The Elected Official Next Door*

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Abstract

This paper examines whether the election of a city council member generates highly localized benefits within their own neighborhoods. We use housing prices as a summary statistic to capture the numerous and difficult to observe ways in which local government allocates localized amenities. Drawing on data on North Carolina city council elections and the universe of housing transactions, we use a close-elections regression discontinuity strategy. We find that housing prices substantially increase for houses very close (within 0.2 miles) to a newly elected councilmember's place of residence, especially when the councilmember is white, male, or Republican.

*Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group.
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1 Introduction

This paper explores whether the election of a city council member generates highly localized benefits within their own neighborhoods. In doing so, we contribute to a line of inquiry on the question: who benefits from the election of one candidate for elected office over another? Political economy offers a variety of theoretical models of candidate behavior with distinct answers to this question. If officials merely serve as trustees of the policy preferences of the median voter, then where benefits are directed by officials should be independent of which particular individuals are elected (Downs, 1957). But a substantial empirical literature pushes against a strict interpretation of that model; policy outcomes and the beneficiaries thereof do, in many cases, vary with candidate characteristics, such as partisan affiliation, race, and gender (e.g. Beach, Jones, Twinam, & Walsh, in press; de Benedictis-Kessner & Warshaw, 2016; Ferrara & Gyourko, 2014; Jones & Walsh, 2018). Much of policy, though, – especially at a local level – has a spatial element: the placement of parks, which roads to prioritize for repair, zoning ordinances, etc. We therefore consider whether and how there are localized changes in outcomes linked to the residential location of an elected councilmember. In other words, are there returns to having a city councilmember next door?

Directly measuring within-city neighborhood-level allocations of spending or changes in regulations is challenging. Thus, to answer our question, we take housing prices as our outcome. In light of a large literature documenting that the local public good, tax, and regulatory environment is capitalized into housing prices, we use prices as a summary statistic to capture the numerous - often difficult to observe - ways in which local government may allocate local amenities or preferential regulatory treatment. Housing prices are also sufficiently granular such that we can examine not only whether housing prices in immediate proximity of a councilmember's residence change, but also to what distance do such changes extend.

We draw on data from North Carolina city council elections and a dataset reporting the universe of housing transactions. To provide causal inference, our main specifications use a close-elections regression discontinuity strategy. Specifically, we focus on narrowly-won city council contests and compare *changes in* housing prices near the narrow-winner's house to *changes in* housing prices near the narrow-loser's house, before vs. after the relevant election. We ultimately find that housing prices *do* substantially increase for houses very close (within 0.2 miles) to a newly elected councilmember's place of residence. Our estimates suggest an increase in housing prices within 0.2 miles of between 7% and 10%.

As with politicians who hold higher levels of office, city council members are largely unrepresentative of their electorate. They are whiter, wealthier, and more likely to be homeowners than the population at large (Einstein, Ornstein, & Palmer, 2022; Schaffner, Rhodes, & La Raja, 2020). If elected officials neglect the policy demands of underrepresented groups, this lack of representativeness has the potential to generate and exacerbate within-city inequality between advantaged and disadvantaged individuals (Beach et al., in press). Given the highly segregated nature of cities in the United States, such inequities could also emerge if council membership can lead to the generation of highly localized benefits (i.e., benefits to a given councilmember's own neighborhood), which we find to be the case.

Our work contributes to a broader discourse on politics and the spatial allocation of public goods and services. A large literature examines so-called pork-barrel spending, wherein - e.g. - members of

Congress attract Federal funds to their home districts presumably to enhance future political prospects (Evans, 2011). A smaller set of papers document that officials above the local level in Italy, Norway, and Germany show some favoritism towards their own place of birth or residence in allocation of spending, even in the absence of re-election incentives (Baskaran & da Fonseca, 2021; Carozzi & Repetto, 2016; Fiva & Halse, 2016). Two recent working papers are closer to our work. Drawing on data from Sweden, Folke, Martén, Rickne, and Dahlberg (2021) find that areas within cities with a larger number of leading-party local elected officials (elected through a flexible party-list proportional representation system) are less likely to be exposed to public bads. Relative to their paper, we adopt a different identification strategy, facilitated via the winner-takes-all elections present in the US context and moreover document that effects occur down to the individual member-level, independent any connection to a broader ruling party. In a concurrent working paper, Billings, Macartney, Park, and Singleton (2022) find that the election of a local school board member is associated with an increase in home prices in proximity to the member's house.

2 Data

We draw on data on city council election outcomes from the entire state of North Carolina for 2008-2019. The data are drawn directly from the North Carolina State Board of Elections (NCSBE), as well as from the Local Elections in America Project, which compiled and processed raw data from the NCSBE. The data report candidates' names, the office for which they ran (e.g., Durham City Council), and candidates' vote shares. We use the vote shares to construct margins of victory for the two marginal candidates – a critical input into our regression discontinuity specification, described in more detail in the next section.

Critical to our analysis is identifying candidates' home addresses. These are not reported in elections outcomes data. Instead, we match candidates based on name and city (from the elections data) to North Carolina Voter Registration Files, also obtained from the North Carolina State Board of Elections. We were able to successfully match roughly 65 percent of candidates to voter files to obtain their addresses.

Our sample is ultimately restricted to pairs of marginal candidates (last-place winners and first-place losers) in contests where we observe residential addresses of *both* relevant candidates from 2008-2019. This results in 685 unique contests; 366 of these contests were decided by a margin of 10 percentage points or less.

Our outcome variable - housing prices - is drawn from Zillow's ZTRAX database (Zillow, 2022), which reports the universe of housing transactions. We observe the price of the house at the time of transaction as well as additional property characteristics including: location (including latitude and longitude), building age, lot size, and the number of bedrooms and bathrooms. Our analysis is limited to residential properties that have at least one bedroom and one bathroom, and were constructed after 1800. We further exclude property transactions with prices in the bottom and top one percentiles of the distribution. We combine data on candidates' residential locations with the Zillow data to identify all housing transactions that occur within one mile of a marginal candidate's address, with "treatment" locations within a mile of the winner's a address and "control" locations within a mile of the loser's address.

Table 1 reports summary statistics for the homes and candidates in our sample. Panel A shows that the pre-election housing prices and characteristics located within 1 mile of the candidates' homes are similar for both winning and losing candidates. Panel B shows the same variables, but for houses within 0.2 miles of candidates' houses, a distance which is ultimately focal for our analysis. Panel C reports the characteristics of candidates in all the council member elections between 2008 and 2019. The table shows that winning and losing candidate do differ along some dimensions; we will show soon though that they do not differ in the narrow elections that we use for identification.

3 Empirical Strategy

Our main empirical approach combines close-election regression discontinuity and difference-in-differences. We compare how logged housing prices evolve in close proximity to a winning council candidate's residential address relative to a losing council candidate's address, before vs. after the relevant election between the two candidates.

To fix ideas, consider the following estimating equation, which simply represents a difference-in-differences approach, without the regression discontinuity element added. The "treated" units are houses located proximate to the winner's house and the "control" units are houses located near the loser's house. In our estimates, we consider all housing transactions that occur in the four years leading up to an election as the pre-period (including the election year) and in the four years after as the post-period.

$$\ln(Price_{ict}) = \beta_1 Post_t \times Win_c + \gamma \mathbf{X_i} + \tau_t + \rho_c + \epsilon_{ict} \tag{1}$$

The outcome variable, $\ln(Price_{ict})$, is the sales price in period t for house i located in close proximity to candidate c's home. This specification includes time fixed effects (τ_t) and fixed effects capturing the area near each candidates' home (ρ_c), which - in the event of multiple elections in the same city during our sample period - are unique to each election. The area fixed effects therefore subsume election fixed effects and city fixed effects. The coefficient on $Post_t \times Win_c$, β_1, measures the relative change in housing prices that occurs near the winning candidate's house, after the relevant election has occurred.[1]

A potential concern is that this specification may violate the parallel trends requirement for consistency in difference in differences models. In particular, when considering elections that were won by large margins, it may well be the case that winning candidates come from neighborhoods that are on systematically different price trajectories than are those of losing candidates. We therefore incorporate a regression discontinuity dimension to our estimating strategy. One approach, which remains relatively agnostic with regards to appropriate functional form for the running variable, is to simply estimate the above equation only for a set of elections with a relatively close margin of victory, e.g., elections where the winner wins (and loser loses) by 10 percentage points or less. We adopt this approach in panel B of Table 2.

Our main specifications, though, fully incorporate a regression discontinuity approach into the DiD framework above. Specifically, we fully interact "Post" and "Treat" in the specification above with each candidates' margin of victory. *Margin of victory* is measured as the difference between the marginal winner's and the marginal loser's voteshares. A margin of zero implies a tie; positive margins indicate that the candidate in question won.

[1] Note that indicator variables for $Post_t$ and Win_c are subsumed in the time τ_t and area ρ_c fixed effects respectively.

Specifically, within a narrow bandwidth of margin of victory, we estimate:

$$\ln(Price_{ict}) = \beta_1 Post_t \times Win_c + \beta_2 Post_t \times Win_c \times Margin_c + \beta_3 Post_t \times Margin_c + \gamma \mathbf{X_i} + \tau_t + \rho_c + \epsilon_{ict} \tag{2}$$

Note, as with Equation 1, some elements of the full interaction between Post, Win, and Margin are absorbed into fixed effects. In this more robust specification, β_1 remains the coefficient of primary interest. Given interactions elsewhere in the specification, it identifies the change in housing prices that occur near the winning candidate's home relative to the loser's home, after the relevant election has occurred. Inclusion of interactions wit the running variable ($Margin_c$) leads β_1 to provide an estimate of the winners impact on proximate housing values for elections where the margin of victory was zero (ε different from zero).[2] Finally, some number of the elections we study aim to fill multiple seats with one election. For instance, an at-large election may fill 3 seats. It is for this reason that we have thus far specified that we compare outcomes between *marginal* winners and *marginal* losers. In the case of an election filling three seats, where the top three vote-getters are elected, our comparison is between the neighborhood of the third-place (last winner) and fourth-place (first loser) candidates.

Next we clarify two additional aspects of our sample and approach: (1) what it means for a house to be "near" a candidate and (2) how the bandwidth in margin of victory is determined.

3.1 Distance

Ex ante, we are agnostic about the appropriate distance over which to evaluate candidate neighborhood effects. Thus, we begin with a broad notion of neighborhood, all houses within one mile of a candidates home. Then, in our first set of analyses, we explicitly test across a range of distances to determine the potential spatial extent of effect. Specifically, we expand the Win_i dummy from the equation 2 into a vector of mutually exclusive indicator variables capturing distance from a candidate's address: 0-0.2 miles, 0.2-0.4 miles, 0.4-0.6 miles, 0.6-0.8 miles, and 0.8-1 mile. We then estimate a model that fully interacts each of these indicator variables with "Post" and "Margin". Through this approach we find that there are clear effects on property values within 0.2 miles of the winning candidates house, but not further out than that. As such, the majority of our analyses defines "near" as <0.2 miles and restricts the sample to housing transactions occuring within that distance of the winning and/or losing candidates homes.

3.2 RD Bandwidth

Several authors have proposed methods to identify the optimal bandwidth in a local linear RD approach (e.g., Calonico, Cattaneo, and Titiunik (2014), Imbens and Kalyanaraman (2012)). These methods balance the benefits of a narrower bandwidth (estimates drawn from observations that are close to the cutoff, increasing confidence in identifying a casual effect) with the benefits of a wider bandwidth (more observations, increasing power). These methods would be well-suited to identifying a bandwidth if one outcome was associated with each election. However, our setting involves a large number of housing transactions,

[2] A similar approach is used in several existing papers (e.g. Beach et al., in press; Cellini, Ferreira, & Rothstein, 2010; Fischer, 2021).

occurring in various neighborhoods within a city, and measured before and after "treatment". Using typical bandwidth selection procedures on our full sample would yield an artificially small bandwidth, as there are many observations close to the cutoff, but many of them belong to the same election. To adapt this approach to our setting, we therefore collapse our observations to the election-by-candidate level. For each election, we take the average of ln(Price) in the four years following the election for all properties within X miles of a candidate's house (where X is 0.2, 0.4, 0.6, 0.8, or 1, acknowledging that we do not know how far the effect will extend ex ante and mirrioring the distinct distances we will use in our analysis). For each distance, this yields a single observation per election. We then use the Calonico et al. (2014) bandwidth selection procedure on each of the different distances and take the average of the resulting bandwidths, which suggests that the optimal bandwidth in our setting is roughly 12.5 percentage points.[3] Thus, we anchor on 12.5 percent as our bandwidth bandwidth. However, given the range of bandwidths that are possible in our setting, we show robustness to bandwidths both above and below this ideal in our analysis.[4]

3.3 Regression Discontinuity Validity

Appendix Figure A.1 depicts a McCrary Density plot (McCrary, 2008), documenting that the density of observations is balanced around the cutoff, a necessary assumption for a regression discontinuity design.

Appendix Table A.1 reports balance tests for housing and candidate characteristics. For any regression discontinuity, it is important that other observables do not systematically vary with treatment. Our housing characteristic balance tests use a panel RD model like our main specification, documenting no significant change in characteristics of transacted houses in winners' neighborhoods post-victory. Our candidate characteristic balance tests uses a simpler cross-sectional RD model[5], documenting that narrow-winning candidates in our sample are not systematically different than narrow-losers with regards to race, gender, or partisan affiliation.

We also conduct an additional test aimed at a similar idea as the balance tests, but more directly addressing whether the types of houses that sell change after a relevant election. Namely, we calculate "predicted prices" based only on housing characteristics (sq. footage, age, bedrooms, bathrooms) and city fixed effects.[6] We then run our main panel RD and DiD specifications, but taking "predicted price" (instead of actual price) as the outcome. The predicted price is a summary statistic of housing characteristic. If the composition of houses (with respect to size, number of bedrooms, etc.) being sold change as a result of the election, then this would be captured in the predicted price. (Of course, we do note that we control for these characteristics in our main specification as well.) Results are presented in Appendix Table A.3. Across all presented specifications, drawing on various bandwidths, there is no relationship between

[3] Resulting optimal bandwdiths range from 10.9-16.1 percentage points.

[4] An alternative method of selecting a single bandwidth might have been to simply use all observations within a distance of 1 mile of a candidate's house - the maximum distance in our analysis - and find the bandwidth for that sample. Doing so yields a very similar optimal bandwidth: 12.9.

[5] Recall that our main panel model includes a fixed effect for the circle around a candidate's house, which, in practice is essentially a candidate fixed effect. Thus, we must adopt a simpler approach to test for balance on candidate characteristics.

[6] We run a regression of (logged) housing prices on these characteristics, restricted to a sample of transactions in years prior to a treatment election. We then calculate predicted prices based on those variables and the coefficients resulting from the regression just described.

proximity to a winning candidate and the predicted price of transacted houses. This further validates our regression discontinuity approach.

4 Results

Figure 1 depicts results from the specification described in the previous section, allowing for different effects of a nearby winning candidate at different distances. Specifically, the specification restricts to: housing transactions within one mile of both winning and losing candidates; housing transactions in the four years before and four years after the election; and elections decided by a margin of less than 12 percentage points. It then interacts "Post", "Win", and "Margin" variables (and all interactions) with a series of dummies capturing distance from the candidate's residential address – listed along the x-axis of the figure. The plotted coefficients are those on $Post \times Win \times [distance]$ terms. For example, the coefficient depicted above "0-0.2 miles" in the figure captures the difference in the change in housing prices after (relative to before) an election within 0-0.2 miles of the winner's residence (relative to the loser's) – estimated specifically for very narrowly determined elections.

Most notably, as already previewed, there is a large positive effect on housing prices within 0-0.2 miles of the winner's residence (relative to within 0-0.2 miles of the loser's), but no effect at distances beyond that. Thus, the neighborhood returns to sitting on the city council are highly localized.[7]

Given the large pronounced effects within 0.2 miles, we adopt properties within 0-0.2 miles from the candidates' residences as our main sample for the remainder of the paper. Panel A of Table 2 reports results of RD-DiD estimates, restricted to within 0.2 miles, but testing for robustness across different bandwidths. By restricting to within 0.2 miles, these estimates return to the estimating equation 2. We report the coefficients on "Post"x"Win". Results are generally similar across different bandwidths and the estimate is consistently statistically significant as well. Our preferred estimate in Panel A is Column 4 (using a bandwidth closest to the optimal), which suggests a 10.2 percent increase in housing prices near a winning candidate's house.

Our main estimates are a hybrid of regression discontinuity and difference-in-differences. For the sake of robustness, Panel B estimates a simpler difference-in-differences model, dropping the "Margin" term and any interactions with it; but in doing so, we still estimate within narrow bandwidths. This provides some of the identification advantage of RD: focusing on relatively close elections prevents naive comparisons between all neighborhoods with winners vs. all neighborhoods with losers. However, regression discontinuity relies on fitting lines to either side of the cutoff, which imposes a particular functional form on the data. In this regard, the simpler DiD (within a narrow bandwidth) is more flexible, simply taking averages on either side of the cutoff. However, we ultimately observe similar estimates under this alternative approach. We performed the same bandwidth selection procedure as outlined in a previous section for this type of specification, yielding a preferred bandwidth of roughly 0.075. The estimate for that bandwdith appears in Column 2. There, we observe a 7% increase in housing prices. Equally notably, we find that our results are generally robust to this approach across the same array of bandwidths, albeit

[7] In the appendix, we graphically depict the results of a cross-sectional regression discontinuity model (Appendix Figure A.2). It reveals a similar result. Likewise, we report a cross-sectional regression discontinuity, using years *prior* to the relevant election and find no discontinuity at the cutoff (Appendix Figure A.3).

with some loss of precision in some estimates.

Figure 2 takes an event study approach to our data. We estimate a version of equation 2, replacing the "Post" indicator with a series of indicator variables indicating how many years before/after the election the housing transaction is.[8] Doing so allows us to document two findings. First, in comparing the estimate "2-3 years before" relative to the reference category of "0-1 years before", we observe no evidence of a differential trend in winner's neighborhoods in the years preceding an election. Second, comparing the post-election estimates, we find that our estimated effects grow over time. This may suggest that the positive effect of a councilmember on nearby housing prices may have less to do with forward-looking expectations and instead observable changes occurring near the member's residence.

Finally, we consider whether the effects we observe differ with candidate characteristics. To do so, we interact the relevant treatment variables (margin of victory, post, winner, and all relevant interactions) with some candidate characteristic (party, race, gender). Results are reported in Appendix Table A.2. In short, we find positive significant effects on nearby housing prices when the winner is Republican, white, or male, and insignificant smaller positive effects when the winner is a Democrat, not white, or female. However, we cannot reject that the differences in effects across categories (e.g., the effect of a Democrat vs. Republican winning, or the effect of a woman vs. a man winning) are different.

5 Conclusion

Seventy-five years ago, with his seminal work on the median voter, Downs (1957) launched a rich literature focused on how electorate preferences translate into the policy positions (votes) taken by elected officials. In the years since Downs, a more recent literature has arisen which considers the link between the preferences/attributes of elected officials themselves and the policies that they support. Empirical work in this tradition provides evidence on a variety of candidate characteristics, including among others: gender, political party, race, and ethnicity. Much of this work is motivated by a concern about how well the political system meets, or fails to meet, the needs of different disadvantaged groups within society. In this vein a central theme is the potential importance of descriptive representation, meaning representation by members of one's own group, in providing access to the fruits of government.

In our analysis we take a spatial lens to this literature. Rather than assess the value of electing members of one's own race, gender or party, we consider the value of city council members who are spatially close to the voter - i.e. are neighbors. Because it is almost impossible to systematically measure spatial variation in local policy at the fine spatial scales that we wish to study, we proxy for the localized impacts of such policies using housing prices. Working with data from North Carolina, we find that, on average, electing a council member who lives within two-tenths of a mile of one's home leads to added home value appreciation on the order of 6 to 19 percent.

One interpretation of these results is that politicians work against the public interest to direct policy so as to increase the value of their own homes. There are however potentially more benign mechanisms at play. Politicians may simply be more aware of the issues facing their immediate neighborhood than they

[8] The estimating equation maintains the interactions with margin of victory, and as such maintains the close-election regression discontinuity interpretation.

are for those neighborhoods in other parts of the city. As such, a politician's efforts to address those issues that she finds most salient could unintentionally yield differential improvements that are disproportionately relevant for her neighborhood. Along a similar vein, if political access is important in determining government policy, differential treatment could arise solely as a result of spatial proximity providing increased political access to those individuals living in a given council-member's neighborhood. Of course, even if "more benign" with respect to councilmember intentions, given the lack of representativeness of councilmembers in the United States, any of these mechanisms can exacerbate urban inequality.

Given the paucity of large systematic and highly-localized data on city policies, unpacking the specific motivations/processes at play here will likely involve a markedly different analytical approach to that taken in our current work. Regardless of mechanism, given that U.S. cities remain highly segregated along key dimensions such as race, our results provide evidence of a further channel through which descriptive representation may be important for providing equal access to the benefits of government.

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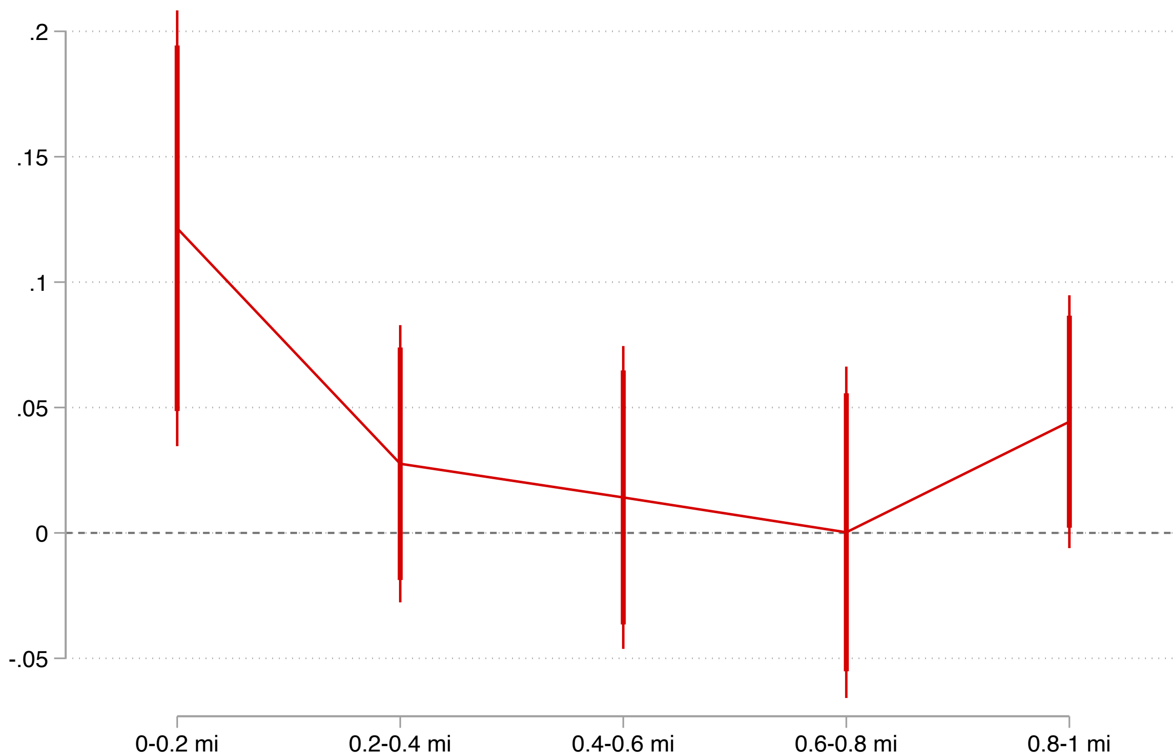
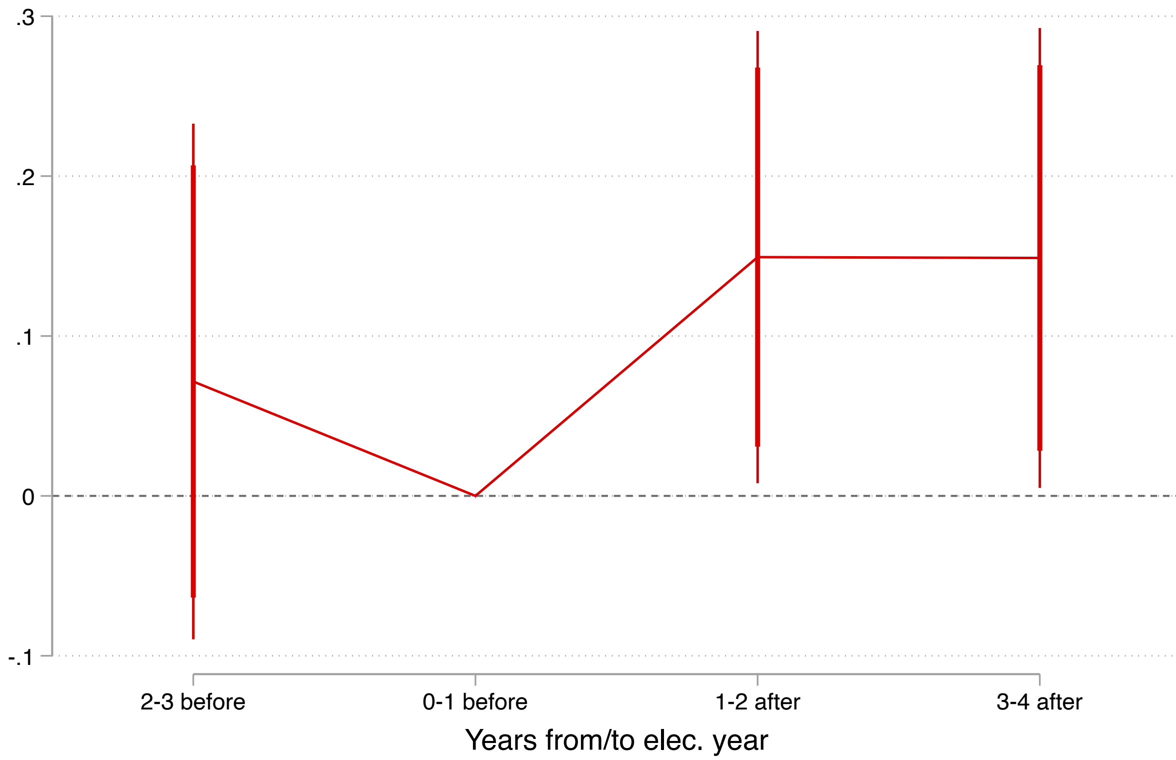
Figure 1: Effect of Council Candidate Election by Distance from Candidates' Residential Address



Notes: Sample restricted to narrow elections (decided by <10 percentage points). Sample includes housing transactions within 1 mile of a marginal city council candidate's residential address (including both the marginal winner and marginal loser) and within 4 years before and after elections. Figure plots coefficients from triple interactions between distance from council candidate residential address, post-election dummy, and council candidate victory dummy. X-axis reports effects at various distance ranges from candidates' address. Y-axis reports effects near winner's address relative to loser's address on logged prices. Thick bars emanating from estimates are 90% confidence intervals; thin bars are 95% confidence intervals.

Figure 2: RD-Event Study – Effects at Distinct Points in Time Relative to Election Years



Notes: Sample restricted to narrow elections (decided by <10 percentage points). Sample includes housing transactions within 1 mile of a marginal city council candidate's residential address (including both the marginal winner and marginal loser) and within 4 years before and after elections. Figure plots coefficients from triple interactions between distance from council candidate residential address, post-election dummy, and council candidate victory dummy. X-axis reports effects at different points in time relative to the election years. Y-axis reports effects near winner's address relative to loser's address on logged prices. Thick bars emanating from estimates are 90% confidence intervals; thin bars are 95% confidence intervals.

Table 1: Summary Statistics

	Winner	Loser
Panel A: Pre-election Housing prices and characteristics		
	Winner	Loser
Housing price	232702.4	225291
Area by Sq. Ft.	2527.42	2522.11
Age of House	27.76	28.32
Total Bedrooms	2.83	2.79
Panel B: Pre-election Housing prices and characteristics within 0.2 mile		
	Winner	Loser
Housing price	224140.2	215199.8
Area by Sq. Ft.	2353.07	2394.01
Age of House	32.28	35.70
Total Bedrooms	2.73	2.73
Panel C: Candidates characteristics %		
	Winner	Loser
Black share	26.59	26.45
White share	71.53	71.39
Others race/ethnicity	1.88	2.16
Democratican	56.75	47.67
Republican	28.01	34.59
Unaffiliated	15.09	17.59
Other party	0.15	0.15
Female/Male	51.06	40.73

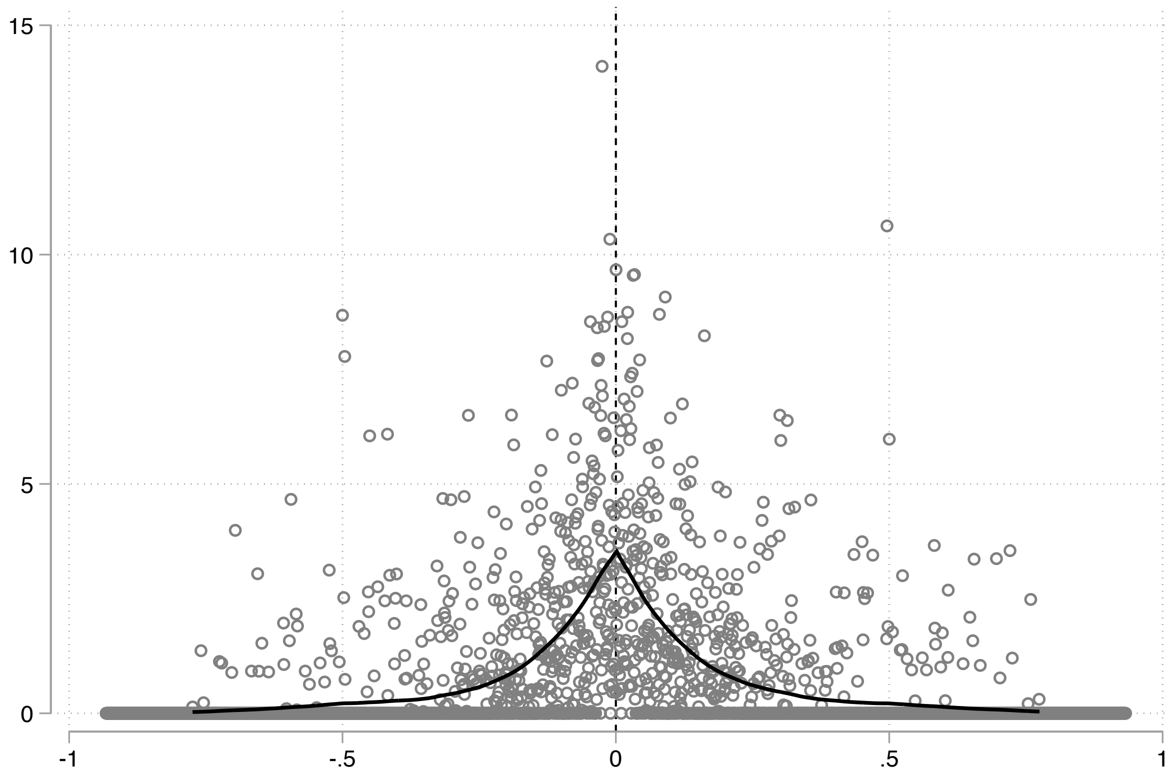
Table 2: RD-DiD and DiD Specifications Across Various Margin of Victory Bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
	BW=0.05	BW=0.075	BW=0.10	BW=0.125	BW=0.15	BW=0.175
Panel A: RD-DiD within 0.2 mile						
Post * Winner N'hood	0.145**	0.146**	0.154***	0.110**	0.083**	0.085**
	(0.059)	(0.058)	(0.050)	(0.043)	(0.041)	(0.038)
Observations	6965	8879	10430	11777	13177	14130
Panel B: DiD within 0.2 mile						
Post * Winner N'hood	0.086**	0.066*	0.039	0.043	0.049*	0.043
	(0.036)	(0.034)	(0.028)	(0.027)	(0.027)	(0.028)
Observations	6965	8879	10430	11777	13177	14130

Notes: Sample restricted to narrow elections (decided by less than amount indicated in column header). Sample includes housing transactions within 4 years before and after elections. Both panels are restricted to transactions within 0.2 miles of candidate's address. Table reports interactions between post-election dummy and council candidate victory dummy, which estimates causal effects within the 0.2 miles of winner's house relative to within 0.2 miles of loser's house. All specifications include election, candidate, year, bedroom, and bathroom fixed effects. Significance levels are indicated by $* < .1$, ** $< .05$, *** $< .01$.

A Appendix

Figure A.1: McCrary Density Plot



Notes: The McCrary Density test does not reject the null hypothesis that there is no gap in density at the cutoff.

Table A.1: Balance Tests: Housing and Candidate Characteristics

	(1) Housing: Age	(2) Housing: Area	(3) Housing: Bedrooms	(4) Housing: Bathrooms	(5) Cand.: White	(6) Cand.: Female	(7) Cand.: Repub.
Winner	-0.389	144.090	-0.002	0.049	-0.032	0.033	-0.044
	(1.043)	(123.570)	(0.058)	(0.040)	(0.044)	(0.051)	(0.057)
Observations	18891	18758	12583	16778	818	819	687

Notes: Sample restricted to narrow elections (decided by less than 10 pct. pts.). Cols. 1-4: Sample includes housing transactions within 4 years before and after elections. Models are panel RD-DiD, as in main analysis. All specifications include election, candidate, and year fixed effects, and also full interaction of post, margin, and winner indicator. Cols. 5-7: Analysis is at candidate level. Models are post-election cross-sectional RD. All specifications include year fixed effects and also full interaction of margin and winner indicator. Significance levels are indicated by $* < .1$, ** $< .05$, *** $< .01$.

Table A.2: Heterogeneity by Candidate Characteristics

VARIABLES	(1) lp	(2) lp	(3) lp
Post * Winner N'Hood * Repub.	0.206**		
	(0.095)		
Post * Winner N'Hood * Dem.	0.085		
	(0.064)		
Post * Winner N'Hood * White		0.158***	
		(0.048)	
Post * Winner N'Hood * Not White		0.011	
		(0.128)	
Post * Winner N'Hood * Male			0.149**
			(0.061)
Post * Winner N'Hood * Female			0.001
			(0.080)
Observations	9,864	11,668	11,668
R-squared	0.524	0.514	0.513

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample restricted to narrow elections (decided by less than 10 pct. pts.). Cols. 1-4: Sample includes housing transactions within 4 years before and after elections. Models are panel RD-DiD, as in main analysis. All specifications include election, candidate, and year fixed effects, and also full interaction of post, margin, and winner indicator. Cols. 5-7: Analysis is at candidate level. Models are post-election cross-sectional RD. All specifications include year fixed effects and also full interaction of margin and winner indicator. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Table A.3: RD-DiD and DiD Specifications Using Predicted Housing Prices

	(1)	(2)	(3)	(4)	(5)
	BW=0.05	BW=0.10	BW=0.15	BW=0.2	Full
Panel A: RD-DiD within 0.2 mile					
Post * Winner N'hood	0.009	0.020	-0.004	-0.012	-0.001
	(0.033)	(0.028)	(0.024)	(0.022)	(0.015)
Observations	7034	10585	13380	15349	21424
Panel B: DiD within 0.2 mile					
Post * Winner N'hood	-0.000	-0.012	-0.008	-0.003	-0.014
	(0.019)	(0.016)	(0.014)	(0.012)	(0.012)
Observations	7034	10585	13380	15349	21424

Notes: Sample restricted to narrow elections (decided by less than amount indicated in column header). Sample includes housing transactions within 4 years before and after elections. Both panels are restricted to transactions within 0.2 miles of candidate's address. Table reports interactions between post-election dummy and council candidate victory dummy, which estimates causal effects within the 0.2 miles of winner's house relative to within 0.2 miles of loser's house. All specifications include election, candidate, year, bedroom, and bathroom fixed effects. Significance levels are indicated by $* < .1$, ** $< .05$, *** $< .01$.

Figure A.2: Cross-Sectional RD Plot, <0.2 Miles from Candidates' Residence

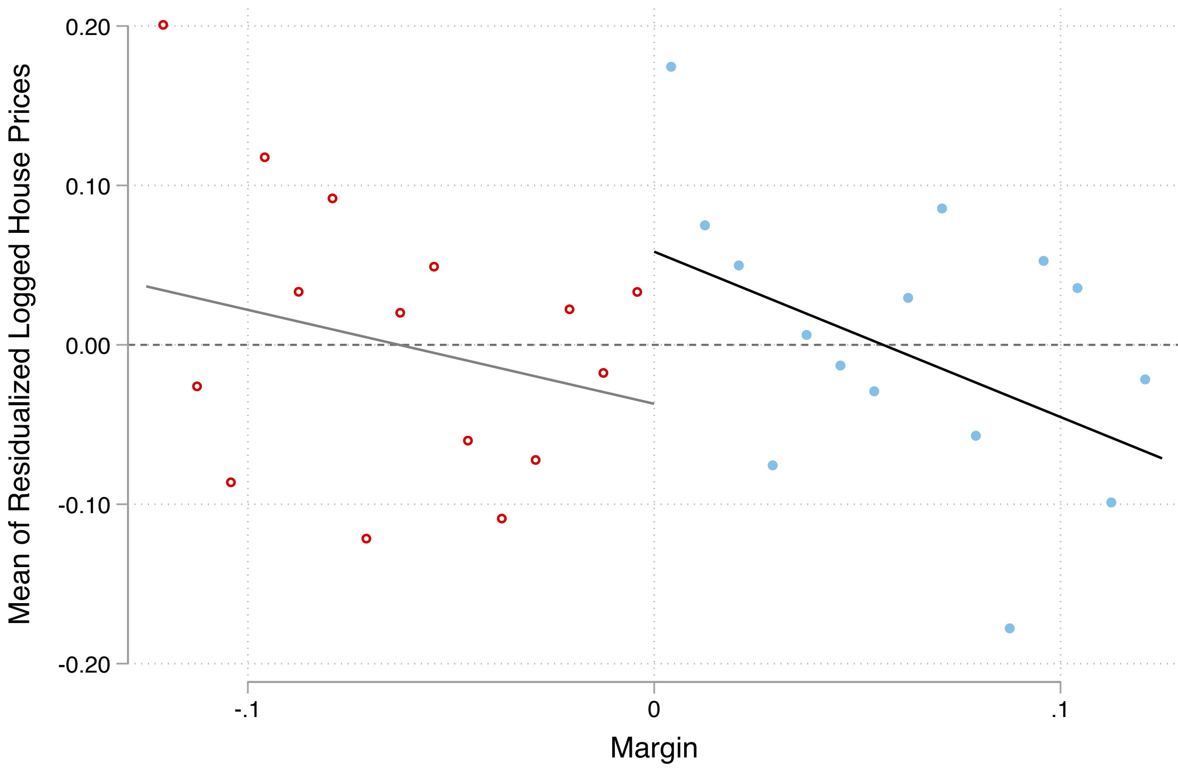


Figure A.3: Placebo Cross-Sectional RD Plot, <0.2 Miles from Candidates' Residence in Years Prior to Election