NBER WORKING PAPER SERIES

SELECTION INTO PUBLIC SERVICE: EVIDENCE FROM SCHOOL BOARD ELECTIONS

Stephen B. Billings
Hugh Macartney
Geunyong Park
John D. Singleton

Working Paper 29791
http://www.nber.org/papers/w29791

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2022, Revised September 2025

We thank Leah Boustan, Jonathan Guryan, Jacob Krimmel, and participants at University of Rochester, UNLV, the 2020 Urban Economics Association, and the 2021 European Urban Economics Association meetings for helpful comments and discussions. Data provided by Zillow through the Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the authors and do not reflect the position of Zillow Group. Data also provided by the North Carolina Education Research Data Center (NCERDC). All remaining errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2022 by Stephen B. Billings, Hugh Macartney, Geunyong Park, and John D. Singleton. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Selection Into Public Service: Evidence from School Board Elections
Stephen B. Billings, Hugh Macartney, Geunyong Park, and John D. Singleton
NBER Working Paper No. 29791
February 2022, Revised September 2025
JEL No. D72, H75, I24

ABSTRACT

In this paper, we show that the election of a new school board member causes home values in their neighborhood to rise. This effect is identified using narrowly-decided contests and is driven by members with non-Democratic political affiliations. We do not find corresponding evidence for improvements in local school productivity, but show that neighborhood public schools of non-Democratic board members also shift to serving fewer minority students through attendance zone adjustments. In contrast, we detect no differential changes when comparing neighborhood or scholastic outcomes between winning and losing Democratic school board candidates.

Stephen B. Billings
University of Colorado Boulder
Department of Economics
stephen.billings@colorado.edu

Hugh Macartney
Duke University
Department of Economics
hugh.macartney@icloud.com

Geunyong Park
National University of Singapore
park.geunyong@nus.edu.sg

John D. Singleton
University of Rochester
Department of Economics
and NBER
john.singleton@rochester.edu

1 Introduction

School boards in the U.S collectively govern one of the largest public budgets and exercise discretion over decisions—such as drawing attendance zones and allocating resources across schools—that can alter the distribution of educational opportunities and neighborhood amenities. Yet, despite an extensive literature on rent-seeking in higher-level offices, little is known about whether rent-seeking occurs in low-paid, non-partisan settings like local school boards.[1] This gap is important because these institutions are designed to rely on citizen-volunteers motivated by the public good, but the same discretion that can serve district-wide interests can also be used to disproportionately advantage a member's own neighborhood.

In this paper, we apply a standard test for private returns from public office to the novel context of school boards in the U.S. The test compares outcomes, such as wealth, between candidates who narrowly win an election and those who narrowly lose, leveraging the quasi-random assignment of office in close contests to identify causal effects (Eggers and Hainmueller, 2009; Fisman, Schulz and Vig, 2014). Our outcome is a neighborhood-level house price index, which we construct from transaction-level housing data to capture changes in the perceived quality of the winning candidate's neighborhood. This allows us to estimate the neighborhood returns to school board election and, if positive, to investigate the sources of those gains using rich administrative data on students and schools.

We find that election to the school board causes home values in winners' neighborhoods to appreciate relative to election losers' neighborhoods. Moreover, this effect is entirely due to candidates who are not registered Democratic: our main estimate suggests prices in the neighborhoods of marginally-elected non-Democrats increase by as much as 6% on

[1]Other than a few isolated districts (e.g. Los Angeles Unified), Florida stands as the major exception to negligible pay for school board members. Salaries can be up to $50,000 depending on district size. However, recent legislative discussions suggest eliminating or sharply cutting salaries precisely due to concerns that they "skew motivations" for seeking office. In the words of its sponsor, "the focus that I have on this is public service" (https://www.news4jax.com/news/2022/02/02/school-board-salaries-would-be-eliminated-under-florida-house-bill-now-poised-for-debate/). 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).

average post-election. In addition, this effect is highly local; while we detect effects when a neighborhood is defined by the candidate's Census block group, estimates cannot detect appreciation in other block groups belonging to the same Census tract.[2] We show that the increases in the neighborhoods of non-Democratic members cannot be attributed to effects on teacher quality or improvements in school productivity, as measured by test score value-added. Rather, we find that the neighborhood schools of non-Democratic school board members become relatively less minority and less economically disadvantaged on average via changes in the attendance zones of local public schools. 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 North Carolina school board election results from 2006 through 2016 with annual voter registry snapshots. The merged sample includes demographic, political affiliation, and residential information for anyone who ran for a seat on their local school board. We use this dataset to apply a regression discontinuity design based on vote shares to isolate quasi-random variation in whether a candidate wins an election. This empirical approach recovers causal effects by controlling for unobserved differences between school board members and non-members that correlate with outcomes, an assumption we subject to numerous validity checks (e.g. covariate balance within the bandwidth, no bunching). In addition, we stratify the sample to estimate separate effects for non-Democratic and Democratic members, allowing us to compare winners and losers with similar policy preferences. This choice is driven by evidence that public service motivation is correlated with political identity (Ritz, Brewer and Neumann, 2016), which is hidden from voters in school board races.[3]

[2]A Census block group is usually populated with 600-3,000 people and a Census tract is comprised of around three Census block groups on average. North Carolina, the state our data are drawn from, has 2,195 Census tracts and 6,155 block groups across 115 school districts.

[3]Ritz, Brewer and Neumann (2016)'s review of the public administration literature highlights the finding that liberal political ideology predicts self-reported commitment to the public interest and civic duty. In addition, differential motivation is consistent with voter registration records showing that U.S. federal civil servants are more likely to be registered Democratic (Spenkuch, Teso and Xu, 2021). Note that we also investigate (but do not find evidence for) heterogeneity along other observation dimensions of candidates

Our primary causal effects focus on home values in school board members' neighborhoods, which we measure using a price index constructed from transaction-level microdata. The index is built from a panel of year by neighborhood-specific fixed effects, estimated using a hedonic price regression that residualizes the transaction prices with respect to property attributes. By design, these fixed effects capture differences between neighborhoods in perceived local public school quality (Rosen, 1974; Black, 1999; Biasi, Lafortune and Schönholzer, 2025). The responsibility of school boards for a wide range of public school district decisions, including allocating resources across schools and drawing attendance zone boundaries, suggests significant latitude to influence perceptions of neighborhood quality. Given our focus on small geographic areas (we treat Census block groups as neighborhoods), standard hedonic methods can be heavily influenced by outliers and low transaction volumes. We therefore integrate methodologies from the teacher value-added and hedonic literature to obtain the index values using an empirical Bayes approach that shrinks the fixed effects towards priors that depend on the underlying noise in the point estimates.[4] Our results are robust to decisions made in building the index and, importantly for our design, we also show there is no evidence of differences in neighborhood prices between narrow winners and losers *before* the school board election (i.e., no placebo effects).

The positive impact of winning a school board election on home prices is consistent with school quality (or its perception) increasing in the neighborhoods of school board election winners. 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 use these data in two ways: First, using geocoded student addresses, we construct measures of student composition by neighborhood as well

(e.g. gender, age).

[4]Morris (1983) shows 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 (see Kane, Rockoff and Staiger 2008, Chetty, Friedman and Rockoff 2014, and Jackson 2018*a*, among others).

as track student moves across neighborhoods over time. Second, we associate a local elementary and middle school with each school board candidate based on the schools attended by students residing in the candidate's same block group at the time of the election. This allows us to characterize how candidates' neighborhood schools change due to winning a school board seat. We use the panel of student end-of-grade exams in the data to generate school-level estimates of school and teacher quality and construct school-level variables summarizing student demographics over time. In addition, we use the existing attributes of the neighborhoods resided in by students attending each candidate's neighborhood schools to capture qualitative changes in attendance zone boundaries.

We find that the housing value appreciation in non-Democratic school board members' neighborhoods is not associated with differential changes in test scores, teacher attributes, or school quality, as measured by value-added. Rather, we show that the neighborhood public schools of non-Democratic members become relatively less minority, an effect which is achieved by influencing which neighborhoods the local public schools draw students from. The mechanism—attendance zone adjustments—and partisan heterogeneity we find connects with Macartney and Singleton (2018)'s finding that Democratic school board members reduce student segregation (relative to non-Democratic members), but the results in our paper distinctly speak to whether and how those policy impacts are distributed across the district. Our findings that non-Democratic—but not Democratic—school board members affect local school attributes in ways that benefit neighborhood home values are consistent with between-party differences in the balance between public versus private returns emphasized in models of political selection (Besley, 2005).

Our analysis is connected to classic models of political office, in which politicians maximize their self-interest subject to constraints (Barro, 1973; Buchanan, 1989). Methodologically, our paper is most directly related to prior work that similarly uses electoral discontinuities to estimate elected leaders' private returns from office (e.g., Eggers and Hainmueller 2009; Querubin and Snyder 2011; Fisman, Schulz and Vig 2014). We adapt this research design to

the setting of U.S. school boards.[5] Importantly, we quantify impacts that immediately follow an election (i.e., during the winner's term) and examine associated effects on comprehensive aspects of local public schooling—the sole policy domain at issue. Our use of neighborhood home values connects with work that infers rent-seeking from connections to politicians (e.g., Fisman 2001).[6] Our paper is also related to work that measures the importance of public service motivations for seeking office, such as Barfort et al. (2019), which presents experimental evidence of pro-social motivations among Danish public servants. Other prior work has studied political selection using wage variation in diverse settings (e.g., Ferraz and Finan 2009; Kotakorpi and Poutvaara 2011; Gagliarducci and Nannicini 2013).

Our paper also contributes to a growing body of work aimed at quantifying whether and how school boards matter. That, despite evidence of limited or weak electoral accountability (e.g., Holbein 2016; Kogan, Lavertu and Peskowitz 2021), we find neighborhood representation can be capitalized in home values, which suggests what the school board does matters to local voters (Carlson et al., 2025). A major strand of the recent literature attempts to infer quality governance by determining how education outcomes change when the observed makeup of the school board changes in a quasi-random fashion (e.g., Shi and Singleton 2020; Fischer 2023). Christian, Jacob and Singleton (2022) instead analyzes decision patterns of school boards with the same motivation. Our paper uniquely sheds light on school board candidates' unobserved motivations for seeking office, which has implications for policies that switch to partisan races (in which voters could more easily perceive a candidates' type) or that change compensation of members.

[5]Prior work similarly considers heterogeneity in party terms: Eggers and Hainmueller (2009) estimates effects separately among Conservative and Labour UK Parliament members, while Fisman, Schulz and Vig (2014) considers heterogeneity based on belonging to the ruling party.

[6]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 and Data

School boards in the U.S. are intended to keep the "public" in public education. School board members, traditionally lay citizens elected by local voters in non-partisan contests, represent the largest group of elected officials in the country. Around 90,000 board members serve on approximately 15,000 boards. 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 budget allocation and oversight. These responsibilities imply that school boards can potentially influence perceived neighborhood quality via several channels.

2.1 Data Sources

Our empirical analysis draws on five main data sources: (1) publicly available school board election results; (2) voter registration records; (3) house transaction records; (4) school and student data; and (5) neighborhood 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 between 2000 and 2018 inclusive.[7] On average, a school board election has 3.6 candidates, 1.3 winners, and a contest winner receives about 40% of the votes. 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 (ward-based contest).[8] About 74% of the school board contests are ward-based, with such contests having a smaller number of candidates than at-large contests (2.8 candidates vs. 5.8 candidates). While several North Carolina

[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 school districts. We also manually collect information about the number of winners from school board rosters when not reported by the State Board of Elections.

[8]A ward is a local authority area used for electoral purposes. A school district holding ward-based elections has 4 wards on average, and a ward covers 16 block groups on average, while a school district holding at-large elections covers 56 block groups on average.

districts switched to partisan school board elections during our sample period, the vast majority (about 87%) of contests that we observe are non-partisan (i.e. candidates' party affiliation is not listed on the ballot), typical of races for school board nationwide.[9]

To identify candidate characteristics, we link the election records with North Carolina voter registration annual snapshots (from 2005 through 2018). 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 practically impossible to match all candidates to the voter registration database perfectly, we link the databases using a within-county fuzzy match based on name and county. We collect only unique matches to construct our baseline sample, thereby minimizing measurement errors that could arise from incorrect matches. While we could increase the matching rate by including non-unique matches, this method allows us to achieve a unique match for approximately 65.1% of the entries in the linked election records and the North Carolina voter registration database.[10]

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 (lower than $30,000 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

[9]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.

[10]The detailed matching procedure is presented in the appendix. Our appendix also provides results that demonstrate the robustness of our later findings to a matching procedure that allows for non-unique matches. Moreover, Appendix Figure A.1 and Appendix Table A.2 show that the likelihood of matching to the voter database is not discontinuous at the threshold.

footage is too large/small to be accurate).

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, school attendance zone, block group of residence, academic achievement, and economically disadvantaged status. We use these records to summarize characteristics of candidates' neighborhood public schools (e.g., student composition, teacher experience, test scores) as well as to consider student residential sorting and determine attendance zone changes.[11] 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.

Lastly, we also obtain population, median income, and racial composition information across Census block groups from IPUMS NHGIS (Integrated Public Use Microdata Series National Historical Geographic Information System), which are constructed based on 5-year ACS (American Community Survey) data for 2010-2014. We use this information to control for the characteristics of the neighborhood in the estimation and to capture qualitative changes in school attendance zones, the construction of which is explained in the following subsection.

[11]The NCERDC data is recorded according to the academic year, which means that the timing of elections does not align perfectly with the timing of educational records. We assume that the school data collected after the academic year is recorded post-election. For instance, the NCERDC data for the 2015/2016 academic year represents the first set of records collected after the elections that took place in 2015. In our data, about 30% of the elections were held in March and May, and the remainder typically takes place in November.

2.2 Variable Construction

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

2.2.1 Neighborhood House Price Index

We generate a neighborhood house price index that captures differences in perceived quality between neighborhoods using two main steps: First, we estimate a hedonic-style regression to obtain estimates of prices by Census block group (neighborhood) and year net of differences in house and property attributes. 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 per sqaure foot of house i in Census block group j in year t, X_{it} includes housing attributes (e.g., bedrooms, bathrooms, stories, square footage, age of home, and lot size), and π_{jt} is a year-by-block group fixed effect.[12]

Second, we apply an empirical Bayes shrinkage adjustment to the residualized block group fixed effects ($\hat{\pi}_{jt}$). This adjustment is made to address concerns about precision, which is affected by the number of transactions and the variance in home attributes of sold properties. The results in Section 4.2 show that this issue is salient. The final 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 Census block group fixed effects among group c in year t. The group c indicates the (larger) geographical areas, such as counties, over 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

[12]The exact specification includes a cubic polynomial in the house characteristics as well as the year of the transaction, t, which allows for the effects to vary over the sample.

in block group j in t, with $\hat{\sigma}^2_{c(j)t}$ representing the estimated variance of the neighborhood-level prices within group c in year t and $\hat{\lambda}_{jt}$ representing the estimated variance of $\hat{\pi}_{jt}$.[13] Intuitively, the estimate shrinks imprecise fixed effects towards a group c-level prior. In our context, we want a prior that captures a larger, higher transaction volume housing market, but that still reflects trends in the local housing market. In our main results, we use counties (there are 100 in North Carolina), but we show that the results are robust when using Census tracts rather than counties.

2.2.2 Neighborhood Schools

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 Census block group in the year that the candidate ran for school board and define the elementary/middle schools where the largest share of the neighborhood students attend in the year as the candidate's "neighborhood schools." The neighborhood schools cover 76.3% of neighborhood students on average in our sample. A school board member may target the neighborhood schools if she would like to improve the educational environment in her neighborhood. So, we investigate how the attributes of the neighborhood schools change after school board elections. This variable definition allows us to separate out direct impacts on neighborhood schools from changes in school assignments or student sorting across neighborhoods, which may also be affected by a candidate's election.

To understand the role of school attendance zone shifts by the school board, we also construct qualitative measures of the shift in school attendance zones.[14] Let X_{jt_0} represent

[13] Note that if a Census block group has a unique house transaction in a year, we exclude it from the sample.

[14] A quantitative measure of the change in a boundary shift is not sufficient to conclude anything meaningful about benefits to election winners' neighborhood schools since boundary changes essentially alter the attendance zone of other schools which may include the neighborhood school of election losers—the control group.

a characteristic of block group j in the base year t_0.[15] The measure of boundary shift for candidate i's neighborhood school in year t with respect to the characteristic X is given by the following change in the weighted average:

$$Shift(X)_{it} = \sum_{j \in J(i,t)} w_{jt} X_{jt_0} - \sum_{j' \in J(i,t_0)} w_{j't_0} X_{j't_0} \tag{3}$$

where $J(i,t)$ is the set of Census block groups in which students attending a neighborhood school of candidate i reside in year t. w_{jt} represents the share of the students attending a neighborhood school of candidate i at time t who reside in block group j. Formally, $w_{j(i)t} = \frac{N_{ijt}}{N_{it}}$ where N_{ijt} denotes the number of students who reside in j and attend the neighborhood school of candidate i at time t and N_{it} is the number of students who attend the neighborhood school of candidate i at time t. Note that we fix the characteristic of each block group at the election year, so this measure reflects the change in the composition of block groups sending students to the school solely through enrollment changes (a proxy for school boundary shifts).

2.3 Descriptive Statistics

In this subsection, we present descriptive statistics and summarize key patterns in the data. Table 1 reports the characteristics of the winners and losers of all non-minority candidates who are matched with the voter registration in our sample.[16] Panel A reveals statistically significant disparities in age, gender, political affiliation, and election year between winners and losers in general, which may influence the candidates' preferred policies. These findings indicate that a naive post-election comparison of winners and losers could reflect confounding factors, underscoring the need for an empirical strategy to identify the causal effects of the

[15] We use the values at the year of election for student characteristics and the values in 2010-2014 for neighborhood characteristics from NHGIS.

[16] We define non-Hispanic White candidates as "non-minority" throughout this paper. Our main analysis is limited to non-minority candidates due to a limited sample of non-Democratic minority school board members and to control for heterogeneity in policy differences between racial groups. Later robustness checks highlight that our main results are unchanged with the inclusion of minority candidates.

Table 1: Descriptive Statistics

	Winners Mean	Losers Mean	Difference t-statistic
Panel A: Candidate Characteristics			
Age	48.71	46.46	3.98
Prop. Female	0.49	0.35	6.07
Prop. Democratic	0.45	0.35	4.79
Prop. Republican	0.46	0.48	0.49
Prop. Unaffilated	0.08	0.18	6.22
Prop. Incumbent	0.35	0.09	14.57
Prop. At-large Contest	0.45	0.56	4.71
Election Year	2012.12	2011.67	2.18
Obs.	905	997	1902
Panel B: Neighborhood Characteristics			
Median Income	52849	53458	0.51
Prop. College Graduates	0.36	0.34	1.42
Avg. House Price	183236	174446	1.52
House Price Index	0.07	0.02	1.69
Prop. Urban Areas	0.46	0.45	0.18
Obs.	614	714	1328
Panel C: School Characteristics			
Standardized Score	0.03	0.02	0.21
Prop. Black Students	0.18	0.19	0.33
Prop. Hispanic Students	0.11	0.12	1.00
Prop. White Students	0.65	0.63	1.46
Prop. EDS Students	0.56	0.56	0.51
Obs.	608	668	1276

Notes: This table reports the summary statistics of school board candidates. Column 3 reports the t-statistics of the differences in the characteristics between election winners and losers. In Panel B, median income and share of college graduates are for the period of 2010-2014 from IPUMS NHGIS. The average house price and median income are in 2010 inflation-adjusted dollars. In Panel C, each observation is the neighborhood school for a candidate. Economically disadvantaged students are defined by students in the free lunch program.

election results.

Panel B of Table 1 additionally provides 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, expressed in 2010 inflation-adjusted dollars. We normalize the house price index from equation 2 by subtracting the average of the index, allowing it to be interpreted as the percentage gap in home values relative to the average.

To gain insight into the socioeconomic status of candidate neighborhoods, we also report the median income and the share of college graduates at the block group level for the years 2010-2014, from IPUMS NHGIS. The panel indicates that neighborhood characteristics, including income levels, educational backgrounds, house prices, and urbanicity, are generally not statistically different between winning and losing candidates. Note that the gap in the house price index is statistically significant at the 90% significance level. This suggests a potential disparity in neighborhood quality between non-Democratic winners and losers. Panel C presents the summary statistics for the neighborhood schools of the school board candidates. We construct the normalized scores of end-of-grade tests and the racial composition of the student body at the neighborhood school where the largest share of the neighborhood students in a candidate's Census block group attend in the year of the election. We do not find any statistically significant differences in the normalized scores or in the composition of students between the neighborhood schools of winning and losing candidates.

3 Research Design

We are interested in the causal effects of a candidate's election to a school board. To estimate these effects, our approach is to use an electoral regression discontinuity (RD) design to compare winning with losing candidates. In this section, we describe our research design leveraging narrowly-decided contests in detail and then present validity checks of the key assumptions.

3.1 Empirical Specification

To identify candidate-level impacts of election to the school board, we adopt an RD design based on vote shares that compares outcomes between narrow winners and narrow losers. Because vote shares are difficult to manipulate, crossing the margin between winning and losing can be treated as quasi-random (Lee, Moretti and Butler, 2004). Our approach follows

Eggers and Hainmueller (2009) and Fisman, Schulz and Vig (2014), who use similar designs to estimate private returns from public office, but we adapt it to the multi-candidate, often multi-winner, setting of school board elections.

For each candidate i (running for a school board seat in calendar year $t(i)$), we let x_i represent their vote margin, which is the running variable in the RD design. 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, x_i is computed as the difference between their vote share and the vote share of the least popular winner, making it negative. In the school board context, nearly all races have more than two candidates and many elect multiple members, so $x_i i$ is defined for all candidates in a contest.

We are interested in estimates of:

$$Y_i = \alpha + \beta D_i + \gamma_1 x_i + \gamma_2 D_i \cdot x_i + \gamma_3 Z_i + \epsilon_i \tag{4}$$

within the MSE-optimal bandwidth (Calonico, Cattaneo and Farrell, 2020), where Y_i represents an outcome associated with candidate i and $D_i = 1(x_i > 0)$ indicates whether i won the election. We construct Y_i by averaging a given outcome variable over up to four post-election years (the term length) and then differencing out the average over the same pre-election horizon, which improves precision.[17] The coefficient β 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 assignment to office is discontinuous at $x_i = 0$. Z_i represents pre-determined covariates—candidate demographics (age, sex, and race), pre-election demographic composition of the school board and students in the district in the base year, an urban area indicator, and election-year fixed effects—included in our preferred specification for increasing precision. Standard errors are clustered at the election contest level to allow for the correlation in outcomes between candidates in the same

[17] We show that there is no discontinuity in average house price levels prior to the election in Table 2.

election.[18]

In estimating equation (4), we include all candidates in the sample who are successfully matched to the voter database, meaning whose residence (and, hence, neighborhood home prices) we observe. This retains valid observations from candidates who are near the margin in a statistical sense, even if they were not literally the most marginal winner or loser. This choice, which differs from designs that restrict only to the top and bottom finishers, increases statistical power. We later show our main findings are robust to focusing solely on the most marginal candidates.

Before presenting results, we conduct standard RD validity checks: (i) testing for manipulation of vote shares; (ii) testing for apparent effects at placebo thresholds; (iii) testing for discontinuities in observed covariates; and (iv) placebo tests using pre-election outcomes. Following our main findings, we examine robustness to the bandwidth choice, construction of the price index, and to including fixed effects that remove comparisons between winning and losing candidates from different school districts and election years.

To examine heterogeneity, particularly by political affiliation, given evidence on differences in public service motivation, we stratify the sample and re-estimate the RD in equation (4) separately for Democratic and non-Democratic candidates (with bandwidths chosen separately). Parallel validity, placebo, and robustness checks are performed for each subsample.

3.2 Validity Checks and Placebo Tests

Before presenting the main results, we first conduct validity and specification tests for our regression discontinuity design. A discontinuity in vote share around the threshold or a concentration of candidates on one side of the cutoff would indicate a violation of the non-manipulation assumption (Imbens and Lemieux, 2008). Appendix Figure A.3 illustrates the continuity of vote density among candidates around the vote margin threshold. The shaded area indicates the confidence interval for the kernel density estimate. From this figure, we

[18]We estimate local linear regressions, and use the uniform kernel function and MSE-optimal bandwidth selector. Observations are not weighted.

Table 2: Balance Checks

Panel A: Candidates Characteristics at the Time of Election							
	(1) Age	(2) Female	(3) Incumbent	(4) Price Index Level	(5) Log Price Level	(6) Log Price Change	(7) Log Median Income
Vote margin > 0	0.357	-0.018	0.084	-0.041	-0.021	-0.027	-0.031
	(1.551)	(0.072)	(0.059)	(0.030)	(0.091)	(0.024)	(0.052)
Panel B: Contest Characteristics							
	(1) Contest At-large		(2) Log Total Votes		(3) # Winners		(4) General Election Date
Vote margin > 0	0.025		0.161		0.237		-0.026
	(0.062)		(0.213)		(0.225)		(0.047)
Panel C: School District Characteristics at the Time of Election							
	(1) Share(Black Students)	(2) Share(Econ. Disadv. Students)	(3) Share(Urban Area)	(4) # Board Members	(5) Share(Black Board Members)		
Vote margin > 0	-0.011	-0.010	-0.024	-0.145	0.026		
	(0.022)	(0.023)	(0.073)	(0.304)	(0.050)		

Notes: RD estimates are computed using a local linear regression. The bandwidths are set at the optimal level of the main analysis in Table 4. 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. "Price Index Level" is the level of price index value from equation 2 for one year prior to the elections. Similarly, "Log Price Level" indicates the log of average house prices at the block group level one year prior to the elections. "Log Price Change" indicates the change in the log of average house prices during the four pre-election years. The median income at the Census block group level is from IPUMS NHGIS 2010. In Panel 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 as those who are in the free lunch program. Column 3 indicates the estimates of the indicators of urban clusters and urbanized areas.

do not observe any discontinuity in the vote margin density with all candidates. In addition, Appendix Table A.3 provides the p-values from the non-parametric test of the smoothness assumption suggested by Cattaneo, Jansson and Ma (2020). The null hypothesis states that the probability density functions of the vote share from both sides of the cutoff are identical. Appendix Table A.3 shows high p-values, indicating that we cannot reject the hypothesis that the vote share density is smooth around the threshold.

Table 2 presents RD estimates of discontinuities in covariates at the vote margin, serving as balance checks. Discovering a discontinuity in a covariate raises concerns about the identifying assumption underlying the RD design. We utilize the optimal bandwidth from

the main RD designs of house prices and account for election-year fixed effects. Panel A of Table 2 displays RD estimates related to candidate-level characteristics. The first three columns indicate that election winners are not statistically more likely to be older, female, or incumbent school board members than election losers. "Price Index Level" in column 4 is the level of price index value from equation 2 for one year prior to the elections. The dependent variable in column 5 is the log of average house prices at the block group level one year prior to the elections, and that in column 6 is the change in the log of average house prices during the four pre-election years. The RD estimates suggest no significant differences in housing values between the communities of winners and losers around the threshold of the vote margin. Similarly, we do not observe any discontinuity in the median income levels of the Census block groups for winners and losers, as shown in the last column.

Panel B of Table 2 checks for discontinuities in the characteristics of school board contests. The RD estimates presented in the first three columns show that the indicator for at-large contests, the logarithm of total votes in a contest, and the number of winners are all balanced. Additionally, it is well-known that voter turnout in off-cycle local elections tends to be lower because voters have less incentive to participate. The last columns indicate that election winners are not more likely to have run in off-cycle elections. Therefore, our findings are not influenced by differences in electoral systems or by policy preferences related to the characteristics of school board elections.

Maintaining a balance of district characteristics is also crucial since our RD design compares the local neighborhoods of election winners and losers. Panel C of Table 2 presents the balance checks for these district characteristics. Columns 1 and 2 show the effects of winning a school board election on the demographic composition of students, specifically the proportions of Black and economically disadvantaged students within a school district. The third column examines whether the winners are disproportionately more likely to come from urban districts compared to the losers. The dependent variables in the last two columns are the number of school board members and the demographic composition of incumbents, with

Table 3: Placebo Effects of Election on Past Neighborhood Home Prices

	(1)	(2)	(3)	(4)
	Price Index	Price Index	Log Price	Log Price
Vote margin > 0	0.014	0.006	0.024	0.013
	(0.028)	(0.027)	(0.031)	(0.029)
Optimal BW	0.168	0.146	0.157	0.150
Obs. within BW	801	740	768	752
Obs.	1,395	1,395	1,395	1,395
Controls	None	Full	None	Full

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four *pre-election* years and subtracting the average over the four years *preceding* the four pre-election years. The outcome variable is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level following Calonico, Cattaneo and Farrell (2020). 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) for columns 2 and 4. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

a focus on the proportion of Black school board members. All variables at the school district level show no significant discontinuity at the vote margin.

In addition to these balance 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. Specifically, in Table 3, the outcome variables are calculated by taking the average of the house price index in equation 2 (columns 1 and 2) or the average of the log of house price (columns 3 and 4) up to four *pre-election* years and subtracting the average over the four years *preceding* the four pre-election years. This method of constructing variables is consistent with the approach used for post-election outcomes in the main analysis in section 4. We cannot observe a statistically significant discontinuity between the block groups of election winners and losers. The results are robust to the inclusion of covariates, including 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. These results suggest that the block groups of winners and losers are homogeneous around the threshold prior to the election.

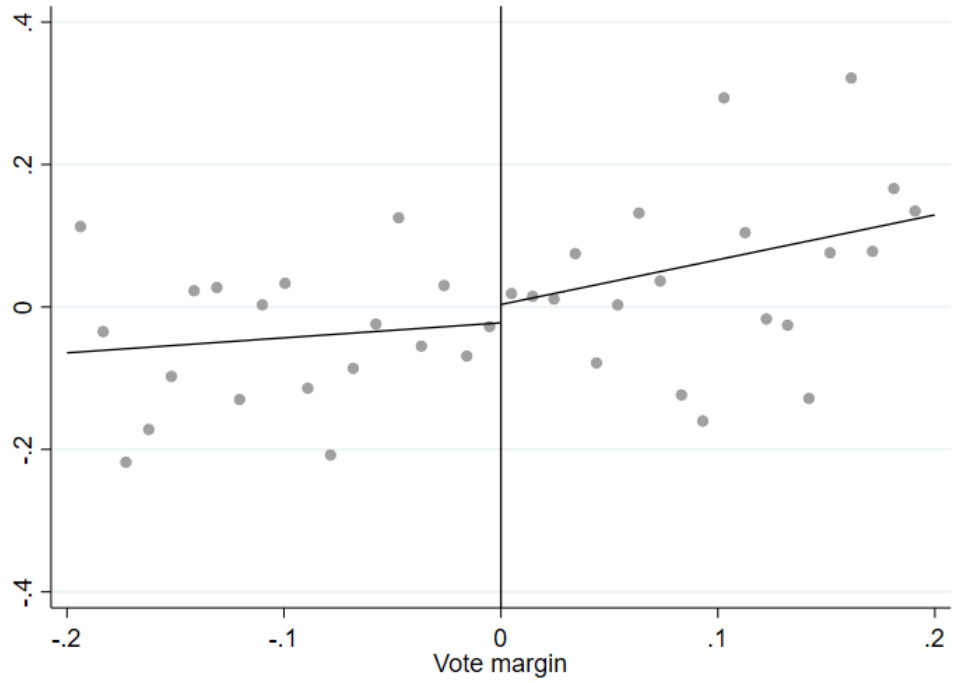
Since we also conduct heterogeneity analysis by political partisanship, we report the parallel summary statistics and validity and specification tests for each political identity: Democratic and non-Democratic candidates. Appendix Table A.4 reports candidate, neighborhood, and school characteristics for non-Democratic and Democratic candidates, separately. As for the pooled sample, we observe substantial disparities in these attributes between winning and losing candidates within each political group. However, the results of balance checks in Appendix Table A.5 and Appendix Figures A.5 and A.6 confirm that the attributes are not statistically different between winners and losers within the optimal bandwidth from the RD designs of house prices. Appendix Table A.3 and Appendix Figure A.7 confirm the continuity of vote density around the vote margin threshold for both non-Democratic and Democratic candidates. In Table A.6, we cannot observe a statistically significant discontinuity of the change in house price *prior to* the elections within each political group.

4 Neighborhood 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 perceived quality of winners' neighborhoods. 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 1 shows visual evidence of the causal effect of winning a school board election on the house price index in the winner's neighborhood. We report the RD plots with linear fits and bandwidth of [-0.2, 0.2]. The figure illustrates a discontinuous increase in home prices within the block group of election winners relative to home prices within the block group of

Figure 1: Effect of Election on Neighborhood Home Prices



Notes: The y-axis is the change in the home price index, and the x-axis measures the vote margin. We report the RD plots of the house price index with linear fits and bandwidth of [-0.2, 0.2]. We set equally spaced bins for both sides of the threshold, and each point indicates the average of the outcome 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.

election losers.

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, relative to the loser's neighborhood. As reported in column 1, we find that home prices in the block group of an election winner increase by around 5.2% relative to prices in a loser's block group during four years after the election – the term of a school board member – from the RD specification without controlling for covariates. Column 2 adds 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

Table 4: Effects of Election on Neighborhood Home Prices

	(1)	(2)	(3)	(4)
	Price Index	Price Index	Log Price	Log Price
Vote margin > 0	0.052*	0.034	0.064**	0.038
	(0.029)	(0.024)	(0.031)	(0.028)
Optimal BW	0.108	0.126	0.123	0.117
Obs. within BW	607	657	643	625
Obs.	1,295	1,295	1,295	1,295
Controls	None	Full	None	Full

Notes: RD estimates are computed using local linear regressions. The outcomes are calculated by taking the averages of the house price index in equation 2 (columns 1 and 2) and of the log of block-group-level house prices (columns 3 and 4) up to four post-election years (the term length) and subtracting the averages over the same pre-election horizon. The bandwidths are set at the MSE-optimal level following Calonico, Cattaneo and Farrell (2020). 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) for columns 2 and 4. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

member demographics/characteristics controls (proportions of black, female, and Democratic members), an urban indicator, and election year fixed effects. The RD estimate of the price effect becomes smaller and statistically insignificant when these controls are included. In columns 3 and 4, we also present the parallel effects on the log of the house price, rather than the house price index. This allows us to evaluate whether the Bayes shrinkage adjustment is responsible for the observed results. The findings indicate that the point estimates are consistent with those derived from the house price index. Therefore, we do not find robust evidence that the election winners receive disproportionate benefits in terms of neighborhood home prices.

The above results raise the question of how localized the impacts of a winner on neighborhood public good quality are. For example, school board members may benefit a larger neighborhood, such as a Census tract, or other nearby block groups, which could be capitalized into the home values there. [19] To examine this, in Table 5, we estimate the causal

[19] A Census tract is a collection of multiple block groups (2.78 block groups on average) and contains less than 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.

Table 5: Effects of Election on Nearby Neighborhood Home Prices

	(1)	(2)	(3)	(4)
	Census Tract	Census Tract	Leave-one-out	Leave-one-out
Vote margin > 0	0.032	0.008	0.025	0.007
	(0.025)	(0.019)	(0.029)	(0.023)
Optimal BW	0.115	0.122	0.121	0.122
Obs. within BW	619	637	621	624
Obs.	1,295	1,295	1,246	1,246
Controls	None	Full	None	Full

Notes: RD estimates are computed using local linear regressions. The outcome in columns 1 and 2 is calculated by taking the average of the Census-tract-level house price index up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. We use the average of the block-group-level house price indices at the Census tract level, excluding the house price index of the candidate's own block group, when constructing the outcome variable in columns 3 and 4. The bandwidths are set at the MSE-optimal level of the house price results in Table 4. 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) in all columns. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

effects of winning a school board election on home prices in broader neighborhoods. We begin, in columns 1 and 2 of Table 5, with the parallel house price index at the Census tract level (constructed as the transaction-weighted average across block groups within the tract). Winning a school board election has no statistically significant effects on tract-level prices. To directly examine the effects of winning a school board election on nearby neighborhoods, we also look at block groups within candidates' Census tracts, excluding their own. Columns 3 and 4 confirm that house prices in other block groups in the same Census tract are not affected by the election results.

Although the overall price effect for the four-year term is not statistically significant, it may reflect fluctuating dynamic effects over the period. To further explore these dynamic effects, we estimate the following "event-study"-style RD specification of the *level* of house

price index around the election year:

$$Y_{it} = \alpha + \sum_{k=-4}^{3} 1(k = t) \times (\beta_k D_i + \gamma_{1k} x_i + \gamma_{2k} D_i \cdot x_i + \gamma_{3k}) + \gamma_3 Z_i + \epsilon_{it} \tag{5}$$

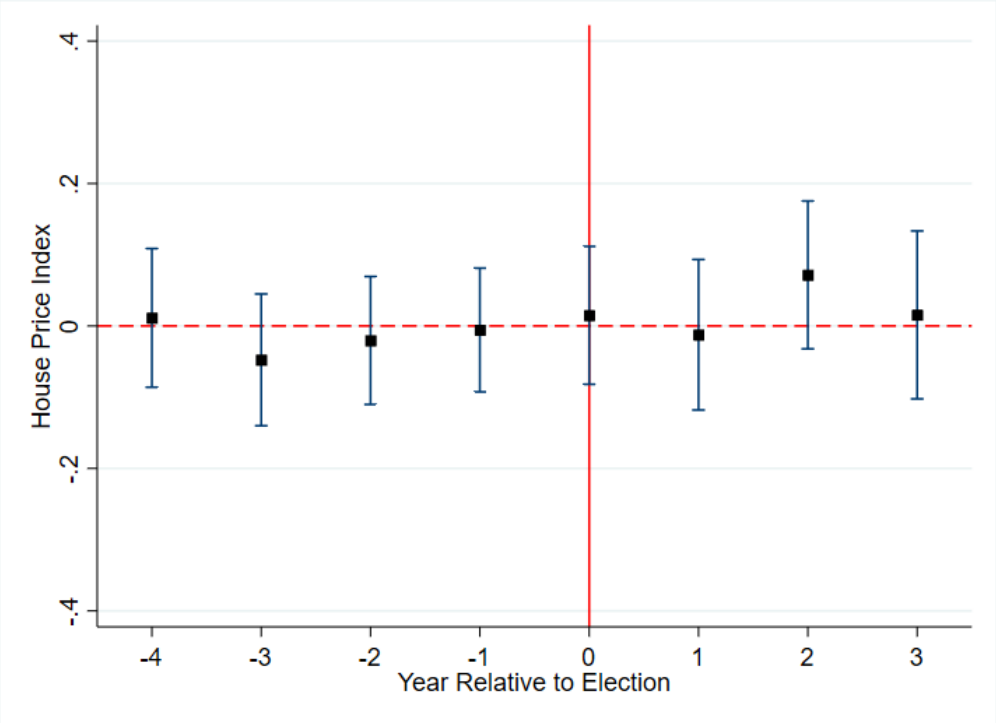
where Y_{it} represents the level of the house price index in the period from four years before the election to three years after. The term $1(k = t)$ represents the indicator for years relative to the election year ($k = 0$). Here, $beta_k$ captures the dynamic effects of winning a school board position on the house price index. We use the MSE-optimal bandwidths in Table 4 to infer the dynamic effects corresponding to the baseline results. Figure 2 presents the dynamic effects with or without the covariates, showing that there are no statistically significant gaps in home prices between winners and losers over the period. It also reports that there is no pre-trend in the house price index, which is consistent with the placebo test in Table 3.

4.1 Heterogeneity by Political Identity

The limited impact of winning a school board position on neighborhood home values may mask varying effects based on candidates' motivations and policy preferences. It is well established that political partisanship influences the policy outcomes of politicians at both the national and local levels (Ferreira and Gyourko, 2009; Gerber and Hopkins, 2011; Macartney and Singleton, 2018; Carlino et al., 2023). For instance, Macartney and Singleton (2018) demonstrate that Democratic school board members tend to reduce student segregation compared to their non-Democratic counterparts.

In this subsection, we analyze the effect on house prices for Democratic and non-Democratic school board members separately, revealing significant differences in the focus of their policies. We categorize all other political affiliations alongside Republicans for two main reasons. First, it follows the categorization of Macartney and Singleton (2018), which allows us to interpret our results in light of policy differences highlighted in previous work. Second, our sample size becomes quite small if we further split it among additional political affiliations,

Figure 2: Dynamic Effects of Election on Neighborhood Home Prices



Notes: This figure reports the estimation results of equation 5. Period 0 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 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). Standard errors are clustered at the election level. The confidence intervals are at 95%.

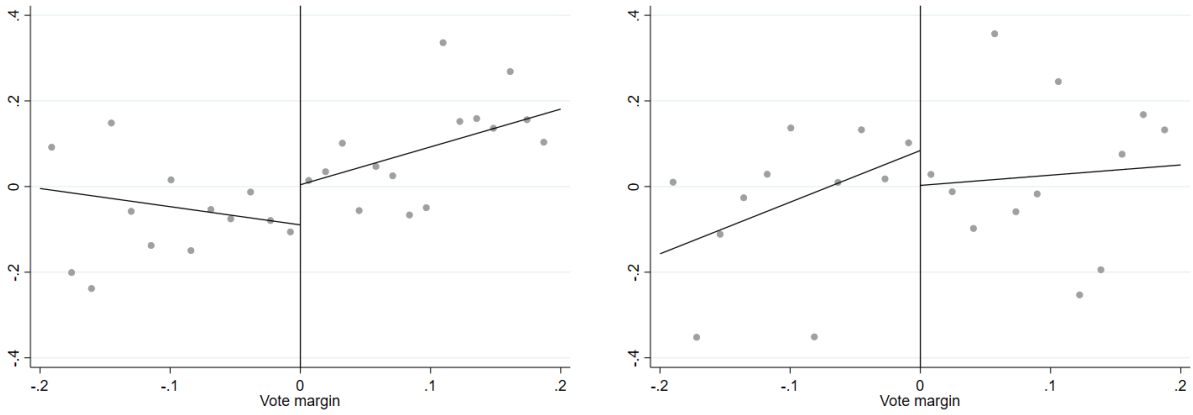
generating less precise estimates.

Figure 3 shows clear evidence of the heterogeneity in the causal effect of winning a school board election on the house price index in the winner's neighborhood. The figures indicate that winning a school board election raises home prices in the block group of non-Democratic candidates more than in the overall group of candidates, while winning the election slightly lowers the home prices in the block group of Democratic winners compared to those of Democratic losers.

To be more concrete about the effects, Table 6 presents RD estimates of the price effect by partisanship. In column 1, the house price index in the block group of a non-Democratic election winner increases by around 8.5% relative to prices in a non-Democratic loser's block

Figure 3: Effects of Election on Neighborhood Home Prices by Political Identity

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



Notes: The y-axis is the change in the home price index, and the x-axis measures the vote margin. We report the RD plots of the house price index with linear fits and bandwidth of [-0.2, 0.2]. We set equally spaced bins for both sides of the threshold, and each point indicates the average of the outcome 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.

group without any covariates. In contrast, column 3 reveals no statistically significant discontinuity in neighborhood home prices between winning and losing Democratic school board candidates. In columns 2 and 4, the inclusion of the controls mitigates the point estimate for non-Democratic candidates to 6.8%, while it does not affect the result for Democratic candidates qualitatively. These results suggest that non-Democratic winners may take actions as elected officials, which are to the disproportionate benefit of their own neighborhood, and that this is capitalized into neighborhood home prices.

Although we combine Republicans with other political affiliations for the reasons mentioned above, one might wonder if the disproportionate effects observed for non-Democratic candidates are specifically driven by Republican candidates. Our sample shows that approximately 78% of non-Democratic candidates are Republicans, but, as illustrated in Appendix Table A.7, the RD estimates of the housing price index are smaller and not statistically significant when considering only the Republican candidates. This suggests that the price

Table 6: Effects of Election on Neighborhood Home Prices by Political Identity

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	0.085**	0.068**	-0.002	-0.009
	(0.036)	(0.032)	(0.039)	(0.040)
Optimal BW	0.099	0.114	0.172	0.101
Obs. within BW	380	403	269	206
Obs.	794	794	501	501
Controls	None	Full	None	Full

Notes: RD estimates are computed using local linear regressions. The outcome variable is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level following Calonico, Cattaneo and Farrell (2020). 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) for columns 2 and 4. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

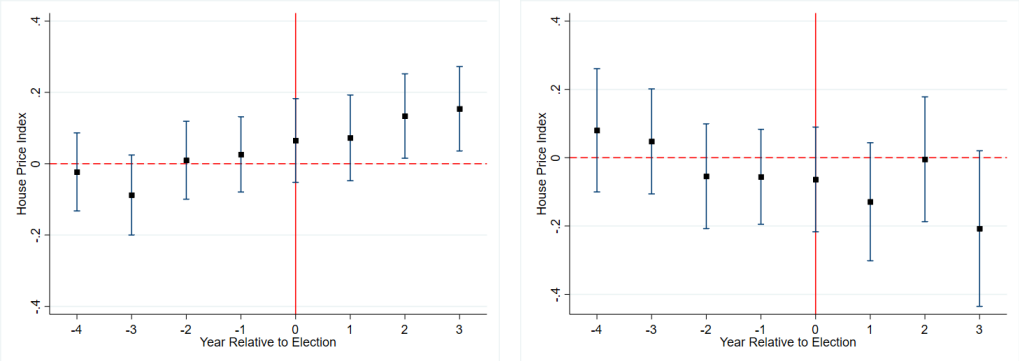
impact is particularly pronounced for unaffiliated candidates. Unaffiliated candidates contain a sizable number of independent and libertarian candidates whose beliefs may be more focused on their neighborhood schools than Republican candidates.

Although we do not see any price effects at the Census tract level when analyzing the pooled sample, an impact may exist within political partisanship. For instance, the rise in home values in non-Democratic winners' block groups may spill over to neighboring areas within the same Census tract. However, as shown in Columns 1 and 3 of Table A.8, there are no statistically significant effects on tract-level prices among any political affiliation. Similarly, Columns 2 and 4 indicate that there are no observable effects on house prices in other block groups within the same Census tract. This suggests that the victory of a non-Democratic candidate raises home values only within a narrow neighborhood, the Census block group. We will explore potential mechanisms that could explain this limited effect on perceived neighborhood quality in the next section.

We also present the event-study-style RD estimates from equation 5 for non-Democratic

Figure 4: Dynamic Effects of Election on Neighborhood Home Prices by Political Identity

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



Notes: This figure reports the estimation results of equation 5 for non-Democratic and Democratic candidates, separately. Period 0 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 6. 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). Standard errors are clustered at the election level. The confidence intervals are at 95%.

and Democratic candidates separately in Figure 4. Panels (a) indicate that the RD estimates for non-Democratic candidates show a noticeable increase after elections, though the standard errors are larger than the results of the baseline regressions.[20] This increase is particularly strong during the third and fourth years following the election, suggesting that it may take time for the election results and any relevant neighborhood changes to be reflected in home values in the neighborhoods of non-Democratic winners. In contrast, in panels (b), we do not find statistically significant effects for Democratic candidates that align with the baseline RD estimates.

[20]One could argue that the increase in non-Democratic winners' house prices *relative to* the house prices of non-Democratic losers may be driven by a decrease in loser's house prices. A limitation of the RD design is that this gap or relative difference does not shed light on whether, in absolute terms, it is non-Democratic winners' prices that are increasing or non-Democratic losers' prices decreasing because of the electoral outcome. Instead, we compare the raw trend in the average block-group house price around the year of the election for marginal non-Democratic winners and losers in Appendix Figure A.8. The graph clearly shows that the house price *increases* in the neighborhood of non-Democratic winners, while that in the neighborhood of non-Democratic losers does not decline in the post-election periods.

4.2 Robustness

We find that home prices increase relatively in the neighborhoods of election winners, especially among non-Democratic candidates, and that this effect is highly localized. We also do not find evidence that Democratic winners similarly benefit their neighborhood relative to Democratic losers. These results are based on many research choices, such as sampling, variable construction, and RD design. So, we report the robustness of these findings to these choices in this subsection.

First, we demonstrate that our main findings are robust across different sample choices. We focus on non-Hispanic White candidates who are uniquely matched between the election records and the voter registration database. To assess whether our conservative matching algorithm impacts the baseline results, we include candidates who are matched to multiple identities in the voter registration database. Specifically, we include candidates with 4 and 8 identities at most and use the inverse of the number of matches as the weight for each identity. For candidates with unique matches, we assign a weight of 1. Appendix Table A.9 confirms that the point estimates of the price effect remain consistent with this expanded sample. The exclusion of Black and Hispanic candidates from our sample may also influence the price effects. However, as shown in Appendix Table A.10, our results remain qualitatively similar when we include minority board candidates.

The way to split the sample is also important for our results. While we focus on heterogeneity by partisanship, considering different motivations and policy preferences, there is a concern that the disproportionate effects for non-Democratic candidates may spuriously reflect other forms of heterogeneity. For instance, previous research on private returns to office and political selection has shown that the gender of political leaders can influence investment in education (Clots-Figueras, 2012). In our case, the differing policy priorities between genders might affect home value appreciation. However, as shown in Appendix Table A.11, alternative methods of dividing the sample by gender and age do not reveal significant heterogeneity in the impact of winning a school board election on home prices.

Second, since using an empirical Bayes estimator of neighborhood housing prices is new, we explore how our shrunken estimate performs relative to standard home price measures in Appendix Table A.13. 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 log house price in Appendix Table A.13.[21] The estimates of the housing price index are robust to winsorizing at the 1% and 5% tails of the house price distribution and the number of house transactions in a neighborhood. More interestingly, our housing price index is more precise relative to a standard price measure and is less sensitive to outlier prices as given by the results in Panel A of Appendix Table A.13.

Additionally, we demonstrate that the RD estimates remain consistent regardless of the geographic area selected for the common prior used to construct the Bayes estimator. Specifically, we show that using house prices at the Census tract level, rather than at the county level, yields consistent estimates, which are reported in Appendix Table A.14.

Third, our baseline results may depend on the structure of the RD design. We first check the RD estimates of the price index across placebo thresholds. Panels (a) of Appendix Figure A.10 report that the RD estimate of house price index peaks at the original threshold for non-Democratic candidates and is not statistically significant at placebo thresholds ranging from -0.3 to 0.3.[22]

In Appendix Table A.12, we also examine the robustness of the price effects with alternative bandwidths, 0.05, 0.10, 0.15, and 0.20. Each cell represents a separate regression result, and we control for the full set of covariates in all specifications. The results show that our baseline findings are robust to the bandwidth choice. Appendix Figure A.9 visually shows the robustness to a broader range of bandwidths from 0.05 to 0.35. The RD estimate for

[21] 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.

[22] A threshold outside of this range makes the effective observations not enough (mostly less than 10) to run the RD model because our observations are very concentrated around the true threshold, 0. In the model for the figure, we excluded the controls and applied different optimal bandwidths for the left and right sides of a threshold to raise the effective number of observations.

non-Democratic candidates is statistically significant and stable around our baseline point estimate by 0.2 and slightly declines with wider bandwidths. In comparison, the estimate is statistically insignificant with any bandwidth for Democratic candidates.

Another concern is that we pool variations in configurations of the political identities of the marginal winner and loser in our sample, which may invalidate the seemingly arbitrary nature of the close votes. To test if this setting affects the RD estimate of the house price index, we estimate the effects by contest type: (1) Democratic (narrowest) winner versus Democratic (narrowest) loser, (2) Democratic winner versus non-Democratic loser, (3) non-Democratic winner versus Democratic loser, and (4) non-Democratic winner versus non-Democratic loser. In all case, we only use contests where both the least-vote-getting winner and the most-vote-getting loser are matched to the voter records, leaving only 413 candidates. Columns 1 to 3 in Appendix Table A.15 show that the estimated effect of winning a school board seat on home prices in the winner's neighborhood is negative, but not statistically different from zero, for all types of contests except for the case of a non-Democratic winner versus a non-Democratic loser. However, the estimated effect is positive when the marginal winner and loser are non-Democratic, which corresponds to our results that stratify by non-Democratic candidates, though the estimate is not statistically different from zero. We conclude that narrowing in this way leads to a lot less precision, but produces a pattern of findings that affirms our main results.

To further verify that the positive impact on housing prices for non-Democratic winners is not driven by differences in local housing markets across districts, we additionally control for location fixed effects. Specifically, we include school district-by-urban[23] fixed effects to account for time-invariant location differences and commuting zone-by-year fixed effects to capture trends in local housing markets. Given the limited sample size in the marginal sample of non-Democratic winners versus non-Democratic losers, we expand it with the

[23]Urban is an indicator for "urban area" or an "urban cluster" from the definition of the Census. An urban cluster (UC) consists of densely settled territory that has at least 2,500 people but fewer than 50,000 people. The Census Bureau defines urban areas (UA) as densely settled areas of 50,000 or more people.

spirit of the previous exercise. Specifically, we add to the sample the following candidates: a) in contests where the marginal winner is non-Democratic but the marginal loser is not non-Democratic (can be Dem or not matched to the voter records), the marginal winner and any non-Democratic loser; and b) in contests where the marginal loser is non-Democratic but the marginal winner is not non-Democratic, the marginal loser and any non-Democratic winner. In Table A.16, the RD estimates with the location fixed effects support our main finding regarding the effects of non-Democratic winners: relative to non-Democratic losers, home prices in their neighborhoods tend to rise.

5 Mechanisms

The results in the prior section reveal that winning a school board election causes home values in winners' neighborhoods to increase (relative to losers' neighborhoods). The effects are pronounced among non-Democratic candidates. Since the perceived quality of assigned schools is an important neighborhood amenity, we turn to education data to examine how neighborhood school attributes may be impacted by school board members. Because the pooled estimates mask sharp partisan heterogeneity, our exploration of mechanisms examines the causal impacts of winning a school board seat separately among Democratic and non-Democratic candidates.

There are several potential ways for school board members to induce changes in local school quality and, hence, 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.

5.1 Effects on Student and Teacher Quality

One way for school board members to increase home prices in their neighborhoods is by improving the perceived quality of schools through student and teacher quality. While some papers report that the perception of true school quality, such as school value-added, is limited (Imberman and Lovenheim, 2016; Ainsworth et al., 2023; Beuermann et al., 2023), the literature finds that observable school quality, such as test scores, is valued by parents and the housing market (Black, 1999; Figlio and Lucas, 2004; Ries and Somerville, 2010; Dhar and Ross, 2012; Wada and Zahirovic-Herbert, 2013; Biasi, Lafortune and Schönholzer, 2025). Therefore, we would expect to see a corresponding improvement at least in observable student achievement in neighborhoods where non-Democratic candidates win compared to those where non-Democratic candidates lose, assuming that the enhancement in school quality drives the housing market effects associated with winning a school board seat.

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. Note that the EOG test is conducted for 3-8 graders. We report the estimation results for 3-5 graders attending the neighborhood elementary school and 6-8 graders attending the neighborhood middle school separately. 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 the house price change and control for the same covariates (election year fixed effects, candidate, and school district characteristics).[24]

We first report the estimated effect of winning a school board election on the change in average test scores (overall, math, and reading) of students residing in the Census block group of candidates for non-Democratic and Democratic candidates, respectively. Both panels of Table 7 indicate that winning a school board position does not raise or lower the average

[24]As a robustness check, we report the parallel RD estimates of school outcomes from the specification with the optimal bandwidth for the corresponding dependent variable in Appendix Table A.18. As we show for the house price change, we also present the results of school outcomes including minority candidates (Appendix Table A.17) and those for Republican candidates (Appendix Table A.19). All of these results are consistent with our main findings.

Table 7: Effects of Election on Average Test Scores of Neighborhood Students

	Non-Democratic			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Math	Reading	Overall	Math	Reading
Panel A: Middle School Students						
Vote margin > 0	0.002	-0.013	0.016	0.007	0.018	-0.005
	(0.054)	(0.052)	(0.060)	(0.058)	(0.060)	(0.061)
Pre-election Mean	0.092	0.100	0.084	0.064	0.069	0.058
Panel B: Elementary School Students						
Vote margin > 0	-0.062	-0.054	-0.069	-0.019	-0.008	-0.030
	(0.041)	(0.044)	(0.044)	(0.061)	(0.065)	(0.062)
Pre-election Mean	0.106	0.114	0.098	0.053	0.063	0.042
Obs. within BW	322	322	322	176	176	176
Obs.	624	624	624	431	431	431

Notes: RD estimates are computed using local linear regressions. The outcome variable is calculated by taking the average of the average test scores of neighborhood students in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The neighborhood students are those residing in the Census block group of candidates (middle school students for Panel A and elementary school students for Panel B) at the time of elections. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

test scores of students residing in the Census block group of candidates compared to those living in the neighborhood of election losers from the same political affiliation. These results show that the increase in house prices in the block groups of non-Democratic school board members is not the result of an observable change in the academic performance of students living in their neighborhoods.

Even if a school board member does not directly affect the test scores of students "residing" in her block group, she may target and improve the achievement of students attending her "neighborhood schools" because the neighborhood schools are reasonable policy targets of school board members. Table 8 reports the estimated winner's effect on average test scores at the neighborhood middle school (panel A) and at the neighborhood elementary school

Table 8: Effects of Election on Average Test Scores of Students in Neighborhood Schools

	Non-Democratic			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Math	Reading	Overall	Math	Reading
Panel A: Middle School						
Vote margin > 0	0.017	0.026	0.006	0.020	0.028	0.010
	(0.023)	(0.020)	(0.025)	(0.024)	(0.022)	(0.025)
Pre-election Mean	0.027	0.030	0.021	-0.042	-0.031	-0.048
Panel B: Elementary School						
Vote margin > 0	0.037	0.028	0.040	0.044	0.039	0.044
	(0.027)	(0.021)	(0.032)	(0.027)	(0.024)	(0.031)
Pre-election Mean	0.048	0.052	0.038	0.011	0.020	-0.000
Obs. within BW	360	360	360	195	195	195
Obs.	691	691	691	471	471	471

Notes: RD estimates are computed using local linear regressions. The outcome variable is calculated by taking the average of the average test scores at the neighborhood school (middle school for Panel A and elementary school for Panel B) up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

(panel B). We do not observe any statistically significant effect of winning a school board position on the average test scores at the neighborhood schools for both political groups.

The productivity or quality of the neighborhood schools, as measured by test-score value-added, may be affected by election results. We separately estimate school and teacher value-added at the neighborhood schools following Chetty, Friedman and Rockoff (2014), and use those estimates as the dependent variables in the RD estimation. Specifically, using the student-level test score data, we residualize observed scores with respect to controls, including lagged test score interacted with grade, its square, demographics, and the number of students and student composition in the class and the school. Next, we take the average of the residualized test scores at each school-by-year for school value-added. Similarly, we calculate the average of the residualized test scores at each teacher-by-year for teacher value-

Table 9: Effects of Election on School and Teacher Value-added of Neighborhood Schools

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
	School VA	Teacher VA	School VA	Teacher VA
Panel A: Middle School				
Vote margin > 0	0.015	-0.019	0.044*	0.002
	(0.023)	(0.024)	(0.026)	(0.009)
Pre-election Mean	0.004	-0.071	-0.022	-0.058
Panel B: Elementary School				
Vote margin > 0	0.022	-0.011	0.066	0.016
	(0.025)	(0.011)	(0.044)	(0.015)
Pre-election Mean	0.028	0.005	0.033	0.004
Obs. within BW	333	315	181	162
Obs.	630	594	421	386

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the average of school and teacher value-added at the neighborhood school (middle school for Panel A and elementary school for Panel B) up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

added. The value-added for a school (and a teacher) in a particular year is defined as the best linear predictor based on the same average residualized test score from the previous year.[25] For teacher value-added, we use the average value at the neighborhood school level in our RD estimation.

Columns 1 and 2 in Table 9 present no effects on school and teacher value-added at the neighborhood schools, indicating that a newly elected non-Democratic school board member does not improve the quality of neighborhood schools. In contrast, when a Democratic candidate is elected to the school board, the value-added for the neighboring middle school increases by 0.044 test score standard deviations. However, this point estimate is sensitive to the choice of bandwidth. We find that this effect is not statistically significant at most other

[25]For more detailed derivation, refer to section I of Chetty, Friedman and Rockoff (2014).

bandwidths surrounding the baseline.[26] If this positive effect is indeed accurate, our analysis of housing prices suggests that this increase in school quality does not translate into higher home prices, which is consistent with the findings of Imberman and Lovenheim (2016).

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. The coefficients in Panel A of Appendix Table A.21 indicate that neither the average experience years nor the composition of teachers in the neighborhood middle school responds to the school board election results for both political groups. Panel B similarly shows the muted effects in the neighborhood elementary school of non-Democratic winners. On the contrary, Democratic winners change the composition of teachers, lowering the share of moderately experienced (2-9 years) teachers by 4.6 percentage points (15%) and raising the share of highly experienced (10+ years) ones by 3.3 percentage points (5.3%). However, the average teacher experience does not show a statistically significant increase as the share of less experienced teachers moderately rises at the same time.

5.2 Effects on Student Sorting and School Assignments

5.2.1 Student Sorting

Other factors that can lead to changes in house prices include non-test-based characteristics of schools. In particular, we focus on student sorting and changes in school attendance zones. Adjusting these zones, which can influence the composition of students, is a key policy tool for school boards (Macartney and Singleton, 2018; Monarrez, 2023), and parents and the housing market values the composition of students in schools (Clapp, Nanda and Ross, 2008;

[26]To elaborate, we vary the bandwidth from 0.07 to 0.13 in increments of 0.01 (surrounding the baseline bandwidth of 0.101), and the RD estimate is statistically significant only at 0.1 and 0.12. It is not statistically significant at larger bandwidths.

Table 10: Effects of Election on Composition of Neighborhood Students

	Non-Democratic				Democratic			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Prop. White	Prop. Black	Prop. Hispanic	Prop. EDS	Prop. White	Prop. Black	Prop. Hispanic	Prop. EDS
Panel A: Middle School Students								
Vote margin > 0	0.022	0.005	-0.013	-0.032	-0.009	0.026	-0.007	0.006
	(0.014)	(0.013)	(0.010)	(0.022)	(0.027)	(0.023)	(0.013)	(0.031)
Pre-election Mean	0.724	0.135	0.081	0.423	0.687	0.179	0.071	0.449
Panel B: Elementary School Students								
Vote margin > 0	-0.010	-0.007	0.014	0.010	-0.020	0.018	-0.006	-0.011
	(0.016)	(0.011)	(0.012)	(0.021)	(0.025)	(0.018)	(0.015)	(0.030)
Pre-election Mean	0.719	0.126	0.092	0.445	0.677	0.171	0.083	0.477
Obs. within BW	395	395	395	395	215	215	215	215
Obs.	770	770	770	770	527	527	527	527

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the average of the proportions of subgroup neighborhood students in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The neighborhood students are those residing in the Census block group of candidates (middle school students for Panel A and elementary school students for Panel B) at the time of elections. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Ainsworth et al., 2023).

In Table 10, we first investigate the effect of winning a school board position on the composition of students who reside in the block group of a winner (proportions of white, black, Hispanic, and economically disadvantaged students (EDS)). Similar to the findings regarding test scores, winning a school board election does not significantly influence the composition of middle and elementary school students in these neighborhoods. Consequently, the increase in home values in the neighborhoods of non-Democratic candidates does not correspond to any changes in student demographics in those areas.

In Table 11, we further investigate discontinuities in the student composition of neighborhood middle and elementary schools. Panel A presents that winning a school board position

Table 11: Effects of Election on Composition of Students in Neighborhood Schools

	Non-Democratic				Democratic			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Prop. White	Prop. Black	Prop. Hispanic	Prop. EDS	Prop. White	Prop. Black	Prop. Hispanic	Prop. EDS
Panel A: Middle School								
Vote margin > 0	0.018***	-0.011*	-0.007*	0.013	0.003	-0.004	-0.002	-0.030
	(0.007)	(0.006)	(0.004)	(0.018)	(0.006)	(0.005)	(0.004)	(0.024)
Pre-election Mean	0.660	0.193	0.098	0.479	0.614	0.248	0.086	0.528
Panel B: Elementary School								
Vote margin > 0	0.007	0.007	-0.013**	0.015	0.006	-0.004	-0.003	-0.027
	(0.006)	(0.006)	(0.005)	(0.016)	(0.006)	(0.007)	(0.005)	(0.025)
Pre-election Mean	0.673	0.162	0.114	0.502	0.627	0.218	0.101	0.550
Obs. within BW	381	381	381	381	205	205	205	205
Obs.	745	745	745	745	511	511	511	511

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the average of the proportions of subgroup students at the neighborhood school (middle school for Panel A and elementary school for Panel B) up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

raises the proportion of white students by 1.8 percentage points (2.7%) at the neighborhood middle school of non-Democratic winners in the four years following the election, while the proportions of black and Hispanic students decrease by 1.1 percentage points (5.2%) and 0.7 percentage points (7.1%), respectively. The share of EDS does not respond. Panel B of Table 11 indicates that the proportion of Hispanic students declines by 1.3 percentage points (11.4%) at the neighborhood elementary school of non-Democratic winners. On the contrary, we cannot observe any statistically significant changes in the neighborhood schools of Democratic winners compared to those of Democratic losers.

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

Table 12: Effects of Election on Composition of Structural Movers in Neighborhood Schools

	Non-Democratic				Democratic			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Prop. White	Prop. Black	Prop. Hispanic	Prop. EDS	Prop. White	Prop. Black	Prop. Hispanic	Prop. EDS
Vote margin > 0	0.024**	-0.011	-0.011*	-0.010	0.009	-0.004	-0.003	-0.001
	(0.010)	(0.007)	(0.006)	(0.011)	(0.009)	(0.008)	(0.006)	(0.014)
Pre-election Mean	0.656	0.179	0.104	0.520	0.604	0.241	0.093	0.565
Obs. within BW	346	346	346	346	185	185	185	185
Obs.	673	673	673	673	453	453	453	453

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the average of the proportions of subgroup students among structural movers at the neighborhood school (middle school for Panel A and elementary school for Panel B) up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

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. Table 12 reports the results of the corresponding composition among the structural movers. Column 1 shows that the estimated discontinuity in the proportion of white structural movers is 2.4 percentage points (3.7%) at the threshold, which is consistent with the effect on the proportion of white students in Table 11. Correspondingly, the point estimates of the proportions of Hispanic structural movers are -1.1 percentage points (10%), respectively. By contrast, for Democratic candidates, we observe no jumps in the composition measures at the threshold.

The notable change in the neighborhood schools of non-Democratic candidates in terms of student composition may contribute to rising house prices in those neighborhoods, as home values reflect the demographics of students in local schools (Clapp, Nanda and Ross, 2008; Ainsworth et al., 2023). However, it is also possible that other school characteristics

associated with a higher proportion of white students and a lower proportion of Hispanic students could be driving the effects on housing prices. Yet, we do not find any significant evidence of associated effects on some observable school characteristics, such as classroom assignments, advanced classes, and resources, including the number of books and Internet access.[27]

In order to connect these estimates of demographics to our price results, we examined the existing literature on the capitalized values of student racial composition which finds between a 2% and 10% decrease in home prices for a 10 p.p. increase in minority composition (Clapp, Nanda and Ross, 2008; Collins and Kaplan, 2022; Wigger, 2025). Even though our effects are quite a bit larger, one could imagine home prices may capture a variety of current and future expectations around neighborhood school improvements that are correlated with racial composition – such as school climate (Crespin, 2023), teacher quality (Jackson, 2018*b*) and other educational expenditures (Jackson, 2018*b*; Baron, 2022) .

5.2.2 School Assignments

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 those data are generally not available at an annual frequency, we indirectly test for school boundary changes by examining the shifts in the composition of the school attendance zone at residentially assigned schools for the school board candidate's neighborhood.

As explained in Section 2.2, we construct compositional measures of the shift in school attendance zones in terms of racial composition and income level as dependent variables. These measures capture the changes in the composition of the block groups sending students to the neighborhood school (because the block-group-level characteristics are fixed prior to the candidate's term). Table 13 reports the RD estimates of the measures of attendance zone shift for the neighborhood middle school (Panel A) and the neighborhood elementary

[27]For example, Table A.22 shows that the interquartile ranges (IQR) of classrooms with respect to student demographics and test scores and the share of advanced courses do not respond to the election results.

school (Panel B), separately. Columns 1 to 3 of Panel A report that, the proportion of white students is 2 percentage points (2.8%) higher, the proportion of black students is 1.2 percentage points (6.9%) lower, and the proportion of economically disadvantageous students (EDS) is 2 percentage points (3.8%) lower in the neighborhood middle schools of non-Democratic winners than in the neighborhood middle schools of non-Democratic losers. In other words, the neighborhood middle schools of non-Democratic winners are capturing more students from the block groups with a higher proportion of white students, fewer black students, and fewer economically disadvantaged students than those of non-Democratic losers after school board elections.

Columns 4 to 6 of Panel A show the corresponding changes in the characteristics of the general population in the same locations. The point estimate of the proportion of white population is positive and statistically significant, which is consistent with that of white students. A non-Democratic candidate's winning a school board position raises the median income of the middle school attendance zone by 3.5%, corresponding to the decrease in the proportion of EDS in the same locations. Considering that there is no such discontinuity before school board elections, as shown in Appendix Table A.20, this gap is solely driven by the change in school attendance in the post-election period. These results consistently imply that non-Democratic school board members disproportionately shift the attendance zone of their neighborhood middle schools toward more "preferable" neighborhoods. Similar to other results, there is no discontinuity for the Democratic party, with the exception that the proportion of black students slightly increases in the middle school attendance zone of Democratic winners.

We provide evidence on how these effects evolve over time by running our RD model separately for each year relative to the election year, as shown in Appendix Figure A.11. Notably, panel (b) of the figure demonstrates that the share of white students assigned to the neighborhood school increases starting from the academic year immediately following the election (t=0), without any pre-existing trends. Considering that the house price index

rises during the third and fourth years post-election (t=2 and t=3), as shown in Figure 2, these findings suggest that homebuyers respond to changes in the neighborhood school with a noticeable time lag. This is understandable since it may take time for such information to be widely disseminated.

Conversely, Panel B of Table 13 reports the muted effects on the composition of the block groups sending students to the neighborhood elementary school for both political groups. There are two potential reasons for these effects, especially for non-Democratic winners: First, there is a larger scope for a school board member to shift the attendance zone for a middle school than an elementary school because a middle school covers a much broader area than an elementary school. In our sample, a middle school serves 56 block groups on average, while an elementary school covers 39 block groups. Given existing patterns of segregation, a school board member has a smaller scope for an elementary school to change the quality of neighborhoods where attending students come from, and the effect may not be large enough to be captured by the RD model. Second, it could be easier for a school board member to manipulate the boundary of a middle school without highlighting clear benefits to their own neighborhood. A smaller boundary lends less scope for moving and clearly shows who benefits from a change.

Table 13: Effect of Election on Attendance Zone Composition of Neighborhood Schools

	Non-Democratic						Democratic					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Boundary Shift w.r.t.	White Stu.	Black Stu.	EDS Stu.	White Pop.	Black Pop.	Log Median Income	White Stu.	Black Stu.	EDS Stu.	White Pop.	Black Pop.	Log Median Income
Panel A: Middle School												
Vote margin > 0	0.019**	-0.012**	-0.020**	0.013*	-0.010	0.035**	-0.008	0.013	0.010	-0.004	0.007	-0.022
	(0.009)	(0.006)	(0.008)	(0.007)	(0.006)	(0.014)	(0.009)	(0.008)	(0.011)	(0.007)	(0.008)	(0.021)
Pre-election Mean	0.656	0.174	0.528	0.770	0.150	10.731	0.616	0.227	0.584	0.728	0.196	10.639
Panel B: Elementary School												
Vote margin > 0	-0.003	0.004	-0.001	-0.005	0.005	0.006	-0.002	0.001	-0.000	0.001	-0.001	-0.005
	(0.008)	(0.006)	(0.007)	(0.007)	(0.006)	(0.012)	(0.009)	(0.010)	(0.008)	(0.010)	(0.010)	(0.018)
Pre-election Mean	0.679	0.159	0.518	0.785	0.139	10.729	0.635	0.214	0.568	0.745	0.181	10.639
Obs. within BW	275	275	275	275	275	275	136	136	136	136	136	136
Observations	505	505	505	505	505	505	337	337	337	337	337	337

Notes: RD estimates are computed using local linear regressions. The outcome variables are measures of the shift in attendance zone composition at the neighborhood school (middle school for Panel A and elementary school for Panel B), as defined in equation 3. The population composition and median income, used for constructing the outcomes in columns 4, 5, 6, 10, 11, and 12, are from NHGIS 2010. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

5.3 Heterogeneity in Effects

In this subsection, we investigate how the impact of winning on home values varies by contextual and policy factors to better understand the linkage between the return to winning the office and the actions of school board members. Specifically, we are interested in estimates of:

$$Y_i = \alpha + \beta_1 D_i + \beta_2 D_i \times \text{Indicator}_i + \gamma_1 x_i + \gamma_2 D_i \cdot x_i \tag{6}$$
$$+ (\gamma_3 x_i + \gamma_4 D_i \cdot x_i + \gamma_5) \times \text{Indicator}_i + \gamma_4 Z_i + \epsilon_i$$

where Indicator_i denotes the dummy for the contextual and policy factors that a candidate i is exposed to at the time of the election. So, β_2 captures the additional impacts of winning a school board election on home values when the winner is in a certain environment. While the RD specifications are underpowered to detect statistically significant differences, they do provide suggestive evidence that the benefits of winning vary based on the factors that affect the effectiveness and intentions of school board members in exercising their authority.

Appendix Table A.23 report the results for non-Democratic candidates (panel A) and Democratic candidates (panel B), separately. First, if changes to school attendance zones and the resulting shifts in student demographics lead to increased home values in neighborhoods where non-Democratic candidates win, we would expect to see a more significant impact on home prices in districts where school board members have more control over the boundaries and composition of students. This situation is more likely in districts with rapid student enrollment growth, as it necessitates frequent adjustments to school attendance zones. To identify these districts, we examine the growth of student enrollment prior to a school board election. Specifically, we calculate the four-year growth rate of student enrollment for each district by year. A school district is classified as expanding if its growth rate is above the median. As indicated in column 1, the increase in the house price index is not particularly more pronounced for winners from expanding school districts.

As an alternative approach, we calculate the Kullback–Leibler (KL) divergence to quantitatively measure shift in school attendance zones. [28] The KL divergence is a simple and widely used metric that quantifies how much one distribution differs from a reference distribution. We utilize this measure to estimate how the distribution of block groups that a neighboring school covers changes from year to year. A higher KL divergence indicates greater changes in the composition of the covered block groups. We define a neighboring school as experiencing more frequent catchment zone changes if its KL divergence during the four years leading up to the election is above the median. Column 2 indicates that the price effect for a non-Democratic winner may be larger when the neighboring school has undergone more frequent catchment zone changes, as measured by the KL divergence.

Second, the impact of changes in public school attendance zones on the composition of the student body and neighborhood home values may also depend on the availability of school options. For instance, in North Carolina, the presence of charter schools, which are not subject to the local school board's control over enrollment, can reduce the effects of changes in traditional public school catchment zones. Therefore, we examine how the price effect differs when the school district of a candidate is more accessible to charter schools. We define a school district more accessible to charter schools if, in the school district, the percentage of students who attend a charter school is higher than the median percentage during the four years leading up to the election. Column 3 of Appendix Table A.23 provides suggestive evidence that the price effect for non-Democratic winners is less pronounced if their local communities have greater access to charter schools, although we do not find statistically significant differences. These results support the hypothesis that the ability of board members to alter school attendance zones and student distribution contributes to increases in housing prices within the neighborhoods of non-Democratic winners.

We also report heterogeneity in the return to winning the office by election type. In North

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

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 the wards have a stronger incentive to influence policy choices in the benefit of local neighborhood voters. Column 4 of Appendix Table A.23 shows that the increase in neighborhood home prices of non-Democratic winners may be smaller for at-large contests, though the power is not enough to identify the statistical difference. This possibly larger effect for candidates serving more local constituents is consistent with the nature of these elections and their respective constituents.

In addition, the appreciation of home values in the non-Democratic winners' neighborhoods may be driven by school board members residing in high-income neighborhoods, as residents in these neighborhoods tend to place a much higher value on local school quality (Wada and Zahirovic-Herbert, 2013). However, the RD estimates in column 5 show the opposite. Specifically, home value appreciation in non-Democratic winners' neighborhoods is significantly lower in areas where the average income is above the median. Although the income level may not directly cause this variation, we can reject the hypothesis that a disproportionate response of high-income neighborhoods drives the increase in house prices in the winners' neighborhoods.

6 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 relative to a losing non-Democratic candidate's neighborhood in the four years following the election. For Democratic winners, in contrast, we find no evidence of changes in neighborhood home values or in the composition or performance of their neighborhood schools.

The price appreciation observed in neighborhoods of non-Democratic winners reflects

improvements in perceived neighborhood quality, driven in part by changes in local schools. Using administrative school records, we show evidence of neighborhood public schools of non-Democratic winners shifting to serve more students who are more white, achieved through the manipulation of attendance zones. This mechanism connects to the institutional and historical importance attached to school attendance zone boundaries in North Carolina (e.g. Williams and Houck 2013; Parcel and Taylor 2015; Macartney and Singleton 2018; Carlson et al. 2025). Geographically large school districts combined with the state's lack of both inter- and intra-district open-enrollment policies (as well as growth in the large diverse districts, where charter schools and magnet programs are more available) implies that attendance zones are an active area of school board concern. In settings where open enrollment, between-district traditional school choice, or broader school choice policies dilute the importance of catchment areas, similar findings may be less likely to obtain.

A traditional view in political theory is that low compensation for public officials helps ensure that only those with strong public service motivations choose to run for office. In the framework of Besley (2005), the prediction depends on the relative strength of public service motivation among those in the candidate pool relative to non-wage private returns from office. When the latter outweighs the former, higher wages can actually improve political selection by attracting "good" politicians who would not otherwise run. Our findings that non-Democratic—but not Democratic—school board members affect local school attributes in ways that increase neighborhood home values are consistent with between-party differences in this balance. The pattern for Democratic candidates is consistent with a zero-pay setting successfully attracting primarily public-minded individuals, whereas the differential effects for non-Democratic members suggest selection on non-wage returns in that subgroup. We stress that "private returns" here refers to any changes that make the candidate personally better off, including benefits to constituencies, regardless of whether such effects were intended.

These results carry several implications for school board governance. Raising board mem-

ber salaries could improve the selection of non-Democratic candidates by attracting those with greater public service motivation, but it might also risk crowding out Democrats whose motivation is already high and attracting candidates primarily interested in wage returns. The recent shift to partisan school board elections in North Carolina (after our study period) and elsewhere may help voters better infer candidates' likely motivations as well as their policy preferences.

References

Ainsworth, Robert, Rajeev Dehejia, Cristian Pop-Eleches, and Miguel Urquiola. 2023. "Why Do Households Leave School Value Added on the Table? The Roles of Information and Preferences." *American Economic Review*, 113(4): 1049–1082.

Barfort, Sebastian, Nikolaj A Harmon, Frederik Hjorth, and Asmus Leth Olsen. 2019. "Sustaining honesty in public service: The role of selection." *American Economic Journal: Economic Policy*, 11(4): 96–123.

Baron, E Jason. 2022. "School spending and student outcomes: Evidence from revenue limit elections in Wisconsin." *American Economic Journal: Economic Policy*, 14(1): 1–39.

Barro, Robert J. 1973. "The Control of Politicians: An Economic Model." *Public Choice*, 14: 19–42.

Besley, Timothy. 2005. "Political Selection." *Journal of Economic Perspectives*, 19(3): 43–60.

Beuermann, Diether W, C Kirabo Jackson, Laia Navarro-Sola, and Francisco Pardo. 2023. "What is a good school, and can parents tell? Evidence on the multidimensionality of school output." *The Review of Economic Studies*, 90(1): 65–101.

Biasi, Barbara, Julien Lafortune, and David Schönholzer. 2025. "What Works and for Whom? Effectiveness and Efficiency of School Capital Investments Across the U.S." *The Quarterly Journal of Economics*, 1–51.

Black, Sandra E. 1999. "Do better schools matter? Parental valuation of elementary education." *The quarterly journal of economics*, 114(2): 577–599.

Buchanan, James M. 1989. *Essays on the Political Economy.* University of Hawaii Press.

Calonico, Sebastian, Matias D Cattaneo, and Max H Farrell. 2020. "Optimal Bandwidth Choice for Robust Bias-Corrected Inference in Regression Discontinuity Designs." *The Econometrics Journal*, 23(2): 192–210.

Carlino, Gerald, Thorsten Drautzburg, Robert Inman, and Nicholas Zarra. 2023. "Partisanship and Fiscal Policy in Economic Unions: Evidence from US States." *American Economic Review*, 113(3): 701–737.

Carlson, Deven, Thurston Domina, Nathan Barron, James Carter III, Rachel Perera, and Matthew Lenard. 2025. "Backlash? Schooling Reassignments and the Politics of School Desegregation. EdWorkingPaper No. 25-1150." *Annenberg Institute for School Reform at Brown University*.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2020. "Simple Local Polynomial Density Estimators." *Journal of the American Statistical Association*, 115(531): 1449–1455.

Chetty, Raj, John N Friedman, and Jonah E Rockoff. 2014. "Measuring the impacts of teachers I: Evaluating bias in teacher value-added estimates." *American Economic Review*, 104(9): 2593–2632.

Christian, Alvin, Brian Jacob, and John D Singleton. 2022. "Assessing school district decision-making: Evidence from the COVID-19 pandemic." National Bureau of Economic Research.

Clapp, John M, Anupam Nanda, and Stephen L Ross. 2008. "Which school attributes matter? The influence of school district performance and demographic composition on property values." *Journal of urban Economics*, 63(2): 451–466.

Clots-Figueras, Irma. 2012. "Are Female Leaders Good for Education? Evidence from India." *American Economic Journal: Applied Economics*, 4(1): 212–244.

Collins, Courtney A, and Erin K Kaplan. 2022. "Demand for School Quality and Local District Administration." *Economics of Education Review*, 88: 102252.

Crespin, R. 2023. "The Value of School Social Climate Information: Evidence from Chicago Housing Transactions." *Working Paper*.

Dhar, Paramita, and Stephen L Ross. 2012. "School district quality and property values: Examining differences along school district boundaries." *Journal of Urban Economics*, 71(1): 18–25.

Eggers, Andrew C, and Jens Hainmueller. 2009. "MPs for sale? Returns to office in postwar British politics." *American Political Science Review*, 103(4): 513–533.

Fay III, Robert E, and Roger A Herriot. 1979. "Estimates of income for small places: an application of James-Stein procedures to census data." *Journal of the American Statistical Association*, 74(366a): 269–277.

Ferraz, Claudio, and Frederico Finan. 2009. "Motivating politicians: The impacts of monetary incentives on quality and performance." National Bureau of Economic Research.

Ferreira, Fernando, and Joseph Gyourko. 2009. "Do Political Parties Matter? Evidence from U.S. Cities." *The Quarterly Journal of Economics*, 124: 399–422.

Figlio, David N, and Maurice E Lucas. 2004. "What's in a grade? School report cards and the housing market." *American Economic Review*, 94(3): 591–604.

Fischer, Brett. 2023. "No Spending without Representation: School Boards and the Racial Gap in Education Finance." *American Economic Journal: Economic Policy*, 15(2): 198–235.

Fisman, Raymond. 2001. "Estimating the Value of Political Connections." *American Economic Review*, 91(4): 1095–1102.

Fisman, Raymond, Florian Schulz, and Vikrant Vig. 2014. "The private returns to public office." *Journal of Political Economy*, 122(4): 806–862.

Folke, Olle, Linna Martén, Johanna Rickne, and Matz Dahlberg. 2021. "Politicians' neighbourhoods: Where do they live and does it matter?"

Gagliarducci, Stefano, and Tommaso Nannicini. 2013. "Do better paid politicians perform better? Disentangling incentives from selection." *Journal of the European Economic Association*, 11(2): 369–398.

Gerber, Elisabeth R, and Daniel J Hopkins. 2011. "When Mayors Matter: Estimating the Impact of Mayoral Partisanship on City Policy." *American Journal of Political Science*, 55(2): 326–339.

Harjunen, Oskari, Tuukka Saarimaa, and Janne Tukiainen. 2021. "Political Representation and Effects of Municipal Mergers." *Political Science Research and Methods*, 9(1): 72–88.

Holbein, John. 2016. "Left Behind? Citizen Responsiveness to Government Performance Information." *American Political Science Review*, 110(2): 353–368.

Imbens, Guido W., and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics*, 142(2): 615–635.

Imberman, Scott A., and Michael F. Lovenheim. 2016. "Does the Market Value Value-Added? Evidence from Housing Prices after a Public Release of School and Teacher Value-Added." *Journal of Urban Economics*, 91: 104–121.

Jackson, C Kirabo. 2018*a*. "What do test scores miss? The importance of teacher effects on non–test score outcomes." *Journal of Political Economy*, 126(5): 2072–2107.

Jackson, C Kirabo. 2018*b*. "What do test scores miss? The importance of teacher effects on non–test score outcomes." *Journal of Political Economy*, 126(5): 2072–2107.

Kane, Thomas J, Jonah E Rockoff, and Douglas O Staiger. 2008. "What does certification tell us about teacher effectiveness? Evidence from New York City." *Economics of Education review*, 27(6): 615–631.

Kogan, Vladimir, Stéphane Lavertu, and Zachary Peskowitz. 2021. "The Democratic Deficit in U.S. Education Governance." *American Political Science Review*, 115(3): 1082–1089.

Kotakorpi, Kaisa, and Panu Poutvaara. 2011. "Pay for politicians and candidate selection: An empirical analysis." *Journal of Public Economics*, 95(7-8): 877–885.

Lee, David S., Enrico Moretti, and Matthew J. Butler. 2004. "Do Voters Affect or Elect Policies? Evidence from the U. S. House." *The Quarterly Journal of Economics*, 119(3): 807–859.

Macartney, Hugh, and John D Singleton. 2018. "School boards and student segregation." *Journal of Public Economics*, 164: 165–182.

Monarrez, Tomás E. 2023. "School Attendance Boundaries and the Segregation of Public Schools in the United States." *American Economic Journal: Applied Economics*, 15(3): 210–237.

Morris, Carl N. 1983. "Parametric empirical Bayes inference: theory and applications." *Journal of the American statistical Association*, 78(381): 47–55.

Parcel, Toby L, and Andrew J Taylor. 2015. *The end of consensus: Diversity, neighborhoods, and the politics of public school assignments.* UNC Press Books.

Querubin, Pablo, and Jr. Snyder, James M. 2011. "The Control of Politicians in Normal Times and Times of Crisis: Wealth Accumulation by U.S. Congressmen, 1850-1880." National Bureau of Economic Research Working Paper 17634.

Ries, John, and Tsur Somerville. 2010. "School quality and residential property values: evidence from Vancouver rezoning." *The Review of Economics and Statistics*, 92(4): 928–944.

Ritz, Adrian, Gene A Brewer, and Oliver Neumann. 2016. "Public service motivation: A systematic literature review and outlook." *Public Administration Review*, 76(3): 414–426.

Rosen, Sherwin. 1974. "Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition." *Journal of Political Economy*, 82(1): 34–55.

Shi, Ying, and John D Singleton. 2020. "School Boards and Education Production: Evidence from Randomized Ballot Order."

Spenkuch, Jorg L, Edoardo Teso, and Guo Xu. 2021. "Ideology and performance in public organizations." National Bureau of Economic Research.

Wada, Roy, and Velma Zahirovic-Herbert. 2013. "Distribution of Demand for School Quality: Evidence from Quantile Regression." *Journal of Housing Research*, 22(1): 17–31.

Wigger, Cora JL. 2025. "Persistence and variation of schools as housing amenities." *Regional Science and Urban Economics*, 110: 104058.

Williams, Sheneka M, and Eric A Houck. 2013. "The life and death of desegregation policy in Wake County public school system and Charlotte-Mecklenburg schools." *Education and Urban Society*, 45(5): 571–588.

Zillow. 2020. "Zillow Transaction and Assessment Dataset (ZTRAX)." *https://www.zillow.com/research/ztrax/*.

Appendices

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. middle 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.[29] After that, we use the same matching function in the 1st trial and pick up the unique exact matches.

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 Appendix Table A.9. The observations include non-unique matches along with the main observations in Tables 4 and 6. The non-unique matches have no evidence that guarantees that the 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 63% to 74%.

Appendix Table A.1 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. Appendix Table A.2 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.

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

We also provide evidence of treatment among the matched candidates in Appendix 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. Appendix Figure A.2 confirms that the collected information is consistent with the number of votes from the election data from NCSBE.

Additional Tables and Figures

Table A.1: 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.2: Balance Checks for 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

Notes: RD estimates are computed using a local linear regression. The outcome variable in columns 1, 2, and 3 is the final match rate and that in columns 4, 5, and 6 are the match rate from the most conservative algorithm (1st trial). The bandwidths are set at 0.1, 0.15, and 0.2. All regressions include election year fixed effects.

Table A.3: Test of Smoothness Assumption

	All Candidates	Non-Democratic	Democratic
T-statistic	0.61	0.764	-0.926
P-value	0.542	0.445	0.354
Effective obs. (left)	416	283	161
Effective obs. (right)	296	196	129

Notes: The t-statistics measure how much the densities of observations right and left of the threshold differ from each other following Cattaneo, Jansson and Ma (2020).

Table A.4: Descriptive Statistics by Political Identity

	Non-Democratic			Democratic		
	Winner Mean	Loser Mean	Diff. t-stat	Winner Mean	Loser Mean	Diff. t-stat
Panel A: Candidate Characteristics						
Age	45.76	44.34	1.96	52.27	50.43	2.14
Prop. Female	0.52	0.31	7.31	0.44	0.42	0.67
Prop. Democratic	0.00	0.00		1.00	1.00	
Prop. Republican	0.85	0.73	5.00	0.00	0.00	
Prop. Unaffilated	0.15	0.27	-5.00	0.00	0.00	
Prop. Incumbent	0.34	0.07	12.13	0.36	0.12	7.91
Prop. At-large Contest	0.52	0.58	-2.23	0.38	0.52	4.00
Election Year	2012.31	2011.70	2.28	2011.90	2011.62	0.84
Observations	494	651	1145	411	346	757
Panel B: Neighborhood Characteristics						
Median Income	54984	54413	0.36	50285	51447	-0.63
Prop. College Graduates	0.36	0.34	1.13	0.36	0.34	0.87
Avg. House Price	186968	175314	1.58	178772	172616	0.65
House Price Index	0.07	0.00	1.95	0.07	0.06	0.02
Prop. Urban Areas	0.41	0.45	-1.06	0.51	0.46	1.16
Observations	335	484	819	279	230	509
Panel C: School Characteristics						
Standardized Score	0.06	0.04	1.02	-0.01	-0.01	-0.17
Prop. Black Students	0.15	0.17	-2.05	0.22	0.21	0.48
Prop. Hispanic Students	0.12	0.12	0.02	0.10	0.11	-1.20
Prop. White Students	0.68	0.65	2.19	0.62	0.61	0.57
Prop. EDS Students	0.54	0.54	0.29	0.59	0.60	-0.49
Observations	317	440	757	291	228	519

Notes: This table reports the summary statistics of school board election winners and losers for non-Democratic and Democratic candidates. Columns 3 and 6 report the t-statistics of the differences in the characteristics within each group. In Panel B, median income and share of college graduates are from IPUMS NHGIS. The average house price and median income are in 2010 inflation-adjusted dollars. In Panel C, each observation is the neighborhood school for a candidate. Economically disadvantaged students are defined by students in the free lunch program.

Table A.5: Balance Checks by Political Identity

Panel A: Candidates Characteristics at the Time of Election, Non-Democratic

	(1) Age	(2) Female	(3) Incumbent	(4) Price Index Level	(5) Log Price Level	(6) Log Price Change	(7) Log Median Income
Vote margin > 0	1.638	0.070	0.151	0.006	0.058	-0.018	-0.041
	(1.957)	(0.088)	(0.066)	(0.036)	(0.115)	(0.032)	(0.070)

Panel B: Contest Characteristics, Non-Democratic

	(1) Contest At-large	(2) Log Total Votes	(3) # Winners	(4) General Election Date
Vote margin > 0	0.011	0.300	0.365	-0.001
	(0.073)	(0.280)	(0.229)	(0.051)

Panel C: School District Characteristics at the Time of Election, Non-Democratic

	(1) Share(Black Students)	(2) Share(Econ. Disadv. Students)	(3) Share(Urban Area)	(4) # Board Members	(5) Share(Black Board Members)
Vote margin > 0	0.029	0.004	0.026	-0.088	0.048
	(0.024)	(0.026)	(0.086)	(0.328)	(0.062)

Panel D: Candidates Characteristics at the Time of Election, Democratic

	(1) Age	(2) Female	(3) Incumbent	(4) Price Index Level	(5) Log Price Level	(6) Log Price Change	(7) Log Median Income
Vote margin > 0	0.427	-0.021	0.071	-0.061	-0.068	-0.027	-0.042
	(1.983)	(0.106)	(0.079)	(0.043)	(0.112)	(0.027)	(0.074)

Panel E: Contest Characteristics, Democratic

	(1) Contest At-large	(2) Log Total Votes	(3) # Winners	(4) General Election Date
Vote margin > 0	0.031	-0.050	-0.015	-0.075
	(0.089)	(0.281)	(0.302)	(0.071)

Panel F: School District Characteristics at the Time of Election, Democratic

	(1) Share(Black Students)	(2) Share(Econ. Disadv. Students)	(3) Share(Urban Area)	(4) # Board Members	(5) Share(Black Board Members)
Vote margin > 0	-0.054	0.001	-0.096	-0.187	0.041
	(0.032)	(0.036)	(0.102)	(0.439)	(0.075)

Notes: Regression discontinuity estimates are computed using a local linear regression. The bandwidths are set at the optimal level of the main analysis in Table 6. All regressions include election year fixed effects. Columns 2 and 3 of Panels A and D are the estimates of the indicators of female and an incumbent school board member. "Price Index Level" is the level of price index value from equation 2 for one year prior to the elections. Similarly, "Log Price Level" indicates the log of average house prices at the block group level one year prior to the elections. "Log Price Change" indicates the change in the log of average house prices during the four pre-election years. The median income at the Census block group level is from IPUMS NHGIS 2010. In Panels C and F, 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 as those who are in the free lunch program. Column 3 indicates the estimates of the indicators of urban clusters and urbanized areas.

Table A.6: Placebo Effects of Election on Past Neighborhood Home Prices by Political Identity

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	0.015	0.018	0.022	-0.004
	(0.043)	(0.038)	(0.037)	(0.034)
Optimal BW	0.126	0.139	0.207	0.205
Obs. within BW	454	480	300	298
Obs.	869	869	526	526
Controls	None	Full	None	Full

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four *pre-election* years and subtracting the average over the four years *preceding* the four pre-election years. The bandwidths are set at the MSE-optimal level following Calonico, Cattaneo and Farrell (2020). 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) for columns 2 and 4. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.7: Effects of Election on Neighborhood Home Prices for Republican Candidates

	(1)	(2)
	Price Index	Price Index
Vote margin > 0	0.056	0.047
	(0.037)	(0.031)
Optimal BW	0.108	0.110
Obs. within BW	317	317
Obs.	630	630
Controls	None	Full

Notes: RD estimates are computed using local linear regressions for Republican candidates. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level for the post-election period following Calonico, Cattaneo and Farrell (2020). The controls in column 2 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.8: Effects of Election on Nearby Neighborhood Home Prices by Political Identity

	Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)
	Census Tract	Leave-one-out	Census Tract	Leave-one-out
Vote margin > 0	0.021	-0.004	-0.012	0.011
	(0.022)	(0.028)	(0.030)	(0.032)
Optimal BW	0.109	0.109	0.125	0.131
Obs. within BW	396	383	227	229
Obs.	794	762	501	484
Controls	Full	Full	Full	Full

Notes: RD estimates are computed using local linear regressions. The outcome in columns 1 and 3 is calculated by taking the average of the Census-tract-level house price index up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. We use the average of the block-group-level house price indices at the Census tract level, excluding the house price index of the candidate's own block group, when constructing the outcome variable in columns 2 and 4. The bandwidths are set at the MSE-optimal level for the post-election period following Calonico, Cattaneo and Farrell (2020). 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) in all columns. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.9: Effects of Election on Neighborhood Home Prices Including Candidates with Multiple Matches

	All Candidates		Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)
	Price Index	Price Index	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	0.033	0.024	0.068**	0.057**	-0.009	-0.004
	(0.024)	(0.022)	(0.033)	(0.028)	(0.040)	(0.039)
Max. # Matches	1	5	1	5	1	5
Obs. within BW	656	924	403	570	206	294
Obs.	1,295	1,813	794	1,135	501	678

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the optimal level of the main analysis in Tables 4 and 6. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.10: Effects of Election on Neighborhood Home Prices Including Minority Candidates

	All Candidates		Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)
	Price Index	Price Index	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	0.015	0.001	0.061*	0.050*	-0.022	-0.029
	(0.030)	(0.028)	(0.036)	(0.030)	(0.046)	(0.046)
Optimal BW	0.149	0.140	0.102	0.125	0.235	0.183
Obs. within BW	934	909	415	456	530	478
Obs.	1,741	1,741	860	860	881	881
Controls	None	Full	None	Full	None	Full

Notes: RD estimates are computed using local linear regressions including minority (Black or Hispanic) candidates. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level for the post-election period following Calonico, Cattaneo and Farrell (2020). The controls in columns 2, 4, and 6 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) for columns 2, 4, and 6. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.11: Effects of Election on Neighborhood Home Prices by Demographics

	Female		Male		Younger		Older	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Price Index	Price Index	Price Index	Price Index	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	0.063*	0.014	0.043	0.044	0.062	0.031	0.059*	0.032
	(0.037)	(0.034)	(0.036)	(0.034)	(0.040)	(0.034)	(0.035)	(0.031)
Group	Female	Female	Male	Male	Younger	Younger	Older	Older
Optimal BW	0.134	0.111	0.142	0.125	0.106	0.103	0.134	0.135
Obs. within BW	308	283	386	354	279	274	356	360
Obs.	537	537	758	758	584	584	711	711
Controls	None	Full	None	Full	None	Full	None	Full

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level for the post-election period following Calonico, Cattaneo and Farrell (2020). The age groups are split at 47, the median age of the candidates. The controls in columns 2, 4, 6, and 8 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) for columns 2, 4, 6, and 8. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.12: Effects of Election on Neighborhood Home Prices with Varying Bandwidths

	(1)	(2)	(3)	(4)
	Price Index	Price Index	Price Index	Price Index
Panel A: All Candidates				
Vote margin > 0	0.041	0.031	0.033	0.030
	(0.031)	(0.025)	(0.022)	(0.020)
Bandwidth	0.05	0.10	0.15	0.20
Obs. within BW	367	587	716	806
Obs.	1,295	1,295	1,295	1,295
Panel B: Non-Democratic Candidates				
Vote margin > 0	0.094**	0.059**	0.062**	0.052**
	(0.038)	(0.030)	(0.027)	(0.023)
Bandwidth	0.05	0.10	0.15	0.20
Obs. within BW	230	382	470	525
Obs.	794	794	794	794
Panel C: Democratic Candidates				
Vote margin > 0	-0.022	-0.005	-0.008	0.005
	(0.042)	(0.040)	(0.037)	(0.034)
Bandwidth	0.05	0.10	0.15	0.20
Obs. within BW	137	205	246	281
Obs.	501	501	501	501

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at 0.05, 0,10, 0.15, and 0.20. 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) in all columns. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.13: Effects of Election on Neighborhood Home Prices without Outliers

Panel A: Bounded by Average House Prices

	All Candidates		Non-Democratic		Democratic	
	Price Index	Log Price	Price Index	Log Price	Price Index	Log Price
All Observations	0.033	0.037	0.068**	0.061*	-0.002	0.019
	(0.024)	(0.026)	(0.033)	(0.035)	(0.040)	(0.043)
1st pct. $\leq$ House Price $\leq$ 99th pct.	0.041*	0.047*	0.067**	0.062*	0.014	0.038
	(0.024)	(0.026)	(0.033)	(0.035)	(0.038)	(0.040)
5th pct. $\leq$ House Price $\leq$ 95th pct.	0.035	0.044*	0.056*	0.056	0.008	0.030
	(0.024)	(0.027)	(0.033)	(0.036)	(0.039)	(0.041)
10th pct. $\leq$ House Price $\leq$ 90th pct.	0.025	0.032	0.047	0.042	-0.007	0.011
	(0.026)	(0.029)	(0.036)	(0.040)	(0.041)	(0.043)

Panel B: Bounded by Number of Transactions

	All Candidates		Non-Democratic		Democratic	
	Price Index	Log Price	Price Index	Log Price	Price Index	Log Price
All Observations	0.033	0.037	0.068**	0.061*	-0.009	0.013
	(0.024)	(0.026)	(0.033)	(0.035)	(0.040)	(0.043)
1st pct. $\leq$ House Price $\leq$ 99th pct.	0.027	0.030	0.054*	0.047	-0.009	0.013
	(0.023)	(0.025)	(0.028)	(0.030)	(0.040)	(0.043)
5th pct. $\leq$ House Price $\leq$ 95th pct.	0.030	0.030	0.054*	0.047	0.015	0.036
	(0.025)	(0.027)	(0.030)	(0.032)	(0.042)	(0.045)
10th pct. $\leq$ House Price $\leq$ 90th pct.	0.031	0.027	0.037	0.023	0.021	0.043
	(0.026)	(0.027)	(0.029)	(0.031)	(0.046)	(0.050)

Notes: RD estimates are computed using local linear regressions. The outcomes are calculated by taking the averages of the house price index in equation 2 (columns 1, 3, and 5) and of the log of block-group-level house prices (columns 2, 4, and 6) up to four post-election years (the term length) and subtracting the averages over the same pre-election horizon. Panel A reports the estimates excluding outliers regarding the average house price, and Panel B reports the parallel estimates after excluding outliers regarding the number of house transactions. For example, 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. The bandwidths are set at the optimal level of the main results in Table 4 and Table 6. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.14: Effects of Election on Neighborhood Home Prices Constructed with Different Priors

	All Candidates		Non-Democratic		Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)
	Price Index	Price Index	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	0.034	0.041	0.068**	0.069**	-0.009	-0.019
	(0.024)	(0.027)	(0.032)	(0.031)	(0.040)	(0.048)
Common Prior	County Prior	Tract Prior	County Prior	Tract Prior	County Prior	Tract Prior
Optimal BW	0.126	0.119	0.114	0.112	0.101	0.119
Obs. within BW	657	630	403	397	206	218
Obs.	1,295	1,290	794	790	501	500
Controls	Full	Full	Full	Full	Full	Full

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. When constructing the house price index in equation 2, we use the average residualized house price at the county level as the common prior in columns 1, 3, and 5, while that at the Census Tract is used to construct the outcome in columns 2, 4, and 6. The bandwidths are set at the MSE-optimal level for the post-election period following Calonico, Cattaneo and Farrell (2020). 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.15: Effects of Election on Neighborhood Home Prices by Election Type

	(1)	(2)	(3)	(4)
	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	-0.010	-0.112	-0.015	0.070
	(0.122)	(0.160)	(0.057)	(0.069)
Optimal BW	0.105	0.104	0.063	0.121
Obs. within BW	62	48	50	58
Obs.	114	108	92	106
Marginal Winner	Democratic	Democratic	Non-Democratic	Non-Democratic
Marginal Loser	Democratic	Non-Democratic	Democratic	Non-Democratic

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level for the post-election period following Calonico, Cattaneo and Farrell (2020). The controls include election year fixed effects. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.16: Effects of Election on Neighborhood Home Prices for Non-Democratic Marginal Sample with Fixed Effects

	(1)	(2)
	Price Index	Price Index
Vote margin > 0	0.099**	0.082***
	(0.042)	(0.030)
Optimal BW	0.104	0.041
Obs. within BW	193	112
Obs.	416	416
Election-Year FE	Yes	No
Czone-Election-Year FE	No	Yes
District-Urban FE	No	Yes

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level for the post-election period following Calonico, Cattaneo and Farrell (2020). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.17: Effects of Election on School Outcomes in Neighborhood Schools Including Minority Candidates

Panel A: Non-Democratic, School Outcomes

	(1) Prop. White Students	(2) Prop. Black Students	(3) Prop. Hispanic Students	(4) Prop. EDS Students	(5) Prop. White Movers	(6) Prop. Black Movers	(7) Prop. Hispanic Movers	(8) Prop. EDS Movers
Vote margin > 0	0.016***	-0.014**	-0.005	0.008	0.022**	-0.011*	-0.010	-0.004
	(0.006)	(0.005)	(0.004)	(0.019)	(0.009)	(0.007)	(0.006)	(0.011)
Mean	0.632	0.188	0.122	0.531	0.630	0.181	0.126	0.536
Obs. within BW	350	350	350	350	330	330	330	330
Obs.	679	679	679	679	635	635	635	635

Panel B: Non-Democratic, Boundary Shift

Boundary Shift w.r.t.	(1) White Students	(2) Black Students	(3) EDS Students	(4) White Population	(5) Black Population	(6) Log Median Income
Vote margin > 0	0.019**	-0.015**	-0.020**	0.014*	-0.012*	0.034**
	(0.009)	(0.007)	(0.009)	(0.007)	(0.006)	(0.015)
Mean	0.649	0.181	0.528	0.764	0.156	10.737
Obs. within BW	276	276	276	276	276	276
Obs.	493	493	493	493	493	493

Panel C: Democratic, School Outcomes

	(1) Prop. White Students	(2) Prop. Black Students	(3) Prop. Hispanic Students	(4) Prop. EDS Students	(5) Prop. White Movers	(6) Prop. Black Movers	(7) Prop. Hispanic Movers	(8) Prop. EDS Movers
Vote margin > 0	-0.003	-0.001	-0.002	-0.004	0.001	-0.000	-0.007	0.004
	(0.005)	(0.005)	(0.004)	(0.025)	(0.007)	(0.007)	(0.005)	(0.012)
Mean	0.491	0.329	0.113	0.632	0.488	0.323	0.117	0.615
Obs. within BW	297	297	297	297	286	286	286	286
Obs.	723	723	723	723	687	687	687	687

Panel D: Democratic, Boundary Shift

Boundary Shift w.r.t.	(1) White Students	(2) Black Students	(3) EDS Students	(4) White Population	(5) Black Population	(6) Log Median Income
Vote margin > 0	-0.013*	0.015**	0.011	-0.011*	0.013**	-0.018
	(0.007)	(0.007)	(0.008)	(0.006)	(0.006)	(0.013)
Mean	0.502	0.330	0.624	0.629	0.281	10.606
Obs. within BW	230	230	230	230	230	230
Obs.	566	566	566	566	566	566

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the averages at the neighborhood middle school up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.18: Effects of Election on School Outcomes in Neighborhood Schools with Repicking Optimal Bandwidths

Panel A: Non-Democratic, School Outcomes

	(1) Prop. White Students	(2) Prop. Black Students	(3) Prop. Hispanic Students	(4) Prop. EDS Students	(5) Prop. White Movers	(6) Prop. Black Movers	(7) Prop. Hispanic Movers	(8) Prop. EDS Movers
Vote margin > 0	0.012**	-0.008	-0.006*	0.017	0.022**	-0.009	-0.012*	0.001
	(0.006)	(0.005)	(0.004)	(0.020)	(0.010)	(0.007)	(0.007)	(0.011)
Mean	0.644	0.178	0.121	0.528	0.642	0.171	0.124	0.534
Obs. within BW	367	358	346	288	283	311	276	279
Obs.	630	630	630	630	586	586	586	586

Panel B: Non-Democratic, Boundary Shift

Boundary Shift w.r.t.	(1) White Students	(2) Black Students	(3) EDS Students	(4) White Population	(5) Black Population	(6) Log Median Income
Vote margin > 0	0.018*	-0.013**	-0.017*	0.013*	-0.010	0.034**
	(0.009)	(0.007)	(0.009)	(0.008)	(0.007)	(0.016)
Mean	0.657	0.173	0.528	0.771	0.149	10.732
Obs. within BW	271	252	264	252	244	257
Obs.	461	461	461	461	461	461

Panel C: Democratic, School Outcomes

	(1) Prop. White Students	(2) Prop. Black Students	(3) Prop. Hispanic Students	(4) Prop. EDS Students	(5) Prop. White Movers	(6) Prop. Black Movers	(7) Prop. Hispanic Movers	(8) Prop. EDS Movers
Vote margin > 0	-0.002	0.005	-0.002	-0.006	0.003	0.006	-0.005	0.014
	(0.007)	(0.005)	(0.004)	(0.024)	(0.009)	(0.008)	(0.006)	(0.013)
Mean	0.605	0.227	0.111	0.583	0.601	0.221	0.114	0.573
Obs. within BW	171	226	212	167	163	183	182	197
Obs.	400	400	400	400	374	374	374	374

Panel D: Democratic, Boundary Shift

Boundary Shift w.r.t.	(1) White Students	(2) Black Students	(3) EDS Students	(4) White Population	(5) Black Population	(6) Log Median Income
Vote margin > 0	-0.007	0.009	0.008	-0.003	0.004	-0.023
	(0.009)	(0.007)	(0.009)	(0.006)	(0.007)	(0.017)
Mean	0.619	0.224	0.583	0.730	0.194	10.640
Obs. within BW	143	169	176	163	169	170
Obs.	308	308	308	308	308	308

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the averages at the neighborhood middle school up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level following Calonico, Cattaneo and Farrell (2020). 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.19: Effects of Election on School Outcomes in Neighborhood Schools for Republican Candidates

Panel A: School Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Prop. White Students	Prop. Black Students	Prop. Hispanic Students	Prop. EDS Students	Prop. White Movers	Prop. Black Movers	Prop. Hispanic Movers	Prop. EDS Movers
Vote margin > 0	0.015**	-0.010*	-0.004	0.017	0.020*	-0.009	-0.008	-0.005
	(0.007)	(0.005)	(0.004)	(0.020)	(0.011)	(0.007)	(0.007)	(0.011)
Mean	0.653	0.171	0.120	0.527	0.651	0.164	0.122	0.534
Obs. within BW	263	263	263	263	248	248	248	248
Obs.	506	506	506	506	471	471	471	471

Panel B: Boundary Shift

	(1)	(2)	(3)	(4)	(5)	(6)
Boundary Shift w.r.t.	White Students	Black Students	EDS Students	White Population	Black Population	Log Median Income
Vote margin > 0	0.012	-0.010	-0.014	0.008	-0.006	0.029*
	(0.010)	(0.007)	(0.010)	(0.007)	(0.006)	(0.016)
Mean	0.660	0.171	0.531	0.774	0.148	10.729
Obs. within BW	212	212	212	212	212	212
Obs.	375	375	375	375	375	375

Notes: RD estimates are computed using local linear regressions for Republican candidates. The outcome variables are calculated by taking the averages at the neighborhood middle school up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Appendix Table A.7. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.20: Placebo Effects of Election on Past School Outcomes in Neighborhood Schools

Panel A: Non-Democratic, School Outcomes

	(1) Prop. White Students	(2) Prop. Black Students	(3) Prop. Hispanic Students	(4) Prop. EDS Students	(5) Prop. White Movers	(6) Prop. Black Movers	(7) Prop. Hispanic Movers	(8) Prop. EDS Movers
Vote margin > 0	-0.015	-0.003	0.014	0.006	-0.002	-0.015	0.010	-0.002
	(0.028)	(0.018)	(0.017)	(0.021)	(0.029)	(0.018)	(0.018)	(0.022)
Mean	0.662	0.189	0.099	0.477	0.657	0.176	0.105	0.517
Obs. within BW	334	334	334	334	314	314	314	314
Obs.	662	662	662	662	621	621	621	621

Panel B: Non-Democratic, Boundary Shift

Boundary Shift w.r.t.	(1) White Students	(2) Black Students	(3) EDS Students	(4) White Population	(5) Black Population	(6) Log Median Income
Vote margin > 0	-0.016	0.004	0.006	-0.017	0.007	-0.039
	(0.027)	(0.014)	(0.021)	(0.018)	(0.014)	(0.030)
Mean	0.657	0.173	0.529	0.771	0.149	10.732
Obs. within BW	304	304	304	304	304	304
Obs.	590	590	590	590	590	590

Panel C: Democratic, School Outcomes

	(1) Prop. White Students	(2) Prop. Black Students	(3) Prop. Hispanic Students	(4) Prop. EDS Students	(5) Prop. White Movers	(6) Prop. Black Movers	(7) Prop. Hispanic Movers	(8) Prop. EDS Movers
Vote margin > 0	-0.024	-0.011	0.027*	0.018	-0.025	-0.012	0.025	0.004
	(0.030)	(0.023)	(0.015)	(0.029)	(0.032)	(0.024)	(0.017)	(0.032)
Mean	0.617	0.244	0.089	0.523	0.606	0.237	0.096	0.559
Obs. within BW	177	177	177	177	169	169	169	169
Obs.	435	435	435	435	409	409	409	409

Panel D: Democratic, Boundary Shift

Boundary Shift w.r.t.	(1) White Students	(2) Black Students	(3) EDS Students	(4) White Population	(5) Black Population	(6) Log Median Income
Vote margin > 0	-0.047	0.008	0.038*	-0.020	-0.000	-0.024
	(0.030)	(0.021)	(0.023)	(0.022)	(0.018)	(0.046)
Mean	0.618	0.225	0.583	0.730	0.195	10.640
Obs. within BW	148	148	148	148	148	148
Obs.	391	391	391	391	391	391

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the averages at the neighborhood middle school up to four *pre-election* years and subtracting the average over the four years *preceding* the four pre-election years. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.21: Effects of Election on Teacher Composition in Neighborhood Schools

	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
Panel A: Middle School								
Vote margin > 0	-0.243	0.009	-0.001	-0.008	0.355	-0.004	-0.015	0.019
	(0.353)	(0.007)	(0.015)	(0.016)	(0.337)	(0.012)	(0.017)	(0.018)
Pre-election Mean	13.632	0.077	0.318	0.605	13.984	0.085	0.307	0.608
Panel B: Elementary School								
Vote margin > 0	-0.086	-0.004	-0.013	0.017	0.292	0.013	-0.046***	0.033*
	(0.285)	(0.008)	(0.014)	(0.015)	(0.331)	(0.010)	(0.018)	(0.018)
Pre-election Mean	13.758	0.070	0.320	0.609	14.027	0.073	0.307	0.620
Obs. within BW	365	365	365	365	198	198	198	198
Obs.	704	704	704	704	486	486	486	486

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the average of the school-level average experience years (column 1) and that of the proportions of teachers with the specified experience years (columns 2 through 4) at the neighborhood school (middle school for Panel A and elementary school for Panel B) up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Table A.22: Effects of Election on Classroom Sorting in Neighborhood Schools

	Non-Democratic				Democratic			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IQR(White)	IQR(Black)	IQR(Hispanic)	Sh(Adv. Courses)	IQR(White)	IQR(Black)	IQR(Hispanic)	Sh(Adv. Courses)
Panel A: Middle School								
Vote margin > 0	0.003	0.003	-0.002	0.010	-0.002	0.004	-0.008	-0.001
	(0.007)	(0.008)	(0.005)	(0.011)	(0.011)	(0.008)	(0.008)	(0.013)
Pre-election Mean	0.165	0.118	0.097	0.026	0.171	0.134	0.090	0.030
Panel B: Elementary School								
Vote margin > 0	0.016	0.008	-0.018	-0.000	0.003	-0.013	0.018	-0.002
	(0.021)	(0.010)	(0.015)	(0.001)	(0.025)	(0.013)	(0.013)	(0.001)
Pre-election Mean	0.145	0.086	0.094	0.001	0.148	0.100	0.090	0.000
Obs. within BW	338	338	338	338	176	176	176	176
Obs.	640	640	640	641	429	429	429	430

Notes: RD estimates are computed using local linear regressions. The outcome variables are calculated by taking the average of the interquartile ranges (IQR) of student composition across classes (columns 1 through 3 and 5 through 7) and that of the share of advanced courses (columns 4 and 8) at the neighborhood school (middle school for Panel A and elementary school for Panel B) up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. The observations are smaller than those in the estimation results of the house price index (Table 6) because of missing values in the dependent variables. 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). Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

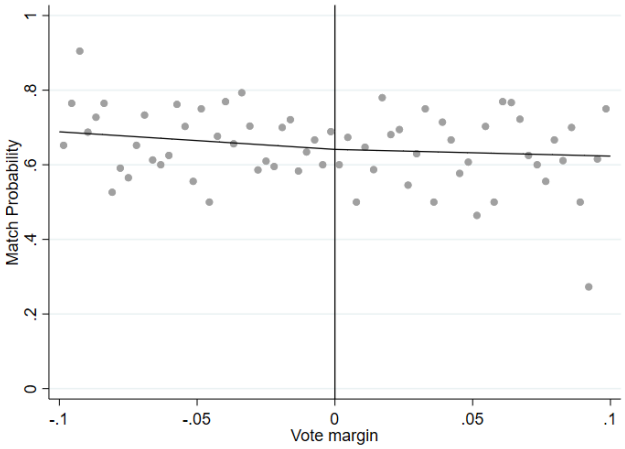
Table A.23: Effects of Election on Neighborhood Home Prices by Contextual Factors

	(1)	(2)	(3)	(4)	(5)
	Price Index	Price Index	Price Index	Price Index	Price Index
Panel A: Non-Democratic					
Vote margin > 0	0.055	0.016	0.123**	0.163***	0.123**
	(0.052)	(0.042)	(0.058)	(0.059)	(0.049)
(Vote margin > 0) × Indicator	0.019	0.083	-0.088	-0.117	-0.114*
	(0.062)	(0.060)	(0.068)	(0.071)	(0.063)
Indicator	-0.039	-0.028	0.100**	0.047	0.103*
	(0.057)	(0.048)	(0.047)	(0.051)	(0.053)
Obs. within BW	403	403	395	403	403
Obs.	794	794	780	794	794
Panel B: Democratic					
Vote margin > 0	0.007	-0.010	0.022	0.068	-0.001
	(0.065)	(0.076)	(0.102)	(0.081)	(0.064)
(Vote margin > 0) × Indicator	-0.035	-0.011	-0.062	-0.101	-0.015
	(0.085)	(0.092)	(0.115)	(0.092)	(0.093)
Indicator	-0.014	-0.044	0.039	0.111	-0.037
	(0.070)	(0.078)	(0.098)	(0.074)	(0.077)
Obs. within BW	206	206	203	206	206
Obs.	501	501	487	501	501
Indicator	Expanding District	Frequent Zone Shifts	Accessible to Charter Schools	At-large Contests	Higher-income

Notes: RD estimates are computed using local linear regressions. The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The bandwidths are set at the MSE-optimal level of the house price results in Table 6. "Higher-income" indicates that a candidate lives in the block group with an above-median average income in 2000 based on NHGIS (National Historical Geographic Information System). 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) in all specifications. Standard errors are clustered at the election level. The coefficients are statistically significant at the *10%, **5%, and ***1% levels.

Figure A.1: RD Plots of Match Rates

(a) Overall Match Rate



(b) Match Rate in 1st Trial

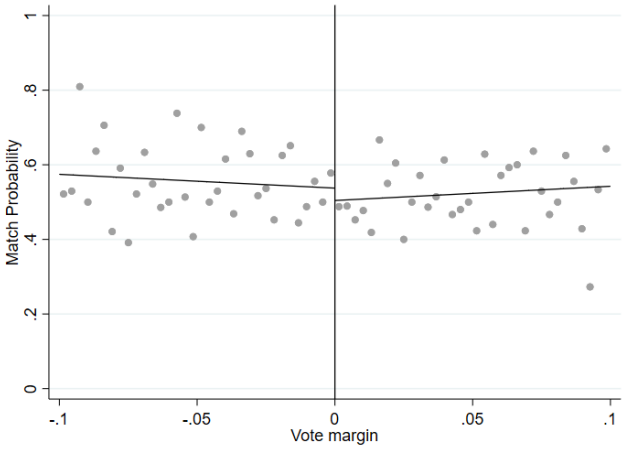


Figure A.2: RD Plot of Winning a School Board Position

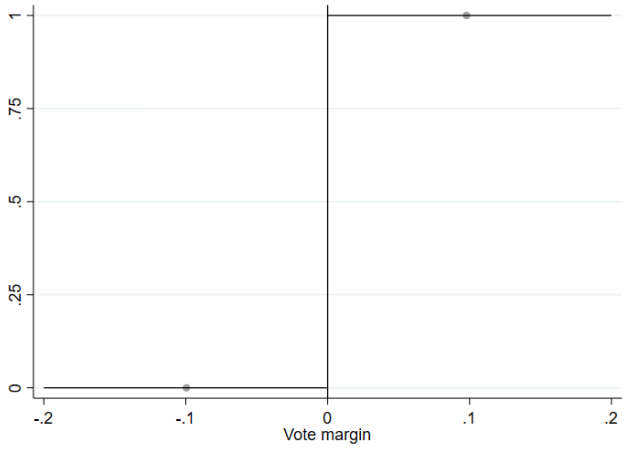
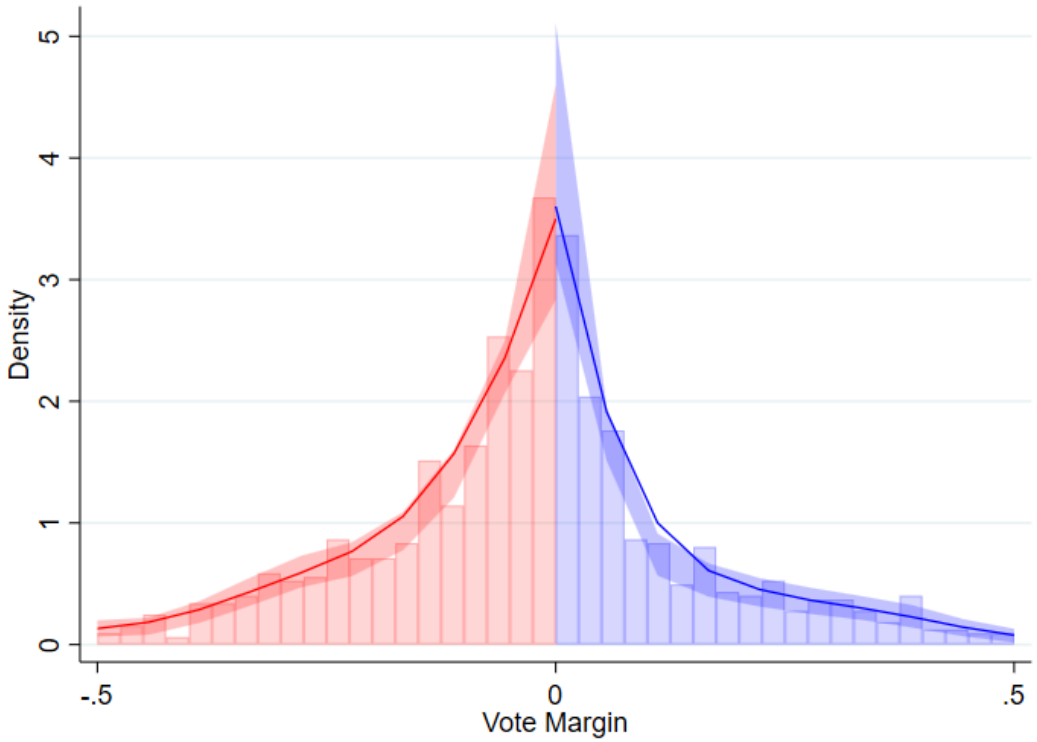


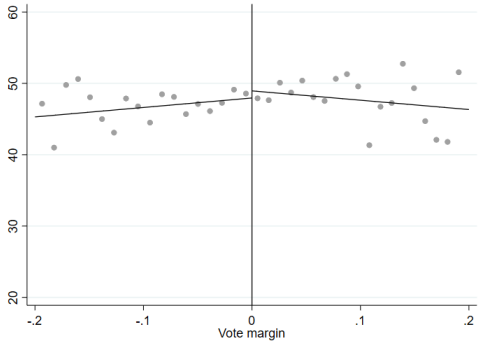
Figure A.3: Density of Vote Margin



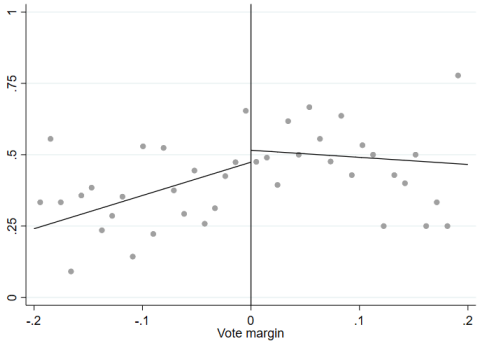
Notes: The figure depicts the distribution of vote margin around the cutoff that determines whether a candidate wins. The x-axis measures vote margin. For candidates successfully elected to the board, vote margin is defined by the difference between their vote share and that of the most popular loser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

Figure A.4: RD Plots of Covariates

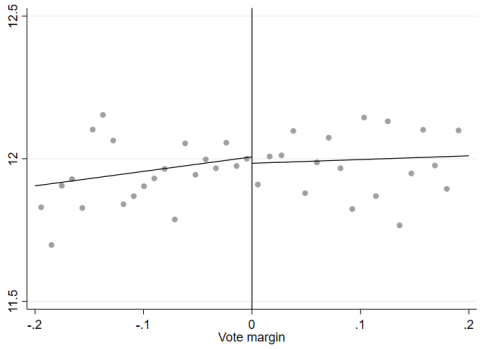
(a) Age



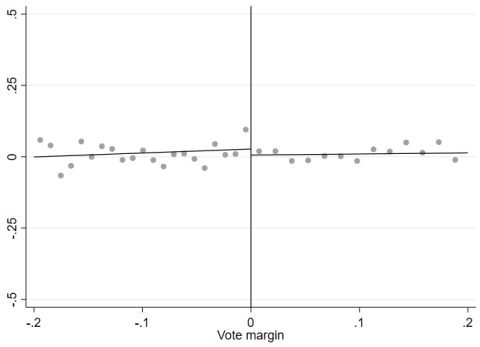
(b) Female



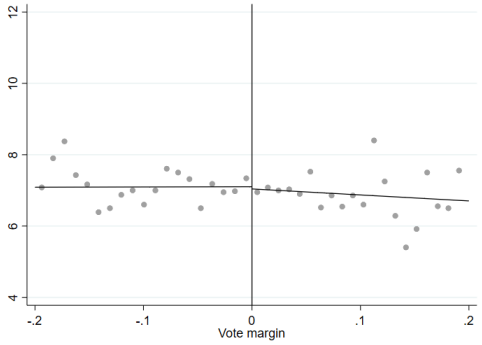
(c) House Price before Elections



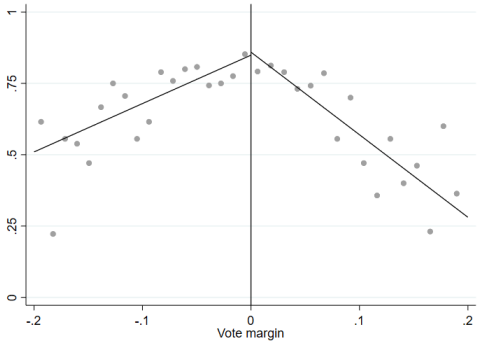
(d) Growth of House Price before Elections



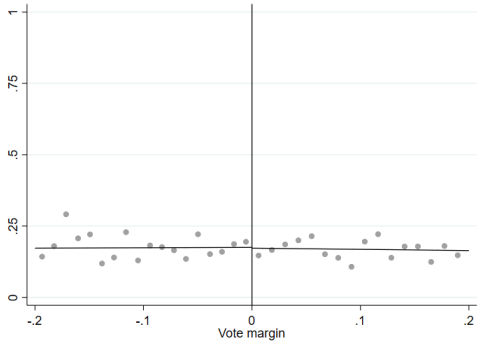
(e) Number of Board Members



(f) At-large Contest



(g) Prop. Black Students



(h) Prop. Students in Free Lunch Program

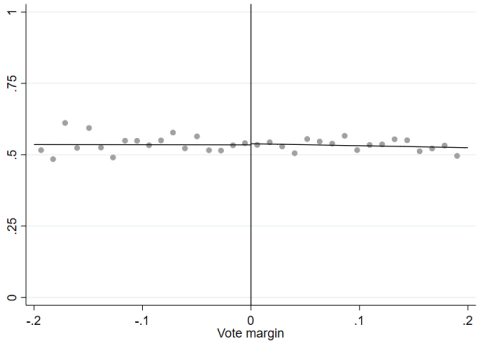
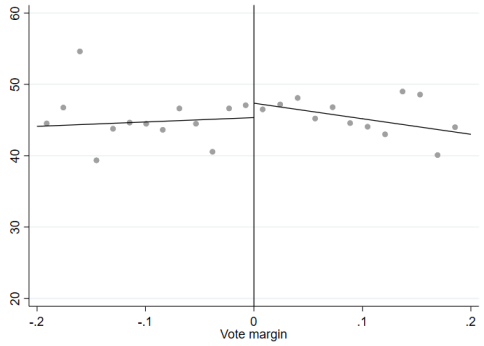
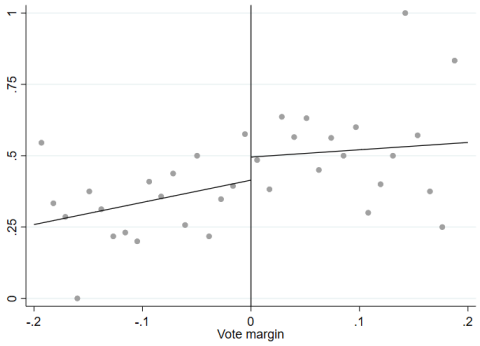


Figure A.5: RD Plots of Covariates for Non-Democratic Candidates

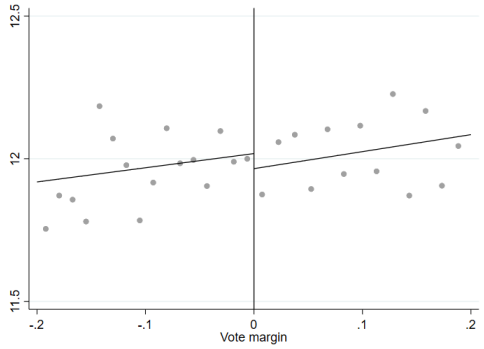
(a) Age



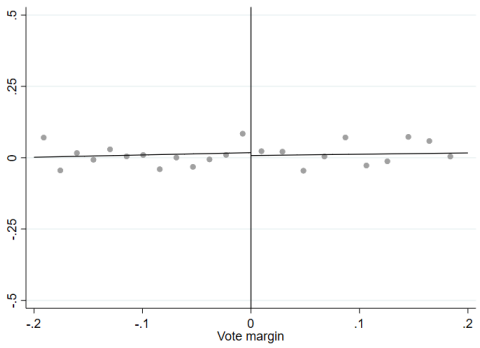
(b) Female



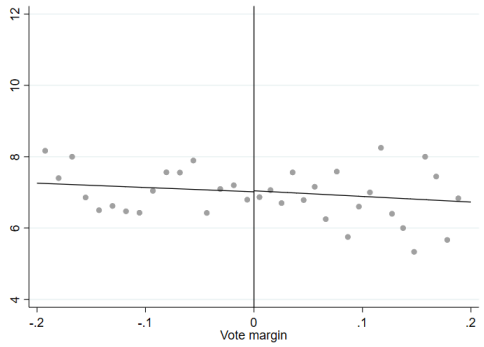
(c) House Price before Elections



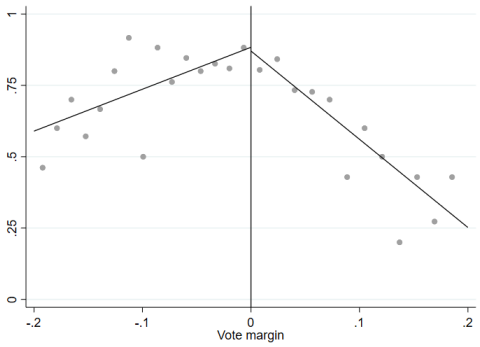
(d) Growth of House Price before Elections



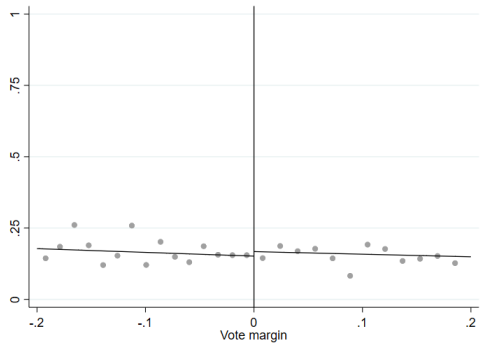
(e) Number of Board Members



(f) At-large Contest



(g) Prop. Black Students



(h) Prop. Students in Free Lunch Program

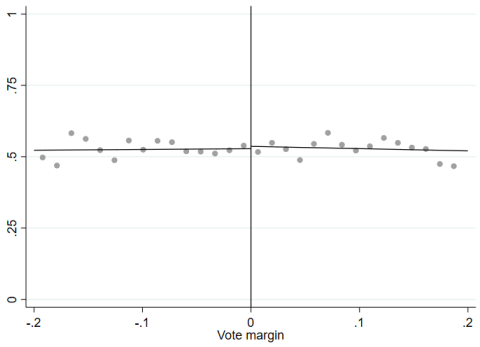
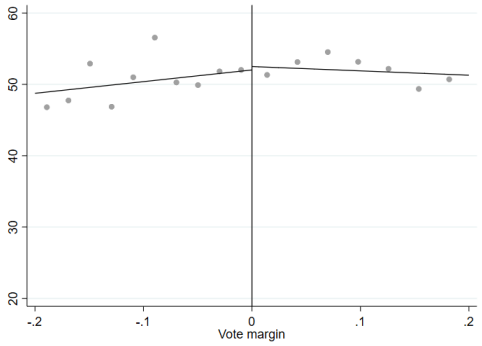
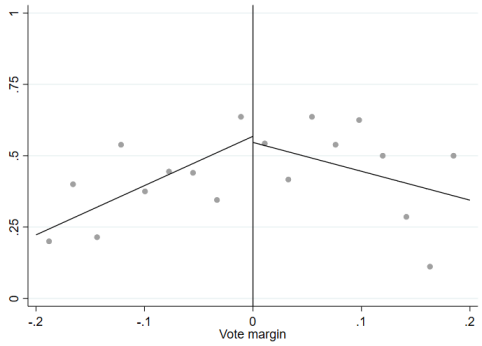


Figure A.6: RD Plots of Covariates for Democratic Candidates

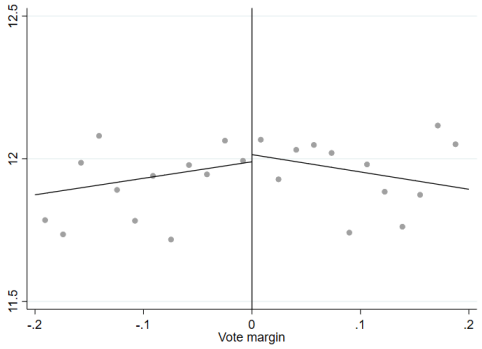
(a) Age



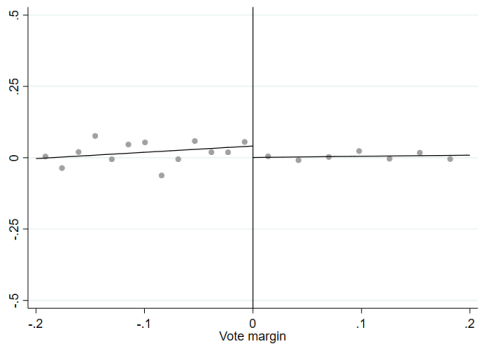
(b) Female



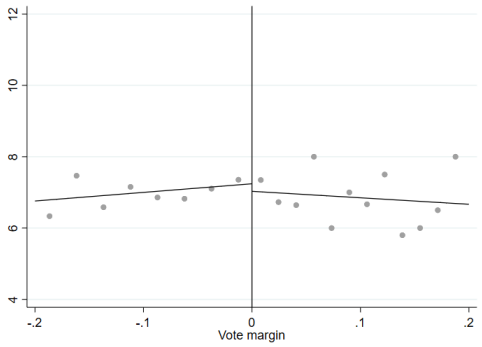
(c) House Price before Elections



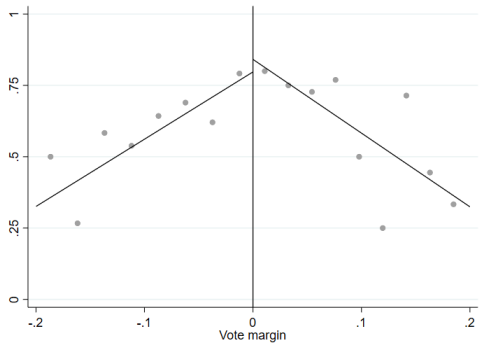
(d) Growth of House Price before Elections



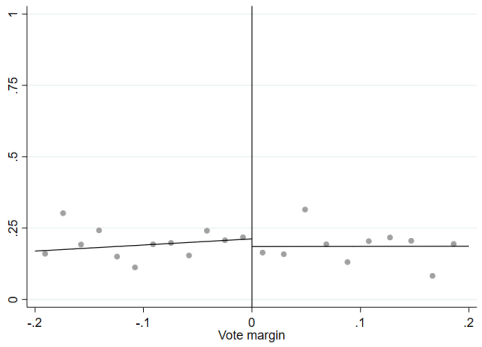
(e) Number of Board Members



(f) At-large Contest



(g) Prop. Black Students



(h) Prop. Students in Free Lunch Program

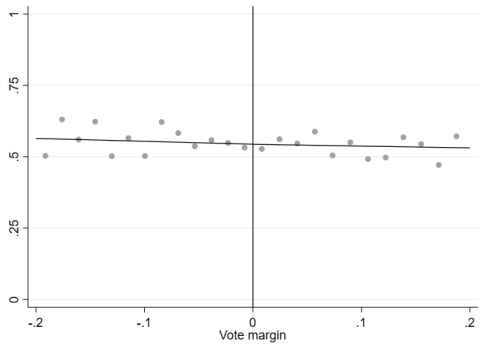
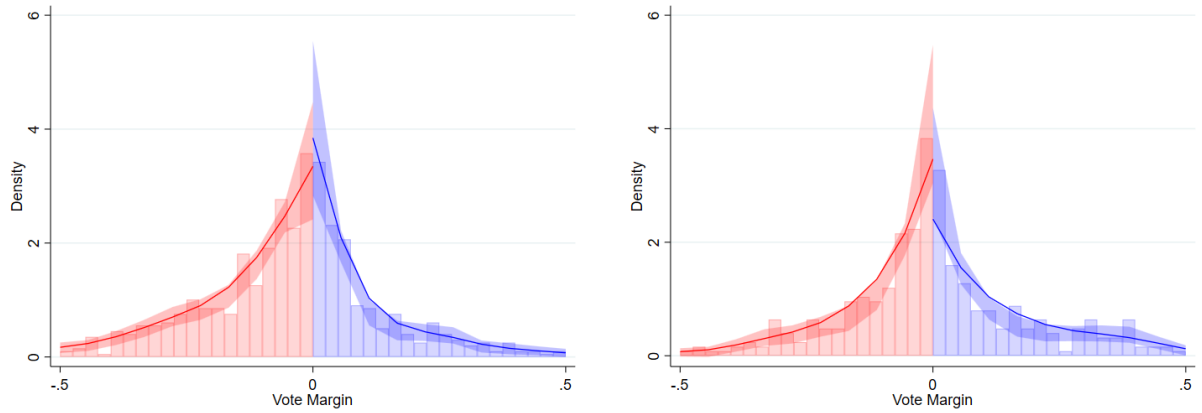


Figure A.7: Density of Vote Margin by Political Identity

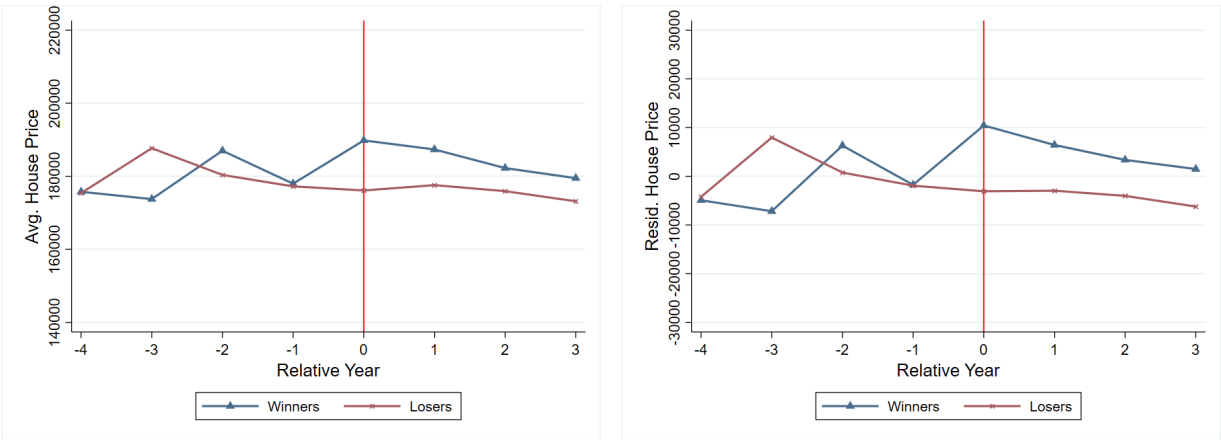
(a) Non-Democratic **(b)** Democratic



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 A.8: Trend of Average and Residualized House Price for Marginal Non-Democratic Candidates

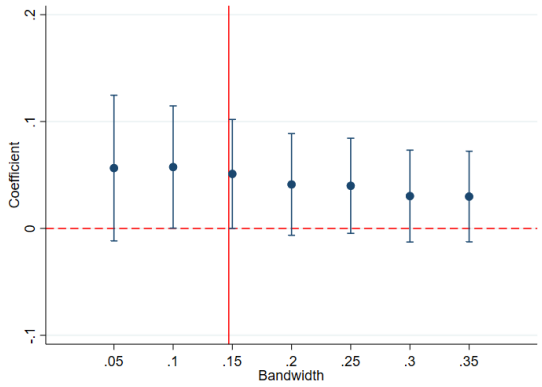
(a) Average Price **(b)** Residualized Price



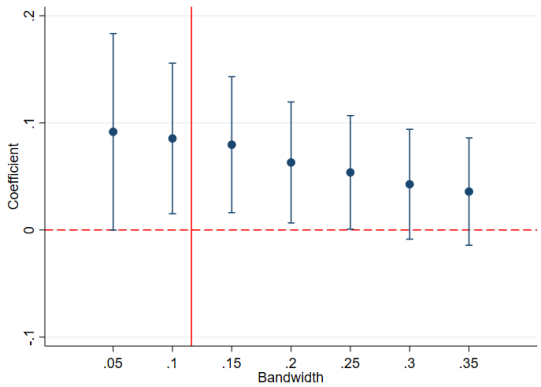
Notes: The figures report the trends of the house price index for non-Democratic winners (blue line) and losers (red line) within the optimal bandwidth of the baseline RD specification. The y-axis is the average house price (panel (a)) and the average of house prices after controlling for election years (panel (b)). The x-axis represents the year relative to the election year. The year of the election (period 0) is indicated by the red vertical line.

Figure A.9: Robustness Check with Other Bandwidths

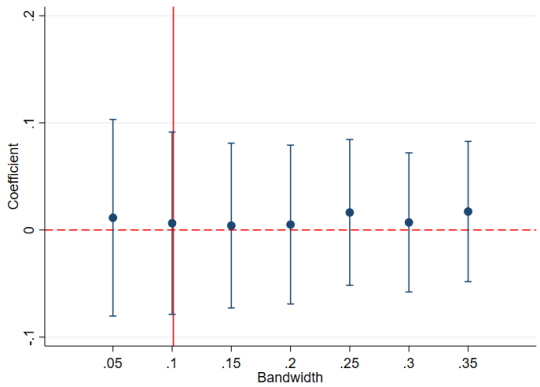
(a) All Candidates



(b) Non-Democratic Candidates



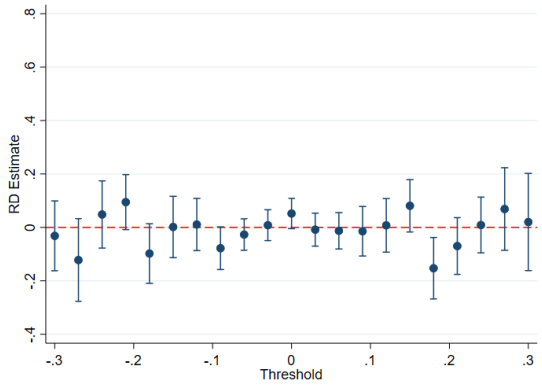
(c) Democratic Candidates



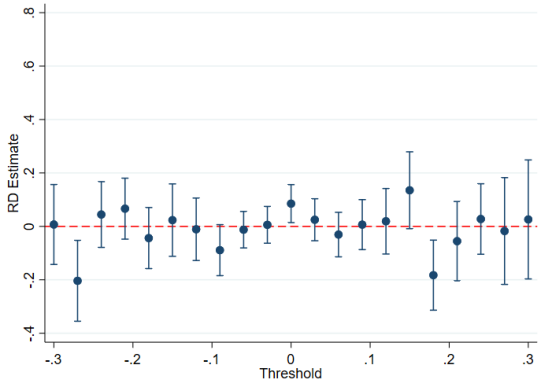
Notes: The figures show the RD estimates of the house price index with bandwidths varying from 0.05 to 0.35, denoted on the x-axis, for all (panel (a)), non-Democratic (panel (b)), and Democratic candidates (panel (c)). The outcome is calculated by taking the average of the house price index in equation 2 up to four post-election years (the term length) and subtracting the average over the same pre-election horizon. The solid red vertical lines indicate the MSE-optimal bandwidths in the baseline model. 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) in all specifications. Standard errors are clustered at the election level, and the confidence intervals are at 95%.

Figure A.10: Placebo Test with Other Thresholds

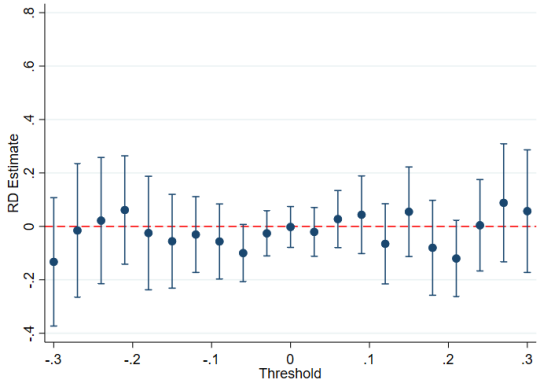
(a) All Candidates



(b) Non-Democratic Candidates



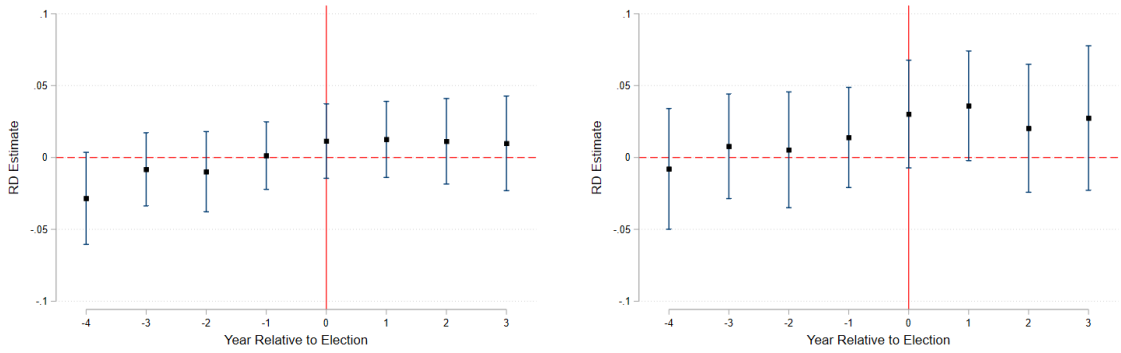
(c) Democratic Candidates



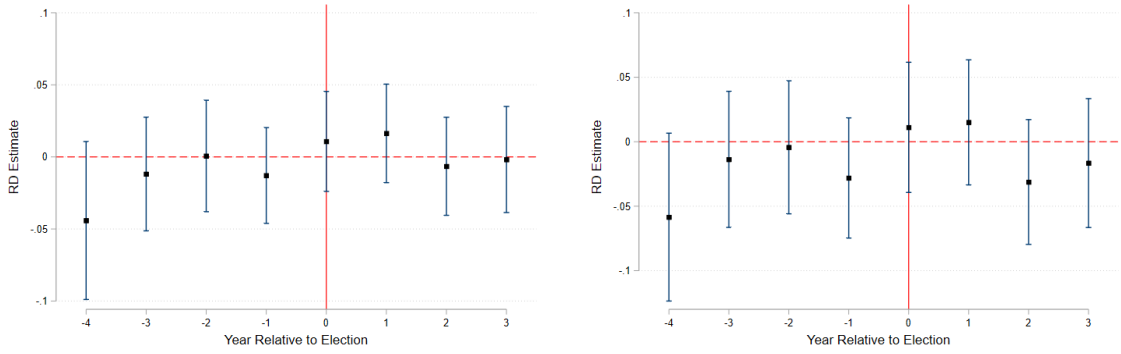
Notes: The figures show the RD estimates of the house price index with placebo thresholds varying from -0.3 to 0.3, denoted on the x-axis, for all (panel (a)), non-Democratic (panel (b)), and Democratic candidates (panel (c)). The effective observations are not enough out of this range. The bandwidths are set at the MSE-optimal level differently for the left and right sides of the thresholds. No controls are included to maximize the number of observations. Standard errors are clustered at the election level, and the confidence intervals are at 95%.

Figure A.11: Dynamic Effects of Election on Boundary Shift Composition by Political Identity

(a) Non-Democratic, Prop. of White Population **(b)** Non-Democratic, Prop. of White Students



(c) Democratic, Prop. of White Population **(d)** Democratic, Prop. of White Students



Notes: This figure reports the estimates of boundary shift composition in terms of the proportions of the white population and students following equation 5 for non-Democratic and Democratic candidates. Period 0 indicates the year of election, and each point represents the RD estimate in each year relative to the election year with the optimal bandwidths in Table 6. 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). Standard errors are clustered at the election level. The confidence intervals are at 95%.