NBER WORKING PAPER SERIES

SELECTION INTO PUBLIC SERVICE: EVIDENCE FROM SCHOOL BOARD ELECTIONS

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

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2022, Revised May 2026

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Data Availability Statement: This paper draws on five data sources, three of which are publicly available and two of which are restricted. (1) School board election results are publicly available from the North Carolina State Board of Elections (NCSBE) at https://www.ncsbe.gov. (2) Voter registration annual snapshots for 2005–2018 are publicly distributed by NCSBE at https://www.ncsbe.gov/results-data/voter-registration-data. (3) Census block-group demographic, income, and racial-composition tabulations from the five-year American Community Survey for 2010–2014 are publicly available through IPUMS NHGIS at https://www.nhgis.org. (4) The Zillow Transaction and Assessment Dataset (ZTRAX) was provided under a research-use agreement that has since been discontinued; researchers interested in obtaining historical ZTRAX extracts should consult Zillow Research at https://www.zillow.com/research/ztrax. The results and opinions are those of the authors and do not reflect the position of Zillow Group. (5) Confidential administrative records on students, teachers, and schools were provided under restricted-data agreement by the North Carolina Education Research Data Center (NCERDC); access may be requested by filing a research application with NCERDC, hosted at the Duke University Center for Child and Family Policy (https://childandfamilypolicy.duke.edu). A fuller description of the data sources, sample-construction steps, and access procedures is provided in the Data Appendix at the start of the Online Appendix.

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JEL No. D72, H75, I24

ABSTRACT

In this paper, we use narrowly-decided elections and housing transactions data to test for private returns from public office in the context of school boards in the U.S. We find no detectable effect of election to the school board on home prices in the neighborhoods of winning candidates. Stratifying by political affiliation reveals that homes appreciate in winning Republicans' neighborhoods, but the estimate is not statistically robust. We do not find corresponding evidence for improvements in local school productivity, but we show that neighborhood public schools of Republican members also shift to serving more white students through attendance zone adjustments.

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I. 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 produces no detectable effect on average home values in winners' neighborhoods. Stratifying the sample by political affiliation, however, reveals suggestive heterogeneity: prices in the neighborhoods of narrowly elected Republicans increase by as much as 6% on average post-election (relative to those of narrowly rejected Republicans), though the estimate's statistical significance is sensitive to specification. This effect is highly

local; while we detect appreciation when a neighborhood is defined by the candidate's Census block group, estimates do not detect impacts on other block groups belonging to the same Census tract.[2] We show that a price increase in the neighborhoods of Republican 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 Republican 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 (for example, covariate balance within the bandwidth, no bunching). In addition, we stratify the sample to estimate separate effects for Republican 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]

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 (that is, no placebo effects).

A positive impact of winning a school board election on home prices could be 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, by using geocoded student addresses, we

construct measures of student composition by neighborhood and 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 to construct school-level variables summarizing student demographics over time. In addition, we use the existing attributes of the neighborhoods inhabited by students attending each candidate's neighborhood schools to capture qualitative changes in attendance zone boundaries.

We find that any housing value appreciation in Republican 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 public schools in neighborhoods represented by Republican members become relatively less minority, driven by changes in the neighborhoods from which they draw students. 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 Republican—but not Democratic—school board members affect local school attributes in ways that may 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 (for example, Eggers and Hainmueller 2009; Querubin and Snyder 2013; 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 (that is, 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 (for example, 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 (for example, 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. Despite evidence of limited or weak electoral accountability (for example, Holbein 2016; Kogan, Lavertu, and Peskowitz 2021), we are able to test if neighborhood representation is capitalized in home values, which would indicate that what school boards do 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 (for example, Fischer 2023; Shi and Singleton 2023). Christian, Jacob, and Singleton (2025) 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.

II. 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 (for example, 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.

A. 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 names 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 districts switched to partisan school board elections during our sample period, the vast majority (about 87%) of contests that we observe are non-partisan (that is, 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, and a candidate should register as a voter. We define neighborhoods of candidates according to their Census block group of residence. Because voters do not always report their political affiliations in certain years, we impute missing affiliations when a voter has reported a unique political party in other years, even if some values are missing. If there is no clear party affiliation, we classify those voters as "unaffiliated."

We link candidates to the voter registration database via a within-county fuzzy match based on name and county, as detailed in the appendix. This method allows us to achieve a unique match for approximately 63% of entries in the linked election records and the North Carolina voter registration database. We also include non-unique matches in our baseline sample to increase the matching rate to 74%. We weight a non-unique match by the inverse of the total number of non-unique matches for the candidate in our regressions. Appendix Table A.9 provides results that demonstrate the robustness of our later findings to a matching procedure that only allows for unique matches.[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 US$30,000 and higher than US$2,000,000), as well as by excluding transactions that are missing or have zero values for key attributes (for example, no bathrooms, recorded square 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 (for example, 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.

B. 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.

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 square foot of house i in Census block group j in year t, X_{it} includes housing attributes (for example, 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 IV.B show that this issue is salient. The final index value is given by:

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

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 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 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. 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 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:

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

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).

C. 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 who are matched with the voter registration in our sample. Panel A shows statistically significant disparities across age, gender, race, political affiliation, and other characteristics between winners and losers overall, which

may influence 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 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 some neighborhood characteristics, including income levels and urbanicity, are statistically different between winning and losing candidates. 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. School characteristics are generally balanced, except that the proportion of black students is statistically higher in the winners' neighborhood schools. This gap could be driven by the fact that Black candidates, who may live in neighborhoods with more black students, are more likely to win a school board position, as shown in Panel A.

III. 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.

A. 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 is defined for all candidates in a contest.

We are interested in estimates of:

$$Y_i = \alpha + \beta D_i + \gamma_1 x_i + \gamma_2 D_i \cdot x_i + \gamma_3 Z_i + \epsilon_i \quad (4)$$

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.[16] 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 election.[17]

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 that 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 Republican and Democratic candidates (with bandwidths chosen separately). Parallel validity, placebo, and robustness checks are performed for each subsample.

B. 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 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 from four years prior to the election to a year prior to the election. 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 because 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, focusing 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 IV. 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 pre-election 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.

Because we also conduct heterogeneity analysis by political partisanship, we report the parallel summary statistics and validity and specification tests for each political identity: Republican and Democratic candidates. Appendix Table A.4 reports candidate, neighborhood, and school characteristics for Republican 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 Appendix Figure 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 Republican and Democratic candidates. In Appendix Table A.6, we cannot observe a statistically significant discontinuity of the change in house price prior to the elections within each political group.

IV. 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 presents visual evidence of the causal effect of winning a school board election on the house price index in the neighborhoods of the winners. We provide the RD plot with a linear fit and a bandwidth of [-0.2, 0.2]. The figure demonstrates that there is no noticeable discontinuity in home prices within the block group of election winners compared to those in the block group of election losers.

To be more concrete about the null effect, 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 the effect of winning a school board election is 1.9% but statistically insignificant relative to prices in a loser's block group during the 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 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 when these controls are included.[18] In columns 3 and 4, we also present the parallel effects on the log of the house price. 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 find no evidence that 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 would be. 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 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:

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

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, β_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 the covariates, showing that there are no statistically significant gaps in home prices between winners and losers over the period. It also reports no pre-trend in the house price index, consistent with the placebo test in Table 3.

A. 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 Republican and Democratic school board members, separately.

Figure 3 does not show clear evidence of 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 may slightly raise home prices in the block group of Republican candidates, whereas winning the election may slightly lower home prices in the block group of Democratic winners relative to 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 Republican election winner increases by around 5.8% relative to prices in a Republican loser's block 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 Republican candidates to 4.1%, which is not statistically significant. The point estimate for Democratic candidates quantitatively decreases, but it is not distinguishable from zero. These results provide suggestive evidence that Republican winners may take actions as elected officials that are capitalized into their neighborhood home prices, but the effect is not robust. We also do not find a robust price effect for unaffiliated candidates, as shown in Appendix Table A.7.

Although we do not observe any price effects at the Census tract level in the pooled sample, an impact may exist within political partisanship. However, as shown in Columns 1 and 3 of Appendix 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 neighboring block groups within the same Census tract. This suggests that the possible actions of a Republican winner may affect home values only within a narrow

neighborhood, the Census block group. In the next section, we explore potential mechanisms that could explain the limited effect on perceived neighborhood quality.

We also present the event-study-style RD estimates from Equation 5 for Republican and Democratic candidates separately in Figure 4. Panel (a) indicates that the RD estimates for Republican candidates show a modest increase after elections.[20] This increase is only observed 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 Republican winners. In contrast, in panel (b), we observe a decline in home values in the fourth year following the election for Democratic candidates, which may imply that policy choices made by Democratic school board members could have adverse impacts on their neighborhood home values in a longer run.

B. Robustness

We find that home prices do not respond to the outcomes of school board elections in general. We also report suggestive evidence that they may increase in neighborhoods of Republican winners relative to those of Republican losers, although the price effect is not robust to inclusion of covariates. These results are based on many research choices, such as sampling, variable construction, and RD design. In this subsection, we assess the robustness of these findings to these choices.

First, we demonstrate that our main findings are robust across different sample choices. We include candidates matched to multiple entities in the voter registration database using a proper weighting method, but this may introduce measurement errors arising from incorrect matches. To assess whether our matching algorithm affects the baseline results, we restrict the sample to unique matches and estimate price effects. Appendix Table A.9 confirms that the point estimates

of the price effect remain consistent and that the increase in home values for Republican winners is robust to inclusion of covariates with this narrowed sample.

The way the sample is split is also important for our results. While we report potential heterogeneity by political partisanship, accounting for different motivations and policy preferences, there may be disproportionate price effects across other candidate characteristics. For instance, previous research on political selection has shown that political leaders' gender can influence investment in education (Clots-Figueras, 2012). In our case, differences in policy priorities between genders may affect home value appreciation. Age could be another source of heterogeneity in motivations and policy preferences, considering that the position has control over the local public education. However, as shown in Appendix Table A.10, alternative methods of dividing the sample by gender and age do not reveal heterogeneity in the impact of winning a school board election on home prices.

Second, because using an empirical Bayes estimator of neighborhood housing prices is novel, we examine how our shrunken estimate compares with standard home price measures in Appendix Table A.12. 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.12.[21] The estimates of the housing price index are robust to winsorizing at the 1% and 5% tails of the house price distribution, as well as to the number of house transactions in a neighborhood.

We further 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 as the common prior, rather than at the county level, yields consistent price estimates, which are reported in Appendix Table A.13.

Additionally, the price effect for Republican winners is more pronounced and statistically significant when using this alternative price measure.

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. Panel (a) of Appendix Figure A.10 reports that the RD estimate of house price index peaks at the original threshold for Republican candidates and is not statistically significant at placebo thresholds ranging from -0.3 to 0.3.[22]

In Appendix Table A.11, 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 the RD estimate for Republican candidates is sensitive to the choice of bandwidth. Appendix Figure A.9 visually confirms that the estimate for Republican candidates is not precise across a broader range of bandwidths from 0.05 to 0.35. Thus, we interpret the disproportionate impact on Republican candidates as merely suggestive. The estimate is statistically insignificant with any bandwidth for Democratic candidates.

Another concern is that we pool variation in the 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 Republican loser, (3) Republican winner versus Democratic loser, and (4) Republican winner versus Republican loser. In all cases, we use contests where both the least-vote-getting winner and the most-vote-getting loser are matched to the voter records, leaving only 239 candidates. Columns 1 and 2 in Appendix Table A.14 show that the estimated effect of winning a school board seat on home prices in the winner's

neighborhood is negative in the contests with a Democratic winner, though they are not robust. When the marginal winner is Republican, the estimated effect is positive, although the estimate is not statistically different from zero. We conclude that narrowing in this way substantially reduces sample size and precision but yields a pattern of findings that affirms our main results. We still cannot rule out the possibility that the observed differences by party affiliation reflect the effects of election types rather than candidate types.

To further verify that the positive impact on housing prices for Republican 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 Republican winners versus Republican losers, we expand it with the spirit of the previous exercise. Specifically, we add to the sample the following candidates: a) in contests where the marginal winner is Republican but the marginal loser is not Republican (can be Dem or not matched to the voter records), the marginal winner and any Republican loser; and b) in contests where the marginal loser is Republican but the marginal winner is not Republican, the marginal loser and any Republican winner. In Appendix Table A.15, the RD estimates with the location fixed effects support our main finding regarding the effects of Republican winners: relative to Republican losers, home prices in their neighborhoods tend to rise.

V. Effects of Election on School Outcomes

The results in the prior section reveal that winning a school board election does not cause home values in winners' neighborhoods to increase (relative to losers' neighborhoods). At the same time, we find that the price effect is suggestively positive for Republican winners relative

to Republican losers, whereas there is no effect for Democratic candidates. Because the perceived quality of assigned schools is an important neighborhood amenity, we turn to education data to examine how neighborhood school attributes may be affected by school board members, even if they may not be effectively capitalized in house values. Because the pooled estimates mask suggestive partisan heterogeneity, our exploration of school outcomes examines the causal impacts of winning a school board seat separately among Republican and Democratic candidates.

There are several potential ways for school board members to induce changes in local school quality. First, school board members are charged with local education production by allocating resources. Allocating more resources to the neighborhood schools serving board members may improve students' academic performance. 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 can directly change the composition of students in their neighborhood schools by adjusting school attendance zones within the school district.

A. Effects on Student and Teacher Quality

One way for school board members to improve the perceived quality of schools is 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; 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 may expect a corresponding improvement in observable student achievement in

neighborhoods where Republican candidates win, compared to those where Republican candidates lose, assuming that the enhancement in school quality drives the suggestive 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) for middle and elementary school students residing in the Census block group of Republican and Democratic candidates. Both panels of Table 7 indicate that winning a school board position does not raise or lower the average 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 an observable change in the academic performance of students living in the neighborhoods of Republican school board members does not drive the possible increase in house prices in the block groups.

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 (panel B). While we do not observe any statistically significant effect of winning a school board position on average test scores at the neighborhood middle schools for both political groups, we find suggestive evidence that winning the position raises scores at the neighborhood elementary schools, especially for Republican school board members. The coefficient indicates that a Republican winner raises the average overall, math, and reading test scores at her neighborhood elementary school by 0.048, 0.040, and 0.050 standard deviations, respectively. Winning of a Democratic candidate increases the average math score by 0.025, but the other effects are not statistically significant.

Productivity or quality changes in the neighborhood elementary schools, as measured by test-score value-added, may drive the observed improvements in test scores. 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 compute the average residualized test scores at each school-by-year to obtain the school value-added. Similarly, we compute the average residualized test scores for each teacher-by-year to obtain teacher value-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. Table 9 presents no statistically significant

effects on school and teacher value-added at the neighborhood schools. These results indicate that a newly elected school board member does not improve the quality of neighborhood schools, regardless of political affiliation, or that we lack sufficient statistical power to detect a precise effect.

We also 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 using 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) as dependent variables at the neighborhood school level. Appendix Table A.18 indicates that neither the average experience years nor the composition of teachers in the neighborhood school responds to the school board election results for both political groups.

B. Effects on Student Sorting and School Assignments

1. Student Sorting

Other school outcomes that a school board member might affect 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).

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)). Panel A indicates that winning a

school board election has distinctive effects on the composition of middle school students in the neighborhood across political parties. It raises the proportion of white students by 2.7 percentage points (3.7%) and lowers that of Hispanic students by 2.0 percentage points (24.4%) in the neighborhood of Republican winners. In contrast, the proportion of white students decreases by 2.3 percentage points (4.4%), and that of black students increases by 2.3 percentage points (7.0%) when a Democratic candidate wins the election. However, winning the election does not significantly influence the composition of elementary school students in these neighborhoods.

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 raises the proportion of white students by 1.1 percentage points (1.7%) at the neighborhood middle school of Republican winners in the four years following the election, while the proportion of black students decreases by 0.9 percentage points (4.8%). The shares of EDS and Hispanic students do not respond. Panel B of Table 11 indicates that the proportion of Hispanic students declines by 1.0 percentage points (8.8%) at the neighborhood elementary school of Republican 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 Republican school board members induce changes in student composition at neighborhood schools, we expect these effects to be more discernible for the structural movers — those students who switch from an elementary school to a middle school at 5th or 6th grade. Changes in the composition of structural movers will be jointly influenced by board-determined attendance zone shifts (that is, 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 1.6 percentage points (2.4%) 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 Black structural movers are -1.1 percentage points (6.3%), respectively. By contrast, for Democratic candidates, we observe no jumps in the composition measures at the threshold.

The observed changes in the composition of students in the neighborhood and the neighborhood schools of Republican candidates may contribute to the suggestive increase in house prices in those neighborhoods, as home values reflect the demographics of students in local schools (Clapp, Nanda and Ross, 2008). However, it is also possible that other school characteristics correlated with a higher proportion of white students and a lower proportion of minority 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.[26]

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

As explained in Section II.B, 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 school (Panel B), separately. Columns 3 and 6 of Panel A report that the proportion of EDS is 1.5 percentage points (2.8%) lower, and the median income is 2.4% higher in the neighborhood middle schools of Republican winners than in the neighborhood middle schools of Republican losers. In other words, the neighborhood middle schools of Republican winners capture more students from block groups with a lower proportion of economically disadvantaged students and a higher median income than those of Republican losers after school board elections. Considering that there is no such discontinuity before school board elections, as shown in Appendix Table A.17, this gap is solely driven by the change in school attendance in the post-election period. These results imply that Republican school board members disproportionately shift the attendance zone of their neighborhood middle schools toward more "preferable" neighborhoods. There is no such discontinuity for the Democratic candidates.

In contrast, Panel B of Table 13 reports the muted effects on the composition of the block groups that send students to the neighborhood elementary school for both political groups. There are two potential reasons for these effects, especially for Republican winners: First, a school board member has greater latitude to shift the attendance zone for a middle school than for an elementary school because a middle school covers a much larger 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.

C. Heterogeneity in Effects

In this subsection, we investigate how the impact of winning a school board election on home values varies across contextual and policy factors to understand the possible link between the return to office and the actions of school board members. Specifically, we estimate an extended specification of Equation 4:

$$Y_i = \alpha + \beta_1 D_i + \beta_2 D_i \times \text{Indicator}_i + \gamma_1 x_i + \gamma_2 D_i \cdot x_i + (\gamma_3 x_i + \gamma_4 D_i \cdot x_i + \gamma_5) \times \text{Indicator}_i + \gamma_6 Z_i + \epsilon_i \quad (6)$$

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 provide suggestive evidence that the change in home values vary across some factors that affect the effectiveness and intentions of school board members in exercising their authority.

Appendix Table A.20 reports the results for Republican 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 higher home values in neighborhoods where Republican candidates win, we would expect a more pronounced impact on home prices in districts where school board members have greater 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 change 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. [27] The KL divergence is a simple and widely used metric that quantifies how much one distribution differs from a reference distribution. We use this measure to estimate how the distribution of block groups covered by a neighborhood school changes over time. A higher KL divergence indicates greater compositional changes in the covered block groups. We define a neighborhood school as experiencing more frequent catchment zone changes if its KL divergence over the four years leading up to the election exceeds the median. Column 2 shows that the price effect for a Republican winner is not larger when the neighborhood 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 local school boards' control over enrollment, can attenuate the effects of changes in traditional public school catchment zones. Therefore, we examine how the price effect varies when a candidate's school district is more accessible to charter schools. We define a school district as more accessible to charter schools if, within that district, the percentage of students attending

charter schools is higher than the median percentage over the four years leading up to the election. Column 3 provides suggestive evidence that the price effect for Republican winners is less pronounced if their local communities have greater access to charter schools, although we do not have sufficient power to detect statistically significant differences. These results may support the hypothesis that board members' ability to alter school attendance zones and student distribution contributes to increases in housing prices in the neighborhoods of Republican winners.

We also report heterogeneity in the return to winning the office by election type. In North Carolina, school board candidates run for office either across an entire district (at-large election systems) or within a specific sub-district or wards (region-based election systems). One possibility is that candidates representing the wards have a stronger incentive to influence policy choices in the best interest of local neighborhood voters. In addition, the appreciation of home values may be more pronounced for Republican 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 columns 4 and 5 indicate that these hypotheses are not supported. We also find no statistically significant heterogeneity across these contextual and policy factors for Democratic candidates.

VI. 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 economically-meaninful, but statistically imprecise evidence that a Republican winner’s neighborhood appreciates in value relative to a losing Republican 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.

Using administrative school records, we show evidence of neighborhood public schools of Republican 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 (for example, 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 Republican—but not Democratic—school board members affect local school attributes in ways that may 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 Republican 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 member salaries could improve the selection of Republican 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.

Our study does not provide a complete welfare assessment. While we identify localized effects of school board election outcomes—comparing winners to losers—a full evaluation would need to account for general equilibrium effects beyond our estimates. The attendance zone adjustments we document reallocate students across schools within a fixed district, making them inherently zero-sum. As a result, the gains observed for Republican winners may be partially or fully offset by losses elsewhere.

That said, existing literature points to broader welfare implications. Work in the Tiebout tradition shows that changes in school quality can trigger residential re-sorting, with aggregate effects on stratification that far exceed direct estimates. For example, Bayer, Ferreira, and McMillan (2007) finds that general equilibrium effects can be several times larger than direct demand responses. In our context, the compositional shifts we document may therefore initiate self-reinforcing cycles of residential sorting, amplifying inequality in school quality and neighborhood wealth beyond what is captured in our estimates.

Endnotes

1. Other than a few isolated districts (for example, Los Angeles Unified), Florida stands as the major exception to negligible pay for school board members. Salaries were up to US$56,500 in 2025 depending on district size (https://edr.state.fl.us/Content/local-government/reports/finsal25.pdf). 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 US$1,800 (Rutherford County) to US$6,300 (Burke County).

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, 2023). Note that we also investigate (but do not find evidence for) heterogeneity along other observation dimensions of candidates (for example, gender, age).

4. Morris (1983) shows that this class of estimators is efficient in samples with larger variances and Fay 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, among others).

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. Dahlberg et al. (2024) and Harjunen, Saarimaa, and Tukiainen (2021) similarly show that the neighborhood of politicians matters for local public goods in Sweden and Finland, respectively.

7. Because 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.

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. 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.

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.

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.

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 because boundary changes essentially alter the attendance zone of other schools which may include the neighborhood school of election losers—the control group.

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

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

18. As Imbens and Lemieux (2008) point out, adding controls influence the RD estimates. First, even though the observations close to the threshold are theoretically randomly assigned in large sample, the covariates can be used to eliminate sample biases in a finite sample, and improve the precision. Second, while the theoretical argument of RDD postulates a range of the running variable very close to the threshold, the estimated optimal bandwidth may not be too close to the threshold in practice.

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.

20. One could argue that the increase in Republican winners' house prices relative to the house prices of Republican 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 Republican winners' prices that are increasing or Republican 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 Republican winners and losers in Appendix Figure A.8. The graph suggests that the house price changes in the neighborhood of Republican winners if there is any effect, while that in the neighborhood of Republican losers does not decline in the post-election periods.

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 ($\overline{\epsilon}_{jt} = \frac{\sum_{i \epsilon j} \epsilon_{ij}}{N_i}$) 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 used for the figure, we excluded the controls and applied different optimal bandwidths on the left and right sides of the threshold to increase the effective number of observations.

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.

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.16. All of these results are consistent with our main findings.

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

26. For example, Appendix Table A.19 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.

27. 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 students living in the block group of a candidate attend, and Q and P are the distributions of neighborhood students over schools in the year of election and in post-election years, respectively.

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Tables

Table 1. Descriptive Statistics

	Winners	**Losers**	**Difference**
	Mean	*Mean*	*t-statistic*
Panel A: Candidate Characteristics			
Age	49.95	46.58	7.00
Prop. Female	0.53	0.39	7.32
Prop. Black	0.26	0.23	1.97
Prop. Democratic	0.61	0.51	6.16
Prop. Republican	0.35	0.40	−2.50
Prop. Unaffiliated	0.03	0.09	−7.29
Prop. Incumbent	0.35	0.10	19.22
Prop. At-large Contest	0.45	0.55	−5.76
Election Year	2012.13	2011.95	1.89
Observations	1,262	1,383	
Panel B: Neighborhood Characteristics			
Median Income	48,295	49,963	1.96
Prop. College Graduates	0.32	0.33	1.17
Avg. House Price	169,007	168,996	0.00
House Price Index	0.04	0.04	0.01
Prop. Urban Areas	0.39	0.44	2.78
Obs.	1,528	1,653	
Panel C: School Characteristics			
Standardized Score	−0.08	−0.05	1.47

Prop. Black Students	0.28	0.25	2.62
Prop. Hispanic Students	0.11	0.12	1.18
Prop. White Students	0.55	0.55	0.27
Prop. EDS Students	0.61	0.60	0.48
Obs.	851	925	

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.

Table 2. Balance Checks

Panel A: Candidate 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	0.474	0.027	0.075	−0.023	−0.030	−0.024	−0.005
	(1.410)	(0.060)	(0.041)	(0.033)	(0.086)	(0.021)	(0.048)

Panel B: Contest Characteristics

	(1) Contest At-large	(2) Log Total Votes	(3) # Winners	(4) General Election Date
Vote margin > 0	0.028	0.230	0.379	−0.023
	(0.057)	(0.217)	(0.240)	(0.033)

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	−0.004	0.007	−0.004	0.145	0.007
	(0.020)	(0.022)	(0.065)	(0.330)	(0.055)

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 (1) 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 from four years prior to the election to a year prior to the election. 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. Column 3 indicates the estimates of the indicators of urban clusters and urbanized areas.

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	0.019	0.012	0.019	0.018
	(0.027)	(0.025)	(0.029)	(0.028)
Optimal BW	0.143	0.129	0.132	0.121
Obs. within BW	1,293	1,229	1,244	1,171
Obs.	2,500	2,500	2,500	2,500
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 house price index in equation (1) up to four pre-election years and subtracting the average over the four years preceding the four pre-election years. The outcome in columns (3) and (4) is calculated by taking the average of logged house prices 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 pre-election 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 4. Effects of Election on Neighborhood Home Prices

	(1)	(2)	(3)	(4)
	Price Index	Price Index	Log Price	Log Price
Vote margin > 0	0.019	−0.004	0.034	0.012
	(0.028)	(0.024)	(0.032)	(0.028)
Optimal BW	0.147	0.157	0.130	0.133
Obs. within BW	1,264	1,293	1,192	1,207
Obs.	2,330	2,330	2,330	2,330
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 (1) (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 pre-election 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 5. Effects of Election on Nearby Neighborhood Home Prices

	(1)	(2)	(3)	(4)
	Census Tract	Census Tract	Excl. Own Block Group	Excl. Own Block Group
Vote margin > 0	0.010	0.010	0.014	0.014
	(0.024)	(0.024)	(0.026)	(0.026)
Optimal BW	0.123	0.123	0.113	0.113
Obs. within BW	1,141	1,141	1,076	1,076
Obs.	2,330	2,330	2,242	2,242
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 3 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 pre-election 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 6. Effects of Election on Neighborhood Home Prices by Political Identity

	Republican		Democratic	
	(1)	(2)	(3)	(4)
	Price Index	Price Index	Price Index	Price Index
Vote margin > 0	0.058*	0.041	−0.005	−0.029
	(0.033)	(0.029)	(0.043)	(0.038)
Optimal BW	0.142	0.112	0.188	0.215
Obs. within BW	514	459	691	741
Obs.	915	915	1,219	1,219
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 (1) 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 pre-election 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 7. Effects of Election on Average Test Scores of Neighborhood Students

	Republican			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Math	Reading	Overall	Math	Reading
Panel A: Middle School Students						
Vote margin > 0	−0.005	−0.027	0.016	−0.000	0.021	−0.021
	(0.048)	(0.048)	(0.053)	(0.042)	(0.042)	(0.045)
Pre-election Mean	0.068	0.071	0.065	−0.083	−0.077	−0.089
Panel B: Elementary School Students						
Vote margin > 0	−0.027	−0.010	−0.044	−0.026	−0.021	−0.031
	(0.040)	(0.043)	(0.041)	(0.030)	(0.030)	(0.033)
Pre-election Mean	0.089	0.095	0.083	−0.102	−0.090	−0.114
Obs. within BW	380	380	380	682	682	682
Obs.	759	759	759	1,138	1,138	1,138

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 (1) 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 pre-election 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 8. Effects of Election on Average Test Scores of Students in Neighborhood Schools

	Republican			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Math	Reading	Overall	Math	Reading
Panel A: Middle School						
Vote margin > 0	−0.012	−0.004	−0.018	−0.005	0.004	−0.013
	(0.020)	(0.019)	(0.023)	(0.016)	(0.015)	(0.018)
Pre-election Mean	0.022	0.024	0.018	−0.170	−0.152	−0.164
Panel B: Elementary School						
Vote margin > 0	0.048*	0.040*	0.050*	0.026	0.025*	0.023
	(0.025)	(0.021)	(0.030)	(0.019)	(0.015)	(0.022)
Pre-election Mean	0.050	0.052	0.041	−0.133	−0.114	−0.134
Obs. within BW	428	428	428	751	751	751
Obs.	840	840	840	1,231	1,231	1,231

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 pre-election 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 9. Effects of Election on School and Teacher Value-added of Neighborhood Schools

	Republican		Democratic	
	(1)	(2)	(3)	(4)
	School VA	Teacher VA	School VA	Teacher VA
Panel A: Middle School				
Vote margin > 0	−0.005	−0.018	0.007	−0.013
	(0.021)	(0.023)	(0.018)	(0.012)
Pre-election Mean	−0.000	−0.072	−0.025	−0.056
Panel B: Elementary School				
Vote margin > 0	0.033	−0.019	0.018	0.005
	(0.025)	(0.016)	(0.029)	(0.008)
Pre-election Mean	0.028	0.005	0.034	0.005
Obs. within BW	398	374	693	658
Obs.	765	722	1,100	1,050

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 pre-election 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 10. Effects of Election on Composition of Neighborhood Students

	Republican				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	0.027*	−0.001	−0.020**	−0.024	−0.023*	0.023*	0.003	0.004
	(0.015)	(0.012)	(0.009)	(0.019)	(0.013)	(0.012)	(0.007)	(0.017)
Pre-election Mean	0.729	0.133	0.082	0.428	0.516	0.326	0.077	0.539
Panel B: Elementary School Students								
Vote margin > 0	−0.002	−0.007	0.009	0.010	−0.005	0.011	−0.007	0.007
	(0.015)	(0.010)	(0.011)	(0.021)	(0.013)	(0.011)	(0.008)	(0.016)
Pre-election Mean	0.724	0.122	0.092	0.456	0.510	0.313	0.091	0.567
Obs. within BW	465	465	465	465	816	816	816	816
Obs.	919	919	919	919	1,365	1,365	1,365	1,365

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 (1) 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 pre-election 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 11. Effects of Election on Composition of Students in Neighborhood Schools

	Republican				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	0.011*	−0.009*	−0.001	0.006	−0.005	0.005	−0.003	0.004
	(0.006)	(0.005)	(0.004)	(0.018)	(0.004)	(0.004)	(0.003)	(0.017)
Pre-election Mean	0.666	0.188	0.096	0.482	0.497	0.340	0.090	0.578
Panel B: Elementary School								
Vote margin > 0	0.001	0.007	−0.010*	0.007	0.004	−0.002	−0.002	0.011
	(0.005)	(0.006)	(0.005)	(0.015)	(0.004)	(0.004)	(0.003)	(0.016)
Pre-election Mean	0.676	0.159	0.114	0.507	0.489	0.326	0.111	0.612
Obs. within BW	445	445	445	445	791	791	791	791
Obs.	886	886	886	886	1,314	1,314	1,314	1,314

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 pre-election 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 12. Effects of Election on Composition of Structural Movers in Neighborhood Schools

	Republican				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	0.016*	−0.011*	−0.003	−0.007	−0.002	0.006	−0.005	−0.004
	(0.009)	(0.006)	(0.006)	(0.010)	(0.005)	(0.005)	(0.003)	(0.012)
Pre-election Mean	0.662	0.176	0.103	0.518	0.490	0.332	0.096	0.616
Obs. within BW	415	415	415	415	742	742	742	742
Obs.	822	822	822	822	1,224	1,224	1,224	1,224

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 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 pre-election 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.

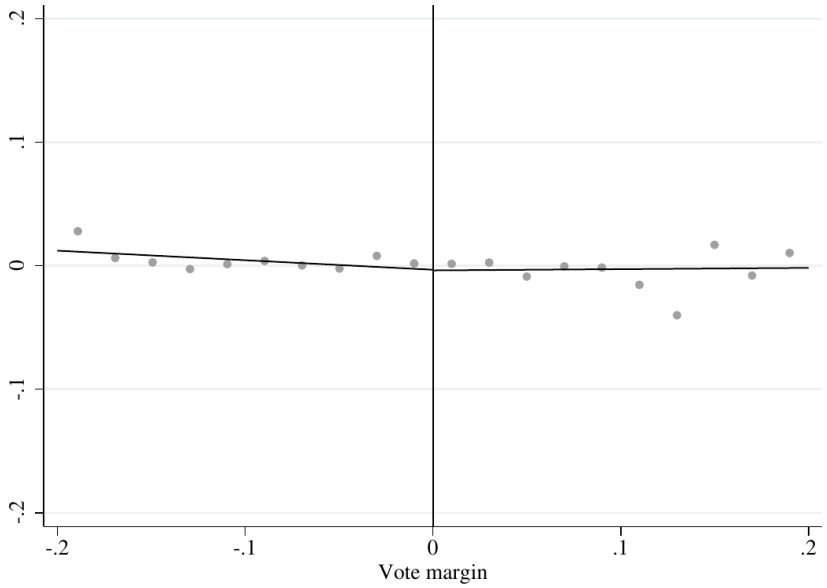
Table 13. Effect of Election on Attendance Zone Composition of Neighborhood Schools

	Republican						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	0.013	−0.010	−0.015*	0.008	−0.007	0.024*	−0.004	0.008	0.006	−0.002	0.003	−0.009
	(0.009)	(0.006)	(0.009)	(0.007)	(0.006)	(0.014)	(0.006)	(0.005)	(0.006)	(0.005)	(0.004)	(0.009)
Pre-election Mean	0.656	0.168	0.528	0.774	0.147	10.735	0.481	0.339	0.619	0.615	0.290	10.596
Panel B: Elementary School												
Vote margin > 0	−0.005	0.005	−0.000	−0.008	0.007	0.001	0.005	−0.003	−0.003	0.006	−0.006	0.009
	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.014)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.009)
Pre-election Mean	0.677	0.153	0.515	0.791	0.134	10.737	0.485	0.336	0.615	0.617	0.287	10.584
Obs. within BW	275	275	275	275	275	275	407	407	407	407	407	407
Obs.	500	500	500	500	500	500	703	703	703	703	703	703

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 pre-election 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.

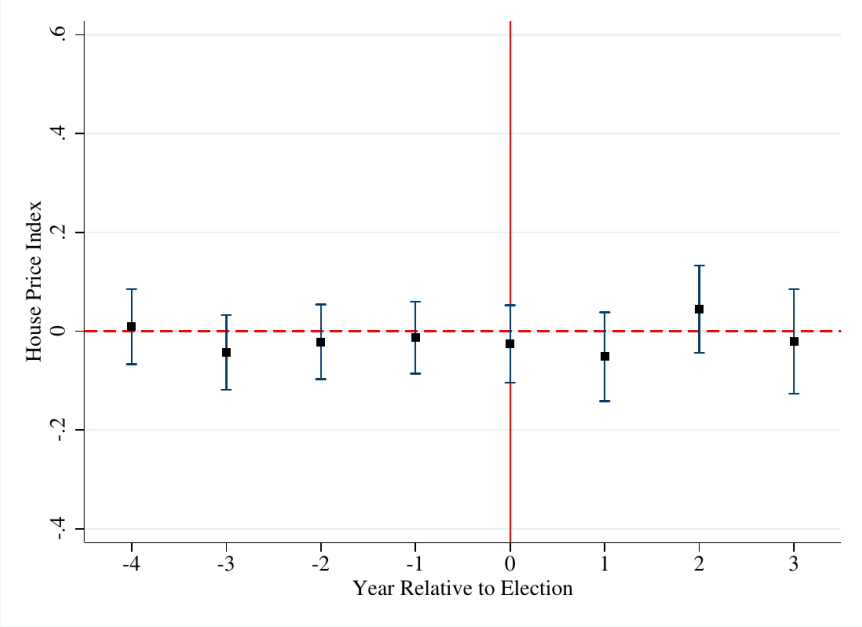
Figures

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 pre-election school board members (proportions of black, female, and Democratic members), and the indicator of urban areas.

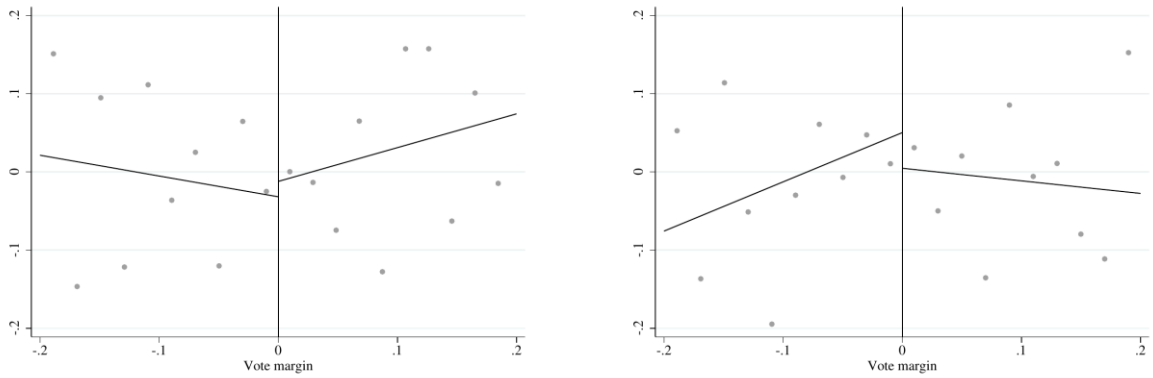
Figure 2. Dynamic Effects of Election on Neighborhood Home Prices



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 pre-election school board members (proportions of black, female, and Democratic members). Standard errors are clustered at the election level. The confidence intervals are at 95%.

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

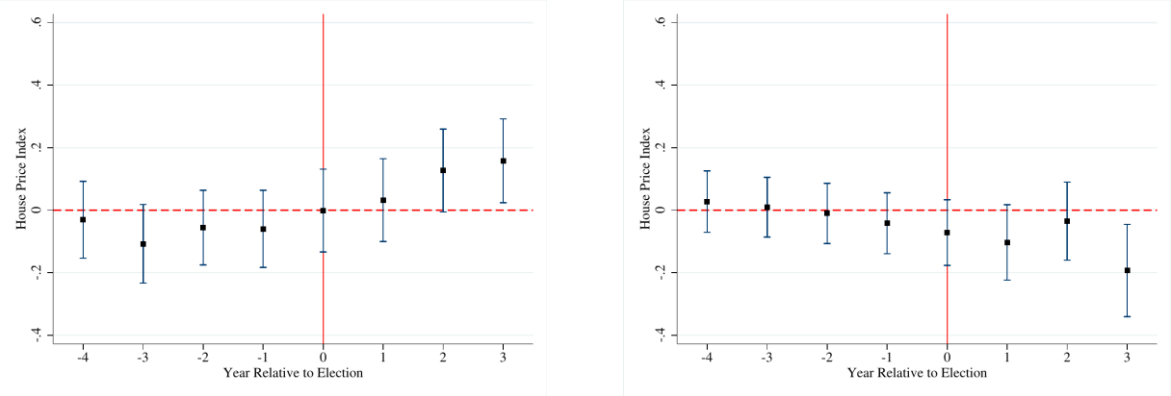
(a) Republican **(b) Democratic**



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 pre-election school board members (proportions of black, female, and Democratic members), and the indicator of urban areas.

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

(a) Republican **(b) Democratic**



Notes: This figure reports the estimation results of equation (5) for Republican 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 pre-election school board members (proportions of black, female, and Democratic members). Standard errors are clustered at the election level. The confidence intervals are at 95%.