Individual and Social Effects of Shelter for People Experiencing Homelessness: Evidence From Los Angeles County's Winter Shelters Program

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We exploit variation from shocks to shelter availability from Los Angeles County's winter shelters program to study the effects of shelter provision and sheltered (versus unsheltered) homelessness. We leverage records of homeless services to generate daily, site-level counts of shelter beds and occupants from 2014 to 2019. We pair shelter counts with block-level crime data and facility-level data on ER visits to assess the impacts of shelter on crime and health outcomes. Further, using enrollment-level homeless services data, we evaluate the effect of shelter on returns to homeless services, observed exits from homelessness, and observed mortality. While preliminary estimates suggest that street outreach may be just as effective as shelter in reducing an individual's likelihood of still being homeless 6-18 months in the future, our findings indicate that increased investment in shelter provision may be optimal given its ability to mitigate both private and social costs of homelessness.

JEL Classification: I38, H53, R2, H51, H75, K42

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1. Introduction

More than 650,000 citizens of the wealthiest country on earth sleep at shelters, in vehicles, and on sidewalks every night. If the homeless population in the United States were its own city, it would be the 25th largest in the country (tied with Boston). The state of California has a GDP greater than all but four of the world's countries. It is also home to more than a quarter of people experiencing homelessness in the United States. In Silicon Valley, self-driving cars pass people sleeping on sidewalks. In Los Angeles County, every day, property taxes generate $19 million in local revenue while five people die homeless.[1]

Homelessness is a highly visible economic problem affecting communities across the United States. For example, Oklahoma has experienced similar growth in per capita homelessness as New York and San Francisco since 2015, and Alaska has outpaced Seattle.[2] However, the issue seems particularly intractable in California where growth in (already high) housing costs has far outpaced the national average.[3]

Since 2019, California has spent $24 billion on homelessness, but over this same period, the state's homeless population increased by 20%.[4] As of 2023, California now accounts for 28% of the country's homeless population, and Los Angeles County alone accounts for nearly 12%. With these facts in mind, it is likely that the persistence of homelessness is less an issue of insufficient resources and more an issue of the inefficient allocation of resources. When policymakers lack the knowledge necessary to optimally direct their resources, economics must provide answers.

California has been both willing and able to spend billions of dollars every year to address homelessness. However, there is widespread disagreement about how those dollars should be spent because there is widespread disagreement about the causes of homelessness and the effectiveness of various interventions on which the funds may be spent. This disagreement is only exacerbated by a lack of evidence with which policymakers can arm themselves as they make the case for any proposed use of the funds.

One persistent point of disagreement is on the effectiveness of interim housing or temporary shelter. While New York and Los Angeles, as the localities with the largest homeless populations,[5] account for similar shares of the country's total homelessness, LA has an unsheltered population more than 12 times greater than NYC. Perhaps the fact that NYC remains the location with the largest homeless population is indicative of shelter's ineffectiveness. Alternatively, perhaps New

[1] See Mitchell et al. (2023).

[2] See https://endhomelessness.org/homelessness-in-america/homelessness-statistics/state-of-homelessness/.

[3] See California Legislative Analyst's Office, California Housing Affordability Tracker (2nd Quarter 2024), https://lao.ca.gov/LAOEconTax/Article/Detail/793; see also Sightline Institute, Homelessness is a Housing Problem, https://www.sightline.org/2022/03/16/homelessness-is-a-housing-problem/.

[4] See https://www.hoover.org/research/despite-california-spending-24-billion-it-2019-homelessness-increased-what-happened.

[5] As of the 2023 point-in-time count, New York City had a homeless population of 88,025, and Los Angeles County had a homeless population of 75,518, which was higher than the next seven localities combined. Both localities also have among the highest *rates* (per capita) of homelessness (see https://www.brookings.edu/articles/homelessness-in-us-cities-and-downtowns/).

York's high rates of shelter are responsible for its slower growth of homelessness.[6]

Proponents of expanding temporary shelter argue that it may serve as a low-cost alternative to more intensive homelessness interventions that is still effective in helping many individuals out of homelessness and that may mitigate the consequences of street (unsheltered) homelessness. Opponents argue that investment in temporary shelter diverts resources away from programs that may be more effective in achieving reductions in homelessness. These sound like reasonable arguments, but to-date, no study has been able to identify the effects of shelter in a way that would allow for a comparative assessment of cost-effectiveness.

Los Angeles does not have enough shelter beds to serve its entire homeless population, and this has been true for at least a decade. Should localities expand shelter supply to more completely accommodate their homeless populations as New York has? Through accounting exercises, policymakers can estimate the costs of such expansions, but to-date, there exists little research to speak to the benefits. Would expanding shelter reduce homelessness? Would it improve the welfare of the most economically disadvantaged members of society? Would it reduce the social costs of homelessness?

In Los Angeles, more than 25% of all enrollments in homeless services are for emergency shelter, second only to street outreach (at just over 30%) and more than all other housing projects combined. We quantify the effects of this previously understudied but abundantly used intervention and provide among the first answers to these questions, finding that shelter reduces crime incidents, ER visits for psychiatric conditions, and (observable) mortality. We do not observe evidence that temporary shelter is any more effective than street outreach in reducing homelessness. With these findings, local leaders can begin determining whether increasing emergency shelter is a cost-effective policy intervention.

2. Literature Review

Rigorous assessments of homelessness interventions are relatively uncommon due largely to data limitations. It is challenging to use survey data because survey participants tend to be recruited through their home address (e.g., by mail). Even in nonpublic, administrative data, it is often challenging, at best, to identify when an individual is homeless. Further, the population is highly mobile, and it is common that even case managers are unable to locate homeless clients.

In the absence of data conducive to quasi-experimental methods, researchers have relied on surveys and experiments for causal identification of homelessness interventions.[7] Additionally, due to the challenges of obtaining longitudinal data on individuals who have no permanent address by which they can be identified, work on housing insecurity or homelessness prevention (upstream) tends to be more feasible.[8] Such studies are important, but they leave a gap in the literature

[6] NYC homelessness has risen by roughly 30% since 2014. Over the same period, LA homelessness nearly doubled.

[7] See, for instance, Phillips and Sullivan (2023), Gulcur et al. (2003), Evans, Phillips and Ruffini (2021) (and studies within), and developing work at the Notre Dame Lab for Economic Opportunities as well as the USC BIG:LEAP study.

[8] See, for instance, Phillips and Sullivan (2023) and Von Wachter et al. (2021).

where comparatively little attention has been given to the evaluation of downstream homeless interventions (i.e., interventions that target people already experiencing homelessness and designed to increase exits from homelessness).

Perhaps the most notable exception is the recent work of Cohen (2024), who leverages administrative records of homeless services and a judge (case manager) fixed effects design to evaluate the impacts of permanent housing interventions. Appropriately in his setting, and like most of the extant literature evaluating more intensive interventions (e.g., Gubits et al. (2018) and Culhane, Metraux and Hadley (2002)), the counterfactual group is primarily composed of people in temporary shelter. As a result, these studies identify the effect of interventions relative to the effect of shelter.[9] However, to our knowledge, no study has identified the effects of shelter on the trajectories of people experiencing homelessness (PEH). Experimental methods would need to overcome several (potentially insurmountable) hurdles, including recruitment of homeless participants outside of shelter, ethically introducing random assignment to shelter, reliably ensuring compliance,[10] and following up with participants who regularly vanish even from administrative records. Quasi-experimental methods would require sufficient data on both sheltered and unsheltered PEH, a source of plausibly exogenous variation to leverage, and an ability to identify individuals over time.

Recent work like that of Ward, Garvey and Hunter (2024) and Kuhn, Henwood and Chien (2023) has had some success in the recruitment of unsheltered participants in Los Angeles. Such work is instrumental in understanding the unsheltered population, especially in a location where the overwhelming majority of PEH are unsheltered. These studies and other descriptive analyses of the homeless population to supplement administrative records such as Kushel and Moore (2023) and Meyer, Wyse and Corinth (2023) as well as insights shared with us by service providers in Los Angeles have been crucial to our understanding of the coverage and reliability of administrative records on homeless services - the primary data source in our study.

We are aware of one other study that assesses the impact of temporary shelter. In criminology, Faraji, Ridgeway and Wu (2018) also leverage seasonal variation in available shelter in Vancouver, finding that the opening of shelters introduces negative externalities in the form of increased monthly property crime close to shelter sites. Our approach is conceptually similar but has several advantages. First, Faraji, Ridgeway and Wu (2018) are unable to assess impacts on additional crime types (as they lack data on offenses against a person) or other outcomes. Second, at the peak of their sample period, the homeless population in Vancouver is under 2,000 (limiting the scale of any observable localized effect of the intervention), and they lack data on individuals experiencing homelessness, preventing an assessment of the impacts of shelter on individual outcomes.

In our setting, we are able to leverage variation (in shelter beds, not just sites) across locations *and* over time at a daily level, affecting a homeless population more than 30 times as large and over

[9] This leads to the common misconception that such studies can be interpreted as evidence that shelter is ineffective (because it is, by construction, ineffective *relative* to the treatment at hand).

[10] It would need to be the case that those who are not randomly assigned to shelter do not find shelter elsewhere, which would be both legally and practically unenforceable.

a geography almost 100 times the size of Vancouver. We are able to assess the impacts of shelter on aggregate-level outcomes, including crime, but we also incorporate nonpublic data on ER visits and hospitalizations to assess impacts on health outcomes. Further, our administrative data allows for the construction of a measure of occupancy, allowing us to compute both intent-to-treat and local average treatment effects on such outcomes.[11] Finally, our use of individual enrollment-level data containing records of people experiencing unsheltered homelessness permits a first-of-its-kind causal evaluation of the effects of shelter on the trajectories of *individuals* experiencing homelessness.

Finally, Richards and Kuhn (2023) provide a review of the literature on the role of unsheltered homelessness, specifically. They present evidence that people experiencing unsheltered homelessness tend to experience worse health outcomes, but they note a need for research identifying causal mechanisms. To what extent are the outcomes faced by those who are unsheltered the direct result of their lack of shelter, and how much is the result of correlated factors that lead them to be unsheltered? Importantly, if policymakers are to understand the effect of expanding shelter and reducing unsheltered homelessness, we must know the causal effect of shelter, not merely the observed outcomes of those who are in shelter contrasted against the observed outcomes of those who are not.

In summary, homelessness research, in general, is sparse, and the causal research that does exist focuses largely on upstream prevention (like housing subsidies) or more intensive interventions (like permanent supportive housing). If we are to understand how best to allocate the billions of dollars directed to homelessness services annually, it is critical that we understand the effects of one of the most widely used homelessness interventions.

3. Research Questions

A primary objective of this research is to understand the role that shelter plays in an individual's trajectory through homelessness. Our intent is to focus on the most actionable findings for policymakers to make use of immediately. Broadly, we seek to answer the following:

- Does shelter help people out of homelessness? If so, is it more cost-effective than alternative interventions?
- Does shelter meaningfully protect people experiencing homelessness from the consequences of homelessness (e.g., crime, health)? If so, should policymakers consider it as a cost-effective criminal justice and/or public health intervention?

Our analysis simultaneously sheds light on the consequences of unsheltered homelessness, specifically, providing valuable insights to policymakers concerned about public safety and related externalities. For instance, does shelter reduce the consequences of homelessness for law enforce-

[11] Additionally, the first-stage effect of shelter on the number of people in shelter answers a non-trivial policy question (do people enter shelter when it's provided?).

ment? Hospitals? If so, is shelter provision justifiable on the basis of its ability to mitigate negative externalities?

Finally, leveraging extensive administrative data, what basic facts can we provide about shelter, especially utilization? Is there credibility to the argument that people experiencing homelessness don't want shelter and won't use it even if it's provided? If so, policymakers should know that it is wasteful. Alternatively, do we see immediate spikes in shelter enrollment when a shelter opens? If so, policymakers should be aware that shelter demand likely exceeds supply, and we can put the argument that PEH "don't even want shelter" to rest.

4. Background

4.1. Emergency Shelter

Emergency shelter is a broad category of intervention that may refer to anything from a nightly, congregate setting (e.g., simple beds available at an armory for a place to shelter overnight) to somewhat more intensive, single-unit settings (e.g., hotel rooms). Importantly, shelter is "interim housing" and not designed to be a permanent solution to one's homelessness.

Shelter provision may vary widely across locations and over time as it is largely dependent on executive decisions of city and county leaders. Some leaders, like San Jose Mayor Matt Mahan, have chosen to invest more heavily in shelter, and as a result, San Jose boasts among the highest reductions in unsheltered homelessness in the state. However, many are critical of this strategy, characterizing the decision to shift funding from permanent housing interventions to expand shelter as "ultimately a losing game." Extant literature like the recent work of Cohen (2024) supports the claim that more intensive programs are more effective in achieving homelessness reductions. Does this mean that proponents of increasing the share of funding allocated to shelter are wrong and that the opportunity cost of investment in shelter is unjustifiably high? This is not necessarily the case as research to-date has not addressed two related considerations.

1. Is shelter more cost-effective in achieving homelessness reductions? In other words, while intensive interventions are more effective in achieving sustained exits from homelessness, they tend to be far more costly, meaning shelter may, in fact, be more effective per dollar spent.

2. Is shelter cost-effective in reducing the consequences (negative externalities) of homelessness? In other words, even if other interventions are more effective in reducing homelessness, if unsheltered homelessness is significantly more socially costly than sheltered homelessness, greater shelter investment may be rational from a social welfare perspective.

If the answer to both of the above questions is "no," then policymakers ought to divert funds from shelter interventions towards other interventions like permanent supportive housing. However, to preview our results, we will show that while the answer to the first question is "no," the answer to the second is almost certainly "yes." As a result, our findings serve to promote a more informed policy debate and a better understanding of the implications of the allocation of homelessness resources. We provide further discussion of our results in Section 7.

4.2. Shelter in Los Angeles

To start to illustrate the state of homelessness in Los Angeles during our time period of observation, we begin with the depiction available from national-level data. There are two primary reports that the Department of Housing and Urban Development (HUD) requires regions across the country (organized as "Continuums of Care" or roughly counties) to submit. The first is the Point-in-Time (PIT) count of homelessness, which is a count of sheltered and unsheltered people experiencing homelessness on a single night in January. According to the PIT data, 34,393 people in Los Angeles were experiencing homelessness in 2014, the first year of our study.[12] Approximately 22,590 were unsheltered, and 6,214 of those who were sheltered were in emergency shelters.[13] By 2019, the final year of our study, the homeless population in Los Angeles had risen to 56,257, with 42,471 being unsheltered, and 10,834 of those sheltered residing in emergency shelters. It is worth noting that PIT counts likely drastically understate the true scale of homelessness, primarily because they are conducted only once annually. In fact, administrative records collected by California's Interagency Council on Homelessness indicate that the number of people who experienced homelessness in California in 2023 was >85% higher than the PIT count reported.[14]

The other primary report that regions must provide is the Housing Inventory Count (HIC), which provides a sense of the provision of shelters throughout the region. In 2014, Los Angeles offered 4,798 emergency shelter beds.[15] These emergency shelters were offered by 66 organizations across 108 projects or facilities. The average emergency shelter project had 56 beds and housed 49 people at the time of the HIC and PIT counts, respectively. In 2019, Los Angeles offered 10,967 emergency shelter beds.[16]

4.3. LA's Winter Shelter Program

Not all shelters in Los Angeles are open year-round. The Los Angeles Homeless Services Authority (LAHSA) runs a countywide program that temporarily expands shelter supply by more than 1,000 beds (serving several thousand unique individuals) annually.[17] The sudden changes in availability (opening and closing) of shelters serve as shocks to the number of people who are

[12] HUD Exchange, PIT and HIC Data Since 2007, https://www.hudexchange.info/resource/3031/pit-and-hic-data-since-2007/.

[13] Individuals in "transitional housing" are considered sheltered homeless and comprise most of the remaining sheltered population recorded by PIT.

[14] See https://bcsh.ca.gov/calich/hdis.html.

[15] HUD's 2014 Housing Inventory Count Report, CoC Number CA-600, `https://files.hudexchange.info/reports/published/CoC_HIC_CoC_CA-600-2014_CA_2014.pdf`.

[16] HUD's 2019 Housing Inventory Count Report, CoC Number CA-600, `https://files.hudexchange.info/reports/published/CoC_HIC_CoC_CA-600-2019_CA_2019.pdf`.

[17] LAHSA reports just under 7,000 people served by the program in the 2013-2014 season (see http://documents.lahsa.org/Planning/2014/CoCMeetings/WinterShelterProgram-FY2013-2014.pdf.), which represents the *earliest* dates in our choice sample.

sheltered (or unsheltered). Notably, shelter locations and number of beds vary both *within* and *across* years. Moreover, opening and closing dates vary both across years and across locations *within years*. Finally, except in rare circumstances, these shelters are open from 5 P.M. to 7.A.M.

Importantly, the sites that will operate and their dates of operation are planned months in advance of the winter season. Thus, the number of beds, locations, and opening dates are not determined endogenously in the days or weeks leading up to their opening.[18]

5. Aggregate-level Analysis

5.1. Data

Our empirical approach aggregates data on homelessness from Los Angeles's Homeless Management Information System, data on crime from the L.A. Police and Sheriff's Departments (LAPD and LASD), and data on hospital utilization from the California Department of Health Care Access and Information (HCAI).

5.1.1. Homelessness Management Information System (HMIS)

The Los Angeles Homeless Services Authority (LAHSA) is responsible for collecting and maintaining the county's records of interactions with most homeless service providers. The federal government mandates the use of the Homelessness Management Information System (HMIS) by any entity that receives any amount of federal funding (directly or indirectly) for the provision of homeless services. Additionally, even service providers who are not required to use HMIS[19] may opt in to using HMIS.[20] Because Los Angeles County is so large, LAHSA divides its operation into eight geographic areas called Service Planning Areas (SPAs) to target services to the specific needs of residents in different areas.[21] We follow this structure in our analysis and show the geographical division of SPAs in Figure 1 below.[22]

[18] For instance, in 2015, LAHSA issued a request for proposals for the program in July and announced funding recommendations in September. However, closing dates were extended by a month for most sites in the 2016-2017 season and 2017-2018 season.

[19] It seems rare that a service provider would not receive some amount of federal funding, but we cannot rule out the possibility that, for instance, a church operates its own nightly shelter program using its building and relying on private donations for funding.

[20] Anecdotally, we have been told that this *does* happen, but we have no data to estimate exactly how common it is in practice.

[21] Los Angeles County Public Health Department, What Is a Service Planning Area?, http://publichealth.lacounty.gov/chs/SPAMain/ServicePlanningAreas.htm

[22] Source: LA County Public Health (http://publichealth.lacounty.gov/chs/Docs/CITIES-FINAL.pdf).

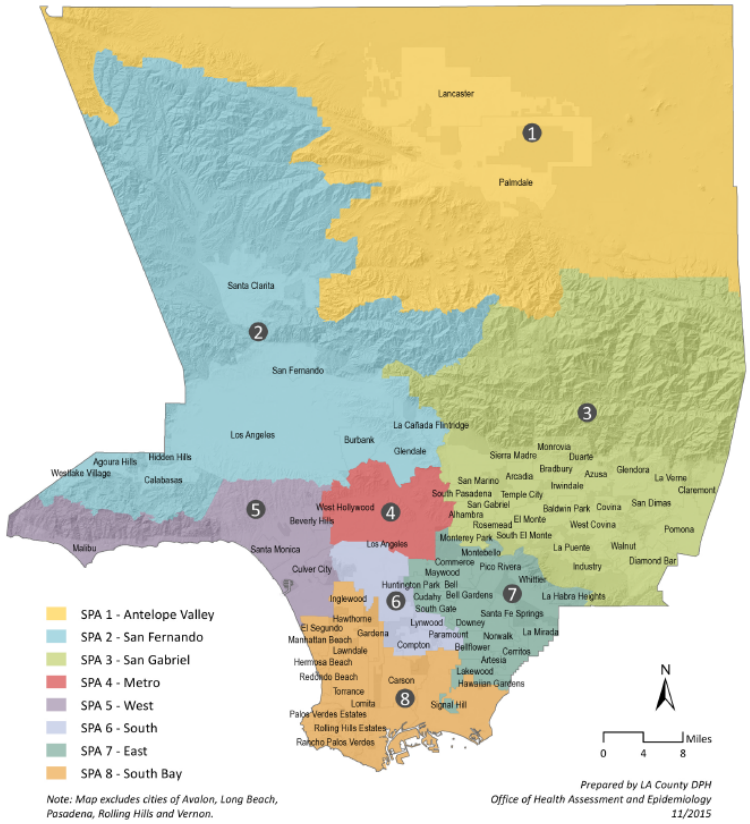

Figure 1: Los Angeles County's Eight Service Planning Areas (SPAs)

An advantage of studying Los Angeles is that even county subdivisions are large (equivalent in population to large cities). For instance, in 2017, the *least* populous SPA is still home to nearly 400,000 people (comparable in size to New Orleans). Similarly, the SPA with the *least* homelessness, according to PIT counts, recorded more than 3,500 PEH (comparable to the homeless population of Atlanta). In other words, from a population standpoint, we are comparing 8 geographic areas ranging in size from New Orleans to (nearly) Houston, *each* with homeless populations comparable to those of Atlanta to as high as (above) Seattle.[23]

Through an agreement with the California Policy Lab facilitated by the Homelessness Policy Research Institute (HPRI),[24] LAHSA has made de-identified HMIS records available to approved, qualified researchers, making Los Angeles one of the few places in the country where administrative data on homelessness can be leveraged by researchers to provide objective, third-party evaluations of homelessness programs.[25]

The HMIS records include the date of entry into a "project," the type of project (including

[23]Counts sourced from LAHSA via LA Almanac, LA Public Health, and Census.

[24]HPRI is a USC-based collaborative composed of service providers, individuals with lived experience, and social scientists from many of the top research institutions in California and beyond.

[25]A key recommendation of our research is that localities across California and the United States follow LA's example in solution-oriented data transparency.

"emergency shelter"), the ZIP code of the service provider for each project,[26] the last known number of beds available at the project, an indicator for whether a shelter project is "seasonal," and the date of exit from the project. Thus, we can construct site-by-day counts of shelter beds and shelter occupancy, including variables for the location (SPA) of the shelter and whether it is a "seasonal" shelter.[27]

While LAHSA makes annual announcements regarding the opening and closing dates of winter shelters, these reports are imperfect,[28] and the HMIS data does not contain reliable flags for whether a shelter is "opened" or "closed." To resolve this, we identify shelter opening dates based on when records of individuals entering the shelter begin and closing dates based on when the shelter is vacant.[29] To handle outliers in which shelters report implausible numbers of occupants for their bed count, we impose a 120% cap on occupancy such that shelters with a higher occupancy rate are revised down to have a 120% occupancy rate.[30] Our full data cleaning approach is documented in Online Appendix III.

After imposing these occupancy rules and identifying shelters as open or closed, we aggregate across sites to produce daily counts of shelter beds and people in shelter by SPA for 2014-2019.[31] Figure 2 presents daily counts of shelter beds in Los Angeles. Counts in light blue are counts of seasonal shelter beds. Vertical black lines denote January 1 of each year. Figure 3 presents the corresponding person counts.[32] As is documented in Appendix A, the spikes in bed and person counts generally closely correspond to the exact opening and closing dates gathered form public

[26]Note that this may be different from the ZIP code of the shelter site (if a service provider operates from an office in one location and runs a shelter in a separate location). However, inspection of the data suggests that ZIP codes of winter shelter project service providers tend to match publicly available records of winter shelter addresses in many cases, and when ZIP codes do not match, it appears that the broader geographic location (SPA) does (i.e., it looks like shelter providers rarely, if ever, operate a shelter outside of the SPA in which they are headquartered, even if they operate from a different ZIP code). The upshot is that the ZIP code in the data appears to represent the true site location inconsistently, but the SPA in the data seems to correspond to the true site location in almost all cases. Therefore, our preferred specifications use SPA as the geographic unit of analysis.

[27]Pre-COVID, as far as we can tell, "seasonal" exclusively refers to winter shelters. Beginning in 2020, other temporary shelters start appearing (outside of winter months) with seasonal flags.

[28]For instance, LAHSA announces when a site is *supposed* to open and close and does not always retroactively note deviations from schedule (such as delayed openings).

[29]Unfortunately, due to poor exit recording (Meyer, Wyse and Corinth (2023)), a shelter that is truly vacant may be *recorded* to have a few occupants, presenting a challenge for this method of identifying openings and closings. So, more specifically, our solution is to categorize a shelter as closed if (1) the shelter operates below 15% occupancy for three consecutive weeks (21 straight days) or (2) the exact number of occupants has remained unchanged (and at less than half the beds available) for at least 60 days. For a more detailed discussion of the data cleaning procedures, see Online Appendix III.

[30]As we show in Table A.2, our results are robust to other occupancy rate caps.

[31]We have been advised of extensive data quality issues prior to 2014, and we restrict our analysis to no later than 2019 to avoid potential confounding effects of the COVID-19 pandemic, leaving us with 6 full years of data.

[32]Note that our records of shelter beds and people in shelter on a given date reflect slightly lower counts than those reported in HUD's annual counts. There are three factors that explain the undercounts. First, HUD's counts of people and beds are recorded for all service providers, regardless of whether those service providers report to HMIS (e.g., a shelter operating without the support of federal funding is not required to record their data in HMIS but would be counted during HUD's annual counts). Second, we exclude "day shelter" (see Table 3) beds and enrollments from the construction of our counts. If "day shelter" is included in HUD's definition of "emergency shelter" for the purpose of annual counts, those counts will naturally be higher. Third, in the course of data cleaning, poor record quality causes us to drop a handful of shelters from our sample altogether.

records of the program's operation (see, in particular, discussion of Figure A.3).[33]

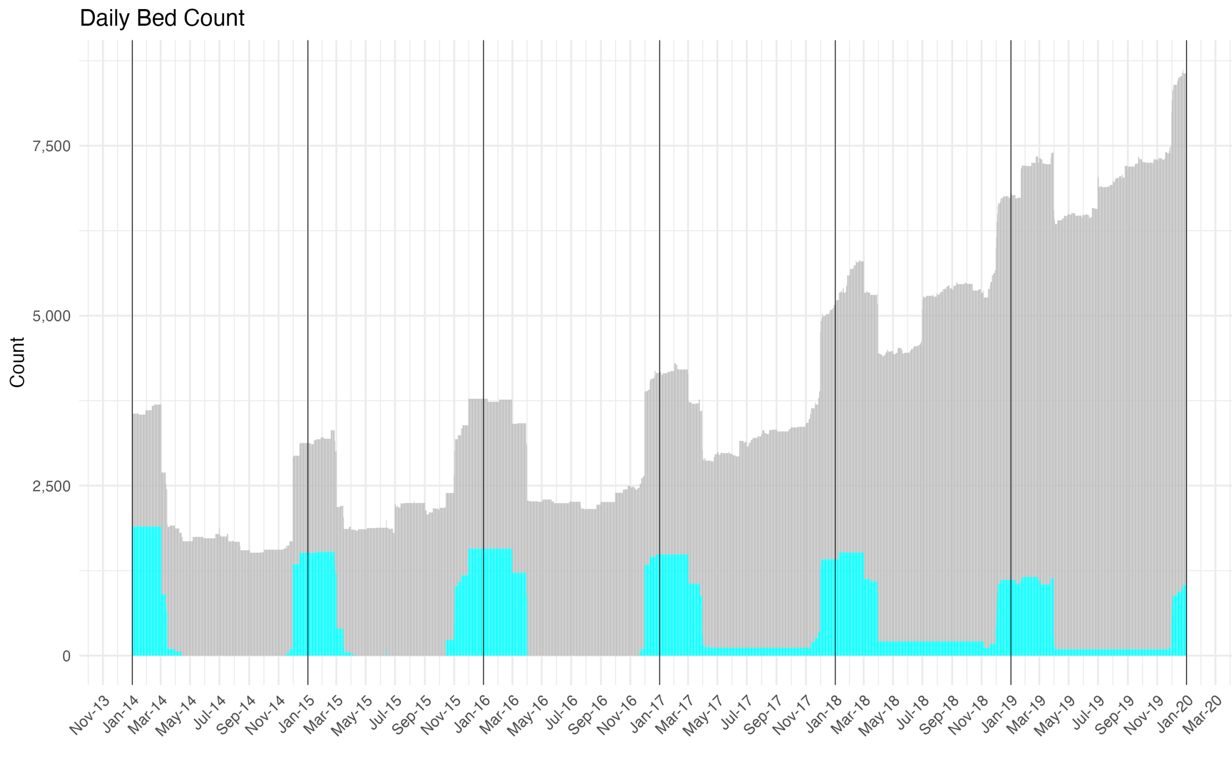

Figure 2: Daily Bed Count by Winter Shelter (Blue) vs. Other Shelter (Grey)

[33]See, for example, Los Angeles Homeless Services Authority, 2015-16 Winter Shelters Program, https://file.lacounty.gov/SDSInter/dmh/236341_WinterShelterLACounty2015-2016.pdf

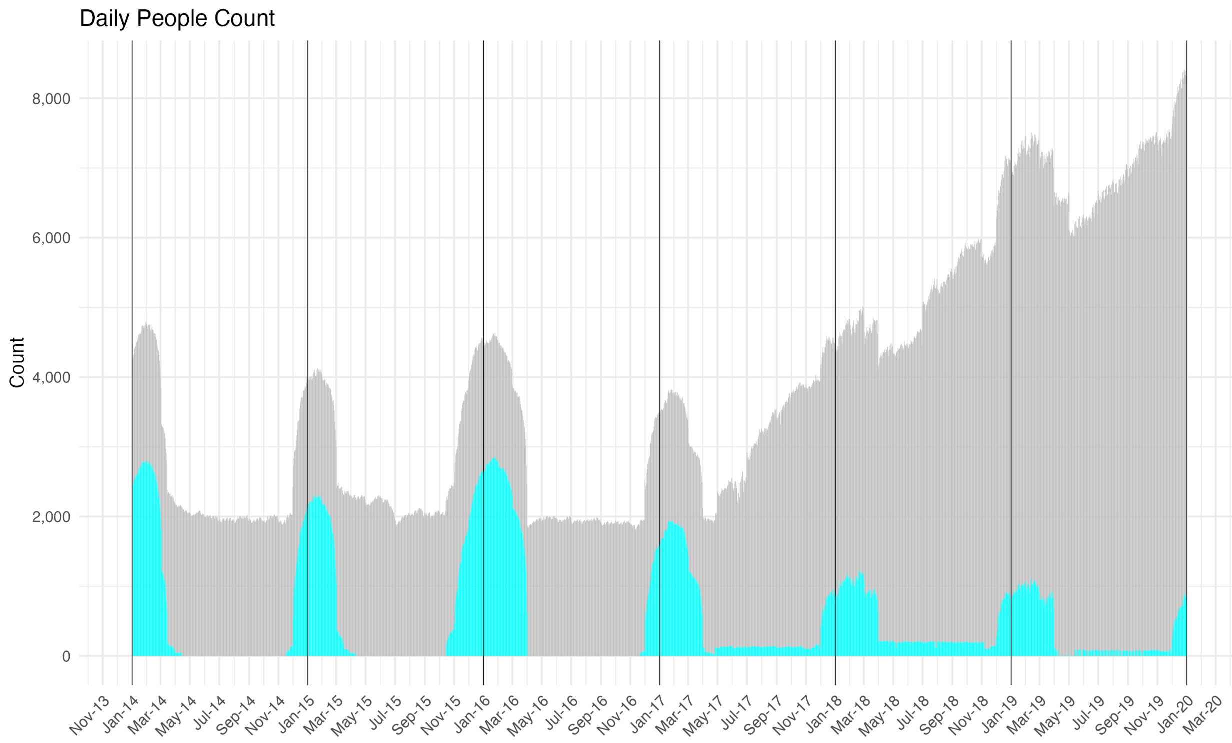

Figure 3: Daily People Count by Winter Shelter (Blue) vs. Other Shelter (Grey)

Because our empirical approach makes use of geographic variation, we provide figures with these counts for each of the county's 8 Service Planning Areas.

Figure 4: Daily Bed Count by SPA and Winter Shelter (Blue) vs. Other Shelter (Grey)

5.1.2. LAPD and LASD

The first outcome we consider is crime. Both the Los Angeles Police Department (LAPD) and the Los Angeles Sheriff's Department (LASD) make incident-level crime records publicly available. There are some discrepancies across agencies in the variables provided, but crucially, the data from both agencies include, for each known crime incident, date and time, coordinates (with block-level precision), and crime category.[34] Using geocoding software, we map all coordinates to ZIP codes and crosswalk those ZIP codes to Service Planning Areas. We then combine records from the two agencies and generate daily crime counts by SPA. Figure 5 plots these counts for the full county, and Figure A.2 breaks the counts out by SPA.[35]

5.1.3. HCAI

The second set of outcomes involves hospital utilization. We acquired non-public, de-identified, encounter-level data on ER visits and hospital admissions from the California Department of Health Care Access and Information (HCAI). This data includes records of all inpatient hospitalizations and ER visits at California-licensed hospitals. Importantly, for all records, the date and exact facility are recorded. Each record also includes up to 25 diagnostic codes recorded by the physician.

[34]Categories are not consistent across agencies and must be unified.

[35]We exclude the first day of each year in this figure as departments frequently report backlogged counts on this day, causing spikes on the scale of 2,000-3,000 crimes reported in one day.

Because people experiencing homelessness are overwhelmingly over-represented among patients with psychiatric diagnoses,[36] we restrict to ER visits for psychiatric conditions.[37] We map facilities to Service Planning Areas based on facility ZIP code[38] and generate SPA-by-day counts of psych ER visits. We do not impose demographic restrictions, and these counts do not distinguish between ER visits where a patient was or was not subsequently admitted to inpatient care. Moreover, cells smaller than 15 are censored for confidentiality. Daily counts of psychiatric ER visits are shown in Figure 5.

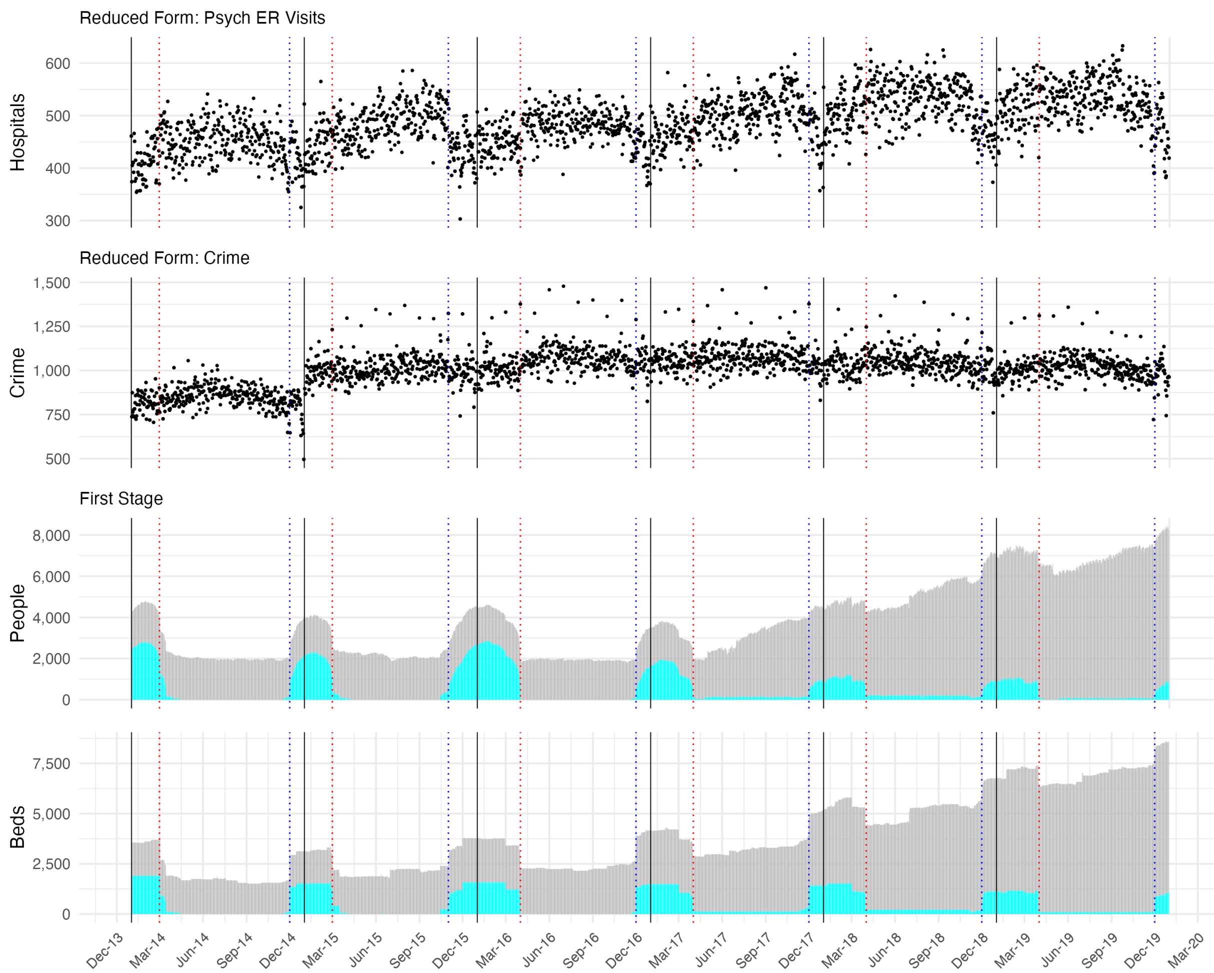

Figure 5: (From bottom to top) Daily counts of shelter beds, people in shelter, crime, and psych ER visits.

[36]In our sample period, hospital admissions for patients who are recorded to be homeless are more than 5 times as likely to be for mental health conditions than admissions for housed patients.

[37]A visit is defined to be for a psychiatric condition if the principle diagnosis corresponds to an ICD-10 code starting with "F" or codes in the range "R44-46."

[38]Rarely, a facility will appear in a different SPA than the one occupied by most of its ZIP code (because ZIP codes do not map perfectly to service planning areas). In these cases, SPA is determined by ZIP code.

5.2. Empirical Approach

We estimate the effect of shelter using a instrumental variables approach that leverages the variation in timing and location of the supply of (winter) shelter beds to eschew any potential confounding effects of seasonal trends in outcomes. The equation that we would like to estimate is the following:

$$Y_{symd} = \psi_{sy} + \mu_m + \delta_d + \beta_1 \textit{people sheltered}_{symd} + \varepsilon_{symd} \tag{1}$$

where Y_{symd} represents some outcome Y in year y, month m, day d, and SPA s, and $\textit{people sheltered}_{symd}$ is the number of individuals sheltered. ψ_{sy} are SPA-Year fixed effects, μ_m are month fixed effects, and δ_d are day fixed effects.[39] We are primarily interested in β_1: the effect of an additional person sheltered on outcome Y.

To resolve potential issues of endogeneity in this equation, we instrument for people sheltered with the number of shelter beds offered. We now run a first-stage regression. Here, "people sheltered" is the dependent variable, and the number of winter shelter beds captures the plausibly exogenous shock to shelter in a given location s on a given date ymd.

$$\begin{aligned} \textit{people sheltered}_{symd} = {} & \psi^f_{sy} + \mu^f_m + \delta^f_d \\ & + \gamma_1 \textit{other beds}_{symd} + \gamma_2 WS\ \textit{beds}_{symd} + u_{symd} \end{aligned} \tag{FS}$$

$$\begin{aligned} Y_{symd} = {} & \psi^r_{sy} + \mu^r_m + \delta^r_d \\ & + \beta_1 \textit{other beds}_{symd} + \beta_2 WS\ \textit{beds}_{symd} + \varepsilon_{symd} \end{aligned} \tag{RF}$$

In other words, the reduced-form estimates are intent-to-treat effects, which might actually be of greater interest to policymakers, but there is value in estimating both effects. As we'll see, because take-up of treatment is nearly 1 (putting to rest the argument that "people don't even want to be inside"), IV estimates are very close to ITT estimates, anyway.

5.3. Results

Results of regressions from our first-stage and reduced-form equations are presented in the following tables. In each table, the first column reports estimates from the first-stage regression. Subsequent columns report reduced-form and IV estimates (denoted by RF and IV, respectively).

[39] We include SPA-year fixed effects to control for different settings and treatment intensities across SPAs and years suggested by Figures 4 and A.1. We include month-level fixed effects to control for seasonality, and we incorporate day-level fixed effects to account for potential bunching in reporting backlogged data across agencies as well as potentially income shocks associated with certain parts of the month, such as the first of the month when individuals may receive government benefits and owe rent.

5.3.1. Crime

The first column of Table 1 presents the first stage results for the effect of an additional shelter bed on the number of sheltered people. Subsequent columns provide reduced form and IV estimates of the effects of shelter on all crime, daytime (7am-5pm) crime, and night (5pm-7am) crime. We drop SPAs 3, 7, and 8 which are predominantly not covered by LAPD or LASD crime records; however, our results are robust to the inclusion of all SPAs (see Table A.1).[40]

There is a strong first-stage effect for winter shelter beds with a coefficient estimate exceeding 0.8 with an F-statistic of 4336. This means that for every 100 additional shelter beds provided (on a given day, in a given SPA), there are 83 additional people in shelter. Taken together with Figures 2 and 4, these results indicate that shelter provision leads to rapid take-up and sustained utilization. In other words, in our setting, our results strongly contradict the notion that people experiencing homelessness do not want shelter or that they *prefer* to sleep outside. When shelter beds are added, more than 80% of them are filled.

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
other beds	0.6965^{***}	-0.0032		-0.0028		-0.0004	
	(0.0082)	(0.0030)		(0.0019)		(0.0020)	
WS beds	0.8313^{***}	-0.0092^{***}		-0.0022		-0.0070^{***}	
	(0.0142)	(0.0031)		(0.0022)		(0.0020)	
sheltered			-0.0111^{***}		-0.0026		-0.0085^{***}
			(0.0038)		(0.0026)		(0.0024)
Outcome Mean	528	154	154	73	73	80	80
Adj. R^2	0.9864	0.9430	0.9429	0.8840	0.8840	0.9131	0.9131
Num. obs.	10,955	10,955	10,955	10,955	10,955	10,955	10,955

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 1: The Effect of Winter Shelters on Crime (Daytime and Nighttime)

Our estimates indicate that the addition of 100 shelter beds prevents just under 1 crime per day and similarly, that sheltering 100 additional people prevents about 1 crime per day. At first glance, this may appear small. However, recall that, during this period, LAHSA's winter shelter program operates around 1,500 beds per day for roughly 4 months every year. So, in other words, in total, the program prevents nearly 15 crimes every day it operates or more than 1,500 crime incidents every year.

Because the crime data also includes the time of the incident, we can split the crime data into incidents that occur outside of the standard hours of winter shelter operation (i.e., from 7 A.M. to 5 P.M.) and those that occur during the usual hours of operation (5 P.M. to 7 A.M.). We refer to these as "daytime crime" and "night crime," respectively. Consistent with expectations, regression estimates in columns (4) - (7) reveal that the observed crime reduction is driven almost entirely by

[40] These SPAs are generally covered by the records of separate city-level police records to which we do not currently have access.

reductions in crime that occurs a night - i.e., when the shelters are operating.[41]

The observed reductions in crime may be the result of reductions in crimes committed by people who would otherwise be unsheltered. However, high rates of victimization among PEH (see, for instance, Padwa et al. (2024)) suggest that reduced crime rates may also be indicative of reduced victimization of PEH.

5.3.2. ER Visits

Table 2 presents the reduced form and IV estimates of the effects of shelter on psychiatric ER visits, with the first column providing the first-stage estimate for reference.[42]

	FS	RF	IV
other beds	0.7544^{***}	0.0045^{***}	
	(0.0079)	(0.0011)	
WS beds	0.8926^{***}	-0.0024^{**}	
	(0.0085)	(0.0009)	
sheltered			-0.0027^{**}
			(0.0011)
Outcome Mean	391	61	61
Adj. R^2	0.9860	0.9260	0.9261
Num. obs.	17,528	17,528	17,528

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 2: The Effect of Winter Shelters on Psychiatric ER Visits

Results from the reduced-form estimation indicate that providing 400 additional (winter) shelter beds prevents 1 ER visit for psychiatric conditions every day. Similarly, IV estimates indicate that sheltering 400 additional people prevents just over 1 such visit per day. Extrapolating to the full winter shelters program, on average, LAHSA's shelter providers are responsible for the prevention of over 300 ER visits for psychiatric conditions alone each year.

6. Individual-level Analysis

Recall that the HMIS data is enrollment-level data. Unfortunately, because all records are de-identified and must be accessed on separate servers, we cannot link individuals across datasets (e.g., we cannot observe whether an individual appearing in the HMIS data also appears in the HCAI data), which motivated our aggregate-level empirical approach. However, while there are

[41] *A priori*, it's not clear that the effect on daytime crime should be zero. The program may have spillover effects (e.g., better-rested people commit less crime), the program may have direct effects (e.g., shelters provide sack lunches to people as they leave in the morning, and hungry people commit more crime), or regressions may capture some effect of non-standard shelter operation hours (e.g., for some days each year, shelters can operate 24 hours).

[42] We define a psychiatric ER visit to be any ER visit where the principle diagnosis has an ICD-10 code starting with "F" or in the range, "R44-46." No demographic restrictions have been imposed, and these counts do not distinguish between ER visits where a patient was subsequently admitted to inpatient care versus not. Cells smaller than 15–approximately 3.5% of the sample–are censored for confidentiality.

well-known challenges in working with HMIS data in isolation, the enrollment-level data does contain a number of high-quality variables. In particular, the variables for entry date, project type (e.g., emergency shelter, PSH, street outreach), and personal ID (that allows for following the same individual over time) are always populated and sensible in almost all cases. Additionally, while exit destination is missing in more than half of the records,[43] exit dates are present more than 90% of the time (even if their accuracy is suspect[44]). We can leverage this information to assess additional outcomes at the individual-level.

6.1. Data and Empirical Design

HMIS data contains records of every program "enrollment." An enrollment may be for any one of 13 "project types," which may be something like PSH or shelter, but also includes "street outreach" and "services only" project types. Table 3 presents a list of the project types and their representation in the enrollment data for 2 periods - 2013-2023 (the full set of years with reliable records) and 2014-2019 (the sample period for most of our analysis). Note that 3 of the most common project types in the HMIS data (accounting for more than 70% of all enrollments) are emergency shelter and 2 non-shelter, non-housing services - street outreach and services only. We will restrict to these project types when we form our analytical sample.

	2013-2023		2014-2019	
Project Type	enrollments	share (%)	enrollments	share (%)
Street Outreach (SO)	403,139	32.96	160,917	30.56
Emergency Shelter (ES)	322,035	26.33	153,557	29.16
Services Only (SSO)	169,694	13.88	58,816	11.17
RRH	139,964	11.44	79,140	15.03
Day Shelter	43,102	3.52	14,932	2.84
CES	37,789	3.09	78	0.01
Prevention	35,250	2.88	19,240	3.65
PSH	32,324	2.64	16,879	3.21
TH	31,765	2.60	19,842	3.77
Other	4255	0.35	1019	0.19
PH Only	1806	0.15	1332	0.25
Safe Haven	1359	0.11	485	0.09
PH Services	524	0.04	384	0.07

Table 3: Number (and share of total) of enrollments by project type entered for 2 time periods. RRH is rapid re-housing, CES is "coordinated entry system," PSH is permanent supportive housing, TH is transitional housing, and PH refers to permanent housing (which may or may not include services beyond housing).

Data limitations have led researchers to rely extensively on experiments for causal identifi-

[43] See Table B.2.

[44] Meyer, Wyse and Corinth (2023) have identified inconsistencies in recording exits from HMIS, noting "purge dates" on which a large number of exits are recorded - exits that presumably should have been recorded weeks or months prior. Upon inspection of the data, we find evidence of such a phenomenon in many enrollments but note that it does not appear to affect most cases in our sample period.

cation of homelessness policy interventions, and experiment participants tend to be recruited from shelters, resulting in "sheltered homeless" as the *de facto* counterfactual group. It is hard to imagine a setting in which people experiencing homelessness can be randomly assigned to shelter or unsheltered homelessness in a way that allows for compelling analysis.[45] However, administrative records from HMIS allow us to identify enrollments in both sheltered and unsheltered settings.[46] We seek to compare the outcomes of individuals who receive emergency shelter (ES) to those of individuals who do not receive shelter - those in street outreach (SO) or services only (SSO) projects. Leveraging variation in the availability of shelter beds, we identify an estimate of the causal effect of shelter.

Restricting to emergency shelter, street outreach, and services only enrollments, we offer the following analogy to clarify the data structure and empirical approach. In a health setting, it is as if we observe every patient who shows up to a licensed ER and we know if they interact with the ER and leave without being admitted (street outreach or services only) or if they are admitted (emergency shelter). Then, every year, on different dates and in different locations, several hundred inpatient (shelter) beds are temporarily added. What does this do to the probability that an observed interaction with the ER is an admission (an ES enrollment)?

More explicitly, when individual i appears in the data on a given date ymd in a given location (SPA) s, what is the probability that their enrollment e is for shelter (ES) versus street outreach or services only? This is our first-stage equation.

$$\begin{aligned} ES_{eisymd} = {} & \psi^f_{sy} + \mu^f_m + \delta^f_d \\ & + \gamma_1 other\ beds_{symd} + \gamma_2 WS\ beds_{symd} + X_i\Theta^f + u_{eisymd} \end{aligned} \tag{FS}$$

$$\begin{aligned} Y_{eisymd} = {} & \psi^r_{sy} + \mu^r_m + \delta^r_d \\ & + \beta_1 other\ beds_{symd} + \beta_2 WS\ beds_{symd} + X_i\Theta^r + \varepsilon_{eisymd} \end{aligned} \tag{RF}$$

where ES takes value 1 if enrollment e by individual i in SPA s in year y in month m on day of month d is an emergency shelter enrollment and 0 otherwise.

Figures 6 and 7 further motivate our approach. Figure 6 plots daily counts of project enrollments over time, split into ES enrollments and all other project types. Vertical lines have been superimposed to denote the most common winter shelter opening and closing dates each season.[47] Outside of winter shelter operation dates, the trend in shelter enrollments over time appears rela-

[45] Even a lottery system that randomly assigns shelter beds would have to occur in a situation where (a) there is excess demand for the beds, (b) rationing them randomly is approved by IRB, (c) participants in both treatment and control groups can be tracked over time, and (d) compliance can be enforced to a meaningful degree (e.g., those who are not assigned to shelter must not receive shelter elsewhere, which would be neither ethical nor legally enforceable).

[46] Table B.1 provides a breakdown of enrollments by the reported living situation of the enrolled prior to entering the project. While there are valid concerns about missingness and record quality, the data indicate that the overwhelming majority of individuals receiving homeless services are in "literally homeless" situations.

[47] Note that enrollments may not *completely* spike or dip at exactly these dates because these are only the *most common* opening and closing dates and such dates do vary across sites, within a year.

tively smooth, slowly increasing over the sample period, but there are clear, abrupt breaks in this trend, corresponding to the exact opening and closing dates of winter shelters.[48]

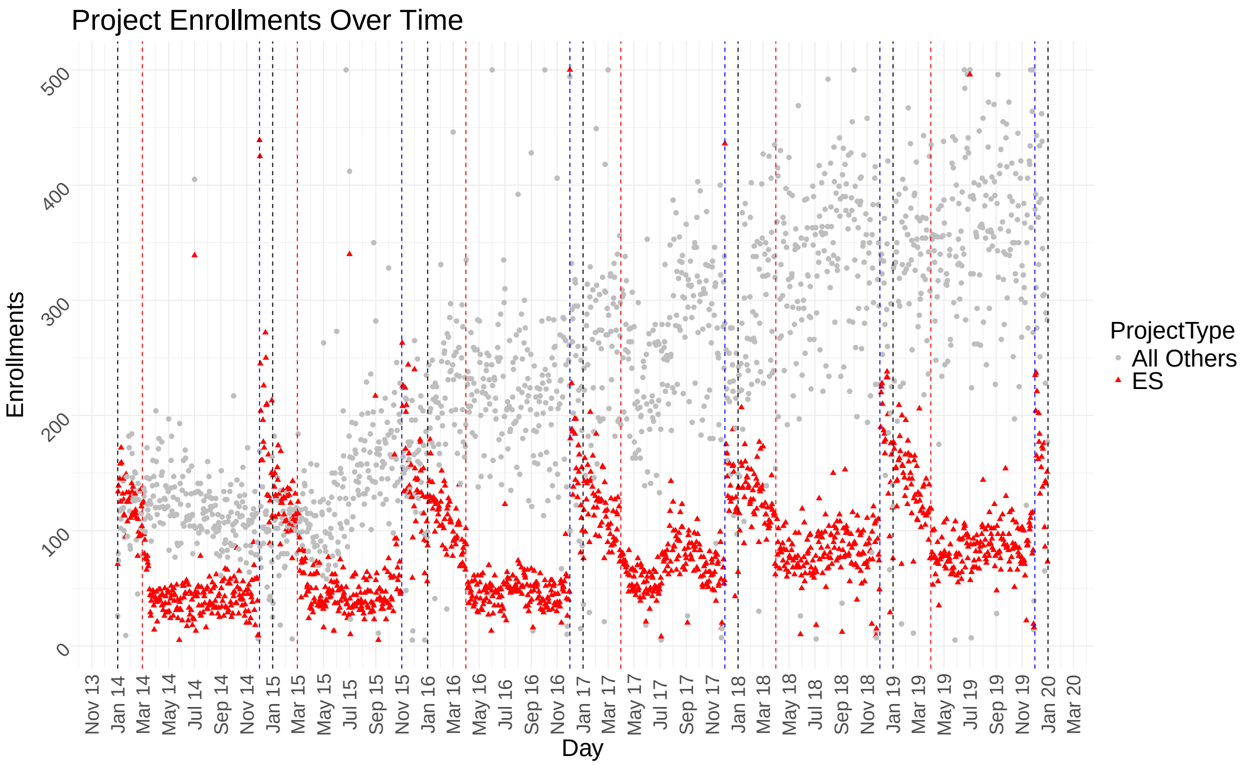

Figure 6: Daily enrollments in projects (emergency shelter versus all others). For clarity, values in excess of 500 are shown at 500, and dates corresponding to weekends (where enrollments are systematically lower) are dropped. 11 additional points are omitted because they do not meet minimum cell size requirements for disclosure.

Similarly, Figure 7 presents weekly counts of enrollments by project type. The data can be noisy but exhibit clear spikes in ES enrollments corresponding to shelter opening dates. As these figures make clear, the probability that a given encounter in the data is for emergency shelter is a function of shelter availability.

[48]Note that counts exhibit a downward trend over the course of a winter shelter season. This is because many of the individuals who enroll in the shelter at opening remain enrolled and the points represent *new enrollments* in shelter, not the number of people in shelter as in Section 5.

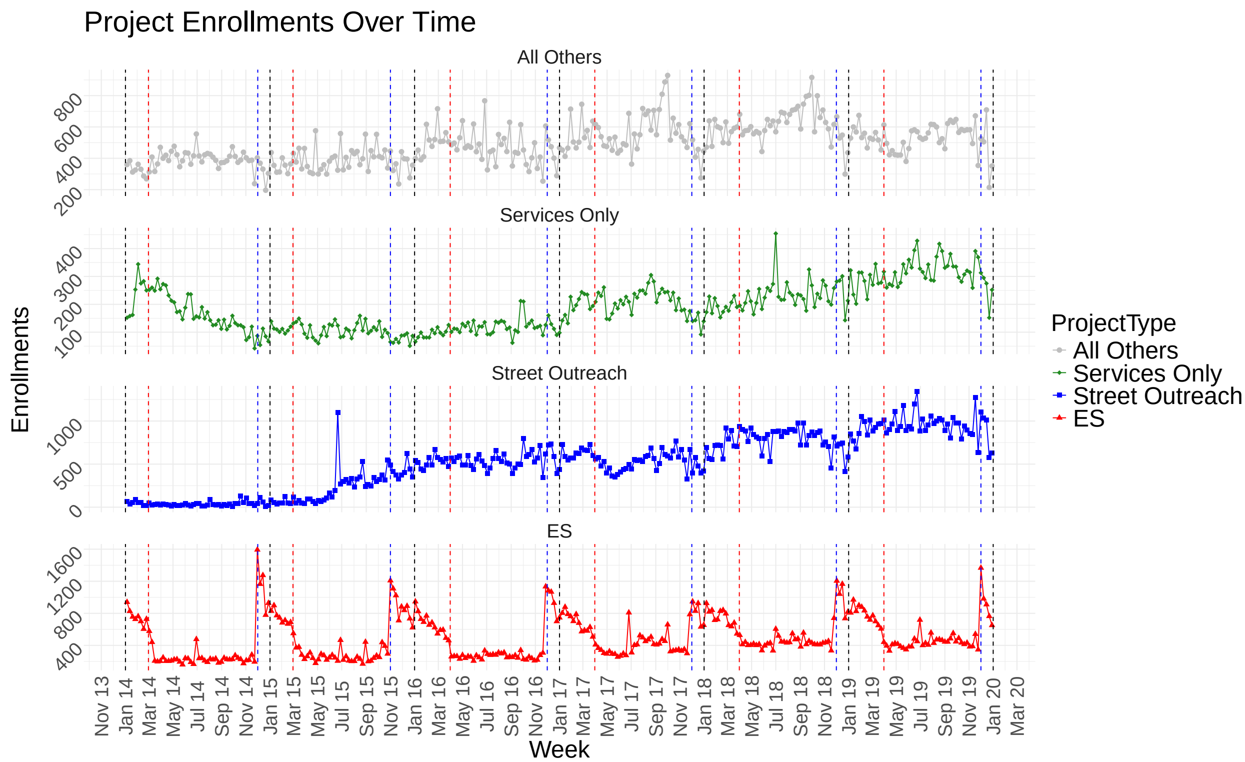

Figure 7: Weekly enrollments by project type. Daily counts are aggregated to the weekly level to avoid small cells.

6.2. Variable Definitions and Regression Results

We begin by restricting our sample to enrollments in emergency shelter (ES), Street Outreach (SO), or Services Only (SSO); with entry dates between 2014 and 2019; among individuals aged 16-99; and where location (SPA) can be determined. Except for the addition of individual-level controls,[49] the right-hand sides of the regression equations are almost exactly the same as they were in the aggregate-level analysis. Because dependent variables will be binary, bounded between 0 and 1, for ease of interpretation, the measures of "beds" have been scaled by 100 (e.g., WS beds is now 100's of WS beds). Select descriptive statistics, including means of choice control variables are presented in Table 4. The more challenging piece is meaningfully defining outcome variable Y.

[49] X_i is a vector of controls for gender, race, ethnicity, a quadratic in age, and an indicator for the presence of a (self-reported) disabling condition.

Independent Variables	Full Sample	ES	SO	SSO
Age	43.4655	43.1724	43.6245	43.7860
Male	0.6100	0.6357	0.5972	0.5793
White	0.4457	0.4243	0.4800	0.4115
Hispanic	0.2797	0.2693	0.2933	0.2708
Disabled	0.3409	0.3803	0.2675	0.4293
n	332,343	136,673	140,251	55,419

Dependent Variables	Full Sample	ES	SO	SSO
recid_6_18	0.4023	0.4350	0.3848	0.3659
new project	0.3459	0.3780	0.3335	0.2981
existing project	0.3107	0.3078	0.3328	0.2622
success_18	0.1941	0.2355	0.1213	0.2763
own/rent	0.1483	0.1775	0.0895	0.2254
friends/family	0.0534	0.0690	0.0354	0.0606
newly homeless	0.1546	0.1985	0.1039	0.0991
mortality_18	0.0048	0.0041	0.0055	0.0049

Table 4: Descriptive statistics for individual-level analysis. Except sample size, all values reported are sample means.

A natural definition for Y_{eisymd} would be a simple indicator for whether individual i exited homelessness (or entered housing) within k months of project enrollment e. However, HMIS records are notoriously flawed and incomplete, in part because the homeless population is uniquely challenging to keep track of over time.[50] For any number of reasons, a client may simply stop appearing for services, disappearing from the data altogether. They may lose contact with their case manager. They may exit a project but refuse to answer where they are going. They may find housing on their own or move in with relatives without informing their case manager. They may die without their case manager being made aware. Table B.2 presents a summary of exits by exit destination recorded in HMIS data during our sample period. An individual's destination upon exiting an enrollment is missing for more than half of all enrollments.

Because individuals who appear in the HMIS data can be identified by a personal ID number, a common workaround[51] is to assess whether an intervention reduces the propensity for an individual to reappear for homeless services (the logic is that an individual reappearing for homeless services has not exited homelessness and is still homeless at the time of reappearance). This motivates our first reduced-form outcome.

6.2.1. Returns to Homeless Services

The question we seek to answer is, "does shelter make an individual less likely to reappear for homeless services?" or, focusing on the counterfactual, "in the absence of shelter intervention,

[50] Additionally, high cost of living and low compensation in the homeless response sector have been noted to pose challenges for worker retention and continuity of care. See, for instance, Abraham et al. (2023).

[51] See Cohen (2024) and concurrent work by Jared Schachner and Gary Painter.

would this individual have been more (or less) likely to reappear for services?". For the less-intensive project types we have focused on, it is very common for the same individual to appear multiple times over a short window. For example, individual i might be contacted by a street outreach team and entered into HMIS. Then, a different street outreach team might follow up a week later and give i information about where they can find toiletries, leading i to appear for (toiletries) services there the next day. In this example, i would have 3 separate enrollments in a period of 8 days. To avoid characterizing subsequent enrollments like these as "recidivism," in choice specifications, we allow for an "adjustment period" - a window of time after project entry before we consider reappearing for services to be indicative of recidivism or failure to exit homelessness.

Formally, $recid\_A\_B_{eisymd} \equiv 1$ if the enrolled individual reappears ("recidivates") in a new project or for additional services under an existing enrollment where the entry or service date is recorded to be $\in$ [A,B] months after entering the current project. In choice specifications, A = 6 and B = 18, but we present results under various other definitions in the appendix. Table 5 presents regressions results. Fixed effects and controls are included in all regressions, but for clarity, we report estimates for only select parameters.

		Recid_6_18		New Enrollment		Reappearance	
	FS	RF	IV	RF	IV	RF	IV
other beds	0.0044***	−0.0008		0.0001		−0.0038***	
	(0.0008)	(0.0010)		(0.0010)		(0.0009)	
WS beds	0.0475***	0.0014		0.0061***		0.0011	
	(0.0009)	(0.0011)		(0.0011)		(0.0010)	
ES			0.0297		0.1290***		0.0222
			(0.0239)		(0.0234)		(0.0211)
Outcome Mean	0.4112	0.4023	0.4023	0.3459	0.3459	0.3107	0.3107
Adj. R^2	0.3727	0.0536	0.0553	0.0431	0.0400	0.0823	0.0830
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 5: Estimated effects of shelter on choice recidivism outcome. Column 1 presents results from the first stage regression. Columns 2-3 present estimated effects on reappearing for homeless services. The remaining columns break out the measure of recidivism by whether the reappearance was for services in a new project enrollment or for services issued during a reappearance for services under an existing enrollment.

Column 1 presents estimates from the first-stage regression. We observe a very strong first-stage, indicating that the addition of 100 winter shelter beds translates to a 4.75 percentage point (or more than 10%) increase in the probability that an enrollment is for emergency shelter (as opposed to street outreach or services only). Reduced-form and IV estimates are statistically insignificant, indicating no detectable effect of shelter on returning to homeless services.

Because recidivism can be broken down into its 2 components, we evaluate shelter's effect on each separately in columns 4-7. Results indicate that winter shelters have a positive effect on an individual's propensity to return to homeless services for the purpose of enrolling in a new project but no detectable effect on reappearance for services under any project. Our primary interpretation of this finding is that those who enroll in temporary shelter are, at best, just as likely to return to homeless services as those who did not receive shelter.

Tables B.3 and B.4 report results under various other definitions of recidivism (alternative values for A and B). Across all specifications, the estimated effect of shelter is, at best, 0 and, often, an indication that shelter actually *increases* returns to homelessness or extends the length of one's homelessness.

6.2.2. Successful Exits

The results above are concerning, but they may not be conclusive proof that temporary shelter is ineffective in reducing homelessness (or even exacerbates homelessness). Namely, it is possible that people who appear in shelter (relative to those in street outreach or services only) are simply more likely to reappear in HMIS, conditional on being homeless. For instance, those who are less resistant to treatment (easily induced into shelter) may be more trustworthy of service providers in general and therefore, conditional on being homeless, are more likely to appear in the HMIS data (as opposed to others who, conditional on being homeless, might avoid interaction with service providers). We investigate this possibility further.

If shelter causes people to be "mechanically" more likely to reappear in HMIS records while homeless (a possible explanation for the observed recidivism results) and the probability of "successfully" exiting the HMIS data (defined as having a recorded exit destination of one's own housing or the home of family or friends) conditional on being observed is > 0 (which is demonstrably true[52]), then all else equal, we should observe a positive effect of shelter on "successful exits."[53]

Formally, we define $success\_B_{eisymd} \equiv 1$ if the enrolled individual is observed to have exited to a living situation that is either (i) their own housing (rent or own) or (ii) with family or friends[54] within B months of entering enrollment e. We run this analysis for various values of B but report only results for B = 18 here as results are highly consistent across all alternative definitions.[55] Table 6 presents regression results.

[52]By definition, this probability must be weakly greater than zero, and Table 4 shows that it's approximately 0.2.

[53]In other words, if the explanation for the recidivism results is that people in shelter are just observed more, then it must be the case that people in shelter have more chances to be observed exiting, mechanically increasing the probability of an observed successful exit.

[54]Exit destination is missing in a majority of cases, but as shown in Table B.2, around 10% of enrollments in ES, SO, or SSO do result in a recorded "successful" exit. Further, this measure is coded as 1 if i exits *any* enrollment successfully within the timeframe, not only if the specific enrollment e results in a recorded successful exit. For example, if i appears in street outreach on March 1, exits that street outreach enrollment on June 10 to a hotel (unsuccessful), enters rapid-rehousing (RRH) on June 15, and exits RRH on November 20 to their own apartment, this would be coded as a successful exit within 18 months.

[55]Results under different values of B are presented in Table B.5.

		success_18		rent/own		family/friends	
	FS	RF	IV	RF	IV	RF	IV
other beds	0.0044***	0.0051***		0.0021***		0.0029***	
	(0.0008)	(0.0008)		(0.0007)		(0.0005)	
WS beds	0.0475***	−0.0213***		−0.0180***		−0.0044***	
	(0.0009)	(0.0009)		(0.0008)		(0.0005)	
ES			−0.4488***		−0.3795***		−0.0918***
			(0.0209)		(0.0188)		(0.0107)
Outcome Mean	0.4112	0.1941	0.1941	0.1483	0.1483	0.0534	0.0534
Adj. R^2	0.3727	0.0520	−0.2433	0.0420	−0.2081	0.0315	−0.0144
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 6: Estimated effects of shelter on "successful" exits from HMIS. Column 1 presents results from the first stage regression. Columns 2-3 present estimated effects on having an observed exit destination to one's own rental or owned housing or to a housing situation in which they are living with family or friends. The remaining columns break out the measure by more specific destination (rent/own or friends/family).

Columns 2 and 3 show a significant *negative* effect of temporary shelter on successful exits. In other words, if the explanation of the recidivism results is that they are driven by a higher probability of appearing in the HMIS data while homeless, then shelter must also have a massive effect on reducing the probability that an individual successfully exits homelessness. Therefore, the interpretation of these findings must be that either (a) temporary shelter doesn't reduce returns to homelessness or (b) temporary shelter *does* reduce the probability of successfully exiting homelessness.

The last 4 columns of Table 6 break down "success" into its 2 components. We observe significant negative effects both on the probability of exiting to one's own housing and on the probability of exiting to the home of family or friends. Effects are driven more by exits to one's own housing, but relative to baseline means, the percentage reduction is similar across outcome definitions.

6.2.3. Newly Homeless

The above results are troubling. If shelter does not reduce homelessness (or possibly even lengthens spells of homelessness), is it possible that shelter provision actually *induces* homelessness? Economic models of homelessness suggest that if the cost to remaining in one's housing (inclusive of rent, psychological cost, etc.) exceeds the expected cost of being homeless, an individual will optimally choose to enter homelessness.[56] One example is that a person whose rent exhausts 90% of their income has little to allocate to consumption and, depending on their preferences, may choose to live in their car to increase consumption. Another example may be that a person living in crowded housing or enduring abusive circumstances views the psychological costs of remaining in such circumstances as higher than the costs of homelessness. Therefore, if an intervention reduces

[56] For a more formalized model, see Quigley and Raphael (2001).

the expected cost of homelessness (because sheltered homelessness is perceived as less costly than unsheltered homelessness), then, on the margin, homelessness may become the optimal housing choice for some individuals. We assess this possibility below.

The HMIS data contains (imperfect) information on the number of months an individual has been homeless. When present, these values range from 1 (meaning they are in their first month of homelessness) to 12+. If shelter is inducing homelessness, we should observe a positive effect on the probability that an enrollment is for someone who reports being in their first month of homelessness. Formally, $Newly\ Homeless_{eisymd} \equiv 1$ if individual i reports a "months homeless" value of 1 at enrollment e. Results are presented alongside choice results for recidivism and success outcomes in columns 6 and 7 of Table 7.

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0044***	−0.0008		0.0051***		0.0011		0.0000	
	(0.0008)	(0.0010)		(0.0008)		(0.0010)		(0.0001)	
WS beds	0.0475***	0.0014		−0.0213***		−0.0000		−0.0006***	
	(0.0009)	(0.0011)		(0.0009)		(0.0013)		(0.0002)	
ES			0.0297		−0.4488***		−0.0011		−0.0117***
			(0.0239)		(0.0209)		(0.0326)		(0.0032)
Outcome Mean	0.4112	0.4023	0.4023	0.1941	0.1941	0.1546	0.1546	0.0048	0.0048
Adj. R^2	0.3727	0.0536	0.0553	0.0520	−0.2433	0.0415	0.0412	0.0040	0.0001
Num. obs.	332,343	332,343	332,343	332,343	332,343	181,419	181,419	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 7: Effect of shelter on select individual-level outcomes.

Estimated effects are indistinguishable from zero, and point estimates are very small in magnitude (and negative). A 95% confidence interval indicates that, in the worst case scenario, every 100 additional temporary shelter beds translates to a roughly 0.25 percentage point (or less than 2%) increase in the probability that an individual is newly homeless at the time of enrollment. Thus, we observe no indication that shelter provision induces entries into homelessness to any meaningful degree.[57]

6.2.4. Mortality

Finally, with the understanding that HMIS records of exit destinations are incomplete in mind, we investigate possible effects of shelter on observed mortality over various time horizons. We define $mortality\_B_{eisymd} \equiv 1$ if the enrolled individual is observed to have exited any project enrollment due to being "deceased" within B months of enrollment e. Results are presented for B = 18 alongside previous results in Table 7 and for select other values of B in Table B.6.

We cannot rule out the possibility that these results are driven by a potential effect of shelter on the probability that one has been *recorded* to have died in the HMIS data, so we urge the

[57]In future work, we will investigate whether this holds across populations. For instance, do those who report identifying as LGBTQ+ respond to shelter availability more than others (e.g., because they are more likely to be victims of domestic abuse) or less than others (e.g., because they are more fearful of experiencing violence because of their identity in a shelter setting)?

reader to interpret our findings with caution and with this caveat in mind. However, the results in Table 7 suggest that temporary shelter significantly reduces mortality among people experiencing homelessness, which would be consistent with our findings of shelter's effects on ER visits.

6.3. Summary of Individual-level Results

Results from the individual-level analysis indicate that temporary shelter is ineffective in reducing returns to homeless services or increasing exits from homelessness. This allows us to return to 2 policy implications discussed earlier. First, if temporary shelter is ineffective in reducing homelessness, then regardless of its relative cost, compared to interventions such as PSH, it is not more cost-effective. Second, if the sole objective of a policy is homelessness reduction, investment in temporary shelter is, in fact, "ultimately a losing game." One argument consistent with our results is that increased engagement with people experiencing homelessness is what drives exits. Our results suggest that simply increasing street outreach would be just as effective to reduce homelessness as providing temporary shelter.

Despite our estimates suggesting that shelter may actually *reduce* exits from homelessness, we observe no evidence that shelter provision induces entries into homelessness. This is important independently (as theoretical models would suggest this is possible), but it also implies that our aggregate-level results may be interpreted as not just the effects of "increasing sheltered homelessness," but also as the effects of "reducing unsheltered homelessness."[58] Finally, our estimates also suggest that shelter reduces (at least short-run) mortality.

Taken together, temporary shelter appears to function as a sort of bandage. Even if it does not reduce duration of homelessness, it may be a highly effective intervention if it reduces the utility loss to those experiencing homelessness, makes their communities safer, and alleviates burdens on public systems. To the extent our findings in Section 5 and for mortality are robust and generalizable, they suggest that our current understanding of shelter may underestimate its benefits to communities. In addition to the outcomes we can explicitly assess here, the implications of our findings for potential (unobserved) positive externalities through spillover effects is worth consideration when attempting to estimate the full return to shelter investment. For instance, if adults are less likely to end up in the ER, their children may have greater stability, improving their health, safety, and education outcomes. Similarly, lower crime may increase economic activity (e.g., going out to dinner is more appealing when crime rates are lower).

6.4. Robustness and Extensions

In the following sections, we adjust various assumptions and impose varying sample restrictions to test the robustness of our findings.

[58] More accurately, our estimates represent a lower bound on such an effect. To the extent that shelter shifts people from one service planning area to another, the effect we measure will capture the combined effect of reducing local unsheltered homelessness and increasing total homelessness, and to the extent that more total homelessness contributes to more crime or ER visits, our estimates will understate the magnitude of the true effect of reducing unsheltered homelessness.

6.4.1. Recidivism by Project Type at Reappearance

One possible explanation for the recidivism results is that shelter induces people to reappear because they are moving into more intensive or permanent housing provided by homeless service providers. For instance, it may be that shelter causes individuals to reappear in the data because they have been connected to permanent supportive housing, which one may consider to be more of an indicator of success (in navigating homeless services to a program that is more stable) than failure (to exit homelessness). We investigate this possibility below.

Because recidivism is coded as a reappearance in HMIS for a new project enrollment or services related to an existing project enrollment, we can determine the type of project (e.g., from the list provided in Table 3) at which the reappearance occurs in every case. In Tables B.7 and B.8, we present regression results for the recid_6_18 outcome, except the variable is defined as recidivism only if the future appearance in HMIS is for services from the project of the type stated above each set of columns.

The results in Tables B.7 and B.8 contradict the hypothesis that shelter increases reappearances because it causes individuals to become enrolled in more intensive programs in the future. Shelter increases the probability of future appearances in shelter and reduces the probability of appearing in street outreach records in the future, but it is associated with *reductions* in the probability that future appearances are for rapid re-housing, transitional housing, or permanent housing programs. Thus, the interpretation that temporary shelter has, at best, zero effect on reducing future homelessness is further supported.

6.4.2. Effects for Unhoused at Entry

Another concern is that enrollments, especially in "services only" (SSO) projects, are comprised of people who are housed and are therefore already predisposed to positive future outcomes. While Table B.1 shows that it is rare for an HMIS enrollment to be for someone who is coming from housing, this alone does not rule out the possibility that this small group may influence our results. Table 8 presents regression results for select outcomes (replicating Table 7) after dropping all enrollments where living situation at time of project entry indicates living in one's own rental or owned housing or living with friends or family.[59] Additionally, because one might argue that individuals in "street outreach" are a more valid "unsheltered" counterfactual group than individuals in "services only," Table 9 replicates Table 7 after dropping enrollments in services only (i.e., comparing only emergency shelter and street outreach).

[59] This likely drops too many observations as many people are entering homeless services because they are losing or exiting their housing.

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0058^{***}	0.0019^{*}		0.0060^{***}		0.0008		0.0001	
	(0.0009)	(0.0011)		(0.0008)		(0.0010)		(0.0002)	
WS beds	0.0449^{***}	0.0009		-0.0214^{***}		-0.0004		-0.0006^{***}	
	(0.0010)	(0.0012)		(0.0010)		(0.0013)		(0.0002)	
ES			0.0197		-0.4767^{***}		-0.0098		-0.0133^{***}
			(0.0272)		(0.0238)		(0.0329)		(0.0038)
Outcome Mean	0.4429	0.4185	0.4185	0.1937	0.1937	0.1432	0.1432	0.0051	0.0051
Adj. R^2	0.3584	0.0624	0.0633	0.0523	-0.2871	0.0344	0.0320	0.0043	-0.0005
Num. obs.	273,980	273,980	273,980	273,980	273,980	168,480	168,480	273,980	273,980

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 8: Replication of Table 7, dropping enrollments where living situation at entry is rent, own, friends, or family.

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0084^{***}	-0.0004		0.0057^{***}		-0.0004		0.0000	
	(0.0008)	(0.0010)		(0.0008)		(0.0011)		(0.0002)	
WS beds	0.0333^{***}	0.0001		-0.0189^{***}		-0.0008		-0.0003^{**}	
	(0.0009)	(0.0012)		(0.0009)		(0.0014)		(0.0002)	
ES			0.0029		-0.5681^{***}		-0.0387		-0.0103^{**}
			(0.0364)		(0.0330)		(0.0661)		(0.0047)
Outcome Mean	0.4935	0.4096	0.4096	0.1777	0.1777	0.1643	0.1643	0.0048	0.0048
Adj. R^2	0.4086	0.0619	0.0621	0.0540	-0.4704	0.0432	0.0334	0.0039	0.0012
Num. obs.	276,924	276,924	276,924	276,924	276,924	154,226	154,226	276,924	276,924

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 9: Replication of Table 7, dropping enrollments in services only (SSO) projects.

In both cases, results are consistent with choice results from specifications in Table 7.

6.4.3. Restrict Sample Window

One might reasonably argue that our results would be most compelling when we restrict the sample to focus on the periods surrounding winter shelter opening (or closing) dates (disallowing the possibility that fluctuations in shelter enrollments outside of these periods somehow influence results). Additionally, assuming that *entries* to homelessness are independent of the availability of shelter beds (as our results above for "newly homeless" indicate), we would, ideally, want to compare individuals who become homeless in a world where there are few shelter beds (outside of winter shelter operation) to those who become homeless in a world where there are many shelter beds (during winter shelter operation). This would help address the possibility that the effects of shelter that we are detecting are driven by the composition of the group who are induced into shelter only when availability is expanded (compliers). In other words, currently, our results answer the policy-relevant question of what would happen if shelter beds increased, inducing more (marginal) people to enter shelter. It is possible that the effect of shelter on those who are "on the margin" (those who only enter when it's more abundant) is different than the effect of shelter on the average person who falls into homelessness. If this is the case, we would observe different effects of shelter in a setting where an individual happens to become homeless just before more shelter is available versus just after more shelter is available.

We cannot perfectly replicate such a setting. Our primary limitation is that measures of when an individual became homeless are imperfect (e.g., the "months homeless" variable used in the construction of our measure of "newly homeless" is missing in nearly half of our enrollments). In our best approximation, we can restrict our sample of enrollments to enrollments e that represent the very first time i appeared in the HMIS data as a proxy for when i entered homelessness.

Below, Table 10 restricts the sample to enrollments that occur between September and December (a period just before winter shelters usually open and a period just after winter shelters usually open), and Table 11 adds the restriction that each enrollment e must represent the first time the individual i is observed. Similarly, Table B.9 restricts to entries between January and April (around shelter closing dates), and Table B.10 adds the restriction that this enrollment must be the first time i is observed. Finally, Table 12 trims the outer half of the sample period (restricting to entries between July, 2015 and June, 2018) to address concerns about potential data reliability issues in early periods and/or concerns about outcomes defined based on what happens 18 months later being driven by effects of the COVID-19 pandemic (i.e., ending the sample of entries in mid-2018 means that outcomes defined by what happens to an individual in the next 18 months will never include outcomes during 2020 or later).

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	-0.0182^{***}	-0.0049		0.0191^{***}		-0.0034		0.0002	
	(0.0035)	(0.0043)		(0.0034)		(0.0040)		(0.0007)	
WS beds	0.0727^{***}	0.0113^{***}		-0.0286^{***}		-0.0003		-0.0009^{**}	
	(0.0020)	(0.0024)		(0.0019)		(0.0022)		(0.0004)	
ES			0.1551^{***}		-0.3939^{***}		-0.0059		-0.0120^{**}
			(0.0325)		(0.0282)		(0.0401)		(0.0050)
Outcome Mean	0.4340	0.4184	0.4184	0.1897	0.1897	0.1476	0.1476	0.0056	0.0056
Adj. R^2	0.3982	0.0538	0.0520	0.0596	-0.1782	0.0414	0.0401	0.0043	0.0010
Num. obs.	115,808	115,808	115,808	115,808	115,808	69,143	69,143	115,808	115,808

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 10: Replication of Table 7, restricting to September-December entries only.

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	-0.0411^{***}	0.0004		0.0139^{***}		0.0003		-0.0004	
	(0.0053)	(0.0062)		(0.0047)		(0.0081)		(0.0008)	
WS beds	0.0792^{***}	0.0047		-0.0204^{***}		0.0014		-0.0013^{***}	
	(0.0029)	(0.0032)		(0.0026)		(0.0042)		(0.0004)	
ES			0.0591		-0.2573^{***}		0.0233		-0.0163^{***}
			(0.0405)		(0.0342)		(0.0691)		(0.0055)
Outcome Mean	0.3658	0.2581	0.2581	0.1332	0.1332	0.2177	0.2177	0.0028	0.0028
Adj. R^2	0.4421	0.0441	0.0466	0.0741	-0.0750	0.0574	0.0641	0.0027	-0.0073
Num. obs.	49,770	49,770	49,770	49,770	49,770	24,046	24,046	49,770	49,770

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 11: Replication of Table 7, restricting to September-December entries AND first time i is observed only.

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0155^{***}	0.0040^{**}		0.0053^{***}		0.0028		0.0001	
	(0.0016)	(0.0020)		(0.0016)		(0.0021)		(0.0003)	
WS beds	0.0485^{***}	0.0016		-0.0163^{***}		-0.0067^{***}		-0.0006^{***}	
	(0.0013)	(0.0015)		(0.0012)		(0.0015)		(0.0002)	
ES			0.0339		-0.3367^{***}		-0.2235^{***}		-0.0121^{***}
			(0.0319)		(0.0269)		(0.0532)		(0.0042)
Outcome Mean	0.4143	0.3669	0.3669	0.1840	0.1840	0.1652	0.1652	0.0039	0.0039
Adj. R^2	0.3961	0.0584	0.0604	0.0499	-0.1341	0.0411	-0.0768	0.0037	-0.0010
Num. obs.	168,263	168,263	168,263	168,263	168,263	97,545	97,545	168,263	168,263

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 12: Replication of Table 7, restricting to July 2015 through June 2018 (i.e., trim outer half of sample period).

Throughout, results are almost always consistent with those from choice specifications in Table 7. The only significant exceptions across all 5 tables are the positive effect of shelter on recidivism detected in Table 10 (which is consistent with some other tests suggesting shelter may actually increase returns to homeless services and does not contradict our finding that, at best, shelter is ineffective in achieving homelessness reductions), the positive effect on "newly homeless" that appears in Table B.10, and the negative effect on "newly homeless" that appears in Table 12. With the overall consistency of the estimates, we hesitate to read too much into these select differences, but one possible explanation is that shelters did more to "catch people" as they fell into homelessness in earlier periods. Shelter represented a greater share of enrollments, relative to street outreach and services only, in earlier periods of the sample, and chronic homelessness has risen in Los Angeles. Restricting to January-April observations where an individual is first observed may disproportionately capture people from earlier periods who had fewer opportunities to interact with service providers outside of shelters, and alternatively, dropping those earlier periods may concentrate the sample on repeat (chronic) users of services. This may suggest that, in recent years, the marginal group of people affected by increased shelter supply are individuals who have more extensive homeless histories. In future work, we do more inspection of this population.

6.4.4. Additional Empirical Validation

Finally, in analyses not reported here,[60] we investigate the extent to which the composition of individuals observed in shelters may be able to explain our estimates. We are already controlling for basic demographics in all enrollment-level regressions, but we consider the (unlikely) possibility that these observable characteristics are affected by shelter, making them "bad controls." Results are broadly robust to omitting demographic controls.

Further, we consider the possibility that individuals induced into shelter by treatment or "compliers" (i.e., those who appear in shelter when winter shelter beds are available but otherwise would not) are different in unobservable ways. The estimates we report are local average treatment effects, which capture the average effect of shelter for those who are induced into it by treatment.

[60]Details available upon request.

If this group is unique in ways that are meaningfully correlated with outcomes, it may be the case that the individual effects of shelter would be different in different settings.

7. Conclusion

As the most widely used homelessness intervention aside from street outreach, it is crucial that we understand the effects of homeless shelter. In a setting where more than half of people experiencing homelessness are unsheltered, it is critical that we understand the consequences of unsheltered homelessness. In this study, we provide among the first causal estimates of the effects of temporary shelter on a range of outcomes.

Shelter causes significant reductions in crime and ER visits for psychiatric conditions. Consistent with these results, we also find evidence that shelter reduces mortality. Specifically, our results suggest that the addition of 100 temporary shelter beds prevents approximately 1 crime and 0.25 psychiatric ER visits per day and reduces the probability of (observed) death within the next 18 months by more than 10%.

Estimates of the effect of shelter on returns to homeless services are weakly positive and generally statistically insignificant.[61] We are inclined to interpret these results, cautiously, as evidence that shelter does not achieve meaningful reductions in the average duration of homelessness. Confidence intervals on choice estimates imply that, at best, the addition of 100 temporary shelter beds results in no more than a 0.2% reduction in the probability of reappearing in homeless services in the next 6-18 months. However, in many robustness tests, we do detect statistically significant effects that suggest shelter may extend homelessness (by contrast, we never detect statistically significant effects of shelter reducing homelessness). Due to challenges with variable construction (and frequent statistically insignificant estimates), our present interpretation is that the true effect is "not negative." If the effect of shelter in reducing homelessness is practically zero, then our results should be interpreted as evidence that engagement with homeless services *at all* (e.g., via street outreach) is just as effective as providing shelter for the purpose of reducing homelessness. Alternatively, if positive estimates are taken at face value, they would be interpreted as evidence that shelter lengthens the amount of time a person is homeless. In this case, the provision of shelter may have analogous effects to the provision of unemployment benefits, where increasing generosity raises the utility of those who have lost their jobs (homes) and sustains them while they search for employment (housing) but may also extend the length of unemployment (homelessness).[62]

With these findings in mind, policymakers can make better-informed decisions about the homeless services to which they allocate resources. If one's sole objective is to reduce total homelessness, our results do not support investment in temporary shelter. Our findings imply that increasing street outreach would be just as effective, and extant literature would support directing funds towards more intensive projects like permanent supportive housing, instead. However, leaders in localities with large unsheltered populations and burdened health and criminal justice systems

[61] Similarly, effects on *observed* exits to one's own housing or living with family or friends are consistently negative.

[62] See, for instance, Card et al. (2015) and Lalive (2008).

could expand shelter to mitigate the social consequences of homelessness and improve health and criminal justice outcomes for this particularly vulnerable population.

In work to come, we consider the effects of shelter on additional health outcomes. Results for psychiatric conditions are most pronounced, consistent with the over-representation of PEH among those with mental health conditions, but it may be the case that shelter also reduces, say, ER visits for conditions related to exposure to elements or injuries associated with weapons. We also investigate which crimes are most affected by shelter to provide greater detail on the criminal justice consequences of unsheltered homelessness. Additionally, we explore the possibility of heterogeneous treatment effects on a number of margins (e.g., by demographic characteristics) with the aim to help policymakers target shelter services in circumstances in which they are most effective or essential. Further, related to the work of Faraji, Ridgeway and Wu (2018), we assess the extent to which shelter may "shift" the consequences of homelessness to jurisdictions closest to the sites. In other words, while the overall effect of shelter may be to reduce crime and ER visits, we have yet to rule out the possibility that shelter concentrates homelessness in specific locations where crime and ER visits may, locally, increase.

Future work should give greater attention to the details of shelters and the experiences of individuals while in shelter. We note that there is likely a wide range of unobserved heterogeneity in the operation of shelters in our data. For instance, we have little insight into details like the scope of services offered at each shelter, whether the site is congregate or offers private sleeping quarters, staffing, rules and eligibility (e.g., number of bags or pets permitted), and more. Such details may be important considerations for increasing the effectiveness of future interim housing interventions. It is also worth noting that shelter may impact outcomes that we were not able to address (e.g., education, employment, and local economic activity all may reasonably be affected by shelter provision and have meaningful implications for both individual and social welfare). These warrant attention.

We also encourage further investigation of the role of sustained contact with case managers or service providers. By definition, the intervention we evaluate is temporary. A hypothesis consistent with our findings is that the quantity, quality, or duration of contact with service providers matters more than the type of service provided at the time of contact. Understanding the effects of shelter as a homelessness intervention is an important step, but homelessness will not be solved in the absence of a more holistic understanding of the effectiveness of the policy interventions at our disposal.

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Appendix

Appendix A. Additional Aggregate-Level Tables and Figures

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
other beds	0.7544***	−0.0020		−0.0031*		0.0011	
	(0.0079)	(0.0027)		(0.0017)		(0.0017)	
WS beds	0.8926***	−0.0068***		−0.0017		−0.0051***	
	(0.0085)	(0.0018)		(0.0012)		(0.0012)	
sheltered			−0.0076***		−0.0019		−0.0057***
			(0.0020)		(0.0014)		(0.0013)
Outcome Mean	391	126	126	59	59	67	67
Adj. R^2	0.9860	0.9461	0.9460	0.8960	0.8960	0.9168	0.9168
Num. obs.	17,528	17,528	17,528	17,528	17,528	17,528	17,528

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A.1: Replication of Table 1, including all 8 SPAs (not dropping SPAs 3, 7, and 8).

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
other beds	0.8240***	−0.0029		−0.0036**		0.0007	
	(0.0101)	(0.0026)		(0.0017)		(0.0017)	
WS beds	0.9675***	−0.0070***		−0.0019		−0.0051***	
	(0.0100)	(0.0018)		(0.0012)		(0.0012)	
sheltered			−0.0072***		−0.0019		−0.0053***
			(0.0018)		(0.0013)		(0.0012)
Outcome Mean	429	126	126	59	59	67	67
Adj. R^2	0.9823	0.9461	0.9461	0.8960	0.8960	0.9168	0.9168
Num. obs.	17,528	17,528	17,528	17,528	17,528	17,528	17,528

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A.2: Replication of Table 1, including all 8 SPAs and imposing 150% cap on occupancy instead of 120%

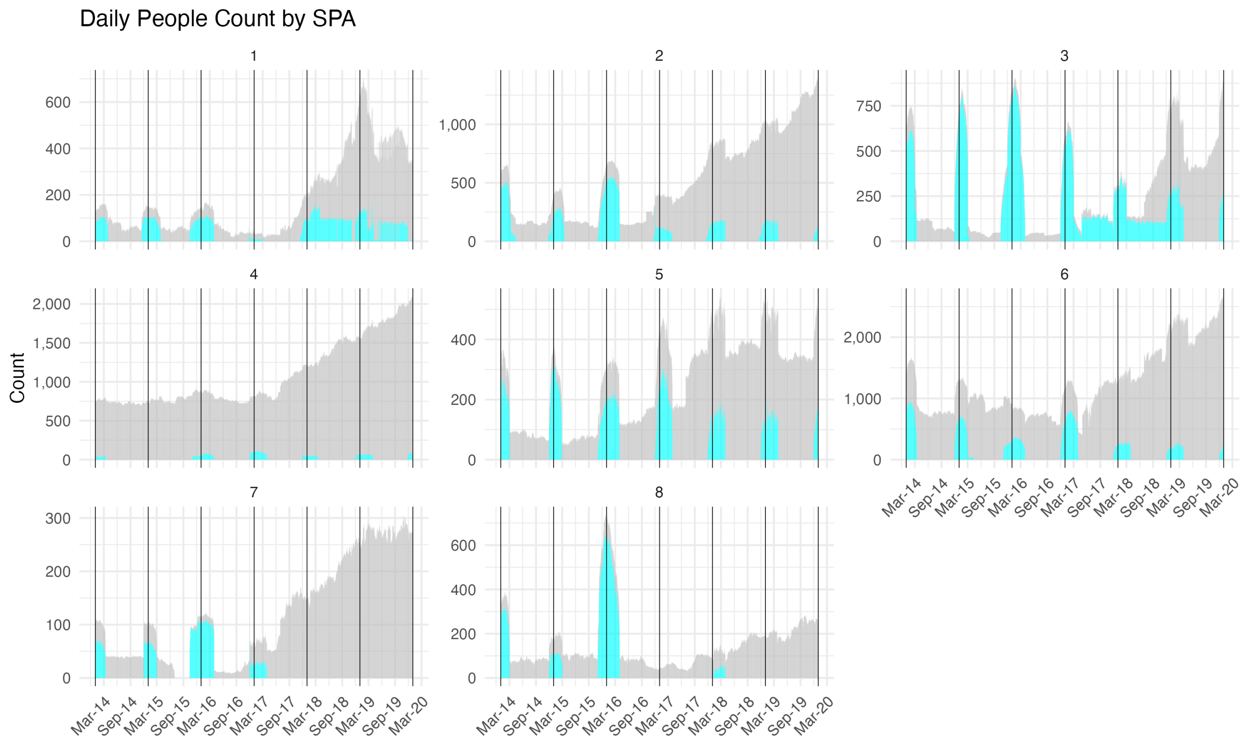

Figure A.1: Daily People Count by SPA and Winter Shelter (Blue) vs. Other Shelter (Grey)

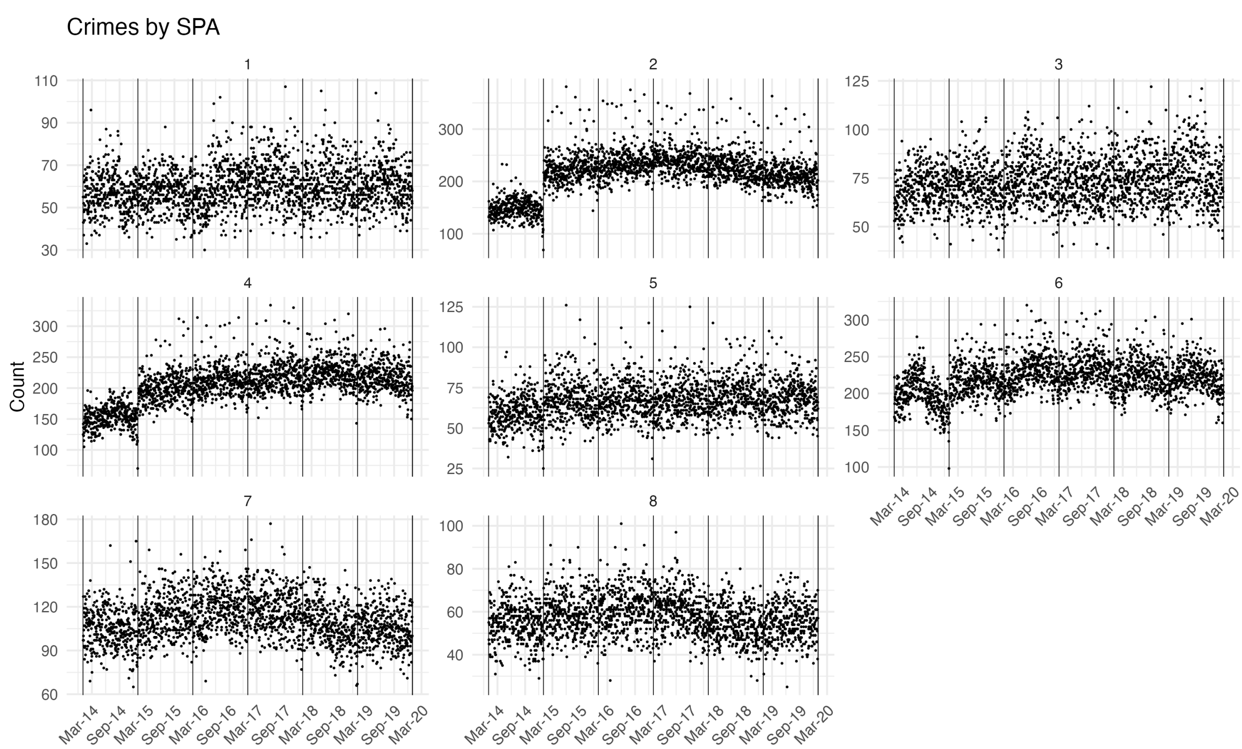

Figure A.2: Daily Count of Crimes Reported by LASD and LAPD by SPA

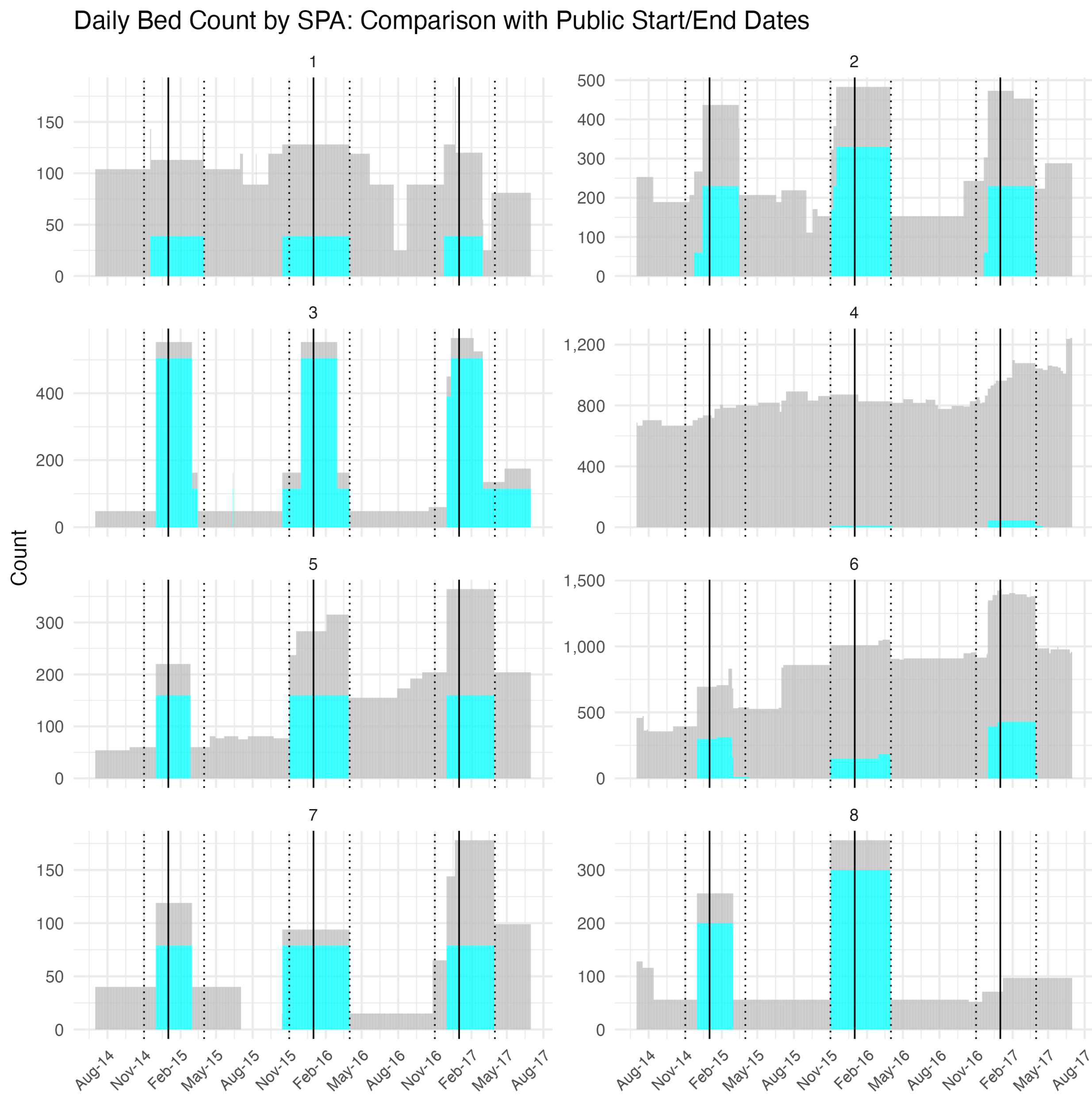

Figure A.3: Daily Bed Count for 2014-17 Seasons with Lines for November 1 and April 1.

Figure A.3 provides evidence that we have accurately identified shelter sites of interest in administrative data and presents a visual example of the variation our empirical approach exploits.

First, consider the 2015-2016 season as an example. Public records (announcement from LAHSA) reported that, in the 2015-2016 winter season, three shelter sites would open in mid-October (prior to November 1). These shelters were reported to be located in SPAs 1, 3, and 7. In Figure A.3, in 2015, note that we are identifying open winter shelters prior to November in exactly three SPAs - SPAs 1, 3, and 7 (and no others), reassuring us that the sites and dates of operation we have identified are accurate.

Second, inspection of any SPA shows variation in the opening and closing dates in different years. For instance, consider SPA 5. Winter shelter beds are available during the month of November in 2015 but not in other years shown. Thus, we are comparing outcomes in a November in which winter shelter beds are available to outcomes in Novembers in which winter shelter beds are *not* available. Similarly, winter shelter beds are available during the month of March in 2016 and 2017 but not in 2015. Thus, we are comparing outcomes in Marches when winter shelter beds are available to outcomes in a March when winter shelter beds are *not* available.

Appendix B. Additional Individual-Level Tables and Figures

Appendix B.1. Enrollments by Entry and Exit Circumstances

In the following tables, “sub” and “no sub” indicate whether the housing (rental or owned) is supported by a subsidy. “no ESV” indicates the absence of an emergency shelter voucher. “LTC facility” indicates a long-term care facility. “perm” and “temp” indicate whether the living situation (destination) with friends or family is permanent or temporary. “SH” represents “safe haven.” Detox and psych refer to settings for care of the specified type.

	2013-2023		2014-2019		2014-2019 shares (%)		
Living Situation	enrollments	share (%)	enrollments	share (%)	Emergency Shelter (ES)	Street Outreach (SO)	Services Only (SSO)
Street	544,634	44.53	205,854	39.09	43.34	45.36	38.64
NA (missing)	269,020	22.00	85,066	16.15	8.02	20.05	17.36
ES	154,256	12.61	82,183	15.61	24.21	3.93	19.05
Not collected	61,768	5.05	50,755	9.64	2.67	25.92	2.57
Family	27,580	2.26	16,154	3.07	4.31	0.89	2.52
Rental, no sub	26,084	2.13	13,584	2.58	0.91	0.30	1.38
Friend	25,449	2.08	14,036	2.67	4.26	0.64	2.82
Rental, sub	25,144	2.06	10,222	1.94	0.44	0.33	2.41
TH	23,015	1.88	12,289	2.33	1.22	0.38	2.17
Hotel, no ESV	15,158	1.24	8144	1.55	2.74	0.30	0.94
Jail	15,052	1.23	9156	1.74	1.58	0.26	7.61
Hospital	9044	0.74	5302	1.01	2.51	0.28	0.42
Detox	7920	0.65	3522	0.67	0.56	0.36	0.92
Safe haven	5164	0.42	2817	0.53	0.88	0.30	0.32
Psych	3802	0.31	2937	0.56	1.59	0.10	0.14
Unknown	2293	0.19	859	0.16	0.16	0.14	0.33
Remainder	2085	0.17	1168	0.22	0.23	0.17	0.07
Refused	1793	0.15	820	0.16	0.09	0.20	0.03
Owned, no sub	1250	0.10	583	0.11	0.14	0.01	0.08
LTC facility	924	0.08	655	0.12	0.06	0.03	0.10
Halfway house	791	0.06	249	0.05	0.04	0.02	0.06
Owned, sub	780	0.06	266	0.05	0.03	0.01	0.06

Table B.1: Enrollments by living situation prior to project entry. The final 3 columns present shares for the 3 project types included in our analysis.

Exit Destination	Enrollments	share (%)	ES (%)	SO (%)	SSO (%)
No interview	172,870	32.83	28.96	59.48	22.50
Not collected	69,101	13.12	26.44	8.63	16.02
Rental, sub	54,272	10.31	8.49	2.09	7.90
NA (missing)	45,052	8.55	0.51	10.02	25.16
Rental, no sub	44,426	8.44	3.07	0.46	3.74
ES	29,196	5.54	8.40	4.72	4.09
Street	28,483	5.41	2.88	7.65	6.80
Other	16,786	3.19	3.01	1.98	3.29
TH	11,384	2.16	3.08	0.82	2.35
Unknown	10,361	1.97	4.95	0.19	2.35
Family, perm	9266	1.76	1.53	0.76	1.28
Family, temp	6790	1.29	1.84	0.21	0.89
Refused	6014	1.14	1.70	1.22	0.36
Friends, temp	4276	0.81	1.00	0.20	0.63
Detox	2734	0.52	0.78	0.44	0.26
Deceased	2279	0.43	0.11	0.20	0.32
Friends, perm	2259	0.43	0.38	0.09	0.29
Hotel, no ESV	2218	0.42	0.75	0.08	0.20
Jail	2192	0.42	0.48	0.13	0.66
Hospital	1441	0.27	0.53	0.09	0.16
Remainder	1280	0.24	0.37	0.23	0.05
SH	998	0.19	0.33	0.07	0.15
LTC facility	926	0.18	0.12	0.11	0.25
Owned, no sub	729	0.14	0.07	0.01	0.08
Owned, sub	613	0.12	0.07	0.03	0.12
Psych	476	0.09	0.12	0.08	0.05
Halfway house	199	0.04	0.04	0.02	0.05

Table B.2: Enrollments by recorded exit destination. The final 3 columns present shares for the 3 project types included in our analysis.

Appendix B.2. Dependent Variables Under Alternative Windows

		Recid_0_6		Recid_0_12		Recid_0_18		Recid_0_∞	
	FS	RF	IV	RF	IV	RF	IV		
other beds	0.0044***	−0.0044***		−0.0028***		−0.0048***		−0.0036***	
	(0.0008)	(0.0009)		(0.0009)		(0.0009)		(0.0008)	
WS beds	0.0475***	0.0041***		0.0065***		0.0059***		0.0041***	
	(0.0009)	(0.0011)		(0.0011)		(0.0011)		(0.0010)	
ES			0.0869***		0.1363***		0.1231***		0.0873***
			(0.0234)		(0.0229)		(0.0225)		(0.0202)
Outcome Mean	0.4112	0.5542	0.5542	0.6214	0.6214	0.6553	0.6553	0.7534	0.7534
Adj. R^2	0.3727	0.1085	0.1234	0.1054	0.1235	0.1054	0.1224	0.1024	0.1143
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.3: Effects on recidivism outcome under different windows (different values of A and B).

		Recid_6_18		Recid_6_∞		Recid_12_24		Recid_12_∞	
	FS	RF	IV	RF	IV	RF	IV		
other beds	0.0044***	−0.0008		−0.0011		−0.0017*		−0.0029***	
	(0.0008)	(0.0010)		(0.0010)		(0.0010)		(0.0010)	
WS beds	0.0475***	0.0014		0.0010		0.0044***		0.0033***	
	(0.0009)	(0.0011)		(0.0011)		(0.0011)		(0.0012)	
ES			0.0297		0.0217		0.0924***		0.0702***
			(0.0239)		(0.0237)		(0.0232)		(0.0243)
Outcome Mean	0.4112	0.4023	0.4023	0.5992	0.5992	0.3378	0.3378	0.5362	0.5362
Adj. R^2	0.3727	0.0536	0.0553	0.0647	0.0659	0.0436	0.0426	0.0536	0.0539
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.4: Effects on recidivism outcome under different windows (different values of A and B).

		Success_0_6		Success_0_12		Success_0_18		Success_0_∞	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0044^{***}	0.0028^{***}		0.0037^{***}		0.0051^{***}		0.0021^{**}	
	(0.0008)	(0.0006)		(0.0007)		(0.0008)		(0.0009)	
WS beds	0.0475^{***}	-0.0135^{***}		-0.0179^{***}		-0.0213^{***}		-0.0251^{***}	
	(0.0009)	(0.0007)		(0.0008)		(0.0009)		(0.0011)	
ES			-0.2829^{***}		-0.3757^{***}		-0.4488^{***}		-0.5272^{***}
			(0.0161)		(0.0191)		(0.0209)		(0.0250)
Outcome Mean	0.4112	0.1084	0.1084	0.1599	0.1599	0.1941	0.1941	0.3276	0.3276
Adj. R^2	0.3727	0.0367	-0.1509	0.0470	-0.1961	0.0520	-0.2433	0.0617	-0.2211
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.5: Effects on success outcome under different windows (different values of B).

		mortality_6		mortality_12		mortality_18		mortality_∞	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0044^{***}	0.0001		0.0001		0.0000		-0.0001	
	(0.0008)	(0.0001)		(0.0001)		(0.0001)		(0.0003)	
WS beds	0.0475^{***}	-0.0001		-0.0004^{***}		-0.0006^{***}		-0.0005	
	(0.0009)	(0.0001)		(0.0001)		(0.0002)		(0.0004)	
ES			-0.0026		-0.0086^{***}		-0.0117^{***}		-0.0101
			(0.0019)		(0.0027)		(0.0032)		(0.0076)
Outcome Mean	0.4112	0.0017	0.0017	0.0032	0.0032	0.0048	0.0048	0.0239	0.0239
Adj. R^2	0.3727	0.0012	0.0008	0.0025	-0.0005	0.0040	0.0001	0.0147	0.0144
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.6: Effects on mortality outcome under different windows (different values of B).

Appendix B.3. Recidivism by Project at Reappearance

		Emergency Shelter		Day Shelter		Street Outreach		Services Only	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0044***	0.0014*		0.0004		−0.0029***		0.0009	
	(0.0008)	(0.0008)		(0.0003)		(0.0008)		(0.0006)	
WS beds	0.0475***	0.0145***		0.0004		−0.0042***		0.0009	
	(0.0009)	(0.0010)		(0.0003)		(0.0008)		(0.0007)	
ES			0.3050***		0.0077		−0.0889***		0.0181
			(0.0208)		(0.0073)		(0.0175)		(0.0146)
Outcome Mean	0.4112	0.2025	0.2025	0.0250	0.0250	0.1988	0.1988	0.1020	0.1020
Adj. R^2	0.3727	0.0403	0.0329	0.0101	0.0110	0.0523	0.0574	0.0296	0.0290
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.7: Recidivism by project type at reappearance. Columns 2-3 present effects of shelter on recidivism to emergency shelter in 6-18 months. Subsequent columns present the estimated effects of shelter on recidivism to day shelter, street outreach, and services only, respectively.

		Rapid Re-housing		Transitional Housing		Permanent Housing		Other	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0044***	0.0021***		0.0003		0.0006		−0.0006**	
	(0.0008)	(0.0004)		(0.0002)		(0.0004)		(0.0003)	
WS beds	0.0475***	−0.0055***		−0.0013***		−0.0040***		−0.0010***	
	(0.0009)	(0.0005)		(0.0003)		(0.0005)		(0.0003)	
ES			−0.1148***		−0.0275***		−0.0832***		−0.0213***
			(0.0104)		(0.0055)		(0.0099)		(0.0064)
Outcome Mean	0.4112	0.0500	0.0500	0.0106	0.0106	0.0405	0.0405	0.0151	0.0151
Adj. R^2	0.3727	0.0196	−0.0470	0.0108	−0.0067	0.0157	−0.0112	0.0071	−0.0003
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.8: Recidivism by project type at reappearance. Columns 2-3 present effects of shelter on recidivism to rapid re-housing in 6-18 months. Subsequent columns present the estimated effects of shelter on recidivism to transitional housing, permanent housing, and any other project type (not specified in this table or the previous), respectively.

Appendix B.4. Other Sample Window Restrictions

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0417^{***}	0.0048		-0.0045		0.0076^{**}		-0.0004	
	(0.0031)	(0.0037)		(0.0030)		(0.0038)		(0.0005)	
WS beds	0.0344^{***}	-0.0016		-0.0184^{***}		0.0040		-0.0006^{**}	
	(0.0017)	(0.0020)		(0.0017)		(0.0025)		(0.0003)	
ES			-0.0474		-0.5339^{***}		0.1237		-0.0188^{**}
			(0.0594)		(0.0548)		(0.0784)		(0.0076)
Outcome Mean	0.4793	0.3917	0.3917	0.1787	0.1787	0.1689	0.1689	0.0038	0.0038
Adj. R^2	0.4020	0.0542	0.0507	0.0529	-0.3086	0.0477	0.0561	0.0034	-0.0089
Num. obs.	118,972	118,972	118,972	118,972	118,972	62,470	62,470	118,972	118,972

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.9: Replication of Table 7, restricting to January-April entries only.

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
other beds	0.0428^{***}	0.0016		-0.0029		0.0188^{***}		-0.0009^{*}	
	(0.0043)	(0.0050)		(0.0038)		(0.0070)		(0.0005)	
WS beds	0.0461^{***}	-0.0022		-0.0213^{***}		0.0100^{**}		-0.0011^{***}	
	(0.0026)	(0.0027)		(0.0024)		(0.0047)		(0.0004)	
ES			-0.0478		-0.4628^{***}		0.2154^{**}		-0.0228^{***}
			(0.0586)		(0.0566)		(0.1013)		(0.0089)
Outcome Mean	0.4321	0.2411	0.2411	0.1252	0.1252	0.2384	0.2384	0.0018	0.0018
Adj. R^2	0.4547	0.0435	0.0396	0.0677	-0.2598	0.0670	0.0915	0.0042	-0.0349
Num. obs.	53,599	53,599	53,599	53,599	53,599	23,158	23,158	53,599	53,599

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.10: Replication of Table 7, restricting to January-April entries AND first time i is observed only.

Online Appendix

Online Appendix I. Supplemental Aggregate-Level Content

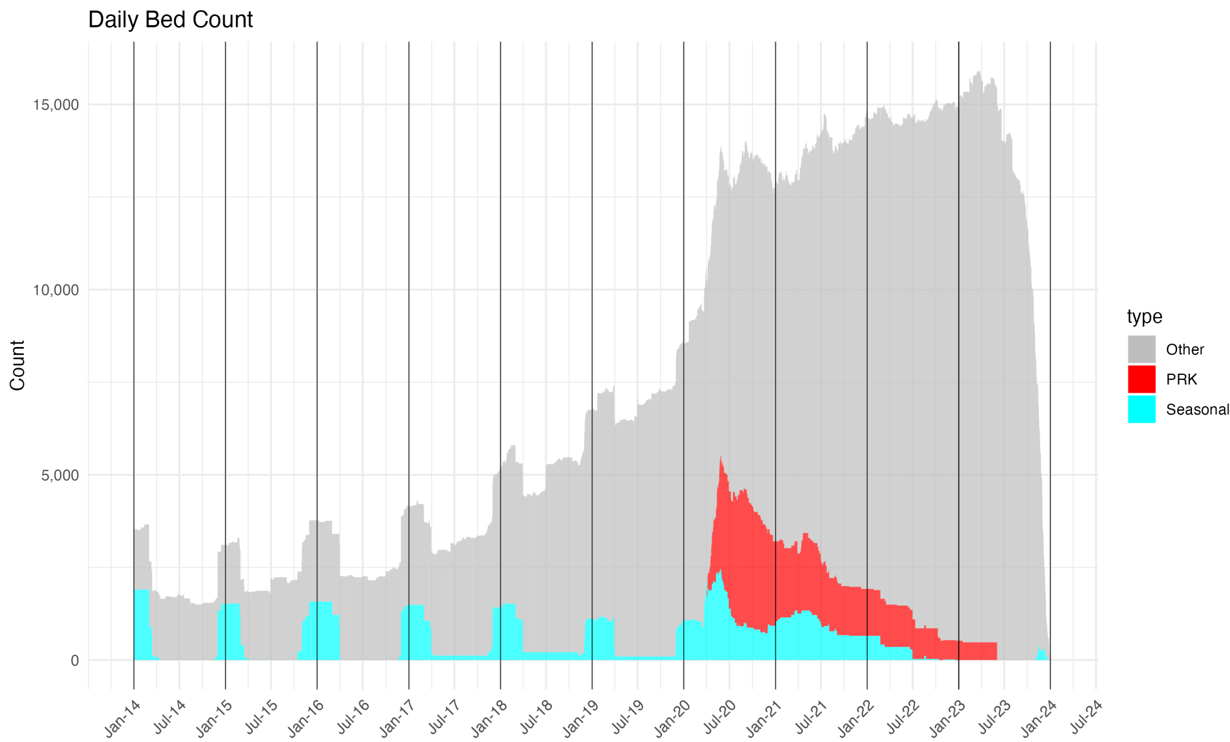

Figure I.1: Daily Bed Count through End of 2023 (including Project Room Key)

Online Appendix II. Supplemental Individual-Level Content

		Recid_6_18		Success_18		Newly Homeless		Mortality_18	
	FS	RF	IV	RF	IV	RF	IV		
Female	−0.0186***	0.0311***	0.0316***	0.0617***	0.0534***	0.0097***	0.0097***	−0.0019***	−0.0021***
	(0.0014)	(0.0018)	(0.0018)	(0.0015)	(0.0017)	(0.0018)	(0.0019)	(0.0002)	(0.0003)
Other Gender	−0.0284***	−0.0573***	−0.0565***	−0.0174***	−0.0301***	−0.0360***	−0.0361***	−0.0013*	−0.0016**
	(0.0043)	(0.0050)	(0.0051)	(0.0037)	(0.0045)	(0.0111)	(0.0115)	(0.0007)	(0.0007)
Asian	0.0467***	−0.0196***	−0.0210***	0.0133**	0.0342***	0.0364***	0.0364***	−0.0008	−0.0002
	(0.0062)	(0.0076)	(0.0077)	(0.0059)	(0.0068)	(0.0082)	(0.0084)	(0.0012)	(0.0012)
Black	0.0475***	0.0461***	0.0447***	0.0756***	0.0969***	−0.0119***	−0.0119***	−0.0018***	−0.0013***
	(0.0017)	(0.0021)	(0.0024)	(0.0017)	(0.0022)	(0.0021)	(0.0027)	(0.0003)	(0.0003)
Multiple Races	0.0437***	0.0842***	0.0829***	0.0468***	0.0664***	−0.0282***	−0.0281***	−0.0015**	−0.0010
	(0.0047)	(0.0057)	(0.0058)	(0.0047)	(0.0054)	(0.0052)	(0.0054)	(0.0007)	(0.0007)
Native Am	0.0088*	0.0514***	0.0511***	0.0138***	0.0178***	−0.0134**	−0.0134**	−0.0004	−0.0003
	(0.0051)	(0.0065)	(0.0065)	(0.0051)	(0.0059)	(0.0061)	(0.0061)	(0.0009)	(0.0010)
Native HI/Pacific	0.0028	−0.0277***	−0.0278***	−0.0061	−0.0048	−0.0035	−0.0035	−0.0003	−0.0003
	(0.0069)	(0.0087)	(0.0087)	(0.0067)	(0.0077)	(0.0091)	(0.0091)	(0.0013)	(0.0013)
Unknown Race	−0.1010***	−0.1855***	−0.1825***	−0.0677***	−0.1131***	0.0140**	0.0139**	−0.0016***	−0.0028***
	(0.0030)	(0.0035)	(0.0043)	(0.0028)	(0.0040)	(0.0055)	(0.0060)	(0.0004)	(0.0005)
Hispanic	0.0338***	0.0095***	0.0084***	0.0262***	0.0414***	0.0105***	0.0106***	−0.0013***	−0.0009***
	(0.0018)	(0.0022)	(0.0024)	(0.0017)	(0.0021)	(0.0023)	(0.0025)	(0.0003)	(0.0003)
Age	−0.0047***	0.0130***	0.0131***	−0.0105***	−0.0127***	−0.0130***	−0.0130***	−0.0002***	−0.0003***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0001)	(0.0001)
Age^2	0.0000***	−0.0001***	−0.0001***	0.0001***	0.0001***	0.0001***	0.0001***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Disabled	0.0459***	0.0734***	0.0720***	0.0555***	0.0761***	−0.0638***	−0.0638***	0.0038***	0.0044***
	(0.0015)	(0.0019)	(0.0022)	(0.0015)	(0.0020)	(0.0017)	(0.0024)	(0.0003)	(0.0003)
other beds	0.0044***	−0.0008	−0.0009	0.0051***	0.0071***	0.0011	0.0011	0.0000	0.0001
	(0.0008)	(0.0010)	(0.0010)	(0.0008)	(0.0009)	(0.0010)	(0.0010)	(0.0001)	(0.0001)
WS beds	0.0475***	0.0014		−0.0213***		−0.0000		−0.0006***	
	(0.0009)	(0.0011)		(0.0009)		(0.0013)		(0.0002)	
ES			0.0297		−0.4488***		−0.0011		−0.0117***
			(0.0239)		(0.0209)		(0.0326)		(0.0032)
Outcome Mean	0.4112	0.4023	0.4023	0.1941	0.1941	0.1546	0.1546	0.0048	0.0048
Adj. R^2	0.3727	0.0536	0.0553	0.0520	−0.2433	0.0415	0.0412	0.0040	0.0001
Num. obs.	332,343	332,343	332,343	332,343	332,343	181,419	181,419	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table II.1: Replication of Table 7, reporting all coefficient estimates (except fixed effects).

	Recid_6_18			Success_18		
Female		0.0373^{***}	0.0323^{***}		0.0701^{***}	0.0650^{***}
		(0.0018)	(0.0018)		(0.0015)	(0.0015)
Other Gender		-0.0848^{***}	-0.0552^{***}		-0.0218^{***}	-0.0086^{**}
		(0.0049)	(0.0050)		(0.0036)	(0.0037)
Asian		-0.0196^{**}	-0.0225^{***}		0.0045	0.0083
		(0.0076)	(0.0076)		(0.0059)	(0.0058)
Black		0.0429^{***}	0.0433^{***}		0.0665^{***}	0.0710^{***}
		(0.0021)	(0.0021)		(0.0017)	(0.0017)
Multiple Races		0.0725^{***}	0.0815^{***}		0.0401^{***}	0.0421^{***}
		(0.0057)	(0.0057)		(0.0047)	(0.0047)
Native Am		0.0470^{***}	0.0509^{***}		0.0119^{**}	0.0128^{**}
		(0.0065)	(0.0064)		(0.0051)	(0.0050)
Native HI/Pacific		-0.0333^{***}	-0.0279^{***}		-0.0057	-0.0076
		(0.0088)	(0.0087)		(0.0067)	(0.0067)
Unknown Race		-0.1850^{***}	-0.1792^{***}		-0.0563^{***}	-0.0554^{***}
		(0.0036)	(0.0035)		(0.0027)	(0.0027)
Hispanic		0.0138^{***}	0.0074^{***}		0.0248^{***}	0.0221^{***}
		(0.0022)	(0.0022)		(0.0017)	(0.0017)
Age		0.0126^{***}	0.0132^{***}		-0.0113^{***}	-0.0102^{***}
		(0.0003)	(0.0003)		(0.0003)	(0.0003)
Age^2		-0.0001^{***}	-0.0001^{***}		0.0001^{***}	0.0001^{***}
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
Disabled		0.0721^{***}	0.0706^{***}		0.0548^{***}	0.0507^{***}
		(0.0019)	(0.0019)		(0.0015)	(0.0015)
ES	0.0556^{***}	0.0365^{***}	0.0607^{***}	0.0703^{***}	0.0576^{***}	0.1101^{***}
	(0.0017)	(0.0017)	(0.0021)	(0.0014)	(0.0014)	(0.0020)
Fixed Effects	No	No	Yes	No	No	Yes
Outcome Mean	0.4023	0.4023	0.4023	0.1941	0.1941	0.1941
Adj. R^2	0.0031	0.0402	0.0559	0.0077	0.0340	0.0622
Num. obs.	332,343	332,343	332,343	332,343	332,343	332,343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table II.2: Naive regressions, estimating the correlation between ES and choice outcomes.

	Newly Homeless			Mortality_18		
Female		0.0161^{***}	0.0119^{***}		-0.0017^{***}	-0.0019^{***}
		(0.0017)	(0.0017)		(0.0002)	(0.0002)
Other Gender		-0.0361^{***}	-0.0287^{***}		-0.0019^{***}	-0.0012^{*}
		(0.0110)	(0.0110)		(0.0007)	(0.0007)
Asian		0.0301^{***}	0.0319^{***}		-0.0007	-0.0007
		(0.0082)	(0.0082)		(0.0012)	(0.0012)
Black		-0.0110^{***}	-0.0162^{***}		-0.0018^{***}	-0.0018^{***}
		(0.0020)	(0.0021)		(0.0003)	(0.0003)
Multiple Races		-0.0298^{***}	-0.0321^{***}		-0.0017^{**}	-0.0015^{**}
		(0.0052)	(0.0052)		(0.0007)	(0.0007)
Native Am		-0.0123^{**}	-0.0147^{**}		-0.0005	-0.0004
		(0.0061)	(0.0060)		(0.0009)	(0.0009)
Native HI/Pacific		-0.0018	-0.0038		-0.0006	-0.0003
		(0.0091)	(0.0090)		(0.0013)	(0.0013)
Unknown Race		0.0267^{***}	0.0200^{***}		-0.0019^{***}	-0.0016^{***}
		(0.0055)	(0.0055)		(0.0004)	(0.0004)
Hispanic		0.0064^{***}	0.0075^{***}		-0.0010^{***}	-0.0013^{***}
		(0.0022)	(0.0022)		(0.0003)	(0.0003)
Age		-0.0137^{***}	-0.0128^{***}		-0.0002^{***}	-0.0002^{***}
		(0.0004)	(0.0004)		(0.0001)	(0.0001)
Age^2		0.0001^{***}	0.0001^{***}		0.0000^{***}	0.0000^{***}
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
Disabled		-0.0592^{***}	-0.0595^{***}		0.0040^{***}	0.0039^{***}
		(0.0017)	(0.0017)		(0.0003)	(0.0003)
ES	0.0962^{***}	0.0901^{***}	0.0850^{***}	-0.0012^{***}	-0.0014^{***}	-0.0007^{**}
	(0.0017)	(0.0016)	(0.0019)	(0.0002)	(0.0002)	(0.0003)
Fixed Effects	No	No	Yes	No	No	Yes
Outcome Mean	0.1546	0.1546	0.1546	0.0048	0.0048	0.0048
Adj. R^2	0.0176	0.0406	0.0515	0.0001	0.0034	0.0040
Num. obs.	181, 419	181, 419	181, 419	332, 343	332, 343	332, 343

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table II.3: Naive regressions, estimating the correlation between ES and choice outcomes.

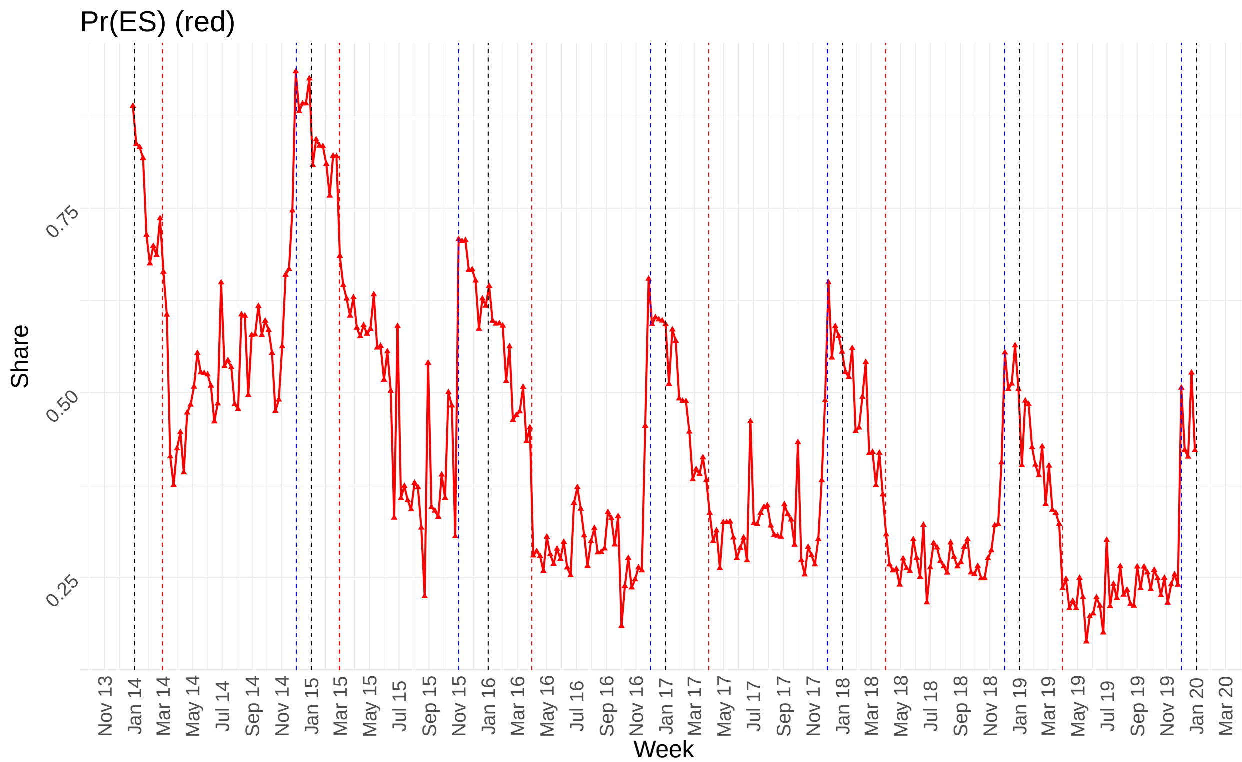

Figure II.2: Weekly share of sample enrollments that are for ES (as opposed to SO or SSO).

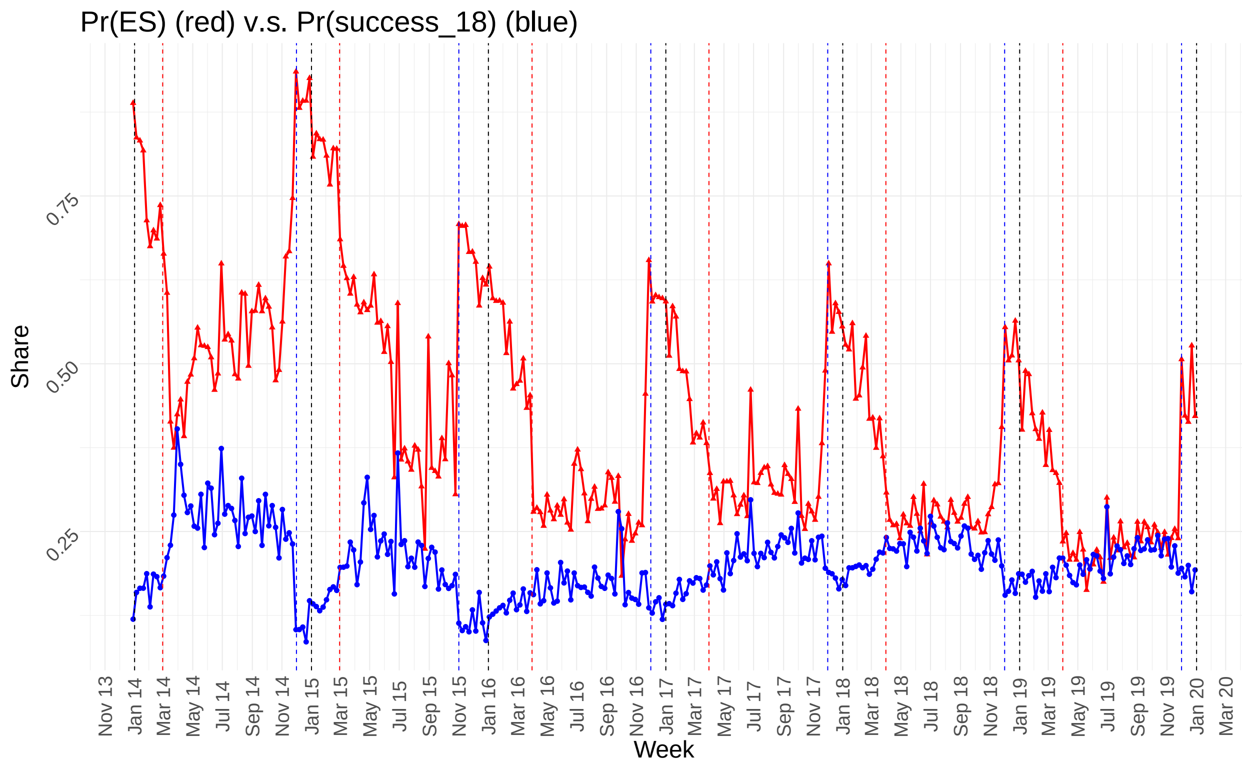

Figure II.3: Weekly share of sample enrollments that are for ES (as opposed to SO or SSO) + weekly share of enrollments where a successful exit is observed within 18 months. Notice the dip at the same time the share of shelter enrollments spike, providing visual evidence of the negative relationship we can identify.

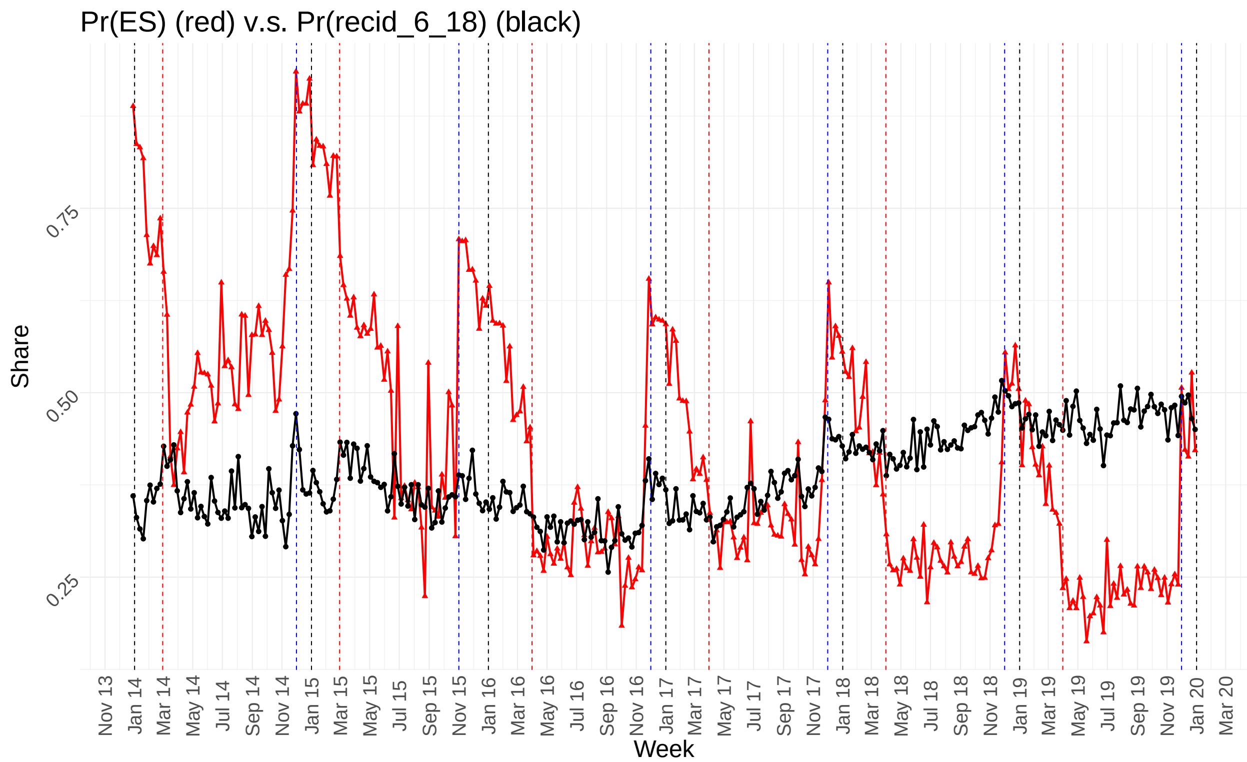

Figure II.4: Weekly share of sample enrollments that are for ES (as opposed to SO or SSO) + weekly share of enrollments where i is observed to recidivate within 6 to 18 months, showing possible evidence of a weak positive effect, consistent with regression results.

Online Appendix III. Construction of Daily Shelter Counts

Online Appendix III.1. Identifying Site Locations

All enrollments in the HMIS data are associated with a project ID to identify (anonymously) the project in which an individual is enrolling. Each project is categorized as one of 13 different project types as outlined in the text.[63] Each project may, however, operate at one or many locations (e.g., a specific shelter project that has an office in SPA 2 and a separate office in SPA 4). More than 97.5% of all enrollments can be matched uniquely on the combination of project ID and location and contain valid location information. For the enrollments that do not match uniquely on this combination, we attempt to match on project ID alone, understanding that most projects only operate at one location.

- More than 80% of these unmatched enrollments are matched uniquely on project ID
  - Just over 10% of these have valid location information (meaning the location code in the original records was missing or incorrect).
  - The rest, while matched uniquely, are missing location information. Fortunately, none of these correspond to emergency shelter project types and are therefore, unnecessary for the construction of data on shelters.
- The remaining 20% of these unmatched enrollments corresponded to projects that had multiple possible locations
  - Over 99% of these with duplicated matches only had 1 match where the location information was valid (namely, ZIP code was present). We assume that any such duplicates with missing location information is an error and identify location of the project based on the only valid ZIP code reported.
  - None of the other duplicates correspond to emergency shelter project types and are therefore, unnecessary for the construction of data on shelters.

Ultimately, over 98% of all enrollments are matched to a valid project site location, and all others are for non-emergency shelter projects (and overwhelmingly are for enrollments outside of our sample period, as well). Thus, all enrollments at emergency shelters are matched to a unique project location.

Online Appendix III.2. Site Characteristics

All emergency shelter projects are then linked to inventory records. Inventory records are unreliable (even HMIS guidelines state that start dates can be approximated) and appear to be infrequently updated. For instance, it is common for individuals to enter projects years before

[63] Technically, the most recent version of the data available to us splits ES into two separate categories, resulting in 14 project types.

inventory records would indicate that such projects existed. We have validated that this is usually the result of inventory start dates being determined incorrectly or retroactively with some error.[64] When an enrollment at a project occurs on a date outside of the recorded operation period of the shelter, the bed count on that date is inferred from the most recently observed bed count at the site.

Thus, we have bed counts by site location.

Online Appendix III.3. Occupancy

We next determine the number of occupants at all sites across all dates from 2013-2023. We begin by restricting to enrollments where exit date is valid (i.e., it is present and occurs on or after the entry date). This condition eliminates just over 5% of enrollments, driven largely by enrollments in 2023 (recent periods are more likely to contain enrollments without valid exit dates because no exit had occurred at the time the data was generated).

Shelter enrollments may be recorded two different ways - "entry-exit" (about 20% of enrollments) or "night-by-night" (about 80% of enrollments). Entry-exit provides only an entry date and an exit date and indicates that the individual enrolled was present at the shelter at all dates within that window. Night-by-night indicates that the individual enrolled was present on some subset of the dates between the entry and exit date. These "service dates" are recorded in a separate file and matched to enrollments to determine all dates on which an individual was present at the shelter site. Unfortunately, while all of these "night-by-night" enrollments should have service date records, such records are only present for about 60% (i.e., 40% of these enrollments do not match to any service records). The enrollments that do not match to service records are assumed to be erroneously coded as "night-by-night" when they should have been coded as "entry-exit." In other words, in the absence of service dates, we assume that the individual is present (unless observed in shelter elsewhere) at the site at all dates within the entry-exit window. To the extent that this assumption fails to hold, we will overstate the number of occupants on any site-day.

Combined with the noted issues with timely recording of project exits, occupancy will likely be overstated, sometimes by large margins. We address this later by testing the robustness of our findings under the imposition of various occupancy caps. Perhaps most importantly, overstated occupancy does not directly affect the counts of available shelter beds. So, all reduced-form estimates and the entirety of the individual-level analysis are unaffected by potentially inflated counts of occupants.

Online Appendix III.4. Finalizing Counts

After the above procedures are applied to the HMIS data, we are left with a data set containing bed counts by location (SPA) of service provider (and a flag for whether the project is operating

[64] We note that the data has improved quite drastically over the last 5 to 10 years and are optimistic that LAHSA's records will be more reliable going forward. Data users should exercise caution with older records, however.

as a "seasonal" shelter) plus records of all nights in which individuals are recorded to be present at each site, which we aggregate to produce a file of site-by-day counts of beds and occupants.

Two challenges with this data remain. First, because inventory records do not have reliable start or end dates, there are an overwhelming number of instances in which the recorded opening and/or closing of a shelter is verifiably incorrect.[65] Because project entry dates are more reliably and consistently recorded, we identify dates of shelter operation based on when people are present at the site as determined by enrollment records.

This leads us to the second challenge - exits from enrollments are not recorded in a timely manner.[66] This means that people are often recorded to be present in the shelter when their record should have been updated to reflect their exit. This has two consequences.

1. People are observed to be present even when the shelter is actually closed, which, absent reliable open/close dates from inventory records, would lead us to often erroneously conclude that the shelter is open (e.g., if all but one person at shelter A have their exit dates recorded accurately, the presence of one person who has not exited would indicate that the shelter is still operating when this is clearly false).

2. Too many people are observed to be present when the shelter *is* open.

To address the first, we apply the following procedure. A shelter is assumed to be open unless:

- it is observed to be operating at $< 15\%$ capacity for 3 consecutive weeks
- it is observed to have the exact same number of occupants for 2 consecutive months[67]

Additionally, we compute the maximum occupancy rate (people divided by beds) over the full sample period for each site. Any site that never reports at least 25% occupancy on any date is dropped altogether due to data quality.

To address the second, we impose a cap on occupancy as described in the text (e.g., 120% or 150%).

[65] Recall, as evidence, we observe entries occurring years before the earliest reported opening dates, and even when open/close dates are updated in a timely manner or retroactively, they are commonly approximated.

[66] See discussion of "purge dates" in the text.

[67] This condition is only applied when that number of occupants represents $< 50\%$ of bed capacity (to avoid dropping shelters that persistently record the same number of occupants because they are full). The motivation for adding this condition is that, occasionally, it is the case that, say, a 100-bed shelter reporting 119 occupants suddenly drops to a reported 19 occupants and continues reporting 19 occupants persistently. The reality is that those 19 records probably belong to enrollments that should have been updated with an exit date and that the shelter legitimately closed when we saw occupancy drop from 119 to 19. However, because 19 people corresponds to $> 15\%$ capacity, the first condition does not bind.