# School Boards and Student Segregation*

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**Abstract**

This paper provides the first causal evidence about how elected local school boards affect student segregation across schools. The key identification challenge is that the composition of a school board is potentially correlated with unobserved determinants of school segregation, such as the pattern of household sorting and the degree to which boards are geographically constrained in defining zones of attendance. We overcome this issue using a regression discontinuity design at the electoral contest level, exploiting quasi-random variation from narrowly-decided elections. Such an approach is made possible by a unique dataset, which combines matched information about North Carolina school board candidates (including vote shares and political affiliation) with time-varying district-level racial and economic segregation outcomes. Focusing on the political composition of school board members, two-stage least squares estimates reveal that (relative to their non-Democrat counterparts) Democrat board members decrease racial segregation across schools. These estimates significantly differ from their ordinary least squares counterparts, indicating that the latter are biased upward (understating the effects). Our findings suggest that school boards realize such reductions in segregation by shifting attendance zones, a novel measure of which we construct without the need for exact geocoded boundaries. While the effect of adjusting boundaries does not appear to be offset by within-district neighborhood re-sorting in the short run, we uncover causal evidence of "white flight" out of public schools in districts in which boards have acted to reduce segregation.

**Keywords**: School Segregation, School Boards, Regression Discontinuity, Elections

**JEL Classifications**: I21, I24, I28

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# 1 Introduction

Policymakers have long been preoccupied with the degree of student segregation across schools. As busing and desegregation orders have become less prevalent in recent years, school segregation has been on the rise in many public school districts throughout the United States. This trend has been driven by continued residential segregation from household sorting across neighborhoods (Tiebout 1956; Bayer, Ferreira and McMillan 2004) and a lack of open enrollment in many cases tying residences to particular schools.[1] Addressing student segregation across schools has increasingly fallen under the purview of elected local school boards, principally through the drawing of attendance zone boundaries. Yet, despite the documented importance of peers to educational outcomes,[2] there exists little evidence (causal or otherwise) about the role of school boards in the allocation of students to schools.

In this paper, we examine the causal effect of school board decisions on student segregation. Identification hinges on ruling out confounding factors; most notably, sorting patterns across neighborhoods (via household preferences) that are correlated with both board composition and segregation across schools. For example, if voters who reside in less segregated neighborhoods tend to prefer board members of a particular political outlook, those members might erroneously appear to reduce segregation across schools. While controlling for residential segregation would eliminate such endogeneity, this approach would be infeasible for unobserved sources of bias. In particular, the mapping between the residential concentration of student types within a district and the feasible ways in which they can be allocated to schools (owing to optimal school size, transportation costs and political constraints) is unknown. Correlation of school board composition with any of these factors would undercut a casual claim.

We address such issues by adopting a regression discontinuity approach implemented at the electoral contest level to exploit quasi-random variation from elections that are narrowly decided. Intuitively, we compare segregation outcomes associated with marginal winners of one type (we

[1] Empirical evidence of education-motivated residential choices in Washington D.C. and North Carolina is presented by Barrow (2002) and Caetano and Macartney (2013), respectively. Both papers find that families sort differentially based on race, with white families more likely to place a higher value on better schools. Heterogeneous preferences are also observed in Bifulco, Ladd and Ross (2009), who contend that school choice in Durham gives rise to more highly segregated schools than would occur from simple proximity-based rules. Hastings, Kane and Staiger (2006) find similar patterns from Charlotte-Mecklenburg's school choice program.

[2] Prominent examples include Hoxby (2000), Hoxby and Weingarth (2005), Graham (2008), and Fruehwirth (2013).

focus on political affiliation in our implementation) to those associated with marginal winners of the opposite type, assuming that the opposite winner outcome is a valid counterfactual for the unobserved opposite loser analogue. The assumption implies that confounding factors of winners and losers are continuous at the margin. The comparison then yields the causal effect of one type versus the opposite type on student segregation.

Our empirical strategy leverages a unique dataset, assembled from several sources. We first compiled a list of members for 109 school boards in North Carolina from the year 2000 onward. This was a laborious process, as no central repository for such records exists. We then linked board members to North Carolina State Board of Elections (henceforth 'NCSBE') voter registration records, which contain information such as full name, address, age, ethnicity and (most notably for our purposes) stated political party for each voter. Based on a within-county fuzzy match by name, we are able to uncover the characteristics of 73 percent of board members in our sample.

Also from the NCSBE, we were able to obtain a list of candidates for each electoral contest held in North Carolina from 2008 to 2013 inclusive, along with the total number of votes each received. This allows us to calculate vote shares for election winners and losers, with the characteristics of losers obtained in a similar fashion to board members (winners) through voter registration records. We then connect this school board and election information to time-varying district-level racial and economic segregation outcomes, constructed using administrative records of each student's residential location and school attended, which are provided by the North Carolina Education Research Data Center (henceforth 'NCERDC').

We focus on the political composition of school boards in our analysis. The results indicate that (relative to their non-Democrat counterparts) Democrat board members decrease racial segregation across schools: an electoral victory that shifts the board to majority Democrat causes a reduction in the black dissimilarity index across schools of 12 percentage points, while the election of at least one Democrat (potentially in the minority) leads to a reduction of 18 percentage points. Contrasting these two-stage least squares estimates with their OLS counterparts, we find that the latter methodology understates the causal effect, highlighting the bias inherent in more naive approaches.

To establish the main mechanism underlying these effects, we then use student addresses to construct a novel measure of attendance zone shifts without needing to observe exact geocoded

boundaries. We show that such shifts increase by 0.19 standard deviations per year when an additional Democrat (relative to non-Democrat) is elected, consistent with them reversing the effects of neighborhood sorting. Based on this evidence, we consider whether board efforts to reduce segregation cause households to re-sort in response. While there is no indication that board policy gives rise to differential within-district neighborhood re-sorting in the short run, we detect compelling evidence of "white flight" out of the district or public school system soon after a board takes measures to reduce school segregation. This household response highlights a key tradeoff associated with policies aimed at reducing school segregation.

Our paper is the first to identify the key role that school boards play in influencing student segregation. This is relevant to three strands of literature. The first one seeks to estimate the contribution of inputs (such as teacher and school quality) to the production of student achievement, focusing primarily on the school, teacher and student (rather than district) levels (Rivkin, Hanushek and Kain 2005; Chetty, Friedman and Rockoff 2014). Our results, along with related prior research about school board activity (Billings, Deming and Rockoff 2014; Hoxby and Weingarth 2005), suggest that decisions made at the district level may play an important role in the education production process. The second strand of literature measures the willingness-to-pay for school quality, using discontinuities across school attendance zone boundaries (Black 1999; Bayer, Ferreira and McMillan 2007). Our work complements these demand-side analyses by providing supply-side insight into how boundaries are drawn in the first place, with boards actively altering them according to heterogenous preferences over student segregation. This serves as a first step in reaching a broader general equilibrium understanding of how the peer composition within schools is determined. Lastly, our work connects with a growing third strand of literature about school choice mechanisms (Abdulkadiroğlu and Sönmez 2003; Kapor, Neilson and Zimmerman 2017). Building upon these analyses which evaluate the allocation of students to schools primarily in terms of households' preferences, we consider the potential for the manipulation of attendance zone boundaries by school boards. This suggests a more complicated two-sided matching process in which the preferences of both households and boards influence outcomes.

The remainder of the paper proceeds as follows: The next section provides background, describes the data and sets out the measures that we exploit in our analysis. Section 3 details our research design and Section 4 presents the associated results. Section 5 discusses the mechanisms underlying

those effects, and Section 6 then concludes.

## 2 Background and Data

Local school boards are a distinctive feature of the American education system in which civilian officials, elected by local voters, administer public education within districts. This system of local governance and representation purportedly enables boards and school administrators to meet the needs and preferences of local households. While boards are generally charged with setting district policies (such as through hiring the superintendent), their responsibility for allocating students to schools, with its attendant consequences for school segregation, has been at the center of multiple landmark Supreme Court decisions.

Federal court orders and grants subsequent to *Brown v. Board of Education* induced many districts in the United States to desegregate schools along racial lines. A large literature examines the effectiveness of such policies and the implications for student outcomes (Reber 2005; Cascio et al. 2008; Hanushek, Kain and Rivkin 2009; Johnson 2011). Often constrained by considerable residential segregation, this requirement was frequently achieved through reassignments and involuntary busing. With the end of the court orders to desegregate, household sorting has contributed to recent increases in school segregation (Reardon and Yun 2002; Clotfelter, Ladd and Vigdor 2008; Lutz 2011) and school board priorities have become the purview of electoral politics.[3] Nonetheless, despite school boards' influence over local education policy, little evidence exists regarding either the characteristics, activities, or objectives of school boards.[4] Further, due to a lack of available data, the role of school boards in education production has largely remained unexamined (Land 2002).[5]

We construct a unique dataset that combines matched information about North Carolina school board candidates (including political affiliation and vote shares) with time-varying district-level

[3]For example, Republican-affiliated board members gained a majority of the Wake County, North Carolina school board in 2009 and ended busing intended to equalize diversity by implementing a neighborhood-based attendance zone plan (Parcel and Taylor 2015).

[4]One recent survey of school board members finds that a majority earn no income from their service, serve for four-years on the board, and tend to be better educated, higher income, more white and more conservative than their districts (Hess 2002). Board members also cite student achievement as a leading concern.

[5]An exception is work examining the relationship between minority representation on school boards and education policies that affect minority students, though not the segregation of students across schools (Meier and England 1984; Fraga, Meier and England 1986). These analyses, however, are not able to credibly rule out omitted sources of bias.

racial and economic outcomes in order to examine the causal effects of school boards on student segregation across schools. This section describes the sources and construction of the dataset and presents summaries of the key variables.

### 2.1 School Boards in North Carolina

We assemble a new panel of North Carolina school board members that we are then able to match with voter registration records. In North Carolina, in all but one of the 115 traditional public school districts, school board members are elected by voters in regular elections. While most school board elections are non-partisan (i.e., party affiliation is not disclosed on ballots to voters), candidates typically register with a political party, allowing us to construct the political composition of each school board. The primary challenge in tracking school board composition is that historical records are held by each district individually. Therefore, in order to reconstruct the past boards of each school district, we directly contacted each North Carolina school district to obtain their records – a labor-intensive process that yielded the names of school board members representing 109 districts, in some cases as far back as the year 2000.[6]

The NCSBE voter registration database contains the voter identification number, full name, address, age, political party, race and ethnicity, and voter history (among other variables) of all registered voters. To run for office, school board members are required to register in their district's county. As a result, we linked the names of board members with their voter registration record through a within-county fuzzy match based on name. This meticulous process resulted in nearly 75 percent of board members being matched to a unique record in the voter database. It is important to note that the composition of a school board can change frequently, as a subset of seats are regularly contested. The number of contested seats tends to be smaller than the total number of seats on a board, since term lengths typically exceed the election interval for a district.

The data gathering and matching process yields data for 251 school boards (that can also be linked to election results), for which Table 1 presents descriptive characteristics.[7] The average school board, which is defined by a unique school district and election year combination, consists of nearly seven board members. On average, 45 percent of school board members are female and

[6] In total, we obtained over 5,700 unique board member names between the years 2000 and 2014.
[7] 103 North Carolina school districts are included in the sample.

25 percent are black in our sample. In terms of the political composition of the school board, we construct three variables that are our treatments of interest in the analysis: the proportion of the board that is registered Democrat, whether the board consists of at least one Democrat, and whether the board consists of a majority of Democrats. As summarized in the table, 61 percent of the average school board is registered as a Democrat, 87 percent of school boards contain at least one member who is a Democrat, while 61 percent of school boards have a majority of Democrats.[8]

Table 1: School Board Characteristics

| | Mean | SD | Min | Max |
|---|---|---|---|---|
| Board Size | 6.73 | 1.61 | 5 | 12 |
| Proportion Female | 0.45 | 0.25 | 0 | 1 |
| Proportion Black | 0.25 | 0.26 | 0 | 1 |
| Proportion Democrat | 0.61 | 0.33 | 0 | 1 |
| At Least One Democrat | 0.87 | 0.34 | 0 | 1 |
| Majority Democrat | 0.61 | 0.49 | 0 | 1 |
| Proportion Board Missing | 0.27 | 0.20 | 0 | 0.86 |
| Electoral Contests | 2.38 | 1.40 | 1 | 8 |

*Notes*: 251 school boards (unique by district-election year).

Our regression discontinuity design uses quasi-random variation from electoral contests to isolate exogenous variation in these treatment variables. As shown in Table 1, there are a little over two electoral contests for the average school board, with every board having at least one contest and some of them having up to eight contests. We summarize characteristics of the electoral contests in Table 2. Our records span the 2008 through 2013 election years, with most occurring in even numbered years.[9] For the vast majority of North Carolina districts (and therefore boards), elections occur every two years. Notably, some electoral contests (termed "at-large" contests) contain multiple winners, represented by an average of 1.53 per contest. On average, an election also contains 2.86 (non-write in) candidates.

To calculate the running variable in our regression discontinuity design, we identify the political identity (Democrat or Non-Democrat) of the least popular election winner and the most popular opposite-identity loser in each contest. We then take the absolute value of the difference between the pair's vote percentages and assign a positive (negative) sign if the winning candidate in the pair is a Democrat (non-Democrat). The margin thus reflects the distance, in percentage points

[8]These variables are constructed excluding board members who were unable to be matched with voter records.
[9]Unfortunately, the State Board of Elections does not have records for school board elections prior to 2008.

Table 2: Electoral Contest Characteristics

| | Mean | SD | Min | Max |
|---|---|---|---|---|
| Election Year | 2010.12 | 1.61 | 2008 | 2012 |
| Cycle Length | 2.06 | 0.33 | 2 | 4 |
| # Winners | 1.53 | 0.97 | 1 | 5 |
| # Candidates | 2.86 | 2.36 | 1 | 17 |
| Proportion Cand. Missing | 0.12 | 0.19 | 0 | 0.8 |
| Vote Margin | 21.06 | 63.67 | -100 | 100 |

*Notes*: 470 electoral contests.

of the total vote, from switching the political identity of the marginal winner of a contest. To do this, we must match electoral contest losers with the voter registration database (in addition to school board members). The match is fuzzy and based on within-country name, and we keep both unique matches and any non-unique matches for which the political affiliation is the same across them.[10] As summarized in Table 2, this matching process results in 88 percent of electoral contest candidates linked to voter registration records on average. Note that the process of constructing these pairs for each electoral contest is designed to ignore and skip over the most popular loser if, for example, that candidate and the least popular winner have the same political affiliation.[11] The Democrat vote margin variable we create spans -100 to 100 with an average of about 21 percentage points.

## 2.2 School District Characteristics and Outcomes

To connect the composition of school boards with district outcomes, we draw on rich student-level data, provided by the NCERDC. We restrict our sample to traditional public schools in North Carolina serving kindergarten through the fifth grade, using students' race or ethnicity and economic disadvantage status to calculate district-level sociodemographic variables by year. More importantly for our analysis, we use such information to calculate dissimilarity indices to measure segregation in each district by year. For characteristic $z$ (e.g., economic disadvantage), the dissimilarity index for a given district in a given year is computed using:

$$\frac{1}{2}\sum_{k}\left|\frac{z_k}{Z}-\frac{n_k-z_k}{N-Z}\right|,$$

[10] This is in contrast to the board member match, where we include only unique matches.

[11] We treat the mass of write-in candidates in each contest as an opposite-type loser by construction.

where $Z$ is the total number of students with that characteristic and $N$ is total district enrollment. In this expression, $k$ indexes units over which segregation is calculated. Thus, for our primary outcomes of interest (involving segregation across schools), $k$ indexes schools in the district. For the purpose of assessing the validity of our research design, we additionally measure residential segregation using students' geocoded addresses (also obtained from the NCERDC) to compute dissimilarity indices over Census block groups (as opposed to schools). Beyond its frequent use in the literature, the dissimilarity index has attractive properties for quantifying segregation. Bounded between 0 and 1, the calculated value can be interpreted as the share of characteristic $z$ students that would need to be reallocated in order to equalize their share across $k$.

We compute dissimilarity indices by year and district for black and economically disadvantaged students. In focusing on these dimensions of segregation, our analysis examines whether the political composition of the school board causally influences school segregation and reveals information about school boards' objectives from their behavior. For instance, school boards of different political compositions may prioritize addressing different dimensions (racial or economic) of segregation.[12] On the other hand, it may instead be the case that a type of school board member does not act to offset household sorting, revealing a limited preference for equality (relative to the costs imposed by economic or political constraints or to other objectives) in general.[13]

Table 3 summarizes characteristics and outcomes for school boards. The statistics reported correspond to district variables in the year of the next election (i.e., at the end of the board's "term"). The average school board represents a district that is 26 percent black and in which 64 percent of students are economically disadvantaged. Further, there is considerable heterogeneity in these characteristics, with some entirely white districts and others nearly all black. The average black dissimilarity index across block groups (our measure of residential segregation) is 0.46, indicating that 46 percent of black students would need to be reassigned to new block groups to equalize the black share across them. This high degree of residential segregation on average highlights the challenges facing a school board for integrating schools. By comparison, the average residential dissimilarity index for economic disadvantage is 0.34.

[12]For example, in the year 2000, Wake County switched from an attendance zone plan prioritizing racial balance to one prioritizing socioeconomic balance. Hoxby and Weingarth (2005) use this policy change to investigate the structure of classroom peer effects.

[13]In addition, the costs and benefits of desegregation (in terms of student outcomes) will generally depend on how peers affect education production.

Table 3: District Characteristics and Outcomes

| | Mean | SD | Min | Max |
|---|---|---|---|---|
| % Black | 0.26 | 0.22 | 0 | 0.95 |
| % Economic Disadvantaged | 0.64 | 0.13 | 0.08 | 0.93 |
| Residential Segregation – Black | 0.46 | 0.17 | 0 | 1 |
| Residential Segregation – Economic Disadvantaged | 0.34 | 0.11 | 0.06 | 0.93 |
| School Segregation – Black | 0.31 | 0.16 | 0 | 0.75 |
| School Segregation – Economic Disadvantaged | 0.22 | 0.13 | 0 | 0.69 |

*Notes*: 251 school boards (unique by district-election year). Residential segregation measure is missing for one board.

Table 3 also summarizes our primary outcome variables of interest, which are racial and economic segregation across schools. In general, districts are more segregated residentially than the schools are segregated: to equalize the share of black students in schools within the district, the average district would need to reassign 31 percent of black students to new schools. However, some school boards preside over districts with a high degree of integration, while other districts are acutely segregated. In terms of economic school dissimilarity, the average district would need to reassign 22 percent of economically disadvantaged students to equalize their share across schools, with similarly large variability across districts.

## 3 Research Design

To determine the effect of school board composition on within-district segregation across schools, a reasonable starting point would be to estimate the following simple ordinary least squares specification:

$$Y_{j\tau} = \alpha + \gamma T_j + \delta W_{j0} + \epsilon_j \,, \tag{1}$$

where $Y_{j\tau}$ is a school racial or economic dissimilarity measure for school board $j$ (uniquely defined by a district and election year combination) at the end of an election cycle (indicated by $\tau$), $T_j$ is the treatment status of the school board, and $W_{j0}$ is a vector of observed covariates measured at the beginning of the election cycle. While there are multiple types of treatment in which one might be interested, we consider three treatments based on political composition (as mentioned earlier): the proportion of Democrat school board members, whether at least one board member is

a Democrat, and whether a majority of members are Democrats.

The reason for including these different dimensions in our analysis is that it is unclear how a school board's political composition maps into actions and outcomes. For example, it may be that broad agreement on objectives, as might occur if a majority of board members are politically aligned, is key for effectual action. On the other hand, it is also conceivable that minority voices on boards are important, a possibility which we consider using the treatment of "at least one Democrat."[14] Our multi-treatment analysis helps provide a clearer picture of the importance of these different compositional factors.

Obtaining credible causal estimates of the primary parameter of interest, $\gamma$, depends on addressing probable sources of endogeneity. In particular, estimating equation (1) will produce a biased value of $\hat{\gamma}$ if an omitted variable $Q_j$ is correlated with the outcome of interest ($cov(Q_j, \epsilon_j) \neq 0$) and with treatment ($cov(Q_j, T_j) \neq 0$). Two potentially problematic candidates for $Q_j$ both stem from household sorting patterns across neighborhoods that is correlated with board composition and segregation across schools, but the candidates differ according to the direction of bias implied by their omission. In the first case, it may be that people who reside in less segregated neighborhoods politically align more closely with Democrats. Given that neighborhood segregation is likely to be correlated with segregation across schools, omitting a measure of neighborhood segregation from the regression would lead to downward-biased estimates.[15] Fortunately, we are able to rule out such endogeneity by controlling for residential dissimilarity (i.e., including it in $W_{j0}$).[16]

The second case is not so easily addressed. It stems from the unobserved mapping between the residential concentration of student types within a district and the feasible ways in which types can be allocated to schools.[17] In a setting without constraints, a planner would be able to select any many-to-one matching function, which assigns every student in a district to a school, irrespective of

[14] Note that this need not be due to a single Democrat on a school board being persuasive. It could also indirectly arise from an overall shift in the distribution of the board's preferences (i.e., by altering who is decisive). For a discussion of this point in the context of juries, see Anwar, Bayer and Hjalmarsson (2012).

[15] As Appendix Table A.1 shows, districts represented by a majority Democrat school board indeed feature lower racial residential and school segregation.

[16] While the results we present do not include residential dissimilarity as a control, estimates from specifications with it included (available upon request) suggest that this form of bias is not of first-order importance.

[17] For our purposes, concentration describes how geographically dispersed student types are within a district. It is related to, but not necessarily the same as, residential segregation. Whereas segregation would be high if particular types reside only in a subset of neighborhoods (which we define as Census block groups in our analysis), the degree to which those neighborhoods are clustered within a particular geographic region of the district is what determines concentration.

where they reside. It would then be trivial for the planner to achieve whatever level of segregation across schools she desires. However, matching will be constrained in practice by several factors. First and foremost, infrastructure and organizational constraints should imply an optimal school size well below total district enrollment, resulting in multiple schools per district.[18] A second key factor is the cost of transporting students to schools, which is a function of the distance between a student's residence and her assigned school. A third factor that is likely to be important is policy opposition by constituents, in which parents resist the adoption of unusually shaped attendance zones (a key channel through which school boards alter school segregation, as we will establish in Section 5) that fragment school assignment within local neighborhoods.

Given such constraints, the concentration of student types within a district will determine the scope for reducing segregation through the manipulation of attendance zones. At one extreme, essentially no neighborhood dissimilarity should result for a particular type if such students are located uniformly throughout a district. Consequently, any set of school attendance zones would imply low segregation across schools. At the other extreme, suppose that all students of a particular type reside in one concentrated area of a district. Given some fixed number of schools and non-trivial transportation and political constraints, it would be potentially very costly to allocate those students equally across all schools. While the reality lies in between these two extremes, the main takeaway is that concentration of student types is likely to be associated with greater student segregation across schools.

Even if one includes a suitable control for the concentration of types in equation (1), the fact that board constraints (which determine the linkage to the set of feasible school allocations) are unobserved is likely to bias estimates.[19] For example, more severe constraints $Q_j$ (through higher transportation costs and/or greater opposition) for a given degree of concentration should be associated with increased segregation across schools: $cov(Q_j, \epsilon_j) > 0$. If highly constrained districts are more likely to elect Democrats to counteract high costs or opposition, so that $cov(Q_j, T_j) > 0$, then upward bias would result.

We tackle the empirical challenge that results from the omission of such variables by imple-

[18] An obvious example of an infrastructure constraint is building size, while an example of an organizational constraint is potential deleterious effects of having to manage too many classrooms within a school.

[19] Moreover, given that the constraints are almost certainly time-varying, they are unlikely to be addressed by including fixed effects using panel variation.

menting a regression discontinuity design at the electoral contest level, leveraging quasi-random variation from narrowly-decided electoral contests.[20] We observe $N_j$ electoral contests for each school board $j$, which are indexed by $i$. Recall that to construct the Democrat vote margin, we identify the political identity (Democrat/Non-Democrat) of the least popular election winner (noting that some contests have multiple winners) and the most popular opposite-identity loser for each electoral contest. By construction, this implies that a Democrat in contest $i$ wins a seat on school board $j$ if the vote margin $x_{ij}$ is positive, where $x_{ij}$ is the difference in vote shares. We define the indicator $D_{ij} = \mathbf{1}(x_{ij} \geq 0)$ to reflect this. These potential discontinuities form the basis of our empirical strategy.

Given that our setting is characterized by potentially multiple discontinuities for each school board (in contrast to typical applications of the regression discontinuity design), we pool electoral contests in the following first-stage regression:

$$T_j = \pi + \theta D_{ij} + f(x_{ij}) + \kappa Z_{ij} + \upsilon_{ij}\,, \tag{2}$$

where $Z_{ij}$ includes a vector of observed election-level covariates in addition to the board-level variables that appear in $W_{j0}$. In pooling the data this way, we require that $\upsilon_{ij}$ is independent across contests within a school board.

Based on the preceding equations, our two-stage least squares estimation procedure proceeds in the following way. In the first stage, we estimate the parameters of equation (2) and calculate a predicted value of treatment $\hat{T}_j$. In the second stage, we then carry out the regression in equation (1), using $\hat{T}_j$ in place of $T_j$. The resulting estimate of $\gamma$ ($\hat{\gamma}_{2SLS}$) should be free of the type of bias discussed previously. We can compare it to the naive estimate from equation (1) without instrumenting for $T_j$ ($\hat{\gamma}_{OLS}$) to determine the direction and magnitude of bias. Of course, we are also able to estimate reduced-form effects across all electoral contests by directly regressing $Y_{j\tau}$ on $D_{ij}$, which we do to show visual evidence of a discontinuity in outcomes. However, we view the two-stage results as more compelling, both in terms of interpretation (where interpreting the board treatment is more straightforward) and their capacity to filter out noise from contests that are held in inconsequential districts.

[20] Our design is in the spirit of similar regression discontinuity designs that use electoral outcomes (Lee, Moretti and Butler 2004; Ferreira and Gyourko 2009; Beach and Jones 2017).

It is worth noting that both our first- and second-stage specifications include observable characteristics as controls. Strictly speaking, such controls should not be necessary if the validity assumption holds for our regression discontinuity design.[21] We choose to include them for two reasons. First, the set of controls, which includes the composition of board members not involved in an election, reduce the variance of our estimates. This is key, given the relatively small number of district-year combinations to which we have access in our data. The second reason for including controls is that some of them affect the probability density function of vote share differences. For example, a greater number of candidates in an election would make smaller differences in vote share $x_{ij}$ more likely, as a candidate's vote share is decreasing in the number of candidates.

In addition to our main analysis, we are able to adapt our two-stage procedure to analyze the mechanisms behind any detected changes in segregation. In particular, we alternatively replace the dependent variable in equation (1) with a measure of attendance zone boundary changes (detailed in Section 5), the proportion of households that move within a school district, and the proportion of students attending public schools in a district who are white. This allows us to causally examine a key channel through which school boards alter segregation and uncover household responses that result.

# 4 Results

The results of our regression discontinuity analysis are grouped into four categories. In the first subsection, we provide evidence of a strong first stage along two dimensions. First, we document large discontinuities in electoral contest-level outcomes as a result of the Democratic vote margin switching from negative to positive, providing a foundation for our reduced-form analysis. Second, with a view toward our two-stage least squares estimates, we detail similar discontinuities at the school board level, revealing substantial first-stage effects of Democrat electoral wins on the proportion of Democrat school board members, whether at least one board member is a Democrat, and whether a majority of members are Democrats. In the second subsection, we provide reduced-form graphical and statistical evidence of the causal effect that Democrats have on segregation across schools (relative to non-Democrats). In the third subsection, we build upon these results by re-

[21] We assess validity by separately replacing the dependent variable in equation (1) with $W_{j0}$ and $Y_{j0}$, and alternatively implementing our two-stage procedure for counterfactual placebo thresholds, where no effects should exist.

porting two-stage least squares estimates of the three types of treatment outlined in the first stage, contrasting each with an analogous non-causal estimate using a comparatively naive ordinary least squares approach. In the fourth subsection, we assess the validity of our causal claims, testing for discontinuities in observable characteristics and pre-treatment outcomes, and in post-treatment outcomes at counterfactual placebo cutoffs.

### 4.1 First Stage

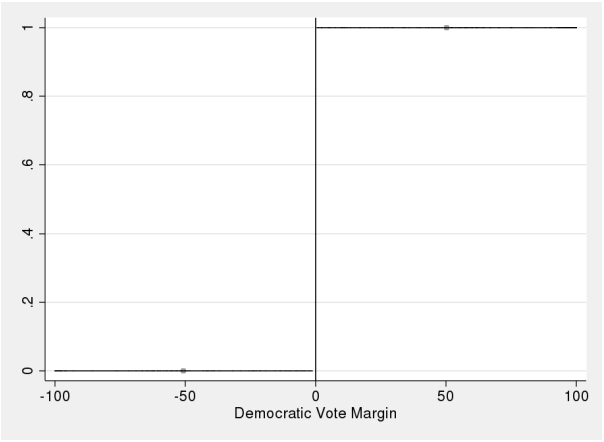
Before presenting our main results (both reduced-form and two-stage least squares), it is important to establish that the vote shares of candidates in election contests have a pronounced effect on various treatment definitions of interest. Figure 1 reveals the discontinuous effect of a Democrat winning a contest on the proportion of Democrat winners along three dimensions: (a) winners in single-winner electoral contests; (b) winners in single- and multi-winner electoral contests; and (c) winners across all electoral contests for a given school board. By construction, the first discontinuity should be sharp and the others should be fuzzy, which is precisely what Figure 1 shows. Panel (a) features a sharp jump from 0 to 1 at the threshold, while panels (b) and (c) display discontinuities of approximately 50 and 20 percentage points in Democrat share, respectively.

As one would expect, these discontinuities translate into discontinuities in key treatment variables at the school board level.[22] The panels of Figure 2 detail strong first-stage treatments along three dimensions: (a) the proportion of school board members who are Democrats; (b) the presence of at least one Democrat on the school board; and (c) whether a majority of school board members are Democrats. All three corresponding panels feature clear discontinuities, albeit fuzzy (as one would expect).[23] Using global polynomial fits without controls, panels (a), (b) and (c) suggest discontinuities of approximately 35, 45 and 40 percentage points, respectively.

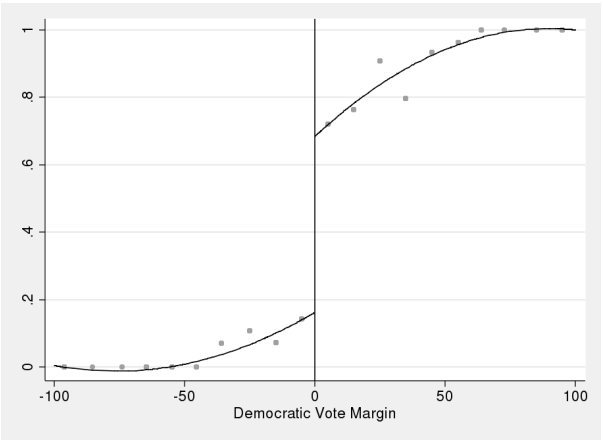
The preceding values provide an indication of what we use to compute our two-stage least squares estimates in subsection 4.3. We estimate equation (2) for each of our three treatment variables to obtain specific estimates for this purpose. Using standard errors clustered at the board level, they are reported in Table 4 for four different specifications: (1) a local linear regression

[22]The main distinction between the electoral and board level being that the latter includes both members who are and members who are not on the ballot of an electoral contest in a given year. So board-level treatments depend on the composition of both incumbent and newly-elected members.

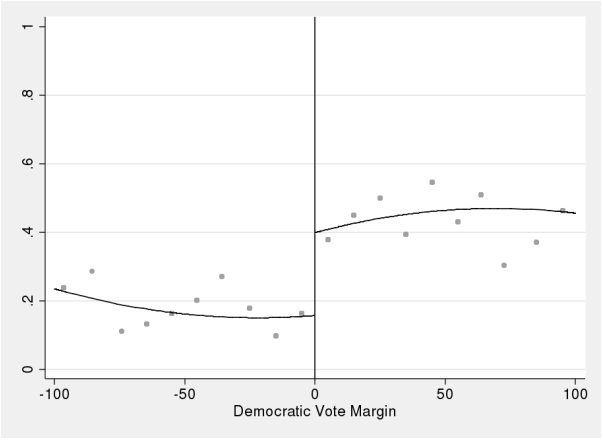
[23]Panel (b) shows a jump to a value of one to the right of the threshold, which must be true since, by definition, there would be at least one Democrat on a board if a Democrat wins an electoral contest.




(a) Single Winner Contests



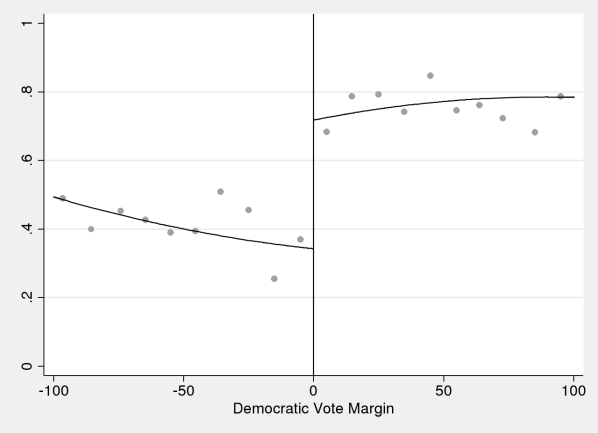

(b) All Contest Winners



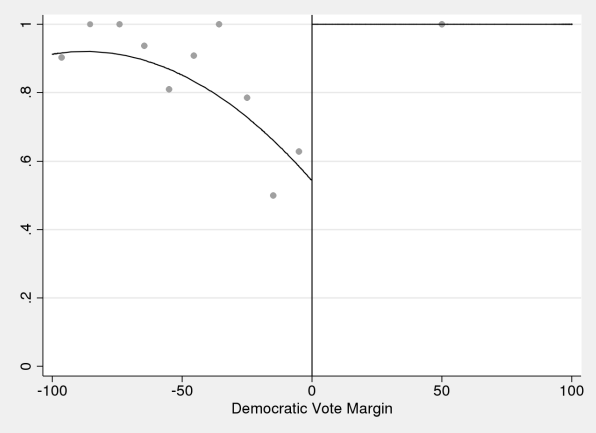

(c) All Election Winners

*Notes*: In panel (a), (b) and (c), the vertical axis is the proportion of Democrat winners in single-winner contests, the proportion of Democrat winners in single- and multi-winner contests, and the proportion of Democrat winners across all contests, respectively. Each figure is created by plotting the average of the relevant proportion within each of ten evenly-spaced bins of the Democrat vote margin on either side of the margin threshold, and fitting the data using a quadratic polynomial without controls.

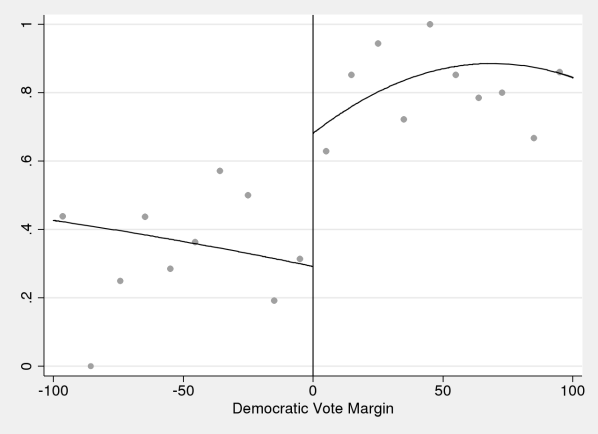
Figure 1: First Stage – Election Contest Treatments




(a) Proportion Democrat




(b) At Least One Democrat




(c) Majority Democrat

*Notes*: In panel (a), (b) and (c), the vertical axis is the proportion of school board members who are Democrats, whether a school board has at least one Democrat member, and whether a majority of school board members are Democrats, respectively. Each figure is created by plotting the average of the relevant variable across boards within each of ten evenly-spaced bins of the Democrat vote margin on either side of the margin threshold, and fitting the data using a quadratic polynomial without controls.

Figure 2: First Stage – School Board Treatments

Table 4: First-Stage Treatments – School Board Level

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Proportion Democrat | | | |
| Margin $> 0$ | 0.292*** | 0.222*** | 0.287*** | 0.227*** |
| | (0.0850) | (0.0609) | (0.0452) | (0.0578) |
| Controls | N | Y | Y | Y |
| No. Obs. | 146 | 146 | 470 | 470 |
| BW / Order | 27.61 | 27.61 | 2 | 3 |
| | At Least One Democrat | | | |
| Margin $> 0$ | 0.301*** | 0.315*** | 0.385*** | 0.335*** |
| | (0.108) | (0.109) | (0.0663) | (0.0907) |
| Controls | N | Y | Y | Y |
| No. Obs. | 122 | 122 | 470 | 470 |
| BW / Order | 19.41 | 19.41 | 2 | 3 |
| | Majority Democrat | | | |
| Margin $> 0$ | 0.325** | 0.170 | 0.231*** | 0.153 |
| | (0.135) | (0.108) | (0.0842) | (0.109) |
| Controls | N | Y | Y | Y |
| No. Obs. | 156 | 156 | 470 | 470 |
| BW / Order | 30.23 | 30.23 | 2 | 3 |

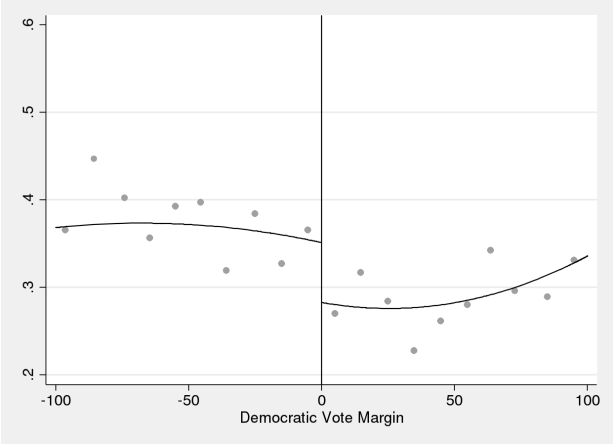
*Notes*: Standard errors clustered by board. ** and *** indicates statistical significance at the 5% and 1% levels, respectively. Control variables in columns (2) through (4) include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, and year fixed effects.

without controls, using an optimal bandwidth based on Calonico, Cattaneo and Titiunik (2014) with a uniform kernel (hereafter, 'optimal CCT bandwidth'); (2) an analogous local linear regression with district and election controls; (3) a global quadratic in Democrat vote margin that is flexibly allowed to differ on either side of the threshold and includes controls; and (4) a global cubic analogue of the quadratic specification. The first stage estimated discontinuity in treatment ranges from 22 to 29 percentage points for the proportion of school board members who are Democrats, from 30 to 39 percentage points for the presence of at least one Democrat on the school board, and from 15 to 33 percentage points for the probability that the school board is majority Democrat. These results establish the relevance of our regression discontinuity design: a close election has significant implications for the political composition of the seated school board.

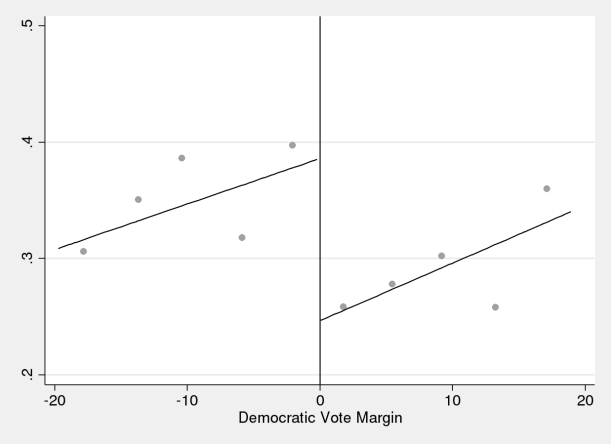
### 4.2 Reduced Form

In Figures 3 and 4, we consider whether visual evidence exists of discontinuities in racial and economic segregation, respectively. Based on data at the electoral contest level, Figures 3(a) and 3(b) indicate a notable discontinuous reduction in the black dissimilarity index at the vote margin threshold, using a global quadratic fit and local linear fit, respectively (both without conditioning on controls). This is consistent with Democrat board members preferring to reduce racial segregation of students across schools more than their non-Democrat counterparts. Based on Figures 4(a) and 4(b), it is less clear that any analogous reduction in the economic dissimilarity index occurs.

To be more concrete about the magnitude and significance of the effects, Tables 5 and 6 present reduced-form estimates of the effect of a Democrat electoral win on segregation across schools along racial and economic lines, respectively. In each case, estimates are reported for several specifications. Columns (1) and (2) use a local linear regression with optimal CCT bandwidth, column (3) uses a local linear regression with half the CCT bandwidth, and column (4) and (5) uses a global quadratic and cubic polynomial fit, respectively, which is allowed to differ on either side of the threshold. With the exception of column (1), all specifications incorporate controls, including the number of electoral contest winners and candidates (and interaction), whether the contest is unopposed, the proportion of unmatched candidates in the contest, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length



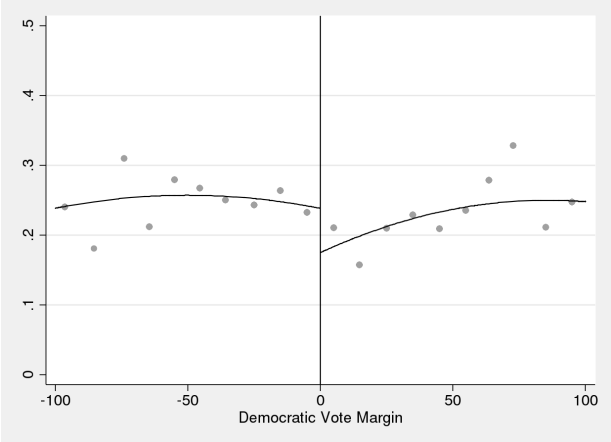

(a) Global Quadratic Fit



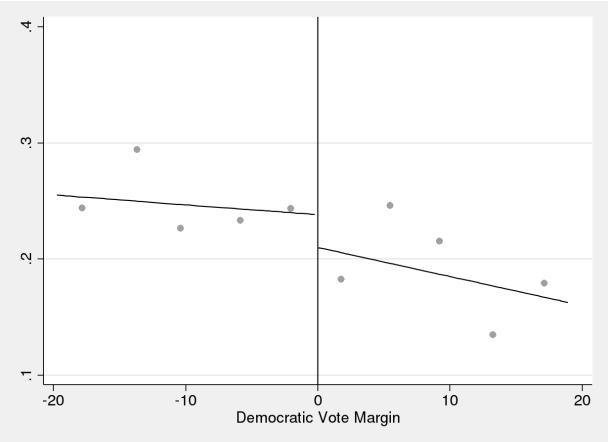

(b) Local Linear Fit

*Notes*: The vertical axis is the black dissimilarity index in both panels. Panel (a) is created by plotting the average of the index across boards within each of ten evenly-spaced bins of the Democrat vote margin on either side of the margin threshold, and fitting the data using a quadratic polynomial without controls. Panel (b) is created by plotting the average of the index across boards within each of five evenly-spaced bins of the Democrat vote margin on either side of the margin threshold (for the domain $-20 < x_{ij} < 20$), and fitting the data using a linear regression without controls.

Figure 3: Reduced-Form Effect on School Racial Segregation




(a) Global Quadratic Fit




(b) Local Linear Fit

*Notes*: The vertical axis is the economic dissimilarity index in both panels. Panel (a) is created by plotting the average of the index across boards within each of ten evenly-spaced bins of the Democrat vote margin on either side of the margin threshold, and fitting the data using a quadratic polynomial without controls. Panel (b) is created by plotting the average of the index across boards within each of five evenly-spaced bins of the Democrat vote margin on either side of the margin threshold (for the domain $-20 < x_{ij} < 20$), and fitting the data using a linear regression without controls.

Figure 4: Reduced-Form Effect on School Economic Segregation

Table 5: Reduced-Form Effects on School Racial Segregation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Margin > 0 | -0.134** | -0.143*** | -0.125* | -0.0397 | -0.0962** |
| | (0.0558) | (0.0534) | (0.0716) | (0.0352) | (0.0464) |
| Controls | N | Y | Y | Y | Y |
| No. Obs. | 128 | 128 | 74 | 470 | 470 |
| BW / Order | 22.42 | 22.42 | 11.21 | 2 | 3 |

*Notes*: Standard errors clustered by board. *, **, and *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively. Control variables in columns (2) through (5) include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, and year fixed effects.

Table 6: Reduced-Form Effects on School Economic Segregation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Margin > 0 | -0.0418 | 0.00385 | 0.00455 | -0.0338 | -0.0283 |
| | (0.0368) | (0.0266) | (0.0279) | (0.0234) | (0.0319) |
| Controls | N | Y | Y | Y | Y |
| No. Obs. | 159 | 159 | 99 | 470 | 470 |
| BW / Order | 31.73 | 31.73 | 15.87 | 2 | 3 |

*Notes*: Standard errors clustered by board. Control variables in columns (2) through (5) include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, and year fixed effects.

of electoral cycle, and year fixed effects.

Confirming visual evidence from Figures 3 and 4, we find large and statistically significant negative effects for segregation along racial lines, but small and statistically insignificant effects for segregation along economic lines. With respect to the effect on racial segregation across schools, the local linear estimate without controls shows that an electoral win by a Democrat is associated with an approximately 13 percentage point reduction in the black dissimilarity index across schools within a district. To provide context, the average proportion of black students who would need to

be reallocated to equalize their school share across schools is 0.31.

### 4.3 Second-Stage Treatment Effects

Building upon the reduced-form results, we now present estimated effects on racial and economic segregation across schools for the three types of treatment outlined in subsection 4.1: the percentage of Democrat school board members, whether at least one board member is a Democrat, and whether a majority of members are Democrats. To do so, we compute two-stage least squares estimates of $\beta$ in equation (1), using electoral contest discontinuities as instruments for treatment (as in equation (2)).[24] To assess the extent to which such a procedure is necessary (and, if so, the direction of bias caused by omitted variables), we also present naive ordinary least squares analogue estimates and then report the p-value of a Hausman specification test of whether the two types of estimates differ statistically.

Estimates for racial segregation across schools are shown in Table 7. For all treatments, the two-stage least squares estimates are significant at the 5 percent level and reveal substantial effects. The first row of column (2) indicates that a 14 percentage point increase in the proportion of Democrats on a school board (which is equivalent to one new Democrat member for the average board) causes a 2 percentage point reduction in the black school dissimilarity index (or 13 percent of its standard deviation of 0.16). The second and third rows of column (2) reveal that the election of a new Democrat board member in the minority (the "at least one Democrat" treatment) leads to a reduction in the black dissimilarity index across schools of nearly 18 percentage points, while a shift to a Democrat majority on a board causes a 12 percentage point decrease. Expressed in terms of variation in the black school dissimilarity index, these effects are equivalent to 1.1 and 0.75 of its standard deviation, respectively.[25] These findings suggest that a diversity of opinions and preferences on school boards may be as important (if not more important, in terms of magnitude) as having a wider agreement about objectives.

Comparing each of the estimates to their ordinary least squares counterparts in column (1),

[24] All two-stage least squares results depend on a first stage that uses a quadratic global polynomial in vote margin (the specification in column (3) of Table 4) to estimate the discontinuity. The polynomial terms are used as excluded instruments.

[25] Analogous results using a cubic polynomial fit in the first stage (available upon request) are very similar. Note that while we do not find statistical significance for the corresponding quadratic-based reduced-form results in Table 5, the two-stage least squares procedure filters out noise from the first stage that enters into the suggestive reduced-form analysis.

Table 7: Treatment Effects on School Racial Segregation

| | (1) OLS | (2) 2SLS | (3) $H_0 : \beta_{OLS} = \beta_{2SLS}$ |
|---|---|---|---|
| Proportion Democrat | -0.0816* | -0.140** | 0.059 |
| | (0.0487) | (0.0575) | |
| At Least One Democrat | -0.0613 | -0.175** | 0.056 |
| | (0.0387) | (0.0723) | |
| Majority Democrat | -0.0405 | -0.122** | 0.023 |
| | (0.0289) | (0.0475) | |

*Notes*: 470 electoral contests. Standard errors clustered by board. * and ** indicates statistical significance at the 10% and 5% levels, respectively. Control variables include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, and year fixed effects. First stage uses quadratic polynomial in vote margin.

Table 8: Treatment Effects on School Economic Segregation

| | (1) OLS | (2) 2SLS | (3) $H_0 : \beta_{OLS} = \beta_{2SLS}$ |
|---|---|---|---|
| Proportion Democrat | -0.00594 | -0.0345 | 0.222 |
| | (0.0322) | (0.0412) | |
| At Least One Democrat | 0.0112 | -0.0622 | 0.139 |
| | (0.0197) | (0.0529) | |
| Majority Democrat | -0.0102 | -0.0114 | 0.967 |
| | (0.0175) | (0.0326) | |

*Notes*: 470 electoral contests. Standard errors clustered by board. Control variables include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, and year fixed effects. First stage uses quadratic polynomial in vote margin.

we see that the naive approach suffers from upward bias. In particular, properly correcting for endogeneity leads to negative point estimates that are between 1.7 and 3 times larger in magnitude than when not doing so. The third column provides a test of the null that the ordinary least squares and two-stage least squares estimates are statistically identical: we reject the null at the 10 percent level for the proportion Democrat and at least one Democrat treatments, and we reject it at the 5 percent level for the majority Democrat treatment.

An analogous analysis of treatment effects for the economic segregation outcome are reported in Table 8. Consistent with the reduced-form findings, all of the point estimates (two-stage least squares and ordinary least squares included) are insignificant and close to zero in magnitude. This is consistent with the interpretation that, relative to non-Democrats, Democrats do not differ in the degree to which they tolerate economic segregation.

### 4.4 Validity

The first-stage results indicate that elections generate a discontinuity in the composition of school boards, but the ability of our research design to produce unbiased estimates requires that the assumption of validity holds. While it is not directly testable for unobserved characteristics, we are able to provide three types of suggestive evidence based on observables to lend credence to our causal claim. First, we examine whether any observed covariates are discontinuous at the voting margin threshold. For instance, at the vote margin threshold, there should be no discontinuity in the composition of school board members not involved in an election. Table 9 reports discontinuity estimates for both quadratic and cubic polynomial fits. We see that, out of a total of eight characteristics, only one is significant at the 5 percent level for the quadratic fit and none are significant for the cubic fit. At the margin, a Democrat win is associated with boards that have fewer missing members (which is dictated by our ability to match members to the database of registered voters) and a greater proportion of uncontested Democrats (where the vote margin for their election is 100 percent).

The second type of evidence for validity entails a test of whether any pre-treatment outcomes (prior to an elected school board's tenure) are discontinuous at the threshold. Table 10 establishes that no such discontinuities exist. In particular, across six outcomes (the proportion, residential dissimilarity and school dissimilarity for both racial and economic dimensions), none of the point

Table 9: Validity – Testing for Discontinuities in Control Variables

| Order | (1) # Winners | (2) # Candidates | (3) Electoral Contests | (4) Board Members |
|---|---|---|---|---|
| 2 | -0.132 | -0.811 | 0.157 | 0.115 |
| | (0.283) | (0.644) | (0.343) | (0.410) |
| 3 | -0.150 | -0.171 | 0.526 | 0.0125 |
| | (0.351) | (0.867) | (0.404) | (0.558) |
| Order | Prop. Cand. Missing | Prop. Board Missing | Prop. Uncontested Dem | Prop. Uncontested Black |
| 2 | -3.997 | -0.0992** | 0.167* | 0.0959 |
| | (4.282) | (0.0500) | (0.0930) | (0.0730) |
| 3 | 4.288 | -0.103 | 0.0259 | 0.0196 |
| | (5.155) | (0.0639) | (0.123) | (0.100) |

*Notes*: 470 electoral contests. Standard errors clustered by board. * and ** indicates statistical significance at the 10% and 5% levels, respectively.

Table 10: Validity – Testing for Discontinuities in Pre-Treatment Outcomes

| Order | (1) % Black | (2) % Econ. Dis. | (3) Black Res. Seg. | (4) Econ. Res. Seg | (5) Black School Seg. | (6) Econ. School Seg. |
|---|---|---|---|---|---|---|
| 2 | 0.0537 | 0.0215 | -0.0214 | -0.00344 | -0.0220 | -0.0281 |
| | (0.0407) | (0.0242) | (0.0475) | (0.0192) | (0.0341) | (0.0223) |
| 3 | 0.0379 | 0.0230 | -0.0430 | -0.00266 | -0.0741 | -0.0402 |
| | (0.0474) | (0.0318) | (0.0626) | (0.0253) | (0.0451) | (0.0303) |

*Notes*: 470 electoral contests. Standard errors clustered by board. Control variables include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, and year fixed effects.

Table 11: Validity – Testing for Discontinuities in Segregation at Placebo Breaks

| Panel A: Racial Segregation Across Schools | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Order | (1) -40 | (2) -36 | (3) -32 | (4) -28 | (5) -24 | (6) -20 | (7) -16 | (8) -12 | (9) -8 | (10) -4 |
| 2 | 0.0108 | 0.0580 | 0.0806 | 0.0508 | -0.0394 | -0.00369 | 0.0375 | 0.0363 | 0.0194 | 0.0314 |
| | (0.0465) | (0.0510) | (0.0503) | (0.0511) | (0.0653) | (0.0549) | (0.0434) | (0.0402) | (0.0403) | (0.0357) |
| 3 | -0.116 | -0.0290 | 0.0248 | -0.0120 | -0.128 | -0.0551 | -0.00385 | 0.00536 | -0.00848 | 0.0223 |
| | (0.0769) | (0.0759) | (0.0718) | (0.0740) | (0.0887) | (0.0654) | (0.0586) | (0.0594) | (0.0563) | (0.0475) |
| | 4 | 8 | 12 | 16 | 20 | 24 | 28 | 32 | 36 | 40 |
| 2 | 0.00645 | 0.0216 | 0.0184 | 0.000100 | -0.0467 | -0.0474 | -0.0418 | -0.000954 | 0.0305 | -0.0124 |
| | (0.0370) | (0.0373) | (0.0375) | (0.0370) | (0.0363) | (0.0383) | (0.0415) | (0.0456) | (0.0491) | (0.0414) |
| 3 | -0.0126 | 0.0197 | 0.0249 | -0.00503 | -0.0850* | -0.0731 | -0.0692 | -0.0253 | 0.0252 | -0.0482 |
| | (0.0466) | (0.0450) | (0.0466) | (0.0501) | (0.0442) | (0.0502) | (0.0540) | (0.0563) | (0.0660) | (0.0509) |
| Panel B: Economic Segregation Across Schools | | | | | | | | | | |
| Order | (1) -40 | (2) -36 | (3) -32 | (4) -28 | (5) -24 | (6) -20 | (7) -16 | (8) -12 | (9) -8 | (10) -4 |
| 2 | -0.00888 | -0.00962 | 0.0115 | 0.00238 | -0.00304 | -0.0153 | -0.0375 | -0.0641** | -0.0416 | -0.00811 |
| | (0.0324) | (0.0339) | (0.0367) | (0.0404) | (0.0351) | (0.0320) | (0.0284) | (0.0269) | (0.0259) | (0.0240) |
| 3 | -0.00783 | -0.000497 | 0.0383 | 0.0284 | 0.0195 | 0.00417 | -0.0157 | -0.0702** | -0.0289 | 0.0224 |
| | (0.0419) | (0.0438) | (0.0513) | (0.0537) | (0.0424) | (0.0331) | (0.0351) | (0.0328) | (0.0307) | (0.0346) |
| | 4 | 8 | 12 | 16 | 20 | 24 | 28 | 32 | 36 | 40 |
| 2 | -0.00605 | -0.0421* | -0.0217 | 0.00812 | 0.0283 | 0.00831 | 0.0368 | 0.126** | 0.109 | 0.00406 |
| | (0.0246) | (0.0253) | (0.0256) | (0.0273) | (0.0319) | (0.0339) | (0.0387) | (0.0538) | (0.0674) | (0.0440) |
| 3 | 0.0117 | -0.0628 | -0.0365 | -0.00301 | 0.0166 | -0.0316 | 0.00494 | 0.142** | 0.131 | -0.0367 |
| | (0.0380) | (0.0408) | (0.0368) | (0.0357) | (0.0421) | (0.0419) | (0.0409) | (0.0601) | (0.0906) | (0.0676) |

*Notes*: 470 electoral contests. Standard errors clustered by board. * and ** indicates statistical significance at the 10% and 5% levels, respectively. Control variables include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, and year fixed effects.

estimates are significant for either the quadratic or cubic specifications.

The third type of evidence in favor of validity holding is an analysis of whether the primary post-treatment outcomes of interest are discontinuous at counterfactual placebo cutoffs. Table 11 presents estimates for both quadratic and cubic polynomial fits, using placebo Democrat vote margin thresholds placed at every 4 percentage points between -40 and 40 percentage points inclusive. Strikingly, none of the twenty placebos produce significant racial segregation estimates for the quadratic specification, and only one is significant at the 10 percent level for the cubic specification. While economic segregation results do indicate discontinuities for two of twenty placebos (across

both quadratic and cubic specifications), this is the dimension of segregation for which we find no significant causal effects.

# 5 Mechanisms

We have demonstrated that school boards causally affect student segregation, with Democrat board members reducing (relative to their non-Democrat counterparts) black dissimilarity across schools. In this section, we explore potential mechanisms underlying these causal effects. First, we establish that a key channel through which school boards reduce segregation is through the adjustment of attendance zone boundaries, using a novel measure of attendance zone shifts. Second, we consider how the overall segregation effects are mediated by the response of households to board actions through re-sorting, both within and across school districts.

## 5.1 Attendance Zone Shifts

To determine whether school boards alter attendance zone boundaries to affect segregation, we modify our two-stage least squares procedure using a new measure of boundary changes as the dependent variable. Before doing so, we first detail our methodology for constructing such a measure.

### 5.1.1 Obtaining a Measure of Attendance Zone Shifts

Attendance zone boundaries partition school districts into areas that link residences to specific schools, such that students in the same grade but residing on opposite sides of a boundary are assigned to different schools. To analyze how boundaries change over time, it would be ideal to possess a reliable dataset of geocoded boundaries over time for all districts in North Carolina, but such dynamic information is very difficult to procure.[26] Fortunately, information provided in the NCERDC dataset contains the elements necessary to infer boundary shifts without observing the boundaries themselves. In particular, the data contain information about where a student

[26]Third-party companies have started to provide attendance zone boundary information for a fee. However, it is available only for the very recent past and tends to be updated infrequently, so that year-to-year changes cannot be computed. In lieu of such aggregators, the relevant data could in principle be procured on a district-by-district basis, but there is large variability in the degree to which districts are willing to furnish such contemporaneous information, and historic boundaries are not always readily available to district staff themselves.

resides in terms of Census block group (through an encrypted address, using the North Carolina Transportation Information Management System) and which school they attend. This linkage allows us to measure changes in attendance zones, using year-to-year variation in school shares within a fixed geographic area.

The measure of boundary shifts that we construct is probabilistic, allowing for a block group to be served by more than one school. It depends on the proportion of students residing in block group $k$ who attend school $s$ under the purview of school board $j$ in year $t$ of the board's election cycle, which is given by:

$$P_{ksjt} = \frac{N_{ksjt}}{\sum_{s \in \mathcal{S}_j} N_{ksjt}},$$

where $N_{ksjt}$ is the corresponding number of students for the $k$-$s$-$j$-$t$ combination and $\mathcal{S}_j$ denotes the set of all public schools for board $j$. The fact that this proportion can be computed annually is key to creating a measure of year-to-year boundary changes. It is defined in the following way:

$$\phi_{kjt} = \frac{1}{2} \sum_{s \in \mathcal{S}_j} |P_{ksjt} - P_{ksjt-1}|. \tag{3}$$

An appealing property of our attendance zone shift measure is that it is bounded between 0 and 1 ($\phi_{kjt} \in [0, 1]$). It also has a straightforward interpretation: a larger value implies that a greater proportion of students from block group $k$ attend a different school than students in the prior cohort. At one extreme, $\phi_{kjt} = 1$, so that block group $k$ is served by an entirely different set of schools than in the previous year. This would indicate a boundary shift with respect to the block group. At the other extreme, $\phi_{kjt} = 0$, so that the block group is served by the same set of schools as in the previous year (in the same proportion), indicating no change in the boundary. We then aggregate $\phi_{kjt}$ across all block groups in a district to obtain a new and convenient board-level measure of attendance zone shifts for each year of a board's tenure, $\phi_{jt}$, where the aggregation procedure is described in the next sub-subsection.

It is worth noting that the $\phi_{jt}$ measure is not without its limitations. First, while it reveals that attendance zones have shifted for a given year, it does not indicate whether those shifts are associated with higher or lower school segregation. Second, institutional features may introduce noise into the measure. For instance, most districts offer students the option to transfer schools

subject to capacity and other limitations.[27] Third, the geographical size of block groups may be too large, in the sense that some of them could be bisected by attendance zones. In such cases, our measure would not be able to detect a swap of those zones. While this is an unavoidable data restriction in our setting, we do not view it as a first-order concern, since it would only bias us away from finding boundary shift effects. Fourth, to the extent that block group $k$ is served by multiple schools, noise in $\phi_{jt}$ (that is unrelated to school attendance zone shifts) may be introduced through year-to-year changes in cohort size. We take two actions to strengthen the signal contained within our measure (relative to potential noise): (1) compute $\phi_{kjt}$ only for block group-years in which at least 30 elementary school students' addresses are available; and (2) use measures of $\phi_{kjt}$ over time to normalize it within each school district.

### 5.1.2 Treatment Effects on Attendance Zone Shifts

We adapt our research design to examine how school boards influence attendance zone boundaries over time. To do so, we calculate the weighted average (by enrollment) of the normalized value of $\phi_{kjt}$, obtaining $\phi_{jt}$ for years $t = 1$ through $t = \tau$ of the school board's tenure.[28] We then replace the dependent variable of equation (1) with $\phi_{jt}$ and determine the effect of treatment on attendance zone shifts by estimating $\gamma$ using the two-stage procedure.[29]

Before presenting the effects from our boundary shift analysis, it is worth briefly discussing how they are related to our main results. Recall our finding that Democrat treatments cause reductions in segregation across schools, relative to non-Democrat counterfactuals. If Democrats act to reduce segregation, this result is consistent with non-Democrats preferring to either reduce segregation to a lesser extent or increase it. While we cannot differentiate between these non-Democrat objectives, we view them as having a preference for greater segregation (relative to its economic and political costs). As a result, we are able to use the effect of treatment on $\phi_{jt}$ to distinguish between two rival hypotheses about how they might accomplish this objective, exploiting the fact that the measure only captures the quantity (as opposed to the quantity and quality) of attendance zone

[27] Students must typically arrange their own transportation if they transfer, and transfer applications are subject to the school board's approval, often with the advice and consent of the receiving school principal. It is important to note that open enrollment is not practiced by any districts in North Carolina for our time period of interest.

[28] We provide some descriptive statistics of our attendance zone boundary shift measure, according to majority political affiliation of the board, in Appendix Table A.2.

[29] Note that we are pooling all years of a school board's tenure (years 1 through $\tau$) in this specification, as our attendance zone measure uses year-to-year changes.

manipulation. Assuming that residential sorting tends toward greater segregation over time (which is a key rationale for school boards intervening to address segregation), the goal could either be accomplished through suitable boundary changes or through inaction. Our analysis of the boundary change mechanism allows us to determine which type of non-Democrat behavior prevails.

Table 12: Effects on Measured Attendance Zone Shifts

| | (1) RF | (2) 2SLS | (3) 2SLS | (4) 2SLS |
|---|---|---|---|---|
| Margin $> 0$ | 0.187** | | | |
| | (0.0728) | | | |
| Proportion Democrat | | 0.782* | | |
| | | (0.465) | | |
| At Least One Democrat | | | 0.977** | |
| | | | (0.478) | |
| Majority Democrat | | | | 0.403 |
| | | | | (0.342) |
| $R^2$ | 0.308 | 0.297 | 0.280 | 0.250 |

*Notes*: 936 electoral contest-year observations. Standard errors clustered by board. * and ** indicates statistical significance at the 10% and 5% levels, respectively. Control variables in columns include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, year fixed effects, year of cycle intercepts, and school district fixed effects. Reduced form and first stages use a quadratic global polynomial in vote margin.

The results of our boundary analysis are presented in Table 12. The first column reports reduced-form evidence of the effect of vote margin threshold crossing on attendance zone manipulation, while successive columns present two-stage least squares estimates for each treatment. Column (1) reveals that the marginal election of a Democrat (relative to non-Democrat) board member causes a 0.19 standard deviation increase in the magnitude of attendance zone shifts experienced by students in a district for each year of the board's term. In keeping with this result, we find meaningful treatment effects on attendance zone manipulation, where all point estimates are positive and all but majority Democrat are significant. Given the main results in Table 7, the positive effects are consistent with non-Democrats tending toward inaction (relative to Democrats, who prioritize racial inequality), potentially allowing residential sorting to increase segregation without

substantial intervention. In addition, based on a comparatively lower point estimate, the local average treatment effect for majority Democrat is suggestive that non-Democrats may be more active with respect to boundary changes along that dimension.

### 5.2 Household Response

Given causal evidence of Democrats reducing segregation across schools and manipulating attendance boundaries to do so (relative to non-Democrats), we now consider the potential for offsetting effects through household re-sorting. Two outcomes are of particular interest. First, households may move within their district to select an alternative school attendance zone. Second, households may leave the district or public school system altogether, which we measure using the share of students attending public schools in the district who are white.

Table 13: Effects on Proportion of Within-District Moves

| | (1) RF | (2) 2SLS | (3) 2SLS | (4) 2SLS |
|---|---|---|---|---|
| Margin $> 0$ | 0.00414 | | | |
| | (0.0106) | | | |
| Proportion Democrat | | 0.0577 | | |
| | | (0.0542) | | |
| At Least One Democrat | | | 0.0368 | |
| | | | (0.0521) | |
| Majority Democrat | | | | 0.0362 |
| | | | | (0.0377) |
| $R^2$ | 0.700 | 0.701 | 0.699 | 0.712 |

*Notes*: 965 electoral contest-year observations. Control variables in columns include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, year fixed effects, year of cycle intercepts, and school district fixed effects. Reduced form and first stages use quadratic global polynomial in vote margin.

With respect to the first outcome, Table 13 reports reduced-form and treatment effects for year-to-year within-district changes in the block group of a student's residence.[30] All of the point

[30]We use student address records to calculate the share of students who move block groups within the district (year-over-year) for each year and district. Selected descriptive statistics of this variable are presented in Appendix Table A.2. The specifications we estimate are the same as for attendance zone shifts.

estimates are positive, which is suggestive of a response to counteract Democrat board actions. However, none of the effects are statistically significant. While we may simply not possess enough power to detect significant effects, it is also possible that within-district effects would be limited in the short term, given uncertainty about future attendance zone boundary changes by an activist board elsewhere in the district. While such uncertainty may affect moving decisions within districts, this should not be the case for moves across them or to private or charter schools.

Table 14: Effects on District Share White

| | (1) RF | (2) 2SLS | (3) 2SLS | (4) 2SLS |
|---|---|---|---|---|
| Margin > 0 | 0.0022 | | | |
| | (0.0183) | | | |
| Proportion Democrat | | -0.0718* | | |
| | | (0.0366) | | |
| At Least One Democrat | | | -0.0584 | |
| | | | (0.0439) | |
| Majority Democrat | | | | -0.0647** |
| | | | | (0.0306) |
| $R^2$ | 0.893 | 0.899 | 0.887 | 0.884 |

*Notes*: 470 electoral contests. Standard errors clustered by board. * and ** indicates statistical significance at the 10% and 5% levels, respectively. Control variables in include the number of electoral contest winners and candidates and their interaction, whether the contest is unopposed, the proportion of candidates in the contest unmatched, the proportions of board members not involved in an election who are black and who are Democrats, the proportion of board members unmatched, a quadratic in the number of elementary schools in the district, length of electoral cycle, year fixed effects, and pre-treatment proportions of black and economically disadvantaged students. Reduced form and first stages use a quadratic global polynomial in vote margin.

Table 14 examines the causal effect of Democrat board members on net moves out of the district or public system, by focusing on the district share of white students. We find negative treatment effects, which are significant for the proportion of Democrat members on a board and boards with a majority of Democrat members.[31] We interpret these results as evidence of "white flight" in response to segregation reduction efforts by boards. The estimate in column (2) indicates that a 14 percentage point increase in the proportion of Democrats on a school board (which is equivalent to one new Democrat member for the average board) causes a 1 percentage point reduction in a

[31]The reduced-form effect is essentially zero and not significant. We speculate that this is due to much greater noise in the reduced-form regression, which the two-stage least squares procedure filters out.

district's proportion of white students. The treatment effect of switching to a board with a majority of Democrats, reported in column (4), is about 6 percentage points in the white share (the point estimate for there being at least one Democrat on the board is about the same in magnitude, but imprecise). Thus, "white flight" out of the district or public system appears to be driven primarily by a change in the political majority of a board.[32]

# 6 Conclusion

Very little existing research has examined the actions and influence of local school boards, under whose purview the drawing of attendance zone boundaries has increasingly fallen. Moreover, analysis of school boards objectives in the aggregate is scarce to nonexistent. This is the case despite substantial and compelling evidence documenting numerous links between peers, sorting, and education outcomes, hinting at the important role that school boards might play in the production of student achievement.

This paper has addressed this gap by assembling a unique dataset that matches school board election candidates from 2008 to 2013 with the North Carolina voter registration database to examine the causal effects of school board decisions on student segregation. As the composition of a school board is likely correlated with unobserved transportation costs and political constraints, we developed and implemented a regression discontinuity design at the electoral contest level to exploit quasi-random variation arising from narrowly-decided elections.

We focused on the political composition of the school board in our analysis, measuring segregation in each school district according to racial and economic dissimilarity indices across schools. The results indicate that (relative to their non-Democrat counterparts) Democrat board members decrease racial segregation across schools: an electoral victory that shifts the board to have a majority of Democrats causes a reduction in the black dissimilarity index across schools of 12 percentage points, while the election of even a single Democrat in the minority leads to a reduction of 18 percentage points.

[32]To interpret the large magnitude of this effect (the average white share for a district is 0.56), note that white share is determined by the sorting behavior of white and non-white students, and our causal estimates compare Democrat treatments to non-Democrat counterfactuals. While we label the effect "white flight," it may be driven not only by net movement of white students out of the public school district controlled by Democrats, but also by net movement of three additional groups: (1) non-white students moving into the Democrat district; (2) white students moving into non-Democrat districts; and (3) non-white students moving out of non-Democrat districts.

To establish a key mechanism underlying these effects, we then used student addresses to construct a novel measure of attendance zone shifts. Our results show that such shifts increase by 0.19 standard deviations when an additional Democrat (relative to non-Democrat) is elected, consistent with them reversing the effects of neighborhood sorting. These results establish that school boards causally influence how students (and thus peers) are allocated across schools.

Finally, we investigated whether board efforts to reduce segregation cause households to re-sort in response. While we detected no differential within-district neighborhood re-sorting in the short run, we found compelling evidence of "white flight" out of the district or public school system soon after boards act. This highlights a salient constraint facing school boards interested in implementing policies aimed at reducing segregation across public schools. Indeed, it is possible that actions taken to reduce within-district public school segregation could have the perverse effect of increasing segregation across districts and between public, charter and private systems. Taken together, our findings underscore the central role that school boards play in allocating students to schools, which is likely to have important ramifications for the production of education and social inequality in general. We plan to explore these themes further in future work.

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# Appendix Tables

Table A.1: Descriptive Statistics by Majority Democrat

| | Majority | |
|---|---|---|
| | Non-Dem | Dem |
| % Black | 0.12 | 0.35 |
| % Economic Disadvantaged | 0.56 | 0.69 |
| Residential Segregation – Black | 0.48 | 0.44 |
| Residential Segregation – Economic Disadvantaged | 0.33 | 0.34 |
| School Segregation – Black | 0.36 | 0.27 |
| School Segregation – Economic Disadvantaged | 0.24 | 0.21 |

*Notes*: 251 school boards (unique by district-election year). Residential segregation missing for one board.

Table A.2: Attendance Zone Shifts and Within-District Moves by Majority Democrat

| | | Majority | |
|---|---|---|---|
| | | Non-Dem | Dem |
| Attendance Zone Shift | Mean | 0.00 | -0.02 |
| | SD | 0.69 | 0.47 |
| Within-District Moves | Mean | 0.15 | 0.17 |
| | SD | 0.09 | 0.10 |

*Notes*: 936 and 965 electoral contest-year observations for Attendance Zone Shift and Within-District Moves, respectively.