# Exposure to Homelessness and Support for Policy Remedies*

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**Abstract**

Majorities of voters consistently support spending measures to ameliorate homelessness, yet many oppose the proposed siting of homeless shelters. We study how voters react to these facilities once they are built and operational, conducting a within-neighborhood difference-in-differences analysis on a pair of statewide ballot propositions to fund new homeless services in California. We find that voters living closest to newly opened facilities became more supportive of spending on homelessness services compared to those living further away. We investigate the source of this positive policy feedback, finding no evidence of positive localized externalities (e.g. decreases in crime) that may activate voter self-interest. Rather, we find that new facilities increase exposure to homeless people and raise the salience of the problem. This is consistent both with exposure activating positive attitudes towards the homeless and with voters becoming more concerned and willing to support policy remedies. Our findings suggest that voters reward policy action and that NIMBY opposition may be more prospective than retrospective.

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# 1 Introduction

In 2025, at least 745,000 people in the United States were experiencing homelessness, a near 30 percent increase since 2013 (U.S. Department of Housing and Urban Development, 2025). The suffering and precarity of not having consistent shelter has many knock-on effects such as poor health, reduced job prospects, and lower political participation (Slee & Desmond, 2023; Metraux et al., 2018). Reducing homelessness is not only a pressing policy challenge to improve the conditions of the unhoused but also because it presents a set of second-order political challenges: residents who live near street homelessness, in particular, see it as a wider symptom of disorder, crime, and poor urban governance (Vitale, 2008). In places with deep housing affordability problems, a major driver of homelessness (Colburn & Aldern, 2022; Lee et al., 2010), more residents are exposed to homelessness and its visible impacts and consider it a major problem to be addressed.

Despite public concern, many concrete proposals to site homeless services encounter intense not-in-my-backyard (NIMBY) opposition. However, once built, how do homeless service facilities impact support for homelessness policy? In this paper, we examine the policy feedback effects of homelessness policies (Pierson, 1993). We test if localized exposure to new and expanded homeless service facilities impacts political support for the funding of additional facilities. We posit two models with competing predictions for how voters may respond to a new shelter opening close to their home. The first model expects that voters will respond negatively to increased exposure to homelessness in their local neighborhood, either due to negative attitudes or due to potential negative externalities such as increased crime, public vagrancy, and drug use (Brown & Zoorob, 2022). Voters with greater exposure to these externalities may experience reduced sympathy for the homeless and may become disenchanted with government solutions to the issue (Clifford & Piston, 2017; Gross & Wronski, 2021; Sands, 2017; Schneider & Ingram, 1993; Petersen, 2012). This reaction would therefore produce a negative policy feedback loop that will reduce the support for public investment in policy remedies for homelessness (Stokes, 2016; A. M. Jacobs & Weaver, 2015; L. R. Jacobs & Mettler, 2018). Conversely, positive policy feedback responses may result instead (e.g. Hankinson et al. (2026)), with proximate shelters raising the salience of the issue (Campbell, 2012; Mettler, 2011; L. R. Jacobs & Mettler, 2018; Michener, 2013) – highlighting both the beneficial aspects of the policy remedy and the need for further investment – and potentially engendering greater sympathy for the homeless that translates into increased policy support (Lee et al., 2004; Laniyonu & Byerly, 2021; Gross & Wronski, 2021; Kalla & Broockman, 2020). These models offer divergent hypotheses with implications for the efficacy of homeless policy, as well as evidence on voter psychology and how local contextual experiences are translated into voting choices.

To test these competing models, we collect data on new homeless service facilities in California opened between 2018 and 2024. We combine these data with precinct electoral data covering California elections from 2014-2024, individual-level data on California voters, and geolocated 911 and 311 calls, homeless encampments, and police stop data. To measure whether the opening of new homeless service facilities influenced voter support for policies to address homelessness, we focus on two statewide ballot propositions in 2018 and 2024 that provide billions of additional funding for expanded homeless services. With these data, we conduct a difference-in-differences design comparing within-precinct change in support for the referenda for precincts immediately proximate to new or expanded homeless facilities compared to those just slightly further away. This focused comparison allows us to compare voters who live in the same larger neighborhoods but differ in location relative to new shelters by a few street blocks, helping to minimize issues of endogeneity as the siting of homeless facilities is not random. Treated precincts thus contain voters with more immediate day-to-day exposure to new facilities, potentially bringing them into more frequent contact with the occupants of homeless shelters, as well as more frequent visible reminders of the policy remedy in question on the ballot referenda.

We find that areas immediately nearby new homeless facilities grew more favorable toward expanded homeless spending than comparable areas further away. That is, homeless services generate *positive* localized policy feedback effects. We find that precincts near (100-300 meters) new or expanded homeless facilities show increased vote shares for the post-treatment ballot proposition of around 2 percentage points, and this effect diminishes with increasing distance from the new facility (400-1000 meters). This effect size constitutes a substantial electoral shift, and is even more impactful when considering that the final vote share for the second ballot proposition was decided by just 0.36 percentage points. Pre-trend placebo tests support the identifying assumption of parallel trends in our difference-in-differences design, as does a placebo test using pre-existing shelters, which shows that voters who live closer to shelters generally do not have divergent trends in support for homeless shelter funding. Moreover, we show that this effect cannot be explained by movers either towards or away from treated areas and also show that our results hold under a natural experiment of commercial hotel conversions (which bypass typical review procedures), further minimizing concerns that selection effects are driving results.

Why do new homeless service facilities lead to positive policy feedback? We test the predictions of multiple competing theories. One possibility is that the new facilities produce localized positive externalities, and increased support stems from voters updating about the benefits of new shelters, activating self-interested considerations. To ascertain if this is the case, we measure the effects of new shelters on outcomes such as crime, disorder, and improvements over prior land-uses. On crime and disorder, we find consistent null effects of new facilities on street encampments, 311 and 911 calls, and police stops in areas around new shelters directly following their opening. We investigate whether new facilities potentially improve the area by examining heterogeneous effects based on prior land-uses. If shelters are replacing vacant lots or other 'locally unwanted land-uses' they may be seen as an improvement over existing conditions (Hankinson et al., 2026). While effects are slightly larger for new facilities on such sites, effects remain positive regardless of prior land-use.

Together, these results suggest it is unlikely that the positive policy feedback effects are driven by positive externalities activating voter self-interest.

Another set of mechanisms stems from the possibility that new facilities change the level of exposure to homeless people. Using individual-level survey data, we find that new local homeless service facilities predict increased self-reported interactions with homeless people. This increased exposure may activate affective attitudes about the homeless. Prior literature finds that many people hold two views in tension: positive attitudes towards the homeless in the form of compassion and a belief in structural causes underpinning homelessness (Lee et al., 2004), while also viewing the homeless with disgust and holding negative stereotypes about them or attributing their condition to personal failures (Cuddy et al., 2008). Although we do not have direct measures of affect, we show that respondents view both structural and individual factors as contributing, and find evidence of increased attribution to both stemming from new homeless facilities. Despite this, we find only attribution to structural causes strongly predicts support for homeless policy remedies. Furthermore, we do not find evidence that repeat exposure to homeless people reinforces negative attitudes in the form of "compassion fatigue," a prediction of some theories of negative attitudes (Cameron et al., 2016). We view this evidence as generally inconsistent with activation of negative affect, and suggestive that increased exposure may strengthen positive attitudes.

Increased exposure is consistent with another channel: that people in areas with new shelter construction become more aware of the problem and potential policy solutions. Again, using survey data, we show that residents living in neighborhoods with new homeless facilities are substantially more likely to report it as a pressing concern that needs addressing. They also are more likely to support the ballot proposition that we study, as well as a host of related policy remedies. We bolster the idea that voters learning about new facilities makes them more supportive by examining how politicians engage in credit claiming when new facilities open. Across 296 cities, we searched for press releases, coverage of ribbon cuttings, and social media posts by local elected officials (mayor and city councilors) for the 1,538 shelters that treat a district in our analysis, finding that 26% of projects had a local politician credit claim, a lower-bound estimate. Local elected officials are behaving as if they expect nearby voters to reward them, not punish them, for these new facilities. Together, we believe that in addition to potentially activating positive attitudes about the homeless, new homeless services facilities may increase the salience of the issue, thereby reinforcing voter demands to solve it.

Our study makes two major contributions. First, we present comprehensive evidence on the political effects of new homeless service investments. We adjudicate between competing directional hypotheses of policy feedback, revealing that voters respond with increased policy support rather than electoral backlash. Second, by enriching our analysis with data from several sources, including data on crime, 311 calls, street-view images of tents, and parcel-level land use, we show evidence consistent with this positive policy feedback being driven by exposure to shelters increasing the saliency of homelessness as an issue worthy of public investment, and potentially increasing voter sympathy for the homeless through increased exposure. We show evidence against the mechanism of positive externalities driving voter support through self-interest. Our findings imply that local politicians can gain from expanding homeless services and reducing homelessness. However, the continued severity of the homelessness crisis suggests that public opinion may not be the only constraining factor, suggesting that local fiscal concerns may be the main barrier to reducing homelessness. What's more, our findings reveal the prospective versus retrospective frames for local political responses, wherein the initial siting of such services may be met with NIMBY opposition but the retrospective political reaction ameliorates these attitudes and reinforces policy investment.

# 2 Policy Feedback Effects of Homeless Service Facilities

## 2.1 Prospective attitudes: NIMBYism and opposition to controversial land-uses

The politics of siting unwanted land-use are often contentious. Physical infrastructure needed to solve salient and pressing policy challenges, such as housing affordability, climate change, waste collection, or homelessness, is often opposed by local communities where it will be built (Schively, 2007). For example, the new housing needed to lower housing costs often receives localized ‘not-in-my-backyard’ (NIMBY) opposition, despite most people wanting more housing in the abstract (Hankinson, 2018; Elmendorf et al., 2024). Existing studies have proposed four mechanisms for this opposition. Voters may find new physical infrastructure, such as tall and aesthetically undesirable buildings (Larsen & Nyholt, 2024; Pietrzak & Mendelberg, 2025) or large wind turbines (Susskind et al., 2022), aesthetically unappealing. Symbolic or affective attitudes about who is building the project (Broockman et al., 2025; Monkkonen & Manville, 2019), or residing in it (Mendelberg et al., n.d.; Tighe, 2012; Sahn, 2025) may extend negative attitudes towards the proposal itself. Concerns over whether new residents attracted by these projects will strain public services such as schools, or cause localized negative externalities associated with congestion, such as increased traffic (Grabar, 2024; Oates, 1981; Trounstine, 2023) also weigh on the minds of voters. Finally, all of these concerns are internalized into residents’ beliefs about how new proposed infrastructure will affect their own and nearby property values or rents, activating financial self-interest (Fischel, 2005; Hankinson, 2018; Marble & Nall, 2021). This is particularly relevant for homeowners, who are highly active in local politics regarding land-use issues (Hall & Yoder, 2022; Yoder, 2020).

Each of these explanations applies to homeless service facilities. Like housing, new homeless facilities may increase density and with it may bring unwanted localized externalities such as higher traffic or strain on local public services. The public may perceive homeless services facilities as depressing property values, which may activate homeowner opposition. In addition, there is good reason to believe that homeless service facilities are a particularly unwanted local land use. The homeless people who will inhabit them are subject to intense negative stereotypes (Cuddy et al., 2008), above and beyond negative stereotypes about minorities, the poor, and people suffering from drug addiction or mental illness. For many, seeing homeless people arouses disgust, an attitude that begets a desire for physical distance and separation (Clifford & Piston, 2017). In addition to negative affective attitudes about the residents of these new facilities, concerns about crime and disorder predominate in meetings about proposed homeless shelters (Eisenberg, 2017) and political narratives (Vitale, 2008). Together, these studies suggest homeless service siting is particularly controversial. Indeed, reporting suggests many proposed shelters or supportive housing facilities elicit intense public backlash

## 2.2 Retrospective attitudes: public response to homelessness and policy feedback

*Prospective* opposition to new physical infrastructure may be fierce, but once facilities are built, do they factor into voters' *retrospective* reactions? The types of projects that generate opposition when proposed also may lead to voter backlash in the following election (Stokes, 2020). But physical infrastructure can also generate positive localized reactions, once built. Direct beneficiaries of new infrastructure, such as solar panels or public transit, may reward future efforts to fund policies or support the political parties that championed them (Rendleman & Yoder, 2025; Alberdi, 2024; de Benedictis-Kessner et al., 2026).

For many infrastructure projects, the second-order effects on users (what Hankinson et al. (2026) call the 'policy adjacent') may dwarf the effects among direct beneficiaries. For example, voters may support transit projects they will never use because it decongests roads that they do use (Holland, 2023). Alternatively, voters who supported a party that campaigned on delivering infrastructure may reward them for fulfilling their promises (Otteni & Weisskircher, 2022). But as with prospective opposition, proximity and the relative incidence of costs and benefits structure voters' retrospective evaluations. For instance, homeowner voters may support the same affordable housing facilities that renters oppose, based on how they stand to gain from neighborhood improvement and rising prices (Hankinson et al., 2026).

The second-order concerns of nearby residents who will never use homeless services facilities directly are particularly relevant, since the unhoused share of the population is relatively small, and votes at a very low rate (Brown & Zoorob, 2020). While the second-order costs of new homeless service facilities are undoubtedly spatially concentrated, it is less clear how the benefits are distributed. If homeless service facilities draw residents and users from across the city, the benefits on street homelessness reduction will be diffuse, with the local costs outweighing local benefits. But if these facilities primarily drive spatially concentrated improvements in neighborhood conditions, reducing street homelessness, lowering crime, and raising property values, the local benefits may outweigh the local costs.

Prior evidence on the local incidence of costs and benefits diverges by facility type. While homeless services facilities come in a diverse array of types, these facilities are generally of two forms: housing-based facilities (such as permanent supportive housing or transitional housing) and shelter-based facilities (such as emergency shelters and safe havens) (we elaborate on these differences in Section 3.2). Studies have generally shown that housing-based facilities, particularly permanent supportive housing, generate few negative externalities. These housing facilities often increase nearby property values (Galster et al., 2004; Been et al., 2008; Sitti et al., 2025). Moreover, studies do not find that these housing-based facilities increase localized crime (Galster et al., 2002), and studies which follow homeless people placed into permanent supportive housing over time show that their likelihood of committing a crime decreases substantially after placement (Ellsworth, 2022). The localized effects of shelter-based facilities, on the other hand, lean negative. Some studies find that new shelters can decrease nearby property values (Sitti et al., 2025; Owusu-Ansah, 2019). Others have found that some categories of crime Faraji et al. (2018) or 911 calls (Brown & Zoorob, 2022) can increase immediately near emergency shelter facilities.[1] Finally, both types of facilities can reduce the prevalence of street homelessness immediately near new facilities (Cohen, 2019)[2], which may be particularly consequential as perceptions of disorder may matter more than actual crime for behavior (Michener, 2013).

An alternative possibility is that, once built, homeless service facilities expose nearby residents to homeless residents who they did not previously encounter. These impacts may activate affective or symbolic attitudes about homeless people.[3] Prior studies about the impact of homeless services facilities and public attitudes about the homeless can therefore inform expectations about whether new facilities will generate positive (new facilities increase support for future investment in homeless services) or negative policy feedback.

Prior studies about affect towards the homeless have conflicting findings. Some studies highlight the negative attitudes that the public holds about the homeless. Homeless people are intensely negatively stereotyped Cuddy et al. (2008), dehumanized (Harris & Fiske, 2006), and viewed with disgust (Tan & Harris, 2021). These negative attitudes go above and beyond those held about commonly inferred attributes of homeless people, namely drug addiction or mental illness, leading to layered negative stereotyping as many associate homeless people with drug addiction and mental illness (Phelan et al., 1997). Repeated exposure to stereotyped groups can reinforce stereotypes (Smith et al., 2006), or cause long-

[1] Additionally, studies demonstrate a positive relationship between people's exposure to homelessness and crime (Roy et al., 2014).

[2] However, this study does not break shelters apart by type.

[3] One could imagine many other mechanisms that impact support for homeless services facilities, but not hyper-locally. For example, media coverage is likely to evoke affective feelings about homeless people. However, media markets typically cover entire cities, regions, or even states. It's unlikely such a mechanism would explain localized policy feedback among the policy adjacent.

run emotional exhaustion (sometimes called ‘compassion fatigue’) potentially aggravating these negative attitudes (Cameron et al., 2016). Compassion fatigue in particular has been theorized by some to have led to policies that criminalize homelessness and contributed to retrenchment of spending on shelter-and-housing based solutions in the 1980s and 90s (Vitale, 2008; Markee, 2025). Some studies also directly connect these negative attitudes to policy preferences. For example, Clifford & Piston (2017) find that disgust towards the homeless predicts higher support for policies criminalizing street homelessness, such as public sleeping bans. Studies have found that higher rates of blatant dehumanization (explicitly viewing certain groups as less than human) predict voting for candidates and policies to punish the dehumanized group (Kteily & Bruneau, 2017). Finally, Sands (2017) finds that passive exposure to people who appear low-income (and may be perceived as homeless) on the street reduces support for redistributive taxation.

However, other studies present an alternative view. Studies have repeatedly shown that the public concurrently holds positive attitudes about the homeless, namely sympathy and a belief in structural causes (as opposed to belief in personal failure) underpinning homelessness (Lee et al., 2004; Tsai et al., 2019; Henig, 1994). Majorities of Americans report willingness to engage in costly behavior to address homelessness, with Link et al. (1995) finding that most are willing to pay $100 a year ($260 in 2026 dollars) to fund new shelters and three-quarters are willing to “have a shelter for homeless people located near your home.”[4] One repeatedly replicated finding from these studies is a strong positive association between self-reported exposure to homeless individuals and positive attitudes, including willingness to support government intervention in the form of new shelters or supportive housing facilities. These studies interpret this relationship as evidence in support of the influential Contact Hypothesis (Allport, 1954), despite contact (causally observing

[4]As further evidence, Mendelberg et al. (n.d.) find high support for constructing new deeply-affordable housing (with units set aside for households making less than 30% of area median income) in a respondent’s own neighborhood, despite a majority of supporters listing homeless people as a ’major’ beneficiary of the new housing.

homeless people in your neighborhood) occurring under less than ideal conditions. While self-reports suffer from selection, social desirability, and issues with recall, these positive associations persist even when subset to types of exposure to homelessness that are unlikely to be driven by selection (such as a close family member experiencing it) and attempts to control for social desirability through batteries of questions (Link & Cullen, 1986; Lee et al., 2004). Moreover, higher objective rates of unsheltered homelessness are also associated with higher vote shares for a ballot initiative to increase spending on homeless services (Laniyonu & Byerly, 2021), lessening concerns about poor recall. Gross & Wronski (2021) also find that 70% of respondents donated a portion of their survey compensation to a homeless charity, suggesting willingness to engage in costly behavior is genuine. Finally, while Clifford & Piston (2017) find that disgust predicts higher support for criminalization, critically, disgust has no effect on support for additional government funding for shelters or supportive housing.

These studies yield conflicting predictions about the direction of policy feedback to new homeless services facilities. If self-interest dominates, studies about the localized impacts of homeless services facilities suggest that the direction of policy feedback will be moderated by the type of facility. The generally positive externalities of housing-based facilities should produce positive feedback, while more mixed findings about shelter-based facilities should produce negative or no feedback. If new facilities activate affective attitudes about the homeless, predictions are less clear. First, it is unclear if new facilities increase or decrease the presence of homeless people. New facilities may plausibly reduce street homelessness due to additional capacity in the shelter system or if paired with policies that reduce street homelessness (such as encampment sweeps). Alternatively, new facilities may increase localized exposure to homeless people, as clients move in and out of the facilities throughout the day, and the facility itself represents a salient and visible reminder of homelessness. If facilities increase (decrease) the presence of homeless people, studies highlighting negative affective attitudes imply negative (positive) policy feedback, whereas studies which highlight positive affective attitudes imply positive (negative) policy feedback.

In addition to these conflicting findings, one problem plaguing multiple studies is selection. For example, Link et al. (1995); Lee et al. (2004); Henig (1994); Laniyonu & Byerly (2021) all find that increased exposure to homelessness increases sympathy towards the homeless or policies to address homelessness. However, residents may select into neighborhoods based on their preexisting attitudes about the homeless. Even among those who do self-select into a neighborhood with high rates of homelessness, residents may change their behaviors. For example, residents who are particularly averse to interactions with homeless people may deliberately change their daily routines to avoid encounters in areas with known shelters or encampments, lowering their self-reported exposure. While these studies attempt to address these issues in a variety of ways such as by controlling for partisanship, social desirability, or by using objective measures, selection is difficult to fully address without (quasi-) experimental treatments that exogenously assign exposure to homelessness. The few studies that do exist have diverging results. Sands (2017) experimentally manipulates exposure by randomly assigning if residents of an affluent Boston neighborhood encounter an affluent or low-income presenting individual before being intercepted by an enumerator who administers a survey. Those who saw low-income individuals became less supportive of a redistributive tax. However, it is unclear if subjects of the study perceived these individuals as homeless or simply poor, if attitudes about redistribution map onto attitudes about additional homeless services spending, if these attitudes persist over time, and finally whether reactions in an affluent neighborhood generalize to less affluent settings. A more direct test comes from Brown & Zoorob (2022), who quasi-experimentally manipulate exposure to homelessness after an emergency forced a homeless shelter located on a remote island off the coast of Boston to relocate in a dense neighborhood. Here, exposure to homeless people and demand for government services increases, as evidenced by an increase in 311 calls. They find that the relocated homeless facilities increased turnout and vote share for the incumbent Mayor among voters living nearby the new facilities. However, this study is not able to measure how such a relocation impacts attitudes towards homelessness *policies*, directly testing policy feedback. Moreover, it is unclear if increased turnout and vote share for the incumbent better reflects a positive or negative reaction by the public.

## 3 Data and Measurement

We study the localized effects of new homeless service facilities in California. California, with its temperate climate, lack of a mandate for indoor shelter, and stark inequality, has become a symbol of the United States' homeless crisis. California voters are increasingly frustrated by the persistence of unsheltered homelessness, with a plurality of residents frequently identifying homelessness as the state's most important problem (Baldassare et al., 2020).

Concurrent with rising voter frustration, California policymakers have made large investments in building new homeless service facilities to address the problem. Between 2014 and 2024, the total number of year-round beds in shelters and housing facilities for people experiencing homelessness increased from about $95,000$ to over $208,000$, a more than doubling of capacity (U.S. Department of Housing and Urban Development, 2014, 2024b).[5] These facilities were often funded directly by voters, who have, since 2016, voted on two statewide propositions, authorizing $8.4 billion for homeless services and treatment. Residents of Los Angeles, San Francisco, and other localities have approved billions more in city and county measures. These repeated, voter-supported ballot measures present a unique research design in which to study the localized effects of new homeless service facilities on revealed, costly preferences for continuing funding. We leverage precinct-level differences in support between two statewide ballot propositions in 2018 and 2024 using a differences-in-differences design. This design makes progress on selection issues in prior cross-sectional studies of local effects of homeless service facilities.

[5]Figure represents total beds reported to HUD. However, this figure undercounts the total growth in homeless services facilities in California. As we detail in Appendix A, these figures exclude many locally funded projects. Our approach adds these facilities for a more complete picture of new facilities.

## 3.1 Measuring support: statewide ballot propositions

Our measure of support for homelessness policy is vote share on two ballot propositions asking California voters to authorize bonds to finance homeless services. The first proposition, Prop 2, was held as part of the general election on November 6, 2018; the second proposition, Prop 1, was included on the ballot in the primary election on March 5, 2024 (See Table 1).[6]

Table 1: Ballot Propositions in Main Analysis

| |
|---|
| **Proposition 2 (November 2018)**<br>Passed 63–37 |
| *Authorizes Bonds to Fund Existing Housing Program for Individuals With Mental Illness. Legislative Statute.*<br>Proposition 2 authorizes the state to sell up to $2 billion in bonds to fund the No Place Like Home program, which builds and rehabilitates housing for individuals experiencing homelessness, or at risk of homelessness, who have mental illness. The measure permits the state to use up to $140 million annually from existing county mental-health funds to repay these bonds over roughly thirty years. |
| **Proposition 1 (March 2024)**<br>Passed 50–50 |
| *Bonds for Mental Health Treatment Facilities*<br>Proposition 1 authorizes $6.4 billion in new bonds to build more facilities for mental-health care and drug or alcohol treatment and more housing for people experiencing homelessness, or at risk of it, who have behavioral-health or substance-use challenges. It modifies California's Mental Health Services Act—renaming it the Behavioral Health Services Act—shifting roughly $140 million annually in tax revenue from counties to the state and requiring counties to dedicate more funding to housing and support services. About $4.4 billion of the bond proceeds would support treatment facilities and $2 billion would fund housing, with over half of the housing funds reserved for veterans. |

We harmonize all vote returns to the boundaries of the 2024 precincts from precinct shapefiles for both the 2018 and 2024 elections from California's Statewide Database, the

[6] While these measures funded more than just homeless services, heavily emphasizing behavioral and mental health services, most people associate drug addiction and mental illness with homelessness. For example, a 2023 Public Policy Institute of California (PPIC) poll found that 80% of Californians attribute homelessness to drug addiction and 66% attribute it to a lack of mental health services. Additionally, coverage about the propositions was largely focused on homelessness, with headlines such as "Homelessness is on the California ballot this March" (Kendall, 2024). Governor Gavin Newsom, who proposed the 2024 Proposition, explicitly linked it to homelessness, calling a yes vote a way to "prioritize getting people off the streets, out of tents, and into treatment" (Yes on Prop 1 – Governor Newsom's Ballot Measure Committee, 2023).

state's official redistricting database maintained by the University of California, Berkeley (`https://statewidedatabase.org`). We overlay the 2018 precinct shapefiles with the 2024 precinct shapefiles, producing a set of intersection polygons representing every area of overlap between a 2018 and a 2024 precinct. We then use individual-level voter registration data from L2 to geocode each voter who cast a ballot in the 2018 election into these intersection polygons. This allows us to count the number of 2018 voters residing in each intersection polygon. We use these voter counts to apportion 2018 precinct-level vote shares to 2024 precinct geographies. Specifically, for each intersection polygon, we assign the vote share observed in the corresponding 2018 precinct and weight it by the share of that precinct's 2018 voters falling within the polygon. We then aggregate across all intersection polygons within each 2024 precinct to produce a population-weighted estimate of 2018 vote share in 2024 precinct boundaries.

The resulting dataset is a panel of 2024 precincts, each with an observed 2024 vote share and a reconstructed 2018 vote share. Our approach improves on simpler alternatives. Apportioning by area overlap alone would introduce error in a county like Los Angeles where population density varies considerably across space. Using Census block crosswalks — as provided by the Statewide Database — would require moving through two intermediate geographies (2018 precincts to 2010 blocks to 2020 blocks to 2024 precincts), compounding measurement error at each step. Our approach avoids these issues by using the actual residential locations of validated 2018 voters to weight the apportionment directly.[7]

## 3.2 Measuring treatment: new homeless service facilities

We measure the effect of new or expanded homeless services facilities opening nearby voters between the two ballot propositions. To identify these facilities, we use data from two sources. The first is the U.S. Department of Housing and Urban Development (HUD) Hous-

[7] We note, however, that this method assumes vote shares are uniform within 2018 precincts: we observe where individual voters live but not how they voted, so we apportion the precinct-wide ballot support equally to all intersection polygons involving that precinct.

ing Inventory Count (HIC). HIC provides addresses for nearly all homeless service facilities that participate in the federal Homeless Management Information System (HMIS), required as a condition for receiving federal money from HUD.[8] The second source is the California Homeless Data Integration System (HDIS), obtained via request from the California Interagency Council on Homelessness (Cal ICH). We use this data because it includes data not available at the federal level through HUD. Specifically, this dataset includes many new housing-based facilities funded by state-and-local initiatives that include dedicated set-asides for current or formerly homeless populations.

For both datasets, we subset to single-site 'project-based' facilities, since scattered site and voucher-based programs do not produce the spatially concentrated effects we are interested in studying. These capture the majority of shelter-based facilities and housing-based facilities (see Appendix Table A2 for a count of all facilities dropped from the analysis sample).[9] Shelter-based facilities include emergency shelters (short-term beds, often in congregate facilities, with minimal services) and safe havens (drop-in facilities that serve hard-to-reach populations, often with severe mental illness). Housing-based facilities include supportive housing projects (permanent apartments with no limits on length of stay with dedicated on-site social services) and transitional housing (short-term beds in buildings often configured as apartments that charge affordable rents).[10] The alternative type of program, 'tenant-based' facilities such as rapid-rehousing (rental assistance, such as vouchers and payments for arrears, for individuals to use in privately-owned rental housing), are typically not possible to geocode (although a few single-site rapid-rehousing facilities do appear in our sample) nor do they represent the type of brick-and-mortar homelessness facilities we are interested in.

[8]Reported facilities also include those that are not actively receiving federal money. These include all emergency shelters, transitional housing, rapid-rehousing, permanent supportive housing, and both residential and shelter programs operated by private, faith-based, or hospital networks. See: `https://www.hudexchange.info/faqs/1822/what-should-be-included-in-the-hic/`

[9]For example in 2023, 81% of shelter-based and 64% of housing-based facilities were classified as single-site.

[10]CA Health & Safety Code § 50801 (2024).

We de-duplicate the HUD and Cal ICH datasets at the address level (See Appendix A for details). As HDIS did not include addresses, we manually identified these by triangulating various fields provided, such as the name, Zip Code, and number of beds. We then identify new projects, which are those which appear after November 2018.[11] New projects can encompass both new sites as well as expanded services at existing sites. Our final dataset contains 1,538 new facilities, representing over $88,000$ new beds.[12] Table 2 breaks down our new facilities by type, which are roughly equally split between shelter and housing-based facilities.

Table 2: Single-site homeless service facilities opened in CA, November 2018–March 2024

| **Category** | **Facility Type** | **New Facilities** | **New Beds** | **% of Total Beds** |
|---|---|---|---|---|
| Shelter-Based | Emergency Shelters | 623 | 48,051 | 54% |
| | Safe Havens | 9 | 525 | 1% |
| Housing-Based | Permanent Supportive Housing | 576 | 27,241 | 31% |
| | Transitional Housing | 230 | 6,808 | 8% |
| | Other Permanent Housing | 100 | 5,670 | 6% |
| **Total** | | **1,538** | **88,295** | **100%** |

To assign precincts as treated or control, we link the data on homeless services facilities to our panel of 2024 precincts and calculate the geodesic distance in meters from each precinct's centroid to the closest new homeless service facility that appears between 2018 and 2024.

# 4 Research Design: difference-in-differences

To identify the effect of new homeless services facilities on support for additional spending on said facilities, we conduct a differences-in-differences analysis comparing changes in vote share and turnout between two ballot propositions. One challenge is that homeless services

[11] To identify precise opening dates, we supplemented the administrative records with google searches of facility openings, drawing from social media posts, press releases, and facility websites. This results in 635 facilities with corrected dates.

[12] Note this is larger than the approximately $79,000$ new beds added in the HUD HIC dataset alone due to the inclusion of the HDIS dataset.

facilities are not sited or opened at random. Neighborhoods that receive facilities are likely different from those which do not (for example, targeting neighborhoods with lower incomes, greater need for facilities, or siting for strategic political reasons related to expected electoral backlash (Takahashi & Dear, 1997; Lyon-Callo, 2001)). To overcome these selection issues, we compare precincts located close to new homeless shelter sites to those located slightly further away. By limiting to a small geography of places that plausibly could have received new facilities, we minimize issues of endogeneity in homeless facilities siting. This is because *within* neighborhood decisions in siting are likely driven more so by idiosyncratic factors, such as the availability of development sites (for new construction) or vacant properties (for conversions), rather than strategic political considerations that would threaten our empirical strategy (Reid et al., 2022; California State Legislature, 2020; Fiore et al., 2024; California Senate Committee on Housing, 2024). By comparing changes in support for the ballot propositions within the same geographic area, and comparing between areas more or less proximate to new sites, we are able to isolate the effect of homeless facility siting on changes in homelessness policy support.

We classify precincts as being treated if the centroid of the precinct is within a certain radius of a new shelter. Our main results show estimations from definitions of treatment defined by increasing cutoff distances. To illustrate our assignment strategy here, we will consider a version of our estimation where all precincts within 300 meters of a new site are considered as treated. At this distance, we treat all precincts between 300 meters and 650 meters as being in the "control" group while all other precincts beyond this distance are excluded from the estimation. We then measure within-precinct changes in referendum voting in the 2018 and 2024 elections and compare these average changes across treated and control groups.

When presenting our differences-in-differences estimates we report results at increasing distance cutoffs from 100m to 800m, mirroring the approach of studies such as Enos

(2016) or Brown & Zoorob (2022). We do so for two reasons. First, increasing cutoffs test whether effects are largest for areas closest to new shelters, as these would be the most affected and potentially have the largest exposure to the new impacts of the policy. This is informed by prior literature that political behavior in response to controversial land-uses is typically isolated to extremely small distances (Hankinson, 2018; Sahn, 2024). Second, we increase cutoffs in recognition that the treatment cutoff will always be somewhat arbitrary. We also present results from differing control bandwidths (e.g., 500 meters rather than our preferred specification of 350 meters) in Appendix C.1. Figure 1 visually demonstrates how these distance cutoffs change the treatment assignment.[13]

To formalize our estimation strategy, let $y_{it}$ be the proportion of precinct $i$ that voted in support of the homeless shelter referendum in time $t$. Let $\alpha_i$ be the precinct fixed effect, and $I(t = 2024)$ an indicator variable taking a value of 1 if the observation is for the 2024 election, 0 otherwise. $I(D_i < d^*)$ is an indicator variable taking a value of 1 if the centroid of precinct $i$ is less than $d^*$ meters from a new (by 2024) shelter. The inclusion of precinct fixed effects controls for all characteristics of precincts that are constant in 2018 and 2024, while the dummy variable for 2024 versus 2018 controls for all common factors for a given election year that are constant across precincts within a given year. Standard errors are clustered at the shelter level to account for correlation among treated and control precincts whose treatment status is defined relative to the same facility (Abadie et al., 2023).[14] We estimate the following linear model:

$$y_{it} = \alpha_i + \beta \ I(t = 2024) + \theta \ I(t = 2024) \times I(D_i < d^*) + \epsilon_s \tag{1}$$

[13] We consider two alternative approaches to treatment assignment in the Appendix. A first strategy is to define a fixed radius of 1km and to assign the same units across all specifications to treatment and control. The second approach is to construct a continuous measure of treatment by measuring the proportion of the precinct polygon that intersects with a facility radius. Both approaches are discussed in more detail in Appendix C.1.

[14] Because residual correlation could also operate over broader geography, in Table A8 in the Appendix we demonstrate the robustness of our results to alternative standard error estimation strategies: clustering at a higher geographic level (county) and Conley spatial standard errors using triangular kernels of 5 and 10 kilometers (Conley, 1999).

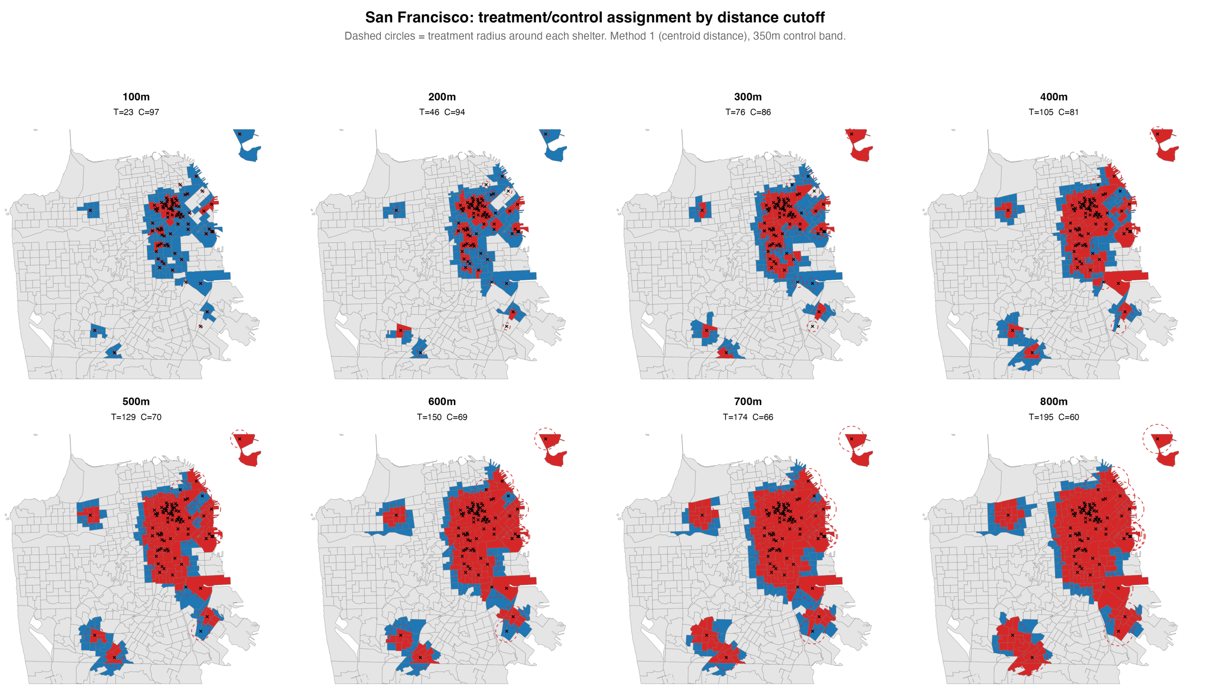



Figure 1: **Treatment and control assignment by treatment distance cutoff, San Francisco:** map shows treated precincts in red, control precincts in blue. New homeless services facilities represented by the figure "x." Treatment assignments increase in 100 meter increments. Control increments always add 350 meters to the treatment increment.

Our quantity of interest is $\theta$, which represents the difference-in-differences estimator comparing changes in policy support before and after new shelters appear between areas more/less proximate to the new shelter sites.[15]

These difference-in-differences estimates can be interpreted as the effect of proximity to a new shelter on support for public investment in housing for homeless people. In the context of this study, the identifying assumption of parallel trends between treated and control groups requires that precincts slightly further away (i.e. between 300 and 650 meters from a site) from a shelter offer an appropriate counterfactual for how policy support *would* have trended in precincts within 300 meters of a new site, had that new shelter not appeared. We present results supporting this identifying assumption after the presentation of our main results.

# 5 Results

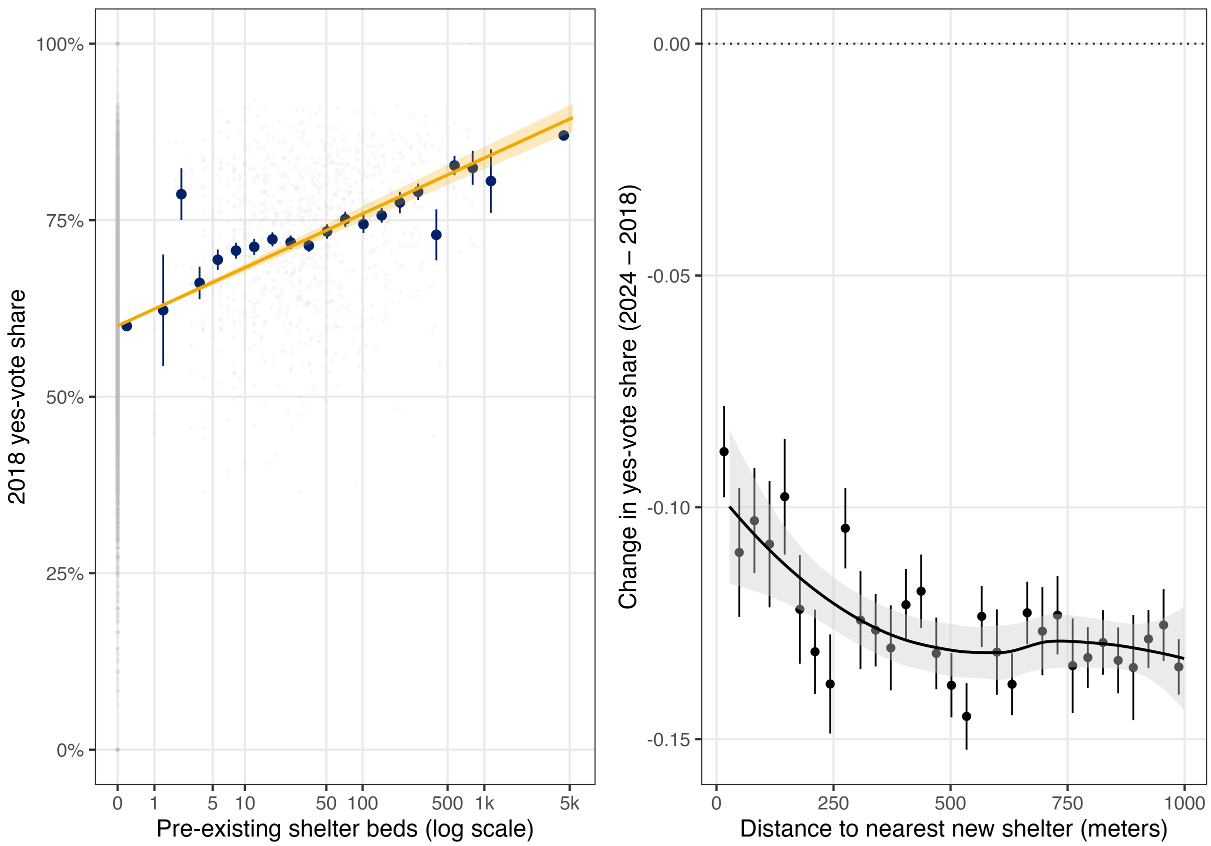
Before presenting our econometric results, we begin by showing descriptive patterns of proposition support and distance to homeless services facilities. The left panel of Figure 2 shows the relationship between the preexisting number of beds in facilities in a precinct in 2018 and support for the 2018 ballot proposition. Precincts that already had beds before 2018 showed higher support for the 2018 proposition. This pattern is suggestive of positive selection and motivates our within-neighborhood research design, which controls for these baseline differences in support for homeless services by comparing precincts in close geographic proximity to one another. The right panel shows that shifts in support across time are correlated with proximity to new shelters. While the 'yes' vote share drops, on average, 13 percentage points between 2018 and 2024, this decline is smaller near new facilities, with a decreasing

[15] In Tables A11–A13 in the Appendix, we further estimate a continuous specification that uses logged distance directly:

$$\Delta y_i = \alpha + \beta \log(D_i) + \boldsymbol{X}_i' \boldsymbol{\gamma} + \epsilon_i \tag{2}$$

where $\Delta y_i = y_{i,2024} - y_{i,2018}$ is the change in vote share for precinct $i$, $D_i$ is the geodesic distance from precinct $i$'s centroid to the nearest new shelter, $\boldsymbol{X}_i$ is a vector of precinct-level demographic controls.

relationship between distance and change in vote share until about 400 meters, at which point the relationship flattens. Critically, temporal changes in ‘yes’ vote are driven by many reasons unrelated to homelessness. For example, the November 2018 election was a strong showing for Democrats nationwide (winning the popular vote by 8.6%), whereas 2024 was a stronger year for Republicans. This further motivates our within-neighborhood differences-in-differences design, which isolates just the localized effect of a new homeless facility on vote share.




(a) 2018 support by preexisting beds (b) Change in support by distance to new facility

Figure 2: **Voters in areas with pre-existing and newly opened shelters are more likely to support homeless spending measures:** left panel plots 2018 ballot proposition support against pre-existing shelter exposure at the precinct level. The right panel shows a binned scatter plot of the within-precinct change in vote share from 2018 to 2024 plotted against the distance from the precinct centroid to the nearest new shelter, for precincts within 1,000 meters of a new facility.

Figure 3 plots the difference-in-differences estimates from our main specification across increasing distances at which the treated group is defined, from 100 to 800 meters.[16] When comparing precincts within 100 meters of a new shelter to precincts between 100 and 450 meters away, we find that more proximate precincts increased their support for funding homeless services by approximately 2 percentage points (or a 4% increase over average support for the proposition). Comparing precincts less than 200 meters to those between 200 and 550 meters, the difference-in-differences estimate is 2.0 percentage points, and with a treatment cutoff of 300 meters the effect is 1.7 percentage points. Effects remain statistically significant through 500 meters and attenuate steadily with distance. This causal estimate of the effect of shelter openings on support for homelessness spending mirrors the descriptive relationship: for areas closest to new homeless facilities, a new opening produces a positive effect on support for investment in homeless housing, and the effect is largest for precincts immediately adjacent to the new shelters.[17]

These effect sizes are substantively meaningful when set against the overall decline in support. The average precinct in our sample decreased support for the 2024 referendum (relative to 2018) by approximately 13 percentage points. A 2 percentage point effect at 100 meters thus offsets roughly one-sixth of this average decline. The 2024 referendum was decided by less than one percentage point, so localized shifts of this magnitude across many treated precincts can have a meaningful impact on the final outcome.[18]

[16]Our main result is robust to alternative treatment specifications. In the Appendix Section C.1, we evaluate three alternatives: a continuous-exposure design, a fixed-sample design that holds the sample of non-excluded precincts fixed while varying the threshold between treatment and control, and alternative control-band constructions that matched treated units on area and number of precincts. We also show that our main results are robust to alternative control distance specifications – increasing the bandwidth that defines the control set of precincts at a given treatment distance cutoff.

[17]In Appendix Figure A12 we present results measuring new shelter effects on individual-level turnout for voters living near new shelter sites across the two elections. We find similarly localized effects on turnout, albeit at smaller magnitudes than our vote share effects.

[18]Appendix Section D.2 shows that proximity to new shelters does not influence congressional Democratic vote share, meaning these results are specific policy-focused response rather than a broader political change.

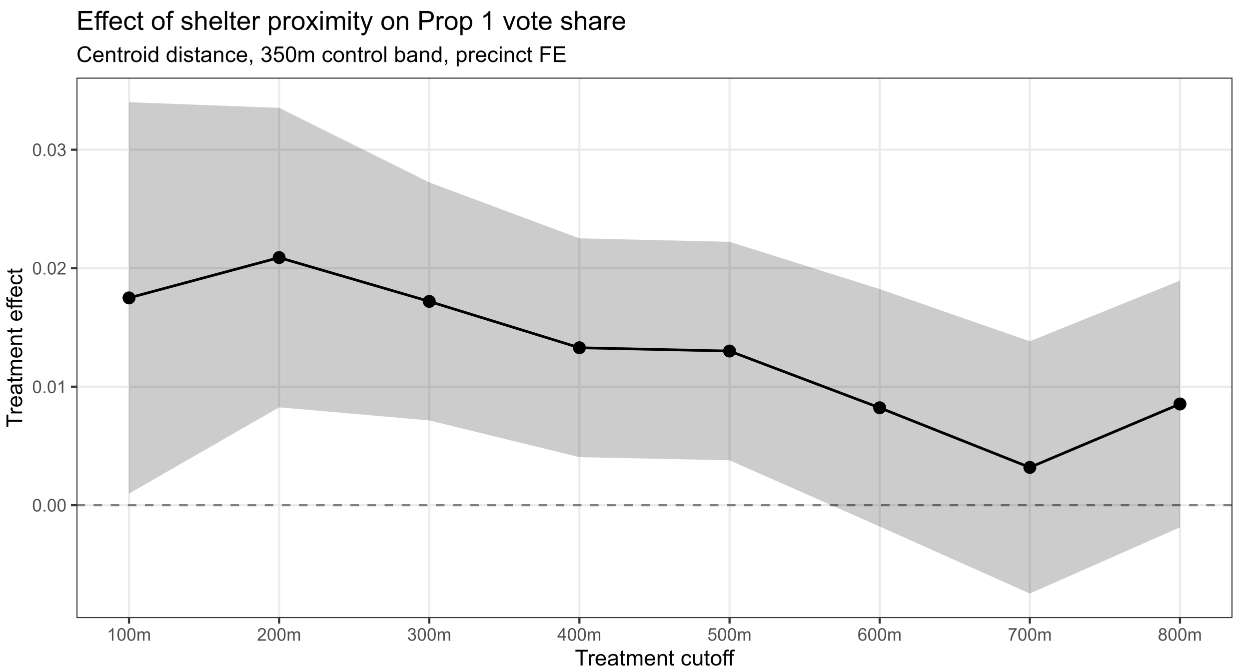



Figure 3: **Voters in precincts nearby new shelters are more supportive of future homelessness spending:** points represent difference-in-differences coefficients from Equation 1, estimated with increasing distances (x-axis) at which the treated group is defined. For each treatment cutoff, the control group consists of precincts between the treatment cutoff and the cutoff plus 350 meters from the new site. The shaded area represents 95% confidence intervals with standard errors clustered at the facility level.

## 5.1 Validity

To assess the validity of our empirical results, we conduct a series of analyses each designed to test different potential concerns with our design and estimation strategy. These include analyses of parallel trends, placebo tests, and subsample analyses to support the difference-in-differences identification assumption. We also diagnose sensitivity to different electorates across the two referenda, decomposing our treatment effects into mobilization and persuasion components. Next we test whether residential sorting in response to new shelters could influence the results. Finally, we conduct a conceptual replication using individual-level poll data at a coarser geography (Zip Code), utilizing two PPIC polls fielded just before the March 2024 election.

### 5.1.1 Parallel Trends

The identifying assumption of the difference-in-differences design is that treated and control precincts would have followed parallel trends in the outcome absent treatment. In the context of our study, this means that shelters must not be placed in precincts that are already diverging in terms of support for homeless shelter funding from the precincts that are slightly further away from new shelter sites. We evaluate these pretreatment trends using a previous statewide measure from 2014, Proposition 41. This proposition was more explicitly focused on housing for homeless veterans, rather than the more general Propositions 2 and 1. We also estimate pre-trends for Los Angeles, specifically, using Measure HHH, a city proposition from November 2016 and Measure H, a county proposition from March 2017. Both funded homeless service facility construction. We also evaluate pretreatment trends on two other electoral outcomes: Congressional Democratic vote share and turnout in the 2014, 2016, and 2018 cycles. All analyses use data harmonized to the same 2024 precincts as the main analysis.

Figure 4 presents the results of the difference in pre-trends between treated and control units at each radius. For each outcome, we show the coefficient on the interaction between the pre-2018 trend and an indicator for treatment versus control units. We see no significant pre-trends across outcomes, with estimates close to zero. This evidence of minimal pre-trend differences is most apparent for the statewide homeless proposition analysis comparing the 2014 Proposition 41 to Proposition 2 in 2018 – the most direct test of pre-trends in our main analysis. Taken in sum, the pre-trend evidence presents strong evidence in support of our identifying assumption: it is unlikely given the near zero pre-trend estimates that our treatment effects are driven by places that do and do not receive new homeless shelter facilities diverging politically prior to 2018 and those trends continuing after new shelters appeared.[19]

In addition to examining parallel-trends, we further validate the comparability of these areas on observable characteristics at the most proximate distance treatment cutoffs. In Appendix Tables A4-A6 we present balance tables across the 100, 200, and 300 meter cutoffs. Across the 100m, 200m, and 300m balance tables, treated precincts are consistently smaller in population and have more exposure to pre-existing shelters, but they otherwise look broadly similar to nearby control precincts on baseline characteristics.

As a complementary falsification test, we re-estimate the main specification using proximity to *pre-existing* (pre-2018) shelters as a placebo treatment in place of new shelter openings. Because pre-existing shelters were present in both the 2018 and 2024 elections, proximity to one should produce no differential change in referendum support; a non-null estimate would instead indicate that shelter-proximate precincts are on differential trends. At each distance cutoff we exclude precincts that are actually treated by a new (2018–2024) shelter from both the placebo-treated and placebo-control groups, so the placebo is not contaminated by the real treatment effect. As Appendix Figure A6 shows, the placebo estimates are small and statistically indistinguishable from zero across all cutoffs, consistent with the parallel-trends evidence above.

[19]Raw pre-trends are shown in Appendix Figure A4.

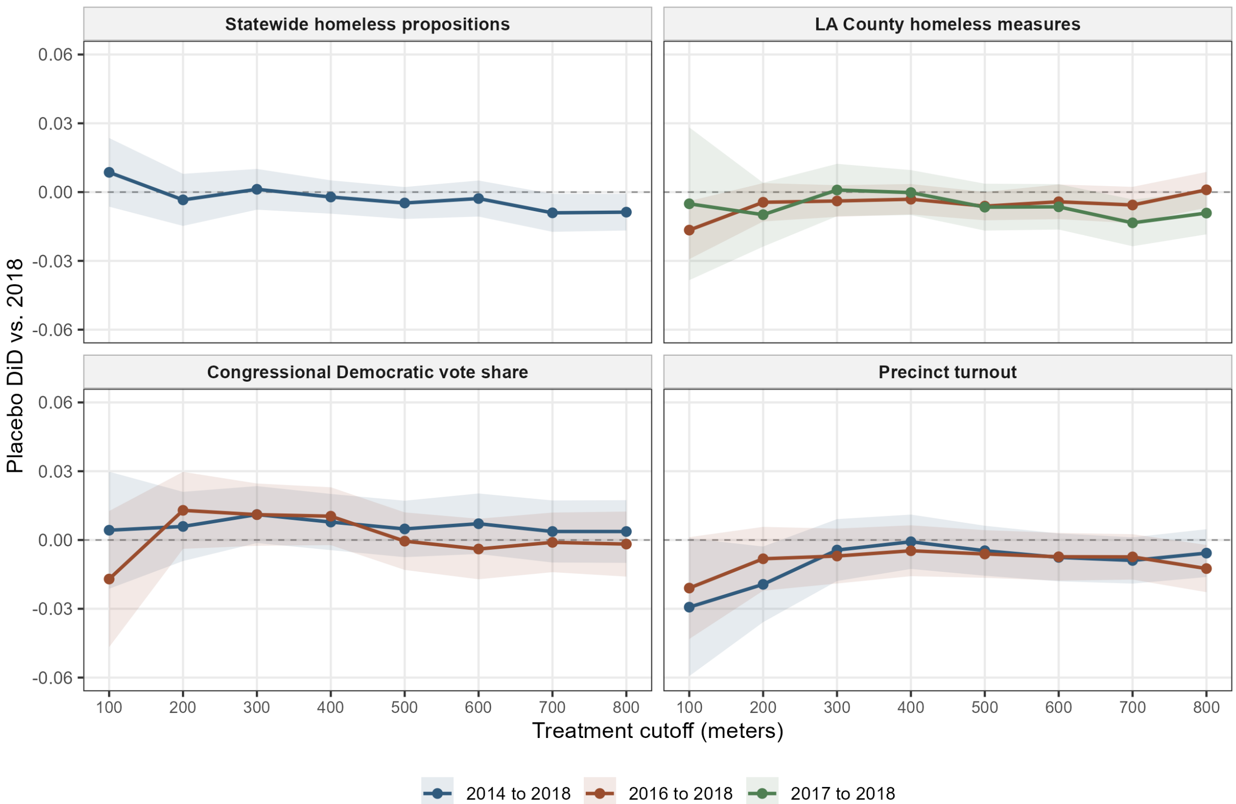



Figure 4: **Pre-trends on homeless support, Congressional vote, and turnout indicate no difference between treated and control precincts:** each panel plots the placebo DiD coefficient comparing the change in outcome from the pre-period year to 2018 as a function of exposure to a new shelter within the distance cutoff between 2018 and 2024. The shaded ribbon is the 95% confidence interval, with standard errors clustered at the shelter level. Panels: (A) statewide homeless propositions (2014 Proposition 41 and 2018 Proposition 2); (B) LA County homeless measures (2016 Measure HHH, 2017 Measure H); (C) Congressional Democratic vote share (2014, 2016, 2018); (D) precinct turnout (2014, 2016, 2018).

Lastly, we re-estimate our effects on a sub-sample of shelters with expedited discretionary review procedures, thus testing our results on a sample where it is less likely that environmental review or public hearings may enable discretionary small-scale changes in facility siting within neighborhoods to avoid blocks where politicians anticipate backlash or expect support. To do so, we leverage a natural experiment in facility siting. During the COVID-19 pandemic, California acquired hotels and motels for rapid conversion to permanent housing under Project Homekey. Launched by State A.B. 83 in 2020, Homekey exempted qualifying acquisitions from the California Environmental Quality Act and authorized by-right conversion of hotel and motel units to residential use without triggering discretionary zoning reviews. These two procedural steps together constitute the bulk of conventional facility siting timelines and introduce opportunities for local feedback (through public hearings) that may encourage local decision-makers to change their siting decisions.[20] A federally imposed December 2020 expenditure deadline further constrained site selection: local governments had to identify properties for which they could reach preliminary agreements with willing sellers before submitting applications, limiting the feasible choice set to immediately available, distressed-market hotel properties irrespective of neighborhood characteristics (Reid et al., 2022). Unlike standard facility siting, these acquisitions bypassed local siting review and were determined primarily by building availability, not neighborhood characteristics. Therefore, we believe these conversions represent an exogenous treatment.

Restricting the main differences-in-differences specification to facilities identified as Homekey conversions (18.5% of new facilities in our sample), we find an effect of approximately 2 percentage points at 200m (Appendix Figure A5), and generally find effect estimates across distance cutoffs consistent with our full sample effects.[21] While estimates are noisier than our main effects due to the limited sample size, we are reassured that the effects

[20]These two steps typically require at least a year of environmental review, public hearings, and conditional use permits.

[21]Although there is no consistent identifier in our data sources for Homekey sites, we identify them as such if they meet one of the following criteria: whether the parcel land use was recorded as hotel prior to 2020 and whether the shelter name matched California Homekey award tracking documents.

are directionally consistent and similar in magnitude, lessening concerns that hyper-local endogenous shelter siting is driving our main effects.

Parallel trends could also be violated if there are time varying shocks that differentially impact treated versus control precincts. In the context of our study, such shocks are more narrow in confounding potential because our design compares neighboring precincts that differ only slightly in proximity to new shelters. Still, to measure the robustness of our results to such shocks in Appendix Table A9, we demonstrate the consistency of the estimates to the inclusion of 2018 baseline controls – precinct Congressional Democratic vote share, turnout rate, total registration, and the number of pre-existing shelters within the treatment distance cutoff from each precinct – interacted with the year indicator, to control for differential time trends by baseline covariates. These controls account for time-varying shocks that might be differentially correlated with differences in baseline covariates across treatment and control precincts.

Similarly, we estimate in Appendix Table A10 alternative specifications that account for unobserved spatial confounders: time-varying shocks that differ across places within California and differentially impact areas closest to shelters. Our results are robust to inclusion of latitude-by-time and longitude-by-time trends, as well as county, Zip Code, Census place, and major city specific time trends. We do observe moderately attenuated estimates in these more restrictive geography-specific time trend specifications, but the results in the 200 meter group, where we observe the most prominent effects in the main estimation, are consistently positive and significant across specifications.

#### 5.1.2 Different Electorates

A major potential concern is that by comparing the March 2024 primary election to the November 2018 general election, we are comparing different electorates. If primary voters near shelters are systematically more pro-homeless than general election voters, independent of any shelter effect, this composition difference could inflate the measured treatment effect estimates.

To address this, first we re-estimate our main specification using November 2024 local ballot measures that concerned homelessness in two large jurisdictions: Los Angeles County (Measure A) and San Francisco (Prop B). We compare these two elections to the November 2018 statewide election, allowing us to compare two general elections. Appendix C.5 details these ballot measures and documents their results. Both estimates are statistically significant and essentially match the main statewide estimate from November 2018 - March 2024, indicating the result is not likely an artifact of comparing across primary and general electorates (See Figure A7).

Then, we complement the general election to general election robustness check with a test that estimates the decomposed effect of mobilization and persuasion at treatment cutoffs of 100, 200, and 300 meters. To do so, we first re-estimate our effects assuming that the effect had no persuasion effects, and estimate how much of the change in yes vote we would observe from mobilization alone: based on estimated effects on the total number of yes and no votes in each precinct.[22] Figure A8 presents the estimated composition shares. While there is some evidence of positive mobilization effects at 100 meters, accounting for roughly 19% of ATT($S$), at 200 and 300 meters, mobilization effects are negative and persuasion effects are positive. This analysis supports the assertion that our estimated effect of exposure to new shelters represents a political response by voters at the ballot box rather than being an artifact of selective migration in response to said new shelter.

#### 5.1.3 Residential Sorting

Another potential concern with our result is that the observed effect on support for spending on homeless shelters could partly reflect differences in movers in and out of treated precincts, rather than changes among a stable electorate. For instance, one could imagine that new homeless services facilities lead residents who dislike these facilities or have anti-homeless

[22] Full details of this analysis are provided in Appendix Section C.5.

attitudes to move away; if they are replaced by new residents with even average attitudes towards new homeless shelter spending, this would mechanically increase the pro-shelter share of the electorate.

We address this concern by assessing whether compositional differences in in- and out-migration are large enough to plausibly account for our vote share results. For residential sorting to explain a substantial share of the result, two things must be true. First, treated precincts must exhibit meaningful differences in turnover-related composition relative to nearby controls. Second, those differences must be strongly correlated with propensity to vote yes or no on the ballot propositions.

We examine these in turn. First, using our L2 linked voter sample, we calculate the number of "stayers:" voters present in the same precinct in the 2018 and 2024 elections, and the number of "movers" both in and out of precincts. At each distance cutoff, we then compare these turnover measures between treated precincts and nearby controls by estimating a cross-sectional regression. We find positive effects on out migration across distance cutoffs: precincts near shelters have approximately a 4 percentage point higher rate of out migration than control precincts further away. We also find larger but more imprecise estimates on in-migration. Consequently, areas closer to shelters are experiencing higher turnover in the composition of their electorates.

For this to be a problem, differences in turnover must be correlated with propensity to vote yes or no on the ballot propositions. While we cannot directly address this, we conduct a sensitivity analysis under different realistic scenarios wherein leavers and stayers have different preferences about the ballot propositions (we benchmark the range of realistic gaps between leaver and stayer support as the half standard deviation and standard deviation across treated precincts, but also compute more extreme scenarios, see Appendix C.6). Figure A10 shows the results from this sensitivity analysis for treatment cutoffs at 100, 200, and 300 meters. At 100 meters, we bound the share of vote share effects explained by mobility to be realistically somewhere between 11.4% and 22.8%. At 200 and 300 meters, this range is 9.0%–18.0% and 19.8%–39.5%, respectively. Therefore, small pluralities of our effects may be driven by compositional changes in the electorate due to in- and out-migration, but the large majority of our effect is likely a political behavior response to new shelters opening. In order for the entirety of our effect to be explained by mobility, the gap between leaver and stayer support for the referendum at 100, 200, and 300 meters would have to be 32.3, 52.6, and 24.3 percentage points, respectively—highly unlikely scenarios that are not supported by the variation in the data.

A final concern related to residential sorting is that the residents of new facilities themselves are voting for the ballot propositions. However, we think this is highly unlikely. Prior studies have demonstrated that homeless individuals are extremely unlikely to participate in politics (Brown & Zoorob, 2020).[23]

#### 5.1.4 Replication Using Individual-Level Survey Responses

Finally, we conduct a conceptual replication of our main design using individual-level survey data. We take advantage of three Public Policy Institute of California (PPIC) polls of Californians, two fielded just before the March 2024 post-treatment homeless ballot proposition and one fielded in October 2025 (combined $N = 4,995$). To assign treatment to each respondent, we use a coarser geography, the Zip Code. We consider a respondent to be treated if their Zip Code received a new homeless services facility between 2018 and 2024, and control otherwise. As this is less precise than our within-neighborhood design, we attempt to mitigate the selection concerns that motivated our econometric approach through a series of controls on individual-level observables.[24] Critically, we run all models with fixed effects at the county level to absorb geographic variation, and we control for the number of homeless

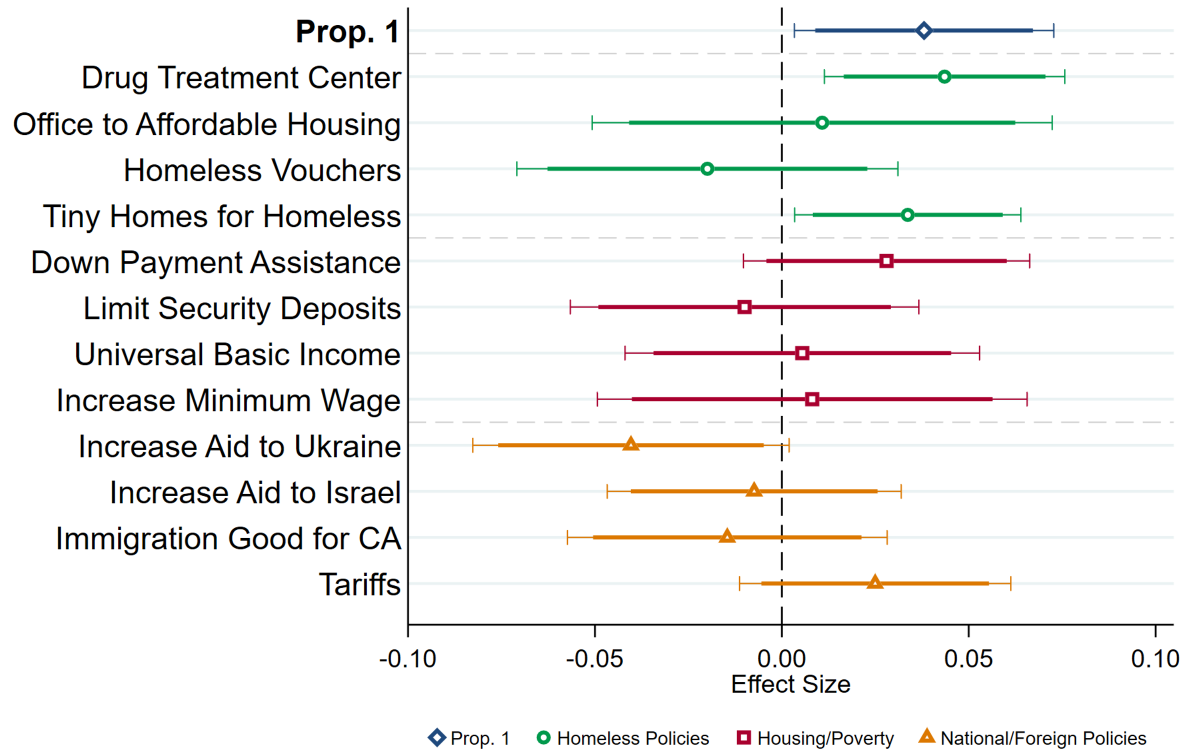
[23] Additionally, as we will report later, our replication using survey data finds that respondents in treatment Zip Codes are no more likely to report being homeless themselves (Figure 9).

[24] These are: homeownership, partisanship, race, length of residency, gender, age, self-reported likelihood of voting, and voter registration status.

beds in the pretreatment period at the Zip Code level so that the treatment captures the net effect of new or expanded homeless facilities.[25]

We evaluate if the treatment replicates on support for Prop 1, our post-treatment outcome. Respondents were asked if they would vote yes or no if the election was held today. Figure 5 plots these effects. Those in treated Zip Codes expressed support for the measure about 4 percentage points (6 percent) higher than control Zip Codes. We find similar effects for two of the other four homelessness policies asked about: "establishing addiction treatment centers in your local area" (5 p.p.) and "building developments of tiny or small homes" to "help homeless people" (3 p.p.). We find null effects on all other non-homeless housing and welfare policies, along with attitudes about national policies and foreign affairs, which we treat as placebo tests. This reassures us that our treatment is isolating the effects of new homeless facilities and not capturing preexisting differences in unobservable characteristics that predict more liberal policy tendencies. In addition to bolstering our main findings using an alternative dataset, we will use these survey data to disentangle possible mechanisms.

[25]This specification unsurprisingly produces imbalance across some observables, namely homeownership, race, and age. While we control on these observables, we also provide alternative specifications in Appendix D.6, such as one which defines the control Zip Codes as those which had pre-treatment shelters. Results are substantively similar.




**Note:** 95% CIs in caps; 90% CIs in bold.
OLS with county FE, SEs two-way clustered by county and ZIP.
Controls: homeownership, race, age, income, length of residence, likelihood to vote, ideology, education, gender, 2018 homeless beds, survey wave.

Figure 5: **Proximity to New Shelters Predicts Support for Spending on Homeless Service Facilities in Individual-Level Public Opinion Data:** plot shows the effect of Zip Code-level proximity to new shelters on a number of policies. Proposition 1, the 2024 CA ballot measure funding homeless services is shown in blue. Other homeless policies are shown in green. Housing and economic redistribution policies are shown in red. Unrelated (placebo) policies are shown in orange. Points indicate point estimates of regression controlling for homeownership, race, age, income, length of residence, vote likelihood, ideology, education, gender, and Zip Code-level pre-2018 shelters, as well as survey wave and county fixed effects. Standard errors are clustered at Zip Code and county levels. Error bars indicate 90 and 95% confidence intervals.

# 6 Mechanisms

In the previous section, we demonstrated that support for ballot propositions to fund homeless service facilities increases immediately near areas which recently were exposed to said facilities, evidence of positive policy feedback. We now turn to understanding the mechanisms driving this spatially concentrated response to new facilities. We consider distinct mechanisms channels. First, we test whether self-interest drives the positive feedback: voters observe decreases in crime and public vagrancy or increase in property values in response to new shelters opening and increase their support for further public investment. Second, we test whether exposure leads to improvements in affective attitudes towards the homeless, where increased sympathy or understanding of the homeless's plight leads to increase referenda support. Lastly, we measure whether exposure to new shelters raise the awareness of the homelessness issue, increasing the salience of the issue and driving voter support. As we detail below, we find evidence against the self-interest channel and evidence in support of the affective attitude and salience channels.

## 6.1 Self-interest

One potential reason why new homeless facilities increase nearby support is because they generate positive local externalities. These positive externalities might come in the form of a reduction in the number of tents or encampments, lower crime rates, increased property values (for homeowners) or decreased rents (for renters), or replacement of previously blighted uses. Voters tracing these positive externalities to new shelter construction could generate the positive policy feedback we observe.

As discussed previously, prior literature finds that different types of homeless services cause different localized effects. Housing-based facilities, particularly permanent supportive housing, have been repeatedly found to generate positive externalities. Shelter-based facilities, on the other hand, may sometimes cause negative externalities. This suggests a heterogeneous treatment effect by facility type.

Figure 6 shows the effect of our main differences-in-differences specification split by facility type. We see that both permanent housing and emergency shelters show similar, positive results. We find no substantive or statistically significant differences between the two types. This pattern is inconsistent with prior findings of diverging externalities by facility types. However, it is possible that, contrary to the previous literature's findings, both types of facilities produced positive localized externalities. We now turn to evaluating localized effects of new shelter openings on objective and observed neighborhood conditions.

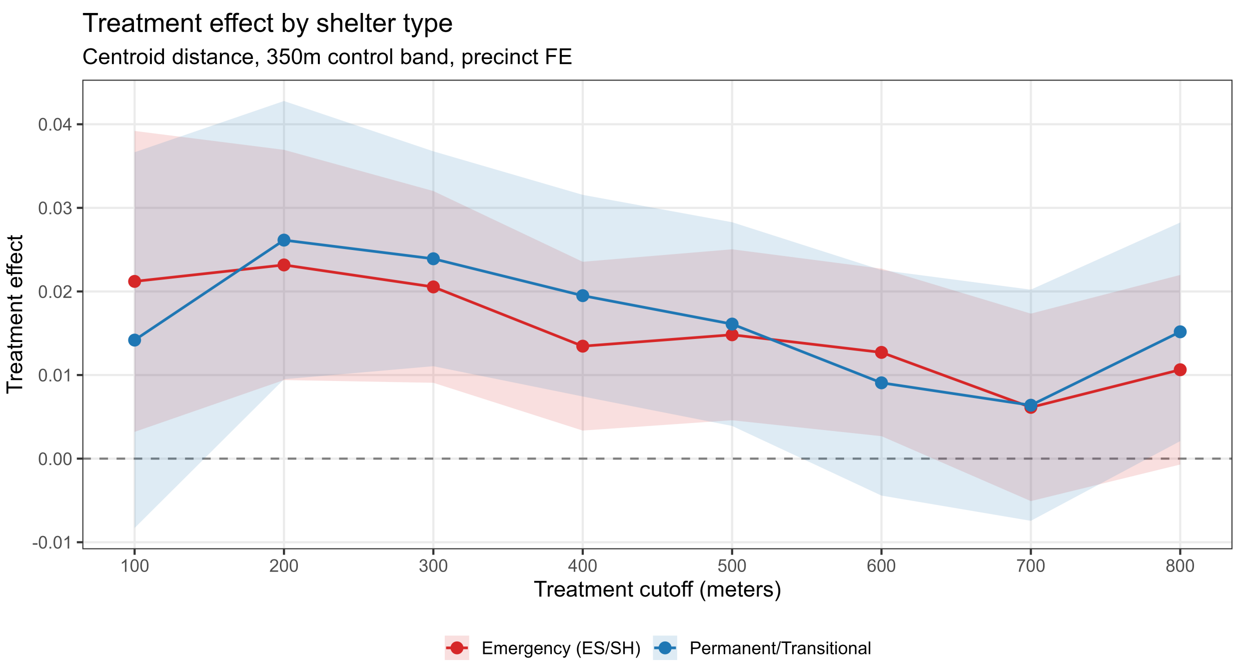



Figure 6: **No differences in new shelter effects between emergency and permanent shelters:** points represent difference-in-difference coefficients from Equation 1, estimated with increasing distances (x-axis) at which the treated group is defined. For each treatment cutoff, the control group consists of precincts between the treatment cutoff and the cutoff plus 350 meters from the new site. Bars represent 95% confidence intervals with standard errors clustered by facility. Coefficients are from separate regressions of vote share on opening of emergency and permanent shelters.

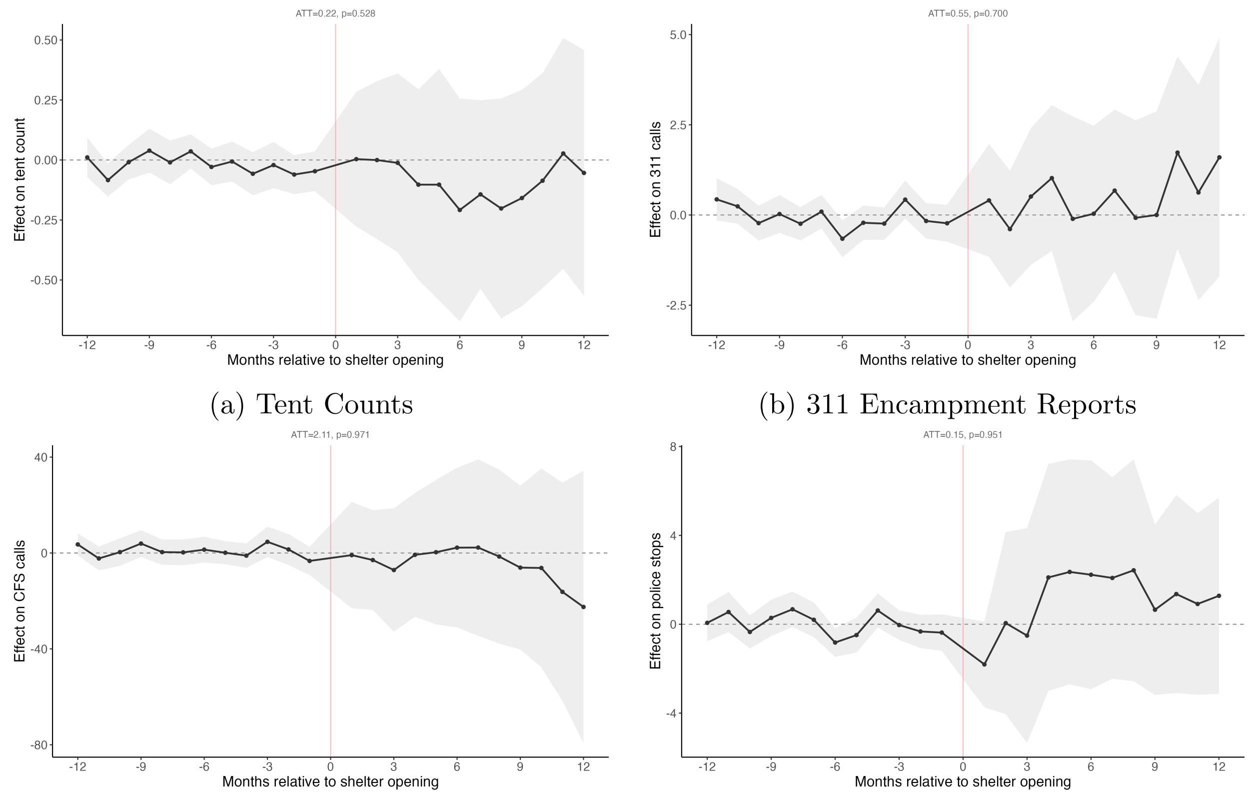
### 6.1.1 Crime and Disorder

Discussions of crime and disorder often dominate at local meetings about proposed homeless shelters (Eisenberg, 2017). Previous work both supports and opposes this view, depending on the context. We collect data on visible street homelessness, 311 reports of encampments and tents, 911 calls, and police stops.[26] Visible street homelessness, measured from street view images taken in San Francisco during the study period, is our closest measure of objective conditions. 311 and 911 calls, which are available for a wider set of cities, reflect both street conditions, residents' presence, and residents' propensity to report encampments or crime. Finally, stops are a function of both citizen reporting (through 911 calls) and officer discretion. Our second approach is to exploit the staggered monthly opening of facilities to conduct stacked event study analyses to measure the localized effects on these outcomes of interest. Finally, we take advantage of a public opinion data that ask about perceptions of crime to observe voters' perceptions of these conditions.

We use an event study design to assess the effects of new shelter openings. Around each new facility, we draw a 300 meter buffer and count the incidence of our geocoded outcomes at the monthly level. Our unit of analysis is the facility-month, allowing us to exploit variation in opening dates across shelters. We estimate dynamic treatment effects using the interactive fixed effects (IFE) counterfactual estimator implemented in the `fect` package (Liu et al., 2024). This approach constructs counterfactual outcomes for treated units using a factor model that captures unobserved time-varying confounders through latent factors and unit-specific factor loadings, selecting the number of factors via cross-validation. The IFE estimator accommodates the staggered adoption and treatment reversal that we observe in our data.

Figure 7 plots the dynamic treatment effects on each outcome 12 months before and after facility opening. Across all outcomes, we find null effects. This pattern holds for both

[26] See Appendix B.2 for details about data sources and coverage.




(a) Tent Counts

(b) 311 Encampment Reports

(c) 911 Calls for Service

(d) Police Stops

Figure 7: **New shelter openings do not affect neighborhood disorder and crime:** plot shows dynamic treatment effects of 2018-2024 facility openings on counts of outcomes within 300m. The vertical line marks the facility opening date. Shaded areas represent 95% confidence intervals. Panel (a) shows effects on visible tent encampments in San Francisco; panel (b) shows effects on 311 reports of encampments in Los Angeles, San Francisco, San Diego, Oakland, San Jose, Pasadena, and Sacramento; panel (c) shows effects on all 911 calls for service in San Francisco, San Jose, and Sacramento; panel (d) shows effects on all police stops in San Francisco, San Diego, Oakland, and Long Beach.

shelter (ATT = 0.25, $p = 0.34$) and housing facilities (ATT = 0.01, $p = 0.97$), contrary to prior findings about different facility types leading to diverging localized externalities.

While we find no evidence that new facilities decrease nearby calls for service or police stops, perceptions of crime may matter more than objective reality. To examine these perceptions, we take advantage of a 2025 PPIC poll asking residents if crime in their "local community" has increased, decreased, or stayed the same. We find no effects of the treatment on an increased perception of both violent and property crime (Figure 9).[27]. Taken together with our findings about objective rates of crime, we find the balance of evidence does not support the notion that positive policy feedback is driven by the externalities of new facilities decreasing crime or perceptions of disorder.

### 6.1.2 Improvements over Prior Conditions

Another explanation for the positive feedback effects of shelters is that new facilities improve the built environment. This mechanism is posited as a driver behind the positive effects of new affordable housing projects, which often replaced blighted uses such as vacant lots, parking lots, or abandoned commercial facilities (Hankinson et al., 2026).

We test this mechanism systematically with data on the preexisting land-use of the new facilities that we observe. We collect data from multiple sources, including parcel-level land-use data for most of the Bay Area and nearly all of Southern California (See Appendix B for details). Using lot-level information about prior land-uses, we code homeless service facilities as previously residential (including mixed use), commercial (including hotels and offices), or industrial (including vacant land and parking lots). We believe that facilities which replace the latter two categories likely represent the most substantial improvements over prior conditions.

[27] If anything, when measuring the treatment on the intensive margin, high exposure Zip Codes (above median new beds) report *higher* perceptions of increased crime compared to low exposure (below median new beds) Zip Codes

Figure 8 plots these effects. While the effects are less precise due to limited sample size, the general pattern identified in the main analysis replicates across all prior land-uses. While replacements of prior commercial or industrial uses show slightly larger effects, these cannot be distinguished from the effects of prior residential uses. Together, we interpret these findings as inconsistent with the notion that improvements over prior land-use conditions are overwhelmingly driving the positive effects.

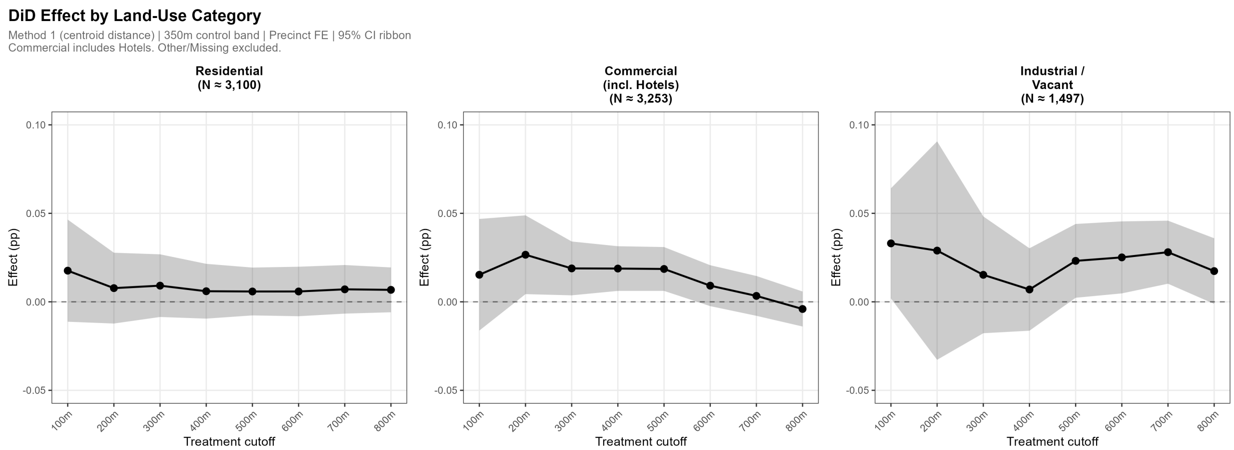



Figure 8: **Effects of new shelters are strongest when they replace former commercial and industrial sites:** points represent difference-in-differences coefficients from Equation 1, estimated with increasing distances (x-axis) at which the treated group is defined by the land-use of the homeless facility parcel in the pretreatment period. For each treatment cutoff, the control group consists of precincts between the treatment cutoff and the cutoff plus 350 meters from the new site. The shaded area represents 95% confidence intervals with standard errors clustered at the facility level.

### 6.1.3 Property Values and Rents

Finally, new homeless services facilities may have localized spillovers on nearby property values or rents. Supportive housing facilities in particular have been repeatedly shown to increase nearby property values. While we did not find heterogeneous treatment effects by facility type, it is possible that new facilities could have heterogeneous effects by homeownership. Again, Hankinson et al. (2026) cite this as a mechanism explaining their findings of positive policy feedback from new low-income affordable housing. They show that new affordable apartments increase property values, generating positive effects in predominately homeowner neighborhoods, and small negative effects in predominately renter neighborhoods (as higher property values likely corresponds to higher rents).

To test if this mechanism applies in the case of new homeless services, we decompose the effect of facility type based on whether the facility is in a predominately renter or homeowner neighborhood. Appendix Table A14 details the results. We find positive and precisely estimated treatment effects in predominately renter neighborhoods. The effects in predominately homeowner neighborhoods are also positive, but imprecisely estimated. We do not find evidence that effects substantially differ by homeownership. We view this as inconsistent with a financial self-interest channel driving the main results. If new facilities increased nearby property values, and these impacts indeed motivated homeowners' positive policy feedback, we should expect larger effects in homeowner-dominated neighborhoods, and smaller or even negative effects in renter neighborhoods, but this is not what we find.

## 6.2 Exposure and Affective Attitudes

So far, we have found little evidence to support the notion that new homeless services facilities cause tangible, localized externalities (positive or negative) that may be visible to voters, potentially activating self-interested concerns. We find no evidence that new facilities impact crime, disorder (measured by the presence of encampments and related 311 service calls), prior land-uses, or differential impacts by homeownership.

This leaves open the possibility that new homeless service facilities instead activate or even alter affective attitudes about the homeless. As detailed earlier, some studies highlight how negative stereotypes and attitudes of disgust may reduce support for homeless remedies. Other studies, on the other hand, theorize that exposure to homelessness may reinforce positive attitudes, including belief in structural causes and sympathy, leading to increased support for homeless remedies.

For affective attitudes to operate, a precondition is that nearby residents should be aware of the new facilities and their inhabitants. One problem, however, is that it is not clear whether new facilities necessarily increase or decrease casual exposure to homeless people. On one hand, the new capacity created by these facilities may reduce unsheltered homelessness on the street, causing a net reduction in exposure to homeless people. On the other hand, new facilities may relocate those previously living in less visible areas (e.g. highway underpasses, remote sections of parks) into residential neighborhoods. The new facilities may similarly increase exposure to homeless people as they move in and out of facilities throughout the day. Finally, the presence of the facility itself may serve as a symbolic reminder of homelessness, activating affective attitudes about its residents regardless of whether nearby neighbors observe or interact with them.

Earlier, we showed that new facilities caused no change in the volume of 311 service calls nearby. However, this may not necessarily mean that new facilities go unnoticed. 311 calls represent a bundle of demand for government services to fix a problem and the individual-level attributes that predict participation (White & Trump, 2018). For example, homeowners are more likely to call 311 (Minkoff, 2016), and calls are sometimes driven by a small subset of "superusers." This complicates the use of 311 calls as a direct proof of exposure to homelessness, as individual-level attributes may bias 311 reporting (Kontokosta & Hong, 2021).

To better understand whether new facilities change exposure to homelessness, we turn to self-reported measures. We use PPIC polling data, which asked respondents about seeing and interacting with homeless people (daily, every few days, weekly, seldom, or never). Figure 9 plots the relationship between respondents in Zip Codes with new shelters and these self-reports: respondents in treated Zip Codes were 6 percentage points more likely to report seeing someone experiencing homelessness weekly or more frequently. The same is true for self-reported interactions with homeless people. Respondents are no more or less likely to report being homeless themselves or knowing a close family member who is homeless. We view this as a manipulation check that voters in areas nearby new shelters are indeed being treated by the new facilities in the form of higher exposure to homeless people.

Next, we ask if nearby residents change their affect toward homelessness. While we do not have direct measures of such attitudes, we test the theoretical predictions of theories of positive and negative affect and take advantage of indirect measures.

First, does increased exposure to homeless people increase *negative* attitudes towards homeless people? One prediction of this view is that repeated exposure to stigmatized populations causes emotional fatigue, lowering compassion (Cameron et al., 2016). If this were the case, residents in places which already had high rates of exposure to homeless people may experience "compassion fatigue," causing backlash and negative policy feedback to new facilities. Therefore, we examine heterogeneity by preexisting exposure to homelessness (the existence of a homeless service facility in 2017 and existing 311 calls). Appendix Figure A15 plots the effects. We find that the treatment effect is being driven by places with higher preexisting exposure to homelessness. Precincts where the facility is novel or had low baseline 311 results, on the other hand, show null effects. These results contradict studies that argue that repeated exposure to homeless people can lead to 'compassion fatigue,' and we view this as suggestive evidence that new facilities are not reinforcing negative attitudes about the homeless.

Does increased exposure to homeless people instead reinforce *positive* attitudes? Prior studies posit a relationship between exposure and belief that homelessness is driven by structural, rather than individual, causes. Belief in structural causes predicts pro-social behavior such as donating money to charity for the homeless (Lee et al., 2004). Our public opinion data has a series of questions on the beliefs of the major factors contributing to homelessness in a respondent's local community. We classify three as structural (loss of income, unaffordable housing, and lack of mental health services) and one as individual (substance abuse).

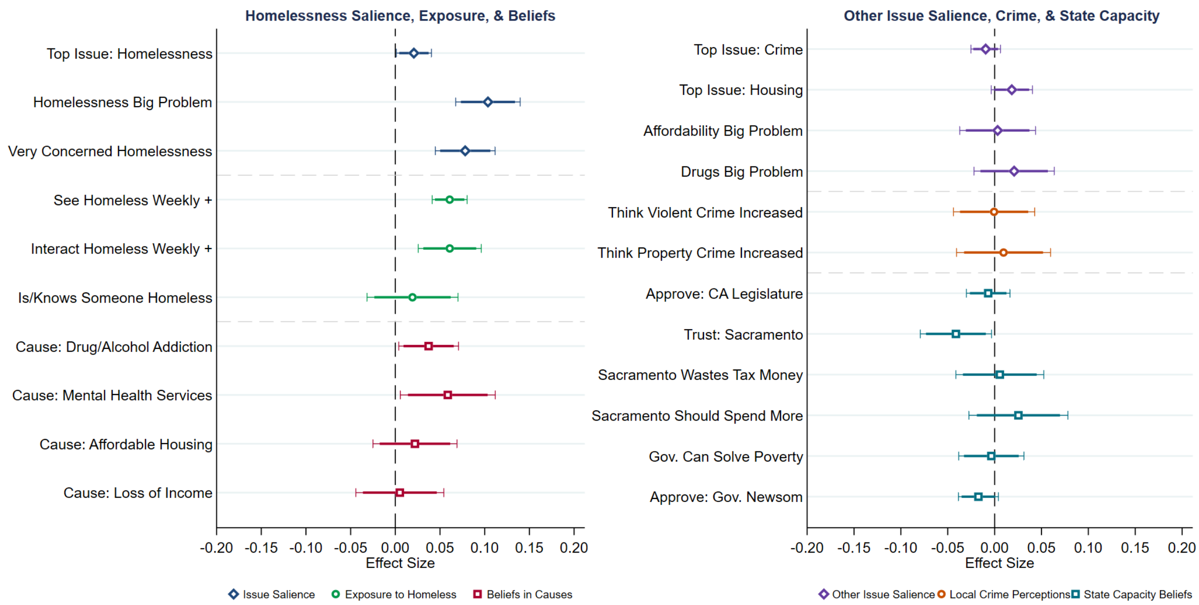
Figure 9 plots the effect of the treatment on these beliefs. Proximity to new shelters is associated with increased belief in two causes: treated respondents were about 6 percentage points more likely to cite a lack of mental health services as a major cause of homelessness in their community, and 4 percentage points more likely to cite substance abuse. However, respondents were not more likely to cite housing affordability or loss of income as a cause. On balance, these results are inconclusive about whether positive attitudes are activated, as attribution to both structural and individual causes appears to increase. However, we note that unlike prior studies which find positive effects of self-reported exposure only on structural causes, we find it strongly predicts attribution of *all* causes (See Appendix Table A15). Additionally, as we will discuss later, we find that belief in substance abuse being a major cause has only a small negative effect on policy support. The large positive effect of belief in a lack of mental health services more than offsets this negative effect. Together, we view this evidence as generally inconsistent with activation of negative affect, and suggestive that increased exposure may strengthen positive attitudes.[28]

## 6.3 Issue Salience and Beliefs in State Capacity

A further set of explanations is that the opening of new homeless facilities makes the issue of homelessness more salient to voters. This could have several potential effects. First, it could raise the level of concern over homelessness, further raising willingness to spend on proposed solutions. Second, it could raise the belief that the state is capable of addressing the problem, which, even holding concern over homelessness constant, would raise support for future spending on policy remedies. Finally, voters could see the provision of new homeless service facilities as a kept promise by politicians, and they feel more positively towards continuation of the program.

We return to the public opinion data to evaluate evidence consistent with these

[28] Appendix Figure A14 plots the effects by the population type of the residents in the homeless services facility. We split single adults from facilities which primarily serve families with children or both population types.




**Note:** 95% CIs in caps; 90% CIs in bold.
OLS with county FE, SEs two-way clustered by county and ZIP.
Controls: homeownership, race, age, income, length of residence, likelihood to vote, ideology, education, gender, 2018 homeless beds, survey wave.

Figure 9: **Respondents in Zip Codes with new shelter openings more likely to report exposure to homeless people and see homelessness as a problem:** plot shows the effect of Zip Code-level proximity to new shelters on evaluation of problems, exposure to homeless people, beliefs about causes of homelessness, and other issues. Points indicate point estimates of regression controlling for homeownership, race, age, income, length of residence, vote likelihood, ideology, education, gender, and Zip Code-level pre-2018 shelters, as well as survey wave and county fixed effects. Standard errors are clustered at Zip Code. Error bars indicate 90 and 95% confidence intervals.

mechanisms. First, we present evidence in Figure 9 that respondents in Zip Codes with new homeless services facilities opened are also more likely to believe that homelessness is a salient issue. Specifically, they are 2 percentage points more likely to say homelessness is the number one issue facing the state. While small in absolute terms, this represents a 15 percent increase over the baseline rate of picking homelessness as the top issue, as respondents have 39 issues to select from. As alternative but less precise measures of issue salience, we also find that respondents were 10 percentage points more likely to say homelessness is a "big problem" and 8 percentage points more likely to say they are "very concerned" about the "presence of homeless people" in their local community.

This high issue salience is also predictive of support for policy remedies (See Appendix Table A16). When predicting support for Prop 1, ranking homelessness as the number one issue produces large effects (an 11 percentage point increase, about half the effect of the largest predictor, liberal ideology). These effects are nearly identical when sub-setting to just respondents in control Zip Codes (to avoid post-treatment bias). Attribution of a lack of mental health services is another strong predictor of support for the ballot (a 22 percentage point increase, about the size of liberal ideology). Other attitudes are less consistent predictors or not impacted by the treatment.

Additionally, the right-hand panel of Figure 9 tests alternative mechanisms. We find no effects of new homeless services facilities increasing the salience of other issues, such as housing. Similarly, we find no increased salience of crime nor an increased perception of crime in a respondent's local community. Finally, we find no consistent effects on proxies for beliefs in state capacity. The treatment does not appear to increase trust in state actors, or move beliefs about capacity, such as belief that "government policies and programs can ... reduce poverty in California."

Viewed together, we interpret these results as supportive of the view that new homeless services facilities may increase exposure to homeless people and increase the salience of homelessness as an issue that needs addressing. Given that most citizens have, at baseline, warm feelings about the homeless and believe in solutions such as shelter and supportive housing, higher issue salience should increase support for homeless remedies.[29] Finally, while we do not have conclusive evidence that new facilities reinforce positive attitudes, we do find some evidence of increased attribution to a lack of mental health services causing homelessness. Together, the powerful effects of salience and belief in this structural cause may jointly drive the positive policy feedback we observe.

# 7 Conclusion

This study tests the effects of new homeless service facilities on support for public investment in policy solutions to address homelessness. We address issues of selection that afflict prior studies using a within-neighborhood differences-in-differences design that measures changes in support for two ballot initiatives in 2018 and 2024 where voters chose whether to authorize bonds for construction of homeless service facilities. In doing so, we identify a positive causal effect, evidence of positive policy feedback. We show these positive results are robust to a number of alternative specifications, including on a subset of plausibly exogenous hotel conversions.

In addition to isolating this effect, this paper advances our understanding of the mechanisms driving public response to homelessness. By enriching our analysis with many datasets, we find little evidence that new facilities are producing positive localized externalities, which could activate self-interested attitudes. Rather, new facilities appear to increase exposure to homeless people, suggesting that the facilities may activate positive affective attitudes about the homeless. Additionally, the facilities themselves may increase the salience of the issue, one which many people want government to do something about.

These findings inform our understanding of voter psychology as it relates to exposure

[29]Indeed, we find some evidence for this moderating effect.

to homelessness, inequality, and related services, and further how these features interact with geography to influence voter behavior. Voters will respond to effective policies when issues are most salient, or directly impacting their day-to-day lives. The voter living on the same street, a block or two from a new facility is reminded daily of the policy response to homelessness by the visible presence of the facility itself – even though, as our externalities analysis suggests, observable street conditions do not measurably improve. The voter living even 100 meters further down the street may have a wholly different experience, and the effects of the new shelter are less visible, if present at all. Social geography thus shapes political behavior in part through structuring the daily elements to which voters are exposed.

Collectively, these results demonstrate the conditions under which homeless shelter services produce positive policy feedback dynamics. Rather than produce an electoral backlash, shelter services increase support for further investment in policy solutions to homelessness – and this is true for both permanent housing and emergency shelters. As homelessness continues to increase across American cities, the demand for these policy solutions will only increase, and the affected populations amenable to a positive policy feedback response will further grow. Policymakers concerned about electoral backlash from siting homeless facilities can take confidence that visible policy action builds support rather than eroding it.

While these results show that providing homeless services can generate long-term positive policy feedback, California is a unique case in many ways, potentially limiting generalizability. In 2024, the state ranked 6th highest in the per capita rate of homelessness. While places such as New York State or Washington D.C. have higher rates, California has a uniquely high rate of unsheltered homelessness. In 2024, 66% of homeless people in California were unsheltered, the highest rate in the nation (U.S. Department of Housing and Urban Development, 2024a). As we find that areas with high baseline rates of unsheltered homelessness show stronger effects, it's possible that positive policy feedback may not generalize to places with lower rates. Indeed, there are anecdotal reports of backlash to newly sited facilities in places with low rates of unsheltered homelessness. In New York City, the conversion of the Lucerne Hotel in the affluent Upper West Side neighborhood was met with protest and intense community backlash that eventually forced the facility to close (Stewart, 2020). Brown & Zoorob (2022) similarly cite reporting that the relocation of homeless facilities in Boston led to concessions by the Mayoral administration after community backlash, including increased police presence or nearby beautification efforts. However, both cities are unique in their own way by providing the Right to Shelter, meaning that no homeless person may be turned away from shelter, and consequently, unsheltered homelessness rates are among the lowest in the nation (routinely below 5%). In the few places with such a Right, the political economy of homeless services siting may differ, as new facilities represent potentially novel introductions of a stigmatized group while doing little to solve what is otherwise a much less widespread problem of street homelessness. This may help to explain why our results differ from the Boston field experiment by Sands (2017). Still, many places in the United States have severe and growing unsheltered homelessness crises, including states such as Alabama, Florida, Oregon, or Washington. Even in colder cities with more robust emergency shelter systems, some neighborhoods have highly concentrated unsheltered homelessness, such as Philadelphia's Kensington (City of Philadelphia Office of Homeless Services, 2024). We believe it is likely our results generalize to such cases, but also emphasize that we found no evidence of backlash in places within California that had lower pre-existing rates of homelessness.

Finally, our study also suggests that public opinion may be less of a roadblock to policies that address homelessness than is commonly thought. Again, we find no evidence of long-term backlash to homeless services siting. In fact, many local politicians appear to be more than willing to engage in credit claiming over these new facilities, suggesting they expect electoral benefits. This corroborates prior findings that many residents are willing to pass and pay for policies that build new homeless shelters and supportive housing, even in their neighborhoods. To be sure, politicians perceive the initial siting of these facilities as controversial. In a survey of Mayors, Einstein & Willison (2025) find that 63% list "public opposition to new housing and shelters" as a formidable barrier to addressing homelessness. However, once built, our findings suggest politicians know they stand to gain from credit claiming. Rather, our results suggest that federalist fiscal constraints may be the major culprit behind lagging local policy responses to homelessness (Peterson, 1981). By far the biggest concern of mayors is cost – with 79% of mayors listing this as a barrier to addressing homelessness. Municipal budgets reflect this concern. For example, operating New York City's shelter system cost $4.3 billion in fiscal year 2025, an underestimate of total city spending on homelessness response (New York City Office of Management and Budget, 2026).[30] In Los Angeles, analysis suggests an additional $15 billion over 10 years is needed to address homelessness at current levels (Los Angeles Housing Department, 2024). Given the hard constraints placed on the local governments tasked with addressing homelessness, the competing considerations of fiscal stewardship vis-a-vis public opinion may help explain why homelessness remains a pervasive issue despite public support for policies to address it (also see Hankinson & Pietrzak (n.d.)).

[30]This figure is an underestimate as it does not include: capital spending on new supportive housing nor operating spending on domestic violence shelters, HIV/AIDS shelters, police response, or public hospitalization costs, among others. See: Alexander (2017).

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# A Data: Identifying Homeless Services Facilities

To identify homeless services facilities, we use data from two sources. The first is the U.S. Department of Housing and Urban Development (HUD) Housing Inventory Count (HIC). We use the 2024 and 2019 files, which are current as of January of those years, the closest dates to the November 2018 and March 2024 elections. HIC provides addresses for nearly all homeless service facilities that participate in the federal Homeless Management Information System (HMIS), required as a condition for receiving federal money from HUD.[31] We subset this data only to site-based facilities, those operated out of (typically) one physical address.[32] After cleaning the address strings, we geocode these addresses using the ArcGIS geocoder. While HUD typically de-duplicates projects at the address level, such that one facility that serves multiple programs or populations are not disaggregated, inconsistent address information typically in the form of multiple address aliases often leads to slightly different geocoding for each project. We de-duplicate projects, defining projects as the same facility if they are within a 25-meter radius of each other. We preserve information about all facilities de-duplicated in this process, aggregating the number of beds and identifying each of the population(s) served. Then, we identify treatment facilities – those opened after November 2018 – by identifying the subset of facilities that appear in the 2024 data but did not appear in the 2019 data.[33]

The second source is the California Homeless Data Integration System (HDIS), obtained via request from the California Interagency Council on Homelessness (Cal ICH). We were provided the 2018 through 2023 files. We use this data because it includes data not available at the federal level through HUD. Specifically, facilities that only rely on state-

[31] Addresses for sensitive populations, specifically victims of domestic violence and HIV/AIDS facilities, are not typically available to protect the privacy of clients.

[32] A smaller subset of facilities are site-based cluster facilities, meaning that services may be provided at multiple physical locations. We remove these facilities in robustness checks.

[33] We also have information about the “inventory start date.” It is unclear how these start dates are determined. For example, it is unclear if a facility which was used in the past but since closed and has been re-opened would be coded. We flag all facilities with start dates prior to November 2018 and exclude them in robustness checks. Our de-duplication method leads to a very small number of facilities with such a flag.

and-local funding sources, facilities that serve formerly homeless populations, and facilities that serve populations at very high risk of homelessness. The first two types represent a large number of facilities in California, as state-and-local ballot propositions have funded many new facilities that do not rely on federal money. A representative example of such projects would be supportive housing facilities that dedicate certain percentages of units for people currently in the shelter system. HDIS does not include address information about each project. As a result, we use a different method to de-duplicate and geocode these facilities.

To de-duplicate HDIS facilities, we clean the project name provided, and consider a project to be the same if it shares the same name, Zip Code, Continuum of Care (CoC) name, project-type (emergency shelters, supportive housing, safe havens, and rapid re-housing), and housing type (site-based single-sites, site-based cluster-sites, and tenant-based scatter-sites). Cal ICH typically does not de-duplicate projects at the address level, so most facilities that serve multiple programs or populations are disaggregated. This method resolves most of these duplications. We preserve information about all facilities de-duplicated in this process, aggregating the number of beds and identifying each of the population(s) served. Then, we identify treatment facilities – those opened after November 2018 – by identifying the subset of facilities that appear in the 2023 data but did not appear in the 2019 data.[34]

To geocode HDIS facilities, we attempt to find the address of all de-duplicated site-based projects in the state by triangulating several fields – project name, CoC name, number of beds, project type, and Zip Code. We use multiple sources, including homeless service providers (pamphlets, help finding websites), reporting on homeless facilities in the press, city services databases, or Google reviews. This process also helped identify additional duplicate entries at the same address. We found an address for 1,427 new site-

[34]At the time of request, only the 2023 data was available. It is possible that some facilities have closed between 2023 and 2024. However, this error should, if anything, attenuate our main treatment effects towards zero, as we are considering areas treated which should be considered control.

Table A1: Geocoding Coverage of Cal ICH HDIS New Site-Based Projects, by Type

| **Project Type** | **Not Found** | **Geocoded** | **Total** |
|---|---|---|---|
| Emergency Shelter | 39 | 388 | 427 |
| | *9%* | *91%* | *100%* |
| Supportive Housing | 89 | 809 | 898 |
| | *10%* | *90%* | *100%* |
| Rapid Re-Housing | 102 | 24 | 126 |
| | *81%* | *19%* | *100%* |
| Safe Haven | 1 | 16 | 17 |
| | *6%* | *94%* | *100%* |
| Transitional Housing | 233 | 190 | 423 |
| | *55%* | *45%* | *100%* |
| **Total** | **464** | **1,427** | **1,891** |
| | *25%* | *75%* | *100%* |

based projects of a possible 1,891 (75%). Our coverage is higher for emergency shelters, safe havens, and supportive housing sites (See Appendix Table A1). After cleaning the address strings, we geocode these addresses using the ArcGIS geocoder. To reflect uncertainty in our address collection process, our coders identified if their confidence in the site location was high or low, and we include it in some models as a robustness check.

Next, we combine the two data sources. We replicate the method used for the HUD data, defining projects as the same facility if they are within a 25-meter radius of each other. Where possible, we preserve information about all facilities de-duplicated in this process, aggregating the number of beds and identifying each of the population(s) served.[35] Our final dataset of treated facilities contains 2,129 unique addresses. Of these, 934 appear only in the HUD HIC, 876 appear only in the Cal ICH HDIS, and 319 appeared in both. Of the facilities unique to each dataset, HIC represents more emergency shelters (47% of all HUD HIC-unique facilities), whereas HDIS represents more permanent supportive housing (51% of all Cal ICH HDIS-unique facilities).

Finally, we trim our dataset to drop potentially erroneous facilities. Appendix Ta-

[35] We note, however, that not all HDIS facilities had complete information about the populations served at each facility. Therefore, analyses disaggregated by population served will have a smaller N.

ble A2 documents these exclusions. All results presented hold regardless of the exclusion strategy (available upon request). The majority of dropped facilities are due to administrative errors in the reported opening date of the facility.

Table A2: Treatment shelter sample: exclusion waterfall

| Stage | Sites removed | Remaining |
|---|---|---|
| Raw HIC new-site inventory (deduplicated) | — | 1,937 |
| Drop tenant-based housing | 115 | 1,822 |
| Drop rapid re-housing (RRH) | 35 | 1,787 |
| Drop sites with unusable geocodes | 2 | 1,785 |
| Drop sites with unverifiable opening dates | 7 | 1,778 |
| Reclassify as pre-existing (opened before Nov. 2018) | 182 | 1,596 |
| Drop post-window sites (opened after Apr. 2024) | 55 | 1,541 |
| **Final treatment sample** | | **1,541** |

Starting sample is HUD's annual Housing Inventory Count (HIC) of new California facilities, deduplicated to first appearance per site. Tenant-based and rapid-re-housing programs are dropped because they have no fixed facility location. Geocode and date exclusions follow hand verification against primary sources. Sites opening before November 2018 are reclassified as pre-existing and used in placebo tests rather than as treatment; sites opening after April 2024 have no post-period election and are dropped. Of the 1,541 treatment sites, 1,538 carry usable coordinates and so define precinct-to-shelter distances.

# B Data

## B.1 Voter File Data

We measure individual-level turnout in our analyses using statewide voter registration lists. These data consist of publicly available records that are collected from the state. We acquired these data from the vendor L2. These data list every registered voter in California for each year from 2014 to 2025. The data include information on voters' names, dates of birth, residential addresses, gender, partisan registration, and whether or not they voted in general, primary, and municipal elections. The data also include voter race, which is imputed by the vendor using information on voter location and name (Imai & Khanna, 2016).

With this information, we define an analysis sample of voters by first limiting our sample to voters living at the same residential address in 2018 as in 2024. We then measure, in 2018 and 2024, how close voters live to shelters that appear between 2018 and 2024. As with precincts, we do so by measuring the geodesic distance in meters from each voter's residential address to the address of the closest new shelter.

## B.2 Data on Shelter Externalities

Our externalities analysis draws on four measures of street conditions and the public response to them: tent counts from street-level imagery, 311 reports of encampments, 911 calls for service, and police stops. The cities covered by each measure are dictated by data availability, so coverage differs across outcomes; we describe each source in turn below. Throughout, treatment is defined by the staggered monthly openings of homeless service facilities between 2018 and 2024.

### B.2.1 Street-Level Imagery

For San Francisco, we obtain predicted tent counts from Jung et al. (2025), who develop a computer vision pipeline to measure visible homelessness from street-level imagery. Their approach collects geotagged photographs from Mapillary, a crowdsourced platform, and applies GroundingDINO—a zero-shot object detection model—to identify tents in each image. They then generate daily predictions on a grid of 100m cells covering San Francisco using these detections paired with 311 reports and features of the built environment, such as overpasses. This provides a more objective measure of street conditions than administrative complaint data, which requires human reporting.

### B.2.2 311 Reports

Many California cities allow residents to report street homelessness in public rights-of-way through their 311 systems. We obtain encampment-related incident reports for seven of the largest cities in the state: Los Angeles, San Francisco, San Diego, Oakland, San

Jose, Pasadena, and Sacramento. For each city we collect both the homelessness-specific requests and the full volume of 311 requests, which we use to construct denominators. Each city exposes encampment requests through its own category field; Table A3 lists the exact field and value we retain for each. Most of these series span the full study period; the exceptions are San Jose and Sacramento, whose 311 encampment data begin only in mid-2021 and 2020, respectively, when those systems came online.

Table A3: 311 encampment request subsets, by city

| City | Category field | Retained value |
|---|---|---|
| Los Angeles | `requesttype` | "Homeless Encampment" |
| San Francisco | `service_name` | "Encampments" |
| San Diego | `service_name` | "Encampment" |
| Oakland | `reqcategory` | "HOMELESS EMT" |
| San Jose | `Category` | "Homeless concerns" |
| Pasadena | `MAJOR_CATEGORY` | "UNHOUSED" |
| Sacramento | `CategoryLevel1` | "Homeless Camp" |

#### B.2.3 911 Calls for Service

We supplement these measures with the public's calls to police dispatch, collecting all 911 calls for service for San Francisco, San Jose, and Sacramento. Unlike the 311 series, which we restrict to encampment-specific requests, the calls-for-service measure counts the full volume of calls near each shelter: it is a broad gauge of the demand residents place on emergency response rather than a homelessness-specific complaint. Calls for service capture residents' propensity to summon a response to perceived street conditions, and so reflect both the underlying conditions and residents' willingness to report them. San Jose additionally records whether the call concerned an unhoused individual, which we use in a supplementary analysis of homelessness-flagged calls.

### B.2.4 Police Stops

Finally, we measure police stops, which are a function of both citizen reporting and officer discretion. We obtain geocoded stop records—reported at the address or block level rather than the beat level—for San Francisco, San Diego, Oakland, and Long Beach, and count all stops within each shelter radius. Where the agency records the officer's perception of whether the stopped individual was unhoused, we separately examine stops of perceived homeless persons as a supplementary analysis. Only San Francisco and Long Beach populate such a field in usable form: San Francisco's `is_person_unhoused` (which we treat as flagged when it reads TRUE, Y, YES, or 1) and Long Beach's `perceived_unhoused` (flagged when YES). San Diego's records nominally contain a `perceivedunhoused` field but leave it unpopulated, and Oakland records none, so neither contributes to the unhoused-stop analysis. Both usable fields reflect the officer's perception rather than a verified housing status.

### B.2.5 Aggregation

For each outcome we draw a 300m radius around the site of every new shelter that opens during the study period in the cities where the measure is available, in order to capture the localized effect of its opening. We aggregate each measure to the shelter-by-month level. For the tent detections we take the median of the daily predicted counts within each month; for 311 reports, calls for service, and police stops we sum the number of records falling within the shelter radius in each month.

## B.3 Public Opinion Data: PPIC Surveys

To probe the mechanisms behind our voting results, we draw on statewide opinion surveys conducted by the Public Policy Institute of California (PPIC), which periodically field questions on homelessness, exposure to it, and support for shelters and related interventions. Although PPIC asked comparable items as far back as its November 2019 wave, that wave did not record respondents' ZIP codes, which precludes linking respondents to local shelter treatment and rules out a pre/post comparison. We therefore restrict attention to three post-treatment waves that do geocode respondents to ZIP codes: a December 2023 survey ($N$ = 1,660) and February 2024 survey ($N$ = 1,628) fielded just before the March 2024 Proposition 1 vote and an October 2025 survey ($N$ = 1,707). PPIC uses a high quality probability-based online panel (Ipsos KnowledgePanel), primarily recruited via address-based sampling (Baldassare & PPIC Statewide Survey Staff, 2023).

We link each respondent to the shelter-treatment status of their ZIP code using the same treatment definition as our main analysis: a ZIP is treated if a new facility opened between January 2019 and January 2024. To capture the intensive margin, we also sum the new beds added in each ZIP over the period and split treated ZIPs at the median—90 new beds—into lower- and higher-intensity groups. Among the 1,660 December 2023 respondents, 677 fall in untreated ZIPs, 479 in lower-intensity treated ZIPs, and 504 in higher-intensity treated ZIPs.

Treated and untreated ZIPs differ systematically: treated ZIPs have markedly fewer homeowners, fewer white residents, and younger populations. These characteristics are strong predictors of attitudes toward housing. We therefore report estimates from a fully-saturated model that controls on individual-level observable characteristics available to us: homeowership, race (White, Black, Latino, Asian, and other/mixed), income (under \$20,000, \$20,000 to under \$40,000, \$40,000 to under \$60,000, \$60,000 to under \$80,000, \$80,000 to under \$100,000, \$100,000 to under \$200,000, and \$200,000 or more), age (below 45, 45 or older), length of residence (less than 10 years, more than 10 years), ideology (liberal, not liberal), education (less than 4-year college degree, 4-year college degree or more), gender (male, female or other), self-reported likelihood of voting (likely, unlikely), and self-reported voter registration status (registered, not registered or do not know).

The surveys support several families of outcomes used in our mechanism tests: stated support for Proposition 1 and for other homelessness interventions (such as local addiction-treatment centers, converting offices to housing, and building tiny homes); the salience of and concern about homelessness (whether it is the state's most important problem, whether it is a "big problem" locally, and degree of personal concern with the presence of homeless people in a respondents community); self-reported exposure (how often respondents see and interact with homeless people); attribution of its causes (housing affordability, loss of income, substance abuse, and lack of mental-health services); and proxies for confidence in state government (gubernatorial and legislative approval, the state's direction, trust in Sacramento, and beliefs about government waste). Two retrospective items in the October 2025 wave—perceived changes in local homelessness and in crime over the prior twelve months—reference a window that falls after the treatment period, so we read them as merely suggestive.

## B.4 Land-Use Data

To identify pretreatment parcel-level land-uses, we draw on three sources. The first is the SFEI Enhanced MTC 2020 Land Use database. We download this dataset from the San Francisco Estuary Institute. This dataset includes a 2020 land use layer which categorizes uses for each parcel in: Alameda, Contra Costa, Marin, Napa, San Francisco, San Mateo, Santa Clara, Santa Cruz, Sonoma, and Solano counties. The second is the Southern California Association of Governments (SCAG) 2019 Annual Land Use database. This dataset includes a 2019 land use layer which categorizes uses for each parcel in: Orange, Imperial, Los Angeles, Riverside, San Bernardino, and Ventura counties. Finally, the third dataset is the 2019 San Diego County Land Use Map database, obtained via request from the city GIS office (SanGIS).

Based on the detailed land-use descriptions in each of the three datasets, we harmonize land-use into categories: residential (including single family and multifamily), commercial (including general commercial, retail, mixed use, and commercial offices), industrial and vacant (including light industrial and warehousing, heavy industrial, public utilities, and vacant land). A residual other category encompasses agriculture, open space, parcels used for public institutions, and unknown uses.

# C Validation and Robustness Checks

## C.1 Alternative Treatment and Control Specifications

One set of threats to the robustness of our main result comes from the definition of treatment and control precincts. Since we are working with precincts of various shapes and sizes, researcher choices about how to define these units may influence the results. Our main specification defines treatment as proximity within a given distance cutoff and control as precincts in a 350-meter donut. We evaluate three alternatives: a continuous-exposure design, a fixed-sample design that holds the sample of non-excluded precincts fixed while varying the threshold between treatment and control, and alternative control-band constructions that matched treated units on area and number of precincts.

We first replace the binary treatment indicator with a continuous exposure measure: the share of a precinct's area falling within a given buffer radius of any new shelter. This design relaxes the threshold assumption entirely and uses the full gradient of spatial overlap. The results closely mirror the main specification—the distance-gradient in support is sustained and of comparable magnitude across the distance spectrum (Figure A1).

We then implement a fixed-sample design that holds the precincts included in the analysis constant. We include all precincts within 1 kilometer of a new shelter and vary only the treated-versus-control threshold. This eliminates any concern that changes in the comparison sample across cutoffs drive the distance gradient. The estimates are consistent with the main results across all thresholds (Figure A2a).

Finally, we replace the 350-meter donut with three alternative control-band constructions: matched-area, which picks control units until the geographic area is roughly the same as the treatment units; matched-$N$, which picks a similar number of control

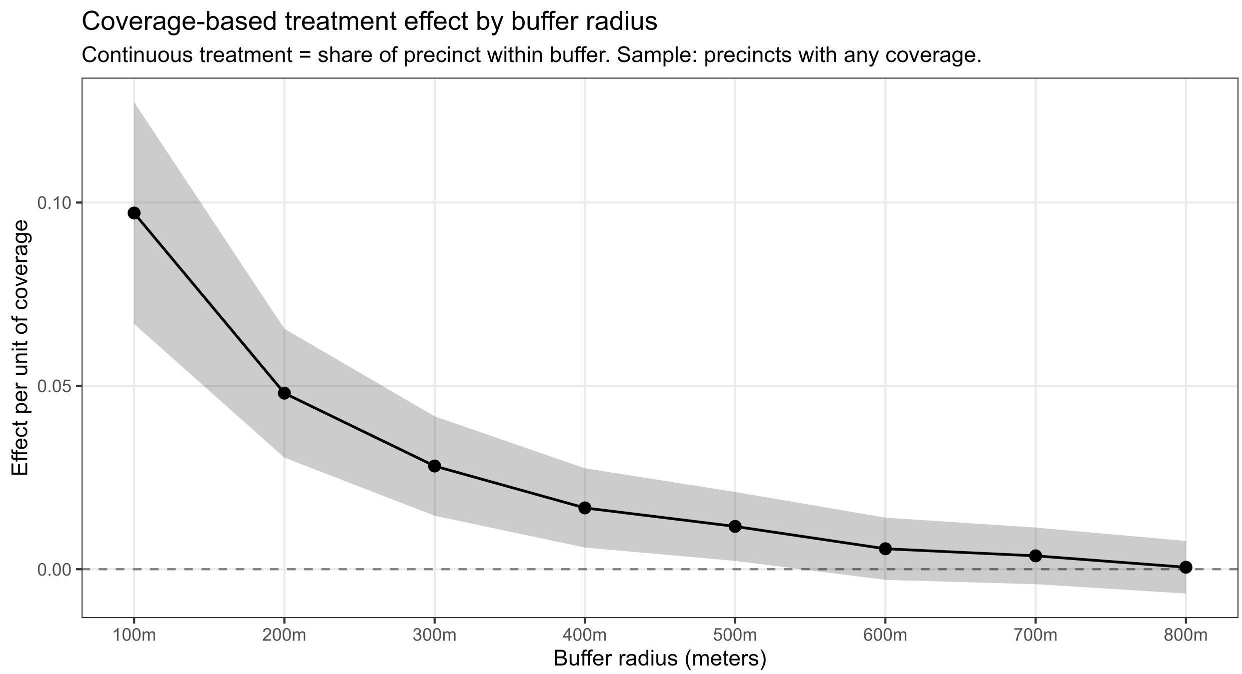


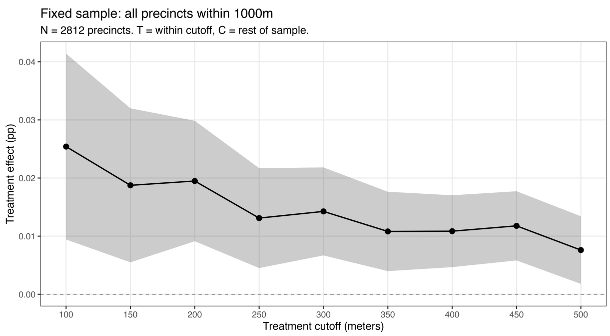

Figure A1: Effect of shelter proximity on vote share, continuous-exposure design. Treatment is the share of precinct area falling within a given buffer radius of any new shelter, replacing the binary treated/control indicator. Bars are 95% confidence intervals with standard errors clustered by shelter.

precincts; and geometric-area, which varies the control donut around the treated ring to equal the same area. All three yield estimates that are directionally consistent and of similar magnitude across distance cutoffs (Figure A2b; matched-area and geometric-area in Appendix Figures A2c and A2d).

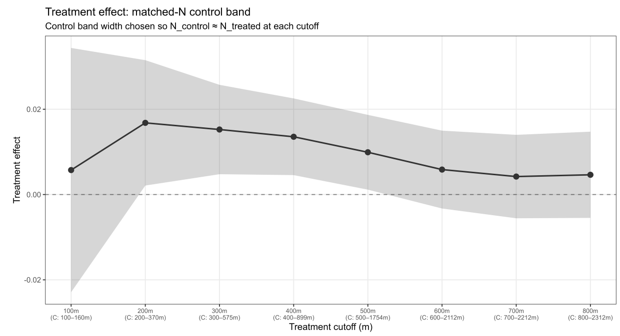
We additionally define our treatment in a continuous fashion using the count of new shelters and the count of new beds, instead of our binary treatment of a new shelter. We show the results in Figure A3.

## C.2 Endogeneity: Homekey Shelters

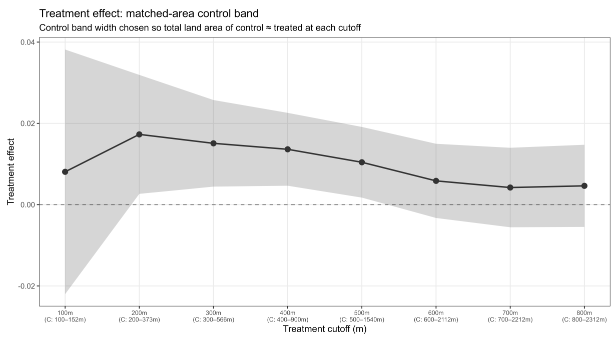
Appendix Figure A5 plots the main treatment effects for the subset of facilities that were coded as a commercial hotel (based on land-use data) and which are likely Homekey sites (the facility name matched California Homekey award tracking documents).



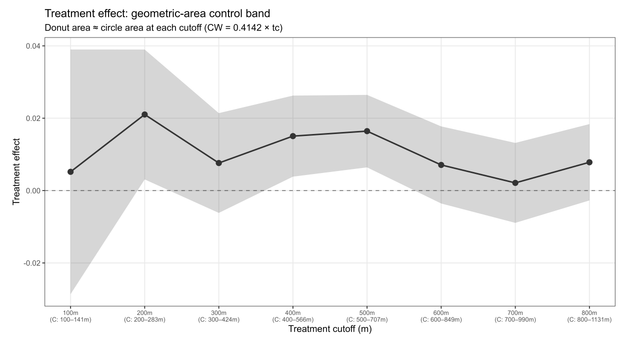

(a) Fixed Sample




(b) Balanced Precinct Counts




(c) Balanced Precinct Area




(d) Balanced T/C Radii

Figure A2: Main effect robust to alternative specifications and treatment/control definitions

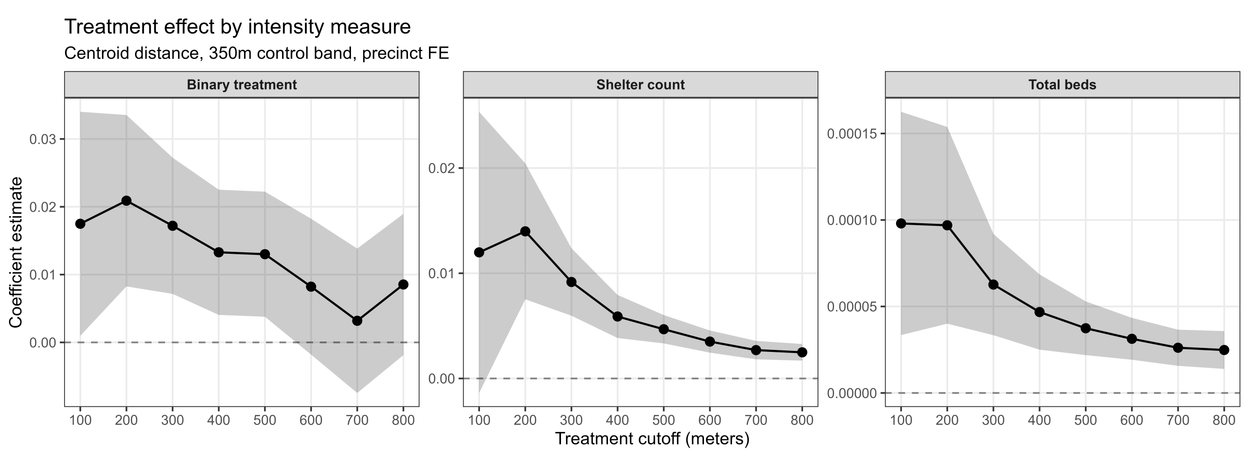



Figure A3: Effect of shelter intensity on vote share

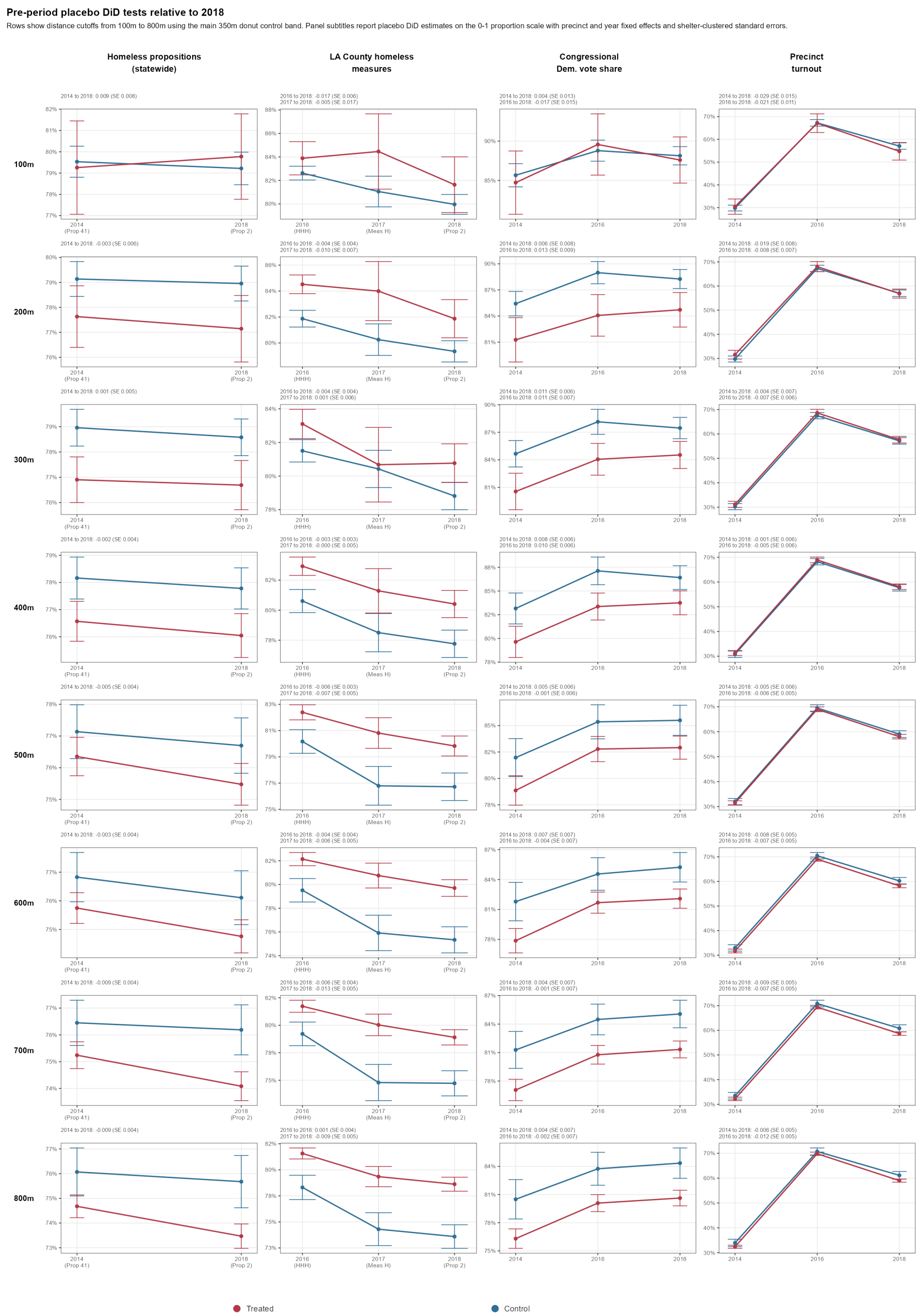



Figure A4: Raw Pre-trend across all distance cutoffs (100m–800m) and four outcomes. Each panel plots group means over time for treated (red) and control (blue) precincts; subtitle annotations report the differential pre-period slope, standard error, and $p$-value. Rows: distance cutoffs from 100m to 800m in 100m steps. Columns: (A) statewide homeless proposition vote share, using the 2014 primary (Proposition 41) and 2018 general (Proposition 2); (B) LA County homeless measure vote share, using November 2016 (Measure HHH), March 2017 (Measure H), and November 2018 (Proposition 2); (C) Congressional Democratic vote share in 2014, 2016, and 2018; (D) precinct turnout in 2014, 2016, and 2018.

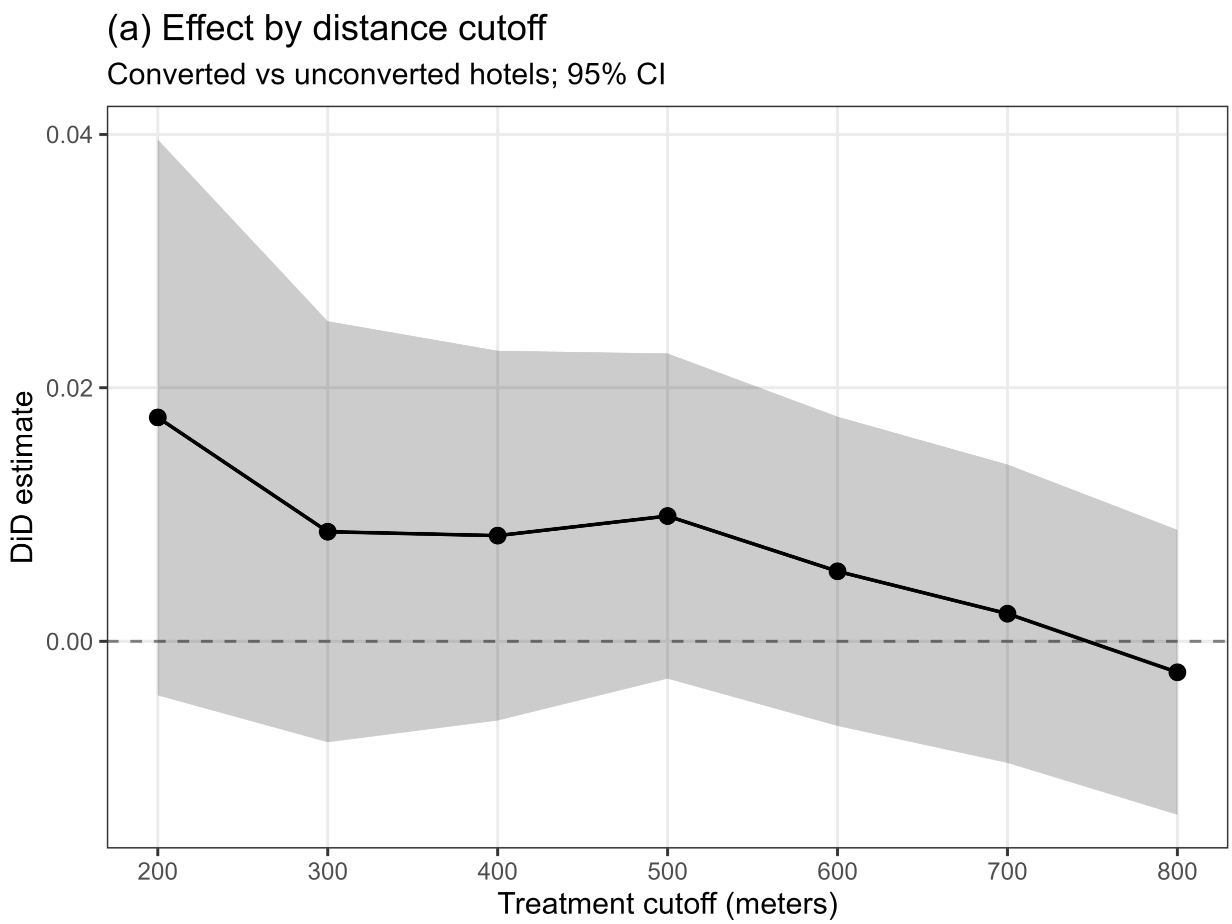



Figure A5: Effect of Homekey shelter proximity on vote share. Panel shows difference-in-differences estimates at each distance cutoff restricting to shelters identified as Homekey hotel conversions. Control precincts are assigned by proximity to unconverted hotels.

### C.3 Placebo: Pre-existing Shelters

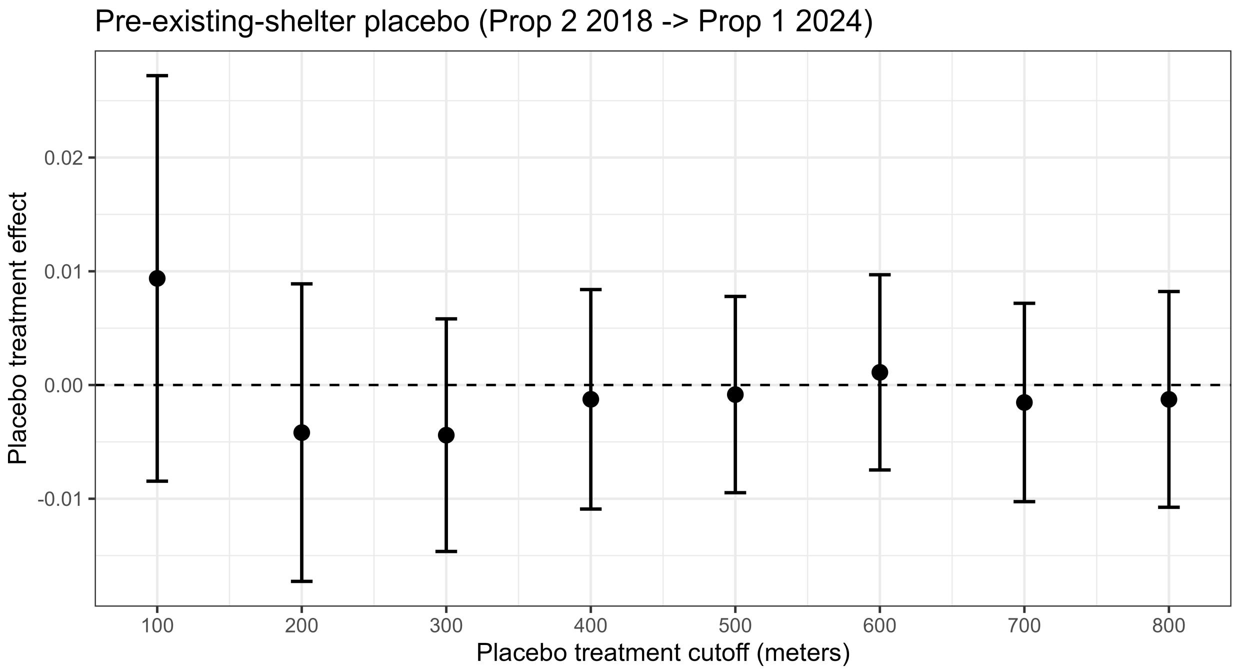



Figure A6: Placebo test using pre-existing (pre-2018) shelters as the treatment. Points are difference-in-differences coefficients from the main specification, with the change in homeless-spending referendum support (Proposition 2 in 2018 to Proposition 1 in 2024) as the outcome. Treated precincts are within the cutoff of a pre-existing shelter; control precincts are between the cutoff and the cutoff plus 350m. At each cutoff, precincts within the cutoff of a new (2018–2024) shelter are excluded from both groups to avoid contamination by the real treatment. Bars are 95% confidence intervals with standard errors clustered by nearest pre-existing shelter.

### C.4 Balance

Table A4: Balance table: 100m treatment cutoff

| | Treated ($N = 57$) | | Control ($N = 433$) | | | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Diff | $p$ |
| 2018 yes-vote share | 0.798 | 0.077 | 0.792 | 0.081 | 0.006 | 0.613 |
| 2018 turnout | 0.548 | 0.147 | 0.570 | 0.149 | -0.023 | 0.278 |
| 2024 turnout | 0.263 | 0.124 | 0.280 | 0.131 | -0.018 | 0.321 |
| 2018 total votes | 716.024 | 454.098 | 904.541 | 596.988 | -188.516 | 0.006 |
| 2024 total votes | 343.614 | 207.664 | 443.568 | 320.154 | -99.954 | 0.002 |
| 2016 Dem share (cong.) | 0.896 | 0.150 | 0.888 | 0.142 | 0.008 | 0.711 |
| Pre-existing shelters | 2.877 | 5.782 | 0.986 | 1.810 | 1.891 | 0.017 |
| Pre-existing beds | 248.018 | 801.882 | 55.882 | 136.921 | 192.135 | 0.076 |
| % White | 0.304 | 0.179 | 0.326 | 0.204 | -0.021 | 0.407 |
| Log population density | 9.119 | 1.121 | 8.700 | 0.921 | 0.419 | 0.009 |
| Log population | 7.781 | 1.000 | 7.882 | 0.978 | -0.100 | 0.477 |
| Log median home value | 13.259 | 0.601 | 13.295 | 0.558 | -0.035 | 0.679 |
| Log median household income | 10.970 | 0.585 | 10.981 | 0.534 | -0.012 | 0.887 |
| % BA+ | 0.334 | 0.216 | 0.344 | 0.241 | -0.011 | 0.734 |
| Homeownership rate | 0.194 | 0.177 | 0.258 | 0.180 | -0.065 | 0.012 |

Table A5: Balance table: 200m treatment cutoff

| | Treated ($N = 210$) | | Control ($N = 480$) | | | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Diff | $p$ |
| 2018 yes-vote share | 0.771 | 0.098 | 0.790 | 0.078 | -0.018 | 0.019 |
| 2018 turnout | 0.569 | 0.137 | 0.570 | 0.149 | -0.001 | 0.906 |
| 2024 turnout | 0.272 | 0.126 | 0.282 | 0.132 | -0.010 | 0.346 |
| 2018 total votes | 708.002 | 510.002 | 973.219 | 641.732 | -265.217 | <0.001 |
| 2024 total votes | 345.752 | 252.824 | 481.317 | 363.303 | -135.564 | <0.001 |
| 2016 Dem share (cong.) | 0.841 | 0.176 | 0.890 | 0.142 | -0.049 | <0.001 |
| Pre-existing shelters | 1.652 | 3.534 | 0.771 | 1.504 | 0.882 | <0.001 |
| Pre-existing beds | 116.719 | 437.777 | 42.510 | 121.149 | 74.209 | 0.016 |
| % White | 0.324 | 0.194 | 0.322 | 0.203 | 0.003 | 0.865 |
| Log population density | 8.595 | 1.234 | 8.689 | 0.831 | -0.094 | 0.316 |
| Log population | 7.590 | 1.181 | 7.954 | 0.910 | -0.364 | <0.001 |
| Log median home value | 13.203 | 0.602 | 13.284 | 0.537 | -0.081 | 0.097 |
| Log median household income | 10.969 | 0.540 | 10.993 | 0.516 | -0.024 | 0.579 |
| % BA+ | 0.315 | 0.211 | 0.340 | 0.240 | -0.025 | 0.165 |
| Homeownership rate | 0.253 | 0.201 | 0.283 | 0.191 | -0.030 | 0.066 |

Table A6: Balance table: 300m treatment cutoff

| | Treated ($N = 386$) | | Control ($N = 471$) | | | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Diff | $p$ |
| 2018 yes-vote share | 0.767 | 0.097 | 0.786 | 0.080 | -0.019 | 0.002 |
| 2018 turnout | 0.576 | 0.136 | 0.571 | 0.148 | 0.005 | 0.610 |
| 2024 turnout | 0.281 | 0.125 | 0.286 | 0.135 | -0.005 | 0.560 |
| 2018 total votes | 779.708 | 559.018 | 1000.478 | 670.709 | -220.770 | <0.001 |
| 2024 total votes | 391.376 | 309.437 | 498.259 | 380.164 | -106.883 | <0.001 |
| 2016 Dem share (cong.) | 0.841 | 0.173 | 0.881 | 0.150 | -0.041 | <0.001 |
| Pre-existing shelters | 1.231 | 2.783 | 0.686 | 1.450 | 0.545 | <0.001 |
| Pre-existing beds | 83.731 | 336.735 | 34.036 | 97.995 | 49.694 | 0.005 |
| % White | 0.328 | 0.199 | 0.327 | 0.211 | 0.001 | 0.941 |
| Log population density | 8.595 | 1.095 | 8.647 | 0.796 | -0.053 | 0.430 |
| Log population | 7.683 | 1.091 | 7.982 | 0.882 | -0.299 | <0.001 |
| Log median home value | 13.213 | 0.576 | 13.288 | 0.538 | -0.075 | 0.051 |
| Log median household income | 11.007 | 0.512 | 10.998 | 0.512 | 0.009 | 0.801 |
| % BA+ | 0.325 | 0.219 | 0.334 | 0.239 | -0.009 | 0.553 |
| Homeownership rate | 0.281 | 0.210 | 0.294 | 0.190 | -0.013 | 0.356 |

## C.5 Different Electorates

We re-estimate our main specification using ballot measures in two large jurisdictions where a homeless-spending measure appeared on a November 2024 ballot. In Los Angeles County, we compare CA Prop 2 (November 2018) to LA Measure A (November 2024) while in San Francisco we compare the same Prop 2 to San Francisco Prop B (November 2024). The additional ballot measures are summarized in Table A7, below.

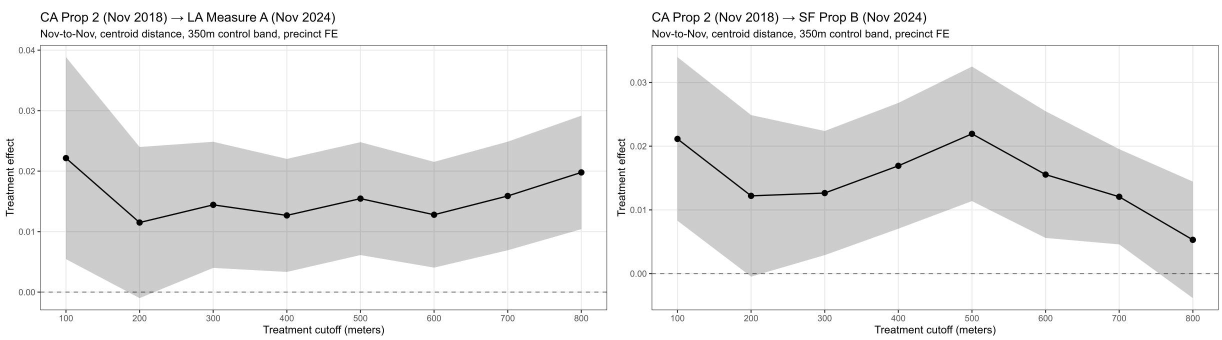
Table A7: California ballot measures on homelessness spending

| Measure | Date | Geography | Revenue type | Directed to | % Yes |
|---|---|---|---|---|---|
| Measure HHH | Nov. 2016 | City of Los Angeles | GO bonds ($1.2B) | Permanent supportive housing | 77.1 |
| Measure H | Mar. 2017 | Los Angeles County | Sales tax (0.25%) | Homelessness services | 69.3 |
| Measure C | Nov. 2018 | San Francisco | Gross receipts tax | Homelessness fund | 61.3 |
| **Prop 2** | **Nov. 2018** | **California** | **Revenue bonds ($2B)** | **Permanent supportive housing** | **63.4** |
| **Prop 1** | **Mar. 2024** | **California** | **GO bonds ($6.4B)** | **Behavioral health + housing** | **50.2** |
| Prop B | Nov. 2024 | San Francisco | GO bonds ($390M) | Shelters and clinics | 72.8 |
| Measure A | Nov. 2024 | Los Angeles County | Sales tax (0.5%) | Housing and homelessness | 57.8 |

Ballot measures used in this article's analyses are shown in **bold**. The two primary election measures (Prop 1, March 2024; Measure H, March 2017) are distinguished by their election type; all others were decided in November general elections.

Both estimates are statistically significant and essentially match the main statewide estimate from November 2018 - March 2024, indicating the result is not likely an artifact of comparing across primary and general electorates (Figure A7).

Figure A7: November-to-November re-estimation of shelter proximity effects




(a) Los Angeles County (Prop 2 to Measure A) (b) San Francisco (Prop 2 to Prop B)

Difference-in-differences estimates of shelter proximity on homeless-spending vote share, holding election type fixed by comparing November 2018 to November 2024 ballot measures. Bars are 95% confidence intervals with standard errors clustered by shelter.

We complement the Nov-to-Nov robustness check with a direct test of the no-persuasion null using observed aggregate mobilization patterns. Under the null that all change in per-baseline yes and no vote rates reflects selective participation rather than persuasion, the observed $\text{ATT}(Y/R_0)$ and $\text{ATT}(N/R_0)$ mechanically imply a predicted yes-share effect:

$$\widehat{\text{ATT}}^{\text{comp}}(S) = \frac{(1-\pi_{0,T})\,M_Y - \pi_{0,T}\,M_N}{b_T},$$

where $M_Y = \text{ATT}(Y/R_0)$ and $M_N = \text{ATT}(N/R_0)$ are the precinct-level DiD estimates of yes and no votes per 2018 baseline registrant, $\pi_{0,T}$ is the treated-precinct 2018 yes share, and $b_T = P_{2018}/R_0$ is the baseline turnout rate. If the no-persuasion null holds, $\widehat{\text{ATT}}^{\text{comp}}(S)$ should equal the observed $\text{ATT}(S)$.

Figure A8 presents the implied composition share $\widehat{\text{ATT}}^{\text{comp}}(S)/\text{ATT}(S)$ at 100, 200, and 300 meters. At 100 meters, the composition prediction is positive—accounting for roughly 19% of $\text{ATT}(S)$—so we cannot rule out a modest participation channel at the narrowest cutoff. But at 200 and 300 meters, the prediction is sharply negative: the observed asymmetry in per-baseline yes and no mobilization would predict *lower* yes shares near shelters, not higher. Because $\pi_{0,T} \approx 0.79$, the no-voter mobilization term is weighted nearly four times as heavily as the yes-voter mobilization term, so the small but positive $M_N$ dominates and the composition contribution goes the wrong direction. The no-persuasion null is internally incoherent with the 200m and 300m estimates: those effects must be largely persuasion by construction.

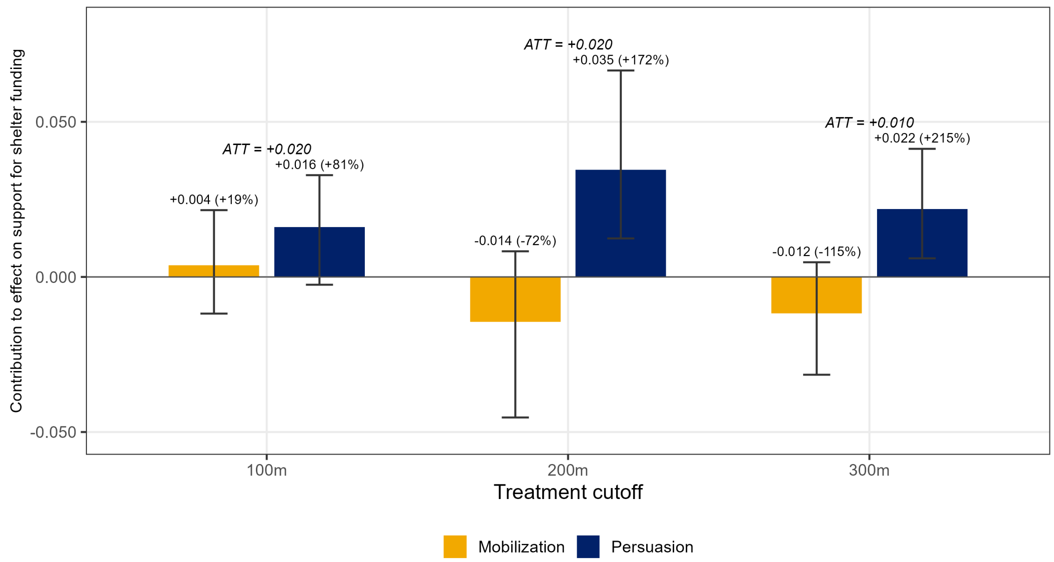



Figure A8: Predicted composition contribution to ATT($S$) under the no-persuasion null. Bars show $\widehat{\text{ATT}}^{\text{comp}}(S)/\text{ATT}(S)$, the share of the observed yes-share effect that selective participation alone would mechanically imply. Negative values indicate the observed mobilization pattern would predict the opposite-signed effect—formally rejecting the no-persuasion null. Bars are 95% bootstrap confidence intervals.

## C.6 Residential Sorting

For mobility to explain a substantial share of the result, two things must be true. First, treated precincts must exhibit meaningful differences in turnover-related composition relative to nearby controls. Second, those differences must be strongly correlated with propensity to vote yes or no on the ballot propositions.

We study these conditions one at a time. First, using our L2 linked voter sample, we calculate the number of "stayers", voters present in the same precinct in the 2018 and 2024 elections, standardized to 2024 precincts as in our analysis data. Let $L_i^{\text{stayer}}$ denote this number. We also calculate the total numbers of 2018 and 2024 registrants from the L2 data in each precinct, $R_{i,2018}^{\text{L2}}$ and $R_{i,2024}^{\text{L2}}$. From this we compute in-mover and out-mover shares for each precinct:

$$\text{In movers}_i = \frac{R_{i,2024}^{\text{L2}} - L_i^{\text{stayer}}}{R_{i,2018}^{\text{L2}}}, \quad \text{Out movers}_i = \frac{R_{i,2018}^{\text{L2}} - L_i^{\text{stayer}}}{R_{i,2018}^{\text{L2}}}.$$

At each distance cutoff, we then compare these turnover measures between treated precincts and nearby controls by estimating a cross-sectional regression, with standard errors clustered at the shelter level

$$\text{outcome}_i = \alpha + \beta \, \text{treated}_i + \varepsilon_i,$$

Our quantity of interest is $\beta$ which is the difference in in-migration or out-migration share between treated and control precincts. Figure A9 presents $\beta$ estimates across distance cutoffs. We find positive effects on out migration across distance cutoffs: precincts near shelters have approximately a 4 percentage point higher rate of out migration than control precincts further away. We also find larger but more imprecise estimates on in-migration. Consequently, areas closer to shelters are experiencing higher turnover in the composition of their electorates.

What this analysis cannot tell us, however, is the extent to which these composition changes are directly a result of new shelters moving in (cross-sectional estimation creates more difficult identification assumptions than our main difference-in-differences estimates). The fact that both in- and out-migration increase perhaps points against new shelters driving people away en masse, since other populations are willing to replace out-migrants. However, these effect estimates do establish a pre-condition by which mobility

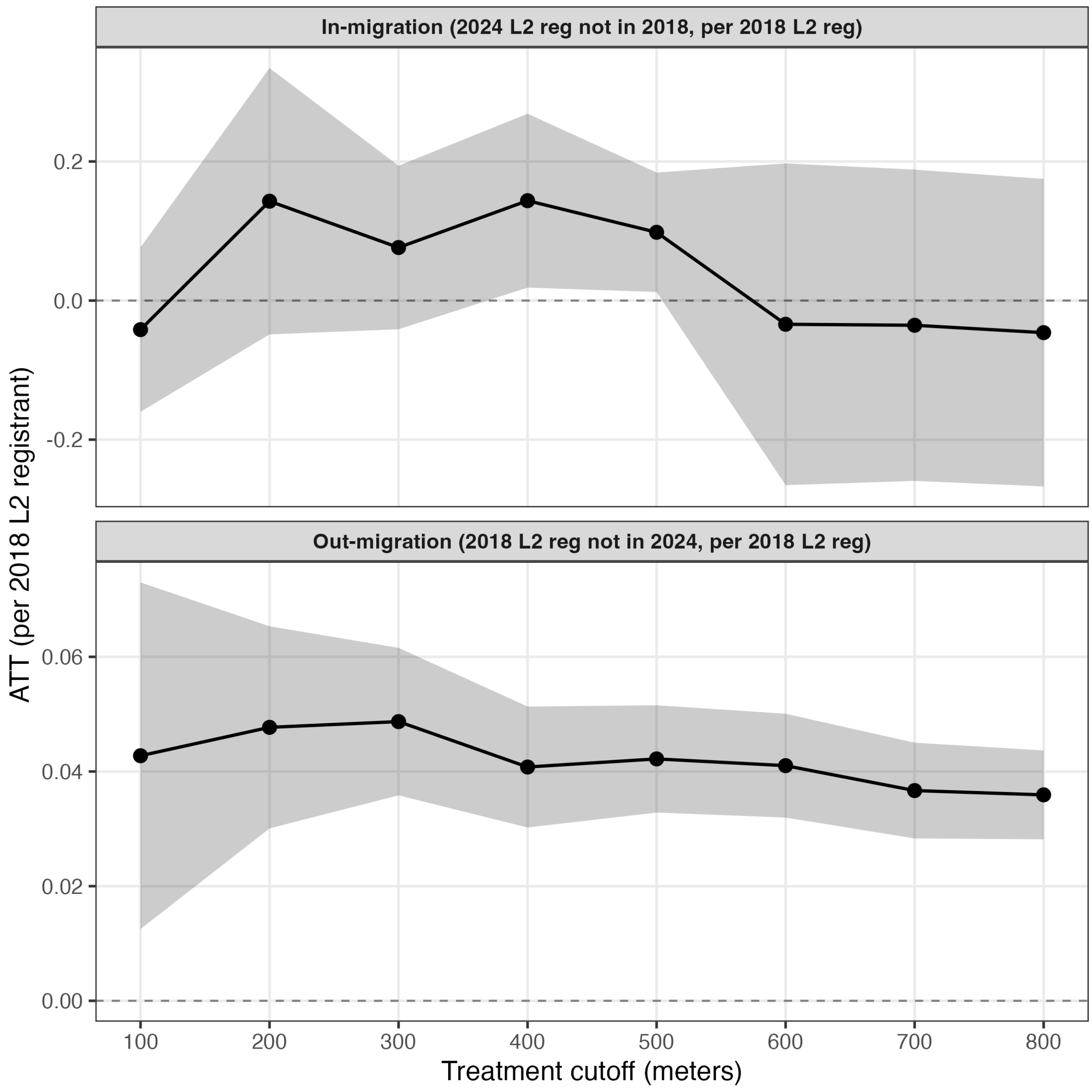



Figure A9: In- and out-migration differences between treated and control precincts, 2018-2024 may explain some of our results. To the extent that people who leave are less supportive of new shelters and people who stay or enter are more tolerant, changes in the composition of the electorate may explain shifts in vote share, rather than from persuasion or mobilization channels. We cannot directly estimate these associations from our data, but we present sensitivity analyses under different realistic scenarios wherein leavers and stayers have different preferences with respect to the shelter referenda. From this we bound how much of our main vote share effects mobility could plausibly explain.

We estimate the contribution of mobility to the effect on referenda vote support under a selective-migration counterfactual. We partition voters into stayers, leavers, and arrivers based on our L2 linked sample. Let $\delta_{\text{out}} = \pi^{\text{stayer}} - \pi^{\text{leaver}}$ and $\delta_{\text{in}} = \pi^{\text{arriver}} - \pi^{\text{stayer}}$ be the differences in referenda support between leavers and stayers and between arrivers and stayers. Let $\rho_t^{\text{stayer}}$ be the share of stayers in the electorate in 2018 or 2024. Under the scenario of no within-stayer response to new shelters – where no stayer changes their mind about how or whether to vote – we define the vote share effect from migration as:

$$\widehat{\text{ATT}}^{\text{mig}}(S) \;=\; \delta_{\text{in}}\,\text{ATT}(1-\rho_{2024}^{\text{stayer}}) \;+\; \delta_{\text{out}}\,\text{ATT}(1-\rho_{2018}^{\text{stayer}}).$$

We then interpret $\widehat{\text{ATT}}(S) - \widehat{\text{ATT}}^{\text{mig}}(S)$ as the residual explained by the effect on political behavior. We set $\delta_{\text{in}} = 0$, assuming that stayers and arrivers are equally tolerant of new shelters. We then consider benchmark values of $\delta_{\text{out}}$, the extent to which leavers are less supportive of new shelters. We benchmark the range of realistic gaps between leaver and stayer support for the referenda as the half standard deviation and standard deviation across treated precincts. For 100 meters the standard deviation is 3.7–7.4 percentage points, for 200 meters it is 4.75–9.5 percentage points, and for 300 meters it is 4.8–9.6 percentage points. We consider gaps outside of these ranges to be unlikely given the data, but we do compute sensitivity under extreme scenarios.

Figure A10 shows the results from this sensitivity analysis for treatment cutoffs at 100, 200, and 300 meters. At 100 meters, we bound the share of vote share effects explained by mobility to be realistically somewhere between 11.4% and 22.8%. At 200 and 300 meters, this range is 9.0%–18.0% and 19.8%–39.5%, respectively. Therefore, small pluralities of our effects may be driven by compositional changes in the electorate due to in- and out-migration, but the large majority of our effect is likely a political behavior response to new shelters opening. In order for the entirety of our effect to be explained by mobility, the gap between leaver and stayer support for the referendum at 100, 200, and 300 meters would have to be 32.3, 52.6, and 24.3 percentage points, respectively—highly unlikely scenarios that are not supported by the variation in the data.

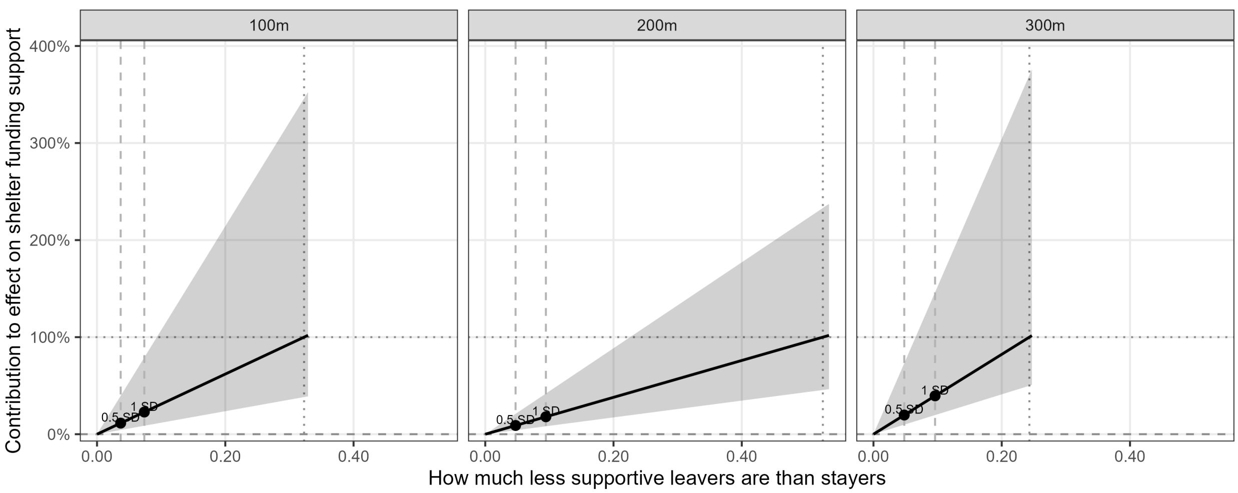



Figure A10: Mobility sensitivity analysis

A separate bound operates on the incumbent electorate rather than on movers. Using within-individual L2 data, we estimate a voter fixed effects specification among stayers—voters registered at the same address in both 2018 and 2024—to recover $m_S =$ ATT(stayer ballot cast), the effect of shelter proximity on stayer referendum participation. Because the sample is held fixed at the individual level, $m_S$ is clean of any migration confound. We then ask: under the most optimistic scenario for the composition channel, where every marginally-mobilized stayer votes yes ($\pi_{\text{marg}} = 1$), how much of ATT($S$) could stayer mobilization mechanically explain? At 100 meters, this extreme upper bound is 12.3% of ATT($S$) (Figure A11). In more realistic scenarios—where the marginal stayer's preferences mirror those of the existing electorate—the mobilization contribution is negligible. The stayer mobilization channel cannot explain a meaningful share of the main result.

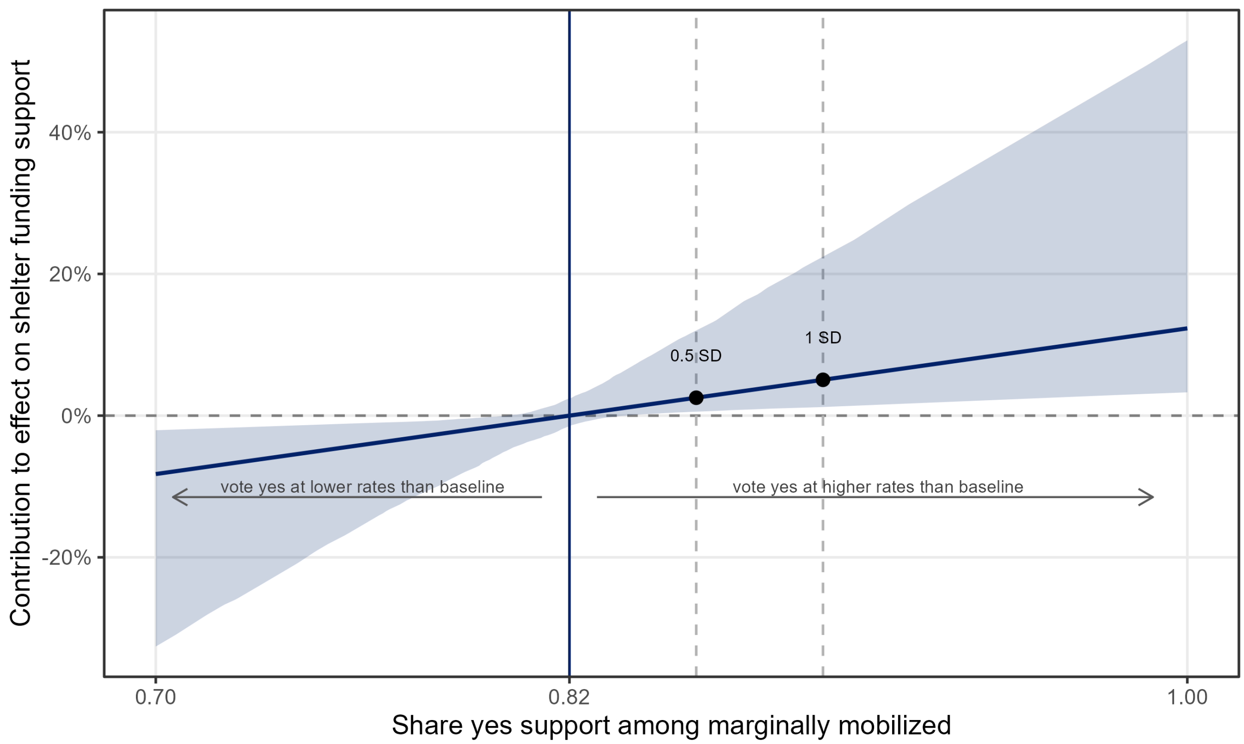



Figure A11: Stayer mobilization bound. Each bar shows the share of ATT($S$) attributable to stayer mobilization under a given assumption about the yes rate among marginally-mobilized stayers ($\pi_{\text{marg}}$), from the extreme pessimistic case ($\pi_{\text{marg}} = 0$, all vote no) to the extreme optimistic case ($\pi_{\text{marg}} = 1$, all vote yes). The center scenario pins $\pi_{\text{marg}} = \pi_{0,T}$ (no asymmetry). Estimates at 100m; bars are 95% bootstrap confidence intervals.

## C.7 Standard errors

Table A8: Main vote-share effect under alternative standard error strategies. The estimate is the coefficient on treatment × post from the main precinct fixed-effects difference-in-differences specification. Treatment is defined by whether the precinct centroid is within the listed cutoff of a new shelter; controls are precincts in the cutoff-to-cutoff-plus-350m band with no other new shelter within the cutoff. Standard errors are in parentheses.

| | | | | Cluster-robust SE | | Conley spatial SE | |
|---|---|---|---|---|---|---|---|
| Cutoff | Precincts | Treated | Estimate | Shelter | County | 5 km | 10 km |
| 100m | 485 | 55 | 0.0175 | (0.0084)$^{*}$ | (0.0093)$^{\cdot}$ | (0.0169) | (0.0194) |
| 200m | 683 | 205 | 0.0209 | (0.0064)$^{**}$ | (0.0058)$^{***}$ | (0.0091)$^{*}$ | (0.0107)$^{\cdot}$ |
| 300m | 849 | 380 | 0.0172 | (0.0051)$^{***}$ | (0.0053)$^{**}$ | (0.0054)$^{**}$ | (0.0073)$^{*}$ |
| 400m | 1,041 | 598 | 0.0133 | (0.0047)$^{**}$ | (0.0039)$^{***}$ | (0.0055)$^{*}$ | (0.0060)$^{*}$ |
| 500m | 1,274 | 875 | 0.0130 | (0.0047)$^{**}$ | (0.0033)$^{***}$ | (0.0055)$^{*}$ | (0.0049)$^{**}$ |
| 600m | 1,532 | 1,153 | 0.0082 | (0.0051) | (0.0041)$^{*}$ | (0.0066) | (0.0070) |
| 700m | 1,772 | 1,416 | 0.0032 | (0.0054) | (0.0059) | (0.0071) | (0.0080) |
| 800m | 2,012 | 1,703 | 0.0085 | (0.0053) | (0.0064) | (0.0088) | (0.0092) |

*Notes:* $^{\cdot}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Conley standard errors use a triangular spatial kernel with the listed decay distance, computed from land-clipped precinct centroids.

## C.8 Time interacted baseline controls

Table A9: Main vote-share effect with covariate time-trend controls. Each coefficient is from a separate precinct fixed-effects difference-in-differences regression using the listed treatment cutoff and a 350-meter comparison band.

| Covariate × post control | 100m | 200m | 300m | 400m | 500m | 600m | 700m | 800m |
|---|---|---|---|---|---|---|---|---|
| None | 0.017* | 0.021** | 0.017*** | 0.013** | 0.013** | 0.008 | 0.003 | 0.009 |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| Dem. share 2018 | 0.017* | 0.018** | 0.016** | 0.012* | 0.012** | 0.007 | 0.002 | 0.007 |
| | (0.008) | (0.007) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| Turnout 2018 | 0.019* | 0.021** | 0.017** | 0.013** | 0.014** | 0.010* | 0.005 | 0.010* |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| Registration 2018 | 0.011 | 0.012· | 0.010· | 0.008· | 0.009· | 0.004 | -0.000 | 0.006 |
| | (0.009) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| Pre-existing shelters | 0.014 | 0.019** | 0.016** | 0.012* | 0.012* | 0.007 | 0.002 | 0.008 |
| | (0.009) | (0.007) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| % White | 0.019* | 0.020** | 0.017** | 0.013** | 0.014** | 0.011* | 0.007 | 0.011* |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 968 | 1,364 | 1,694 | 2,076 | 2,540 | 3,052 | 3,530 | 4,006 |
| Log pop. density | 0.014· | 0.021** | 0.017** | 0.014** | 0.013** | 0.010* | 0.006 | 0.010* |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 968 | 1,364 | 1,694 | 2,076 | 2,540 | 3,052 | 3,530 | 4,006 |
| Log population | 0.018* | 0.015* | 0.012* | 0.010* | 0.012* | 0.009· | 0.004 | 0.009· |
| | (0.008) | (0.007) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 968 | 1,364 | 1,694 | 2,076 | 2,540 | 3,052 | 3,530 | 4,006 |
| Log home value | 0.017* | 0.022*** | 0.018*** | 0.013** | 0.013** | 0.008 | 0.003 | 0.008 |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 964 | 1,358 | 1,688 | 2,072 | 2,538 | 3,054 | 3,532 | 4,012 |
| Log income | 0.018* | 0.022*** | 0.017*** | 0.013** | 0.013** | 0.010· | 0.005 | 0.010· |
| | (0.008) | (0.007) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| % BA+ | 0.018* | 0.022*** | 0.018*** | 0.013** | 0.013** | 0.009· | 0.004 | 0.010· |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| Homeownership | 0.020* | 0.021** | 0.017** | 0.014** | 0.013** | 0.011* | 0.006 | 0.010* |
| | (0.009) | (0.007) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 966 | 1,362 | 1,692 | 2,072 | 2,536 | 3,048 | 3,526 | 4,002 |
| Precinct fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Shelter-clustered SE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Comparison band | 350m | 350m | 350m | 350m | 350m | 350m | 350m | 350m |

*Notes:* Coefficients are effects on support for the 2024 homeless-services ballot measure relative to the 2018 baseline measure, reported on the 0–1 proportion scale. Standard errors are in parentheses and clustered by nearest new shelter. Treatment is defined by whether the precinct centroid is within the listed cutoff of a new shelter; controls are precincts in the cutoff-to-cutoff-plus-350m band with no other new shelter within the cutoff. Each row adds the listed time-invariant precinct covariate interacted with the post-treatment indicator; the 'None' row is the main specification without covariate time-trend controls. $^{\cdot}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

## C.9 Spatially Varying Unobserved Confounders

Table A10: Robustness to spatially varying unobserved confounders.

| Specification | 100m | 200m | 300m | 400m | 500m | 600m | 700m | 800m |
|---|---|---|---|---|---|---|---|---|
| Main | 0.017* | 0.021** | 0.017*** | 0.013** | 0.013** | 0.008 | 0.003 | 0.009 |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| Latitude/longitude × trend | 0.007 | 0.016* | 0.015** | 0.011* | 0.013** | 0.010* | 0.005 | 0.011* |
| | (0.008) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| County-specific trend | 0.004 | 0.013* | 0.007 | 0.003 | 0.005 | 0.004 | 0.001 | 0.007· |
| | (0.008) | (0.006) | (0.004) | (0.004) | (0.004) | (0.004) | (0.005) | (0.004) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| ZCTA × post FE | -0.001 | 0.014* | 0.009· | 0.006 | 0.005 | 0.001 | -0.001 | 0.004 |
| | (0.010) | (0.007) | (0.005) | (0.005) | (0.005) | (0.005) | (0.006) | (0.005) |
| Observations | 798 | 1,146 | 1,452 | 1,810 | 2,246 | 2,734 | 3,214 | 3,730 |
| Major-city × post FE | 0.005 | 0.013* | 0.009· | 0.006 | 0.008· | 0.005 | 0.003 | 0.007 |
| | (0.008) | (0.006) | (0.005) | (0.004) | (0.004) | (0.005) | (0.005) | (0.005) |
| Observations | 970 | 1,366 | 1,698 | 2,082 | 2,548 | 3,064 | 3,544 | 4,024 |
| Census place × post FE | 0.008 | 0.014* | 0.008· | 0.006 | 0.005 | 0.002 | 0.001 | 0.008· |
| | (0.008) | (0.006) | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) | (0.004) |
| Observations | 924 | 1,280 | 1,594 | 1,948 | 2,410 | 2,930 | 3,382 | 3,848 |
| Precinct fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Shelter-clustered SE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Comparison band | 350m | 350m | 350m | 350m | 350m | 350m | 350m | 350m |

*Notes:* Coefficients report the effect on support for the 2024 homeless-services ballot measure relative to the 2018 baseline measure, on the 0–1 vote-share scale. Treatment is defined by whether the precinct centroid is within the listed cutoff of a new shelter; controls are precincts in the cutoff-to-cutoff-plus-350m band with no other new shelter within the cutoff. The latitude/longitude trend specification interacts mean-centered land-clipped precinct centroid coordinates with a linear year trend. ZCTA and Census place assignments come from spatial joins between precinct centroids and 2020 Census TIGER/Line cartographic boundaries. The selected major-city row allows separate post-period effects for Los Angeles, San Diego, San Jose, San Francisco, and all other precincts. With the two-period 2018/2024 vote panel, county-specific trends, ZCTA-by-post fixed effects, major-city-by-post fixed effects, and Census-place-by-post fixed effects should be read as different ways of absorbing spatially varying post-period shocks. Standard errors are in parentheses and clustered by nearest new shelter. $^{\cdot}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

## C.10 Logged distance specification

Table A11: Logged-distance first-difference estimates, 800m distance window

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Intercept | -0.0600*** | -0.0837** | 0.0178 | 0.0266 |
| | (0.0177) | (0.0257) | (0.1039) | (0.1094) |
| Log distance to nearest new shelter | -0.0110*** | -0.0091** | -0.0097** | -0.0073* |
| | (0.0029) | (0.0030) | (0.0031) | (0.0031) |
| 2018 Democratic vote share | | -0.0243 | | -0.0325. |
| | | (0.0158) | | (0.0168) |
| 2018 turnout | | 0.0812*** | | 0.1672*** |
| | | (0.0160) | | (0.0332) |
| 2018 total registration | | -0.0000*** | | -0.0000* |
| | | (0.0000) | | (0.0000) |
| Pre-existing shelters | | 0.0037*** | | 0.0038*** |
| | | (0.0010) | | (0.0010) |
| % White | | | -0.0015 | -0.0559** |
| | | | (0.0170) | (0.0194) |
| Log population density | | | 0.0042 | 0.0036 |
| | | | (0.0044) | (0.0041) |
| Log population | | | -0.0121*** | -0.0027 |
| | | | (0.0036) | (0.0064) |
| Log median home value | | | -0.0232*** | -0.0242*** |
| | | | (0.0068) | (0.0067) |
| Log median household income | | | 0.0254** | 0.0154. |
| | | | (0.0088) | (0.0091) |
| % BA+ | | | 0.0265 | 0.0072 |
| | | | (0.0195) | (0.0205) |
| Homeownership rate | | | -0.0301. | -0.0357* |
| | | | (0.0156) | (0.0163) |
| Observations | 1,703 | 1,703 | 1,688 | 1,688 |
| Nearest-shelter clusters | 945 | 945 | 945 | 945 |
| $R^2$ | 0.008 | 0.073 | 0.059 | 0.101 |

*Notes:* Outcome is share $\text{yes}_{2024}$ − share $\text{yes}_{2018}$. The sample is restricted to precincts within the listed distance of a new shelter. Standard errors, in parentheses, are clustered by nearest new shelter. Blank cells indicate that the term is not included in that specification. $^{\cdot}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

Table A12: Logged-distance first-difference estimates, 1km distance window

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Intercept | -0.0702*** | -0.0888*** | 0.0275 | 0.0457 |
| | (0.0157) | (0.0229) | (0.0883) | (0.0924) |
| Log distance to nearest new shelter | -0.0092*** | -0.0083** | -0.0086*** | -0.0066* |
| | (0.0025) | (0.0026) | (0.0026) | (0.0026) |
| 2018 Democratic vote share | | -0.0224. | | -0.0245. |
| | | (0.0131) | | (0.0143) |
| 2018 turnout | | 0.0753*** | | 0.1764*** |
| | | (0.0148) | | (0.0285) |
| 2018 total registration | | -0.0000*** | | -0.0000* |
| | | (0.0000) | | (0.0000) |
| Pre-existing shelters | | 0.0035*** | | 0.0036*** |
| | | (0.0010) | | (0.0010) |
| % White | | | 0.0044 | -0.0494** |
| | | | (0.0146) | (0.0160) |
| Log population density | | | 0.0039 | 0.0034 |
| | | | (0.0038) | (0.0036) |
| Log population | | | -0.0095** | -0.0002 |
| | | | (0.0031) | (0.0054) |
| Log median home value | | | -0.0244*** | -0.0265*** |
| | | | (0.0054) | (0.0053) |
| Log median household income | | | 0.0235** | 0.0129. |
| | | | (0.0074) | (0.0077) |
| % BA+ | | | 0.0273 | 0.0077 |
| | | | (0.0169) | (0.0177) |
| Homeownership rate | | | -0.0235. | -0.0298* |
| | | | (0.0123) | (0.0131) |
| Observations | 2,345 | 2,345 | 2,327 | 2,327 |
| Nearest-shelter clusters | 1,059 | 1,059 | 1,059 | 1,059 |
| $R^2$ | 0.006 | 0.058 | 0.049 | 0.093 |

*Notes:* Outcome is share $\text{yes}_{2024}$ − share $\text{yes}_{2018}$. The sample is restricted to precincts within the listed distance of a new shelter. Standard errors, in parentheses, are clustered by nearest new shelter. Blank cells indicate that the term is not included in that specification. $^{\cdot}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

Table A13: Logged-distance first-difference estimates, 1150m distance window

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Intercept | -0.0719*** | -0.0860*** | 0.0229 | 0.0313 |
| | (0.0145) | (0.0213) | (0.0847) | (0.0888) |
| Log distance to nearest new shelter | -0.0089*** | -0.0087*** | -0.0086*** | -0.0070** |
| | (0.0023) | (0.0024) | (0.0024) | (0.0024) |
| 2018 Democratic vote share | | -0.0196 | | -0.0212 |
| | | (0.0125) | | (0.0133) |
| 2018 turnout | | 0.0704*** | | 0.1696*** |
| | | (0.0149) | | (0.0268) |
| 2018 total registration | | -0.0000*** | | -0.0000** |
| | | (0.0000) | | (0.0000) |
| Pre-existing shelters | | 0.0033*** | | 0.0034*** |
| | | (0.0009) | | (0.0009) |
| % White | | | 0.0020 | -0.0486** |
| | | | (0.0136) | (0.0148) |
| Log population density | | | 0.0025 | 0.0020 |
| | | | (0.0036) | (0.0034) |
| Log population | | | -0.0089** | 0.0004 |
| | | | (0.0029) | (0.0050) |
| Log median home value | | | -0.0250*** | -0.0265*** |
| | | | (0.0051) | (0.0052) |
| Log median household income | | | 0.0254*** | 0.0153* |
| | | | (0.0070) | (0.0073) |
| % BA+ | | | 0.0281. | 0.0086 |
| | | | (0.0156) | (0.0165) |
| Homeownership rate | | | -0.0263* | -0.0325** |
| | | | (0.0116) | (0.0124) |
| Observations | 2,772 | 2,772 | 2,747 | 2,747 |
| Nearest-shelter clusters | 1,104 | 1,104 | 1,104 | 1,104 |
| $R^2$ | 0.006 | 0.052 | 0.049 | 0.089 |

*Notes:* Outcome is share $\text{yes}_{2024}$ − share $\text{yes}_{2018}$. The sample is restricted to precincts within the listed distance of a new shelter. Standard errors, in parentheses, are clustered by nearest new shelter. Blank cells indicate that the term is not included in that specification. $^{\cdot}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

# D Additional Analyses

## D.1 Individual-Level Voter Turnout

We also estimate the effect of new shelter proximity on individual voter turnout using the L2 voter file. This analysis restricts to voters observed at the same address in both the 2018 and 2024 elections and replaces precinct fixed effects with individual voter fixed effects. Figure A12 reports the distance-gradient estimates. The turnout response is concentrated at the closest cutoff: voters within 100 meters of a new shelter are more likely to turn out, while the estimates attenuate toward zero and become slightly negative at wider cutoffs.

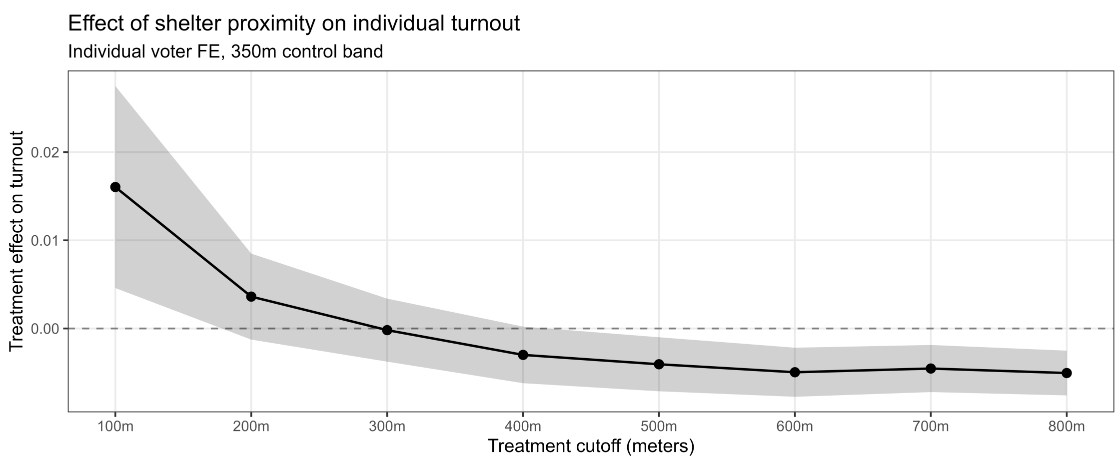



Figure A12: Effect of new shelter proximity on individual voter turnout. Points report difference-in-differences estimates from individual voter fixed-effects models, estimated across treatment distance cutoffs from 100m to 800m. Treatment is defined by whether a voter lives within the listed cutoff of a new shelter; controls are voters in the cutoff-to-cutoff-plus-350m band. Shaded bands are 95% confidence intervals with standard errors clustered by nearest shelter.

## D.2 Effects on Congressional Vote Share

Beyond ballot-measure support, we ask whether proximity to a new shelter shifts *partisan* voting more broadly. We re-estimate the main difference-in-differences specification (centroid-distance treatment, 350m donut control band, precinct fixed effects, shelter-clustered standard errors) using the change in U.S. House Democratic two-party vote share between 2018 and 2024 as the outcome—the same window as our main ballot-measure result—restricting to precincts with a contested Democrat-versus-Republican general election in both years. Figure A13 reports the results across distance cutoffs. The estimated effects are small, statistically indistinguishable from zero at close range, and only slightly negative at wider cutoffs. Proximity to a new shelter does not produce a meaningful shift in congressional partisan vote share, indicating that the increase in homeless-spending support documented in our main results reflects a measure-specific response rather than a broad realignment of partisan preferences near shelter sites.

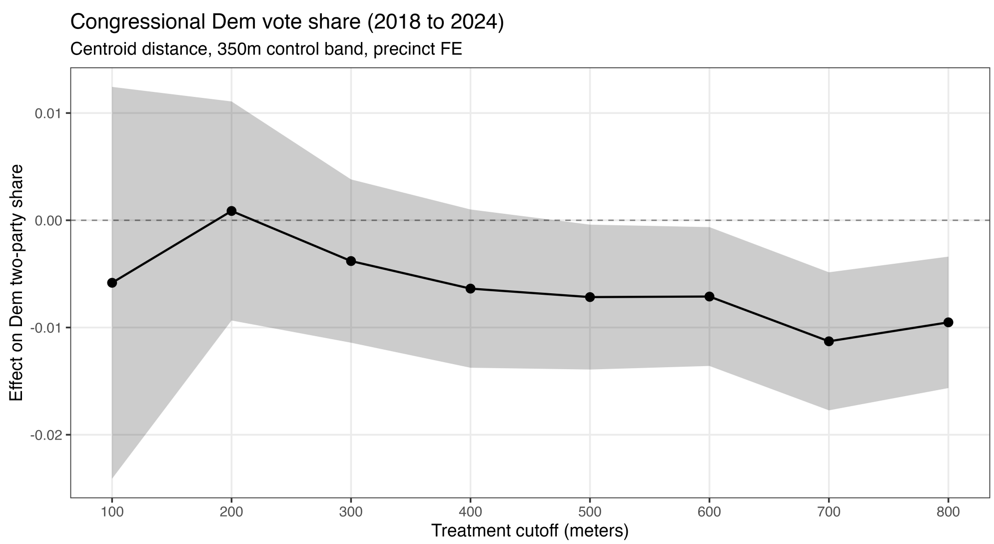



Figure A13: Effect of new shelter proximity on congressional (U.S. House) Democratic two-party vote share, 2018 to 2024. Points are difference-in-differences coefficients from the main specification. The sample is restricted to precincts with a contested Democrat-versus-Republican House election in both 2018 and 2024. Bars are 95% confidence intervals with standard errors clustered by shelter.

## D.3 Effect heterogeneity by housing tenure, racial demographics, population density, home value, income, and education

## D.4 Population Type

Table A14: Difference-in-differences estimates by covariate tercile and continuous triple interaction.

| Moderator | Cutoff | Low tercile | Middle tercile | High tercile | Interaction slope |
|---|---|---|---|---|---|
| % White | 100m | 0.013<br>(0.011) | 0.023$\cdot$<br>(0.012) | 0.025<br>(0.044) | 0.021$\cdot$<br>(0.011) |
| % White | 200m | 0.018*<br>(0.008) | 0.019$\cdot$<br>(0.010) | 0.041*<br>(0.021) | 0.012<br>(0.007) |
| % White | 300m | 0.017*<br>(0.007) | 0.013<br>(0.009) | 0.025$\cdot$<br>(0.015) | 0.006<br>(0.006) |
| Log population density | 100m | -0.052$\cdot$<br>(0.029) | 0.025<br>(0.019) | 0.020*<br>(0.009) | 0.029*<br>(0.014) |
| Log population density | 200m | 0.014<br>(0.040) | 0.032*<br>(0.016) | 0.018**<br>(0.007) | -0.017<br>(0.015) |
| Log population density | 300m | 0.017<br>(0.038) | 0.020<br>(0.013) | 0.014**<br>(0.005) | -0.017<br>(0.015) |
| Log population | 100m | -0.001<br>(0.035) | 0.008<br>(0.010) | 0.024*<br>(0.010) | 0.012<br>(0.018) |
| Log population | 200m | 0.011<br>(0.023) | 0.010<br>(0.008) | 0.022**<br>(0.008) | 0.008<br>(0.012) |
| Log population | 300m | 0.011<br>(0.021) | 0.008<br>(0.007) | 0.014*<br>(0.006) | 0.001<br>(0.012) |
| Log median home value | 100m | -0.013<br>(0.020) | 0.015<br>(0.013) | 0.032*<br>(0.013) | 0.003<br>(0.010) |
| Log median home value | 200m | 0.030**<br>(0.012) | 0.023*<br>(0.010) | 0.014<br>(0.011) | -0.018*<br>(0.007) |
| Log median home value | 300m | 0.019$\cdot$<br>(0.011) | 0.016*<br>(0.008) | 0.017*<br>(0.009) | -0.008<br>(0.006) |
| Log median household income | 100m | 0.020$\cdot$<br>(0.011) | 0.041*<br>(0.021) | -0.013<br>(0.015) | -0.011<br>(0.007) |
| Log median household income | 200m | 0.029***<br>(0.009) | 0.029*<br>(0.013) | -0.016<br>(0.011) | -0.014*<br>(0.006) |
| Log median household income | 300m | 0.019**<br>(0.007) | 0.034**<br>(0.011) | -0.006<br>(0.010) | -0.006<br>(0.005) |
| % BA+ | 100m | -0.005<br>(0.013) | 0.038*<br>(0.018) | 0.031*<br>(0.012) | 0.010<br>(0.006) |
| % BA+ | 200m | 0.023*<br>(0.010) | 0.037**<br>(0.013) | 0.013<br>(0.010) | -0.003<br>(0.005) |
| % BA+ | 300m | 0.020*<br>(0.008) | 0.022*<br>(0.011) | 0.016$\cdot$<br>(0.009) | -0.002<br>(0.005) |
| Homeownership rate | 100m | 0.018*<br>(0.009) | 0.036$\cdot$<br>(0.021) | – | -0.020$\cdot$<br>(0.010) |
| Homeownership rate | 200m | 0.022**<br>(0.007) | 0.010<br>(0.016) | -0.055<br>(0.069) | -0.014<br>(0.008) |
| Homeownership rate | 300m | 0.017**<br>(0.005) | 0.015<br>(0.013) | -0.050<br>(0.078) | -0.006<br>(0.008) |

Tercile columns report the `treatment:post` coefficient from separate DiD models estimated within moderator terciles. The interaction slope is the coefficient on `treatment` × `post` × `moderator_z` from the pooled continuous triple-DiD model, where `moderator_z` is centered and scaled over unique precincts. Coefficients are reported on the 0–1 proportion scale; standard errors, in parentheses, are clustered by nearest shelter. $\cdot p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

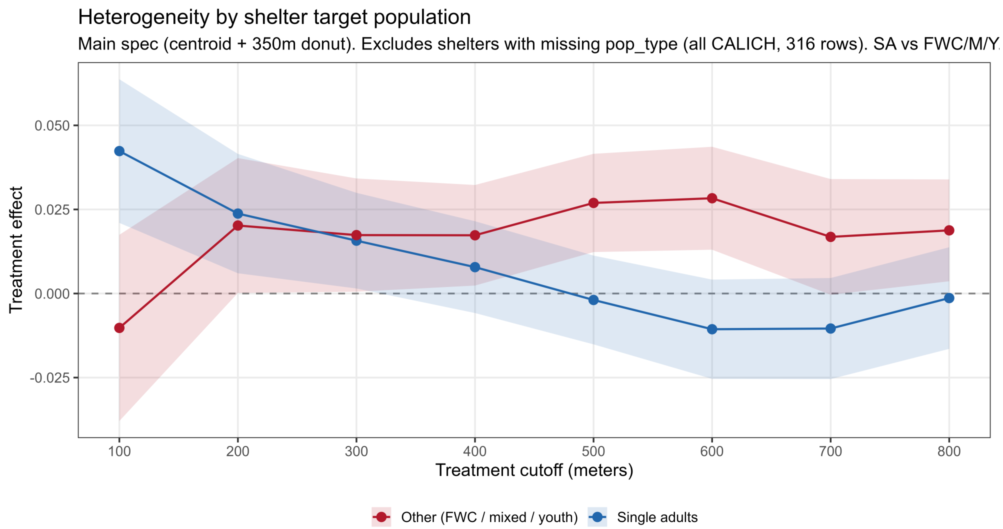
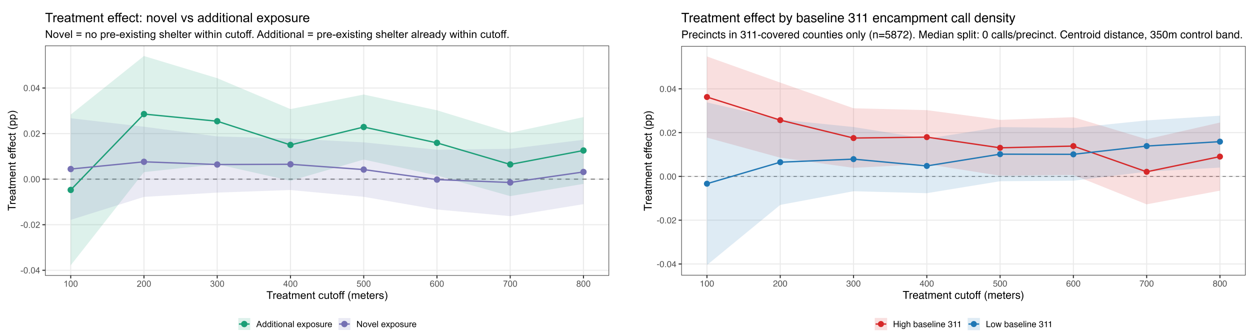



Figure A14: Heterogeneous effects by shelter population type

## D.5 Preexisting Exposure to Homelessness




(a) Preexisting Shelters

(b) Preexisting 311 Calls

Figure A15: Effect of New Facility by Preexisting Exposure to Homelessness

## D.6 Additional Survey Results

Appendix Table A15 presents a regression where self-reported exposure to homeless people (seeing a homeless person at least weekly) is the independent variable and various attitudes are the dependent variables. Column 1 is concern about the presence of homeless people, column 2 is belief in the cause being mental health services, column 3 is belief in the cause being substance abuse, column 4 is belief in the cause being lack of affordable housing, column 5 is belief in the cause being loss of income, column 6 is considering homelessness to be a big problem, and column 7 is ranking homelessness as the top issue facing the state. These results mirror that of Lee et al. (2004), with the notable exception that self-reported exposure also increases attribution to substance abuse as a cause of homelessness. All models include controls for observable demographics.

Table A15: Effect of Observing Homelessness on Attitudes

| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| See Homeless Weekly | 0.304*** | 0.145*** | 0.163*** | 0.112** | 0.0984** | 0.329*** | 0.0440** |
| | (0.0254) | (0.0352) | (0.0294) | (0.0349) | (0.0362) | (0.0345) | (0.0165) |
| Constant | 0.151*** | 0.532*** | 0.660*** | 0.576*** | 0.529*** | 0.362*** | 0.0728*** |
| | (0.0238) | (0.0330) | (0.0275) | (0.0327) | (0.0338) | (0.0323) | (0.0155) |
| Observations | 3,345 | 1,644 | 1,646 | 1,645 | 1,645 | 1,650 | 3,349 |
| R-squared | 0.0409 | 0.0102 | 0.0183 | 0.00621 | 0.004 | 0.052 | 0.002 |

Standard errors in parentheses

. p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Note: Columns 1 and 7 include the December 2023 and October 2025 surveys. Columns 2 to 6 only include the December 2023 survey. Controls include homeowner status, race, age, income, length of residence, ideology, education, and gender.

Appendix Table A16 presents a regression where voting for Prop 1 is the dependent variable. Attributing structural causes increases support for the Proposition, while attributing substance abuse decreases support (although effects are smaller). Concern about the presence of homeless people in one's community has inconsistent effects depending on if the Zip Code is in the treatment or in the control. Ranking homelessness as the top issue produces positive effects. Finally, thinking that homelessness is a big problem and self-reported exposure to homelessness do not directly predict the outcome. We note, however, that self-reported exposure predicts all of the other attitudes, which is consistent with exposure operating as a mediator.

Table A16: Effect of Homelessness Attitudes on Proposition Vote by Treatment Group

| Variables | Full Sample | Control Zips | Treated Zips |
|---|---|---|---|
| Concern Presence | -0.0668** | -0.137*** | -0.0209 |
| | (0.0241) | (0.0358) | (0.0306) |
| See Homeless Weekly | -0.0127 | -0.00706 | -0.0274 |
| | (0.0341) | (0.0406) | (0.0529) |
| Cause: Mental Services | 0.222*** | 0.258*** | 0.188*** |
| | (0.0260) | (0.0580) | (0.0420) |
| Cause: Drugs | -0.0594** | -0.0850* | -0.0475 |
| | (0.0229) | (0.0392) | (0.0340) |
| Cause: Affordable Housing | 0.202*** | 0.118* | 0.249*** |
| | (0.0313) | (0.0572) | (0.0434) |
| Cause: Loss of Income | 0.123** | 0.165*** | 0.105. |
| | (0.0423) | (0.0245) | (0.0586) |
| Homeless Top Issue | 0.107** | 0.133** | 0.0901* |
| | (0.0341) | (0.0440) | (0.0392) |
| Homelessness Big Problem | 0.0216 | 0.0433 | 0.00977 |
| | (0.0314) | (0.0355) | (0.0413) |
| Constant | 0.394*** | 0.418*** | 0.396*** |
| | (0.0380) | (0.0566) | (0.0665) |
| Observations | 1,369 | 569 | 792 |
| R-squared | 0.207 | 0.234 | 0.216 |

Robust standard errors in parentheses

. $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Note: December 2023 survey. All models include county fixed effects and clustered standard errors at the county and Zip Code level. Controls include homeowner status, race, age, income, length of residence, ideology, education, and gender.