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

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Abstract

Does a policy's implementation affect its support in the abstract among indirect recipients? We test policy feedback effects using two nearly identical statewide ballot propositions; each allocating over $2 billion for affordable housing in California. As our treatment, we geocode 463 affordable housing developments placed in service between these two bonds (2003-2006), allowing us to estimate the causal effect of new, nearby affordable housing on changes in block-level support for funding affordable housing statewide. We find that the construction of nearby affordable housing causes majority-homeowner blocks to increase their support for the housing bond, while causing majority-renter blocks to decrease their support. We attribute the positive effect among homeowners — "the policy adjacent" — 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, these "policy hungry" renters may attribute the new development to further increasing the rising rents around them. In turn, policy implementation can lead to negative feedback even among the policy's intended beneficiaries.

Keywords: policy feedback, local political economy, housing

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1 Introduction

Public policies create winners and losers. The policy ecosystem is composed not just of direct beneficiaries, but those whose circumstances are shaped by policy spillovers. Even policies with locally-concentrated beneficiaries can generate broad ripple effects, with far-reaching political implications. Much has been written about policy feedback loops: the economic and social impacts of a policy shape the political behavior of the individuals who experience its costs or benefits, which in turn reinforces or cuts against the political fortunes of the policy (Pierson 1993). Our focus is on a relatively understudied part of the policy ecosystem: policy feedbacks that occur not among direct policy recipients but rather among those indirectly affected.

Individuals living in close proximity to new affordable housing developments are one such group. We examine how their political behavior — in particular, their support for ballot measures that would provide for funding for more affordable housing – changes as a result of new affordable construction in their neighborhood. To do so, we combine geolocated data on 463 Low-Income Housing Tax Credit (LIHTC) developments with fine-grained electoral data on statewide ballot propositions in California. We leverage two nearly identical bond measures, one on the ballot in November of 2002 and the other in November of 2006, each of which allocated over $2 billion for affordable housing in the state. We find that voters who experience new LIHTC construction in their immediate neighborhoods in the intervening years respond in systematic ways relative to voters who did not experience this change. Namely, the construction of nearby affordable housing causes majority-homeowner blocks to increase their support for the statewide housing bond by 1 to 3 percentage points, while causing majority-renter blocks to decrease their support by a comparable amount.

As a mechanism, the positive impact of LIHTC on property values in distressed neighborhoods is well-documented (see Dillman, Horn, and Verrilli (2017) for a review), as is the tendency for LIHTC housing to be built in disadvantaged neighborhoods. Consequently, we propose that the positive feedback effects among homeowners are likely driven by an increase in surrounding property values due to replacement of blight (e.g., dilapidated structures, vacant housing, and empty lots). However, we show that these positive effects on bond support are not found in wealthier, majority white neighborhoods, suggesting that the historic racial intolerance against affordable housing may stymie the positive effects of housing quality improvement. Among renters, we find that the decrease in support for the housing bonds is concentrated in gentrifying neighborhoods. Even though the housing is reserved for low-income residents, current renters may associate the physical upgrading of the neighborhood as part of gentrification. Without a guarantee that they will receive an affordable housing unit, these market-rate renters may see program expansion as a risk to their housing stability.

In addition to our findings, we provide a framework for integrating these findings into the broader policy feedback literature. The homeowners in our study are *the policy adjacent*. Typically resistant to affordable housing programs, once these homeowners experience the positive localized spillover effects of new LIHTC housing, they increase support for the program. Homeowner tendency to participate more in politics makes them an unexpected ally in building support for policy expansion. In contrast, the renters in our analysis are often eligible for housing assistance but do not receive assistance due to the program's limited funding. These renters are *the policy hungry*, individuals who either are or near eligible for the policy but who instead are left vulnerable to experience detrimental policy spillover effects. The backlash that we observe from a natural constituency of expanding rental assistance risks undermining attempts to build a broad-based, renter coalition around policy expansion. This suggests that a more nuanced view of policy feedback is in order; policies can create adversaries out of those constituents who just barely miss out on the policy's benefits.

The application of our theory to LIHTC is a rare opportunity to understanding how policy shapes politics. First, causal identification of feedback effects if often difficult to establish. Individual-level data on policy uptake and behavior are restricted due to privacy concerns. Even overcoming these restrictions, identifying the indirect winners and losers requires observing how direct beneficiaries interact with the world around them. In contrast, a new LIHTC development is geographically fixed and its effects are spatially concentrated, allowing us to identify both voters treated by spillover effects and nearby untreated voters who are likely to be similar on unobservables. Second, common dependent variables are either attitudinal outcomes (e.g., external efficacy) or behavioral outcomes (e.g., turnout) too general to always predict their consequences for the policy of interest.[1] Our statewide housing bonds are meaningful pretreatment and posttreatment

[1] "Many existing feedback studies show the feed but not the back (or they just assume the back). Such studies

behavioral outcomes — votes dictating funding for housing assistance. That the siting of just one LIHTC development nearby affects vote choice for a \$2 billion bond suggests an array of other attitudinal and possibly behavioral outcomes among the indirect winners and losers.

In the remainder of this paper, we first detail our theoretical framework of the policy hungry and the policy adjacent using examples from other social welfare policies. Second, we provide background on the LIHTC program and how our theory explains feedback among LIHTC's neighboring residents. Third, we describe the data and construction of our dataset. Fourth, we explain our main identification strategies. Fifth, we present our main results and the results from robustness checks designed to bolster confidence in our empirical findings. We then probe a series of potential mechanisms that might explain our results. Finally, we discuss our findings and conclude.

2 Theory

Policy feedback is built around the ability of new policies to shape their own politics. Canonical examples include the ability of Social Security benefits to transform senior citizens from a low turnout demographic to an organized mass interest group (Campbell 2011). But much of what we know about policy feedback comes from the effects of the policy on the direct beneficiaries. While these recipients are expected to have the largest response to the policy, provided the benefits are both visible and traceable (Campbell 2012; Patashnik and Zelizer 2013), the focus on direct beneficiaries misses the local ecosystem created by policy spillovers.

For example, the Supplemental Nutrition Assistant Program (SNAP, formerly known as "The Food Stamps Program") provides electronic benefits to low-income families which 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 SNAP benefits use causes a 0.8% increase 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 *the policy adjacent.* Less commonly thought of as part of the coalition

show that policies affect the public in some way, altering attitudes or behaviors. But often such studies do not take the next step. They do not demonstrate that those attitudinal or behavioral patterns owing to program design affect subsequent policy outcomes" (Campbell 2012, 347).

in favor of SNAP expansion, retail grocers in low-income communities are apt to increase their support for the program once experiencing its spillover benefits.

But the increased ability to pay in the local grocery economy has a spillover costs as well. For low-income community members either just above the SNAP income eligibility threshold or no longer qualifying for the program due to strict time limits, this increase in local food prices is a concentrated burden. Already, the non-beneficiary may feel disempowered by not being viewed as worthy of help (Soss 1999). But if the non-beneficiary connects the price increase of groceries to the influx of SNAP benefits and believes they are unlikely to become eligible for SNAP benefits in the future, the non-beneficiary may see the expansion of SNAP as increasingly harmful to their well-being. These low-income shoppers are *the policy hungry.* While they are the natural constituency for supporting SNAP expansion, the vulnerable position of these non-beneficiaries combined with the SNAP's negative spillovers may demobilize them or persuade them that the program is not worth expanding without guarantee that they will be covered.

Our framework of the policy hungry and the policy adjacent extends to other social welfare policies. The Housing Choice Voucher Program (HCVP, formerly called "Section 8") provides vouchers to low-income individuals to subsidize their rental payments. The voucher is a payment to landlords 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. Landlords are not required to accept vouchers and audit experiments have found that most landlords prefer non-voucher holding applicants (Aliprantis, Martin, and Phillips 2021). However, the steady stream of benefits passing through voucher holders to landlords makes landlords — the policy adjacent — 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). The rationing of rental assistance means that most low-income renters are policy hungry, 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 and the renter believes that they are unlikely to receive a voucher due to the federal rationing of housing assistance, 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.

Whether the positive effects among the policy adjacent balance the negative effects on the policy hungry is likely context dependent. And whether the spillovers manifest as mobilization in turnout or persuasion in vote choice is unknown. To test our theory, we leverage the implementation of the Low-Income Housing Tax Credit.

2.1 The Policy Feedback of LIHTC Implementation

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.[2] 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. For scale, 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 (Diamond and McQuade 2019). Over 2 million units have been constructed or rehabilitated since the program's inception.

Even though LIHTC is a federal program, its impact — at least at first glance — is highly localized. LIHTC is much less visible to the general public than many public works projects (Pfeiffer 2009). Built by private developers, primarily funded through the tax system rather than more visible public budgets, and typically smaller in scale than the massive public housing projects of the 1960s and 1970s, LIHTC developments tend not to be immediately recognizable as affordable housing. They also tend to be sited in "less desirable" locations where they are unlikely to meet strong "Not In My Backyard" (NIMBY)-style opposition. A study of LIHTC siting decisions in Southern California found that, between 2000 and 2005, LIHTC units targeted to families tended

[2] 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.

to be located in high poverty, heavily Latinx neighborhoods (Pfeiffer 2009). Because LIHTC developments often fly under the radar to all but those directly affected or most geographicaly proximate to them, we focus on individuals living in immediate proximity to new LIHTC developments.

Like all public policies, LIHTC creates winners and losers, either in real or self-perceived economic or ideological terms. We anticipate that responses to LIHTC are conditioned by whether an individual perceives benefits or harms from LIHTC development near their home. When thinking about benefits and harms, our focus is on material self-interest in the short- or medium-term (i.e., Sears and Funk (1991)). Compared to ideology, self-interest has been found to be the primary decision-making driver in neighborhood-level politics (de Benedictis-Kessner and Hankinson 2019), particularly in the context of housing development and housing costs (Hankinson 2018; Marble and Nall 2021). The most obvious "winners" on the demand side are the direct policy winners who are allocated affordable units within the LIHTC housing. These are relatively few individuals, with the average LIHTC development having 67 units. Instead, most residents in the neighborhood of a LIHTC development are *indirect* policy winners and losers.

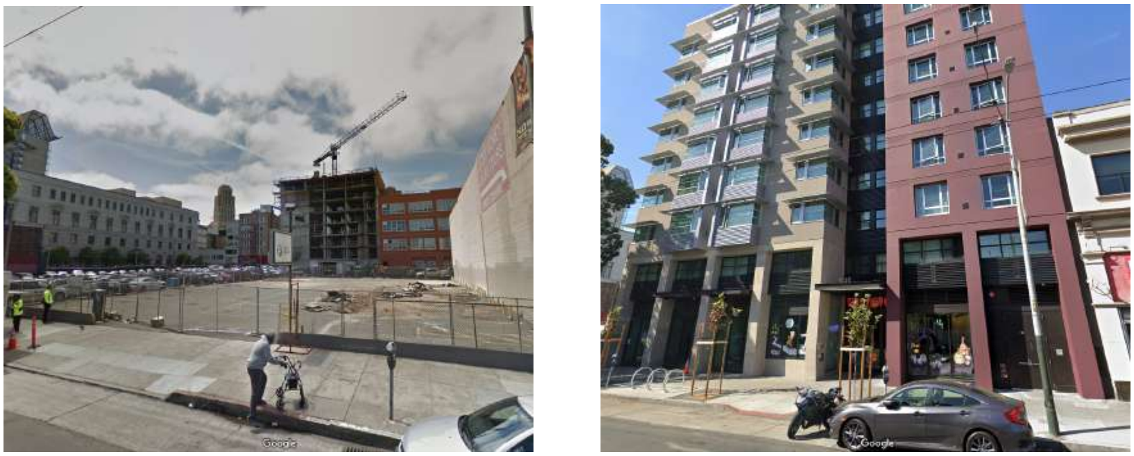
The indirectly affected are composed of two types: the policy hungry and the policy adjacent. Even though individual LIHTC developments prioritize local residents in the lottery for units, "local" is often defined at the city or even metropolitan level. Thus, while low-income renters living in market-rate units near the site of new LIHTC housing developments are likely eligible for housing assistance, they almost certainly will not receiving one of the newly constructed, affordable housing units. Instead, these renters — the policy hungry — are left to experience the spillover effects of the new LIHTC development on the local housing market.

In contrast, homeowners living nearby the affordable housing are the policy adjacent. While homeownership provides more housing stability, these homeowners are by no means wealthy. Like the policy hungry, they live in distressed or gentrifying areas. Though they own their own homes or live in high-homeownership neighborhoods, they likely lack substantial accumulated wealth outside of the equity in their homes. Though these groups are in many ways distinct — the policy adjacent, on average, have higher household incomes and are more likely to identify as white and non-Hispanic as compared to the policy hungry — they both experience some degree of housing insecurity, which makes them sensitive to local housing prices and conditions. This is largely a function of the fact that LIHTC housing tends to be built in disadvantaged neighborhoods.

The economic implications of a new LIHTC development in one's neighborhood is expected to affect these two groups differently. Diamond and McQuade (2019) finds that LIHTC development increases property values by replacing blight in economically depressed neighborhoods (e.g., Figure 1). If that is the case in our California data from 2002 to 2006, we expect homeowners — the policy adjacent — treated by LIHTC to become more supportive of the statewide affordable housing bonds. Meanwhile, if rising property values are perceived as driving gentrification and contributing to housing instability, renters — the policy hungry — should be less supportive of program expansion via the housing bonds. This is consistent with the idea that relative deprivation can generate political disgruntlement (Kosecy and Mo 2019). Of course, if program expansion were to guarantee that they would receive rent subsidization, then these renters may become more supportive.

Economic interests, of course, may not be the only factors at play. "Quality of life" considerations are relevant to all local residents, regardless of homeownership status. There is evidence that LIHTC development construction in distressed areas reduces crime (Diamond and McQuade 2019; Freedman and Owens 2011; Woo and Joh 2015). Though fewer studies on the topic exist, there is no evidence that LIHTC affects local school quality in distressed neighborhoods (Di and Murdoch 2013), though it is notable that LIHTC units tend to be concentrated in neighborhoods where local public schools are under-performing (Pfeiffer 2009). If voters attribute lower levels of crime to LIHTC construction, this should contribute increased levels of support for LIHTC among both the policy adjacent and the policy hungry.

Another consideration is racial threat (Key Jr 1949): residents might resist affordable housing on the basis that it will bring racial or ethnic minorities into their neighborhood. This type of backlash has been demonstrated in the context of (predominantly Black) public housing sited adjacent to (predominantly white) market-rate housing (Enos 2016). In the context of LIHTC, the expected implications of the racial composition of new developments on political behavior are expected to be conditioned by the demographics of the neighborhood. Where LIHTC is constructed in predominantly white neighborhoods, we would expect a negative backlash amongst homeowners and renters alike. Given that the average neighborhood in our study is majority non-white our expectations for the role of racial threat in this particular context are less clear. If anything, we might expect a negative backlash among the *policy adjacent* because they are more likely to be white and non-Hispanic. In areas where LIHTC developments improve property values, this would



(a) Before LIHTC (2016) (b) After LIHTC (2019)

Figure 1: 1036 Mission Street, San Francisco, CA

cut against homeowners' positive response to LIHTC, and bolster renters' negative response, if racial threat processes are at play.[3]

3 Data

To test our theory of the policy hungry and the policy adjacent through the implementation of LIHTC housing, we utilize the presence of two affordable housing bonds placed on the California statewide general election ballot in 2002 and 2006. We combine block-level voter returns with the geocoded locations of new LIHTC-funded housing projects built between these two elections. Below, we detail the structure and process for combining these datasets.

Dependent variable Our primary outcome of interest is the change in support between two $2+ billion housing bonds placed on statewide ballots in California, with the first in November 2002 (Proposition 46)[4] and the second in November 2006 (Proposition 1C)[5]. Both bonds were placed on the ballot by the Housing and Emergency Shelter Trust Fund Acts of 2002 and 2006. Helpfully, the two bonds were very similar in content and wording. In 2002, a $2.1 billion bond was set to provide:

[3] We note here that since policy adjacent areas in our data are, on average, 42% white, and policy hungry areas are 29% white, the effects of racial threat are likely to be muted. We have planned further analyses to explore this idea.

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

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

> 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, a $2.85 billion 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.[6] The Secretary of State's office has disaggregated these precinct-level returns to the Census block level to aid in the 2011 redistricting. Because the block level allows us to use Census data with minimal measurement error, we define our dependent variable throughout our analysis as the change in support for bonds from 2002 to 2006 at the block level.

Figure 2 shows the change in support for the housing bonds between the two elections at the block level. Observations are weighted by the average number of votes recorded on the housing bonds across both elections.[7] 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.

Along with support for the housing bond, the election data also allows us to account for other behavior outcomes stemming from new LIHTC development. First, comparing the number of votes to the number of registered voters in each election, we measure the block-level change in turnout across elections. Likewise, by comparing the number of votes to the number of votes on the housing bond in each election, we can measure the change in roll-off at the block-level. Both changes in turnout and roll-off may be responsible for changes we see in the block-level support for the housing bonds.

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

[7] 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.

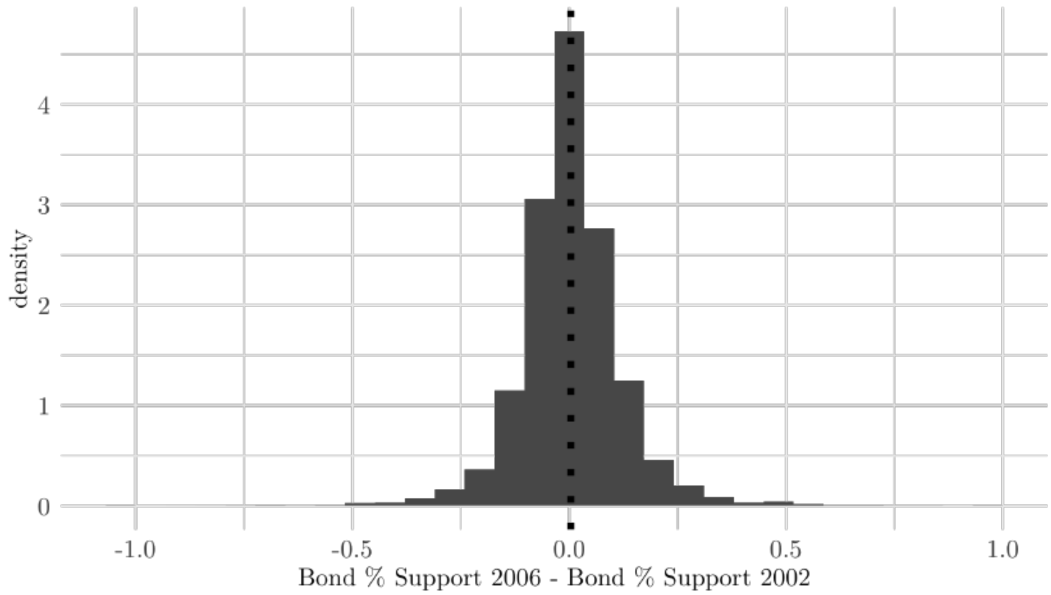


Figure 2: Histogram of block-level change in support for housing bonds from 2002 to 2006, weighted by the average number of voters across elections. Weighted mean change in support shown by dashed vertical line.

Treatment Our treatment of interest is the presence of a new, nearby LIHTC-funded affordable housing development 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. To isolate the influx of new people and density that comes with affordable housing, we only use projects which were new construction and drop those dedicated towards building rehabilitation.[8]

We obtain the location and number of units for every LIHTC-funded project from the HUD National Low Income Housing Tax Credit (LIHTC) Database, which covers the years 1987 to 2019. This database includes information about when credits were allocated as well as when the buildings they funded were placed into service. We define our treatment as LIHTC developments placed into service between 2003 to 2006, the window between the 2002 and 2006 November elections.[9] During

[8] 72% of of the California LIHTC projects in our 2003-2006 treatment window were new construction.

[9] 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 even non-physical changes in the area that may affect the long-term value of the large asset they are about to purchase. We believe it less likely that nearby homeowners and especially renters will be as informed about LIHTC credit allocation in a way likely to affect their voting behavior. It is more believable that nearby voters will be responsive to the physical changes in their environment, which come with the advent of the physical infrastructure. Likewise, the primary mechanism for contact

this window, 463 LIHTC buildings were placed into service in California. The median dwelling had 80 units, of which 73 (91% of units) were reserved for low-income residents.

Along with new development between the two elections, we also geocode LIHTC developments built from 1999 to 2002, allowing us to identify blocks which were treated by a LIHTC development just prior to the 2002 election. Such exposure to LIHTC prior to our "pretreatment" measurement would desensitize voters to the effects of new LIHTC, biasing our estimates downwards. We expect less biased treatment effects when we exclude these previously treated blocks. Additionally, we geocode LIHTC developments placed in service from 2007 to 2010 to identify areas deemed appropriate for LIHTC development but untreated during our period of interest between 2002 and 2006. These later treated blocks provide counterfactual controls for our treated blocks in our near-near design, discussed below.

Other variables To test our theory of the policy hungry and the policy adjacent in the absence of individual-level data, we rely on block-level homeownership data from the 2000 Census. We also compare the moderating effect of homeownership to other block-level covariates which are believed to shape local housing politics, including race and ethnicity, vacancy rates, and population density.[10] All of these Census variables are measured as of 2000, ensuring that they pretreatment.

For all moderating variables, we categorize blocks based on the tercile cutpoints of the variable among treated units. 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 $< 18\%$ as of 2000.[11] 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

or conflict theory is exposure to the residents of the affordable housing, suggesting we define treatment based on when each building is placed into service.

[10]Note that 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.

[11]For reference, the population weighted mean homeownership rate in our sample of blocks is 33%, compared to the statewide homeownership rate of 57% in 2000.

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 3 shows that there are relatively few blocks treated by LIHTC that meet this stricter definition of homeowner block. To account for the subjectiveness of the cutpoint, we present our results across a range of homeownership cutpoints in Appendix B.

Other datasets beyond the Census allow us to assess additional mechanisms. First, 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 purchases 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.[12] 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 pretreatment period to the posttreatment period.[13]

To refine the turnout analysis, we use a 2007 California voter file. The granularity of these individual level data allow us to draw even more precise treatment and control cutoffs rather than relying on outcomes aggregated to the block level. Additionally, the voter files enable us to measure treatment effects on voters that we know were registered at addresses surrounding future LIHTC developments prior to the 2002 election. This helps us account for the possibility of newly registered or recently relocated voters driving turnout effects.

Dataset construction Connecting multi-year voting returns, LIHTC locations, housing transactions, individual voter locations, and Census covariates requires converting across geographies. The foundation of the dataset is the California election data, which is dissolved and aggregated

[12]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.

[13]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.

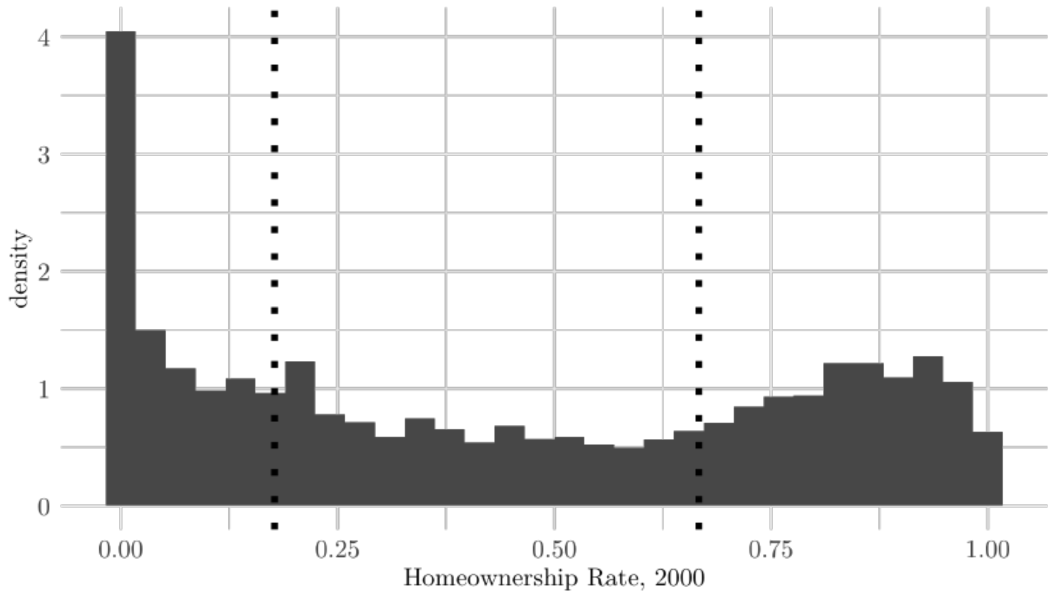


Figure 3: Histogram of block-level homeownership rate among treated units, weighted by the average number of voters across elections. Tercile cutpoints shown by dashed vertical line.

to the 2010 Census block level by the State of California to aid in the 2011 redistricting (McCue 2011). This aggregation avoids the unreliability of changing precinct locations and ad hoc precinct-bundling that can vary within even a 4-year election cycle.

Next, we geocode LIHTC developments and overlay their location on the 2010 Census block shapefile. We define treated and control blocks using a ring difference-in-differences method with a binary indicator for treatment. In our primary model (labeled "Binary"), blocks are considered treated if they are within 375 meters of a LIHTC development. Blocks that are between 375 meters and 600 meters away from the LIHTC development are used as the control group for our near-far design, as they are close enough to be comparable on observable covariates but far enough away to be unaffected by the new affordable housing. Selecting the appropriate radius for defining treatment and control requires identifying the point at which the spatially concentrated effects of the LIHTC development sufficiently decay. We explore this optimization further and present results across a range of distance bands in Appendix A.

As robustness checks, we also operationalize treatment as a continuous variable based the proximity of the block to the LIHTC development (labeled "Distance (cont.)"). We first measure the distance from each block to their respective LIHTC development. We then scale this distance to a [0, 1] interval by dividing each block's value by the maximum block-to-LIHTC distance. For intuitiveness, we subtract this scale distance from 1, making the treatment value for blocks that touch the LIHTC development equal to 1 and the treatment value for blocks at the maximum distance from the LIHTC development equal to 0.

As a third conceptualization of treatment, we calculate the cumulative number of new LIHTC units a block is exposed to between 2002 and 2006 (labeled "Units (cont.)"). Control blocks are coded as receiving 0 units. We scale our treatment by centering the variable and dividing by two standard deviations, making the estimate interpretable as ± 1 standard deviation around the mean ($\hat{\mu} = 83$ units, $\hat{\sigma} = 61$ units). Effectively, this scales the treatment of housing units so that is comparable to binary indicator while also leveraging the granularity of the continuous distribution (Gelman 2008).

Finally, we connect our pretreatment 2000 Census block-level data to the 2010 Census block shapefile using a crosswalk provided by the CA Statewide Database. We use a similar Census crosswalk to join 2000 tract-level data to the 2010 geographies for our mechanism tests. ZTRAX and voter file data are aggregated to the block level using a spatial overlay. The end result is a dataset of 21,719 unique blocks containing voting returns, treatment status, and a bevy of block- and tract-level characteristics.

These 21,719 unique blocks are fewer than the roughly 710,000 census blocks in California because our analysis requires that blocks are not only within 600 meters of a LIHTC development placed in service between 2003 and 2006 but also that blocks have votes recorded for both of the two housing bond propositions. Still, even with these constraints, the blocks in our analysis contain 9.6% of the population of California as of 2000. The residents living in these blocks are not representative of California as a whole. Using data from 2000, residents of these blocks are poorer, more racially diverse, and less like to be homeowners compared to the rest of California. This is because LIHTC developments are less likely to be approved in wealthier areas due to the strength of political opposition to locally unwanted housing development (Einstein, Glick, and Palmer 2020; Hankinson 2018; Hankinson and Magazinnik N.p.). As a result, the treatment effects are not generalizable to high-income areas where LIHTC is generally not sited, a limitation we consider furhter in our Discussion.

4 Identification

We measure the causal effect of LIHTC development on the policy hungry and policy adjacent using two analytical strategies: a "near-far" design and a "near-near" design.[14]

4.1 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 (Tobler 1970). Accordingly, we compare blocks that are treated by new LIHTC developments to nearby blocks assumed to be just outside of the influence of the new housing. As discussed, the radius at which the effect of LIHTC housing appears to fully decay is 375 meters. In defining the control group, we select blocks that are between 375 meters and 600 meters away from the LIHTC development.[15] Figure 4 shows a visualization of the near-far design in San Francisco, CA. Blocks outlined in red are treated units and blocks outlined in black are control units.

Because our theoretical predictions diverge for the policy hungry versus the policy adjacent, we subset the data to high-homeownership blocks ("homeowner blocks") and low-homeownership blocks ("renter blocks") based on terciles as described above. Section E.1 presents the pretreatment 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 balance suggests that within the 600 meter radius, the exact location of the LIHTC development is exogenous to common confounders in local housing politics. To help account for unobservables, we model the effect of a LIHTC development on our dependent variable using a LIHTC-level fixed effect. This approach effectively compares treated blocks within the 375 meter radius to control blocks just beyond the 375 meter radius surrounding the same LIHTC development. Our parametric approach also clusters Huber-White standard errors at the LIHTC level.

In defining our binary 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

[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.

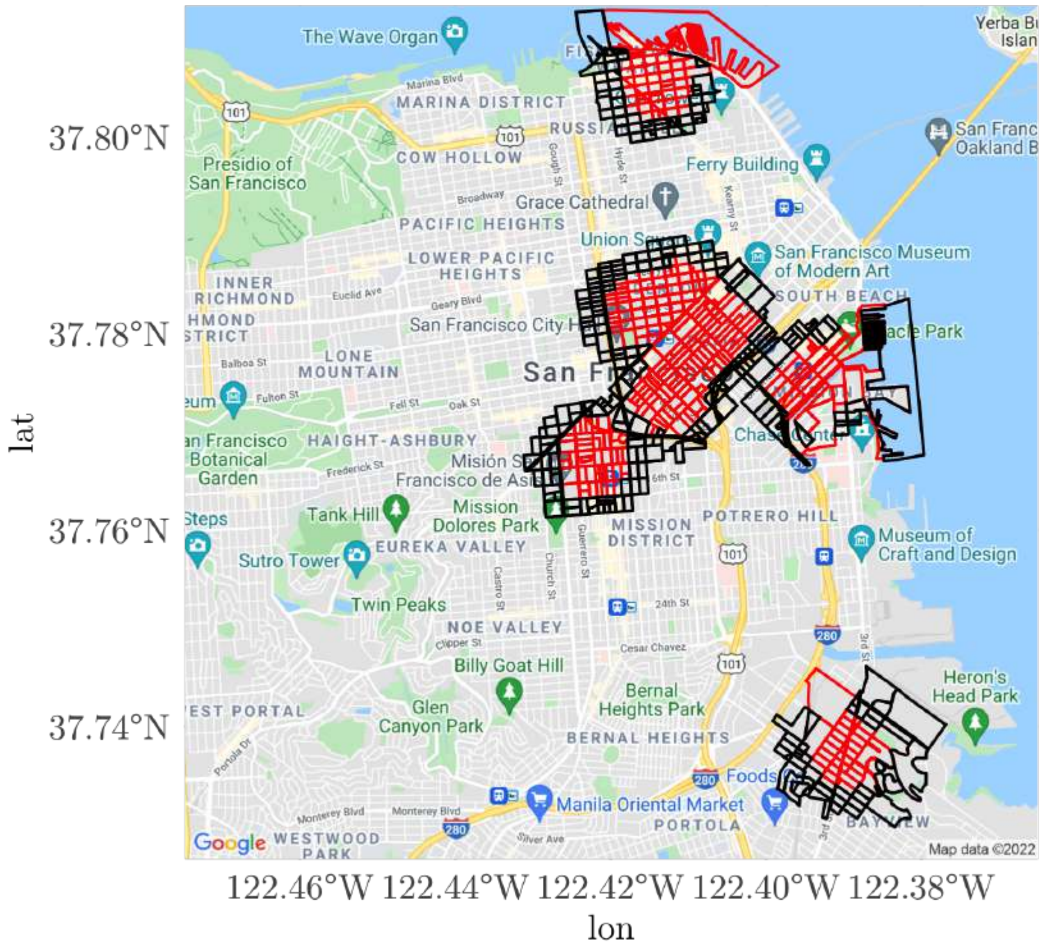


Figure 4: Visualization of the near-far design in San Francisco, CA. Treated blocks — those that are near LIHTC developments built in 2002-2006 — are drawn in red, control blocks — those that are far from LIHTC developments built in 2002-2006 — are drawn in black.

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 different 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.[16] For our continuous units treatment, we pool from all the total number of LIHTC units that any treated block is exposed to during the treatment window.

4.2 Near-Near Design

Although the near-far design assumes that the blocks just beyond 375 meters are the most comparable to those within a 375 meter radius, there is always the risk of selection based on unobservables. LIHTC developers may target specific areas within this 600 meter radius where new housing is politically feasible to permit, even after accounting for observable covariates. These areas — more amendable to LIHTC development — may differ politically in unobservable ways, confounding our causal estimates.

Our near-near design accounts for this possibility by comparing areas that are 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 equally politically amenable (or vulnerable) to new LIHTC development, but with different timings of treatment. Because the development in these later-treated block happens after the 2006 election, the blocks can serve as controls. The primary confounder of this near-near design would be if the timing of a LIHTC development to be either pre- and post-2006 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

[16] 90% of blocks only appear once in the near-far data. The maximum number of appearances for a block is six times (<.05% of all blocks), meaning a few blocks were within 375 meters of six LIHTC buildings placed in service between 2002 and 2006.

LIHTC funding each year suggests a large degree of chance in when a project is funded. Thus, whether a LIHTC development is placed in service between 2002-2006 or 2007-2010 is relatively unlikely to be a matter of the on-the-ground politics of nearby residents.

Figure 5 shows a visualization of the near-near strategy, with blocks outlined in red as treated units and blocks outlined in black as control units. Like the near-far design, the fixed effects model 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.

Because the near-near design limits us to blocks within 375 meters of LIHTC developments, we can no longer use on a LIHTC-level fixed effect to help account for unobservables. Instead, our parametric models include a CBSA-level fixed effect, allowing us to compare treated and control blocks that are within the same narrowly defined metropolitan area. Supporting the assumption that blocks near 2003-06 LIHTC developments are comparable to blocks near 2007-10 LIHTC developments, Section E.2 shows the weighted mean difference in our block-level covariates within homeownership terciles and the sample as a whole. Like the near-far design, our treated and control units are comparable on observable pretreatment covariates within both the homeowner tercile and the renter tercile.

4.3 Model Specifications

Both the near-far and near-near designs are built on the same model foundation. First, we show a non-parametric difference-in-means in change in support for the housing bonds within each design. Next, we present parametric results from OLS models regressing the change in support for the housing bonds on a binary treatment of new, nearby LIHTC development. All OLS models include the fixed effects described above, i.e., LIHTC fixed effects for the near-far design and CBSA fixed effects for the near-near design. Both designs use Huber-White standard errors clustered at the LIHTC-development level. Across both designs, our unconditional results — i.e., those pooling all terciles of homeownership — are null (Tables C-1 and C-2).

However, our theory predicts divergent effects among homeowners and renters. To test our

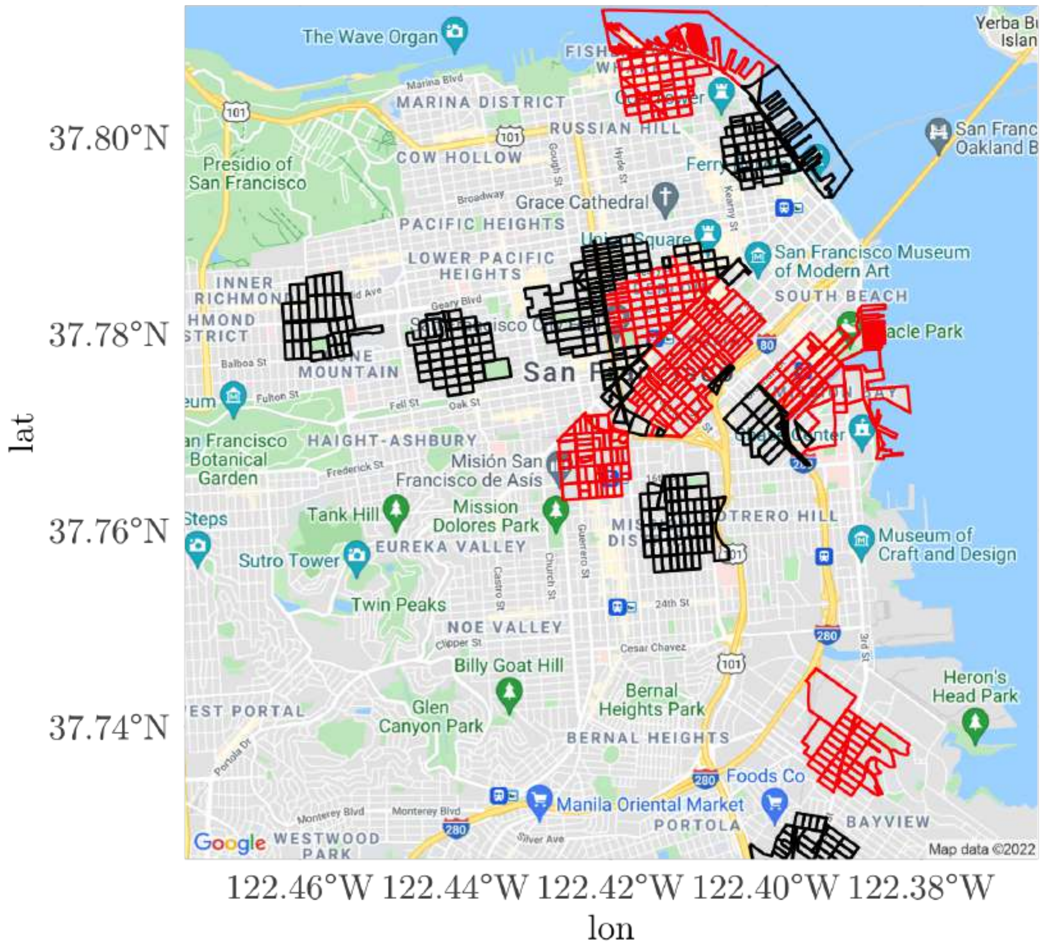


Figure 5: Visualization of near-near design in San Francisco, CA. Treated blocks — those that receive LIHTC developments in 2002-2006 — are drawn in red, control blocks — those that receive LIHTC developments in 2007-2010 — are drawn in black.

theory of the policy hungry and the policy adjacent, we include interactions between treatment and homeownership terciles, dropping the middle tercile. The homeownership tercile interaction is a binary variable, with 0 if the observation is a homeowner block and 1 if the observation is a renter block. Thus, when there is a significant interaction, our main treatment effect represents the relationship in homeowner blocks. To measure the treatment effect in renter blocks, we add the lower order treatment term with the tercile interaction and calculate the pooled variance. This approach allows us to compare treatment effects in homeowner blocks to effects in renter blocks.

Reviewing our analytical approaches, our near-far design generates 4 models. Rather than favor one result over the other, we visualization results from all four models in the main text. The narrow range of treatment effects increases our confidence in the robustness of our results. The four near-far models are:

1. **Binary** - The treatment is a binary based only on spatial proximity. Units are coded 1 if $\leq$375 meters away from LIHTC, 0 if 375 to 600 meters away from LIHTC. The interaction with renter blocks is a binary variable (1 = low homeownership tercile, 0 = high homeownership tercile).

2. **Distance (cont.)** - The treatment is continuous variable based only all spatial proximity. Units are coded as a value between 0 and 1, with 1 signifying the maximum distance away from the LIHTC development. The interaction with renter blocks is a binary variable (1 = low homeownership tercile, 0 = high homeownership tercile).

3. **Units (cont.)** - The treatment is a continuous variable based on the number of units a block is cumulatively exposed to. Exposure is based on the binary definition of spatial proximity (1 if $\leq$375 meters away from LIHTC, 0 if 375 to 600 meters away from LIHTC). Values of the treatment variable are standardized by subtracting the mean value and dividing by 2 standard deviations, producing a range from -0.23 to 4.94. The interaction with renter blocks is measured as a binary (1 = low homeownership tercile, 0 = high homeownership tercile).

4. **Interaction (cont.)** - The treatment is a binary based only on spatial proximity. Units are coded 1 if $\leq$375 meters away from LIHTC, 0 if 375 to 600 meters away from LIHTC. The interaction with renter blocks is a continuous variable based on the percent of households in

a block that are renters, ranging from 0% to 100%.

Unlike the near-far design, we cannot use the “Distance (cont.)” specification with the near-near design because the control units are not spatial proximate to their comparable treated units. Again, rather than favor one result over the other, we visualization results from all three models in the main text.

For each analysis, we present the same model using increasingly stringent subsets of the full data. We first show the results of the model using the full sample of blocks that meet each design’s criteria (“All”). But because new LIHTC developments are constantly being built, some of our blocks may have been treated by a new LIHTC development just prior to the 2002 election. This recent exposure to new LIHTC may have not only affected our “pretreatment” value of support for affordable housing in the 2002 election, but also desensitized residents to new LIHTC construction. Both effects would likely attenuate the impact of new nearby LIHTC development that occurred during our treatment window of 2002 to 2006. Consequently, our second model in each table shows the estimated effects only on blocks which were not treated in the four-year period preceding the 2002 election (“Clean”).

The impact of LIHTC housing may also be larger for developments of substantial size, which are likely to be more noticeable and have greater implications for the local housing market and neighborhood conditions. The third model in each table shows our results when only looking at LIHTC developments that are larger than the median LIHTC development in our sample, $\geq$80 units (“Big”). Finally, we present results from our model on the union of these two conditions (“Big Clean”). In almost every table, we see larger estimated effects of LIHTC development as we use these increasingly stricter sample criteria.

5 Results

We first present results for our near-far design, using both parametric and non-parametric approaches. Next, we present results from the near-far design. We continue by discussing robustness checks, including a placebo test and an assessment of whether our conditional results are driven by cleavages other than homeownership. Additionally, we test the sensitivity of our results to changes in the distance band defining treatment and the tercile cutpoints defining homeowner and renter blocks. We close by assessing whether our results are driven by changes in the residential churn.

5.1 Near-Far Design

We first show our the non-parametric difference-in-means between our treated and control units among homeowner and renter blocks, respectively. Table 1 shows our dependent variable, the change in support for the housing bonds, as a voter-weighted mean within both the treated and control group. Starting with homeowner blocks (top row), we find that treated blocks on average increased their support for the housing bonds by 1.6 percentage points whereas control blocks only increased their support by 0.5 percentage points. The difference in means between treated and control blocks is 1.1 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 0.9 percentage points while the average control renter block increased support for funding by 0.3 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 1.1 percentage points compared to renter blocks farther away.

	Treated	Control	Difference
Homeowners	0.016	0.005	0.011
	n = 2,524	n = 2,965	
Renters	-.009	0.003	-0.011
	n = 2,370	n = 1,858	

Table 1: 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.

Our parametric models tests the stability of these estimates by comparing blocks surrounding the same LIHTC development via a LIHTC fixed effect. Figure 6 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 0.7 to 1.4 percentage points. For a sense of scale, this is a roughly 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 the more averse to affordable housing than renters — increase their support for funding affordable housing once exposed to its implementation.

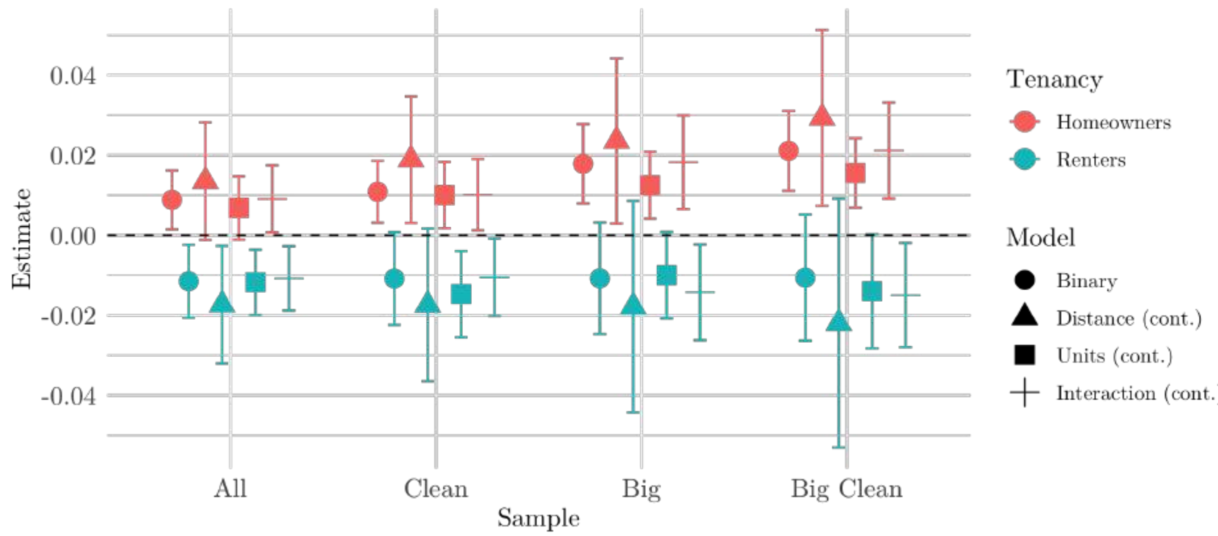


Figure 6: Results from the near-far design across four model specifications.

The data on predominantly renter blocks tell a different story. The estimated coefficient on the interaction between indicators for treatment and renter blocks is negative and twice the magnitude of the coefficient on the treatment indicator. Combining the interaction with the lower order treatment term, a new, nearby LIHTC development causes voters in renter blocks to decrease their support for the funding affordable housing by 1.1 to 1.7 percentage points.

Moving across Figure 6, the effect of LIHTC grows with each more stringent subset of data. Removing blocks which had been treated in the lead-up to the 2002 election increases the point estimate by a small degree ("Clean"). Subsetting to only developments with ≥80 units predictably cuts our number of clusters — LIHTC developments — in half, but also doubles the size of the estimated treatment effect among homeowner blocks ("Big"). The combination of these two restrictions reveals the largest effects, with treated homeowners increasing support for affordable housing bonds by 1.6 to 2.9 percentage points on average ("Big Clean"). Across all specifications, the estimated causal effect of LIHTC among renter blocks remains stable between a 1.0 to 2.2 percentage point decrease in support for the housing bonds.

5.2 Near-Near Design

For the near-near design, we again start with our non-parametric difference-in-means between treated and control units in Table 2. 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 even more confident in the analytical strategy.

Starting with homeowner blocks (top row), treated blocks on average increased their support for the housing bonds by 1.6 percentage points whereas control blocks only increased their support by 0.4 percentage points. The difference in means is 1.2 percentage points, a treatment effect similar to both the non-parametric 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 0.9 percentage points while the average control renter block experienced no change in support. Again, the renter difference-in-means of -0.9 percentage points is comparable to the estimates from the near-far design.

	Treated	Control	Difference
Homeowners	0.016	0.004	0.012
	n = 2,524	n = 1,858	
Renters	-.009	0.000	-0.09
	n = 2,370	n = 1,927	

Table 2: 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.

Our parametric model tests the stability of these estimates when comparing treated and control blocks within the same metropolitan area via a CBSA fixed effect. Figure 7 shows estimated treatment effects not only consistent with the nonparametric estimates but also with the near-far design (Figure 6). On average, a new, nearby LIHTC development causes homeowner blocks to increase their support for the housing bonds by 0.7 to 1.3 percentage points. But the effect is unique to homeowner blocks, with renter blocks experiencing a 0.9 to 1.6 percentage point decrease in bond support. While this matches the non-parametric estimate, the renter effects are generally not statistically significant.

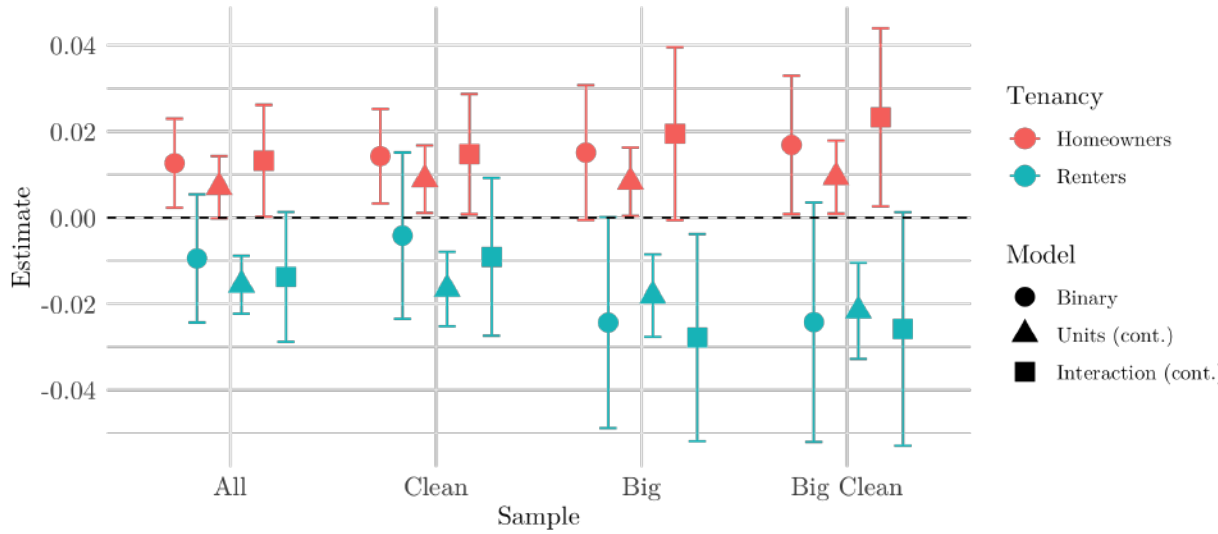


Figure 7: Results from the near-far design across three model specifications.

Moving across Figure 7, the treatment effect and interaction grow in magnitude. Large LIHTC developments show larger, but noisier estimated effects on homeowner blocks ranging from 0.8 to 2.3 percentage points. Combining the treatment and interaction terms, renter blocks also experience substantially larger effects with a 0.4 to 2.8 percentage point decrease in affordable housing support

5.3 Robustness Checks

Our results suggest that the construction of a LIHTC-funded affordable housing development has a positive effect on homeowner support for affordable housing funding and a negative effect on renter support for affordable housing funding. While the near-far design shows the blocks most comparable to each other based on spatial proximity, the near-near design compares blocks most similar in their likelihood of being treated by LIHTC development conditional on timing.

To further account for a potentially spurious relationship, we conduct a placebo test by replicating the near-far design using LIHTC developments built between 2007 and 2010. Because these LIHTC developments occurred 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 C-9 shows this to be true. Homeowner and renter blocks treated between 2007 and 2010 show estimated effects between border a null effect and they are not statistically significant. The null effects from our placebo model support the assumption that there is not something inherently different between "near" and "far" blocks driving our dependent variable in the absence of treatment. Instead, it is the presence of a new nearby affordable housing which changes voter preferences towards the housing bond.

Additionally, we have built our theory on homeownership as the primary cleavage in the response to a new nearby LIHTC development. To validate this claim, we repeat our near-far and near-designs on our other block-level covariates: racial demographics (% non-Hispanic white, % Black, % Latinx), vacancy rate, and population density. While variables occasionally show significant interactions along the high and low values, none are as large, stable, and consistently statistically significant as homeownership.

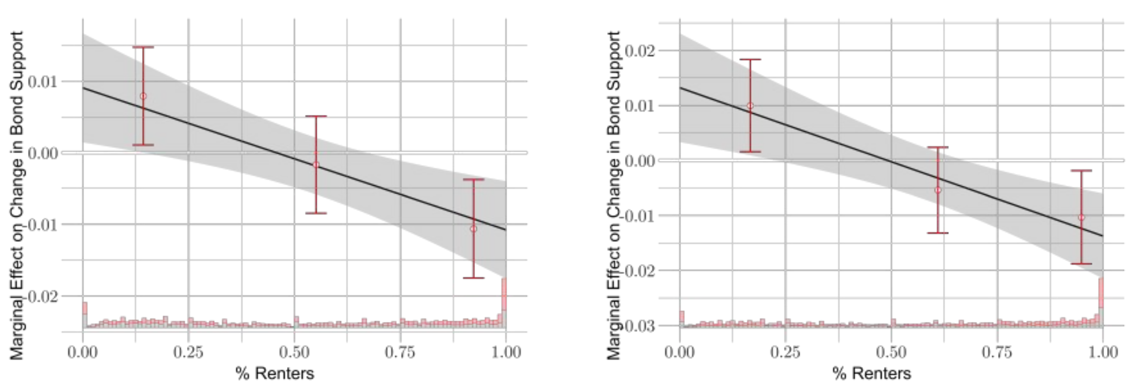
We conduct additional sensitivity analyses to see how our results respond to different distance band cutpoints and tercile cutpoints. Appendix A replicates our near-far and near-near designs with the binary treatment moving the distance radius defining treatment from 250 meters to 400 meters in 25 meter increments. Results are consistent up until 425 meters at which point the treatment effects appear to decay. This is expected. Once the treatment radius expands beyond the extent of the spatial effect of LIHTC, the "treated" group will begin to include blocks which should be categorized as control units. This would bias our effects downwards, as reflected in the figures.

We also repeat our analyses on the 375 meter distance band specification but moving the tercile cutpoints which define our homeowner blocks and renter blocks. As stated previously, a tercile cutpoint which is too extreme will leave too few units to estimate an effect, while a tercile cutpoint which is too moderate risks diluting the composition of the conditional effect we are trying to estimate. Appendix B shows that our results are stable across cutpoints 5 percentage points higher or lower than the natural tercile cutpoint produced by the distribution of treated blocks. Even cutpoints 10 percentage points above or below our preferred settings produce substantively similar results.

Additionally, to show that homeownership cutpoints does not drive our results, we operationalize the homeownership interaction as a continuous variable. Using the `interflex` package (Hainmueller, Mummolo, and Xu 2019), we plot treatment effects across the full range of block-level homeownership rates using the fixed effects models discussed above and all of our block-level observations ("All"). Figure 8 plots our marginal effects first as a line showing the statistically significant continuous interaction of homeownership with our binary treatment. Then, to account for potential non-linearities, the package bins our treatment effects by terciles of block-level homeownership. This approach shows near identical effects in both the near-far and near-design: a 1 percentage point increase in support among homeowners and a comparable decrease among renters.

An additional challenge to estimating the causal effect of LIHTC on voter behavior comes from residential churn. If new LIHTC development causes those least tolerant of affordable housing to move away, then the positive effect of LIHTC on support for the housing bonds may be an artifact of replacement, not persuasion or mobilization. Presumably, these "exiters" would be replaced by residents more tolerant of the affordable housing, causing the average support for an affordable housing bond to rise when measured at the block-level. We assess this possibility by aggregating the number of housing transactions that occur in these blocks between the two elections using ZTRAX data. We then run the same models as before, but 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 C-3 and C-5 show the effect of a LIHTC development on the change in block-level turnout using the near-far and near-near designs, respectively. We see negative or null but not statistically significant effect estimates among homeowners. This suggests that we should not be too concerned that our positive homeowner effect is being driven by an increase in replacement. Renter blocks show similar null effects, as the interaction is not statistically significant. However, using transactions as measure of churn is less reliable for renters as a landlord can easily rent



(a) Near-Far ("All") (b) Near-Near ("All")

Figure 8: Homeownership rate as continuous variable with LIHTC as binary treatment using `interflex` package.

the property to a new tenant without ZTRAX recording a new transaction. Still, it is unlikely that LIHTC's negative effect on renters is driven by residential churn. Such an explanation would require renters who leave at higher rates in response to nearby LIHTC development to be replaced by renters *even less* tolerant of affordable housing.

6 Mechanisms

We have shown, across a variety of specifications, that high-homeownership neighborhoods that receive new LIHTC developments subsequently become more supportive of additional public spending on affordable housing, while low-homeownership neighborhoods become less supportive. Though our theory supposes that these effects are driven by changes in the behavior of individual voters, our ability to test this directly is hindered by our inability to observe individual voting decisions. We are constrained by the data to make ecological inferences based on block-level voting behavior, which are not necessarily correct. In particular, changes in the composition of the electorate from the pretreatment to posttreatment 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. We test whether our findings are driven by mobilization and differential roll-off.

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.

Tables D-7 and D-9 show the estimated effect of a LIHTC development on the change in block-level turnout using the near-far and near-near designs, respectively. Point estimates are small and none reach statistical significance. However, the estimated treatment effects on homeowner blocks are consistently negative and the interaction term for renter blocks is generally positive. Treatment as a continuous number of units shows a similar relationship, but with statistically significant estimates for treatment in the near-near design (Tables D-8 and D-10). The variability of these effects and general lack of statistical significance make its unclear whether turnout may be driving 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.[17] 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 D-15 and D-16). Using the near-far design, voters in homeowner blocks are 1.4 to 1.6 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.

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

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

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. However, because of the inconsistency in size and direction of the effects, we are unconvinced our results are explained by differential roll-off between treated and control blocks.

6.1 Homeowners and Race

After exploring mechanisms for how LIHTC development may be affecting voter behavior to produce a change in block-level support for affordable housing, our next question is *why* these changes occur? Why would homeowners increase their support for funding affordable housing, which they traditionally oppose when it comes to their own neighborhood?

A wide array of empirical work has found that new affordable housing increases nearby property values. However, Diamond and McQuade (2019) also find that LIHTC development causes, on average, a 2 percent decrease in home prices in relatively wealthier and whiter areas, which they define as neighborhoods that are greater than 50% non-Hispanic white and have median household incomes of more than $54,000 as of 2012. While the authors are unable to disentangle whether these effects are driven by the appearance of a new physical structure in the neighborhood or by the racial composition of those who move into the new housing, their findings signal the importance of race in how current residents respond to affordable housing. Still, it is challenging to empirically separate the effects of "racial threat" or racial discrimination more broadly from those of fluctuations in property values, which can themselves reflect changes in the demographic composition of an area (e.g., Clapp, Nanda, and Ross 2008). Because this effect is not found in majority non-white neighborhoods of similar wealth, it is possible that the racial integration brought by affordable housing is driving down the market value. While the exact mechanism is unclear, we test whether a similar pattern of results can be found in the policy feedback effects of new LIHTC housing. Do well-off, majority white neighborhoods show a differential response to affordable housing compared to similarly wealthy non-white neighborhoods?

We assess the role of race in wealthy, high homeownership neighborhoods by interacting the treatment – first as a binary indicator and then as a continuous measure of the number of LIHTC units – with an indicator for whether the neighborhood is >50% non-Hispanic white. For this analysis, we restrict the sample to blocks that fall within the top tercile of household median income (>$46,898 in 2000 dollars).[18] The interaction captures neighborhoods that are >50% non-Hispanic white, matching Diamond and McQuade (2019)'s cutpoint. To create contrast but also preserve sample size, the base category includes similarly high-income, homeowner blocks that are <20% non-Hispanic white. However, our Interaction (cont.) model uses the full range of % non-Hispanic white.

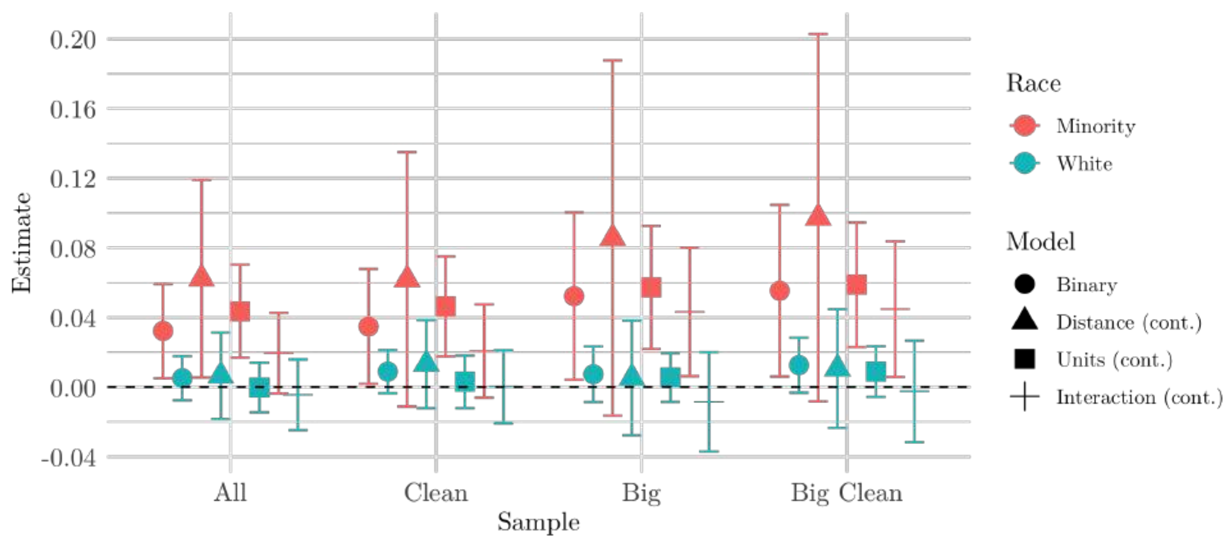


Figure 9: Results from near-far design for homeowners and race analysis.

Regardless of whether new LIHTC developments are operationalized as binary or continuous, results from the empirical models tell a similar story: the predominantly non-white blocks are driving the positive effects of LIHTC construction on homeowner support for affordable housing bonds. Figures 9 and 10 show the estimated effect of treatment operationalized across our multiple model specifications. Large positive effects of near identical size are found among non-white areas using both the near-far and near-near designs. Meanwhile, majority white blocks show small and always null effects that are often statistically different than those of the minority blocks. These findings are consistent with the idea that demographic change brought about by new affordable

[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 Tabla 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 distribution of California as a whole.

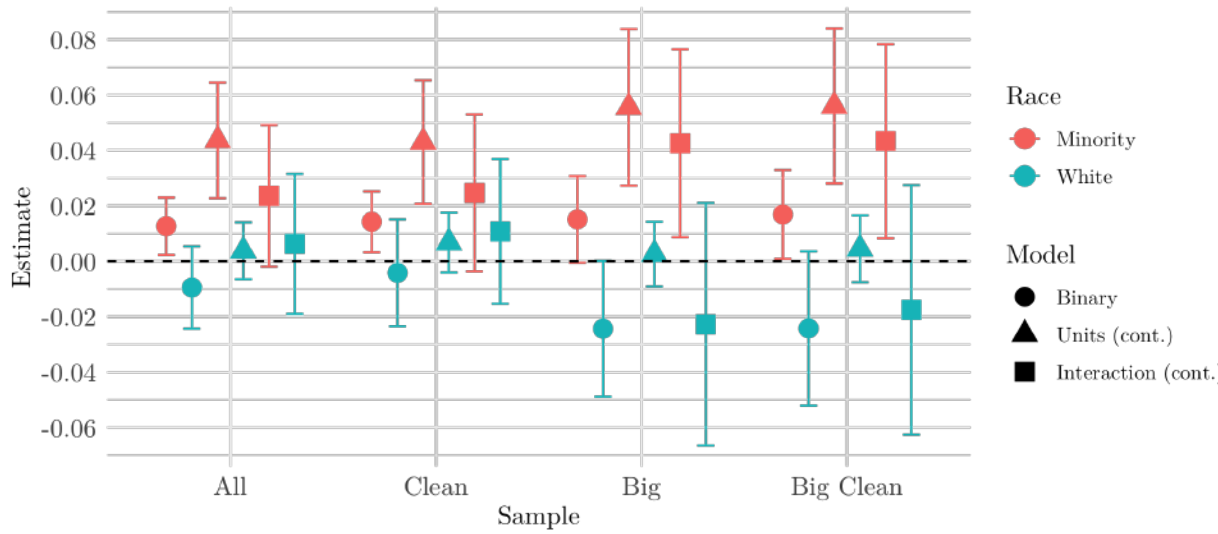


Figure 10: Results from near-near design for homeowners and race analysis.

housing construction has an impact on support for future affordable housing. LIHTC developments sited in wealthier, whiter areas may generate a backlash among homeowners. This tendency may limit the positive policy feedback effects that we observe among the policy adjacent.

6.2 Renters and Gentrification

One theory for why treated renters would oppose new affordable housing bonds is because they associate the new LIHTC affordable housing with gentrification, creating a causal attribution to the new development. Renters in cities with a high housing prices have been found to oppose nearby market-rate development because they attribute the new supply to increasing their housing instability (Hankinson 2018). If new LIHTC housing increases nearby property values by replacing blight, policy hungry renters may also see the affordable housing as against their short-term self-interest. Applied to our results, if we find that the negative effect among renters is concentrated in gentrifying neighborhoods where renters are most anxious about rising prices, that would support our theory of renter's prioritizing short-term self-interest in opposing funding more affordable housing.

We first identify blocks which were gentrifying from 1990 to 2000, the pretreatment run-up period to the 2002 election. Following the best practices of the gentrification literature, we first identify blocks deemed to be eligible for gentrification as of 1990. The gentrification literature builds their definitions based on data only available at the tract level, requiring us to assign block-level attributes based on the outcomes of their tracts. Building on prior approaches to measure gentrification (Freeman 2009; Wyly and Hammel 1999), Hwang (2020) defines a tract as eligible for gentrification if the tract's median income in 1990 is below its city's median income.[19] Using Hwang (2020)'s definition, 67% of our blocks are in a tract eligible for gentrification as of 1990. Ultimately this criteria does not matter, as 100% of our renter blocks are gentrification eligible.

To identify tracts which underwent gentrification from 1990 to 2000, Hwang (2020) specifies two conditions:

1. The tract's median rent or home value increases more quickly than its city's.
2. The tract's % with at least a BA or median household income increases more quickly than its city's.

If both conditions are met over the ten-year period, the tract is considered to be gentrifying. From 1990 to 2000, 62% of our renter blocks qualify as gentrifying. To assess whether gentrification matters for renter response to LIHTC, we run our near-far and near-near models on renter blocks only and include an indicator for whether that block was gentrifying from 1990 to 2000.

Using the near-far design, renter blocks in non-gentrifying tracts show essentially no response to LIHTC (Figure 11). In contrast, combining the treatment effect and interaction suggests that renter blocks in gentrifying tracts decrease support for the housing bond by 2.2 to 3.5 percentage points when exposed to a LIHTC development. The near-near model again shows near null effects for non-gentrifying tracts and a large negative interaction with gentrifying tracts, but the interaction is not statistically significant (Figure 12). Importantly, in both the near-far and near-near designs, gentrification has no interaction with the change in support among homeowner blocks. These results provide evidence that gentrification is a key condition causing the policy hungry to oppose the very policy meant to help improve housing instability.

7 Discussion

We show that voters respond to the construction of new, nearby affordable housing when asked to fund affordable housing in the abstract, and that this effect is largely conditioned by economic im-

[19]This limits gentrifiable tracts to those within a Census-designated place.

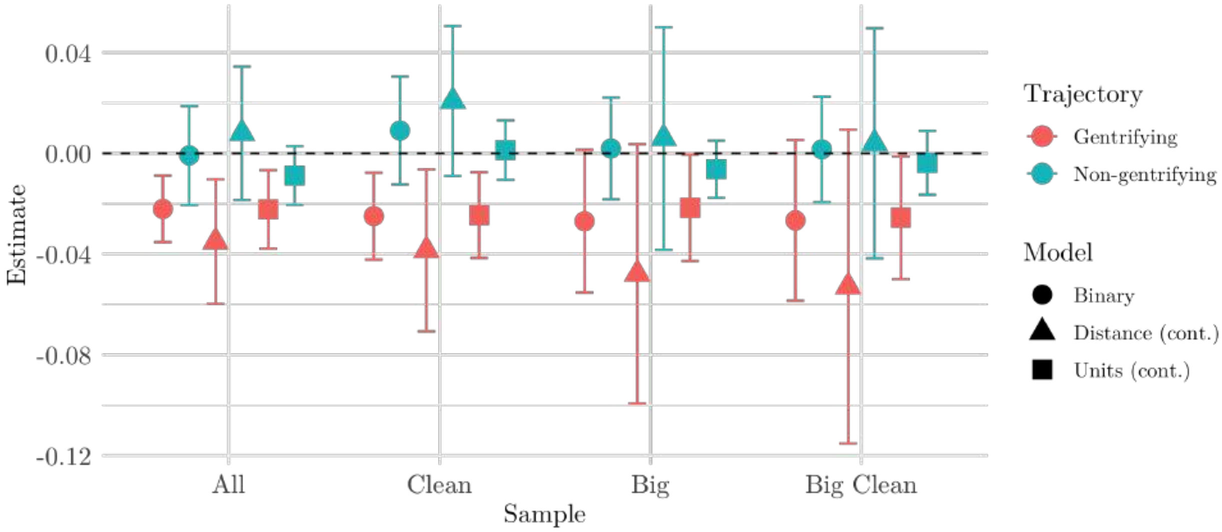


Figure 11: Results from near-far design for renters and gentrification analysis.

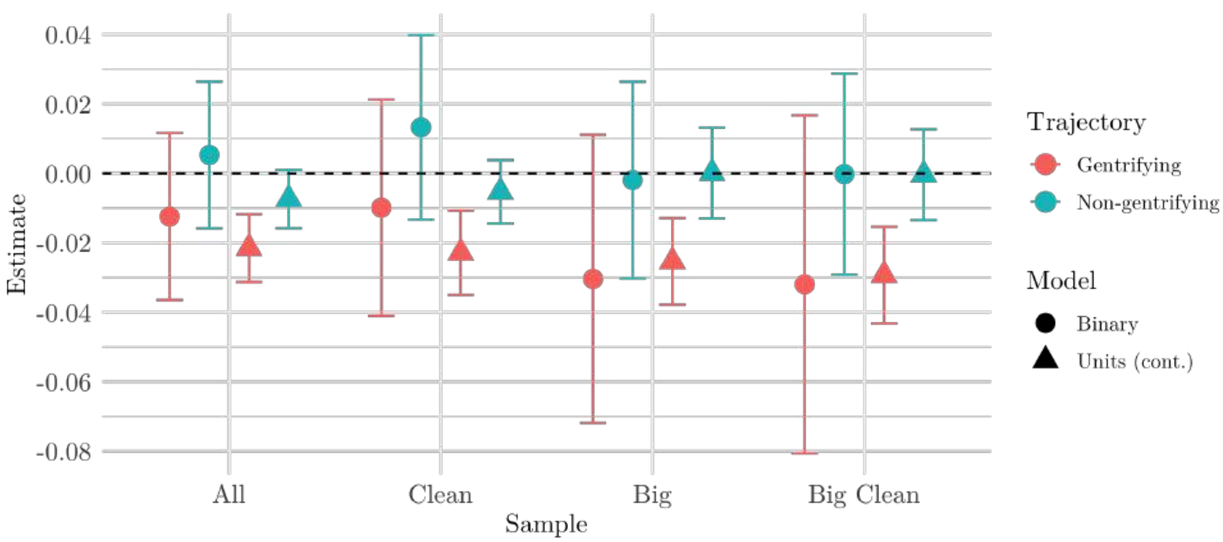


Figure 12: Results from near-near design for renters and gentrification analysis.

plications. To the extent that affordable housing represents a physical upgrade to or investment in the local neighborhood, homeowners stand to benefit from increasing property values. Meanwhile, the effects of affordable housing can be more of a detriment for renters, who see rising property values as a threat due to rising rents. This represents an uncomfortable quandary: building affordable housing provides a direct benefit to a few lucky recipients, but potentially harms (or is perceived to harm) a larger but still economically precarious group: renters in depressed areas who may get priced out of the neighborhood.

Because our outcome of interest — change in support for affordable housing bonds on the ballot — is directly linked to the experience of new nearby affordable housing, we are uniquely positioned to study policy feedback effects. That a treatment as small as one LIHTC development changes down-ballot voting behavior in a consistent way suggests that housing policymakers consider policy implementation as strategic coalition building. While homeowners could be mobilized to expand housing, renters in gentrifying areas may need greater prioritization in getting first access to new units. At the same time, we surmise that the direction of these policy feedback effects are a function of where the housing is being sited: depressed areas. We largely set aside the processes that lead affordable housing to be built in depressed or stagnating areas (Trounstine 2018). We suspect — though cannot directly test — that responses to the economic impact of new affordable housing built in higher income areas would be quite different.

More broadly, our results show how the implementation of policy with spatially concentrated spillover effects can manifest as indirect policy feedback in unexpected ways. We have shown how eligibility thresholds and rationing can leave the vulnerable exposed to the effects of the benefits on the local economy. In this way, implementation can alienate the policy hungry, the natural constituency expected to most favor policy expansion. At the same time, we have shown how the positive spillover effects of benefits can convert unexpected supporters, the policy adjacent. We recommend that this framework for understanding the feedback effects of policy spillovers be applied to the long-term viability of not only social welfare programs, but any policy with spatially concentrated costs and benefits.

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A Sensitivity Analysis - Distance Band

The following figures show the treatment effects based on different radii for defining treated blocks versus control blocks. 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 375 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 (375 meters). The cutpoints are >66.7% homeownership rate for homeowner blocks and <17.7% homeownership rate for renter blocks

A.1 Near-Far Design

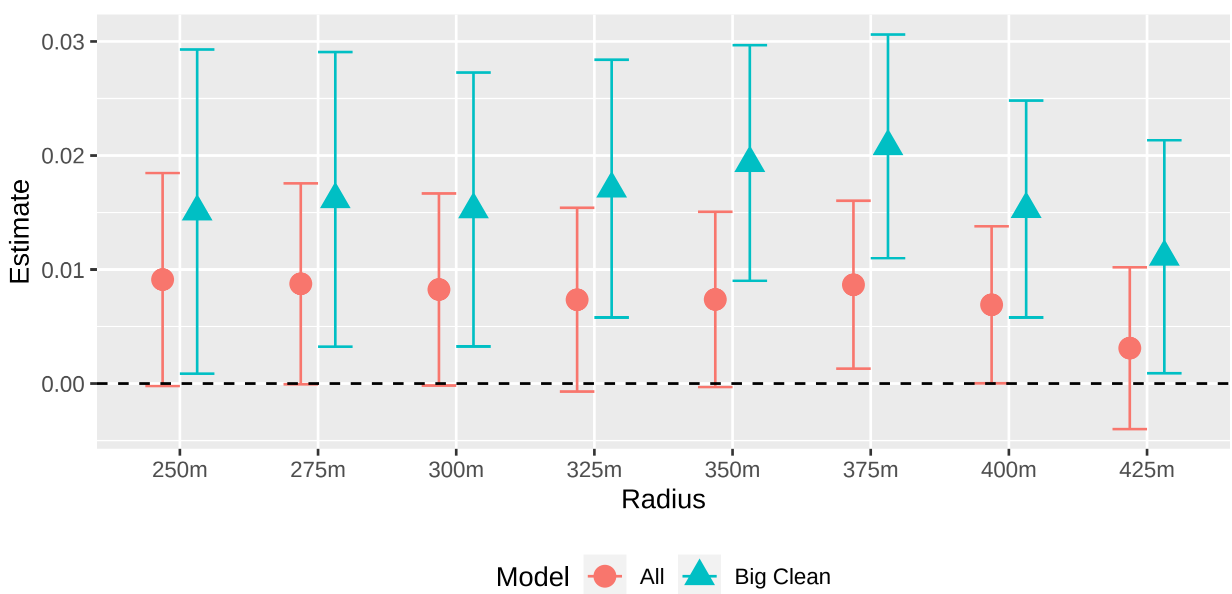

Figure A-1: Sensitivity of results for homeowner blocks across various distance band specifications. Near-far design.

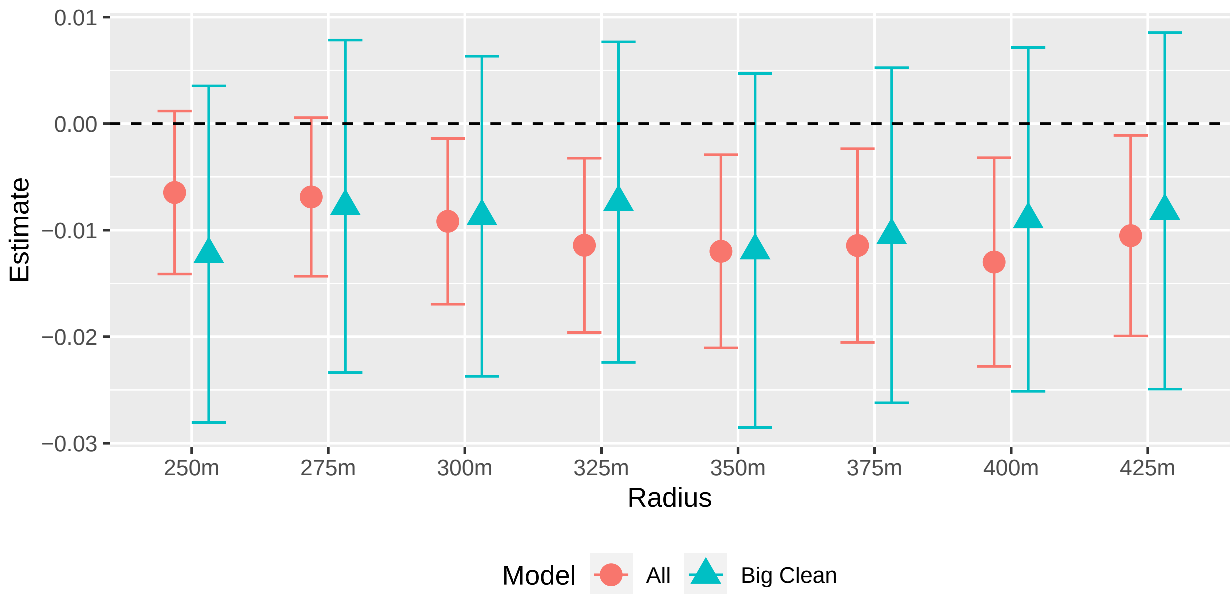

Figure A-2: Sensitivity of results for renter blocks across various distance band specifications. Near-far design.

A.2 Near-Near Design

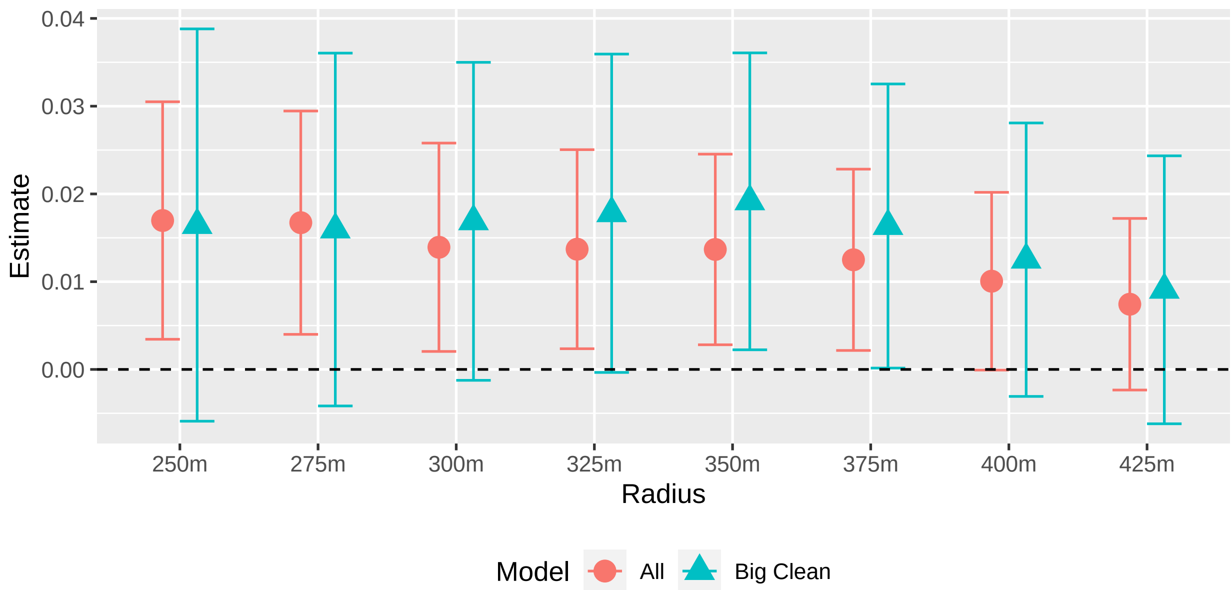

Figure A-3: Sensitivity of results for homeowner blocks across various distance band specifications. Near-near design.

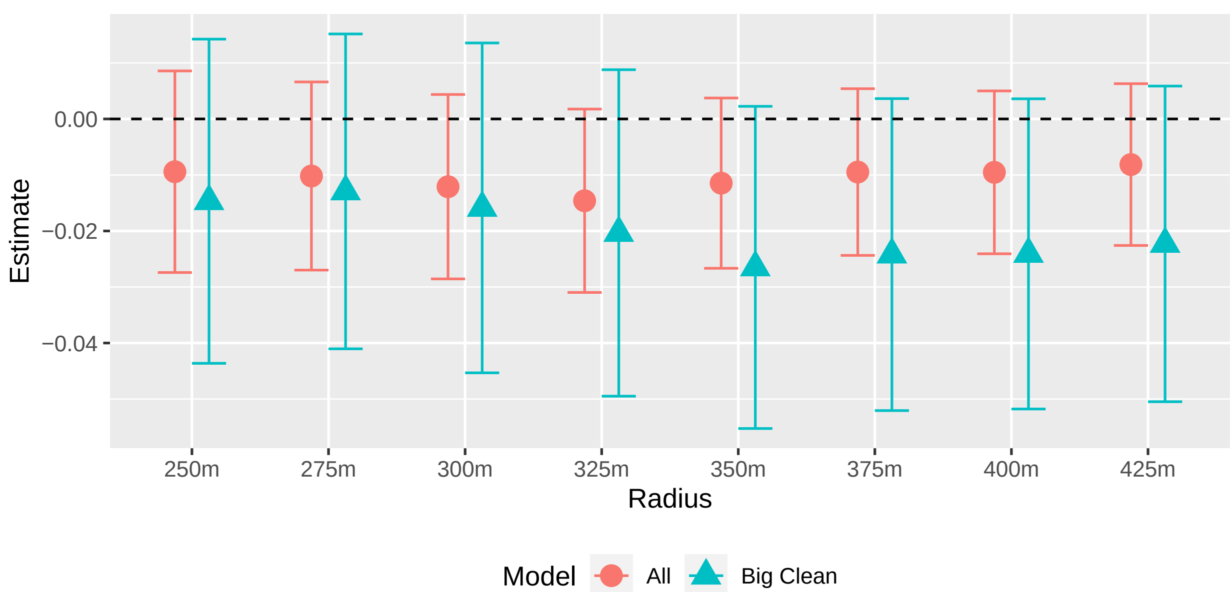

Figure A-4: Sensitivity of results for renter blocks across various distance band specifications. Near-near design.

B Sensitivity Analysis - Homeownership Cutpoints

B.1 Near-Far Design

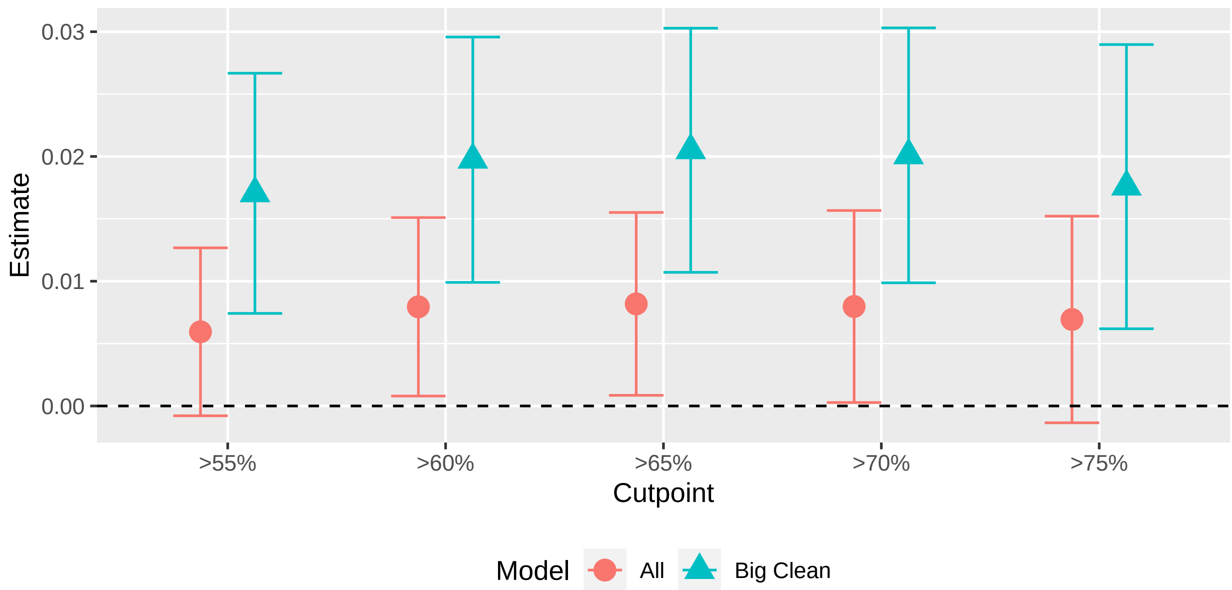

Figure B-5: Sensitivity of results to homeowner cutpoints for all blocks and blocks treated by large developments without previous development. Near-far design.

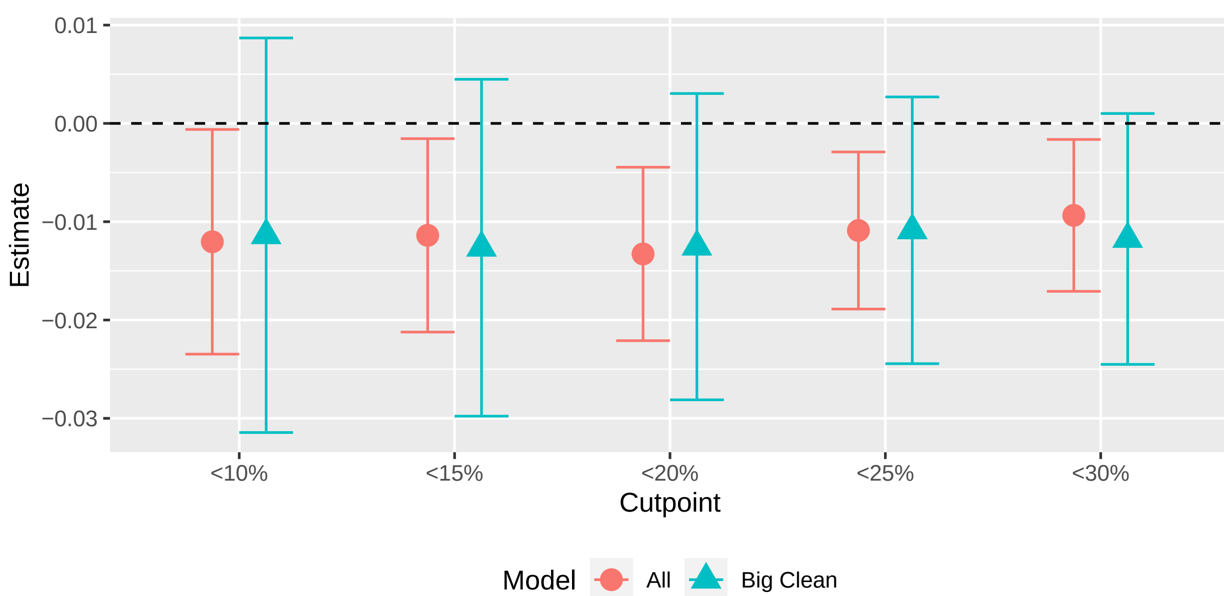

Figure B-6: Sensitivity of results to renter cutpoints for all blocks and blocks treated by large developments without previous development. Near-far design.

B.2 Near-Near Design

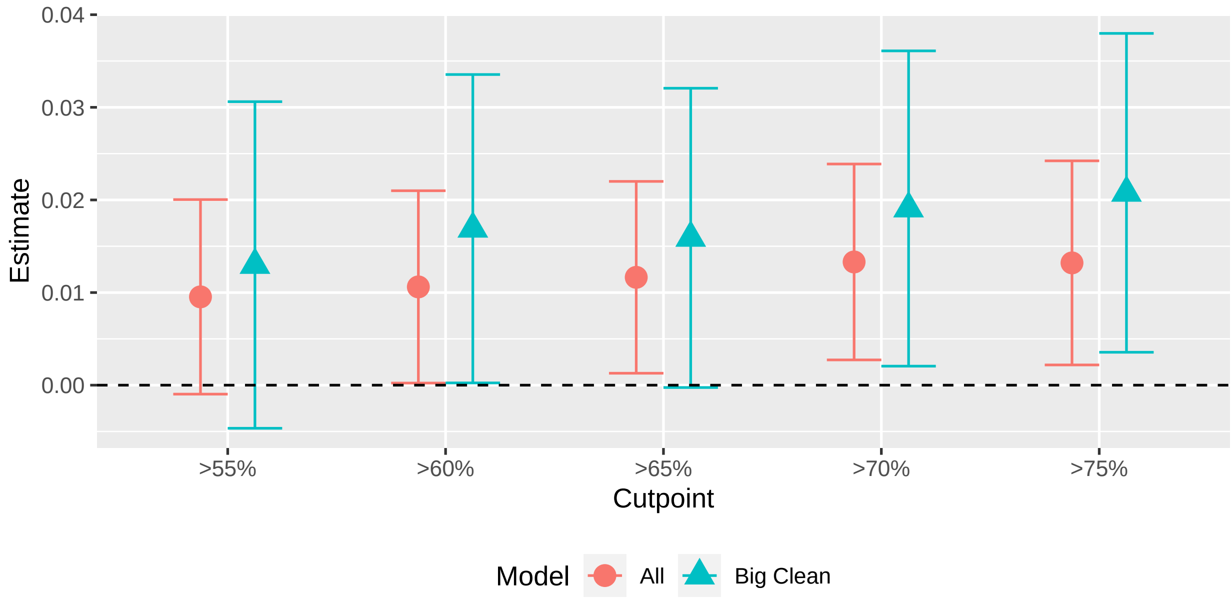

Figure B-7: Sensitivity of results to homeowner cutpoints for all blocks and blocks treated by large developments without previous development. Near-near design.

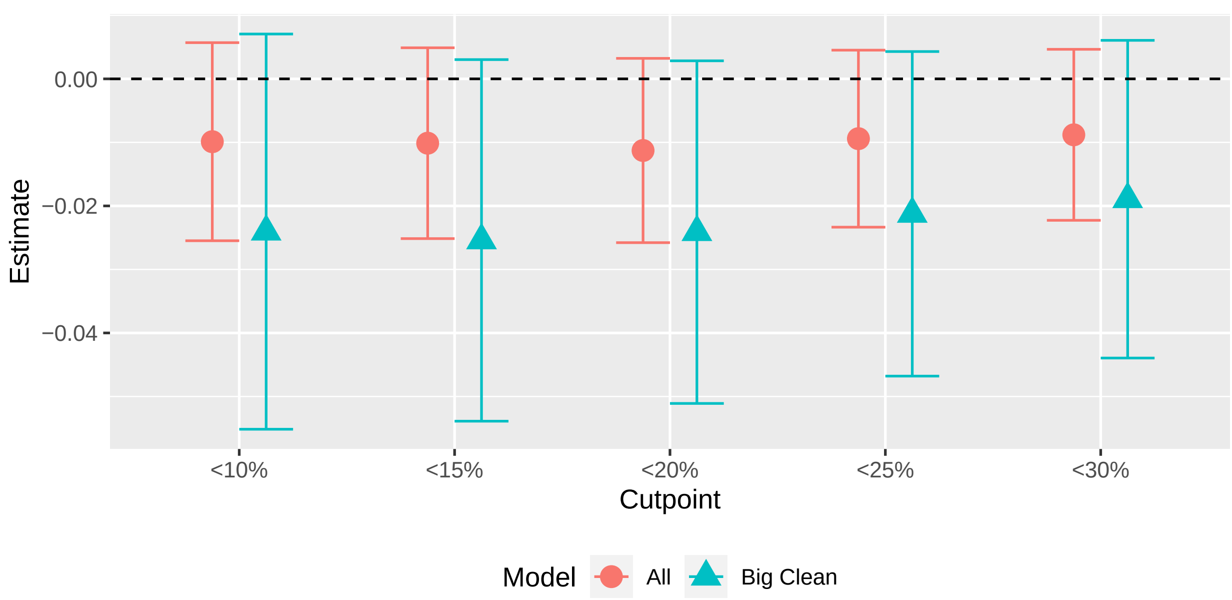

Figure B-8: Sensitivity of results to renter cutpoints for all blocks and blocks treated by large developments without previous development. Near-near design.

C Robustness Checks

C.1 Unconditional Results

	All	Clean	Big	Big Clean
LIHTC Project	−0.000	0.001	0.005	0.007
	(0.002)	(0.002)	(0.003)	(0.004)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.195	0.208	0.156	0.165
Adj. R^2	0.167	0.177	0.123	0.130
Num. obs.	13676	12235	6138	5649
RMSE	0.478	0.479	0.506	0.507
N Clusters	457	457	228	228

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

Table C-1: Near-Far Design, Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Project	−0.002	0.003	−0.003	0.002
	(0.005)	(0.005)	(0.008)	(0.009)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.054	0.057	0.046	0.051
Adj. R^2	0.051	0.054	0.041	0.046
Num. obs.	12580	11183	4744	4297
RMSE	0.523	0.528	0.547	0.551
N Clusters	851	829	369	365

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

Table C-2: Near-Near Design, Binary Treatment

C.2 Placebo Tests

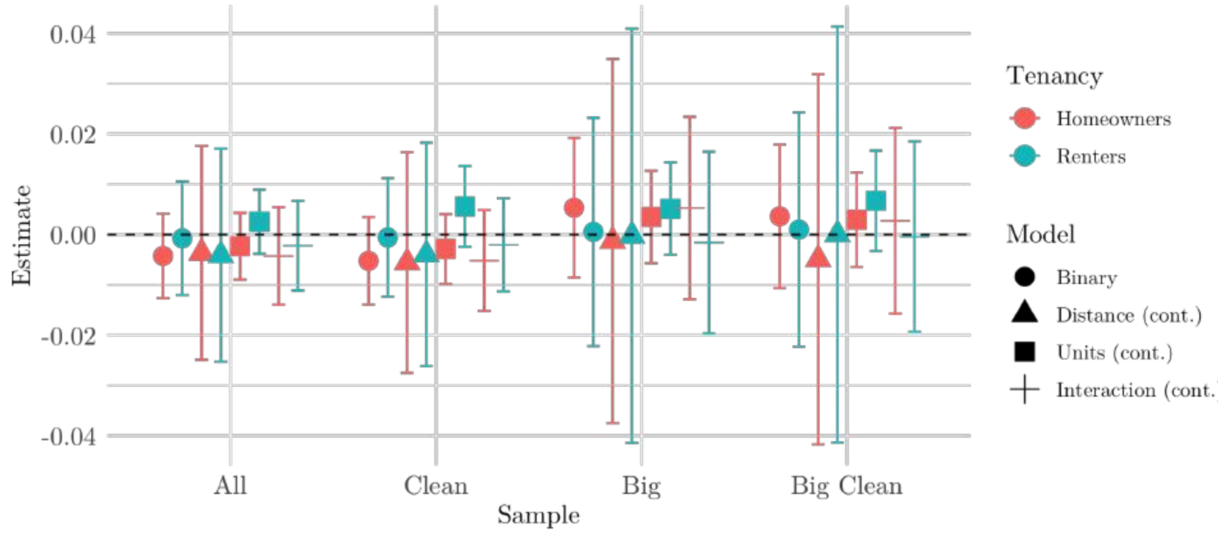

Figure C-9: Placebo results from the near-far design across four model specifications.

C.3 Residential Churn

	All	Clean	Big	Big Clean
LIHTC Project	−0.014	−0.011	−0.031	−0.022
	(0.017)	(0.019)	(0.027)	(0.029)
Renter Blocks	−0.077***	−0.072***	−0.075*	−0.065
	(0.019)	(0.020)	(0.031)	(0.033)
LIHTC x Renter Blocks	0.011	0.010	−0.004	−0.006
	(0.022)	(0.025)	(0.035)	(0.038)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.222	0.222	0.225	0.219
Adj. R^2	0.174	0.170	0.175	0.167
Num. obs.	5040	4689	2789	2624
RMSE	1.997	1.959	2.201	2.143
N Clusters	295	294	165	164

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

Table C-3: Near-Far Design with Homeownership on Churn (4-year), Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	0.001	−0.011	−0.005	−0.015
	(0.021)	(0.021)	(0.022)	(0.022)
Renter Blocks	-0.075^{***}	-0.075^{***}	-0.074^{***}	-0.070^{**}
	(0.013)	(0.014)	(0.022)	(0.024)
LIHTC x Renter Blocks	0.000	0.010	−0.011	−0.003
	(0.025)	(0.027)	(0.027)	(0.029)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.235	0.235	0.244	0.241
Adj. R^2	0.202	0.200	0.210	0.205
Num. obs.	13109	12243	6371	6024
RMSE	1.890	1.858	2.115	2.061
N Clusters	547	542	274	271

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

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

	All	Clean	Big	Big Clean
LIHTC Units	0.000	0.005	−0.010	0.001
	(0.024)	(0.023)	(0.037)	(0.036)
Renter Blocks	-0.044^{*}	-0.046^{*}	−0.039	−0.040
	(0.018)	(0.018)	(0.033)	(0.033)
LIHTC x Renter Blocks	0.022	0.021	0.045	0.031
	(0.028)	(0.028)	(0.047)	(0.046)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.018	0.018	0.028	0.032
Adj. R^2	0.012	0.012	0.017	0.021
Num. obs.	4256	3939	2076	1953
RMSE	2.204	2.155	2.499	2.404
N Clusters	500	485	248	241

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

Table C-5: Near-Near Design with Homeownership on Churn (4-year), Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	0.016	0.017	0.023	0.028
	(0.030)	(0.028)	(0.033)	(0.031)
Renter Blocks	-0.046^{***}	-0.047^{***}	-0.027	-0.032
	(0.011)	(0.011)	(0.020)	(0.020)
LIHTC x Renter Blocks	0.013	0.013	0.007	0.004
	(0.031)	(0.030)	(0.035)	(0.033)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.022	0.023	0.026	0.030
Adj. R^2	0.020	0.020	0.023	0.027
Num. obs.	13064	12198	6366	6019
RMSE	2.082	2.043	2.351	2.279
N Clusters	538	533	273	270

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

Table C-6: Near-Near Design with Homeownership on Churn (4-year), Continuous Units Treatment

D Mechanisms

D.1 Turnout

	All	Clean	Big	Big Clean
LIHTC Project	−0.002	−0.003	−0.005	−0.006
	(0.004)	(0.004)	(0.005)	(0.005)
Renter Blocks	0.021***	0.023***	0.020*	0.020*
	(0.005)	(0.006)	(0.008)	(0.008)
LIHTC x Renter Blocks	0.009	0.009	0.013	0.015
	(0.005)	(0.006)	(0.009)	(0.009)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.383	0.403	0.342	0.352
Adj. R^2	0.348	0.365	0.303	0.311
Num. obs.	8594	7610	4066	3755
RMSE	0.527	0.522	0.543	0.536
N Clusters	455	454	227	226

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

Table D-7: Near-Far Design with Homeownership on Turnout, Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	−0.003	−0.004	−0.005	−0.006
	(0.004)	(0.004)	(0.004)	(0.005)
Renter Blocks	0.022***	0.023***	0.024***	0.026***
	(0.003)	(0.004)	(0.006)	(0.006)
LIHTC x Renter Blocks	0.012*	0.014*	0.011	0.012
	(0.005)	(0.006)	(0.006)	(0.007)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.382	0.402	0.387	0.404
Adj. R^2	0.357	0.376	0.361	0.378
Num. obs.	23659	21273	9690	8992
RMSE	0.513	0.505	0.535	0.523
N Clusters	884	878	385	380

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

Table D-8: Near-Far Design with Homeownership on Turnout, Continuous Units Treatment

	All	Clean	Big	Big Clean
LIHTC Project	−0.006	−0.008	−0.014	−0.017
	(0.007)	(0.007)	(0.011)	(0.011)
Renter Blocks	0.032***	0.030***	0.034*	0.026
	(0.007)	(0.007)	(0.014)	(0.014)
LIHTC x Renter Blocks	0.004	0.012	−0.003	0.012
	(0.010)	(0.011)	(0.016)	(0.016)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.157	0.173	0.174	0.192
Adj. R^2	0.154	0.169	0.167	0.184
Num. obs.	7810	6873	3151	2880
RMSE	0.596	0.591	0.621	0.608
N Clusters	831	795	355	344

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

Table D-9: Near-Near Design with Homeownership on Turnout, Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	−0.014**	−0.015**	−0.014*	−0.015*
	(0.005)	(0.006)	(0.006)	(0.006)
Renter Blocks	0.028***	0.029***	0.021*	0.020*
	(0.005)	(0.005)	(0.008)	(0.009)
LIHTC x Renter Blocks	0.013*	0.018**	0.015*	0.019*
	(0.006)	(0.006)	(0.007)	(0.007)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.155	0.167	0.163	0.175
Adj. R^2	0.154	0.166	0.160	0.172
Num. obs.	23502	21116	9642	8944
RMSE	0.589	0.585	0.613	0.604
N Clusters	874	868	383	378

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

Table D-10: Near-Near Design with Homeownership on Turnout, Continuous Units Treatment

D.2 Roll-Off

	All	Clean	Big	Big Clean
LIHTC Project	0.000	0.001	−0.003	−0.002
	(0.005)	(0.005)	(0.006)	(0.007)
Renter Blocks	−0.011	−0.015	−0.005	−0.013
	(0.008)	(0.009)	(0.013)	(0.014)
LIHTC x Renter Blocks	−0.002	−0.002	−0.004	−0.006
	(0.007)	(0.008)	(0.012)	(0.014)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.573	0.603	0.596	0.605
Adj. R^2	0.550	0.578	0.572	0.579
Num. obs.	8594	7610	4066	3755
RMSE	0.648	0.632	0.629	0.631
N Clusters	455	454	227	226

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

Table D-11: Near-Far Design with Homeownership on Roll-Off, Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	−0.000	0.003	0.001	0.003
	(0.006)	(0.007)	(0.006)	(0.006)
Renter Blocks	−0.012*	−0.013*	−0.004	−0.009
	(0.006)	(0.006)	(0.008)	(0.009)
LIHTC x Renter Blocks	0.001	−0.004	−0.001	−0.005
	(0.010)	(0.011)	(0.010)	(0.011)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.567	0.597	0.581	0.598
Adj. R^2	0.550	0.580	0.564	0.580
Num. obs.	23659	21273	9690	8992
RMSE	0.620	0.599	0.598	0.585
N Clusters	884	878	385	380

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

Table D-12: Near-Far Design with Homeownership on Roll-Off, Continuous Units Treatment

	All	Clean	Big	Big Clean
LIHTC Project	−0.026	−0.030	−0.045**	−0.043*
	(0.015)	(0.015)	(0.017)	(0.018)
Renter Blocks	0.034**	0.036**	0.001	0.009
	(0.013)	(0.013)	(0.015)	(0.015)
LIHTC x Renter Blocks	0.015	0.009	0.035	0.025
	(0.016)	(0.018)	(0.023)	(0.025)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.226	0.234	0.297	0.295
Adj. R^2	0.223	0.230	0.291	0.288
Num. obs.	7810	6873	3151	2880
RMSE	0.830	0.825	0.779	0.778
N Clusters	831	795	355	344

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

Table D-13: Near-Near Design with Homeownership on Roll-Off, Binary Treatment

	All	Clean	Big	Big Clean
LIHTC Units	−0.013	−0.013	−0.014	−0.014
	(0.010)	(0.010)	(0.009)	(0.009)
Renter Blocks	0.037***	0.037***	0.020	0.021
	(0.010)	(0.010)	(0.013)	(0.013)
LIHTC x Renter Blocks	0.009	0.008	0.014	0.014
	(0.010)	(0.012)	(0.010)	(0.012)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.226	0.232	0.278	0.278
Adj. R^2	0.225	0.231	0.276	0.276
Num. obs.	23502	21116	9642	8944
RMSE	0.815	0.812	0.772	0.769
N Clusters	874	868	383	378

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

Table D-14: Near-Near Design with Homeownership on Roll-Off, Continuous Units Treatment

D.3 Turnout via Voter File Data

	All	Clean	Big	Big Clean
LIHTC Project	−0.016**	−0.014**	−0.016*	−0.015*
	(0.005)	(0.005)	(0.006)	(0.006)
Renter Blocks	−0.023*	−0.026**	−0.030*	−0.031*
	(0.009)	(0.009)	(0.015)	(0.015)
LIHTC x Renter Blocks	0.012	0.012	0.022*	0.020
	(0.007)	(0.008)	(0.010)	(0.012)
LIHTC FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.197	0.189	0.142	0.142
Adj. R^2	0.194	0.186	0.138	0.138
Num. obs.	130220	115009	56803	53541
RMSE	0.514	0.512	0.521	0.519
N Clusters	455	455	227	227

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

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

	All	Clean	Big	Big Clean
LIHTC Project	−0.046	−0.050*	−0.051	−0.053
	(0.024)	(0.024)	(0.031)	(0.032)
Renter Blocks	0.050*	0.047	0.015	0.016
	(0.023)	(0.025)	(0.036)	(0.040)
LIHTC x Renter Blocks	0.016	0.018	0.009	0.016
	(0.032)	(0.035)	(0.048)	(0.053)
CBSA FE	Yes	Yes	Yes	Yes
LIHTC SE Clusters	Yes	Yes	Yes	Yes
R^2	0.131	0.118	0.080	0.076
Adj. R^2	0.131	0.117	0.079	0.075
Num. obs.	98008	85031	36246	33246
RMSE	0.545	0.544	0.550	0.548
N Clusters	836	812	357	347

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

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

E Balance Tables

E.1 Near-Far Balance

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.44	0.51	-0.19
Percent voted in 2002	0.44	0.46	-0.09
Percent non-Hispanic white	0.39	0.42	-0.10
Percent non-Hispanic Black	0.08	0.09	-0.05
Percent Hispanic	0.34	0.34	0.03
Vacancy rate	0.05	0.04	0.05
Density	9697.39	7601.40	0.12

Table E-17: Balance, Near-Far Analysis, All Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.85	0.85	-0.02
Percent voted in 2002	0.52	0.53	-0.02
Percent non-Hispanic white	0.53	0.53	-0.01
Percent non-Hispanic Black	0.05	0.06	-0.05
Percent Hispanic	0.25	0.26	-0.01
Vacancy rate	0.04	0.04	0.10
Density	2693.07	3444.26	-0.05

Table E-18: Balance, Near-Far Analysis, Owner Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.05	0.07	-0.04
Percent voted in 2002	0.38	0.38	-0.04
Percent non-Hispanic white	0.31	0.34	-0.13
Percent non-Hispanic Black	0.10	0.09	0.02
Percent Hispanic	0.38	0.37	0.01
Vacancy rate	0.05	0.05	0.03
Density	19611.45	15446.03	0.29

Table E-19: Balance, Near-Far Analysis, Renter Blocks

E.2 Near-Near Balance

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.44	0.43	0.02
Percent voted in 2002	0.44	0.44	-0.02
Percent non-Hispanic white	0.39	0.41	-0.05
Percent non-Hispanic Black	0.08	0.09	-0.03
Percent Hispanic	0.34	0.36	-0.05
Vacancy rate	0.05	0.05	0.00
Density	9697.39	8525.25	0.08

Table E-20: Balance, Near-Near Analysis, All Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.85	0.84	0.01
Percent voted in 2002	0.52	0.52	0.02
Percent non-Hispanic white	0.53	0.48	0.16
Percent non-Hispanic Black	0.05	0.07	-0.10
Percent Hispanic	0.25	0.30	-0.15
Vacancy rate	0.04	0.04	0.06
Density	2693.07	3551.94	-0.06

Table E-21: Balance, Near-Near Analysis, Owner Blocks

	Mean (treated)	Mean (control)	Std. mean difference
Homeownership rate	0.05	0.06	-0.03
Percent voted in 2002	0.38	0.38	-0.03
Percent non-Hispanic white	0.31	0.37	-0.21
Percent non-Hispanic Black	0.10	0.10	-0.01
Percent Hispanic	0.38	0.37	0.03
Vacancy rate	0.05	0.05	-0.02
Density	19611.45	15242.39	0.29

Table E-22: Balance, Near-Near Analysis, Renter Blocks