The spillover effects of affordable housing developments on neighbors' political participation

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Abstract

This paper examines the impacts of affordable housing developments funded by the Low-Income Housing Tax Credit (LIHTC) on political participation in surrounding neighborhoods. Using voter file data from North Carolina and a near-far ring design, we compare outcomes over time for people who lived closer to versus farther from an LIHTC project in the election before it was finished. We find that LIHTC developments cause a steady decline in both registration and turnout for nearby residents, with each falling by about 0.25 percentage points per post-completion general election. Most of the drop in registration reflects people being de-registered due to persistent inactivity, not moving away, receiving a felony conviction, or dying. A potential explanation for our findings is that LIHTC developments provide an influx of residents with low political engagement, which can reduce participation for existing residents via factors such as negative peer effects, weakened norms, or decreased outreach efforts by parties and campaigns.

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1 Introduction

With the continued rise in home values and rental prices, housing costs have become an increasingly significant portion of families' monthly expenses, particularly for low-income populations (Feiveson, Levinson and Wertz, 2024). This trend has intensified the demand for subsidized housing that can alleviate cost burdens. A key attribute of subsidized housing is that it doesn't just impact the residents who move into the subsidized units; it also may have spillover effects that ripple through surrounding neighborhoods. Understanding these broader community impacts is crucial for making well-informed decisions about housing policy.

In this paper, we contribute to the literature on the spillover effects of subsidized housing by studying the impacts of new affordable housing developments on neighborhood political participation. Political participation is both an important outcome in its own right and a proxy for community wellbeing (Nelson, Sloan and Chandra, 2019). Our setting for the analysis is the state of North Carolina (NC). We focus on housing developments funded by the Low-Income Housing Tax Credit (LIHTC), which is currently the largest source of new affordable housing in the U.S. We obtain information on the location, timing, and characteristics of all LIHTC developments constructed in NC during 2007-2022 from the U.S. Department of Housing and Urban Development (HUD). We gain individual-level panel data on political participation by linking voter records from the NC voter file for the 2006-2022 November general elections. These records reveal voting behavior, demographics, and exact addresses for each person registered in the state (a "registrant") during a given election.

Our empirical strategy is an adaptation of the commonly used ring method. A number of other papers have used the ring method to identify the causal effects of changes in the urban environment, such as new developments (Schwartz et al., 2006; Rossi-Hansberg, Sarte and Owens, 2010; Campbell, Giglio and Pathak, 2011; Autor, Palmer and Pathak, 2014; Asquith, Mast and Reed, 2023). In our case, we restrict attention to people who were registered in the neighborhood of an LIHTC development in the election prior to the development's completion, which we call the "baseline" election. We then compare turnout and registration over time for people who lived very close to the development (inner circle, the treatment group) with those who were slightly farther away (outer ring, the control group).

The ring method posits that the outer-ring control group should be a good counterfactual for the inner-circle treatment group due to frictions in the development process—in particular, because developers have imprecise control over the location and timing of new projects (Diamond and McQuade, 2019). Nonetheless, we make two refinements to the approach to enhance the credibility of our results. First, we do not compare all treated and control registrants over all LIHTC developments. Instead, using exact-matching, we conduct comparisons among sets of registrants who lived near the same development at baseline and who exhibited the same voting behavior in pre-completion ("pre-period") elections. Second, we find that, for many projects, treated registrants are more disadvantaged than control registrants. To improve covariate balance, we trim the sample by dropping a small number of projects where the treatment group is the most disadvantaged relative to control.

Two points about our paper are worth highlighting. First, we examine the effects of LIHTC developments on their immediate surroundings. For example, in our main specification, we set the inner circle as the area within 0.2 miles of a development and the outer ring as the area between 0.2 and 0.3 miles away. Our focus on localized effects is consistent with the existing ring-method literature, which shows that housing developments have spillover effects but only over short distances. Second, within the small areas we study, completion of an LIHTC development constitutes a large treatment. On average across projects in our sample, a development contains about 70 units while about 140 people are registered in the inner circle at baseline. Thus, it seems plausible that a development is a big enough change to a neighborhood to have spillover effects on existing residents.[1]

In terms of results, we find that completion of an LIHTC project causes a reduction in the political participation of people registered nearby at baseline. Effects on turnout and registration both grow gradually over time, in an almost linear manner. Specifically, relative to control, treatment-group turnout and registration both fall by about 0.25 percentage points (pp) per post-completion election. This continues for at least eight elections after completion, the most we can observe in the data. Effects exist for all types of voters and neighborhoods and—reassuringly for our empirical strategy—are larger for new developments versus rehabilitation projects and for bigger developments. We also show that effects pass a battery of robustness tests.

Our data lets us test four mechanisms that could conceivably explain the reduction in political participation. First, completion of an LIHTC development may spur some neighborhood residents to move away, which could reduce their registration and turnout via disruption, administrative burdens, and loss of community (Squire, Wolfinger and Glass, 1987; Gay, 2012; Knight and Zhang, 2024). Second, an LIHTC development could make neighborhood residents more likely to commit crime, which would preclude their political participation. Third, a development could also cause more registrants to die. Finally, an LIHTC project involves an influx of low-income residents, and low-income people tend to have low political engagement, including low rates of registration and turnout (Broockman et al., 2024), low political knowledge (Lind and Rohner, 2017), and a low propensity to make campaign contributions (Bouton et al., 2024). This could reduce participation for existing residents even without causing them to move away or become unable to participate. For instance, it could have impacts via negative peer effects (Nickerson, 2008; Coriale, Kaplan and Nesbit, 2023) or weakened norms and expectations regarding participation (Dellavigna et al., 2016). Alternatively, it may cause political parties and campaigns to spend less effort on mobilization and outreach in the neighborhood, as

[1]An LIHTC development may also affect people who move into the development and people who move to the surrounding area after the development is finished. Due to identification challenges, we cannot estimate effects for these groups. However, considerable research shows that obtaining stable and affordable housing increases political participation, which suggests that effects for people who move into the development may be positive.

they may ignore places where the population is politically disengaged (Miller, 2008; Stromberg, 2008; Bartels, 2009; Gilens, 2012; Cascio and Washington, 2013; Enos, Fowler and Vavreck, 2014; Fujiwara, 2015; Kalla and Broockman, 2016; Enos and Fowler, 2018).

To assess the potential mechanisms, we exploit the fact that North Carolina's voter file lists why registrants lose their registration status. We show that completion of an LIHTC project doesn't affect the probability that someone is de-registered due to moving away, receiving a felony conviction, or dying. Instead, most de-registration is due simply to persistent inactivity—i.e., it's because people stop voting and/or responding to government communications. Thus, the evidence suggests that the fourth mechanism is most likely.

Our paper contributes to multiple strands of literature. First, we add to research on the spillover effects of LIHTC developments. Prominent in this literature is Diamond and McQuade (2019), who document differences in spillovers by neighborhood income. Specifically, they show that, in low-income areas, LIHTC developments increase property values, reduce violent and property crime, and raise the average income of homebuyers; meanwhile, in high-income areas, the developments decrease property values, have no effect on crime, and lower homebuyers' average income. Baum-Snow and Marion (2009) and Freedman and McGavock (2015) show that LIHTC developments increase homeowner turnover rates and slow the construction of unsubsidized housing. Also, Di and Murdoch (2013) detect no effects on student performance in nearby elementary schools. To our knowledge, we are the first to study the impacts of LIHTC developments on political participation. Interestingly, we find effects in both low- and high-income areas, and we show that effects are likely not due to homeowner turnover.

Second, we add to research on the relationship between housing and political participation. This literature establishes that negative housing shocks, such as eviction (Slee and Desmond, 2023), foreclosure (Shah and Wichowsky, 2019; Hall, Yoder and Karandikar, 2021), and house price declines (McCartney, 2021), make people less likely to vote and that positive shocks, such as becoming a homeowner (Hall and Yoder, 2022) or growing up with a housing voucher (Chyn and Haggag, 2023), make people more likely to vote. Research also finds that registration and turnout are particularly low for the homeless (Ruth, Matusitz and Simi, 2017). We contribute by studying one of the U.S.'s most important housing programs and showing that it has spillover effects on the political participation of people who are not direct program beneficiaries.

Finally, we add to research that studies how places affect the political participation of their residents. Here, Cantoni and Pons (2022) use changes in outcomes for people who move to different types of places to show that place effects explain a sizable share of the variation in registration and turnout across both states and counties. Brown et al. (2023) show a similar result but focus on the role of a person's place of residence during childhood. Our paper provides further evidence for the importance of place effects; in addition, it does so using a new source of identification: a change to a place, rather than movement between places.

In the rest of the paper, Section 2 provides more information on the setting and data, Section 3 details our empirical strategy, Section 4 presents the main results, along with robustness and heterogeneity, Section 5 explores mechanisms, and Section 6 concludes.

2 Setting and data

2.1 The Low-Income Housing Tax Credit Program

The Low-Income Housing Tax Credit (LIHTC) was established by the Tax Reform Act of 1986 to address the nationwide shortage of affordable housing. Its primary goal is to stimulate private investment in the construction and rehabilitation of affordable rental housing by providing tax credits to private developers. These tax credits lower development costs, making it profitable to operate properties while charging below-market rents. The LIHTC is now the largest program financing affordable rental housing in the U.S., with expenditures of $13.5 billion in 2023 (Keightley, 2023).

Several aspects of the LIHTC program are worth highlighting. First, each state generates its own allocation plan to determine which proposals receive funding. In North Carolina, proposed projects incur significant scoring penalties if they include market-rate units. As a result, the vast majority of LIHTC developments in the state are comprised exclusively of affordable units, and the developments serve a highly disadvantaged population. According to 2022 tenant statistics, the median household income in a North Carolina LIHTC development is $17,400, with 48% of households earning below 30% of the Area Median Income (AMI).[2] Over half of the households rely on additional rental assistance in order to cover their housing costs. Demographically, 71% of residents who report their race are African American, and 35% of households include at least one child while 40% have an elderly member.

Second, LIHTC projects are required to maintain affordable rents for a minimum of fifteen years. This means that rents remain affordable for the entire post-completion period that we can observe. Third, in our sample, LIHTC projects contain an average of 67 units, often in a single building. In this way, completion of a development constitutes a large and long-lasting change to a neighborhood, which may be significant enough to influence the political participation of neighborhood residents.

Fourth, as with many types of residential development, LIHTC developers face frictions that influence both the location and timing of their projects. Namely, site selection is affected by factors such as zoning restrictions, land availability, and uncertainty in local approval processes. Similarly, project timelines are subject to permitting delays, construction issues, financing problems, and other factors beyond a developer's control. These frictions in the development process reduce concerns that

[2] https://www.huduser.gov/portal/Datasets/lihtc/2022-LIHTC-Tenant-Tables.pdf.

strategic location or timing choices might confound our analysis.

We collected data from the U.S. Department of Housing and Urban Development (HUD) on all LIHTC developments completed in the U.S. between 2007 and 2022. The data provides detailed information on the projects, including the type of development (new construction or rehabilitation), the number of units, the exact address, the year funding was allocated, and the year a project was placed in service. As seen in Appendix Table A.1, projects in North Carolina are more likely to be new construction than elsewhere in the U.S. (76% for NC versus 54% for the rest of the country). In addition, North Carolina projects on average have a higher share of low-income units (99% vs. 92%).

We conducted a multi-step process to geocode projects and obtain their latitude and longitude. We started with the U.S. Census geocoder. Then, for projects that the Census geocoder could not find, we used the OpenStreetMap (OSM) geocoder, followed by manual searches using Google Maps. Our process located 598 of the total of 605 projects in North Carolina during our sample period.[3]

2.2 North Carolina voting data

We focus our analysis on North Carolina because it is an advantageous setting to study effects on political participation. First, North Carolina is a large and demographically diverse state, and prior research finds that voting behavior in North Carolina is similar to that in the United States as a whole (Ainsworth, Garcia Munoz and Munoz Gomez, 2024). Second, the state provides rich and publicly available administrative data on voting.

The voting data comes from the North Carolina State Board of Elections (NC SBE) and consists of historical snapshots of the state's registration database (or "voter file"), measured during biennial November general elections. Importantly, the data includes a unique statewide identifier (the "NCID"), which allows the snapshots to be linked over time. We use the snapshots to construct an individual-level panel dataset for all people registered in the state during 2006 to 2022. The dataset lists each registrant's demographic information (gender, race, and age), as well as their registered address, party affiliation, and turnout behavior in each election when they were registered. We geocode the addresses using the U.S. Census geocoder and then measure distances to nearby LIHTC projects.[4]

We construct two outcomes that capture different dimensions of political participation: registration and turnout. We define both outcomes as binary variables. Registration equals 100 if the voting data designates a person as eligible to vote in a given November general election and zero otherwise. Turnout equals 100 if a registrant submits a ballot in the election and zero otherwise. By scaling the variables as 100 or 0, we ensure that treatment effects are denominated in percentage points. Im-

[3]We drop the 7 projects that we could not find. Of the located projects, we found 447 using the Census geocoder, 61 using the OSM geocoder, and 90 via manual searching. See Appendix C for more details.

[4]We were able to locate more than 95% of the registrants from baseline elections; we drop those we could not find.

portantly, our data is only for North Carolina; thus, any registrant who leaves the state would have outcome values of 0 even if they register and turn out in a different state. In this way, our estimates reflect effects on political participation within North Carolina.[5]

When a registrant loses their registration status (i.e., becomes de-registered and ineligible to vote), the NC SBE data specifies the reason. Reasons include moving out of state, moving to a different county within-state and not re-registering in the new county, receiving a felony conviction, dying, or being persistently inactive.[6] Becoming de-registered due to inactivity is a somewhat lengthy process. First, a registrant must be disengaged for two federal election cycles (4 years), meaning that the registrant never votes or updates their registration. After this period, the registrant is sent a formal address confirmation notice by the NC SBE. If the registrant fails to respond, their status is marked as inactive. If the registrant remains inactive for two additional election cycles (by continuing to not vote or update their registration), they are de-registered. This multi-step procedure means that there is a lag between when a registrant first stops participating and eventually becomes de-registered.

Finally, we supplement our LIHTC and political participation data with data on neighborhood characteristics. From the U.S. Census Bureau, we obtain population density at the census block level, as well as the share of college graduates and household median income at the block-group level. From the NC One Map project, we collect property values for individual parcels. This additional data contextualizes the neighborhoods where projects are built and voters live.

3 Empirical strategy

3.1 Overview

Our empirical strategy adapts the commonly used ring method. In our implementation, we define an "event" as an LIHTC development being completed (or "placed in service").[7] For each development, we define the baseline election as the last general election before completion.[8] We focus on people registered near a development at baseline, and we draw two concentric circles around the development, designating the registrants in the inner circle as the development's treatment group and those in the outer ring as its control group. In our main analysis, we choose radii of 0.2 and 0.3 miles for the inner and outer circles. (Thus, the outer ring is the area 0.2-0.3 miles away from the development.) Figure 1 shows the treatment and control areas for an example development in the city of Charlotte.

In order to increase statistical power, we stack the samples for all developments, repeating ob-

[5]Later, we show that our treatment does not affect the probability of leaving the state.

[6]If a voter moves within their county, they must inform the county board of elections of their new address but do not need to fully re-register.

[7]We show in Appendix H that completion is the correct treatment timing, not the year of credit allocation.

[8]For instance, developments placed in service in 2007 or 2008 have a baseline election of 2006.

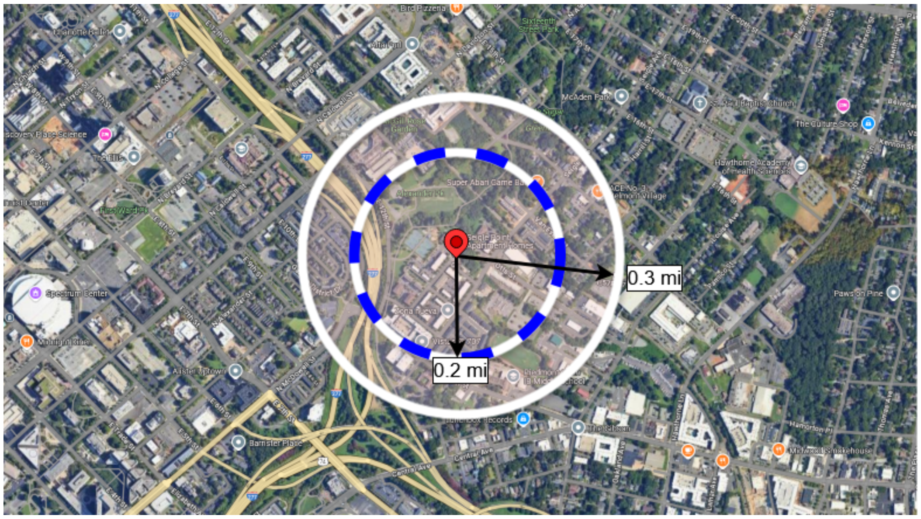


Figure 1: The ring method for an example development

The figure depicts the ring method for an LIHTC development in Charlotte. The development is marked by the red dot. People registered at an address within the dashed circle during the baseline election comprise the development's treatment group. People with a baseline address in the ring between the dashed and white circles are the control group.

servations for registrants who are in-sample for multiple developments. We let i index a registrant-development combination, although for brevity we sometimes refer to i as simply a registrant. We also switch to event time, denoting elections relative to development completion by τ, with $\tau = 0$ being the baseline and $\tau = 1$ being the first post-completion (or "post-period") election.

To exploit the richness of our data, we do not simply compare the combined treatment and control groups over all developments. Instead, we use an exact-matching procedure that lets us conduct fine-grained comparisons. Specifically, we divide the registrant-development combinations into a set of mutually exclusive groups, which we call "match-groups". Match-groups are constructed such that all members share the same values of certain key variables. In our main specification, these variables are: (a) the development, (b) the first election in which a registrant is registered, and (c) turnout behavior and registration status in the three elections prior to development completion.[9] In our regressions, we include match-group fixed effects that vary by relative election. Thus, in each relative election, we conduct comparisons among people who lived near the same development at baseline and who had a similar history of political participation before the treatment occurred.[10]

Next, we find that on average the treated population is more disadvantaged than the control group, even conditional on match-groups (Appendix Tables B.1 and B.2). To address the imbalance, we compute a disadvantage index for each i using the methodology from Anderson (2008). The index is based on four components: the log of the value of the property parcel associated with a registrant's baseline address, the share of college graduates in the registrant's baseline block-group, the log of median

[9] We match on turnout in both the November general elections and their primaries.

[10] For some match-groups, there is no variation in treatment status. These do not contribute to our estimation, so we drop them. 13.5% of the sample gets dropped for this reason.

household income in the baseline block-group, and an indicator for whether the registrant died during our sample period. For each development, we calculate the difference in the average of the index for the development's treatment and control groups. We then trim the sample by excluding 73 developments where the treatment group is the most disadvantaged relative to control. Appendix Table B.2 shows that the trimming procedure eliminates statistically significant differences in most variables.[11]

Table 1: Summary statistics for the analysis sample

	Treatment		Control	
	Mean	SD	Mean	SD
Distance	0.13	0.06	0.25	0.03
Age	46.4	18.8	45.7	18.7
Male	0.40	0.49	0.42	0.49
White	0.43	0.50	0.45	0.50
Non-White	0.57	0.50	0.55	0.50
Democrat	0.58	0.49	0.56	0.50
Republican	0.18	0.38	0.18	0.39
Unaffiliated	0.24	0.43	0.26	0.44
Turnout	0.46	0.50	0.48	0.50
Dies in the sample period	0.10	0.30	0.09	0.29
Log(parcel value)	10.5	1.1	10.5	1.0
Log(block population density)	7.2	1.0	7.2	1.0
Block-group share college graduates	0.25	0.19	0.26	0.19
Log(block-group median hhld. income)	10.4	0.50	10.5	0.52
Registrant-developments	69,549		78,796	

The table presents summary statistics for the paper's analysis sample. An observation is a registrant-development combination. All variables except "Dies in the sample period" are measured during the baseline election. "Distance" is the distance between a registrant's address and their development, in miles. We classify all registrants who are not registered with the Democratic or Republican Parties as Unaffiliated. "Dies in the sample period" is whether a registrant dies between the baseline election and 2022. "Log(parcel value)" is the natural log of the value of the property parcel associated with a registrant's address divided by the number of people registered at the address. The sample includes 508 developments and 9,133 match-groups, with an average of about 16 registrant-developments per match-group. There are 134,034 distinct registrants, of whom 90% are exposed to a single development, 9% are exposed to two developments and the remaining 1% are exposed to between three and five developments. The sample excludes 20,080 registrant-developments who are in match-groups with no variation in treatment status.

Table 1 displays summary statistics for the paper's final analysis sample. It shows that the analysis sample contains 508 developments and almost 150,000 registrant-development combinations.[12]

[11] Differences in demographics and in party affiliation remain; however, we run a robustness check where we include these variables when constructing the match-groups, and our results hold.

[12] In addition to the 73 developments that get dropped via trimming, 17 developments are dropped due to having no match-groups with variation in treatment status.

3.2 Estimation

To estimate the effects of completion of an LIHTC development, we start with the following event-study specification:

$$Y_{i\tau} = \beta_\tau \cdot \text{Treat}_i + \gamma_{g\tau} + \epsilon_{i\tau}. \tag{1}$$

In this equation, $Y_{i\tau}$ is an outcome of interest (such as turnout) for registrant-development i in relative election τ, Treat_i is an indicator variable equal to one if i is in the treatment group, $\gamma_{g\tau}$ is a τ-specific fixed effect for i's match-group g, and $\epsilon_{i\tau}$ is the error term. The coefficient of interest is β_τ, which represents the τ-specific within-match-group difference in the outcome between treatment and control. For β_τ to have a causal interpretation, there must not be any unobserved factors that are correlated with both the outcome and treatment status in τ, conditional on match-groups. This claim is plausible given the construction of match-groups and given our trimming procedure described previously. Further, it can be tested by showing that there are no differences between treatment and control in pre-period elections that are not used in matching.

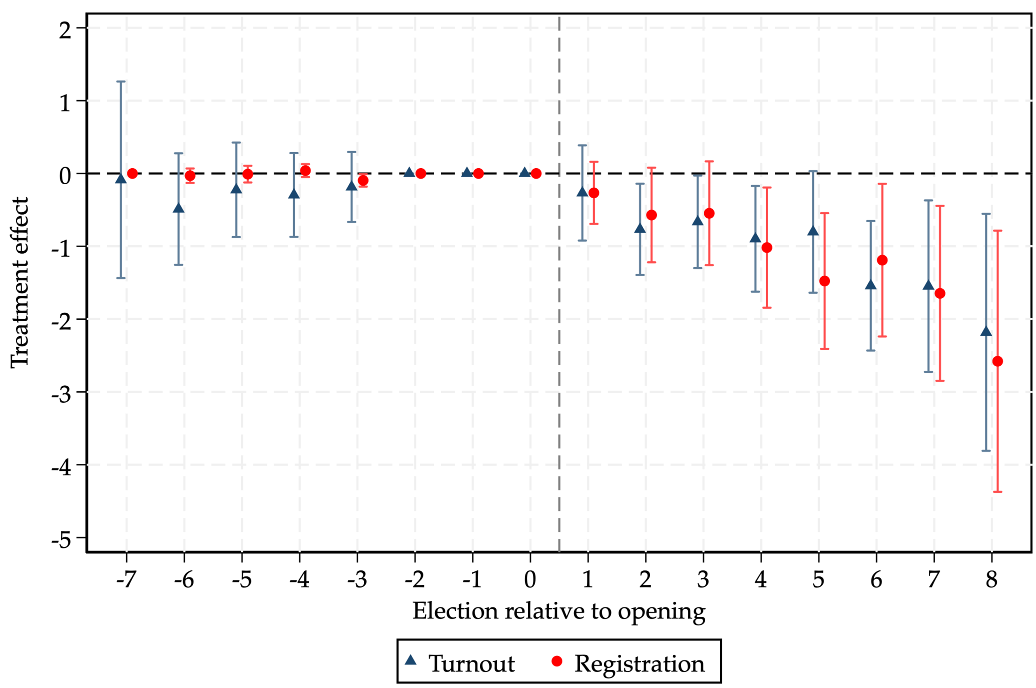


Figure 2: β_τ estimates from Equation (1)

The figure displays an event-study plot for the β_τ coefficients from Equation (1). In the figure, the dots and triangles are coefficient estimates and the lines are 95% confidence intervals. Treatment effects are measured in percentage points, and standard errors are clustered by development.

Results for Equation (1) are presented in Figure 2. The figure plots coefficient estimates and 95% confidence intervals for the β_τ coefficients, separately for turnout and registration. The dashed vertical line delineates the pre- and post-periods. The figure yields three main takeaways. First, lending confidence to our empirical strategy, there are no statistically significant differences between the treatment and control groups in the pre-period for either outcome, even in elections that are not used in the construction of match-groups. Second, the completion of an LIHTC project causes a reduction in both turnout and registration for nearby baseline registrants. Third, the treatment effects have an interesting shape: they begin small but grow approximately linearly over time through the last relative election that we can observe, $\tau = 8$.

Given the linearity seen in Figure 2, we next turn to a more parsimonious specification that summarizes the effects in the figure in a single parameter:

$$Y_{i\tau} = \beta \cdot (\text{Treat}_i \times \tau \times \mathbb{1}\{\tau > 0\}) + \gamma_{g\tau} + \epsilon_{i\tau}. \tag{2}$$

Here, $Y_{i\tau}$, Treat_i, $\gamma_{g\tau}$, and $\epsilon_{i\tau}$ retain the same interpretations as before. $\mathbb{1}\{\tau > 0\}$ is an indicator equal to one if the relative election occurs in the post-period. The coefficient of interest is β, which reveals the per-election effect of completion of an LIHTC project.[13] We present results for this specification in the next section.

4 Results

4.1 Main results and robustness

This section provides our main results, followed by a series of robustness checks.

The columns labeled (1) in Table 2 present coefficient estimates and standard errors for β from Equation (2). As before, the results show that completion of an LIHTC project causes a fall in turnout and registration for nearby baseline registrants. The magnitudes are, respectively, about 0.23 and 0.25 percentage points (pp) per post-completion election. These magnitudes are consistent with the effects in Figure 1. For instance, the figure shows that, in the fourth post-completion election, declines are approximately one pp, which is just as is predicted by the mentioned estimates from Table 2.

The columns labeled (2) and (3) in Table 2 are for alternative specifications. The former are for a version of Equation (2) that adds linear controls for neighborhood attributes, interacted with relative election.[14] The latter instead add registrant-development fixed effects.[15] Both alternative specifications yield estimates that are extremely close to those from our primary specification.

[13]Namely, the effect in the first post-completion election is β, that in the second is 2β, and that in the n^{th} is $n\beta$.

[14]The included neighborhood attributes are: the log of the baseline parcel value, the log of population density in the baseline census block, the share of college graduates in the baseline block-group, and the log of median household income in the baseline block-group.

[15]The β estimates from the main specification and from the specification with neighborhood controls are not affected by the inclusion of pre-period data. To be transparent about the relevant sample size, we drop pre-period elections when fitting these specifications. In contrast, pre-period data is necessary for identifying registrant-development fixed effects. Thus, in fitting the third specification, we include all available pre-period elections.

Table 2: β estimates

	Turnout			Registration		
	(1)	(2)	(3)	(1)	(2)	(3)
Effect per election	-0.233***	-0.220***	-0.225***	-0.249***	-0.225***	-0.243***
	(0.069)	(0.069)	(0.074)	(0.080)	(0.077)	(0.084)
Pre-treatment mean	38.6	38.6	38.6	75.1	75.1	75.1
Neighborhood controls		✓			✓	
Registrant-dev. FE			✓			✓
Registrant-developments	148,345	148,345	148,345	148,345	148,345	148,345
Registrant-dev.-elections	698,333	698,333	1,335,105	698,333	698,333	1,335,105

The table presents results from estimating Equation (2). An observation is a registrant-development-election. "Effect per election" is the coefficient estimate and standard error for β. "Pre-treatment mean" is the mean of a given outcome across all registrant-development-elections in the pre-period. The results in the columns labeled (1) are for the main version of Equation (2). The results in the columns labeled (2) and (3) are for versions that respectively add controls for neighborhood attributes interacted with relative election and registrant-development fixed effects. See the text of Section 4.1 for more details on these specifications. As in Figure 2, treatment effects are measured in percentage points, and standard errors are clustered by development. $^{*}p < 0.10$,** $p < 0.05$,*** $p < 0.01$.

In the appendix, we conduct a series of additional robustness exercises, which we summarize here. First, we verify that our results are not affected by the fact that we use multiple geocoding methods. In Appendix C, we re-estimate Equation (1) using only the LIHTC developments that were geocoded with the U.S. Census geocoder, which is the same method that we use for geocoding registrants' addresses. We find that the results are virtually unchanged.

Second, we confirm that our results are not distorted by the fact that some registrants are exposed to multiple developments. In Appendix D, we restrict the sample to registrants exposed to only a single development and again find that results are unchanged.

Next, we explore robustness to alternative ways of specifying the treatment variable. In Appendix E, we present results for different treatment radii, ranging from 0.05 to 0.35 miles. In Appendix F, we instead model the treatment as varying linearly depending on one's distance to an LIHTC development. The evidence suggests that effects are larger at smaller distances. Yet, it also suggests that our preferred treatment radius of 0.2 miles does a good job of representing the area where registrants are significantly impacted.

Appendix G shows results for alternative sets of match-groups. The match-groups differ in the number of pre-period outcomes that they match on and in whether they match on demographics and party affiliation. In all cases, estimates differ little from our main results.

Appendix H assesses our decision to focus on project completion as the treatment of interest. It sets the year of treatment equal to the year a development receives funding, rather than the year of completion. Effects are small and statistically insignificant, indicating that completion is a much stronger treatment than funding allocation.

Finally, in Appendix I, we conduct placebo tests where we pretend that a project's completion occurred prior to when it actually did. We offset treatment timing by one to four relative elections and then estimate effects using the elections after a pretend treatment but before the true treatment. This exercise lets us see if we would obtain our main findings even in the absence of treatment. The results—while noisy—suggest that we would not.[16]

4.2 Heterogeneity

We next probe heterogeneity in the political participation effects of LIHTC developments. We estimate the main version of Equation (2) for various subsamples that differ according to registrant, place, and development characteristics. The results are summarized visually in Figure 3. Precise estimates are provided in Appendix Tables A.2 and A.3.

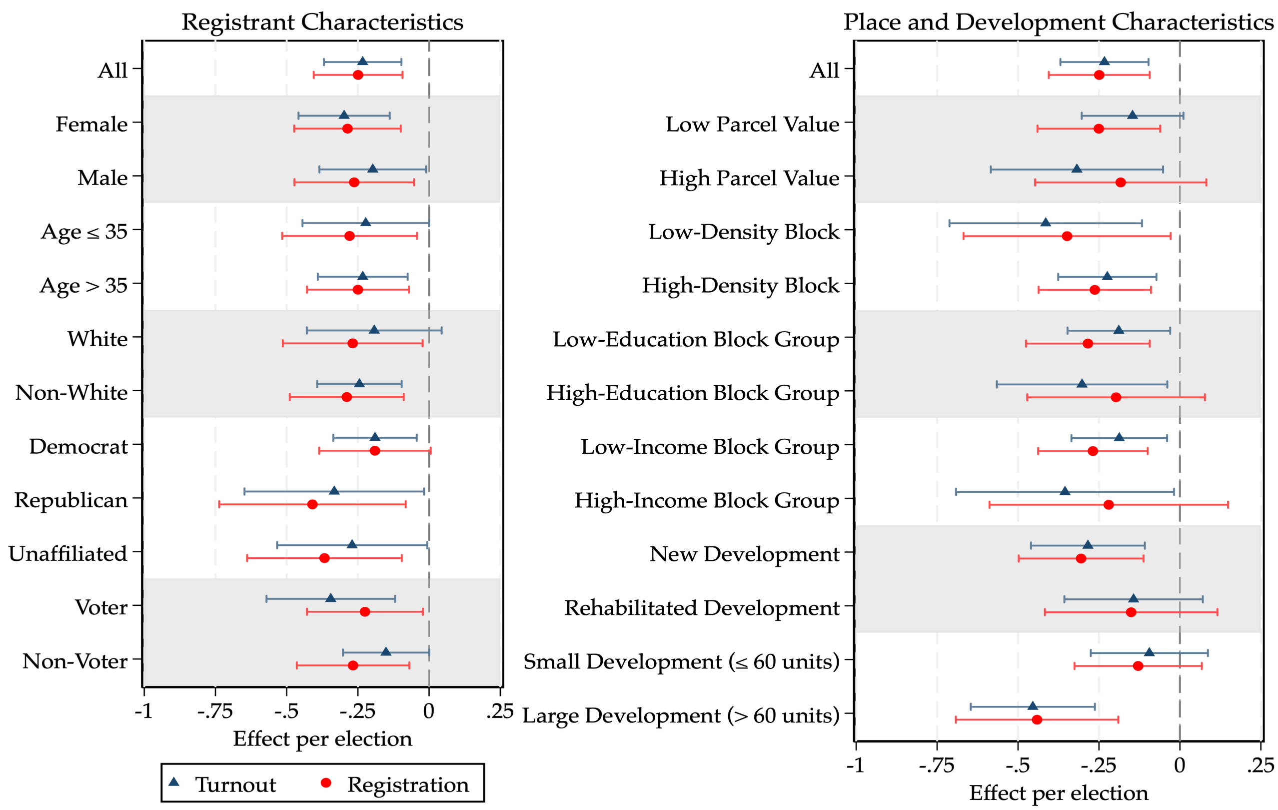


Figure 3: Heterogeneity in β estimates

The figure summarizes heterogeneity in the per-election effect of completion of an LIHTC development. The dots/triangles and lines are coefficient estimates and 95% confidence intervals for β from the main version of Equation (2), estimated on the listed samples. "All" indicates the full sample and corresponds to the results in the columns labeled (1) of Table 2. The other rows are for subsamples. Registrant and place characteristics are measured at baseline. High/Low divisions use the statewide median from a registrant-development's baseline election. Numerical results for each subsample are in Appendix Tables A.2 and A.3.

In Figure 3, the labels indicate the subsamples, while the dots/triangles and lines are coefficient

[16]A limitation of the placebo tests in our setting is that treated registrants may be subject to some impacts from an LIHTC project before its completion (such as via disruption from construction). Thus, the pretend post-period elections are not fully without treatment. Nonetheless, the results from Appendix H imply that this treatment is considerably weaker than that from project completion.

estimates and 95% confidence intervals for β. The left panel is for subsamples that differ by registrant characteristics; the right panel shows heterogeneity by place and development characteristics. In both panels, "All" is for the full sample, and matches the values in the columns labeled (1) in Table 2.

In terms of results, there are a few takeaways. First, completion of an LIHTC development reduces turnout and registration across almost all types of registrants, places, and developments. Second, there is little heterogeneity in treatment effects by a registrant's gender, age, or race. In contrast, there are differences by baseline party affiliation. Namely, effects are more negative for registrants registered as Republicans at baseline than for those registered as Democrats. Also, there is heterogeneity by baseline turnout that differs for the turnout and registration outcomes. In particular, the effect on registration is larger in magnitude for baseline non-voters than for baseline voters, while the opposite holds for the effect on turnout. The fact that the registration effect is larger for baseline non-voters is not surprising, as these registrants have a head start in the process of becoming de-registered due to inactivity.[17] Meanwhile, the fact that the turnout effect is larger for baseline voters merely suggests that baseline voters tend to be closer to the turnout margin than baseline non-voters.

The same pattern of differential heterogeneity for registration and turnout appears for sample splits based on the socioeconomic status of a registrant's neighborhood. Whether splitting by parcel value, education, or income, the effect on registration is more severe for less advantaged areas, while the effect on turnout is more pronounced for more advantaged areas.[18]

Finally, we find that new developments cause greater reductions in both turnout and registration than rehabilitation projects, and large developments are more impactful than small developments. Notably, for developments with more than 60 units, the estimated effects on turnout and registration are both about -0.45 pp per election.

5 Exploring mechanisms

We now turn to exploring the mechanisms that may drive our results. We first discuss possible mechanisms, and then we test them empirically.

5.1 Possible mechanisms

Our data lets us distinguish between the following four mechanisms:

1. Moving away: The construction of a new low-income housing development may prompt some

[17] Baseline voters may not be de-registered for inactivity until after four post-period elections. However, they may still be de-registered for other reasons, such as moving away, receiving a felony, or dying.

[18] The difference in registration effects between more and less advantaged areas is again unsurprising: Less advantaged areas have lower turnout rates and thus include more registrants who are at risk of being de-registered due to inactivity.

incumbent residents to move away. In turn, moving may lead to lower political participation via channels such as disruption, administrative burdens, and loss of community. Administrative burdens are particularly severe if a registrant moves to a different county or state. If the registrant moves to a different county, they must re-register in order to maintain their registration status. If the registrant moves to a different state, they become unable to participate in North Carolina elections.

2. Felonies: LIHTC developments may alter neighborhood dynamics in ways that increase local crime rates, which could cause existing residents to receive felony convictions. Under North Carolina law, becoming a felon mechanically causes a registrant to become de-registered.

3. Death: Neighborhood changes following completion of an LIHTC development could raise local mortality rates via factors such as increased population density and pollution exposure, changes in local amenities, or shifts in neighborhood safety and social cohesion.

4. Inactivity and disengagement: As discussed in the introduction, LIHTC developments involve an influx of low-income residents, and low-income people tend to have low rates of political participation. This could depress political participation for existing residents even without spurring them to move away or making them unable to participate. Notably, an increase in inactivity and disengagement could arise to due to factors such as negative peer effects, weakened norms regarding participation, or reduced mobilization efforts by parties and campaigns.

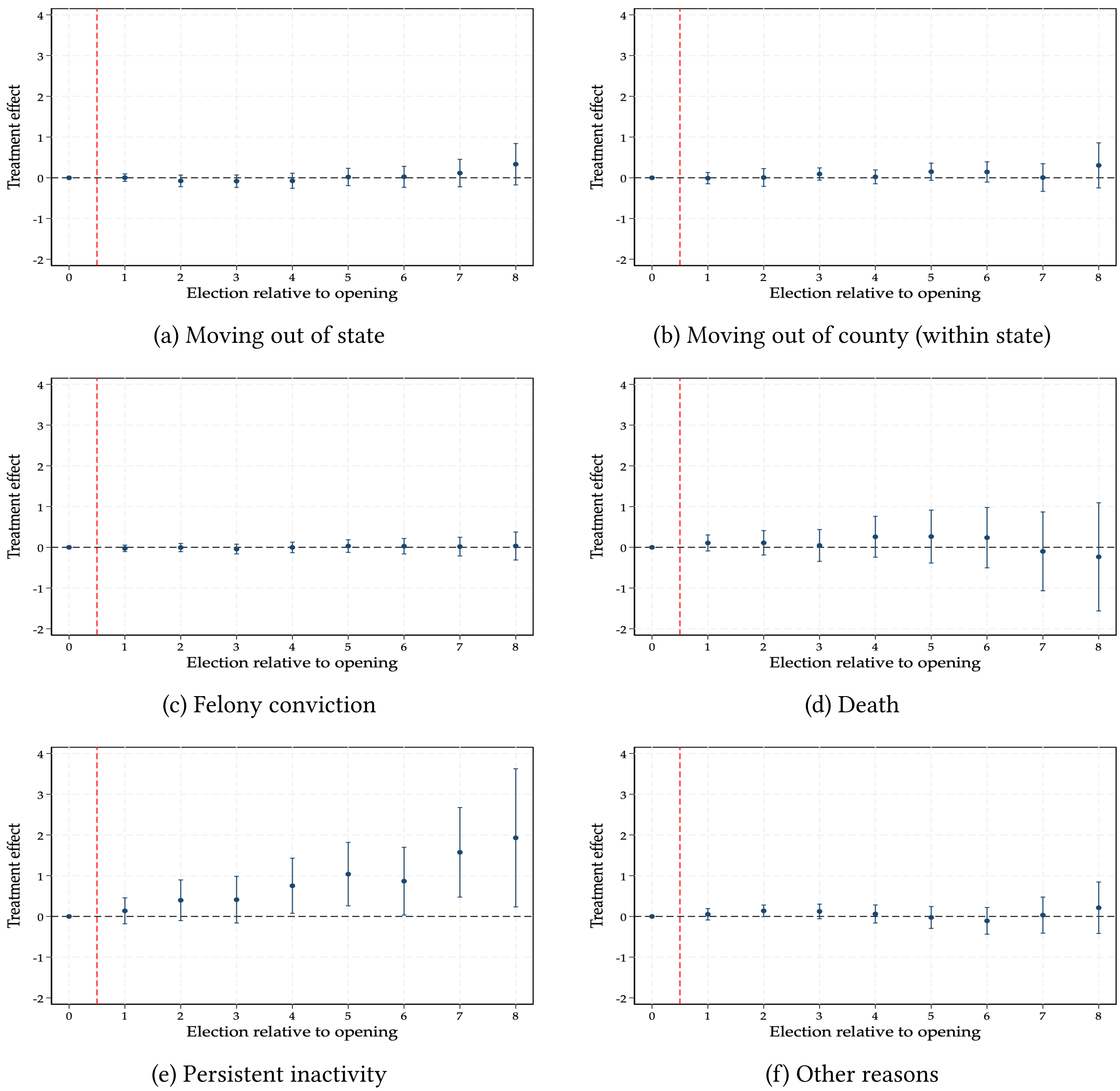
5.2 Testing the possible mechanisms

To test the possible mechanisms, we leverage the fact that the North Carolina voting data specifies why a registrant who has become de-registered lost their registration status. As mentioned, the data documents the following reasons for de-registration: moving to a different state, moving to a different county without re-registering, receiving a felony conviction, dying, or being persistently inactive. In addition, there are a few miscellaneous reasons that we group into an "Other" category.

We construct a set of mutually exclusive indicator variables that equal 100 if a registrant has become de-registered for a given reason and 0 otherwise.[19] We then estimate Equation (1) using each indicator variable as the outcome. Since we can't observe why someone is not registered in elections prior to when they first register, we fit Equation (1) only for the post-period (which is after all baseline registrants have been registered).

Figure 4 presents the results. The figure is similar to Figure 2, but it has six panels, which show coefficient estimates and 95% confidence intervals for β_τ for the different de-registration outcomes.

[19]The multiplication by 100 is again so that treatment effects are measured in percentage points. The indicator variables sum to the reverse of our main registration outcome. Sometimes, a registrant who has become de-registered re-registers in a later election. When this happens, the indicator variable that equals 100 "turns off" and becomes zero again.



(a) Moving out of state

(b) Moving out of county (within state)

(c) Felony conviction

(d) Death

(e) Persistent inactivity

(f) Other reasons

Figure 4: Effects on sources of de-registration

The figure is similar to Figure 2, but showing effects on indicator variables that equal 100 if a registrant has become de-registered for a given reason and 0 otherwise. See the text of Section 5.2 for details on these outcomes. All other details are the same as in Figure 2.

Figure 4 lets us rule out Mechanisms 1-3. Panels (a) through (d) show that completion of an LIHTC development does not significantly impact the probability that a resident becomes de-registered due to out-migration, felony conviction, or death. In addition, Panel (f) shows that there is no effect on the miscellaneous reasons that we grouped into the “Other" category.

Instead, the figure backs Mechanism 4. Panel (e) shows that LIHTC developments cause an increase in de-registration due to persistent inactivity. Further, the effects grow linearly over time, just as in Figure 2. Thus, it seems that a rise in political disengagement is the explanation behind the declines in turnout and registration that we observe.[20]

The finding that the evidence in this section favors Mechanism 4 is consistent with the heterogeneity analysis presented earlier. That analysis found that effects on registration are more pronounced for registrants who likely have a head start in being de-registered due to inactivity (namely, baseline non-voters and registrants from less advantaged neighborhoods), which is intuitive if the relevant mechanism is that LIHTC developments spur political disengagement. In addition, Figure 3 shows that effects on turnout are larger than effects on registration for registrants with high rates of participation (baseline voters and registrants from more advantaged areas). This is again intuitive under a disengagement story—i.e., it would reflect that voters who are not at risk of becoming de-registered due to inactivity stop turning out but do not lose their registration status. In contrast, the result would not be possible for mechanisms where changes in turnout are due to de-registration.

6 Conclusion

This paper shows that the completion of an LIHTC development prompts a steady decline in neighbors' political participation. Specifically, it causes a reduction in turnout and registration of about 0.23 and 0.25 pp per post-completion election. Our mechanisms analysis reveals that the key channel is not that people move away, nor is it an increase in felony convictions or mortality. Rather, LIHTC developments simply cause a rise in political inactivity: neighbors disengage from the electoral process, which makes them turn out less and eventually lose their registration status.

A comparison with the existing literature suggests that our effect estimates have a plausible magnitude. Prior papers find that experiencing foreclosure or a ten percent decline in home value depresses turnout by approximately 1-1.8 and 0.8 pp, respectively (Shah and Wichowsky, 2019; Hall, Yoder and Karandikar, 2021; McCartney, 2021). A one percent decrease in the neighborhood residential eviction rate, becoming a homeowner, and receiving a housing voucher as a child increase turnout by about 2.7, 2.7, and 2.9 pp, respectively (Slee and Desmond, 2023; Hall and Yoder, 2022; Chyn and Haggag, 2023). Finally, moving to a state with a 5% higher turnout rate increases the mover's turnout by about 1.9 pp (Cantoni and Pons, 2022). In comparison, our estimates imply that completion of an LIHTC development reduces neighbors' turnout by 0.9 pp in the fourth post-completion election, with bigger effects in later elections. The corresponding effect for a large development (> 60 units) is 1.8 pp.

[20] To be precise, Figure 4 rules out Mechanisms 1-3 as the explanation for the effects on registration. In addition, Mechanisms 2 and 3 cannot be the explanation for the effects on turnout if they are not the explanation for the effects on registration, as the only way they affect turnout is through de-registration. However, Figure 4 does not rule out that Mechanism 1 could drive the effects on turnout. For instance, an LIHTC development could spur an increase in within-county migration or an increase in the probability of moving to a different county and re-registering, both of which would not affect registration but may affect turnout. In results available upon request, we show that LIHTC developments do not impact these alternative mobility measures.

Our paper points to a few tasks for future research. First is to study the effects of LIHTC developments on the political participation of the residents of the developments, rather than the neighbors. Second is to further probe whether there are ways to design LIHTC developments that could mitigate the effects we document. We find smaller effects from smaller projects and from rehabilitation projects. One might hypothesize that the same would hold for mixed-income developments, where only a fraction of units are affordable; the fact that almost all North Carolina LIHTC developments are 100% affordable prevents us from assessing this. Finally, it would be interesting to see if there are spillover effects on political participation from other types of affordable housing policies, such as voucher policies.

Ultimately, our results highlight that affordable housing can impact neighborhoods in ways that deserve careful study. In addition, they add to a growing body of research that shows that political participation is a complex phenomenon that can be influenced by a variety of types of policies, including housing policies. This research suggests that policymakers should consider effects on political participation in their decision-making.

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A Additional tables

Table A.1: Summary statistics for LIHTC developments placed in service during 2007-2022

	Analysis sample		Dropped sample		North Carolina		Rest of the U.S.	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Year placed in service	2014.1	4.5	2013.8	4.8	2014.2	4.6	2013.6	4.5
New construction	0.75	0.44	0.83	0.38	0.76	0.43	0.54	0.50
Years between credit allocation and placement	2.33	2.07	2.78	3.90	2.39	2.42	1.61	3.13
Total units	66.5	31.9	70.1	36.4	67.0	32.4	80.0	81.4
Share of rent-capped units	0.99	0.06	1.00	0.01	0.99	0.05	0.92	0.23
Developments	508		90		605		18,369	

The table presents summary statistics for LIHTC developments placed in service during 2007-2022. “North Carolina" is all developments in the state during the period. “Rest of U.S." is all developments in other states during the period. “Analysis sample" is the sample we use when conducting our estimation. “Dropped sample" is the developments we dropped from the analysis sample—either because the treated population was heavily disadvantaged relative to the control group (73 developments) or because there was no variation in treatment status after conditioning on match-groups (17 developments). Also, the analysis and dropped samples exclude 7 projects that we were unable to locate geographically.

Table A.2: Heterogeneity for effects on the main outcomes by registrant characteristics

	Gender		Age		Race		Party Affiliation			Voted in Baseline	
	Female	Male	≤ 35	> 35	White	Non-White	Democrat	Republican	Unaffiliated	Yes	No
Panel A: Turnout											
Effect per election	-0.298***	-0.198**	-0.222*	-0.233***	-0.192	-0.245***	-0.190**	-0.333**	-0.270**	-0.345***	-0.151*
	(0.082)	(0.096)	(0.113)	(0.080)	(0.121)	(0.076)	(0.075)	(0.161)	(0.134)	(0.115)	(0.077)
Pre-treatment mean	40.2	37.4	22.6	49.0	43.7	35.5	42.4	44.5	29.3	61.8	15.2
Registrant-dev.-elections	430,568	289,525	246,079	477,077	335,199	389,404	425,394	129,121	155,979	332,034	405,961
Panel B: Registration											
Effect per election	-0.286***	-0.263**	-0.279**	-0.250***	-0.268**	-0.289***	-0.190*	-0.410**	-0.367***	-0.225**	-0.267***
	(0.095)	(0.107)	(0.121)	(0.091)	(0.125)	(0.102)	(0.100)	(0.167)	(0.139)	(0.104)	(0.101)
Pre-treatment mean	75.0	73.7	59.0	83.7	79.1	70.8	78.0	79.3	64.6	78.5	70.3
Registrant-dev.-elections	430,568	289,525	246,079	477,077	335,199	389,404	425,394	129,121	155,979	332,034	405,961

The table presents results corresponding to the left panel of Figure 3.

Table A.3: Heterogeneity for effects on the main outcomes by place and development characteristics

	Parcel Value		Block Population Density		Block-Group Education		Block-Group Income		Development Type		Development Size	
	Low	High	Low	High	Low	High	Low	High	New	Rehabilitated	Small	Large
Panel A: Turnout												
Effect per election	-0.146*	-0.318**	-0.414***	-0.224***	-0.189**	-0.302**	-0.187**	-0.355**	-0.284***	-0.143	-0.094	-0.454***
	(0.080)	(0.136)	(0.152)	(0.077)	(0.081)	(0.134)	(0.075)	(0.172)	(0.090)	(0.109)	(0.092)	(0.098)
Pre-treatment mean	36.2	45.0	43.3	38.1	37.8	40.6	38.9	39.5	39.6	37.4	42.8	37.2
Registrant-dev.-elections	500,168	225,043	124,538	602,147	452,394	282,064	531,880	202,811	504,157	233,838	379,925	358,070
Panel B: Registration												
Effect per election	-0.250***	-0.183	-0.349**	-0.263***	-0.284***	-0.197	-0.269***	-0.220	-0.305***	-0.151	-0.129	-0.441***
	(0.097)	(0.135)	(0.163)	(0.089)	(0.097)	(0.140)	(0.086)	(0.188)	(0.098)	(0.136)	(0.100)	(0.128)
Pre-treatment mean	73.9	75.5	79.3	73.3	76.2	72.3	76.0	71.2	73.8	76.4	80.5	71.4
Registrant-dev.-elections	500,168	225,043	124,538	602,147	452,394	282,064	531,880	202,811	504,157	233,838	379,925	358,070

The table presents results corresponding to the right panel of Figure 3.

B Details on trimming

As mentioned in Section 3, we trim the sample using an index which we created following the methodology of Anderson (2008).[21] Our index incorporates four variables: the log of the value of the property parcel associated with a registrant's baseline address, the share of college graduates in the registrant's baseline block-group, the log of median household income in the baseline block-group, and an indicator for whether the registrant died during the sample period.

The index combines the component variables by giving more weight to those that offer more unique information. It is constructed in four steps. First, we standardize the variables. Second, we generate an inverse covariance matrix of the standardized variables. Third, we use the inverse covariance matrix to obtain weights that we use to calculate a weighted average of the standardized variables. Fourth, we standardize the weighted average so that the resulting index has mean zero and standard deviation one.

After obtaining an index value for each i, we calculate the difference in average values of the index for each development's treatment and control groups. In the differences, a negative (positive) value means that the treatment group is more (less) disadvantaged than the control. Next, we drop the developments with the most negative differences until there are no statistically significant differences in the component variables, conditional on match-groups, between the overall treatment and control groups for the remaining developments. In total, the trimming procedure led to 73 projects being removed from the analysis sample.

B.1 Conditional balance tests before and after trimming

We present summary statistics for the sample before trimming in Table B.1. Analogous summary statistics for the sample after trimming were in Table 1.

Next, we run balance tests, conditioning on our main-specification match-groups, both before and after trimming. The balance tests are performed by estimating the equation below for the pre- and post-trimming samples during the baseline election:

$$\text{Characteristic}_i = \beta \cdot \text{Treat}_i + \gamma_g + \epsilon_i.$$

In the equation, Characteristic_i is a variable of interest, Treat_i is the treatment indicator, γ_g is a match-group fixed effect, ϵ_i is the error term, and β is the within-match-group difference in the characteristic between treatment and control.

Table B.2 reports the coefficient estimate and standard error for β for each characteristic and sample. As always, the standard errors are clustered by development.

As seen in the table, the treatment group is more disadvantaged than the control before trimming. After dropping the developments with the most relatively disadvantaged treatment groups, statistical differences in variables related to disadvantage disappear. Interestingly, differences in demographics and party affiliation persist, although they grow smaller after trimming. In Appendix G.1, we run robustness checks where we employ match-groups that are constructed using demographics and party affiliation; we find that doing so hardly changes our results.

[21] The methodology is explained in Appendix A of that paper.

Table B.1: Summary statistics for the sample before trimming

	Treatment		Control	
	Mean	SD	Mean	SD
Distance	0.13	0.06	0.25	0.03
Age	46.9	19.0	45.6	18.6
Male	0.40	0.49	0.42	0.49
White	0.45	0.50	0.46	0.50
Non-White	0.55	0.50	0.54	0.50
Democrat	0.58	0.49	0.56	0.50
Republican	0.18	0.38	0.19	0.39
Unaffiliated	0.24	0.43	0.26	0.44
Turnout	0.46	0.50	0.48	0.50
Dies in the sample period	0.10	0.31	0.09	0.29
Log(parcel value)	10.4	1.2	10.5	1.1
Log(block population density)	7.2	1.0	7.2	1.0
Block-group share college graduates	0.26	0.19	0.27	0.20
Log(block-group median hhld. income)	10.4	0.50	10.5	0.53
Registrant-developments	77,856		87,072	

This table is analogous to Table 1, but before trimming events where the treated population is substantially more disadvantaged than the control.

Table B.2: Conditional balance of characteristics before and after trimming

	Treatment − Control	
	Before trimming	After trimming
Age	1.040***	0.467*
	(0.266)	(0.246)
Male	-0.022***	-0.020***
	(0.004)	(0.004)
White	-0.027***	-0.025***
	(0.008)	(0.009)
Democrat	0.019***	0.016***
	(0.004)	(0.004)
Republican	-0.011***	-0.009***
	(0.003)	(0.003)
Turnout	0.000	0.000
	(0.000)	(0.000)
Dies in the sample period	0.010***	0.002
	(0.003)	(0.002)
Log(parcel value)	-0.082**	0.021
	(0.032)	(0.025)
Log(block population density)	0.015	0.019
	(0.025)	(0.022)
Block-group share college graduates	-0.003	-0.000
	(0.003)	(0.003)
Log(block-group median hhld. income)	-0.022**	-0.009
	(0.009)	(0.008)
Registrant-developments	164,928	148,345

The table presents results for conditional balance tests before and after trimming. See the text of Appendix B.1 for details on the balance tests. Standard errors are clustered by development. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C Robustness: Using only Census-geocoded projects

Our empirical strategy relies on accurately locating registrants and LIHTC projects. To do so, we geocoded registrants' and projects' addresses.[22] We started by using the U.S. Census Bureau's geocoder, which gained us coordinates for most but not all registrants and projects. For the projects that the Census geocoder could not locate, we then used the OpenStreetMap (OSM) geocoder, followed by manual searching on Google Maps. The full sample of geocoded LIHTC projects consists of 447 projects located via the Census geocoder, 61 via OSM, and 90 via manual geocoding. For computational reasons, we did not attempt to geocode the registrants who were not located by the Census geocoder. These registrants make up less than 5% of all registrants, yet they still represent a large absolute number, given the size of the North Carolina voting data. Further, the OSM and manual methods cannot handle large datasets.

To confirm that our results are not distorted by small geographical discrepancies stemming from the different geocoding techniques, we run a robustness test where we restrict attention to projects geocoded using the Census geocoder. For this test, the coordinates for the projects and registrants were obtained in the same manner. Thus, the sample ensures alignment between the measured locations of registrants and developments.

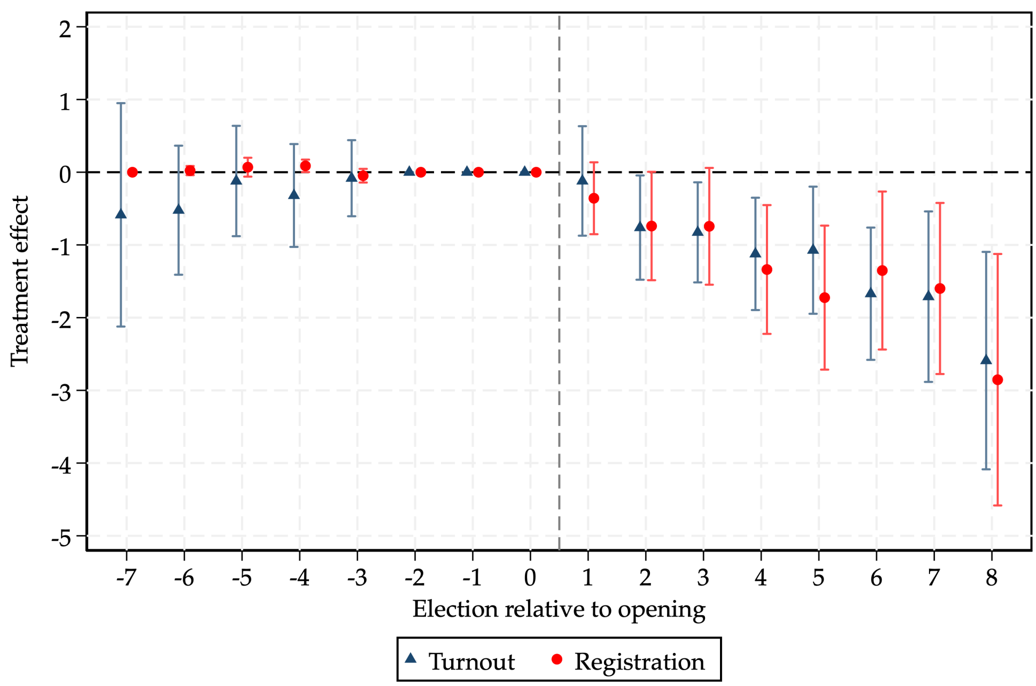

Figure C.1: Results for Census-geocoded projects

The figure is analogous to Figure 2, but for projects that were geocoded using the Census geocoder.

Figure C.1 presents coefficient estimates and 95% confidence intervals for the β_τ coefficients from Equation (1) using the Census-geocoded projects. Despite the smaller sample size leading to wider confidence intervals, the estimated effects for both turnout and registration are similar to the main results in Figure 2. This suggests that measurement error stemming from our geocoding procedure is not a major issue.

[22] HUD provides coordinates for each project, but these are insufficiently precise for identifying treatment boundaries.

D Robustness: Exposure to only one development

In the main analysis, people can be in the sample for multiple developments if they live within 0.3 miles of multiple LIHTC projects during the study period. In this appendix, we conduct a robustness check where we restrict attention to people who are in-sample for only one LIHTC development over the study period.

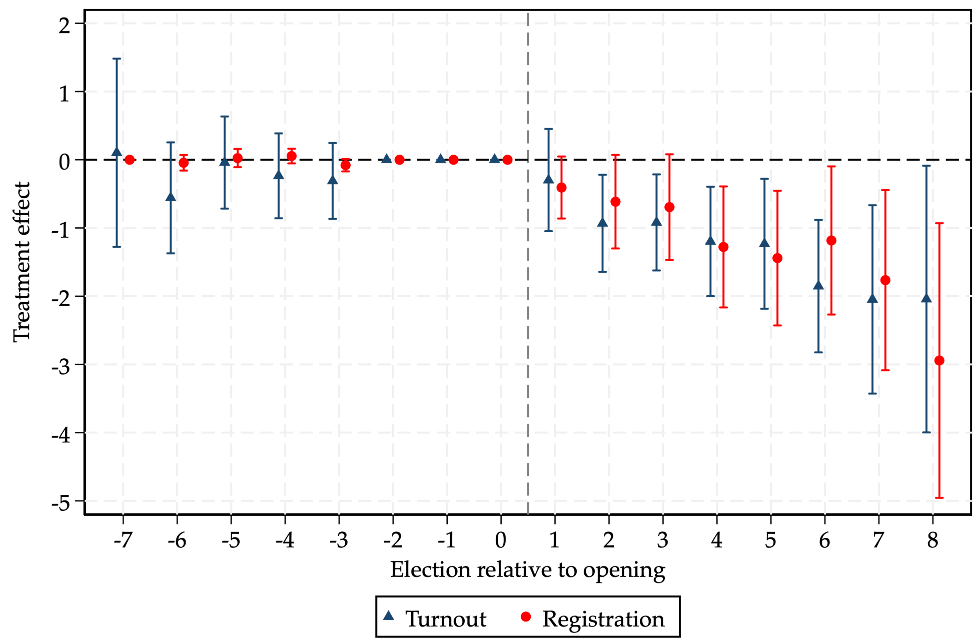

Figure D.1: Results for people exposed to only one development

The figure is analogous to Figure 2, but for people who are exposed to only one development during our sample period.

The results for the restricted sample, presented in Figure D.1, are similar to the results for our main sample (in Figure 2). This suggests that our effect estimates are not distorted by including people exposed to multiple developments.

E Robustness: Alternative treatment radii

An important part of our empirical strategy is our choice of radii in defining treatment and control groups. In the main analysis, we define the treatment group as registrants with baseline addresses within 0.2 miles of a LIHTC development and the control group as those between 0.2 and 0.3 miles away. In this appendix, we assess the robustness of our findings to alternative treatment radii.

Specifically, we re-estimate Equation (2) while varying the treatment radius to include individuals registered within 0.05, 0.10, 0.15, 0.20, 0.30, or 0.35 miles of a LIHTC project. In each case, we define the control group as people in the ring 0.10 miles beyond the treatment radius.

Coefficient estimates and 95% confidence intervals for β for the alternative treatment radii are in Figure E.1. β estimates are qualitatively consistent across radii and follow an intuitive pattern: they decline in magnitude while increasing in statistical precision as the treatment boundary expands. The decline is intuitive because the treatment group includes people farther away from a building (who are thus less impacted by it) as the treatment boundary expands. The increase in precision is also intuitive because the treatment group includes a larger number of people as the treatment boundary expands. Overall, our main choice of treatment radius of 0.2 seems to be a reasonable compromise between effect size and precision.

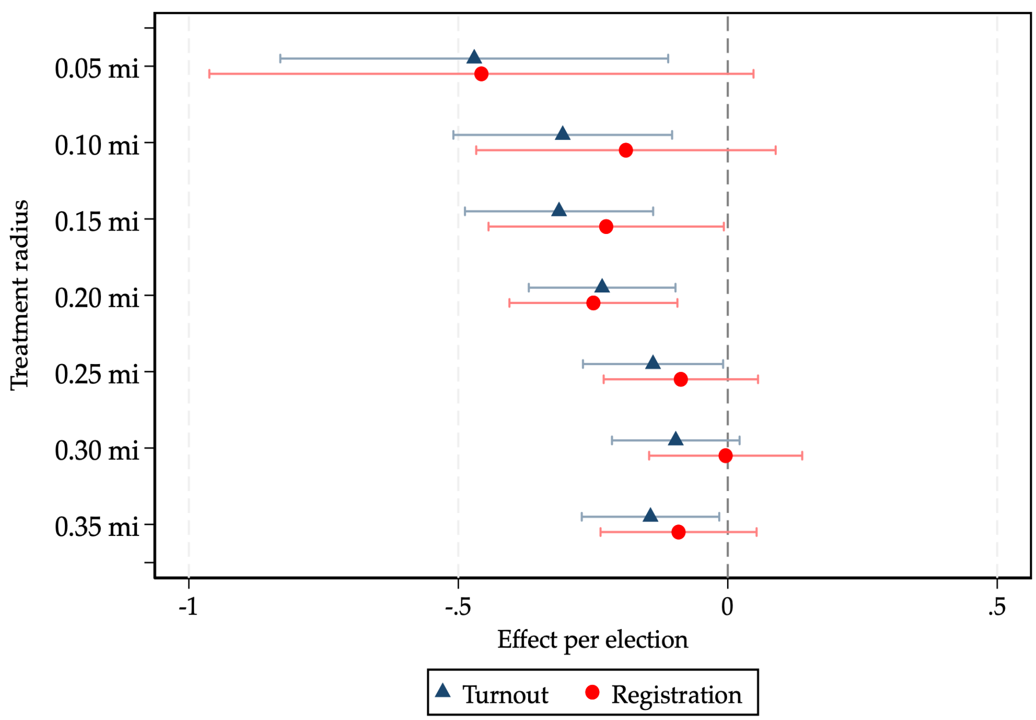

Figure E.1: Results for alternative treatment radii

The table presents coefficient estimates and 95% confidence intervals for β from Equation (2) for the listed treatment radii. The control group is always people in the ring 0.10 miles beyond a treatment radius. All other details are the same as for the columns labeled (1) in Table 2.

F Robustness: Linear treatment

In the main analysis, treatment is defined as a binary indicator for whether an individual lives within 0.2 miles of a LIHTC development during the baseline election, with untreated individuals defined as those residing between 0.2 and 0.3 miles away. While this treatment-control definition is intuitive and easy to interpret, it involves a sharp cutoff between the two groups that may not capture the gradual nature of how proximity influences residents.

In this appendix, we estimate a version of Equation (2) that uses a linear treatment specification. This specification allows treatment to vary smoothly with distance, capturing the idea that proximity may influence behavior along a gradient. Since treatment is no longer binary, a smaller number of match-groups are dropped due to lack of variation in the treatment variable, which gives us a slightly larger sample.

We specify treatment to decline linearly with distance up to 0.3 miles, which is the far boundary of the control group in our main analysis. Specifically, the new treatment variable is:

$$\text{Treat}_i(\text{distance}_i) = \frac{0.3 - \text{distance}_i}{0.3}$$

where distance_i is the straight-line distance (in miles) between an individual's residence and their LIHTC project. Individuals directly adjacent to a development receive the maximum treatment intensity of 1, while those at 0.3 miles have a treatment value of 0. As in the main analysis, individuals beyond 0.3 miles are excluded.

When using the linear treatment specification to estimate Equation (2), the β coefficient represents the effect of being right at a development (0 miles away) versus being 0.3 miles away. However, our main results in Table 2 represent the effect of being in the area near a development (up to 0.2 miles away) versus being 0.2-0.3 miles away. Thus, to ensure that results are comparable, we re-scale the treatment variable in the linear specification such that β represents the effect of being at the average distance in the main-specification treatment group (0.125 miles) versus the average distance in the main-specification control group (0.253 miles). We do this by multiplying the treatment variable by $(0.253 - 0.125)/0.300$.

Table F.1: Results for a linear treatment specification

	Turnout		Registration	
	Main	Linear	Main	Linear
Effect per election	-0.233***	-0.240***	-0.249***	-0.229***
	(0.069)	(0.060)	(0.080)	(0.080)
Pre-treatment mean	38.6	38.9	75.1	74.7
Registrant-developments	148,345	159,105	148,345	159,105
Registrant-dev.-elections	698,333	734,445	698,333	734,445

The table presents coefficient estimates and standard errors for β from Equation (2) for our main specification and for a specification using a linear treatment variable. See the text of Appendix F for details on the linear specification. The values in the "Main" columns are the same as those in the columns labeled (1) in Table 2.

Results for the linear treatment specification, together with those for the main specification, are in Table F.1. For both turnout and registration, the β estimate is extremely similar across specifications. Thus, our binary treatment specification seems to be a good approximation to the more involved linear specification.

G Robustness: Alternative match-groups

A key component of the empirical strategy is the use of exact-matching to ensure comparisons between similar treated and control individuals. To assess the sensitivity of the findings to our construction of match-groups, we re-estimate Equation (2) using six alternative definitions of match-groups. The variables in our main matching specification and in the six alternative specifications are specified below. Alternative specifications 1-3 vary the amount of pre-treatment voting behavior used in constructing match-groups, while specifications 4 and 5 add registrant demographic characteristics and specification 6 adds registrant party affiliation.

- **Main Specification**: Turnout history (in both general and primary elections) for the three most recent election cycles pre-treatment, registration status in general elections for the three most recent election cycles pre-treatment, the development, and the election-year in which the registrant is first registered.
- **Alternative Specification 1**: The development and the election-year in which the registrant is first registered.
- **Alternative Specification 2**: Turnout history (in both general and primary elections) for the most recent election cycle pre-treatment, registration status in the general election for the most recent election cycle pre-treatment, the development, and the election-year in which the registrant is first registered.
- **Alternative Specification 3**: Turnout history (in both general and primary elections) for the two most recent election cycles pre-treatment, registration status in general elections for the two most recent election cycles pre-treatment, the development, and the election-year in which the registrant is first registered.
- **Alternative Specification 4**: Turnout history (in both general and primary elections) for the two most recent election cycles pre-treatment, registration status in general elections for the two most recent election cycles pre-treatment, the development, the election-year in which the registrant is first registered, and registrant demographic variables (age category, gender, and race/ethnicity).
- **Alternative Specification 5**: Turnout history (in both general and primary elections) for the three most recent election cycles pre-treatment, registration status in general elections for the three most recent election cycles pre-treatment, the development, the election-year in which the registrant is first registered, and registrant demographic variables (age category, gender, and race/ethnicity).
- **Alternative Specification 6**: Turnout history (in both general and primary elections) for the two most recent election cycles pre-treatment, registration status in general elections for the two most recent election cycles pre-treatment, the development, the election-year in which the registrant is first registered, registrant demographic variables (age category, gender, and race/ethnicity), and party affiliation (Democrat, Republican, or other) in the baseline election.

Coefficient estimates and 95% confidence intervals for β for the different specifications are shown in Figure G.1. Values for the alternative specifications are always quite similar to those for our main specification, suggesting that our results are not contingent on our exact construction of match-groups.

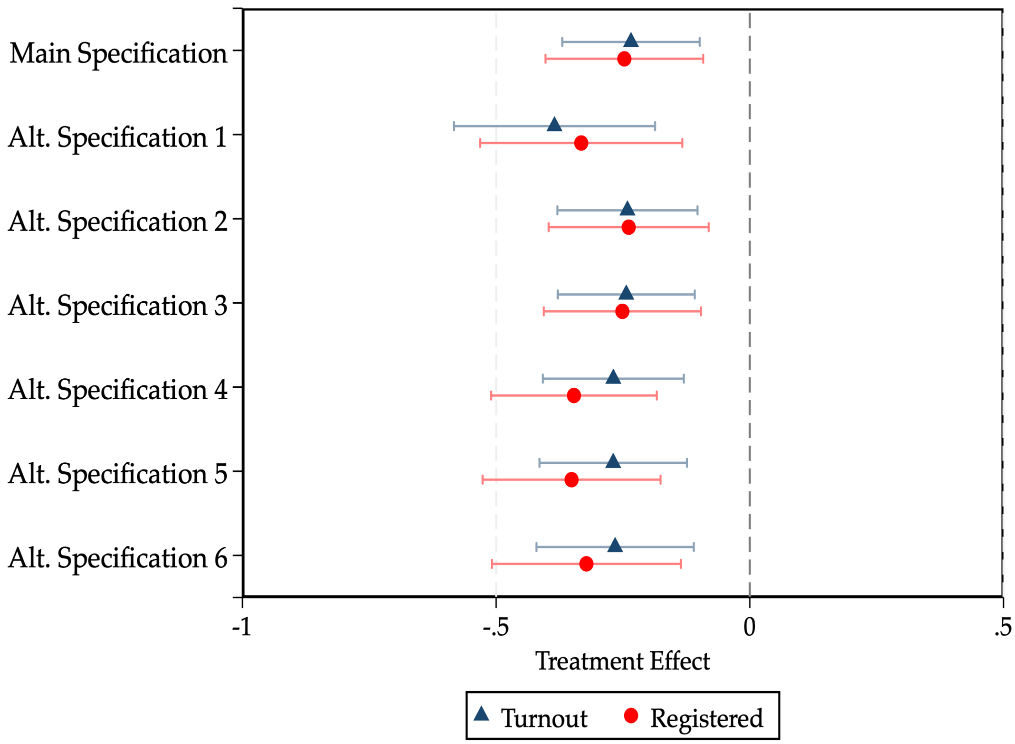

Figure G.1: Results for alternative match-groups

The figure presents coefficient estimates and 95% confidence intervals for β from Equation (2) for different specifications of match-groups. See the text of Appendix G for details on the match-group specifications. All other details are the same as for the columns labeled (1) in Table 2.

H Robustness: Defining the treatment year using funding allocation

In the main text, we define the treatment year for an LIHTC project as the year when the project is placed in service. In this appendix, we assess whether the treatment year should instead be set as the year when funding is allocated. Specifically, we estimate Equation (2) while defining the baseline election as the last general election prior to a project receiving funding. As shown in Appendix Table A.1, there is an average of 2.33 years between funding allocation and placement for LIHTC projects in our analysis sample.

Ex ante, there are reasons to believe that treatment could start as soon as a project receives funding. Funding allocation could align with the start of construction, with advertisements for the project, or both. If construction causes significant disruption or if residents decide to move away once they become aware that low-income housing will be built close to them, there could be changes in political behavior prior to project completion. Similarly, if political parties are strategic in their resource allocation, they could preemptively de-emphasize a neighborhood they believe will soon be comprised of a higher share of people with typically lower political participation.

Table H.1 presents the results. For each outcome, "Placement" corresponds to the β estimate for treatment defined based on project completion (as in the main text), while "Allocation" is for treatment defined based on funding allocation. The table shows that effects are smaller and less statistically significant when defining treatment based on funding allocation. Thus, while it is plausible that a project's funding provides some treatment to nearby registrants, the evidence suggests that completion is a much larger treatment. This backs our decision to study completion in the main text.

Table H.1: Results for defining treatment based on placement or funding allocation

	Turnout		Registration	
	Placement	Allocation	Placement	Allocation
Effect per election	-0.233***	-0.120*	-0.249***	-0.081
	(0.069)	(0.072)	(0.080)	(0.084)
Pre-treatment mean	38.6	38.2	75.1	77.5
Registrant-developments	148,345	127,997	148,345	127,997
Registrant-dev.-elections	698,333	672,501	698,333	672,501

The table presents coefficient estimates and standard errors for β from Equation (2) for specifications defining treatment based on when projects are placed in service or when their funding is allocated. The values in the "Placement" columns are the same as those in the columns labeled (1) in Table 2.

I Robustness: Placebo treatment timing

In this appendix, we perform placebo tests where we vary the treatment timing. The tests help to assess whether we could obtain results similar to our main ones even if there was no treatment.

For each LIHTC development, we generate placebo events by pretending that the treatment occurred 1, 2, 3, or 4 elections prior to when it actually did. Similarly, we re-set the baseline election as the election before a pretend treatment. We estimate Equation (2) for the offset treatments, excluding observations for elections that occurred after the true treatment in order to avoid contamination. Thus, the number of elections by which we offset the treatment determines the number of placebo post-period elections that can be used to estimate our coefficients.[23]

Before providing the placebo results, we modify our main results to serve as a comparison. Specifically, in Table I.1, we fit Equation (2) using only 1, 2, 3, or 4 post-period elections, which is the same number of post-period elections that we exploit in the placebo tests. The table shows that the main results are hardly affected by the number of elections used to obtain them. Statistical significance is lower for more restricted samples, but coefficient estimates are remarkably stable.

Table I.1: Results computed using limited numbers of post-period elections

	Turnout					Registration				
Number of post-period elections	All	1	2	3	4	All	1	2	3	4
Effect per election	-0.233***	-0.267	-0.358**	-0.274**	-0.250***	-0.249***	-0.266	-0.281*	-0.220*	-0.237**
	(0.069)	(0.333)	(0.181)	(0.117)	(0.093)	(0.080)	(0.217)	(0.165)	(0.128)	(0.110)
Pre-treatment mean	38.6	38.6	38.6	38.6	38.6	75.1	75.1	75.1	75.1	75.1
Registrant-developments	148,345	148,345	148,345	148,345	148,345	148,345	148,345	148,345	148,345	148,345
Registrant-dev.-elections	698,333	148,345	279,612	396,584	496,206	698,333	148,345	279,612	396,584	496,206

The table presents coefficient estimates and standard errors for β from Equation (2) for samples that use the listed numbers of post-period elections. The values in the "All" columns match those in the columns labeled (1) in Table 2.

Table I.2 displays the placebo results. All estimated effects are statistically insignificant except one, which is significant at a 10% confidence level. For turnout, the placebo point estimates are sometimes sizable in magnitude but are quite noisy and also seem to get smaller as the number of elections used to estimate them grows. For registration, the point estimates are usually small. Together, the results suggest that we would be unlikely to obtain our main findings—that effects on turnout and registration grow linearly with the number of post-period elections—if there was no treatment.

Table I.2: Results for the placebo tests

	Turnout				Registration			
Treatment offset	1	2	3	4	1	2	3	4
Effect per election	-0.463	-0.305	-0.364*	-0.153	-0.006	-0.275	-0.136	-0.165
	(0.373)	(0.212)	(0.191)	(0.166)	(0.245)	(0.172)	(0.155)	(0.146)
Pre-treatment mean	39.6	39.6	39.3	38.9	77.3	80.0	82.5	84.8
Registrant-developments	135,210	112,077	87,018	70,380	135,210	112,077	87,018	70,380
Registrant-dev.-elections	135,210	224,154	261,054	281,520	135,210	224,154	261,054	281,520

The table presents coefficient estimates and standard errors for β from Equation (2) for the placebo tests. The columns are named according to the number of elections by which the treatment is offset. See the text of Appendix I for details on the placebo tests. All other details are the same as for the columns labeled (1) in Table 2.

[23] As an example, suppose a project is completed in 2019. In our main analysis, we would define 2018 as the baseline election. If we offset the treatment by two elections, the pretend completion year would be 2015, the new baseline would be 2014, and we would calculate the placebo effect using the 2016 and 2018 elections.