Affordable Housing Construction and Local Public Safety: Evidence from Los Angeles*

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Abstract: Affordable housing development is a central policy tool for addressing housing insecurity. Local opposition to new projects often centers on concerns about public safety. We study the effects of new affordable housing on nearby crime in Los Angeles from 2005 to 2023 using administrative records on housing-site openings and permits linked to geocoded public safety data. Exploiting temporal and geographic variation in site openings, we find that any pooled effects are highly localized: 911 calls increase by roughly 20 percent in the immediate vicinity of new sites, but these effects attenuate quickly with distance and become difficult to detect at broader spatial scales. These average effects, however, mask substantial heterogeneity across housing types. Supportive housing developments are associated with much larger and more persistent increases in nearby crime, with the strongest effects concentrated near project sites but extending over a broader surrounding areas. By contrast, effects for non-supportive affordable housing are modest and generally similar to those of market-rate multifamily housing. The results suggest that the local public safety effects of affordable housing are not homogeneous across project types and concerns about public safety are most salient for supportive housing developments. These findings provide new and nuanced evidence that informs a contentious policy debate in which local public safety concerns have often constrained efforts to expand affordable housing and where those concerns are not typically incorporated into siting decisions.

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1 Introduction

Housing affordability has become a central policy concern in the United States. In 2022, about half of all renter households were cost-burdened, up 9.0 percentage points since 2001 (Joint Center for Housing Studies, 2024). In response, state and local policymakers have advanced a range of potential interventions, including zoning reform, rental subsidies, and incentives to build more affordable housing. Within this broader set of proposals, affordable housing development stands out as a central policy tool because it directly expands the supply of below-market units. Yet the stock of affordable housing remains far below need: nationwide, there are only 34 affordable and available rental homes for every 100 extremely low-income renter households, and the shortage grew by more than 480,000 homes between 2019 and 2022 alone (National Low Income Housing Coalition, 2024). Despite this persistent shortage and the rising share of cost-burdened households, new affordable housing development remains politically contentious at the local level. Supporters view new affordable housing developments as essential in high-cost cities like Los Angeles (Bass, 2024), while opponents often argue that they will reduce property values and place additional strain on local services such as schools and police (U.S. Department of Housing and Urban Development, Office of Policy Development and Research, 2018; Scally and Tighe, 2015). Among these concerns, public safety is one of the most salient and often figures prominently in neighborhood opposition to new development (Nguyen et al., 2013).

Despite the prominence of crime-related concerns in neighborhood opposition to new housing, the existing empirical evidence on the public safety effects of affordable housing remains limited in ways that are central to the ongoing debate. The prevailing literature, much of it focused on LIHTC-funded developments and broader neighborhood outcomes, generally finds null effects or modest declines in nearby crime and related indicators. But that evidence does not map cleanly onto the forms of local political conflict that arise most often in practice. Many existing studies pool together developments that differ substantially in the populations they serve, the intensity of resident needs, and the services and supports provided on site, even though there is little reason to expect spillovers to be uniform across housing types. At the same time, much of the existing evidence is not designed to detect highly localized effects concentrated immediately around project sites. If the opening of new housing affects public safety only within a very

small radius, those effects may be diluted or missed entirely in analyses conducted at broader geographic scales. These limitations matter because the most contentious neighborhood debates tend to focus not on affordable housing in the abstract, but on supportive housing for high-need populations, and because the concerns raised in those debates are typically hyper-local. Together, these considerations create a need for evidence that compares local public safety effects across distinct forms of affordable housing and measures those effects at the project-level spatial scales most relevant to neighborhood opposition.

In this paper, we examine the public safety effects of affordable housing construction in a way that speaks directly to the concerns that arise in local debates over new development. We do so in Los Angeles, a setting that combines severe housing affordability pressures with unusually rich administrative data and substantial heterogeneity across affordable housing developments. We assemble administrative records on affordable housing sites opening between 2005 and 2023, combining public financing records with permitting data to identify precise site locations, project characteristics, housing type, and the timing of initial occupancy. These data allow us to distinguish between supportive and non-supportive affordable housing rather than treating affordable housing as a single, homogeneous category. Supportive housing typically combines below-market units with coordinated services for residents facing barriers such as chronic homelessness, disability, or serious mental illness, whereas non-supportive affordable housing serves a broader low-income population, predominantly working families and seniors, without an embedded services component (U.S. Department of Housing and Urban Development, 2025, 2016). We then link these records to geocoded administrative data on 911 calls, arrests, and reported crimes to estimate how housing-site openings affect public safety in the immediate vicinity of project sites and how those effects attenuate with distance. We also construct a comparison sample of market-rate multifamily developments, which helps distinguish effects associated with subsidized housing from the broader neighborhood effects of new residential construction.

Our empirical strategy exploits staggered variation in the timing of housing-site openings to estimate how local public safety changes following the opening of new housing developments. We implement this design using an imputation-based difference-in-differences estimator following Borusyak et al. (2024), which is well suited to our setting given the staggered timing of treatment and the need to avoid biases associated with conventional event-study estimators.

Intuitively, this approach uses untreated site-quarters, i.e. pre-treatment observations for treated areas, to construct the counterfactual path of outcomes for treated sites. We use this framework in two complementary ways. First, we estimate a set of radius-based static difference-in-differences models in which outcomes are measured within treated areas ranging from 0.02 to 0.50 miles around each new development. The purpose of this exercise is to characterize the spatial footprint of any public-safety effects and to assess whether broader geographic definitions of treatment obscure changes that are concentrated very near project sites. Second, we turn to a more conventional event-study design using a fixed 0.25-mile treatment radius chosen on the basis of the radius-based estimates. The event study allows us to test for differential pre-trends and to distinguish between short-run responses around opening and more persistent shifts in local public-safety outcomes.

Our analysis reveals that the public-safety effects of new affordable housing are highly localized, but that pooled averages mask sharper differences across project types. In the difference-in-differences estimates that vary the treatment radius, 911 calls increase by roughly 20 percent in the immediate vicinity of a new housing site (approximately 0.02 to 0.10 miles) but attenuate quickly with distance, approaching zero and becoming statistically indistinguishable from zero when treatment is defined over broader surrounding areas (around 0.4 miles from the site). Consistent with this pattern, our main event-study estimates at a 0.25-mile radius yield comparatively small pooled effects when supportive and non-supportive affordable housing developments are combined. We find little evidence of differential pre-trends for property, nuisance, or violent 911 calls, and the corresponding post-opening estimates are modest relative to baseline levels. Taken together, these results suggest that any pooled public-safety effects of affordable housing openings are concentrated very near project sites and become substantially attenuated when outcomes are measured over broader geographic areas.

Pooling different housing types, however, masks an important finding: supportive affordable housing generates large and persistent increases in nearby crime and calls for service, whereas non-supportive affordable housing shows little evidence of comparable effects. In our main 0.25-mile event-study estimates, supportive housing sites are associated with increases of roughly 17 to 20 percent in property, nuisance, and violent 911 calls, again with little evidence of differential pre-trends, whereas the corresponding estimates for non-supportive housing are

smaller and generally not statistically distinguishable from zero. The radius-based estimates reveal an even sharper contrast at smaller distances from the focal site: for supportive housing, crime-related 911 calls increase by as much as 35 percent at treatment definitions between about 0.02 and 0.20 miles and remain statistically significant over a somewhat broader spatial range, whereas estimated effects for non-supportive housing attenuate much more quickly with distance. We find qualitatively similar patterns in reported crimes and arrests, suggesting that the results do not operate solely through a single reporting margin. By comparison, newly opened market-rate multifamily housing sites generate smaller and more localized changes in 911 calls, with magnitudes closer to those for non-supportive affordable housing than to those for supportive housing. Overall, these findings suggest that broad claims about the crime effects of affordable housing obscure two features that are central to the policy debate: any effects are highly localized, and the most substantial effects are concentrated among supportive housing developments. More broadly, our results suggest that the relevant policy question is not whether affordable housing, in the aggregate, affects neighborhood public safety, but which types of housing developments do so and over what spatial scale.

Our findings contribute to several literatures in urban economics, public economics, and the economics of crime. Most directly, we contribute to the literature on the local effects of affordable housing development on crime. Existing work has focused primarily on LIHTC-funded affordable housing and generally finds null or modestly beneficial effects on nearby crime and neighborhood conditions (Freedman and Owens, 2011; Diamond and McQuade, 2019; Hipp et al., 2022). In many metropolitan areas, however, LIHTC-funded projects are most often (though not exclusively) used to finance non-supportive housing. We show that the local public-safety effects of affordable housing vary dramatically by project type: effects are concentrated among supportive housing developments, while effects of non-supportive affordable housing are modest and difficult to distinguish from those of market-rate multifamily construction. Earlier studies of supportive housing in other settings find limited neighborhood harms (Galster et al., 2002; Been et al., 2008), but they examine much earlier cohorts in substantially different institutional settings. We also show that treatment effects attenuate rapidly with distance from the project site, implying that prior analyses based on broader geographic units may miss meaningful effects when they are concentrated very close to new developments. More broadly, our findings are consistent with

work showing that changes in public housing availability and siting can have different, and even opposing, effects across individual outcomes, local neighborhoods, and citywide crime (Bruhn, 2018; Jacob, 2004; Chyn, 2018). Overall, our results suggest that conclusions from the existing literature may not generalize uniformly to supportive housing and may miss policy-relevant effects at very local spatial scales.

We also contribute to a growing literature on the broader impact of policies addressing homelessness, since supportive housing in Los Angeles is primarily targeted to people who have experienced homelessness. Using linked administrative data on individuals who interact with housing assistance services in Los Angeles County, Cohen (2024) shows that rapid housing placement reduces future homelessness-system re-entry, criminal justice contact, and reliance on emergency cash assistance. In related work, Christopher et al. (2025) finds that expanding temporary shelter access reduces nearby crime and psychiatric hospital visits. A potential explanation for the difference from our findings is that temporary shelters and supportive housing affect the spatial distribution of high-needs populations differently: shelters may draw unsheltered individuals indoors from the surrounding areas, whereas supportive housing permanently concentrates a higher-acuity population at fixed sites and draws incomign residents from a larger geographic area. Consistent with this distinction, longitudinal survey data from Los Angeles document high rates of co-occurring mental health conditions, physical health conditions, and substance use disorders among populations likely to be served by supportive housing (Ward et al., 2024). Our findings are therefore not necessarily inconsistent with studies showing that shelters improve individual and neighborhood outcomes, because supportive housing may improve outcomes for targeted recipients while still increasing localized public safety incidents by concentrating a previously much more dispersed high-needs population at a fixed site. Consistent with this interpretation, we find substantially larger increases in 911 calls around supportive but not non-supportive housing sites, suggesting that resident composition is a key determinant of localized spillovers.

More generally, our paper contributes to a broader literature on housing supply, neighborhood change, and the local effects of new residential development. Recent work on market-rate housing has emphasized supply-side benefits: new construction generates moving-chain effects that reach lower-income households and neighborhoods (Mast, 2023; Bratu et al., 2023),

and nearby market-rate development can reduce rents in low-income areas (Asquith et al., 2023). Our comparison sample of market-rate multifamily developments allows us to speak to this literature from a different angle. We find that new residential development can generate highly localized changes in public safety outcomes, but that these effects are modest for market-rate housing and non-supportive affordable housing and substantially larger for supportive housing. This comparison suggests that the relevant distinction is not simply between subsidized and unsubsidized construction. Rather, the magnitude and spatial reach of local spillovers appear to depend importantly on housing type and the population served.

From a policy perspective, our findings do not imply that supportive housing is socially undesirable. Our estimates are local: they capture changes in measured public safety activity near new developments, not the net citywide effects of affordable housing on crime or disorder. Affordable housing may still generate substantial social benefits, including reductions in homelessness and improvements in housing stability, even if openings are associated with localized increases in 911 calls, reported crimes, and arrests near project sites. These localized costs are also not uniform across housing types, but are concentrated among supportive housing developments serving higher-needs populations at fixed sites. Our data also do not allow us to definitively disentangle the mechanisms underlying these increases. In particular, the observed effects could reflect victimization of new residents, offending by new residents, changes in police enforcement, or changes in neighbors' propensity to report disorder or call the police (Weisburd et al., 2024; Gonçalves and Quijano, 2025; Pandey, 2025). More broadly, the results suggest that the relevant policy question is not whether affordable housing is harmful in general, but how supportive housing is sited, managed, and paired with services so that its potential broader social benefits can be realized while minimizing local public safety costs.

2 Background and Institutional Setting

A commonly cited definition of affordable housing, provided by the Department of Housing and Urban Development (HUD), defines affordable housing as housing for which the occupant is paying no more than 30 percent of their gross income for housing costs, including utilities (U.S. Department of Housing and Urban Development, 2024). That definition is broader than

the one we use in this paper. It includes, for example, tenant-based assistance and naturally occurring affordable housing in low-rent markets. In our setting, we define affordable housing more narrowly as rental housing that is provided at below-market rates and financed at least in part through government subsidies or other public assistance. These developments are typically subject to income-based eligibility restrictions defined relative to Area Median Income (AMI), and may also target particular populations such as seniors, families, or individuals experiencing homelessness. Our analysis focuses on subsidized affordable housing developments in the City of Los Angeles that are recorded in administrative data maintained by the Los Angeles Housing Department (LAHD), which we use to define the universe of affordable housing sites considered in the study.

Los Angeles provides a useful setting for studying the local effects of affordable housing because it combines severe housing unaffordability with an active but heterogeneous subsidized housing pipeline. Over the past decade, housing costs in the Los Angeles area have risen sharply, and in recent years more than 46 percent of households in Los Angeles County were classified as housing cost-burdened, meaning they were paying more than 30 percent of their monthly income toward housing costs (U.S. Census Bureau, 2024). In response, city leaders have increasingly emphasized the need to expand affordable housing production and reduce barriers to development (Bass, 2024). Despite that emphasis, affordable housing production has historically remained limited relative to need. According to the Los Angeles City Planning Annual Housing Reports, the city completed an average of 14.5 affordable housing projects per year between 2015 and 2018, corresponding to roughly 854 units annually, while from 2019 to 2021 that pace rose to an average of 53 projects and 3,776 units per year (Central City Association, 2022). Even with this increase, production remains well below projected regional need: relative to the 2021–2029 Regional Housing Needs Assessment (RHNA), affordable housing output in the Los Angeles region would need to more than triple to meet projected demand.

An important institutional feature of the Los Angeles setting is the distinction between supportive and non-supportive affordable housing. Non-supportive affordable housing provides income-restricted rental units without an embedded services component and generally serves a broader low-income population, including families and seniors. Supportive housing, by contrast, pairs rent-restricted units with coordinated services such as case management, behavioral health

referrals, and benefits assistance for residents facing barriers such as chronic homelessness, disability, or serious mental illness. In Los Angeles, many supportive housing developments follow a Housing First model in which housing is provided without preconditioned treatment compliance or sobriety requirements, with supportive services layered on after placement. This distinction is central to our analysis because supportive and non-supportive housing differ not only in the populations they serve, but also in the service environments they create and the public debate surrounding them. In many local discussions, both are grouped together under the broad label of "affordable housing," even though their resident populations, service models, and potential neighborhood spillovers may differ substantially.

The distinction between supportive and non-supportive housing also matters for how public safety outcomes may be generated in administrative data. Supportive housing sites often include more intensive property management, case management, and other on-site staffing arrangements than standard income-restricted family or senior housing. Those institutional differences may affect not only underlying neighborhood conditions, but also the probability that incidents are formally reported. For that reason, our analysis examines multiple public safety outcomes–911 calls, reported crimes, and arrests–rather than relying on a single measure of neighborhood conditions. Throughout the paper, we therefore estimate effects separately for supportive and non-supportive developments. See Appendix A.1 for further detail on project classification, housing characteristics, and examples of each housing type.

Funding for affordable housing in Los Angeles comes from a number of sources at the local, state, and federal levels. LAHD maintains records on affordable housing projects receiving city financing beginning in 2003, and these records form the basis of our housing sample. Local financing is provided through a number of programs including the Affordable Housing Managed Pipeline (AHMP), the Supportive Housing Program (SHP), the Affordable Housing Bond Program (AHBP), and the Proposition HHH Supportive Housing Loan Program. Federal funding sources, most notably the Low-Income Housing Tax Credit (LIHTC) program, also play a major role in the financing of affordable housing construction and rehabilitation, with programs such as the Community Development Block Grant also providing funds. Over our sample period, supportive housing expanded substantially in Los Angeles, especially through Proposition HHH and related programs, with many supportive housing projects reaching occupancy during

the later years of our sample. That expansion makes Los Angeles particularly well suited for comparing supportive housing to more traditional affordable housing during a period in which homelessness and neighborhood disorder were especially salient in local policy debate.

The choice of location for new affordable housing in Los Angeles is shaped by a combination of land availability, financing constraints, local regulations, and neighborhood politics. Zoning rules have historically limited the development of multifamily housing in many neighborhoods, particularly in more affluent areas dominated by single-family zoning. Recent state and local initiatives, including Senate Bill 4 and Executive Directive 1, have attempted to relax these constraints and expand the set of feasible locations for affordable housing construction (Christopher, 2024). At the same time, longstanding debates over concentrated poverty, neighborhood opportunity, and fair-housing goals have also shaped where subsidized housing can be built. These siting forces matter for interpretation. Our difference-in-differences design absorbs permanent differences across locations, but neighborhoods may still differ in how responsive observed public safety measures are to the opening of a new housing site or institutional anchor.

The timing of project openings is also important in this setting. Affordable housing projects in Los Angeles often face long and variable lags between planning, permitting, financing, construction, and final occupancy. This process spans from initial planning permits to final occupancy certification, which allows sites to begin leasing, and the associated delays can in some cases last for many years (Parker and Mai-Duc, 2023). Because our empirical design is centered on the timing of site openings, this variation in occupancy timing provides a key source of identifying variation. To measure it, we combine LAHD financing records with permitting data from the Los Angeles Department of Building and Safety (LADBS), using certificates of final occupancy when available and supplementing them with LAHD online project listings and Proposition HHH funding reports when necessary in order to identify when sites actually begin occupancy.

3 Data

3.1 Affordable Housing Data

We begin by gathering geolocated records on affordable housing construction between the years 2005 and 2023. We track affordable housing using data from the City of Los Angeles Housing Department's (LAHD) records on affordable housing projects funded by LAHD during this time period. For each housing project, these records contain detailed descriptions of the project including the address, number of units, the type of construction (either the rehabilitation of an existing structure or the construction of a new structure), and the population intended to be served by the project (supportive vs non-supportive).

We augment the LAHD records with detailed permitting data from the Los Angeles Department of Building and Safety (LADBS). For each housing project included in the LAHD records, we use the listed Assessor Parcel Number (APN) to link the site with all permits listed in the LADBS files corresponding to that APN. By doing so, we are able to observe all permits associated with the construction of that housing site, including the Certificate of Final Occupancy (CFO), which is required for that housing site to begin leasing to renters. We use the listed date of the CFO permit as the start of occupancy date for a given affordable housing site when it is available.[1] Given the administrative nature of the LADBS records, we are not always able to find relevant permits for each affordable housing site included in the LAHD records either because we are not able to match any permits to a given site's APN, or we are unable to specifically match a relevant permit from around the LAHD-listed occupancy start year. In these cases, we use two complementary data sources to determine the start of occupancy - the LAHD's online listings of affordable housing sites, and the Prop HHH funding reports provided by LAHD. Using these sources, in addition to LADBS records, we are able to determine the date of the start of occupancy for 334 housing sites listed in the LAHD records.[2] Throughout the paper, we estimate regressions

[1] We also use finaled building permits that describe completed work relevant to the construction or rehabilitation of affordable housing in the event that a CFO permit is not available in the LADBS records for a given housing site in the same year as the LAHD-listed occupancy start year.

[2] The LAHD records list 570 affordable housing sites. Of these, 107 sites were either completed outside the time range considered in this study (before 2005) or were listed as still under development, leaving 463 housing sites under consideration for inclusion in our sample. We identify occupancy start dates for 334 (72%) of these sites, leaving 129 unmatched sites. A given site may fail to match for one of several reasons. First, as noted above, not all sites have permitting or Prop HHH records available. Additionally, a number of these sites were already occupied during the year in which they were listed as having been constructed or rehabilitated according to LAHD records. Thus, being

separately for supportive housing and non-supportive housing projects. See Appendix A.1 for more details on affordable housing sites.

3.2 Market-Rate, Multi-Family Housing

A unique feature of this study within the broader affordable housing literature is our ability to construct a sample of non-affordable housing sites to serve as a comparison group. In order to do this, we leverage the LADBS records described above to identify 1,567 residential, multifamily housing (MFH) sites which were issued certificates of final occupancy (COFO) between 2005 to 2023. For each housing site, we confirm that their permit work descriptions do not include any references to the construction or addition of affordable housing units and confirm are not listed in our sample of affordable housing sites permitted during this same time period. To ensure comparability with the LAHD sample, we restrict attention to MFH sites with unit counts between the 10^{th} and 90^{th} percentiles of the LAHD unit-count distribution (20 to 107 units), yielding an estimation sample of approximately 453 MFH sites. This trim excludes small infill-style developments with no LAHD counterpart while retaining sites of comparable scale to the affordable housing projects in our sample. Using this sample of housing sites, we construct a MFH housing site-by-quarter working data set analogous to that used to generate the affordable housing site results above.

3.3 Public Safety Data

To measure crime across Los Angeles, we collected micro-data on 911 calls (more than 12 million calls made between 2007 to 2021), reported crime (7.4 million incidents reported between 2005 and mid-2023), and arrests (2.2 million arrests made over the years 2005 to 2023) from Los Angeles Police Department (LAPD). Each of these records includes information on the incident date and address of the incident, as well as a description of the offense. We define three broad categories - property, violent, and nuisance-related incidents. Property crimes include larceny, burglary, and theft of motor vehicles. Violent crimes include all crimes against a person, such as assault, threats, sexual offense, and homicide, while nuisance-related include minor disturbances, intoxication,

listed in LAHD records in isolation was not a guarantee that a particular site had in fact opened in the listed year, providing further motivation for our decision to merge these records with permitting and Prop HHH data.

indecent exposure, mental-health and drug-related calls.[3] See Appendix A.2 for more details on public safety data.

3.4 Mapping Outcomes to Housing Sites

We map each public safety incident in our data to nearby housing sites. For each crime incident, we calculate the distance between that incident and nearby housing sites, and retain all incident-site pairings within 0.5 mile. Given a distance cutoff r, we can define a circle around a given affordable housing site with radius r and retain all incident-site pairings within r. This allows us to aggregate our data to the housing site-by-quarter level by calculating the total number of crime incidents within r distance of each affordable housing site for each quarter. Because treatment areas around nearby housing sites may overlap, a single incident can appear in the outcome counts of multiple sites. This is consistent with our site-centric estimand, which measures the change in public safety conditions in the area surrounding each individual housing site opening. Standard errors are computed at the housing-site level to account for serial correlation within site-level outcome series.

3.5 Summary Statistics

Table 1 shows that our final sample of affordable housing sites constructed during our sample time period is comprised of 334 projects and approximately 21,000 units for which we are able to determine the occupancy start date. The median housing site by size contains 55 individual housing units, with the distribution ranging from 21 units for the 10^{th} percentile site and 104 units for the 90^{th} percentile site. Finally, we observe the type of construction - either rehabilitation of an existing structure or the building of a new structure for 316 (95%) of the sites in our sample. Of these 316 sites, 74% were newly constructed, while 24% involved rehabilitation. Among this sample, 154 sites (46%) are supportive housing projects that provide services to their resident population, and 54% are non-supportive affordable housing units serving families and seniors.

Panel II of Table 1 reports summary statistics for the analytical dataset measured at

[3]For arrest and reported crimes, we use offense descriptions contained in text fields from these records to classify incidents into charge categories using the Text-Based Offense Classification algorithm developed by (Choi et al., 2023) to define property and violent crimes. For 911 calls, we use call description to categorize calls into property, violent, and nuisance-related calls.

the housing-site-by-quarter level. Within a 0.25-mile radius of housing sites, the average quarter includes roughly 79 violent 911 calls and 30 property-related 911 calls, along with about 37 reported violent crimes and 44 reported property crimes. Arrests are less frequent, averaging about 10 incidents per quarter for both violent and property offenses. When the radius is expanded to 0.50 miles, incident counts increase, reflecting the larger geographic area included in the measure. For example, the average number of violent 911 calls rises to approximately 247 per quarter, while reported violent crimes average about 121 incidents. Property-related incidents show similar increases, with roughly 106 property-related 911 calls and 160 reported property crimes per quarter. The analytical dataset contains 24,716 housing-site-by-quarter observations.

4 Empirical Strategy

Our empirical approach is designed to answer two related questions. First, how localized are any public-safety effects of housing-site openings? Second, conditional on a reasonable definition of the local treatment area, what is the timing and magnitude of those effects? We therefore begin with a set of radius-based static estimates that vary the radius used to define the treated area around each site. We then turn to a staggered event-study framework, which serves as our main specification for tracing the dynamic effects of site openings and assessing the credibility of the identifying assumptions.

Throughout, the unit of observation is a housing-site-by-quarter, and outcomes are measured as counts of incidents occurring near each site. We estimate all specifications in levels rather than logs because a nontrivial share of site-quarter outcome cells, especially at narrow treatment radii and for lower-frequency outcomes, are zero. The linear specification therefore preserves the full sample and yields coefficients that are directly interpretable as changes in the number of incidents near a site. For presentation, we sometimes scale coefficients by the corresponding pre-treatment mean to express magnitudes in percentage terms. All specifications are weighted by the number of units in the housing development so that larger projects receive greater weight in the estimation. This weighting reflects the fact that a 2-unit rehabilitation and a 20-unit development represent substantively different neighborhood interventions and prevents very small sites from exerting the same influence on the estimates as much larger projects.

A further unifying feature of our empirical approach is that both the radius-based static estimates and the event-study estimates are implemented using the imputation-based estimator of Borusyak et al. (2024). Because treatment timing is staggered across housing sites, this estimator is well suited to our setting. It recovers treatment effects using untreated observations, i.e. pre-treatment observations for treated locations, rather than relying on comparisons in which already-treated units serve as controls for later-treated units. This is important because conventional two-way fixed-effects estimators can produce misleading estimates in staggered-adoption settings when treatment effects are heterogeneous across cohorts or over event time.

4.1 Radius-Based Static Estimates

A central challenge in this setting is that the geographic footprint of any public-safety effect is not known apriori. If the consequences of new housing are highly localized, analyses conducted at broad geographic levels, e.g. census tracts or large fixed buffers, may substantially attenuate true effects by averaging treated micro-areas together with nearby places that are effectively untreated. Conversely, if effects extend farther into surrounding neighborhoods, very small treatment radii may miss relevant spillovers. Our first empirical exercise is therefore meant to characterize the spatial decay of treatment effects and to discipline the choice of treatment radius used in the main specifications below. More generally, our radius-based exercise is in the spirit of urban spillover designs that allow treatment intensity to vary with geographic proximity, such as Autor et al. (2014), who study spillovers using radius-based exposure measures and inner-versus-outer distance bands, and Diamond and McQuade (2019), who develop a difference-in-differences style estimator in which treatment varies smoothly with distance to LIHTC developments.

To do so, we estimate a sequence of radius-specific static difference-in-differences specifications. For each radius r, we construct the outcome using incidents that occur within a circle of radius r around site h and estimate the average post-opening effect within that treated circle. Specifically, for each radius r, we estimate:

$$Y_{hq}(r) = \beta_{Post}(r)\,(Occupancy\ Start_{hq}) + \gamma_h + \tau_q + \epsilon_{hq}, \tag{1}$$

where $Y_{hq}(r)$ is the number of public-safety incidents occurring within a circle of radius r around

housing site h in quarter q, γ_h denotes housing-site fixed effects, and τ_q denotes quarter fixed effects. *Occupancy Start*$_{hq}$ is an indicator equal to one beginning in the quarter in which site h first becomes occupied.

This specification compares outcomes near the same site before and after occupancy start while netting out time-invariant differences across sites and common quarter-level shocks. By re-estimating equation 1 over a fine grid of treatment radii, we can trace out how the magnitude and precision of the estimated effects evolve as the definition of treatment expands outward from the site. In practice, this exercise helps answer whether any detectable effects are concentrated immediately adjacent to new developments or instead persist at broader distances. That distinction is substantively important in its own right and also informs the design of the event-study analysis that follows. We view this exercise as documenting the spatial decay profile of treatment effects rather than as identifying a single mechanically optimal treatment radius.

We estimate equation 1 separately for radii ranging from 0.02 to 0.50 miles in 0.01-mile increments using a housing-site-by-quarter panel. This range allows us to examine whether results are driven by a particular spatial definition of treatment rather than imposed by an arbitrary administrative boundary or a single fixed buffer chosen in advance. To maintain comparability with the dynamic specifications below, we restrict the post-treatment sample to the first eight quarters after occupancy start and exclude the two quarters immediately preceding treatment as a pre-treatment donut. Identification in these radius-specific estimates comes from pre-treatment site-quarters within the staggered-adoption sample rather than from areas outside the candidate treatment radius. All estimates are weighted by the number of units in the housing site. Standard errors for the radius-based estimates are computed using the analytical inference procedure derived in Borusyak et al. (2024) for the imputation estimator.

As mentioned, we view this radius-based exercise as complementary to, rather than a substitute for, the event-study design below. Its main purpose is to document the spatial scale at which any effects appear in the data and to assess whether broader-area specifications risk obscuring highly localized treatment effects.

4.2 Event Study Estimates

After establishing the spatial scale at which treatment effects are most likely to appear, we turn to a staggered event-study design that serves as our main empirical specification. Whereas the radius-based static estimates are primarily diagnostic about the geographic footprint of effects, the event study is designed to recover the timing and persistence of those effects and to evaluate the plausibility of the identifying assumptions. In particular, the event-study framework allows us to test for differential pre-trends, examine whether any changes occur sharply around the opening date, and distinguish short-run responses from effects that persist over time.

Using data aggregated to the housing-site-by-quarter level for sites that eventually open during the sample period, we estimate the following event-study specification:

$$Y_{hq} = \alpha_0 + \sum_{\substack{k=-8 \\ k \notin \{-2,-1\}}}^{7} \beta_k \, D_{hq}^{k} + \gamma_h + \tau_q + \epsilon_{hq}, \tag{2}$$

where Y_{hq} is the count of public-safety incidents within a 0.25-mile radius of housing site h in quarter q, γ_h and τ_q denote housing-site and quarter fixed effects, respectively, and $D_{hq}^{k} = \mathbf{1}\{q - T_h = k\}$ is an indicator equal to one when quarter q is k periods away from the occupancy-start quarter T_h for site h. We define $k = 0$ as the first quarter of occupancy. The estimation window spans event time $k = -8$ through $k = 7$, omitting event times $k \in \{-2, -1\}$ as a donut period and using $k = -9$ as the reference period.

We focus the event-study analysis on a 0.25-mile radius for two reasons. First, this radius is informed by the radius-based estimates, which indicate that any measurable effects are concentrated in relatively local areas around housing sites. Second, a common radius facilitates direct comparison across outcomes and across housing types while preserving enough geographic scope to maintain statistical precision. More generally, fixing a common treatment radius in the event study allows us to shift attention from the spatial reach of effects to their temporal evolution. We therefore interpret the 0.25-mile specification together with the full radius-based profile rather than as a radius selected to maximize statistical significance.

The exclusion of the two quarters immediately prior to occupancy start is important in our setting and intended to avoid treating the construction phase as part of the identify-

ing pre-period. Since construction itself has been shown to affect local crime conditions, the quarters immediately preceding occupancy may not provide a clean baseline for post-opening comparisons or pre-trend tests.[4] We therefore anchor event time to occupancy start and omit the preceding two quarters as a donut period.

The event-study coefficients β_k trace out the path of outcomes relative to the omitted pre-treatment period. The pre-treatment coefficients provide a direct diagnostic for whether treated sites were already on differential trends prior to occupancy start. Site fixed effects address time-invariant differences in where projects are located, while the absence of differential pre-trends is our primary evidence against the concern that sites open contemporaneously to local crime shocks or other time-varying neighborhood changes. Under the identifying assumption that untreated potential outcomes evolve similarly across cohorts conditional on site and quarter fixed effects, the post-treatment coefficients can be interpreted as estimates of the causal effect of site openings on nearby public-safety outcomes at each event time. This dynamic structure is useful because it allows the data to show whether any effects emerge immediately upon opening, build gradually over time, or dissipate after an initial adjustment period.

In addition to the fully dynamic event-study specification, we also report a static post-treatment summary estimate using an indicator equal to one in quarters at or after occupancy start. As above, we restrict the post-treatment window to the first eight quarters after treatment and exclude the two quarters immediately preceding treatment. This static specification is not meant to replace the event study. Rather, it provides a compact summary of the average post-opening effect over the same event window used in the dynamic analysis. Because individual event-time coefficients can be noisy, the static difference-in-differences estimate is useful for summarizing overall magnitudes and facilitating comparisons across outcomes, subsamples, and alternative housing types.

As in the radius-based exercise, identification comes from untreated site-quarters within the staggered-adoption sample rather than from a separate set of nearby never-treated control areas. In other words, we use pre-treatment observations to impute the counterfactual path of

[4] Although we observe permitting data for each site, those records often measure the timing of actual construction imprecisely because permit dates are revised as projects are delayed, modified, or extended. Our review of permitting records for most sites suggests that two quarters is a reasonable uniform proxy for the plausible construction period in our sample, where projects more commonly involve rehabilitation and substantial remodeling than full teardown and new construction.

outcomes for treated site-quarters. Standard errors for the event-study and static post-treatment estimates are computed using the analytical inference procedure derived in Borusyak et al. (2024) for the imputation estimator.

5 Results

5.1 The Relationship Between Proximity to Affordable Housing and Crime

Distance Estimator. We first establish whether proximity to affordable housing sites affects public safety. Panel A of Figure 1 presents the radius-based estimates described in Section 4.1. The radius of the circle defining the treated area around each housing site is indicated by the X-axis, while the average effect is reported on the Y-axis. We report estimated effects and corresponding 95 percent confidence intervals here as percentage changes to account for differences in the frequency of 911 calls (as the treatment radius increases linearly, the treated *area* increases non-linearly, and thus crime frequencies are substantially larger at larger treatment radius values).

The estimated effects are strongest at very small distances from housing sites. For property crime 911 calls, we observe increases of roughly 20 percent for treatment radii between 0.02 and 0.2 miles. As the radius expands beyond 0.2 miles, however, the estimated effects decline and are no longer statistically distinguishable from zero.

This pattern suggests that any changes in crime associated with affordable housing openings are highly localized. As the treatment radius expands, the estimated effect attenuates, consistent with the strongest responses being concentrated near the site and becoming diluted when outcomes are aggregated over broader surrounding areas.For example, the statistically insignificant 5 percent increase in 911 calls for property crime that we estimate with a treatment distance of 0.4 miles suggests that the significant increases observed at a treatment distance of 0 to 0.2 miles is relatively small compared to the muted impact at farther distances, such that the net or overall impact at 0.4 is effectively 0. If we were to either define treated areas over larger distances, or aggregate our data to the tract level, as in several prior studies, we would fail to observe these localized effects.

Event Study. We next estimate dynamic effects using the event study specification described in Equation 2. Treated areas are defined as locations within 0.25 miles of an affordable housing site, and the event window spans eight quarters before and after site occupancy. Panel A of Table 2 and Panel B of Figure 1 report results for property, nuisance-related, and violent 911 calls.

Across all outcomes, we find little evidence of differential pre-treatment trends. Joint tests of the pre-treatment coefficients yield p-values of 0.38 for property calls, 0.30 for nuisance calls, and 0.52 for violent calls, consistent with parallel trends at conventional significance levels.

In the post-period, we find little evidence of changes in 911 calls following the opening of affordable housing sites. For property-related calls, the static difference-in-differences estimate is 1.66 (p-value = 0.09) relative to a sample mean of 30 calls. Estimates for nuisance-related calls (β = 2.14, p-value = 0.22, mean = 52) and violent calls (β = 3.62, p-value = 0.18, mean = 79) are similarly small and statistically insignificant. While the radius-based estimates above indicate localized increases at very short distances, the event study estimates at a fixed 0.25-mile radius suggest that these pooled effects are small relative to baseline levels and not statistically significant at conventional levels.

5.2 Non-Supportive versus Supportive Housing Sites

We next examine whether the effects of affordable housing openings differ between supportive and non-supportive housing developments. Non-supportive affordable housing consists of income-restricted rental units serving a broad population of low-income households, including working families and seniors. Supportive housing differs in that it provides residents with access to coordinated services. The analysis follows the same structure as above.

Distance Estimator. Panel A and Panel B of Figure 2 present radius-based estimates separately for non-supportive and supportive housing sites. For non-supportive housing sites, the spatial pattern of the estimates closely resembles the pooled results presented in Section 5.1: estimated effects are concentrated within very small distances of housing sites and decline rapidly as the treatment radius increases.

In contrast, the estimates for supportive housing sites indicate larger and more persistent effects. For supportive housing openings, we observe increases in crime-related 911 calls of

approximately 35 percent at treatment radii between roughly 0.02 and 0.20 miles. Unlike non-supportive housing sites, these effects remain statistically distinguishable from zero at somewhat larger distances, suggesting a broader spatial footprint.

Event Study. We next estimate dynamic event-study effects separately for non-supportive and supportive housing sites. Figure 3 presents the event-study estimates, while Panels B and C of Table 2 summarize the corresponding post-treatment effects.

For both housing types, we find little evidence of differential pre-treatment trends. Joint tests of the pre-treatment coefficients fail to reject the null of equal pre-trends across specifications.

The post-treatment dynamics, however, differ between the two housing types. For non-supportive housing sites, the estimated effects remain small and statistically indistinguishable from zero across most outcomes, indicating no detectable changes in local crime following site openings. By contrast, estimates for supportive housing sites indicate statistically significant post-treatment increases, particularly for property-related outcomes. We observe immediate and persistent increases following site occupancy. The static difference-in-differences estimate is 13.69 (p-value = 0.01), relative to a pre-treatment mean of 120 calls, corresponding to an increase of approximately 11.5 percent.

Nuisance-related calls also increase following supportive housing openings. Event study coefficients indicate sustained post-treatment increases, with magnitudes ranging from approximately 15 to 40 calls. The static difference-in-differences estimate implies an increase of approximately 13 percent relative to the sample mean. Violent 911 calls similarly increase in the post-period, with an estimated effect of approximately 12 percent. Overall, these results indicate that while public safety responses to affordable housing openings appear highly localized in both cases, supportive housing developments exhibit larger and statistically significant effects relative to non-supportive developments.

5.3 Market-Rate Multi-Family Housing

We next examine the local public safety impacts of market-rate, multifamily housing (MFH) openings. This analysis provides a benchmark for interpreting the preceding results by assessing

whether localized changes in public safety outcomes are specific to affordable or supportive housing, or instead reflect more general effects of new residential development.

Distance Estimator. Figure 4 reports radius-based estimates for MFH sites. Similar to the patterns observed for both non-supportive and supportive housing, MFH openings are associated with changes in 911 calls that are highly localized: estimated effects are largest at very short distances from housing sites and attenuate rapidly as the treatment radius increases. Beyond approximately 0.25 miles, estimated effects become small and are no longer statistically distinguishable from zero, indicating that any public safety responses to MFH openings are concentrated in the immediate vicinity of the development.

Event Study. Figure 4 reports event-study estimates for MFH openings, defining treated areas as locations within 0.25 miles of a housing site. For property and nuisance-related calls, we find little evidence of differential pre-treatment trends (pre-period joint test p-values of 0.811 and 0.550, respectively). For violent calls, the pre-period joint test is marginally significant (p-value = 0.048).

In the post-period, MFH openings are associated with small changes in 911 calls. For property calls, the static DiD estimate is 0.65 (p-value = 0.240) relative to a mean of 21.27 calls (approximately 3.1 percent), which is not statistically significant. For nuisance calls, the estimate is 1.32 (p-value = 0.062) relative to a mean of 38.83 calls (approximately 3.4 percent), a marginally significant increase. For violent calls, the estimated effect is 2.02 (p-value = 0.005) relative to a mean of 37.71 calls (approximately 5.4 percent). We note, however, that the marginally significant pre-period joint test for violent calls (p-value = 0.048) warrants caution in interpreting this estimate.

Taken together, these results suggest that MFH openings are accompanied by modest, spatially concentrated changes in calls for police service. While the spatial pattern of these effects closely resembles that observed for supportive housing, the estimated magnitudes are generally smaller. This comparison suggests that new housing development more broadly may generate short-range public safety spillovers, but that the magnitude of these effects varies systematically by housing type.

5.4 Additional Outcomes and Robustness

We conduct a series of additional robustness checks using alternative outcomes and housing types in Appendix B. We examine additional public safety outcomes using reported crime incidents and arrests, separately for non-supportive housing, supportive housing, and market-rate multifamily housing. Across these alternative measures, the qualitative patterns remain consistent with our baseline findings.

For reported crime outcomes (Figure B1 and Figure B2), the post-treatment estimates suggest modest increases in property- and violent-related incidents following housing site openings, particularly for supportive housing sites. However, the magnitudes of these effects remain small relative to baseline crime levels.

Figures B3 and B4 present analogous results for arrests. As with reported crime, arrest rates show a modest response to non-supportive housing openings. For supportive and market-rate housing, however, arrest outcomes display post-period increases that align closely with the timing and spatial concentration of effects observed for both 911 calls and reported crime. Taken together, these additional outcomes reinforce the main findings of the paper. Non-supportive affordable housing openings are not associated with changes in crime or enforcement activity, while supportive and market-rate housing openings are accompanied by localized increases across multiple public safety measures. The convergence of evidence across public safety outcomes reinforces the conclusion that housing openings generate highly localized responses whose magnitude varies systematically by housing type.

6 Conclusion

This paper examines the localized public safety effects of affordable housing construction in Los Angeles from 2005 to 2023. Using geolocated administrative data on 911 calls, reported crime, and arrests, we estimate staggered event study and radius-based difference-in-differences models to assess how housing site openings affect surrounding areas. We find that affordable housing openings are associated with localized increases in 911 calls in the immediate vicinity of housing sites, with effects concentrated within roughly 0.20 miles and attenuating rapidly at larger distances. These pooled effects, however, mask substantial heterogeneity across housing types.

Supportive housing openings are associated with increases of approximately 17 to 20′ percent in property, nuisance, and violent 911 calls within 0.25 miles, while non-supportive affordable housing openings show no detectable effects. Market-rate, multifamily housing openings produce modest, spatially concentrated effects that are generally smaller than those associated with supportive housing, though we note some evidence of differential pre-trends for violent 911 calls in the MFH sample that warrants caution in interpreting those estimates. These patterns are consistent across reported crime and arrest outcomes.

Importantly, our data do not allow us to distinguish between changes in underlying criminal activity, changes in reporting behavior, and changes in law enforcement activity. Each of these channels could plausibly contribute to the increases we observe for supportive housing, and disentangling them is an important direction for future research. Future research can shed light on the degree to which each of these channels interacts with supportive housing and service provision.

From a policy perspective, the localized increases in public safety incidents associated with supportive housing should be weighed against the substantial benefits of reducing homelessness and expanding housing access for vulnerable populations. Our findings do not imply that supportive housing construction is net harmful; rather, they suggest that the public safety spillovers of affordable housing vary with the type of housing and the population served, and that targeted investment in supportive services and community integration may help mitigate localized effects.

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7 Tables and Figures

Table 1: Summary Statistics: LAHD Affordable Housing Sites

	Mean	Median	SD	Minimum	Maximum
Panel I: Affordable Housing Site Data					
Measured at the Housing-Site Level					
Occupancy Start Year	2014.4	2014	5.1	2005	2023
Number of Units	62.6	55	36.0	6	283
Supportive Housing Indicator	0.461				
Total Number of Housing Sites	334				
Panel II: Analytical Data Set					
Measured at the Housing-Site by Quarter Level					
Incidents Occurring Within 0.25 Miles					
Violent Crime					
911 Calls	78.63	50	104.99	0	833
Reported Crime	37.34	25	48.60	0	382
Arrests	9.56	6	12.50	0	108
Property Crime					
911 Calls	30.10	23	27.84	0	273
Reported Crime	44.19	36	35.41	0	686
Arrests	9.26	5	13.77	0	227
Incidents Occurring Within 0.50 Miles					
Violent Crime					
911 Calls	246.80	181	250.43	0	1655
Reported Crime	121.06	91	115.70	0	770
Arrests	31.04	22	29.83	0	218
Property Crime					
911 Calls	106.18	85	84.91	1	673
Reported Crime	159.85	136	110.63	0	1298
Arrests	31.75	21	35.28	0	460
Total Site-Quarter Observations	24716				

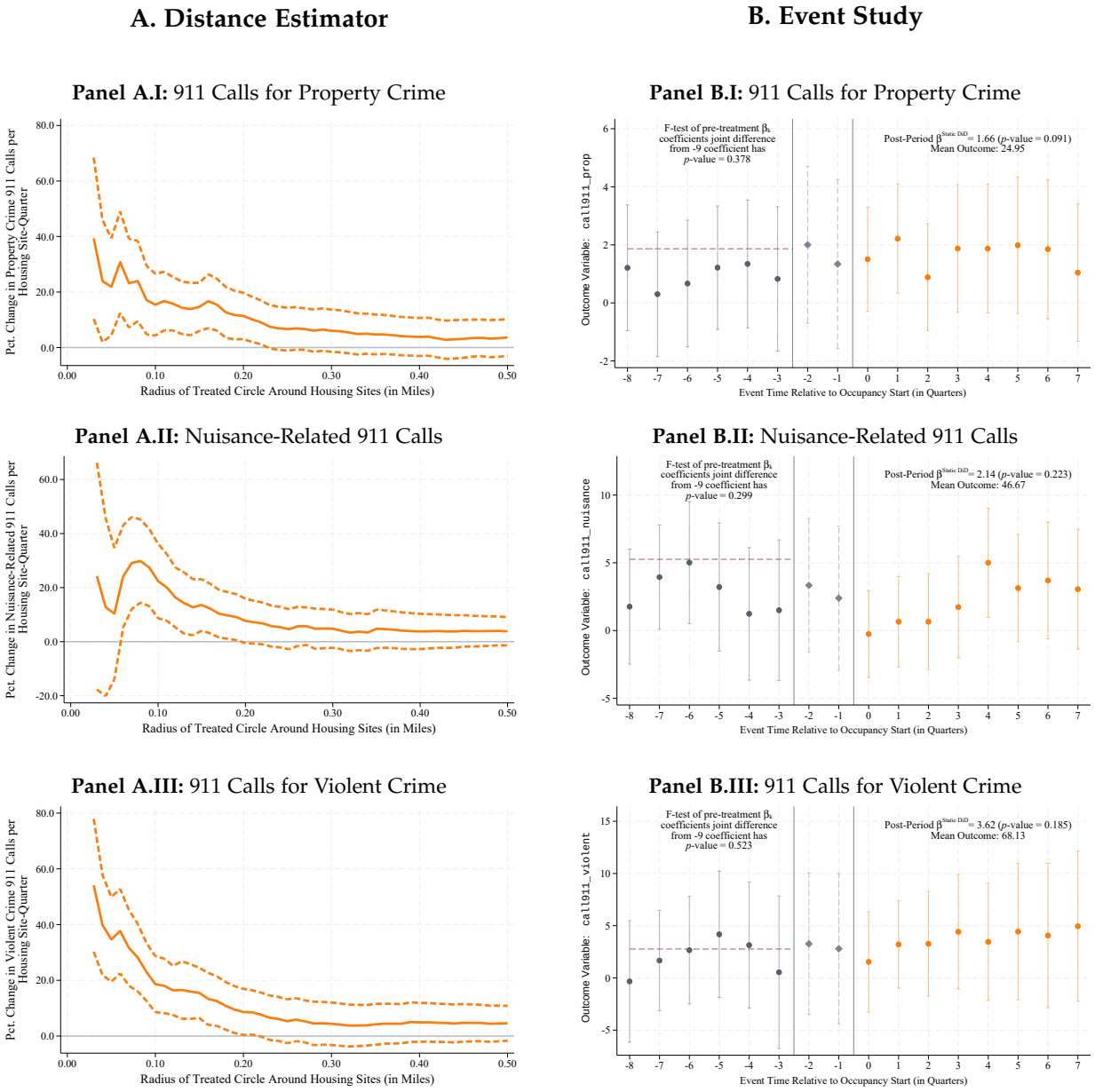
Note: Panel I of this table displays summary statistics for our our sample of affordable housing sites constructed during our sample time period (2005-2023). Panel II reports summary statistics for the analytical dataset measured at the housing-site-by-quarter level. Violent and property crimes include 911 calls, crimes reported to the Los Angeles Police Department (LAPD), and arrests made by LAPD.

Table 2: Condensed Event Study Summary: 911 Calls by Housing Sample

	(1) Property Crime	(2) Nuisance	(3) Violent Crime
Panel A: All Affordable Housing Sites			
Pre-Period Joint Significance Test *p*-Value	0.378	0.299	0.523
Post-Period Average Effect	1.66	2.14	3.62
S.E.	(0.98)	(1.76)	(2.73)
p-Value	[0.091]	[0.223]	[0.185]
Mean Outcome	24.95	46.67	68.13
Panel B: Supportive Housing Sites			
Pre-Period Joint Significance Test *p*-Value	0.309	0.275	0.691
Post-Period Average Effect	5.61**	9.80**	14.35**
S.E.	(1.74)	(3.05)	(4.87)
p-Value	[0.001]	[0.001]	[0.003]
Mean Outcome	28.13	53.70	81.50
Panel C: Non-Supportive Housing Sites			
Pre-Period Joint Significance Test *p*-Value	0.526	0.435	0.081
Post-Period Average Effect	1.73**	-0.14	1.04
S.E.	(0.64)	(0.86)	(1.18)
p-Value	[0.007]	[0.871]	[0.376]
Mean Outcome	20.27	36.31	48.44
Panel D: Market-Rate Housing Sites			
Pre-Period Joint Significance Test *p*-Value	0.811	0.550	0.048
Post-Period Average Effect	0.65	1.32	2.02**
S.E.	(0.55)	(0.71)	(0.72)
p-Value	[0.240]	[0.062]	[0.005]
Mean Outcome	21.27	38.83	37.71

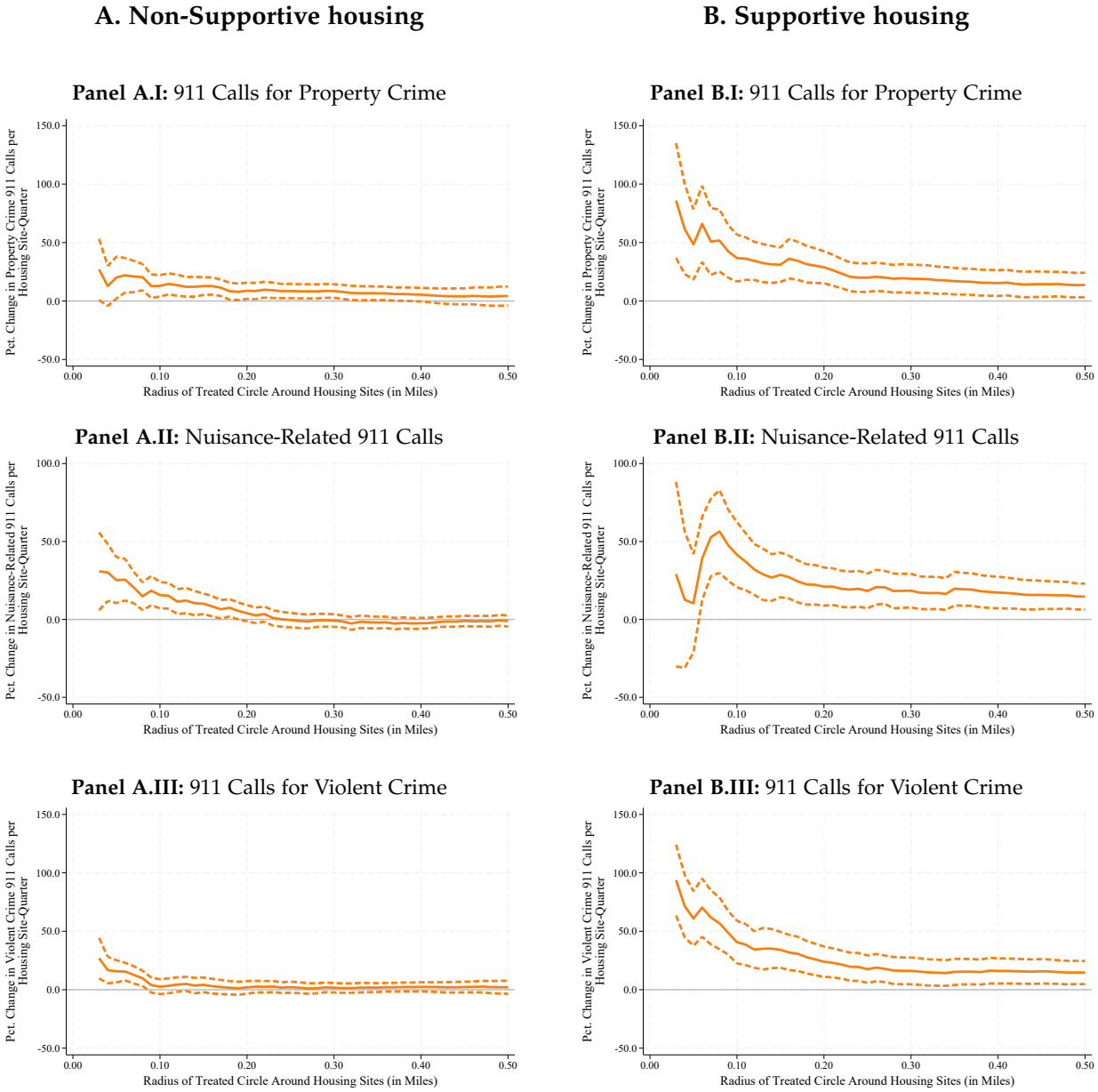
Note: Each panel reports the pre-period joint significance test *p*-value and post-period average effect summary statistics from the event study workflow. Panel A reports results pooling all affordable housing sites, while Panels B–D report supportive housing, non-supportive housing, and market-rate housing samples, respectively. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure 1: *Distance Estimator and Event Study – 911 Calls – All Affordable Housing Sites*



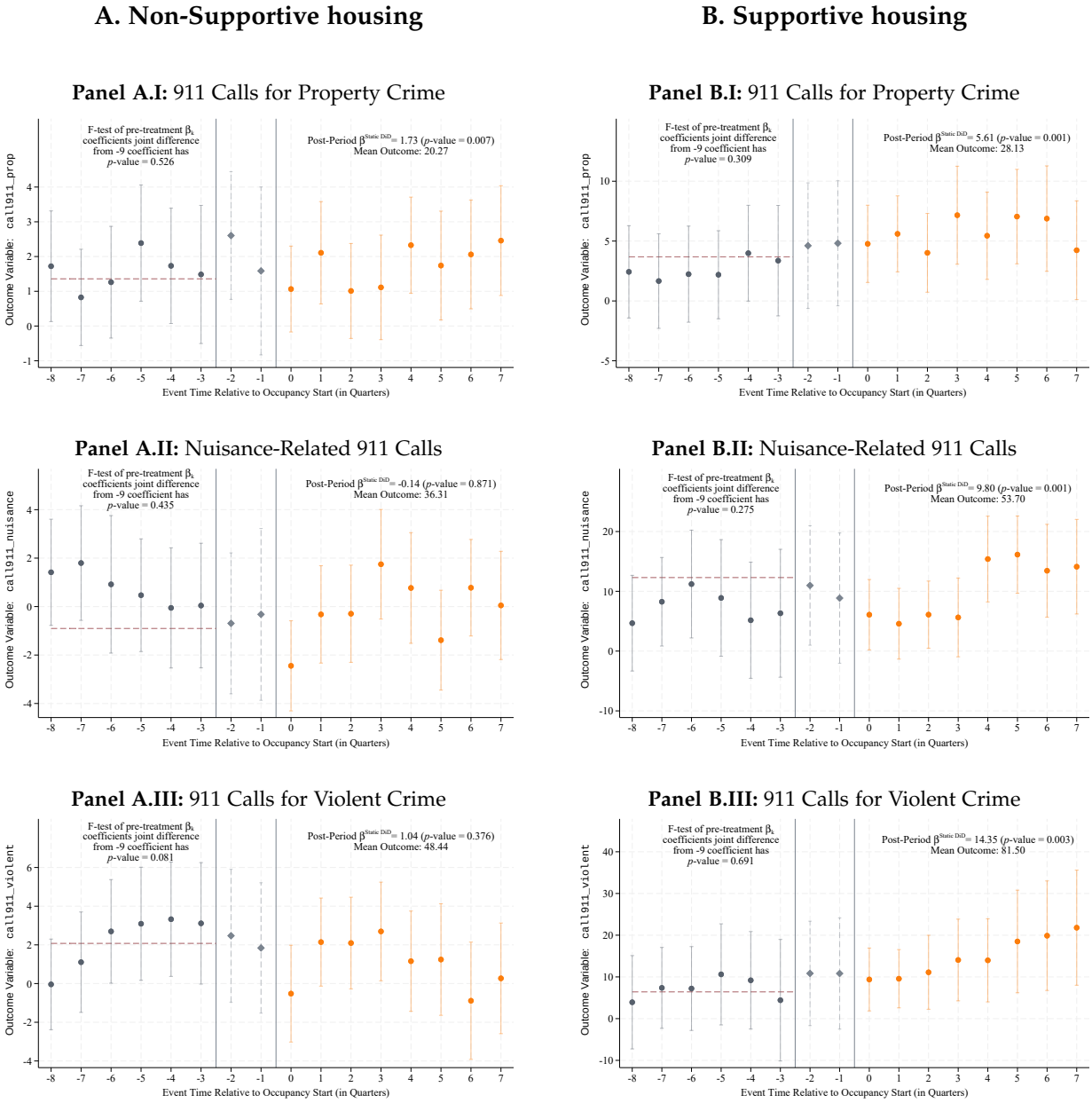
Notes: Sample includes all LAHD sites (supportive and non-supportive). In **Panel A**, we use `did_imputation` to estimate the static DiD effect of site openings for each outcome variable over a treated area defined by a circle with radius r miles around each housing site, where r ranges from 0.02 to 0.50 miles in 0.01-mile increments. Estimation uses an LAHD housing-site-by-quarter dataset. Estimates are weighted by the number of units at each site. The sample is restricted in the post-treatment period to observations within the event-study window (eight quarters after treatment) in addition to a pre-treatment donut with two quarters prior to treatment removed. Percentage changes are calculated as the estimated DiD coefficient at a given distance divided by the pre-treatment average level of outcome Y within the treated circle, multiplied by 100. Dashed lines represent scaled 95 percent confidence intervals around the percentage-change estimates. In **Panel B**, we use `did_imputation` to estimate event-study effects of affordable housing site openings on outcomes within a 0.25-mile radius of each site. Pre- and post-period joint significance tests are conducted relative to the $t = -9$ event-time coefficient (plotted as a dashed line in the pre-period). The donut period at event times $t = -2$ and $t = -1$ is excluded from both the pre-period joint significance test and the estimation of post-treatment effects. All estimates are weighted by the number of units at each housing site.

Figure 2: *Distance Estimator – 911 Calls – Non-Supportive vs Supportive Housing*



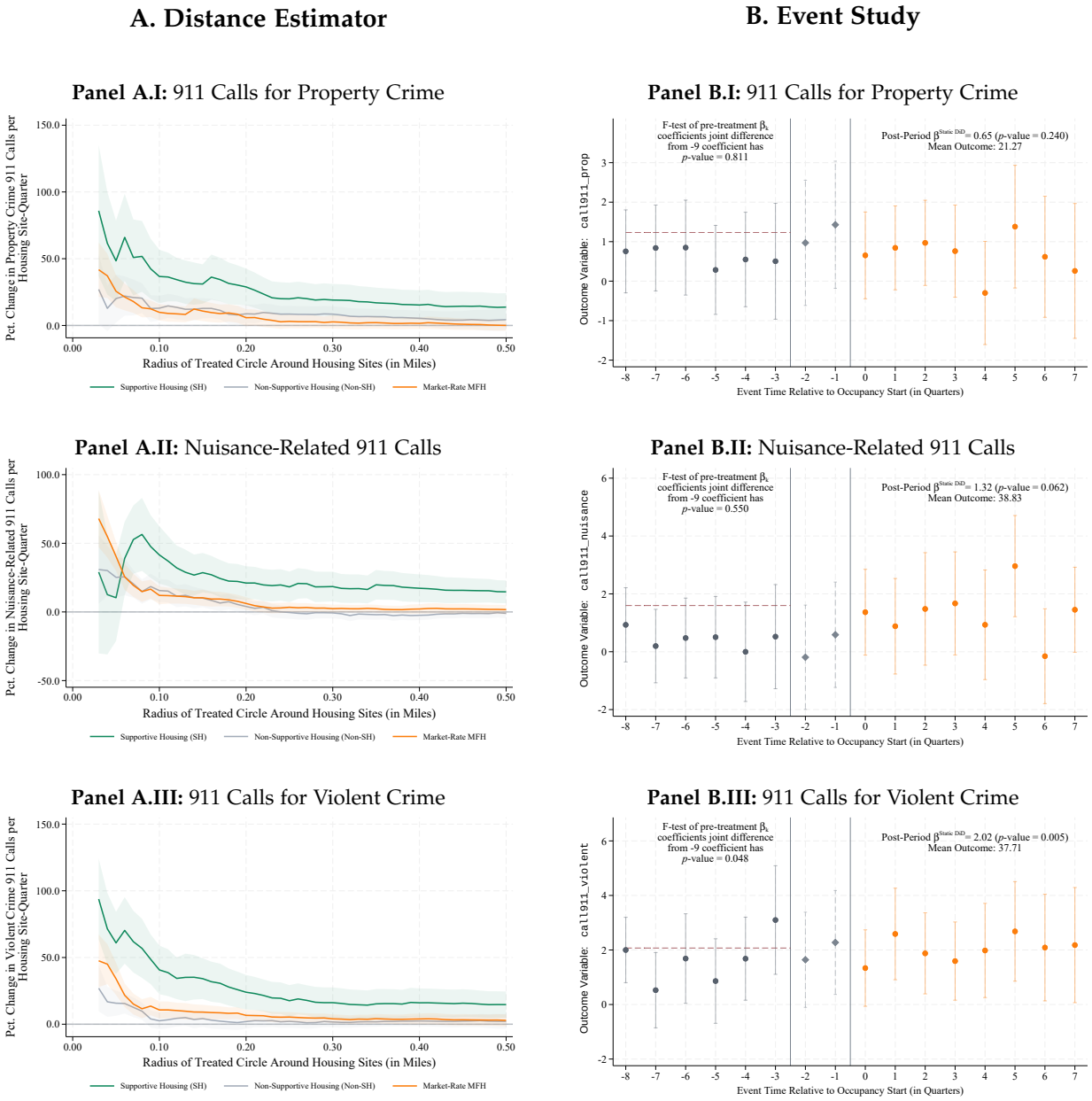
Notes: In **Panel A**, the sample is restricted to *non-supportive* housing sites, while in **Panel B** it is restricted to *supportive* housing sites. We use `did_imputation` to estimate the static DiD effect of site openings for each outcome variable over a treated area defined by a circle with radius r miles around each housing site, where r ranges from 0.02 to 0.50 miles in 0.01-mile increments. Estimation uses an LAHD housing-site-by-quarter dataset. Estimates are weighted by the number of units at each site. The sample is restricted in the post-treatment period to observations within the event-study window (eight quarters after treatment) in addition to a pre-treatment donut with two quarters prior to treatment removed. Percentage changes are calculated as the estimated DiD coefficient at a given distance divided by the pre-treatment average level of outcome Y within the treated circle, multiplied by 100. Dashed lines represent scaled 95 percent confidence intervals around the percentage-change estimates.

Figure 3: *Event Study – 911 Calls – Non-Supportive vs Supportive Housing*



Notes: In **Panel A**, the sample is restricted to *non-supportive* housing sites, while in **Panel B** it is restricted to *supportive* housing sites. We use `did_imputation` to estimate event-study effects of affordable housing site openings on outcomes within a 0.25-mile radius of each site. Pre- and post-period joint significance tests are conducted relative to the $t = -9$ event-time coefficient, which is plotted as a dashed line in the pre-period. The donut period corresponding to event times $t = -2$ and $t = -1$ is excluded from both the pre-period joint significance tests and the estimation of post-treatment effects. All estimates are weighted by the number of units at each housing site.

Figure 4: *Market-Rate Multi-Family Housing sites*



Notes: Sample includes *market-rate multifamily* (MFH) housing sites. In **Panel A**, we use `did_imputation` to estimate the static DiD effect of site openings for each outcome variable over a treated area defined by a circle with radius r miles around each housing site, where r ranges from 0.02 to 0.50 miles in 0.01-mile increments. Estimation uses an MFH housing-site-by-quarter dataset. Estimates are weighted by the number of units at each site. The sample is restricted in the post-treatment period to observations within the event-study window (eight quarters after treatment) in addition to a pre-treatment donut with two quarters prior to treatment removed. Percentage changes are calculated as the estimated DiD coefficient at a given distance divided by the pre-treatment average level of outcome Y within the treated circle, multiplied by 100. Dashed lines represent scaled 95 percent confidence intervals around the percentage-change estimates. In **Panel B**, we use `did_imputation` to estimate event-study effects of MFH housing site openings on outcomes within a 0.25-mile radius of each site. Pre- and post-period joint significance tests are conducted relative to the $t = -9$ event-time coefficient (plotted as a dashed line in the pre-period). The donut period at event times $t = -2$ and $t = -1$ is excluded from both the pre-period joint significance test and the estimation of post-treatment effects. All estimates are weighted by the number of units at each housing site.

A Data Appendix

A.1 Description of Affordable Housing

Affordable housing in Los Angeles consists of rental housing that is subject to income and rent restrictions and reserved for households earning below specified thresholds of Area Median Income (AMI), typically ranging from extremely low income ($\leq$ 30% AMI) to low income ($\leq 60 - 80\%$ AMI). These developments are financed through federal, state, and local subsidy programs, including Low-Income Housing Tax Credits (LIHTC), local housing trust funds, and municipal bond financing, and are commonly developed and operated by nonprofit or mission-driven housing providers.

Within the set of affordable housing developments, an important programmatic distinction exists between supportive housing and non-supportive affordable housing. Supportive affordable housing pairs long-term, rent-restricted housing with an organized package of supportive services intended to promote housing stability among residents facing substantial barriers, including chronic homelessness, disability, or serious health and mental health conditions.[5] While the intensity and delivery of services vary across developments and target populations, supportive housing is distinguished from non-supportive affordable housing by the presence of a structured service component coordinated by housing operators and partner providers. Core services typically include housing-focused case management and care coordination, assistance with benefits and entitlements, and linkages to health, behavioral health, and social services.[6] In the Los Angeles context, service provision has been shaped by coordination among the Los Angeles Homeless Services Authority, the City of Los Angeles Housing Department, and nonprofit service providers, with many supportive housing developments financed under the City's Proposition HHH programs.

Consistent with HUD guidance and Housing First–oriented program design, supportive

[5] See Administration for Community Living (ACL), *Permanent Supportive Housing*, which defines permanent supportive housing as long-term housing assistance combined with supportive services for individuals with disabilities and other barriers to housing stability: https://acl.gov/programs/housing/permanent-supportive-housing

[6] Administration for Community Living (ACL), *Permanent Supportive Housing Two-Pager*, which summarizes common supportive service components associated with permanent supportive housing: https://acl.gov/sites/default/files/programs/2023-03/PSH_TwoPager_Draft_220421_Final.pdf. Also see HUD, *Permanent Supportive Housing Program Components*, which describes typical service categories associated with SH: https://www.hudexchange.info/programs/coc/permanent-supportive-housing/

services are generally offered but not required as a condition of tenancy.[7] Participation in services is voluntary, and tenants are subject to standard lease obligations and subsidy compliance requirements rather than service mandates. Table A2 summarizes the typical supportive housing services.

By contrast, non-supportive affordable housing consists of income-restricted rental units without an embedded supportive-services component. These developments serve a broader population of low-income households, including working families and seniors, and operate similarly to conventional multifamily rental housing subject to affordability covenants. Non-supportive affordable housing constitutes a substantial share of the affordable housing stock in Los Angeles and is financed through many of the same subsidy mechanisms as supportive housing, absent the service-delivery component.

Supportive and non-supportive housing sites in our sample. Appendix Figure A1 and A2 present illustrative supportive and non-supportive housing developments from the study sample, selected to span construction years from the mid-2000s through the early 2020s and to capture variation in neighborhood context. Earlier developments frequently reflect adaptive reuse or smaller-scale multifamily construction, while later projects are increasingly purpose-built mid-rise apartment buildings produced during the expansion of supportive housing financing in the late 2010s and early 2020s.

In panel I of Figure A3, we show annual affordable housing construction across the 2005 through mid-2023 time period covered by our study. The level of affordable housing construction remained relatively stable from 2005 through 2021, with an average of roughly 10 to 15 new affordable housing sites opening each year during this period. Site construction increased substantially in 2022 and 2023, however, with over 25 new sites being opened in each of the two final years covered by our study. While total crime followed a downward trajectory during most of the sample period, Los Angeles experienced an increase in crime beginning in 2021 (note that our crime data for 2023 only covers the first half of the year).

[7] National Alliance to End Homelessness, *Housing First*, which emphasizes that participation in services is not a precondition for obtaining or retaining housing: https://endhomelessness.org/resource/housing-first/ and HUD guidance emphasizes that permanent supportive housing follows Housing First principles, under which tenants are not required to participate in services to obtain or retain housing. See HUD, *Housing First in Permanent Supportive Housing*, https://www.hudexchange.info/resource/3893/housing-first-in-permanent-supportive-housing-brief/

In panel II of Figure A3, we plot the geographic distribution of affordable housing sites across Los Angeles across neighborhoods.[8] Affordable housing is concentrated in Central and South Los Angeles, as well as the San Fernando Valley, areas which also tend to have higher population densities and correspondingly higher crime counts.

[8]We use neighborhood definitions provided as part of the *Los Angeles Times* "Mapping L.A." Project.

Table A1: Cross-Walk Between HUD Permanent Supportive Housing and Proposition HHH Implementation in Los Angeles

HUD SH Requirement	HUD Definition / Guidance	Proposition HHH Implementation
Permanent tenancy	No designated length of stay; housing is permanent	HHH funds permanent, rent-restricted multifamily housing units
Supportive services	Supportive services paired with housing to promote stability	Services provided through nonprofit partners and coordinated with local agencies
Voluntary services	Service participation not required to obtain or retain housing	HHH projects follow Housing First principles; tenancy not contingent on service use
Target population	Individuals experiencing homelessness, disabilities, or other barriers	HHH prioritizes people experiencing homelessness or at risk of homelessness
Program oversight	HUD Continuum of Care framework	City of Los Angeles in coordination with LAHSA and County service systems
Financing structure	Federal rental assistance and program oversight	Municipal bond financing for capital development; services funded separately

Notes: The table summarizes how Proposition HHH implements federal permanent supportive housing (PSH) requirements at the local level. While Proposition HHH primarily finances capital development, service provision and operating subsidies are coordinated through a combination of local, state, and federal programs consistent with HUD guidance. Federal definitions of permanent supportive housing and associated program requirements are drawn from U.S. Department of Housing and Urban Development (HUD) guidance (https://www.hudexchange.info/homelessness-assistance/permanent-supportive-housing/ ; https://www.hudexchange.info/programs/coc/permanent-supportive-housing/). HUD guidance on Housing First principles and the voluntary nature of supportive services is described in https://www.hudexchange.info/resource/3893/housing-first-in-permanent-supportive-housing-brief/. Information on local implementation under Proposition HHH is drawn from the City of Los Angeles Housing and Community Investment Department (https://hcidla.lacity.org/prop-hhh).

Table A2: Typical Supportive Housing Services in Los Angeles

Service Domain	Examples of Services Provided	Delivery Mode	Notes
Housing stability and tenancy support	Housing-focused case management; lease compliance support; landlord – tenant mediation; crisis planning	On-site and/or off-site	Core component aimed at housing retention
Benefits and entitlements assistance	SSI/SSDI application support; Medi-Cal enrollment; veterans' benefits navigation	Primarily off-site with coordination	Supports income stability and access to care
Health care linkage	Primary care referrals; chronic disease management; appointment coordination	Mostly off-site	Intensity varies by population served
Behavioral health services	Mental health counseling; psychiatric services; crisis intervention	On-site or off-site	Common in housing serving chronically homeless populations
Substance use services	Treatment referrals; harm-reduction supports; recovery services	Primarily off-site	Participation is voluntary
Independent living skills	Daily living skills; household management; community integration support	On-site and/or off-site	Often lighter-touch for stabilized tenants
Employment and education supports	Job readiness assistance; vocational referrals; education linkages	Off-site	Not universal across projects
Peer or recovery support	Peer mentoring; support groups	On-site or off-site	More common in behavioral-health–oriented projects
Population-specific supports	Senior services coordination; disability accommodations; specialized case management	Varies by project	Tailored to target population

Notes: The table summarizes service domains commonly associated with permanent supportive housing (PSH) developments in Los Angeles, as defined by U.S. Department of Housing and Urban Development (HUD) guidance. Permanent supportive housing combines long-term, rent-restricted housing with supportive services intended to promote housing stability among individuals or households with disabilities or other barriers to housing retention. Not all developments provide all services listed, and the type, intensity, and mode of service delivery vary by project, target population, and funding source. Consistent with HUD Housing First guidance, participation in supportive services is generally voluntary and is not a condition of tenancy; tenants remain subject to standard lease requirements and subsidy compliance rules rather than service mandates. In the Los Angeles context, Proposition HHH finances the capital development of supportive housing units, while ongoing supportive services and operating costs are funded separately through a combination of local, state, and federal sources, including County-administered service programs and HUD Continuum of Care–related funding streams. Federal definitions and guidance are drawn from: https://www.hudexchange.info/homelessness-assistance/permanent-supportive-housing/;https://www.hudexchange.info/programs/coc/permanent-supportive-housing/; https://www.hudexchange.info/resource/3893/housing-first-in-permanent-supportive-housing-brief/. Information on local implementation under Proposition HHH is drawn from the City of Los Angeles Housing and Community Investment Department: https://hcidla.lacity.org/prop-hhh.

Figure A1: *Supportive Housing Examples*

A supportive housing development in the downtown/Arts District area (2005)



A supportive housing site in the Westlake area (2011)



A supportive housing in Koreatown (2018)



Notes: The figure presents illustrative supportive housing developments from the study sample, selected to span construction years from the mid-2000s through the late 2010s.

Figure A2: *Non-Supportive Housing Examples*

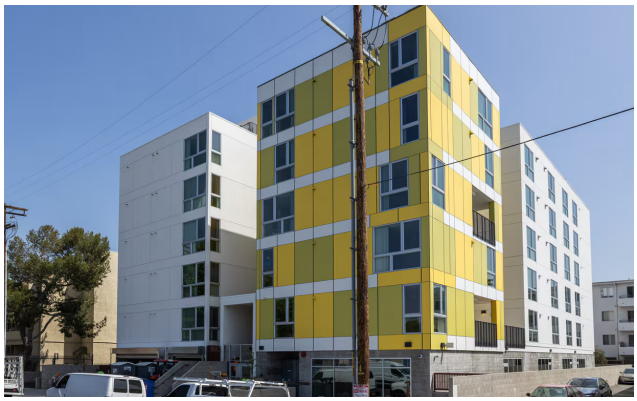
A non-supportive housing for seniors (2005)



A non-supportive affordable housing example in East Hollywood (2010)



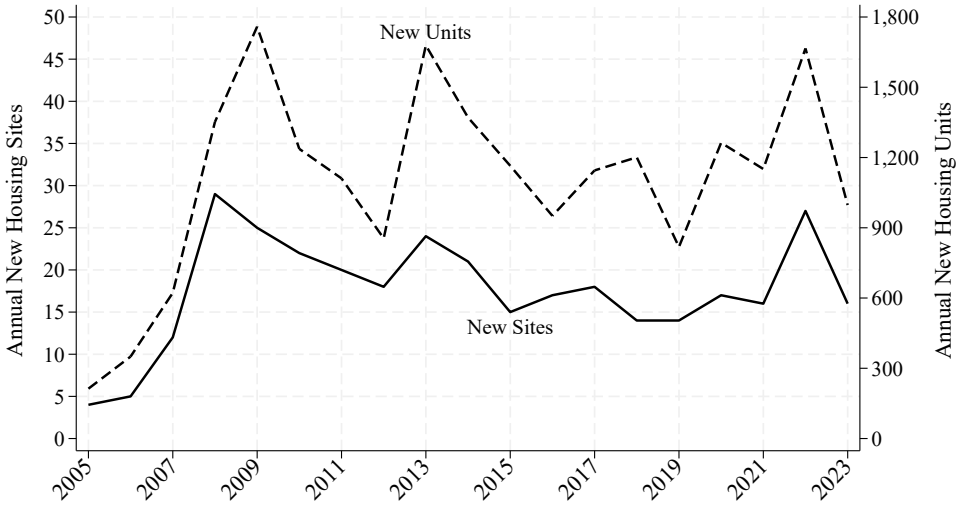
A recent non-supportive affordable housing example on the Westside (2023)



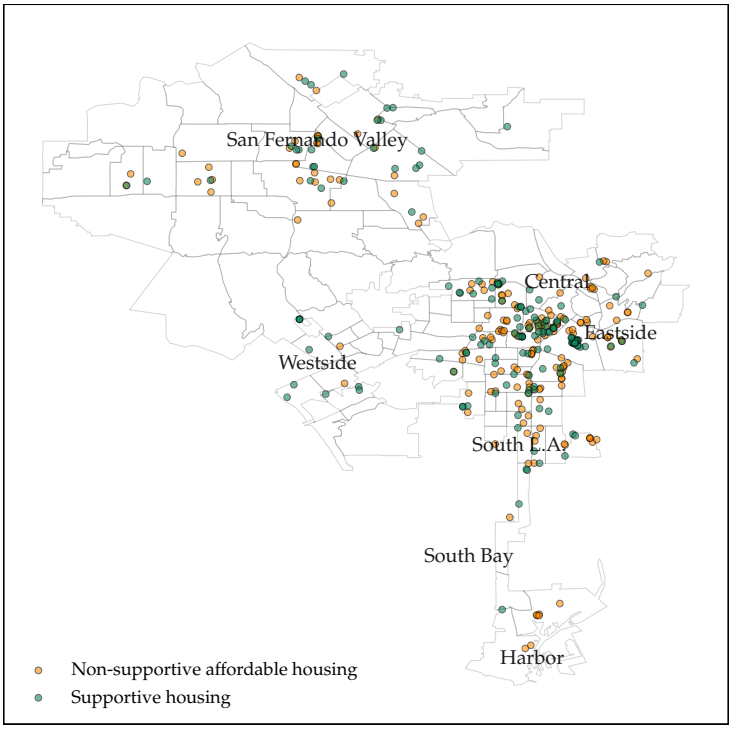
Notes: The figure presents illustrative non-supportive housing developments from the study sample, selected to span construction years from the mid-2000s through the early 2020s.

Figure A3:

Panel I: *Annual New Housing sites and Units*



Panel II: *Affordable Housing Construction Across Los Angeles*



Notes: In Panel I, annual new affordable housing developments are measured as the number of housing sites that began occupancy in each year, along with the total number of housing units delivered. In Panel II, orange and green dots denote non-supportive and supportive affordable housing sites in the sample, respectively, constructed between 2005 and 2023. Polygons represent the boundaries of Los Angeles neighborhoods and regions.

A.2 Description of Public Safety Data

911 Data. The 911 records contain data on more than 12 million calls made between 2007 to 2021 and observation records the date, time, and address of the incident, as well as a basic description of the call type. We define three broad categories - violent, property, and nuisance-related calls. Property crimes include larceny, burglary, and motor vehicle theft, while violent crimes include all crimes against a person, such as assault, threats, sexual offense, and homicide. Nuisance-related calls include minor disturbances, intoxication, indecent exposure, mental-health and drug-related calls.

Table A3: Crime Categories and Description of 911 Calls

Crime Type	Code	Description
Violent	187; 207; 211; 242	Murder; Kidnapping; Robbery; Battery
Property	459; 470; 484; 503; 594	Burglary; Forgery; Theft; Vehicle; Vandalism
Nuisance	314; 390; 507; 918; 930	Indecent Exposure; Intoxicated; Minor disturbance; Mental-health related; Screaming

Crime Incidents. For reported crimes, we use crime incident data from the Los Angeles Police Department (LAPD) and Los Angeles Sheriff's Department (LASD). Taken together, we observe 7.4 million incidents reported between 2005 and mid-2023, Each of these records includes information on the incident date, location (both street address and latitude and longitude), and offense descriptions. We use data from both LAPD and LASD because the city of Los Angeles, and thus the jurisdiction of the LAPD, is somewhat irregular. We refer to the region depicted in Figure A3 as the "broader" Los Angeles area, which contains both the city of Los Angeles as well as communities that are formally part of the surrounding County of Los Angeles. LAPD covers the large majority of all Census blocks within the city of Los Angeles itself, while LASD is contracted to provide police services by jurisdictions such as Compton and West Hollywood located in the broader Los Angeles County area.[9] We use offense descriptions contained in text fields from these records to classify incidents into charge categories using the Text-Based Offense Classification algorithm developed by the Criminal Justice Administrative Records System at the University of Michigan (CJARS) (Choi et al., 2023). Same as the 911 data, we define two broad

[9]The Census blocks in the broader Los Angeles city area depicted in Figure A3 for which we do not have crime data available represent communities such as Santa Monica and Brentwood which host their own police departments.

categories - violent and property crimes.

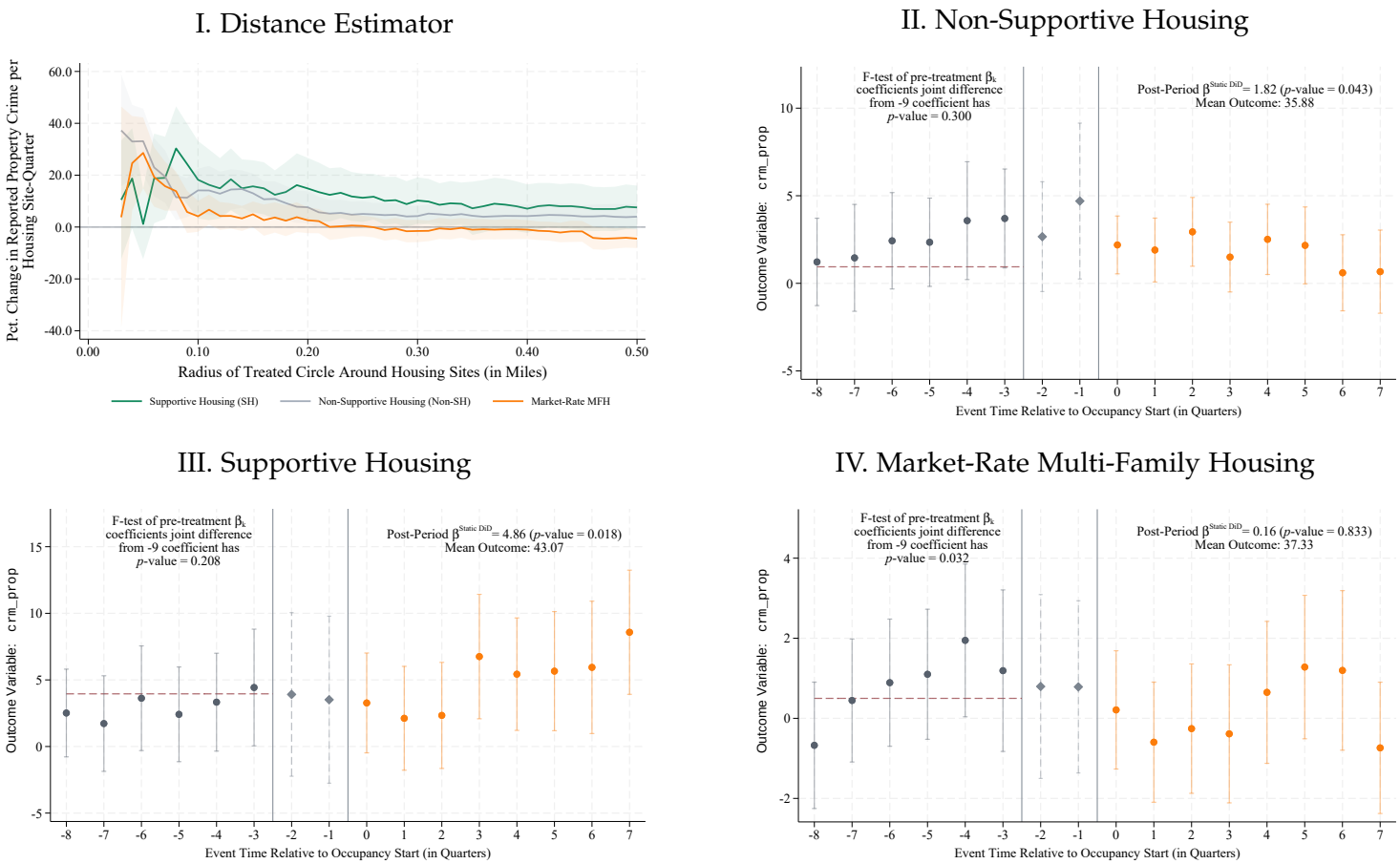
Arrest Data. Finally, we use data on arrests made by the LAPD. Arrests records cover 2.2 million arrests made over the years 2005 to 2023 and includes information on the arrest date, location (both street address and latitude and longitude), and offense descriptions. Using the Text-Based Offense Classification algorithm developed by (Choi et al., 2023), we define our outcome of interest as violent and property arrests.

Table A4: Crime Categories and Recorded Offenses in Crime and Arrest Data

Crime Type	Offenses
Violent	Assault; Rape; Intimidation; Kidnapping; Robbery; Murder; Manslaughter; ...
Property	Forgery; Theft; Burglary; Trespassing; Vehicle theft; Stolen property; Larceny; Destruction of Property ...

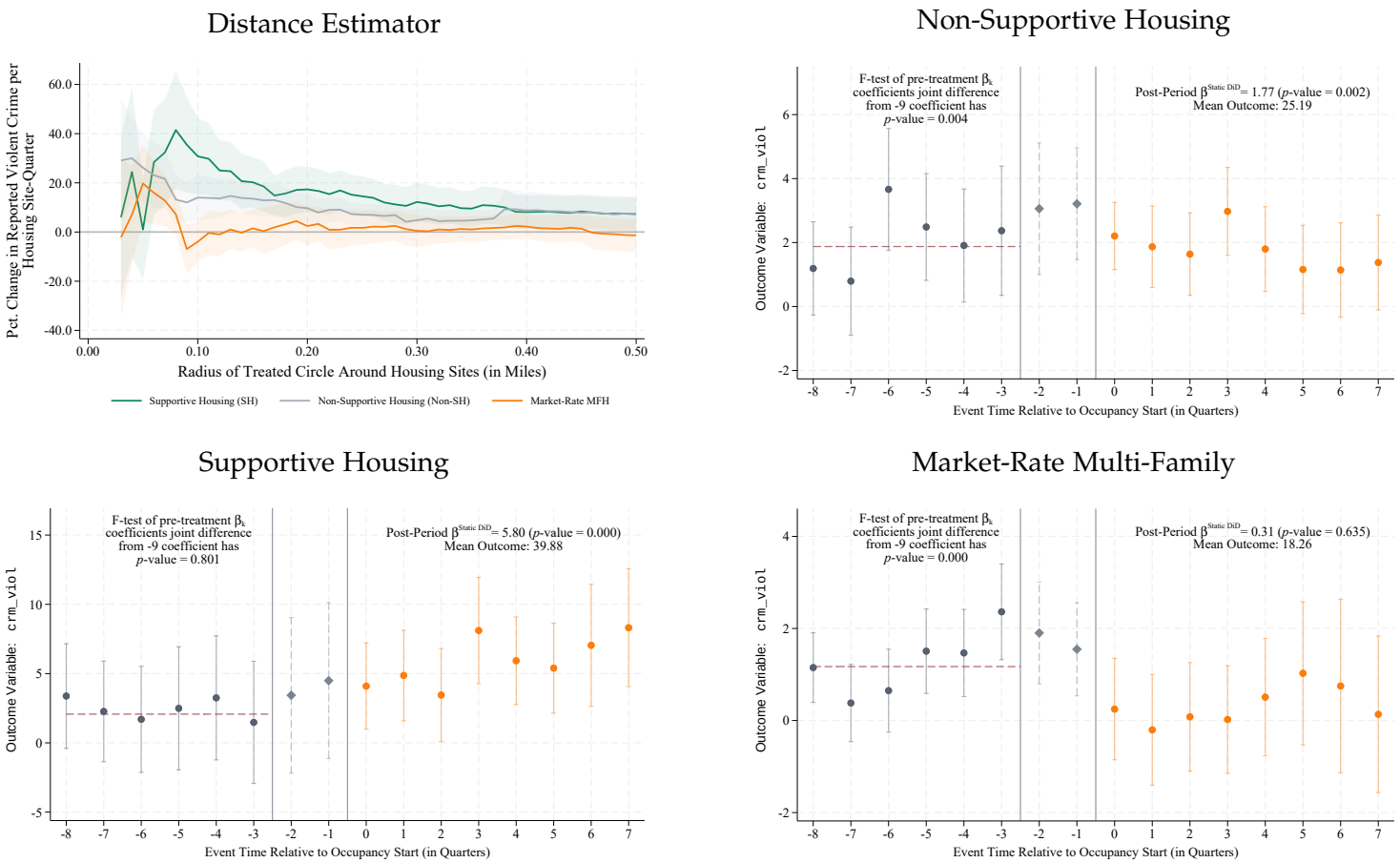
B Additional Outcomes: Reported Crime and Arrest

Figure B1: *Reported Property Crime Incidents – All Samples*



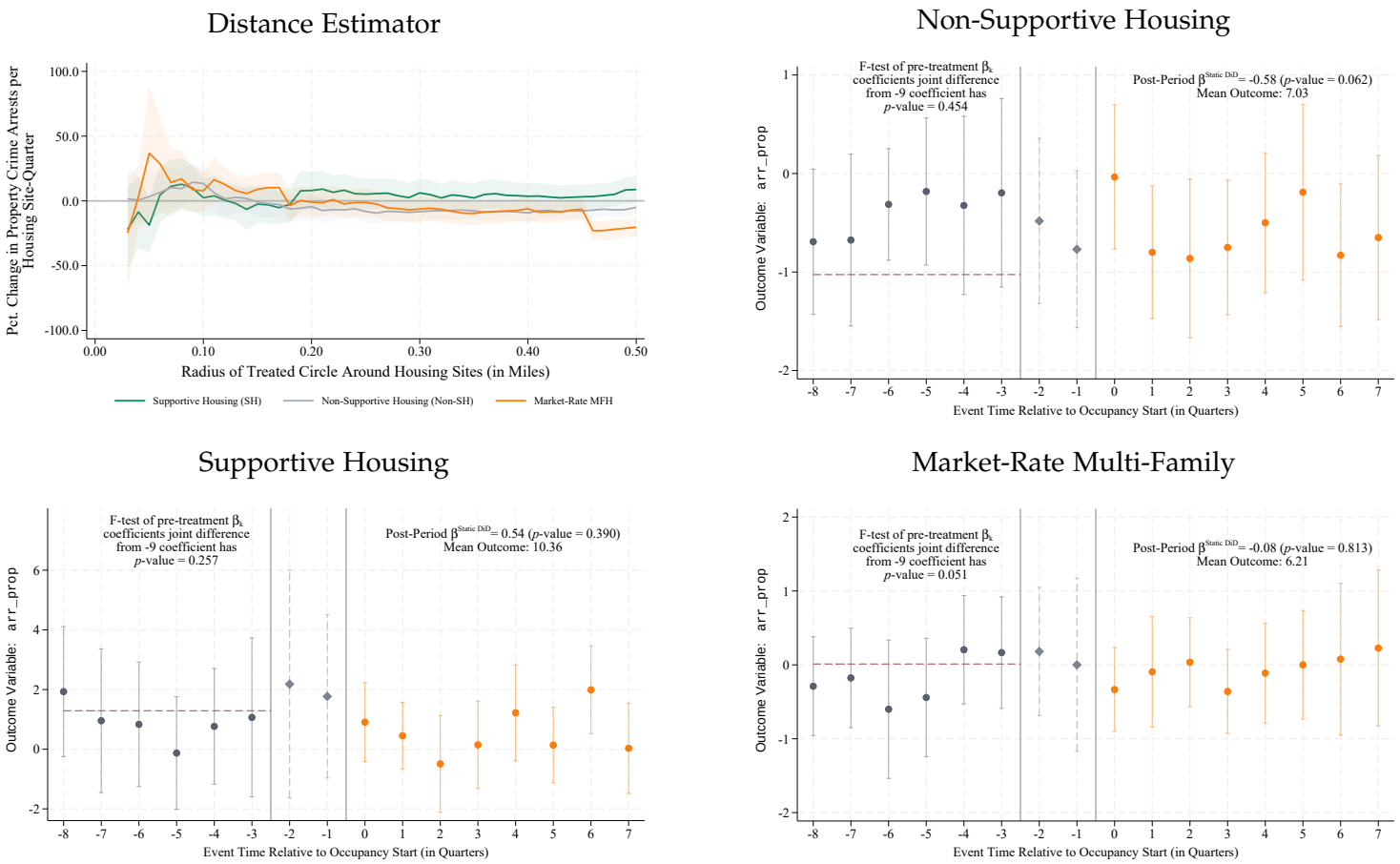
Notes: The outcome is the number of *property crime incidents* reported to the LAPD. In (I), we use `did_imputation` to estimate the static DiD effect of housing site openings for each outcome variable over a treated area defined by a circle with radius r miles around each housing site, where r ranges from 0.02 to 0.5 miles in 0.01-mile increments. Estimation uses a housing-site-by-quarter dataset. We then estimate event-study effects of housing site openings on outcomes within a 0.25-mile radius of each site separately for non-supportive affordable housing (II), supportive affordable housing (III), and market-rate multifamily housing (IV). Pre- and post-period joint significance tests are conducted relative to the $t = -9$ event-time coefficient (plotted as a dashed line in the pre-period). The donut period corresponding to event times $t = -2$ and $t = -1$ is excluded from both the pre-period joint significance tests and the estimation of post-treatment effects. See Figure 1 for additional notes.

Figure B2: *Reported Violent Crime Incidents – All Samples*



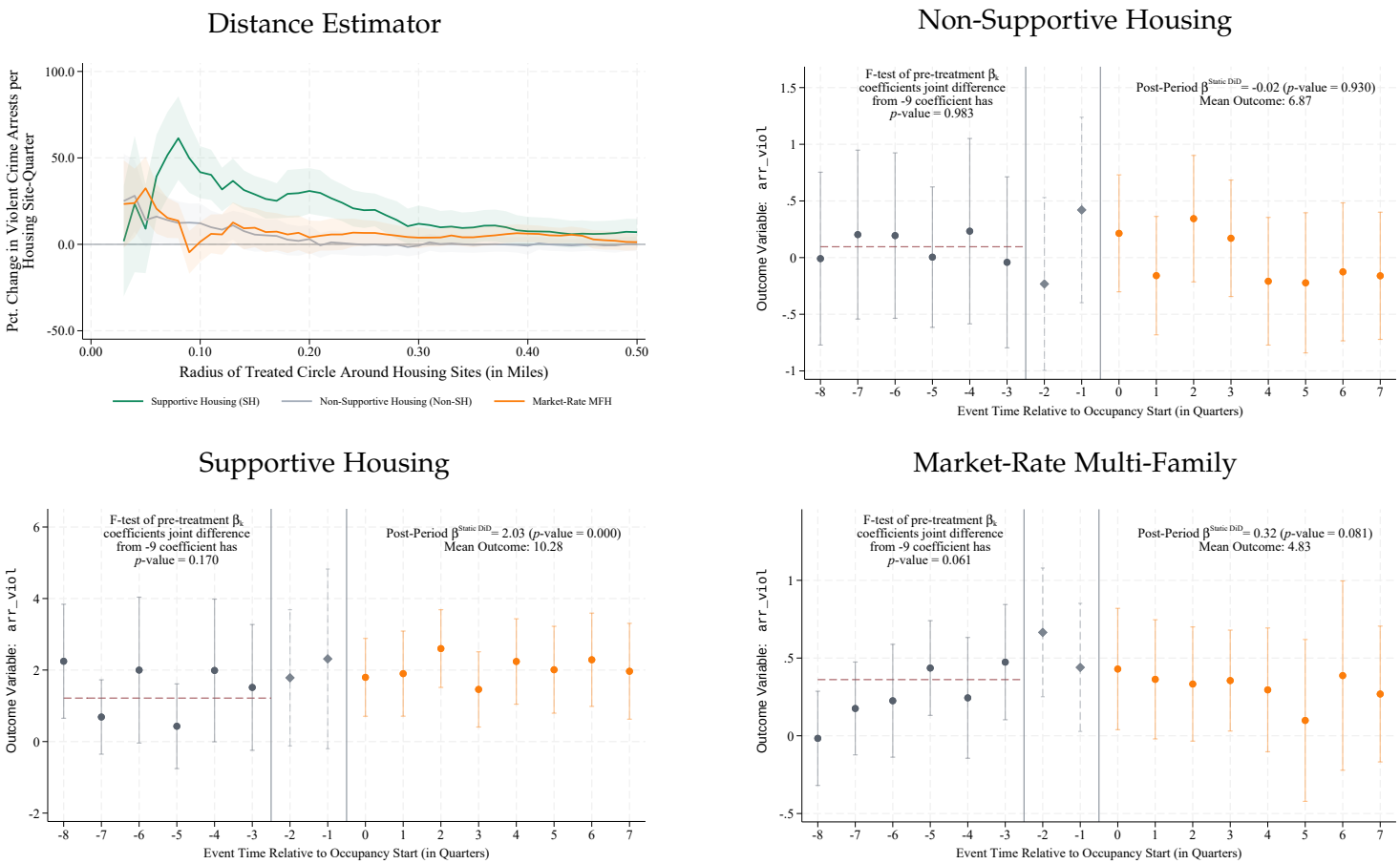
Notes: The outcome is the number of *violent crime incidents* reported to the LAPD. In (I), we use `did_imputation` to estimate the static DiD effect of housing site openings for each outcome variable over a treated area defined by a circle with radius r miles around each housing site, where r ranges from 0.02 to 0.5 miles in 0.01-mile increments. Estimation uses a housing-site-by-quarter dataset. We then estimate event-study effects of housing site openings on outcomes within a 0.25-mile radius of each site separately for non-supportive affordable housing (II), supportive affordable housing (III), and market-rate multifamily housing (IV). Pre- and post-period joint significance tests are conducted relative to the $t = -9$ event-time coefficient (plotted as a dashed line in the pre-period). The donut period corresponding to event times $t = -2$ and $t = -1$ is excluded from both the pre-period joint significance tests and the estimation of post-treatment effects. See Figure 1 for additional notes.

Figure B3: *Property Arrests – All Samples*



Notes: The outcome is the number of *property arrests* made by LAPD. In (I), we use `did_imputation` to estimate the static DiD effect of housing site openings for each outcome variable over a treated area defined by a circle with radius r miles around each housing site, where r ranges from 0.02 to 0.5 miles in 0.01-mile increments. Estimation uses a housing-site-by-quarter dataset. We then estimate event-study effects of housing site openings on outcomes within a 0.25-mile radius of each site separately for non-supportive affordable housing (II), supportive affordable housing (III), and market-rate multifamily housing (IV). Pre- and post-period joint significance tests are conducted relative to the $t = -9$ event-time coefficient (plotted as a dashed line in the pre-period). The donut period corresponding to event times $t = -2$ and $t = -1$ is excluded from both the pre-period joint significance tests and the estimation of post-treatment effects. See Figure 1 for additional notes.

Figure B4: *Violent Arrests – All Samples*



Notes: The outcome is the number of *violent arrests* made by LAPD. In (I), we use `did_imputation` to estimate the static DiD effect of housing site openings for each outcome variable over a treated area defined by a circle with radius r miles around each housing site, where r ranges from 0.02 to 0.5 miles in 0.01-mile increments. Estimation uses a housing-site-by-quarter dataset. We then estimate event-study effects of housing site openings on outcomes within a 0.25-mile radius of each site separately for non-supportive affordable housing (II), supportive affordable housing (III), and market-rate multifamily housing (IV). Pre- and post-period joint significance tests are conducted relative to the $t = -9$ event-time coefficient (plotted as a dashed line in the pre-period). The donut period corresponding to event times $t = -2$ and $t = -1$ is excluded from both the pre-period joint significance tests and the estimation of post-treatment effects. See Figure 1 for additional notes.