The Policy Adjacent: How Affordable Housing Generates Policy Feedback Among Neighboring Residents

Michael Hankinson* Asya Magazinnik† Melissa Sands‡

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Abstract

While scholars have documented feedback effects among a policy's direct winners and losers, less is known about whether such effects can occur among the indirectly affected — "the policy adjacent." Using 458 geocoded housing developments built between two nearly identical statewide ballot propositions funding affordable housing in California, we show that policy generates feedback effects among neighboring residents in systematic ways. New, nearby affordable housing causes majority-homeowner blocks to increase their support for the housing bond, while majority-renter blocks decrease their support. We attribute the positive effect among homeowners to the housing's replacement of blight and improvement of property values. The negative effect among renters is driven by gentrifying neighborhoods. Not receiving an affordable housing unit despite their likely eligibility, these renters may attribute the new development to further increasing the rising rents around them. In turn, policy implementation can undermine support for expansion even among the policy's intended beneficiaries.

Keywords: policy feedback, local political economy, housing, context effects.

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*Assistant Professor, Department of Political Science, GWU. `hankinson@gwu.edu`

†Assistant Professor, Department of Political Science, MIT. `asyam@mit.edu`

‡Assistant Professor of Politics and Data Science, LSE. `m.sands@lse.ac.uk`

Introduction

Public policies create winners and losers. This can generate policy feedback loops: the economic and social impacts of a policy may shape the political behavior of those who experience its costs or benefits, in turn reinforcing or undermining its political fortunes (Pierson 1993). Yet the policy ecosystem is composed of not just direct beneficiaries, but also those whose circumstances are shaped by externalities. Even policies with very specific and localized beneficiaries can generate broader social and economic ripple effects through, for example, shared markets. In this paper we show that such externalities can create policy feedback loops among those only indirectly affected, potentially undermining support among a policy's intended beneficiaries.

We consider the case of the Low-Income Housing Tax Credit (LIHTC, pronounced "lie-tech"), which has accounted for the construction or rehabilitation of over 2 million affordable housing units nationwide since its inception in 1986. We show that voters change their political behavior in systematic ways when new LIHTC developments are built in their neighborhoods. Our focus is on the case of California, which provides a unique laboratory for this study because nearly identical bond measures appeared on state ballots in November 2002 and November 2006, each allocating over \$2 billion for affordable housing. Using fine-grained election data and geolocated data on 458 LIHTC developments within a difference-in-differences framework over the period 2002-2006, we find that the construction of nearby affordable housing causes support for funding affordable housing to *increase* by 2 to 3 percentage points in Census blocks that comprise mostly homeowners. At the same time, it causes a 1 to 2 percentage point *decrease* in support in blocks that comprise mostly renters.

Given that voters respond to changes in local housing markets (Larsen et al. 2019), we argue that these divergent effects are driven by the well-established positive impact of LIHTC developments on property values in distressed neighborhoods (Dillman, Horn, and Verrilli 2017; Voith et al. 2022) combined with the tendency for LIHTC housing to be built in systematically disadvantaged areas.[1] The positive feedback effects among homeowners are likely driven by increases in nearby property values due to the replacement of "blight" (e.g., dilapidated structures, vacant housing, and empty

[1] At least 10 published articles have studied the impact of LIHTC developments on local property values. Together they demonstrate unequivocally that LIHTC increases property values in low-income or distressed neighborhoods in the development's immediate vicinity. See Dillman, Horn, and Verrilli (2017) for a review.

lots). Consistent with this, we find little evidence of positive effects on bond support in wealthier, majority white homeowner neighborhoods. Among renters, the decrease in support for the housing bonds is driven by neighborhoods already in the process of gentrification. Even though affordable housing is reserved for low-income residents, current renters may view physical investment in the neighborhood as a force accelerating their displacement. Without a guarantee that market-rate renters will receive an affordable housing unit, program expansion poses a threat to their housing stability.

We integrate these findings into the broader policy feedback literature using a new theoretical concept: *the policy adjacent*. This category encompasses the individuals who are affected by the spillovers of the policy. Depending on their internal calculus, the policy adjacent may be "winners" or "losers." The homeowners in our study context are *the policy adjacent winners* — indirect beneficiaries who become more supportive of the policy. Homeowners increase their support for affordable housing once they experience the positive localized externalities of new LIHTC developments, making them, potentially, an unexpected ally in building support for policy expansion. In contrast, renters are *the policy adjacent losers* — individuals who, despite generally fitting the profile of those whom the policy is intended to help, do not receive assistance (due in this case to limited public funding) and are instead left vulnerable to its negative externalities. We observe a political backlash from these policy adjacent losers. These individuals, who are typically assumed to be a natural constituency for expanding housing assistance, may in fact undermine attempts to build a broad-based renter coalition around policy expansion because they receive no direct benefits but suffer negative externalities. After presenting our results, we extend this framework to other well-known social policies.

Beyond our theory of the policy adjacent, we are also able to address two major empirical challenges that studies of policy feedback typically face. First, it is usually hard to identify policy winners and losers because individual-level data on eligibility, policy uptake, and usage are typically restricted to protect privacy. By their nature, the externalities of new LIHTC developments are spatially concentrated in the neighborhoods in which they are sited. This allows us to easily identify voters "treated" by these externalities, be they winners (homeowners) or losers (renters). Second, prior studies of policy feedback have typically relied on self-reported attitudinal outcomes (e.g., external efficacy) or behavioral outcomes somewhat removed from the policy of interest (e.g., turnout). As (Campbell 2012, p. 347) writes, "[m]any existing feedback studies show the feed but not the back (or they just assume the back). Such studies show that policies affect the public in some way, altering attitudes or behaviors. But [...they] do not demonstrate that those attitudinal or behavioral patterns owing to program design affect subsequent policy outcomes." California's statewide housing bonds represent meaningful, policy-relevant pre-treatment and post-treatment behavioral outcomes: Votes dictating public funding for housing assistance. That the siting of just one LIHTC development nearby affects support for a $2+ billion bond suggests an array of other possible attitudinal and behavioral outcomes among the policy adjacent that have yet to be investigated.

Policy Feedback and the Low-Income Housing Tax Credit

Affordable housing in the United States is allocated via a complex array of programs at the federal, state, and local levels. The Low-Income Housing Tax Credit (LIHTC), first enacted in 1986, subsidizes the construction and rehabilitation of affordable rental housing for low- and moderate-income tenants. Although federal guidelines allow for a mix of low- and non-low-income housing in a given development, in practice the overwhelming majority of units are earmarked for low-income residents. LIHTC credits were used to fund 90% of new project-based affordable housing units and 21% of all new multifamily housing units nationwide from 1987 to 2008, providing millions of affordable units (Diamond and McQuade 2019).

Though LIHTC is a federal program, it is much less obvious to the general public than many public works projects (Pfeiffer 2009), and its impact is highly localized. Built by private developers, primarily funded through the tax system rather than more visible public budgets, and typically smaller in physical scale than the massive public housing projects of the 1960s and 1970s, LIHTC developments tend not to be immediately recognizable as affordable housing to casual observers. They also are commonly sited in "less desirable" locations where they are unlikely to meet strong "Not In My Backyard" (NIMBY)-style opposition. Indeed, Pfeiffer (2009) documents how in Southern California, between 2000 and 2005, LIHTC units targeted to families tended to be located in high poverty, heavily Latinx neighborhoods. LIHTC developments often fly under the radar of the general public, and any policy feedback effects of LIHTC are thus likely to be geographically concentrated around developments.

LIHTC's Winners and Losers

Like all public policies, LIHTC creates winners and losers. Some of these are direct "winners" — the relatively few people who are allocated affordable units within the LIHTC housing — and direct "losers" — those taxpayers who would prefer not to fund affordable housing. But affordable housing has impacts on the local market for both owned and rented homes, and so LIHTC also creates economic and social externalities in the neighborhoods in which new developments are built. Most residents living near a LIHTC development are thus *indirect* policy winners and losers; the policy's externalities affect their material self interest in the short or medium term (i.e., Sears and Funk (1991)). Indeed, self-interest is the primary driver of decision making in neighborhood-level politics, particularly in the context of housing development and housing costs, and so we expect these externalities to shape political behavior (Einstein, Glick, and Palmer 2020; Marble and Nall 2021). Likewise, because local housing policy lacks a strong partisan lens, we expect the personal experience of exposure to nearby affordable housing to be even more influential than policies with more polarized politics (Lerman and McCabe 2017).

LIHTC's Visibility and Traceability

Policy feedback can only occur when the effects of the policy are visible and traceable to the policy itself (Campbell 2012; Patashnik and Zelizer 2013). This is as true of indirect effects as it is of direct effects, raising two questions. First, do nearby residents know that LIHTC-funded developments are publicly funded affordable housing? The answer is likely yes. Affordable housing developments are almost always subject to public hearings through the process of discretionary review, and developers are typically required to notify residents within 300 feet of the development site that the hearing is occurring. At these hearings, the local planning commission — a city council-appointed legislative body — solicits feedback from neighboring residents, offers suggestions, and ultimately votes on whether to approve the new development. These meetings, as well as literature posted on the building site, almost always highlight the affordable nature of the development.

Second, do residents connect changes in property values to new nearby LIHTC developments? Prior research suggests that again the answer is yes. Residents are typically attuned to issues in their local communities, especially those concerning residential development. This is unsurprising — for many homeowners, their home is not only their largest asset, but highly illiquid, making them particularly wary of threats to its value (Fischel 2001). While renters are often viewed as less engaged in their neighborhood, "defensiveness" similar to that of homeowners has been found among renters in gentrifying communities, who fear that new development will increase surrounding rents and thus their housing instability (Hankinson 2018).

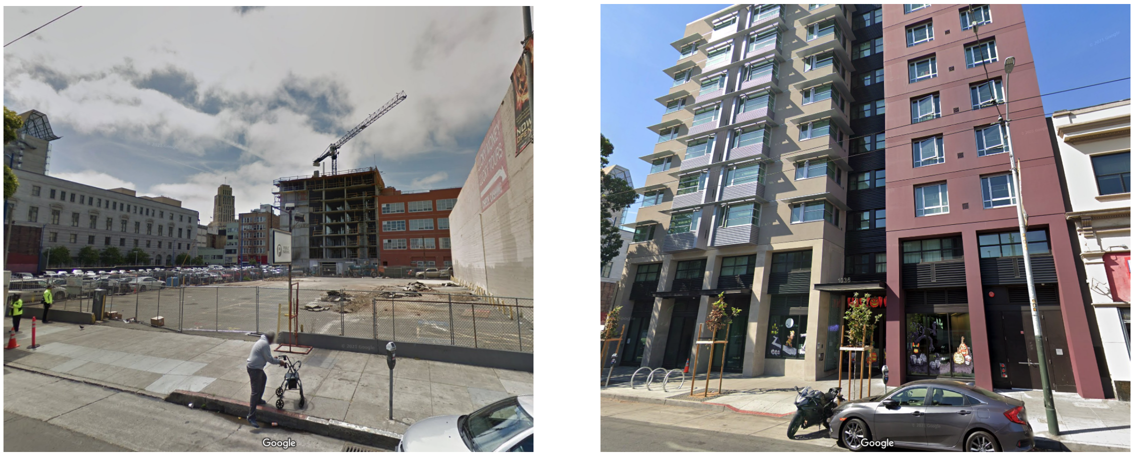
LIHTC's Policy Adjacent

Residents living near affordable housing are the policy adjacent: those subject to the externalities of LIHTC development on the housing market around them. This group comprises both homeowners and renters. Given the types of communities usually targeted for LIHTC developments, these people, whether homeowners or renters, are typically lower-income and likely lack substantial accumulated wealth outside of the equity in their homes. It is these renters that are LIHTC's policy adjacent losers — though likely eligible for housing assistance, it is very unlikely they receive one of the newly constructed, affordable housing units.[2] Instead these renters are left to experience the externalities of the new LIHTC development on the local housing market. Homeowners account for the policy adjacent winners. While homeownership provides more housing stability than renting, and homeowners in our study have, on average, higher household incomes than renters, they still live in distressed or gentrifying areas and are by no means wealthy.

The effect of LIHTC development on local housing prices is expected to affect these two groups — homeowners and renters — differently. Diamond and McQuade (2019) find that LIHTC development increases property values by replacing blight in economically depressed neighborhoods (e.g., Figure 1). If that is the case in California from 2002 to 2006, we expect homeowners treated by LIHTC to become *more* supportive of statewide affordable housing bonds. Meanwhile, if rising property values are perceived as driving gentrification and contributing to housing instability, renters should be *less* supportive of program expansion. If program expansion were to guarantee that they would receive rent subsidization — which it cannot — then these renters may become more supportive. It is worth noting that other processes may be at play here too. For example,

[2] Even though individual LIHTC developments prioritize local residents in the lottery for units, "local" is defined at the city or even metropolitan level.

LIHTC developments may affect quality of life in the neighborhood (e.g. crime rates or school quality), or may signal demographic change, triggering racial threat. We return to these alternative pathways in the Conclusion.



(a) Prior to LIHTC housing (2016) (b) With LIHTC housing (2019)

Figure 1: 1036 Mission Street, San Francisco, CA captured via Google Street View

Data

To test our theory of the policy adjacent in the case of LIHTC housing, we use the presence of two similar affordable housing bonds placed on the California statewide general election ballot in 2002 and 2006. The foundation of the dataset is the California election data, dissolved and aggregated to the 2010 Census block-level, which is our unit of analysis throughout (McCue 2011).[3] Using a series of cross-walks and spatial joins, we construct a dataset of 15,769 unique blocks. For each block we measure voting returns, treatment status, and a bevy of block- and tract-level characteristics, described in detail below.

These 15,769 blocks are far fewer than the roughly 710,000 census blocks in California, but

[3]The voting precinct is the lowest level of aggregation at which vote choice can be directly measured, due to the secret ballot. Thus, block-level vote returns represent an estimate and contain some degree of measurement error. McCue (2011) describes the process of estimating local returns by combining census data, voter files, and election data. We have no reason to believe measurement error introduced by this approach is correlated with treatment assignment or biases our results in any way. First, given we calculate the difference-in-differences, any such confounding would need to affect the trend in treatment or control units from year to year, which seems implausible. Moreover, this confounding would also plague our placebo test using 2010-2017 LIHTC developments. Our placebo test produces a statistical and substantive null effect, bolstering our confidence that the disaggregation of voting returns to the block group level does not introduce problematic bias.

contain 7% of the population of California as of 2000.[4] Of course, the residents living in these blocks are not representative of California as a whole. They are poorer, more racially diverse, and less likely to be homeowners than the average Californian. We return to the generalizability of our findings beyond this sample in the Conclusion.

Key Variables

Dependent Variable Our primary outcome of interest is the change in support between two $2+ billion housing bonds placed on statewide ballots in California, the first in November 2002 (Proposition 46)[5] and the second in November 2006 (Proposition 1C)[6]. Both bonds were placed on the ballot by the Housing and Emergency Shelter Trust Fund Acts of 2002 and 2006, and were very similar in content and wording. In 2002, a $2.1 billion bond was to provide:

> shelters for battered women; clean and safe housing for low-income senior citizens; emergency shelters for homeless families with children; housing with social services for homeless and mentally ill; repairs/accessibility improvements to apartments for families and handicapped citizens; military veteran homeownership assistance; and security improvements/repairs to existing emergency shelters.

And in 2006, another $2.85 billion bond was to provide:

> shelters for battered women and their children, clean and safe housing for low-income senior citizens; homeownership assistance for the disabled, military veterans, and working families; and repairs and accessibility improvements to apartments for families and disabled citizens.

The 2002 bond passed with 57.6% of the vote, the 2006 bond with 57.8% of the vote. We obtain precinct-level returns for the 2002 and 2006 ballot initiatives from the California Secretary of State online repository.[7] As stated, the Secretary of State's office disaggregated these precinct-level returns to the Census block-level to aid redistricting in 2011. We define our dependent variable as the block-level change in support for bonds from 2002 to 2006.[8]

[4] We are able to create this data for the more than 700,000 census blocks in California, but the features of our various research designs effectively limit the sample to 15,769 blocks – those that are within 600m of a 2003-2010 LIHTC development.

[5] `https://repository.uchastings.edu/cgi/viewcontent.cgi?article=2203&context=ca_ballot_props`

[6] `https://repository.uchastings.edu/cgi/viewcontent.cgi?referer=https://www.google.com/&httpsredir=1&article=2260&context=ca_ballot_props`

[7] `https://www.sos.ca.gov/elections/prior-elections/statewide-election-results`

[8] See Section A.1 for more detail.

LIHTC Developments We are interested in the presence of new LIHTC-funded affordable housing developments during the four years between the 2002 and 2006 elections. LIHTC credits can be used both to construct new affordable housing and to rehabilitate existing structures as affordable housing. We only use projects that were new constructions and drop those dedicated towards building rehabilitation.[9]

We obtain the location and number of units for every LIHTC-funded project from the HUD National Low Income Housing Tax Credit Database, which includes information about when credits were allocated as well as when the buildings they funded were placed into service. We focus on the 458 LIHTC developments placed into service in California between 2003 to 2006.[10] The median dwelling had 79 units, of which 72 (92% of units) were reserved for low-income residents.[11] Along with new development between the two elections, we also geocode LIHTC developments built from 1999 to 2002 and developments placed into service from 2007 to 2010, which are deployed in various analyses.

Homeownership and Rentership We rely on block-level homeownership data from the 2000 Census to establish which of our blocks are the policy adjacent winners and losers. We categorize blocks based on the tercile cutpoints of homeownership among all unique blocks. Blocks in the lowest tercile of homeownership are the policy adjacent losers, while blocks in the highest tercile of homeownership are the policy adjacent winners. We exclude blocks falling into the middle tercile, thus comparing high "homeowner blocks" to high "renter blocks." In our data, "homeowner blocks" are those with homeownership rates of $> 67\%$ and "renter blocks" are those with homeownership rates of $< 21\%$ as of 2000.[12]

We also calculate other candidate moderators at the block level, focusing on variables believed to shape local housing politics, including race and ethnicity, vacancy rates, and population density.[13] All of these Census variables are measured as of 2000, ensuring that they are pre-treatment, and

[9] 72% of the California LIHTC projects in our treatment window were new construction.

[10] We do not have month-level data on when developments were placed in service. Thus, we are possibly excluding usable LIHTC developments from late-November and December 2002 as well as inappropriately including developments from late-November and December 2006. Either error would likely only add noise to our estimates, not bias.

[11] See Section A.2 for additional detail on this measure.

[12] We further discuss this decision and present our results across a range of homeownership cutpoints in Section D.

[13] Note that income-related variables are not available at the Census block level. We discuss this trade-off of available data in Section A.3.

are coded using the same tercile approach described above.

Research Designs

We identify the causal effect of LIHTC development on the policy adjacent using two analytical strategies. Both are built on the foundation of a difference-in-differences design where the first of these differences is the change in voter support for the housing bond between 2002 and 2006. We term these two designs the near-far and the near-near design.[14]

Near-Far Design

The near-far design is built on the assumption that blocks closer to each other are more comparable than blocks farther away from each other. Accordingly, we compare blocks that are treated by new LIHTC developments to nearby blocks that are just too far to be affected. The radius at which the effect of LIHTC on voting behavior appears to fully decay is 350 meters. We therefore define as control blocks those that are between 350 meters and 600 meters away from the LIHTC development.[15] Figure 2 visualizes the near-far design in San Francisco, CA. Blocks outlined in red are treated and blocks outlined in black are control.

We subset the data to high-homeownership blocks ("homeowner blocks") and low-homeownership blocks ("renter blocks") defined as described above. Section B.1 presents the pre-treatment covariate balance between treated and control units within each tercile as well as the sample as a whole. For both homeowner and renter blocks, treated units are comparable to control units on observable block-level covariates measured prior to LIHTC construction. This suggests that within the 600 meter radius the exact location of the LIHTC development is plausibly exogenous to some common (observed) confounders. To help account for unobserved confounders, we include LIHTC-level fixed effects, accounting for confounders that do not vary by development.

[14] This approach is inspired by the similar designs used by Asquith, Mast, and Reed (2021).

[15] Defining the extremity of how far control blocks can be from the LIHTC development faces another trade-off. Using blocks too far from the LIHTC development as controls introduces imbalance on covariates. But restricting controls to blocks too near the LIHTC development risks having too few control observations.

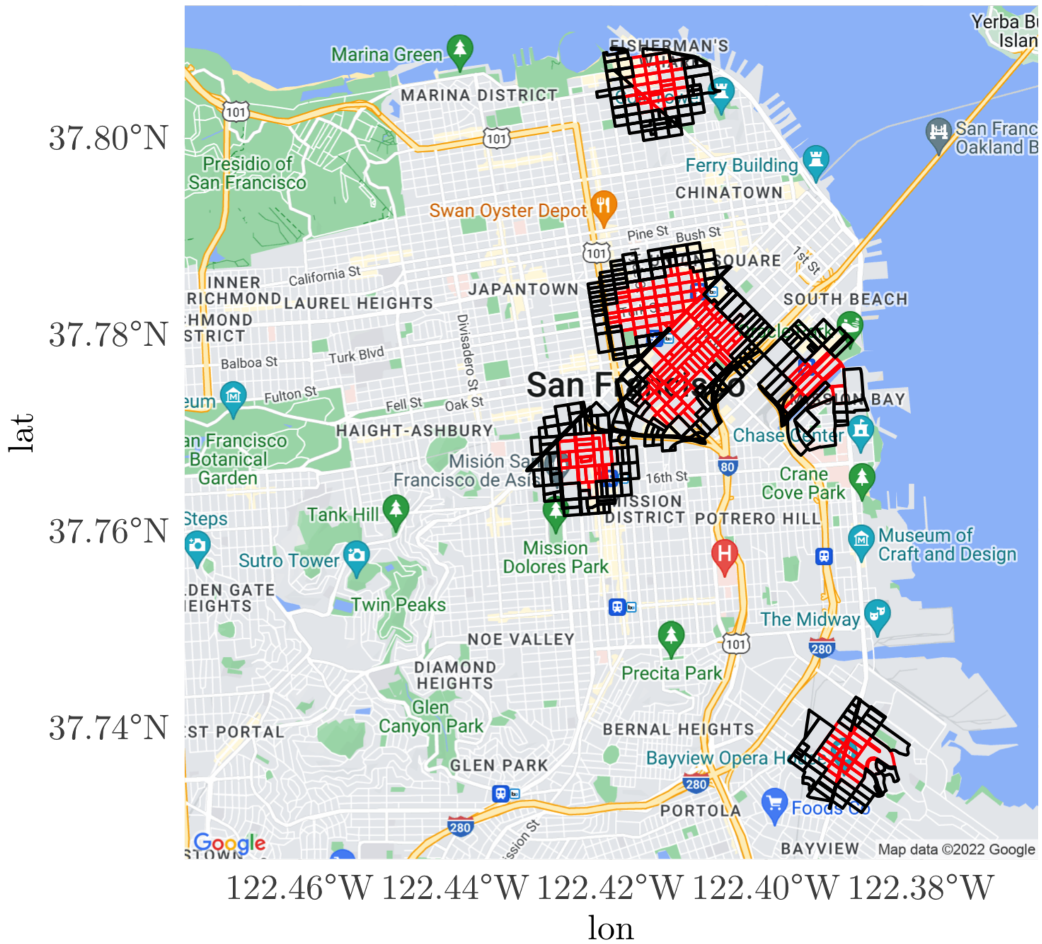


Figure 2: Visualization of the near-far design in San Francisco, CA. Treated blocks — those that are near LIHTC developments built in 2002-2006 — are drawn in red, control blocks — those that are just farther from LIHTC developments built in 2002-2006 — are drawn in black.

Near-Near Design

Though our near-far design shows balance on observable characteristics, unobserved confounders remain a risk. For example, it could be that LIHTC developers target very specific locations where new housing is politically feasible to permit. These areas may differ politically in unobserved ways while also being more amenable (or vulnerable) to LIHTC development, thus confounding our causal estimates.

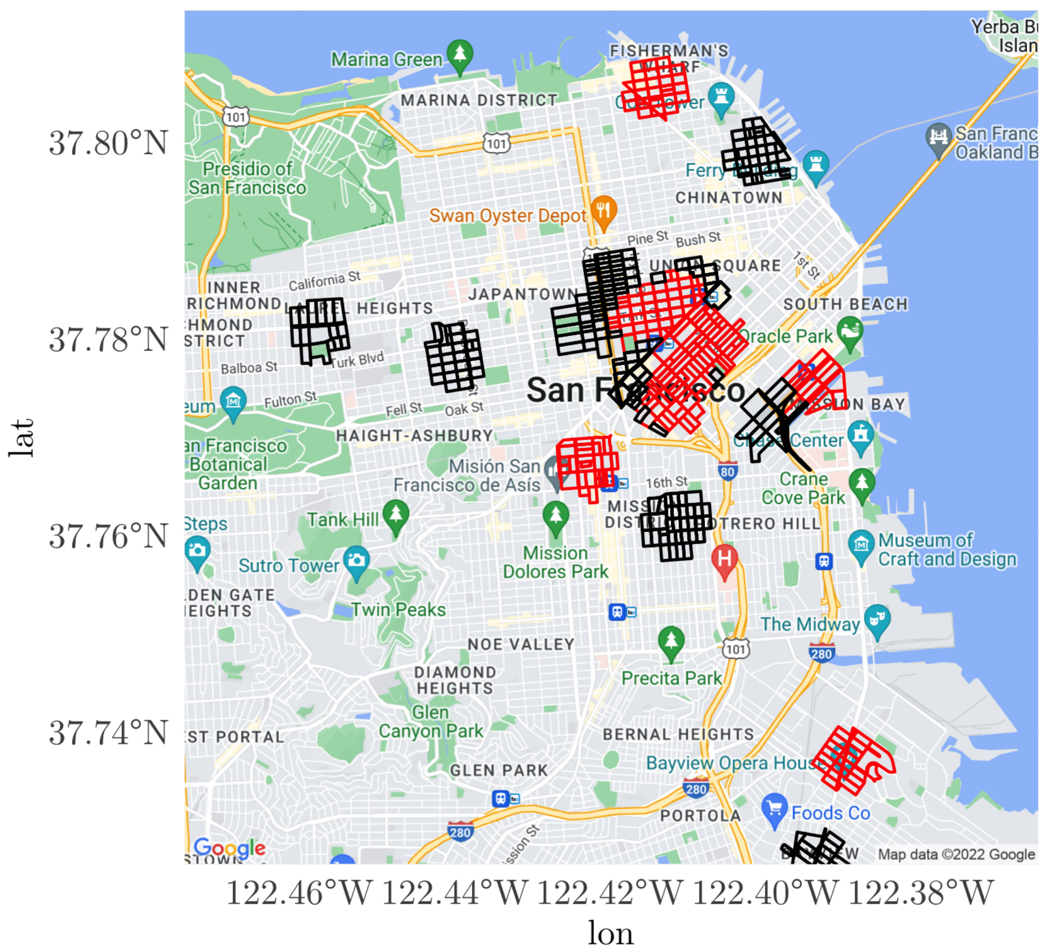


Figure 3: Visualization of near-near design in San Francisco, CA. Treated blocks — those that receive LIHTC developments in 2002-2006 — are drawn in red, and control blocks — those that receive LIHTC developments in 2007-2010 — are drawn in black.

Our near-near design accounts for this possibility by comparing areas treated by LIHTC developments between 2002 and 2006 to areas that *will be treated* by new LIHTC between 2007 and

2010. By defining these later-treated units as controls, we compare areas that are similarly prone to new LIHTC development, but with different treatment timings. The primary risk to this near-near design would be if the timing of a LIHTC development was driven by neighborhood-level political factors correlated with our dependent variable. We believe this risk to be small. The funding process in California is highly competitive, with roughly 27-58% of developer applications funded between 2007 and 2012 (Diamond and McQuade 2019). The high rejection rate of applications for LIHTC funding each year suggests a large degree of variation in when a project is funded that is unrelated to the preferences of nearby residents.

Figure 3 visualizes the near-near strategy, with treated blocks outlined in red and control blocks outlined in black. We can no longer include a LIHTC-level fixed effect in our specification, and instead include CBSA-level fixed effects, comparing treated and control blocks that are within the same narrowly defined metropolitan area. Section B.2 shows the weighted mean difference in our block-level covariates within homeownership terciles and the sample as a whole. As in the near-far design, our treated and control units are comparable on observable pre-treatment covariates within both the homeowner tercile and the renter tercile.

Estimation

For both designs we estimate the effect of nearby LIHTC developments on support for affordable housing using ordinary least squares, with robust standard errors clustered at the LIHTC level. Our theory of the policy adjacent predicts divergent effects among homeowners and renters, so we include an interaction term between treatment and homeownership.

Each regression thus produces two point estimates: a treatment effect estimate that gives the relationship for homeowner blocks, and the sum of the main effect and interaction term, giving the relationship for renter blocks. Our designs together generate four specifications, depending on how treatment is measured, captured below:

1. **Binary**: Treatment is binary based only on spatial proximity. Treatment takes 1 if a block is $\leq$350 meters away from the LIHTC development, 0 if 350 to 600 meters away in the near-far analysis or 0 if within a 350m radius of a LIHTC development placed in service from 2007-2010. The renter variable is binary (1 = low homeownership tercile, 0 = high homeownership

tercile). The interaction term is thus binary (binary times binary).

2. **Proximity (cont.)**: Treatment is a continuous variable based only on spatial proximity. We calculate proximity based on the distance between each block centroid and the relevant LIHTC development, with 1 signifying the minimum distance away from the LIHTC development and 0 the maximum distance. Values of the treatment variable are standardized by subtracting the mean value and dividing by 2 standard deviations (e.g., Gelman 2008). The renter variable is binary (1 = low homeownership tercile, 0 = high homeownership tercile). The interaction term is thus continuous (binary times continuous). (Note that this specification cannot be used for the near-near design because the control units are not spatially proximate to their comparable treated units — although they share the same CBSA.)

3. **Units (cont.)**: Treatment is a continuous variable based on the number of units to which a block is cumulatively exposed (where exposure is based on the binary definition of spatial proximity). Values of the treatment variable are standardized by subtracting the mean value and dividing by 2 standard deviations. The renter variable is binary (1 = low homeownership tercile, 0 = high homeownership tercile). The interaction term is thus continuous (binary times continuous).

4. **Continuous Rentership**: The treatment is a binary variable based only on spatial proximity ("Binary"). The renter variable is continuous, based on the percent of households in a block that are renters, ranging from 0 for 0% renters to 1 for 100% renters. The interaction term is thus continuous (binary times continuous).

For each specification, we re-estimate our results using the four datasets outlined below.

1. **All**: The full sample of blocks that meet each design's criteria.

2. **Clean**: Because new LIHTC developments are constantly being built, some blocks may have been treated by a new LIHTC development just prior to the 2002 election. Exposure to LIHTC developments just prior to our period of study may mean that voters were already treated when voting on the "pre-treatment" housing bond, or were desensitized to the effects of new LIHTC developments. This could add both bias and noise to our estimates, so in the second sample we exclude all such cases.

3. **Big**: The impact of LIHTC housing may be larger for developments of substantial size, which are likely to be more noticeable and have bigger implications for the local housing market and neighborhood conditions. The third sample is thus restricted to LIHTC developments that are larger than the median LIHTC development in our sample, ≥80 units.

4. **Big Clean**: The fourth sample is the intersection of "Big" and "Clean."

Main Results: Changes in Policy Support

Near-Far Design

Our parametric models test the stability of these estimates when comparing blocks surrounding the same LIHTC development using a LIHTC fixed effect.[16] Figure 4 shows the effect of proximity to a LIHTC development on the change in support for the housing bonds across the four specifications described earlier. Starting with our full set of blocks ("All"), the presence of nearby, new affordable housing causes voters in homeowner blocks to increase their support for funding affordable housing by 1.9 to 2.3 percentage points. For a sense of scale, this is an approximately 0.1 standard deviation increase in the voter-weighted average change in support for the housing bonds. Importantly, this effect is positive, meaning homeowners — who as a group are traditionally more averse to affordable housing than renters — increase their support for funding affordable housing once exposed to its implementation. The data on predominantly renter blocks tell a different story: a new, nearby LIHTC development causes these voters to decrease their support by 0.8 to 1.5 percentage points.

Moving across Figure 4, the effect of LIHTC is robust across each subset of data. Removing blocks which had been treated in the lead-up to the 2002 election yields nearly identical estimates ("Clean"). Subsetting to only developments with ≥80 units increases the size of the estimated treatment effect among renter blocks ("Big"). The combination of these two restrictions yields the largest effects, with treated renter blocks decreasing support for affordable housing bonds by 1.3 to 2.5 percentage points on average ("Big Clean"). Across all specifications, the estimated causal effect of LIHTC among homeowner blocks remains stable between a 2.0 to 2.5 percentage point increase in support for the housing bonds.

[16] See Section E.4 for the nonparametric difference in means analysis.

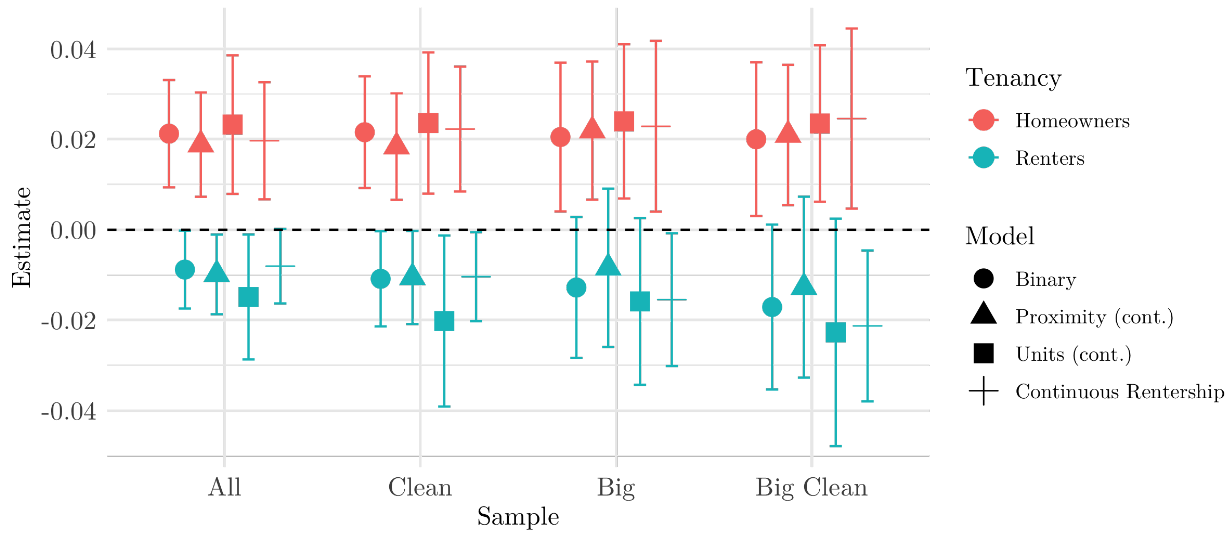


Figure 4: Results from the near-far design.

Near-Near Design

Our parametric model tests the stability of these estimates when comparing treated and control blocks within the same metropolitan area using a CBSA fixed effect. Figure 5 shows estimated treatment effects consistent with not only the nonparametric estimates, but also the near-far design (Figure 4). On average, a new, nearby LIHTC development causes homeowner blocks to increase their support for the housing bonds by 1.8 to 2.5 percentage points. In contrast, renter blocks experience a 1.3 to 2.2 percentage point decrease in bond support, matching the nonparametric estimate.

Moving across Figure 5, the treatment effects among homeowners and renters grow in magnitude. Large LIHTC developments have larger estimated effects, with homeowner blocks increasing bond support from 1.8 to 3.5 percentage points and renter blocks decreasing bond support by 1.5 to 2.5 percentage points.

Validity

To probe the validity of our designs, we conduct a placebo test replicating the near-far design using LIHTC developments built between 2007 and 2010. Because these LIHTC developments were opened after the 2006 election, there should be no difference in change in support for the housing bonds from 2002 to 2006 between the blocks "near" and "far" from them. Figure E-11

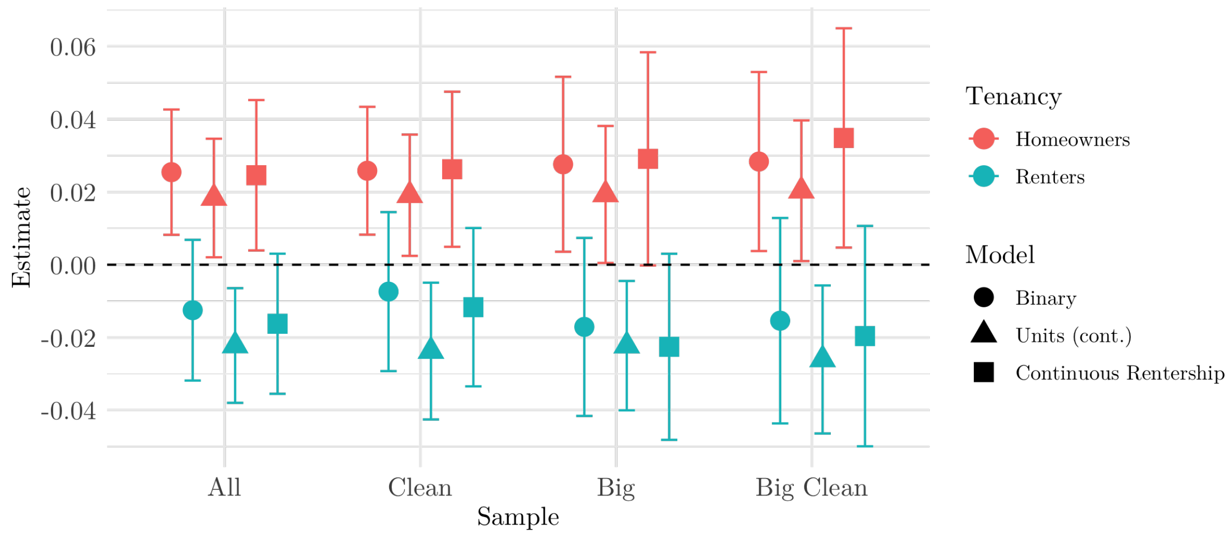


Figure 5: Results from the near-near design.

shows this to be true. Homeowner and renter blocks treated between 2007 and 2010 show a pattern of consistently null results, both in magnitude and statistical significance.

To probe our theoretical claim that homeownership is the primary cleavage driving response to new, nearby LIHTC development, we repeat our near-far and near-near designs on our other block-level covariates: racial demographics (% non-Hispanic white, % Black, % Latinx), vacancy rate, and population density (Figures E-12 and E-13). While we occasionally find statistically significant differences between blocks with high and low values of these variables, homeownership is by far the most powerful and stable moderator.

We conduct additional sensitivity analyses to see how our results respond to different distance bands and homeowner and renter cutpoints. Appendix C replicates our near-far and near-near designs with the binary treatment, moving the distance radius defining treated units from 300 meters to 425 meters in 25-meter increments. Results are consistent but grow noisier as the treatment radius increases. This is to be expected. Once the treatment radius expands beyond a LIHTC development's sphere of influence, the "treated" group will begin to include blocks which should be categorized as control units, biasing downward the estimated effects.

We also repeat our analyses on the 350 meter distance band specification but moving the cutpoints that define our homeowner and renter blocks. Finding the right cutpoints requires balancing two competing considerations: extreme cutpoints may leave insufficient data to estimate an effect, while moderate ones risk diluting the conditional effect we are trying to estimate. Appendix D shows that our results are stable across cutpoints 5 percentage points higher or lower than the tercile cutpoints produced by the distribution of treated blocks. Even cutpoints 10 percentage points above or below the tercile-defined settings yield substantively similar results.

An additional challenge comes from residential churn. If new LIHTC developments cause those least tolerant of affordable housing to move away, being replaced by more tolerant residents, then the positive effect of LIHTC on support for the housing bonds may be an artifact of replacement rather than behavioral change. We assess this possibility by computing the number of block-level housing transactions between the two elections using ZTRAX data. We then estimate the same models as before, replacing the dependent variable — previously the change in support for housing bonds — with the change in residential churn from 1999-2002 compared to 2003-2006.

Tables E-11 and E-13 in the Appendix show the effect of a LIHTC development on the change in block-level transactions using the binary treatment with our near-far and near-near designs, respectively. We find negative (less churn) or null results but not statistically significant estimates among homeowners, suggesting that our positive homeowner effect is unlikely to be driven by replacement. Renter blocks show similar null effects.

To further investigate alternative mechanisms, we explore whether LIHTC developments affected individual turnout (using the 2007 California Voters Roll) and ballot roll-off. We find little consistent evidence that either of these dependent variables is affected by LIHTC development.[17]

Evidence that Housing Prices Play a Key Role

Our theory posits that changes in housing prices — both property values and rental prices — drive divergent behavior among homeowners and renters. The first part of this chain is relatively uncontroversial: there is ample well-established evidence that LIHTC developments do improve housing prices on average. Here we present some suggestive evidence that this dynamic drives our observed behavioral changes.

[17]See Section F for more detail.

Wealthy White Homeowner Neighborhoods Show Smaller Positive Effects

While LIHTC developments typically increase average home prices, Diamond and McQuade (2019) find an important exception: LIHTC developments lead to an average 2 percent decrease in home prices in relatively wealthier and whiter areas — defined as neighborhoods that are majority non-Hispanic white and have median household incomes >$54,000 as of 2012. Strikingly, this effect is not found in majority non-white neighborhoods of similar levels of wealth. While the exact mechanism accounting for this effect is unclear, we use this to devise a test of our theory: Do well-off, majority white neighborhoods show a differential response to affordable housing compared to similarly wealthy non-white neighborhoods?

We interact our treatment measure with an indicator for whether the neighborhood is predominantly non-Hispanic white. We restrict the sample to blocks that fall within the top tercile of household median income (>$45,116 in 2000 dollars) to focus on blocks that are relatively wealthy.[18] Within this subset of wealthy blocks, we use the tercile cutpoints of percent non-Hispanic white to define "white" and "non-white" blocks, dropping the middle tercile. The dummy variable for racial composition takes the value of 1 when blocks are $\geq$69% non-Hispanic white and 0 when blocks are $\leq$33% non-Hispanic white. Both sets of blocks includes similarly high-income, homeowner blocks. But to account for any reliance on cutpoints, our "Continuous White" model interacts treatment with a continuous measure of percent non-Hispanic white.

As Figures 6 and 7 show, among wealthy areas, it is the predominantly non-white blocks that drive the positive effects of LIHTC construction on homeowner support for affordable housing bonds, while support in white blocks does not change. Large positive effects of similar magnitude are found among non-white areas using both the near-far and near-near designs, while majority white blocks show consistently small and statistically insignificant effects. This evidence suggests that where LIHTC developments are less likely to improve property values, so too is there less behavioral change among homeowners.

[18] There is nothing inherently special about the Diamond and McQuade (2019) cutpoint of $54,000 in 2012 dollars in defining "high-income" areas. The cutpoint seems to come from their top quartile of median block-group income (See Appendix Table A1 in Diamond and McQuade (2019). Hence, we subset to high-income neighborhoods based on tercile cutpoints in our data. Even then, our "high-income" neighborhoods are generally middle-income in the distribution of California as a whole.

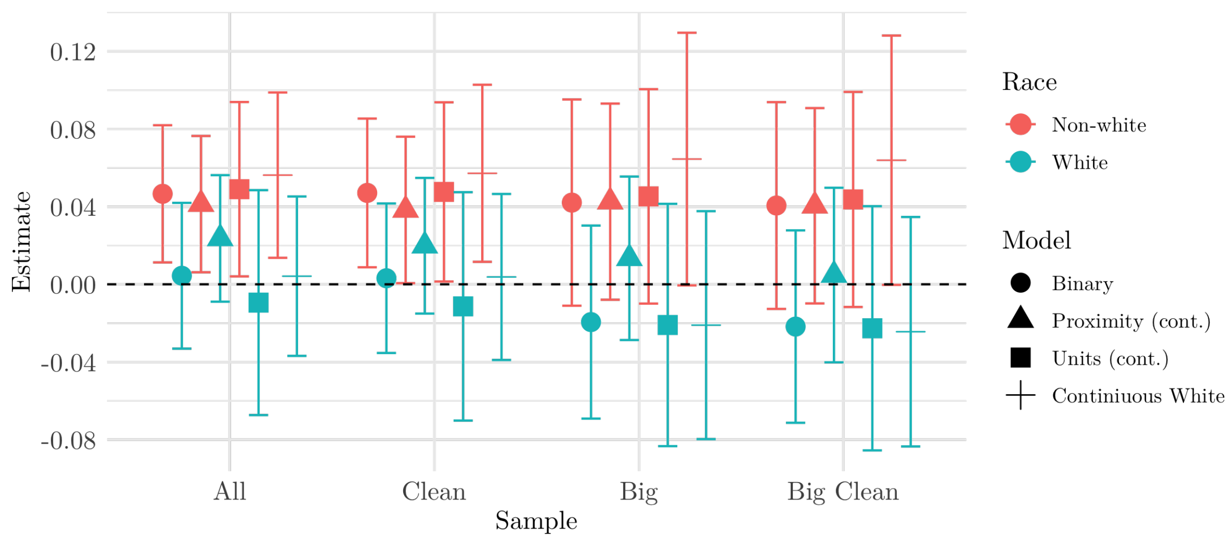


Figure 6: Results from near-far design for homeowners and race analysis. Positive effects of LIHTC construction on homeowner support for affordable housing bonds is predominantly driven by blocks that are $\geq 69\%$ non-Hispanic white.

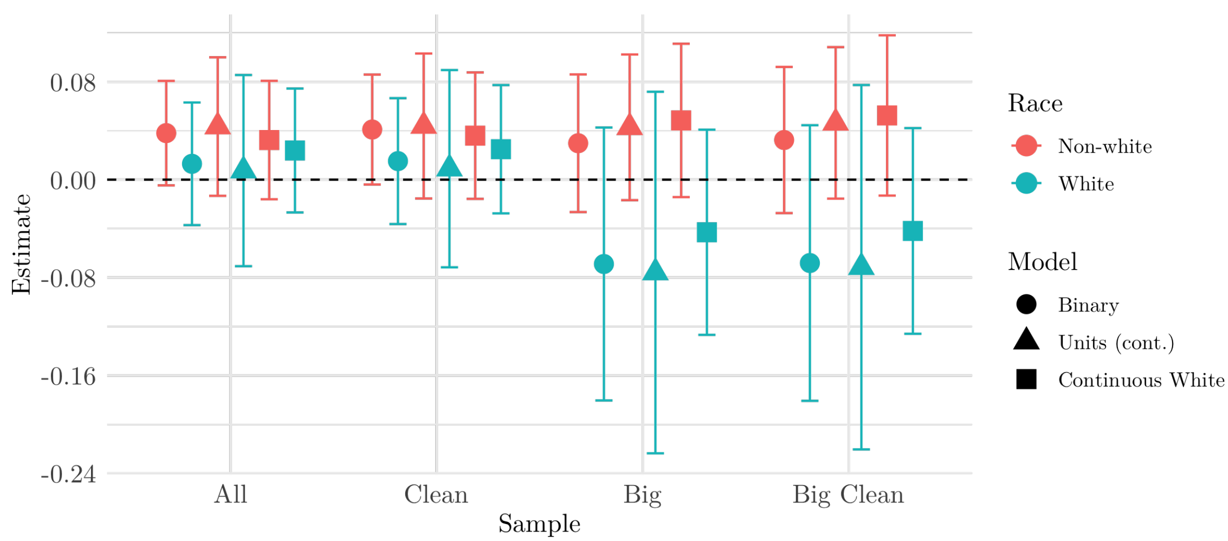


Figure 7: Results from near-near design for homeowners and race analysis. Positive effects of LIHTC construction on homeowner support for affordable housing bonds is predominantly driven by blocks that are $\geq 69\%$ non-Hispanic white.

Gentrifying Renter Neighborhoods Show Larger Negative Effects

Treated renters become more likely to oppose affordable housing bonds when they are exposed to new LIHTC developments. We argue that this is because they associate new LIHTC developments with gentrification and displacement. This is consistent with prior evidence that shows renters in cities with high housing prices typically oppose nearby market-rate development because they believe new supply will increase their housing instability (Hankinson 2018). From this we devise a second test of our theory: Do already-gentrifying renter neighborhoods, where renters are most anxious about rising prices, show a differentially negative response to new LIHTC developments?

We measure blocks which were rapidly gentrifying from 1990 to 2000, prior to the 2002 election. Following the gentrification literature, we first identify blocks deemed eligible for gentrification as of 1990. Hwang (2020) defines a Census tract as eligible for gentrification if the tract's median income in 1990 is below its city's median income.[19] Based on this definition, 83% of our renter blocks fell within gentrification eligible tracts in 1990, which is unsurprising given the nature of LIHTC developments to be sited in lower-income neighborhoods. We then identify tracts, and thus blocks, that actually underwent gentrification from 1990 to 2000. Hwang (2020) specifies two conditions, both of which must be met over a ten-year period for a tract to be considered gentrifying.

1. The tract's median rent or home value increases more quickly than the variable's median increase among all tracts in the city.
2. The tract's % with at least a BA or median household income increases more quickly than the variable's median increase among all tracts in the city.

From 1990 to 2000, 22% of our renter blocks met both criteria and qualify as gentrifying. To assess whether gentrification matters for renter response to LIHTC, we re-estimate our regressions on gentrification-eligible renter blocks only, and include a binary indicator for whether each block was gentrifying from prior to the 2002 election.

As shown in Figures 8 and 9, renter blocks in non-gentrifying tracts show essentially no response to LIHTC. In contrast, renter blocks in gentrifying tracts decreased support for the housing bond by 1.8 to 2.9 percentage points when exposed to a LIHTC development. Reliably, in both the near-far and near-near designs, gentrification status does not affect the change in support for the housing

[19]The gentrification literature typically builds their definitions based on data only available at the tract level, requiring us to assign block-level attributes based on the values of their tracts.

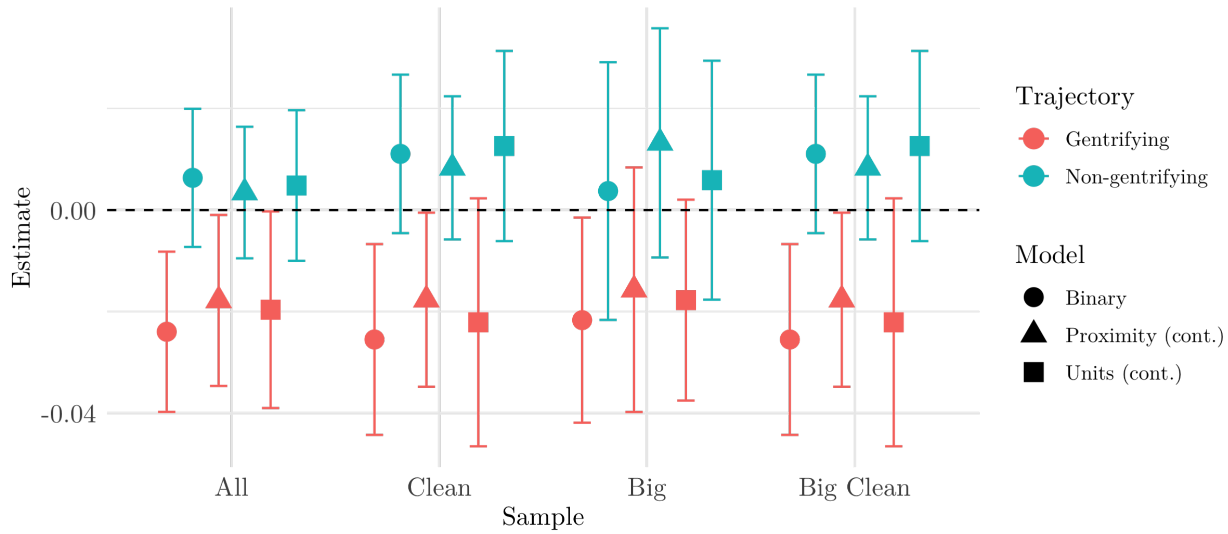


Figure 8: Results from near-far design for renters and gentrification analysis. Renter blocks in gentrifying tracts decreased support for housing bonds when exposed to a LIHTC development, while renter blocks in non-gentrifying tracts do not show statitically significant movement.

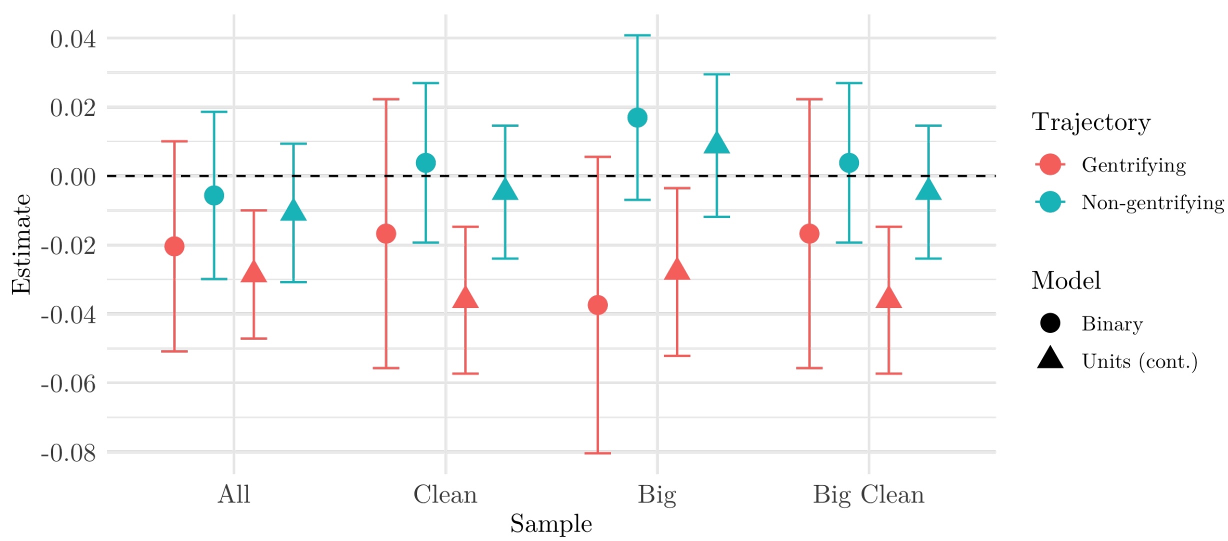


Figure 9: Results from near-near design for renters and gentrification analysis. Renter blocks in gentrifying tracts decreased support for housing bonds when exposed to a LIHTC development, while renter blocks in non-gentrifying tracts do not show statitically significant movement.

bonds among homeowner blocks (Figures F-18 and F-19). This evidence suggests that when LIHTC developments are sited in neighborhoods already experiencing rising rents and displacement, there is stronger behavioral change among renters. Together, these two tests — focusing on wealthy white homeowner neighborhoods and on gentrifying renter neighborhoods — lend further credence to our theory that LIHTC developments change voter behavior by affecting the local market for housing.

Replicating Findings Using a Nationwide Survey

Two important challenges to our findings remain: generalizability and ecological inference. To address both concerns, we use individual-level responses to a nationally representative survey of 3,019 respondents fielded in July 2016.[20] From this survey, we use the question: "Would you support a ban on the construction of new housing (homes and apartments) in your neighborhood?" Respondents answered this question using a 7-point Likert scale from "strongly oppose" to "strongly support."

According to our theory, exposure to new, nearby affordable housing should affect homeowner and renter support for a neighborhood ban on new development differently. We expect homeowners exposed to new LIHTC to see the economic benefits of new development via the increase in nearby housing prices. As a result, treated homeowners should be less supportive of a ban on nearby development compared to homeowners in the control group. Conversely, we expect renters exposed to LIHTC to be more supportive of a ban on nearby development due to concerns that this would cause nearby rents to increase.

To test our theory, we use both the near-far and near-near designs to examine the causal effect of new LIHTC development on support for a neighborhood ban on development. Because the survey was fielded in July of 2016 and we cannot be sure whether developments placed in service in 2016 treated respondents before or after the survey was fielded. As a result, we define our spatial stimuli as LIHTC development placed in service from 2012 to 2015. To merge the LIHTC data with the survey data, we geocode responses using respondents' ZIP codes and define the ZIP code's treatment status based on a similar radii approach as described for the CA data. A ZIP code is

[20]The survey was funded by Time-sharing Experiments for the Social Sciences (TESS) and was fielded by GfK.

considered treated if 50% of the ZIP code's population is within the treatment radius. To measure whether this 50% threshold is met, we geocode Census blocks within their respective ZIP codes and we identify which blocks have ≥50% of their area covered by the treatment radius. We then sum the population among treated blocks and compare it to the population of the entire ZIP code.

Given their large area, there are few ZIP codes with ≥50% of their population within 350 meters of a LIHTC development. Thus, for the near-far design, we extend the radius of the treatment group to 2,400 meters and define the control group as ZIP codes between 2,400 meters and 4,800 meters. We then geocode respondents by their self-reported ZIP code and define their treatment status using a binary indicator (1 if they reside in a ZIP code less then 2,400 meters away from one of these LIHTC development, 0 if living in a ZIP code between 2,400 and 4,800 meters away from a LIHTC development). These distance bands gives us 779 unique treated respondents and 685 unique control respondents across 788 LIHTC developments in 39 states. To account for the possibility that the larger distance bands have created treated and control respondents that are less comparable than in our CA analysis, we now include control variables for race (binary for non-Hispanic white), income (binned continuous variable), and partisanship (7-point scale). These covariates account for the most likely confounders without exhausting degrees of freedom. The model also includes a LIHTC-level fixed effect and Huber-White standard errors clustered at the LIHTC level.

The large area of ZIP codes also presents a challenge for the near-near design. LIHTC developments are typically geographically clustered. While clustering is not an issue when using data as fine-grained as Census blocks, many ZIP codes treated in the 4 years prior to the survey (the near-near treatment group) are also treated in the 4 years following the survey (the near-near control group). As a result, what would be control ZIP codes in the near-near design are instead considered only treated ZIP codes, forcing us to rely on the few ZIP codes treated from 2017 to 2020 that were not treated from 2012 to 2015.

Like the near-far design, we address this challenge by adjusting the distance specification. But unlike the near-far design, expanding the control group also expands the treated group, as both treated and control ZIP codes in the near-near design are equally close to a new LIHTC development. Balancing the goal of more control units without diluting the definition of treatment, we expand the treatment radius from 2,400 meters to 3,200 meters, producing 1,043 unique treated respondents but only 198 unique control respondents across 820 LIHTC developments in 38 states. Like the near-far design, we use respondent-level demographic controls to account for common confounders. Unlike the CA analysis, we do not include a CBSA-level fixed effect as it is too demanding for the 198 control units. Huber-White standard errors are clustered at the LIHTC level.

In both the near-far and near-near designs, we operationalize support for the neighborhood ban as a binary variable and a continuous variable: first, as a binary variable (0 if "strongly oppose" to "neither support nor oppose", 1 if "somewhat support" to "strongly support"), and second, as a continuous variable (from 0 as "strongly oppose" to 1 as "strongly support"). The results are plotted next to each other and are substantively similar.[21]

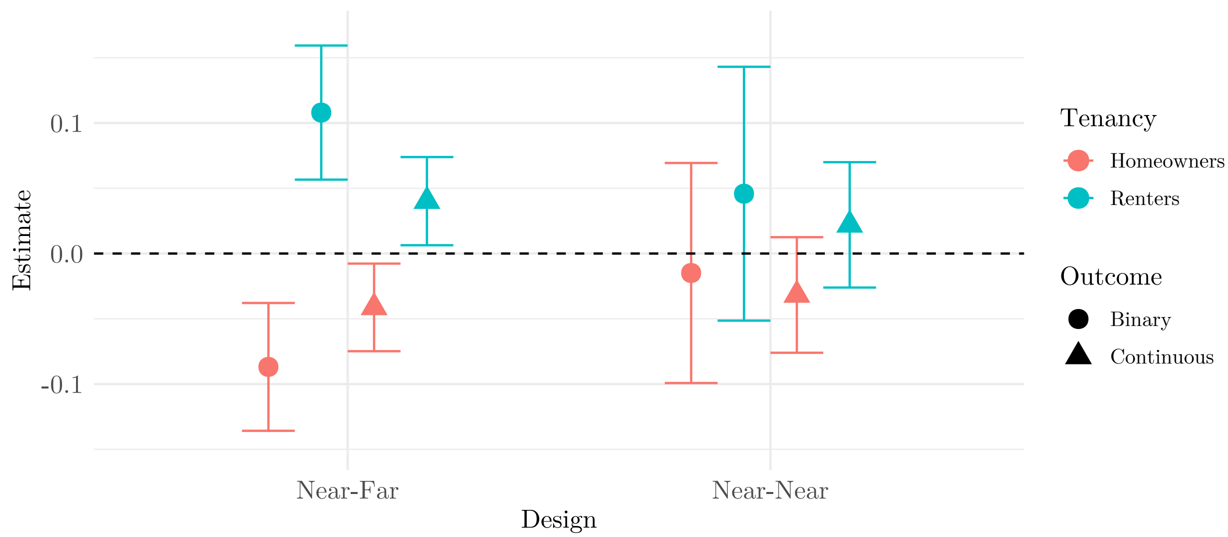


Figure 10: Effect of new, nearby LIHTC development on support for a ban on new housing in the respondent's neighborhood.

Figure 10 shows the effect of new, nearby affordable housing on homeowners (red) and renters (blue). The coefficient estimates visualized are drawn from the interaction terms in the regression models. Using the near-far model, renters near new LIHTC development are 4 to 11 percentage points more supportive of a neighborhood ban on new housing compared to renters farther away, even controlling for race, income, and partisanship. In contrast, homeowners near new LIHTC development are 4 to 9 percentage points less supportive for a neighborhood ban compared to

[21]We do not use the "Big" and "Clean" sample restrictions that we employ with the electoral data due to aforementioned existing statistical power constraints.

homeowners farther way.

To account for the possibility of micro-level selection of where affordable housing is sited, we use the near-near design to compare respondents treated prior to the survey to respondents in similar neighborhoods treated after the survey. As noted, the results are limited in their statistical power, but treatment effects are again polarized by homeownership status. Renters near LIHTC developments built prior to the survey are 2 to 5 percentage points more supportive of a ban on new, nearby housing compared to renters treated by LIHTC after the survey was fielded. Likewise, homeowners exposed to LIHTC prior to the survey are 1 to 3 percentage points less supportive of a neighborhood ban than homeowners treated by LIHTC after the survey was fielded.

In short, results from the geocoded survey analyses support the hypothesis that our primary findings from the housing bond analyses are driven by individual homeowners and renters, not something otherwise unique to "homeowner blocks" and "renter blocks." Taken together, the evidence suggests that homeowners at the individual level respond to new LIHTC development by supporting more spending on affordable housing and decreasing their support for a ban on nearby development. Renters, likely concerned with the rising prices around them, respond to new LIHTC by decreasing their support for funding affordable housing and increasing their support for a ban on nearby development. Additionally, these findings replicate outside of California and during a different time period, boosting our confidence in their generalizability.

Discussion

Beyond housing, the concept of the policy adjacent can be used to examine other familiar social welfare contexts to better understand the local constituencies for and against these policy instruments. First, the Supplemental Nutrition Assistance Program (SNAP, formerly known as "The Food Stamps Program") provides electronic benefits to low-income families that can be used to purchase food. Due to income segregation, SNAP's benefits tend to be a spatially concentrated infusion of federal funds into low-income neighborhoods. This localized boon has effects on the local grocery economy, with evidence that a 1% increase in the use of SNAP benefits causes a 0.8% increase in local grocery prices (Leung and Seo 2018). Thus, while individuals receiving SNAP are the direct policy beneficiaries, nearby grocers indirectly benefit from the federal program's steady stream of revenue. These grocers are policy adjacent winners. Less commonly thought of as part of the coalition in favor of SNAP expansion, retail grocers in low-income communities are apt to increase their support for the program once they have experienced its spillover benefits.

But the infusion of resources into the local grocery economy also has spillover costs. For low-income community members either just above the SNAP income eligibility threshold or no longer qualifying for the program due to its strict time limits, an increase in local food prices is a concentrated burden. If non-beneficiaries connect the price increase to the influx of SNAP benefits and believe they are unlikely to become eligible for SNAP benefits, they may see the expansion of SNAP as increasingly harmful to their well-being. These low-income shoppers are the policy adjacent losers. While they are the natural constituency for supporting SNAP expansion, their vulnerable position combined with SNAP's negative spillovers may demobilize them or persuade them that the program is not worth expanding (absent a guarantee that they will be covered).

A second example comes from the Housing Choice Voucher Program (formerly called "Section 8") which provides vouchers to low-income individuals covering the difference between 30% of the voucher-holder's income and a calculated fair market rent for units in low- to lower-middle income neighborhoods. This steady stream of benefits makes landlords — the policy adjacent winners — a potential constituency in favor of expanding voucher funds.

But far more individuals qualify for vouchers than the federal government provides, meaning vouchers are distributed via lottery. Consequently, the waitlists for vouchers are measured in years and only 1 in 4 families eligible for voucher assistance eventually receive it (Ellen 2020). This rationing of rental assistance means that most low-income renters are the policy adjacent losers, competing against voucher holders for a limited supply of eligible housing units. And like SNAP, the infusion of steady funds into the local housing economy may increase the cost of market-rate rental units (Susin 2002, but see Eriksen and Ross 2015). If a non-voucher holding renter connects this rise in nearby housing prices to competition from voucher holders, the renter may decide that expansion of the voucher program is detrimental to their housing stability. Again, a natural constituency for policy expansion is either demobilized or turned against it via negative indirect policy feedback.

Conclusion

Across multiple designs and specifications, we show that new, nearby affordable housing changes residents' voting behavior when asked to fund affordable housing in the abstract. This policy feedback effect is largely conditioned by the economic externalities created by these developments. To the extent that affordable housing represents an investment in the local neighborhood, homeowners, the policy adjacent winners, stand to benefit from increasing property values. At the same time, affordable housing units can be perceived to harm market-rate renters through rent inflation. This poses an uncomfortable quandary for proponents of public housing: While building affordable housing directly benefits recipients, it can undermine support among the economically precarious policy adjacent losers, who should be natural political allies.

That a single nearby LIHTC development changes down-ballot voting behavior suggests proponents of affordable housing should consider policy implementation as they engage in strategic coalition building. But while treated homeowners could be mobilized to expand housing, it is no coincidence that LIHTC springs up in places where it does not meet strong homeowner opposition (Trounstine 2018). We suspect — though cannot directly test — that attempts to build affordable housing in higher income areas would meet homeowner resistance, reflecting (at least in part) concerns about the potential for declining property values. This opposition to LIHTC not only limits the number of home-owning voters open for such a coalition but also reinforces residential economic and racial segregation.

Similarly, renters in gentrifying areas will likely be less sanguine about local development. In 2019, New York City's Deputy Mayor for Housing noted renter's perception of even affordable housing as a gentrification threat: "If they fear displacement, they will oppose the housing," referring to residents of communities where new affordable housing is built. "And the only way that we get a more integrated city is if we have more affordable housing across a wider range of neighborhoods. If people fear that they're going to be pushed out of their neighborhood, they will not accept housing," she continued.[22] Efforts to win their support – by, for example, prioritizing adjacent residents for new units – may generate undesirable externalities of their own. Indeed, prioritization of local

[22]Goodman, J. David. "What the city didn't want the public to know: It's policy deepens segregation" *New York Times* Published July 17, 2019. `https://www.nytimes.com/2019/07/16/nyregion/segregation-nyc-affordable-housing.html`

residents below the municipality level in housing lotteries is currently prohibited under the U.S. Fair Housing Act, as it may entrench existing patterns of racial segregation. There may thus exist a bind between building sustained local support for affordable housing and pursuing regional goals of integration.

Our results show how the implementation of policies with concentrated externalities can generate indirect policy feedback in unexpected ways. When negative externalities exist, implementation can alienate the policy adjacent losers, the natural constituency expected to most favor policy expansion. At the same time, positive externalities can convert unexpected supporters from the policy adjacent winners. This framework — the policy adjacent winners and losers — extends policy feedback effects to indirect externalities, and should help to deepen our understanding of the long-term viability of not only spatially-based social welfare programs, but any policy with concentrated costs and benefits.

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Online Appendix for "The Policy Adjacent: How Affordable Housing Generates Policy Feedback Among Neighboring Residents"

Contents

A Data

The purpose of this section is to provide additional detail about the data used in the study and how our final dataset was constructed.

A.1 Housing Bond Data

In this study, we use electoral data from two California housing bonds passed by voters statewide in 2002 and 2006. Figure A-1 shows the change in support for the housing bonds between the two elections at the Census block level. Observations are weighted by the average number of votes recorded on the housing bonds across both elections. We drop blocks which recorded zero votes on either the 2002 or the 2006 bonds, as we cannot reliably estimate the change in support between the elections for these blocks. Fortunately, these blocks have very few voters. The vertical line shows the mean change in support for bonds between the two elections, weighted by the average number of voters in each block. Support for the bonds was largely stable across the two elections. The voter-weighted average block only increased support for the bonds by 0.3 percentage points with a weighted standard deviation of 12 percentage points.

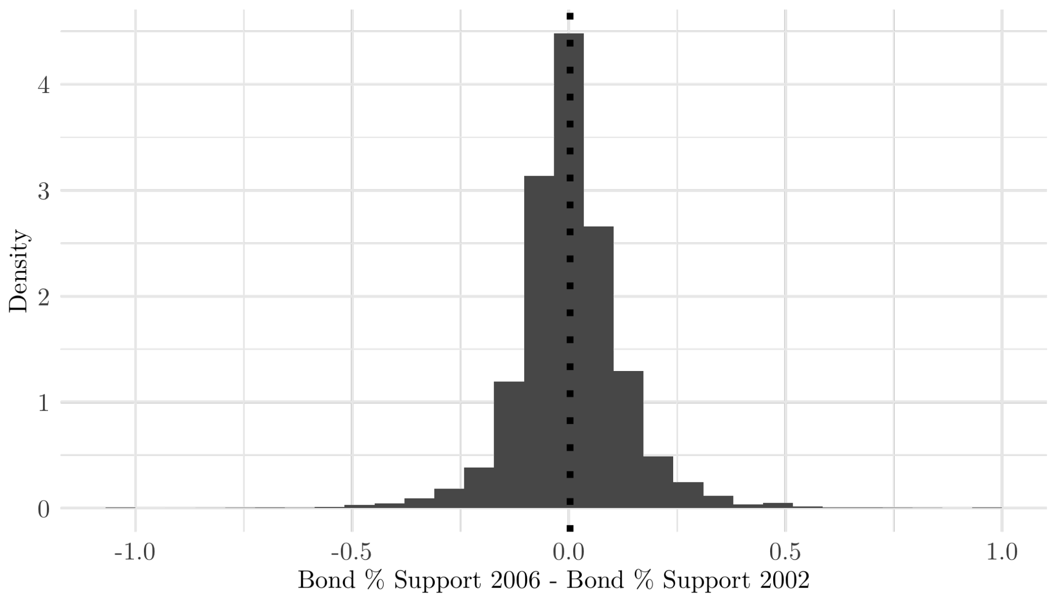

Figure A-1: Histogram of block-level change in support for housing bonds from 2002 to 2006, weighted by the average number of voters across elections. Weighted mean change in support shown by dashed vertical line.

A.2 LIHTC Data

As our treatment, we use exposure to new, nearby LIHTC-funded affordable housing construction that is placed in service during the 4-year period between the 2002 and 2006 election. For context, federal guidelines stipulate that, in order to qualify for the tax credit, at least 20% of tenants in the proposed project must earn below 50% of the area median gross income (AMGI) or, alternatively, at least 40% of tenants must earn less than 60% of AMGI. Developers must restrict rents for low income residents to 30% of the relevant income limit for a minimum of 30 years.

On average, there is a 1 year lag between credit allocation for a development and a development being placed in service. Studying property values via transactions, Diamond and McQuade (2019) use year of credit allocation to measure the effect of affordable housing, arguing that the announcement of affordable housing should immediately affect transactions even before construction begins. This may be reasonable for identifying effects on transaction values, as property purchasers are likely to have a high awareness of changes in the area that may affect the long-term value of the large asset they are about to purchase. We expect nearby voters to become responsive once they become aware of rising housing prices *and* connect this increase to affordable housing policy, which we believe is most likely to come with the advent of new physical infrastructure. Thus we define treatment based on when each building is placed into service.

A.3 Other Variables

Income-related variables are not available at the Census block level. Some of our later analyses involve subsetting the data based on Census tract level household income measures, but we cannot control for income in block-level analyses. To give a sense of scale, tracts generally contain between 1,200 and 8,000 people, with an optimum size of 4,000 people. Census blocks are not defined by population but are much smaller in area, for example one block in a city typically represents a Census block. Thus, tract data is useful as moderating variable across LIHTC developments, but not for capturing variation within the area treated by an individual LIHTC development.

A.4 Dataset Construction

When defining proximity, we use the centroid of each census blocks as its precise location. For the "Binary" treatment, we define treated blocks as those within 350 meters of a LIHTC development. For cases where the treatment radius cuts through a block, we calculated what percent of the blocks area was within the treatment radius. If the $\geq$50% of the block's area was covered by the treatment radius, the block was treated, otherwise the block was part of the near-far control group.

In defining our treatment, some LIHTC developments are sited in close proximity to each other, such that the same block can be treated by more than one LIHTC development. Because our parametric approach relies on a LIHTC-level fixed effect, each block requires a LIHTC covariate, making the unit of observation a block-LIHTC dyad. When a block is treated by more than one development, the block will appear in the near-far dataset however many times it is treated and with each observation referencing a unique LIHTC development.

In contrast, sometimes a block may be a control unit for one LIHTC development but also treated by another LIHTC development. Because the block was treated by any LIHTC development, it is considered a treated unit and all control observations of the block are dropped from the model. This ensures that blocks will only ever appear as treated or control, never both. 91% of blocks only appear once in the near-far data. The maximum number of appearances for a block is six times (<.04% of all blocks), meaning a few blocks were within 350 meters of six LIHTC buildings placed in service between 2002 and 2006.

Like the near-far design, the near-near design requires that blocks appear as many times as they are treated by different LIHTC developments. But if a block is treated not only by new LIHTC housing in 2003-2006 but also by new LIHTC in 2007-2010 — making it a control unit in the near-near design — the block only appears as a treated unit. This leaves as control units blocks which were only treated in 2007-2010, after the second housing bond election.

B Balance Tables

B.1 Near-Far Balance

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.34	0.45	-0.34
Percent voted in 2002	0.42	0.44	-0.14
Percent non-Hispanic white	0.34	0.39	-0.17
Percent non-Hispanic Black	0.10	0.09	0.05
Percent Hispanic	0.37	0.35	0.08
Vacancy rate	0.05	0.05	0.04
Density	13467.50	8754.27	0.28

Table B-1: Balance, Near-Far Analysis, All Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.85	0.85	-0.01
Percent voted in 2002	0.50	0.52	-0.08
Percent non-Hispanic white	0.45	0.49	-0.12
Percent non-Hispanic Black	0.07	0.06	0.01
Percent Hispanic	0.29	0.28	0.04
Vacancy rate	0.04	0.04	0.03
Density	3774.38	3759.55	0.00

Table B-2: Balance, Near-Far Analysis, Homeowner Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.06	0.07	-0.04
Percent voted in 2002	0.38	0.39	-0.05
Percent non-Hispanic white	0.31	0.36	-0.16
Percent non-Hispanic Black	0.10	0.08	0.08
Percent Hispanic	0.38	0.35	0.10
Vacancy rate	0.05	0.05	0.02
Density	22271.75	16211.35	0.36

Table B-3: Balance, Near-Far Analysis, Renter Blocks

B.2 Near-Near Balance

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.34	0.34	-0.01
Percent voted in 2002	0.42	0.42	-0.03
Percent non-Hispanic white	0.34	0.38	-0.12
Percent non-Hispanic Black	0.10	0.09	0.01
Percent Hispanic	0.37	0.37	0.01
Vacancy rate	0.05	0.05	-0.03
Density	13467.50	11011.33	0.12

Table B-4: Balance, Near-Near Analysis, All Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.85	0.84	0.02
Percent voted in 2002	0.50	0.49	0.06
Percent non-Hispanic white	0.45	0.43	0.07
Percent non-Hispanic Black	0.07	0.08	-0.06
Percent Hispanic	0.29	0.35	-0.19
Vacancy rate	0.04	0.04	0.07
Density	3774.38	4836.58	-0.05

Table B-5: Balance, Near-Near Analysis, Homeowner Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.06	0.07	-0.02
Percent voted in 2002	0.38	0.39	-0.04
Percent non-Hispanic white	0.31	0.36	-0.17
Percent non-Hispanic Black	0.10	0.10	-0.02
Percent Hispanic	0.38	0.36	0.08
Vacancy rate	0.05	0.06	-0.05
Density	22271.75	16569.14	0.29

Table B-6: Balance, Near-Near Analysis, Renter Blocks

C Sensitivity Analysis - Distance Band

The following figures show the effects of the "Binary" treatment based on different radii for defining treated blocks versus control blocks using two of our samples of interest, "All" and "Big Clean". All datasets are based on an outer limit of the control ring set at 600 meters. The radii listed on the x-axis represent the radius of the outer limit of the treated ring, which is also the inner limit of the control ring. As the radius increases, more blocks will be defined as treated and fewer will be defined as control. At some hypothetical radius, the entire treatment effect will be captured, providing the clearest contrast with the control blocks. We believe that radius is 350 meters. However, as shown in these figures, the point estimates are stable across a wide variety of distance band specifications.

Here, all tercile cutpoints are set based on our the naturally occurring tercile cutpoints for our preferred distance band specification (350 meters). The cutpoints are $> 67\%$ homeownership rate for homeowner blocks and $< 21\%$ homeownership rate for renter blocks

C.1 Near-Far Design

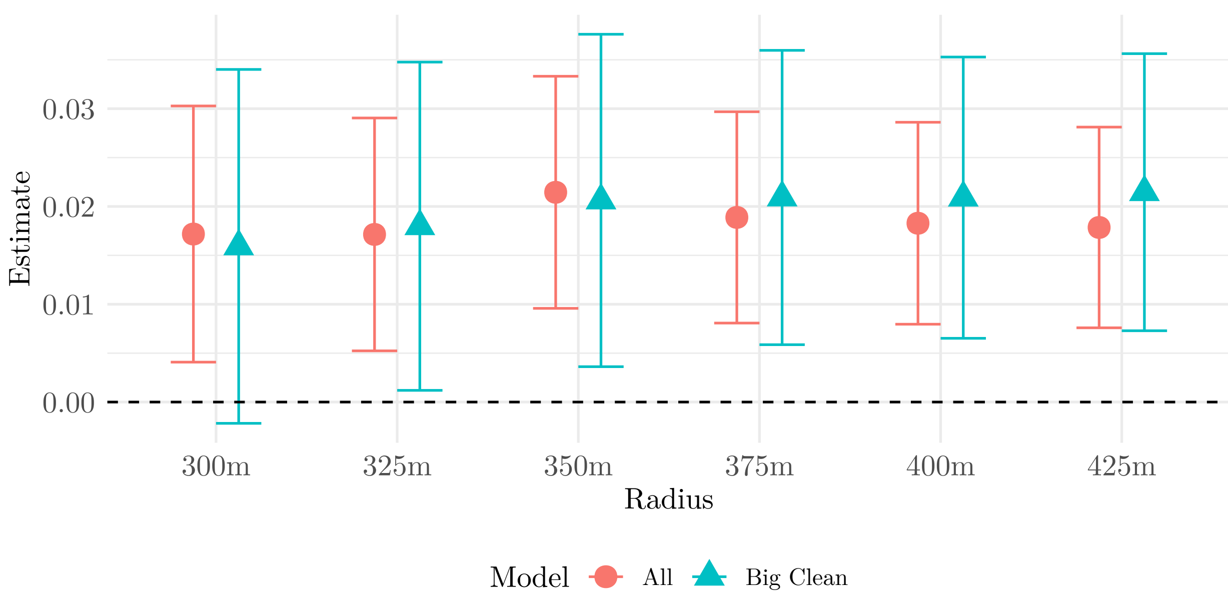

Figure C-2: Sensitivity of results for homeowner blocks across various distance band specifications using the near-far design.

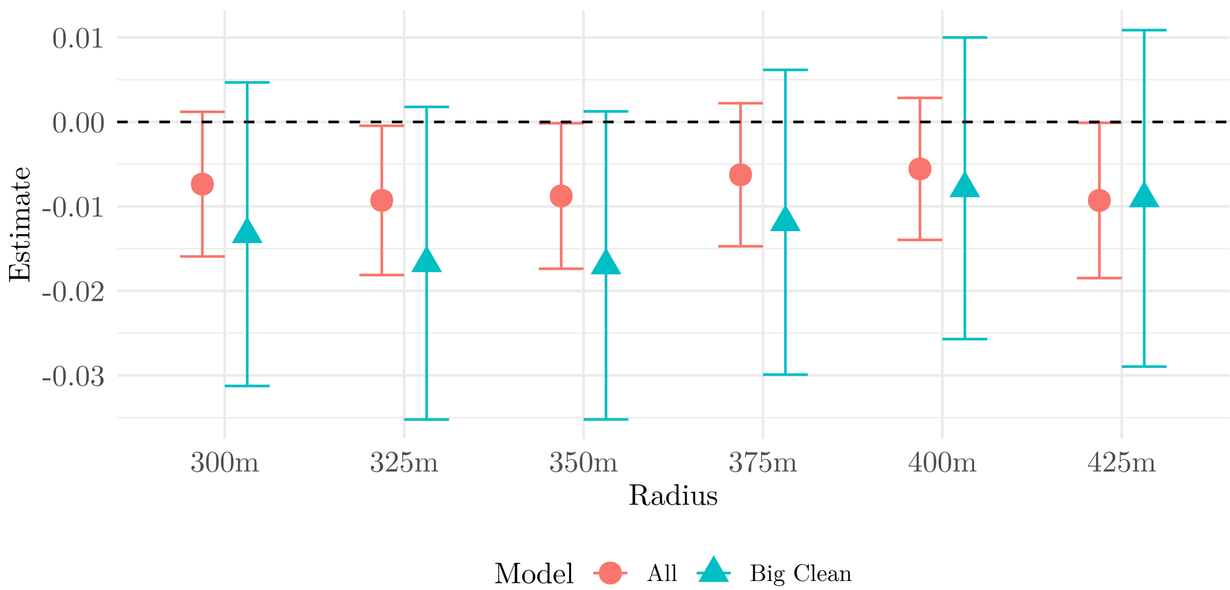

Figure C-3: Sensitivity of results for renter blocks across various distance band specifications using the near-far design.

C.2 Near-Near Design

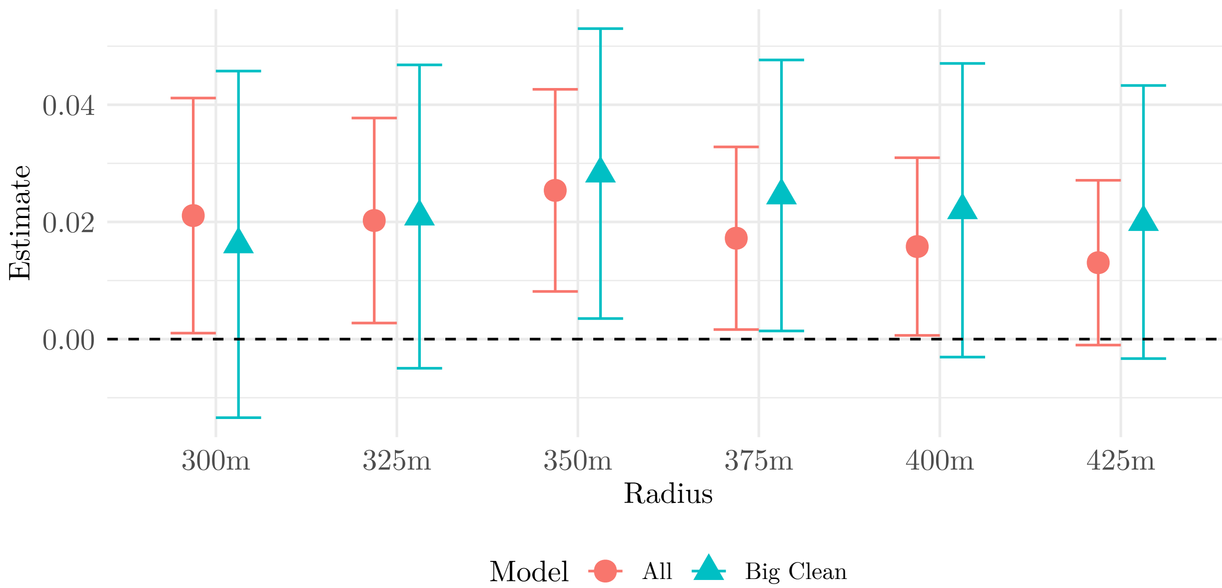

Figure C-4: Sensitivity of results for homeowner blocks across various distance band specifications using the near-near design.

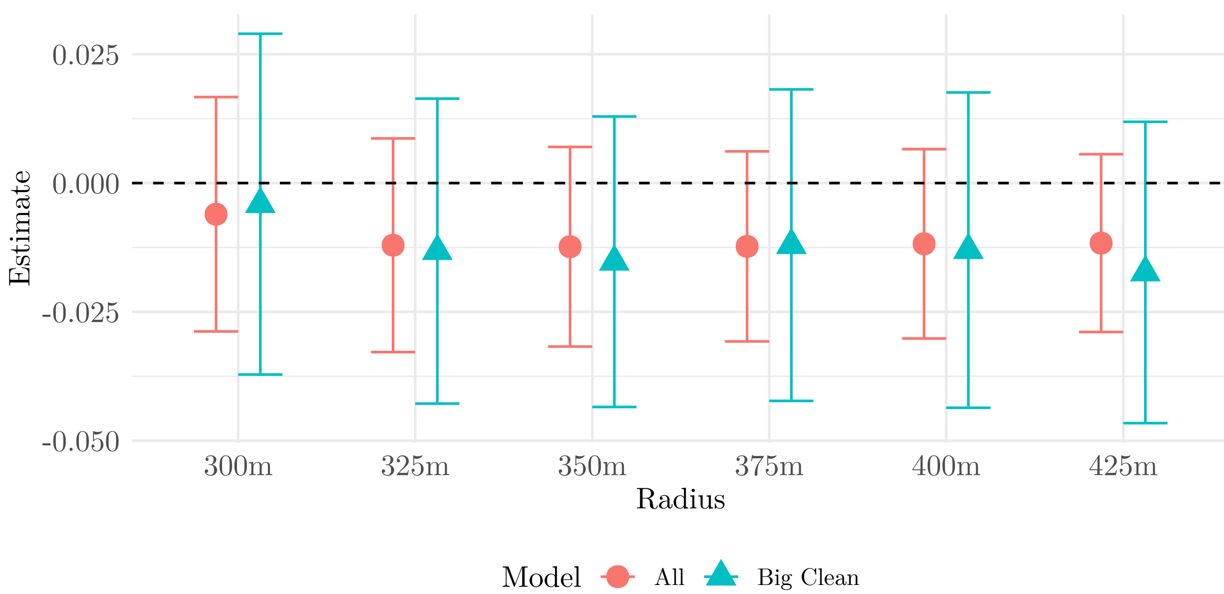

Figure C-5: Sensitivity of results for renter blocks across various distance band specifications using the near-near design.

D Sensitivity Analysis - Homeownership Cutpoints

For all moderating variables, we categorize blocks based on the tercile cutpoints of the variable among all unique blocks. By dropping the middle tercile, we compare the effect of a new LIHTC development in blocks where the variable of interest (e.g., homeownership) is high to blocks where the variable is low. For example, these cutpoints allow us to define "homeowner blocks" as blocks with homeownership rates of $> 67\%$ and "renter blocks" as blocks with homeownership rates of $< 21\%$ as of 2000. For reference, the population weighted mean homeownership rate in our sample of blocks is 29%, compared to the statewide homeownership rate of 57% in 2000.

We use the same approach on the aforementioned block-level covariates, using their corresponding terciles. Ultimately, setting the cutpoints for categorizing high and low subgroups is a balance between maximizing the homogeneity of the residents within that variable while also ensuring that enough units remain in each tercile to estimate a treatment effect should one exist. For example, a homeownership cutpoint of >90% would be more likely to capture the treatment effect among homeowners than our cutpoint of $> 67\%$, but Figure D-6 shows that there are relatively few blocks treated by LIHTC that meet this stricter definition of homeowner block.

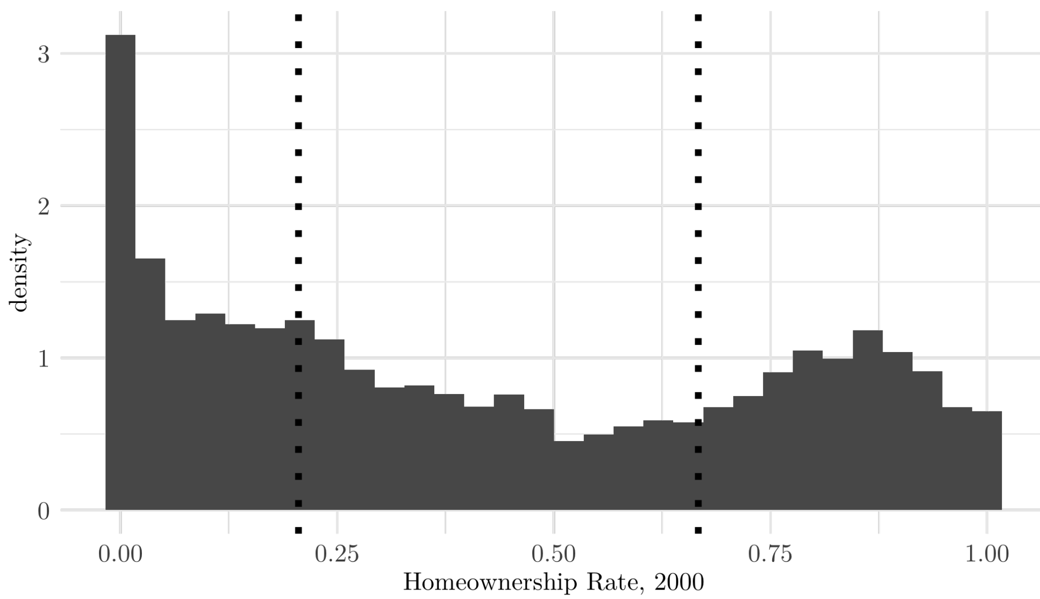

Figure D-6: Histogram of block-level homeownership rate among treated units, weighted by the average number of voters across elections. Tercile cutpoints shown by dashed vertical line.

Nevertheless, we test the sensitivity of our binary treatment specification to different cutpoints in defining homeowner blocks and renter blocks. Figures D-7 through D-10 shows the average treatment effects for the "Binary" treatment on the "All" and "Big Clean" samples for the near-far and near-near designs on homeowner and renters blocks at higher and lower cutpoints. The results are substantively similar at cutpoints ± 10 percentage points for each subgroup and design.

D.1 Near-Far Design

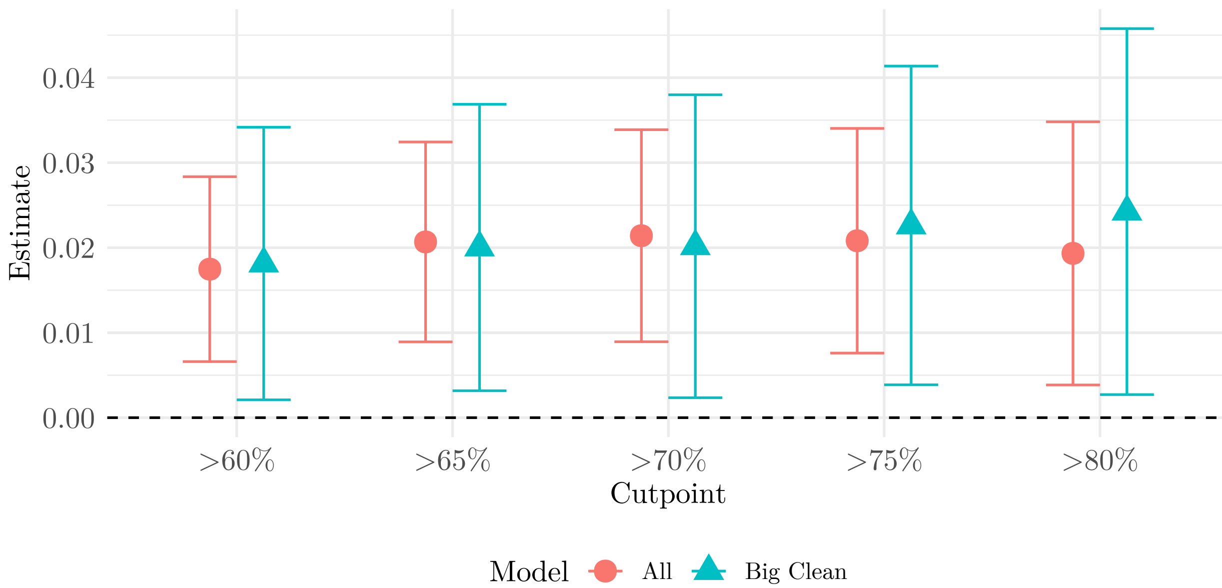

Figure D-7: Sensitivity of results to homeowner cutpoints for all blocks and blocks treated by large developments without previous development using the near-far design.

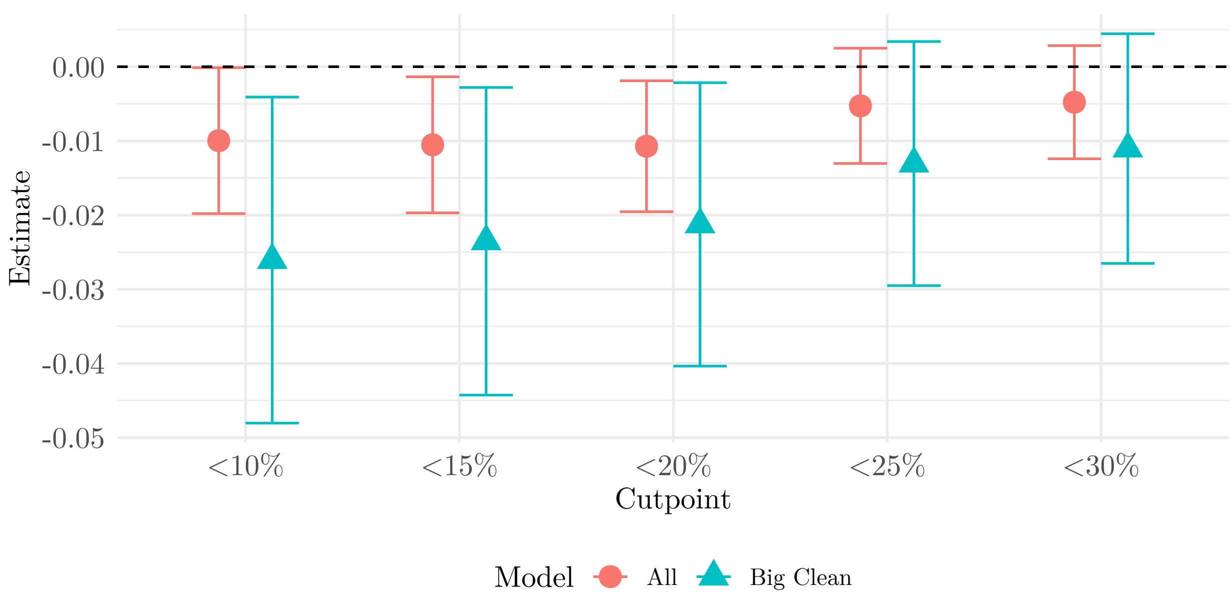

Figure D-8: Sensitivity of results to renter cutpoints for all blocks and blocks treated by large developments without previous development using the near-far design.

D.2 Near-Near Design

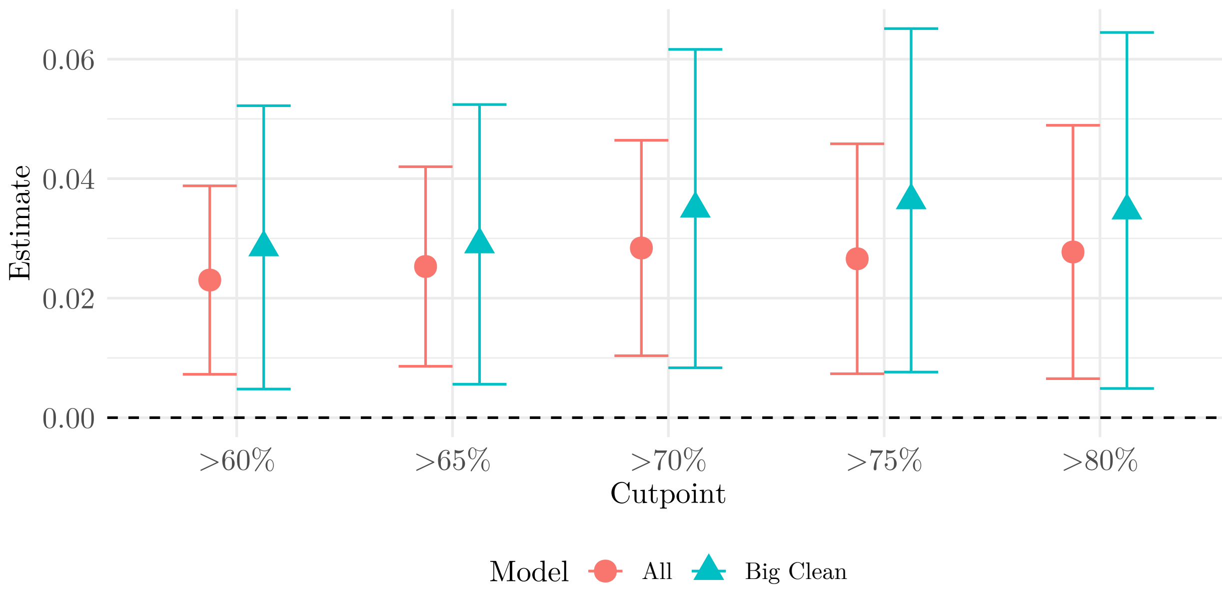

Figure D-9: Sensitivity of results to homeowner cutpoints for all blocks and blocks treated by large developments without previous development using the near-near design.

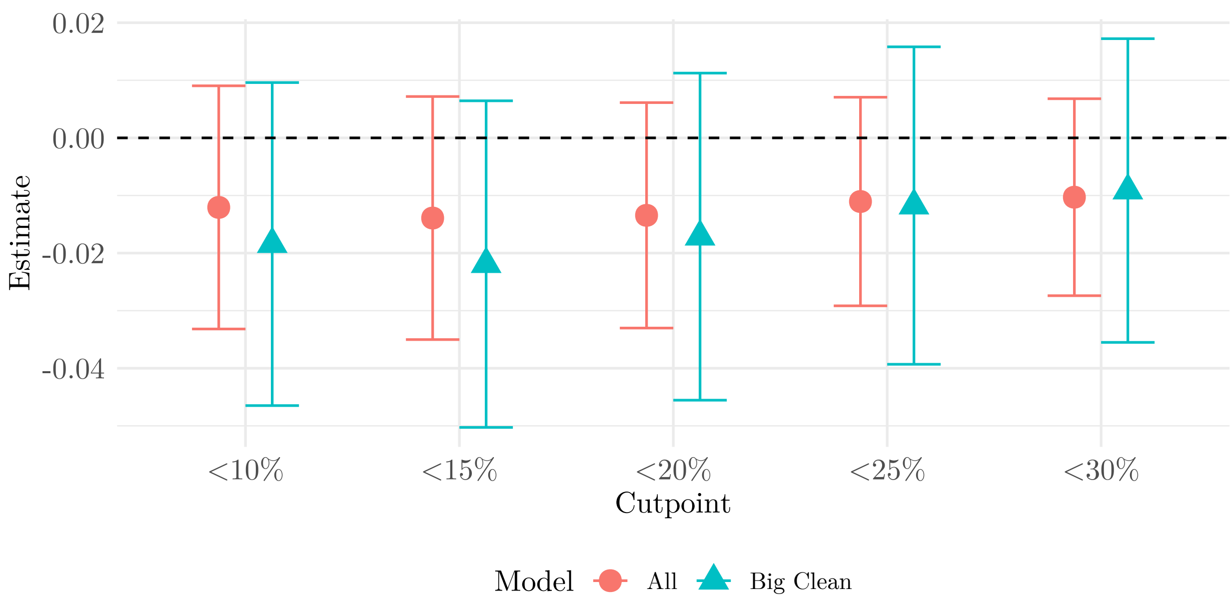

Figure D-10: Sensitivity of results to renter cutpoints for all blocks and blocks treated by large developments without previous development using the near-near design.

E Robustness Checks

E.1 Unconditional Results

	All	Clean	Big	Big Clean
LIHTC Project	0.004	0.004	0.005	0.003
	(0.003)	(0.003)	(0.005)	(0.005)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.217	0.223	0.181	0.183
Adj. R^2	0.178	0.181	0.136	0.136
Num. obs.	9636	8963	4237	4035
RMSE	0.474	0.476	0.500	0.502
N Clusters	451	450	223	222

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table E-7: Near-Far Design, Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Project	−0.002	0.004	−0.000	0.007
	(0.007)	(0.007)	(0.010)	(0.012)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.056	0.060	0.066	0.068
Adj. R^2	0.052	0.055	0.057	0.058
Num. obs.	6411	5847	2330	2164
RMSE	0.511	0.512	0.529	0.532
N Clusters	815	793	338	328

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table E-8: Near-Near Design, Binary Treatment

E.2 Placebo Test

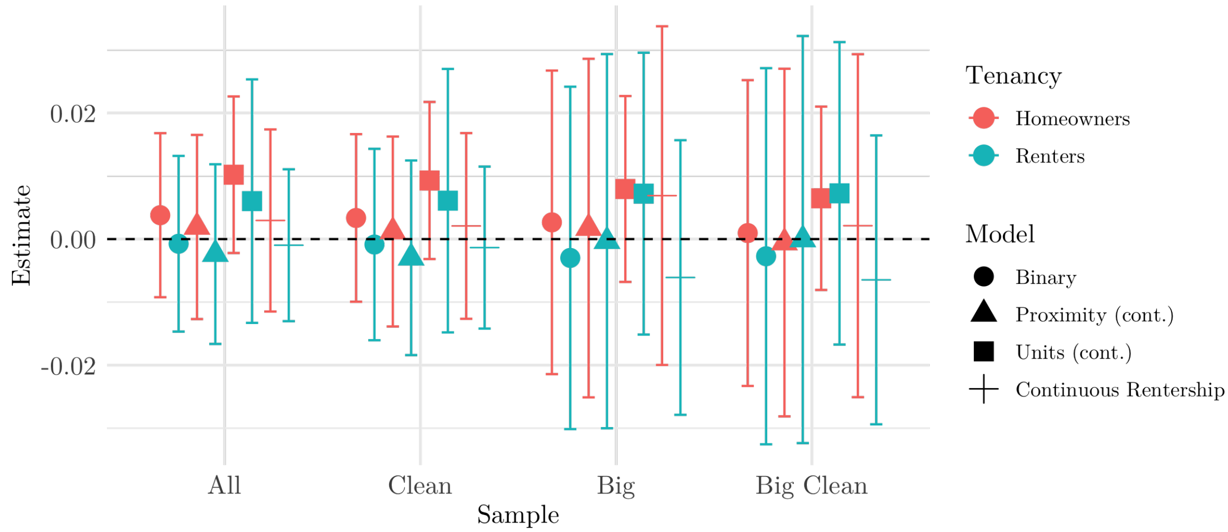

Figure E-11: Placebo results from the near-far design on 2007-2010 treatments.

E.3 Other Block-Level Covariates

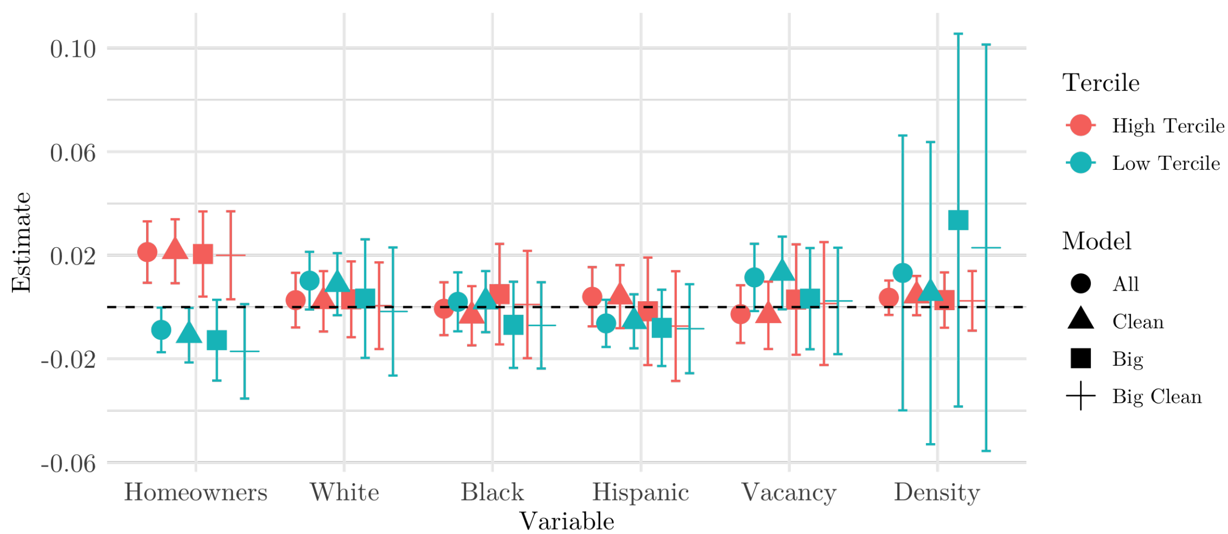

Figure E-12: Results from the near-far design across other block-level covariates using the "Binary" treatment model.

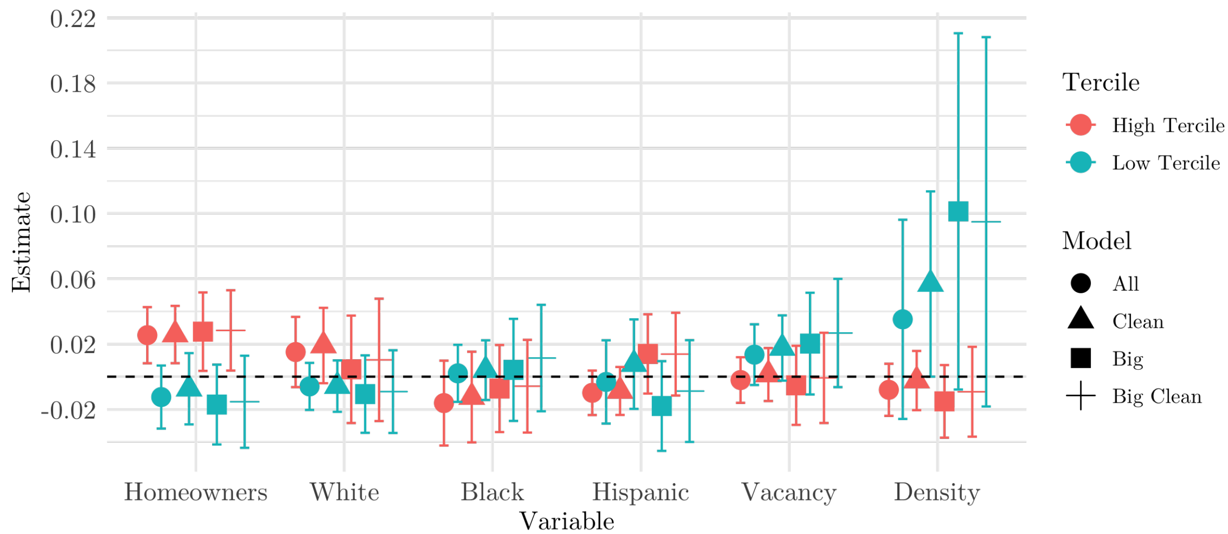

Figure E-13: Results from the near-near design across other block-level covariates using the "Binary" treatment model.

E.4 Naive Difference in Means

As a sense check, we present a non-parametric difference-in-means estimate of the effect of LIHTC developments by calculating the voter-weighted average change in housing bond support between treated and control blocks. We use the "Binary" approach outlined above, as the sharp cutpoints are most intuitive for a simple difference. Table E-9 shows our dependent variable, the change in support for the housing bonds, as a voter-weighted mean within both the treated and control groups. Starting with homeowner blocks (top row), we find that treated blocks on average increased their support for the housing bonds by 2.8 percentage points whereas control blocks increased their support by 1.2 percentage points. The difference in means between treated and control blocks is 1.7 percentage points, representing our nonparametric estimate of the effect of new LIHTC development on voters in homeowner blocks.

The second row shows the change in support among renter blocks. In contrast to homeowners, the average treated renter block *decreased* support for funding affordable housing by 1.3 percentage points while the average control renter block increased support for funding by 0.1 percentage points. The difference of means between treated and control blocks suggests that renter blocks near new LIHTC development decrease their support for affordable housing by 1.4 percentage points compared to renter blocks farther away. Both the differences for homeowner and renter blocks match our parametric estimates from the near-far design.

	Treated	Control	Difference
Homeowners	0.028	0.012	0.017
	n = 1,096	n = 2,506	
Renters	-0.013	0.001	-0.014
	n = 1,569	n = 2,034	

Table E-9: Nonparametric effect of LIHTC on change in support for housing bonds (2002 to 2006) using near-far design. Sample size reported is the number of Census blocks in each subgroup.

For the near-near design, we again start with our nonparametric difference in means between treated and control units in Table E-10. The sample size of treated blocks in the near-near design is the same as the near-far design because the treated units are the same across both approaches. The near-near design only changes the control group against which the treated units are being compared. That the two different control groups show similar stability in housing bond support from 2002 to 2006 makes us more confident in the analytical strategy.

Starting with homeowner blocks (top row), treated blocks on average increased their support for the housing bonds by 2.8 percentage points whereas control blocks only increased their support by 0.5 percentage points. The difference in means is 2.3 percentage points, a treatment effect similar to both the nonparametric and design-based estimates of the near-far design. In contrast to homeowners, the average treated renter block decreased support for funding affordable housing by 1.3 percentage points while the average control renter block experienced no change in support. Again, the renter difference in means of 1.3 percentage points is comparable to the estimates from the near-far design. Additionally, both the differences for homeowner and renter blocks match our parametric estimates from the near-near design.

	Treated	Control	Difference
Homeowners	0.028	0.005	0.023
	n = 1,096	n = 783	
Renters	-0.013	0	-0.013
	n = 1,569	n = 1,409	

Table E-10: Nonparametric effect of LIHTC on change in support for housing bonds (2002 to 2006) using near-near design. Sample size reported is the number of Census blocks in each subgroup.

E.5 Residential Churn

We use Zillow's ZTRAX data, a historical database of all real estate transactions in the United States since 1997 (Zillow 2020). Using Zillow's ZTRAX dataset, which includes every purchase and sale in the United States, we record the sum number of residential transactions during the four years prior the the 2002 election and the four years between the 2002 and 2006 elections. The number of years across which to aggregate transactions faces a trade-off. The four year period risks diluting the treatment effect, as transactions made in 2003 may occur prior to the LIHTC development being placed in service. On the other hand, a period of too few years risks aggregation an insufficient numbers of transactions to be able to measure a treatment effect. We present estimates of residential churn using both a 4-year aggregation and a 2-year aggregation. Estimates from both specifications are not statistically significant.

We then calculate the rate of residential churn by dividing these sums by the number of total housing units in each block in 2002 and 2006, respectively. We then subtract the 2002 rate of residential churn from the 2006 rate, generating the change in the rate of residential churn from the pre-treatment period to the post-treatment period. Because the number of units at the block level is only recorded for decennial censuses, we estimate this denominator in 2002 and 2006 using linear interpolation. The interpolation of the denominator occasionally produces unusually large rates of churn. We believe these values are substantively accurate but imprecise. Consequently, we winsorize the data, truncating outliers beyond the 5th and 95th percentiles. As stated, the effect of new, nearby LIHTC development on the change in block-level residential churn is not statistically significant in any of our models.

Note that because data limitations restrict us to examining property, rather than rental, transactions, we are unable to directly test whether new LIHTC development prompts renters to relocate. However, because our finding is that renters become *less* favorable towards affordable housing, we believe it is unlikely that residential sorting is a source of confounding in the analyses of renter blocks. If new LIHTC construction encourages disapproving renters to move out of the neighborhood, those more favorable towards LIHTC would remain or replace them. Consequently, our negative treatment effect estimate on renters would be a conservative one.

	All	Clean	Big	Big Clean
LIHTC Project	−0.032	−0.036	−0.032	−0.032
	(0.025)	(0.025)	(0.036)	(0.037)
Renter Blocks	−0.033	−0.029	−0.033	−0.027
	(0.021)	(0.021)	(0.029)	(0.030)
LIHTC x Renter Blocks	−0.023	−0.024	−0.046	−0.045
	(0.029)	(0.030)	(0.040)	(0.041)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.277	0.281	0.304	0.306
Adj. R^2	0.213	0.214	0.241	0.241
Num. obs.	3517	3372	1909	1848
RMSE	1.779	1.779	1.865	1.856
N Clusters	285	284	157	156

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table E-11: Near-Far Design with Homeownership on Churn (4-year), Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	−0.010	−0.013	0.001	0.000
	(0.035)	(0.036)	(0.039)	(0.040)
Renter Blocks	−0.049*	−0.044*	−0.054*	−0.046
	(0.020)	(0.021)	(0.027)	(0.028)
LIHTC x Renter Blocks	−0.004	0.000	−0.033	−0.030
	(0.042)	(0.044)	(0.046)	(0.047)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.273	0.277	0.299	0.302
Adj. R^2	0.208	0.210	0.236	0.237
Num. obs.	3517	3372	1909	1848
RMSE	1.784	1.785	1.871	1.862
N Clusters	285	284	157	156

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table E-12: Near-Far Design with Homeownership on Churn (4-year), Continuous Units Treatment

	All	Clean	Big	Big Clean
LIHTC Units	−0.019	−0.022	−0.028	−0.037
	(0.030)	(0.031)	(0.044)	(0.044)
Renter Blocks	−0.057**	−0.055*	−0.065	−0.070*
	(0.021)	(0.022)	(0.034)	(0.035)
LIHTC x Renter Blocks	0.030	0.031	0.042	0.053
	(0.034)	(0.035)	(0.050)	(0.051)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.020	0.023	0.043	0.048
Adj. R^2	0.008	0.010	0.022	0.026
Num. obs.	2058	1948	991	952
RMSE	1.828	1.830	1.946	1.935
N Clusters	447	431	215	207

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table E-13: Near-Near Design with Homeownership on Churn (4-year), Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	0.042	0.038	0.079	0.071
	(0.054)	(0.055)	(0.062)	(0.063)
Renter Blocks	-0.036^{*}	-0.033	-0.026	-0.026
	(0.017)	(0.018)	(0.028)	(0.029)
LIHTC x Renter Blocks	0.006	0.013	-0.029	-0.018
	(0.056)	(0.057)	(0.065)	(0.067)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.031	0.034	0.062	0.067
Adj. R^2	0.019	0.022	0.042	0.046
Num. obs.	2058	1948	991	952
RMSE	1.818	1.818	1.927	1.915
N Clusters	447	431	215	207

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table E-14: Near-Near Design with Homeownership on Churn (4-year), Continuous Units Treatment

F Mechanisms

Turnout

We have shown, across a variety of specifications, conceptualizations of treatment, and data subsets, that high-homeownership neighborhoods receiving new LIHTC developments subsequently become more supportive of additional public spending on affordable housing, while low-homeownership neighborhoods become less supportive. Though we have shown theoretically supported attitudinal effects among individual homeowner and renters in out-of-sample survey data, our ability to test behavioral outcomes directly is hindered by our inability to observe individual voting decisions. In particular, changes in the composition of the electorate from the pre-treatment to post-treatment periods would threaten the validity of our claims about how new LIHTC development shapes voter preferences. Below we interrogate whether the estimated treatment effects can be attributed to changes in *who* votes.

Mobilization could be driving our effects if the new LIHTC development increased or decreased voter turnout. The first question is whether we see differential voter turnout across elections in treated blocks versus control blocks. The CA Statewide Database records both the total number of voters who turned out in each election and the total number of registered voters at the block level.

Figures F-14 and F-15 show the estimated effect of a LIHTC development on the change in block-level turnout using the standard array of models with our near-far and near-near designs, respectively. Point estimates are small and never statistically significant, making it unlikely that turnout is behind our effect.

We also assess effects on turnout using a 2007 California voter file. This approach not only allows us to avoid the problems of ecological inference (at least for turnout), but it allows us to subset to voters who we know were registered at an address within 600 meters of a future LIHTC development prior to the 2002 election. In other words, we are able to study precisely those who are experiencing the advent of LIHTC as a treatment rather than those selecting into the neighborhood after the affordable housing has been built.

To use the voter file data, we geocode all voters who were registered to vote prior to the 2002 general election within 600 meters of our LIHTC treatments. We then assign voters a treatment status according to our near-far and near-near designs. Voters are assigned homeownership terciles according to their block's homeownership rate using the 2000 Census data and the cutpoints used throughout this analysis.[23] We then use the same OLS models as with our block-level outcomes.

Unlike our block-level estimates, we see consistent negative and statistically significant effects of LIHTC development on voter turnout (Tables F-15 and F-16). Using the near-far design, voters in homeowner blocks are 2 percentage points less likely to turnout in the 2006 general election compared to 2002. The interaction with voters in renter blocks shows a substantively large and generally statistically significant effect, one which counters the negative point estimates among homeowner blocks. This is evidence that the negative effect of affordable housing on turnout is concentrated in blocks comprised of more than 2/3rds homeowners. The near-near design shows larger negative effects among homeowners, but with less precision. Here the renter interaction is still positive but not large enough to counter the negative homeowner effect. While voter turnout shows more inconsistency between the near-far and near-near design than the rest of our analyses, the body of evidence suggests that this negative effect on turnout is real among voters who were registered to nearby addresses prior to the 2002 election.

[23] This assignment of homeownership tercile based on Census block implies that even this analysis requires some degree of ecological inference.

F.1 Turnout via Block Data

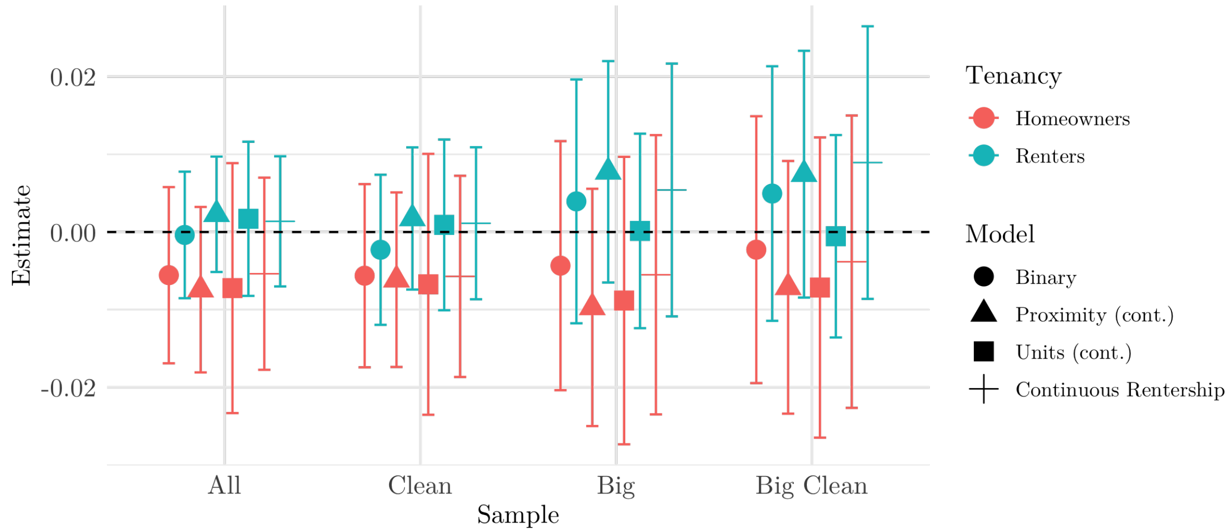

Figure F-14: Results from the near-far design on change in block-level voter turnout.

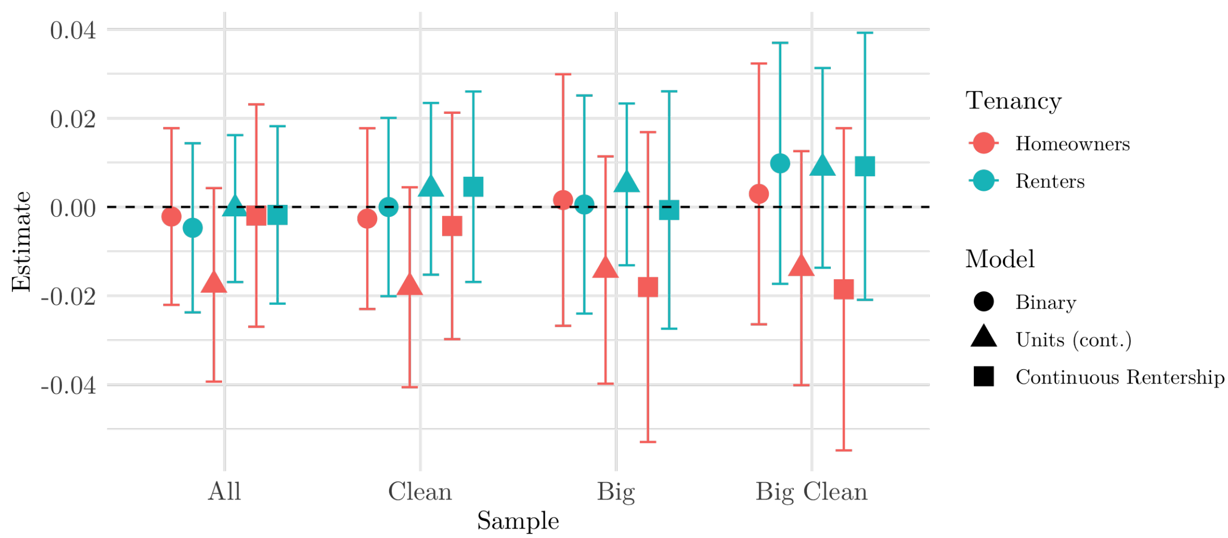

Figure F-15: Results from the near-near design on change in block-level voter turnout.

F.2 Turnout via Voter File Data

	All	Clean	Big	Big Clean
LIHTC Project	-0.019^{***}	-0.021^{***}	-0.020^{**}	-0.022^{**}
	(0.005)	(0.005)	(0.007)	(0.007)
Renter Blocks	-0.021^{*}	-0.023^{**}	-0.027^{*}	-0.030^{*}
	(0.008)	(0.009)	(0.013)	(0.014)
LIHTC x Renter Blocks	0.016^{*}	0.017^{*}	0.025^{*}	0.027^{*}
	(0.007)	(0.009)	(0.011)	(0.013)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.197	0.190	0.139	0.140
Adj. R^2	0.194	0.187	0.136	0.136
Num. obs.	136416	121908	58933	55676
RMSE	0.514	0.511	0.521	0.518
N Clusters	456	456	227	227

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table F-15: Near-Far Design with Homeownership, DV = Voted in 2006 - Voted in 2002

	All	Clean	Big	Big Clean
LIHTC Project	-0.047^{*}	-0.052^{*}	-0.042	-0.046
	(0.023)	(0.023)	(0.033)	(0.034)
Renter Blocks	0.042^{*}	0.039	0.024	0.025
	(0.021)	(0.023)	(0.038)	(0.040)
LIHTC x Renter Blocks	0.021	0.025	0.003	0.010
	(0.030)	(0.033)	(0.047)	(0.052)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.135	0.122	0.084	0.081
Adj. R^2	0.134	0.122	0.083	0.080
Num. obs.	90941	79453	33829	30909
RMSE	0.545	0.543	0.549	0.547
N Clusters	843	824	358	351

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table F-16: Near-Near Design with Homeownership, DV = Voted in 2006 - Voted in 2002

F.3 Roll-Off

Roll-off is a second mechanism which could be behind our treatment effects. Along with federal, state and local races, the 2002 California ballot had seven propositions on it. 2006 had thirteen. Consequently, the voter-weighted average roll-off from turning out to casting a vote for the housing bonds in the 2002 and 2006 elections was 21% and 20%, respectively. Despite this high and stable roll-off rate, it is possible that local LIHTC construction could have mobilized nearby individuals to actually to cast a vote on those down-ballot bonds – an effect that would not be detectable simply by examining turnout (i.e., whether an individual voted in the elections at all). Using data on the number of votes for each housing bond divided by the total number of votes at the block level, we find null effects with the exception of one model: the near-near design with a binary treatment (Figures F-16 and F-17). However, because of the inconsistency in size and direction of the effects, we are not convinced that our results are explained by differential roll-off between treated and control blocks.

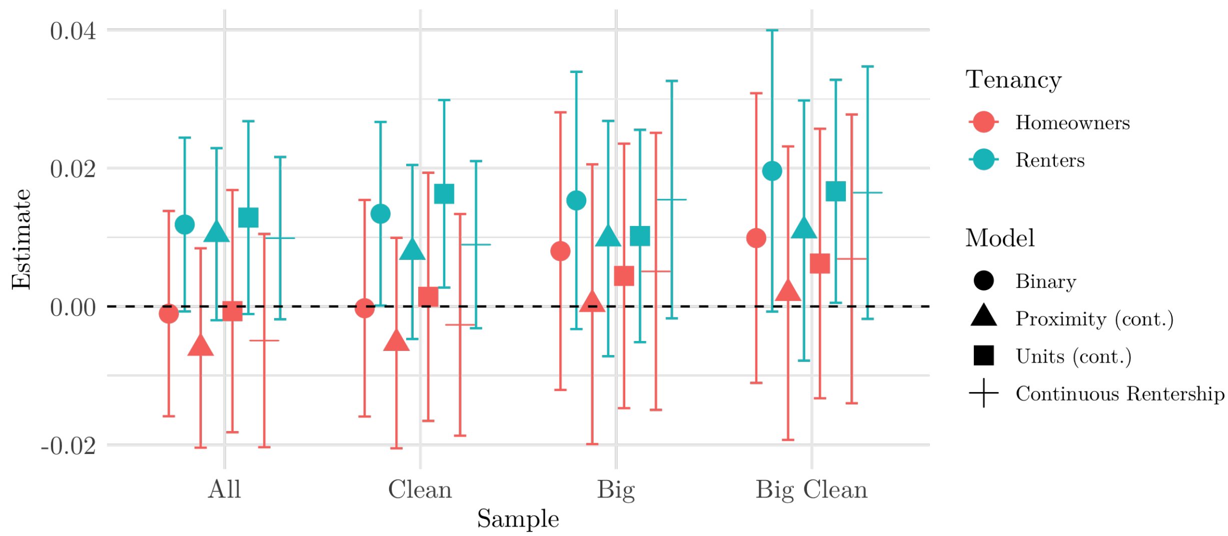

Figure F-16: Results from the near-far design on change in block-level voter roll-off.

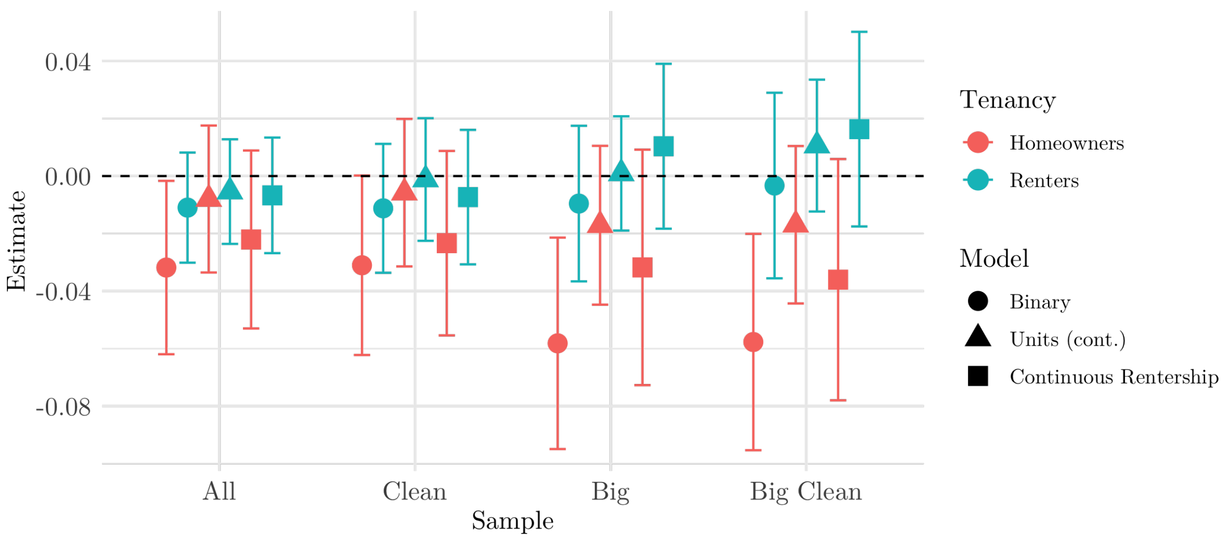

Figure F-17: Results from the near-near design on change in block-level voter roll-off.

F.4 Gentrification and Homeowners

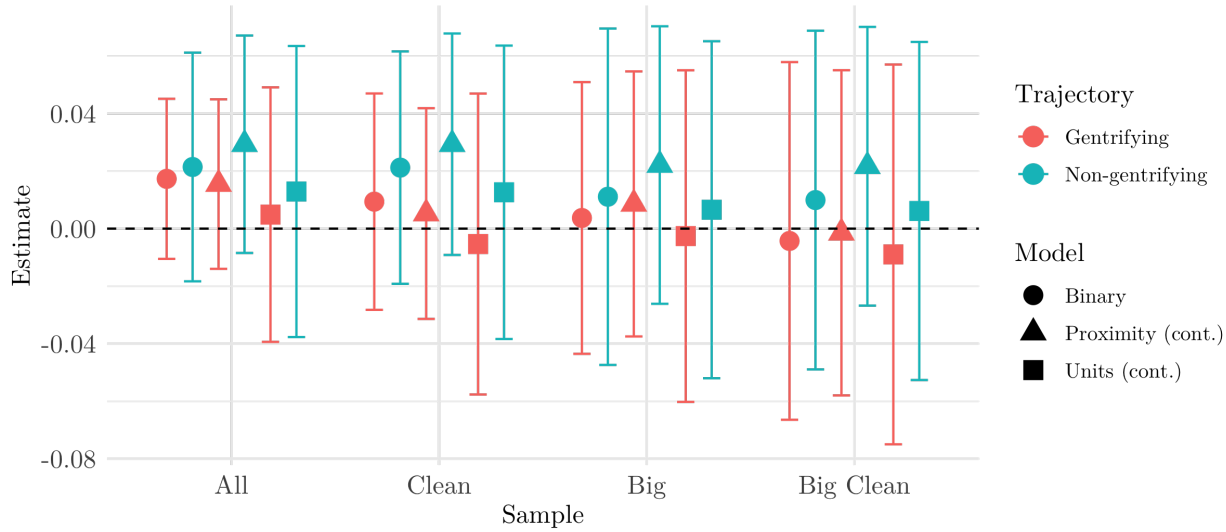

Figure F-18: Results from near-far design for homeowners and gentrification analysis.

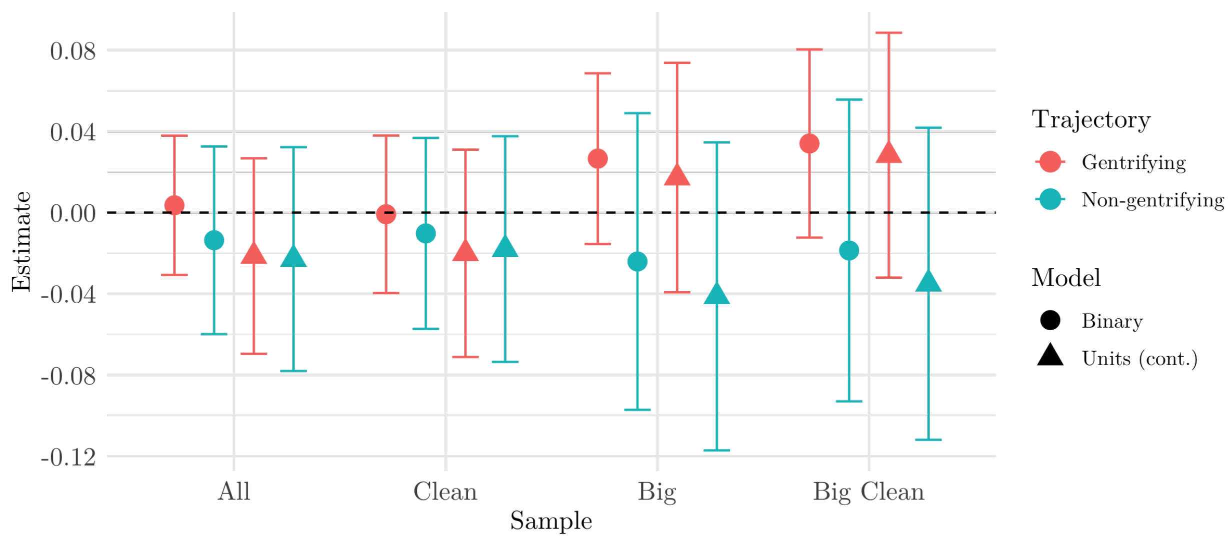

Figure F-19: Results from near-near design for homeowners and gentrification analysis.