Do Permanent Supportive Housing Facilities Affect Neighborhood Crime? Evidence from Los Angeles County

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ABSTRACT

Permanent supportive housing (PSH), which combines subsidized affordable housing with case management and social services, represents an increasingly popular approach to responding to chronic homelessness. However, attempts to develop new PSH facilities often encounter local resistance based on concerns that such facilities threaten the public safety of nearby residents. This preregistered study provides new evidence on whether PSH facilities causally affect crime in proximate areas, using the expansion of these facilities in Los Angeles County as a natural experiment. Combining administrative crime data from 2010 to 2024 with housing data from state and local government sources, we employ a staggered difference-in-differences design to compare crime changes in the area surrounding newly opened PSH facilities to contemporaneous changes in areas where PSH facilities will open in the future. Results show precise null effects of PSH openings on violent and property crime, in general ruling out crime increases of greater than 5 percent. While opening PSH facilities does not lead to a measurable increase in local property or violent crime, we cannot rule out associated increases in local disorder or drug offenses that can shape public perceptions of safety.

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1. Introduction

On a given night in 2025, nearly 750,000 people in the United States were homeless, a roughly 30% increase from just three years prior (U.S. Department of Housing and Urban Development [HUD], 2026a). Despite widespread agreement about the need to reduce the substantial harms and economic costs associated with homelessness, there is little consensus about which policy solutions can best address the various challenges facing people experiencing homelessness (PEH). These include not only the lack of stable housing, but also high rates of substance use, mental health problems, and involvement in illegal activity (McCarthy and Hagan, 2024). To address these multiple challenges simultaneously, many localities have funded the expansion of permanent supportive housing (PSH), a strategy intended to address chronic homelessness through the combined provision of subsidized housing and comprehensive onsite support services. PSH differs from emergency or transitional housing by providing long-term housing wherein residents have standard renters' rights as well as access to onsite case management and specialized supportive services (Harris et al., 2018; NASEM, 2018; Rog et al., 2014). Most PSH facilities practice some version of the "Housing First" model—a "low barrier" approach to addressing homelessness that focuses on immediately housing people then providing voluntary services to address needs related to, for example, mental health and substance use disorders. This framework has been an explicit component of HUD and Veterans' Administration policies since the early 2000s (National Alliance to End Homelessness, 2022; U.S. Department of Housing and Urban Development, 2023).

Studies have shown that becoming housed through PSH can reduce criminal justice involvement among persons experiencing homelessness (Cohen, 2024; Hunter et al., 2022). However, PSH has become increasingly controversial, in large part due to growing concerns that the "Housing First" approach (i.e., PSH residency is not contingent on enrollment in treatment) commonly adopted by PSH fails to adequately address the mental health and substance use disorders common among the chronically homeless population (Eide, 2020; Smith, 2025; Tsai, 2020). Additionally, local communities are often broadly opposed to the development of new, homeless-serving housing facilities; increases in neighborhood crime and disorder are regularly attributed to these openings (Galster, 2002; Harter, 2024; McGahan, 2025; Paige, 2024; Seedorff, 2026; Westside Current Staff, 2026). However, this opposition is not based on empirical research as credible evidence on the causal effects of PSH on local community crime rates has not yet been established.

This study provides new evidence on how PSH facilities affect local crime using data from Los Angeles County, the largest county in the U.S. and home to 9% of the nation's homeless population in 2025 despite representing only 3% of the U.S. population (U.S. Census Bureau, n.d.; U.S. Department of Housing and Urban Development, 2026b). Our approach uses the substantial expansion of the number of these facilities in Los Angeles County from 2010 to 2024 as a natural experiment. We combine administrative crime data with housing data from

state and local government sources in a staggered difference-in-differences design to compare changes in crime in the area surrounding newly opened PSH facilities with contemporaneous changes in crime in areas where PSH facilities will open later in the study period. We estimate precise null effects of PSH openings on violent and property crime, ruling out crime increases of greater than 5 percent across multiple variations on our main model specification. Importantly, however, we cannot rule out associated increases in local disorder or drug offenses that can shape public perceptions of safety. Our estimates for drug and disorder offenses are highly imprecise, a limitation that likely comes from the fact that records of these offenses became exceedingly rare during the study period. These are offense types that often require police enforcement to get recorded, and it is possible policing priorities shifted during this period away from their enforcement.

Our study contributes to the literature in multiple ways. We build on the existing literature on the effects of affordable housing development on crime (MacDonald and Stokes, 2020) by focusing specifically on PSH, an important and growing subset of publicly subsidized affordable housing. We contribute to a small existing literature on the effects of homeless-serving housing facilities on crime, most of which has focused on temporary homeless shelters (Faraji et al., 2018) or a broad "shelter" designation that combines a heterogeneous mix of housing facilities (Christopher et al., 2025; Cohen, 2019; Voss, 2025). In addition to overcoming data challenges that prevented previous research from distinguishing between different types of homeless-serving facilities, our identification strategy employs a careful approach to account for the potential confounding influence of spatial and temporal overlap among these facilities. While recent studies in this literature have employed novel estimators to account for potential heterogeneous effects of affordable or homeless housing over time, less attention has been paid to potential contamination bias related to spatial overlap in where new developments are situated.

Our study also advances research by creating what we believe is the single most accurate database of PSH sites available. Constructing this dataset required merging multiple raw data sources, and cleaning and validating the locations, sizes, and opening dates of scores of PSH facilities using media accounts, Google StreetView, and other web resources. By making this dataset publicly available, we hope to support future research studies on the effects of PSH development.

Finally, our findings can help to address key concerns noted in debates about the expansion of PSH. Between 2018 and 2023, California allocated nearly $21 billion towards addressing homelessness, but this spending has taken place in an environment with little focus on evidence-based decision-making (Kendall, 2023; Parks 2024). Understanding the potential implications of siting new PSH facilities on crime and public safety can contribute to a more transparent and accountable policymaking process.

The next section provides conceptual background, findings from existing literature on the effects of affordable housing on neighborhood crime, and details on the context of homelessness and PSH in Los Angeles County. In the third section, we describe our data and empirical strategy. The fourth section presents our results, and the final section concludes with discussion of implications for research and public policy.

2. Background and Study Setting

2.1. Affordable Housing and Neighborhood Crime

A well-established literature has shown that the built environment influences crime. Crime concentrates in specific places (Sherman, 1995; Weisburd, 2015) and specific types of locations can be crime attractors or repellants (Eck et al., 2007; Kennedy et al., 2016; Muggy et al., 2024). Unlike many other features of the built environment, the effect of affordable housing developments on neighborhood crime is a function not only of the characteristics of the physical structures but of how the new developments change residential composition. Affordable housing sites that spatially concentrate individuals at elevated risk of crime involvement may have different effects on local crime rates than housing sites that replace disamenities or encourage in-migration of higher socioeconomic status residents. Thus, studies showing that public housing demolitions reduced crime levels in nearby neighborhoods (Aliprantis and Hartley, 2015; Cloud and Roll, 2011; Sandler, 2017) may have limited generalizability to the subsidized privately owned developments that now account for the majority of federally assisted rental housing (McClure and Johnson, 2014).

Indeed, studies assessing the neighborhood crime effects of affordable housing developments more recently tend to find public safety benefits. This evidence base most commonly examines the effects of developments incentivized through the Low-Income Housing Tax Credit (LIHTC) program.[1] Freedman and Owens (2011) studied the county-level impacts of LIHTC on crime using changes over time in the formula used to determine eligibility for additional tax credits to developers. Based on their instrumental variables approach, Freedman and Owens (2011) found that housing development incentivized by LIHTC had little effect on property crime and generated small (2%) but significant reductions in robbery and aggravated assault. Comparing crime trends in neighborhoods with versus without new LIHTC developments in Austin, Texas, Woo and Joh (2015) found that LIHTC developments were placed in areas where crime was already high and may reduce subsequent crime. Combining a spatial difference-in-differences approach with crime data from Chicago, San Francisco, and San Diego, Diamond and McQuade (2019) found that LIHTC developments reduced crime in low-income neighborhoods and did not increase crime in high-income neighborhoods.[2]

Consistent findings from the LIHTC literature indicate that subsidized affordable housing development does not inherently increase crime, but the relevance of these findings to questions about the local crime impacts of PSH are unclear given potential differences in resident characteristics. LIHTC properties are selected via a competitive allocation process, with scoring priorities including lower income targeting, developer experience, site proximity to amenities,

[1] The provision of rental vouchers, most notably HUD's Section 8 Housing Choice Voucher program, is another housing policy that has been studied in relation to crime (e.g., Jacob et al., 2014; Kling et al, 2005). Little of this research has focused on neighborhoods, though Ellen and colleagues (2012) found no evidence that increases in voucher use increased crime in neighborhoods. Mast and Wilson's (2013) study of Charlotte, North Carolina found significant crime increases specifically in areas with high concentrations of vouchers.

[2] The vast majority of PSH projects in our data are, strictly speaking, LIHTC projects as well. The three studies discussed above appear to include PSH projects in their samples, as they do not mention excluding them, and Freedman and Owens (2011) mention homeless individuals as a special population type in their LIHTC project data. However, PSH is a small subset of LIHTC housing, comprising perhaps 5 percent at most among all projects in a national database of LIHTC projects (author estimate using the affordable housing data described in Section 3.1.1).

and development in opportunity neighborhoods (Owens and Smith, 2023; Reid, 2019). Compared to all recipients of federally subsidized housing, LIHTC residents tend to have higher incomes and lower rates of disability (O'Regan and Horn, 2013).

Clients served by PSH tend to have high rates of substance use and mental health disorders, and the "low barrier" model that characterizes most PSH means that rates of substance use and overdose among residents is also high (Bagalman and Perl, 2015; Fleury and Armoon, 2024; Henwood, Redline and Lahey, 2020; Petersky and Banta-Green, 2024). PSH also house extremely low-income residents, often targeting households with incomes 30% or less of area median income (whereas other forms of housing assistance, such as HUD housing vouchers, generally use higher thresholds; Los Angeles County Development Authority, undated). Thus, these facilities may have different effects on crime and disorder than the average LIHTC development.

Mixed findings for the effects of providing housing for PEH on neighborhood crime come primarily from studies of temporary shelters intended for short-term residence. An early study of emergency winter shelter openings in Vancouver, Faraji, Ridgeway, and Wu (2018) found that shelters produced large and highly localized increases in thefts and vandalism, effects that were partially offset by lower rates of breaking and entering in commercial establishments; the study could not assess effects on violent crime. However, another study of the seasonal winter shelters program in Los Angeles found that temporary shelters significantly reduced crime, with favored estimates indicating that 100 additional shelter beds prevented 1 crime per day (Christopher et al., 2025). Two other studies examining different cities and employing different identification strategies did not find statistically significant effects of temporary shelters on crime (Camero, 2025; Wilking et al., 2025). Variable findings across this literature may reflect different settings, different identification strategies, or evaluation of different geographic units of analysis that imply different assumptions about spatial spillovers.

Permanent supportive housing (PSH) shares elements of both affordable housing developments—in that they are intended for long term residency—and homeless shelters, in that they are targeted toward PEH. Permanent supportive housing also bundles housing with mental health treatment, substance use counseling, and case management, services intended to help address the underlying conditions prevalent among PEH that may increase propensity for offending or victimization. PSH is an increasingly common policy, particularly in places like California with high prevalence of chronic homelessness and high-cost housing (Eide, 2025; Urban Institute, 2026). However, while studies have found significant benefits of PSH for improving housing stability and reducing criminal justice involvement among clients placed in the housing program (Cohen, 2024; Hunter et al., 2022), little research has examined whether PSH impacts neighborhood crime.

The three studies closest to ours are working papers that use data from Los Angeles to evaluate the effects of "shelters" pooling PSH with other single-site housing for PEH (i.e., emergency shelters, transitional housing, rapid re-housing, and save haven), on local crime rates. Cohen (2019) used a shift-share instrumental variables design to show that communities with greater exposure to shelters from 2015 to 2019 experienced declines in street homelessness and overall crime. Based on the same broad "shelter" operationalization, Voss (2025) instead found that, compared to areas 0.2 to 0.4 miles from a shelter, areas within 0.2 miles of a shelter opening had significantly higher rates of drug arrests but similar rates of property and violent crimes. Extending the analysis period to 2024 and comparing crime rate trends in areas around a shelter opening to trends in areas around a shelter that opened later, Kevane et al. (2026) found some

evidence that shelter openings increased property crime and violent crime in Los Angeles, but results were not significant across alternative specifications.[3]

Mixed findings from these three studies may derive in part from their pooling together of heterogeneous homeless-housing facilities. The stability and substantial investment in PSH may lead to neighborhood improvements that deter or displace crime, a theory suggested by studies finding that PSH developments increase local (i.e., within 500 feet) real estate values (Furman et al., 2008; Sitti et al., 2025) but other shelters do not (Sitti et al., 2025). Conversely, if PSH developments draw in individuals with serious mental or behavioral health conditions that may make them at greater risk of victimization or offending, crime rates would be expected to increase. The fact that PSH facilities often offer case management and other services may attenuate these risks, although practitioners have noted that on-site clinical and social service resources for PSH clients have not kept pace with growing needs (Abramson et al., 2024). Overall, given multiple theories point to PSH having different effects on crime than other shelters, the existing literature does not address questions about the neighborhood public safety impacts of one of the most rapidly growing models for homeless housing.

2.2. Housing and Homelessness in Los Angeles

For years, Los Angeles has been referred to as the "capital" or the "epicenter" of homelessness in the United States (Gorman, 2019; Paterson, 2021; Walters, 2025). The city's Skid Row neighborhood has been a local destination for PEH since the late 1800s (Sheeley et al., 2021). The Great Depression led to a surge in homelessness that was substantially alleviated by rapid economic growth and a housing boom in the post-WWII period (Sheeley et al., 2021). The modern growth of homelessness since the 1960s has been attributed to factors including deinstitutionalization, the collapse of manufacturing jobs for lower-paid service work, the large-scale demolition of single-room-occupancy hotels as part of various urban renewal efforts, and the crack cocaine epidemic (Sheeley et al., 2021).

In recent decades, estimates of homelessness in Los Angeles County have risen from a 2009 low of just over 30,000 people to more than 75,000 by 2023, a 150% increase in 15 years (Flaming and Burns, 2017; Smith and Vives, 2023).[4] By 2026, the estimated population had declined slightly to 73,000. Roughly 70 percent of the county's homeless population live unsheltered (Smith and Vives, 2023), making Los Angeles home to the largest unsheltered population in the country (U.S. Department of Housing and Urban Development, 2026b). Spending on homelessness has risen proportionally with this rise in the homeless population. For example, spending by Los Angeles County rose from $258 million in 2018 ($330 million in 2026 dollars) to $843 million in 2026 (County of Los Angeles, 2017; LA County Homeless Services & Housing, undated).[5] The City of Los Angeles' spending also rose substantially, from $430 million in 2018 ($550 million in 2026) to $750 million in 2026 (LA City Controller, undated; Smith, 2018).

[3] Kevane et al. (2026) conducted a similar exercise for 12 other cities in California. Results varied but were largely statistically insignificant.

[4] The earliest "point in time" counts conducted in Los Angeles County, in 2005 and 2007 led to estimates of as many as 90,000 PEH in the county but a roughly 40% decline between 2007 and 2009 have led to questions about the accuracy of these early counts (Corinth, 2015; Economic Roundtable, 2017).

[5] This and the subsequent adjustment use a chained personal consumption expenditures price index.

Much of this spending has been used to provide funding support for new PSH facilities, including a specific funding program passed by City of Los Angeles voters in 2016, Proposition HHH, that raised $1.2 billion in seed funding aimed at producing up to 10,000 units of PSH. This large, local funding program, combined with LIHTC credits and other state and local programs, led to the development of a substantial pipeline of new PSH in Los Angeles. In the City of Los Angeles, this process was accelerated with key legal reforms that substantially reduced available avenues to block or delay these projects. These included two state bills, one that effectively ended the ability of City of Los Angeles councilmembers to veto projects in their districts (Reyes, 2018), and another that exempted from review under the California Environmental Quality Act (CEQA) any homeless housing facility in Los Angeles that receives state or local funding from several specified sources, including Prop HHH (Reyes & Dillon, 2019). Additionally, Los Angeles passed a PSH ordinance in 2018 that dramatically reduced parking requirements and raised the threshold for a project triggering additional plan review by the city from 50 to 120 units (and higher in the downtown area), among other reforms (City of Los Angeles, 2018).

While this influx of PSH projects comprises buildings of many forms—from small, low-rise buildings of fewer than 25 units, to projects adaptively reusing motels and historical buildings, to 19-story high rises in Skid Row—the modal PSH building in Los Angeles is a 5- or 6-story building featuring a concrete first floor structure with 4 to 5 stories of wood-framed construction above it (Ward, 2024). Commonly called "podium" or "5-Over-1" construction, these buildings have become a ubiquitous form of infill housing in most high-cost metros. One estimate suggests that as much as two thirds of all new multifamily housing deliveries in major metro areas between 2020 and 2025 were podium construction (The McFadden-Finch Holdings Company, undated). Importantly, this means that, from a physical perspective, PSH is generally not clearly distinguishable from a typical new apartment building except that, in many cases, these buildings feature more architecturally-forward (even regularly award-winning) designs (Affordable Housing Liaison Unit, 2023). This suggests that the structures themselves would generally be considered positive amenities from a purely aesthetic perspective at least as much as a typical market rate apartment building.

Residents are placed into most PSH in Los Angeles through a countywide "coordinated entry system" (CES). Obtaining a placement involves registering with the system, being evaluated through an assessment tool aimed at quantifying various measures of acuity, need for intensive services, and membership in categories that affect eligibility for different types of housing projects (LAHSA, 2026). Assessment criteria prioritize PSH placement to those that have a history of unstable housing and co-occurring vulnerabilities (e.g., chronic physical or mental health issues, substance use, problems with daily functioning, legal issues) that impact their safety, health, and long-term housing stability (Community Solutions & OrgCode Consulting, 2015; LAHSA, 2023a; LAHSA, 2023b). Because of the relatively stringent requirements for qualification and the accompanying need to collect key documents that are seldom readily available among the chronically homeless population, the lease-up period for newly opened PSH can be quite slow. For most of the period of our analysis, potential residents were matched to open PSH units one by one and it was not uncommon for a monthslong process of qualification to end with a case worker losing track of a client and for the entire process to start over again.[6] One experienced PSH developer estimated that a typical lease up rate in Los

[6] Since roughly 2023, "batch matching" or qualifying multiple potential residents at the same time, has been adopted more broadly (Abramson et al., 2024).

Angeles through the CES process might be 8 to 10 units per month.[7] So, for example, a typical 50- to 60-unit building could take half a year to lease up fully. This timing is important to bear in mind when we consider over what time period we might expect any effects of new PSH on crime to manifest.

In summary, the funding and regulatory changes in the mid- and late-2010s ushered in a major influx of PSH, roughly doubling the stock across the county from approximately 7,545 units in 130 facilities in 2016, when Proposition HHH was passed, to nearly 16,000 units in roughly 283 facilities in 2024. Much of this expansion in PSH supply took place in areas that had not previously had any PSH facilities. Between 2016 and 2024, the number of census tracts with at least one PSH facility more than doubled.[8] This substantial growth in both the number of PSH facilities and the local areas in which they were built provide the setting for our study.

3. Methods

3.1. Data

3.1.1. Housing data

We constructed our database of PSH facilities using three primary sources. The first source is data for the years 2015 through 2024 from the annual "Housing Inventory Count" (HIC) that is produced by the Los Angeles Homeless Services Authority (LAHSA), an intergovernmental agency that is the designated Los Angeles County "Continuum of Care" (CoC). LAHSA is responsible for administering several key aspects of homelessness services and strategy in the region (HUD Exchange, 2014), including allocating HUD and other state and local financial resources and overseeing the annual "point-in-time" (PIT) count of homelessness in the county. The HIC dataset is one of the annual deliverables from the PIT count.[9] Over time, these data have varied meaningfully in quality, a challenge we discuss in more detail below.

Given variation in the quality of the HIC data over time, we supplement these data with other sources. First, we use two files produced by Los Angeles Housing Department (LAHD) data capture information on the city's permanent supportive housing funding pipeline, tracking PSH projects that received funding via Proposition HHH and PSH projects using funding from other city-affiliated funding sources. Second, we use a statewide database of LIHTC projects—which include both PSH and non-PSH projects—produced by the California Tax Credit Allocation Committee (CTCAC), the agency that administers the state's federal LIHTC credits.

We developed our database by combining and cleaning these three primary sources, a process that first involved deduplicating the data and then identifying and excluding scattered

[7] Author discussions with an experienced affordable housing and PSH developer in 2024. This same expert suggested that a typical lease up time for a similarly sized affordable housing development was roughly 50 to 60 units per month, or about 6 times as fast.

[8] This figure and the growth in total PSH facilities are author calculations from our PSH database.

[9] We note that Long Beach and Pasadena, two municipalities in Los Angeles County, have distinct CoCs not administered by LAHSA. Also, LAHSA is currently undergoing substantial changes after Los Angeles County decided to pull county resources from LAHSA's control and establish its own homelessness services agency, a move that has led the City of Los Angeles to seek substantial control over the agency (Schrank, 2026).

site programs (many are listed in the HIC and commonly use the main address of the organization administering the program), as well as any interim or shelter facilities.[10] In this process, the LAHD data, when available, took precedence over data in the HIC files due to a much higher typical level of quality, completeness, and accuracy. Our complete data set contained information on the following characteristics: unique facility name and identifier, address (used to geocode locations), the number of units, and "placed in service" (project opening) date. This initial dataset was made up of 454 candidate PSH sites.

Our data quality hierarchy used, first, available data on address, units and opening dates from the LAHD sources as this was a relatively high-quality, high-visibility data set that has been regularly updated—particularly with dates that projects area completed and opened—over time. The second preferred data source was the CTCAC database of LIHTC-funded projects. We relied particularly on the placed in service date of a building, as this is a key data point that relates to how soon the actual delivery of awarded tax credits can take place (CTCAC, undated). These two sources covered the majority of our candidate projects, but a substantial number of projects that were drawn from the HIC, particularly older projects, were not readily matchable to these sources leaving missing data we further addressed.

We worked through missing data fields one facility at a time to obtain accurate data on the number of units in each and the opening date. Specifically, we conducted manual web searches for press releases or news coverage of these projects to identify reports of addresses, size, and opening events, as well as features on the facilities from the developer or, in some cases, architects or contractors who worked on the projects. In some cases, these searches allowed us to successfully link projects to the CTCAC data or to deduplicate projects that were present in the LAHD data. Information from these sources on address, building characteristics, and completion date was, in certain cases where we relied on developer websites and media reports for opening dates, we also validated a candidate date by manual inspection of Google Street View photographs of the project to observe visual evidence of construction occurring and concluding. Street View was also used to validate the removal of some suspected scattered site projects (by, for example, identifying a location as a homeless service provider's office rather than a PSH facility). The resulting cleaned PSH database has 298 facilities, with placed in service dates ranging from 1982 to 2025.

We prepared an additional database of non-PSH affordable housing to use in alternative models aimed at assessing whether substituting traditional low-income LIHTC housing developments into our modeling framework produced similar or different results than our PSH analyses. These data were drawn from the same LIHTC database produced by CTCAC, which were subsequently geocoded using the provided addresses. This dataset includes 88 projects that had not opened by the end of 2024, which are eligible to serve as control cases. The cleaned, non-PSH affordable housing database has 864 facilities, with placed in service dates ranging from 1992 to a projected opening in 2028.

[10] We also excluded sites affiliated with the State of California's "Project Homekey" funding program, which provided multiple rounds of grants to affordable housing developers, beginning during the COVID-19 pandemic, for purchasing underutilized hotel/motel properties to be used, first, as temporary shelters during the pandemic, then later permanently converted into PSH. Later Homekey rounds also supported new construction PSH and other types of adaptive reuse projects. In the end, we did not include any Homekey projects in our analytic sample because, for early hotel/motel acquisitions, the prior use as a shelter was not confluent with our identification strategy, and none of the later ground-up new construction projects were completed prior to our minimum post-period cutoff.

3.1.2. Crime data

Crime data come from the Los Angeles Police Department (LAPD) and the Los Angeles County Sheriff's Department (LASD). The LAPD has jurisdiction over the City of Los Angeles, while LASD has jurisdiction over the entire county but is the primary agency in 42 of the 87 municipalities outside of Los Angeles City (e.g., West Hollywood), as well as in unincorporated areas of the county. Geocoded crime data are available from 2010-2024 for both agencies, with locations rounded by the agencies to the hundred block (e.g., a crime at 317 South Broadway is given a location of 300 South Broadway). We measure neighborhood crime as the number of offenses that occurred per quarter in each 500-meter PSH buffer, distinguishing between four offense types. These are violence (assault, robbery, and homicide), property crime (burglary, theft, motor vehicle theft, and vandalism), drug offenses (any narcotic drug law violation), and disorder offenses (disorderly conduct, disturbing the peace, vagrancy, and public intoxication). The LAPD crime dataset contains virtually no drug or disorder offenses, but there are many such offenses recorded in LAPD arrest data. As such, we measure LAPD drug and disorder offenses with arrest rather than reported crime data.[11] In 2024, the LAPD transitioned crime recording systems, as the agency began reporting to the National Incident-Based Reporting System (NIBRS). We cleaned the NIBRS-style data to be consistent with the earlier LAPD crime data (e.g., by applying the hierarchy rule), and combined data across crime recording systems.

3.1.2. Other data sources

We use several other data sources in sensitivity analyses and to evaluate whether PSH development affects crime differentially in neighborhoods with different characteristics. In one such analysis, restricted to the City of Los Angeles, we draw on 2024 zoning data from Los Angeles City Planning to measure land use, defined as the percentage of the buffers surrounding PSH facilities that are zoned for specific purposes, focusing on low-density residential zoning. We do not use time-varying zoning data, since it is highly stable over time. In another analysis, we characterize neighborhoods using tract-level American Community Survey (ACS) five-year data (2006-2010, which pre-dates all facility openings), focusing on median household income. We also use the land use and ACS data to descriptively characterize areas where facilities are located, for instance, in terms of family poverty rates, racial composition, and commercial zoning.

3.1.3. Sample construction and coverage

Using these data sources, we constructed a PSH facility-quarterly panel dataset that spans from Q1 2010 through Q3 2024. Of the 298 PSH facilities in Los Angeles County in the cleaned database, 27 were excluded from analysis because they opened outside jurisdictions for which we have crime data, 12 were older projects excluded due to missing opening date information (they opened at least prior to 2015 based on the data source and most likely opened prior to 2000 based on our ability to track down other opening dates), and 63 were excluded because they opened before 2011 (this criterion ensures each PSH facility has at least one year of pre-opening

[11] As we show, results are robust when not combining LAPD arrest-based measures with LASD drug and disorder crime data. Still, these measures might not fully capture drug or disorder related offending, a limitation we return to.

data since the crime data go back to 2010). The remaining 196 PSH facilities were eligible for inclusion in the study given when and where they opened.

In the main analysis, we focus on crime that occurred within a 500-meter buffer around PSH facilities (i.e., a circle with a 500-meter radius).[12] Several of the 196 PSH facilities have overlapping 500-meter buffers; to account for this, we only use PSH facilities if no other facility had previously opened in the area covered by their buffer.[13] This resulted in a final analytical sample of 100 PSH facilities. Appendix A provides further details on the excluded facilities.

Due to this restriction on overlapping buffers, pre-opening periods are not contaminated by earlier PSH facility openings, although the treatment effects of a given facility may include the effects of subsequent PSH openings in the same area. Conceptually, these restrictions mean that our estimates do not answer: what is the average effect of opening any PSH in Los Angeles? Rather, they answer: what is the average effect of opening a PSH in Los Angeles in an area where one does not already exist? In addition to being easier to answer with our research design, this is arguably the more policy-relevant question.

All pre-opening quarters are included in the panel, but post-PSH observations were trimmed to a maximum of 20 post-opening quarters (5 years). This decision, revisited in a robustness check, reflects that many PSH facilities have not been open long enough to support estimation of longer-term treatment effects with precision. The panel contains 99 PSH facilities that opened during the study period, which serve as control cases before they open and treated cases afterwards, and one PSH facility that opened in Q4 2024 and which only serves as a control case (the reason the panel goes through Q3 2024 is that there are no eligible control cases for this last PSH facility due to the end point of our crime data). In total, the PSH panel contains 5,046 quarter-facility observations.

We followed an analogous procedure to derive the sample of non-PSH affordable housing facilities. A minor difference is that this panel includes Q4 2024 owing to the availability of control cases that opened later. Of the 864 affordable housing facilities in the cleaned database, 400 opened during the study period in jurisdictions where the LAPD or LASD has primary jurisdiction. Of those, 256 were excluded due to spatial overlap with pre-existing facilities, which yielded a final affordable housing sample of 144 facilities, contributing 7,205 quarter-facility observations.

3.2. Empirical Strategy

We estimate the effect of opening PSH facilities on nearby crime levels by comparing areas where those facilities opened (i.e., treated areas) to control areas that had not yet had PSH facilities open, but had them open later. This strategy is inspired by Asquith et al. (2023), who studied the effects of opening apartments in low-income areas on rents and in-migration in part by comparing the treated areas around apartments that opened to control areas where apartments opened at a later date.

We employ the staggered difference-in-differences estimator developed by Callaway and Sant'Anna (2021). This approach is similar to conventional two-way fixed effects estimation but accounts for the fact that staggered treatment adoption—as exists in our context given that PSH

[12] To give a sense of scale, it takes about 7 minutes to walk 500 meters at normal speeds.

[13] This step compared the 182 PSH facilities to each other but also examined whether there was a facility that opened in that area within the 60 preceding months, meaning that PSH openings going back to 2006 were considered.

facilities opened throughout the study period—can bias effect estimates in the presence of treatment effect heterogeneity. The method addresses the potential bias of two-way fixed effect estimation in this context by constructing group-time comparisons where there are clean controls, that is, explicitly comparing treated groups to an appropriate set of contemporaneous control units. The method allows for not-yet-treated units to serve as controls, which is central to our analytic strategy.

The average treatment effect on the treated (ATT) in quarter *t* for a group of PSH facilities that opened in quarter *g* is given by:

$$ATT(g,t) = E\left[Y_t - Y_{g-1} \mid G = g\right] - E\left[Y_t - Y_{g-1} \mid D_t = 0, G \neq g\right], \quad (1)$$

where Y indicates quarterly crime counts (we fit the model separately for different offense types), D is a binary variable indicating whether a unit has been treated by quarter *t*, and $t \geq g$. We do not condition on additional covariates in our main specification. We aggregate the group-time ATTs in two ways: as the overall average treatment effect across groups and time periods, and as an event study showing dynamic treatment effects relative to when PSH facilities opened (Callaway and Sant'Anna, 2021). Confidence intervals were estimated through bootstrapping with 1,000 replications.

The three key identifying assumptions are no anticipation, parallel trends, and the stable unit treatment value assumption (SUTVA). Anticipation in this context would mean that PSH facilities affect the outcome prior to their opening (which is how we define treatment). This seems like a potentially plausible concern given that the new construction or renovations preceding opening may affect crime in the area (for example, theft from an active construction site), so in supplementary analyses we relax this assumption to allow for anticipation in the 8 quarters preceding facilities' opening.[14]

The parallel trends assumption is likely to be satisfied on theoretical grounds: developers do not select directly on crime trends, and insofar as certain areas are more likely to get PSH facilities, those factors should apply equally to our treated and control groups, meaning they are unlikely to lead to a violation of the parallel trends assumption. However, if the factors influencing developer decisions vary over time (e.g., if, over time, developers become more reluctant to site PSH in places they believe will experience crime increases), this could lead to a parallel trends violation. As partial tests of the parallel trends assumption, we empirically examine whether trends were parallel in the pre-opening periods and show that PSH that opened at different times varied little in terms of the types of areas in which they were situated.

SUTVA requires that the outcomes of facilities depend only on their treatment status, not on those of other facilities. In our context, this means that we assume no spillover effects, specifically, that the crime around a given PSH facility in our sample is not affected by other PSH facilities. This assumption is a key reason that our panel is built only with facilities that do not have other facilities that have already opened within 500 meters. With that restriction, we guard against using as controls areas that already have PSH facilities affecting their crime outcomes, and against misestimating treatment effects for opened PSH units by not modeling spillover effects.

In addition to descriptive information and the main results, we report on three further analyses. In one, we examine heterogeneity in crime effects by the type of area in which PSH

[14] Approximately two years is the modal time period for the active construction phase of a mid-sized, "podium" style apartment project typical of Los Angeles PSH developments. See Ward (2024) for estimated times from funding award to completion for a large sample of Proposition HHH-funded projects. This timing is also consistent with what we observed when tracing back Google Streets images for each project (see Section 3.1.1).

facilities are located. Specifically, we stratify the PSH sample by whether it is in an area that is above the median in terms of low-density residential zoning, baseline (2010) crime levels, and median household family incomes, estimating the same model on these six subsets of the PSH facilities. These heterogeneity analyses were preregistered and are motivated by the possibility that PSH effects on crime might vary according to the types of neighborhoods in which they are situated, as was found by Diamond and McQuade (2019)'s analysis of LIHTC developments; specifically, we test the possibility that PSH openings produce differential impacts when built in areas with low crime, high incomes, or that are mostly single-family homes. Next, we use the dataset on affordable housing openings in Los Angeles County (excluding PSH facilities) in a model that is analogous to the main model. This allows us to examine whether our findings are unique to PSH facilities or hold for affordable housing developments more generally. Finally, we summarize a wide range of robustness checks and supplementary analyses.

3.3. Pre-registration

This study was pre-registered through the Open Science Foundation (https://osf.io/wjkxa/overview). Notably, what the pre-registration described as the main approach (a form of spatial difference-in-differences estimation) is a supplementary analysis (Appendix B), whereas our main specification was described as the secondary analysis in the pre-registration. The decision to reverse these was made because the staggered difference-in-differences approach gives more precise estimates, although both approaches lead to the same qualitative conclusions. See Appendix B for results and further details on minor deviations from the pre-registration.

4. Results

4.1. Descriptive information

Figure 1 maps the 100 PSH facilities included in our analytical sample as blue circles and the 96 facilities that were eligible for inclusion given when and where they opened, but which are excluded because a PSH facility had already opened nearby, as orange triangles. Eligible PSH opened between 2011 and 2024 in areas where the LAPD or LASD is the primary law enforcement agency (shaded green in Figure 1). The included PSH facilities are distributed quite widely across Los Angeles County, whereas the excluded PSH are more geographically concentrated (as expected). Of the 96 excluded PSH facilities, 15 are in Skid Row and 32 in neighborhoods surrounding MacArthur Park, two parts of Los Angeles that are major hot spots for both homelessness and crime (Newton, 2025; Lopez, 2024; Landsberg & Holland, 2025; Ward, 2021). Together, these account for 49% of the excluded facilities (see Appendix A for further details on these exclusions).

[Figure 1]

Table 1 contains key descriptive statistics for the included and excluded PSH facilities. This includes the number of restricted units (i.e., units designated for PSH residents, which is

typically equivalent to the size of the facility minus one or two units designated for property management), the proportion of facilities within the City of Los Angeles (and thus policed primarily by the LAPD), several tract characteristics as measured using 2006-2010 ACS data, and land use data (which is available for PSH within the City of Los Angeles only). There are some notable differences between the included and excluded PSH facilities, including a higher share of excluded facilities being in the City of Los Angeles (97% versus 86%), median household income that is about one-quarter lower, less of the surrounding area being low-density residential (19% for excluded versus 35% for included), and much higher baseline crime rates for excluded PSH facilities.

[Table 1]

Table 2 shows descriptive statistics for the included PSH facilities, focusing on the time-varying crime outcomes, and is by spilt pre- versus post-facility opening. There is pre-opening information on all 100 units and post-opening information on the 99 that opened by Q3 2024. Violent, property, and disorder offenses were higher after PSH facilities opened, for example, with an average of 25.4 violent crimes per quarter near PSH facilities after they open compared to 20.6 in the same areas before the facilities opened. See Appendix C for further descriptive information on the 100 PSH facilities that comprise the analytic sample, including a visualization of the panel structure, information characterizing the areas surrounding the PSH facilities, and descriptive evidence on how the characteristics of the neighborhoods with PSH openings varied (or did not vary) over time. Appendix C also contains analogous descriptive information about the affordable housing units included in our sample.

[Table 2]

4.2. Main results

Figure 2 presents the dynamic treatment effects of opening PSH facilities on crime, separately by offense type. In each panel, the quarter relative to facility opening is on the x-axis, whereas effect estimates are on the y-axis on the observed (crime level) scale. Figure 2A focuses on violent offenses; that periods after PSH facilities opened are concentrated near zero (the blue points), with all their confidence intervals overlapping zero (the blue lines), indicates that opening PSH facilities does not affect nearby violent crime levels. The pre-treatment periods (red points) in Figure 2A cluster around zero, indicating that treated and control units had violent crime outcomes that were trending in parallel before PSH facilities opened, in support of the parallel trends assumption.[15]

[Figure 2]

[15] We follow guidance by Callaway (2021) and use varying base periods in Figure 2, meaning that each pre-period is compared to the immediately following period, which allows us to assess the no anticipation assumption more directly but also provides insights on the parallel trends assumption. However, our pre-period estimates also appear parallel when fixing a universal base period (e.g., quarter before opening). Note that, because our estimator does not require parallel trends for all groups and time periods, the larger pre-treatment confidence intervals well before treatment occurred are not a concern (Roth et al., 2023).

Table 3 presents the results for the overall ATT, in both level and percentage terms. For violent offenses, the estimated effect size is -0.54 violent offenses per quarter after PSH facilities opened (95% confidence interval [CI]: -2.14, 1.05). In percentage terms relative to the counterfactual, this corresponds to a 2.23% reduction in violence caused by opening PSH facilities (CI: -8.81%, 4.34%). The overall ATT for violent crimes is not statistically significant ($p > 0.05$) and is precisely estimated, as indicated by the small confidence intervals.

[Table 3]

The findings are similar for property crime. Specifically, the dynamic treatment effects shown in Figure 2B are relatively precise, concentrated near zero, and statistically insignificant ($p > 0.05$), with the pre-treatment periods supporting the plausibility of the parallel trends assumption. In percentage terms relative to the counterfactual, Table 3 shows a 3.80% reduction in property crime caused by opening PSH facilities (CI: -11.06%, 3.46%), which is not statistically significant ($p > 0.05$).

For drug and disorder offenses (which are primarily measured using arrest data), Figure 2C and Figure 2D, respectively, show that the effects of opening PSH facilities are, yet again, null ($p > 0.05$). The point estimates concentrate close to zero and the pre-opening periods supporting the parallel trends assumption. However, unlike the earlier offense types, these are highly imprecise null results, given the size of the confidence intervals relative to the levels of these offense types. For instance, Table 3 indicates a 9.68% increase in drug crime, with the 95% confidence interval spanning from a 17.92% decrease to a 37.29% increase. The disorder effect estimates are even less precise: the point estimate is a 5.37% reduction in these offenses, but the confidence intervals span a 122.43% reduction to a 111.68% increase. A very wide range of effect sizes in relative terms are plausible given these estimates, but that is predominantly due to the very low levels of crime reports for these types of offenses (as Table 2 shows, in the areas surrounding PSH facilities before they opened there were, on average, 5 or 6 of these offense types recorded per quarter).

4.3. Differential Effects by Baseline Crime, Land Use, and Neighborhood Income

Figure 3 presents results testing for differential effects across PSH facilities situated in different areas. Each column corresponds to one of the four outcomes and each row to a split in the PSH sample according to whether it was below or above the median in some characteristic. Owing to the large number of estimates, we present these results visually in overall ATT level terms only (i.e., the average quarterly effect of opening PSH on crime levels). The top row splits by the median of 2010 total crime levels (the four crime outcomes combined), separating PSH facilities situated in lower versus higher crime areas using only pre-PSH opening data. While all the estimates are null ($p > 0.05$), for violent and property crime the point estimates are higher for PSH facilities situated in lower crime areas. This is consistent with the possibility that PSH situated in higher-crime areas reduce nearby crime, but they increase crime in lower-crime areas. However, these point estimates are small, the confidence intervals overlap, and all the results are statistically insignificant.

[Figure 3]

Second, we split the sample by whether it was above the median in terms of the percentage of the surrounding area zoned for low-density residential (i.e., single-family) purposes. Since we only have zoning data for the City of Los Angeles, we focus on the 86 PSH facilities located within the city and only use LAPD data. PSH in the high degree of low-residential housing split were situated in areas that were largely used for single-family housing (on average 49.1% of area around these facilities, including streets and sidewalks, was zoned for single-family housing) whereas those below the median are a combination of single-family housing (20.5% on average), high-density residential (e.g., apartment buildings), commercial, and other uses like schools and parks. As shown in the second row of Figure 3, there is limited heterogeneity of estimates across the sample when split in this way. All the estimates are null ($p > 0.05$) with point estimates close to zero. That said, the point estimate for violent crime is slightly lower, and the property and drug offense estimates slightly higher, in areas made up of a larger share of single-family homes.

Finally, the bottom row of Figure 3 presents the results when splitting by the 2010 median household income of the tract where the PSH is located ($37,469), to test whether effects differ across neighborhoods of different socioeconomic status. With this analysis, too, the results are consistently null ($p > 0.05$) across the income split. The disorder estimates across all of these splits are very imprecise. Taken together, there is no evidence of strongly differing effects of PSH when split by surrounding residential land use, neighborhood income, or baseline crime levels. Estimates are all null and, as in the main analysis, these null estimates are quite precise for violent and property crime.

4.4. Comparison with Other Affordable Housing

In order to examine whether PSH produce markedly different crime effects than other forms of affordable housing, we fit models that are analogous to the overall PSH models, but which uses affordable housing facility data. Table 4 presents the overall ATT for affordable housing in count and percentage terms. For all four individual offense types and the total crime measure, the effect of opening affordable housing on crime is not statistically significant ($p > 0.05$). As with the analogous PSH estimates (Table 3), the estimates are precise and null for violent and property crime. Notably, and in contrast to the PSH findings, the property crime confidence intervals are mostly positive (point estimate of 5.34%, CI: -1.48%, 12.16%). This may be indicative of a real effect that we are underpowered to detect, though if so, it is small—about 1.6 additional property crimes in the surrounding area per quarter.

[Table 4]

As with the PSH estimates, the confidence intervals for the effect of opening affordable housing in percentage terms are notably wider for drug and disorder offenses than the other outcomes. See Appendix D for the affordable housing dynamic treatment effect estimates. Those results support the parallel trends assumption and generally show post-opening effects that are consistently centered around zero. While not statistically significant ($p > 0.05$), the point estimates suggest an increase in property crime beginning about 2 years after affordable housing facilities open.

4.5. Robustness to Alternative Specifications

Figure 4 summarizes the results from ten alternative specifications of the main PSH model, with ATTs expressed in quarterly crime level terms, and the main estimates included for reference. Broadly, these alternatives differ from the main specification by: controlling for land use; aggregating to the yearly level; using a two-stage difference-in-differences estimator (Gardner et al., 2024); using only LAPD and City of Los Angeles data; weighting by the size of PSH facilities; including all available post-treatment periods; allowing PSH to be included in the sample if no other PSH facilities have opened nearby within 6 years; using broader definitions for the crime measures; allowing for anticipation effects; and requiring each treated facility to have at least a year of post-opening data and ten control cases (to remove comparisons without much data).

[Figure 4]

Across these combinations of outcomes and alternative specifications the estimated null effects are quite robust. None of the estimates are statistically significant ($p > 0.05$). Further details on these alternative specifications are provided in Appendix E, which contains three further supplementary analyses. One estimates PSH facility effects on total crime—that is, the four offense types combined—finding null results, which is perhaps unsurprising given the large share of crime made up of property offenses and violent offenses. The second splits the PSH into cohorts that opened before and after 2020 (around when Proposition HHH funding and various legislative reforms became more important for the siting and pace of new PSH development), which also yields results that are not statistically significant ($p > 0.05$).

The third supplementary analysis in Appendix E alters the buffer distance examined, from 500 meters down to 100 meters in 100-meter increments. This is done two ways: one which holds the sample fixed (the same as the main analysis) and another which allows other PSH to enter the sample if they do not overlap with the smaller buffers. None of the effects from this analysis are statistically significant ($p > 0.05$), and many are quite imprecise. However, in some specifications, the percentage effect appears to increase nearer to PSH facilities, including for property and violent crime. Thus, we cannot rule out that the 500-meter buffers in the main analysis may mask some highly localized crime increases, either inside or immediately next to PSH facilities, although again, the estimates are too noisy to conclude this with confidence.

Appendix B contains results from a non-parametric, spatial difference-in-differences design that compares how opening PSH facilities changes crime patterns nearby compared to slightly farther away, inspired by Butts (2023). That design also leads to null effects for all outcomes, although the estimates are not as precise as in the main staggered difference-in-differences design.

5. Discussion

It is essential that housing policy considers the consequences of different forms of affordable or low-income housing on neighborhood crime patterns. The fate of one of America's major initial housing policies—high-density public housing owned and operated by branches of local government—is illustrative: high rates of violence, crime, and disorder were a factor in the

decision to demolish many of these developments. In the 1990s, demolitions became part of federal policy under the Department of Health and Urban Development (HUD)'s Housing Opportunities for People Everywhere (HOPE VI) program. Concerns about how housing for low-income residents affects neighborhood public safety are not only barriers to the siting of new developments but can also lead to their destruction.

This study's findings provide the most rigorous evidence to date that concerns about serious violent and property crime should not be a barrier to PSH siting. Taken together with the broader literature on the individual-level effects of PSH (e.g., Cohen, 2024), our findings are consistent with PSH doing what it is designed to do: stabilizing the lives of its residents without destabilizing neighborhoods. This stands in contrast to findings from earlier studies of temporary shelters, which tended to find significant crime increases in surrounding areas (Faraji et al., 2018).

Our findings for PSH are more broadly consistent with the literature on the neighborhood effects of LIHTC developments, which have found either null or modestly beneficial effects on local crime (Diamond and McQuade, 2019; Freedman and Owens, 2011; Woo and Joh, 2015). Notably, comparing our directional estimates for PSH versus non-PSH affordable housing sites suggests that PSH may be more protective against property crime but more problematic for drug and disorder offenses. While these differences are not statistically significant, they are consistent with the idea that the characteristics that distinguish PSH from standard affordable housing (e.g., the eligible population, the provision of on-site services) may provide some additional protection against property crime with the possible tradeoff of less protection against drug and disorder offenses. This is an area of consideration for future research

Any such future research could greatly benefit from alternative data sources to capture drug, public disorder, or broader quality-of-life type offenses. Our effect estimates for drug and disorder offenses are highly imprecise, with wide enough confidence intervals to cover the near universe of possible effect sizes in relative rate terms. This imprecision likely stems in part from the very low counts we observe for drug and disorder offenses, an issue that becomes more severe over time; toward the end of our study period, the average number of drug and disorder offenses observed in the 500-meter buffer around a PSH facility were only 5 and 6, respectively. There is also likely substantial measurement error in our data on these offenses. To support inclusion of as many of these types of incidents as possible in our analyses, our measure mixes arrests and crime reports across agencies, which may exacerbate the broader issue that these offenses likely reflect enforcement prioritization more than actual behaviors.

Our findings are subject to several additional limitations. First, our analysis is specific to Los Angeles County. Los Angeles is an important study setting given the scale of unsheltered homelessness and PSH development in the jurisdiction, and our results showing limited heterogeneity of effects across neighborhood characteristics helps support generalizability. Still, our findings may not generalize to settings where PSH facilities are smaller, differently-resourced, or sited in different types of neighborhoods. Second, our analytic sample consists of PSH that opened in areas without pre-existing PSH facilities nearby. The 96 PSH facilities we exclude due to spatial overlap are disproportionately located the areas of the county with the highest rates of crime, poverty, homelessness, and homeless service providers. While our results address questions of what happens when PSH opens in an area for the first time, they do not address the effects of opening additional PSH in areas with already-concentrated homeless-serving facilities. Finally, our design relies on the not-yet-treated PSH facilities as the comparison group, which means we cannot speak to the likely effects of PSH in areas that have

never received PSH, which could systematically differ from those comparison areas in our sample. Similar findings across different neighborhood types and using an alternative spatial difference-in-differences approach (which relies on a different source of identifying variation) help bolster against this concern, but only partially.

Overall, we find robust evidence that PSH facility openings do not increase serious crime, with our upper confidence interval estimates ruling out increases of more than 10 property and violent crimes per year. Applying Chalfin (2015)'s estimate of approximately $20,000 in social costs per serious crime, this amounts to approximately $200,000 in annual crime costs per new facility, a figure that would be more than offset by the individual-level cost savings found in prior literature for PSH residents. For example, Hunter et al. (2017) estimate annual cost offsets of $7,700 per PSH resident, and a typical PSH in our sample has about 50 units; this implies approximately $385,000 in annual savings, which is nearly double our worst-case neighborhood crime costs. This result is particularly relevant given recent federal policy shifts that have placed existing and future PSH funding at risk (Finnigan, 2025). At the same time, we recognize that our imprecise estimates for the effects of PSH facilities on drug and disorder offenses means we cannot address community concerns about visible disorder. Future efforts to better measure and evaluate these quality-of-life offenses beyond law enforcement administrative datasets would help broaden our understanding of the social costs and benefits of PSH.

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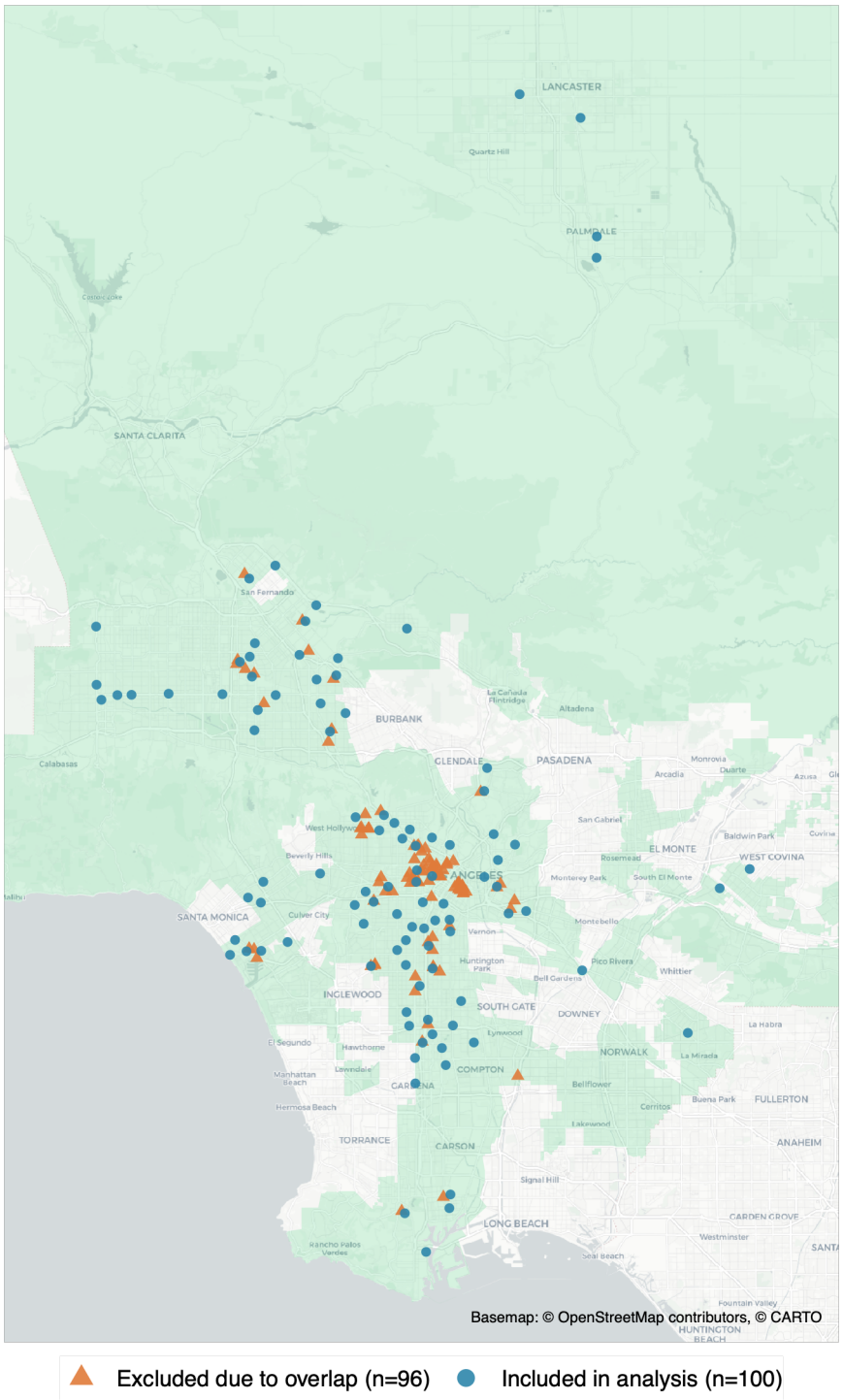
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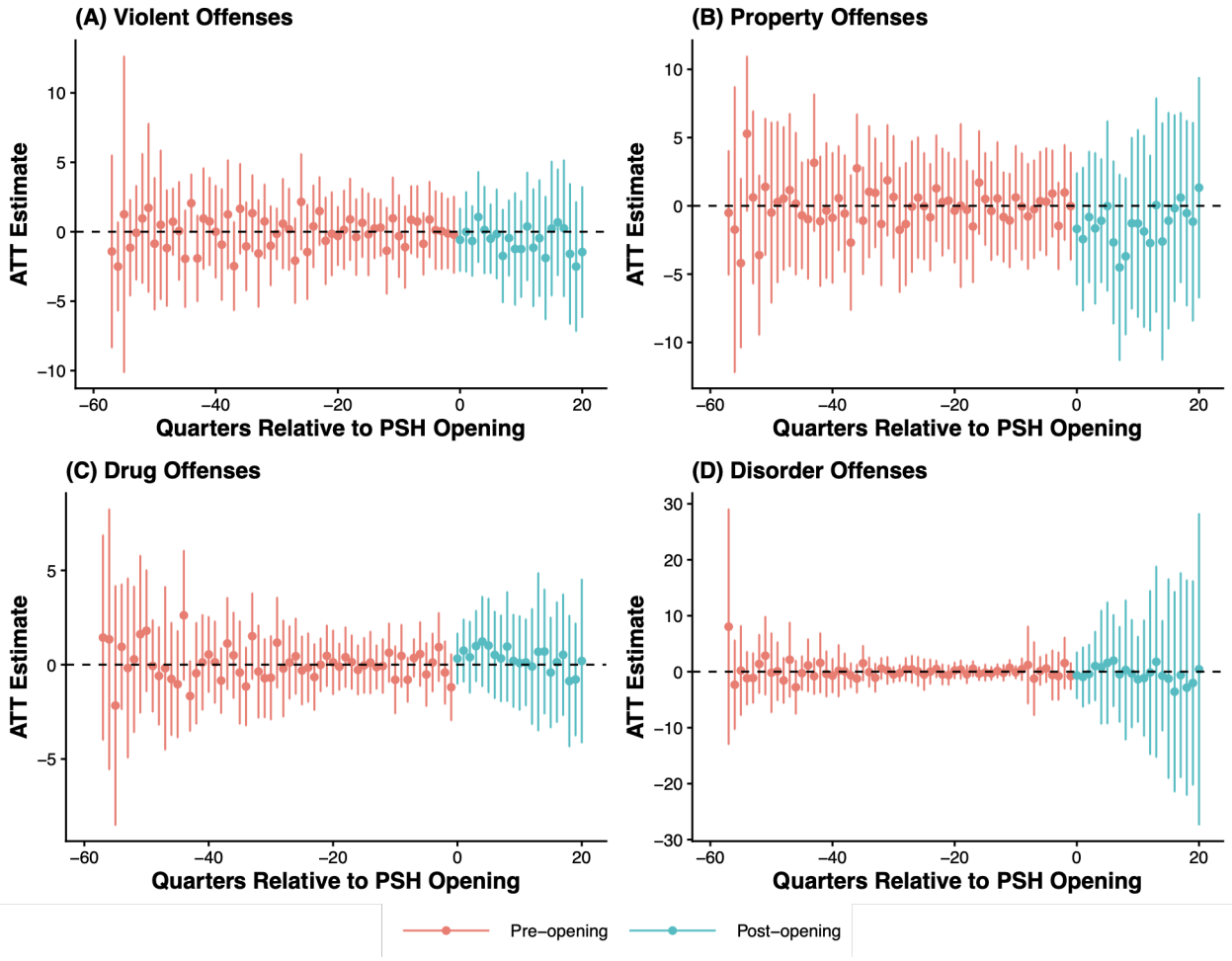
TABLES AND FIGURES

Figure 1: Maps of Permanent Supportive Housing Facilities in Los Angeles County



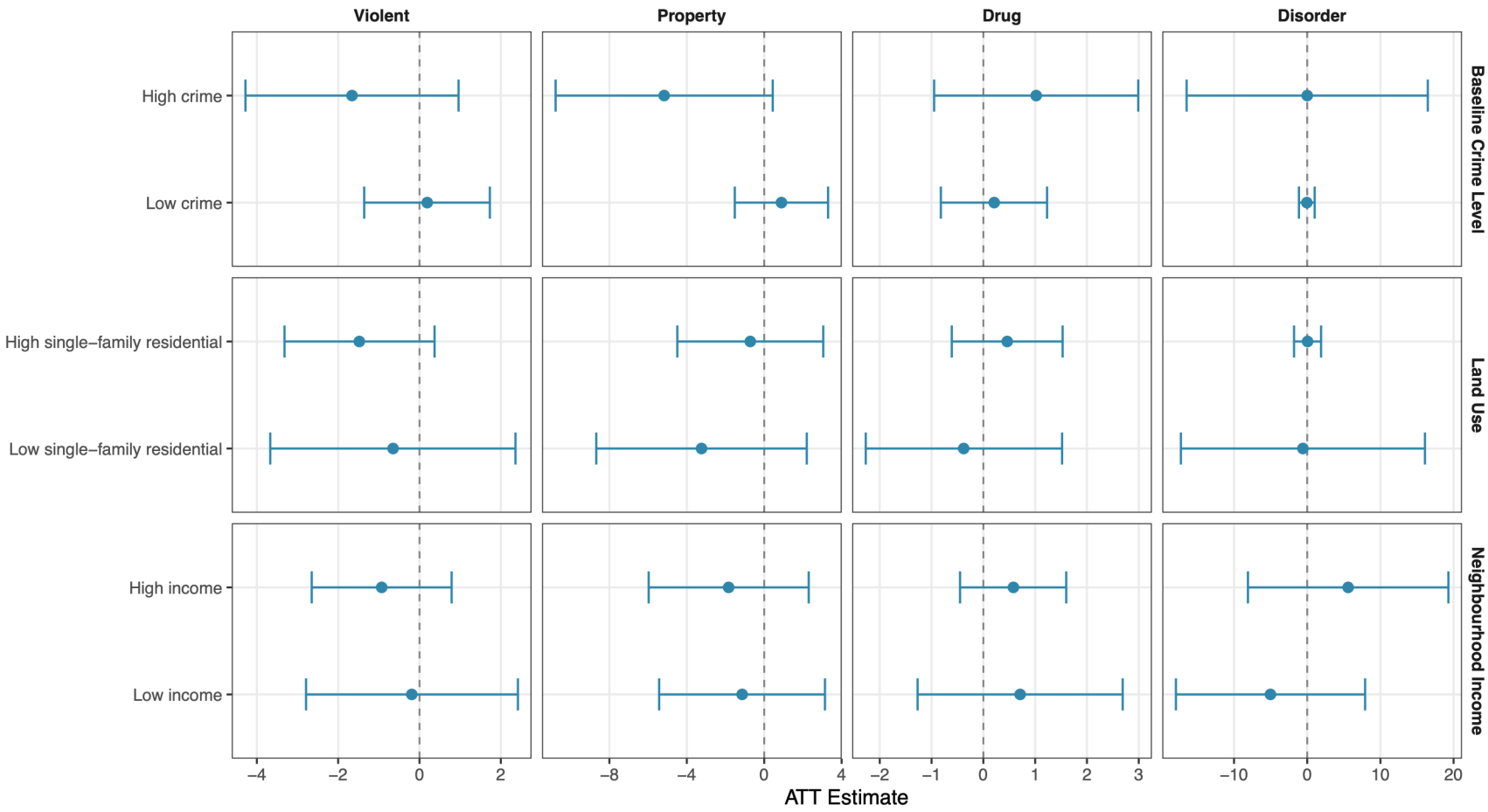
Notes: Our data cover the jurisdictions shaded in green. PSH facilities that opened before 2011 are not shown.

Figure 2: Dynamic Effects of Permanent Supportive Housing on Crime, by Offense Type



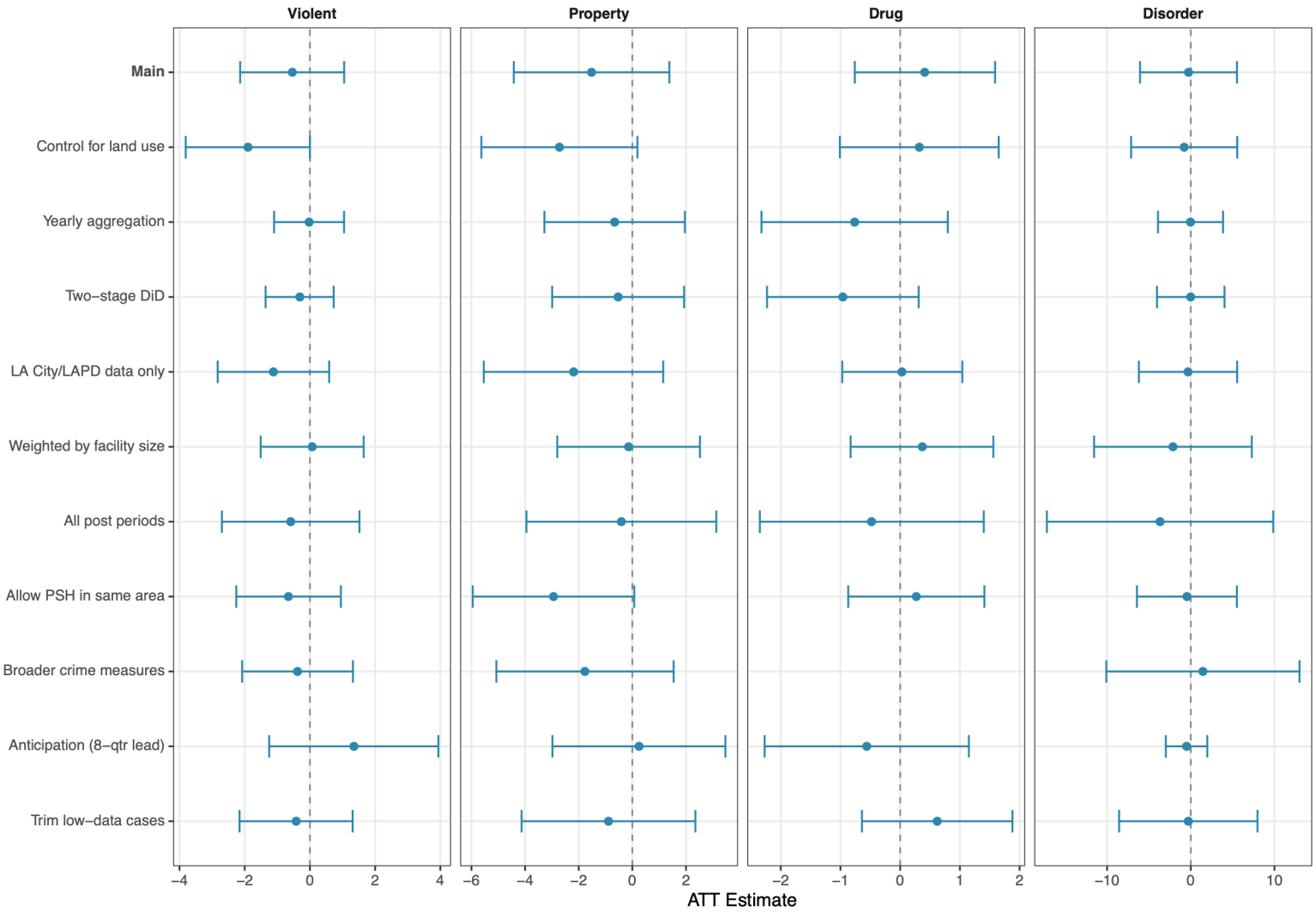
Note: y-axes are on different scales. Effects are shown as crime counts in the 500-meter buffer surrounding PSH facilities. Bars represent 95% confidence intervals.

Figure 3: Estimated Crime Effects when Splitting PSH by Baseline Crime, Land Use, and Income



Notes: x-axes are on different scales. Effects are shown as crime counts per quarter in the 500-meter buffer surrounding PSH facilities. Bars represent 95% confidence intervals.

Figure 4: Robustness Checks for Overall Effects of Opening PSH on Crime



Notes: x-axes are on different scales. Effects are shown as crime counts per quarter in the 500-meter buffer surrounding PSH facilities. Bars represent 95% confidence intervals. The baseline drug measure includes all recorded drug offenses, so there is no broader drug offense category. The yearly estimates are divided by four to put them on the same scale as the other models, which are quarterly.

Table 1: Descriptive Statistics on Facilities Included and Excluded from Analytical Permanent Supportive Housing Sample

	Included PSH (n = 100)	Excluded PSH (n = 96)
Facility characteristics		
Restricted units	51.32 (21.87)	63.46 (36.78)
Opening year	2018.52 (4.38)	2020.34 (3.82)
% in City of Los Angeles	86%	97%
Neighborhood characteristics (ACS 2006-2010)		
Median household income ($)	42147.32 (17422.71)	31140.09 (15132.32)
Family poverty rate	0.24 (0.14)	0.22 (0.16)
% Non-Hispanic white	0.17 (0.22)	0.15 (0.22)
% Children (< 18)	0.28 (0.09)	0.22 (0.12)
Land use (PSH in City of Los Angeles only)		
Low-density residential	0.35 (0.18)	0.19 (0.20)
High-density residential	0.11 (0.12)	0.18 (0.15)
Commercial	0.11 (0.08)	0.16 (0.10)
Other	0.19 (0.17)	0.21 (0.17)
Crime in 2010 (quarterly average)		
Violent crime	23.01 (18.98)	62.40 (57.29)
Property crime	35.59 (19.71)	62.35 (34.90)
Drug crime	11.26 (11.86)	90.04 (184.83)
Disorder crime	10.53 (21.83)	42.99 (67.79)

Note: Mean (SD) except for percent in City of Los Angeles.

Table 2: Time-Varying Statistics on Analytic Permanent Supportive Housing Sample

	Pre-opening	Post-opening
Sample		
PSH facilities (n)	100	99 [1]
PSH facility-quarter observations	3,578	1,468
Quarters per PSH facility (mean)	35.8	14.8
Mean quarterly crime incidents		
Violent Offenses	20.6	25.4
Property Offenses	34.6	38.9
Drug Offenses	6.5	5.8
Disorder Offenses	5.2	6.4

[1] The last PSH facility to open has no valid control group, so it appears only in the pre-opening column and serves solely as a control facility during that period.

Table 3: Average Effects of Permanent Supportive Housing on Crime, by Offense Type

Outcome	ATT as Effect on Crime Levels	ATT as Percentage Effect
Violent Offenses	-0.54	-2.23%
	(-2.14, 1.05)	(-8.81%, 4.34%)
Property Offenses	-1.52	-3.80%
	(-4.42, 1.38)	(-11.06%, 3.46%)
Drug Offenses	0.41	9.68%
	(-0.76, 1.59)	(-17.92%, 37.29%)
Disorder Offenses	-0.27	-5.37%
	(-6.06, 5.52)	(-122.43%, 111.68%)

Notes: 95% confidence intervals in parentheses. Percentage effects calculated relative to the counterfactual. N = 5,046 quarter-facility observations from 100 unique PSH facilities.

Table 4: Average Effects of Non-PSH Affordable Housing on Crime, by Offense Type

Outcome	ATT as Effect on Crime Levels	ATT as Percentage Effect
Violent Offenses	-0.47	-2.49%
	(-1.83, 0.89)	(-9.72%, 4.73%)
Property Offenses	1.64	5.34%
	(-0.45, 3.73)	(-1.48%, 12.16%)
Drug Offenses	-0.59	-11.06%
	(-1.64, 0.45)	(-30.58%, 8.47%)
Disorder Offenses	-0.26	-11.88%
	(-1.27, 0.76)	(-59.06%, 35.30%)

Notes: 95% confidence intervals in parentheses. Percentage effects calculated relative to the counterfactual. N = 7,205 quarter-facility observations from 144 unique affordable housing facilities.

APPENDIX MATERIALS

Appendix A: Details on PSH Excluded from Sample
Figure A.1. Distribution of Cluster Size among Excluded PSH Facilities
Figure A.2: Map of PSH Facilities in Skid Row and Their 500-Meter Buffers
Figure A.3: Map of Select Excluded PSH Facilities and Crime Hot Spots
Figure A.4: Time Since Last PSH Opened Nearby for Excluded PSH Facilities
Figure A.5: Opening Year of Excluded PSH Facilities

Appendix B: Spatial Difference-in-Differences Design and Other Pre-Registration Details
Figure B.1: Spatial Difference-in-Differences Estimates of Effect of Opening PSH on Crime

Appendix C: Further Descriptive Data on Permanent Supportive Housing and Affordable Housing Facilities
Figure C.1: Panel Structure of the PSH Data by Calendar Quarter
Figure C.2: Panel Structure of the AH Data by Calendar Quarter
Figure C.3: Land Use Around PSH and AH Facilities
Figure C.4: Association Between Land Use and Opening Date for PSH and AH Facilities
Figure C.5: Baseline Crime Levels Around Where PSH and AH Facilities Are Situated
Figure C.6: Association Between Baseline Crime and Opening Date for PSH and AH
Figure C.7: Baseline Characteristics of Tracts Where PSH and AH Facilities Are Situated
Figure C.8: Association Between Baseline Tract Characteristics and Opening Date for PSH and AH Facilities
Figure C.9: Average Quarterly Crime Counts Relative to Facility Opening, by Offense Type and Facility Type
Figure C.10: Map of Affordable Housing Facilities Included and Excluded from Analysis
Table C.1: Descriptive Statistics on Facilities Included and Excluded from Analytical Affordable Housing Sample
Table C.2: Time-Varying Statistics on Affordable Housing Included in Main Analysis

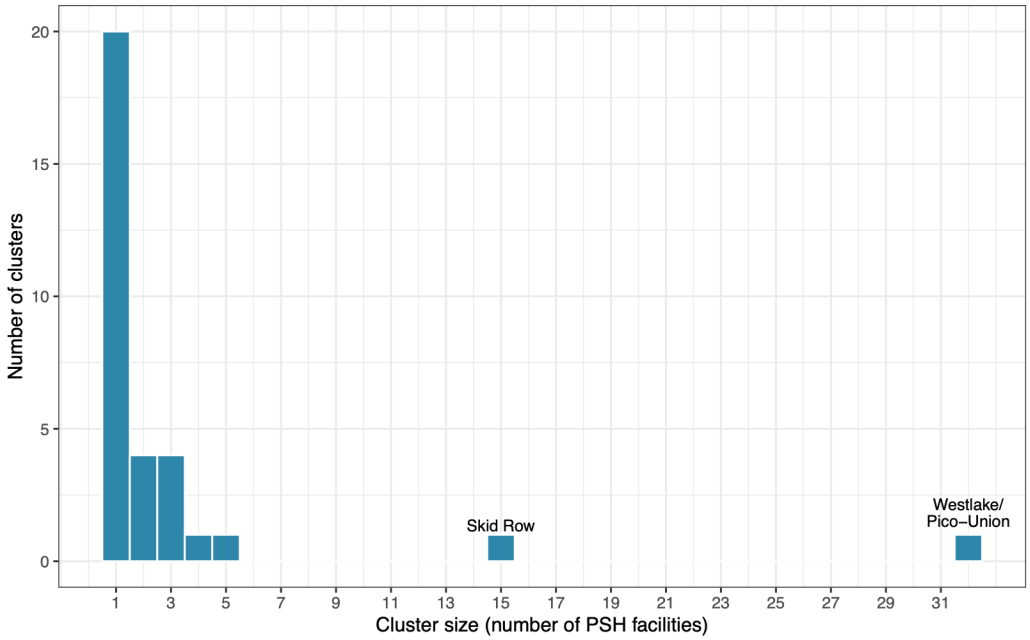
Appendix D: Affordable Housing Dynamic Treatment Effects
Figure D.1: Dynamic Effects of Affordable Housing on Crime, by Offense Type

Appendix E: Robustness Checks for Main PSH Results
Figure E.1: Dynamic Effects of Permanent Supportive Housing on Total Crime
Figure E.2: Effects of PSH Opening on Crime, by Opening Cohort and Offense Type
Figure E.3: PSH Crime Effect Estimates as a Function of Buffer Distance, Using Same Facilities as Main Analysis
Figure E.4: PSH Crime Effect Estimates as a Function of Buffer Distance, With Facilities Varying by Distance

APPENDIX A: DETAILS ON PSH EXCLUDED FROM SAMPLE

In this appendix we provide further details on the 96 PSH that opened during the study period but are excluded from the main analysis due to spatial overlap with pre-existing PSH facilities. Figure A.1 shows the spatial clustering of excluded PSH facilities as a histogram. Clusters are defined as PSH with overlapping 500-meter buffers, using a network analysis approach, such that two units not overlapping each other but overlapping a common third unit are part of the same cluster. About half of all excluded PSH facilities belong to two clusters: Skid Row (n = 15) and Westlake/Pico-Union (n= 32), which includes the area near MacArthur Park. There were 20 excluded PSH that did not overlap with other excluded PSH facilities.

Figure A.1: Distribution of Cluster Size among Excluded PSH Facilities

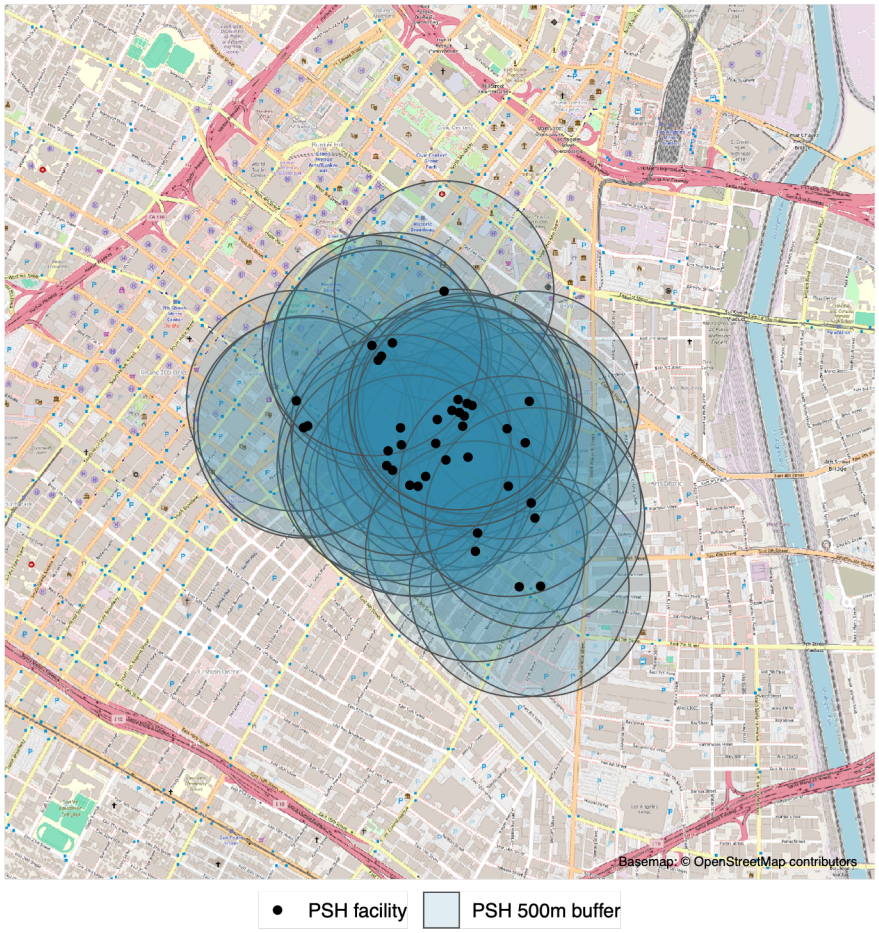


Note: Cluster sizes of one represent excluded PSH facilities that do not overlap with other excluded PSH facilities.

Figure A.2 maps the Skid Row cluster, including the 500-meter buffers surrounding facilities. Estimating treatment effects with these facilities would be very difficult: virtually all

PSH facility openings are preceded by many other PSH opening (including before 2010), meaning that there is no uncontaminated pre-treatment period for these cases, and that this area cannot provide not-yet-treated control cases. Figure A.2 also reinforces that Skid Row is highly atypical in terms of its high concentration of homelessness-related service provision.

Figure A.2: Map of PSH Facilities in Skid Row and Their 500-Meter Buffers

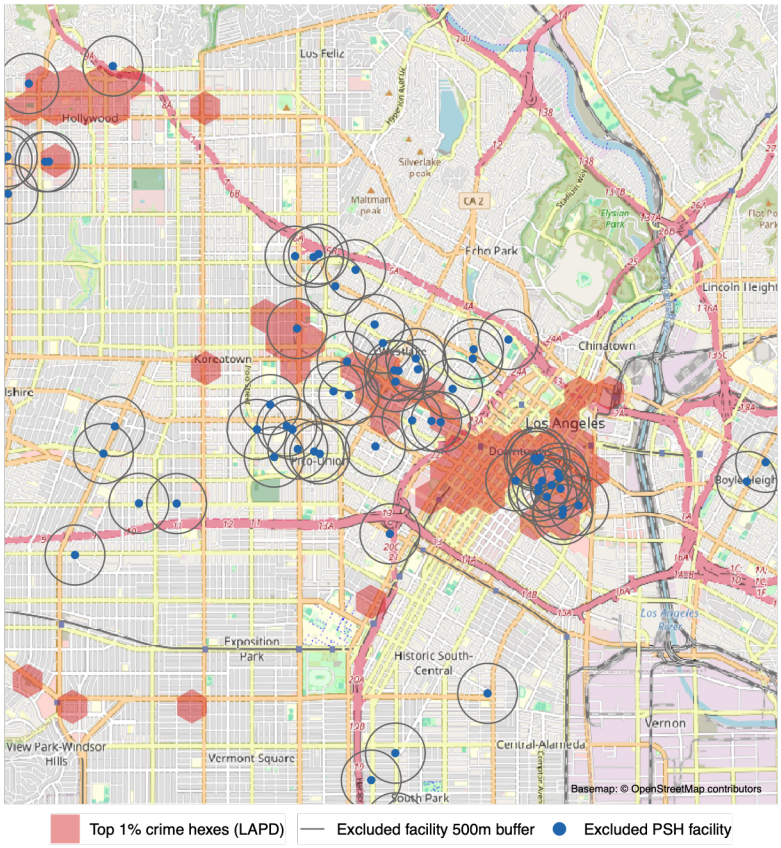


Note: all Skid Row PSH facilities are excluded from the main analysis due to overlap with pre-existing facilities. Basemap is from OpenStreetMap https://www.openstreetmap.org/copyright.

Figure A.3 shows that some excluded PSH facilities, especially the large clusters in Skid Row, Westlake/Pico-Union, and Hollywood, are located at or near the highest 1% of Los

Angeles' area in terms crime. To calculate this, a hex grid was imposed over the City of Los Angeles with the top 1% highest crime hexes—defined as LAPD recorded total crime from 2010-2024—mapped in red. As such, many PSH omitted by our spatially based exclusion criterion are in particularly high-crime areas.

Figure A.3: Map of Select Excluded PSH Facilities and Crime Hot Spots



Note: this map does not show all excluded PSH. Red hexes are crime hotspots that are within the top 1% of Los Angeles' crime distribution. Basemap is from OpenStreetMap https://www.openstreetmap.org/copyright.

Figure A.4 plots the time between when an excluded facility opened and when the next most recent PSH facility opened in the same area, before that excluded facility. It shows that the excluded PSH facilities typically opened relatively soon after another PSH facility (or facilities) opened nearby, usually within two years.

Figure A.4: Time Since Last PSH Opened Nearby for Excluded PSH Facilities

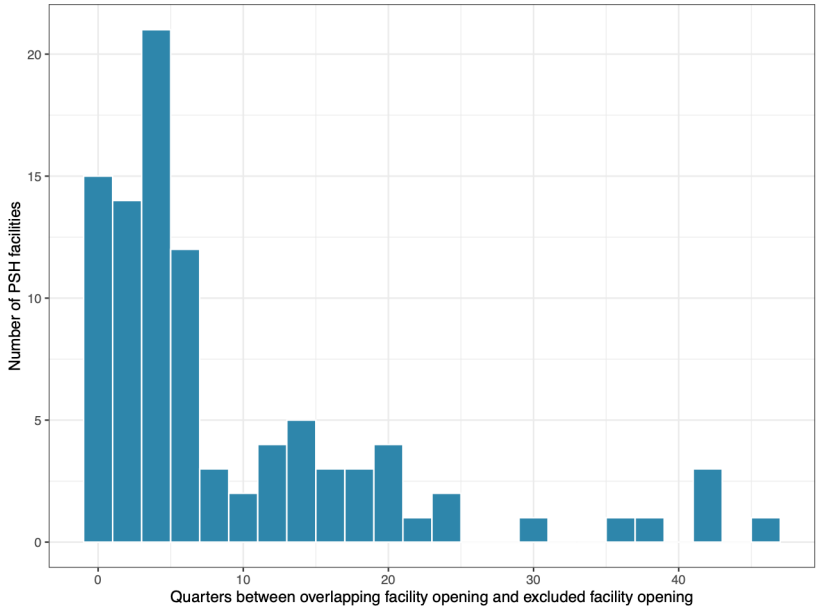
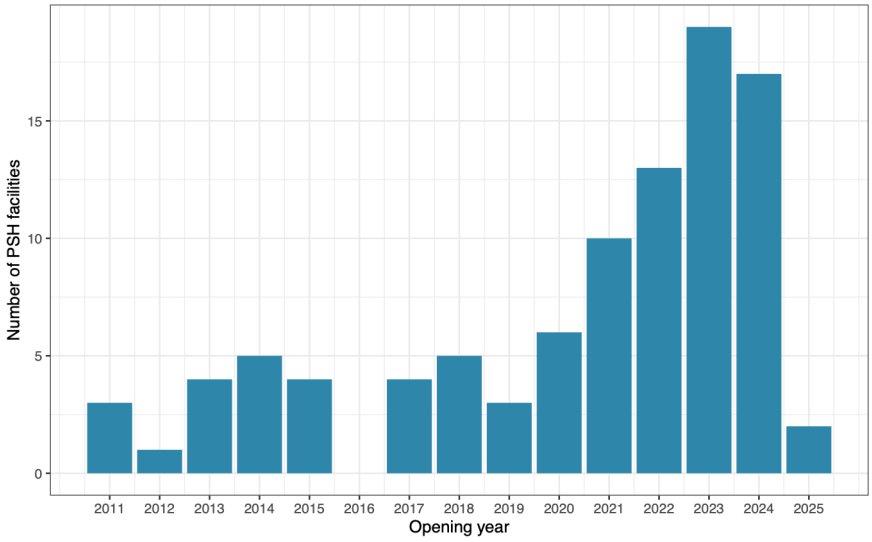


Figure A.5 shows that excluded PSH facility opening dates are concentrated in more recent years (data collection stopped in 2025, which is why there are so few PSH excluded from that year). This reflects that PSH openings are becoming increasingly common in Los Angeles and that, mechanically, facilities that open later are more likely to be excluded given the higher number of pre-existing PSH facilities.

Figure A.5: Opening Year of Excluded PSH Facilities



APPENDIX B: SPATIAL DIFFERENCE-IN-DIFFERENCES DESIGN AND OTHER PRE-REGISTRATION DETAILS

Butts' (2023) difference-in-differences estimator centers on comparing temporal changes in outcomes (in our case, crime levels) in areas that are spatially nearer to the treatment location (the opening PSH facilities) to areas that are farther away. Distance bins are estimated non-parametrically up to some preset distance (we chose 1km) with the farthest bin serving as the control cases in the second difference. The other bins trace out an estimate of the treatment curve relating the size of treatment effects to the distance from the opening facility.

In our spatial difference-in-differences models, we use the same 100 PSH facilities as in the main analysis, the pre-period is the year before they opened and the post-period is the year after they opened. To count crimes by distance, 500-foot hexagons were used, using the same four outcomes as in the main analysis.

Figure B.1: Spatial Difference-in-Differences Estimates of Effect of Opening PSH on Crime

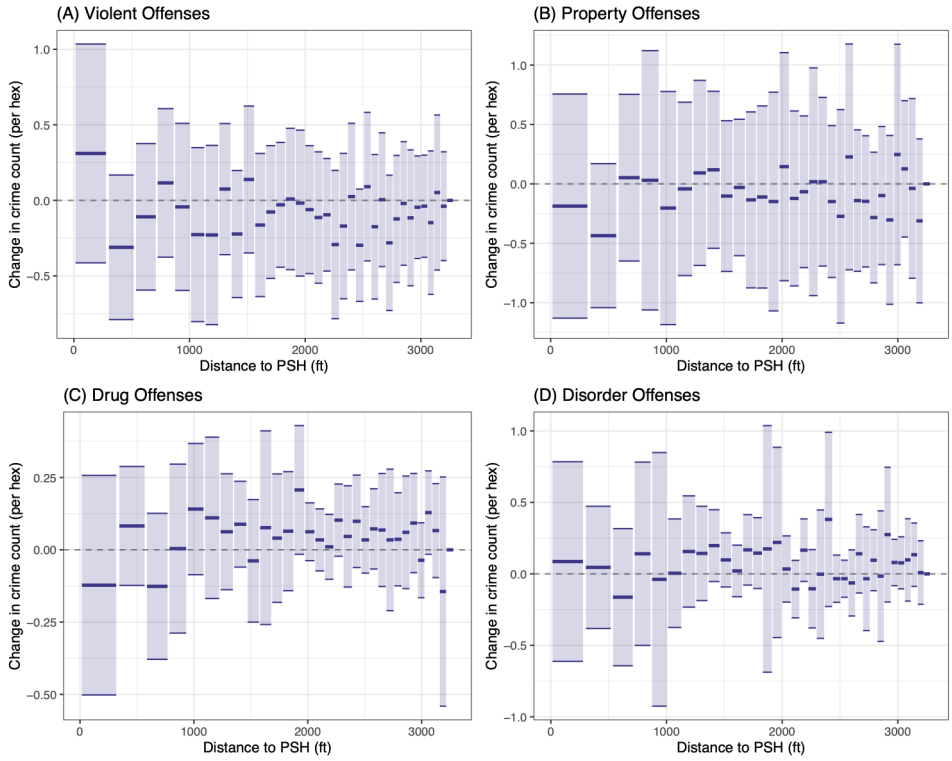


Figure B.1 presents the results from these models. The estimated effects are generally near zero, showing that the change in crime levels before and after PSH facilities opened was the same nearer to PSH facilities as they were to the farther away control areas. This suggests that PSH facilities did not affect nearby crime. The violence point estimate for the first bin (about 200 feet—a small distance that may only encompass the facility itself) is positive. But for that, and all the other estimated distance-based effects, the confidence intervals cross zero.

These results lead to the same conclusions as the main analysis: the opening of PSH facilities does not detectably affect crime levels. In our pre-registration, this spatial difference-in-differences analysis is advanced as the primary method, with the staggered difference-in-difference design advanced as a secondary method. We have swapped these because even though they lead to the same conclusions, the staggered difference-in-difference results are more precise. For example, the innermost bin in the violence spatial difference-in-differences model has a pre-opening violence level of about 3 crimes per hex. The corresponding confidence interval for the treatment effect in Figure B.1 spans roughly -0.5 to 1, which is a very large range relative to that baseline amount of violence. The results in the main text estimating the effects of PSH on violence yield far more precise null estimates.

Other Deviations from the Pre-Registration

Wherever an equivalent decision for the staggered difference-in-differences model exists as for the pre-registered spatial difference-in-differences, we have retained that decision. Sometimes, these analyses are only contained in these appendices. For example, the pre-registration says total crime will be examined as a crime, which we present only in Appendix E,

since it leads to the same conclusion and including it in the main results would increase the volume of results to go over by one-fifth without changing any conclusions.

The pre-registration indicates that we would control for the family-wise error rate with a Holm correction, which we did not do, since all the results are already statistically insignificant (the correction could only make significant findings insignificant).

Some secondary analyses proposed in the pre-registration are not reported here because we did not end up trusting the data enough. For example, we are not sufficiently confident in 311 calls' ability to measure encampment activity over time and place, and we could not be sure LAPD *modus operandi* codes were used consistently enough to be reliable for analyses.

Unlike the pre-registration suggested, our analysis focuses on permanent supportive housing as opposed to all homeless-serving housing facilities. This decision reflects that there are an increasing number of analyses about the effects of shelters, including in Los Angeles County. Permanent supportive housing was thus a larger gap in research knowledge; also, cleaning PSH data took considerable time and effort. Rather than be duplicative with prior work, we chose to focus our efforts on producing high-quality estimates for PSH facilities. We do not have shelter-based estimates that we are not presenting, rather, we have not cleaned shelter data and have not fit any models using shelter data.

APPENDIX C: FURTHER DESCRIPTIVE DATA ON PERMANENT SUPPORTIVE HOUSING AND AFFORDABLE HOUSING FACILITIES

This appendix presents further information on the PSH and affordable housing (AH) facilities that are included in the main analysis. Figure C.1 shows the treatment timing structure for PSH facilities (n = 100), which are on the y-axis rows, with calendar quarters on the x-axis. Areas in blue are pre-opening and serve as the control cases, areas in yellow are the post-opening times that we estimate effects for, and areas in red are excluded from analysis because they exceed the 20 quarter post-opening cutoff.

Figure C.1: Panel Structure of the PSH Data by Calendar Quarter

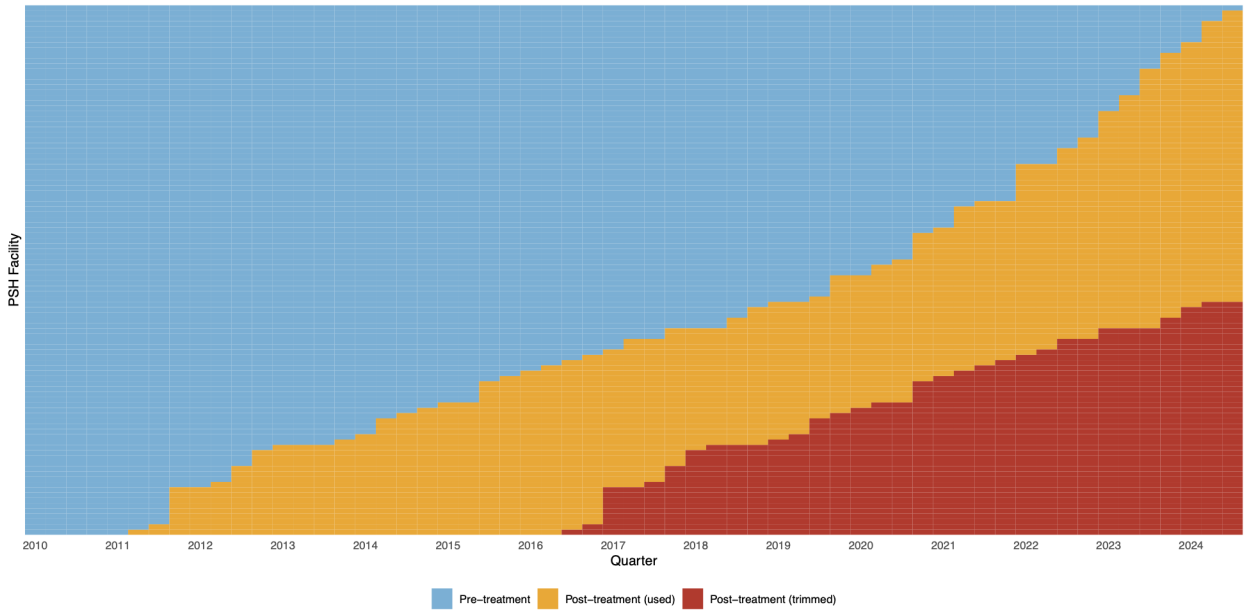
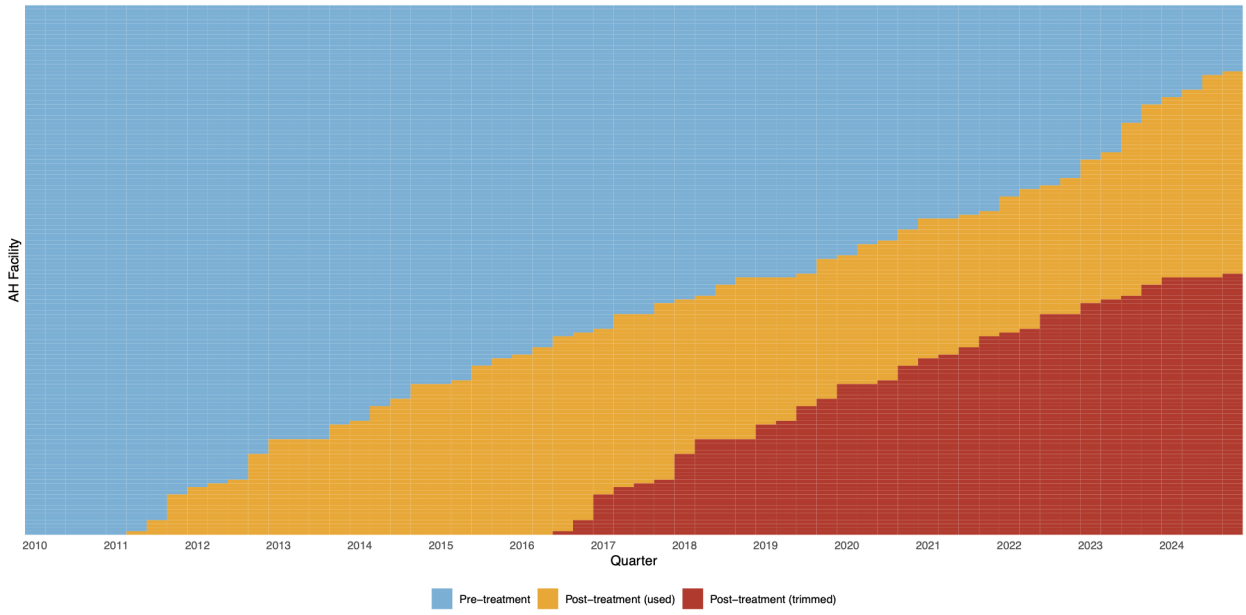


Figure C.2 is like Figure C.1 but focused on the AH facilities (n = 144). A small difference is that this panel extends to Q4 2024 (instead of Q3 2024) because some AH facilities opened in 2025 and thus serve as control cases for the end of 2024, unlike the PSH data for which the last opening in the analytic panel was Q4 2024.

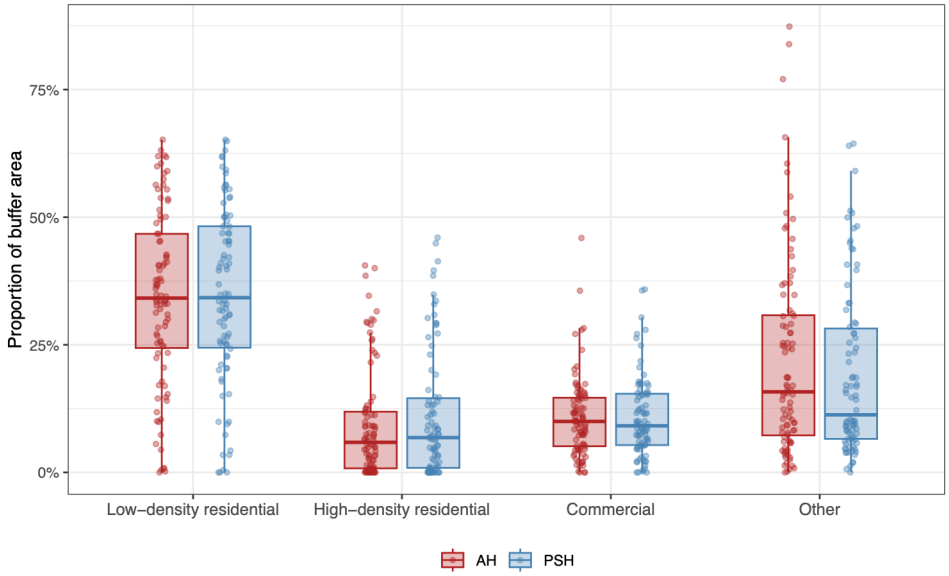
Figure C.2: Panel Structure of the AH Data by Calendar Quarter



Figures C.3 through C.8 follow a standard format: there are two figures each for three sets of characteristics: land use, baseline crime, and baseline tract characteristics. The first plot shows the distribution of the facilities along various metrics, to give a sense for the types of areas where these facilities tend to be situated, and whether there are differences for PSH versus AH facilities. The second plot examines bivariate associations between these same metrics and the opening date of PSH/AH facilities, to examine whether there have been shifts in the types of places where facilities are located over time.

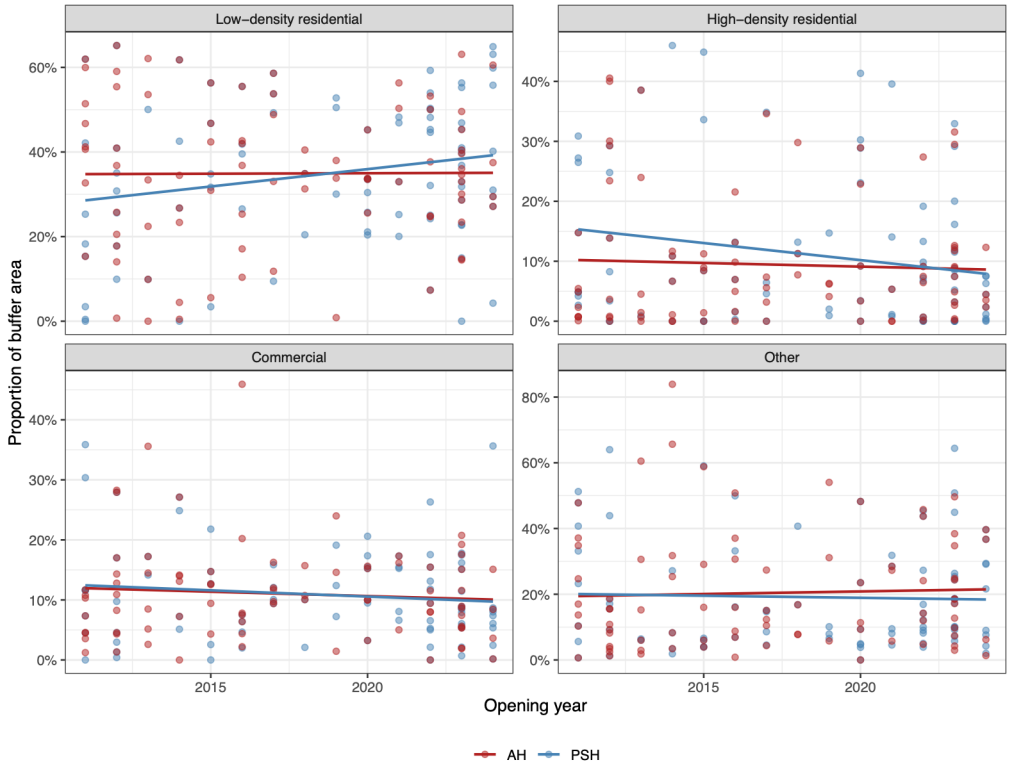
Figure C.3 shows PSH and AH facilities are typically situated in areas that have high levels of low-density residential zoning, and that the land use around PSH and AH facilities is very similar. Figure C.4 shows that there have not been large changes in the land use around where facilities are situated over time. These two figures are limited to facilities the City of Los Angeles, reflecting that we had city (but not county) zoning data, specifically for 2024.

Figure C.3: Land Use Around PSH and AH Facilities



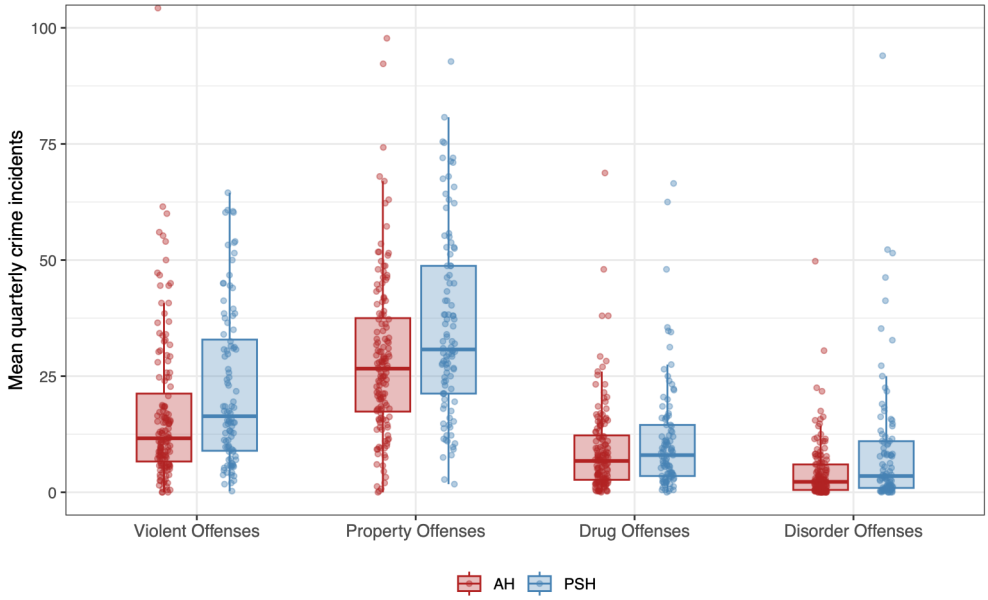
Note: Only AH and PSH facilities located within the City of Los Angeles are plotted. For each facility, the denominator is the portion of the 500m radius circle surrounding it that is located within the City of Los Angeles.

Figure C.4: Association Between Land Use and Opening Date for PSH and AH Facilities



Note: Only AH and PSH facilities located within the City of Los Angeles are plotted. For each facility, the denominator is the portion of the 500m radius circle surrounding it that is located within the City of Los Angeles.

Figure C.5: Baseline Crime Levels Around Where PSH and AH Facilities Are Situated



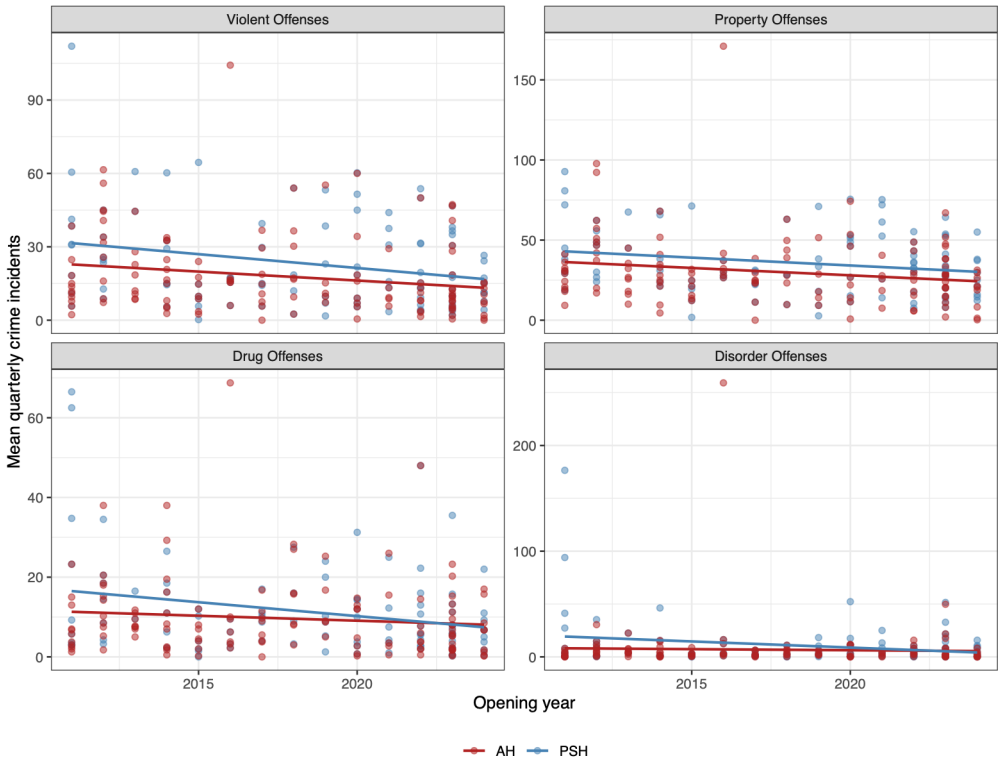
Note: The plotted y-axis range is limited to 100. Five facility-crime-type cases exceed this range; they are still used for the box-and-whisker statistics. The plotted values are the average quarterly crime counts for 2010 in the 500m radius circle surrounding where PSH and AH facilities would be located (2010 is before any of these facilities opened).

Figure C.5 shows that PSH facilities tend to be situated in areas that are slightly higher crime than AH facilities. This plot uses 2010 crime data, before any of the facilities opened. The crime distributions between PSH and AH facilities overlap substantially, but the median and top quartile are higher for all four offense types for PSH facilities. Figure C.6 shows that over time both AH and PSH facilities have been situated in areas that had lower (2010) crime levels, though there is considerable similarity in the baseline crime distribution across years of where facilities are situated.

Figure C.7 shows that PSH and AH facilities are quite similar in terms of the baseline (2010) tract characteristics where they would go on to be situated. Specifically, it shows the distribution for median household income, the percent of residents who are less than 18, the family poverty rate, and the percent of residents who are non-Hispanic White. Only the latter noticeably differs across AH and PSH facility tracts, with AH facilities being located in areas

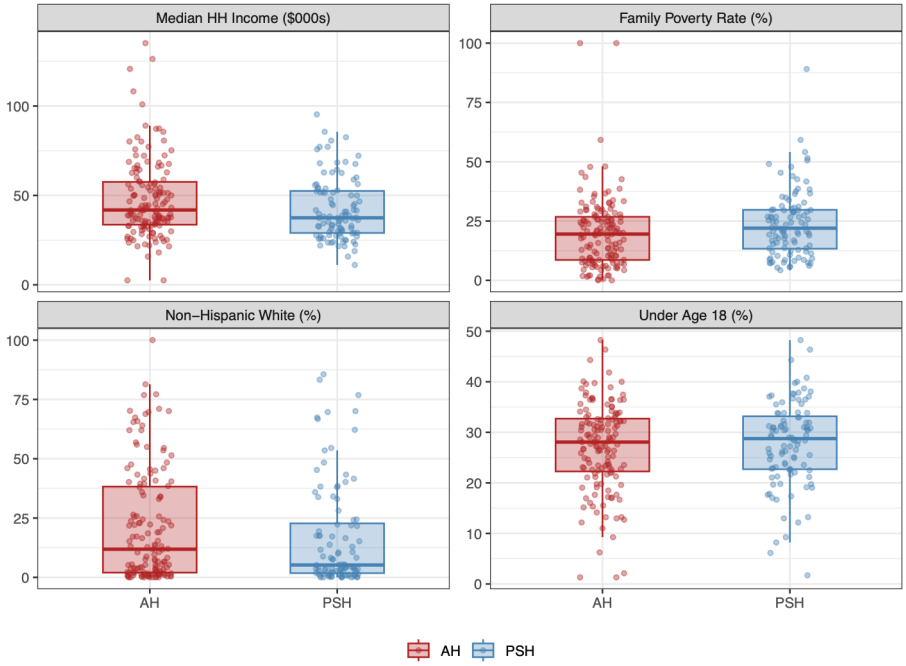
with a larger share of non-Hispanic White residents, based on 2010 racial composition. Figure C.8 shows trends in these metrics, showing only slight shifts in where PSH have been situated over time, with both AH and PSH facilities being increasingly located in areas with higher baseline income and non-Hispanic White population shares and lower family poverty rates.

Figure C.6: Association Between Baseline Crime and Opening Date for PSH and AH Facilities



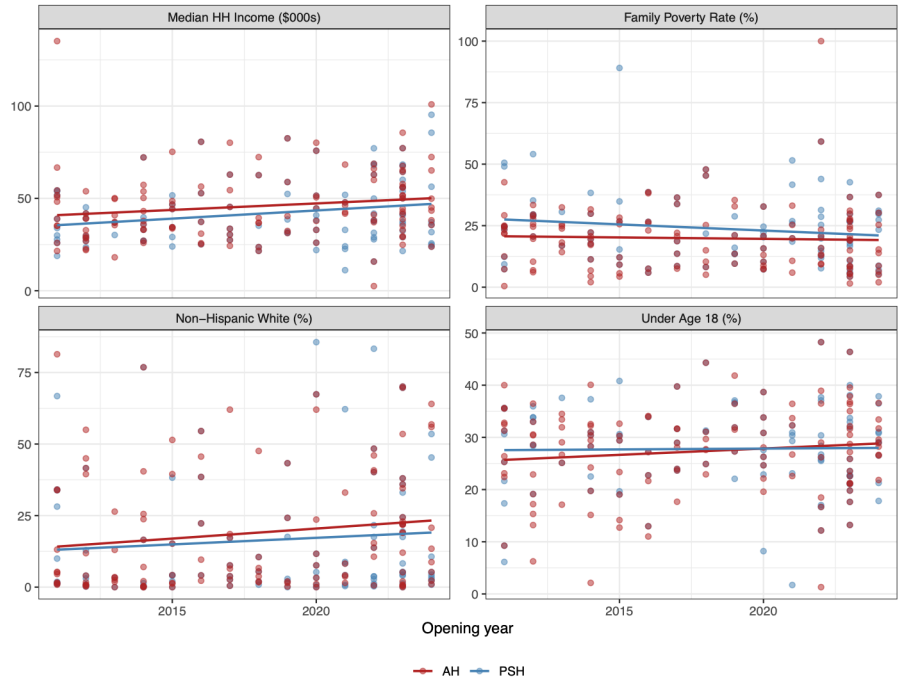
Note: The plotted crime values are the average quarterly counts for 2010 in the 500m radius circle surrounding where PSH and AH facilities would be located (2010 is before any of these facilities opened).

Figure C.7: Baseline Characteristics of Tracts Where PSH and AH Facilities Are Situated



Note: Tract characteristics are measured using 2006-2010 ACS estimates, before any of these facilities opened.

Figure C.8: Association Between Baseline Tract Characteristics and Opening Date for PSH and AH Facilities



Note: Tract characteristics are measured using 2006-2010 ACS estimates, before any of these facilities opened.

Figure C.9 shows the descriptive trends in the four main crime outcomes relative to when facility open, separately for PSH and AH facilities that opened between 2011-2024 (this excludes 18 AH facilities that opened after 2024). While crimes are generally higher in the post-opening period, an especially clear for violent and property crime, these are continuations of trends that existed long before facilities opened. Thus, while areas undergoing development may experience an increase in crime (on average) over a long timespan, it is not surprising given these patterns that our causal estimates yield null effects, especially since the controls are drawn from the same cases in the pre-opening periods.

Figure C.10 maps AH facilities that are included and excluded from the main analysis. It is the analogue of the Figure 1, the PSH map, in the main text. Figure C.10 shows that the AH facilities included in the analytic panel come from a wide swath of Los Angeles County.

Figure C.9: Average Quarterly Crime Counts Relative to Facility Opening, by Offense Type and Facility Type

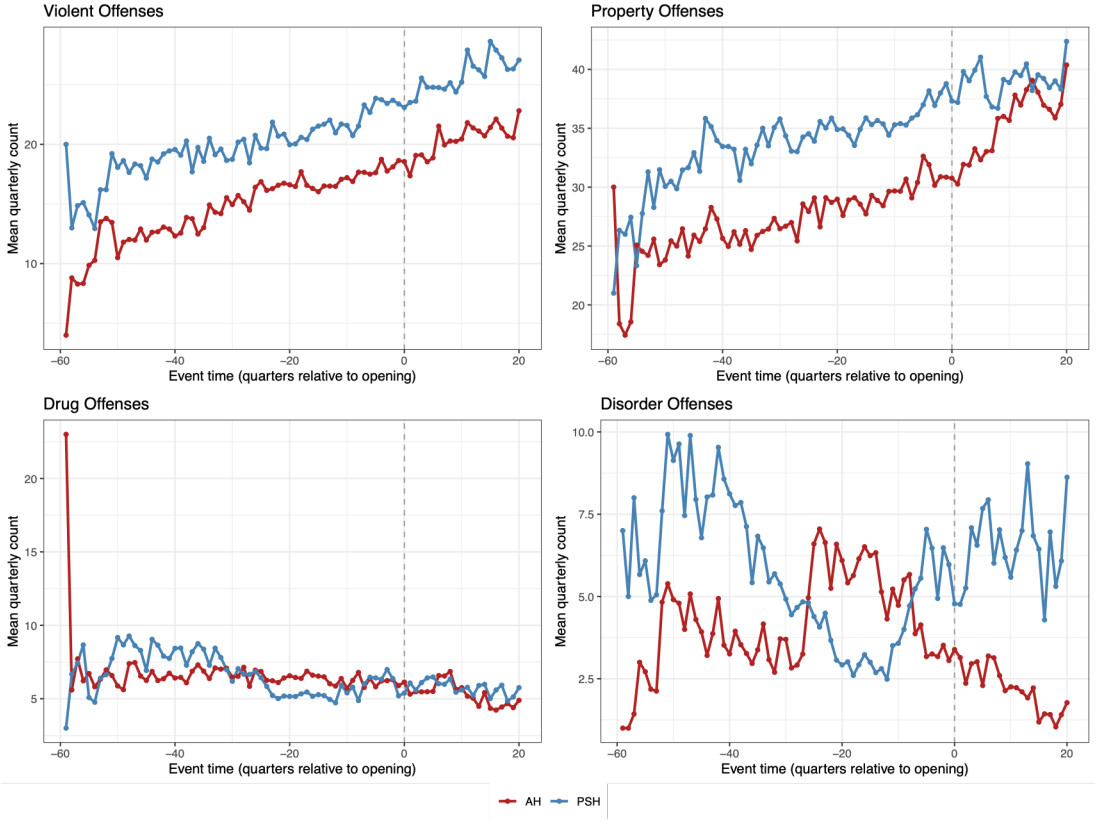
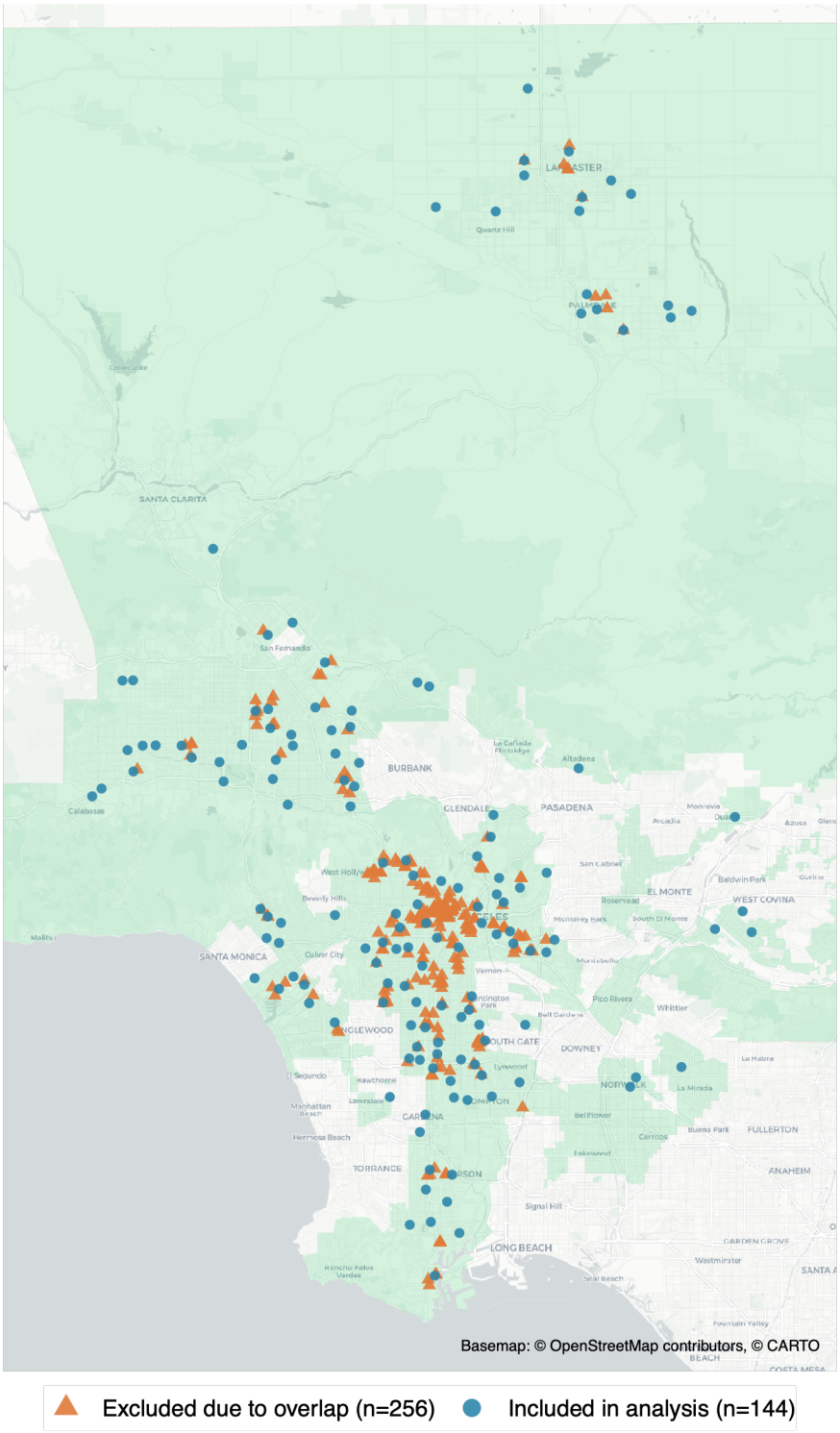


Figure C.10: Map of Affordable Housing Facilities Included and Excluded from Analysis



Notes: Our data cover the jurisdictions shaded in green. AH facilities that opened before 2011 are not shown.

Table C.1 is like Table 1 in the main text focuses on the AH facilities, instead of PSH facilities.

Table C.1: Descriptive Statistics on Facilities Included and Excluded from Analytical Affordable Housing Sample

	Included (n = 144)	Excluded (n = 256)
Facility characteristics		
Affordable units	75.42 (47.65)	73.04 (41.07)
Opening year	2018.70 (5.00)	2020.54 (4.86)
% in City of Los Angeles	64%	83%
Neighborhood characteristics (ACS 2006-2010)		
Median household income ($)	47788.68 (22022.89)	32269.15 (14781.94)
Family poverty rate	0.20 (0.15)	0.27 (0.19)
% Non-Hispanic white	0.21 (0.24)	0.13 (0.19)
% Children (< 18)	0.27 (0.09)	0.25 (0.11)
Land use (500m buffer)		
Low-density residential	0.34 (0.18)	0.21 (0.19)
High-density residential	0.09 (0.11)	0.16 (0.14)
Commercial	0.10 (0.08)	0.15 (0.10)
Other	0.22 (0.21)	0.21 (0.17)
Crime in 2010 (quarterly average)		
Violent crime	16.81 (15.74)	41.35 (42.56)
Property crime	29.41 (20.71)	50.86 (30.26)
Drug crime	9.14 (9.64)	41.33 (121.25)
Disorder crime	6.11 (22.16)	23.20 (48.15)

Note: Mean (SD) except for percent in City of Los Angeles.

Table C.2 is like Table 2 in the main text but focuses on the AH facilities that are included in the analysis. One difference from Table 2 is that because some AH facilities opened after the panel ended, there is an additional column for facilities that were “never-treated” by

2024. These are areas where AH facilities would go on to open after the panel ended, and so serve as control cases in the AH analysis.

Table C.2: Time-Varying Statistics on Affordable Housing Included in Main Analysis

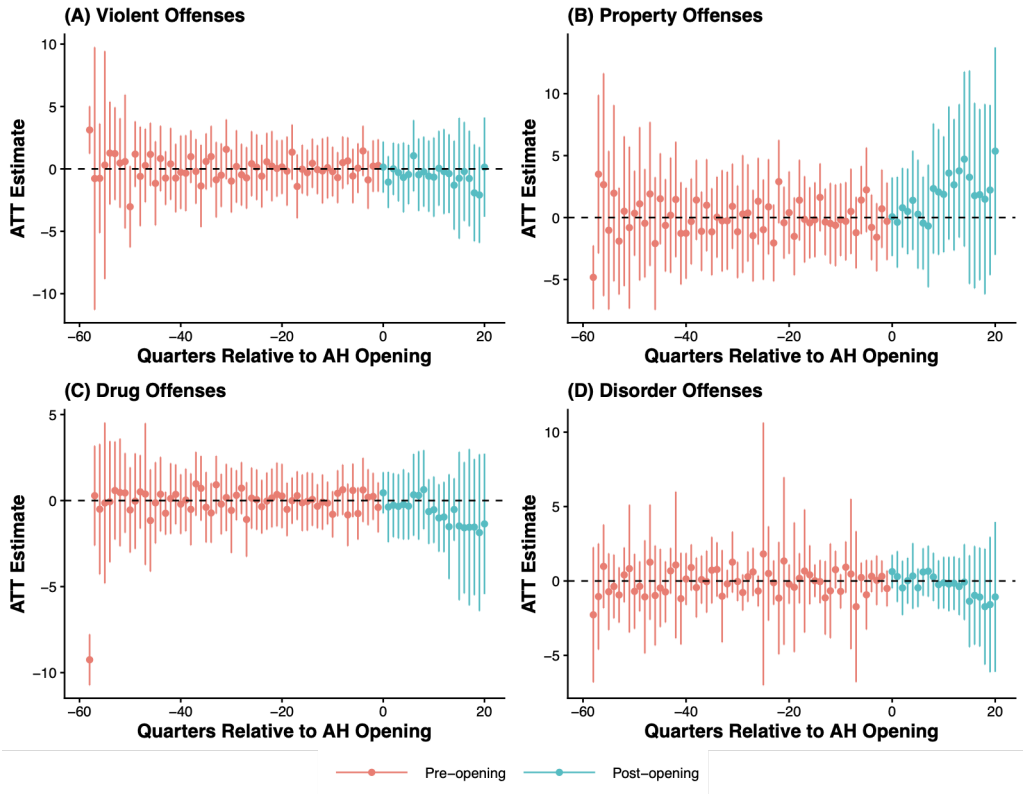
	Pre-opening AH	Post-opening AH	Never-treated AH [1]
Sample			
Facilities (n)	126	126	18
Facility-quarter observations	4,081	2,044	1,080
Quarters per facility (mean)	32.4	16.2	60.0
Mean quarterly crime incidents			
Violent	15.8	20.2	7.9
Property	28.2	34.9	22.0
Drug	6.4	5.4	3.2
Disorder	4.5	2.3	0.9

[1] Never-treated AH facilities have opening dates after the panel's end (Q4 2024), so they serve as controls throughout the sample window.

APPENDIX D: AFFORDABLE HOUSING DYNAMIC TREATMENT EFFECTS

Table 3 in the main text presents the overall ATTs of affordable housing facility openings on crime. To recall, all overall ATTs are statistically insignificant ($p > 0.05$), but the property estimate confidence interval was mostly positive. Figure D.1 presents the corresponding event time estimates, drawn from the same models. It shows that the property crime estimates become more positive over time, especially after affordable housing facilities have been open two years, suggesting that they may lead to increased property crime. Again, the estimates are statistically insignificant, but if they are true effects, this may reflect that property crime becomes more common in areas as they are developed with new properties, due to an increased opportunity to commit these crime types.

Figure D.1: Dynamic Effects of Affordable Housing on Crime, by Offense Type



Note: y-axes are on different scales. Effects are shown as crime counts in the 500-meter buffer surrounding AH facilities. Bars represent 95% confidence intervals.

APPENDIX E: ROBUSTNESS CHECKS FOR MAIN PSH RESULTS

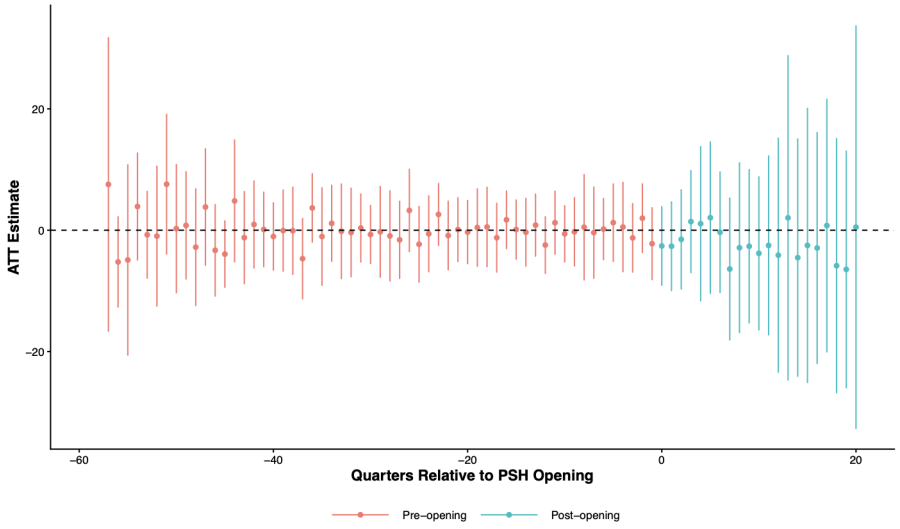
Figure 4 in the main text presents the results for ten robustness checks. Instead of presenting those results again, here we simply elaborate on what these robustness checks do:

1. **Control for land use:** adds measures of land use to the model, specifically, the proportion that is high-density residential, commercial, and other. These measures omit parcels that are zoned for low-density residential uses and areas with no parcel zoning, such as streets and sidewalks. Since we only have land use data from the City of Los Angeles, we limit this analysis to the 86 PSH facilities located within city boundaries and use LAPD data only.
2. **Yearly aggregation:** aggregates the panel to yearly instead of quarterly. In Figure 4, the estimates are divided by four to recover the quarterly scale.
3. **Two-stage DiD:** This uses the two-stage difference-in-differences estimator proposed by Gardner (2022), an imputation-based approach, instead of the Callaway and Sant'Anna (2021) approach.
4. **LA City/LAPD data only:** This analysis is limited to the 86 PSH facilities located within City of Los Angeles boundaries and uses LAPD data only.
5. **Weighted by facility size:** The estimates are weighed by the number of units in facilities that are restricted to the target population (e.g., excluding building managers' units), such that larger developments are upweighted.
6. **All post periods:** All available post-opening quarters are used, the panel is not trimmed at 20 quarters after opening.
7. **Allow PSH in the same area:** A PSH facility can enter the panel even if a PSH facility has opened nearby, so long as sufficient time has passed. Specifically, at least

24 quarters (6 years) must have passed: 20 quarters as the post-opening treatment period and 4 quarters as the pre-opening period.

8. **Broader crime measures:** for violent, property, and disorder offenses, these include a wider range of offense types. For violence, this includes offenses like sexual assault and kidnapping. For property crimes, this includes offenses such as fraud, forgery, and arson. For disorder, this includes liquor violations, prostitution, and gambling.
9. **Anticipation (8-qtr lead):** treats the 8 quarters before each facility's opening as potentially affected, due to the construction period, and so excludes that period from the comparison window.
10. **Trim low data cases:** Truncates the panel to the last quarter with at least 10 control cases (Q3 2023) and only estimates treatment effects for PSH facilities that have at least 4 quarters of post-opening data. The overall ATTs average across PSH facilities, so this check is intended to remove PSH facilities that may be based on limited treatment data or whose effects are estimated with a small control pool.

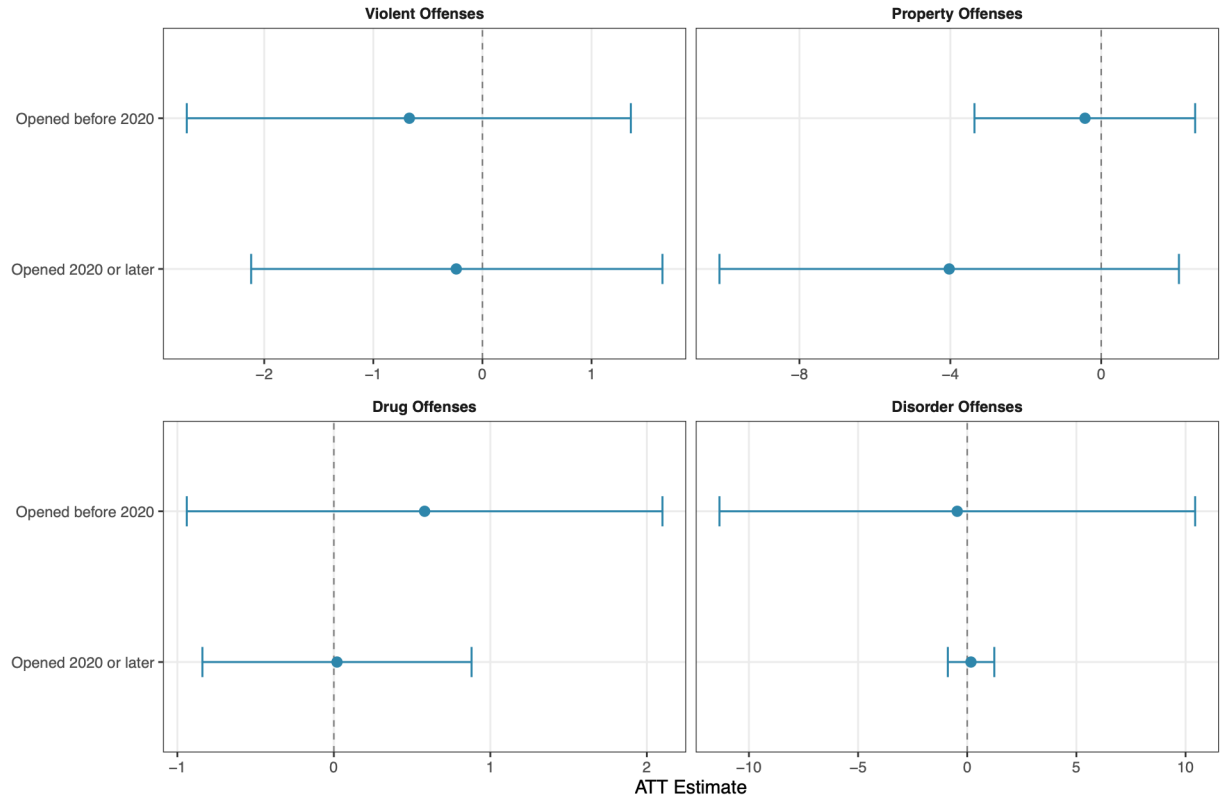
Figure E.1: Dynamic Effects of Permanent Supportive Housing on Total Crime



Note: Effects are shown as crime counts in the 500-meter buffer surrounding PSH facilities. Bars represent 95% confidence intervals.

Figure E.1 presents results from an additional robustness check, which is like the main model but uses total crime as the outcome. Total crime is defined as the sum of our measures of violent, property, drug, and disorder offending. We do not use all recorded crime incidents given differences between agencies and over time in the full set of offenses that are recorded. This model also reveals precise null effects ($p > 0.05$), with an overall ATT of -1.92 crimes per facility-quarter (95% CI: -9.92, 6.09), which in percentage terms relative to the counterfactual is a 2.61% reduction (95% CI: -13.50%, 8.29%). Figure E.1 shows the dynamic treatment effects, with flat pre-trends supporting the parallel trends assumption and null quarterly effects in the post-opening period.

Figure E.2: Effects of PSH Opening on Crime, by Opening Cohort and Offense Type



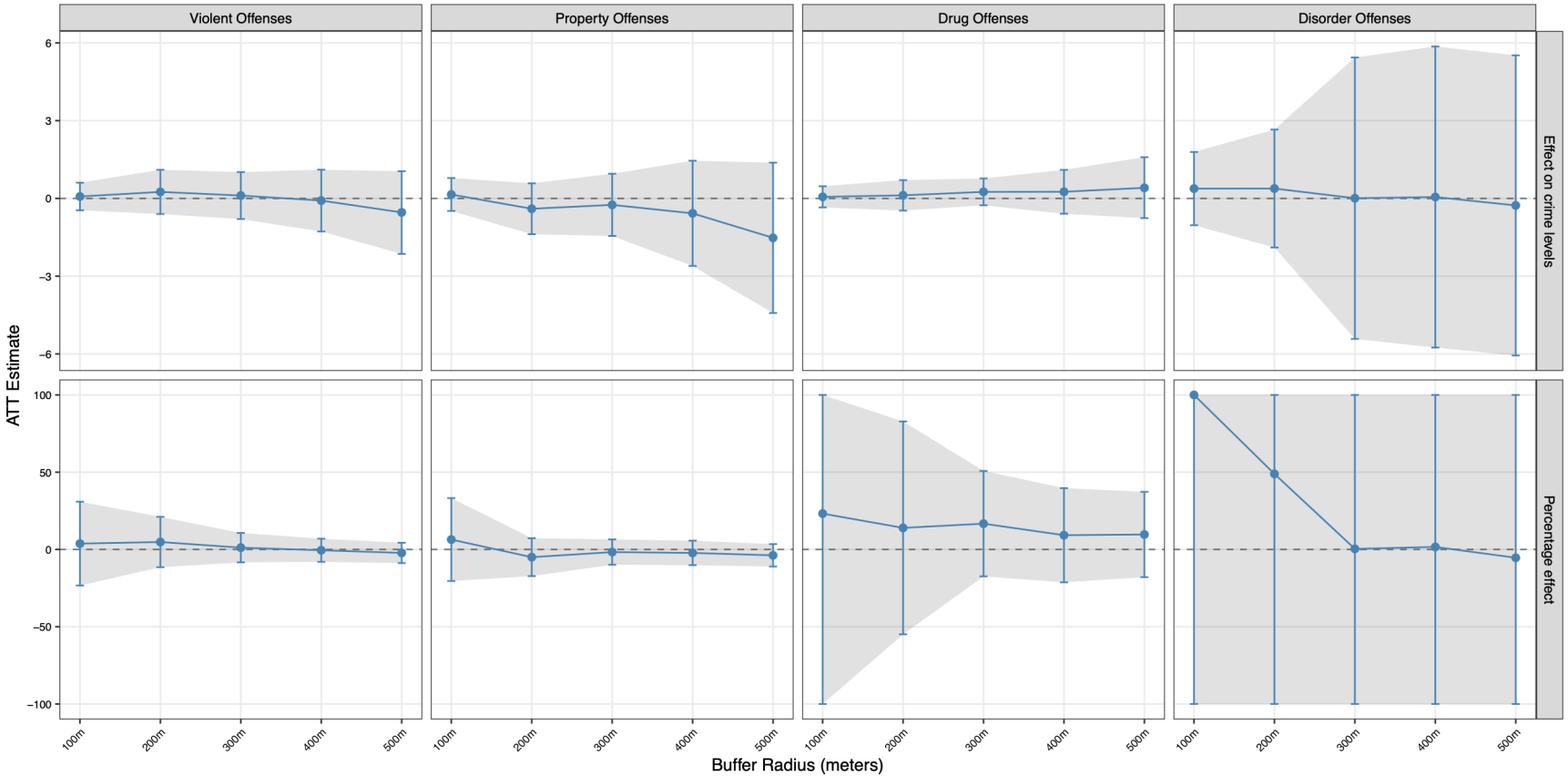
Notes: x-axes are on different scales. Effects are shown as crime counts per quarter in the 500-meter buffer surrounding PSH facilities. Bars represent 95% confidence intervals.

Figure E.2 shows the estimated effects when splitting the PSH sample into two cohorts: those which opened before 2020 ($n = 49$) and those which opened after 2020 ($n = 51$). This timeline is motivated due to Proposition HHH funding, which fundamentally reshaped the pipeline of new PSH development in Los Angeles; while Proposition HHH was passed in 2016, the first project funded under Proposition HHH did not open until January 2020. While Proposition HHH motivates our cohort-based split, we note that this timing also coincides with other changes, most notably the onset of the COVID-19 pandemic, a period which saw large swings in crime rates generally. In Figure E.2, none of the cohort-specific ATTs are statistically significant ($p > 0.05$). The point estimates for a given offense type across cohorts tend to be similar too, with the largest exception being for property crime, for which the estimated effect on property crimes per facility-quarter is about -4 for newer facilities compared to about 0 for the older cohort of PSH. These estimates are imprecise but are not consistent with the possibility that PSH with Proposition HHH funding lead to exceptionally positive crime effects.

Figure E.3 shows how estimated effects change for the 100 PSH facilities when examining different buffer sizes, from 500m (the main estimates) down to 100m, in both crime level and percentage terms. All crime level effect estimates are close to zero and statistically insignificant ($p > 0.05$). In percentage terms, the effects do appear to increase with small buffers. Though, with smaller buffers the overall level of crime is smaller, which amplifies percentage effects, and the estimates are much less precise. Figure E.4 is like Figure E.3 but allows the PSH sample to change, only removing overlapping PSH within the changing buffer distance. This increases the possibility of treatment contamination within this sample from nearby PSH openings. However, overall, Figure E.4 shows the same patterns as Figure E.3. In short, the percentage increase in crime may be higher nearer to PSH facilities, but these estimates are very

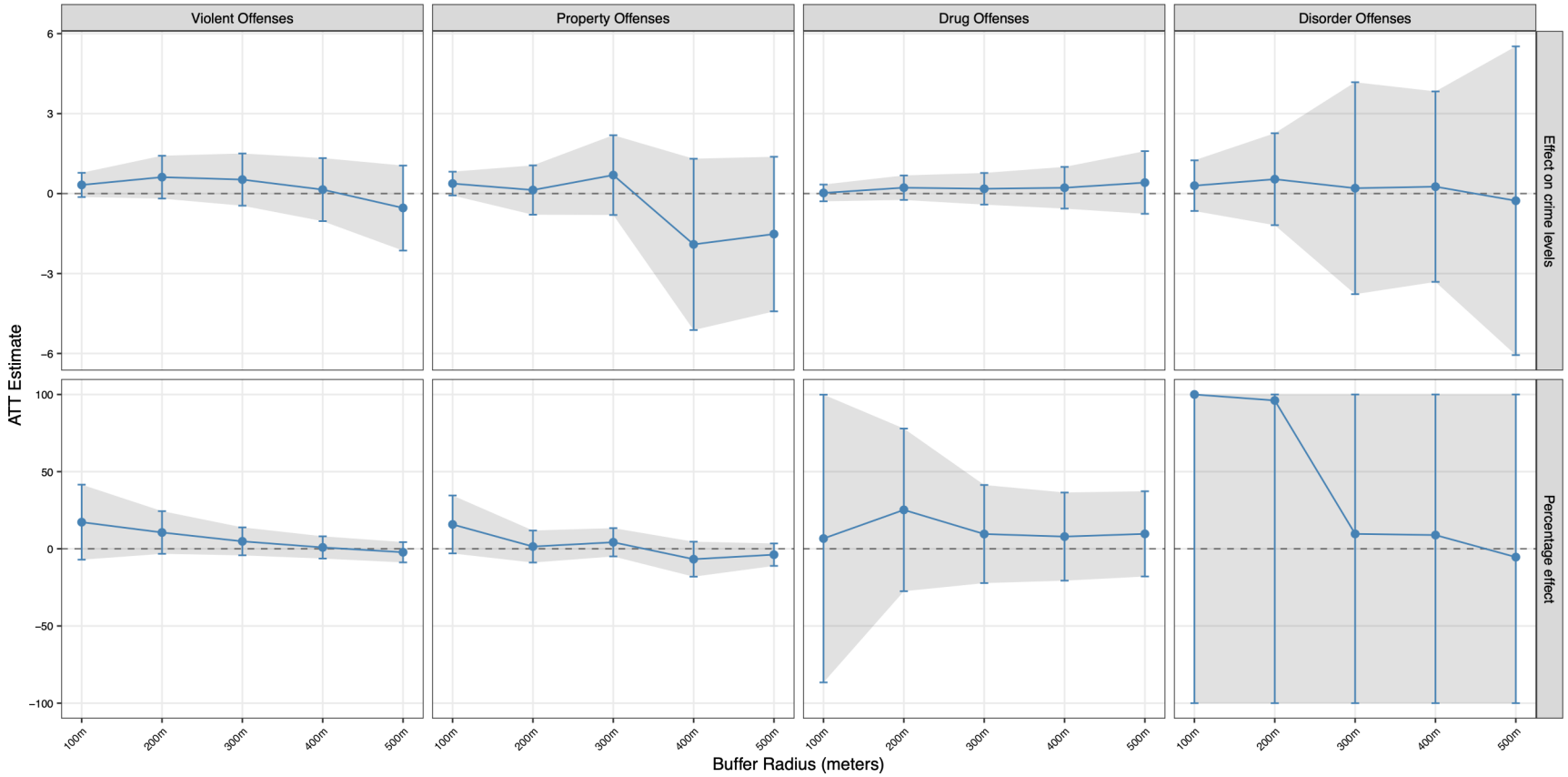
imprecise, based on low counterfactual levels of crime, and all remain statistically insignificant ($p > 0.05$). The disorder estimates are extremely imprecise, to the point of being uninformative.

Figure E.3: PSH Crime Effect Estimates as a Function of Buffer Distance, Using Same Facilities as Main Analysis



Note: Crime levels are averages per facility-quarter, percentage effects are relative to the counterfactual estimate. Bars and shading represent 95% confidence intervals. N = 100 PSH facilities, for every estimate. The percentage effect plot y-axis range is limited to +/- 100%.

Figure E.4: PSH Crime Effect Estimates as a Function of Buffer Distance, With Facilities Varying by Distance



Note: Crime levels are averages per facility-quarter, percentage effects are relative to the counterfactual estimate. Bars and shading represent 95% confidence intervals. The number of PSH facilities (N) varies with buffer distance. The percentage effect plot y-axis range is limited to +/- 100%.