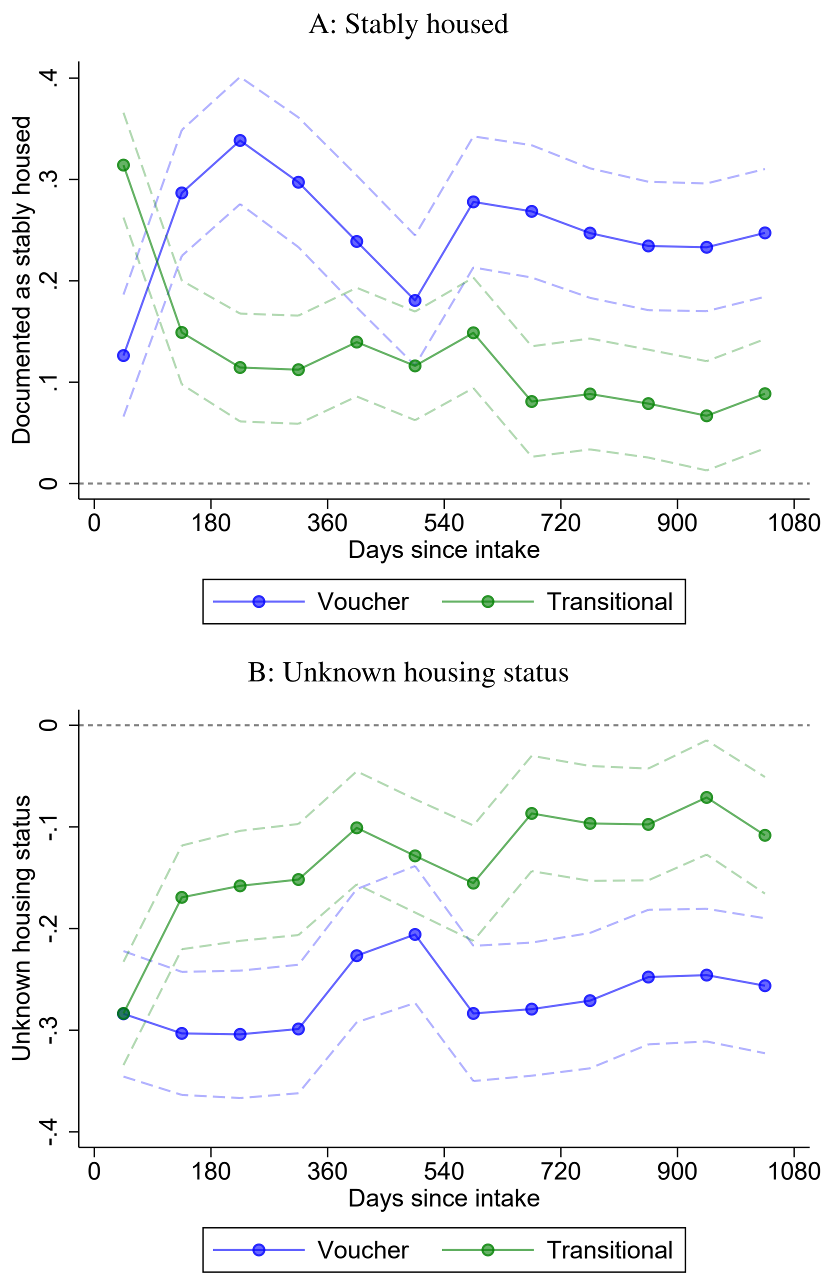
Note: This figure shows balance tests when a given characteristic X_i is the left hand side variable. Dots are point estimates, and bars show standard errors. Results for $Z_{v,i}$ are shown in blue, and results for $Z_{t,i}$ are shown in green. Predicted mortality comes from an OLS regression of 3-year mortality on all characteristics X_i. Baseline controls include VA station $\times$ year, VA station $\times$ month, and indicators for having a prior inpatient stay related to homelessness or mental health. Standard errors for all regressions are clustered by VA station $\times$ year $\times$ month.

Figure 3: Mean predicted mortality across complier groups



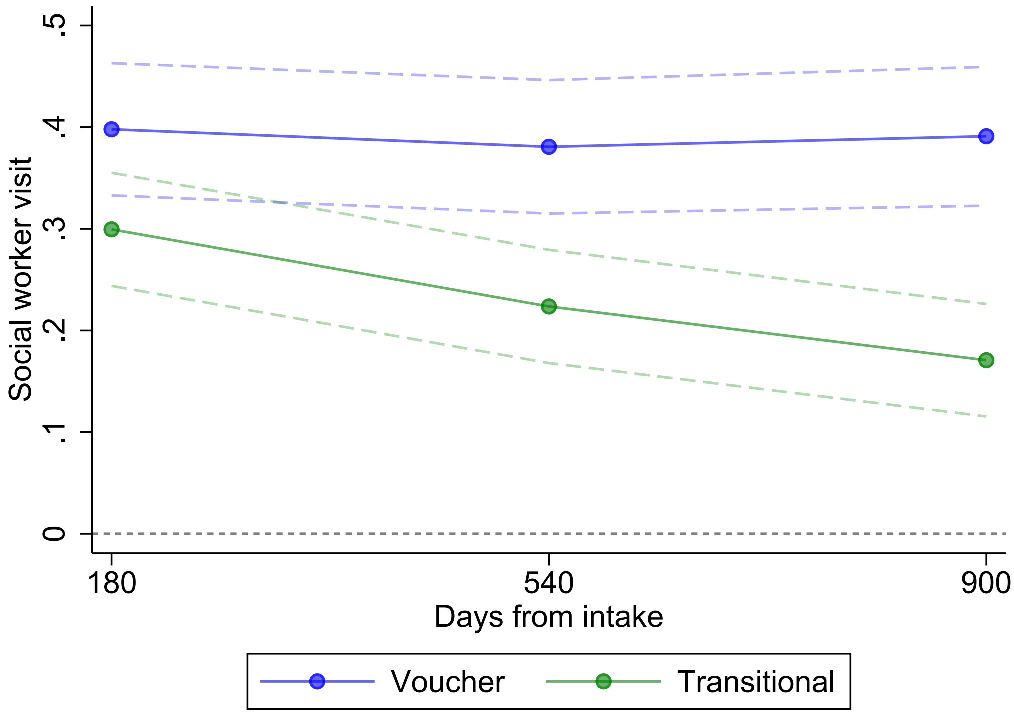
Note: This regression shows mean complier predicted mortality for C_0^v, C_0^t, C_t^v, and C_v^t compliers, respectively.C_0^v compliers are those induced into v from a counterfactual of 0, C_0^t compliers are those induced into t from a counterfactual of 0, C_t^v compliers are those induced from t to v, and C_v^t compliers are those induced from v to t.

Figure 4: Effect of voucher and transitional housing on housing status over time



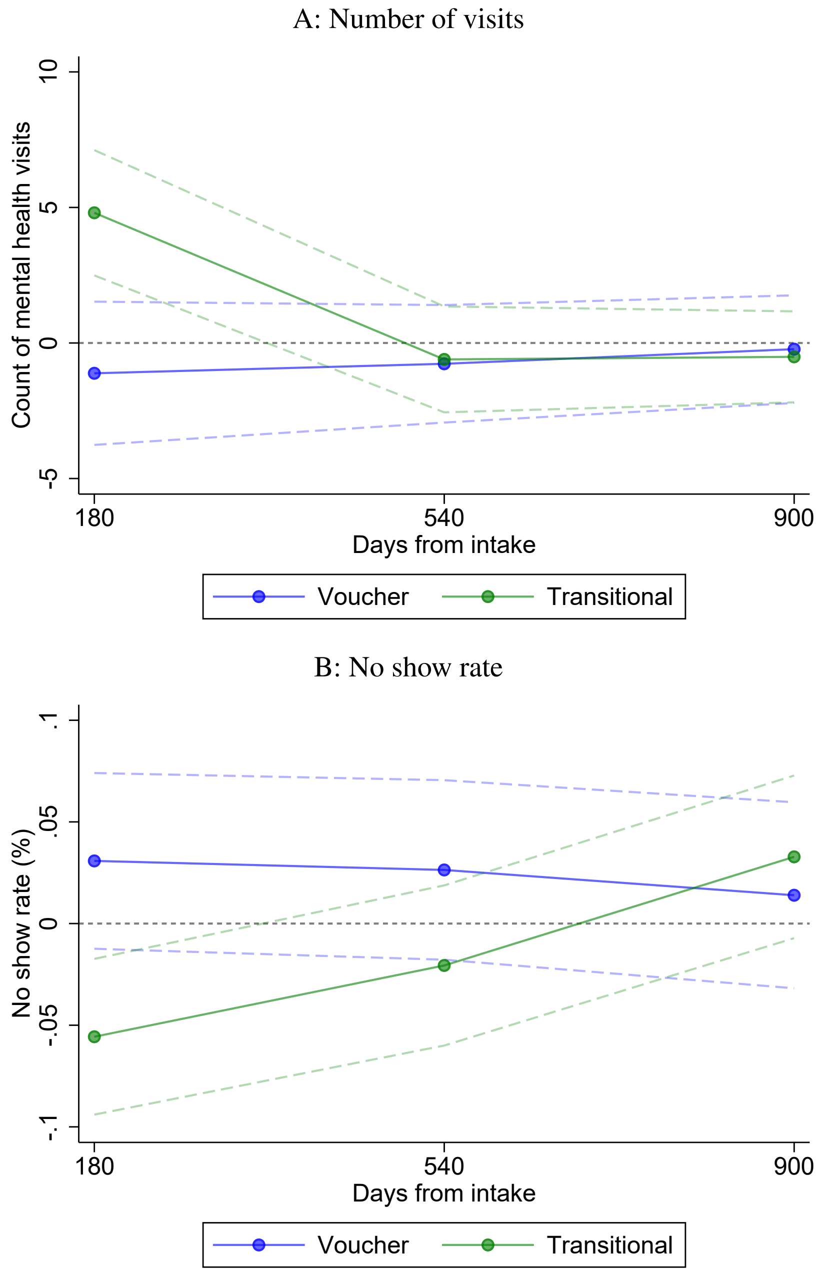
Note: This figure shows 2SLS regression results for the effect of the voucher program and transitional housing on housing status over time. Housing status is broken into quarters from intake. Panel A shows the effect on the probability of being stably housed within that quarter. Panel B shows the effect on the probability of having unknown housing status that quarter, meaning that housing status was not clearly recorded by a social worker. Results for the voucher program are shown in blue, and results for transitional housing are shown in green. Dotted lines show upper and lower bounds.

Figure 5: Engagement with VA homelessness services



Note: This figure shows 2SLS regression results for the effect of the voucher program and transitional housing on engagement with homelessness services in years 1, 2, and 3 following intake. The dependent variable is having any visit with a social worker in that year. Results for the voucher program are shown in blue, and results for transitional housing are shown in green. Dotted lines show upper and lower bounds.

Figure 6: Engagement with VA mental health services



Note: This figure shows 2SLS regression results for the effect of the voucher program and transitional housing on engagement with mental health care. Panel A shows the effect on the number of attended mental health visits. Panel B shows the effect on the no show rate, defined as the percent of visits not attended over the total number scheduled. Results for the voucher program are shown in blue, and results for transitional housing are shown in green. Dotted lines show upper and lower bounds.

Appendix for Housing First or Treatment First?

Evidence from the VA's homelessness programs

Sydney Costantini

A1 Understanding complier types: detail

A1.1 Proof of Proposition 1

Proof. Let:

$$\Pr[D_{k,i}=1|Z_v=1,Z_t=1]-\Pr[D_{k,i}=1|Z_v=1,Z_t=0]=\Pr[D_{k,i}=1|Z_v=0,Z_t=1]-\Pr[D_{k,i}=1|Z_v=0,Z_t=0]$$

$\forall k \in \{0,v,t\}$. Adding and subtracting terms in this expression, the following equalities all must hold (see Appendix Table A2):

$$\underbrace{\Pr[D_0=1|Z_v=1,Z_t=0]-\Pr[D_0=1|Z_v=0,Z_t=0]}_{-(P(C_0^v)+P(C_0^{v+,t-})+P(C_0^{v-,t+}))}=\underbrace{\Pr[D_0=1|Z_v=1,Z_t=1]-\Pr[D_0=1|Z_v=0,Z_t=1]}_{-P(C_0^v)}$$

$$\underbrace{\Pr[D_v=1|Z_v=0,Z_t=1]-P[D_v|Z_v=0,Z_t=0]}_{-P(C_v^t)}=\underbrace{\Pr[D_v=1|Z_v=1,Z_t=1]-\Pr[D_v=1|Z_v=1,Z_t=0]}_{-(P(C_0^{v-,t+})+P(C_v^t)+P(C_t^{v-}))}$$

$$\underbrace{\Pr[D_t=1|Z_v=1,Z_t=0]-\Pr[D_t=1|Z_v=0,Z_t=0]}_{-(P(C_t^v)+P(C_t^{v-}))}=\underbrace{\Pr[D_t=1|Z_v=1,Z_t=1]-\Pr[D_t=1|Z_v=0,Z_t=1]}_{-(P(C_0^{v+,t-})+P(C_t^v))}$$

These equations imply the following equalities hold for complier shares:

$$P(C_0^v) + P(C_0^{v+,t-}) + P(C_0^{v-,t+}) = P(C_0^v)$$

$$P(C_v^t) = P(C_0^{v-,t+}) + P(C_v^t) + P(C_t^{v-})$$

$$P(C_t^v) + P(C_t^{v-}) = P(C_0^{v+,t-}) + P(C_t^v)$$

These three equalities can only hold if $P(C_0^{v+,t-}) = P(C_0^{v-,t+}) = P(C_t^{v-}) = 0$. The remaining complier groups are then point identified from propensity scores as:

$$P(C_0^v) = -(\Pr[D_0 = 1|Z_v = 1, Z_t = 0] - \Pr[D_0 = 1|Z_v = 0, Z_t = 0])$$

$$P(C_0^t) = -(\Pr[D_0 = 1|Z_v = 0, Z_t = 1] - \Pr[D_0 = 1|Z_v = 0, Z_t = 0])$$

$$P(C_t^v) = -(\Pr[D_t = 1|Z_v = 1, Z_t = 0] - \Pr[D_t = 1|Z_v = 0, Z_t = 0])$$

$$P(C_v^t) = -(\Pr[D_v = 1|Z_v = 0, Z_t = 1] - \Pr[D_v = 1|Z_v = 0, Z_t = 0])$$

□

A1.2 Empirical complier shares

Following Proposition 1, to empirically estimate complier shares, I run the following first stage regressions using a binary version of $Z_{v,i}$:

$$D_{0,i} = \alpha_v Z_{v,i} + \alpha_t Z_{t,i} + \alpha_{vt} Z_{v,i} Z_{t,i} + \zeta_{0,h(i)y(i)} + \phi_{0,h(i)m(i)} + X_i \beta_0 + \epsilon_{0,i} \tag{A.1}$$

$$D_{v,i} = \pi_v Z_{v,i} + \pi_t Z_{t,i} + \pi_{vt} Z_{v,i} Z_{t,i} + \zeta_{1,h(i)y(i)} + \phi_{1,h(i)m(i)} + X_i \beta_1 + \epsilon_{1,i} \tag{A.2}$$

$$D_{t,i} = \psi_v Z_{v,i} + \psi_t Z_{t,i} + \psi_{vt} Z_{v,i} Z_{t,i} + \zeta_{2,h(i)y(i)} + \phi_{2,h(i)m(i)} + X_i\beta_2 + \epsilon_{2,i} \tag{A.3}$$

where $D_{0,i}$, $D_{v,i}$, and $D_{t,i}$ are indicators for receiving no intervention, the voucher program, and transitional housing, respectively. In practice, let discrete $Z_{v,i} = 1$ if (residualized) voucher availability is above the 90th percentile. Conditional on the baseline controls, these coefficients yield various complier share mixtures, as described in Appendix Table A2.

In Appendix Table A3, I show coefficient estimates. In all three regressions, the interaction term coefficient is close to 0 and statistically insignificant, implying that the joint values of $Z_{v,i}, Z_{t,i}$ do not affect treatment choice (see proof in Section A1.2). In other words, $P(C_0^{v+,t-}) = P(C_0^{v-,t+}) = P(C_t^{v-}) = 0$. This leaves four complier groups that are actually present empirically: C_0^v, C_0^t, C_v^t,and C_t^v. The shares of these groups are given by $-\alpha_v, -\alpha_t, -\pi_t$, and $-\psi_v$, respectively, corresponding to the equivalent propensity score differences in Proposition 1.

A2 2SLS bounds with multiple instruments

A2.1 Decomposing 2SLS

Below, I decompose the 2SLS coefficient, β_v, into a well-defined LATE and selection terms. This proof follows closely those of Heinesen et al. (2022) and Mountjoy (2022). For simplicity, I ignore control variables X_i and consider the case of two binary instruments, $Z_{v,i}$ and $Z_{t,i}$.

First, following the notation in Appendix A1.2:

$$E[D_{v,i}|\mathbf{Z}_i] = \pi_0 + \pi_v Z_{v,i} + \pi_t Z_{t,i} \tag{A.4}$$

$$E[D_{t,i}|\mathbf{Z}_i] = \psi_0 + \psi_v Z_{v,i} + \psi_t Z_{t,i} \tag{A.5}$$

Similarly, for Y_i:

$$E[Y_i|\mathbf{Z}_i] = \gamma + \beta_v \hat{D}_{v,i} + \beta_t \hat{D}_{t,i} \tag{A.6}$$

where β_v is the 2SLS coefficient for the effect of housing first, and $\hat{D}_{v,i}$ and $\hat{D}_{t,i}$ are predicted from first stage models.

Plugging in Eq. A4 and A5:

$$E[Y_i|\mathbf{Z}_i] = \gamma + \beta_v(\pi_0 + \pi_v Z_{v,i} + \pi_t Z_{t,i}) + \beta_t(\psi_0 + \psi_v Z_{v,i} + \psi_t Z_{t,i})$$

$$= (\gamma + \pi_0\beta_v + \psi_0\beta_t) + \underbrace{(\beta_v\pi_v + \beta_t\psi_v)}_{\zeta_v} Z_{v,i} + \underbrace{(\beta_v\pi_t + \beta_t\psi_t)}_{\zeta_t} Z_{t,i}$$

I can then solve for β_v as a function of $\pi_v, \pi_t, \psi_v, \psi_t, \alpha_v$, and α_t as:

$$\beta_v = \frac{\psi_t\zeta_v - \psi_v\zeta_t}{\pi_v\psi_t - \psi_v\pi_t} \tag{A.7}$$

Now, reduced form coefficients ζ_v and ζ_t can be represented as a weighted sum of LATEs and complier shares, as follows:

$$\zeta_v = E[Y_{v,i} - Y_{0,i}|C_0^v]P(C_0^v) + E[Y_{v,i} - Y_{t,i}|C_t^v]P(C_t^v) \tag{A.8}$$

as $Z_{v,i}$ induces C_0^v compliers from 0 to v and C_t^v compliers from t to v. Similarly,

$$\zeta_t = E[Y_{t,i} - Y_{0,i}|C_0^t]P(C_0^t) + E[Y_{t,i} - Y_{v,i}|C_v^t]P(C_v^t) \tag{A.9}$$

Plugging these definitions into Eq. A7:

$$\beta_v = \frac{\psi_t(E[Y_{v,i} - Y_{0,i}|C_0^v]P(C_0^v) + E[Y_v - Y_t|C_t^v]P(C_t^v)) - \psi_v(E[Y_{t,i} - Y_{0,i}|C_0^t]P(C_0^t) + E[Y_t - Y_v|C_v^t]P(C_v^t))}{\pi_v\psi_t - \psi_v\pi_t}$$

Based on Appendix Table A2, we also know that $\pi_v = P(C_0^v) + P(C_t^v)$, $\pi_t = -P(C_v^t)$, $\psi_v = -P(C_t^v)$, and $\psi_t = P(C_0^t) + P(C_v^t)$. Plugging these in:

$$\beta_v = \frac{(P(C_0^t) + P(C_v^t))(E[Y_{v,i} - Y_{0,i}|C_0^v]P(C_0^v) + E[Y_v - Y_t|C_t^v]P(C_t^v)) + P(C_t^v)(E[Y_{t,i} - Y_{0,i}|C_0^t]P(C_0^t) + E[Y_t - Y_v|C_v^t]P(C_v^t))}{\underbrace{(P(C_0^v) + P(C_t^v))(P(C_0^t) + P(C_v^t)) - P(C_v^t)P(C_t^v)}_{w}}$$

We can interpret $E[Y_{v,i}-Y_{0,i}|C_0^v]$ as the local average treatment effect of housing first (compared to no intervention) for C_0^v compliers. To isolate this term, add and subtract $\frac{W}{W}E[Y_{v,i}-Y_{0,i}|C_0^v]$:

$$\beta_v = E[Y_{v,i}-Y_{0,i}|C_0^v] + \underbrace{\frac{P(C_t^v)P(C_0^t)}{W}}_{A}(E[Y_{v,i}-Y_{t,i}|C_t^v] - E[Y_{v,i}-Y_{0,i}|C_0^v] + E[Y_{t,i}-Y_{0,i}|C_0^t])$$

$$+\underbrace{\frac{P(C_t^v)P(C_v^t)}{W}}_{B}(E[Y_{v,i}-Y_{t,i}|C_t^v] + E[Y_{t,i}-Y_{v,i}|C_v^t])$$

Re-expressing the above in terms of a LATE term and selection terms, I have:

$$\beta_v = \underbrace{E[Y_{v,i}-Y_{0,i}|C_0^v]}_{\text{LATE}_v} + A(E[Y_{v,i}-Y_{0,i}|C_t^v] - E[Y_{v,i}-Y_{0,i}|C_0^v]) + A(E[Y_{t,i}-Y_{0,i}|C_0^t] - E[Y_{t,i}-Y_{0,i}|C_t^v]) \tag{A.10}$$

$$+B(E[Y_{v,i}-Y_{0,i}|C_t^v] - E[Y_{v,i}-Y_{0,i}|C_v^t]) + B(E[Y_{t,i}-Y_{0,i}|C_v^t] - E[Y_{t,i}-Y_{0,i}|C_t^v])$$

A2.2 Defining worst case scenario bounds

Returning to Eq. 11 in the main text, assume $|E[Y_{k,i}-Y_{0,i}|C_i=c] - E[Y_{k,i}-Y_{0,i}|C_i=c']| < \epsilon$ for all complier groups c. Therefore, if all average treatment effects for complier groups differ by at most ϵ, the difference between the true LATE and β_v can be characterized as:

$$|\text{LATE}_v - \beta_v| \leq 2A\epsilon + 2B\epsilon$$

This equation implies I can define bounds on the true LATE as $(\beta_v - 2A\epsilon - 2B\epsilon, \beta_v + 2A\epsilon + 2B\epsilon)$.

A3 Housing status over time: complier means

To gain a fuller picture of housing status over time across complier groups, I follow the logic of Eq. 9 in the main text to generate complier potential outcomes. I run the following regressions:

$$H_{q,i}D_{0,i} = \sigma_q D_{0,i} + \alpha_q Z_{t,i} + \zeta_{8,s(i)y(i)} + \phi_{8,h(i)m(i)} + X_i\beta_8 + \epsilon_{8,i} \tag{A.11}$$

$$H_{q,i}D_{0,i} = \rho_q D_{0,i} + \gamma_q Z_{v,i} + \zeta_{9,h(i)y(i)} + \phi_{9,h(i)m(i)} + X_i\beta_9 + \epsilon_{9,i} \tag{A.12}$$

where I instrument for $D_{0,i}$ with $Z_{v,i}$ in Eq. A11 and with $Z_{t,i}$ in Eq. A12. The coefficients σ_q and ρ_q give the mean housing rate in quarter q, $\bar{H}_q$, for C_0^v and C_0^t compliers who receive no intervention, respectively. Then, to calculate mean housing rates for these compliers when they recieve a voucher or transitional housing, I add each respective treatment effect (as shown in Figure 4) to the means under no intervention.[1] I repeat this analysis for unknown housing status, replacing the outcome with $U_{q,i}D_{0,i}$. Finally, I compute the percent of compliers who are unstably housed in a given quarter as $1 - \bar{H}_q - \bar{U}_q$.

I show means over time for C_0^v and C_0^t complier groups in Appendix Figure A6. Echoing results from Figure 4, C_0^t compliers who enroll in transitional housing have the highest rates of stable housing in quarter 1 (days 0-90), as they can immediately enter transitional housing units. Meanwhile, C_0^v compliers who receive a HUD-VASH voucher have high rates of unstable housing in quarter 1, likely reflecting them searching for an apartment that accepts their voucher. Nevertheless, these compliers have the highest long term rates of stable housing. Compliers who recieve no intervention have very high rates of having unknown housing status, consistent with them dropping out of the VA homelessness support system. While some of these individuals likely find their own independent housing and thus may not need social worker services, others may be at particularly high risk of mortality.

[1] It is not possible to separately identify these means using complier characteristic regressions alone, as running regressions with $H_{q,i}D_{v,i}$ as the left hand side variable would yield a mixture between C_0^v and C_t^v compliers, as both of these complier groups are induced into v by Z_v (see Appendix Table A2 and Mountjoy 2022). However, following the logic of Eq. 9, Eq. A11 and A12 yield untreated potential outcome means.

Table A1: Unhoused Veteran characteristics compared to US unhoused population

	HUD statistics	Veteran sample
Veterans	0.04	1.00
Children	0.19	0.00
Male	0.60	0.90
Black	0.32	0.36
Hispanic	0.31	0.07

Note: This table compares the characteristics of Veterans in my sample to HUD statistics for the US unhoused population in 2024.

Table A2: First stage coefficients and propensity scores interpretation

Coefficient	Interpretation	Complier shares
α_v	$\Pr[D_0=1\mid Z_v=1,Z_t=0]-\Pr[D_0=1\mid Z_v=0,Z_t=0]$	$-(P(C_0^v)+P(C_0^{v+,t-})+P(C_0^{v-,t+}))$
α_t	$\Pr[D_0=1\mid Z_v=0,Z_t=1]-\Pr[D_0=1\mid Z_v=0,Z_t=0]$	$-(P(C_0^t)+P(C_0^{v+,t-})+P(C_0^{v-,t+}))$
$\alpha_v+\alpha_{vt}$	$\Pr[D_0=1\mid Z_v=1,Z_t=1]-\Pr[D_0=1\mid Z_v=0,Z_t=1]$	$-P(C_0^v)$
$\alpha_t+\alpha_{vt}$	$\Pr[D_0=1\mid Z_v=1,Z_t=1]-\Pr[D_0=1\mid Z_v=1,Z_t=0]$	$-P(C_0^t)$
π_v	$\Pr[D_v=1\mid Z_v=1,Z_t=0]-\Pr[D_v=1\mid Z_v=0,Z_t=0]$	$P(C_0^v)+P(C_0^{v+,t-})+P(C_0^{v-,t+})+$ $P(C_t^v)+P(C_t^{v-})$
π_t	$\Pr[D_v=1\mid Z_v=0,Z_t=1]-\Pr[D_v=1\mid Z_v=0,Z_t=0]$	$-P(C_v^t)$
$\pi_v+\pi_{vt}$	$\Pr[D_v=1\mid Z_v=1,Z_t=1]-\Pr[D_v=1\mid Z_v=0,Z_t=1]$	$P(C_0^v)+P(C_0^{v+,t-})+P(C_t^v)$
$\pi_t+\pi_{vt}$	$\Pr[D_v=1\mid Z_v=1,Z_t=1]-\Pr[D_v=1\mid Z_v=1,Z_t=0]$	$-(P(C_0^{v-,t+})+P(C_v^t)+P(C_t^{v-}))$
ψ_v	$\Pr[D_t=1\mid Z_v=1,Z_t=0]-\Pr[D_t=1\mid Z_v=0,Z_t=0]$	$-(P(C_t^v)+P(C_t^{v-}))$
ψ_t	$\Pr[D_t=1\mid Z_v=0,Z_t=1]-\Pr[D_t=1\mid Z_v=0,Z_t=0]$	$P(C_0^t)+P(C_0^{v+,t-})+P(C_0^{v-,t+})+P(C_v^t)$
$\psi_v+\psi_{vt}$	$\Pr[D_t=1\mid Z_v=1,Z_t=1]-\Pr[D_t=1\mid Z_v=0,Z_t=1]$	$-(P(C_0^{v+,t-})+P(C_t^v))$
$\psi_t+\psi_{vt}$	$\Pr[D_t=1\mid Z_v=1,Z_t=1]-\Pr[D_t=1\mid Z_v=1,Z_t=0]$	$P(C_0^t)+P(C_0^{v-,t+})+P(C_v^t)+P(C_t^{v-})$

Note: This table interprets the coefficients from saturated first stage regressions (Eqs. A1, A2, and A3) and maps propensity scores to complier shares. Column 2 interprets the coefficient in terms of propensity scores, and column 3 shows what complier shares these propensity scores correspond to. Complier groups are defined in Table 2.

Table A3: First stage coefficient estimates

Coefficient	Estimate
α_v	-0.052 (0.005)
α_t	-0.075 (0.003)
α_{vt}	-0.009 (0.007)
π_v	0.062 (0.005)
π_t	-0.014 (0.002)
π_{vt}	0.008 (0.007)
ψ_v	-0.010 (0.003)
ψ_t	0.089 (0.002)
ψ_{vt}	0.001 (0.005)

Note: This figure shows the results of the saturated first stage regressions (Eqs. A1 - A3). Standard errors are shown in parentheses. Given that all interaction terms are close to 0 and statistically insignificant, under my treatment choice model, the shares of C_0^v, C_0^t, C_v^t,and C_t^v compliers are given by $-\alpha_v, -\alpha_t, -\pi_t$, and $-\psi_v$, respectively.

Table A4: Robustness related to switching between treatment statuses

	First stage		3-year mortality		
	Voucher (Baseline)	Voucher in 12 months	Baseline	Treatment: voucher in 12 months	Drop those dual-enrolled
Voucher			-0.046	-0.046	-0.049
			(0.017)	(0.018)	(0.017)
Transitional housing			-0.007	0.001	-0.014
			(0.015)	(0.014)	(0.017)
Perc. receiving voucher ($Z_{v,i}$)	0.510	0.445			
	(0.017)	(0.019)			
Tr. liaison available ($Z_{t,i}$)	-0.013	-0.003			
	(0.002)	(0.002)			
	0.118	0.209	0.066	0.066	0.067
Observations	289,932	289,932	289,932	289,932	263,559

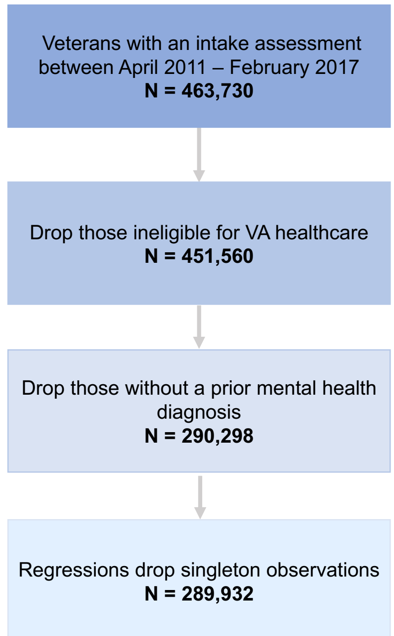
Note: This table shows robustness checks related to individuals switching between treatment statuses across time. In column 1, I show the baseline first stage results when $D_{v,i}$ – or initial voucher program enrollment – is the dependent variable. In column 2, I show the first stage when I re-define $D_{v,i}$ as receiving a housing voucher within 12 months of intake. In column 3, I show the baseline 2SLS results. In column 4, I show 2SLS results when I re-define $D_{v,i}$ as receiving a housing voucher within 12 months. In column 5, I drop those who initially enroll in 0 or tr who also receive a housing voucher within a year.

Table A5: Heterogeneity by low demand/bridge transitional housing

	(1) Baseline	(2) Above median low demand/bridge	(3) Below median low demand/bridge
Trans. housing	-0.007	-0.020	0.002
	(0.015)	(0.024)	(0.019)
Dep. var. mean	0.066	0.068	0.063
Observations	289,932	138,527	151,405

Note: This table shows the effect of transitional housing on mortality based on whether GPD beds are classified as low demand and/or bridge. VISNs are classified into above and below median based on the percent of their GPD beds that are low demand and/or bridge. Column 1 shows the baseline IV estimate, column 2 shows the estimate for above median VISNs, and column 3 shows the estimate for below median VISNs. Baseline controls include VA station × year, VA station × month, and indicators for having a prior inpatient stay related to homelessness or mental health. Standard errors for all regressions are clustered by VA station × year × month.

Figure A1: Sample selection



Note: This figure shows the sample selection process.

Figure A2: Change in treatment status given z_v, z_t for 7 complier groups

A: Treatment status given z_v, z_t

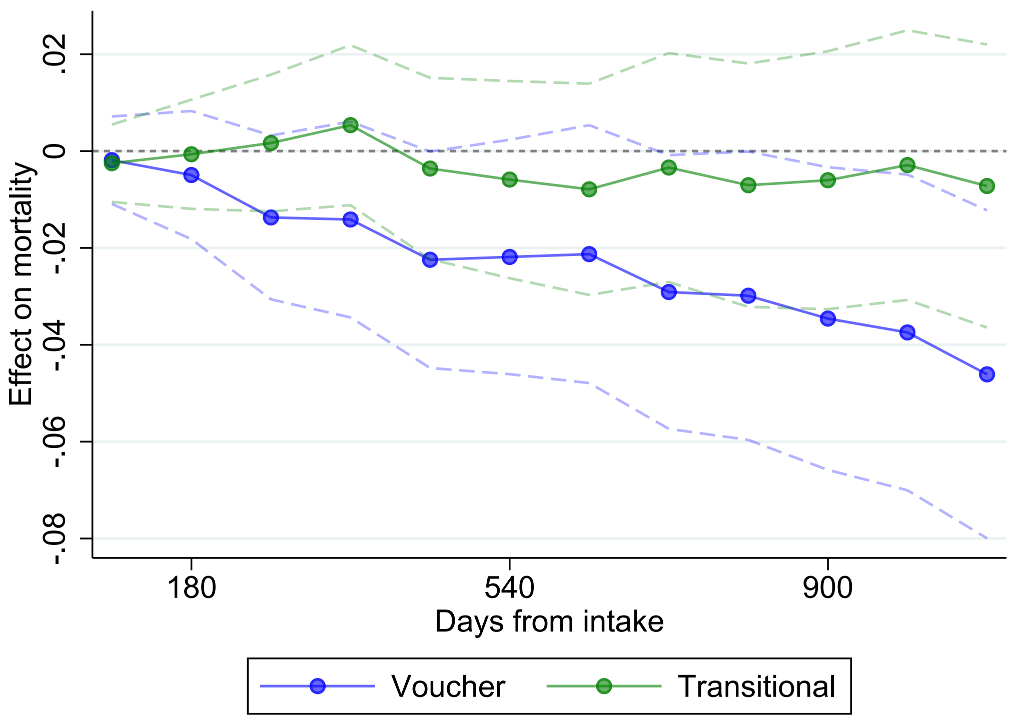
z_v, z_t	C_0^v	C_0^t	$C_0^{v+,t-}$	$C_0^{v-,t+}$	C_t^v	C_t^{v-}	C_v^t
0,0	0	0	0	0	t	t	v
1,0	v	0	v	v	v	v	v
0,1	0	t	t	t	t	t	t
1,1	v	t	v	t	v	t	t

B: Effect of change in z_v, z_t from (0,0) to (1,0)

z_v, z_t	C_0^v	C_0^t	$C_0^{v+,t-}$	$C_0^{v-,t+}$	C_t^v	C_t^{v-}	C_v^t
0,0	0	0	0	0	t	t	v
1,0	v	0	v	v	v	v	v
0,1	0	t	t	t	t	t	t
1,1	v	t	v	t	v	t	t

Note: This figure duplicates information provided in Table 2, highlighting how treatment status changes as values of the instrument change. Rows 1, 2, 3, and 4 show potential treatments for each complier group when z_v and z_t are both 0, when $z_v = 1$ and $z_t = 0$, when $z_v = 0$ and $z_t = 1$, and when both instruments equal 1, respectively. Panel B illuminates the effect of a change in (z_v, z_t) from (0,0) to (1,0). With this change, C_0^v, $C_0^{v+,t-}$, and $C_0^{v-,t+}$ compliers are induced from 0 to v, and C_t^v and C_t^{v-} compliers are induced from t to v.

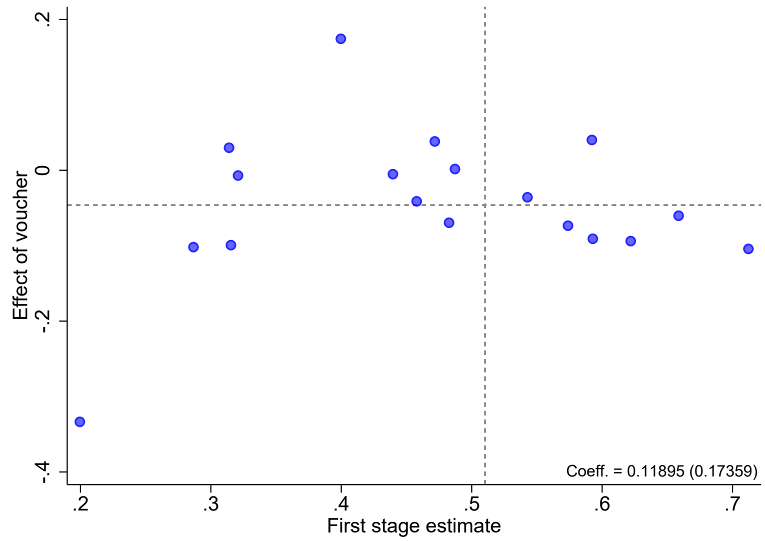
Figure A3: Mortality effect over time



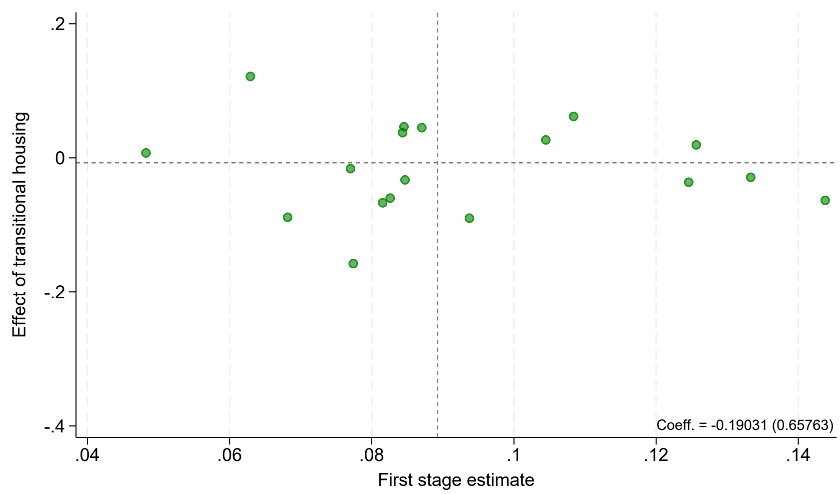
Note: This figure shows 2SLS regression results for the effect of the voucher program and transitional housing on mortality over time. Results for the voucher program are shown in blue, and results for transitional housing are shown in green. Dotted lines show upper and lower bounds.

Figure A4: Treatment effect heterogeneity by propensity to be treated

A: LATE vs. first stage for housing first



B: LATE vs. first stage for transitional housing



Note: This figure shows heterogeneity in 2SLS estimates for the effect of the voucher program on mortality (Panel A) and the effect of transitional housing on mortality (Panel B). In Panel A, I run first stage regressions for the voucher program separately by Veteran Integrated Service Network (VISN), collecting coefficients for the effect of $Z_{v,i}$ on $D_{v,i}$. Then, I estimate the LATE (for the effect of the voucher program on mortality) for each VISN, and plot the relationship between them. Similarly, for Panel B, I run the first stage for transitional housing separately by VISN and collect coefficients for the effect of $Z_{t,i}$ on $D_{t,i}$. Then, I estimate the LATE (for the effect of the transitional housing on mortality) for each VISN, and plot the relationship. Dotted lines show the overall sample first stage estimate (x-axis) and the overall sample 2SLS estimate (y-axis). The bottom right corner of each panel shows the regression coefficient for the relationship between IV estimates and first stages.

Figure A5: Natural language processing algorithm examples

A: Example note for stable housing

STABLY_HOUSED

History of present illness: << HISTORY_OF_PRESENT_ILLNESS >> Veteran is here to discuss HYPOTHETICAL his new apartment. EVIDENCE_OF_HOUSING

PmHX HISTORICAL PmHX: << PAST_MEDICAL_HISTORY >>

- Pneumonia

- Afib

- Homelessness EVIDENCE_OF_HOMELESSNESS

Housing situation IGNORE Housing situation: << HOUSING_STATUS >> The Veteran was homeless EVIDENCE_OF_HOMELESSNESS for the last 3 months HISTORICAL . Last week, he signed a lease EVIDENCE_OF_HOUSING for a new apartment EVIDENCE_OF_HOUSING

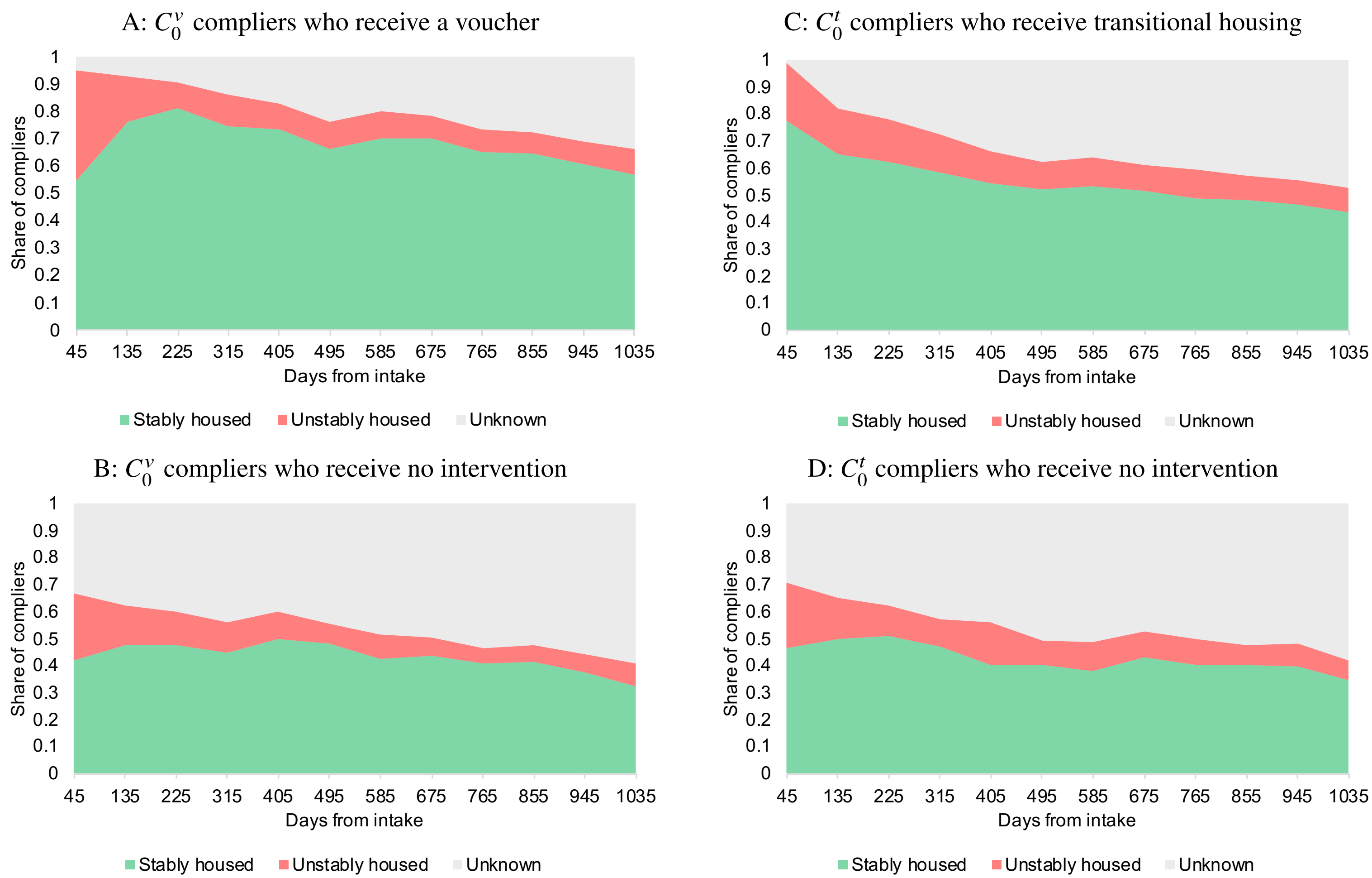
B: Example note for unstable housing

UNSTABLY_HOUSED

The Veteran came to the clinic today to discuss HYPOTHETICAL transitional housing EVIDENCE_OF_HOUSING . They have been living in RESIDES_IN a shelter TEMPORARY_HOUSING for the past 3 weeks.

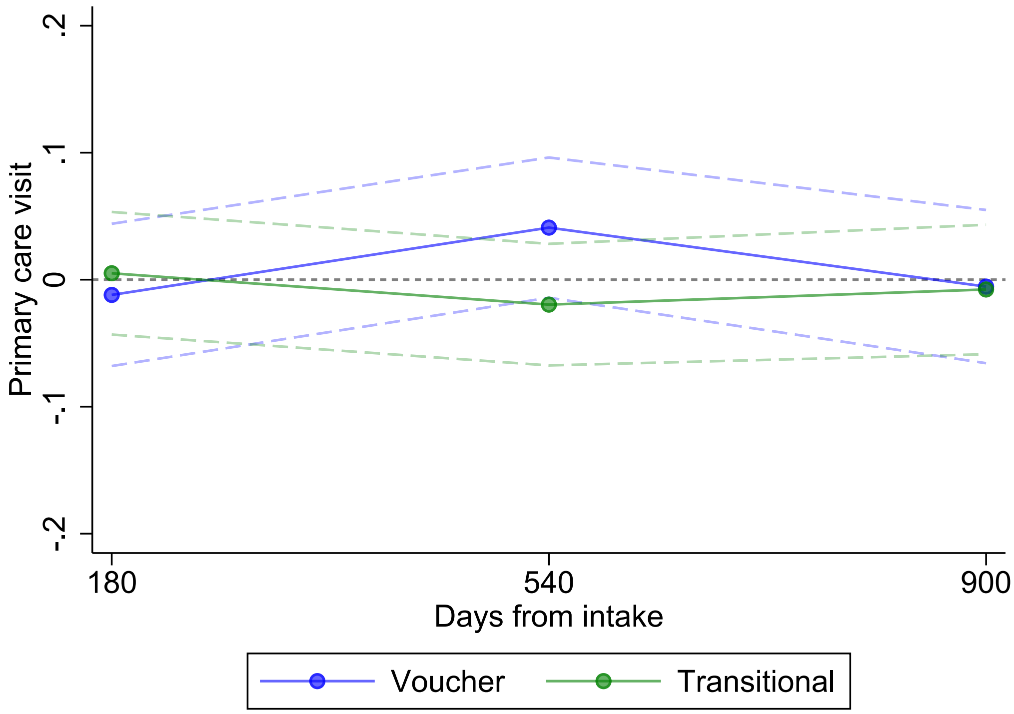
Note: This figure shows two example patient notes processed by the ReHouSED NLP algorithm. The algorithm highlights words either related to housing ("was homeless") or that provide context (such as being hypothetical or historical). Panel A is a note categorized as stably housed, and Panel B is a note categorized as unstably housed. Chapman et al. (2021) provides more precise rules on the logic of the NLP algorithm.

Figure A6: Housing status over time for compliers



Note: This figure shows mean potential housing status over time for C_0^v and C_0^t compliers who do and do not receieve the intervention. Green indicates the share of compliers who are unstably housed in the quarter, red indicates the share that are stably housed, and grey indicates unknown housing status.

Figure A7: Engagement with primary care over time



Note: This figure shows 2SLS regression results for the effect of the voucher program and transitional housing on engagement with primary care in years 1, 2, and 3 following intake (each year corresponds to one dot in the figure). Results for the voucher program are shown in blue, and results for transitional housing are shown in green. Dotted lines show upper and lower bounds.