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THE SOCIAL AND INDIVIDUAL EFFECTS OF HOMELESS SHELTER: EVIDENCE FROM TEMPORARY SHELTER PROVISION

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At least one co-author has disclosed additional relationships of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w34376

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ABSTRACT

What does homeless shelter achieve? We leverage administrative records of homeless services in Los Angeles County to construct a novel dataset of daily, site-level counts of shelter beds and occupants from 2014 to 2019. We pair this with daily, block-level crime incident data and daily, hospital-level data on ER visits to assess the relationship between shelter and area crime and health. We exploit variation from shocks to shelter availability from Los Angeles County's winter shelters program to study the effects of providing temporary shelter. We find that reducing unsheltered homelessness significantly reduces crime and ER visits for psychiatric conditions. We conclude with evidence that entering shelter also reduces short-run mortality but find no evidence that temporary shelter reduces future homelessness more than street outreach or other non-shelter services. Our findings suggest that shelter functions as a public good with high social benefits. When agents charged with provision of homeless services are evaluated on their ability to reduce overall homelessness, they are unlikely to internalize these benefits and may under provide shelter.

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1. Introduction

Homelessness is a highly visible economic and social problem, affecting a wide array of communities. In the U.S., more than 750,000 people are homeless on any given night with more than a third sleeping outdoors in unsheltered conditions.[1] In recent years, homelessness has grown faster in states like Oklahoma, Idaho, and South Carolina than it has in Seattle, San Francisco, and Los Angeles.[2] However, the issue remains particularly intractable in California, which accounts for 45% of the nation's unsheltered population. As the nation's most populous state and the world's fourth largest economy, California spends more than $5 billion annually on homeless services,[3] but homelessness continues to worsen, rising 50% in the last decade with increases every year since 2018. In California and elsewhere, there remains persistent and widespread disagreement on how best to allocate these dollars, stemming from disagreement about the causes and consequences of homelessness and a lack of evidence on the efficacy of interventions intended to address the crisis.

Aside from street outreach efforts, the most common homelessness intervention is emergency shelter, an increasingly central tool as cities seek to clear encampments and reduce unsheltered homelessness within constrained budgets.[4] However, investing in shelter is often contested, in part due to a lack of rigorous evidence on the effects of shelter. Proponents argue that it serves as a cost-effective public good that mitigates the social costs of unsheltered homelessness, while critics assert that it diverts scarce resources from permanent housing solutions that more effectively reduce total homelessness. While it seems plausible that unsheltered homelessness imposes high social costs through increased crime, worse public health, and other negative externalities, causal evidence on both the sign and magnitude of these effects remains virtually nonexistent.

We leverage plausibly exogenous variation in daily shelter availability from staggered openings and closings of seasonal shelters across locations and seasons to provide the first causal evidence on how emergency shelter affects community and individual outcomes. Our primary research questions examine whether shelter provision reduces the negative externalities of homelessness - specifically crime and emergency healthcare utilization - and whether shelter improves individual outcomes (future homelessness and short-run mortality) relative to remaining unsheltered.

Our setting is Los Angeles County, which plays an outsized role in homelessness nationally. Comprised of 88 cities and nearly 10 million people, the county is home to 3% of the nation's population and 10% of the national homeless population. Since 2014, Los Angeles has seen its homeless population more than double, and as of 2024, despite slowing growth in homelessness, it

[1] Annual counts of sheltered and unsheltered homelessness for all localities are available via HUD (e.g., https://www.huduser.gov/portal/datasets/ahar/2024-ahar-part-1-pit-estimates-of-homelessness-in-the-us.html). While reliable estimates are often unavailable, rising homelessness is an issue affecting many developed countries, including Canada (https://madeinca.ca/homelessness-statistics-canada/) and England (https://england.shelter.org.uk/media/press_release/at_least_354000_people_homeless_in_england_today_).

[2] Between 2022 and 2024, 37 states had higher homelessness growth rates than San Francisco or Los Angeles.

[3] California State Auditor (e.g., Parks (2024)) provides recent estimates.

[4] For example, Los Angeles recently announced that its unsheltered homeless count dropped for the second year in a row in 2025, and attributed part of its success to moving people inside (https://mayor.lacity.gov/news/lasting-change-annual-homelessness-count-down-two-years-row-first-time-ever-los-angeles).

accounted for 20% of the nation's unsheltered population, recording more unsheltered people than all of the other top 10 localities by homelessness *combined*.

Like some other localities, Los Angeles operates a winter shelter program, temporarily expanding shelter capacity on select dates each year. In each of the six years of our study period (2014-2019), the program temporarily added 1,000-1,500 shelter beds with site locations, opening and closing dates, and number of beds (each determined months in advance) that varied both within and across years. The staggered expansions across dates and locations serve as shocks to shelter availability that allow us to identify the effect of sheltering (or reducing unsheltered homelessness), leveraging shelter supply as an instrument for the number of people sheltered.

To execute our analysis, we construct a novel dataset linking daily counts of shelter beds (and people in shelter) from administrative Homelessness Management Information System (HMIS) records with daily crime incidents from LA Police and Sheriff Departments and daily ER visits from administrative hospitalization data from the California Department of Health Care Access and Information (HCAI). This daily, site-level panel enables precise measurement of treatment timing and location to identify the effects of increasing shelter (and reducing unsheltered homelessness) on crime and ER visits. Our individual-level analysis tracks more than 330,000 entries to homeless services among more than 170,000 unique individuals over the same time period to identify the effect of receiving shelter as opposed to non-shelter services on one's future homelessness. The dataset makes significant strides in overcoming the siloed nature of homelessness data on key outcome variables such as crime and health that has long impeded causal research on the subject.

Our findings indicate that provision of homeless shelter generates large social benefits. First, an additional 100 shelter beds results in nearly 90 additional people in shelter (our first-stage), contradicting theories about widespread resistance to shelter among people experiencing homelessness (Pena, 2023). Second, adding 100 shelter beds prevents an average of 1 crime (concentrated among violent crime incidents during shelter operating hours) and 0.25 ER visits for psychiatric conditions each day. Third, there is minimal evidence of spatial displacement, indicating that the benefits of shelter accrue both locally and regionally rather than concentrating in select locations. Neighborhoods in which sites are opened may actually, if anything, experience even greater *improvements* in local conditions than those further from shelter sites. Finally, we present evidence that entering shelter temporarily reduces future mortality risk, but we find no evidence that shelter reduces future homelessness relative to non-shelter services.

These results have significant policy implications that likely extend beyond Los Angeles. Homelessness affects every level of government and multiple agencies within each level, creating a complex web of costs and benefits. Temporary shelter functions as a high-value public good that generates substantial social benefits despite not "solving" homelessness through permanent exits. Reduced mortality, ER utilization, and exposure to the criminal justice system represent critical humanitarian benefits that current evaluation frameworks often ignore by focusing primarily on homelessness reduction metrics. Thus, our findings imply a classic public goods problem across providers, government agencies, or levels of government with misaligned incentives: homeless services authorities are evaluated on homelessness reduction, while shelter's primary benefits accrue to other sectors through (at least) reduced crime and reduced emergency healthcare utilization. When homeless services agencies cannot internalize these cross-sector and cross-governmental gains, shelter is likely to be systematically underprovided.

2. Literature Review

Government interventions in housing markets represent a significant category of social spending in developed economies. In the U.S., housing programs span direct provision (public housing), demand-side subsidies (e.g., Housing Choice Vouchers), supply-side incentives (e.g., the Low-Income Housing Tax Credit), legal assistance to prevent eviction, and even indirect tax subsides like the mortgage interest deduction. A rich literature has documented the effects of these interventions on recipient outcomes (e.g., Jacob (2004), Jacob and Ludwig (2012)), neighborhood composition (e.g., Diamond and McQuade (2019), Baum-Snow and Marion (2009)), and market equilibrium (e.g., Diamond, McQuade and Qian (2019), Poterba and Sinai (2008), Collinson et al. (2025*b*)).

Yet this literature has largely overlooked homelessness–arguably the most severe manifestation of housing market failure. The limited existing work falls into three categories: causes of homelessness (e.g., Collinson et al. (2024), Quigley and Raphael (2001)), consequences (e.g., Meyer, Wyse and Logani (2023)), and interventions (e.g., Cohen (2024), Phillips and Sullivan (2023)). Rigorous work is limited due to data limitations (O'Flaherty (2019)). 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 studies of homelessness.[5] 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 (e.g., Phillips and Sullivan (2023), Von Wachter et al. (2021), Collinson et al. (2025*a*)). Such studies are important, but they leave a gap in the literature where comparatively little attention has been given to the evaluation of downstream homelessness interventions (i.e., interventions that target people already experiencing homelessness).

Perhaps the most notable exception is the recent work of Cohen (2024), which 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

[5] See, for instance, Kushel and Moore (2023), 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.

in temporary shelter. As a result, these studies identify the effect of interventions relative to the effect of shelter. However, to our knowledge, no study has identified the effects of shelter on the trajectories of people experiencing homelessness (PEH).

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 shed important light on 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 an effect of temporary shelter. In criminology, Faraji, Ridgeway and Wu (2018) also leverage seasonal variation in shelter availability in Vancouver, finding that shelter opening increases monthly property crime close to shelter sites. Our approach is conceptually similar but has several advantages. First, the authors 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 (or roughly 292 homeless per 100,000 residents), compared to over 44,000 in L.A. County that year (roughly 430 homeless per 100,000 residents in 2016),[6] and the authors lack data on individuals experiencing homelessness, preventing an assessment of the impacts of shelter on individual outcomes.[7]

In our setting, we 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 a geography almost 100 times the size of Vancouver. We assess the impacts of shelter on aggregate-level outcomes, including crime, but we also incorporate nonpublic data on ER visits and hospitalizations to assess health impacts. Further, our administrative data allows us to a construct an occupancy measure, which allows us to compute both intent-to-treat and local average treatment effects on the treated. Finally, our individual enrollment-level data, documenting unsheltered homelessness, permits a first-of-its-kind evaluation of the effects of shelter on the trajectories of *individuals* experiencing homelessness. This addresses the critical gap identified by Richards and Kuhn (2023), who note that while unsheltered individuals experience worse health outcomes, we lack causal evidence distinguishing whether these outcomes result directly from lack of shelter versus correlated factors that predict being unsheltered.

In summary, homelessness research 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 the most widely used homelessness interventions.

[6]That year, *each* of L.A. County's 8 Service Planning Areas served at least 3,000 homeless individuals.

[7]Vancouver Homeless Count 2016, https://vancouver.ca/files/cov/homeless-count-2016-report.pdf.

3. Background

3.1. The Homeless Population

The U.S. homeless population has grown substantially since 2017, reversing a decade of gradual decline. From 2007 to 2017, total homelessness fell by 15%, but has since increased by 40% through 2024. The sheltered share of the homeless population has remained at around 65%; however, in California, the sheltered share has only meagerly improved from 32% to 34%. An increasing share–now nearly 30%–of homeless are categorized as chronically homeless. One extensive survey in California found the median length of homelessness to be 22 months (Kushel and Moore (2023)). Exact estimates vary, but the homeless population generally skews younger, more male, and less white than the general population.

3.2. Emergency Shelter

Emergency shelter is a broad category of intervention that may refer to anything from a nightly, congregate setting (e.g., simple cots available at an armory nightly) to more intensive, single-unit settings (e.g., hotel rooms). Importantly, shelter is "interim housing," not designed to be a permanent solution to homelessness, and people in shelters are still counted as homeless. At the same time, shelter is increasingly important as cities search for cost-effective tools to address growing unsheltered populations and encampments. In 2024, there were 422,000 emergency shelter beds nationally, representing an 18% increase from 2023 and the first time that emergency shelter beds have outnumbered permanent supportive housing ("PSH") beds (397,000).

Shelter provision varies considerably across jurisdictions and over time, often resulting in drastic differences in unsheltered shares across homeless populations. Only 3% of New York City's homeless population is unsheltered, but 70% are unsheltered in Los Angeles. The five localities with the largest homeless populations, collectively accounting for one third of the national homeless population, have unsheltered shares of: 3% (NYC), 70% (LA), 9% (Chicago), 58% (Seattle) and 20% (Denver). These variations largely reflect the discretionary decisions of municipal and county leadership. Some officials have prioritized shelter investment. However, this strategy faces substantial criticism, with opponents characterizing the reallocation of funding from permanent housing interventions toward shelter expansion as ultimately a losing game. Such criticism is supported by recent empirical evidence, including Cohen (2024), that more intensive programs are more effective in realizing exits from homelessness.

These findings raise a fundamental question about resource allocation: do proponents of increased shelter funding err in advocating for interventions with lower effectiveness, thereby imposing unjustifiably high opportunity costs? We argue that this conclusion may be premature, as existing research has not addressed two critical considerations that bear directly on optimal policy design. The first concerns relative cost-effectiveness in achieving homelessness reductions. While intensive interventions may prove more effective in generating sustained exits from homelessness, their substantially higher per-unit costs may render shelter more effective per dollar. The second examines cost-effectiveness in reducing the negative externalities of homelessness. Even if alternative interventions demonstrate superior effectiveness in reducing homelessness per se, if unsheltered homelessness imposes significantly higher social costs than sheltered homelessness, expanded shelter investment may be justified from a social welfare perspective.

3.3. Shelter in Los Angeles

To empirically examine these questions, we focus on Los Angeles County, which provides an ideal natural laboratory to conduct our research. Los Angeles plays an outsized role in homelessness nationally. Los Angeles alone accounts for 10% of the nation's homeless population, and 20% of the unsheltered population–more than all of the other top 10 localities by homelessness combined. Moreover, LA's high unsheltered rate allows for substantial variation in shelter access that enables identification of causal effects. In 2014, the first year of our study, 38,809 people in Los Angeles were experiencing homelessness with approximately 24,977 (66%) remaining unsheltered.[8] By 2019, the homeless population had risen to 58,936, with 44,214 (75%) unsheltered.

Los Angeles County also presents a compelling case for analyzing shelter effectiveness given its size and diversity of jurisdictions. Because the county is so large, the Los Angeles Homeless Services Authority (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.[9] We follow this structure in our analysis and show the geographical division of SPAs in Figure 1 below.[10]

Each SPA is comparable in population to major U.S. cities. For example, SPA 1 covers the Antelope Valley, a relatively sparsely populated high-desert region, with a popular similar to New Orleans (397,432 residents). SPA 2 covers the populous San Fernando Valley with a population of 2.26 million, similar to Houston. SPA 8, in the southernmost part of the county, covers a population of 1.32 million, similar to nearby San Diego. In effect, our analysis spans the equivalent of eight major American cities, each with substantial homeless populations ranging from over 2,500 to more than 15,000 individuals on any night over the 6 years of our sample.

3.4. LA's Winter Shelter Program

Our identification strategy leverages LA's seasonal winter shelters program, which creates natural variation in shelter availability. Each year, LAHSA operates a countywide program that temporarily expands shelter supply by more than 1,000 beds annually, serving several thousand unique individuals over roughly four-month periods.[11] Importantly, seasonal shelters and opening

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

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

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

[11] 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.

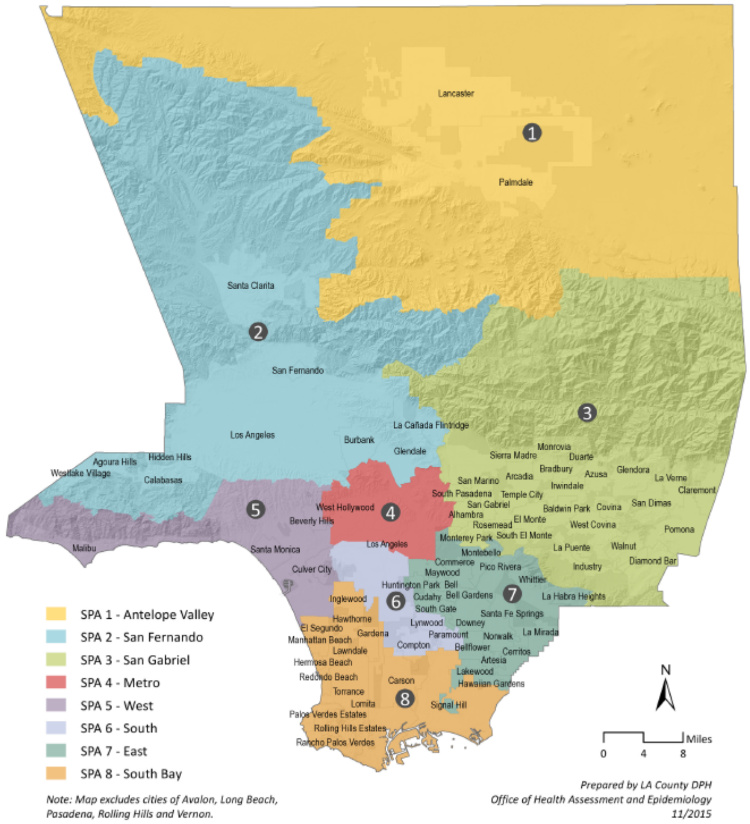

Figure 1: Los Angeles County's Eight Service Planning Areas (SPAs)

dates are planned months in advance of the winter season, meaning the number of beds, locations, and opening dates are not determined endogenously in response to daily conditions.[12]

These winter shelters—typically operating nightly from 5 P.M. to 7 A.M.—create sudden changes in shelter availability, with locations, bed counts, and opening/closing dates varying across years and locations within years. The resulting shocks to shelter capacity provide the variation necessary for causal identification of shelter effects on both individual and area-level outcomes.

In summary, our design leverages variation on several margins. First, shelter open and close dates vary *across* years. So, for example, we can compare changes in outcomes from October to November in years where shelters open in November against changes in outcomes from October to November in years where shelters are not (yet) open in November.[13] Second, shelter open and close dates also vary across locations *within* years. So, for example, *within* 2015, we can compare late October (versus earlier October) outcomes in SPA 7 (where sites open October 15th) against the same trend in SPA 6 (where sites open November 1st). Third, the number of beds provided also varies across locations and years, allowing us to compare places and times where shocks to shelter

[12]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. Our results are robust to restricting to the months of September-December, only leveraging variation in opening dates.

[13]In practice, we do this at the daily, not monthly, level.

supply are larger (more beds) to those where they are smaller (fewer beds). Online Appendix Figure I.1 illustrates these sources of variation for three of the winters in our sample.

4. Region-level Analysis

4.1. Data

We begin by aggregating administrative data on homelessness from Los Angeles's Homeless Management Information System, public crime incident data from the L.A. Police and Sheriff's Departments (LAPD and LASD), and non-public administrative data on hospital utilization from the California Department of Health Care Access and Information (HCAI).

4.1.1. 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[14] may opt in to using HMIS. As noted in the previous section, LAHSA collects data and organizes services at the SPA-level to accommodate the county's enormous size.

Through an agreement with the California Policy Lab (UCLA) and the Homelessness Policy Research Institute (USC), LAHSA makes de-identified HMIS records available to researchers. The HMIS records include the date of entry into a project, the type of project (including emergency shelter), the ZIP code of the service provider for each project,[15] the last known number of beds available at the project, an indicator for whether a shelter project is "seasonal," and exit date from the project. Thus, we can construct site-by-day counts of shelter beds and occupancy, including variables for the location (SPA) of the shelter and whether it is a "seasonal" shelter.[16]

While LAHSA makes annual announcements regarding the opening and closing dates of winter shelters, these reports are imperfect,[17] 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

[14] Most service providers receive some federal funding, but it is possible, for example, that a church operates its own nightly shelter using its building and relying on private donations.

[15] 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, our analysis 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).

[16] 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.

[17] 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).

is vacant.[18] 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.[19] Because HMIS data are notoriously challenging to work with (one of many factors that has been an impediment to research on homelessness), we document our full data cleaning approach in Online Appendix II.

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.[20] Figure 2 presents daily counts of shelter beds. Light blue indicates counts of seasonal shelter beds. Vertical black lines denote January 1 of each year. The spikes in bed and person counts generally closely correspond to the exact opening and closing dates gathered from public announcements of the program's operation.[21]

Next, because our empirical approach exploits geographic variation, we show figures with counts for each of the county's 8 Service Planning Areas. As Figures 2 and 3 show, shocks to shelter vary widely across time and space. While the total number of winter shelter beds is fairly stable across years, relative to the homeless population (and existing shelter stock), expansions are larger in earlier periods. Expansions are also more consistent in some locations (e.g., SPA 5) than others (e.g., SPA 6). Conceptually, our analysis treats every SPA-year as a separate "unit" with shocks to shelter supply varying across units and time (opening dates within each SPA-year). Restricting to opening dates (between October and December), collectively, these 48 units experience 66 "treatments" (unique openings) as several winter shelter sites may open on different dates within a SPA-year (e.g., in 2015, in SPA 2, 170 beds open on November 3rd, 60 more open on November 9th, and 100 more open on November 16th). On average, an opening adds just over 100 beds (range 9–505) or, relative to population, 23 beds per thousand people experiencing homelessness (range < 1–136).[22]

[18]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 II. Varying these assumptions does not meaningfully affect our findings.

[19]See discussion of poor exit recording and "purge dates" in Meyer, Wyse and Corinth (2023). Our results are robust to other occupancy rate caps (see Online Appendix Table I.1).

[20]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.

[21]See discussion of Online Appendix Figure I.1 and, for example, Los Angeles Homeless Services Authority, 2015-16 Winter Shelters Program, `https://file.lacounty.gov/SDSInter/dmh/236341_WinterShelterLACounty2015-2016.pdf`. While our records of seasonal (winter) shelters are comprehensive, our counts do not include most "scattered-site" shelter offered through vouchers (e.g., a motel room voucher may be a bed in the HUD count), sites designated for victims of domestic violence, and sites that receive no federal funding. The differences between our counts and those of HUD can overwhelmingly be explained by HUD's inclusion of scattered-site shelter vouchers. Thus, our count of *non-seasonal* beds may be thought of as understated. While our analysis includes controls for daily counts of observed non-seasonal beds, our results are robust to their exclusion (Tables A.9-A.10).

[22]At the midpoint of our sample, LA County had 320 beds per thousand people experiencing homelessness. For clarity, we define treatment as the number of winter shelter beds, but as discussed in Section 4.4, results are similar if we instead use "shelter coverage" (beds per homeless person) or a binary indicator for any winter shelter beds.

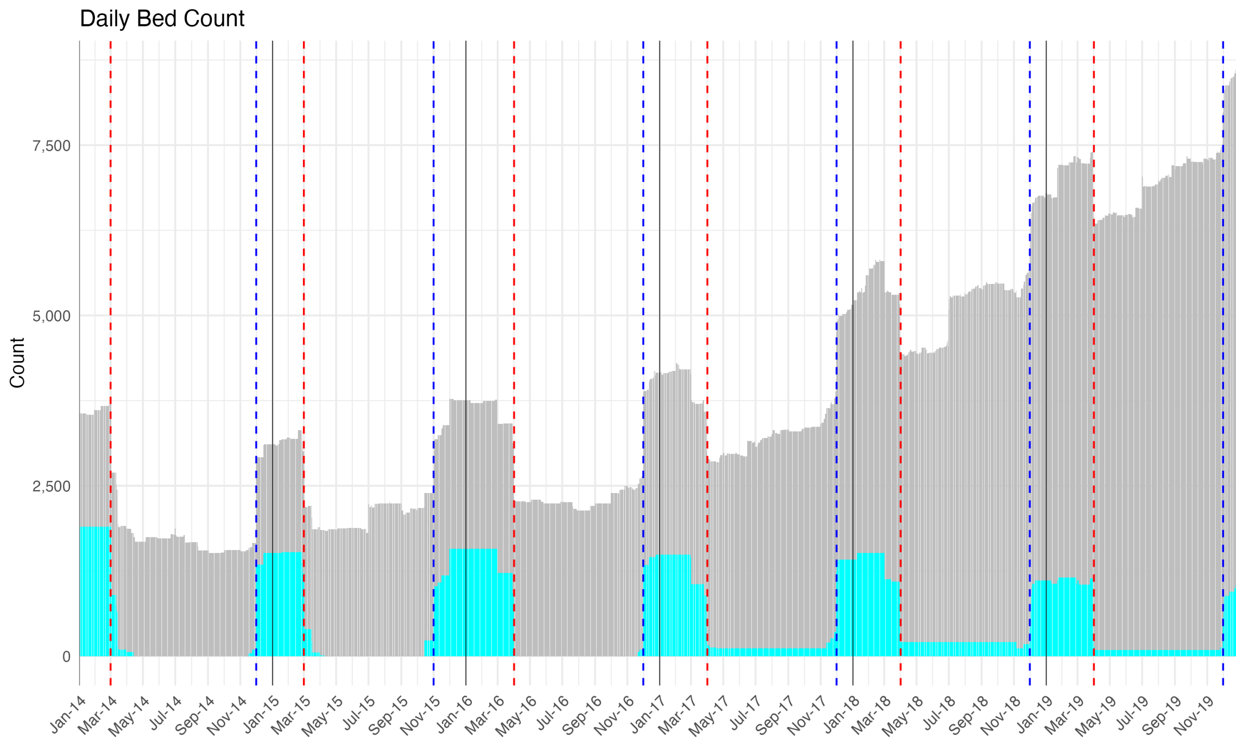

Figure 2: Daily Bed Count by Winter Shelter (Blue) vs. Other Shelter (Grey)

4.1.2. LAPD and LASD

The first outcome we consider is crime, a major externality closely tied to property values (Pope and Pope (2012)). Both the Los Angeles Police Department (LAPD) and the Los Angeles Sheriff's Department (LASD) make incident-level crime records publicly available. While the variables differ somewhat across agencies, both provide, for each known incident, the date and time, block-level coordinates, and crime category. We map coordinates to ZIP codes, crosswalk to Service Planning Areas (SPAs), then merge records to generate daily crime counts by SPA. Figure 4 plots these counts for the full county (Online Appendix Figure I.4 breaks the counts out by SPA).

LAPD and LASD together cover most, but not all, of Los Angeles County (many cities contract with LASD rather than maintain their own police). Coverage is nearly complete for SPAs 1, 2, 4, 5, and 6, but our measure of crime is understated in SPAs 3, 7, and 8. Therefore, in our analysis, we report results both for the full sample and the sample excluding these SPAs where coverage is poor. On average, each SPA records 126 (154) crimes per day in the full (restricted) sample, with just over half occurring at night (5 P.M.–7 A.M.).

4.1.3. 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). These records cover all inpatient hospitalizations and ER

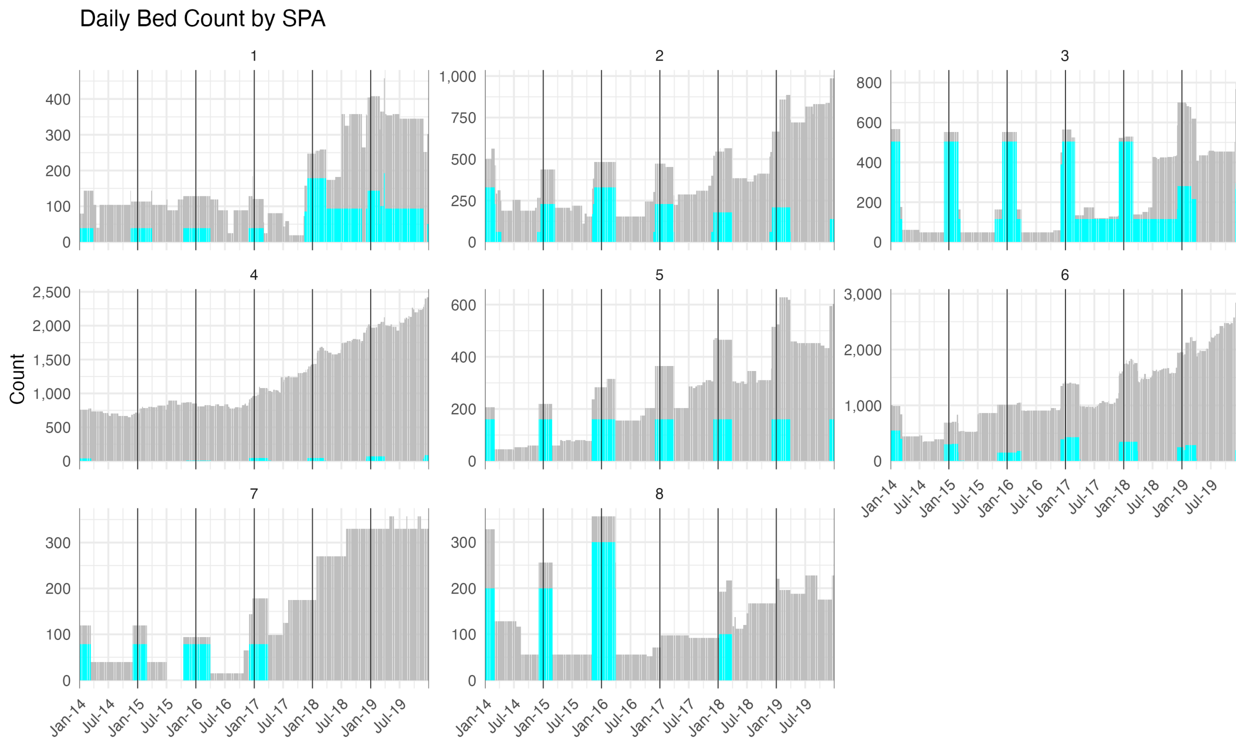

Figure 3: Daily Bed Count by SPA and Winter Shelter (Blue) vs. Other Shelter (Grey)

visits at California-licensed hospitals, including the date, facility, and up to 25 physician diagnostic codes.

Because people experiencing homelessness are overwhelmingly over-represented among psychiatric patients, we restrict to ER visits for psychiatric conditions. We map facilities to SPAs based on facility ZIP code[23] and generate SPA-by-day counts of psych ER visits. Daily counts of psychiatric ER visits are shown in Figure 4; counts under 15 are censored for confidentiality.

[23]Rarely, a facility's SPA differs from most of its ZIP code; in such cases we assign by ZIP code.

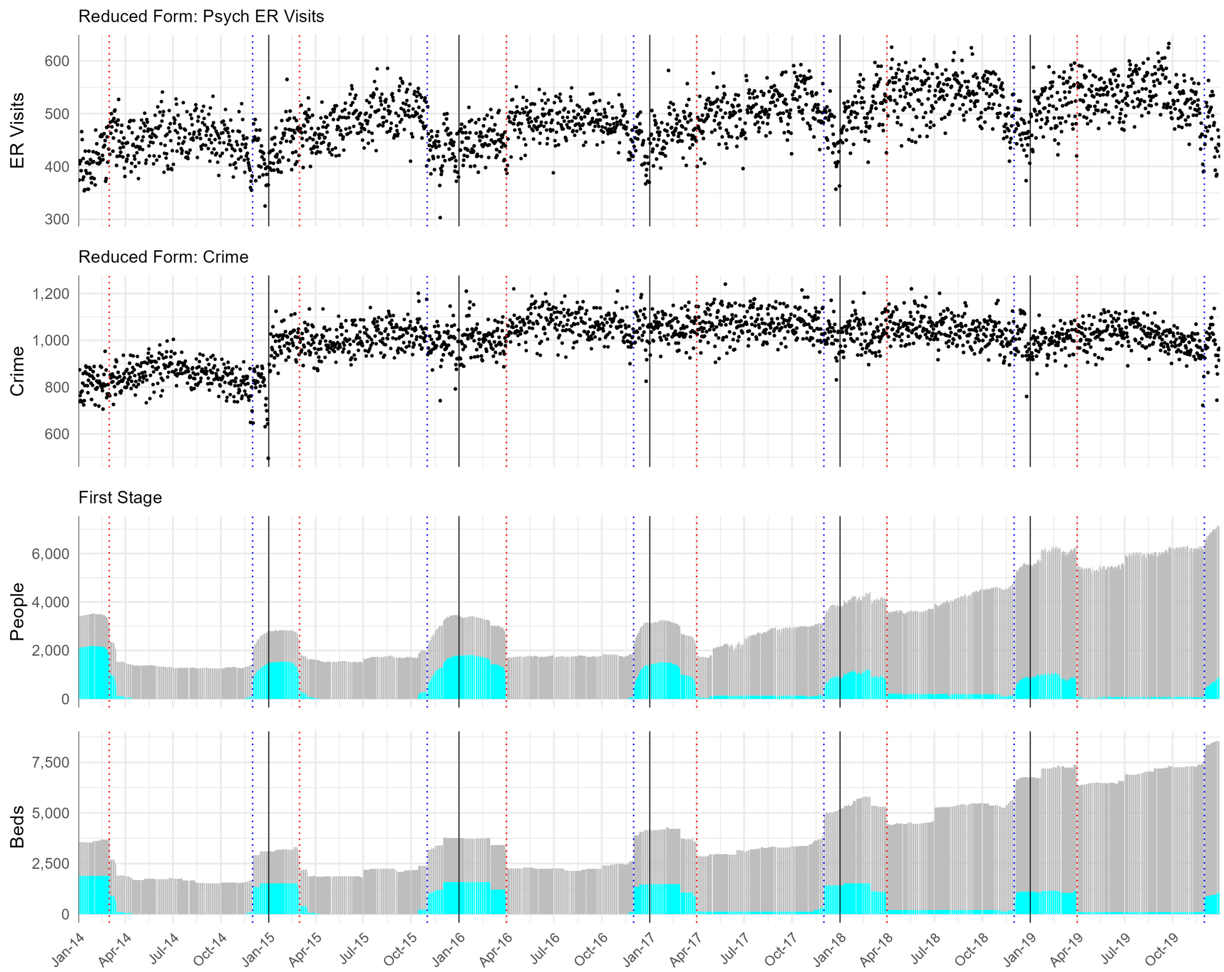

Figure 4: Daily psych ER visits, crime, people in shelter, and shelter beds. The chart excludes the first of the month, when backlogged crime reporting often occurs, for scale.

After merging sources into our panel, Figure 4 shows daily values for key variables aggregated across SPAs.[24]

4.2. Empirical Approach

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

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

where Y_{symd} represents some outcome Y in year y, month m, day d, and SPA s, and

[24]For a clearer example (and preview of our results), Appendix Figure A.1 displays the counts over just the 2015-2016 season, showing noticeable drops in psych ER visits and crime when shelter opens that rebound after sites close.

$people\ sheltered_{symd}$ 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. 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. In the first-stage regression, "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.

In essence, both our reduced-form and first-stage equations carry causal interpretations under traditional identifying assumptions of staggered difference-in-differences. Because specific dates and locations of operation are determined months in advance, within any unit (SPA-year), it is unlikely that any unobserved factor could explain both the (day-over-day) change in shelter availability and the (day-over-day) change in outcomes. While Figure A.1 does not show any obvious seasonal trends, we still account for potential seasonality in outcomes with month and day fixed effects, and in Section 4.4 we provide event studies as a more formal test of the parallel trends assumption.

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

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

The reduced-form estimates are intent-to-treat effects, which might actually be of greater interest to policymakers who might find it more feasible to provide shelter than increase the number of people sheltered by some other means, but there is value in estimating both effects. Because take-up of treatment is nearly 1, IV estimates are very close to the reduced-form, ITT estimate.

SPA-by-year fixed effects account for any underlying differences across locations and years, including homeless population (which is, at best, measured at a SPA-by-year level when annual point-in-time counts are conducted). Month fixed effects account for potential seasonal trends, and day-of-month fixed effects account for potential bunching in reporting (and may capture more granular seasonal trends in robustness tests that restrict to dates between September and December) and possible income shocks associated with certain days of the month (such as receipt of benefits). *other beds* is a time-varying control that accounts for the level of other shelter available at any date and location. Because the availability of these beds is likely endogenously determined,[25] the coefficient on this term does not carry the same causal interpretation as the coefficient on $WS\ beds$.

If properly estimated, β_2 represents the average effect of an additional shelter bed through

[25] If shelters are opened in times and places in response to worsening conditions, the relationship between crime and shelter would appear more positive due to reverse causality, biasing against finding a reduction in crime. Similarly, if crime is lower in the winter or psychiatric ER visits are higher in the winter (e.g., perhaps due to seasonal depression), then naive regressions that fail to account for seasonal trends may be biased towards finding a decrease (increase) in crime (psych ER visits).

the winter shelter program on the outcome variable Y. This specification assumes that there is an immediate effect, though if effects occur with a lag, this could attenuate our estimate. While we cannot rule out the possibility that effects become stronger or weaker over time, event studies and robustness tests restricting to very narrow windows around treatment presented in Section 4.4 indicate strong immediate impacts of shelter that do not appear to diminish substantially over time. A notable weakness of our setting and avenue for future work is that the exogenous shocks to shelter supply are highly temporary in nature (generally lasting no longer than 4-5 months), precluding credible inference about the long-term stability of shelter's effects. However, if short-term shelter interventions provide meaningful benefits, it may be reasonable to expect that more permanent shelter expansions would achieve at least similar results.

4.3. Results

Regression results 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).

4.3.1. 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. As noted above, SPAs 3, 7, and 8 include several large cities for which we lack crime incident data. Therefore, in panel B of Table 1, we reproduce estimates after dropping these 3 SPAs.

Throughout, we find a strong first-stage effect for winter shelter beds, carrying an F-statistic multiple orders of magnitude larger than required by conventional tests of instrument relevance. The estimate implies that every 100 additional shelter beds provided (on a given day, in a given SPA), increases the number of people sheltered by 89. 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 generalizations that they *prefer* to sleep outside. When shelter beds are added, around 90% of them are filled. Importantly, in our individual-level analysis to come, we detect no effect of shelter on new homelessness (Section 6.4.3). Therefore, an increase in sheltered homelessness may be interpreted as equivalent to a decrease in unsheltered homelessness throughout.

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, during this period, LAHSA's winter shelter program operates around 1,500 beds per day for roughly 4 months per year. So, 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 include the time of the incident, we can split our measure into incidents that occur outside of the standard hours of winter shelter operation (i.e., from 7 A.M. to

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)
other beds	0.7583^{***}	-0.0028	0.0032	-0.0035^{**}	-0.0020	0.0008	0.0051^{***}
	(0.0083)	(0.0026)	(0.0029)	(0.0017)	(0.0019)	(0.0017)	(0.0019)
WS beds	0.8911^{***}	-0.0070^{***}		-0.0018		-0.0051^{***}	
	(0.0084)	(0.0018)		(0.0012)		(0.0012)	
sheltered			-0.0078^{***}		-0.0021		-0.0058^{***}
			(0.0020)		(0.0014)		(0.0013)
Outcome Mean	429	126	126	59	59	67	67
Adj. R^2	0.9863	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
Panel B							
other beds	0.7017^{***}	-0.0039	0.0043	-0.0032^{*}	-0.0011	-0.0006	0.0054^{**}
	(0.0086)	(0.0029)	(0.0038)	(0.0019)	(0.0026)	(0.0019)	(0.0025)
WS beds	0.8274^{***}	-0.0096^{***}		-0.0026		-0.0071^{***}	
	(0.0142)	(0.0031)		(0.0022)		(0.0020)	
sheltered			-0.0117^{***}		-0.0031		-0.0085^{***}
			(0.0038)		(0.0027)		(0.0024)
Outcome Mean	528	154	154	73	73	80	80
Adj. R^2	0.9867	0.9430	0.9429	0.8841	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). Panel A includes all 8 SPAs. Panel B drops SPAs 3, 7, and 8 which have poor crime data coverage.

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 reductions in crime that occurs a night - i.e., when the shelters are operating.[26]

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 people experiencing homelessness (see, for instance, Padwa et al. (2024)) suggest that reduced crime rates may also be indicative of reduced *victimization*. In Appendix table A.1 we see that reductions in crime seem to primarily be driven by reductions in violent crime, but we cannot determine whether reductions are driven by reduced offending or reduced victimization.

[26] *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). It may also be the case that crime-reducing interventions are more or less effective at night (Grogger and Ridgeway (2006)).

4.3.2. 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.[27] While the shelter we study does not offer a substitute for direct mental health treatment (e.g., onsite psychiatry), previous work has documented the extent to which unsheltered homelessness (e.g., sleeping on a sidewalk or in a tent) may exacerbate underlying mental health conditions. If shelter protects individuals from the conditions that would lead to a mental health incident severe enough to result in emergency care, it could serve as an effective public health intervention. Alternatively, while little evidence exists to support the hypothesis, we cannot rule out the possibility that people experiencing homelessness appear at the ER, presenting with psychiatric symptoms when they otherwise lack somewhere to sleep, using emergency rooms as a substitute for shelter when it is unavailable. Regardless of the mechanism at work, a reduction in ER visits has important implications for healthcare costs, wait times, and welfare of people experiencing homelessness.

	FS	RF	IV
other beds	0.7583^{***}	0.0041^{***}	0.0062^{***}
	(0.0083)	(0.0011)	(0.0013)
WS beds	0.8911^{***}	-0.0024^{**}	
	(0.0084)	(0.0009)	
sheltered			-0.0027^{**}
			(0.0011)
Outcome Mean	391	61	61
Adj. R^2	0.9863	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

Reduced-form results 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 seasonal shelter providers prevent over 300 ER visits for psychiatric conditions alone each year. As additional robustness, we also show in Appendix Table A.2 a decrease in ER visits due to injury and poisoning, which are frequently referred to as the second most common reason for ER visits among homeless populations (Lin et al. (2015)).[28] However, these effects are not statistically significant, which may be expected given that these ER visits are nearly four times as common as psychiatric ER visits for the general population and are likely driven by a number of factors beyond homelessness.

[27] 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.

[28] We define a ER visit for injury or poisoning to be any ER visit where the principle diagnosis has an ICD-10 code starting with "S" or "T."

4.4. Robustness and Empirical Validation

One concern may be that our estimated effects arise due to pre-existing trends in outcomes or failure of the parallel trends assumption. To probe the credibility of our key assumptions, we next estimate event studies for our first-stage and reduced-form outcomes and present the results in Appendix Figures A.2-A.4. Because a SPA may have multiple treatment intensities within a given season (e.g., if 50 beds open on October 15th and 100 more open up on November 7th), we introduce some noise when restricting to a binary indicator for first treatment date (to define event time 0).[29] Pre-treatment point estimates in Figures A.2-A.4 are generally indistinguishable from 0 and do not suggest a downward trend that would bias our results.

Opening dates of winter shelters are determined months in advance and, with the exception of administrative delays on a couple of occasions, overwhelmingly open as initially planned. On the other hand, particularly in 2017 and 2018, closing dates were extended for most shelters (by about 1 month), citing persisting poor weather conditions and high demand for beds. Thus, closing dates may be considered endogenous in these situations. To account for this possibility, we reproduce our results after restricting our sample to the months of September-December, exploiting only variation in opening dates. These results, presented in Appendix Tables A.3-A.4, are highly robust and, if anything, stronger than estimates from the full sample.

Throughout our analysis, we report heteroskedasticity-robust standard errors. Results remain largely unchanged when clustering at the SPA-year level, as shown in Appendix Table A.5.[30]

Our design makes use of staggered timing of treatment, but due to the complexity of our setting (continuous treatment, within-year variation in treatment intensity, *and* few "never-treated" SPA-year units), there is no straightforward way for us to directly apply the methods outlined in recent innovative work on difference-in-differences (e.g., Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2023)). As recent work highlights, the primary concern in a setting like ours is that staggered treatment timing implicitly weights treatment effect estimates of units that experience treatment earlier (longer) differently than those that experience treatment later. We address this concern by reproducing our results after restricting very narrowly to the months of October and November only. This limits the variation in duration of treatment among units that experience treatment and allows all units treated in December to serve as a "never-treated"

[29]Table A.8 shows that results are robust to the use of a binary open/closed indicator as opposed to making use of the full variation in *number* of winter shelter beds as in choice specifications.

[30]Following Abadie et al. (2023), clustering adjustments are recommended when either (i) the sampling process is clustered or (ii) the assignment mechanism is clustered. In our setting, we observe all SPAs in Los Angeles County, and while there is some correlation of winter shelter opening timing at the SPA-year level, opening dates vary from shelter to shelter, even within a SPA in a year. As such, Abadie et al. (2023)'s proposed CVV estimator does not clearly fit our setting due to the difficulty of discerning q_k and p_k, both of which could be argued to be 1 in our setting. Under this design-based perspective, cluster adjustments may be conservative. Moreover, our setting produces a small number of SPA-year clusters: 30 (when dropping SPAs 3, 7 and 8) or 48 (when including all eight SPAs for all six years). Cluster-robust standard errors have been shown to over-reject with few clusters, generally defined as 30 or fewer (Cameron, Gelbach and Miller (2008)). To remedy this, we also calculate errors via wild cluster bootstrap, which generally performs better with few clusters, following Roodman et al. (2019). While we argue that clustering is not appropriate in our setting, our results, particularly when restricting to months around shelter openings, generally remain significant at conventional levels even with these more conservative standard errors.

counterfactual in this more narrow natural experiment. Results are presented in Appendix Tables A.6-A.7 and are consistent with estimates from the broader sample. While these results cannot speak directly to the evolution of treatment effects over time, they provide compelling evidence that our findings would be similar net of any influence of staggered treatment timing.

Row 1 of Table A.8 shows that results are robust to the use of a binary open/closed indicator as opposed to making use of the full variation in *number* of winter shelter beds as in choice specifications. Row 2 shows that results would be similar if we were to ignore geographic variation altogether (exploiting only variation in treatment timing and intensity across years) by reproducing estimates after aggregating all variables from the SPA-level to the county-level.[31] Row 3 presents results when independent variables for beds are scaled by the SPA's estimated homeless population. Homeless population estimates are only available annually. To avoid introducing mechanical changes in these variables when counts are updated, we again restrict our sample to September-December dates only.[32] Finally, estimates in Appendix Tables A.9-A.10 show that results are not sensitive to the exclusion of the control for other (non-winter shelter) beds.

5. Within-Region Analysis

Section 4 demonstrates that the provision of additional shelter beds reduces both crime and psychiatric ER utilization at the SPA level. One concern may be that this region-wide decrease is inequitably distributed: i.e., the expansion of winter shelters concentrates homeless individuals in certain areas that then experience increased crime and ER utilization. The question this Section addresses is the extent to which this form of "shifting" within the SPA happens.

5.1. Data and Empirical Approach

We use the same data as in Section 4, but aggregated to either (1) the zip-code level, for crime regressions or (2) the hospital-level, for ER visit regressions. During our analysis period, there are roughly 300 populated zip codes and 80 operational hospital facilities in Los Angeles County, shown in the map in Appendix B.1. As a robustness check, we also examine crime effects at a block-level, for an additional level of geographic granularity.

As in Section 4, we estimate the effect of shelter, leveraging the variation in timing and location of shelter beds. Because Section 4 reported first stage results close to 1, for simplicity, we report only reduced-form regressions here. We also simplify to using indicator variables to indicate the presence of winter shelter beds, rather than the number of winter shelter beds.[33] We examine effects at the zip code level for crime outcomes and hospital-level for psychiatric ER visits. For both

[31] While our homeless services records suggest that moving across SPAs for services is rare and while such movement would attenuate our estimates towards 0, this approach would not be influenced by movement across SPAs.

[32] Because homeless counts are conducted in January, we scale our measures by the counts corresponding to the upcoming January as this is likely more representative of true counts in the preceding months (e.g., bed counts in September-December of 2015 are scaled by the homeless population counted in January 2016, not January 2015).

[33] This also aids in interpretation as we are primarily interested in the effects of the presence of a homeless shelter as opposed to a reduction in unsheltered homelessness.

specifications, we decompose the effect of shelter expansion into two components: a *regional effect* that captures the SPA-wide impact of additional beds, and a *local exposure effect* that captures the differential impact on areas that host or are exposed to these beds.

5.2. Crime

As in other work investigating localized crime effects (e.g., Duggan, Hjalmarsson and Jacob (2011), Rosenberg (2025)), we measure crime at the zip-code level. Our baseline specification takes the following form for zip code z located in SPA s on date t (where $t = ymd$):

$$Crime_{zst} = \psi_{sy} + \mu_m + \delta_d + \alpha_z + \beta_1 D_{st} + \beta_2 D_{zt} + \gamma_1 \text{other beds}_{st} + \epsilon_{zst}$$

where $D_{st} = \mathbf{1}\{\text{WS beds}_{st} > 0\}$ and $D_{zt} = \mathbf{1}\{\text{WS beds}_{zt} > 0\}$. The coefficient β_1 therefore captures the regional sheltering effect—how crime in a typical zip code responds to the addition of (winter) shelter beds in the SPA. The coefficient β_2 identifies the local exposure effect—whether zip codes that host winter shelters experience differential changes in crime. Crime may also respond to winter-shelter beds located *near* (but not inside) a zip code. To account for this possibility, we also run specifications that substitute D_{zt} for $D_{zt}^{\leq 3\text{mi}}$, defined as $\mathbf{1}\{\text{WS beds in any zip within 3 miles of } z\}$ and $D_{zt}^{\leq 5\text{mi}}$ equivalently for 5 miles.[34]

Table 3 presents the effect of the presence of winter shelter beds on zip-code level crime. As column 1 shows, we see a modest regional sheltering effect that reduces crime and no evidence of increased crime near shelter sites through a local exposure effect. That is, on average, each zip code should expect to see -0.029 fewer crimes per day when winter shelters are open, or roughly 3.5 fewer crimes throughout the season. Zip codes that are "hosts" to winter shelters should see an additional -0.092 fewer crimes per day when that zip's winter shelter is open, or 11 fewer crimes throughout the season, but this effect is not statistically significant. Column 2 presents the same results without zip-level clustering, showing more precise estimates.[35] As column 3 shows, the zip code of interest also benefits from the opening of winter shelters in zip codes within 3 miles. As column 4 shows, this remains true for zip codes within 5 miles, though the magnitude of the effect is, as expected, somewhat diminished.[36] In other words, regardless of whether "local" is defined as within-ZIP, within 3 miles, or within 5 miles, estimates of the effects of local exposure to expanded shelter are consistently negative, rejecting the hypothesis that shelters concentrate crime.

One concern with our zip code level analysis may be that it is at an insufficiently granular

[34] We define $\leq x$ mi as the zip code with a centroid $\leq x$ miles from the centroid of the zip code of interest z.

[35] We cluster standard errors at the zip level because that treatment is highly serially correlated in a zip code, following Abadie et al. (2023). However, unlike typical geographic clustering units–such as cities or states–zip codes may not be meaningful demarcations of treatment due to their relatively arbitrary nature policy-wise. Zip clustering may also be overly conservative because we observe a large portion of the zip codes in our universe due to the use of administrative data (rather than random sampling). Therefore we present standard errors of both types.

[36] This estimate is about the same magnitude as the main D_{zt} specification for a few reasons. First, each zip code on average has approximately 6 zip codes within 3 miles (and 16 within 5 miles). Hence, the probability of one neighbor having winter shelter beds is mechanically higher. Second, the effect likely decays across distance, countervailing against the higher probability of the variable taking on a value of 1.

	Crimes per Day			
	(1)	(2)	(3)	(4)
D_{st}	-0.029*	-0.029***	-0.018	-0.015
	(0.017)	(0.010)	(0.016)	(0.016)
D_{zt}	-0.092	-0.092**		
	(0.095)	(0.036)		
$D_{zt}^{\leq 3\text{mi}}$			-0.097**	
			(0.044)	
$D_{zt}^{\leq 5\text{mi}}$				-0.063**
				(0.029)
s.e. cluster	zip	hetero.	zip	zip
Outcome Mean	3.32	3.32	3.32	3.32
Adj. R^2	0.76720	0.76720	0.76721	0.76720
N	659,491	659,491	659,491	659,491

Table 3: The Effect of Winter Shelters on Zip-Level Crime

geographic scope to detect nuanced spatial changes in crime. For example, crime close to a winter shelter may increase, but this increase may be smaller than the offsetting decrease in crime in the rest of the zip code. To address this concern, we conduct a simple event study examining crime within 500 meters of a winter shelter within a month of its opening. As Appendix Figure B.2 shows, there is no statistically significant change in crime in this hyper-local region.

5.3. ER Visits

For psychiatric ER visits, we employ a parallel specification at the hospital h level:

$$ERVisits_{hst} = \psi_{sy} + \mu_m + \delta_d + \alpha_h + \beta_1 D_{st} + \beta_2 D_{ht} + \gamma_1 \text{other beds}_{st} + \epsilon_{hst}$$

where $D_{ht} = \mathbf{1}\{\text{WS beds in a zip } z \text{ for which } h \text{ is one of the top 3 receiving hospitals}\}$. We calculate annual average patient flows using Medicaid ER patient records from May-August (outside of seasonal shelter operation) to assign hospitals to zip codes, a similar methodology to that used to create Hospital Service Areas in the Dartmouth Atlas (Wennberg et al. (1998)).

Table 4 presents the effect of the presence of winter shelter beds on hospital-level psychiatric ER visits. As with crime, we continue to see a regional sheltering effect, where hospital-level ER visits are lower at a SPA-level when and where winter shelter beds are open. Unlike with crime, we do see a countervailing small increase in ER visits at hospitals that typically “cover” the zip codes where winter shelters are opening. However, as this effect is statistically insignificant and less than half the magnitude as the region-level effect, hospitals that most “cover” winter shelter zip codes still see an overall reduction in psychiatric ER visits.

This distinction from crime makes sense: the transition from unsheltered to sheltered appears to matter significantly for crime, whereas for psychiatric ER visits, being homeless–in shelter or otherwise–might matter just as much or more. When winter shelters open, they have two effects: (1) they change the sheltered/unsheltered composition in an area, and (2) they increase the number of sheltered people in an area. This second effect matters for ER use, but even for hospitals that generally receive low-income patients from zip codes with winter shelters, seasonal bed expansions continue, on net, to decrease psychiatric ER visits through reductions in unsheltered homelessness.

	Psychiatric ER Visits
D_{st}	-0.180**
	(0.087)
D_{ht}	0.070
	(0.125)
s.e. cluster	hospital
Outcome Mean	6.53
Adj. R^2	0.72283
N	164,615

Table 4: The Effect of Winter Shelters on Hospital-Level ER Visits

6. Future Homelessness and Mortality

Finally, leveraging the longitudinal nature of our homeless services records, we estimate the effect of shelter on future homelessness and observed mortality among those entering homeless services. We follow more than 170,000 unique individuals appearing for homeless services between 2014 and 2019 and compare the outcomes of those who receive shelter against those who receive only non-shelter (street) services, using shocks to shelter supply as an instrument for entry to shelter.

6.1. Data

HMIS data contains records of every program "enrollment," which may be thought of as a new appearance for (or entry to) services.[37] It records the type of services provided, the date and location (SPA), and exit date (if applicable) and destination (if known). Table 5 shows enrollments by type of service provided for our sample period. While great attention is given to programs like permanent supportive housing, in reality, non-shelter services and emergency shelter account for more than 70% of all HMIS enrollments.

[37] A new enrollment record is created every time a person begins receiving a homeless service from a new provider (most accurately, a new "project") or a new homeless service from the same provider. An enrollment may last days, months, or years, and clients may receive services multiple times throughout the duration. The average individual in our sample has 2.3 enrollments during the sample period of January 2014 through December 2019 (the median has 1, 75th percentile has 3, and 90th percentile has 5).

Project Type	Enrollments	Share (%)
Non-shelter services		
Street Outreach (SO)	160,917	30.56
Services Only (SSO)	58,816	11.17
Temporary shelter		
Emergency Shelter (ES)	153,557	29.16
All others		
Rapid Re-Housing	79,140	15.03
Prevention	19,240	3.65
Transitional Housing	19,842	3.77
Permanent Supportive Housing	16,879	3.21
Others	18,230	3.46

Table 5: HMIS enrollments by project type (LA County, 2014-2019).

We restrict our sample to enrollments in emergency shelter (ES), street outreach (SO), or services only (SSO) between 2014 and 2019.[38] We also restrict to clients aged 16-99[39] and drop a small number of enrollments where location (SPA) is missing. For this sample, we construct variables for whether a person reappears for homeless services 6-18 months later[40] and whether a person is known to have died as of 18 months later.[41] Descriptive statistics are presented in Table 6. Consistent with the broader demographics of the homeless population, our sample is relatively young, majority male, and majority non-white.

Independent Variables	Full Sample	ES	SO	SSO
Age	43.47	43.17	43.62	43.79
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
ES	41.12	100.00	0.00	0.00
Reappear	40.23	43.50	38.48	36.59
Mortality	0.48	0.41	0.55	0.49

Table 6: Descriptive statistics for individual-level analysis. Except sample size, all values reported are sample means. Dependent variables are percentages.

Conceptually, if shelter reduces homelessness, then a sudden increase in the share of people

[38] A services project is defined as a project that "does not provide lodging and meets specific needs of people experiencing or at-risk of experiencing homelessness." Results are robust to dropping services only enrollments.

[39] Results are not sensitive to different age restrictions or controlling for age non-parametrically.

[40] This includes any new enrollments of any kind or any record indicating they are continuing to receive services under an existing enrollment. Like Cohen (2024) and Schachner, Schmidt and Painter (2025), we use reappearance for homeless services instead of exit from homelessness due to frequent missingness in HMIS exit variables. However, our conclusions are similar if we instead use an indicator for any observed exit from homelessness to housing.

[41] Results under different time windows are provided in Online Appendix I.3.

receiving shelter (versus non-shelter services) should be accompanied by a sudden decrease in the share of those people who are homeless 6-18 months later. Figure 5 plots share of enrollments for shelter against share of enrollments with returns to homeless services. As we show more formally in the section that follows, we observe no evidence of a reduction in future homelessness when temporary shelter is expanded.

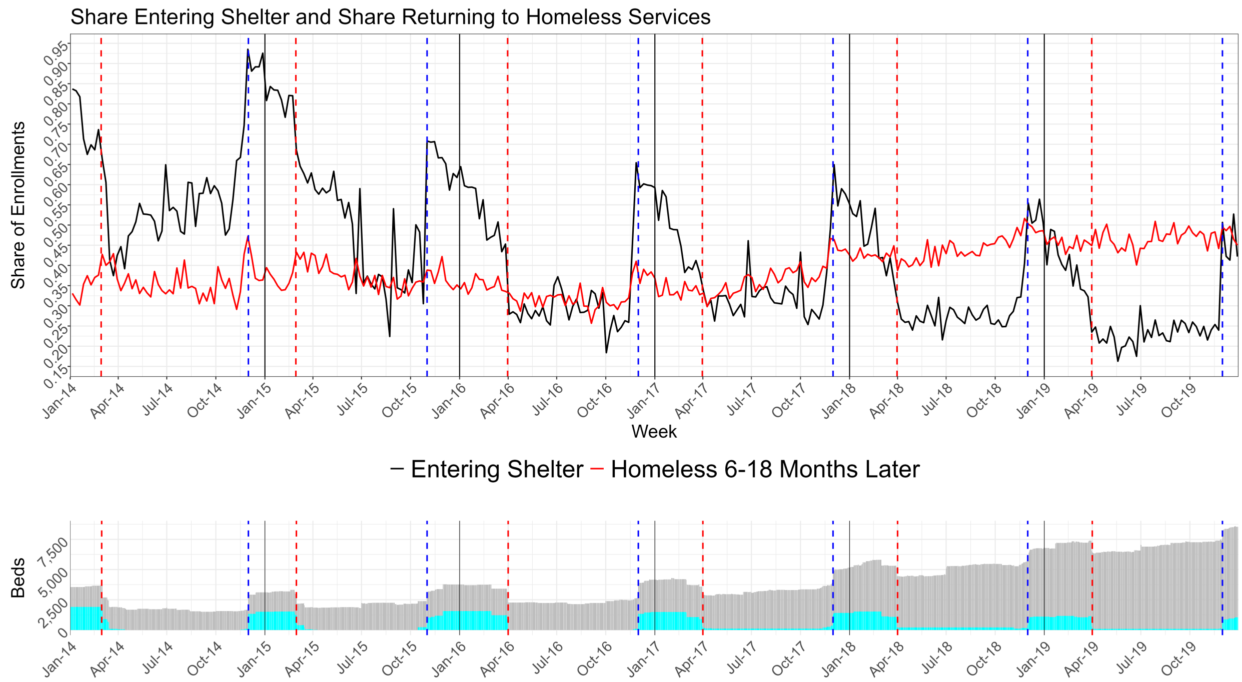

Figure 5: Weekly share of entries to homeless services for shelter (as opposed to non-shelter) and weekly share associated with a reappearance in homeless services 6-18 months later. Daily bed counts provided below for reference. If shelter reduces future homelessness, then when the share of enrollments that are for shelter increases, the share of enrollments associated with future homelessness should also decrease. Each data point is a weekly average of enrollments. The black line is the share of enrollments for emergency shelter (as opposed to non-shelter services), showing clear spikes when shelters open. The red line plots the share of enrollments where the client is still (or again) homeless 6-18 months later.

As Figure 5 shows, when shelter is expanded, the share of individuals entering homeless services who receive shelter (as opposed to non-shelter services) spikes. While expanded shelter availability constitutes a plausibly exogenous shock (e.g., the probability that a person exits homelessness should not depend on how many shelter beds exist in their SPA except through the effect those beds have on the probability they enter shelter) for the purpose of identifying the effect of shelter on individual outcomes, Figure 5 is not sufficient to identify such an effect. In particular, shelter expansion may also change the composition of those entering shelter. Below we provide evidence that, while entries to shelter (unsurprisingly) increase when shelter expands, compositional differences among those induced into shelter are small and cannot explain our estimates.

6.2. Empirical Approach

We run regressions similar to those in previous sections on enrollments in homeless services.

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

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

Now, our first stage captures the share of enrollments e on date $t = ymd$ in SPA s that are for emergency shelter (ES) as opposed to non-shelter services, and our reduced form outcomes will represent the share reappearing in homeless services and share who have died. For ease of interpretation, measures of beds have been scaled to represent hundreds of beds.

To account for potential compositional changes in shelter enrollments, we include controls for individual characteristics (X_i), which include gender, race, ethnicity, (a quadratic in) age, and presence of a disabling condition. Robustness tests in rows 6-8 of Appendix Table C.1 exclude controls, and rows 9-12 show consistent findings across demographic subsamples.

Because expanding shelter could induce people to appear in ES who would not have otherwise appeared in HMIS, our IV estimates only isolate the effect of entering shelter if the composition of enrollments that would have otherwise been "missing" from our services data is not different from those who would have appeared for services even if shelter hadn't been expanded (or if any such compositional differences are independent of outcomes). Appendix Table C.4 reproduces descriptive statistics for the sample of enrollments that occur while winter shelters are open and shows little evidence of observable differences,[42] but we further test for the potential influence of unobservable characteristics in several ways.

First, following an approach common in the health literature,[43] we replicate our analysis using a subset of enrollments that are most likely to be "nondiscretionary" (i.e., those that would have appeared in homeless services regardless of shelter availability). In an ideal setting, our analysis would compare individuals who happened to become homeless when shelter availability was low to individuals who happened to become homeless when shelter availability was high.[44] In an attempt to replicate such a setting, we reproduce our results for the sample of enrollments that occur between September and December and constitute a person's first ever appearance for any kind of homeless services. These results are presented in panel B of Table 7 and are consistent with estimates from the full sample. Additionally, in the final row of Table C.1, we report estimates

[42] Consistent with Meyer, Wyse and Corinth (2023), those entering homeless services when seasonal shelter is available appear slightly older, whiter, and more male. We control for age, race, and gender throughout.

[43] See, for instance, Duggan, Gupta and Jackson (2022).

[44] Robustness tests below show no evidence that shelter availability causes individuals to enter homelessness.

after further restricting to enrollments where "months homeless" is reported to be 1 or missing.[45] These restrictions eliminate 90% of our sample, but results are robust to limiting to these cases flagged as newly homeless. Second, we present results after restricting to individuals who have at least one enrollment in homeless services even when winter shelter beds are not available.[46]

Finally, in Section 6.4.2, we extend away from local average treatment effects and leverage the continuous nature of our instrument to estimate marginal treatment effects,[47] which allow us to more formally explore whether estimated treatment effects are different for individuals with unobserved characteristics that make them most (versus least) likely to select into shelter.

6.3. Results

Results are presented in Table 7. Results in Panel A are for the full sample of enrollments. Results in Panel B are from the sample restricted to entries between September and December and among individuals who have no prior observable history of homelessness.[48] Column 1 presents first-stage estimates. On average, 100 additional shelter beds increases the share of entries to homeless services that are for shelter by roughly 5 percentage points. Results where bed counts are instead replaced with measures of shelter coverage (number of beds divided by annual point-in-time count of the homeless population) are presented in row 15 of Table C.1, are similarly significant, and indicate that a 1 percentage point increase in shelter coverage increases share of shelter enrollments by around 3 percentage points. For consistency with previous sections, we continue to define the independent variable as a measure of beds instead of coverage, noting that estimates using the coverage measure would be similar.

Column 2 (3) reports the reduced-form (IV) estimate for the effect of shelter on reappearance in homeless services 6-18 months after enrollment. Estimates are statistically insignificant and, if anything, positive in direction, suggesting no effect of shelter to reduce future appearances for homeless services relative to non-shelter interventions.

Finally, the last two columns report estimates for the effect of shelter on 18-month mortality rates. Given shelter's effect to reduce ER visits (and crime incidents, which may be associated with life-threatening situations), it may be reasonable to believe that shelter reduces mortality risk.[49] However, our measure of mortality is imperfect. In HMIS data, mortality is recorded as a possible exit destination for any given enrollment, meaning we only observe mortality when case managers

[45] In over 75% of cases where "months homeless" is missing but "date to street/shelter" is populated, the latter indicates that the client is in their first month of homelessness.

[46] The logic here is that this sample constitutes only individuals who have revealed themselves to be the "type" of person who would appear even in the absence of winter shelter beds, cutting out any possible influence of the type of individuals who *only* appear when winter shelter beds are available. These results are presented in row 13 of Table C.1 and are also consistent in direction with choice estimates. However, we note that conditioning on having an appearance outside of the operation of winter shelters likely mechanically raises estimated effects for reappearance.

[47] See, for instance, Bjorklund and Moffitt (1987) and Heckman and Vytlacil (2007).

[48] By considering only each individual's first ever appearance for homeless services, this restriction also eliminates the possibility that our results are driven by repeat enrollments by the same individual.

[49] Indeed, descriptive estimates (e.g., Meyer, Wyse and Logani (2023) and Kuhn, Henwood and Chien (2023)) have found higher mortality rates among the unsheltered.

	ES	Reappearance		Mortality	
Panel A	FS	RF	IV	RF	IV
WS beds	4.75^{***}	0.14		-0.06^{***}	
	(0.09)	(0.11)		(0.02)	
ES			0.0297		-0.0117^{***}
			(0.0239)		(0.0032)
Outcome Mean	41.12	40.23		0.48	
Adj. R^2	0.3727	0.0536	0.0553	0.0040	0.0001
Num. obs.	332,343	332,343	332,343	332,343	332,343
Panel B					
WS beds	7.92^{***}	0.47		-0.13^{***}	
	(0.29)	(0.32)		(0.04)	
ES			0.0591		-0.0163^{***}
			(0.0405)		(0.0055)
Outcome Mean	36.58	25.81		0.28	
Adj. R^2	0.4421	0.0441	0.0466	0.0027	-0.0073
Num. obs.	49,770	49,770	49,770	49,770	49,770

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

Table 7: Effect of shelter on select individual-level outcomes. Panel A results are for the full sample of enrollments described in Table 6. Panel B restricts to enrollments between September and December and represent individual i's first appearance in homeless services. Robust standard errors. Significance unchanged when clustering at the SPA-by-year unit (Appendix Table C.3).

are aware that their client has died. Thus, we almost certainly underestimate baseline levels of mortality and stop short of making strong claims about the precise magnitude of the effects.[50] However, if shelter increases the probability that mortality is *observed*, then our estimates would be biased towards finding a *positive* effect of shelter on mortality. Estimated effects of shelter on short-run mortality are negative and remarkably consistent across all robustness tests (Table C.1), including when enrollments with missing exits are dropped (row 14).

6.4. Robustness

6.4.1. Selection on Observables

In rows 6-7 of Table C.1, we present estimates from regressions that exclude all controls or just demographic controls (age, gender, race, ethnicity, and disability), respectively. When controls are excluded estimated effects on returns to homeless services become significant but remain small in magnitude and close to choice estimates in panel B of Table 7. Estimated effects on mortality are unchanged.

In rows 9-12, we reproduce results when the sample is restricted to include only women, only non-whites, only Hispanics, or only those who are at least 45, respectively. With smaller samples,

[50]For more discussion of homeless mortality, see Meyer, Wyse and Logani (2023) and Henwood et al. (2024).

significance is often lost, but nearly all estimates for all demographic groups match the direction of, and are statistically indistinguishable from, estimates from the full sample.

6.4.2. Selection on Unobservables

The "type" of individuals who only enter ES when availability of shelter beds increases are those who have some unobserved characteristics that make them most "resistant to treatment." If these characteristics also affect outcomes, then our IV estimates capture both the average effect of shelter and the effect of any possible (reverse) selection on gains. Leveraging the multi-valued nature of our instrument, we can compute marginal treatment effects. Conceptually, this allows for a comparison of treatment effects at relatively small values of the instrument (where the difference in characteristics of those induced into shelter would be smallest) to those at relatively large values of the instrument (where average characteristics of those induced into shelter would be most different). We closely follow the approach of Cornelissen et al. (2018), who use the staggered expansion of child care availability across municipalities in Germany as an instrument for entry to child care. While the estimation of marginal treatment effects allows us to evaluate how treatment effects differ across different levels of "resistance to treatment," we cannot identify the factors that constitute such resistance. In particular, in our setting, because expanding shelter increases both the probability of appearing in shelter as opposed to appearing in non-shelter services ("resistance to shelter") and the probability of appearing in shelter as opposed to not appearing for any services ("resistance to any services"), we cannot distinguish between a resistance to entering shelter, specifically, and a resistance to entering homeless services, more broadly. Thus, our estimated marginal treatment effects must be interpreted as inclusive of both.

We plot marginal treatment effects in Appendix Figures C.1 and C.2.[51] Estimated treatment effects for future homelessness are almost always positive and for mortality, are always negative, suggesting that, regardless of whatever unobserved characteristics may drive selection into shelter, estimated effects are consistent in direction with our IV estimates. In other words, while we cannot rule out that unobserved composition influences the *magnitudes* of estimated effects (and thus, future work should devote greater attention to the type of client who would benefit *most* from shelter), we find no subset of compliers for whom effects on future homelessness would be negative or effects on mortality would be positive.

6.4.3. Alternate Samples and Variable Definitions

A possible explanation for the effect of shelter on reappearance for homeless services is that clients are reappearing because they are moving into more intensive or permanent housing provided by homeless service providers, which one might consider to be more of an indicator of successful progression through homeless services than an indication of unresolved homelessness. In Online Appendix Tables I.5 and I.6, we present regression results where reappearance is defined specifically

[51] Full details on estimation are available in Online Appendix III.

as reappearance in the project of the type stated above each set of columns. Results contradict the hypothesis that temporary shelter increases reappearances because it is causing individuals to become enrolled in more intensive programs. Shelter increases the probability of reappearing in shelter and reduces the probability of future appearances in street outreach, but it is associated with *reductions* in the probability of future appearances for rapid re-housing, transitional housing, or permanent housing programs.

Next, because "services only" may (rarely) be provided to individuals who are not yet homeless, we reproduce results for the sample that is identifiably homeless at the time of project entry (row 2 of Table C.1) and for the sample that simply drops entries to services only (SSO) projects (row 3 of Table C.1). To address possible concerns about data reliability in early periods and eliminate the possibility that outcomes (looking 18 months forward) for enrollments occurring later in the sample are affected by the beginning of the COVID-19 pandemic, row 4 of Table C.1 shows that results are robust to trimming the outer half of the sample period (restricting to enrollments between July, 2015, and June, 2018). Row 5 shows that results are robust to a non-linear (probit) regression specification.

Finally, Appendix Table C.2 presents results for two other outcomes. First, we construct a measure of observed exit from homelessness. We define exit as having a recorded exit from any homeless service in the next 18 months to one's own housing or to a living situation with family or friends. If shelter is merely increasing the likelihood of being observed (and has either no effect on future homelessness or actually reduces future homelessness), then the effect on observed exit should be positive. However, results in Table C.2 are significant, large in magnitude, and *negative*.

Second, economic models of homelessness[52] suggest that if the cost of 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. Thus, if shelter reduces this expected cost, then, on the margin, homelessness may become the optimal housing choice for some individuals. For the subsample (approximately half) of HMIS enrollments for which data on months homeless is available, we define an enrollment as "newly homeless" if months homeless = 1. If shelter increases entries into homelessness, we should observe a positive effect of expanding shelter on the share of enrollments for newly homeless individuals. Results in Table C.2 show no such effect.

7. Interpretation, Limitations, and Avenues for Future Work

Taken together, our findings suggest temporary shelter functions as a sort of bandage. Even if it does not reduce future homelessness, it remains a highly effective intervention: it lessens the utility loss of those experiencing homelessness, makes communities safer, and alleviates burdens on public systems. Our current understanding of shelter likely underestimates its community benefits. Beyond the effects we identify, such positive externalities are likely not confined to health and criminal justice settings. Since providing shelter clearly reduces street homelessness, future work

[52] See, for instance, Quigley and Raphael (2001).

might consider broader implications for foot traffic, transit use, and business revenue in areas with high unsheltered homelessness, as well as spillovers such as improved outcomes for children.[53]

While interpreting shelter's effects on crime and health is relatively straightforward, several explanations may account for the absence of effects on future homelessness. One might expect shelter, as a more intensive intervention than street outreach, to promote service engagement and stability that facilitate exits, but other factors may dominate. Our setting exploits LA's seasonal shelter expansion, which may be considered a "lightest-touch" version of shelter, open overnight, for at most four months, almost entirely congregate, and serving adults. Year-round or non-congregate programs may produce different results.

Shelter may also work like unemployment benefits: increasing generosity raises utility and sustains individuals while they search for jobs (or housing) but may lengthen unemployment (or homelessness).[54] Another possibility is disruption of continuity of care. Entering shelter may reassign case managers or reduce contact. Street outreach teams may check in twice weekly, but in shelter, check-ins may fall to twice monthly. Future research should identify how continuous engagement shapes outcomes.

Finally, diminishing returns or selection effects may matter. In our setting, shelter coverage (number of emergency shelter beds per homeless population) never exceeds 42%. Returns may diminish once more of the homeless population is already sheltered. For instance, impacts in New York City, where more than 95% of the homeless population is sheltered, are likely outside the scope of what our analysis can identify. However, outside of New York, roughly half of people experiencing homelessness are in areas where a majority of the homeless population is unsheltered—settings where our findings may be most applicable.

In summary, there is likely extensive heterogeneity in the effect of shelter. More work is needed on whom to prioritize (given scarcity) and what services to provide within shelters. Answers to these questions would greatly improve policymakers' ability to allocate and target homelessness resources.

8. Conclusion

Homelessness has grown 40% in the U.S. since 2017, reversing a decade of decline, with geographic variation in both growth and policy responses. Despite slower growth since 2022, Los Angeles remains marked by persistently high housing costs and homelessness. As homelessness rises more rapidly across the U.S., it is increasingly important for local leaders to understand the consequences of unsheltered homelessness and the efficacy of available interventions.

Emergency shelter is a critical policy tool. As the most widely deployed homelessness intervention after street outreach, shelter's effectiveness has profound implications for resource alloca-

[53] Further, future work should investigate whether shelter location meaningfully affects opportunities for people experiencing homelessness. For instance, work such as Chetty and Hendren (2018) might suggest that the environment in which one experiences homelessness could influence their economic mobility.

[54] See Card et al. (2015); Lalive (2008).

tion. Our study provides among the first causal estimates of shelter's effects, leveraging exogenous variation from LA's winter shelter program to identify impacts on crime, health, mortality, and future homelessness.

We find that shelter functions as a high-value public good with substantial positive externalities. Adding 100 temporary shelter beds for 3-4 months prevents approximately 1 crime and 0.25 psychiatric ER visits per day and significantly reduces 18-month mortality risk. However, we find no evidence that shelter reduces future homelessness. The tension highlighted by these findings is that while shelter dramatically mitigates the social costs of unsheltered homelessness, it does not "solve" homelessness through permanent exits.

These findings expose a classic public goods problem with important policy implications. When the Los Angeles Homeless Services Authority provides shelter, it captures none of the crime reduction benefits (accruing to law enforcement), none of the healthcare savings (accruing to hospitals and health insurers), and none of the mortality prevention benefits (whose value transcends agency boundaries). Agencies evaluated primarily on homelessness reduction metrics thus have incentives to under-invest in shelter in favor of costlier programs that at least reduce future homelessness among participants. Even with a clearer understanding of shelter's welfare gains, unless this misalignment is corrected, shelter will remain under-provided.

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Appendix

Appendix A. Additional Aggregate-Level Tables and Figures

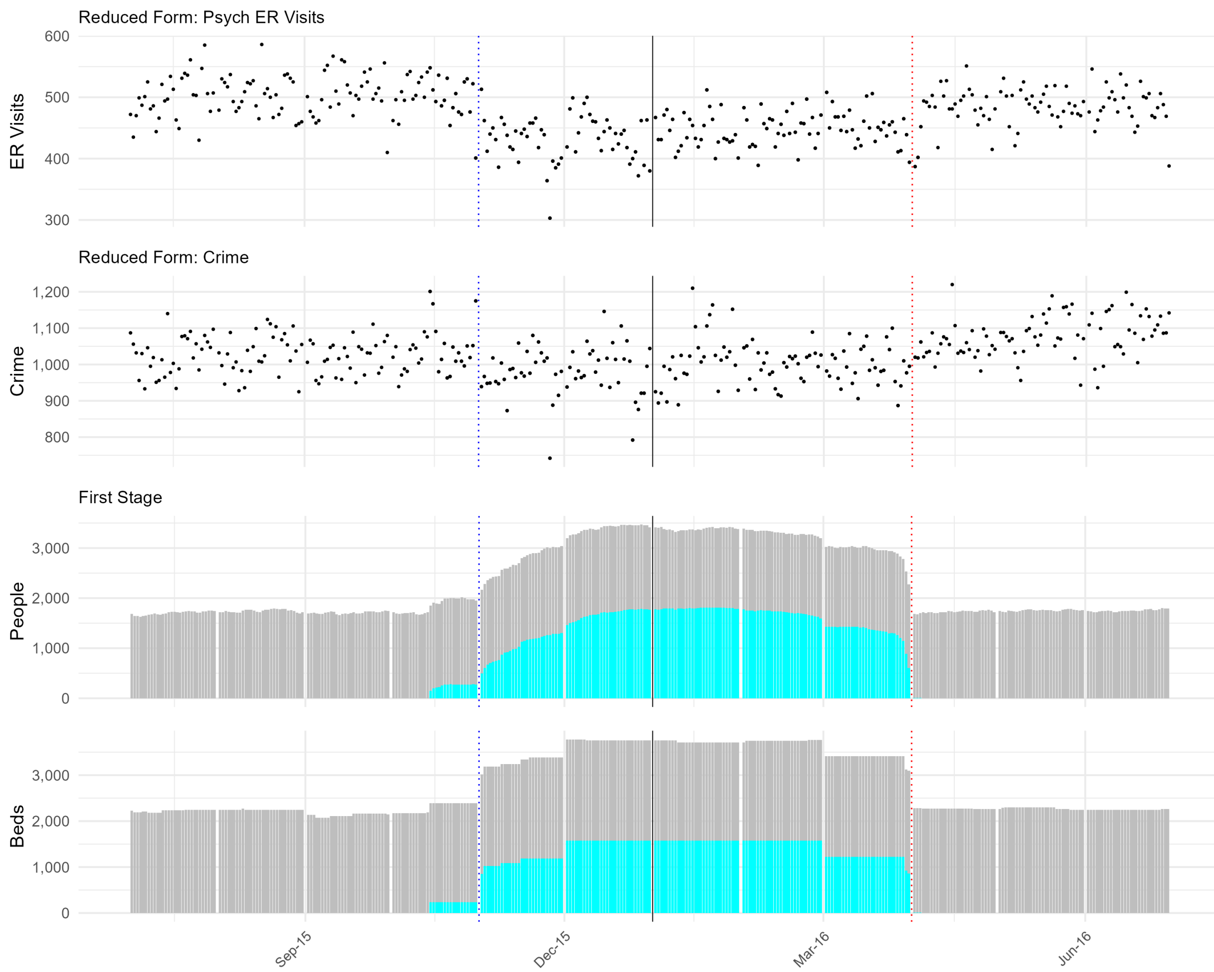

Figure A.1: Figure 4, restricted to July 2015-June 2016. The chart excludes the first of the month, when backlogged crime reporting often occurs, for scale.

Appendix A.1. Event Studies

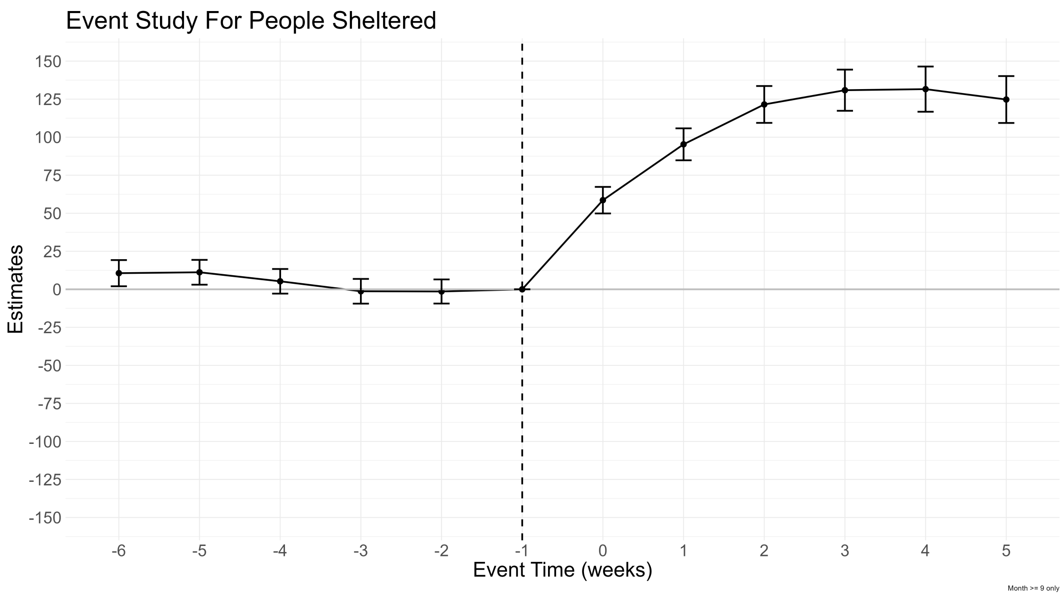

Figure A.2: Event study for first-stage outcome (people in shelter). Event time is pooled to the week-level for clarity.

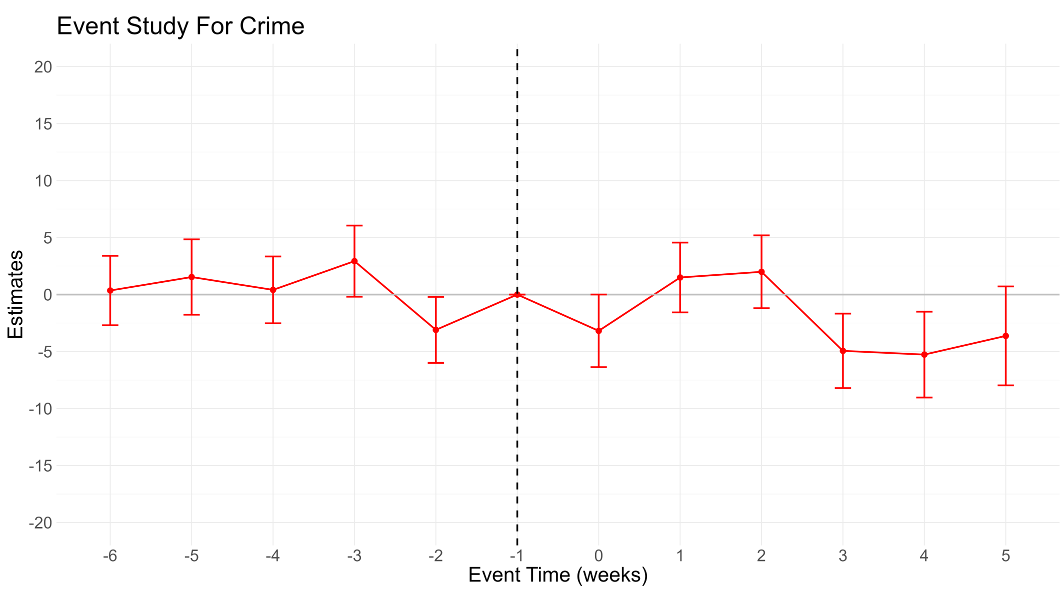

Figure A.3: Event study for crime incidents. Event time is pooled to the week-level for clarity.

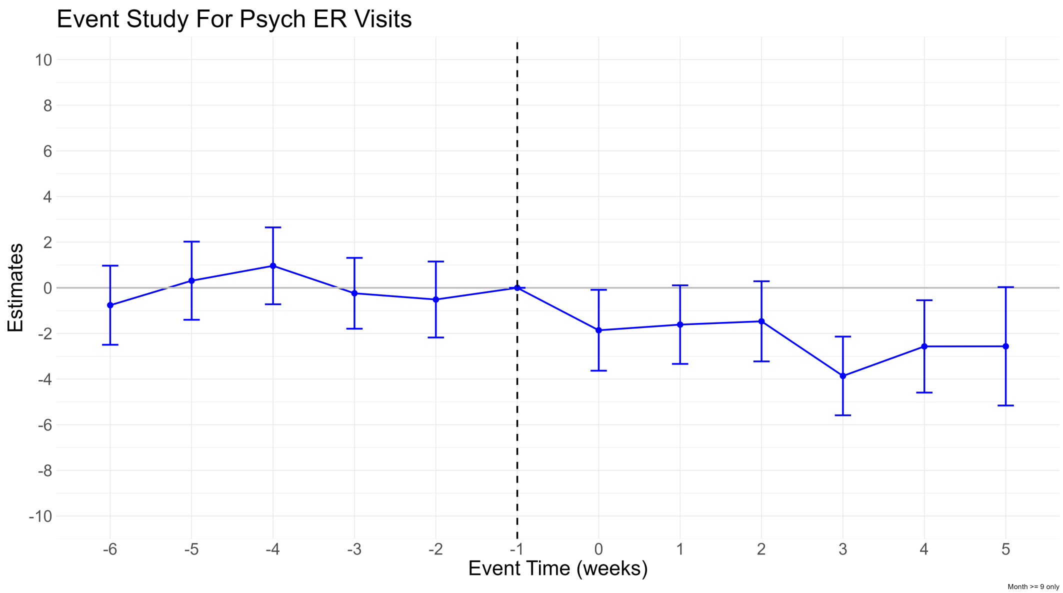

Figure A.4: Event study for psych ER visits. Event time is pooled to the week-level for clarity.

Appendix A.2. Additional Outcomes

	Property	Violent	Drug	Other
sheltered	0.0027	-0.0116^{***}	-0.0007	-0.0020^{*}
	(0.0025)	(0.0019)	(0.0006)	(0.0011)
Outcome Mean	85.43	44.39	5.47	18.34
Adj. R^2	0.9189	0.8945	0.5824	0.7004
Num. obs.	10,955	10,955	10,955	10,955

Table A.1: IV Estimates by Crime Type

	Psychiatric	Injury
sheltered	-0.0027^{**}	-0.0036
	(0.0011)	(0.0024)
Outcome Mean	60.92	224.38
Adj. R^2	0.9261	0.9651
Num. obs.	17,528	17,528

Table A.2: IV Estimates by ER Visit Type

Appendix A.3. September-December Only

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)
other beds	0.2012^{***}	-0.0257^{**}	-0.0243^{**}	-0.0071	-0.0071	-0.0186^{***}	-0.0172^{**}
	(0.0289)	(0.0100)	(0.0102)	(0.0071)	(0.0072)	(0.0069)	(0.0070)
WS beds	0.7604^{***}	-0.0053^{*}		-0.0000		-0.0053^{***}	
	(0.0122)	(0.0031)		(0.0021)		(0.0020)	
sheltered			-0.0069^{*}		-0.0000		-0.0069^{***}
			(0.0040)		(0.0027)		(0.0027)
Outcome Mean	424	125	125	58	58	67	67
Adj. R^2	0.9950	0.9519	0.9519	0.9021	0.9021	0.9202	0.9203
Num. obs.	5856	5856	5856	5856	5856	5856	5856
Panel B							
other beds	0.1536^{***}	-0.0154	-0.0111	-0.0041	-0.0029	-0.0113	-0.0083
	(0.0290)	(0.0113)	(0.0119)	(0.0083)	(0.0087)	(0.0080)	(0.0083)
WS beds	0.7105^{***}	-0.0199^{***}		-0.0059		-0.0140^{***}	
	(0.0163)	(0.0062)		(0.0045)		(0.0041)	
sheltered			-0.0280^{***}		-0.0083		-0.0196^{***}
			(0.0088)		(0.0063)		(0.0057)
Outcome Mean	572	152	152	72	72	80	80
Adj. R^2	0.9963	0.9493	0.9492	0.8894	0.8892	0.9164	0.9167
Num. obs.	3660	3660	3660	3660	3660	3660	3660

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

Table A.3: The Effect of Winter Shelters on Crime (Daytime and Nighttime). Sample is restricted to September-December. Panel A includes all 8 service planning areas (SPAs). Panel B drops SPAs 3, 7, and 8 where coverage of our crime data is poor.

	FS	RF	IV
other beds	0.2012^{***}	-0.0053	-0.0030
	(0.0289)	(0.0047)	(0.0048)
WS beds	0.7604^{***}	-0.0089^{***}	
	(0.0122)	(0.0018)	
sheltered			-0.0117^{***}
			(0.0024)
Outcome Mean	424	60	60
Adj. R^2	0.9950	0.9257	0.9258
Num. obs.	5856	5856	5856

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

Table A.4: The Effect of Winter Shelters on Psychiatric ER Visits. Sample is restricted to September-December.

Appendix A.4. Alternative Standard Error Estimation

	Hetero.	Cluster-Robust	Wild Cluster Bootstrap
Crime: All SPAs			
WS beds	-0.0070^{***}	-0.0070^{**}	-0.0070^{**}
	(0.0018)	(0.0032)	(0.0032)
Crime: Exclude SPAs 3, 7, 8			
WS beds	-0.0096^{***}	-0.0096^{*}	-0.0096
	(0.0031)	(0.0050)	(0.0050)
Crime: Exclude SPAs 3, 7, 8 & Month ≥ 9			
WS beds	-0.0199^{***}	-0.0199^{**}	-0.0199^{**}
	(0.0062)	(0.0074)	(0.0073)
ER Visits			
WS beds	-0.0024^{**}	-0.0024	-0.0024
	(0.0009)	(0.0026)	(0.0026)
ER Visits: Month ≥ 9			
WS beds	-0.0089^{***}	-0.0089^{*}	-0.0089^{*}
	(0.0018)	(0.0049)	(0.0048)

Table A.5: Reduced Form Estimates with Clustered and Bootstrapped Standard Errors

Appendix A.5. October-November Only

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)
other beds	0.2344^{***}	-0.0188	-0.0150	-0.0030	-0.0028	-0.0158	-0.0122
	(0.0330)	(0.0217)	(0.0221)	(0.0162)	(0.0165)	(0.0150)	(0.0152)
WS beds	0.8462^{***}	-0.0136		-0.0005		-0.0131^{**}	
	(0.0241)	(0.0096)		(0.0067)		(0.0061)	
sheltered			-0.0160		-0.0006		-0.0154^{**}
			(0.0114)		(0.0080)		(0.0072)
Outcome Mean	400	125	125	58	58	67	67
Adj. R^2	0.9984	0.9539	0.9540	0.9056	0.9056	0.9181	0.9182
Num. obs.	2928	2928	2928	2928	2928	2928	2928
Panel B							
other beds	0.2509^{***}	-0.0086	-0.0001	-0.0001	0.0018	-0.0085	-0.0018
	(0.0351)	(0.0226)	(0.0236)	(0.0170)	(0.0178)	(0.0160)	(0.0164)
WS beds	0.7567^{***}	-0.0258^{*}		-0.0056		-0.0202^{**}	
	(0.0235)	(0.0142)		(0.0102)		(0.0091)	
sheltered			-0.0341^{*}		-0.0074		-0.0266^{**}
			(0.0185)		(0.0134)		(0.0119)
Outcome Mean	549	152	152	73	73	80	80
Adj. R^2	0.9987	0.9517	0.9518	0.8949	0.8948	0.9129	0.9134
Num. obs.	1830	1830	1830	1830	1830	1830	1830

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

Table A.6: The Effect of Winter Shelters on Crime (Daytime and Nighttime). Sample is restricted to dates in October and November only. Panel A includes all 8 service planning areas (SPAs). Panel B drops SPAs 3, 7, and 8 where coverage of our crime data is poor.

	FS	RF	IV
other beds	0.2344^{***}	-0.0098	-0.0006
	(0.0330)	(0.0085)	(0.0089)
WS beds	0.8462^{***}	-0.0332^{***}	
	(0.0241)	(0.0058)	
sheltered			-0.0392^{***}
			(0.0066)
Outcome Mean	400	61	61
Adj. R^2	0.9984	0.9316	0.9315
Num. obs.	2928	2928	2928

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

Table A.7: The Effect of Winter Shelters on Psychiatric ER Visits. Sample is restricted to dates in October and November only.

Appendix A.6. Select Other Robustness

	All Crime	Night Crime	Psych ER Visits
Binary Treatment			
sheltered	-0.0144^{*}	-0.0135^{***}	-0.0121^{***}
	(0.0075)	(0.0049)	(0.0028)
County-level Estimation			
sheltered	-0.0121^{*}	-0.0066	-0.0104^{***}
	(0.0066)	(0.0043)	(0.0037)
Shelter Coverage Measure			
sheltered	-0.0183^{*}	-0.0134^{**}	-0.0071^{***}
	(0.0100)	(0.0065)	(0.0025)

Table A.8: Select Robustness Tests. IV estimates for effects on crime, night crime, and psych ER visits when treatment (*WS beds*) is made binary, when estimated at the county-level (aggregating across the 8 SPAs), and when measures of beds are replaced with measures of coverage (beds divided by homeless population), respectively.

Appendix A.7. Excluding Control for Other Beds

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)
other beds							
WS beds	0.8562***	−0.0068***		−0.0017		−0.0052***	
	(0.0112)	(0.0018)		(0.0012)		(0.0011)	
sheltered			−0.0080***		−0.0020		−0.0060***
			(0.0021)		(0.0014)		(0.0013)
Outcome Mean	396	126	126	59	59	67	67
Adj. R^2	0.9730	0.9461	0.9460	0.8960	0.8960	0.9168	0.9167
Num. obs.	17528	17528	17528	17528	17528	17528	17528
Panel B							
other beds							
WS beds	0.7819***	−0.0094***		−0.0024		−0.0070***	
	(0.0186)	(0.0031)		(0.0022)		(0.0020)	
sheltered			−0.0120***		−0.0030		−0.0090***
			(0.0040)		(0.0028)		(0.0026)
Outcome Mean	534	154	154	73	73	80	80
Adj. R^2	0.9750	0.9430	0.9429	0.8840	0.8840	0.9131	0.9130
Num. obs.	10955	10955	10955	10955	10955	10955	10955

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

Table A.9: The Effect of Winter Shelters on Crime (Daytime and Nighttime). Control for other beds is excluded. Panel A includes all 8 service planning areas (SPAs). Panel B drops SPAs 3, 7, and 8 where coverage of our crime data is poor.

	FS	RF	IV
other beds			
WS beds	0.8562***	−0.0026***	
	(0.0112)	(0.0009)	
sheltered			−0.0031***
			(0.0011)
Outcome Mean	396	61	61
Adj. R^2	0.9730	0.9260	0.9259
Num. obs.	17528	17528	17528

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

Table A.10: The Effect of Winter Shelters on Psychiatric ER Visits. Control for other beds is excluded.

Appendix B. Additional Within-Region Tables and Figures

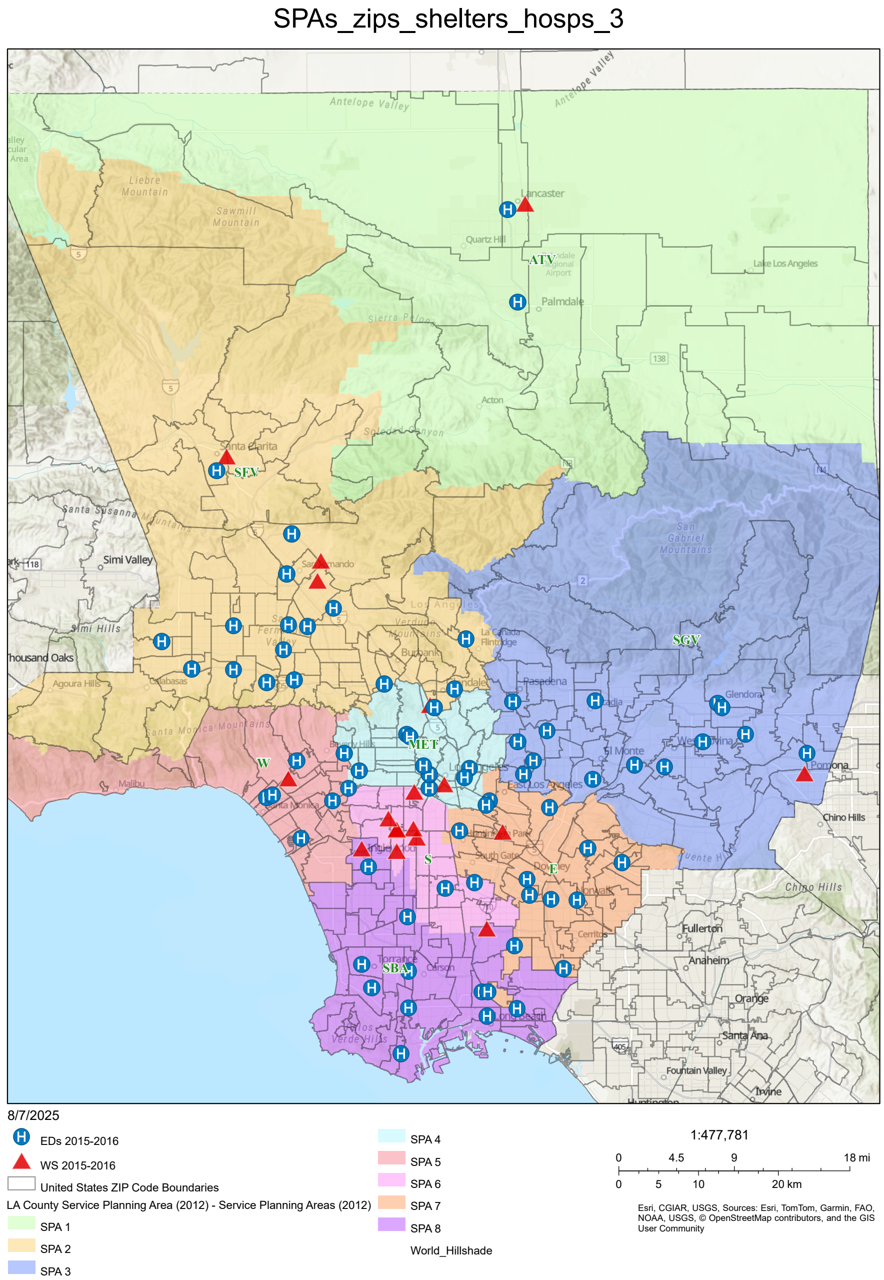

Figure B.1: Map of LA County's SPAs, Zip Codes, Hospitals, and Winter Shelters

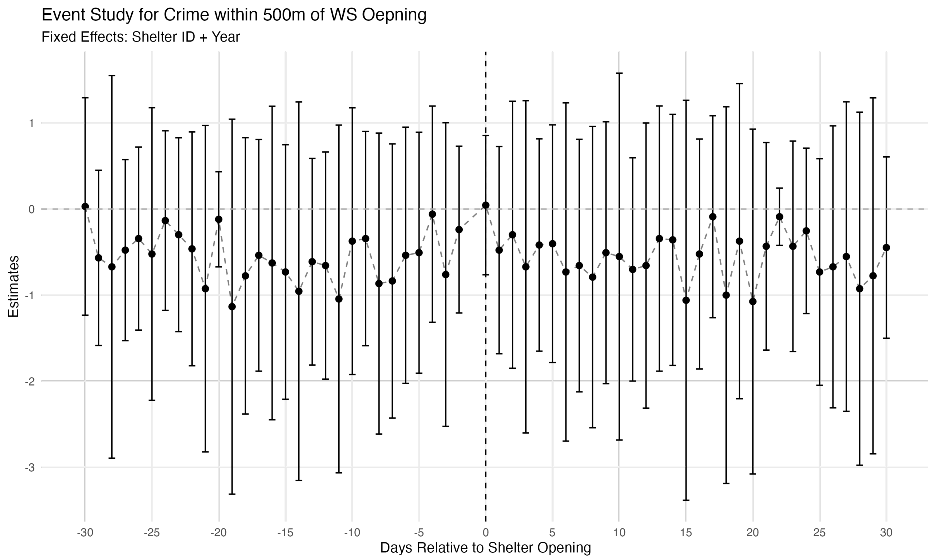

Figure B.2: Event Study of Crime within 500M Winter Shelter Openings

Appendix C. Additional Individual-Level Tables and Figures

Appendix C.1. Robustness Tests

		ES	Reappearance		Mortality	
		FS	RF	IV	RF	IV
1)	Choice Estimates	4.75***	0.14	0.0297	−0.06***	−0.0117***
		(0.09)	(0.11)	(0.0239)	(0.02)	(0.0032)
2)	Homeless at Entry	4.49***	0.09	0.0197	−0.06***	−0.0133***
		(0.10)	(0.12)	(0.0272)	(0.02)	(0.0038)
3)	Drop SSO	3.33***	0.01	0.0029	−0.03**	−0.0103**
		(0.09)	(0.12)	(0.0364)	(0.02)	(0.0047)
4)	July 2015-June 2018	4.85***	0.16	0.0339	−0.06***	−0.0121***
		(0.13)	(0.15)	(0.0319)	(0.02)	(0.0042)
5)	Probit	5.28***	0.18		−0.06***	
		(0.10)	(0.11)		(0.02)	
6)	No controls	15.59***	0.49***	0.0317***	−0.05***	−0.0033***
		(0.05)	(0.06)	(0.0041)	(0.01)	(0.0005)
7)	No demographics	4.85***	0.57***	0.1165***	−0.04***	−0.0082***
		(0.09)	(0.11)	(0.0237)	(0.02)	(0.0031)
8)	No day f.e.	4.77***	0.15	0.0308	−0.06***	−0.0118***
		(0.09)	(0.11)	(0.0238)	(0.02)	(0.0032)
9)	Only Women	4.64***	0.22	0.0467	−0.03	−0.0071***
		(0.16)	(0.19)	(0.0407)	(0.02)	(0.0047)
10)	Only Non-White	3.73***	0.34**	0.0899**	−0.03	−0.0080***
		(0.12)	(0.15)	(0.0401)	(0.02)	(0.0051)
11)	Only Hispanic	6.09***	0.03	0.0050	−0.03	−0.0051
		(0.18)	(0.21)	(0.0348)	(0.03)	(0.0046)
12)	Only Age $\geq$ 45	5.44***	−0.02	−0.0039	−0.11***	−0.0197***
		(0.13)	(0.16)	(0.0303)	(0.03)	(0.0050)
13)	Has non-WS appearance	4.72***	2.15***	0.4555***	−0.07***	−0.0154***
		(0.11)	(0.14)	(0.0321)	(0.02)	(0.0044)
14)	Drop missing exits	3.80***	0.01	0.0020	−0.09***	−0.0227***
		(0.09)	(0.15)	(0.0397)	(0.02)	(0.0063)
15)	Shelter coverage measure	3.16***	0.21***	0.0661***	−0.03***	−0.0111***
		(0.05)	(0.06)	(0.0193)	(0.01)	(0.0027)
16)	Flagged newly homeless	8.62***	0.60	0.0696	−0.11**	−0.0123**
		(0.36)	(0.41)	(0.0476)	(0.05)	(0.0056)

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

Table C.1: Assorted robustness tests for the effect of shelter on key individual-level outcomes.

Appendix C.2. Additional Outcomes, Observables, and Unobservables

	ES	Reappearance		Exit		Newly Homeless		Mortality	
Panel A	FS	RF	IV	RF	IV	RF	IV	RF	IV
WS beds	4.75***	0.14		−2.13***		−0.00		−0.06***	
	(0.09)	(0.11)		(0.09)		(0.13)		(0.02)	
ES			0.0297		−0.4488***		−0.0011		−0.0117***
			(0.0239)		(0.0209)		(0.0326)		(0.0032)
Outcome Mean	41.12	40.23		19.41		15.46		0.48	
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
Panel B									
WS beds	7.92***	0.47		−2.04***		0.14		−0.13***	
	(0.29)	(0.32)		(0.26)		(0.42)		(0.04)	
ES			0.0591		−0.2573***		0.0233		−0.0163***
			(0.0405)		(0.0342)		(0.0691)		(0.0055)
Outcome Mean	36.58	25.81		13.32		21.77		0.28	
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 C.2: Effect of shelter on select individual-level outcomes. Panel A results are for the full sample of enrollments described in Table 6. Panel B restricts to enrollments that occur between September and December and represent individual i's first appearance in homeless services. Robust standard errors.

	ES	Reappearance		Mortality	
Panel A	FS	RF	IV	RF	IV
WS beds	4.75***	0.14		−0.06***	
	(1.27)	(0.30)		(0.02)	
ES			0.0297		−0.0117***
			(0.0632)		(0.0036)
Outcome Mean	41.12	40.23		0.48	
Adj. R^2	0.3727	0.0536	0.0553	0.0040	0.0001
Num. obs.	332,343	332,343	332,343	332,343	332,343
Num. clusters	48	48	48	48	48
Panel B					
WS beds	7.92***	0.47		−0.13***	
	(2.22)	(0.43)		(0.03)	
ES			0.0591		−0.0163**
			(0.0582)		(0.0062)
Outcome Mean	36.58	25.81		0.28	
Adj. R^2	0.4421	0.0441	0.0466	0.0027	−0.0073
Num. obs.	49,770	49,770	49,770	49,770	49,770
Num. clusters	48	48	48	48	48

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

Table C.3: Effect of shelter on select individual-level outcomes with standard errors clustered at the SPA-year level.

Independent Variables	Full Sample	ES	SO	SSO
Age	44.22	44.30	44.23	43.85
Male	0.6312	0.6664	0.5875	0.5817
White	0.4514	0.4544	0.4599	0.4125
Hispanic	0.2755	0.2766	0.2761	0.2679
Disabled	0.3528	0.3727	0.2859	0.4452
n	141,435	79,409	45,879	16,147

Dependent Variables	Full Sample	ES	SO	SSO
ES	56.15	100.00	0.00	0.00
Reappear	41.64	42.57	38.58	36.99
Mortality	0.41	0.35	0.48	0.52

Table C.4: Descriptive statistics for individual-level analysis for the subset of enrollments that occur when winter shelters are open.

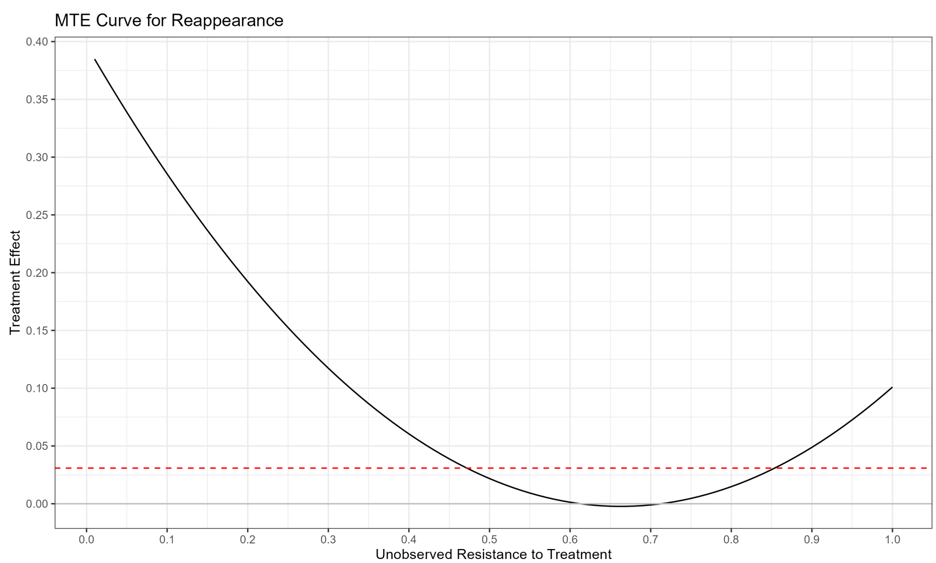

Figure C.1: Marginal treatment effects for *Reappear* outcome. Estimated for polynomial of degree 3 (robust to higher order polynomials). Downward slope suggests possible reverse selection on gains, but estimated treatment effects remain non-negative for (almost) all entries. Evidence suggests that (unobservable) composition of compliers could contribute to the magnitude of point estimates, but composition would not change the sign of estimates for any sub-group.

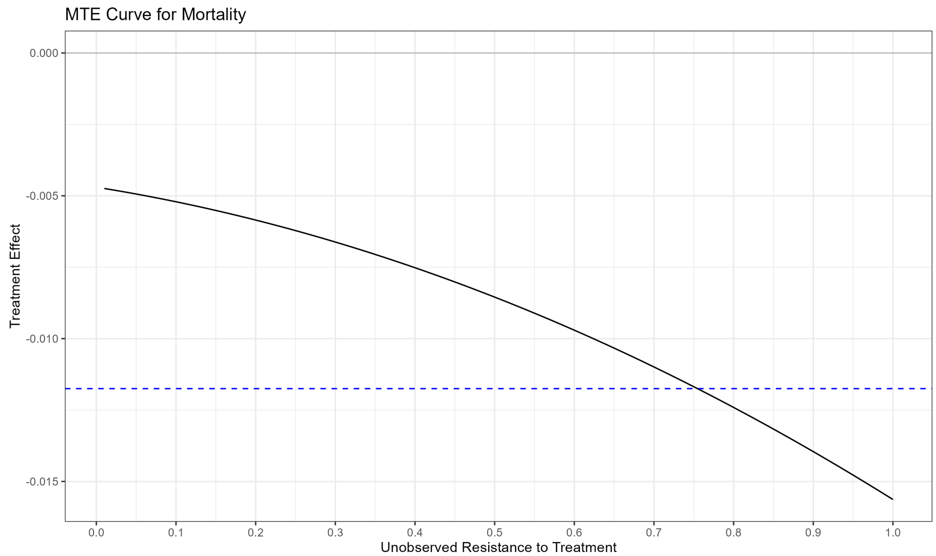

Figure C.2: Marginal treatment effects for $Mortality$ outcome. Estimated for polynomial of degree 3 (robust to higher order polynomials). Downward slope suggests possible reverse selection on gains, but estimated treatment effects are negative for all entries. Evidence suggests that (unobservable) composition of compliers could contribute to the magnitude of point estimates, but composition would not change the sign of estimates for any sub-group.

Online Appendix

Online Appendix I. Supplementary Tables and Figures

Online Appendix I.1. Validation of Open/Close Dates

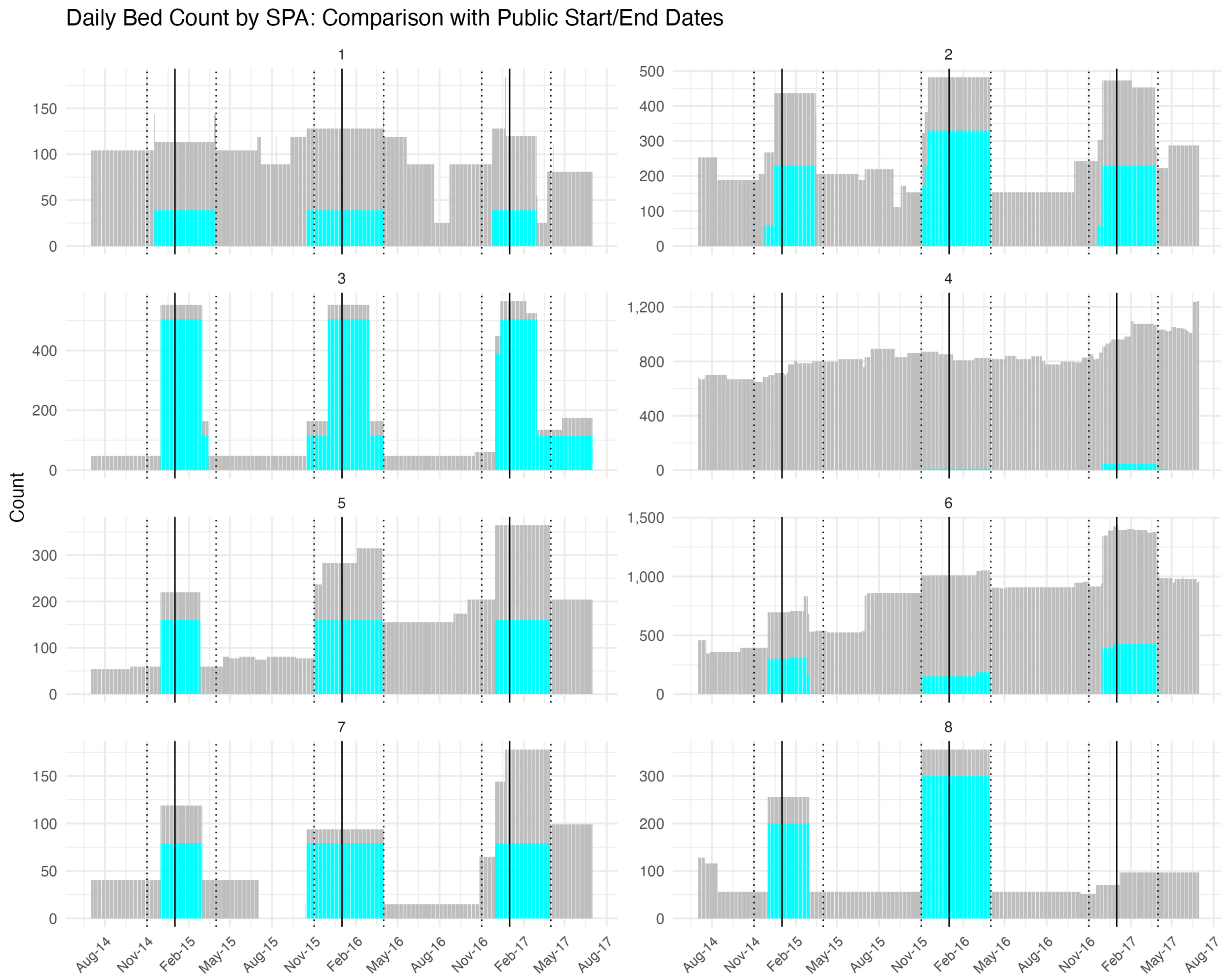

Figure I.1: Daily Bed Count for 2014-17 Seasons with Lines for November 1 and April 1.

Figure I.1 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 I.1, 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.

Online Appendix I.2. Supplementary SPA-Level Content

	People in Shelter	All Crime		Daytime Crime		Night Crime	
	FS	RF	IV	RF	IV	RF	IV
other beds	0.7583***	−0.0028		−0.0035**		0.0008	
	(0.0083)	(0.0026)		(0.0017)		(0.0017)	
WS beds	0.8911***	−0.0070***		−0.0018		−0.0051***	
	(0.0084)	(0.0018)		(0.0012)		(0.0012)	
sheltered			−0.0078***		−0.0021		−0.0058***
			(0.0020)		(0.0014)		(0.0013)
Outcome Mean	429	126	126	59	59	67	67
Adj. R^2	0.9863	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 I.1: Replication of Table 1, including all 8 SPAs and imposing 150% cap on occupancy instead of 120%

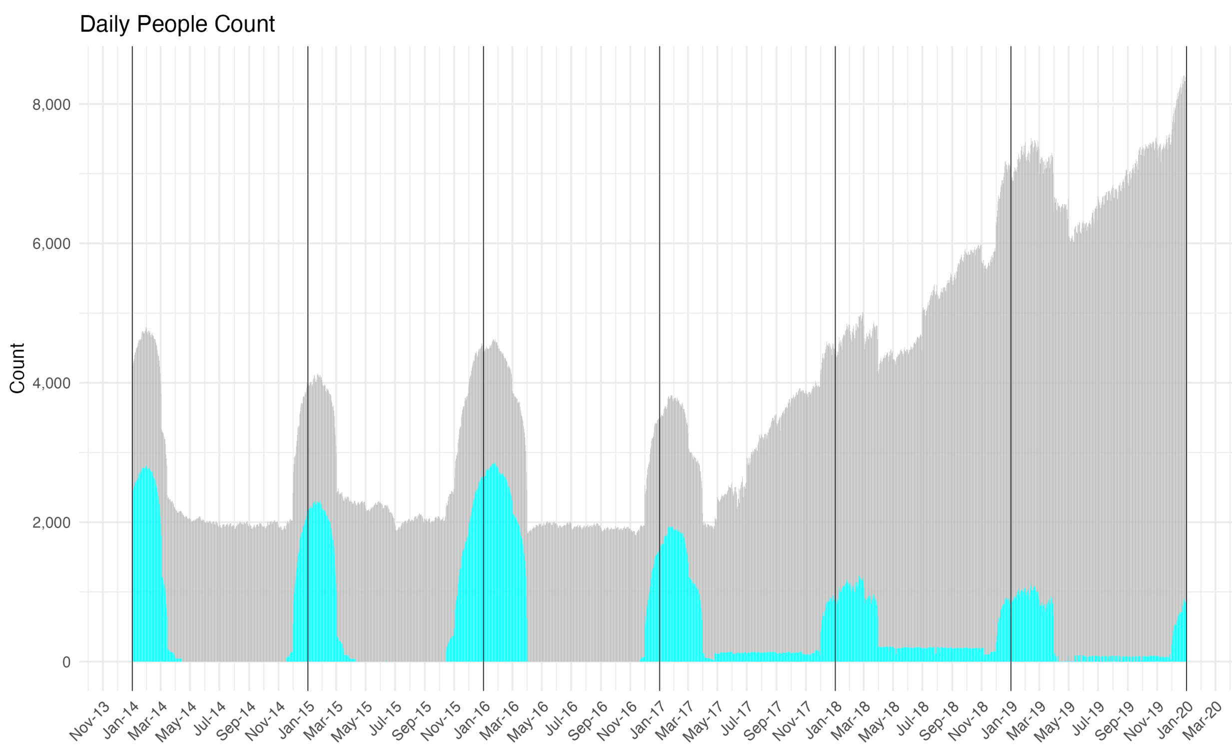

Figure I.2: Daily People Count by Winter Shelter (Blue) vs. Other Shelter (Grey)

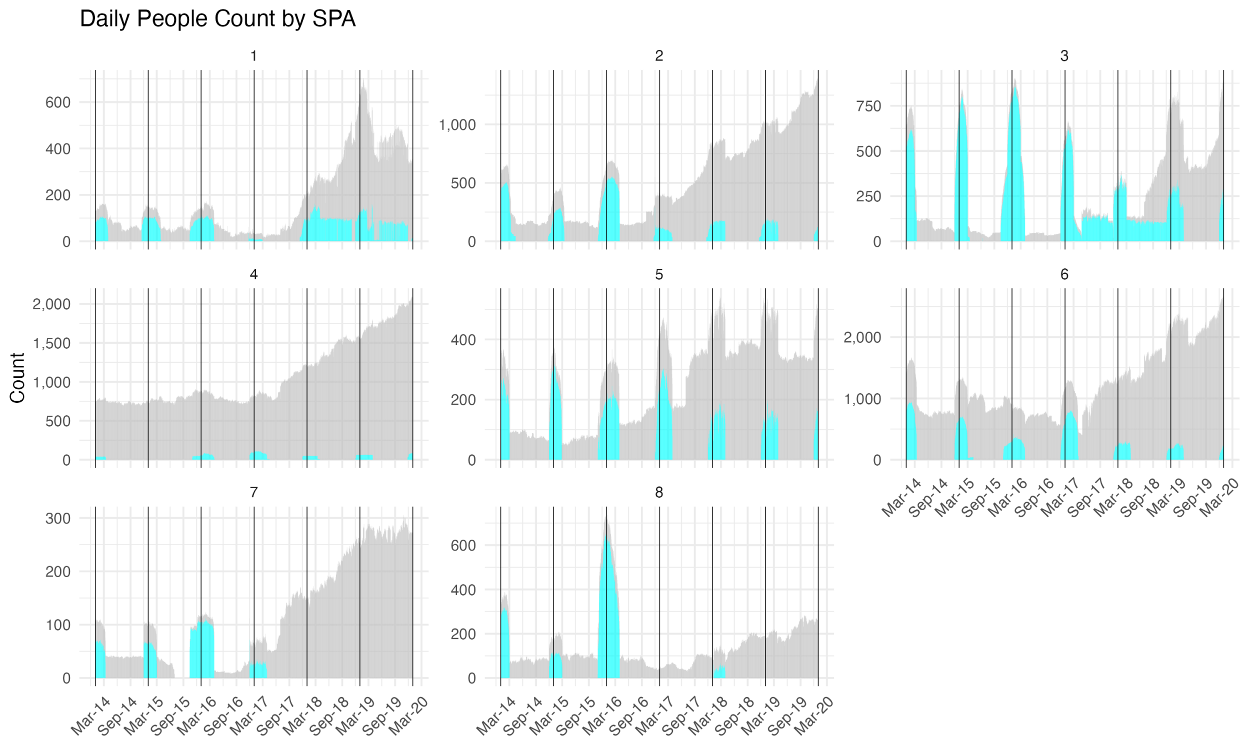

Figure I.3: Daily People Count by SPA and Winter Shelter (Blue) vs. Other Shelter (Grey)

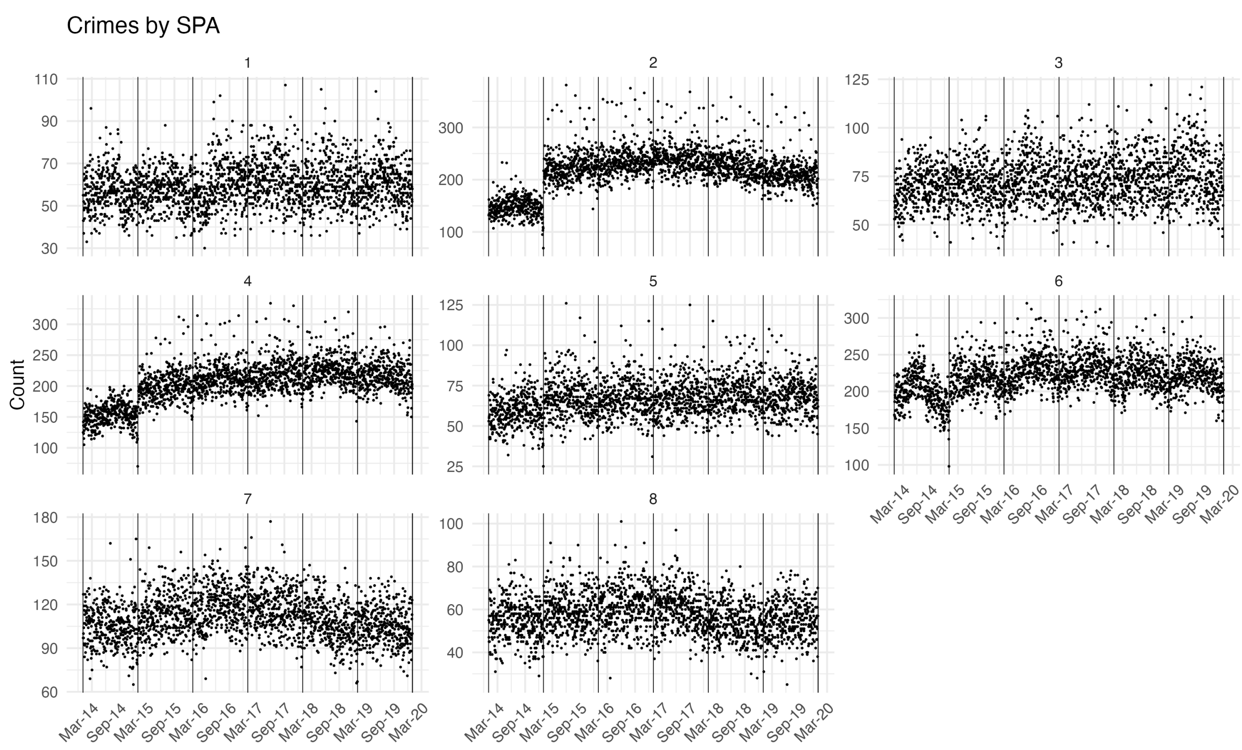

Figure I.4: Daily Count of Crimes Reported by LASD and LAPD by SPA

Online Appendix I.3. Dependent Variables Under Alternative Windows

		Reappear 0-6 months		0-12 months		0-18 months		by 2023	
	FS	RF	IV	RF	IV	RF	IV		
WS beds	4.75^{***}	0.41^{***}		0.65^{***}		0.59^{***}		0.41^{***}	
	(0.09)	(0.11)		(0.11)		(0.11)		(0.10)	
ES			0.0869^{***}		0.1363^{***}		0.1231^{***}		0.0873^{***}
			(0.0234)		(0.0229)		(0.0225)		(0.0202)
Outcome Mean	41.12	55.42		62.14		65.53		75.34	
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 I.2: Effects on reappearance outcome under different windows.

		Reappear 6-18 months		6 months-2023		12-24 months		12 months-2023	
	FS	RF	IV	RF	IV	RF	IV		
WS beds	4.75^{***}	0.14		0.10		0.44^{***}		0.33^{***}	
	(0.09)	(0.11)		(0.11)		(0.11)		(0.12)	
ES			0.0297		0.0217		0.0924^{***}		0.0702^{***}
			(0.0239)		(0.0237)		(0.0232)		(0.0243)
Outcome Mean	41.12	40.23		59.92		33.78		53.62	
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 I.3: Effects on reappearance outcome under different windows.

		mortality 6-month		mortality 12-month		mortality 18-month		mortality by 2023	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
WS beds	4.75^{***}	-0.01		-0.04^{***}		-0.06^{***}		-0.05	
	(0.09)	(0.01)		(0.01)		(0.02)		(0.04)	
ES			-0.0026		-0.0086^{***}		-0.0117^{***}		-0.0101
			(0.0019)		(0.0027)		(0.0032)		(0.0076)
Outcome Mean	41.12	0.17		0.32		0.48		2.39	
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 I.4: Effects on mortality outcome under different windows.

Online Appendix I.4. Recidivism by Project at Reappearance

		Emergency Shelter		Day Shelter		Street Outreach		Services Only	
	FS	RF	IV	RF	IV	RF	IV	RF	IV
WS beds	4.75***	1.45***		0.04		−0.42***		0.09	
	(0.09)	(0.10)		(0.03)		(0.08)		(0.07)	
ES			0.3050***		0.0077		−0.0889***		0.0181
			(0.0208)		(0.0073)		(0.0175)		(0.0146)
Outcome Mean	41.12	20.25		2.50		19.88		10.20	
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 I.5: 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
WS beds	4.75***	−0.55***		−0.13***		−0.40***		−0.10***	
	(0.09)	(0.05)		(0.03)		(0.05)		(0.03)	
ES			−0.1148***		−0.0275***		−0.0832***		−0.0213***
			(0.0104)		(0.0055)		(0.0099)		(0.0064)
Outcome Mean	41.12	5.00		1.06		4.05		1.51	
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 I.6: 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.

Online Appendix II. Construction of Daily Shelter Counts

Online Appendix II.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.[55] 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 II.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

[55] 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.[56] 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 II.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 II.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

[56] 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.[57] 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. 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[58]

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%).

[57]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.

[58]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.

Online Appendix III. Estimation of Marginal Treatment Effects

We estimate marginal treatment effects following the general procedure of Cornelissen et al. (2018) who similarly leverage variation in exposure to treatment from staggered program expansion across localities and over time.

Propensity scores, $P(X,\ SPA,\ year,\ month,\ other\ beds,\ WS\ beds) \equiv p$, are estimated by probit regression and represent the probability that an enrollment with client characteristics X will be for shelter as opposed to non-shelter services, given number of available other beds and winter shelter beds at the time (date) and location (SPA) of appearance for homeless services.

Marginal treatment effects are then estimated by the following equation

$$Y = X\beta_0 + \psi_{sy} + \mu_m + \gamma_1 other\ beds_{symd} + \mu_m^{'}\hat{p} + X(\beta_1 - \beta_0)\hat{p} + \sum_{k=2}^{K} \alpha_k \hat{p}^k + \varepsilon$$

where the interaction of individual characteristics X with estimated propensity score $\hat{p}$ captures any variation in treatment effects attributable to observable characteristics,[59] leaving the polynomial in $\hat{p}$ to capture the marginal effect of increased propensity to enter shelter (due to higher values of the instrument), conditional on the full set of controls and fixed effects.[60] At low propensities (when, conditional on controls, shelter supply shocks from winter shelters are relatively small), only individuals who have unobserved characteristics that make them *most* likely to select into shelter will be induced into treatment. At high propensities, the marginal entrant to shelter has unobserved characteristics that make them *least* likely to select into shelter (otherwise, they would have appeared even in the absence of a large shock to shelter supply).[61]

[59]Similarly, we also capture any seasonal variation in treatment effects by interacting month fixed effects with propensity score.

[60]We omit day fixed effects throughout due to concerns about over-fitting. As shown in Table C.1 (row 8), our estimates are nearly identical without day fixed effects.

[61]As an example, suppose age were unobserved, and assume it is (unbeknownst to the researcher) negatively associated with selection into shelter (i.e., younger individuals are most likely to select into shelter). In this case, absent a control for age, our IV estimate would be the combined effect of shelter, holding age constant + the effect of the increased average age of those in shelter (because the youngest have already selected into shelter, when winter shelter beds increase, the next youngest individuals, who are slightly older than those already in shelter, raise the average age). Because selection into shelter is a function of age (if it were not, then, by definition, our IV estimate would not be influenced by the unobserved characteristic of age), it must be the case that for smaller (relative) values of the instrument, the composition of shelter enrollments is at its youngest. Specifically, the average age of shelter enrollments when winter shelter beds = 0 is at its youngest as the youngest individuals select into shelter even in the absence of the instrument. Shelter enrollments when the value of the instrument is 30 are slightly older as the next youngest individuals select in, shelter enrollments when the instrument is 60 are slightly older, and so on, such that shelter enrollments at the highest value of the instrument constitute the oldest sample of shelter enrollments. Therefore, holding all else equal, any difference in the treatment effect estimated when winter shelter beds = 60 and the treatment effect estimated when winter shelter beds = 30 can be attributed to the increase in age composition (or any other unobserved characteristics that are similarly associated with entry to shelter when availability is expanded). If treatment effects are drastically different when estimated at different values of the instrument, then it must be the case that age plays a role in selecting into shelter and that age is associated with the outcome. If treatment effects are constant across values of the instrument, then either selection is not a function of age or age does not affect the outcome. If our treatment effects are the result of some unobservable characteristic(s) affecting composition of compliers and outcomes, then the effect of the unobservable characteristic(s) should be at its greatest at the highest (relative) values of the instrument.

If estimated treatment effects are different at low propensities than they are at high propensities (or, equivalently, for those with low resistance to entering shelter versus those with high resistance to entering shelter), then it may be that our results are driven by the unobservable composition of those who are induced to enter shelter when beds are expanded. However, as figures show, estimated effects on future homelessness are never significantly negative at any level of resistance, and similarly, estimated effects for mortality are always negative.

The figures plot estimated marginal treatment effects where propensity score is modeled as a polynomial of degree 3 ($K = 3$), but results are similar for higher order polynomials. The general downward slope suggests possible reverse selection on gains, but standard errors (omitted for clarity) are large. As a result, we stop short of making any claims about the relative returns to expanding shelter and emphasize this as a key consideration for future work. For our purposes, the key result is that there is no level of unobserved resistance to treatment at which the estimated effects of shelter would be negative for future homelessness or positive for mortality, indicating that any potential selection on unobservables cannot explain (at least the direction of) our estimated treatment effects.

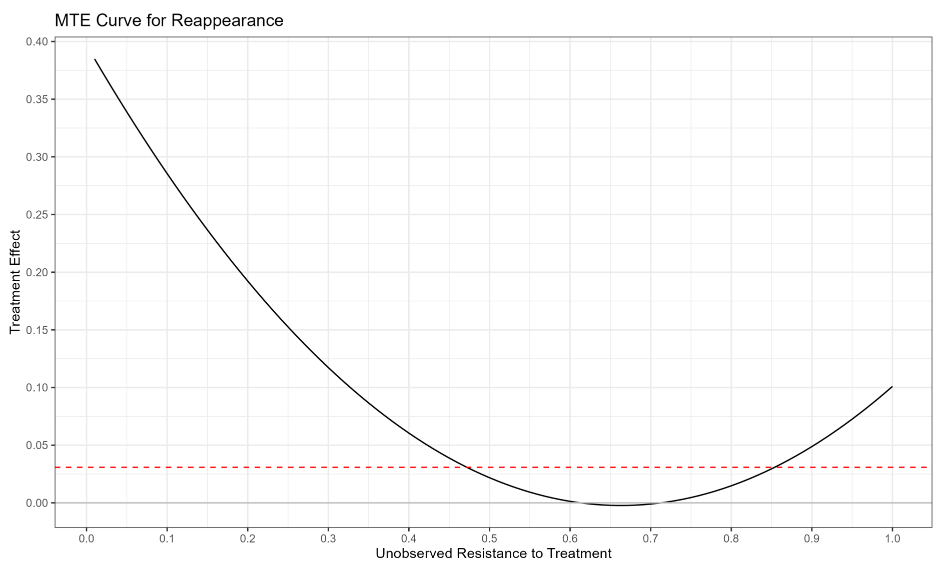

Figure III.5: Marginal treatment effects for *Reappear* outcome. Estimated for polynomial of degree 3 (robust to higher order polynomials). Downward slope suggests possible reverse selection on gains, but estimated treatment effects remain non-negative for (almost) all entries. Evidence suggests that (unobservable) composition of compliers could contribute to the magnitude of point estimates, but composition would not change the sign of estimates for any sub-group.

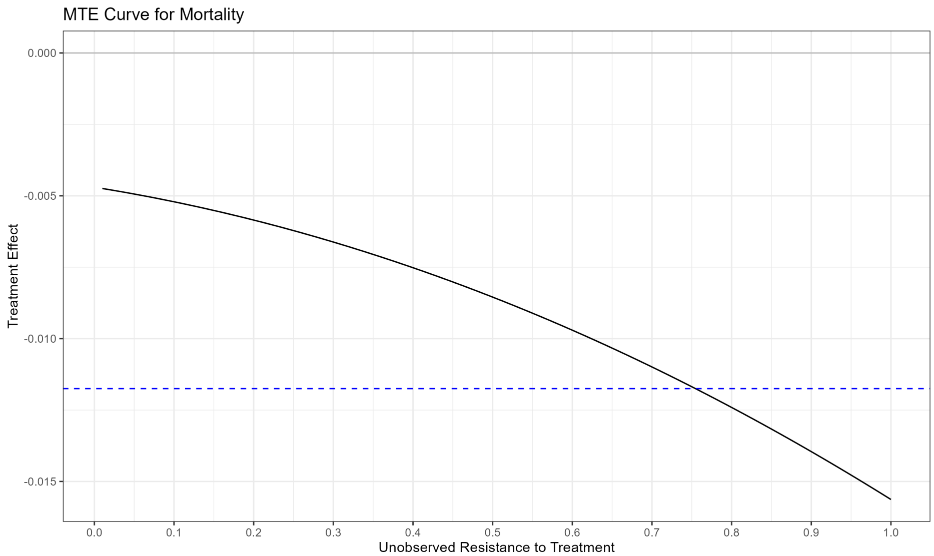

Figure III.6: Marginal treatment effects for $Mortality$ outcome. Estimated for polynomial of degree 3 (robust to higher order polynomials). Downward slope suggests possible reverse selection on gains, but estimated treatment effects are negative for all entries. Evidence suggests that (unobservable) composition of compliers could contribute to the magnitude of point estimates, but composition would not change the sign of estimates for any sub-group.