Does Affordable Housing Lead to Electoral Backlash?

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Abstract

The United States is facing a housing affordability crisis that disproportionately burdens low-income households. Local governments play a large a role in making it difficult to build more housing. While research has documented how residents use participatory institutions to stymie new housing development, the role of local politicians' electoral incentives is less clear. Do voters punish incumbent politicians when new affordable is built near them? To answer this question, I merge a geocoded dataset of new affordable housing placed in service with precinct-level city council election results in Chicago and New York City. Exploiting within-district variation in the distance to new housing, I find no evidence that politicians are punished for new housing in either city. Supplemental analyses using survey data in a wider geographic area corroborate this result. These findings suggest that there may be relatively weak electoral incentives to limit new affordable housing, further spotlighting the role of non-electoral participatory institutions in the politics of housing.

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The United States is experiencing a housing affordability crisis. Nationwide, nearly half of households that rent their homes are rent burdened, meaning they spend over 30% of their income on rent. Among low-income households, that number rises to nearly 90% (National Low Income Housing Coalition, 2019). This affordability crisis is driven by a housing shortage across all levels of the housing market (Glaeser and Gyourko, 2018), with a particular dearth of housing aimed at very low-income households. The National Low Income Housing Coalition estimates a shortage of around 7 million homes for low-income families: it finds that there are only 37 affordable housing units available for every 100 low-income households (National Low Income Housing Coalition, 2021).

One of the primary barriers to expanding the supply of both market-rate and affordable housing is the local regulatory environment. Land use policy in the United States often gives municipal governments substantial power to prevent or delay new housing (Glaeser and Gyourko, 2018). Homeowners, especially, tend to oppose new housing in their areas and use participatory institutions to pressure local officials to block proposals for new housing (Einstein, Glick and Palmer, 2020; Yoder, 2020; Hankinson, 2018; Marble and Nall, 2020).

City councils play a large role in the process of stymieing new housing construction. In many cities with single-member districts, city councils adhere to a norm of "aldermanic privilege," whereby councils give deference to individual councilors over land use decisions in their district. Councilors often use this informal institution to block new multifamily housing (Hankinson and Magazinnik, 2021), leading one commentator to dub city councils the "Villains of the Housing Crisis" (Grabar, 2021).

City council members presumably block new housing in their district because they fear electoral backlash. However, despite widespread mobilization to oppose new housing *ex ante*, it is an open question whether politicians pay an electoral penalty *ex post* when new housing is built. On the one hand, the logic of retrospective voting suggests that voters should punish incumbent officials who pursue policies that oppose their interests (Fiorina, 1981; Ferejohn, 1986). To the extent that voters view new housing as going against their interests, politicians

should fare worse in areas with new housing. On the other hand, there are reasons to doubt this logic applies to housing politics. There is mixed evidence of retrospective voting in local elections (Berry and Howell, 2007; Payson, 2017). And many of the feared deleterious effects of new housing, such as increased crime or degradation of local public goods, generally do not come to pass (Freedman and Owens, 2011; Di and Murdoch, 2013). Voters may, in the end, have little reason to punish politicians.

To evaluate whether voters punish politicians for new housing, I examine construction of affordable housing financed by the federal Low Income Housing Tax Credit (LIHTC). This program subsidizes developers who build housing that includes units set aside for lower-income tenants. The majority of new affordable housing built in the U.S. is funded through LIHTC. Since its inception in 1987, LIHTC has funded nearly 50,000 developments, comprising over 3 million housing units.

I combine a geolocated database of LIHTC projects with geocoded precinct-level results from city council elections in Chicago and New York. I exploit within-district variation in the distance to new affordable housing to estimate the effect of new LIHTC projects on incumbents' vote share. This approach allows me to control for politicians' prior popularity in small geographic areas, along with district-wide swings in incumbents' popularity.

I find little evidence that incumbent politicians are punished at the polls for new affordable housing. I find that incumbents receive roughly the same vote share, on average, in precincts that receive new affordable housing compared to precincts that do not.

To complement the electoral analysis, I use survey data to examine a larger set of locations and investigate individual-level heterogeneity in responses to local affordable housing. I match geographic areas that had new affordable housing open just before versus just after the surveys are conducted. I find little evidence that respondents who live near new affordable housing developments have more unfavorable views of either their local politicians or of the zoning policy in their communities. These null effects hold among both renters and homeowners.

My findings suggest that incumbent politicians do not pay electoral penalties for new affordable housing in their districts. Combined with prior research, this result suggests the central role of non-electoral participatory institutions — rather electoral incentives — in translating anti-housing sentiment into local policy.

1 Local Opposition to Housing Development

One of the premier policy challenges the United States faces is housing scarcity. Decades of undersupply of housing have led to rising prices — especially in the most productive metropolitan regions (Glaeser and Gyourko, 2018). A primary cause of this shortage is a combination of opposition to new housing construction among incumbent residents and institutions that turn this opposition into policy (Einstein, Glick and Palmer, 2020). The effect of such laws (if not always the outright goal) is to further racial and economic inequalities (Trounstine, 2018; Sahn, 2019; Rothstein, 2017) and to distort migration decisions in a way that has large macroeconomic consequences (Hsieh and Moretti, 2019; Ganong and Shoag, 2017). The lack of affordable housing particularly burdens low-income households, who may be at risk of eviction — a process that has a multitude of negative social and economic consequences reinforcing poverty (Desmond, 2016).

Existing research documents the interplay between public opinion and institutions in generating policies that restrict new housing development. Homeowners are more likely to oppose new housing developments (Marble and Nall, 2020), attend and comment at public meetings over housing policy (Yoder, 2020; Einstein, Palmer and Glick, 2019), and more likely to turn out in elections than renters (Hall and Yoder, 2018). Because people's homes are often their most valuable asset, homeowners have a particular incentive to restrict the supply of new housing in order to protect their home values (Fischel, 2001). But even renters tend to oppose new housing located near them, due to concerns about displacement and potential nuisances created by new housing and higher population density (Hankinson, 2018).

Existing research has primarily focused on how participatory institutions — such as zoning board hearings and discretionary review hearings — give citizens a way to voice opposition to new housing. Analyses of meeting minutes find that commenters often cite concerns about crime, traffic, environmental impacts, and neighborhood change in opposition to new housing (Einstein, Palmer and Glick, 2019; Yoder, 2020).

But, ultimately, land use decisions are made by public officials. Participatory institutions, in and of themselves, generally do not provide a direct incentive for officials to enact any particular policies. Instead, accountability happens either directly or indirectly through elections. Despite the theoretically important role of elections, it is unclear whether voters provide politicians electoral incentives in the domain of housing policy. These incentives are likely to be particularly strong for members of city council.

City councils are important actors in shaping housing policy. Many cities with single-member districts adhere to a norm of "aldermanic privilege," in which the entire city council defers to the district representative over land use decisions in their district. Theoretically, this norm may generate policy outcomes that limit the supply of housing. New housing provides diffuse benefits through market mechanisms (Mast, 2021*a*), making it difficult to identify the beneficiaries of any particular new housing project. On the other hand, new housing imposes costs primarily on residents who live close to the new development. As a result, individual city council members often have an incentive to veto new housing construction in their districts. Aggregate-level studies bear out this prediction: cities that switch from at-large city council elections to single-member districts see reductions in the housing supply (Hankinson and Magazinnik, 2021; Mast, 2021*b*).

What is the mechanism driving this outcome? Do participatory institutions play a primary role in shaping city council members' decisions, or are politicians motivated by electoral concerns? Elections are the primary accountability mechanism generating incentives for politicians to be responsive to constituents' preferences (Fearon, 1999). In order for elections to generate such incentives, voters should be willing to punish elected officials who imple-

ment policies that contravene their preferences. Given that local residents tend to oppose affordable housing in their neighborhoods, this logic leads to a hypothesis: Vote share for incumbent politicians should be lower in areas located closer to new affordable housing than in areas farther from it. Additionally, this effect should be especially strong among homeowners, because they have a stronger economic incentive to limit new affordable housing construction in their neighborhoods.

In the following sections, I investigate this hypothesis using a geocoded dataset of affordable housing projects, along with geographically linked city council election data in two cities. To test the hypothesis that homeowners should have stronger reactions than renters, I turn to survey data.

2 Using New Affordable Housing to Measure Backlash

To investigate whether new affordable housing generates backlash among neighbors, I pair data on the opening of projects funded by the Low-Income Housing Tax Credit with precinct-level election data and survey data.

2.1 Data on Affordable Housing Construction

To find new affordable housing projects, I turn to a database of housing developments funded through the Low-Income Housing Tax Credit. Since being enacted in 1986, LIHTC is the largest source of affordable housing funding in the United States (National Housing Law Project, 2017). Under the program, the federal government allocates a fixed amount of LIHTC funding to each state. State agencies then allocate these tax credits to developers who build new affordable housing or upgrade existing housing. Developers then sell these tax credits to investors in order to raise capital for construction or renovation. In order to qualify for LIHTC funding, developers must agree to rent a certain number of units below market rate. Developers have two choices: they can rent out at least 20% of the units at no more than 50% of the area median income; or, they can rent at least 40% of the units at no

more than 60% of area median income.[1]

Importantly for my purposes, HUD makes available a comprehensive database of LIHTC-funded projects that includes geocoded property-level information (Office of Policy Development and Research, 2021). When paired with political outcome data measured at a fine geographic resolution, this dataset allows me to examine the localized effects of new affordable housing construction. In 2019, there were 610 LIHTC projects placed in service nationwide, representing almost 50,000 total housing units and about 43,000 income-restricted units. In that year, about 35% of LIHTC-funded projects had a mix of low-income and market-rate housing.

Prior research has studied the effect of new LIHTC construction on the local housing market, generally finding that LIHTC construction boosts property values in low-income areas and reduces property values in high-income areas (Diamond, McQuade and Qian, 2018; Baum-Snow and Marion, 2009). Additionally, prior research has used LIHTC data to study the effect of affordable housing on other social outcomes. This literature generally suggests that, contrary to concerns from many existing residents, affordable housing generally has no effect or beneficial effects on outcomes such as crime (Freedman and Owens, 2011) and school quality (Di and Murdoch, 2013).

2.2 Election Outcome Data

I gather precinct-level data from two cities, New York and Chicago, on city council elections. These cities are ideal test cases for the possibility that incumbent politicians may be held accountable for new affordable housing. In both cities city council members enjoy "aldermanic privilege" — a norm by which individual city council members are given authority over land use decisions within their districts (Grabar, 2021; Fuller, 2020). In such cities, there is a clear reason for voters to attribute responsibility for new housing to their

[1]The Department of Housing and Urban Development uses Census data to publish "area median income" limits annually for each metropolitan area and non-metropolitan county in the country.

elected representatives.[2] There is also empirical evidence that single-member districts lead to reduced housing supply, suggesting an active role for council members in shaping housing decisions in their districts (Hankinson and Magazinnik, 2021).

To estimate the effect of new housing construction on incumbent vote shares, I use a difference-in-differences style analysis that compares the change in incumbents' vote shares in precincts close to the new housing to the change in precincts farther away from new housing. This strategy leverages within-district variation in proximity to new housing developments — providing more credible inferences than cross-district designs.

In order to implement this research design, I need repeated precinct-level observations. There are two implications. First, I focus on sets of elections conducted under the same electoral map. As redistricting typically occurs every 10 years and city council terms are often 4 years, this leaves two election cycles per city. Second, to focus on an accountability, rather than selection, mechanism, I consider the electoral performance of incumbents running for re-election.

I gather precinct-level data from two elections in each of the two cities: the 2015 and 2019 aldermanic general elections in Chicago, and the 2017 and 2021 city council primary elections for New York. I focus on primary elections in New York because the vast majority of the competition for city council seats is within the Democratic Party. General elections are generally not competitive. In Chicago, there are nonpartisan elections and no primary elections, so I focus on the general elections. These elections are the two most recent elections in each city, and took place under the same electoral maps. This makes it simple to map a candidates' precinct-level vote share in the latter election to their vote share in the prior election.

I merge these precinct-level results with shapefiles of the precinct boundaries. These

[2]There is a debate over the extent to which voters correctly attribute responsibility. Some researchers find that voters do a poor job attributing responsibility (Achen and Bartels, 2016; Healy and Malhotra, 2013), while others challenge those findings (Fowler and Hall, 2017) or find evidence of retrospective voting over issues that politicians can control (Ebeid and Rodden, 2006). If anything, the fact that lines of responsibility are clear in this case should make it more likely to find evidence of retrospective voting.

shapefiles allow me to geocode each LIHTC project into a precinct, as well as calculate the distance from each precinct to the nearest LIHTC unit. I will use this geocoding to measure the treatment variables of interest.

2.3 Survey Data

Election return data is most relevant for assessing real-world retrospective voting. But it is possible that null effects in the election data might reflect competing positive and negative effects among different subgroups. Moreover, it could be that the effects of affordable housing differ outside of the two large cities that I focus my primary analysis on.

To address these shortcomings, I supplement the electoral data with survey data from the Congressional Cooperative Election Study. In 2016 and 2018, the CCES asked respondents questions about their local government. I pair these data with the LIHTC data to measure whether respondents' assessments are affected by the opening of new affordable housing in their community. I present more details about the data and research design in Section 4.

3 The Effect of Affordable Housing on Incumbent Vote Share

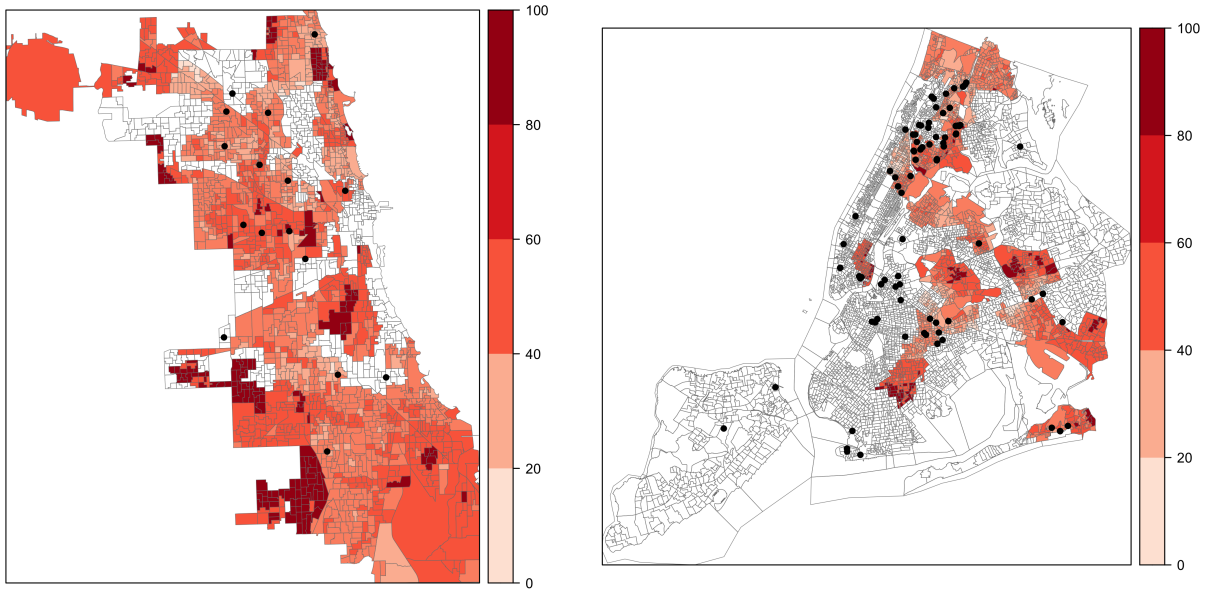
In this section, I present evidence on how LIHTC projects affect incumbents' vote shares in Chicago and New York City. First, I outline the general analysis strategy, then present results for each city.

3.1 Analysis Strategy

The goal is to estimate the causal effect of new LIHTC housing construction on the electoral performance of incumbent city council members. Simple cross-sectional associations between vote share and LIHTC placement are unlikely to recover this causal effect. Places that receive LIHTC projects are systematically different than those that do not on socioeconomic variables that are likely to be correlated with political preferences (Baum-Snow and Marion, 2009). Instead, I exploit within-district variation in voters' distance to new LIHTC projects.

Figure 1: New LIHTC Projects in Four Years Prior to Election

(a) Chicago **(b)** New York City



Notes: Points show the location of new LIHTC units. Shading shows the precinct-level vote share for the incumbent city council member in the most recent election. Precincts without an incumbent running for re-election are unshaded.

To begin, I match every property in the LIHTC database to its corresponding precinct in the election dataset. Then, I generate two separate "treatment" variables for each precinct: (1) an indicator for whether a new LIHTC project was placed in service in the four years prior to an election (i.e., during the incumbent's term); and (2) the distance from the centroid of the precinct to the nearest LIHTC unit placed in service in the prior four years. I also generate a control variable for the total number of pre-existing LIHTC projects in the precinct. Figure 1 maps the location of these new LIHTC projects, along with the precinct-level vote share for the incumbent in the most recent election.

The two treatment variables are useful under different hypotheses about the effect of new affordable housing. The first treatment variable may be useful if there is reason to think that the effect of new affordable housing is intensely localized — such that residents several blocks away are unaware and generally unaffected by new affordable housing development. In that case, we would expect any electoral effects to be geographically concentrated around

the new construction. However, if residents are affected and politically motivated by new affordable housing outside of their immediate environs, the first variable may incorrectly classify some precincts as untreated. This would have the effect of biasing treatment effect estimates toward zero. The second treatment variable addresses this concern. It measures the distance to new housing, allows for the possibility that electoral effects may persist outside of the immediate precinct in which new housing is located. This variable is useful under the assumption that the size of the electoral effects is a function of residents' distance to the new housing.

With these treatment variables in place, I rely on two related identification strategies that exploit within-district variation. First, I estimate models that control for lagged vote share. These models identify the average treatment effect on the treated units (ATT) under an ignorability assumption: conditional on prior vote share, assignment to treatment is independent of the potential outcomes for vote share in the next election. Second, I estimate difference-in-differences models that compare the change in vote share in treated versus control precincts. This method identifies the ATT under a parallel trends assumption: the trend in potential outcomes under control is the same for treated and control units. These two methods also have a so-called "bracketing" relationship. If the ignorability assumption is true, then incorrectly assuming parallel trends will overstate the magnitude of the effect. If the parallel trends assumption is true, then incorrectly assuming ignorability will understate the magnitude of the effect (Angrist and Pischke, 2009; Ding and Li, 2019). In practice, these two methods produce substantively similar results.

Formally, the estimating equations for the lagged dependent variable model and difference-in-differences model, respectively, are:

$$Y_{idt} = \tau D_i + \gamma Y_{idt-1} + \beta X_i + \alpha_d + \epsilon_{idt}$$

$$\Delta Y_{idt} = \tau D_i + \beta X_i + \alpha_d + \epsilon_{idt}.$$

In these equations, Y_{idt} is the vote share for the incumbent in precinct i, in district d, at time t and Y_{idt-1} is the lagged vote share.[3] ΔY_{idt} is defined as the change in vote share from one election to the next, namely $\Delta Y_{idt} := Y_{idt} - Y_{idt-1}$. Next, D_i is the treatment variable for precinct i — either binary or continuous, depending on the specification. X_i a control variable for the number of pre-existing LIHTC units in the precinct. α_d is a district fixed effect, so that the model exploits within-district variation in treatment. Finally, ϵ_{idt} is the error term.

For inference, I take two approaches. First, I compute robust standard errors clustered by council district. However, due to the fact that relatively few precincts are treated, these asymptotic standard error estimates may not be reliable. In the second approach, I use a cluster wild bootstrap to compute p-values for the treatment variable. MacKinnon and Webb (2018) find that this method produces more reliable uncertainty estimates when there are relatively few treated units.

3.2 Chicago

In the four years prior to the 2019 city council election in Chicago, there were 16 new LIHTC projects placed in service in 14 different wards. Table 1 reports estimates of the effect of these new affordable housing developments on incumbents' vote share in the precincts in which they are located. This table reports estimates from the lagged dependent variable specification; the difference-in-differences specification is reported in Appendix Table 8.

The estimates suggest there are minimal electoral effects of new affordable housing projects. The first column of Table 1 reports a simple difference in means in incumbent vote share in 2019 between precincts with and without new LIHTC projects. In aggregate, incumbents receive a statistically insignificant 1.29 percentage point lower vote share in precincts with new LIHTC units than in those without new LIHTC units. The second and third columns add controls for prior vote share and the number of pre-existing LIHTC

[3]The 2021 elections in New York City used rank-choice voting. Below, I discuss how I modify the outcome variable to accommodate this feature of the election.

Table 1: Chicago: Effect of New LIHTC Unit on Incumbent Vote Share

	Vote Share, 2019					
	(1)	(2)	(3)	(4)	(5)	(6)
New LIHTC Project	-1.29	-5.10	-3.08	5.31^{+}	4.46	4.51
	(9.11)	(7.59)	(7.33)	(2.73)	(2.75)	(2.74)
Lagged Vote Share		0.22^{+}	0.22^{+}		0.44^{**}	0.44^{**}
		(0.12)	(0.12)		(0.08)	(0.08)
# Existing LIHTC Projects			-1.25			-0.04
			(1.16)			(0.47)
Ward FE				✓	✓	✓
R^2	0.00	0.07	0.07	0.83	0.87	0.87
Observations	1,803	1,803	1,803	1,803	1,803	1,803
Bootstrap p	0.88	0.52	0.70	0.26	0.38	0.24

Notes: Outcome variable is the precinct-level vote share for the incumbent in 2019, measured from 0 to 100%. Standard errors clustered at the ward level are presented in parentheses. Bootstrap p-value refers to the coefficient on New LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

units, respectively. While the point estimates increase in magnitude, they continue to be statistically insignificant. Moreover, these columns are driven by cross-ward variation.

The models in the fourth through sixth columns include ward fixed effects, and thus leverage within-district variation in treatment exposure. If anything, these models suggest there may be a slight *positive* effect of new affordable housing on incumbents' vote share. However, only in column 4, which reports a model with no control variables other than the fixed effects, is the coefficient statistically significant at even the 10% level. My preferred specification, presented in column 6, controls for lagged vote share as well as the number of pre-existing LIHTC projects. In that model, there is no statistically significant effect. Using the cluster-robust standard errors reported in parentheses to construct 95% confidence intervals, we can rule out decreases in vote share larger than about 1 percentage point and increases larger than about 10 percentage points. Finally, across all models, inference using the cluster wild bootstrap suggests no significant effects of new LIHTC projects on precinct-

Table 2: Chicago: Effect of Distance to New LIHTC Units on Incumbent Vote Share

	Vote Share, 2019					
	(1)	(2)	(3)	(4)	(5)	(6)
Dist. to Nearest LIHTC Project	2.58^{+}	3.37*	3.33*	-0.38	-0.93	-0.93
	(1.45)	(1.57)	(1.58)	(1.04)	(0.92)	(0.91)
Lagged Vote Share		0.25*	0.25*		0.45**	0.45**
		(0.12)	(0.12)		(0.07)	(0.07)
# Existing LIHTC Projects			-0.89			-0.03
			(1.25)			(0.47)
Ward FE				✓	✓	✓
R^2	0.02	0.10	0.11	0.83	0.87	0.87
Observations	1,803	1,803	1,803	1,803	1,803	1,803
Bootstrap p	0.09	0.06	0.06	0.73	0.35	0.35

Notes: Outcome variable is the precinct-level vote share for the incumbent in 2019, measured in percent of total votes. Standard errors clustered at the ward level are presented in parentheses. Bootstrap p-value refers to the coefficient on Distance to LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

level vote share for the incumbent — as shown by the p-values presented in the final row of the table.

This first set of results suggests there are minimal effects of new affordable housing on incumbent vote share in the precincts in which the new housing in located. However, as outlined above, these models do not account for the possibility that there are electoral effects of new affordable housing beyond the immediate precinct in which the project is located.

The next set of models, presented in Table 2, estimates the average association between precinct-level incumbent vote share and the distance to the nearest new LIHTC unit. The set of control variables varies in the same way as in the previous table. If residents closer to the new housing project are more likely to punish the incumbent politician, we should observe a positive coefficient on the distance to the nearest LIHTC unit. I obtain positive point estimates in columns 1-3 — models that leverage both within- and across-ward variation. However, as before, none of these estimates is statistically significant. In the models presented

in columns 4-6, which include ward fixed effects, I again find a small but insignificant *positive* association between proximity to new affordable housing and incumbent vote share. Again, none of the wild bootstrap p-values are significant.

3.3 New York City

Next, I present analogous sets of regressions for the New York City elections. However, there are several differences compared to the Chicago analysis. Most straightforwardly, I use election results from the 2021 Democratic primary instead of general election results. This choice is made because Democrats are all but guaranteed victory in the general election in most parts of New York.

More difficult is a change in electoral system. In 2021, New York City switched to rank-choice voting (RCV) for municipal elections, wherein voters are allowed to rank up to five candidates for each race. Earlier elections were conducted under a standard first-past-the-post format. This change requires a slight modification of the outcome variable, as vote share is no longer so simple to calculate. Instead, I use individual-level cast vote records to compute two summaries of a candidate's performance in each precinct. These records show each voter's ranking profile — i.e., their ranking of their top five candidates.

First, I compute the share of voters within a precinct who ranks each candidate as their top pick. I refer to this outcome as the *first-place share*. This measure is simple to understand and somewhat analogous to vote share in a typical first-past-the-post election. However, it ignores all information in the rankings beyond voters' first choice. Moreover, due to strategic considerations, a voter's top choice in a RCV election may not be the same as their vote in a typical FPTP election.

To incorporate information from the full set of rankings, I compute a second outcome measure based on the candidate's Borda count. I call this outcome variable a candidate's *Borda share*. This method assigns points to each candidate based on where each voter ranks them. Because voters can rank their top 5 candidates, I assign 5 points to a candidate who

is ranked first, 4 points to a candidate who is ranked second, and so on. I then normalize within precinct by dividing by the sum of all candidates' points within a precinct. Formally, denote the set of voters by $\mathcal{V}$, the set of candidates by $\mathcal{C}$, and voter i's ranking of candidates c by x_{ic}. If voter i does not place candidate c among their top 5 choices, then we let $x_{ic} = 6$. A candidate's Borda count b_c is given by:

$$b_c = \sum_{i \in \mathcal{V}} 5\, I(x_{ic} = 1) + 4\, I(x_{ic} = 2) + 3\, I(x_{ic} = 3) + 2\, I(x_{ic} = 4) + 1\, I(x_{ic} = 5).$$

Then, a candidate's Borda share Y_c is simply their Borda count divided by the sum of Borda counts:

$$Y_c = \frac{b_c}{\sum_{c' \in \mathcal{C}} b_{c'}}.$$

While the Borda share outcome uses more information than the first-place share outcome, the two variables are highly correlated. In the 2021 City Council primary election, these two precinct-level measures are correlated at $r = 0.93$.

I conduct same set of analyses using these two outcome variables as in the Chicago analysis. Due to term limits in New York, there are fewer incumbents running for re-election districts that saw at least one new LIHTC project placed in service: there are only three districts with at least one treated unit and a member up for re-election. Nonetheless, because the primary research design leverages within-district variation, the smaller number of districts here is not a primary concern for inference. Again, I focus on the lagged dependent variable models in the main text and relegate the difference-in-differences results — which use just the first-place share outcome — to Appendix Table 9.

Table 3 presents the lagged dependent variable models for the first-place share outcomes. Across specifications, I find mostly null results with relatively small point estimates. The cross-sectional results shown in the first column shows that precincts with new affordable housing have slightly higher incumbent vote share than those without new affordable housing.

Table 3: NYC: Effect of New LIHTC Unit on Incumbents' First-Place Share

	Share of First-Place Ranking, 2021					
	(1)	(2)	(3)	(4)	(5)	(6)
New LIHTC Project	1.27	-2.20	1.77	-3.01*	-1.87	-1.80
	(4.73)	(4.41)	(3.33)	(1.13)	(1.86)	(1.92)
Lagged Vote Share		0.44**	0.49**		0.45*	0.45*
		(0.07)	(0.08)		(0.13)	(0.13)
# Existing LIHTC Projects			-3.13*			-0.10
			(1.06)			(0.14)
District FE				✓	✓	✓
R^2	0.00	0.18	0.27	0.63	0.72	0.72
Observations	851	851	851	851	851	851
Bootstrap p	0.77	0.69	0.69	0.11	0.54	0.58

Notes: Outcome variable is the share of ballots within a precinct ranking the incumbent first in 2021. Standard errors clustered at the district level are presented in parentheses. Bootstrap p-value refers to the coefficient on New LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

While the point estimates vary as control variables and fixed effects are added, none of the estimates is larger than 3 percentage points in magnitude. Only one, in column 4, is statistically significant. This column shows the results of a model that includes district fixed effects and no other control variables. When controls for the lagged vote share are added, as in columns 5 and 6, the estimate is no longer statistically significant.

Table 4 shows the results of models that use the distance from the precinct to the nearest new LIHTC project as the treatment variable. As in Chicago, I find no statistically significant effects. In my preferred specification, in column 6, I estimate that a one-mile increase in distance to the nearest new affordable housing project is associated with a 0.01 percentage point increase in first place vote share for incumbents.

The results are generally similar when using the Borda share outcome variable. Table 5 shows that incumbents receive slightly lower Borda shares in precincts with new LIHTC projects. The far-right column shows a decrease of about 1.1 points, on a scale that ranges

Table 4: NYC: Effect of Distance to New LIHTC Units on Incumbents' First-Place Share

	Share of First-Place Ranking, 2021					
	(1)	(2)	(3)	(4)	(5)	(6)
Dist. to Nearest LIHTC Project	2.79	8.61	6.46	-3.10	0.01	0.01
	(5.57)	(5.54)	(4.76)	(6.29)	(4.39)	(4.38)
Lagged Vote Share		0.53**	0.55**		0.45**	0.45**
		(0.10)	(0.10)		(0.12)	(0.12)
# Existing LIHTC Projects			-2.71*			-0.11
			(0.88)			(0.14)
District FE				✓	✓	✓
R^2	0.01	0.23	0.29	0.63	0.72	0.72
Observations	851	851	851	851	851	851
Bootstrap p	0.64	0.24	0.27	0.64	0.00	0.00

Notes: Outcome variable is the share of ballots within a precinct ranking the incumbent first in 2021. Standard errors clustered at the district level are presented in parentheses. Bootstrap p-value refers to the coefficient on New LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

from 0 to 100. This estimate is statistically significant, but substantively small. Across precincts, the standard deviation of Borda share is about 16.8 — meaning the estimated effect is roughly 6.8% of a standard deviation. When considering the continuous treatment measure, in Table 6, I obtain null results across all model specifications.

3.4 Summary of Electoral Evidence

In both Chicago and New York, I find relatively small and statistically insignificant effects of new affordable housing on incumbent vote shares. Even in the cross-section, there is relatively little evidence that places where new affordable housing is sited support incumbents at a lower rate, and more rigorous models turn up little evidence of electoral backlash. In sum, it appears that despite widespread mobilization against affordable housing in the planning and development stages, there are not many voters who punish their local representatives after the housing is actually built.

Table 5: NYC: Effect of New LIHTC Unit in Precinct on Incumbents' Borda Share

	Share of Total Borda Count in Precinct, 2021					
	(1)	(2)	(3)	(4)	(5)	(6)
New LIHTC Project	-1.09	-3.04	0.54	-1.78	-1.14*	-1.14*
	(5.37)	(4.90)	(4.54)	(0.94)	(0.39)	(0.34)
Lagged Vote Share		0.25+	0.30*		0.25*	0.25*
		(0.12)	(0.12)		(0.07)	(0.07)
# Existing LIHTC Projects			-2.82*			0.00
			(1.14)			(0.12)
District FE				✓	✓	✓
R^2	0.00	0.08	0.18	0.82	0.86	0.86
Observations	851	851	851	851	851	851
Bootstrap p	0.83	0.58	0.93	0.07	0.09	0.08

Notes: Outcome variable is the share of total Borda count obtained by the incumbent within a precinct in 2021. See text for details. Standard errors clustered at the district level are presented in parentheses. Bootstrap p-value refers to the coefficient on New LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

Table 6: NYC: Effect of Distance to New LIHTC Units on Incumbents' Borda Share

	Share of First-Place Ranking, 2021					
	(1)	(2)	(3)	(4)	(5)	(6)
Dist. to Nearest LIHTC Project	2.79	8.61	6.46	-3.10	0.01	0.01
	(5.57)	(5.54)	(4.76)	(6.29)	(4.39)	(4.38)
Lagged Vote Share		0.53**	0.55**		0.45**	0.45**
		(0.10)	(0.10)		(0.12)	(0.12)
# Existing LIHTC Projects			-2.71*			-0.11
			(0.88)			(0.14)
District FE				✓	✓	✓
R^2	0.01	0.23	0.29	0.63	0.72	0.72
Observations	851	851	851	851	851	851
Bootstrap p	0.64	0.24	0.27	0.64	0.00	0.00

Notes: Outcome variable is the share of total Borda count obtained by the incumbent within a precinct in 2021. See text for details. Standard errors clustered at the district level are presented in parentheses. Bootstrap p-value refers to the coefficient on New LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

4 Survey Evidence

The evidence from city council elections in Chicago and New York suggests there is not much electoral backlash against new affordable housing. However, it is possible that these results are not representative of the effect more generally for at least three reasons. First, it could be that LIHTC projects in these large cities do not have the same effect on local housing markets and the built environment as the typical LIHTC project. Second, even if local residents notice and react to the new affordable housing development in their area, it could be that other concerns are more important for their vote choice on the whole — muting the effect of the new developments on incumbents' vote share. Third, different residents may react in different directions. For example, there could be offsetting effects among homeowners and renters, with homeowners punishing and renters rewarding incumbent politicians for new LIHTC construction.

To address these concerns, I turn to data from the Congressional Cooperative Election Study (CCES). The CCES is a large, nationally representative survey fielded every two years, with large sample sizes. In 2016 and 2018, the survey asked respondents to give grades, on an A to F scale, to various actors and policies related to their local governments. I focus on three questions related to land use and local officials: (1) zoning and development; (2) the city or town council; and (3) the mayor or town manager. In 2016, respondents were asked about all three; in 2018, they were only asked about zoning and development.

The CCES includes respondents' zip codes, which enables me to match the LIHTC database to the CCES data. It also includes a wide range of covariates, which enable me to example subsamples of interest. Based on prior research on the importance of home-ownership in political behavior surrounding housing development, I focus on the potentially contrasting responses between homeowners and renters.

4.1 Sample Restrictions and Analysis Strategy

If citizens tend to react negatively to new LIHTC construction, we should expect that grades they give to local zoning policy, their city council, and their mayors would be lower if they are exposed to new LIHTC construction in their area, all else equal. However, not all places are equally likely to get LIHTC developments, and people in places that are more or less likely to have new LIHTC construction may have systematically different views on local government. Thus, naive comparisons of people in different areas will not identify the effect of LIHTC construction on citizens' views.

To mitigate this problem, I restrict my sample substantially to compare "early" and "late" adopters. Specifically, I consider a zip code to be "treated" in year t if any new LIHTC units were placed in service during year t or year $t-1$. To construct a comparable "control" group, I select zip codes which *did not* see a new LIHTC unit placed in service in year t or $t-1$, but *did* see a new LIHTC unit open in years $t+1$ or $t+2$.[4] For example, I compare residents in zip codes that saw new LIHTC units open in 2015 or 2016 to those in zip codes that saw new LIHTC units open in 2017 or 2018. This matching strategy enables me to exploit differential timing of new LIHTC opening, while comparing areas that are arguably equally likely to receive LIHTC units overall.

Given these sample restrictions, I take two analysis approaches: a difference-in-differences approach using the zoning outcome that is observed in both years, and regression adjustment on the matched dataset for the outcomes that are observed only in 2016.

For the difference-in-differences approach, I only select zip codes that had no new LIHTC projects open in 2015 or 2016 but had at least one new project open between 2017 and 2020. The treated zip codes are those that had a project placed in service in 2017 or 2018, and the control units are those that had a project open in 2019 or 2020 (but not 2017 or 2018).

[4] I choose a two-year window because local elections are likely to occur no more frequently than two years and because voters tend to be most responsive to recent events (Huber, Hill and Lenz, 2012).

I then estimate the following regression model:

$$Y_{itz} = \tau D_{tz} + \delta D_{tz} \times \text{Homeowner}_{it} + X'_{it}\beta + \alpha_t + \gamma_z + \epsilon_{itz}. \quad (1)$$

Here, Y_{itz} is the zoning grade respondent i who lives in zip code z gives in year t. D_{tz} is an indicator for whether zip code z was treated in year t, Homeowner_{it} is an indicator for whether the respondent is a homeowner, X_{it} is a vector of individual-level control variables. Finally, α_t and γ_z are time and zip code fixed effects. The coefficients of interest are τ and δ. This is a standard 2×2 difference-in-differences model with repeated cross-sections within each unit.[5]

Under a parallel trends assumption — namely, that the treatment group would have exhibited the same trend in zoning approval as the control group, had it not been treated — τ identifies the average treatment effect on the treated (ATT) among renters, and $\tau + \delta$ identifies the ATT among homeowners. This assumption is fundamentally untestable, and the fact that I observe only two time periods means I cannot conduct common tests for "pre-trends." However, the sample restrictions employed arguably make this trend relatively plausible: the zip codes I include in the sample differ primarily on the treatment timing. All control units become treated units shortly after the outcome is last measured.

The difference-in-differences approach requires outcome data for at least two time periods. The only outcome variable that satisfies this requirement is the grade that respondents give to their community on zoning and development. In order to examine the effect of new LIHTC construction on evaluation of politicians, survey questions asked only in 2016, I instead rely on regression adjustment. I restrict the sample in the way described above to compare zip codes that saw new LIHTC projects placed in service in 2015-2016 to those with new projects placed in service in 2017-2018.

[5]Recent advances in econometric theory show that two-way fixed effects regressions with staggered treatment adoption do not identify average treatment effects (e.g. Goodman-Bacon, 2021; Sun and Abraham, 2021). However, the source of bias in these settings come from incorrect aggregation of treatment effects across groups with different adoption timing. Because all units in my sample are treated at the same time, this critique does not apply.

Then, I estimate regressions of the outcome variable on an indicator for treatment status interacted with homeownership, plus individual-level control variables. I also include fixed effects for the total number of LIHTC projects that existed in the zip code prior as of 2014. The inclusion of these fixed effects accounts for the possibility that pre-existing differences in the affordable housing stock would generate differences between treated and control units. This regression identifies the ATT under the assumption that the timing of new LIHTC projects being placed in service is independent of the potential outcomes, conditional on the control variables and fixed effects. Again, this assumption is fundamentally untestable, but the sample restrictions and rich covariates make it more plausible. Finally, to account for the non-independence across observations within in the same zip code, I cluster standard errors by zip code.

4.2 Survey Results

The results of analysis of the CCES data are presented in Table 7. The outcome for the first three models is respondents' grades given on zoning; the outcome for the fourth column is respondents' grades given on their city councils, and the for the last column is grades given on their mayors.

Across all outcome variables and specifications, I estimate the effect of new LIHTC construction to be close to 0. The first column reports the estimates from the difference-in-differences approach, arguably the more rigorous of the two inferences approaches. I estimate that a new LIHTC project opening in a respondents' zip code increases renters' evaluation of their community's zoning and development policies by 0.03 points on the 1-5 scale. Among homeowners, it depresses evaluations by about the same amount. Neither of these estimates is statistically significant, nor substantively large: the standard deviation of the outcome variable is roughly 0.9. Columns 2 and 3 present the other model specifications for the zoning outcome, which analyze each year individually. They produce similar results that are statistically and substantively insignificant. In sum, there is little evidence that new

affordable housing projects opening affects citizens' views of zoning in their communities.

Perhaps respondents do not update their views of specific policy domains — especially a relatively obscure one such as obscure as zoning — but still assign some blame to local government for unwelcome changes in their areas. Columns 4 and 5 provide little support for this possibility. If anything, people in areas with new LIHTC projects rate their city councils and mayors slightly higher — both homeowners and renters. However, these estimate too are statistically and substantively insignificant. Across all the models in Table 7, none of the 95% confidence intervals for renters exceed a standardized effect size larger than 0.21 nor less than -0.15, and none of the confidence intervals for homeowners includes standardized effect sizes larger than 0.2 or less than -0.17.

Overall, these results fit neatly with the analysis of electoral data. I generally find little evidence that citizens' views about local government are responsive to the opening of new affordable housing in their areas. This finding applies both to evaluation of local elected officials as well as respondents' views on local land use policy, and the result holds among both renters and homeowners.

5 Discussion and Conclusion

There is a housing affordability crisis in the United States that especially impacts low-income families. Many of the nation's most economically productive regions have a shortage of housing, in large part due to local opposition to new housing development. Proposals for new housing often generate opposition among existing residents. Local politicians often have an important role in determining the outcome of new housing proposals — particularly affordable and multifamily projects that often require zoning variances.

In this paper, I investigate whether voters punish their local elected officials for new affordable housing being sited near them. The logic of retrospective voting, paired with theories of spatial politics, suggest that voters who are close to new affordable housing should give less support to incumbent politicians. I test this prediction using data from Chicago

and New York City. I find minimal evidence to support this hypothesis. Vote shares for incumbent city council members are no lower in precincts located close to new affordable housing projects than in precincts farther away. This pattern of results is consistent across the two cities I study.

A supplemental analysis of survey data covers a wider geographic area and enables me to investigate heterogeneous effects by homeownership status. Mirror the election outcome results, I find no evidence that new affordable housing projects depress citizens' evaluations of their local elected officials, nor harm their view of local zoning policy. These null results appear among both those who own their home and those who rent. In sum, I find no evidence of voters punishing local elected officials for new affordable housing located near them.

These findings are at odds with a growing literature documenting local opposition to new housing. What explains this disconnect? There are several possibilities.

First, voters may be insufficiently attuned to local-level policymaking for them to hold city council members accountable for new housing development. In the local politics literature, there is mixed evidence for retrospective voting, even when officials oversee narrower policy domains than city councilors (Payson, 2017; Berry and Howell, 2007). For example, Payson (2017) finds no evidence of retrospective voting in school board elections during off-cycle years — consistent with my findings for the 2019 and 2021 elections in Chicago and New York, respectively.

Second, it is possible that only a relatively small number of voters mobilize in response to new housing. Public comment at planning and zoning meetings is most common among an unrepresentative set of residents who skew older, wealthier, and Whiter than their broader communities (Einstein, Palmer and Glick, 2019; Yoder, 2019). While these voters may take costly action in order to express their preferences, their numbers may be small enough that they have little influence over elections.

Third, existing residents may find that their fears about new affordable housing ultimately do not come to pass. Residents may complain about the potential for new housing to generate

increased traffic, noise, crime, overcrowding in schools, and other nuisances. However, typical affordable housing projects tend not to have the deleterious effects that residents may fear (Freedman and Owens, 2011; Deng, 2007; Di and Murdoch, 2013). Once a new affordable housing project is built, residents may accept it as the new normal in their neighborhood — giving them little reason to punish their elected officials at the ballot box.

A final possibility is that elected officials are highly adept at blocking new affordable housing that would generate backlash. My research design relies on comparing geographic units before and after new affordable housing is placed in service. If city council members manage to quash extremely unpopular proposals, then I should not observe backlash to the projects that actually end up being built. This is an inherent limitation of all difference-in-differences style designs: I estimate the treatment effect on the treated areas. The research design cannot estimate the treatment effect among areas that are never treated. This feature of the research design limits how much one can extrapolate from these results. However, the placement of LIHTC projects is affected not just by local political considerations, but also features of the policy design (Baum-Snow and Marion, 2009). To the extent that non-political factors affect the siting decisions of affordable housing, the results here shed some light on the political effects of such housing.

Overall, the implication of my findings is that affordable housing tends not to generate a strong backlash against elected officials. Highly engaged residents may mobilize against proposed affordable housing *ex ante*. But electorates as a whole tend not to punish local politicians who allow affordable housing to be built *ex post*. This finding further suggests the importance of participatory institutions in translating anti-housing sentiment into policy that stymies new housing. Local politicians — at least in the large cities like the ones I study — have little reason to fear electoral backlash against affordable housing.

Table 7: Effect of New LIHTC Project on Citizens' Views of Local Government

	Zoning			City Council	Mayor
	(1)	(2)	(3)	(4)	(5)
New LIHTC Project	0.03	0.05	0.01	0.08	0.09
	(0.06)	(0.04)	(0.07)	(0.05)	(0.06)
New LIHTC Project × Homeowner	-0.06	-0.02	0.04	-0.04	-0.01
	(0.05)	(0.05)	(0.09)	(0.06)	(0.07)
R^2	0.25	0.05	0.07	0.06	0.05
Observations	9,605	9,591	4,736	9,018	9,024
Homeowner p	0.58	0.63	0.42	0.50	0.20
Outcome SD	0.90	0.90	0.91	0.93	1.01
Zip Code FE	✓				
Year FE	✓				
Prior LIHTC Projects FE		✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓
Model	Diff-in-Diff	2016	2018	2016	2016

Notes: Outcome variables are the grades respondents give to their local community on zoning issues (models 1-3), their city council (model 4), and their mayor (model 5), measured on a scale of 1 (grade F) to 5 (grade A). Model 1 is a difference-in-differences model that uses data from both 2016 and 2018. Models 2, 4, and 5 use only data from 2016; model 3 uses data only from 2018. Individuals are considered treated if a new LIHTC project opened within their zip code in the prior two years. Samples are restricted to zip codes where a new LIHTC project opened in either the two years prior to or subsequent to the survey year. Models include fixed effects for zip code (Model 1) or for the total number of LIHTC projects three years before the survey (Models 2-5). Models also include individual-level controls for age (squared), race, sex, education, family income, homeownership status, and party identification. "Homeowner p" refers to the p-value on the effect for homeowners (i.e., the sum of the two coefficients). Regressions include survey weights and standard errors are clustered at the zip code level. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

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A Difference-in-Differences Specification

Tables 8 and 9 report the results from difference-in-differences models for Chicago and New York, respectively. The outcome variable is the precinct-level vote share for the incumbent in the latter election minus their vote share in the prior election. The first two columns present cross-sectional relationships, while the latter two columns include district fixed effects to exploit within-district variation.

In Chicago, politicians see a larger decline in vote share in precincts with new LIHTC units placed in service. However, this association is driven entirely by cross-ward variation. When comparing precincts in the same ward through the inclusion of ward fixed effects, there is no association between new affordable housing and changes in vote shares. In New York City, there is neither a cross-sectional nor within-district relationship between changes in vote share and new affordable housing.

Table 8: Chicago DiD: Effect of New LIHTC Unit on Change in Incumbent Vote Share

	Δ Vote Share, 2015-2019			
	(1)	(2)	(3)	(4)
New LIHTC Project	-18.63**	-15.82**	3.40	3.24
	(5.78)	(4.80)	(3.35)	(3.17)
# Existing LIHTC Projects		-1.72		0.13
		(1.59)		(0.42)
Ward FE			✓	✓
R^2	0.00	0.00	0.89	0.89
Observations	1,803	1,803	1,803	1,803
Bootstrap p	0.04	0.02	0.62	0.67

Notes: Difference-in-differences models for Chicago. Outcome variable is the change in precinct-level vote share for the incumbent from 2015 to 2019. Standard errors clustered at the ward level are presented in parentheses. Bootstrap p-value refers to the coefficient on New LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$

Table 9: NYC DiD: Effect of New LIHTC Unit on Change in Incumbents' First-Place Share

	Δ First-Place Ranking, 2017-2021			
	(1)	(2)	(3)	(4)
New LIHTC Project	-6.64	-1.06	-0.47	-0.35
	(5.52)	(3.21)	(2.92)	(3.06)
# Existing LIHTC Projects		-3.96**		-0.17
		(0.84)		(0.34)
District FE			✓	✓
R^2	0.00	0.13	0.62	0.62
Observations	851	851	851	851
Bootstrap p	0.25	0.76	0.77	0.81

Notes: Difference-in-differences models for New York City. Outcome variable is the first place share in 2021 minus the vote share in 2017. Standard errors clustered at the district level are presented in parentheses. Bootstrap p-value refers to the coefficient on New LIHTC Project and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$