Self-Interest in Public Service: Evidence from School Board Elections*

Stephen B. Billings, University of Colorado – Boulder
Hugh Macartney, Duke University and NBER
Geunyong Park, University of Rochester
John D. Singleton, University of Rochester

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Abstract

In this paper, we show that the election of a new school board member causes home values in their neighborhood to rise. This increase is identified using narrowly-decided contests and is driven by non-Democratic members, whose neighborhoods appreciate about 4% on average relative to those of losing candidates. We find that student test scores in the neighborhood public schools of non-Democratic winners also relatively increase, but this effect is driven by changing student composition, including via the manipulation of attendance zones, rather than improvements in school quality (as measured by test score value-added). Notably, we detect no differential changes when comparing neighborhood or scholastic outcomes between winning and losing Democratic school board candidates. These results suggest that partisan affiliation is correlated with private motivations for seeking public office.

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1 Introduction

The principles of representative democracy require that elected leaders represent the will of the people and that this will is not thwarted by a leader's own agenda. This tenant of leadership is so important that most modern democracies have strong institutional controls that include oversight protections as well as processes to remove leaders who pursue goals not aligned with the public good. An extant literature focuses on the degree to which institutions and regulations, such as media independence and anti-corruption norms, effectively constrain self-aggrandizing behavior by public officials.[1] Understanding the private returns that politicians realize from office is important for designing effective policies, especially as concerns over the conflicts of interest of elected leaders persist, even in mature democracies.[2]

This paper studies candidates' returns to gaining elected office in a unique setting. Specifically, we consider whether school board members in the U.S. privately benefit in terms of non-salary earnings from their position and, if so, what the sources of those gains are. Volunteer public service by lay citizens is the traditional organizing principle of school boards, whose members are typically elected in non-partisan contests and receive little official remuneration. School boards are also responsible for a wide range of public school district decisions, including the location of school attendance zones and the allocation of resources across schools. This raises the question of whether members influence these and other policy choices in ways that disproportionately benefit themselves and their neighborhoods. We identify the returns to being elected to a school board by constructing a house price index for each school board candidate's neighborhood and then applying a regression discontinuity design based around narrowly-decided electoral contests.

We show that the election of a new school board member causes the home values in their neighborhood to rise. However, this effect is entirely due to candidates who are not registered Democratic: prices in the neighborhoods of marginally-elected non-Democrats increase 4.2% on average post-election. As this appreciation is relative to narrowly-decided election losers who are also non-Democrats, the effect is driven by differences in the local effects of school district policy decisions. We show that student test scores in the neighborhood public schools of non-Democratic winners also increase relative to non-Democratic losers (by 0.05σ), but that this is mostly explained by changes in the student composition of neighborhood schools, including via attendance zone manipulation, rather than improvements in school quality as measured by test score value added. In contrast, we find no differential changes when comparing neighborhood or scholastic outcomes between winning and losing Democratic school board candidates.

Our analysis is made possible by assembling a dataset that links school board election results

[1] There is evidence that social capital (Björkman and Svensson, 2009; Nannicini et al., 2013), anti-corruption audits and e-governance (Olken, 2007; Ferraz and Finan, 2008), the structure of constitutions (Schedler and Plattner, 1999; Persson and Tabellini, 2003), media independence (Besley and Prat, 2006; McMillan and Zoido, 2004; Dyck, Moss and Zingales, 2013), and well-functioning elections (Adsera, Boix and Payne, 2003) serve to align elected officials' actions with the public good.

[2] For example, Donald Trump stated the following about divesting from his businesses as president – "I could actually run my business and run government at the same time. I don't like the way that looks, but I would be able to do that if I wanted to" (2017).

from 2006 to 2016 inclusive for the state of North Carolina with annual voter registry snapshots of active registered voters. The merged sample includes demographic, partisan affiliation, and residential information for anyone who ran for a seat on their local school board that we are able to link to the voter files. Using this dataset, our regression discontinuity-based empirical strategy isolates quasi-random variation in whether a candidate wins an election, which controls for differences in the unobserved characteristics of candidates. We test for heterogeneity in motivations for seeking office by estimating effects separately for non-Democratic and Democratic candidates. Covariate balance across the discontinuity reveals that winning and losing candidates in close elections are also similar based on observables (individual attributes and neighborhood characteristics). We also show that our findings are robust to several specification checks, including bandwidth choices. We further find no evidence for price differences *before* the school board election (i.e. no placebo effects), validating causal inference.

As discussed, our primary causal effects pertain to an index of house prices in school board members' neighborhoods. This variable provides a monetary measure of returns to serving on a school board under the assumption that neighborhood house values serve as a proxy for the asset portfolios of candidates. At the same time, differences in house prices may also reflect the capitalization of public school quality, which may be influenced by the school board (Rosen, 1974; Black, 1999). We construct the house price index from transactions microdata, accounting for property attributes. Given our focus on smaller geographic definitions of neighborhood (Census block group), standard hedonic methods can be heavily influenced by outliers and low transaction volumes. We therefore provide a novel contribution to measuring neighborhood home values by integrating methodologies from the teacher value-added and hedonic literatures to create an empirical Bayes shrinkage-based price index. The resulting index is more efficient than a standard neighborhood price index and later results highlight its advantage over a traditional hedonic price index.[3] We find that, as measured by our index, home prices in the Census block groups of school board election winners relatively increase following election and that this average effect is driven by candidates who are not registered Democratic.

The home value effect is suggestive that local "school quality" increases in the neighborhoods of school board election winners (relative to losers). To determine whether this is the case and, if so, explore the mechanisms behind it, we use data from the North Carolina Education Research Data Center (NCERDC) database, which provides detailed administrative information on all students, teachers, and public schools in North Carolina. We first show that test scores in the neighborhood public schools of winning non-Democrats increase by about 0.05σ post-election, relative to losing non-Democrats.[4] However, we show that this increase does not arise from improvements

[3] Morris (1983) show that this class of estimators is efficient in samples with larger variances and Fay III and Herriot (1979) provides an analogous application of the empirical Bayes estimator to per-capita income estimates for smaller geographic Census areas. Additionally, a number of economics of education scholars have implemented this type of estimator in studying teacher value-added (Kane, Rockoff and Staiger, 2008; Chetty, Friedman and Rockoff, 2014; Jackson, 2018).

[4] Our estimated price effects of 4% may be considered large for a corresponding 0.05σ increase in test scores. Note, however, that the home price effect captures all elements of neighborhood quality that may improve, not just

in school productivity – we find no effect on school or teacher value-added – but rather that the difference stems from relative changes in student composition. Specifically, we find that the composition of structural movers (i.e., students transitioning from elementary to middle school) into the neighborhood public schools of winners becomes higher-achieving, with the schools overall becoming relatively more white and higher-achieving. Consistent also with favorable adjustments to attendance zones, we find that the students residing in the neighborhoods of winners become more likely to attend higher-performing schools post-election, relative to those residing in the the neighborhoods of losers.

The finding that non-Democratic – but not Democratic – school board members affect local school attributes in ways that raise home prices in their neighborhood on a relative basis raises the question of self-interested, as opposed to public service-oriented, motivations for seeking office. While private non-salary returns could motivate these policy choices, the choices could also be a byproduct of serving the desires of voters, especially if voters in the winner's neighborhood are most salient for the winner's political success. With this possibility in mind, we examine the geographical extent of the effects on house prices. We do not find evidence of returns spilling over to the larger surrounding areas (i.e., Census tract). In addition, we find that the our effects are actually driven by winners of at-large elections. In contrast with "ward" representatives, members elected in at-large contests must seek and retain support from voters across the entire school district. Together, the results suggest that partisan affiliation is correlated with private motivations for seeking public office.

Our analysis is motivated by classic models of political office, in which politicians maximize their self-interest subject to constraints (Barro, 1973; Buchanan, 1989). This framework has motivated several areas of empirical investigation. This literature includes questions about the effectiveness of institutions at aligning the behavior of elected officials with the interests of voters – e.g., electoral accountability (Besley and Case, 1995); audits and public disclosure (Djankov et al., 2010; Ferraz and Finan, 2011) – as well as work that quantifies the value of political connections (e.g. Goldman, Rocholl and So 2013; Asher and Novosad 2017; Fafchamps and Labonne 2017). Other studies examine "political selection" (Besley, 2004; Caselli and Morelli, 2004; Dal Bó et al., 2017), showing, for example, that increases in paid remuneration can lead to increases in the quality of legislators (e.g. Ferraz and Finan 2009; Kotakorpi and Poutvaara 2011; Gagliarducci and Nannicini 2013).

Our paper connects most directly with prior work that estimates elected leaders' private returns from office. For example, Fisman, Schulz and Vig (2014) shows that narrowly-elected state officials in India experience 3-5 percent higher asset growth, consistent with rent-seeking behavior. Our setting and data environment are unique in several key respects. The U.S. is a high-income country, where democratic norms are strong and outright corruption by leaders is relatively limited. Although Eggers and Hainmueller (2009) and Querubin and Snyder (2011) examine national legislators in the U.K. and U.S., respectively, the ethic of school board membership (as opposed to

local school quality. Second, the test score effect we estimate will understate the larger changes in the long-term expectations of home buyers about school quality.

career politicians) is specifically oriented around voluntary public service. Moreover, the dataset we assemble allow us to quantify the returns that immediately follow an election (i.e., during the winner's term), as opposed to being based on post-political career differences. The personal gains accruing to elected board members that we estimate also arise from differences in the local effects of legitimate (i.e. not illegal) policy choices made over a single issue – local education – which we identify through changes in public schools.

Our findings also connect with recent work showing a robust connection between school policy and neighborhoods (e.g. Schwartz, Voicu and Horn 2014; Billings, Brunner and Ross 2018; Bibler and Billings 2020). Our results advance this connection through how school board members may impact their own neighborhoods by improving local schools, connecting also with recent work on the causal effects of school boards (e.g. Macartney and Singleton 2018; Shi and Singleton 2020). These results are therefore suggestive of the importance of diversity in school board representation, especially from underserved (i.e., low housing value or low school quality) neighborhoods.[5] Finally, our results highlight partisan-specific aspects of elected politicians' behavior, connecting with a literature on representation and the attributes of public officials (e.g. Chattopadhyay and Duflo 2004; Broockman 2013). Our paper identifies an important dimension which may impact leadership: neighborhood of residence. Folke et al. (2021) and Harjunen, Saarimaa and Tukiainen (2021) similarly show that the neighborhood of politicians matters for local public goods in Sweden and Finland, respectively.

2 Background

School boards in the U.S. are intended to keep the "public" in public education. Traditionally lay citizens who are democratically-elected by local voters in non-partisan contests, school board members compose the largest group of elected officials in the country. Around 90,000 school board members serve on approximately 15,000 boards. For their public service, members typically receive little to no pecuniary compensation: in our setting of North Carolina, the annual salary of a school board member in 2017 ranged from $1,800 (Rutherford County) to $6,300 (Burke County).

Although a historical cornerstone of local democracy in the U.S., serious concerns exist about school board governance. A longstanding criticism is that, as lay citizens, school board members generally lack expertise in developing or implementing effective education policy (Howell, 2005; Maeroff, 2010). Additionally, the low pay for school board members may discourage highly-qualified candidates from running for office as well as attract, on the margin, candidates seeking non-salary returns. There are also concerns regarding how effective elections are at providing accountability (Berry and Howell, 2007; Holbein, 2016; Kogan, Lavertu and Peskowitz, 2021*a*). For example, turnout in school board elections is notoriously low (often lower than 10%), voter information is low (ballot order effects are pronounced in school board elections (Ho and Imai, 2008)), and there

[5]Ward-based elections may limit the impacts of self-interested behavior in the aggregate by legitimizing a focus on school policies that benefit neighborhood constituents.

is substantial scope for the influence of pressure or interest groups, such as teachers' unions (e.g., Hoxby 1996; Moe 2009).

These concerns stand alongside recent evidence that school board decisions can have consequential impacts. School boards are typically responsible for policy development and implementation, hiring and evaluating senior district management (e.g., the superintendent), negotiating with teachers' unions, drawing attendance zone boundaries, and allocating teachers – a key input to local education production – across schools. Shi and Singleton (2020) present evidence from California that the quasi-random election of a board member with professional experience in education leads to a reduction in charter schooling and an increase in teacher pay in the district.[6] Focusing on partisan affiliation, Macartney and Singleton (2018) use an election discontinuity design to show that Democratic (relative to non-Democratic) school board members take actions that reduce segregation of students across schools. These impacts in the aggregate raise the question about how school boards broadly impact the distribution of school quality across students and neighborhoods.

3 Data Sources

Our empirical analysis draws on four main data sources: (1) publicly-available school board election results; (2) voter registration records; (3) house transaction records; and (4) school and student data. This section describes each of these data sources and the construction of our sample.

We begin with the school board election results available from the North Carolina State Board of Elections (NCSBE), which report the name and votes received for candidates of school board contests from 2000 to 2018 inclusive.[7] A school board election has 3.6 candidates and 1.3 winners on average. A contest winner receives about 40% of the votes on average. School board candidates can either be elected at-large by all voters in the school district (at-large contest), or they may be elected by region (region-based contest). About 74% of the school board contests are region-based (i.e. "ward"), with such contests having a smaller number of candidates than at-large contests (2.8 candidates vs. 5.8 candidates). While several North Carolina districts switched to partisan school board elections during our sample period, the vast majority (about 87%) of contests that we observe – typical of races for school board nationwide – are non-partisan (i.e. candidates' party affiliation is not listed on the ballot).[8]

To identify candidate characteristics, we link the election records with North Carolina voter registration annual snapshots (from 2005 to 2018 inclusive). The voter data includes full name, home address, age, political party, and race and ethnicity of all registered voters. We define neighborhoods of candidates according to their Census block group of residence. Because it is

[6] Fischer (2020) and Kogan, Lavertu and Peskowitz (2021*b*) similarly find that minority board members improve the outcomes of minority students.

[7] Since the State Board of Elections does not have electronic records for school board elections prior to 2008 and for some selected districts and years, we manually collect names and votes of those candidates from the official election results of each school district. Because the State Board of Elections does not report the number of election winners, we also manually collect that information from the school board rosters for each school district.

[8] Prior to 2011, fourteen of North Carolina's 115 school districts held partisan elections for school board. Since then, more than twenty districts have switched from non-partisan elections.

practically impossible to match all candidates to the voter registration database perfectly, we link the databases using a within-county fuzzy match based on names. By doing so, we match approximately 65.1% of the entries in the linked election records with the North Carolina voter registration database.[9] Relevant to our later identification discussion, Appendix Figure A.1 and Appendix Table A.3 show that our match rates are not discontinuous at the voting margin threshold for winners and losers.

We obtain information on housing prices and characteristics from transaction-level data provided under the Ztrax program, a public record extract compiled by Zillow for research purposes (Zillow, 2020). The data set covers the universe of real estate sales in North Carolina from 1995 to 2016 and contains sales price, address, and a wide range of house characteristics, such as the number of bedrooms, bathrooms, stories, square footage, year built, and quality and condition assessments. We use these records to create an annual house price index across neighborhoods (Census block groups), the construction of which is detailed in the next subsection. We limit our analysis to arm's length residential transactions by excluding outliers based on extreme prices (lower than $10,000 and higher than $2,000,000), as well as by excluding transactions that are missing or have zero values for key attributes (e.g., no bathrooms, recorded square footage is too large/small to be accurate). We also obtain information on population, median income, and the share of college graduates across Census block groups from IPUMS NHGIS (Integrated Public Use Microdata Series National Historical Geographic Information System) 2010.

Finally, to connect school board elections with the characteristics of schools in the neighborhood of the candidates, we employ rich student-level and school-level data provided by the North Carolina Education Research Data Center (NCERDC). The student-level data include demographics, attending school code, block group of residence, academic achievement, and economic disadvantaged status. We use these records to summarize characteristics of candidates' neighborhood public schools (e.g., student composition and estimated test score value added) as well as to consider student residential sorting and determine attendance zone changes. Our main measure of academic achievement is the reading and math developmental scale scores from the North Carolina End-of-Grade (EOG) test, which measures grade-level competencies. Schools in North Carolina are assessed based on the overall achievement levels of attending students, making the measure a reasonable target for school board members. We limit our sample to kindergarten through eighth grade (elementary and middle grades) and focus on third through eighth grade test scores.

3.1 Variable Construction

In this subsection, we detail the construction of several key variables used in our analysis, including the house price index and the characteristics of candidates' neighborhood schools over time.

We create an annual neighborhood-level house price index from the house transaction records

[9] The detailed matching procedure is presented in Appendix A. Our Appendix also provides results that show our main results are robust to a matching procedure that allows for non-unique matches and obtains a corresponding match rate of 82.4%.

using an Empirical Bayes estimator.[10] This is accomplished in two steps: First, we estimate a hedonic-style regression to obtain estimates of prices by Census block and year that are purged of differences in home quality. Specifically, we estimate the following:

$$\ln price_{ijt} = \alpha + X_{it}\beta + \pi_{jt} + \epsilon_{ijt}\,, \tag{1}$$

where $\ln price_{it}$ is the logarithm of the transaction price of house i in Census block group j in year t, X_{it} includes housing characteristics (e.g., bedrooms, bathrooms, stories, square footage, year built, and quality and condition assessments), and π_{jt} is a year-by-block group fixed effect. Second, we apply a shrinkage adjustment to the residualized block group price estimates ($\hat{\pi}_{jt}$). This adjustment is made to address concerns about the precision of a smaller geographic price index, which is affected by the number of transactions and the variance in home attributes of sold properties. The results in Section 5.1 show that this concern is salient. The final shrunken index value is given by:

$$\hat{\pi}^{s}_{jt} = \alpha_{jt}\hat{\pi}_{jt} + (1-\alpha_{jt})\bar{\pi}_{c(j)t}\,, \tag{2}$$

where $\bar{\pi}_{c(j)t}$ is the average of the Census block group fixed effects among group c in year t. Group c indicates the (larger) geographical area within which a common prior is assumed. $\alpha_{jt} = \frac{\hat{\sigma}^2_{c(j)t}}{\hat{\sigma}^2_{c(j)t}+\hat{\lambda}_{jt}}$ denotes the precision of the estimated average house price in block group j in t, with $\hat{\sigma}^2_{c(j)t}$ representing the estimated variance of the Census block group fixed effects within group c in year t and $\hat{\lambda}_{jt}$ representing the estimated variance of house prices within Census block group j in year t.[11] Intuitively, the estimate shrinks imprecise fixed effects towards a group c-level prior. In out context, we want a prior that captures a larger, higher transaction volume housing market as well as be limited in a size to a single commuting areas around a central city. In our main results, we use Combined Statistical Areas (CSAs) as groups (priors) in estimating our price index ($\hat{\pi}^{s}_{jt}$).[12]

We use the student residence information from the NCERDC data to characterize candidates' local neighborhood schools in terms of quality, student demographics, and other attributes. To do so, we identify those students (in kindergarten through eighth grade) who reside in each candidate's block group in the year that the candidate ran for school board and define the candidate's "neighborhood school" as the composite or average school attended. For candidate i, denote by b the block group and fix τ as the year of the election they appeared in. Let X_{st} represent data for school s at time t. The value of X for candidate i's neighborhood school in year t is given by the

[10] This estimator is common in the teacher value-added literature (e.g., Kane, Rockoff and Staiger 2008) and in essence weights a prior more heavily when current year estimates have a higher variance.

[11] If a Census block group has a unique house transaction in a year, then we cannot define $\hat{\lambda}_{jt}$ and the block group-by-year is excluded from the sample.

[12] There are 11 CSAs in North Carolina (Greensboro-Winston-Salem–High Point, Hickory-Lenoir, Greenville-Washington, Myrtle Beach-Conway, Asheville-Brevard, Charlotte-Concord, Virginia Beach-Norfolk, New Bern-Morehead City, Raleigh-Durham-Chapel Hill, Fayetteville-Lumberton-Laurinburg, Rocky Mount-Wilson-Roanoke Rapids). 37 out of 100 counties are not included in any CSA and are classified as an additional group. We report robustness checks with priors at MSA (Metropolitan Statistical Area) and county levels in Table C.6.

following weighted average:

$$X_{it} = \sum_{s} w_{s(i)} X_{st} \, , \tag{3}$$

where the summation is over all schools, weighted by enrollment among students residing in block group b in election year τ (formally, $w_{s(i)} = \frac{N_{sb\tau}}{N_{b\tau}}$). We fix the weights at election year τ so that they are pre-determined with respect to post-election outcomes that may be influenced by candidate i. This variable definition allows us to separate out direct impacts on neighborhood schools from changes in school assignment or student sorting across neighborhoods, which may also be affected by a candidate's election.

3.2 Descriptive Statistics

We present descriptive statistics and summarize key patterns in the data in this subsection. Table 1 reports characteristics of all voters in column 1. Column 2 reports attributes of non-minority school board candidates who are matched with the voter registration in our sample. We focus on non-minority candidates in our main analysis to rule out heterogeneity in policy preferences among racial groups as well as to acknowledge the limited sample of non-Democratic minority school board members. Columns 1 and 2 indicate that the demographics of school board candidates are similar to those of voters in general, except for fewer unaffiliated voters. However, as subsequent columns indicate, there are substantial disparities in the proportion of female candidates and average age between non-Democratic and Democratic school board candidates, which may affect the preferred policies of candidates. Non-Democratic candidates also participate more in at-large contests and non-Democratic winners are more likely to have won an at-large contest than Democratic winners.

Panel A of Table 2 provides basic summary statistics of school board candidates' Census block groups. The reported house prices are the average prices for a Census block group a year before school board elections and are in 2010 inflation-adjusted dollars. Columns 1 and 2 show that the average house prices in the neighborhood of candidates are similar to the average house prices in the state. Subsequent columns show that the average house prices in the neighborhood of non-Democratic losers are slightly lower than those of the other candidates, but there are no significant differences among them. Our house price index also reflects the gaps in the non-adjusted house prices between those groups. To gain insight into the socioeconomic status of candidate neighborhoods, we also report median income and the share of college graduates from IPUMS NHGIS (Integrated Public Use Microdata Series National Historical Geographic Information System) 2010. The median incomes for the neighborhoods of non-Democratic candidates ($54,486 and $54,583) are about 14% higher than the average in the state ($47,822). Those of Democratic candidates ($51,843 and $49,281) are closer to the average in the state. The share of college graduates does not differ across local communities of school board candidates.

Panel B of Table 2 illustrates the summary statistics of the neighborhood schools of school board candidates. Normalized math and reading scores reveal differences between neighborhood schools

Table 1: Descriptive Statistics – Candidate Characteristics

			Non-Democratic		Democratic	
	All Observations	All Candidates	Winners	Losers	Winners	Losers
Age	52.18	52.70	51.69	49.30	57.81	55.37
Prop. Female	0.53	0.42	0.54	0.31	0.45	0.45
Prop. Democratic	0.36	0.38	0.00	0.00	1.00	1.00
Prop. Republican	0.41	0.49	0.85	0.75	0.00	0.00
Prop. Unaffiliated	0.23	0.13	0.15	0.25	0.00	0.00
Prop. Incumbent		0.17	0.26	0.06	0.29	0.10
Prop. Winner		0.45	1.00	0.00	1.00	0.00
Prop. Contest at Large		0.52	0.50	0.60	0.35	0.55
Election Year		2011.4	2011.6	2011.4	2011.3	2011.4
Number of Individuals	7,163,352	1,287	323	473	265	226
Number of Individual-Years		4,051	997	1,506	835	713

Notes: Summary statistics of all voters are from the voter registration database in 2010. The number of individuals from column 2 through 6 means the number of candidates (individual ID-by-contest). The number of individual-year is the number of observations during 4 years after elections (the term of school board members). Individual-year cells that are not matched or with missing house price information are excluded.

Table 2: Descriptive Statistics – Local Characteristics

			Non-Democratic		Democratic	
	All Observations	All Candidates	Winners	Losers	Winners	Losers
Panel A: Block Groups						
Avg. House Price	175,711	178,203	178,363	176,989	179,170	179,265
House Price Index	-0.04	-0.03	-0.02	-0.04	-0.03	-0.03
Median Income	47,822	53,072	54,486	54,583	51,843	49,281
Share of College Graduates	0.33	0.35	0.35	0.34	0.36	0.33
Prop. Urban Areas	0.56	0.46	0.40	0.46	0.53	0.44
Panel B: Schools						
Avg. Normalized Test Score	-0.09	0.05	0.00	0.00	0.08	0.07
Prop. Black Students	0.28	0.17	0.22	0.19	0.14	0.15
Prop. White Students	0.51	0.67	0.62	0.64	0.70	0.68
Prop. Hispanic Students	0.13	0.10	0.10	0.10	0.10	0.11
Prop. Econ. Disadv.	0.56	0.51	0.54	0.54	0.50	0.49
Avg. Teacher Experience	12.81	13.32	13.48	13.27	13.36	13.24
Number of Students	362.39	473.83	454.38	466.45	467.60	492.44

Notes: In Panel A, median income and share of college graduates are from IPUMS NHGIS 2010. The average house price and median income are in 2010 inflation-adjusted dollars. In Panel B, each observation in the first column is school-by-year and that in the other columns is synthetic school-by-year in the block group of each candidate. Economically disadvantaged students are defined by students in the free lunch program.

of non-Democratic candidates and those of Democratic ones. The test scores in the neighborhood schools of both non-Democratic winners and losers are about 0.1σ lower than the levels in the neighborhood schools of Democratic winners and losers, while there is no significant difference between winners and losers within the same political affiliation. The proportion of white students are more than 5 percentage points lower in the neighborhood schools of non-Democratic candidates than those of Democratic candidates. The summary statistics of housing prices and school characteristics indicate that the residence of Democratic and non-Democratic candidates differ within the state.

4 Research Design

We are interested in the causal effects of a candidate's election to a school board on their subsequent outcomes, such as neighborhood home prices. To estimate these effects, our approach is to compare winning and losing candidates, where *who* won is plausibly random but all else is held equal. In this section, we describe our research design leveraging narrowly-decided contests in detail and present validity checks of the key assumptions.

4.1 Empirical Specification

Our main empirical specification implements a regression discontinuity (RD) design at the individual candidate level using vote shares. To build intuition, we begin by presenting our basic RD model. Let x_i be the vote margin for candidate i (running in calendar year t), which is the running variable. For candidates successfully elected to the board, x_i is the difference between their vote share and that of the most popular loser in the contest and is positive. For losing candidates on the other hand, x_i is computed as the difference between their vote share and the vote share of the least popular winner, making it negative.

We are interested in estimates of:

$$Y_i = \alpha^p + \beta^p D_i + \gamma_1^p x_i + \gamma_2^p D_i \cdot x_i + \epsilon_i \tag{4}$$

within a suitably narrow bandwidth, where Y_i represents a post-election outcome associated with candidate i (e.g., home prices in their neighborhood) and $D_i = 1(x_i > 0)$. The coefficient β^p is the parameter of interest, identifying the effect on Y_i of winning a seat on a school board (relative to losing), and causal inference is justified under the assumption that only the *identity* of the winning candidate is discontinuous at vote margin $x_i = 0$.

Though we estimate a specification of this form while pooling all school board candidates in the data (to understand the average treatment effect), most of our results focus on average treatment effects by partisan affiliation. This heterogeniety is represented by the superscripts in equation (4), which indicate that we estimate causal impacts among Democratic ($p = 1$) and non-Democratic ($p = 0$) candidates by separately estimating (4) for the respective subsamples. In doing so, the stratified specifications ask whether the local impacts of school board policies benefit marginally-elected members relative to narrowly-losing candidates within the same political affiliation.

Although equation (4) adequately identifies causal effects, we pool up to four years of post-election data in practice, when available, for greater efficiency, noting that four years is the modal term length for board members. The specification we take to the data is given by:

$$Y_{i\tau} = \alpha^p + \beta^p D_i + \gamma_1^p x_i + \gamma_2^p D_i \cdot x_i + \gamma_3^p Z_i + \delta_{t(i)}^p + \epsilon_{i\tau} \tag{5}$$

where $\tau \in \{1, 2, 3, 4\}$ indexes available post-election years. While we estimate and present results that do not include controls, we also include several variables in some specifications to examine robustness and improve precision: Z_i includes candidate demographic controls (age, sex, and race), demographic composition of the elected school board and students in the district, and an indicator for urban area. These variables are measured at the time of the election, $t(i)$, so as to be pre-determined. The next subsection examines validity checks using these variables. $\delta_{t(i)}^p$ is an election year fixed effect for candidate i. We estimate equation (5) pooling all candidates and separately for Democratic and non-Democratic candidates, using optimally-selected bandwidths (Calonico, Cattaneo and Farrell, 2020). We investigate the robustness of our main results to this choice and use the bandwidths chosen for our principal variable of interest, neighborhood home prices, throughout the analysis.

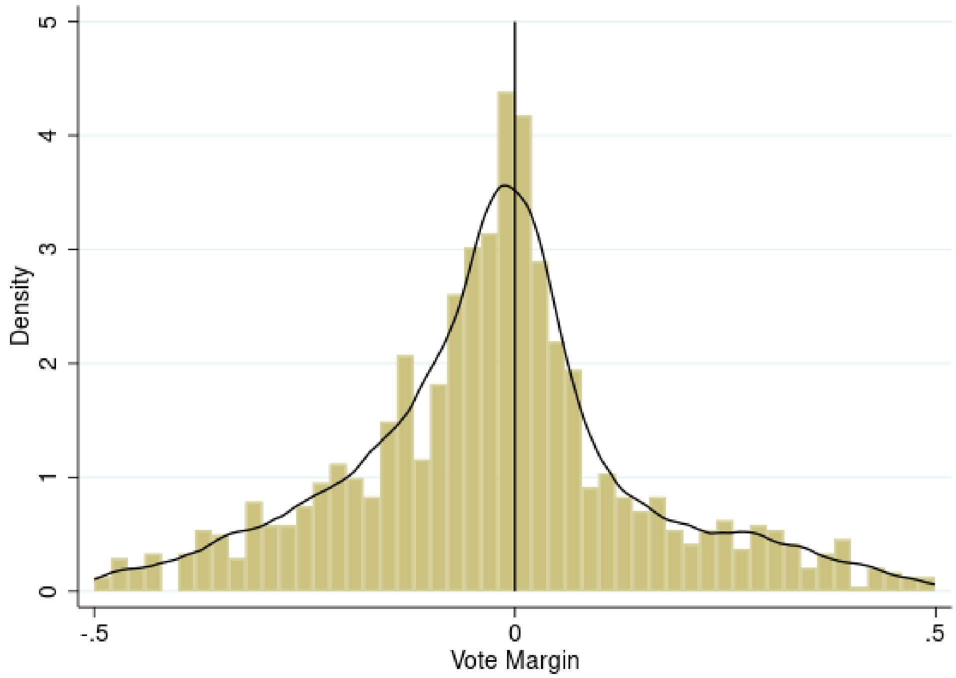
In addition to validity checks to validate our research design, presented next, the variation over time in outcomes allows us to test for placebo effects. After discussing the main results, we therefore present results that re-estimate equation (5) that instead use pooled outcomes from available *pre-election years* (e.g. $\tau \in \{-1, -2\}$), when no differences would be expected.

4.2 Validity

Before presenting the main results, we conduct validity and specification tests of our regression discontinuity design. Figure 1 tests continuity of the vote density among candidates around the threshold of vote margin. A discontinuity around the threshold or a bunching of candidates on one side of the cutoff would be suggestive of violations of non-manipulation (Imbens and Lemieux, 2008). From the figures, we do not observe a discontinuity in the density of vote margin. It is also important to check whether there is bunching of the distribution of votes around the threshold for non-Democratic and Democratic candidates, respectively, considering that we compare winning and losing candidates conditional on political affiliation in our main analysis. Figure C.3 reveals that the density of vote margin is continuous around the cutoff for both parties.

Table 3 reports RD estimates of discontinuities in covariates at the vote margin as validity checks. If we find such a discontinuity in a covariate, it casts doubt on the identifying assumption underlying the RD design. We use the optimal bandwidth of the main RD designs of house prices and control for election year fixed effects. Panel A of Table 3 presents RD estimates of candidate-level characteristics. The first three columns show that the election winners are not more likely to

Figure 1: Density of Vote Margin



Notes: The figure depicts the distribution of vote margin around the cutoff that determines whether a candidate wins. The x-axis measures vote margin. For candidates successfully elected to the board, vote margin is defined by the difference between their vote share and that of the most popular loser in the contest and is positive. For losing candidates, it is computed by the difference between their vote share and the vote share of the least popular winner and is negative.

be older, a female, and an incumbent school board member than elections losers. The dependent variables in columns 4 and 5 are log house prices and their changes in the Census block groups of candidates a year before the elections. The RD estimates suggest that there is no difference between housing values in the communities of winners and losers around the threshold of vote margin. Correspondingly, we also cannot observe any discontinuity in the median income level of the Census block groups of winners and losers in the last column.

Panel B of Table 3 checks for discontinuities in the characteristics of school board contests. The RD estimate in the first three columns indicate that at-large contests, the log of total votes in a contest and the number of winners are all balanced. It is also well known that the turnout in off-cycle local elections is lower than usual because voters have a diminished incentive to vote. The last columns of Panel B indicate that the election winners are not more likely to have run for off-cycle elections. Thus, our results are not driven by the difference in the electoral systems or policy preferences related to the characteristics of school board elections.

The balance of district characteristics is also important because our RD design compares winners' and losers' local neighborhoods. Panel C of Table 3 reports balance checks at the school district

Table 3: Balance Checks

Panel A: Characteristics of Candidates						
	(1) Age	(2) Female	(3) Incumbent	(4) Log House Price	(5) ΔLog House Price	(6) Log Median Income
Vote margin > 0	0.896	-0.036	0.054	0.016	0.007	-0.041
	(1.545)	(0.066)	(0.052)	(0.064)	(0.043)	(0.050)

Panel B: Characteristics of Contests				
	(1) Contest At-large	(2) Log Total Votes	(3) Number of Winners	(4) General Election Date
Vote margin > 0	-0.004	0.004	0.109	0.005
	(0.055)	(0.174)	(0.162)	(0.022)

Panel C: Characteristics of School Districts					
	(1) Share Black (Students)	(2) Share Econ. Disadv. (Students)	(3) Urban Area	(4) Number of Board Members	(5) Share White (Board Members)
Vote margin > 0	0.001	0.012	-0.026	-0.138	0.041
	(0.019)	(0.014)	(0.067)	(0.226)	(0.028)

Notes: Regression discontinuity estimates are computed using a local linear regression. The bandwidth set at the optimal level of the main analysis in Table 4 is 0.115. All regressions include election year fixed effects. Columns 2 and 3 of Panel A are the estimates of the indicators of female, and an incumbent school board member. Log house price is based on the average house price in Census block group of a candidate one year before the school board election and ΔLog house price is based on the change in average house price from two years to one year before the election. Median income at Census block group is from IPUMS NHGIS 2010. In Panels C, columns 1, 2, and 5 report the estimates of the shares of the indicated students and school board members for a given school district. Economically disadvantaged students are defined by those who are in the free lunch program. Column 3 indicates the estimates of the indicators of urban clusters and urbanized areas.

level. Columns 1 and 2 present the effects of winning a school board election on the demographic composition of students (proportions of black and economically disadvantaged students) within in a school district. The third columns assess whether election winners are disproportionately more from an urban district than election losers. The dependent variables in the final two columns of each panel are the number of school board members and the demographic composition of incumbents (proportion of white school board members). All school district-level variables show no discontinuity at the vote margin. Appendix Figure C.4 presents the parallel RD plots showing the consistent continuities of the covariates around the threshold of vote margin.

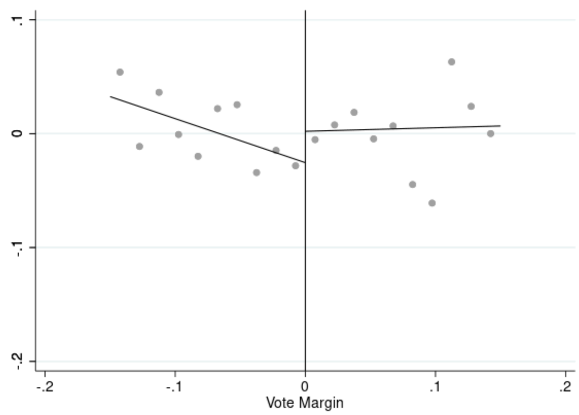
Since we also estimate causal effects within-partisan affiliation by comparing just same-party election winners and losers, we report the parallel results for balance checks separately for non-Democratic and Democratic candidates in Appendix Table C.4. Within each political group, we cannot observe any discontinuity in the same set of covariates. We also report the RD plots of the covariates in Appendix Figures C.5 and C.6 by political party. These results consistently suggest that the characteristics of candidates are well balanced around the threshold of vote margin, even when comparisons are restricted to same-party candidates.

5 The Returns to School Board Election

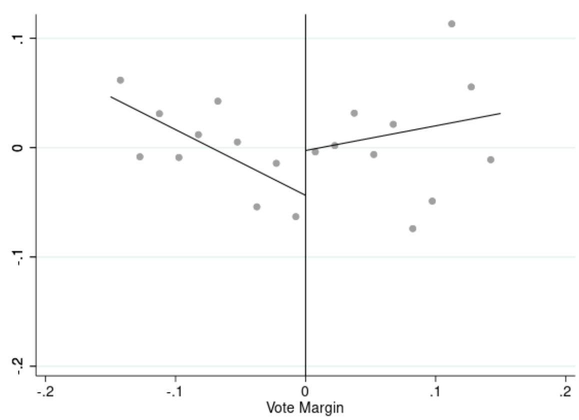
We present our main results here with a focus on the causal effect of being elected to a school board on the house prices in winner's neighborhoods. We examine the effect on average, pooling all candidates, and among Democratic and non-Democratic school board members separately, finding important heterogeneity. We also examine the spatial extent of the effects and present several robustness checks for our main findings. We then turn to the mechanisms behind these results, including impacts on measures of school quality and student sorting.

Figure 2: RD Estimates of house price index

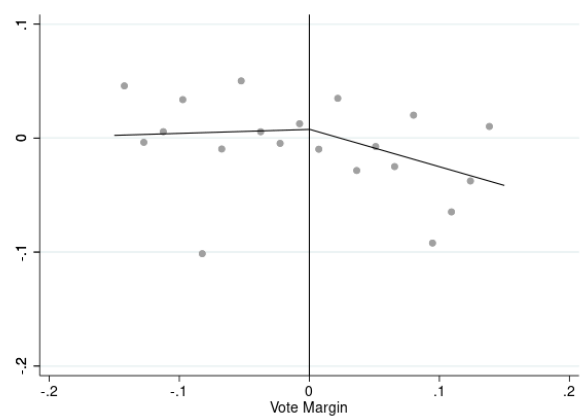
(a) All Candidates



(b) Non-Democratic Candidates



(c) Democratic Candidates



Notes: The y-axis is the home price index and the x-axis measures vote margin. We use the average of the optimal bandwidths for non-Democratic and Democratic candidates (0.150) in all panels. We set equally-spaced bins for both sides of the threshold and each point indicates the average of the home price index within each bin of the vote margin. Each line fits data on either side of the vote margin threshold. We control for the covariates including election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas.

Figure 2 shows visual evidence of the causal effect of winning a school board election on home prices in the winner's neighborhood using a local linear RD design with the optimal bandwidth following Calonico, Cattaneo and Farrell (2020). Panel (a) of Figure 2 reports a discontinuous increase in home prices at the vote margin threshold in general. Panels (b) and (c) report the discontinuity of home prices for non-Democratic and Democratic groups, respectively. Winning a school board election raises home prices in the block group of non-Democratic candidates more than in the block group of all candidates. On the contrary, we can observe a slightly discontinuous reduction in the block group of Democratic school board members.

To be more concrete about the effects, Table 4 presents RD estimates of the effect of winning a school board election on home prices in the winner's neighborhood. As reported in column 1, we find that home prices in the block group of an election winner increase by around 2.3% relative to prices in a loser's block group. Columns 3 and 5 indicate that the increase in home prices is just driven by the non-Democratic school board candidates. The coefficient in column 3 implies that a non-Democratic winner raises house prices in her neighborhood by 4.1% relative to prices in a non-Democratic loser's neighborhood. In contrast, column 5 of Table 4 reveals no discontinuity in neighborhood public good quality among Democratic school board candidates.

Columns 2, 4 and 6 of Table 4, respectively, add candidate-level controls (age, sex, and incumbent), school district-level controls including demographic compositions of students (proportions of black, and economically disadvantaged students), and school board members (proportions of black, female, and Democratic members), an urban indicator, and election year fixed effects, and show that these main findings are unchanged by their inclusion. While the results among Democratic candidates are inconsistent with relative gains for winners, the results for non-Democratic candidates suggest that winners take actions as elected officials which are to the disproportionate benefit of their own neighborhood and that this is capitalized into neighborhood home prices.

Table 4: Estimation Results of House Price Index

	All Candidates		Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)
Vote margin > 0	0.023*	0.028**	0.041**	0.042***	-0.011	-0.012
	(0.013)	(0.012)	(0.016)	(0.016)	(0.018)	(0.018)
Controls	N	Y	N	Y	N	Y

Notes: RD estimates are computed using local linear regressions. The optimal bandwidth estimated following Calonico, Cattaneo and Farrell (2020) are 0.115 for all candidates, 0.125 for non-Democratic and 0.170 for Democratic candidates. The number of observations within the bandwidths are 2,000, 1,317 and 809, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

The above results raise the question of how localized are the impacts of a non-Democratic winner on neighborhood public good quality? For example, do the effects spillover across all neighborhoods in a Census tract or do they remain local only to the candidate's immediate neighborhood?[13] To examine this, in Table 5 we estimate causal effects of winning a school board election on home prices in broader neighborhoods. We begin, in columns 1 and 3 of Table 5, with the Census tract-level house prices (constructed as the population-weighted average across block groups within the tract). Winning a school board election has no statistically significant effects on tract-level prices among either non-Democratic and Democratic candidates.[14] To directly examine the spillover effects of winning a school board election on nearby neighborhoods, we look at block groups within candidates' Census tract excluding their own. Column 2 and 4 confirm that house prices in other block groups in the same tract are not affected by the election results.

Table 5: Estimation Results of House Price in Census Tract

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
Vote margin > 0	0.017	0.007	-0.023	-0.011
	(0.013)	(0.014)	(0.014)	(0.015)
Unit	Census Tract	Leave-one-out Block Groups	Census Tract	Leave-one-out Block Groups

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,317, 1,232, 805, and 778, respectively. Block Groups having no other Block Groups in the same Census Tract are excluded in columns 2 and 4. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. Leave-one-out block groups mean the block groups within the Census tract except for the candidate's own block group.

5.1 Robustness and Placebo Checks

We find that, among non-Democratic candidates for school board, home prices relatively increase in winners' neighborhoods and that this effect is highly localized. We also do not find evidence that Democratic winners similarly benefit their neighborhood relative to Democratic losers. We examine the robustness of these findings as well as carry out placebo tests to support causal inference in this subsection.

Our main results in Table 4 are based on optimal bandwidths and include several control variables in the estimation. In Table 6, we examine robustness to these specification choices. The bandwidth in the first column of each panel is the optimal bandwidth from Table 4 and the following are 0.1, 0.15, and 0.2. The first row reports the estimates from the baseline specification and the second

[13] A Census tract is a collection of multiple block groups and contains less then 8,000 people with an optimum size of 4,000 people. Block groups generally contain between 600 and 3,000 people, with an optimum size of 1,500 people

[14] One concern is that any effect in this case could be muted by the possibility of winning and losing candidates residing in the same tract (such overlap hardly ever occurs at the block group level). To check this possibility, we restrict the observations to candidates from tracts with no other candidates. The results are qualitatively unchanged, showing no differential effects among either group of candidates.

row presents the estimates from the parallel specification excluding all the controls. The results show that the findings are robust to the bandwidth choice. Comparison between two rows also indicates that adding the controls only has minor effects on the estimates across the bandwidths confirming that winners and losers of close elections are as good as randomly assigned.

Since our use of an Empirical Bayes estimator of neighborhood housing prices is new to this literature, we explore how our shrunken estimate performs relative to standard home price indices in Appendix Table C.5. To show how our measure of neighborhood housing prices is robust to outliers, we report the estimates after excluding outliers and compare them with the estimates of residualized house price in Appendix Table C.5.[15] The estimates of the housing price index are robust to winsorizing at the 1%, 5%, and 10% tails of house price distribution and number of house transactions in a neighborhood. More interestingly, our housing price index is more precise relative to a standard residualized price index and is less sensitive to outlier prices as given by the results in Panel A of Appendix Table C.5. This analysis highlights a broader contribution of our results in that using an Empirical Bayes estimator to determine neighborhood housing prices has benefits in any empirical applications that uses small geographical areas.

Table 6: Estimation Results of House Price across Bandwidths

	Non-Democratic				Democratic			
	0.125	0.100	0.150	0.200	0.170	0.100	0.150	0.200
With Controls	0.042***	0.062***	0.040***	0.029**	-0.012	-0.005	-0.002	-0.015
	(0.016)	(0.018)	(0.014)	(0.013)	(0.018)	(0.023)	(0.019)	(0.017)
Without Controls	0.041**	0.060***	0.037**	0.024*	-0.011	-0.017	-0.001	-0.012
	(0.016)	(0.018)	(0.015)	(0.013)	(0.018)	(0.023)	(0.019)	(0.018)
Observations	1,317	1,176	1,429	1,586	809	623	744	856

Notes: RD estimates are computed using local linear regressions and each cell represents a separate regression. The number of observations are within the bandwidths. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

In addition to these robustness checks, we test for placebo effects as a validation of our empirical strategy: under our research design's assumptions, there should be no discontinuity in outcomes between election winners and losers *prior to* the election. Table 7 reports the RD estimates of the house prices during the two years **before** the elections for the same block groups of school board candidates in Table 4. Unlike the main results for the post-election periods, we cannot observe a statistically significant discontinuity between the block groups of winners and losers for any group of candidates. Including the characteristics of school district attenuates further the

[15] As is typical in the real estate and urban economics literature, one often measures neighborhood valuation based on the creation of a neighborhood annual price index. We implement this method by estimating Equation 1 and averaging resulting residuals ($\bar{\epsilon}_{jt} = \frac{\sum_{i \in j} \epsilon_{ij}}{N_i}$) across a given definition of neighborhood j uniquely for each J neighborhood on an annual basis.

coefficient for non-Democratic candidates, while the parallel estimates for the post-election periods stay statistically significant and large as shown in Table 4. In Appendix Figure C.9, we also provide "event study"-style graphs of the RD estimates around the year of election. There are no gaps of home prices between winners and losers within each party in the pre-election years, but the RD estimates jump after elections just for non-Democratic candidates though the standard errors are larger than the results of the main pooled regressions. This is also robust to the exclusion of the controls.

In the Appendix, we further show that our main findings are robust to three other research choices. First, the RD estimates of house price are robust to the choice of prior house price when we build the Empirical Bayes estimator of the house price index. Note that we use the average residualized house price at the CSA level as the prior in our main analysis. Appendix Table C.6 presents that the main results are qualitatively similar with MSA-level and county-level priors. Second, our results are qualitatively similar when we do not exclude minority school board candidates from the sample. Panel A of Appendix Table C.9 reports that the estimated winner's effect on home prices is 2.8% for non-Democratic candidates after controlling for the covariates, while the effect is still statistically insignificant for Democratic candidates. Second, we instead estimate the effects among Republican-registered candidates (about 80% of non-Democratic candidates are Republicans). Panel A of Table C.10 indicates a 6.2% effect on neighborhood home prices for Republican winners relative to Republican losers. While somewhat larger, this effect is not statistically different from the estimate that pools all non-Democratic candidates.

Table 7: Estimation Results of House Price in Block Group before Elections

	All Candidates		Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)
Vote margin > 0	0.006	0.008	-0.001	0.001	0.020	0.006
	(0.015)	(0.014)	(0.019)	(0.018)	(0.023)	(0.022)
Controls	N	Y	N	Y	N	Y

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.115 for all candidates, 0.125 for non-Democratic, and 0.170 for Democratic). The number of observations within the bandwidths are 1,204, 767 and 523, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas.

5.2 Heterogeneity

In this section, we investigate heterogeneity in the return to winning the office across election types and school board characteristics. These results speak to the importance of contextual and policy factors, including at-large contests, the size of the school board, and the aggregate political composition of the school board.

In North Carolina, school board candidates run for office either across an entire district (at-large election systems) or within a specific sub-district or wards (region-based election systems). One possibility is that candidates representing wards are specifically incentivized to influence policy choices to the benefit of local neighborhood voters. To check for this heterogeneity, we interact the indicator for winning with an indicator for at-large contests in the baseline RD model.[16] The results, shown in Column 1 in Table 8, indicate that the relative increase in neighborhood home prices of non-Democratic winners is actually driven by at-large contests. This result is robust to adding the controls as shown in column 2 and there is no discontinuity for Democratic candidates regardless of the type of election systems. That the relative increase in neighborhood prices among Democratic candidates is primarily attributable to winners elected at-large, who must draw support from across the district, is suggestive of the role of self-interest in driving the main results.[17]

Table 8: Estimation Results of House Price by Contest Type

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
Vote margin > 0	-0.024	-0.019	-0.036	-0.036
	(0.036)	(0.039)	(0.030)	(0.030)
(Vote margin > 0)×At-large Election	0.088**	0.084**	0.040	0.032
	(0.040)	(0.043)	(0.038)	(0.038)
At-large Election	-0.044	-0.051	-0.036	-0.071***
	(0.032)	(0.033)	(0.023)	(0.023)
Controls	N	Y	N	Y

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,317 and 809, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Another factor of a school board that may affect results is the number of school board members which varies across school districts. A large number of school board members may mitigate the power of a single member to derive benefits from policy decisions unless their policy preferences accord well. The number of school board members range from 4 to 11 in our sample and we define the school boards with members **more than** 7, the median, as large ones.[18] Table 9 supports this hypothesis by reporting the heterogeneous effects for non-Democratic candidates of the large school boards. In column 1, house prices at the block group of a non-Democratic winner from a small school board increase by around 5.7%, but most of the effect disappears at the neighborhood

[16]We also control for the interactions of the running variable and the indicator for at-large contests, and of the running variable, the indicator for winning, and the indicator for at-large contests.

[17]A related possibility is that success in at-large contests is costlier to attain, raising the incentive for private rent-seeking in office.

[18]The school districts with large school boards account for 40% of the school districts in our observations.

of a non-Democratic winner from a large school board.[19] These results may suggest that a large number of school board members may hinder a non-Democratic school board member from taking private returns in the collective decision-making (Olson, 1971).

Table 9: Estimation Results of House Price by School Board Size

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
Vote margin > 0	0.057***	0.058***	-0.007	-0.008
	(0.019)	(0.020)	(0.020)	(0.020)
(Vote margin > 0)×Large School Board	-0.058**	-0.084***	-0.013	-0.000
	(0.029)	(0.029)	(0.043)	(0.043)
Large School Board	0.094***	0.091***	0.129***	0.079***
	(0.024)	(0.023)	(0.028)	(0.024)
Controls	N	Y	N	Y

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,317 and 809, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. A large school board has more board members than the median and the school districts with large school boards account for 40% of the school districts in our observations. The coefficients are statistically significant at the **5%, or ***1% level.

Apart from the size of a school board, the collective decision making process makes composition of a school board important. It may be easier for a school board member to secure private returns regardless of the number of members if the other members have similar policy preferences. To capture this aspect, we check whether a school board member belonging to the partisan majority for a school board affects the local return to election. A candidate is defined to belong to the majority party if the proportion of her party is larger than 0.5 among incumbent school board members (i.e., those whose term has not yet expired).[20] Table 10 reports the results. Columns 2 reports 3.8% higher effect of a non-Democratic candidate's winning a school board election on neighborhood housing prices if her party is the majority group of the school board after controlling for the covariates though the result is not statistically significant enough. These nosier results suggest larger price increases for non-Democratic winners belonging to the majority party, which is consistent with our hypothesis that self-interested behavior is easier if the other colleagues in the school board are aligned politically. Belonging to the majority party does not generate heterogeneous effects on house price for Democratic school board members.

[19]Table C.7 also indicates that the results are not because the districts with large school boards are more or less likely to be urban areas.

[20]Note that this definition identifies the "potential" majority party conditional on the matched school board candidates because we can observe political affiliation only when a candidate is matched to voter registration database.

Table 10: Estimation Results of House Price by Political Majority

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
Vote margin > 0	0.030	0.023	-0.014	-0.017
	(0.021)	(0.020)	(0.023)	(0.022)
(Vote margin > 0)×Majority	0.026	0.038	0.001	0.007
	(0.032)	(0.029)	(0.039)	(0.038)
Majority	-0.005	-0.014	-0.019	-0.004
	(0.024)	(0.027)	(0.022)	(0.023)
Controls	N	Y	N	Y

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,317 and 809, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the $^{*}10\%$ level.

6 Mechanisms

The results in the prior section reveal that winning a school board election causes home values in non-Democratic winners' neighborhoods to increase (relative to non-Democratic losers' neighborhoods). The corresponding results for Democratic candidates show no causal effect. Since school board members are solely focused on education and the quality of assigned schools is an important part of neighborhood quality, we turn to education data to examine whether there are any causal effects of board members on schools and students over time.

There are several potential ways for school board members to induce changes in local school quality and neighborhood house prices. First, school board members are charged with local education production by allocating resources. Better resources allocated to the neighborhood schools serving board members may improve the academic performance of students. Second, the allocation of education resources may not have direct effects on students' achievement, but may attract better students to their neighborhood schools. Third, school board members are able to directly change the composition of students in their neighborhood schools by shifting school attendance zones within the school district.

6.1 Test Scores and Local School Quality

Our main measure of student achievement is the average normalized test score of End-of-Grade (EOG) math and reading scores for neighborhood schools of school board candidates. We also estimate school and teacher value-added following Chetty, Friedman and Rockoff (2014) and use those estimates as measures of local school quality. The detailed derivation of the value-added is reported in Appendix B. To better compare school-based results to our main results for house prices, we fix the range of bandwidth at the optimal levels in the regressions of house prices and control for the same covariates (election year fixed effects, candidate, and school district characteristics).

Table 11: Estimation Results of Measure of School Quality

	Non-Democratic			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Test Score	School Value-added	Teacher Value-added	Test Score	School Value-added	Teacher Value-added
Vote margin > 0	0.048**	0.001	-0.003	0.005	-0.007	-0.003
	(0.021)	(0.005)	(0.003)	(0.022)	(0.006)	(0.004)

Notes: RD estimates are computed using local linear regressions. The regressions of school and teacher value-added are estimated by shrinkage estimation method following Koedel, Mihaly and Rockoff (2015). The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,235 and 746, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the **5% level.

Columns 1 and 4 of Table 11 report the estimated winner's premium on average test score at neighborhood schools for non-Democratic and Democratic candidates, respectively. The coefficient in column 1 implies that a non-Democratic winner raises the average test scores at her neighborhood schools by 0.048 standard deviations during the post-election periods relative to non-Democratic losers' neighborhood schools. This result along with the muted effect for Democratic candidates in column 4 corresponds with earlier house prices results. Taken together, our results indicate a 0.05 standard deviation increase in test scores corresponding with about a 4% price increase. This effect size is larger than typical estimates of the hedonic value of school quality (e.g. Black 1999), but our context is somewhat different. We are measuring not just changes in school quality but also induced changes in other neighborhood attributes (e.g. lower crime, improvements to homes, etc.) that go along with better schools. Additionally, the test score effect we estimate does not capture expectations about future test scores which will likely be higher, as an improvement in test scores attracts higher-achieving students.

To understand the policy tools that school board members take advantage of, it is important to know what components of the test score drive the gap between the neighborhood schools of winners and losers. In columns 2 and 5, we replace the average test score with school value-added at the neighborhood schools. Column 2 shows that there is no discontinuity of school value-added corresponding to the discontinuity in test score for non-Democratic candidates. Similarly, we cannot observe any significant effects of a non-Democratic candidate's winning election on the average teacher value-added at the neighborhood schools in column 3. As with average test score, the local value-added measures do not respond to the elections results of Democratic candidates in columns 5 and 6. Thus, school board members do not appear to affect neighborhood school actual value-added regardless of their political affiliations. Of course, perceived school quality by parents may not be captured in our measure of school value-added more broadly.

There are two takeaways from these results. First, the effects of winning a school board election on the average test score at the neighborhood schools of non-Democratic candidates is driven by factors other than improvement of local school quality measured by school and teacher value-added, such as changes in student composition. Second, local housing values are more likely to respond to test scores of neighborhood schools rather than the schools' ability to improve academic performance. This is likely because test scores are considerably more observable than school and teacher value-added (Brasington and Haurin, 2006; Imberman and Lovenheim, 2016).

Table 12: Estimation Results of Teacher Experience

	Non-Democratic				Democratic			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Avg. Exp	Prop. New	Prop.2-9 Yrs	Prop. 10+ Yrs	Avg. Exp	Prop. New	Prop.2-9 Yrs	Prop. 10+ Yrs
Vote margin > 0	0.193	-0.009**	-0.012*	0.022***	-0.037	0.010*	-0.011	0.001
	(0.169)	(0.004)	(0.007)	(0.008)	(0.185)	(0.005)	(0.009)	(0.010)
Mean	13.28	0.07	0.33	0.60	13.49	0.07	0.33	0.60

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,235 and 746, respectively. The dependent variables in columns 1 and 5 are the average experience years and those in the other columns are the shares of teachers in the experience groups varying 0 through 1. The means of the variables are calculated an year before the elections. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the *10%, or ***1% level.

We now turn to investigate the allocation of education resources by school board members. One measure of resources often employed in the literature is teacher experience. We run the RD model with average experience years of teachers and shares of teachers by experience year (less than or equal to 1 year, 2-9 years, and longer than or equal to 10 years) at the neighborhood schools as dependent variables. Columns 1 and 5 of Table 12 indicate that average experience years at the neighborhood schools do not respond to the school board election results. In column 2, however, the proportion of new teachers is 0.9 percentage point (12.8%) lower at the neighborhood schools of non-Democratic winners relative to the neighborhood schools of non-Democratic losers. Similarly, the proportion of teachers having 2 to 9 years of experience also declines by 1.2 percentage point (3.6%). The reduced portion of less experienced teachers correspondingly raises the proportion of more experienced teachers. The proportion of teachers with experience longer than or equal to 10 years is 2.2 percentage point (3.7%) higher for non-Democratic winners. On the other hand, a Democratic candidate's winning slightly raises the share of new teachers by 1 percentage point.[21]

Considering the positive correlation between teaching experience and value-added (e.g. Pa-

[21] Appendix Figures C.7 and C.8 provide graphical version of our main results. Appendix Table C.8 shows these results for test score and teachers disappear when using our placebo test for the years prior to an election.

pay and Kraft 2015), the finding of no discontinuity in average experience is consistent with the value-added results in Table 11. However, the significant effects of non-Democratic school board members on the composition of teachers indicate that the board members effectively change education resources in their neighborhood schools. Though these effects evidently do not translate to improvement of local school quality directly, they may signal changes in school quality to students or parents of students and may attract better students to the neighborhood schools of non-Democratic winners.

6.2 Student Sorting and School Assignments

Two mechanisms that may induce improvement in neighborhood test scores and house prices are student sorting and board-determined shifts in school attendance zones. In Panel A of Table 13, we first investigate discontinuities in the student composition of neighborhood schools. High-achieving students are those who get scores higher than the average test score of their cohort in the previous year. We can observe that winning a school board election raises the proportion of white students by 2.4 percentage point (3.5%) at the neighborhood schools of non-Democratic winners in the four years following the election, which is consistent with the estimate of average test scores in Table 11 considering that the average scores of white students are higher than the overall average score. The proportion of the students in the free lunch program slightly declines, but the estimate is not statistically significant. More importantly, the share of high-achieving students is 1.9 percentage point (3.6%) higher at the neighborhood schools of non-Democratic winners than those of non-Democratic losers in the post-election periods. This result confirms that the rise in average test scores at the neighborhood schools of non-Democratic winners must be driven in part from changes in student composition.

If non-Democratic school board members induce student composition changes at the neighborhood schools, we expect that these effects should be more discernible for the structural movers – those students who switch from an elementary school to a middle school at 5th or 6th grade. Changes in the composition of structural movers will be jointly influenced by board-determined attendance zone shifts (i.e. changes to which neighborhoods "feed" each middle school) and how households endogenously respond, including to changes in perceived school quality. Panel B of Table 13 reports the results of the corresponding composition among the structural movers. We define the students' characteristics before moving. Columns 1 shows that the estimated discontinuity in the proportion of white structural movers is 2.2 percentage point (3.2%) at the threshold corresponding to that white students in Panel A. More importantly, winning a school board election raises the proportion of high-achieving structural movers 4.0 percentage point (7.8%) for the non-Democratic group, much higher than 1.9 percentage point (3.6%) in Panel A. By contrast, for Democratic candidates, we observe no jumps in the composition measures at the threshold.

To understand the role of attendance zone shifts by the school board independent of student sorting, it would be ideal to observe attendance zone boundaries and how they change over time. Since that data is not available beyond a couple school districts, however, we indirectly test for school

Table 13: Estimation Results of Student Sorting

Panel A: All Students

	Non-Democratic			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Prop. White	Prop. Free Lunch	Prop. High-achieving	Prop. White	Prop. Free Lunch	Prop. High-achieving
Vote margin > 0	0.024*	-0.009	0.019**	0.007	-0.017	-0.000
	(0.012)	(0.012)	(0.009)	(0.015)	(0.013)	(0.009)
Mean	0.68	0.50	0.53	0.64	0.54	0.50

Panel B: Structural Movers

	Non-Democratic			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Prop. White	Prop. Free Lunch	Prop. High-achieving	Prop. White	Prop. Free Lunch	Prop. High-achieving
Vote margin > 0	0.022*	-0.002	0.040***	-0.009	-0.001	-0.014
	(0.013)	(0.013)	(0.012)	(0.015)	(0.014)	(0.013)
Mean	0.67	0.52	0.51	0.61	0.57	0.48

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The dependant variables are the proportions of each group of students varying 0 through 1. In Panel A, the number of observations within the bandwidths are 1,235 and 746, respectively. High-achieving students are defined by those who get an above-average score an year before. In Panel B, structural movers are defined by the students who are structurally forced to transfer to a middle school at 5th or 6th grade. The number of observations are slightly smaller (1,176 and 711, respectively) than in Panel A because the candidates having no structural movers within their neighborhood schools are excluded. The means of the variables are calculated an year before the elections. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the *10%, or ***1% level.

boundary changes by examining changes in school attendance patterns within a neighborhood.[22] We construct two measures of school boundary changes to capture qualitative and quantitative effects, respectively. First, we fix school-level average test scores in the year of elections and calculate the average, based on their school attended, across students residing in the block group of each candidate in post-election periods. So, this qualitative measure captures changes in (a proxy for) local school quality due to changes in school attendance (because the school-level test scores are fixed prior to the candidate's term).

Second, we calculate the Kullback–Leibler (KL) divergence to measure quantitative shift in school attendance zone.[23] The KL divergence is a simple and widely-used measure of how much a

[22] Given the presence of public school choice, private school and magnet schools, we cannot rule out some elements of opting in/out of neighborhood schools from these options.

[23] For distributions P and Q over the same support X, the Kullback–Leibler divergence (also called relative entropy) from Q to P is defined by $\sum_{x\in X} P(x)\log\left(\frac{P(x)}{Q(x)}\right)$. In our context, X is the set of schools that the students living in the block group of a candidate attend and distributions Q and P are the distributions of the neighborhood students

distribution is different from a reference distribution. We use this measure to estimate how much the distribution of schools that students residing in Census block group of a candidate attend changes during the post-election periods, as compared with the distribution in the year of election. If the KL divergence is larger for election winners relative to that for election losers, it would indicate that the schools attended by students in the neighborhood of the winner have disproportionately changed, consistent with a shift in school attendance zones.

Table 14: Estimation Results of Boundary Changes

	Non-Democrat				Democrat			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Avg. School Test Scores		KL Divergence		Avg. School Test Scores		KL Divergence	
Vote margin > 0	0.060***	0.068***	-0.009	-0.008	-0.018	-0.010	-0.009	-0.001
	(0.020)	(0.022)	(0.007)	(0.008)	(0.022)	(0.022)	(0.007)	(0.011)
Neighboring Students Fixed	N	Y	N	Y	N	Y	N	Y

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,107 and 693, respectively. The outcomes in columns 1, 2, 5, and 6 are the average school test scores in the block group of each candidate with school-level average test scores fixed in the year of elections. The outcomes in columns 3, 4, 7, and 8 are the Kullback–Leibler divergence. In columns 2, 4, 6, and 8, we construct the outcomes after dropping neighborhood students who move into the block groups of candidates following the school board elections. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the **5%, or ***1% level.

Table 14 reports the RD estimates of these measures of school attendance zone shift. Columns 1 and 5 report discontinuities of the qualitative measure of school attendance zone. The average test score, after freezing school-level score in election year, is 0.060 standard deviation higher in the block group of non-Democratic winners than in the block group of non-Democratic losers. In other words, the students living in the block groups of non-Democratic winners are more likely to attend better schools than those living in the block groups of non-Democratic losers after school board elections. Considering that there is no such a discontinuity before school board elections as shown in Table C.8, this gap is solely driven by the change in school attendance in the post-election period. As before, there is no discontinuity for Democratic party.

Since results so far do not eliminate effects from new students who may sort into a neighborhood post-election, Columns 2 and 6 report the parallel results after dropping neighborhood students who move into the block groups of candidates *following* a school board election. If there is no discontinuity for the students who originally reside in the block group of candidates prior to an election, then our results suggesting school boundary changes may simply be driven by the particular school choices of new residents. The statistically indistinguishable estimate in column 2 invalidates this hypothesis and supports the conclusion that a non-Democratic school board mem-

over the schools in the year of election and in the post-election years, respectively.

ber disproportionately shifts school attendance zone for all neighborhood students. The muted impacts for Democratic board members shows they are not improving local school quality in their own neighborhoods by shifting school attendance zone.

In the next columns, we report the RD estimates of quantitative boundary changes. Contrary to the estimates of qualitative measures, we cannot observe any statistically significant discontinuity for non-Democratic candidates. This result, however, is not inconsistent with the results of the qualitative measures in the sense that school boundary changes at the block groups of winners could also entail school boundary changes at the block groups of losers within the same school district. If this is the case, there may be no discontinuity of the KL divergence because it increases both at the neighborhoods of winners and losers in the post-election periods.

7 Conclusion

Our results provide a comprehensive picture of the impacts of winning a school board election on a winner's neighborhood and the schools serving it. Estimates show that a non-democratic winner's neighborhood appreciates in value by 4.2% relative to a school board election loser's neighborhood in the four years following the election. This price appreciation is an overall measure of neighborhood quality, of which schools likely play an important role (Black, 1999; Ries and Somerville, 2010). To understand why such price appreciation occurs, we provide three pieces of evidence consistent with improvement in perceived "school quality."

First, relative to the neighborhood schools for non-Democratic losers, we show that neighborhood schools for non-Democratic winners improve in test score performance. Second, on a relative basis, we provide evidence that a non-Democratic winner's neighborhood school also increases in both the experience of teachers as well as the share of high-achieving (based on past test score performance) students. Finally, we provide evidence that a non-Democratic winner's neighborhood is relatively more likely to be assigned to a higher quality school post-election, and this is not due to greater school district re-alignment in school attendance boundaries.

The evidence we uncover broadly supports a role for self-interested behavior in local governance that varies along partisan lines and does not simply reflect the policy preferences of local constituents. There are some limits from our context of non-partisan elections in North Carolina to generalizing these conclusions to all other states. For example, Crawford (2018) provides survey evidence that nonpartisans tend to express more partisan views about policies than those elected in an explicitly partisan system. Thus our results may be weaker in partisan systems. Additionally, the low turnout of school board elections allows for interest groups to have out-sized influence on election results, especially during off-cycle elections (Anzia, 2011; Oliver, Ha and Callen, 2012). Thus a small group of voters' policy preferences for individual school outcomes may contribute to our findings.

Our results support several actionable policies that would limit self-interested behavior and likely decrease inequality in school quality across neighborhoods. Specifically, our results suggest that neighborhood-based (i.e., ward-based) school board seats would legitimize the existing behavior of school members favoring their own neighborhood and thus better align policies with their constituents relative to at-large school board seats. If wards allow for equal representation across a range of neighborhood types then this would limit favoritism for advantaged neighborhoods in school policies more broadly. Additionally, our results suggest that school boards with more members limit self-interested behavior, suggesting a benefit from expanding the size of school boards. Beyond the type of school board election, efforts to recruit school board candidates from under-represented and lower school quality neighborhoods could help to counteract the self-interested behavior of candidates from more advantaged neighborhoods.

Our findings suggest that school board members are not immune from pursuing policies that may favor themselves at the expense of the larger public good. This result has implications for all public officials as well as points to the importance of oversight at any level of government. More broadly, the stark differences across partisanship suggests that political ideology can mitigate self-interest. This result highlights a role for policy platforms and partisan constituencies to counteract self-interested behavior among public officials.

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Appendices

A Procedure of Candidate Matching

Theoretically, we can find all the election candidates in the voter registration database because they are required to register in their district's county. However, due to differences in naming conventions (e.g. middles names, nicknames), perfect matches are somewhat limited. Furthermore, we cannot use any information of existing board members to ensure the comparability of matching rates for winning and losing candidates. The biggest problem is that some candidates used their nicknames or abbreviations rather than their full name in election records. Plus, middle names are not identified for many candidates. As a result, we employ a within-county fuzzy match based on their names and location.

- 1st Trial
  For each candidate, we first narrow down the voter pool to those from the same county. We first split their names into 4 parts (first, middle, last names, and suffix). We replace the middle name with the initial of the middle name if a person has a middle name because most names from the election records are presented with the initial of the middle names. Stata package **reclink2** generates a similarity score for a pair of names based on varying weights on the components of names. In the baseline algorithm, we double weight first and last names considering the accuracy of the components of original names. Among the matched pairs from this algorithm, we pick up only exact or almost perfect match (with a matching score larger than 0.95) for each candidate if that is the unique match. As a result, we collect the matches with the exact same names or the matches with the same first and last names and consistent abbreviations of middle names or suffixes.

- 2nd Trial
  With the unmatched candidates from the first trial, we replace their first name with potential full first names if the candidate used a nickname instead of their full first name. This process is necessary because many election candidates used nicknames rather than their full first name. We first construct a mapping from the nickname of a candidate to some conventional full first names borrowed from ThoughtCo.[24] After that, we use the same matching function in the 1st trial and pick up the unique exact matches.

Basically, we try to be conservative in picking the right matches rather than maximizing matching rates to limit including voters that were never school board candidates. However, our main RD estimates of house price index are robust to the inclusion of the candidates with non-unique matches as shown in Table A.1. The observations include non-unique matches along with the main observations in Table 4. The non-unique matches have no evidence that guarantees that the

[24]https://www.thoughtco.com/matching-up-nicknames-with-given-names-1421939

matched names indicate different people. In other words, a non-unique match should be included in the main observations if there were no other matches for the candidate. We weight a non-unique match by the inverse of the total number of non-unique matches for the candidate. If we include candidates having non-unique matches, the match rate increases from 65.1% to 82.4%.

Table A.2 shows the random examples of the name matches from our procedure. For instance, "ann b edwards" from the election results is uniquely matched to "ann bare edwards" from voter registration database, which means that there is no other "ann edwards" having a middle name starting from "b" among the voters who live in her county. Table A.3 reports the results of balance checks for match rates. If there is a discontinuity of match rates around the threshold, it should invalidate our matching algorithm. Columns 1 through 3 present the RD estimates of overall match rates with bandwidth of 0.1, 0.15, and 0.2. Columns 4 through 6 report the RD estimates of match rates in the first trial which generates the most accurate matches. The results indicate that there is no difference in match rates between election winners and losers. The RD plots in Figure A.1 also indicate no discontinuity of match rates.

We also provide evidence of treatment among the matched candidates in Figure A.2. The matched candidates with positive vote margins are elected to the school board. This is particularly important in our analysis, as school board election results from NCSBE do not always explicitly indicate the winners and losers of each contest. We hand-collect the number of election winners for such a contest from the website of each school board. Figure A.2 confirms that the collected information is consistent with the number of votes from the election data from NCSBE.

Table A.1: Estimation Results of House Price Including Candidates with Multiple Matches

Panel A: Non-Democratic

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote margin > 0	0.044***	0.047***	0.032**	0.021*	0.045***	0.046***	0.030**	0.018
	(0.016)	(0.015)	(0.012)	(0.011)	(0.016)	(0.015)	(0.013)	(0.011)
Bandwidth	0.086	0.100	0.150	0.200	0.086	0.100	0.150	0.200
Controls	N	N	N	N	Y	Y	Y	Y
Observations	1,787	1,914	2,363	2,629	1,787	1,914	2,363	2,629

Panel B: Democratic

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote margin > 0	-0.016	-0.012	-0.010	-0.016	-0.017	-0.025	-0.010	-0.017
	(0.015)	(0.019)	(0.016)	(0.015)	(0.015)	(0.020)	(0.017)	(0.015)
Bandwidth	0.200	0.100	0.150	0.200	0.200	0.100	0.150	0.200
Controls	N	N	N	N	Y	Y	Y	Y
Observations	1,487	1,057	1,312	1,487	1,487	1,057	1,312	1,487

Notes: RD estimates are computed using local linear regressions. Election candidates matched to multiple identities having similar names from the voter registration database are added to the baseline observations. A matched identity is weighted by the inverse of the total matched identities for the candidate. The optimal bandwidth estimated following Calonico, Cattaneo and Farrell (2020) are 0.086 for all candidates, and 0.200 for non-Democratic. The number of observations within the bandwidths are 1,787 and 1,487, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Table A.2: Examples of Name Matches of School Board Candidates

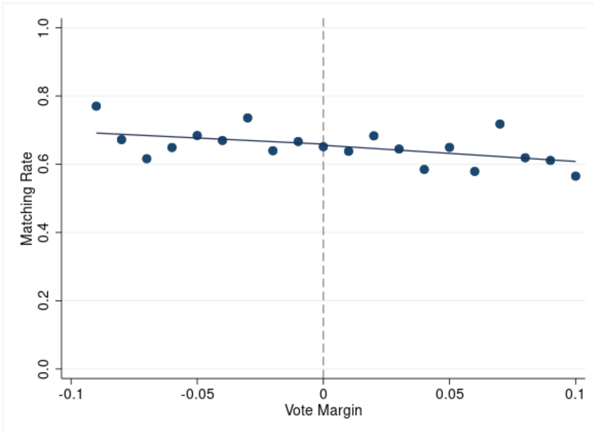
Name in Election	Name in Voter Registration	Matching Score
ann b edwards	ann bare edwards	0.9987
anne mclaurin	anne n mclaurin	0.9997
barbara balmer	barbara ann balmer	0.9993
betty edwards miller	betty edwards miller	1
david woodcox	david earl woodcox jr	0.9541
john robert (rob) mcintyre	john robert mcintyre	0.9999
gary c strickland jr	gary curtis strickland jr	0.9994
hardin c kennedy iii	hardin claude kennedy iii	0.9994
michael a (mike) hodges	michael anthony hodges	0.9969
ronald (ronny) holste	ronald eugene holste	0.9975

Table A.3: Balance Checks of Match Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall Match Rate	Overall Match Rate	Overall Match Rate	Match Rate in 1st Trial	Match Rate in 1st Trial	Match Rate in 1st Trial
Vote margin > 0	0.031	0.014	-0.021	-0.017	-0.016	-0.037
	(0.032)	(0.029)	(0.027)	(0.034)	(0.031)	(0.028)
BW	0.100	0.150	0.200	0.100	0.150	0.200

Figure A.1: RD Plots of Match Rates

(a) Overall Match Rate



(b) Match Rate in 1st Trial

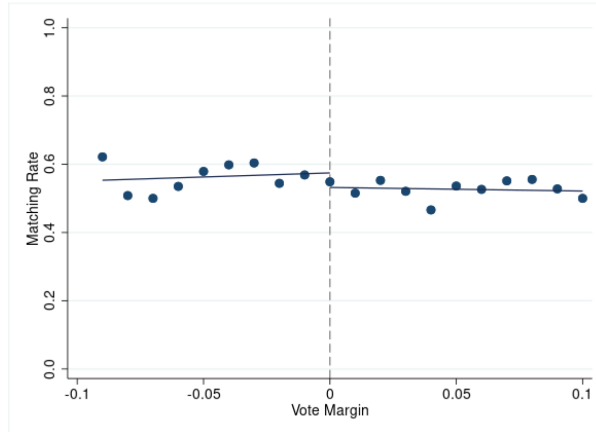
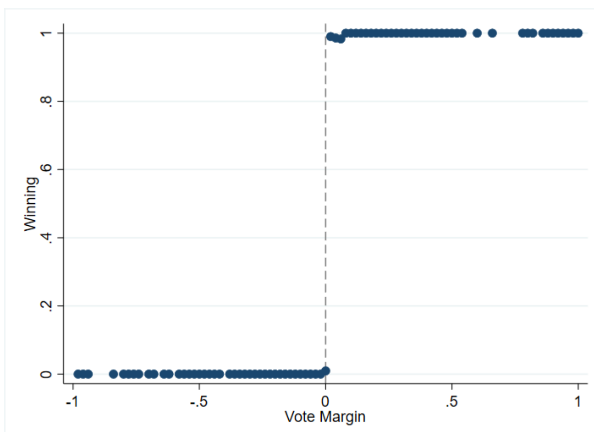


Figure A.2: Winning a School Board Position



B Construction of Value-Added

We follow the way of Chetty, Friedman and Rockoff (2014) to construct value-added measures.[25]. This appendix focuses on teacher value-added, but the derivation is same for school value-added. We begin by matching a student with math (Math K-8) and reading (Languate Arts K-8) teachers from Course Membership Snapshot. We construct math and reading value-added separately and use the average value-added in our main RD estimation. Though we cannot fully match all the students, the North Carolina Education Research Data Center (NCERDC) reports that over 80 percent of the student records were matched to teacher ID. Using the matched data, we residualize observed scores (A^*_{ijt}) with respect to controls (X_{ijt}) including lagged test score interacted with grade, its square, demographics, and the number of students and student composition in the class and the school by running the following OLS regressions with the teacher fixed effects (α_j):

$$A^*_{ijt} = \alpha_j + X_{ijt}\beta + \epsilon_{ijt}.$$

Then, the residualized test score is $A_{ijt} = A^*_{ijt} - X_{ijt}\hat{\beta}$ and we take the average of the residualized test score for each teacher-year, $\bar{A}_{jt}$.

Teacher j's value-added in year t is defined as the best linear predictor of $\bar{A}_{jt}$ based on the previous residualized test scores, $\{\bar{A}_{j1}, \ldots, \bar{A}_{jt-1}\}$:

$$\hat{\mu}_{jt} = \sum_{s=1}^{t-1} \hat{\psi}_s \bar{A}_{js},$$

$$\text{s.t. } \hat{\psi}_s = argmin \sum_j (\bar{A}_{jt} - \sum_{s=1}^{t-1} \psi_s \bar{A}_{js})^2.$$

Then, we aggregate $\hat{\mu}_{jt}$ at the neighborhood schools of school board candidates in each year. Construction of school value-added just requires us to replace the teacher fixed effects (α_j) with the school fixed effects (α_s).

[25] The RD estimation results do not change much when we use other ways of construction such as Koedel, Mihaly and Rockoff (2015)

C Tables and Figures

Table C.4: Balance Checks by Political Affiliation

Panel A: Characteristics of Candidates, Non-Democratic

	(1) Age	(2) Female	(3) Incumbent	(4) Log House Price	(5) ΔLog House Price	(6) Log Median Income
Vote margin > 0	2.128	0.071	0.069	0.039	-0.023	-0.025
	(1.936)	(0.079)	(0.065)	(0.077)	(0.063)	(0.061)

Panel B: Characteristics of Candidates, Democratic

	(1) Age	(2) Female	(3) Incumbent	(4) Log House Price	(5) ΔLog House Price	(6) Log Median Income
Vote margin > 0	1.699	-0.040	0.055	0.035	0.112	0.004
	(2.077)	(0.090)	(0.072)	(0.091)	(0.071)	(0.067)

Panel C: Characteristics of Contests, Non-Democratic

	(1) Contest At-large	(2) Log Total Votes	(3) Number of Winners	(4) General Election Date
Vote margin > 0	0.007	0.203	0.104	0.028
	(0.063)	(0.212)	(0.191)	(0.024)

Panel D: Characteristics of Contests, Democratic

	(1) Contest At-large	(2) Log Total Votes	(3) Number of Winners	(4) General Election Date
Vote margin > 0	0.022	0.019	0.136	0.020
	(0.081)	(0.244)	(0.233)	(0.034)

Panel E: Characteristics of School Districts, Non-Democratic

VARIABLES	(1) Sh. Black Students	(2) Sh. Economically Disadvantaged Students	(3) Urban Area	(4) Number of Board Members	(5) Sh. White Board Members
Vote margin > 0	0.021	-0.005	-0.010	-0.107	0.007
	(0.022)	(0.017)	(0.082)	(0.266)	(0.032)

Panel F: Characteristics of School Districts, Democratic

VARIABLES	(1) Sh. Black Students	(2) Sh. Economically Disadvantaged Students	(3) Urban Area	(4) Number of Board Members	(5) Sh. White Board Members
Vote margin > 0	-0.018	0.020	-0.064	-0.249	0.060
	(0.031)	(0.021)	(0.096)	(0.343)	(0.045)

Notes: Regression discontinuity estimates are computed using a local linear regression. The bandwidths are set at the optimal level of the main results in Table 4. All regressions include election year fixed effects. Columns 2 and 3 of Panel A and B are the estimates of the indicators of female, and an incumbent school board member. Log house price and ΔLog house price are based on the average house price in the block group of a candidate one year before the school board election. Median income at block group level is from IPUMS NHGIS 2010. In Panels E and F, columns 1, 2, and 5 report the estimates of the shares of the indicated students and school board members are at school district. Economically disadvantaged students are defined by those who are in the free lunch program.

Table C.5: Estimation Results of House Price without Outliers

Panel A: Bounded by Average House Prices

	Non-Democratic			Democratic		
	Price Index	Residual Price	Observation	Price Index	Residual Price	Observation
All Observations	0.042***	0.059*	1,317	-0.012	-0.012	809
	(0.016)	(0.033)		(0.018)	(0.041)	
1st pct. $\leq$ House Price $\leq$ 99th pct.	0.036**	0.054*	1,297	-0.009	-0.007	793
	(0.015)	(0.032)		(0.017)	(0.040)	
5th pct. $\leq$ House Price $\leq$ 95th pct.	0.039***	0.060*	1,200	-0.010	0.001	731
	(0.014)	(0.031)		(0.017)	(0.038)	
10th pct. $\leq$ House Price $\leq$ 90th pct.	0.041***	0.067**	1,070	-0.020	-0.020	652
	(0.014)	(0.032)		(0.017)	(0.038)	

Panel B: Bounded by Number of Transactions

	Non-Democratic			Democratic		
	Price Index	Residual Price	Observation	Price Index	Residual Price	Observation
All Observations	0.042***	0.059*	1,317	-0.012	-0.012	809
	(0.016)	(0.033)		(0.018)	(0.041)	
1st pct. $\leq$ Transactions $\leq$ 99th pct.	0.042***	0.060*	1,303	-0.013	-0.014	802
	(0.016)	(0.033)		(0.018)	(0.041)	
5th pct. $\leq$ Transactions $\leq$ 95th pct.	0.029**	0.042	1,203	-0.013	0.001	739
	(0.014)	(0.033)		(0.018)	(0.042)	
10th pct. $\leq$ Transactions $\leq$ 90th pct.	0.030**	0.045	1,114	-0.019	-0.028	647
	(0.014)	(0.034)		(0.020)	(0.044)	

Notes: Panel A reports the RD results of house price index and residual house price after excluding outliers in terms of average house price at the neighborhood of candidates. Panel B reports the parallel estimates after excluding outliers in terms of number of house transactions at the neighborhood of candidates. For instance, the first rows present the baseline RD estimates with all observations and the second rows show the RD estimates after excluding the upper and lower 1% of observations. RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). All regressions include election year fixed effects, candidate controls (age, sex, and incumbent) and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The urban areas cover Census urbanized areas and urban clusters. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Table C.6: Estimation Results of House Price Index with Different Priors

	Non-Democratic				Democratic			
Prior	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CSA Level	0.042***	0.062***	0.040***	0.029**	-0.012	-0.005	-0.002	-0.015
	(0.016)	(0.018)	(0.014)	(0.013)	(0.018)	(0.023)	(0.019)	(0.017)
MSA Level	0.047**	0.068***	0.040**	0.031*	-0.027	0.014	-0.007	-0.023
	(0.020)	(0.022)	(0.019)	(0.017)	(0.024)	(0.029)	(0.025)	(0.022)
County Level	0.056**	0.076***	0.049**	0.037*	-0.044	-0.015	-0.028	-0.046
	(0.025)	(0.027)	(0.023)	(0.021)	(0.030)	(0.037)	(0.031)	(0.028)
No Prior	0.059*	0.091**	0.071**	0.053*	-0.012	0.005	0.009	-0.019
	(0.033)	(0.036)	(0.031)	(0.029)	(0.041)	(0.052)	(0.043)	(0.040)
Bandwidth	0.125	0.100	0.150	0.200	0.170	0.100	0.150	0.200
Observations	1,317	1,176	1,429	1,586	809	623	744	856

Notes: Each cell reports the RD estimate of house price index with the corresponding prior of house price and bandwidth. RD estimates are computed using local linear regressions. The bandwidths in columns (1) and (5) are set at the optimal level of the main results in Table 4. All regressions include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members). The urban areas cover Census urbanized areas and urban clusters. The coefficients are statistically significant at the *10%, **5%, or *1% level.

Table C.7: Estimation Results of House Price by Urban Classification

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
Vote margin > 0	0.020	0.029	0.030	0.016
	(0.020)	(0.021)	(0.020)	(0.019)
(Vote margin > 0)$\times$Urban Area	0.040	0.022	-0.086**	-0.058
	(0.033)	(0.032)	(0.037)	(0.039)
Urban Area	-0.052**	-0.036	-0.011	0.013
	(0.026)	(0.025)	(0.023)	(0.023)
Controls	N	Y	N	Y

Notes: RD estimates are computed using local linear regressions. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). The number of observations within the bandwidths are 1,317 and 809, respectively. The controls include election year fixed effects, candidate-level controls (age, indicators of sex, and incumbent), and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members). The urban areas cover Census urbanized areas and urban clusters. The coefficients are statistically significant at the *10%, or **5% level.

Table C.8: Placebo Tests

Panel A: School Quality

	Non-Democratic			Democratic		
	(1) Test Score	(2) School Value-added	(3) Teacher Value-added	(4) Test Score	(5) School Value-added	(6) Teacher Value-added
Vote margin > 0	-0.031	-0.009	-0.004	-0.014	0.001	-0.007
	(0.029)	(0.007)	(0.007)	(0.032)	(0.008)	(0.006)
Observations	633	633	633	404	404	404

Panel B: Teacher Experience

	(1) Avg. Exp	(2) Prop. New	(3) Prop.2-9 Yrs	(4) Prop. 10+ Yrs	(5) Avg. Exp	(6) Prop. New	(7) Prop.2-9 Yrs	(8) Prop. 10+ Yrs
Vote margin > 0	0.056	-0.005	-0.001	0.006	0.237	-0.004	0.001	0.004
	(0.225)	(0.006)	(0.010)	(0.012)	(0.280)	(0.007)	(0.011)	(0.014)
Observations	633	633	633	633	404	404	404	404

Panel C: Student Composition

	(1) Prop. White	(2) Prop. Free Lunch	(3) Prop. High-achieving	(4) Prop. White	(5) Prop. Free Lunch	(6) Prop. High-achieving
Vote margin > 0	-0.007	-0.016	-0.003	0.006	0.012	-0.016
	(0.020)	(0.016)	(0.014)	(0.017)	(0.016)	(0.012)
Observations	633	633	633	404	404	404

Panel D: Composition of Structural Movers

	(1) Prop. White	(2) Prop. Free Lunch	(3) Prop. High-achieving	(4) Prop. White	(5) Prop. Free Lunch	(6) Prop. High-achieving
Vote margin > 0	-0.001	0.008	0.011	-0.018	0.003	-0.012
	(0.018)	(0.017)	(0.016)	(0.022)	(0.019)	(0.019)
Observations	600	600	600	383	383	383

Panel E: Boundary Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Avg. Score		KL Divergence		Avg. Score		KL Divergence	
Vote margin > 0	0.008	0.008	-0.009	-0.004	0.002	0.016	-0.022	-0.020
	(0.028)	(0.032)	(0.015)	(0.015)	(0.033)	(0.034)	(0.015)	(0.014)
Neighboring Students Fixed	N	Y	N	Y	N	Y	N	Y
Observations	529	529	529	529	341	341	341	341

Notes: RD estimates are computed using local linear regressions. The left panels report the estimates for non-Democratic candidates and the right panels report the estimates for Democratic candidates. For each candidate, we include observations in the three years before election. The bandwidths are set at the optimal level of the main results in Table 4 (0.125 for non-Democratic and 0.170 for Democratic). All regressions include election year fixed effects, candidate controls (age, sex, and incumbent) and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The urban areas cover Census urbanized areas and urban clusters.

Table C.9: Estimation Results Including Minority Candidates

Panel A: House Price Index

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
Vote margin > 0	0.043**	0.050***	0.011	0.017
	(0.018)	(0.017)	(0.018)	(0.017)
Controls	N	Y	N	Y
Observations	1,254	1,254	1,123	1,123

Panel B: School Quality

	(1) Test Score	(2) School Value-added	(3) Teacher Value-added	(4) Test Score	(5) School Value-added	(6) Teacher Value-added
Vote margin > 0	0.056**	0.003	-0.005	0.005	0.003	-0.001
	(0.022)	(0.005)	(0.004)	(0.021)	(0.005)	(0.003)
Observations	1,177	1,177	1,177	1,047	1,047	1,047

Panel C: Teacher Experience

	(1) Avg. Exp	(2) Prop. New	(3) Prop.2-9 Yrs	(4) Prop. 10+ Yrs	(5) Avg. Exp	(6) Prop. New	(7) Prop.2-9 Yrs	(8) Prop. 10+ Yrs
Vote margin > 0	-0.036	-0.002	-0.013*	0.015*	-0.199	0.014***	-0.013	-0.001
	(0.172)	(0.004)	(0.007)	(0.009)	(0.182)	(0.005)	(0.008)	(0.009)
Observations	1,177	1,177	1,177	1,177	1,047	1,047	1,047	1,047

Panel D: Student Composition

	(1) Prop. White	(2) Prop. Free Lunch	(3) Prop. High-achieving	(4) Prop. White	(5) Prop. Free Lunch	(6) Prop. High-achieving
Vote margin > 0	0.030**	-0.020	0.022**	0.028**	-0.009	0.005
	(0.012)	(0.013)	(0.010)	(0.012)	(0.012)	(0.009)
Observations	1,177	1,177	1,177	1,047	1,047	1,047

Panel E: Composition of Structural Movers

	(1) Prop. White	(2) Prop. Free Lunch	(3) Prop. High-achieving	(4) Prop. White	(5) Prop. Free Lunch	(6) Prop. High-achieving
Vote margin > 0	0.025*	-0.014	0.032**	0.019	0.003	-0.016
	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.012)
Observations	1,115	1,115	1,115	1,012	1,012	1,012

Panel F: Boundary Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Avg. Score		KL Divergence		Avg. Score		KL Divergence	
Vote margin > 0	0.063***	0.065***	-0.009	-0.008	-0.030	-0.028	-0.003	-0.004
	(0.021)	(0.021)	(0.008)	(0.008)	(0.020)	(0.020)	(0.006)	(0.009)
Neighboring Students Fixed	N	Y	N	Y	N	Y	N	Y
Observations	1,059	1,059	1,059	1,059	963	963	963	963

Notes: RD estimates are computed using local linear regressions. Non-white candidates are added to the white candidates from the main analysis. The left panels report the estimates for non-Democratic candidates and the right panels report the estimates for Democratic candidates. The optimal bandwidth estimated following Calonico, Cattaneo and Farrell (2020) are 0.10 for non-Democratic and 0.12 for Democratic candidates. Except for columns 1 and 3 in Panel A, all regressions include election year fixed effects, candidate controls (age, sex, and incumbent) and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The urban areas cover Census urbanized areas and urban clusters. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Table C.10: RD Estimates for Republican Candidates

Panel A: House Price Index and School Quality				
	(1) House Price Index	(2) Test Score	(3) School Value-added	(4) Teacher Value-added
Vote margin > 0	0.060***	0.052**	0.000	-0.005
	(0.018)	(0.023)	(0.006)	(0.004)
Observations	1,036	972	972	972

Panel B: Teacher Experience				
	(1) Avg. Exp	(2) Prop. New	(3) Prop.2-9 Yrs	(4) Prop. 10+ Yrs
Vote margin > 0	0.072	-0.010**	-0.007	0.016*
	(0.190)	(0.005)	(0.008)	(0.009)
Observations	972	972	972	972

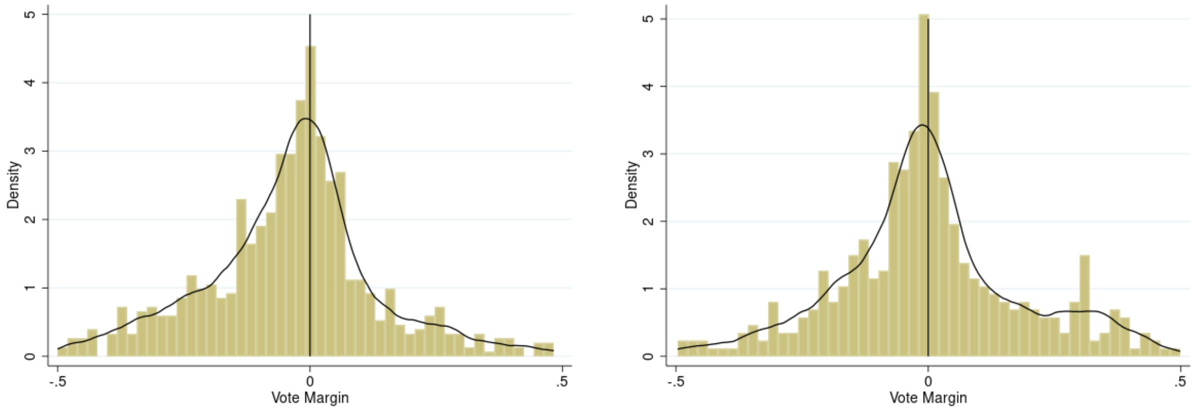
Panel C: Student Composition						
	(1)	(2)	(3)	(4)	(5)	(6)
	Attending Students			Structural Movers		
	Prop. White	Prop. Free Lunch	Prop. High-achieving	Prop. White	Prop. Free Lunch	Prop. High-achieving
Vote margin > 0	0.040***	-0.031**	0.033***	0.017	-0.006	0.040***
	(0.013)	(0.013)	(0.011)	(0.015)	(0.014)	(0.014)
Observations	972	972	972	936	936	936

Panel D: Boundary Change				
	(1)	(2)	(3)	(4)
	Avg. Score		KL Divergence	
Vote margin > 0	0.060***	0.071***	-0.015*	-0.008
	(0.022)	(0.024)	(0.008)	(0.009)
Neighboring Students Fixed	N	Y	N	Y
Observations	877	877	877	877

Notes: RD estimates are computed using local linear regressions. The optimal bandwidth estimated following Calonico, Cattaneo and Farrell (2020) is 0.125. All regressions include election year fixed effects, candidate controls (age, sex, and incumbent) and school district controls including demographic compositions of students (proportions of black, and economically disadvantaged students) and school board members (proportions of black, female, and Democratic members), and the indicator of urban areas. The urban areas cover Census urbanized areas and urban clusters. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Figure C.3: Density of Vote Margin

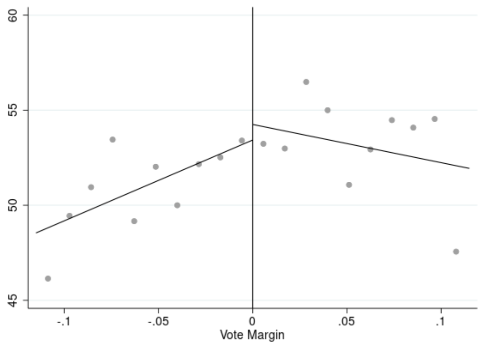
(a) Non-Democratic **(b)** Democratic



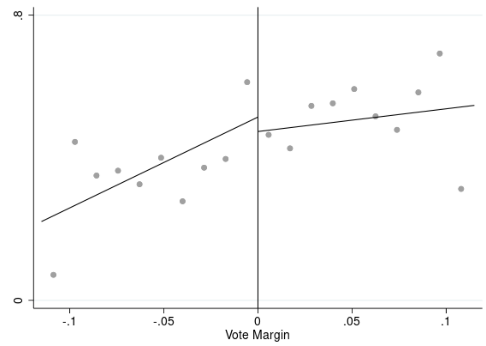
Notes: The figures depict the distributions of vote margin around the cutoff that determines whether a candidate wins for non-Democratic and Democratic candidates, respectively. The x-axis measures vote margin. For candidates successfully elected to the board, vote margin is defined by the difference between their vote share and that of the most popular loser in the contest and is positive. For losing candidates on the other hand, it is computed by the difference between their vote share and the vote share of the least popular winner and is negative.

Figure C.4: RD Plots of Covariates for All Candidates

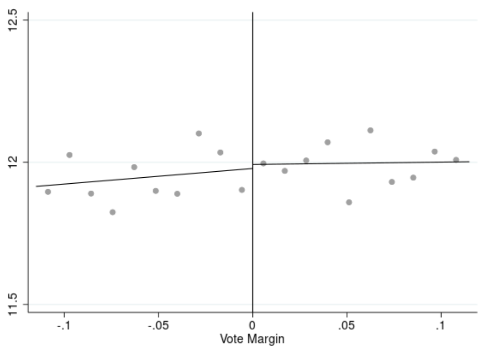
(a) Age



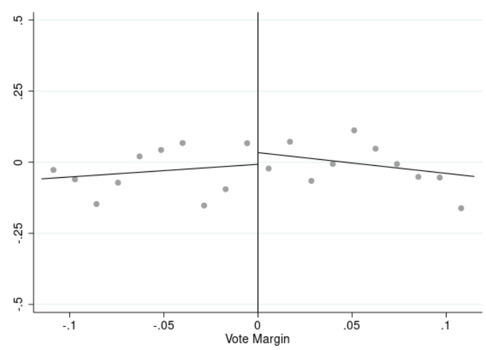
(b) Female



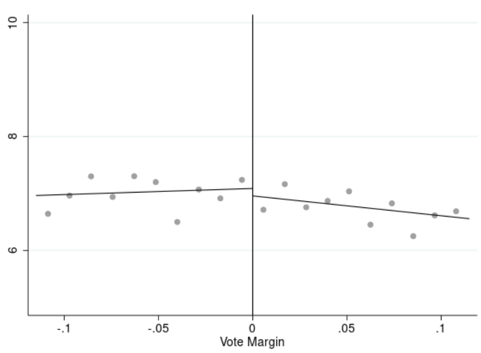
(c) House Price before Elections



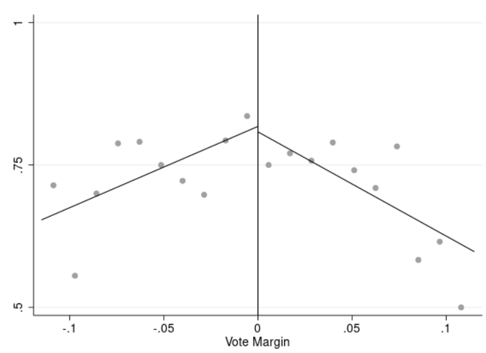
(d) Growth of House Price before Elections



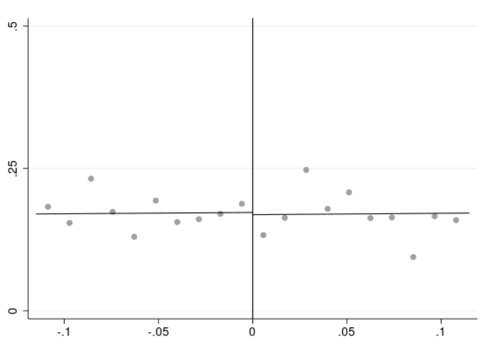
(e) Number of Board Members



(f) At-large Contest



(g) Prop. Black Students



(h) Prop. Students in Free Lunch Program

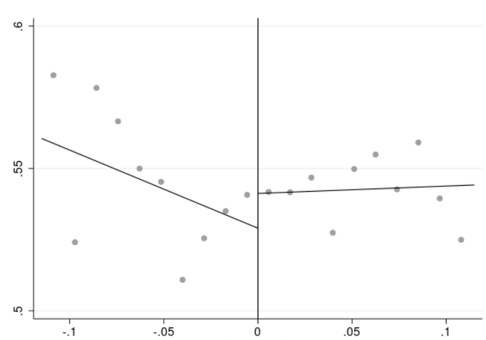
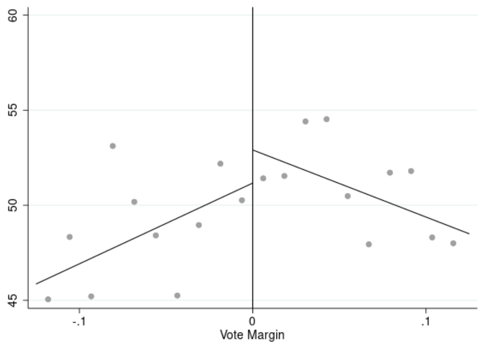
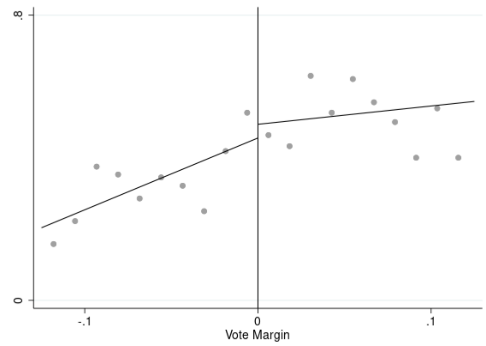


Figure C.5: RD Plots of Covariates for Non-Democratic Candidates

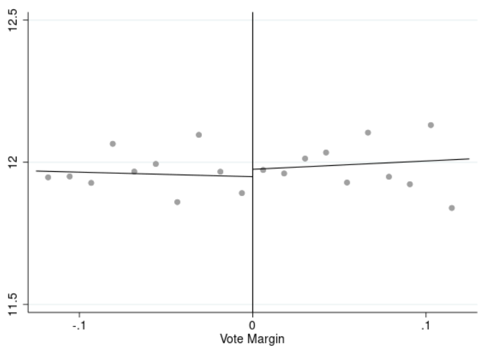
(a) Age



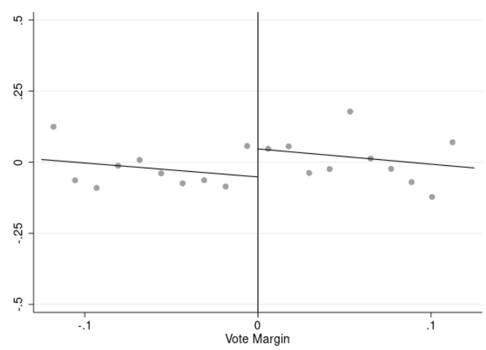
(b) Female



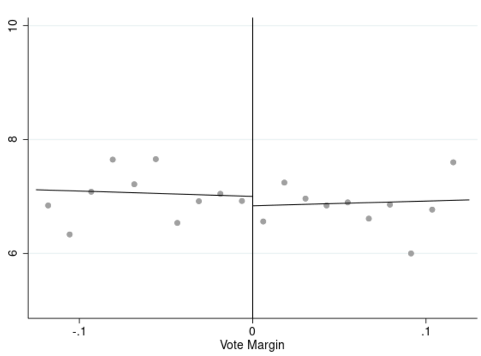
(c) House Price before Elections



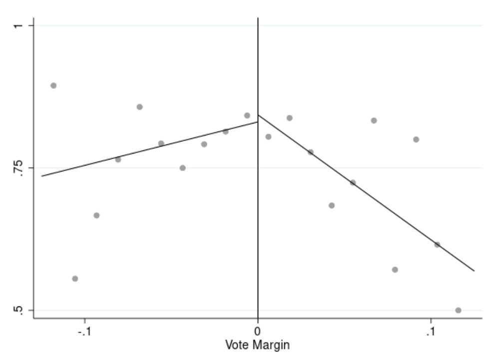
(d) Growth of House Price before Elections



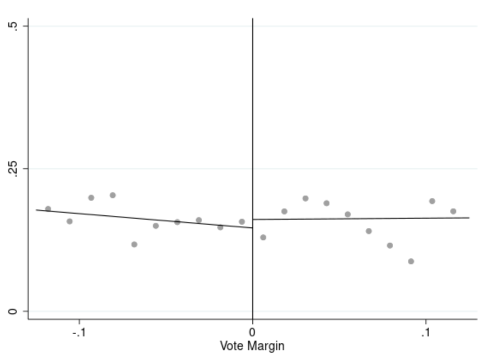
(e) Number of Board Members



(f) At-large Contest



(g) Prop. Black Students



(h) Prop. Students in Free Lunch Program

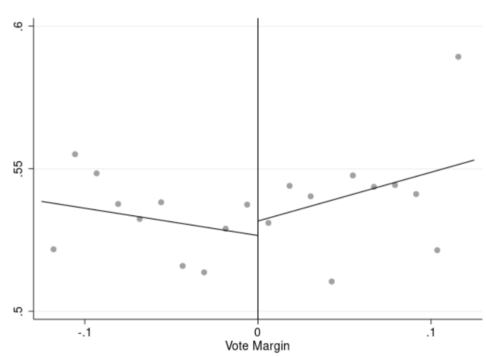
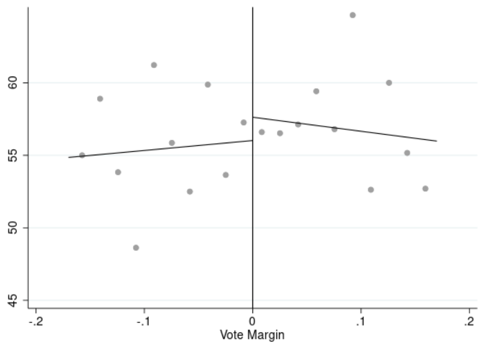
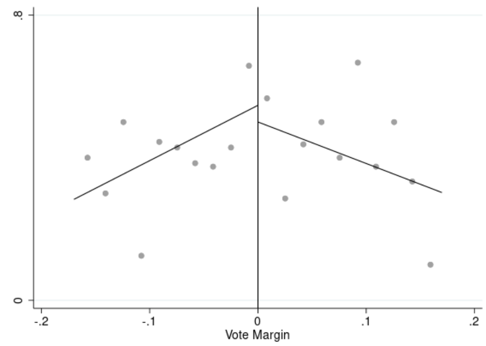


Figure C.6: RD Plots of Covariates for Democratic Candidates

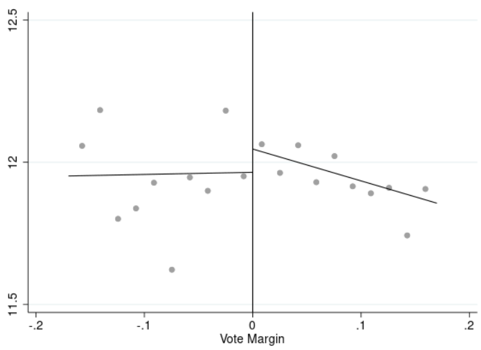
(a) Age



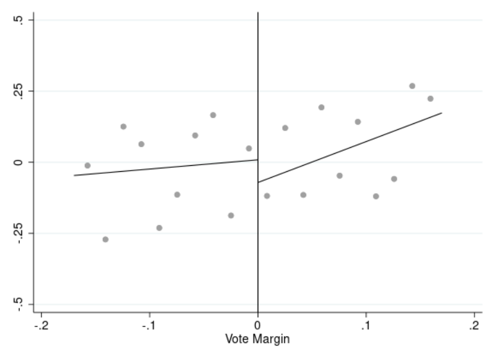
(b) Female



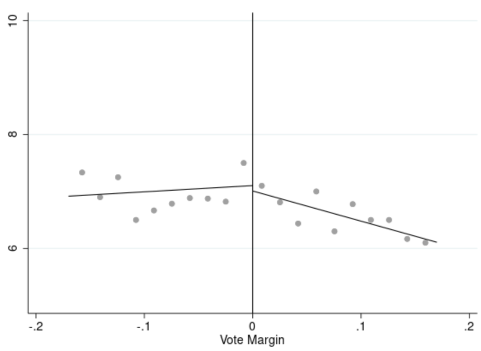
(c) House Price before Elections



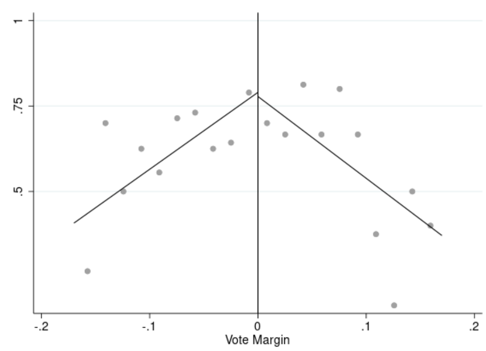
(d) Growth of House Price before Elections



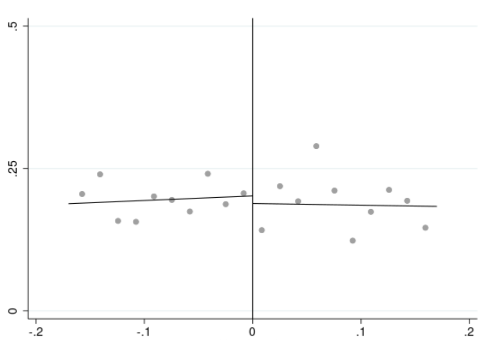
(e) Number of Board Members



(f) At-large Contest



(g) Prop. Black Students



(h) Prop. Students in Free Lunch Program

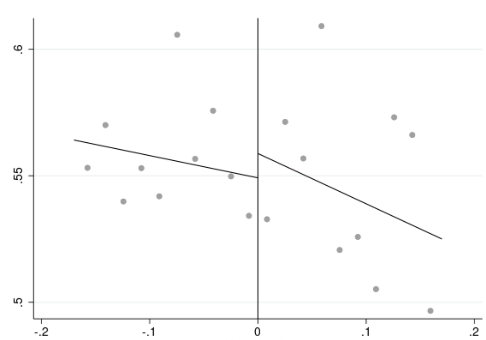
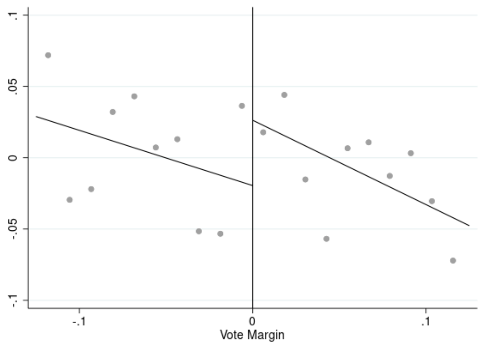
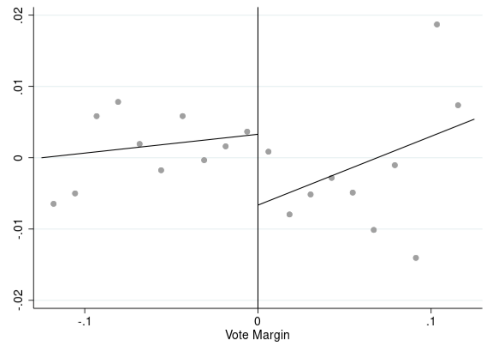


Figure C.7: RD Plots of Main Outcomes for Non-Democratic Candidates

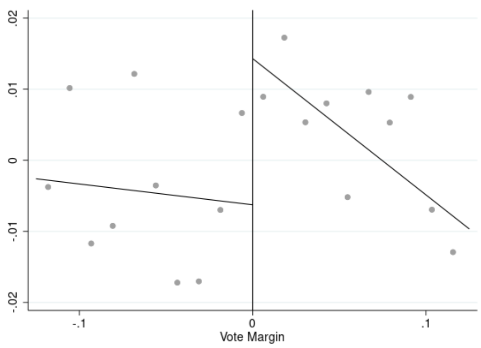
(a) Test Scores



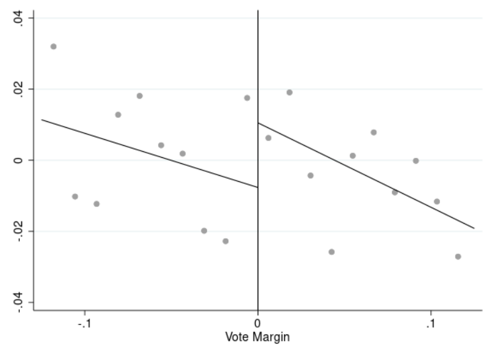
(b) Prop. New Teachers



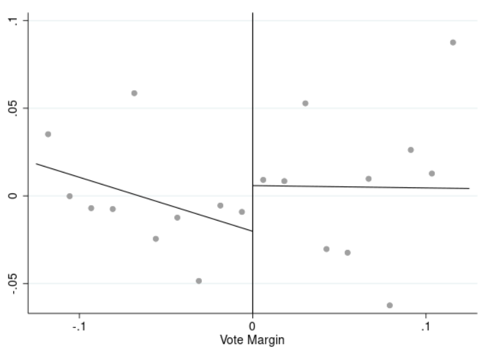
(c) Prop. Teachers w/ 10+ Years of Exp.



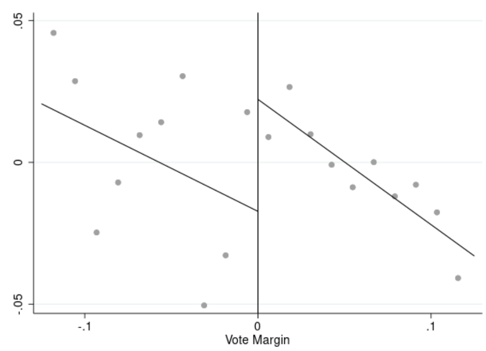
(d) Prop. High-achieving Students



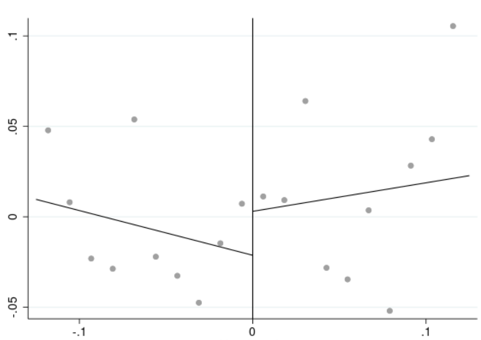
(e) Prop. White Students



(f) Prop. High-achieving Structural Movers



(g) Prop. White Structural Movers



(h) Qualitative Boundary Shift

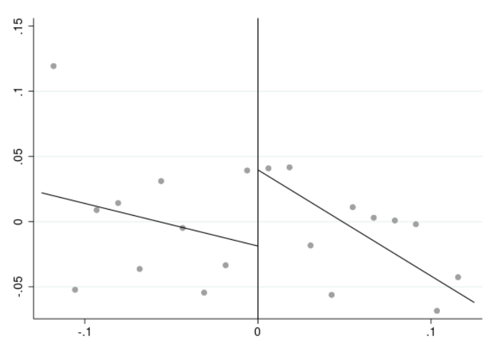
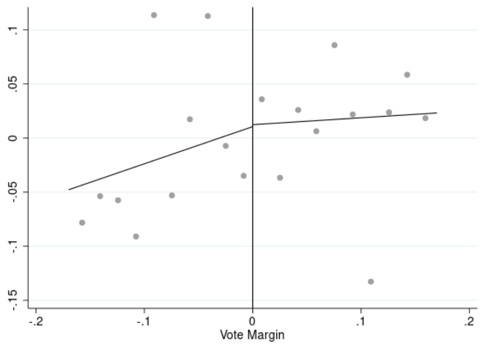
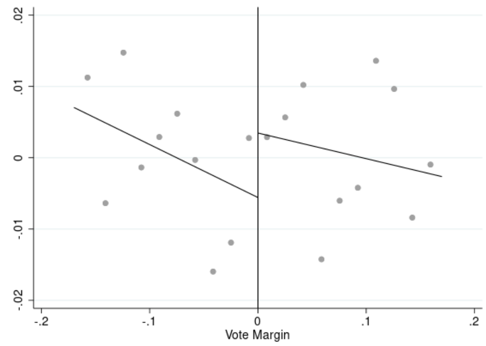


Figure C.8: RD Plots of Main Outcomes for Democratic Candidates

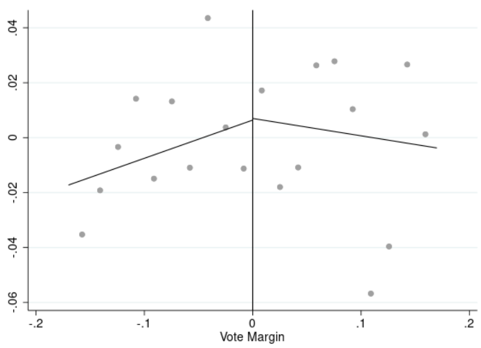
(a) Test Scores



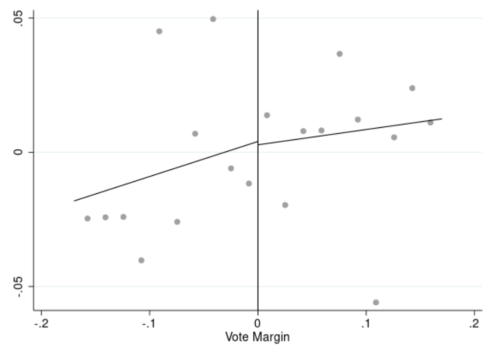
(b) Prop. New Teachers



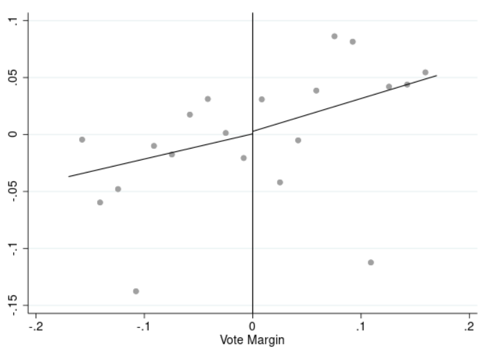
(c) Prop. Teachers w/ 10+ Years of Exp.



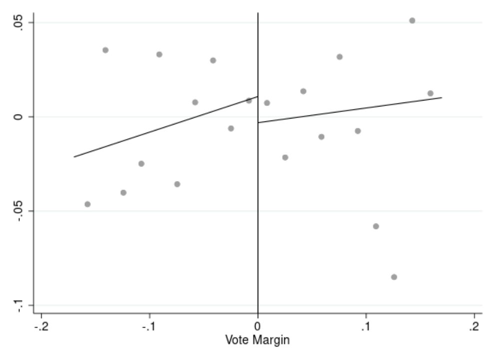
(d) Prop. High-achieving Students



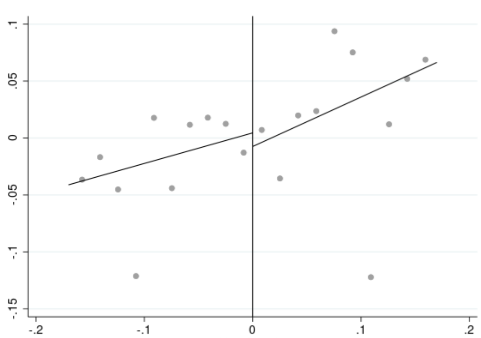
(e) Prop. White Students



(f) Prop. High-achieving Structural Movers



(g) Prop. White Structural Movers



(h) Qualitative Boundary Shift

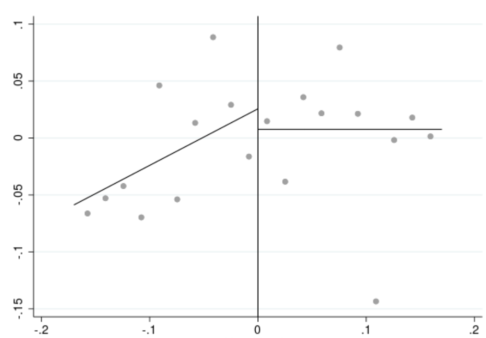
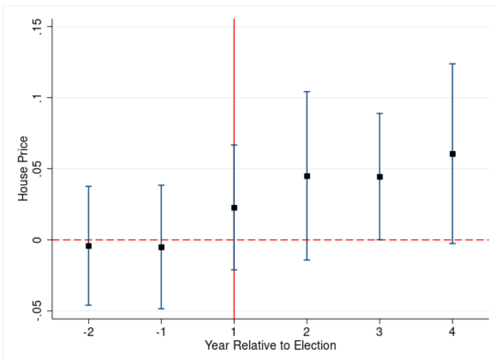
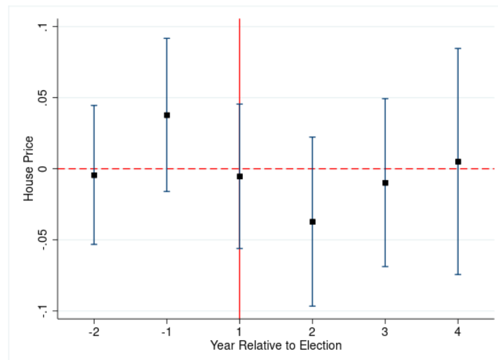


Figure C.9: RD Event Studies of House Price

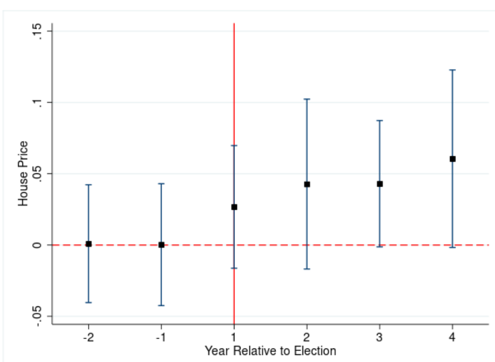
(a) Non-Democratic, w/o Controls



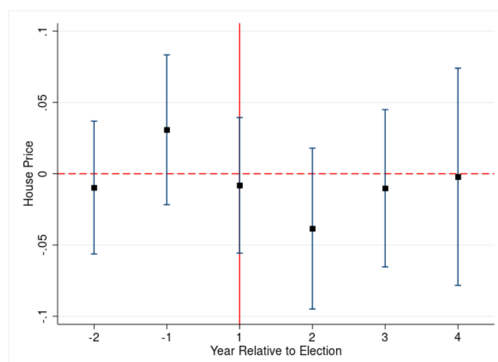
(b) Democratic, w/o Controls



(c) Non-Democratic, w/ Controls



(d) Democratic, w/ Controls



Notes: Period 1 indicates the year of election and each point represents the RD estimate of the house price index in each year relative to the election year with the optimal bandwidths in Table 4. The confidence intervals are at 95%.