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Do time-limited subsidy programs reduce homelessness for single adults?

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Abstract

This paper evaluates the impacts on future homelessness of time-limited subsidy (TLS) programs for single adults experiencing homelessness. These programs help individuals move into market rentals and financially support tenancy, with typical two-year time limits. Using 10 years of linked administrative data from Los Angeles, we find robust non-experimental evidence that enrollment in a TLS program reduces future use of homelessness services by 9.2 percentage points (off a base of 38.4%) through 4 years. This impact exists even though only 62% of participants receive a TLS-supported move-in. Positive impacts exist for Latinx, Black, and White participants, although they are smallest for Black participants. We also find that these impacts extend to populations at higher risk of future homelessness for whom policies prioritize offers of permanent housing with supportive services. Given constraints to expand permanent housing with supportive services, we find that TLS programs provide an alternative solution for single adults across a range of populations.

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1 Introduction

Over half a million people experience homelessness on a given night in the United States (de Sousa et al., 2022). The policy response for these individuals is a mix of short-term solutions (e.g. shelters) along with interventions to establish long-term housing stability. One of these longer-term interventions — time-limited subsidy (TLS) programs, often referred to as Rapid Re-housing (RRH) — has steadily grown from 7% of the "beds" counted nationally in 2013 to 24% of the beds in 2021.[1] TLS programs were originally designed to quickly rehouse individuals who were homeless due to a financial shock. The idea was that helping these individuals find a market-rate rental and temporarily subsidize their rent would stabilize their housing. However, evidence for whether this works is shallow (Byrne et al., 2021; Evans, Phillips, and Ruffini, 2021), and the program's effectiveness can be limited by implementation challenges (like documentation requirements), tight rental markets, and/or rejection by landlords. Finally, expansions are occurring alongside a competing alternative intervention — Permanent Supportive Housing (PSH) — which is not time-limited and intended for individuals with more complex needs.

We study the impact of TLS programs on the future housing stability of single adults experiencing homelessness in Los Angeles.[2] Using 10 years of linked administrative data from six county agencies, we estimate non-experimental impacts on the future use of homelessness services by comparing outcomes for 3,677 TLS participants to 29,843 individuals who entered a homelessness spell at the same time but were not enrolled in an intervention to establish long-term housing stability.[3] We rely on both weighting and event study approaches. The credibility of our strategy is assessed using placebo tests from the five years of pre-enrollment data for both designs, as well as the creation of lower-bound estimates from a positively-selected comparison group. The relevance of the findings comes from the four years of post-enrollment data that allow for an assessment of impacts beyond the length of the intervention, which is typically limited to two years. Given that TLS was

[1] We create this as the share of "Permanent Housing" beds that are "Rapid Re-housing" from the Housing Inventory Count: https://www.hudexchange.info/resource/3031/pit-and-hic-data-since-2007/ (last accessed 11/13/2023).

[2] Single Adults are defined as those aged 25 and older with no identified dependents.

[3] The outcome is defined as street outreach, or staying in a shelter, save haven, or transitional housing program.

originally designed to serve individuals who may only need temporary financial help, we also search for evidence of impacts for groups with varying needs. Specifically, we assess whether results from a risk model to predict future homelessness are correlated with potential barriers to housing, and then we estimate subgroup impacts of TLS by risk group.

We make two primary contributions. Our first contribution is that we provide robust evidence that TLS enrollment persistently reduces future homelessness over four years. The cumulative four-year impact is a 9.2 percentage point decrease in future use of homelessness services off a base of 38.4%. Further, annualized impacts show a decrease in future use of homelessness services for each of the four years in the follow-up period, providing evidence that the impacts extend beyond the length of the program. These impacts are consistent across both empirical strategies and are robust to placebo checks. Specifically, pre-period null effects are identified for the event study as well as for hold-out time periods when using weighting. Further, the findings hold when estimating impacts using a positively-selected comparison group, which we interpret as lower bounds, as well as when adjusting various analysis decisions. Importantly, these impacts are based on all individuals who enroll in TLS even though only 62% eventually move into a TLS-supported unit. The positive impacts also exist separately for Latinx, Black, and White participants, although they are smallest for Black participants.[4]

Our second contribution is that, proportionally, we find that TLS reduces future use of homelessness services equally across populations at varying risk of future homelessness. To do this, we first show that we can accurately predict the future use of homelessness services using a simple predictive analytics model. We then show that higher predicted future risk is associated with past health, mental health, legal-system involvement, and housing stability at the start of a homelessness spell. Finally, we show that the beneficial impacts of TLS extend to three terciles of predicted risk. Specifically, because the base rate of homeless service use is larger for higher risk groups, we estimate impacts in percentage terms and show that enrollment in TLS decreases the four-year cumulative rate of homeless services by approximately 25% across all three risk groups. Naturally,

[4]Other race/ethnicity groups were not included because sample sizes were insufficient.

because the highest-risk tercile experienced the highest base rate of future homelessness (58.4%), this tercile also experienced the largest level drops of future homelessness (14.4 percentage points) across all groups.

These findings inform the current debate around strategies to eradicate homelessness. Although some program guidance emphasizes that TLS should be made available to all persons experiencing homelessness (National Alliance to End Homelessness, 2016), that does not align with the initial motivation for the program (Gubits, Shinn, Bell, et al., 2015) and it may not align with current practice (Fisher et al., 2014). Without further evidence, policymakers may not consider TLS even though it is relatively easier to scale when compared to permanent supportive housing. Although there are areas for improvement, we provide evidence that TLS meaningfully reduces homelessness across populations with a range of needs, and it does this beyond the length of the time-limited program.

2 Background

Starting in the 2000s, policies to serve individuals experiencing homelessness have shifted from a treatment-first approach to a housing-first approach (Padgett, Henwood, and Tsemberis, 2016). In treatment-first models, preconditions have to be met, such as compliance with substance use treatment, before a longer-term offer of housing was made.[5] In the housing-first model, "rapid placement" into a permanent-housing unit is prioritized. After evidence of a successful housing-first approach for people with serious mental illness and long histories of homelessness was presented by Tsemberis and Eisenberg (2000), the policy landscape shifted under the idea that preconditions to permanent housing were too difficult, and stable housing was actually a precondition to improving other aspects of one's life (Padgett, Henwood, and Tsemberis, 2016). It is in this context that TLS programs were introduced at the federal level.

[5] There are some interim-housing programs that still have these requirements, like Transitional Housing, but they are not considered long-term solutions because the residential unit is time limited (Gubits, Spellman, et al., 2013; Burt, 2010).

TLS programs have steadily increased in relevance over the past decade. The first federally-funded TLS program was offered as the Rapid Re-Housing Demonstration Project by the U.S. Department of Housing and Urban Development in 2008, and the programs were largely expanded in the wake of the Great Recession by both the American Recovery and Reinvestment (ARRA) Act of 2009 and the Homeless Emergency Assistance and Rapid Transition to Housing (HEARTH) Act of 2009 (National Alliance to End Homelessness, 2014). Combined, these laws classified spending on TLS programs as allowable versions of "permanent housing" expenses and expanded their funding. Other laws have continued to include TLS provisions, and from 2013 to 2021, the number of TLS beds — the unit used to document inventory — steadily increased over 500% from 22 thousand to 137 thousand.[6]

Although there is no federally mandated program model for TLS, the National Association to End Homelessness (NAEH; 2014) identifies three core components. The first is housing navigation services to identify rental units. The second is a time-limited subsidy that can be used for security deposits, move-in costs, and/or rent. Importantly, this financial assistance is generally provided directly to a third party. Finally, the program should include case management to support housing stability. Although this guidance can be considered as a baseline, each program is locally designed and administered, and there is significant variation in implementation (Burt et al., 2016).

The current evidence suggests that individuals are positively selected for the program. A widely-accepted theory of TLS is that the program is most beneficial for people experiencing a short-term crisis or struggling with housing affordability. Because of this, administrators may offer it to individuals who they think can return to housing stability with a quick and minimal intervention (Gubits, Shinn, Bell, et al., 2015; Gubits, Shinn, Wood, et al., 2018). In particular, some TLS administrators may think the program is inappropriate for those facing long-term barriers to housing related to addiction, chronic homelessness, or the need for therapeutic residential care (U.S. Department of Housing and Urban Development, 2014). In practice, this may lead program administrators to

[6] Author's calculations from the Housing Inventory Count where TLS programs are identified as "RRH" project types. This includes all RRH-classified projects that are recorded, regardless of local or federal funding source. Note that "beds" refers to the count of housing resources available to individuals on a given night.

place preconditions on participation, such as demonstrating sufficient income to cover rent after the subsidy ends (Evans, Phillips, and Ruffini, 2021; Fisher et al., 2014; Shinn, Scott Brown, et al., 2017).

The primary housing-first alternative to TLS programs is Permanent Supportive Housing (PSH). PSH provides non-time-limited housing placements with wraparound services. The theory is that some individuals, particularly those experiencing chronic homelessness, have higher needs that require more intensive and long-term services (National Academies of Sciences and Medicine, 2018). At the national level, the overall number of PSH beds was 381,000 in 2021, representing a 27% increase since 2013. However, the share of housing-first beds that are PSH has fallen from 93% to 66% over the same period.[7] This drop in share of PSH aligns with the expansion of TLS over the last decade. PSH is still prioritized for individuals who enter the homeless-service system with high needs. However, the number of available PSH beds is an order of magnitude smaller than the annual homelessness count, and there are fiscal, political, and bureaucratic constraints to expanding PSH. This may place more pressure on alternatives that have fewer constraints on expansion – like TLS.

The remaining interventions to serve individuals experiencing homelessness are focused on addressing short-term need. The closest service to unsheltered homelessness is Street Outreach programs, which include basic sanitation, food, and connection to services, but also includes non-consensual street sweeps. Other services include Interim Housing programs that provide short- and medium-term housing. This includes congregate shelters, motel vouchers, and transitional-housing programs, among others. As previously mentioned, transitional housing is not permanent but can be intensive and have preconditions to enrollment, such as sobriety requirements (Burt, 2010). For any individual who has been served by a housing-first program, subsequent enrollment in any of these non-permanent homeless services represents a regression back towards homelessness.

[7] Authors' calculations using the Housing Inventory Count.

2.1 Effectiveness of TLS programs

Existing evidence on TLS programs is limited, particularly for single adults (Byrne et al., 2021). The most well-known evidence on TLS comes from the Family Options Study, where over 2,000 families with children were randomly prioritized to four treatment arms: a permanent-housing subsidy; a time-limited subsidy; transitional housing (that includes intensive supportive services); or a control group that could also access these services but without prioritization (Gubits, Shinn, Wood, et al., 2018). No intent-to-treat differences were found on housing outcomes between the TLS and control groups. However, there was meaningful non-compliance (58% of the TLS-assigned group and 22% of the control group received the TLS subsidy).

For Single Adults, there are three studies that estimate the impacts of TLS programs on housing outcomes. One experiment involved 236 single adults with HIV/AIDS in New York and found that TLS led to faster placement into housing compared to regular care (150 days vs 243 days; Towe et al., 2019). The other two studies relied on non-experimental matching methods that compared people who participated in TLS programs to people receiving homelessness services but who were not enrolled in TLS. The first study pooled 1,169 families and single adults who received TLS and used caliper matching on a propensity score to compare housing outcomes for people who were only receiving shelter services in Philadelphia (Taylor, 2014). They find a decrease of 26 percentage points (off a base of 39%) in "return to homelessness" within one year of entering the homeless-service system.[8] The second study included 117 single adults who received TLS and used one-to-one matching on a propensity score to compare housing outcomes for those receiving shelter-services only in Georgia (Rodriguez and Eidelman, 2017). They find a decrease of 25.6 percentage points (off a base of 39%) on returns to shelter within 2 years.

These studies make valuable contributions to the evidence on TLS, but they are limited in various ways. The primary limitation is the short-term outcome windows that do not allow for clear post-participation impacts of TLS.[9] The remaining limitations relate to being focused on

[8] The actual outcome of "return to homelessness" is not explicitly defined in the study.

[9] The exception is Gubits, Shinn, Wood, et al. (2018), which measures outcomes for 3 years, but this is for families and not single adults.

samples with particular needs (Towe et al., 2019), not isolating the impacts for single adults (Taylor, 2014), or having relatively small samples (Rodriguez and Eidelman, 2017). Our study builds on the extant literature by expanding the outcome window, increasing the data available to implement the non-experimental designs, proposing two independent identification strategies, including a series of placebo and sensitivity checks, and estimating impacts for policy relevant subgroups.

We want to note that this study is closely related to that done by Cohen (2023). That study uses the same data source and an overlapping sample of individuals from Los Angeles. The goal of that study is to estimate the pooled impact of "Housing First" interventions (TLS and PSH, combined) on homelessness and socioeconomic outcomes over a 30-month period. Their identifying strategy is an assumption of random assignment to caseworkers with different propensities to assign treatment options. Using that approach, they find a 15 percentage point reduction in returns to homelessness over a 30-month follow-up period. Although it is an important study for the field, we address a different question. Specifically, the policy distinctions between TLS and PSH are important in terms of cost, feasibility, and targeting.[10] Grouping these two programs into one category makes the policy implications less clear.[11]

2.2 TLS and Homelessness-Prevention Programs

The expansion of TLS programs has coincided with the expansion of homelessness-prevention programs (Culhane, Metraux, and Byrne, 2011). An important example of this is the Homelessness Prevention and Rapid Re-Housing (HPRP) program which was funded under the ARRA of 2009. HPRP is essentially a TLS program, but includes a prevention component, which means individuals do not have to be experiencing homelessness to be eligible (Housing and Development, 2016).

However, unlike TLS programs, homelessness prevention introduces a critical efficiency challenge

[10] There is also considerable quality variation in PSH programs in Los Angeles that is not well understood and make interpretations of impacts less actionable (Milburn et al., 2021).

[11] We also have a concern with the empirical approach. Although the study is well executed in providing an empirical case for exogenous assignment to caseworkers, we have strong reservations over the exclusion restriction. Specifically, caseworkers are service providers that vary in skill. We believe this skill is directly related to a participant's outcomes in addition to whatever housing-first intervention an individual receives. This assumption is not directly testable and their approach was to provide anecdotal evidence.

for targeting that has been recognized for over 20 years (Shinn, Baumohl, and Hopper, 2001). Specifically, homelessness is a rare outcome, and identifying individuals who are housed but at high risk of homelessness is difficult – even for those with housing barriers or low incomes. For example, in a study of individuals who were required to attend eviction court in Chicago and New York, the effect of an eviction on future use of homelessness services was a 3 percentage point increase on a base rate of 1% (Collinson et al., 2022). In a study of a homelessness-prevention program in Chicago, Evans, Sullivan, and Wallskog (2016) use a natural experiment to find that the program reduces the use of homeless services after 6 months by 1.6 percentage points off a base of 2.1%. The primary difference between prevention and TLS programs is that all individuals in a TLS program are experiencing homelessness, so efficient targeting is less of a concern, but program effectiveness is.

2.3 TLS programs in Los Angeles

Housing-first interventions in Los Angeles are oversubscribed. These resources are allocated in Los Angeles through the Coordinated Entry System (CES),[12] and of those entering the CES in 2019, only 11% were enrolled in a housing-first option within 12 months (7% in a TLS program and 4% in a PSH program).[13] In Los Angeles, 64% of housing-first enrollments were TLS in 2019. In contrast, at the national level, only 24% or permanent housing enrollments were TLS. This highlights the relative constraints of PSH in Los Angeles and the importance of TLS as a potential housing solution.

The policies to allocate resources to individuals in the CES are flexible. When an individual enters the system, they are assessed to determine if they are actually experiencing homelessness.[14] They are then given a manually administered triage tool in the form of a questionnaire, where higher

[12] The U.S. Department of Housing and Urban Development (HUD) groups the country into 392 geographic regions called Continuums of Care (CoCs). Each CoC has their own procedures to coordinate services which are partially funded by the federal government. In Los Angeles, the CES has a lead agency, the Los Angeles Homeless Services Authority (LAHSA), that partners with contracted service providers and other County agencies.

[13] Authors calculations using program enrollments in Los Angeles' Homelessness Management Information System (HMIS).

[14] HUD has specific requirements to classify an individual as experiencing homelessness, which generally includes sleeping in a location unintended for nighttime residence.

scores are meant to reflect higher vulnerability.[15] Scores on the triage tool are then used as an input to allocate resources, but discretion by system administrators and service providers is still allowed. For example, the scores on the tool range between 0 and 17 and are divided into score bands. During the time period of the study, either TLS programs or "light-touch" services were recommended for participants with scores from 0 to 7, TLS programs or PSH were recommended for scores from 8 to 11, and PSH was recommended for scores above 12. There are several reasons why assignments might fall outside of these scoring bands, including constrained supply and specific eligibility criteria for some resources, such as units funded to serve women fleeing domestic violence or veterans. Other than relatively few individuals being enrolled in a housing-first option, the distribution of scores generally reflects the policy, with TLS participants having lower scores and PSH participants having higher scores.[16] That said, there is considerable overlap across the distribution. Adding to the lack of distinction in these distributions is existing evidence that the triage score is not very effective at differentiating future risk of homelessness (M. Brown et al., 2018).

TLS programs in Los Angeles generally follow the program guidance of the NAEH. Once enrolled, TLS participants can receive flexible financial assistance to support tenancy for up to 24 months, with housing navigation and case managers working with participants to determine an appropriate exit point. There are multiple TLS providers in the space with various funding sources, so the exact details of each program differs.[17] Through conversations with local providers, we learned that the general cap on offered financial assistance varied between $6,000 to $8,000 per enrollment. For our study sample, the average financial assistance documented by caseworkers is $5,815 (see results section below). However, the official average per-person financial assistance amount was

[15] This is officially called the Vulnerability Index - Service Prioritization Decision Assistance Tool (VI-SPDAT), although at the time of writing it is being phased out in Los Angeles. A copy of this can be found here: http://ph.lacounty.gov/sapc/Event/HomelessServices/050318/CES-individuals-survey-packet.pdf (last accessed, 8/16/2023).

[16] For more details on the enrollment process, including distributions of scores across TLS, PSH, and no PH populations from 2017 and 2018, see Appendix A and Figure A2, in particular.

[17] For example, funding for TLS programs in Los Angeles County during the study period included Emergency Solutions Grants (ESG), Continuum of Care (CoC) funding, First Five funding from the state of California, Supportive Services for Veteran Families (SSVF) funding, funding from the LA County Department of Public Social Services, and city and county general funds, such as Measure H.

$7,280 in 2016.[18] We do not have estimates on total per-person program costs during the study period, but more recent estimates are $22,099, which includes all program and administrative components and may also reflect longer time-periods on aid that align with the start of the pandemic.[19] A participant's eligibility is regularly re-evaluated with monthly updates to establish: (1) the participant does not have an annual income that exceeds 50% of median income for the area, and (2) the participant lacks sufficient resources and support networks necessary to retain housing without TLS assistance.

2.4 TLS Barriers and Discrimination

There are bureaucratic, market, and societal reasons why TLS programs might not work. The largest evidence of barriers that we document is that approximately 40% of TLS-program participants never receive a TLS-supported move-in (see results section below). Even without an official move-in, participants may still receive some financial assistance, case management, and housing navigation services. However, conversations with TLS case managers suggest that participating in the program without a move-in is never a goal. Anecdotal evidence from providers for why move-ins may never occur include an inability to produce the required documents and an inability to identify a unit.

Related to identifying a unit, discrimination in housing markets has long been documented (Yinger, 1978), and evidence on discrimination in rental markets continues to be regularly produced (Ahmed and Hammarstedt, 2008; Early, Carrillo, and Olsen, 2019; Faber and Mercier, 2022; Gaddis and DiRago, 2021; Phillips, 2017). Differences in program experience have also been documented in Los Angeles for Black participants in the PSH program (Milburn et al., 2021), so understanding program impacts in this dimension is critically important, and we include an assessment of impacts for Black, Latinx, and White participants, which are the groups with sufficient sample sizes to support the design.

[18] This number was presented as an answer to a FAQ in 2016 to potential service providers: https://documents.lahsa.org/programs/funding/2016/CESRFPQ&A.pdf, last accessed 8/9/2023.

[19] This was shared with us by a current administrator of the Los Angeles Homeless Service Authority (LAHSA).

3 Data and Strategy

Here we discuss the data and strategy used to estimate impacts.

3.1 Data Sources

We use ten years of individual-level de-identified linked administrative data from LA County's Chief Information Office (CIO). This novel dataset is referred to as the "Information Hub." The origins of the Information Hub lie in a CIO project started in 2006 to link health services and benefits receipt data for adults in LA County. In subsequent years, the CIO and County agencies have worked intensively to forge legal agreements and build data-engineering pipelines to link administrative data from 11 County agencies into a regularly refreshed data warehouse. The resulting dataset is a critical piece of data infrastructure for both analytical and operational use cases in LA County, and includes health, mental health, social service benefits, sheriff arrests, parole, and homelessness service records for millions of individuals from 2010 onwards. For a more complete description of the included county agencies and data elements, see Appendix A.

3.2 Study sample

Identifying the study sample requires a number of researcher decisions. This is because entry into the homeless service system and assignment to interventions can be a non-linear process with occasional repeat engagement. Some individuals start engaging with the system through contact with outreach workers on the street, some through self-identification with service providers, and some through direct referral for specific interventions from other social services. Regardless of their entry point, all people should have their needs assessed with the triage tool. Even though we show below that not everyone is actually assessed with the tool, we build the study sample around the triage tool assessment since it represents a meaningful point where caseworkers should be considering specific intervention assignments. It is also the most accurate way for us to get a measure of time (the date of the assessment) and place (a person's location when completing the assessment), which are critical for any design.

We identify the TLS program group based on the timing of the triage tool assessment and the timing of enrollment in permanent housing interventions. The numbers for TLS study eligibility are identified in Panel A of Table 1. Specifically, we start with all Single Adults who enrolled in a TLS program during the 2016 and 2017 program years ($N = 7,103$).[20] For these individuals, the first exclusion we make is whether we can observe them ever being previously offered a housing-first intervention – either PSH or TLS enrollment – in the five years before the focal TLS enrollment (excludes $N = 998$, remaining $N = 6,105$). We do this because we are specifically interested in learning about how the intervention works for those being offered a housing-first intervention for the first time.[21]

We next require that these individuals have a triage tool assessment (81% of first-time TLS participants). For these individuals, we jointly apply the following conditions: First, we identify TLS participants as those who enroll in the program within 6 months of their triage assessment. We rely on the 6-month "enrollment window" because it reflects an expected time period within which someone may have been enrolled in the program based on their CES intake. Over 90% of TLS entrants with a triage assessment are enrolled within 6 months, but the tail of this distribution is long and suggests some enrollments might not be tied to the focal triage assessment. Importantly, we show that the results are not sensitive to this decision by presenting results from study samples that apply 3-month and 12-month enrollment windows. Second, we require enrollment in the CES, which implies receipt of some homeless service, on the same day or prior to the date of triage assessment. This ensures that an individual is tied to some services related to the triage assessment. Third, we exclude anyone that is subsequently enrolled in an additional housing-first program (either PSH, or another TLS program), during the 6-month enrollment window. This is meant to ensure that impacts are not bolstered by receipt of multiple interventions within the enrollment window.[22]

[20] Program years begin on July 1st of the year and end on June 30th of the following year to align with funding cycles.

[21] We believe this allows for a cleaner estimate of the impacts of the program – although our main sample does not make any exclusions (from TLS or comparison group) on future enrollments in housing-first programs.

[22] It should be noted that a post-pandemic strategy that is being locally considered is to use TLS as a "bridge" to PSH, but that is less relevant for enrollments during the study period.

Finally, we require all participants to have complete demographic data as reported in the assessment because this is important for the selection-on-observables approach. Combined, 60% of first-time TLS participants are included in the study sample for a total of $N = 3,677$.

Although this excludes a meaningful number of participants, we believe this trade-off is necessary given the value of relying on the triage assessment and its completeness. Although most of these exclusions are made due to missing data in the homeless-service system that makes the design untenable, the integrated data allows us to compare the included and excluded samples using connections to other County agencies from the time of actual TLS enrollment (instead of the triage tool). When doing this, it appears as if the included sample is slightly negatively selected based on more involvement with county agencies (See Table A1 in Appendix B), but the differences are relatively small, with a few exceptions. For example, the included sample is more likely to identify as female (35% vs 30%) and more likely to claim earned income (29% vs 24%). Taken together, we move forward with the smaller sample because it will result in more internally valid estimates and the differences with excluded groups is not large.

We identify the comparison group in a similar way. First, we consider the intake period based on the timing of the triage assessment. Applying the same 6-month enrollment window, we include CES entrants with a triage assessment from January 1, 2016 through June 30, 2018 to match when the TLS program group could have taken the assessment. We also apply the same relevant restrictions of no previous housing-first intervention (for five years) before the triage assessment, not permanent housing intervention up to 6 months after the triage assessment (i.e. during the enrollment window), CES entry on or prior to the day of the assessment, and complete demographic data. These eligibility restrictions result in a non-Housing-first comparison group of $29,843$ individuals.

For more details on sample creation, see Appendix B. Importantly, for both the TLS program and comparison groups, additional permanent housing options could be received after the 6-month enrollment window. In fact, 7% of the study-eligible comparison group eventually received PSH 6 months (or later) after their triage assessment (not shown) while 6% of the study-eligible TLS group received PSH after 6 months (or later) after their triage assessment (see results section below).

3.3 Outcome

The outcomes for the study are binary indicators for enrollment in Street Outreach or Interim Housing projects over different time periods.[23] Other studies have focused on just emergency shelter Rodriguez and Eidelman (2017) or were more inclusive of all homeless services Taylor (2014), but we focus on Street Outreach and Interim Housing because they imply that an individual has become homeless again after they were offered permanent housing. For simplicity, we will refer to this outcome simply as "homeless services." Using this outcome, we create annualized binary indicators for five years before the triage assessment and four years after the triage assessment, as well as a cumulative measure of this after assessment. The benefit of annualized measures is that they allow for estimates of long-term impacts that can occur after program participation, while the benefit of a cumulative measure is that it provides a summative impact measure over the available outcome window.

Similar to the study sample, we define outcome periods relative to the six-month 'enrollment period' following the triage assessment date. Once an assessment has been completed, an individual can be offered various services to help them work towards a viable housing-first solution. This can include Interim Housing options as a way to stabilize individuals during their TLS enrollment. Because of this, short-term receipt of Interim Housing is less meaningful as an outcome measure since it is common to provide it while individuals work toward identifying a unit. Because of this, we exclude the six-month enrollment window before we consider additional services that are more likely to reflect regressions to homelessness. This is consistent with guidance we received from local administrators, policymakers, and providers to define 'sustained homelessness' as returns to homelessness more than six months after assessment. Recall, we verify that the findings are not sensitive to this enrollment window by re-defining outcomes based on 3-month and 12-month enrollment windows.

[23] Interim housing projects include emergency shelters, transitional housing, safe haven, and day shelters.

3.4 Identification Strategy

We rely on two distinct identification strategies. Our first strategy is an event-study design.[24] Using this selection-on-unobservables approach, we estimate the impacts of TLS by comparing program participants to all individuals who entered the CES over the same period. Specifically, we estimate the following model:

$$y_{it} = \eta_i + \gamma_t + \sum_{t \neq -5} \tau_t \times \text{TLS}_i + \varepsilon_{it}, \quad (1)$$

where y is a binary indicator for receiving street outreach or interim housing for individual i in time period t, η is an individual fixed effect, γ is a time fixed effect, τ is a time-period impact of enrollment in a TLS program relative to 5 years before taking the triage assessment, TLS is an indicator for enrollment in the program, and ε is an error term. In this specification, values of τ before the assessment reflect tests of the parallel assumption and after the assessment reflect annualized impacts.

Given the expected positive selection into the program, the strength of this design is our ability to include individual-level fixed effects and assess the parallel-trend assumption in the pre-period. However, we have two concerns. The first is that experiencing homelessness is not as prevalent in the pre-period, so assessing parallel trends during that time provides a relatively limited test. The second is that we are assuming parallel trends for a rare binary outcome with positive selection. Because binary outcomes are bounded below, the parallel nature of shifts between populations with different outcome propensities might not hold.

Our second strategy applies entropy-balancing weights to construct a comparison group that is exactly balanced on a wide range of covariates (Hainmueller, 2012). Estimation is straightforward

[24] There is an active literature on the methodological issues with event studies, or 'staggered adoption difference-in-differences' designs (Sun and Abraham, 2021; Borusyak, Jaravel, and Spiess, 2021; Baker, Larcker, and Wang, 2022). The problem in such designs arises when comparisons are made between newly-treated and already-treated units. In the presence of heterogeneous treatment effects, this problem can lead to a range of issues, including false affirmation or rejection of the parallel trends assumption and biases in effect estimates. Our study avoids these issues by using only 'clean controls' – observations which never receive the treatment at any point in the study period – and thus ruling out 'forbidden' comparisons between newly-treated and already-treated units.

in that impacts are estimated as a difference-in-means between the TLS and weighted comparison groups with inference performed by bootstrap.[25] We prefer this approach over other matching and weighting estimators because it focuses on balancing covariates as the primary goal rather than the two-step process of estimating propensities and then assessing balance for the resulting design.[26]

The selection-on-observables assumption is motivated by the complexity of prioritizing individuals for TLS programs in the CES and the likely randomness that is introduced during enrollment – particularly because demand for housing far outstrips supply. Some of the randomness comes directly from the system – including the triage tool and associated program-recommendation policies – while some comes from individual preparedness and the bureaucracy of provider workflows. Anecdotally, the timing of when someone arrives and if they have the needed documentation can influence their access to a caseworker and their experience entering the system. Finally, as we provide evidence for below, the triage tool itself does not accurately measure the risk of future homelessness spells, so that introduces further randomness into assignment to the extent that scores influence resource prioritization.[27]

To bolster our justification of conditional random selection, we also include a rich set of variables that align with theories of housing stability (Batterham, 2019; Corinth and Rossi-de Vries, 2018; Early, 1999; Early, 2004; Early, 2005). We include at least one measure for each of the seven factors identified in these theories. These factors include: housing markets, social stratification, labor markets, relationships/social capital, support through institutions, health and well-being, and past experience of homelessness. We start with rental markets by stratifying individuals within three geographic regions and including indicators for quarter and year of the triage assessment.[28] We then include four categories of variables to cover the remaining factors. The first is demographics, and includes self reports of: age, female, Latinx, Black, White, or reporting a disability. The second

[25]This includes 1,000 iterations around both the weighting and impact estimation steps.

[26]The resulting weight can technically be described as a function of the propensity score, and it has been shown to have beneficial properties, including being "doubly robust" (Zhao and Percival, 2017).

[27]To be clear, we assessed a regression discontinuity design around the program-recommendation policies and concluded that it was not viable. First, the small number of discrete scores makes model implementation limited. Second, there are no empirical discontinuities in the first stage.

[28]The regions include Central and South Los Angeles; the Westside and South Bay; and the Valleys.

is responses to the triage assessment, and includes: the triage score, an indicator for self-reported income, indicators for having an email or phone, and indicators on relationships and social capital.[29] The third is involvement with various county agencies, and includes flexible historic indicators for: enrollment in the social social safety net;[30] involvement with the criminal legal system;[31] health and well-being;[32] as well as past experiences of homelessness as measured by five years of annualized pre-assessment enrollments in Street Outreach or Interim Housing (our homeless-service outcome). The final category is project-level historic homeless service utilization aggregated to the project level.[33] This last category is critical as it provides a measure of the level of need within each organization where services are delivered.

The strength of this design comes from our ability to demonstrate the similarity of a weighted comparison sample on a wide range of policy, institutional, and individual-level characteristics. We first demonstrate the empirical equivalence on the full set of characteristics once entropy balance weights are applied. We then build familiarity with the findings by progressively including covariates starting with an unadjusted difference-in-means to the full entropy-balanced estimates. Finally, we introduce placebo tests using two holdout periods. Specifically, we assess impacts of the program before the timing of the triage-tool assessment by withholding either one pre-enrollment year or two pre-enrollment years of involvement with county agencies when conducting entropy balancing. This allows us to test for impacts on the primary outcome (enrollment in Street Outreach or Interim Housing) in the two years before triage-tool assessment when we would expect a null effect. These tests are analogous to assessing impacts in the pre-enrollment periods for the event study design, and we present the results from both estimates together.

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[29] These are binaries created from the following questions: "Do you have planned activities, other than just surviving, that make you feel happy and fulfilled?", and "Is your current homelessness in any way caused by a relationship that broke down, an unhealthy or abusive relationship, or because family or friends caused you to become evicted?"

[30] This includes receipt of the Supplemental Nutrition Assistance Program, Temporary Assistance for Needy Families, and General Relief.

[31] This includes Sheriff bookings and parole.

[32] This includes emergency/inpatient, outpatient, crisis stabilization, and non-crisis services in Department of Health Services (DHS) or Department of Mental Health (DMH) facilities, as well as indicators for Elixhauser or Charlson comorbidity diagnoses, diagnoses of serious mental illness (SMI), and diagnoses related to substance use disorder.

[33] In the system, a "project" refers to a specific grant or organization to provide a specific program.

The holdout tests are particularly important in our positive-selection context because balancing designs that rely on pre-intervention outcomes may reflect mean-reversion. For example, if the balancing estimator effectively captured random deviations from the comparison-group distribution rather than a true sample that matched the TLS population, then we would expect the distribution to 'snap back' in the periods that were not used for weighting. By estimating models that exclude pre-assessment service outcomes, we are able to test the mean-reversion challenge directly.[34] To be clear, these 'holdout tests' are technically not a completely faithful simulation of a data collection scenario occurring one or two years prior to assessment since we are still including information from the time of assessment. However, the variables we include from the assessment – including location – are central to our research design and, by excluding them, the tests would not provide a meaningful assessment of the entropy-balancing design.

The other main threat to the validity of both our event-study and entropy-balancing designs would be selection into TLS based on unobserved individual-level "shocks" during the enrollment window. For example, if an individual fails to enroll in TLS due to a personal crisis shortly after the time of assessment, or if a provider is more likely to enroll individuals in TLS who have independently found a more stable living situation, then our impact estimates may be optimistically biased if those events also affect longer-term homelessness. To address this concern, we create lower-bound estimates by purposefully identifying a positively-selected comparison group. Specifically, from the comparison donor pool, we identify $N = 3,220$ people who indicated that they exited homelessness in a non-institutional housing situation during the six-month enrollment window, which is a positive outcome.[35] By contrast, only 45% of the TLS study sample reported this outcome. Focusing on this subset of the comparison group, we re-estimated the entropy-balancing design and interpret the results as lower-bound impacts given the comparison group is positively selected after the design

[34] To implement the holdout models, we recreate the county-service covariates by always including a binary for the year before the holdout year and a second binary for the remaining years before the holdout year. For example, when the holdout period is 2 years, for every service variable, we create an indicator for year = -3 and a second indicator for years -4 and -5 combined.

[35] This includes situations such as living with family or friends, staying in a hotel or motel without a voucher, staying in a market rate rental, and so on. For a full list, see the HUD documentation for Living Situation Options in https://files.hudexchange.info/resources/documents/HMIS-Data-Dictionary-2024.pdf.

was implemented.[36]

3.5 Identifying High-needs Populations

In order to test theories of targeting for the program, we need to estimate impacts of the program for individuals with varying needs. In particular, the TLS model is grounded on the idea that some people only need short-term rental market assistance to stabilize their housing. However, we know the program is being provided to a broader population of individuals – some of whom may need more intensive case management services to overcome housing barriers, such as those who may be prioritized for PSH. Clearly, TLS programs are not a direct substitute for PSH, but as long as the supply of PSH is constrained, we should test whether the program works for this broader population.

An immediate challenge to estimating impacts by need is grouping individuals by need. The obvious option would be to use the triage score (i.e. the VI-SPDAT), which is the 'official' measure used for prioritization of homelessness services during the time period of the study. However, the triage score has been shown to perform poorly in predicting future homelessness (M. Brown et al., 2018), and we empirically verify this in the results section below.

Instead, we use a simple predictive analytics approach to group TLS program participants into three risk levels of future homelessness. To start, we limit a "training sample" to comparison individuals in the study that were not offered a permanent housing intervention. This is to remove the potential treatment effect of TLS or PSH on any future outcomes. We then define the outcome as an enrollment in Street Outreach or Interim Housing in the year following the enrollment window, which aligns with the year 1 outcome for the study. Next, we train a logistic regression model using the same set of covariates used for entropy-balancing with out-of-sample predictions using 10-fold cross-validation.[37] We then apply the resulting predictive model to the full study sample and split

[36]We thank David Phillips for suggesting a bounding approach to deal with potential bias due to unobserved shocks (see Brough, Phillips, and Turner, 2023 for an example).

[37]We also estimated random forest and gradient boosting (XGBoost) models, but the predictive performance was similar across the three (AUC around 0.65), so we chose the logistic model for simplicity.

the full sample into terciles that include both comparison and TLS program participants.[38] Next, we empirically assess whether these terciles of future homelessness risk are effective at separating the sample by need relative to the triage tool. Finally, we estimate impacts by risk tercile using the entropy-balancing approach.

4 Results

We present results in the following section. First, we describe the TLS program experience as recorded in homeless-service administrative records. We then present the characteristics of the TLS and comparison groups before presenting impacts on future homeless services. We then present the results from placebo tests for the two strategies, the influence of design decisions, and present the lower-bound estimates. After demonstrating the robustness of our approach, we then present subgroup impacts for Black, Latinx, and White participants. Next, we turn to impact estimates by predicted risk levels. We first provide evidence that a predictive risk model can meaningfully distinguish the sample by characteristics that are associated with potential housing barriers. Finally, we present impact results by risk level.

4.1 Program participation

Study eligibility (Panel A) and homeless service outcomes (Panels B and C) for TLS study participants are presented in Table 1. Here we see that 81% of first-time TLS participants completed the triage assessment, which implies that we have to exclude nearly 20% of participants for this requirement alone. This drops another 8 percentage points (to 73%) when limiting the sample to those enrolled in TLS within 6 months of the assessment. Looking across all study eligibility criteria – including enrollment in other permanent housing interventions – only 60% of first-time TLS participants remain in the study. Although this is a meaningful decrease in the TLS sample, we believe

[38] Although endogenous stratification can lead to impact estimates that are biased due to over fitting (Abadie, Chingos, and West, 2018), our cross-validation approach, along the lines of the one proposed by Harvill, Peck, and Bell (2013), avoids these issues by ensuring that the risk score is generated out-of-sample for each observation.

these criteria are critical for the internal validity of the research design. Further, as described above, the excluded participants are not meaningfully different when comparing engagement with other County agencies from the time of TLS enrollment (see Appendix B).

TLS participants experienced a wide range of program outcomes. In Panel B of Table 1, we can see that only 62% of TLS study participants had evidence of a TLS-supported move-in.[39] Although participants without a move-in may receive other benefits from case management and housing navigation, this clearly reflects barriers to program implementation. As expected, many individuals (20%) are jointly enrolled in Interim Housing within the first 6 months of the triage assessment which we interpret as a natural progression of services. We also note various co-enrollments with other homelessness-services. Forty-five percent of individuals receive some service from multiple providers, some individuals have multiple TLS enrollments within 6 months (5%), and some have a TLS re-enrollment after the 6-month enrollment window for the study. Finally, we can see that 6% of TLS participants are enrolled in a PSH program after the 6-month enrollment window as well. Although we include these individuals in the study because the additional enrollments occur after the 6-month enrollment window, we want to note that 7% of the comparison group also is enrolled in PSH after the enrollment window (not shown). We also use these additional enrollments as motivation to run sensitivity checks on the main findings where we progressively exclude TLS participants with more services to see if these individuals are driving any results.

Panel C of Table 1 presents distributions on continuous outcomes. The average number of days from triage assessment to TLS enrollment is 40 days for the study sample with half enrolling within 15 days. The average number of months enrolled is 8.9 and 75% of participants are enrolled for 12 months or less. Although there is a long tail, 95% of participants are enrolled for 25 or fewer months. We also present the distribution of recorded financial assistance provided to participants with evidence of a move-in. It is important to note that these numbers reflect case manager entries – not the system to record actual money sent to third parties, which is unavailable to us – so they can include under-reporting or data entry errors. The recorded amounts also represent a wide

[39] Evidence of a move-in is defined as administrative records indicating a move-in date or record of rental assistance.

distribution, with average financial assistance of $5,783 and a median value of $4,083.[40]

4.2 Sample Characteristics

The TLS program group is positively selected across a range of characteristics before applying entropy balancing. Table 2 presents characteristics grouped by demographics, intake information during the triage assessment, historic individual-level outcomes from administrative records across six county agencies, and project-level characteristics by providers. Across all 49 characteristics, there are only two that do not have a statistically significant difference at a 99% level of confidence. This is partially related to sample size, but the differences are economically meaningful as well. For example, TLS participants are less likely to self-report a disability (62% vs 72%), more likely to report having earned income (23% vs 7%), more likely to have a contact email (31% vs 18%), and more likely to have a phone (74% vs 56%).

TLS participants are also less likely to engage with other county agencies, and this could reflect fewer needs for support overall. In the year before assessment, this includes being less likely to have an emergency room visit (9% vs 18%), less likely to receive an Elixhauser/Charlson comorbidity diagnosis (8% vs 12%),[41] less likely to receive a Substance Use Disorder (SUD) diagnosis (9% vs 17%), and less likely to have a Serious Mental Illness (SMI) diagnosis (12% vs 21%). To be clear, these measures only reflect engagement with county agencies so they represent lower bounds. TLS participants were also less likely to have criminal legal involvement as represented by a Sheriff booking in jail or being on parole (18% vs 35%). Finally, at the project level, TLS program providers are less likely to serve individuals who had historically received the homeless service outcome in each of the five years before the focal triage assessment. However, after entropy balancing, all differences across the TLS program and weighted comparison groups are precisely zero.

[40] There were five entries over $50,000, but we exclude them from all summary statistics because they likely reflect data entry errors.

[41] These reflect two separate measures of comorbidity that we pool using available medical codes.

4.3 Impacts of TLS program enrollment

We present the impacts of being enrolled in a TLS program relative to the comparison group in Table 3. All estimates should be interpreted as outcome means for TLS program participants minus means for the comparison group. To build confidence in the impact estimates, we start with unadjusted difference-in-means estimates in the first column and progressively add covariates in OLS models until we get to the two preferred entropy-balanced and event-study models. Starting with the difference-in-means column, positive selection is clearly observed in the pre-assessment time periods with TLS participants being 2.3 to 3.9 percentage points less likely to receive homeless services in any of the pre-assessment years. The next two columns provide impact estimates for OLS models when including the assessment-only or full-set of baseline characteristics from Table 2. Notice how the differences either grow or stay roughly the same in the post-assessment period when including just the variables from the assessment, while the differences start to decrease when including the full set of covariates. This provides evidence on the additional value of the integrated dataset in explaining differences across groups. Next, the entropy-balanced column verifies the removal of differences in the outcome during the pre-assessment time periods (by construction) and presents impact estimates that are the smallest across all columns. Finally, the event-study column shows evidence of the parallel trends assumption holding in the pre-assessment period and impact estimates that are larger than entropy-balancing in the post period (the exception is the -1.3 percentage point difference in Year $= -3$). To help interpret the magnitude of these impacts, we provide the base rate for each year in the final column that is constructed by applying the entropy-balanced weights to the outcomes for the comparison group. Relative to these weighted base rates, the impacts are relatively large and represent over a 25% reduction in the outcome across years. Importantly, the annualized impacts persist into years 3 and 4, which is beyond the expected length of the program. Finally, the cumulative entropy-balanced impact across all four years is a decrease in the outcome by 9.2 percentage points off a base of 38.4%, reflecting a 24% decrease in the outcome over the four year period.

To assess the validity of the two preferred designs, we introduce placebo tests and compare the resulting impact estimates. Although the event-study has naturally built-in placebo tests from the pre-assessment period, we introduce placebo tests for the entropy-balanced models by excluding pre-assessment service variables for either 1 or 2 holdout years before estimating annualized impacts. Results from these estimates are grouped by year from assessment and presented in Figure 1. We observe two take-aways from this figure. First, confidence intervals around the impact estimates in the pre-assessment years are generally inclusive of zero. The exception is the event-study impact in Year $= -3$, but it is inclusive of zero in years -2 and -1 when the incidence of the outcome rises appreciably. Specifically, the base rate of the outcome is 9.5% in year -2 and 31.2% in year -1, and the impact estimate is indistinguishable from zero during this time. We interpret this as strong evidence that both models are working as intended. Second, in the post-assessment years, the impacts are all similar in magnitude across the different models, but they are generally smallest when applying entropy-balancing to the full set of pre-assessment variables. Taking this evidence together, we favor the entropy-balancing model given that the holdout tests provide evidence that regression to the mean is not an issue and the estimates are the most conservative.

These results are not sensitive to TLS participants that may have received more intensive interventions, researcher decisions on the enrollment window, or alternative outcomes to measures homelessness. For those that may have had more intensive TLS interventions, as noted above, some individuals were enrolled in the program for over 24 months (or had an unknown time of enrollment). We also noted that some individuals re-enrolled in a TLS program after the 6-month enrollment window, while others enrolled in PSH. Although this information is revealed after the enrollment window, to assess if these groups were driving the results, we progressively excluded them from the TLS program group to see if the entropy-balanced differences hold. The results are qualitatively unchanged (Table 4). For research decisions on the enrollment window, we rebuild the analysis sample and definition of outcomes using either a 3-month enrollment window or a 12-month enrollment window. Again, the results are qualitatively unchanged (Table 4). Finally, we estimate the entropy-balanced model using two alternative outcomes: (1) Street Outreach or shelter, or (2) any homeless service except for housing-first interventions. The results are again qualitatively the same for Street Outreach or shelter, but are somewhat muted for any homeless service (Table 4). That said, this most inclusive definition includes a "services only" project type that represents a base rate of 15% on its own. Because this includes light touch services that are not directly related to housing, we do not think they detract from the main findings.

Finally, we estimate impacts using a positively-selected sample from the comparison group to represent a lower-bound on the findings. The positively-selected subgroup represents 11% of the overall comparison group that reported exiting homelessness to a non-institutional setting within the enrollment window. Because all individuals in this subset self-reported a stable housing situation during the enrollment window, we argue that any impacts are likely to represent an underestimate, or a lower bound, on the true treatment effect. Applying the entropy-balancing design, we find that the overall impacts are in line with the overall findings (final column of Table 4).[42] Specifically, TLS reduces future homelessness over a four-year window by 7.4 percentage points off a base of 36.6%. This is a 20% reduction and is similar to the 24% reduction for the full sample. We also observe statistically significant reductions in homelessness for years 1 and 2 that are similar in magnitude to the full sample, but smaller and not statistically significant impacts for years 3 and 4. As we show and discuss more below, it is noteworthy that the pattern of results for this positively-selected sample are similar to the impacts we identify for the low-risk tercile. Overall, however, we find this evidence reassuring that the impacts of TLS are robust to the construction of our sample.

4.4 Subgroup impacts by race/ethnicity

We next turn to impact results by race and ethnicity. Given well-documented discrimination in the housing market, it is important to assess the impacts of this market-focused program for individuals from different race/ethnicity groups. Because of the data-intensive requirements of the approach, we estimate impacts for the subgroups with sample sizes that can support the design. We end up estimating impacts for Latinx, Black, and White participants, but not for Asian, American Indian or Alaska Native, or Native Hawaiian or Other Pacific Islander participants.

[42] Although we do not present the baseline characteristics for the sensitivity comparisons, the entropy-weighted baseline differences were precisely zero across all characteristics as they were for the main analysis sample.

To help interpret these impacts, we present base rates using the entropy-balanced comparison group for each time period and subgroup as well as estimate impacts divided by each group's base rate. Impacts divided by the base rate can be interpreted as the percentage increase/decrease in the incidence of the outcome for each group. We find this useful when comparing impacts across groups because the incidence of homelessness varies and this is a way of normalizing those impacts. Finally, we estimate statistical tests of equality across these normalized measures to identify differences across groups.

Impact estimates by race/ethnicity from entropy-balancing models are presented in Table 5.[43] Looking across groups, the pattern of annualized and cumulative impacts are qualitatively similar for all three groups when compared to the overall results. Specifically, each group experiences a decrease in the outcome for each of the four years and experiences a statistically significant cumulative decrease across all four years. However, some differences emerge across groups when looking at impacts normalized by each group's base rate. For example, Latinx participants experienced the largest reduction (50%) in homeless-service use in the first year, which was statistically distinguishable from both Black and White participants. Although the differences in annualized impacts largely went away after the first year, they remain statistically significant for the 48-month cumulative impacts. Specifically, over four years, TLS enrollment reduced homeless service utilization for Latinx participants by 30.6%, while only reducing use for Black participants by 19.1% – a difference that is distinguishable at a 95% level of confidence. Finally, we want to note that these differences cannot be explained by differences in move-in rates. The move-in rates for Black, Latinx, and White participants were 62.5%, 63.8%, and 59.9%, respectively. This implies that Black participants were similarly successful in moving into a TLS-supported unit relative to Latinx participants, but they were still more likely to need homelessness services in the future.

[43]Summary statistics for each TLS and comparison subgroup by race/ethnicity, including differences before and after entropy balancing, are provided in Appendix C.

4.5 Impacts by level of need

We now turn to estimating impacts by level of need. We start by presenting the value of using a predictive analytics model to group individuals in the study as compared to using triage scores. Recall, the predictive model uses a logistic regression to estimate the future use of homelessness services in the first outcome year. Study population terciles are then created using the estimated model and we compare them to the three triage-score groupings from prioritization policies that were in place during the study period. The first piece of evidence we provide is trends in homeless services for TLS participants when using the two groupings in Figure 2. The left figure shows groupings by triage score bands. Although those with scores of 0 to 7 are sometimes distinguishable across the trend, they are largely overlapping. The right figure shows groupings by predictive terciles. Here we can see that groups at higher risk of experiencing the outcome in year 1 are much more separable across the entire distribution.

Even though the predictive tercile groupings are effective at separating the TLS participants for the outcome, we still need to verify that the groupings are effective at separating the TLS participants by potential barriers to housing. To do this, we present baseline characteristics of TLS participants using the two grouping types in Table 6. The first thing to note is the sample sizes in each group. Triage score grouping is heavily skewed toward the lowest risk group (2,444 individuals), while they are more evenly spread when grouped by predictive terciles.[44] When looking at demographics, note that when using the triage score, Black individuals make up a larger share of the low scorers while they are more evenly spread out when using the predictive model. This provides some evidence that the triage assessment may be less accurate for Black individuals, which has been shown by others (Cronley, 2022). When looking at self-reported earned income, health diagnoses, Serious Mental Illness (SMI) diagnoses, Substance Use Disorder (SUD) diagnoses, criminal legal involvement, and past experience with Street Outreach or Interim Housing, the predictive terciles always differentiate potential barriers to housing more than the triage score groupings. We take

[44]They are not evenly split across TLS terciles because terciles were created when including the comparison group which generally had higher risk scores.

this as evidence that the predictive terciles are effective at identifying TLS participants that might have different needs.

Estimates of impacts from entropy balancing models by predictive tercile are presented in Table 7.[45] As we did when presenting impacts by race/ethnicity, we include entropy-balanced base rates from the comparison group for each time period and subgroup. This is particularly important here given the incidence of the outcome varies meaningfully across terciles. The first thing to notice is that the base rates increase meaningfully across terciles. For example, looking at cumulative 48-month base rates, 26.9% of the low tercile, 41.3% of the medium tercile, and 58.4% of the high tercile received future homeless services. This implies that the magnitude of impacts are harder to compare relative to impacts as a percent of the base rate. When looking at impacts divided by the base rate, some interesting patterns emerge. First, when looking at annualized impacts, people in the lowest risk tercile experience large decreases in the outcome, 41.4% in the first year and 34.6% in the second year, but these impacts fade in years 3 and 4, which are when the subsidy should have ended. However, for both the medium and high tercile groups, the annualized impacts remain stable and statistically significant across all four years, and are both statistically distinguishable from the lowest tercile in year 4. Finally, when looking at cumulative impacts after 48 months, all three terciles experience an approximate 25% decrease in experiencing the outcome. In other words, even though TLS participants at highest risk were more likely to receive Street Outreach or Interim Housing in the future, the cumulative impacts of the program were relatively the same across all three groups. And, in terms of levels, the the largest reductions in homelessness came from the highest tercile.

[45] Summary statistics for TLS and comparison subgroups, by predictive tercile, including differences before and after entropy balancing, are provided in Appendix C.

5 Discussion

We provide robust non-experimental evidence that TLS programs decrease the use of future homelessness services, both annually and cumulatively, for four years from entry into the homeless-service system. These impacts exist even though nearly 40% of program participants do not have evidence of a move-in. Although those without a move-in still receive services (including some financial assistance), an exploratory analysis of weighted differences by move-in suggests the positive impacts are driven by those with an observed move-in (see Appendix D). Although exploratory, this implies that increasing move-in rates could increase the identified impacts even further. The impacts also exist across race/ethnicity groups and across groups with varying risk to future homelessness. For race/ethnicity, the qualitative pattern of impacts is the same across all groups, but the magnitude of impacts is distinguishably largest for Latinx participants compared to Black participants. This aligns with extant evidence that Black individuals continue to face discrimination in the rental market, although we do not see race/ethnicity differences by move-in status (see Appendix D Table A8).

For groups with varying risk to future homelessness, the qualitative patterns of the impacts differ. Specifically, participants at lower risk experience their gains from the program more immediately, and these gains fade after the program ends in years 3 and 4. However, groups with higher risk experience ongoing annualized benefits from program participation for four years. We do not produce any empirical evidence that would explain these differences, but it is possible that those with lower barriers are able to self-resolve after a couple years while the benefits for those with higher barriers comes from shifting individuals away from a path of chronic homelessness. We also want to note that the pattern of impacts for the low-risk group align more closely with the lower-bound estimates when using a positively-selected comparison group. What is clear is that heterogeneity exists even amongst this population of individuals experiencing homelessness, and theories of what could work should be tested and not assumed. We were not expecting these exact patterns at the start of the study, but evidence of larger impacts for families with higher needs in the prevention space has been shown before (Shinn, Greer, et al., 2013). We believe our findings suggest that TLS programs provide a viable option for a broader range of individuals than is currently recognized by local policy and practice.

There are clear limitations to this study. First, like with all non-experimental studies, assumptions need to be made and positive selection into the program is always a concern. To remove our own skepticism of the results, we introduced various placebo assessments and sensitivity analyses to demonstrate the credibility of the strategies and stability of findings to various researcher decisions. We believe both strategies passed these tests, although we believe they are more consistent for the entropy-balancing results, which we also rely on because they are more conservative. Second, the outcome window for the study included the pandemic and this likely influenced the findings as well as changes what impacts we should expect moving forward — including changes in the rental market. That said, these findings were identified in an extremely tight rental market that has remained relatively stable in terms of affordability since the pandemic (Aurand et al., 2023).

Time is required to assess the impacts beyond the pandemic, but we can consider how program participation has evolved. To do this, we use administrative data on individuals who entered the TLS program through quarter 3 of 2021 and study trends in (1) whether they have evidence of a TLS-supported move-in within 12 months, and (2) whether they were enrolled in the program for over 12 months.[46] These trends are presented in Figure 3. The move-in rates are relatively stable over time at just under 60%, which suggests there continues to be challenges in getting individuals leased up, but these challenges do not appear related to market changes from the pandemic. At the same time, the share of individuals participating for over 12 months appears to be directly related to the pandemic and has steadily grown before leveling out at just under 60%. Whatever the case, it does imply that the cost of TLS programs will increase if the average length of time on the program increases.

Although we mention that costs may increase based on TLS participation patterns during the pandemic, we do not have access to any cost data that would allow us to perform a cost-benefit analysis. Although we acknowledge this is a major limitation to the conclusions we can make,

[46] Although we can observe enrollments in TLS beyond this time period, there was an administrative change in quarter 4 of 2022 that makes observations on these program outcomes less clear.

we have some important observations that would improve the policy relevance of any future cost study. These observations are centered around the fact that we identified heterogeneous effects by risk of future homelessness. Specifically, since all individuals are already experiencing homelessness, the costs should be relative to the intended counterfactual service. Individuals with lower risk may receive alternative services that might be lighter touch (and less expensive), but individuals at higher risk may be offered more intensive services, like PSH, where costs are higher. Recent estimates of the annual cost of serving any individual experiencing homelessness in Santa Clara County in California was $5,148 (Flaming, Toros, and Burns, 2015). At the same time, another recent estimate of annual maintenance and services costs for PSH in the Bay Area in California (not including the base housing cost) was $17,000 (Reid et al., 2023). In other words, if TLS is not an option, people may instead receive other services, and those services may be more expensive than TLS, especially for individuals with many housing barriers.

In addition to targeted benefit-cost analyses, there are a number of additional areas for future research. The first relates to improving our understanding of low move-in rates and how they can be improved. The 60% move-in rate is not surprising when considering documented challenges of landlords accepting vouchers (Aliprantis, Martin, and Phillips, 2022). Learning from the housing voucher literature, more work could also be done to improve our understanding of strategies that could improve successful move-ins (Bergman et al., 2019). Importantly, some local agencies may consider the low move-in rate as a sign of failure and change how people are identified as TLS participants based on only those who have successfully identified a unit. We believe this would be a mistake since it would make the challenge unobservable, and, thus, preclude the ability to measure improvements. Finally, the smaller benefits identified for Black participants needs to be more fully understood. We do not identify differences in move-in rates by race/ethnicity, but returns to housing are highest for Black participants in TLS, and similar patterns were also found for Black PSH participants in Los Angeles (Milburn et al., 2021). More work on understanding how to counteract these inequities should be prioritized.

Other future work could relate to the rental market itself. For example, a change in policy to expand the TLS program would be similar to expanding demand in the market. Although the number of TLS units is relatively small, there will always be questions about whether there are any general equilibrium impacts on unit prices. We also have questions on how these programs may influence those leasing out their units. Although they are a type of service provider in this context, specific policies around engaging with landlords could relate to future program success. At this time, we currently have no information related to how landlords of TLS units fared with the transactions.

Even with the above limitations and open questions, we find the results credible and relevant. The fact that PSH units continue to be constrained creates a need to find alternative solutions, and we provide evidence that TLS reduces homelessness across a range of populations with varying risk to future homelessness. There is also room for improvement in the program. Despite our estimates that TLS programs decrease the future need for homeless services, nearly 30% of participants still experience the outcome. For those at higher risk, one option could be to provide additional supports, like in-home supportive services, along with TLS. Because the alternative for high-needs individuals is PSH, providing this additional support within the TLS model could make the program more effective at preventing homelessness while still being more cost-effective and scalable than PSH.

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Tables

Table 1: Study eligibility and homelessness service outcomes for TLS participants

Panel A: Study eligibility criteria	Share
Has triage tool assessment	0.81
Has triage tool assessment and...	
TLS enrolled within 6 months	0.73
Prior or same day CES entry	0.65
no PSH/family TLS within 6 months	0.70
complete demographic data	0.71
Meets all study-eligibility criteria	0.60
N (all first-time TLS participants)	6,105

Panel B: Binary outcomes (TLS in study)	Share
Evidence of TLS-supported move-in	0.62
Interim Housing in 6 months	0.27
Services from more than one provider	0.45
Multiple TLS enrollments in 6 months	0.05
TLS re-enrollment after 6 months	0.13
PSH after 6 months	0.06
N (TLS in study)	3,677

Panel C: Continous outcomes (TLS in study)			
	Days to enrollment	Months enrolled	Financial assistance[a]
Mean	40	8.9	$5,815
SD	51	7.6	$6,034
Centile			
5th	0	1	$0
25th	0	3	$1,364
50th	15	7	$4,156
75th	65	12	$8,470
95th	154	25	$17,697
Max	183	46	$45,437
Missing	0%	3%	4%

Notes: Calculations based on administrative records for Single Adults enrolled in a TLS program from July 1, 2017 through June 20, 2019 with no prior history of a housing-first service. This excluded 998 Single Adults (total TLS enrollment of 7,103) because we focus on first-time housing-first participants. Note, financial assistance values are based on manual entry by caseworkers and can include data-entry errors. For this reason, we suppressed outliers for five individuals that had recorded amounts above $50,000, which likely reflects data entry errors.
[a] Total financial assistance is reported for the 2,284 TLS participants in the study with evidence of a TLS-supported move-in.

Table 2: Sample Characteristics

	TLS		Comparison		Differences	
	Mean	SD	Mean	SD	Unadjusted	E-Balanced
Demographics						
Age	47.86	(13.58)	45.09	(13.62)	2.77^{**}	0.00
Gender: female	0.35	(0.48)	0.33	(0.47)	0.02^{**}	0.00
Race/ethnicity: Latinx	0.20	(0.40)	0.27	(0.45)	-0.08^{**}	0.00
Race/ethnicity: Black	0.57	(0.50)	0.46	(0.50)	0.11^{**}	0.00
Race/ethnicity: White	0.40	(0.49)	0.46	(0.50)	-0.07^{**}	0.00
Disability	0.62	(0.49)	0.72	(0.45)	-0.10^{**}	0.00
Earned income (self-reported)	0.23	(0.42)	0.07	(0.25)	0.16^{**}	0.00
Intake Assessment Score and Questions						
Score	6.32	(3.19)	7.45	(3.47)	-1.13^{**}	0.00
Has planned activities for happiness and fulfillment	0.46	(0.50)	0.52	(0.50)	-0.06^{**}	0.00
Homelessness caused by relationship breakdown	0.52	(0.50)	0.56	(0.50)	-0.04^{**}	0.00
Has contact email	0.31	(0.46)	0.18	(0.38)	0.13^{**}	0.00
Has contact phone	0.74	(0.44)	0.56	(0.50)	0.18^{**}	0.00
Individual-Level Service Utilization						
DHS emergency/inpatient (Years -5 to -2)	0.15	(0.36)	0.24	(0.43)	-0.09^{**}	0.00
DHS emergency/inpatient (Year -1)	0.09	(0.29)	0.18	(0.39)	-0.09^{**}	0.00
DHS outpatient (Years -5 to -2)	0.22	(0.42)	0.26	(0.44)	-0.04^{**}	0.00
DHS outpatient (Year -1)	0.11	(0.31)	0.12	(0.33)	-0.02^{**}	0.00
DMH crisis stabilization (Years -5 to -2)	0.05	(0.23)	0.11	(0.32)	-0.06^{**}	0.00
DMH crisis stabilization (Year -1)	0.05	(0.21)	0.10	(0.30)	-0.05^{**}	0.00
DMH non-crisis service (Years -5 to -2)	0.23	(0.42)	0.38	(0.48)	-0.15^{**}	0.00
DMH non-crisis service (Year -1)	0.19	(0.39)	0.30	(0.46)	-0.11^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.11	(0.31)	0.15	(0.35)	-0.04^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Year -1)	0.08	(0.27)	0.12	(0.32)	-0.04^{**}	0.00
DHS/DMH SUD diagnosis (Years -5 to -2)	0.07	(0.26)	0.14	(0.34)	-0.06^{**}	0.00
DHS/DMH SUD diagnosis (Year -1)	0.09	(0.29)	0.17	(0.37)	-0.08^{**}	0.00
DHS/DMH SMI diagnosis (Years -5 to -2)	0.15	(0.36)	0.24	(0.43)	-0.09^{**}	0.00
DHS/DMH SMI diagnosis (Year -1)	0.12	(0.33)	0.21	(0.40)	-0.08^{**}	0.00
TANF receipt (Years -5 to -2)	0.09	(0.29)	0.11	(0.32)	-0.02^{**}	0.00
TANF receipt (Year -1)	0.04	(0.18)	0.04	(0.20)	-0.01^{*}	0.00
SNAP receipt (Years -5 to -2)	0.49	(0.50)	0.51	(0.50)	-0.02^{*}	0.00
SNAP receipt (Year -1)	0.46	(0.50)	0.50	(0.50)	-0.04^{**}	0.00
General Relief receipt (Years -5 to -2)	0.32	(0.47)	0.36	(0.48)	-0.05^{**}	0.00
General Relief receipt (Year -1)	0.28	(0.45)	0.33	(0.47)	-0.05^{**}	0.00
Other HMIS enrollments (Years -5 to -2)	0.08	(0.27)	0.07	(0.26)	0.00	0.00
Other HMIS enrollments (Year -1)	0.09	(0.28)	0.08	(0.26)	0.01^{**}	0.00
Criminal legal involvement (Years -5 to -2)	0.31	(0.46)	0.46	(0.50)	-0.14^{**}	0.00
Criminal legal involvement (Year -1)	0.18	(0.39)	0.35	(0.48)	-0.17^{**}	0.00
Interim housing/street outreach (Year -5)	0.04	(0.20)	0.07	(0.25)	-0.03^{**}	0.00
Interim housing/street outreach (Year -4)	0.05	(0.21)	0.07	(0.26)	-0.03^{**}	0.00
Interim housing/street outreach (Year -3)	0.05	(0.23)	0.09	(0.29)	-0.04^{**}	0.00
Interim housing/street outreach (Year -2)	0.10	(0.29)	0.12	(0.32)	-0.02^{**}	0.00
Interim housing/street outreach (Year -1)	0.31	(0.46)	0.34	(0.47)	-0.03^{**}	0.00
DPSS homeless flag (Years -5 to -2)	0.39	(0.49)	0.45	(0.50)	-0.06^{**}	0.00
DPSS homeless flag (Year -1)	0.41	(0.49)	0.47	(0.50)	-0.06^{**}	0.00
Project-Level Service Utilization						
Project Log Num Assessments	4.62	(0.83)	5.45	(1.66)	-0.83^{**}	0.00
Project Interim/Street Rate (Year -5)	0.04	(0.02)	0.07	(0.05)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -4)	0.05	(0.03)	0.07	(0.06)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -3)	0.05	(0.03)	0.09	(0.07)	-0.04^{**}	0.00
Project Interim/Street Rate (Year -2)	0.10	(0.06)	0.12	(0.08)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -1)	0.31	(0.14)	0.34	(0.24)	-0.03^{**}	0.00
N	3,677		29,843			

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry. The comparison group consists of all individuals entering the CES who are experiencing homelessness over the same intake period as the TLS group, but they were not offered a permanent housing intervention within six months of their assessment.
**: $p < 0.01$; *: $p < 0.05$.

Table 3: Differences and Impacts of TLS on receipt of Street Outreach or Interim Housing

Time Period	Difference-in-Means	OLS: Assessment	OLS: Full	Entropy-Balanced	Event-Study	Base Rate
Pre-assessment						
Year = −5	-0.025^{**}	-	-	0.000	-	0.041
	(0.004)			(0.000)		
Year = −4	-0.028^{**}	-	-	0.000	−0.002	0.045
	(0.004)			(0.000)	(0.005)	
Year = −3	-0.039^{**}	-	-	0.000	-0.013^{**}	0.055
	(0.004)			(0.000)	(0.005)	
Year = −2	-0.023^{**}	-	-	0.000	0.002	0.096
	(0.005)			(0.000)	(0.006)	
Year = −1	-0.029^{**}	-	-	0.000	−0.004	0.312
	(0.008)			(0.000)	(0.009)	
Post-assessment (annualized)						
Year = 1	-0.108^{**}	-0.114^{**}	-0.075^{**}	-0.067^{**}	-0.083^{***}	0.219
	(0.006)	(0.006)	(0.007)	(0.008)	(0.007)	
Year = 2	-0.087^{**}	-0.089^{**}	-0.060^{**}	-0.059^{**}	-0.062^{***}	0.176
	(0.006)	(0.006)	(0.007)	(0.008)	(0.007)	
Year = 3	-0.073^{**}	-0.073^{**}	-0.049^{**}	-0.040^{**}	-0.048^{***}	0.140
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	
Year = 4	-0.064^{**}	-0.063^{**}	-0.041^{**}	-0.030^{**}	-0.039^{***}	0.112
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	
Post-assessment (cumulative)						
48 months	-0.156^{**}	-0.161^{**}	-0.103^{**}	-0.092^{**}	-	0.384
	(0.008)	(0.008)	(0.009)	(0.010)		

Notes: This table shows differences in outcomes between the 3,677 TLS participants and the 29,843 comparison individuals using (1) a simple difference-in-means; (2) an OLS model using the demographic and assessment results presented in Table 2; (3) an OLS model using all characteristics in Table 2; (4) entropy balancing on all characteristics in Table 2; and, (5) the event-study model relative to Year = −5. The final column presents the base incidence rate for each time period using the entropy-weighted comparison sample means. Also note that the post-assessment outcome window starts 6 months after assessment to allow time for program enrollment. During the six-month enrollment period, 66.2% of the comparison group and 44.9% of the TLS group received Street Outreach or Interim Housing Services, a difference of 21.3%. Standard errors are presented in parentheses and are based on 1,000 bootstrap iterations.
**: $p < 0.01$; *: $p < 0.05$.

Table 4: Sensitivity of entropy-balanced impacts and lower-bound estimates

	Excluded Samples			Enrollment Window		Alternative Outcomes		Positive Selection
Time Period	> 24 mo	Future TLS/ > 24 mo	Future PH/ > 24 mo	3 mo	12 mo	Shelter/ Street	Any Homeless Service (not HF)	Stable Housing Survey Response
Post-assessment (annualized)								
Year = 1								
Base Rate	0.221	0.220	0.217	0.253	0.187	0.201	0.279	0.221
Impact	-0.069^{**}	-0.088^{**}	-0.092^{**}	-0.097^{**}	-0.051^{**}	-0.065^{**}	-0.063^{**}	-0.069^{**}
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.017)
Year = 2								
Base Rate	0.177	0.175	0.173	0.172	0.164	0.162	0.204	0.162
Impact	-0.060^{**}	-0.068^{**}	-0.070^{**}	-0.052^{**}	-0.048^{**}	-0.056^{**}	-0.044^{**}	-0.045^{**}
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.015)
Year = 3								
Base Rate	0.142	0.141	0.139	0.147	0.133	0.131	0.167	0.121
Impact	-0.040^{**}	-0.047^{**}	-0.043^{**}	-0.050^{**}	-0.035^{**}	-0.038^{**}	-0.037^{**}	-0.021
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.016)
Year = 4								
Base Rate	0.114	0.113	0.112	0.120	0.102	0.106	0.131	0.088
Impact	-0.032^{**}	-0.029^{**}	-0.025^{**}	-0.039^{**}	-0.024^{**}	-0.032^{**}	-0.029^{**}	-0.006
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.010)
Post-assessment (cumulative)								
48 months								
Base Rate	0.386	0.383	0.379	0.412	0.348	0.357	0.455	0.366
Impact	-0.094^{**}	-0.117^{**}	-0.118^{**}	-0.114^{**}	-0.078^{**}	-0.092^{**}	-0.060^{**}	-0.074^{**}
	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)	(0.009)	(0.011)	(0.019)
N (TLS)	3,365	2,943	2,795	3,293	3,306	3,677	3,677	3,677
N (Comparison)	29,843	29,843	29,843	29,228	21,972	29,843	29,843	3,220

Notes: The table presents entropy-balanced impacts and base rates after changing the sample or outcome definitions. The "Excluded Samples" columns progressively exclude TLS participants with exits (or missing months) after 24 months, with future TLS enrollments after 6 months, or any future PH enrollments after 6 months. The "Enrollment Window" columns present estimates when re-defining the enrollment window to 3 or 12 months, respectively. The "Alternative Outcomes" columns present estimates when changing the outcome to (1) Shelter/Streeet Outreah, or (2) Any service provided by homeless service providers in the HMIS system (excluding permanent housing project types 3, 9, and 10). The "Positive Selection" columns present estimates when restricting the comparison group to a subset who indicate that they are in a "Temporary or Permanent Housing Situation" (as classified by the HUD HMIS specification) in a CES intake or exit survey in the 6-month enrollment window.
**: $p < 0.01$; *: $p < 0.05$.

Table 5: Impacts of TLS on incidence of Street Outreach or Interim Housing, by race/ethnicity

	Impacts			Impacts/Base Rate		
Time period	Latinx	Black	White	Latinx	Black	White
Post-assessment (annualized)						
Year = 1						
Base Rate	0.225	0.204	0.257			
Impact	-0.112^{**}	-0.050^{**}	-0.077^{**}	$-0.500^{**(BB)(WW)}$	$-0.246^{**(LL)}$	$-0.298^{**(LL)}$
	(0.018)	(0.011)	(0.019)	(0.059)	(0.046)	(0.063)
Year = 2						
Base Rate	0.188	0.157	0.191			
Impact	-0.085^{**}	-0.043^{**}	-0.058^{**}	$-0.454^{**(B)}$	$-0.273^{**(L)}$	-0.305^{**}
	(0.017)	(0.010)	(0.016)	(0.071)	(0.056)	(0.070)
Year = 3						
Base Rate	0.140	0.128	0.158			
Impact	-0.044^{**}	-0.031^{**}	-0.045^{**}	-0.316^{**}	-0.240^{**}	-0.284^{**}
	(0.015)	(0.009)	(0.015)	(0.093)	(0.062)	(0.086)
Year = 4						
Base Rate	0.115	0.109	0.115			
Impact	-0.032^{*}	-0.026^{**}	-0.037^{**}	-0.279^{*}	-0.241^{**}	-0.323^{**}
	(0.014)	(0.008)	(0.012)	(0.108)	(0.068)	(0.093)
Post-assessment (cumulative)						
48 months						
Base Rate	0.383	0.368	0.409			
Impact	-0.117^{**}	-0.070^{**}	-0.108^{**}	$-0.306^{**(B)}$	$-0.191^{**(L)}$	-0.263^{**}
	(0.022)	(0.013)	(0.022)	(0.049)	(0.033)	(0.047)
N: TLS	721	2,029	822			
N: Comparison	8,151	13,480	7,161			

Notes: This table includes entropy-balanced impacts by race/ethnicity, along with the impact as a percent of the entropy-balanced base rate for the comparison group. Non-overlapping race/ethnicity groups are constructed using overlapping HUD HMIS race/ethnicity indicators in the following order of precedence: (i) participants who answer 'Yes' to Hispanic/Latino Ethnicity are grouped under 'Latinx'; (ii) participants who answer 'Yes' to Black/African American race are grouped under 'Black'; (iii) participants who answer 'Yes' to White race, and answer 'No' to all other race/ethnicity categories, are grouped under 'White'. The entropy balancing characteristics are those included in Table 2 for each group, and balance is demonstrated for each comparison in Appendix C. Note that the post-assessment outcome window starts 6 months after assessment to allow time for program enrollment. Standard errors are presented in parentheses and are based on 1,000 bootstrap iterations.
**: $p < 0.01$; *: $p < 0.05$. LL/L, BB/B, WW/W give p-values (0.01 and 0.05) for differences between race/ethnicity groups.

Table 6: Characteristics of TLS participants, by risk group

	Triage Score Grouping			Predictive Model Grouping		
	0-to-7	8-to-11	12-to-17	Tercile L	Tercile M	Tercile H
Evidence of TLS move-in	0.64	0.58	0.60	0.66	0.60	0.56
Demographics						
Age	47.1	49.4	49.8	46.0	49.1	49.5
Gender: female	0.37	0.33	0.28	0.36	0.37	0.33
Race/ethnicity: Latinx	0.19	0.21	0.22	0.23	0.18	0.16
Race/ethnicity: Black	0.61	0.49	0.44	0.53	0.60	0.60
Race/ethnicity: White	0.35	0.48	0.53	0.40	0.38	0.40
Disability (self-reported)	0.48	0.87	0.97	0.55	0.64	0.71
Triage assessment						
Score	4.4	9.2	13.1	5.7	6.5	7.3
Earned income (self-reported)	0.28	0.13	0.13	0.33	0.18	0.12
Pre-assessment Administrative System						
DHS Elixhauser/Charlson Diagnosis	0.15	0.16	0.12	0.14	0.13	0.19
DHS/DMH SMI Diagnosis	0.17	0.27	0.24	0.12	0.21	0.35
DHS/DMH SUD Diagnosis	0.10	0.16	0.18	0.07	0.11	0.23
Criminal legal involvement	0.33	0.40	0.49	0.16	0.40	0.69
Street Outreach or Interim Housing	0.36	0.47	0.51	0.11	0.46	0.86
N	2,444	974	259	1,595	1,242	840

Notes: The table represents characteristics of the 3,677 TLS participants in the study sample by risk group. Risk groups are created using scores on the CES triage tool or the predictive-analytics model. The predictive analytics model is a logistic regression trained on the comparison group that was not offered a housing-first intervention during the 6-month enrollment window. Terciles are based on prediction score thresholds from the full study sample, which is why they are not evenly split in the table.

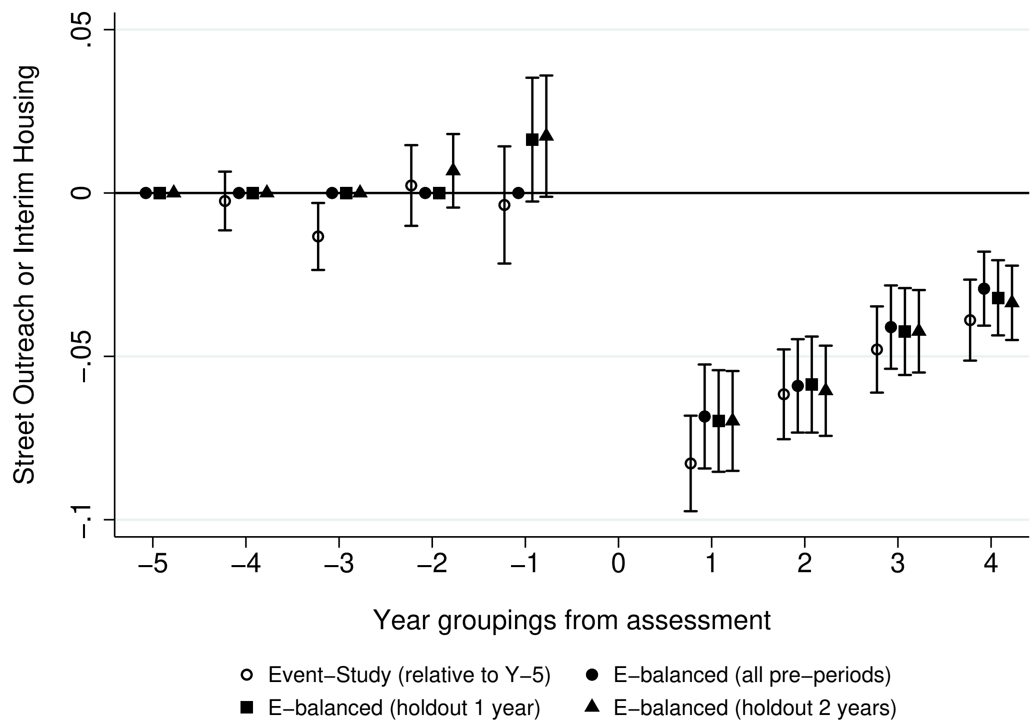
Table 7: Impacts of TLS on incidence of Street Outreach or Interim Housing, by risk group

	Impacts			Impacts/Base Rate		
Time period	Tercile L	Tercile M	Tercile H	Tercile L	Tercile M	Tercile H
Post-assessment (annualized)						
Year = 1						
Base Rate	0.148	0.238	0.356			
Impact	-0.061^{**}	-0.061^{**}	-0.114^{**}	-0.414^{**}	-0.258^{**}	-0.320^{**}
	(0.011)	(0.016)	(0.021)	(0.059)	(0.059)	(0.052)
Year = 2						
Base Rate	0.112	0.183	0.286			
Impact	-0.039^{**}	-0.062^{**}	-0.094^{**}	-0.346^{**}	-0.337^{**}	-0.329^{**}
	(0.011)	(0.013)	(0.018)	(0.075)	(0.060)	(0.053)
Year = 3						
Base Rate	0.088	0.140	0.245			
Impact	-0.015	-0.048^{**}	-0.082^{**}	-0.167	-0.340^{**}	-0.334^{**}
	(0.009)	(0.011)	(0.018)	(0.096)	(0.071)	(0.063)
Year = 4						
Base Rate	0.061	0.119	0.192			
Impact	0.001	-0.047^{**}	-0.056^{**}	$0.018^{(MM)(H)}$	$-0.393^{**(LL)}$	$-0.293^{**(L)}$
	(0.008)	(0.011)	(0.016)	(0.126)	(0.071)	(0.073)
Post-assessment (cumulative)						
48 months						
Base Rate	0.269	0.413	0.584			
Impact	-0.067^{**}	-0.103^{**}	-0.144^{**}	-0.249^{**}	-0.251^{**}	-0.247^{**}
	(0.015)	(0.018)	(0.023)	(0.047)	(0.040)	(0.036)
N: TLS	1,595	1,242	840			
N: Comparison	8,949	10,040	10,854			

Notes: This table includes entropy-balanced impacts for the three risk groups created by the predictive model along with the impact as a percent of the entorpy-balanced base rate for the comparison group. Each risk group applies entropy balancing using all characteristics in Table 2, and the results from balancing tests for each tercile are available in Appendix C. Note that the post-assessment outcome window starts 6 months after assessment to allow time for program enrollment. Standard errors are presented in parentheses and are based on 1,000 bootstrap iterations. **: $p < 0.01$; *: $p < 0.05$. LL/L, MM/M, HH/H give p-values (0.01 and 0.05) for differences between terciles.

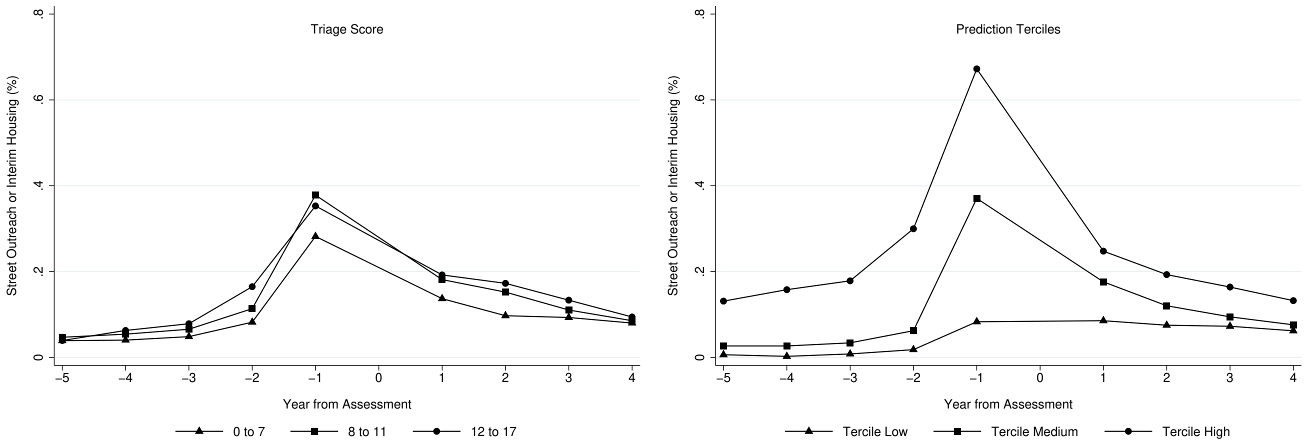
Figures

Figure 1: Impacts and placebo impacts of TLS for Event-Study and E-Balanced models



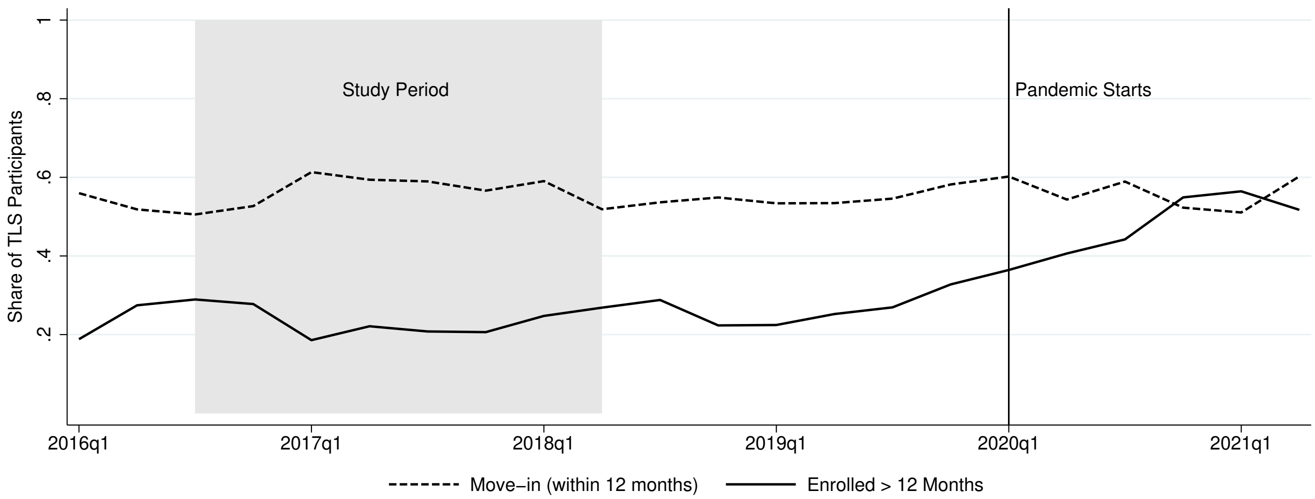
Notes: The figure represents percentage point impact estimates and 95% confidence intervals for different models grouped by year from assessment as presented in Table 3. E-balanced holdout samples reflect impacts when applying entropy balance while excluding 1 or 2 years of all pre-assessment service variables presented in Table 2. Both holdout estimates still include data from the assessment.

Figure 2: Incidence of Street Outreach or Interim Housing, by risk-level groupings



Notes: The figure represents the incidence of individuals receiving Street Outreach or Interim Housing by risk groups for the 3,677 TLS participants in the study sample. Risk groups are created using scores on the CES triage tool (left) or the predictive-analytics model (right). The predictive analytics model was trained on the comparison group that was not offered a permanent housing intervention within 6 months of assessment. Features include all characteristics in Table 2 and terciles are based on the full study sample.

Figure 3: TLS program outcomes, by quarter of entry



Notes: The figure represents program outcomes based on all program entries by calendar quarter from 2016q1 through 2021q2. If an individual enrolled in more than one TLS program in a quarter, we use the enrollment at the later date. The source of data is the Infohub from 2017q1 forward and a direct extract of data from Los Angeles' HMIS for 2016q1 through 2016q4. This was due to differences in data coverage. Finally, the panel ends in 2021q2 due to a change in how TLS enrollment was tracked in administrative records around 2022q4, which makes program outcomes through that time period less clear.

A Data sources

Our primary data source is a de-identified mirror of the "Information Hub" or "InfoHub", an individual-level linked dataset of administrative records built and maintained by the Los Angeles County Chief Information Office (CIO). This dataset contains records going back to 2010 or earlier, depending on agency. The CIO uses its own matching algorithm to link records across County agencies. Because some important data elements (for example, receipt of financial assistance) are missing from the InfoHub HMIS data, we link the InfoHub to the California Policy Lab's own de-identified mirror of the LA HMIS data received via a separate data-sharing agreement with the Los Angeles Homeles Services Authority (LAHSA). County agencies and included data are described here:

- Los Angeles Homeless Services Authority (LAHSA) Homeless Management Information System (HMIS): enrollment and service receipt information for homeless service programs (interim housing, street outreach, permanent housing, and other CES services); demographics (race/ethnicity, age, gender, disabilities); intake and screening assessments
- Department of Health Services (DHS): admission dates, service types (outpatient, emergency, inpatient), facility names, diagnosis and procedure codes from service encounters in County health facilities
- Department of Mental Health (DMH): admission dates, service types (outpatient, residential, crisis stabilization), facility names, diagnosis and procedure codes from service encounters in County mental health facilities
- Department of Public and Social Services: benefit receipt dates and homeless flags in CalFresh (SNAP), CalWORKs (TANF), and General Relief (GR)
- County Sheriff: arrest and booking dates; charge codes
- Probation: dates of probation spell

Finally, we use publicly available data from HUD to study national and local data on permanent

housing beds. This comes from the Housing Inventory Count:
https://www.hudexchange.info/resource/3031/pit-and-hic-data-since-2007/

B Sample and Outcome Definitions

Here we provide additional details on the business rules used to define the sample and outcomes. We determined single adult status in HMIS by using the participant's age at enrollment (25 or older), and being the sole member of the household attached to the enrollment. We also filtered out HMIS projects targeted at families with children and transition-age youth by excluding enrollments with the strings 'family', 'youth', or 'FSC' (short for Family Solutions Center) in the HMIS project name.

Enrollment in TLS is defined as an enrollment in 'Rapid Re-Housing' (project type 13) in the HUD HMIS specification. We define 'evidence of move-in' as using business rules developed in collaboration with LAHSA. TLS with evidence of move-in is defined as an enrollment with either non-missing move-in date, or an enrollment with one of the following types of service record: 'Rental Assistance', 'Security Deposit', 'Essential Furnishings', 'Landlord Incentive Fee', 'Moving Costs', 'Rental Arrears', 'Utility Payments'.

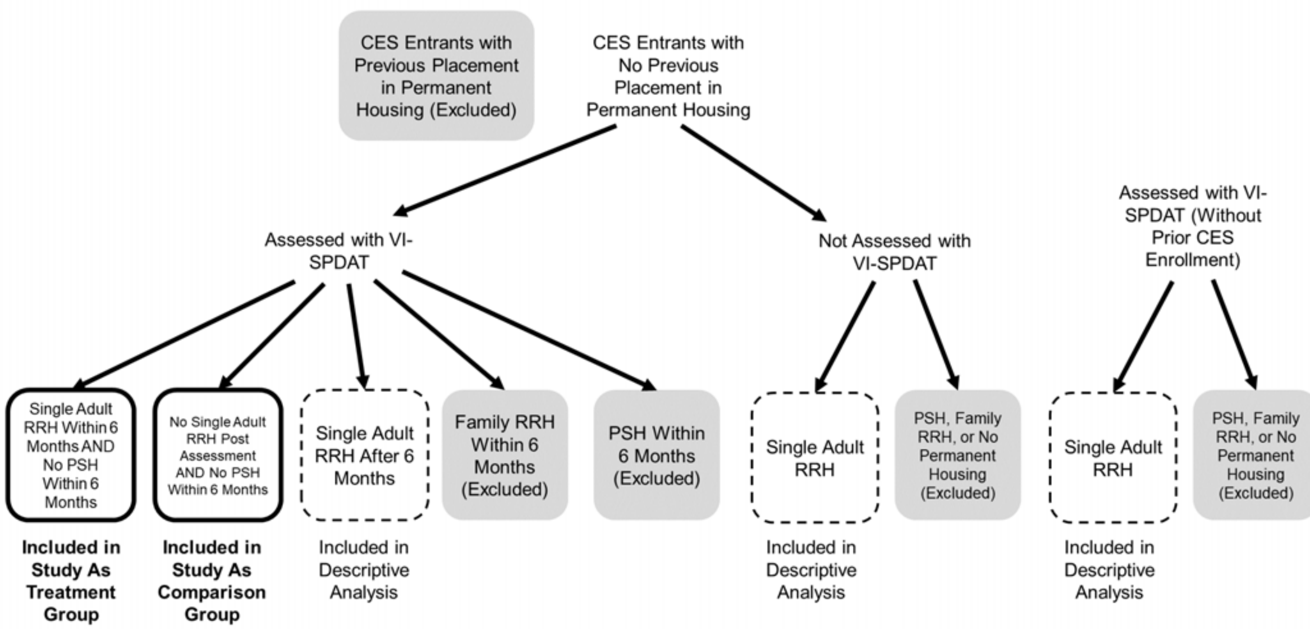
We use LAHSA's definition of Permanent Supportive Housing, which includes HUD project type 3 (Permanent Supportive Housing), as well as project types 9 (Permanent Housing without Services, no disability required) and 10 (Permanent Housing with Services, no disability required). In practice, project types 9 and 10 represent very small numbers of enrollments in Los Angeles.

Our primary outcome for the study, 'Interim Housing or Street Outreach', includes project type 4 (street outreach), as well as project types classified by LAHSA as 'interim housing': HUD HMIS project types 1 (emergency shelter), 2 (transitional housing), 8 (safe haven) and 11 (day shelter). Note that LAHSA differs from other CoCs in classifying transitional housing together with emergency shelter (whereas in other CoCs, transitional housing is usually regarded as a separate and more intensive intervention).

B.1 Sample construction

To help visualize the construction of our study sample, we created the following figure to demonstrate pathways through the homeless service system as it relates to the study.

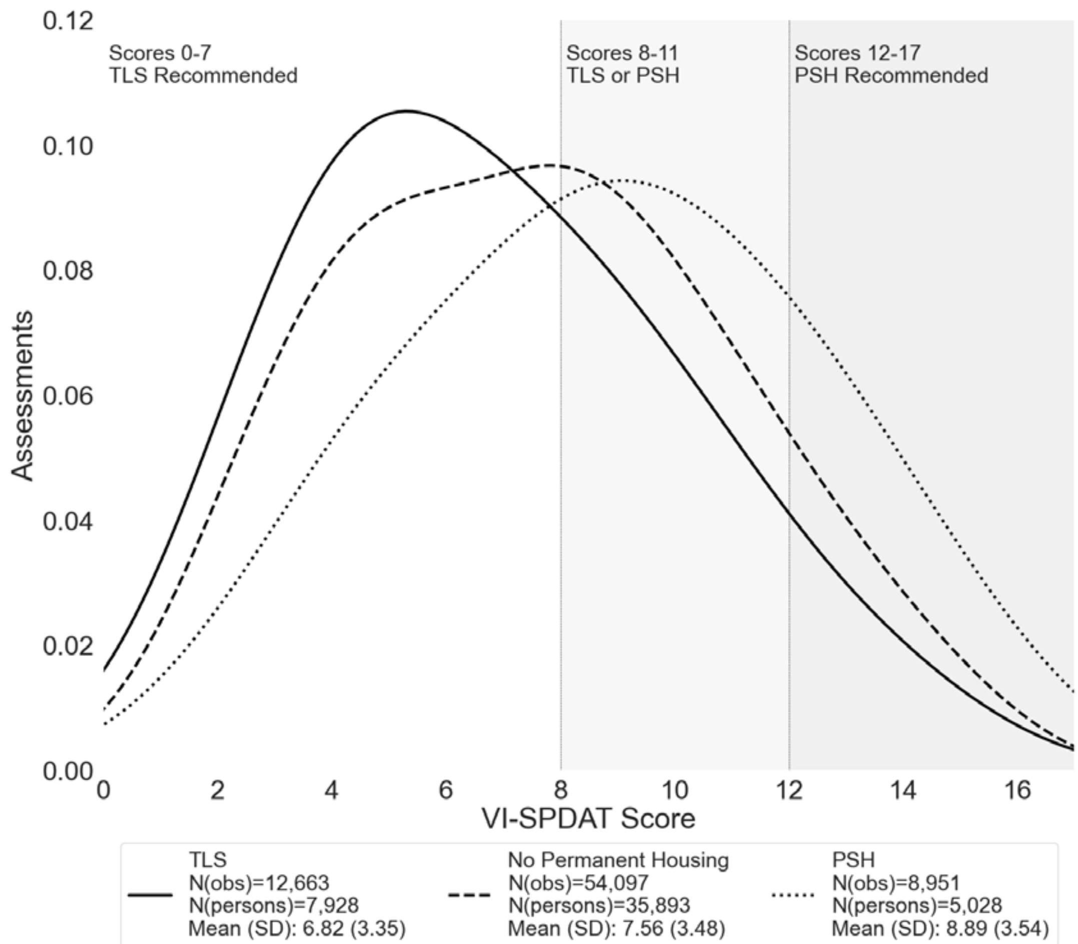
Figure A1: Pathways into Rapid Re-Housing - Sample Construction Criteria



B.2 Triage score distribution

The following figure represents the distribution of triage scores for three intervention groups across a three year period. Note, although this is inclusive of the study sample, it provides a broader population of individuals who entered homelessness services from 2016 through 2018. The figure demonstrate how scores relate to eventually received interventions, including TLS, PSH, or neither housing-first option, which is also referred to as no permanent housing. Although there is separation in the distributions that align with policy recommendations, the samples are largely overlapping.

Figure A2: Distribution of triage tool scores, by permanent housing enrollment



Notes: The figure represents kernel density plots of triage-tool scores by those assigned to different housing-first interventions. The sample includes CES entrants with VI-SPDAT assessments occurring between 1/1/2016 and 1/1/2018. Anyone who was assigned PSH in the two years following the assessment date is PSH, any remaining individuals assigned to TLS in two years is TLS, any remaining individuals are designated as no permanent housing.

Table A1: Sample Characteristics for TLS Participants Included in Study vs. Excluded from Study

	Included		Excluded		Difference
	Mean	SD	Mean	SD	
Demographics					
Age	47.98	(13.59)	47.77	(13.91)	0.20
Gender: female	0.35	(0.48)	0.30	(0.46)	0.06**
Race/ethnicity: Black	0.57	(0.50)	0.54	(0.50)	0.03*
Race/ethnicity: Latinx	0.20	(0.40)	0.20	(0.40)	0.00
Race/ethnicity: White	0.40	(0.49)	0.40	(0.49)	0.00
Earned income	0.29	(0.45)	0.24	(0.42)	0.05**
Individual-Level Service Utilization					
DHS Emergency/Inpatient (Years -5 to -2)	0.15	(0.36)	0.13	(0.34)	0.02**
DHS Emergency/Inpatient (Year -1)	0.10	(0.30)	0.08	(0.27)	0.02**
DHS Outpatient (Years -5 to -2)	0.22	(0.42)	0.19	(0.40)	0.03**
DHS Outpatient (Year -1)	0.11	(0.31)	0.08	(0.28)	0.03**
DMH Crisis Stabilization (Years -5 to -2)	0.06	(0.23)	0.05	(0.21)	0.01
DMH Crisis Stabilization (Year -1)	0.05	(0.22)	0.04	(0.18)	0.02**
DMH Non-Crisis Service (Years -5 to -2)	0.23	(0.42)	0.23	(0.42)	0.00
DMH Non-Crisis Service (Year -1)	0.21	(0.40)	0.18	(0.39)	0.02*
DHS Comorbid Diagnosis (Years -5 to -2)	0.11	(0.31)	0.09	(0.29)	0.02*
DHS Comorbid Diagnosis (Year -1)	0.08	(0.28)	0.06	(0.24)	0.02**
Substance Use Diagnosis (Years -5 to -2)	0.08	(0.27)	0.07	(0.25)	0.01
Substance Use Diagnosis (Year -1)	0.10	(0.30)	0.08	(0.27)	0.02**
Serious Mental Illness Diagnosis (Years -5 to -2)	0.15	(0.36)	0.13	(0.34)	0.02*
Serious Mental Illness Diagnosis (Year -1)	0.14	(0.35)	0.12	(0.33)	0.01
TANF Receipt (Years -5 to -2)	0.09	(0.29)	0.09	(0.29)	0.00
TANF Receipt (Year -1)	0.04	(0.19)	0.04	(0.20)	−0.01
SNAP Receipt (Years -5 to -2)	0.49	(0.50)	0.48	(0.50)	0.01
SNAP Receipt (Year -1)	0.47	(0.50)	0.46	(0.50)	0.01
General Relief Receipt (Years -5 to -2)	0.32	(0.47)	0.32	(0.47)	0.00
General Relief Receipt (Year -1)	0.30	(0.46)	0.28	(0.45)	0.01
DPSS Homelessness (Years -5 to -2)	0.40	(0.49)	0.39	(0.49)	0.01
DPSS Homelessness (Year -1)	0.42	(0.49)	0.41	(0.49)	0.01
Criminal legal involvement (Years -5 to -2)	0.32	(0.46)	0.31	(0.46)	0.01
Criminal legal involvement (Year -1)	0.18	(0.39)	0.17	(0.38)	0.01
Interim housing/street outreach (Year -5)	0.04	(0.20)	0.03	(0.17)	0.01**
Interim housing/street outreach (Year -4)	0.05	(0.21)	0.03	(0.18)	0.01*
Interim housing/street outreach (Year -3)	0.06	(0.24)	0.06	(0.24)	0.00
Interim housing/street outreach (Year -2)	0.10	(0.30)	0.13	(0.34)	−0.03**
Interim housing/street outreach (Year -1)	0.55	(0.50)	0.33	(0.47)	0.22**
Evidence of Move-In	0.62	(0.49)	0.65	(0.48)	−0.03*
N	3,677		2,428		

Note: Service utilization characteristics are measured from the date of TLS enrollment, rather than the date of triage tool assessment, as is the case for the impact estimates.
**: $p < 0.01$; *: $p < 0.05$.

C Summary statistics for subgroups

Table A2: Sample Characteristics (Black race/ethnicity)

	TLS		Comparison		Differences	
	Mean	SD	Mean	SD	Unadjusted	E-Balanced
Demographics						
Age	47.35	(13.63)	45.35	(13.71)	2.00^{**}	0.00
Gender: female	0.39	(0.49)	0.34	(0.47)	0.06^{**}	0.00
Disability	0.57	(0.50)	0.68	(0.46)	-0.12^{**}	0.00
Earned income (self-reported)	0.26	(0.44)	0.09	(0.28)	0.17^{**}	0.00
Intake Assessment Score and Questions						
Score	5.89	(3.05)	7.03	(3.50)	-1.15^{**}	0.00
Has planned activities for happiness and fulfillment	0.42	(0.49)	0.49	(0.50)	-0.07^{**}	0.00
Homelessness caused by relationship breakdown	0.50	(0.50)	0.54	(0.50)	-0.04^{**}	0.00
Has contact email	0.30	(0.46)	0.17	(0.38)	0.12^{**}	0.00
Has contact phone	0.75	(0.44)	0.58	(0.49)	0.16^{**}	0.00
Individual-Level Service Utilization						
DHS emergency/inpatient (Years -5 to -2)	0.15	(0.36)	0.24	(0.43)	-0.09^{**}	0.00
DHS emergency/inpatient (Year -1)	0.08	(0.27)	0.18	(0.38)	-0.10^{**}	0.00
DHS outpatient (Years -5 to -2)	0.26	(0.44)	0.30	(0.46)	-0.04^{**}	0.00
DHS outpatient (Year -1)	0.12	(0.33)	0.15	(0.35)	-0.02^{*}	0.00
DMH crisis stabilization (Years -5 to -2)	0.06	(0.24)	0.12	(0.33)	-0.07^{**}	0.00
DMH crisis stabilization (Year -1)	0.04	(0.21)	0.10	(0.30)	-0.06^{**}	0.00
DMH non-crisis service (Years -5 to -2)	0.24	(0.43)	0.40	(0.49)	-0.16^{**}	0.00
DMH non-crisis service (Year -1)	0.19	(0.39)	0.31	(0.46)	-0.12^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.12	(0.32)	0.16	(0.37)	-0.04^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Year -1)	0.08	(0.27)	0.13	(0.33)	-0.04^{**}	0.00
DHS/DMH SUD diagnosis (Years -5 to -2)	0.06	(0.25)	0.14	(0.35)	-0.08^{**}	0.00
DHS/DMH SUD diagnosis (Year -1)	0.08	(0.28)	0.17	(0.38)	-0.09^{**}	0.00
DHS/DMH SMI diagnosis (Years -5 to -2)	0.16	(0.37)	0.27	(0.44)	-0.11^{**}	0.00
DHS/DMH SMI diagnosis (Year -1)	0.12	(0.33)	0.22	(0.41)	-0.10^{**}	0.00
TANF receipt (Years -5 to -2)	0.11	(0.31)	0.12	(0.33)	-0.02^{*}	0.00
TANF receipt (Year -1)	0.04	(0.20)	0.05	(0.21)	-0.01	0.00
SNAP receipt (Years -5 to -2)	0.52	(0.50)	0.53	(0.50)	-0.01	0.00
SNAP receipt (Year -1)	0.47	(0.50)	0.50	(0.50)	-0.03^{*}	0.00
General Relief receipt (Years -5 to -2)	0.35	(0.48)	0.41	(0.49)	-0.05^{**}	0.00
General Relief receipt (Year -1)	0.29	(0.45)	0.36	(0.48)	-0.07^{**}	0.00
Other HMIS enrollments (Years -5 to -2)	0.09	(0.29)	0.10	(0.30)	-0.01	0.00
Other HMIS enrollments (Year -1)	0.09	(0.29)	0.09	(0.28)	0.01	0.00
Criminal legal involvement (Years -5 to -2)	0.33	(0.47)	0.47	(0.50)	-0.14^{**}	0.00
Criminal legal involvement (Year -1)	0.17	(0.38)	0.33	(0.47)	-0.16^{**}	0.00
Interim housing/street outreach (Year -5)	0.05	(0.21)	0.09	(0.28)	-0.04^{**}	0.00
Interim housing/street outreach (Year -4)	0.04	(0.20)	0.09	(0.29)	-0.05^{**}	0.00
Interim housing/street outreach (Year -3)	0.06	(0.23)	0.11	(0.32)	-0.06^{**}	0.00
Interim housing/street outreach (Year -2)	0.09	(0.29)	0.13	(0.34)	-0.04^{**}	0.00
Interim housing/street outreach (Year -1)	0.30	(0.46)	0.33	(0.47)	-0.03^{**}	0.00
DPSS homeless flag (Years -5 to -2)	0.44	(0.50)	0.49	(0.50)	-0.05^{**}	0.00
DPSS homeless flag (Year -1)	0.43	(0.50)	0.48	(0.50)	-0.05^{**}	0.00
Project-Level Service Utilization						
Project Log Num Assessments	4.70	(0.88)	5.52	(1.66)	-0.82^{**}	0.00
Project Interim/Street Rate (Year -5)	0.04	(0.02)	0.07	(0.05)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -4)	0.04	(0.03)	0.08	(0.06)	-0.04^{**}	0.00
Project Interim/Street Rate (Year -3)	0.05	(0.03)	0.10	(0.07)	-0.05^{**}	0.00
Project Interim/Street Rate (Year -2)	0.09	(0.05)	0.12	(0.08)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -1)	0.30	(0.13)	0.34	(0.23)	-0.04^{**}	0.00
N	2,029		13,480			

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry. The comparison group consists of all individuals entering the CES who are experiencing homelessness over the same intake period as the TLS group, but they were not offered a permanent housing intervention within six months of their assessment.
**: $p < 0.01$; *: $p < 0.05$.

Table A3: Sample Characteristics (Latinx race/ethnicity)

	TLS		Comparison		Differences	
	Mean	SD	Mean	SD	Unadjusted	E-Balanced
Demographics						
Age	45.17	(13.38)	42.81	(13.56)	2.36^{**}	0.00
Gender: female	0.32	(0.47)	0.33	(0.47)	-0.01	0.00
Disability	0.63	(0.48)	0.70	(0.46)	-0.07^{**}	0.00
Earned income (self-reported)	0.29	(0.45)	0.06	(0.24)	0.22^{**}	0.00
Intake Assessment Score and Questions						
Score	6.56	(3.29)	7.52	(3.41)	-0.96^{**}	0.00
Has planned activities for happiness and fulfillment	0.49	(0.50)	0.53	(0.50)	-0.03	0.00
Homelessness caused by relationship breakdown	0.56	(0.50)	0.58	(0.49)	-0.02	0.00
Has contact email	0.30	(0.46)	0.16	(0.36)	0.14^{**}	0.00
Has contact phone	0.74	(0.44)	0.55	(0.50)	0.20^{**}	0.00
Individual-Level Service Utilization						
DHS emergency/inpatient (Years -5 to -2)	0.15	(0.36)	0.27	(0.44)	-0.11^{**}	0.00
DHS emergency/inpatient (Year -1)	0.11	(0.31)	0.20	(0.40)	-0.09^{**}	0.00
DHS outpatient (Years -5 to -2)	0.20	(0.40)	0.25	(0.44)	-0.06^{**}	0.00
DHS outpatient (Year -1)	0.10	(0.30)	0.12	(0.32)	-0.02	0.00
DMH crisis stabilization (Years -5 to -2)	0.05	(0.21)	0.11	(0.31)	-0.06^{**}	0.00
DMH crisis stabilization (Year -1)	0.04	(0.20)	0.09	(0.28)	-0.05^{**}	0.00
DMH non-crisis service (Years -5 to -2)	0.20	(0.40)	0.36	(0.48)	-0.16^{**}	0.00
DMH non-crisis service (Year -1)	0.17	(0.37)	0.28	(0.45)	-0.11^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.10	(0.30)	0.15	(0.35)	-0.04^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Year -1)	0.07	(0.26)	0.11	(0.31)	-0.04^{**}	0.00
DHS/DMH SUD diagnosis (Years -5 to -2)	0.08	(0.27)	0.13	(0.34)	-0.05^{**}	0.00
DHS/DMH SUD diagnosis (Year -1)	0.10	(0.30)	0.16	(0.37)	-0.06^{**}	0.00
DHS/DMH SMI diagnosis (Years -5 to -2)	0.12	(0.32)	0.22	(0.41)	-0.10^{**}	0.00
DHS/DMH SMI diagnosis (Year -1)	0.11	(0.31)	0.19	(0.39)	-0.08^{**}	0.00
TANF receipt (Years -5 to -2)	0.12	(0.33)	0.14	(0.34)	-0.01	0.00
TANF receipt (Year -1)	0.05	(0.23)	0.06	(0.24)	0.00	0.00
SNAP receipt (Years -5 to -2)	0.53	(0.50)	0.54	(0.50)	-0.01	0.00
SNAP receipt (Year -1)	0.50	(0.50)	0.52	(0.50)	-0.02	0.00
General Relief receipt (Years -5 to -2)	0.31	(0.46)	0.35	(0.48)	-0.05^{**}	0.00
General Relief receipt (Year -1)	0.28	(0.45)	0.32	(0.47)	-0.04^{*}	0.00
Other HMIS enrollments (Years -5 to -2)	0.07	(0.25)	0.05	(0.22)	0.02	0.00
Other HMIS enrollments (Year -1)	0.07	(0.25)	0.07	(0.25)	0.00	0.00
Criminal legal involvement (Years -5 to -2)	0.34	(0.47)	0.48	(0.50)	-0.14^{**}	0.00
Criminal legal involvement (Year -1)	0.23	(0.42)	0.40	(0.49)	-0.17^{**}	0.00
Interim housing/street outreach (Year -5)	0.02	(0.16)	0.05	(0.22)	-0.02^{**}	0.00
Interim housing/street outreach (Year -4)	0.05	(0.22)	0.06	(0.23)	-0.01	0.00
Interim housing/street outreach (Year -3)	0.05	(0.22)	0.08	(0.27)	-0.03^{*}	0.00
Interim housing/street outreach (Year -2)	0.08	(0.27)	0.10	(0.30)	-0.03^{*}	0.00
Interim housing/street outreach (Year -1)	0.28	(0.45)	0.32	(0.47)	-0.04^{*}	0.00
DPSS homeless flag (Years -5 to -2)	0.39	(0.49)	0.45	(0.50)	-0.06^{**}	0.00
DPSS homeless flag (Year -1)	0.41	(0.49)	0.47	(0.50)	-0.06^{**}	0.00
Project-Level Service Utilization						
Project Log Num Assessments	4.49	(0.75)	5.53	(1.71)	-1.04^{**}	0.00
Project Interim/Street Rate (Year -5)	0.04	(0.03)	0.06	(0.05)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -4)	0.05	(0.04)	0.07	(0.06)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -3)	0.06	(0.03)	0.09	(0.06)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -2)	0.10	(0.06)	0.11	(0.08)	-0.01^{**}	0.00
Project Interim/Street Rate (Year -1)	0.32	(0.17)	0.32	(0.25)	0.00	0.00
N	721		8,151			

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry. The comparison group consists of all individuals entering the CES who are experiencing homelessness over the same intake period as the TLS group, but they were not offered a permanent housing intervention within six months of their assessment.
**: $p < 0.01$; *: $p < 0.05$.

Table A4: Sample Characteristics (White race/ethnicity)

	TLS		Comparison		Differences	
	Mean	SD	Mean	SD	Unadjusted	E-Balanced
Demographics						
Age	51.50	(12.92)	47.07	(13.17)	4.42^{**}	0.00
Gender: female	0.29	(0.46)	0.32	(0.47)	-0.03	0.00
Disability	0.72	(0.45)	0.80	(0.40)	-0.07^{**}	0.00
Earned income (self-reported)	0.13	(0.33)	0.04	(0.20)	0.09^{**}	0.00
Intake Assessment Score and Questions						
Score	7.19	(3.28)	8.17	(3.39)	-0.99^{**}	0.00
Has planned activities for happiness and fulfillment	0.55	(0.50)	0.56	(0.50)	-0.01	0.00
Homelessness caused by relationship breakdown	0.51	(0.50)	0.57	(0.50)	-0.06^{**}	0.00
Has contact email	0.34	(0.47)	0.21	(0.41)	0.13^{**}	0.00
Has contact phone	0.73	(0.44)	0.55	(0.50)	0.18^{**}	0.00
Individual-Level Service Utilization						
DHS emergency/inpatient (Years -5 to -2)	0.14	(0.35)	0.22	(0.41)	-0.08^{**}	0.00
DHS emergency/inpatient (Year -1)	0.09	(0.29)	0.18	(0.38)	-0.08^{**}	0.00
DHS outpatient (Years -5 to -2)	0.16	(0.37)	0.20	(0.40)	-0.04^{**}	0.00
DHS outpatient (Year -1)	0.07	(0.25)	0.09	(0.28)	-0.02	0.00
DMH crisis stabilization (Years -5 to -2)	0.05	(0.22)	0.10	(0.30)	-0.05^{**}	0.00
DMH crisis stabilization (Year -1)	0.05	(0.22)	0.10	(0.30)	-0.05^{**}	0.00
DMH non-crisis service (Years -5 to -2)	0.23	(0.42)	0.36	(0.48)	-0.13^{**}	0.00
DMH non-crisis service (Year -1)	0.19	(0.39)	0.29	(0.45)	-0.10^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.07	(0.26)	0.12	(0.33)	-0.05^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Year -1)	0.07	(0.26)	0.10	(0.30)	-0.03^{**}	0.00
DHS/DMH SUD diagnosis (Years -5 to -2)	0.09	(0.29)	0.13	(0.34)	-0.04^{**}	0.00
DHS/DMH SUD diagnosis (Year -1)	0.10	(0.31)	0.17	(0.38)	-0.07^{**}	0.00
DHS/DMH SMI diagnosis (Years -5 to -2)	0.14	(0.34)	0.21	(0.41)	-0.07^{**}	0.00
DHS/DMH SMI diagnosis (Year -1)	0.12	(0.33)	0.20	(0.40)	-0.08^{**}	0.00
TANF receipt (Years -5 to -2)	0.04	(0.20)	0.07	(0.26)	-0.03^{**}	0.00
TANF receipt (Year -1)	0.01	(0.09)	0.02	(0.14)	-0.01^{**}	0.00
SNAP receipt (Years -5 to -2)	0.38	(0.49)	0.46	(0.50)	-0.08^{**}	0.00
SNAP receipt (Year -1)	0.41	(0.49)	0.49	(0.50)	-0.08^{**}	0.00
General Relief receipt (Years -5 to -2)	0.24	(0.43)	0.30	(0.46)	-0.06^{**}	0.00
General Relief receipt (Year -1)	0.26	(0.44)	0.31	(0.46)	-0.04^{**}	0.00
Other HMIS enrollments (Years -5 to -2)	0.06	(0.24)	0.06	(0.24)	0.00	0.00
Other HMIS enrollments (Year -1)	0.10	(0.30)	0.07	(0.25)	0.03^{**}	0.00
Criminal legal involvement (Years -5 to -2)	0.27	(0.44)	0.41	(0.49)	-0.15^{**}	0.00
Criminal legal involvement (Year -1)	0.17	(0.38)	0.34	(0.47)	-0.17^{**}	0.00
Interim housing/street outreach (Year -5)	0.05	(0.21)	0.05	(0.22)	-0.01	0.00
Interim housing/street outreach (Year -4)	0.05	(0.21)	0.06	(0.24)	-0.01	0.00
Interim housing/street outreach (Year -3)	0.05	(0.22)	0.08	(0.27)	-0.03^{**}	0.00
Interim housing/street outreach (Year -2)	0.13	(0.33)	0.12	(0.32)	0.01	0.00
Interim housing/street outreach (Year -1)	0.36	(0.48)	0.38	(0.49)	-0.02	0.00
DPSS homeless flag (Years -5 to -2)	0.29	(0.45)	0.40	(0.49)	-0.11^{**}	0.00
DPSS homeless flag (Year -1)	0.35	(0.48)	0.45	(0.50)	-0.09^{**}	0.00
Project-Level Service Utilization						
Project Log Num Assessments	4.53	(0.73)	5.21	(1.59)	-0.68^{**}	0.00
Project Interim/Street Rate (Year -5)	0.04	(0.02)	0.06	(0.05)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -4)	0.05	(0.03)	0.07	(0.06)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -3)	0.06	(0.03)	0.09	(0.06)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -2)	0.11	(0.06)	0.12	(0.08)	-0.01^{**}	0.00
Project Interim/Street Rate (Year -1)	0.33	(0.15)	0.37	(0.25)	-0.04^{**}	0.00
N	822		7,161			

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry. The comparison group consists of all individuals entering the CES who are experiencing homelessness over the same intake period as the TLS group, but they were not offered a permanent housing intervention within six months of their assessment.
**: p < 0.01; *: p < 0.05.

Table A5: Sample Characteristics (Acuity Tercile 1)

	TLS		Comparison		Differences	
	Mean	SD	Mean	SD	Unadjusted	E-Balanced
Demographics						
Age	45.99	(14.58)	42.23	(14.89)	3.76^{**}	0.00
Gender: female	0.36	(0.48)	0.32	(0.47)	0.03^{**}	0.00
Race/ethnicity: Latinx	0.23	(0.42)	0.32	(0.47)	-0.09^{**}	0.00
Race/ethnicity: Black	0.53	(0.50)	0.40	(0.49)	0.13^{**}	0.00
Race/ethnicity: White	0.40	(0.49)	0.46	(0.50)	-0.06^{**}	0.00
Disability	0.55	(0.50)	0.62	(0.49)	-0.06^{**}	0.00
Earned income (self-reported)	0.33	(0.47)	0.12	(0.32)	0.22^{**}	0.00
Intake Assessment Score and Questions						
Score	5.66	(2.92)	6.54	(3.25)	-0.89^{**}	0.00
Has planned activities for happiness and fulfillment	0.44	(0.50)	0.48	(0.50)	-0.04^{**}	0.00
Homelessness caused by relationship breakdown	0.49	(0.50)	0.53	(0.50)	-0.04^{**}	0.00
Has contact email	0.38	(0.49)	0.23	(0.42)	0.15^{**}	0.00
Has contact phone	0.80	(0.40)	0.61	(0.49)	0.18^{**}	0.00
Individual-Level Service Utilization						
DHS emergency/inpatient (Years -5 to -2)	0.10	(0.30)	0.12	(0.33)	-0.03^{**}	0.00
DHS emergency/inpatient (Year -1)	0.06	(0.24)	0.11	(0.31)	-0.05^{**}	0.00
DHS outpatient (Years -5 to -2)	0.18	(0.39)	0.18	(0.38)	0.00	0.00
DHS outpatient (Year -1)	0.11	(0.32)	0.12	(0.33)	-0.01	0.00
DMH crisis stabilization (Years -5 to -2)	0.01	(0.11)	0.03	(0.16)	-0.01^{**}	0.00
DMH crisis stabilization (Year -1)	0.01	(0.11)	0.04	(0.19)	-0.02^{**}	0.00
DMH non-crisis service (Years -5 to -2)	0.11	(0.31)	0.17	(0.38)	-0.07^{**}	0.00
DMH non-crisis service (Year -1)	0.12	(0.33)	0.20	(0.40)	-0.08^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.10	(0.30)	0.11	(0.31)	0.00	0.00
DHS Elixhauser/Charlson diagnosis (Year -1)	0.08	(0.28)	0.10	(0.30)	-0.02	0.00
DHS/DMH SUD diagnosis (Years -5 to -2)	0.04	(0.20)	0.06	(0.23)	-0.02^{**}	0.00
DHS/DMH SUD diagnosis (Year -1)	0.06	(0.24)	0.08	(0.28)	-0.02^{**}	0.00
DHS/DMH SMI diagnosis (Years -5 to -2)	0.07	(0.26)	0.11	(0.32)	-0.04^{**}	0.00
DHS/DMH SMI diagnosis (Year -1)	0.08	(0.27)	0.14	(0.35)	-0.06^{**}	0.00
TANF receipt (Years -5 to -2)	0.07	(0.26)	0.09	(0.28)	-0.02^{*}	0.00
TANF receipt (Year -1)	0.04	(0.19)	0.05	(0.21)	-0.01	0.00
SNAP receipt (Years -5 to -2)	0.39	(0.49)	0.34	(0.47)	0.05^{**}	0.00
SNAP receipt (Year -1)	0.27	(0.44)	0.25	(0.44)	0.01	0.00
General Relief receipt (Years -5 to -2)	0.21	(0.41)	0.18	(0.38)	0.03^{**}	0.00
General Relief receipt (Year -1)	0.11	(0.32)	0.13	(0.34)	-0.02^{*}	0.00
Other HMIS enrollments (Years -5 to -2)	0.03	(0.16)	0.02	(0.14)	0.01	0.00
Other HMIS enrollments (Year -1)	0.06	(0.24)	0.06	(0.24)	0.00	0.00
Criminal legal involvement (Years -5 to -2)	0.12	(0.32)	0.15	(0.36)	-0.04^{**}	0.00
Criminal legal involvement (Year -1)	0.08	(0.26)	0.17	(0.38)	-0.09^{**}	0.00
Interim housing/street outreach (Year -5)	0.01	(0.08)	0.01	(0.09)	0.00	0.00
Interim housing/street outreach (Year -4)	0.00	(0.04)	0.01	(0.08)	-0.01^{**}	0.00
Interim housing/street outreach (Year -3)	0.01	(0.08)	0.02	(0.12)	-0.01^{**}	0.00
Interim housing/street outreach (Year -2)	0.02	(0.13)	0.01	(0.10)	0.01^{*}	0.00
Interim housing/street outreach (Year -1)	0.08	(0.27)	0.08	(0.26)	0.01	0.00
DPSS homeless flag (Years -5 to -2)	0.24	(0.42)	0.21	(0.40)	0.03^{**}	0.00
DPSS homeless flag (Year -1)	0.18	(0.38)	0.19	(0.39)	-0.01	0.00
Project-Level Service Utilization						
Project Log Num Assessments	4.54	(0.78)	5.72	(1.70)	-1.19^{**}	0.00
Project Interim/Street Rate (Year -5)	0.04	(0.02)	0.06	(0.04)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -4)	0.04	(0.03)	0.06	(0.05)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -3)	0.05	(0.03)	0.08	(0.05)	-0.03^{**}	0.00^{*}
Project Interim/Street Rate (Year -2)	0.09	(0.05)	0.10	(0.06)	-0.01^{**}	0.00
Project Interim/Street Rate (Year -1)	0.28	(0.13)	0.26	(0.19)	0.03^{**}	0.00
N	1,595		8,949			

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry. The comparison group consists of all individuals entering the CES who are experiencing homelessness over the same intake period as the TLS group, but they were not offered a permanent housing intervention within six months of their assessment.
**: $p < 0.01$; *: $p < 0.05$.

Table A6: Sample Characteristics (Acuity Tercile 2)

	TLS		Comparison		Differences	
	Mean	SD	Mean	SD	Unadjusted	E-Balanced
Demographics						
Age	49.13	(12.92)	45.48	(13.64)	3.65^{**}	0.00
Gender: female	0.37	(0.48)	0.34	(0.47)	0.03	0.00
Race/ethnicity: Latinx	0.18	(0.38)	0.28	(0.45)	-0.10^{**}	0.00
Race/ethnicity: Black	0.60	(0.49)	0.46	(0.50)	0.14^{**}	0.00
Race/ethnicity: White	0.38	(0.49)	0.47	(0.50)	-0.09^{**}	0.00
Disability	0.64	(0.48)	0.72	(0.45)	-0.08^{**}	0.00
Earned income (self-reported)	0.18	(0.38)	0.06	(0.23)	0.12^{**}	0.00
Intake Assessment Score and Questions						
Score	6.50	(3.12)	7.39	(3.40)	-0.89^{**}	0.00
Has planned activities for happiness and fulfillment	0.49	(0.50)	0.51	(0.50)	-0.02	0.00
Homelessness caused by relationship breakdown	0.54	(0.50)	0.57	(0.50)	-0.03	0.00
Has contact email	0.26	(0.44)	0.18	(0.39)	0.08^{**}	0.00
Has contact phone	0.74	(0.44)	0.58	(0.49)	0.16^{**}	0.00
Individual-Level Service Utilization						
DHS emergency/inpatient (Years -5 to -2)	0.12	(0.33)	0.21	(0.40)	-0.08^{**}	0.00
DHS emergency/inpatient (Year -1)	0.09	(0.28)	0.16	(0.37)	-0.08^{**}	0.00
DHS outpatient (Years -5 to -2)	0.22	(0.42)	0.23	(0.42)	-0.01	0.00
DHS outpatient (Year -1)	0.09	(0.29)	0.12	(0.32)	-0.02^{*}	0.00
DMH crisis stabilization (Years -5 to -2)	0.05	(0.21)	0.08	(0.27)	-0.03^{**}	0.00
DMH crisis stabilization (Year -1)	0.05	(0.22)	0.08	(0.27)	-0.03^{**}	0.00
DMH non-crisis service (Years -5 to -2)	0.23	(0.42)	0.34	(0.47)	-0.11^{**}	0.00
DMH non-crisis service (Year -1)	0.19	(0.40)	0.28	(0.45)	-0.09^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.09	(0.29)	0.13	(0.34)	-0.04^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Year -1)	0.06	(0.25)	0.10	(0.31)	-0.04^{**}	0.00
DHS/DMH SUD diagnosis (Years -5 to -2)	0.06	(0.24)	0.12	(0.32)	-0.05^{**}	0.00
DHS/DMH SUD diagnosis (Year -1)	0.08	(0.27)	0.15	(0.35)	-0.06^{**}	0.00
DHS/DMH SMI diagnosis (Years -5 to -2)	0.16	(0.36)	0.20	(0.40)	-0.04^{**}	0.00
DHS/DMH SMI diagnosis (Year -1)	0.13	(0.33)	0.19	(0.39)	-0.06^{**}	0.00
TANF receipt (Years -5 to -2)	0.10	(0.29)	0.10	(0.30)	-0.01	0.00
TANF receipt (Year -1)	0.03	(0.18)	0.05	(0.22)	-0.02^{**}	0.00
SNAP receipt (Years -5 to -2)	0.50	(0.50)	0.50	(0.50)	0.00	0.00
SNAP receipt (Year -1)	0.56	(0.50)	0.52	(0.50)	0.04^{*}	0.00
General Relief receipt (Years -5 to -2)	0.32	(0.47)	0.34	(0.47)	-0.02	0.00
General Relief receipt (Year -1)	0.34	(0.47)	0.33	(0.47)	0.02	0.00
Other HMIS enrollments (Years -5 to -2)	0.05	(0.22)	0.04	(0.20)	0.01	0.00
Other HMIS enrollments (Year -1)	0.09	(0.29)	0.08	(0.27)	0.02	0.00
Criminal legal involvement (Years -5 to -2)	0.34	(0.48)	0.43	(0.49)	-0.09^{**}	0.00
Criminal legal involvement (Year -1)	0.19	(0.39)	0.33	(0.47)	-0.14^{**}	0.00
Interim housing/street outreach (Year -5)	0.02	(0.15)	0.03	(0.17)	0.00	0.00
Interim housing/street outreach (Year -4)	0.03	(0.16)	0.03	(0.16)	0.00	0.00
Interim housing/street outreach (Year -3)	0.03	(0.18)	0.04	(0.20)	-0.01	0.00
Interim housing/street outreach (Year -2)	0.06	(0.23)	0.05	(0.22)	0.01	0.00
Interim housing/street outreach (Year -1)	0.36	(0.48)	0.31	(0.46)	0.05^{**}	0.00
DPSS homeless flag (Years -5 to -2)	0.43	(0.50)	0.44	(0.50)	-0.01	0.00
DPSS homeless flag (Year -1)	0.51	(0.50)	0.48	(0.50)	0.03^{*}	0.00
Project-Level Service Utilization						
Project Log Num Assessments	4.68	(0.87)	5.44	(1.67)	-0.76^{**}	0.00
Project Interim/Street Rate (Year -5)	0.04	(0.02)	0.06	(0.05)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -4)	0.05	(0.03)	0.07	(0.05)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -3)	0.06	(0.03)	0.09	(0.06)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -2)	0.10	(0.06)	0.11	(0.07)	-0.02^{**}	0.00
Project Interim/Street Rate (Year -1)	0.33	(0.15)	0.34	(0.25)	-0.01^{*}	0.00
N	1,242		10,040			

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry. The comparison group consists of all individuals entering the CES who are experiencing homelessness over the same intake period as the TLS group, but they were not offered a permanent housing intervention within six months of their assessment.
**: $p < 0.01$; *: $p < 0.05$.

Table A7: Sample Characteristics (Acuity Tercile 3)

	TLS		Comparison		Differences	
	Mean	SD	Mean	SD	Unadjusted	E-Balanced
Demographics						
Age	49.53	(12.06)	47.08	(12.01)	2.45^{**}	0.00
Gender: female	0.33	(0.47)	0.33	(0.47)	0.00	0.00
Race/ethnicity: Latinx	0.16	(0.37)	0.23	(0.42)	-0.07^{**}	0.00
Race/ethnicity: Black	0.60	(0.49)	0.52	(0.50)	0.08^{**}	0.00
Race/ethnicity: White	0.40	(0.49)	0.46	(0.50)	-0.06^{**}	0.00
Disability	0.71	(0.45)	0.79	(0.41)	-0.08^{**}	0.00
Earned income (self-reported)	0.12	(0.33)	0.04	(0.20)	0.08^{**}	0.00
Intake Assessment Score and Questions						
Score	7.31	(3.48)	8.24	(3.53)	-0.93^{**}	0.00
Has planned activities for happiness and fulfillment	0.48	(0.50)	0.56	(0.50)	-0.08^{**}	0.00
Homelessness caused by relationship breakdown	0.54	(0.50)	0.58	(0.49)	-0.04^{*}	0.00
Has contact email	0.24	(0.43)	0.13	(0.34)	0.10^{**}	0.00
Has contact phone	0.65	(0.48)	0.51	(0.50)	0.14^{**}	0.00
Individual-Level Service Utilization						
DHS emergency/inpatient (Years -5 to -2)	0.28	(0.45)	0.37	(0.48)	-0.09^{**}	0.00
DHS emergency/inpatient (Year -1)	0.16	(0.36)	0.27	(0.44)	-0.11^{**}	0.00
DHS outpatient (Years -5 to -2)	0.30	(0.46)	0.35	(0.48)	-0.05^{**}	0.00
DHS outpatient (Year -1)	0.11	(0.32)	0.13	(0.34)	-0.02	0.00
DMH crisis stabilization (Years -5 to -2)	0.14	(0.35)	0.22	(0.41)	-0.07^{**}	0.00
DMH crisis stabilization (Year -1)	0.10	(0.30)	0.16	(0.37)	-0.06^{**}	0.00
DMH non-crisis service (Years -5 to -2)	0.47	(0.50)	0.58	(0.49)	-0.11^{**}	0.00
DMH non-crisis service (Year -1)	0.29	(0.46)	0.40	(0.49)	-0.10^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.14	(0.34)	0.19	(0.40)	-0.06^{**}	0.00
DHS Elixhauser/Charlson diagnosis (Year -1)	0.09	(0.29)	0.14	(0.35)	-0.05^{**}	0.00
DHS/DMH SUD diagnosis (Years -5 to -2)	0.16	(0.36)	0.22	(0.41)	-0.06^{**}	0.00
DHS/DMH SUD diagnosis (Year -1)	0.16	(0.37)	0.25	(0.44)	-0.09^{**}	0.00
DHS/DMH SMI diagnosis (Years -5 to -2)	0.28	(0.45)	0.38	(0.49)	-0.10^{**}	0.00
DHS/DMH SMI diagnosis (Year -1)	0.20	(0.40)	0.27	(0.45)	-0.08^{**}	0.00
TANF receipt (Years -5 to -2)	0.13	(0.34)	0.15	(0.35)	-0.01	0.00
TANF receipt (Year -1)	0.03	(0.18)	0.03	(0.18)	0.00	0.00
SNAP receipt (Years -5 to -2)	0.65	(0.48)	0.66	(0.47)	0.00	0.00
SNAP receipt (Year -1)	0.69	(0.46)	0.68	(0.46)	0.00	0.00
General Relief receipt (Years -5 to -2)	0.51	(0.50)	0.54	(0.50)	-0.02	0.00
General Relief receipt (Year -1)	0.50	(0.50)	0.50	(0.50)	0.00	0.00
Other HMIS enrollments (Years -5 to -2)	0.22	(0.41)	0.15	(0.35)	0.07^{**}	0.00
Other HMIS enrollments (Year -1)	0.14	(0.35)	0.09	(0.28)	0.05^{**}	0.00
Criminal legal involvement (Years -5 to -2)	0.64	(0.48)	0.73	(0.44)	-0.09^{**}	0.00
Criminal legal involvement (Year -1)	0.38	(0.49)	0.53	(0.50)	-0.15^{**}	0.00
Interim housing/street outreach (Year -5)	0.13	(0.34)	0.15	(0.36)	-0.02	0.00
Interim housing/street outreach (Year -4)	0.16	(0.36)	0.17	(0.38)	-0.01	0.00
Interim housing/street outreach (Year -3)	0.18	(0.38)	0.21	(0.40)	-0.03^{*}	0.00
Interim housing/street outreach (Year -2)	0.30	(0.46)	0.27	(0.44)	0.03	0.00
Interim housing/street outreach (Year -1)	0.68	(0.47)	0.59	(0.49)	0.09^{**}	0.00
DPSS homeless flag (Years -5 to -2)	0.63	(0.48)	0.66	(0.48)	-0.02	0.00
DPSS homeless flag (Year -1)	0.68	(0.47)	0.68	(0.47)	0.00	0.00
Project-Level Service Utilization						
Project Log Num Assessments	4.67	(0.83)	5.22	(1.59)	-0.55^{**}	0.00
Project Interim/Street Rate (Year -5)	0.05	(0.02)	0.08	(0.06)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -4)	0.05	(0.03)	0.09	(0.07)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -3)	0.06	(0.04)	0.11	(0.07)	-0.05^{**}	0.00
Project Interim/Street Rate (Year -2)	0.11	(0.06)	0.14	(0.09)	-0.03^{**}	0.00
Project Interim/Street Rate (Year -1)	0.34	(0.14)	0.41	(0.25)	-0.07^{**}	0.00
N	840		10,854			

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry. The comparison group consists of all individuals entering the CES who are experiencing homelessness over the same intake period as the TLS group, but they were not offered a permanent housing intervention within six months of their assessment.
**: $p < 0.01$; *: $p < 0.05$.

D Analysis by evidence of move-in

Table A8: Sample Characteristics, by Move-In

	Moved In		Not Moved In		Difference
	Mean	SD	Mean	SD	
Demographics					
Age	48.18	(13.69)	47.34	(13.39)	0.85
Gender: female	0.36	(0.48)	0.34	(0.48)	0.02
Race/ethnicity: Latinx	0.20	(0.40)	0.19	(0.39)	0.02
Race/ethnicity: Black	0.57	(0.49)	0.56	(0.50)	0.01
Race/ethnicity: White	0.39	(0.49)	0.40	(0.49)	−0.01
Disability	0.62	(0.49)	0.62	(0.49)	0.00
Earned income (self-reported)	0.24	(0.43)	0.21	(0.41)	0.03*
Intake Assessment Score and Questions					
Score	6.21	(3.15)	6.49	(3.24)	−0.28**
Has planned activities for happiness and fulfillment	0.46	(0.50)	0.47	(0.50)	−0.01
Homelessness caused by relationship breakdown	0.52	(0.50)	0.51	(0.50)	0.00
Has contact email	0.31	(0.46)	0.30	(0.46)	0.01
Has contact phone	0.75	(0.43)	0.73	(0.44)	0.02
Individual Level Service Utilization					
DHS emergency/inpatient (Years -5 to -2)	0.14	(0.35)	0.16	(0.37)	−0.02
DHS emergency/inpatient (Year -1)	0.08	(0.27)	0.11	(0.31)	−0.02*
DHS outpatient (Years -5 to -2)	0.22	(0.41)	0.22	(0.42)	0.00
DHS outpatient (Year -1)	0.09	(0.29)	0.13	(0.34)	−0.04**
DMH crisis stabilization (Years -5 to -2)	0.05	(0.22)	0.06	(0.23)	−0.01
DMH crisis stabilization (Year -1)	0.04	(0.20)	0.05	(0.23)	−0.01
DMH non-crisis service (Years -5 to -2)	0.22	(0.41)	0.25	(0.43)	−0.03
DMH non-crisis service (Year -1)	0.17	(0.38)	0.21	(0.41)	−0.04**
DHS Elixhauser/Charlson diagnosis (Years -5 to -2)	0.11	(0.31)	0.10	(0.30)	0.01
DHS Elixhauser/Charlson diagnosis (Year -1)	0.08	(0.27)	0.08	(0.28)	−0.01
DHS/DMH SUD diagnosis (Years -5 to -2)	0.07	(0.26)	0.08	(0.27)	0.00
DHS/DMH SUD diagnosis (Year -1)	0.09	(0.28)	0.10	(0.30)	−0.01
DHS/DMH SMI diagnosis (Years -5 to -2)	0.14	(0.35)	0.16	(0.37)	−0.02
DHS/DMH SMI diagnosis (Year -1)	0.12	(0.32)	0.13	(0.34)	−0.01
TANF receipt (Years -5 to -2)	0.09	(0.29)	0.09	(0.29)	0.00
TANF receipt (Year -1)	0.04	(0.18)	0.04	(0.19)	0.00
SNAP receipt (Years -5 to -2)	0.49	(0.50)	0.49	(0.50)	−0.01
SNAP receipt (Year -1)	0.45	(0.50)	0.48	(0.50)	−0.02
General Relief receipt (Years -5 to -2)	0.31	(0.46)	0.33	(0.47)	−0.02
General Relief receipt (Year -1)	0.26	(0.44)	0.32	(0.47)	−0.06**
Other HMIS enrollments (Years -5 to -2)	0.07	(0.26)	0.09	(0.28)	−0.01
Other HMIS enrollments (Year -1)	0.09	(0.29)	0.08	(0.27)	0.01
Criminal legal involvement (Years -5 to -2)	0.30	(0.46)	0.34	(0.47)	−0.05**
Criminal legal involvement (Year -1)	0.16	(0.37)	0.21	(0.41)	−0.05**
Interim housing/street outreach (Year -5)	0.04	(0.19)	0.05	(0.21)	−0.01
Interim housing/street outreach (Year -4)	0.04	(0.20)	0.05	(0.22)	−0.01
Interim housing/street outreach (Year -3)	0.05	(0.22)	0.06	(0.24)	−0.01
Interim housing/street outreach (Year -2)	0.09	(0.28)	0.11	(0.31)	−0.02*
Interim housing/street outreach (Year -1)	0.30	(0.46)	0.33	(0.47)	−0.03*
DPSS homeless flag (Years -5 to -2)	0.39	(0.49)	0.40	(0.49)	−0.02
DPSS homeless flag (Year -1)	0.39	(0.49)	0.44	(0.50)	−0.05**
Project Level Service Utilization					
Project Log Num Assessments	4.56	(0.79)	4.70	(0.88)	−0.14**
Project Interim/Street Rate (Year -5)	0.04	(0.02)	0.04	(0.02)	0.00
Project Interim/Street Rate (Year -4)	0.04	(0.03)	0.05	(0.03)	0.00**
Project Interim/Street Rate (Year -3)	0.05	(0.03)	0.06	(0.04)	0.00**
Project Interim/Street Rate (Year -2)	0.09	(0.06)	0.10	(0.06)	−0.01**
Project Interim/Street Rate (Year -1)	0.31	(0.15)	0.32	(0.13)	−0.01**
N	2,277		1,400		

Note: Individuals are also balanced into three geographic regions, quarter of entry, and year of entry.
**: $p < 0.01$; *: $p < 0.05$.

Table A9: Impacts of TLS on incidence of Street Outreach or Interim Housing, by move-in

	Impacts		Impacts/Base Rate	
Time period	Move-In	No Move-In	Move-In	No Move-In
Post-assessment (annualized)				
Year = 1				
Base Rate	0.212	0.230		
Impact	-0.098^{**}	-0.015	$-0.464^{**(NN)}$	$-0.066^{(MM)}$
	(0.009)	(0.012)	(0.033)	(0.053)
Year = 2				
Base Rate	0.169	0.187		
Impact	-0.080^{**}	-0.025^{*}	$-0.474^{**(NN)}$	$-0.135^{*(MM)}$
	(0.008)	(0.011)	(0.038)	(0.057)
Year = 3				
Base Rate	0.134	0.150		
Impact	-0.048^{**}	-0.026^{**}	$-0.356^{**(N)}$	$-0.176^{**(M)}$
	(0.007)	(0.010)	(0.049)	(0.064)
Year = 4				
Base Rate	0.107	0.120		
Impact	-0.038^{**}	-0.015	$-0.360^{**(NN)}$	$-0.129^{(MM)}$
	(0.007)	(0.009)	(0.054)	(0.072)
Post-assessment (cumulative)				
Base Rate	0.371	0.404		
Impact	-0.132^{**}	-0.026	$-0.354^{**(NN)}$	$-0.064^{(MM)}$
	(0.011)	(0.014)	(0.026)	(0.035)
N: TLS	2,277	1,400		
N: Comparison	29,843	29,843		

Notes: This table includes entropy-balanced impacts for TLS recipients with evidence of move-in vs. those without evidence of move-in. Each group applies entropy balancing using all characteristics in Table 2.
**: $p < 0.01$; *: $p < 0.05$. NN/N and MM/M give p-values (0.01 and 0.05) for differences between the two groups.