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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. 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 with time-varying district-level racial and economic segregation outcomes. Focusing on the political identity of school board members, regression discontinuity estimates reveal that (relative to their non-Democratic counterparts) Democratic board members decrease racial segregation across schools. Our findings suggest that school boards realize such reductions in segregation by shifting attendance zones (which we infer without the need for exact geocoded boundaries) and that white families differentially exit the traditional public school system for local charter schools in response.

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 risen 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 et al., 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

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[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 et al. (2009), who contend that school choice in Durham gives rise to more highly segregated schools than would occur from simple proximity-based rules. Hastings et al. (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).

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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 with 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 causal 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 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. From the North Carolina State Board of Elections (henceforth 'NCSBE'), we obtain a list of candidates for each electoral contest held in North Carolina from 2008 to 2012 inclusive, along with the total number of votes each received. We merge these records with a list of school board members for 109 districts in North Carolina, allowing us to identify election winners and losers. We then link the election candidates and board members to NCSBE voter registration records, which contain information such as the 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 74% of school board candidates in our sample. Finally, we 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 identity of school board members in our analysis. The results indicate that (relative to their non-Democratic counterparts) Democratic board members decrease racial segregation across schools: the estimated causal effect of an electoral win by a Democrat is an 8 percentage point reduction in the black dissimilarity index across schools at the time of the subsequent school board election. Contrasting this estimate with its ordinary least squares counterpart, we find that the latter methodology understates the causal effect, highlighting the bias inherent in more naive approaches.

To establish the main mechanism underlying this effect, 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 occur more frequently following the election of an additional Democrat (relative to non-Democrat), which is in line with them counteracting the effects of neighborhood sorting. Based on this evidence, we consider whether board-induced boundary adjustments lead households to choose a different school or district by moving or opting out of the traditional public school system for private or charter schools. Although we find no short-run evidence of re-sorting or other household responses overall, the findings suggest that board actions to lower segregation cause white families to differentially leave traditional public schools for charter schools in districts where this option is available.

Our paper is the first to identify the key role that school boards play in influencing student segregation. This is relevant to several strands of literature. The first one seeks to estimate the contribution of schooling inputs to the production of student achievement, focusing primarily on the school and teacher, rather than district, levels (Rivkin et al., 2005; Chetty et al., 2014). Our results, along with related prior research about school board activity (Billings et al., 2014; Hoxby and Weingarth, 2005), suggest that decisions made at the district level by school boards 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 et al., 2007). Our work complements these demand-side analyses by providing supply-side insight into how boundaries are drawn, with boards actively altering them according to heterogeneous preferences over student segregation. This serves as an initial step in reaching a broader general equilibrium understanding of how the peer composition within schools is determined.

In addition, our paper connects with the literature on school choice mechanisms (Abdulkadiroğlu and Sönmez, 2003; Kapor et al., 2017). While analyses typically evaluate the allocation of students to schools primarily in terms of household preferences, we examine the role that elected school board member preferences play in the matching of students to schools. In this regard, our consideration of shifting school attendance zones as a key mechanism for affecting student-school matches relates to work examining the location and shape of such zones (Saporito and Riper, 2016; Monarrez, 2017).

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 et al., 2009; Johnson, 2011). Often constrained by considerable residential segregation, this requirement was frequently achieved through reassignments and involuntary busing. With the end of court desegregation orders, household sorting has contributed to recent increases in school segregation (Reardon and Yun, 2002; Clotfelter et al., 2008; Lutz, 2011) and school board priorities have become increasingly political.[3]

[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).

Nevertheless, despite the influence of school boards over local education policy, little evidence exists regarding the characteristics, activities or objectives of school boards.[4] Moreover, due to a dearth of applicable data, the role of school boards in education production has remained mostly 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 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 construct our sample of North Carolina school boards from three data sources: (1) publicly-available school board election results; (2) a new panel of school boards; and (3) voter registration records. Our sample construction begins with the election records (obtained from the NCSBE), which report the name and vote tallies for all candidates by electoral contest for the years 2008–2012. In all but three of the 115 traditional public school districts in North Carolina, school board members are elected by voters in regular elections.[6] School board elections are typically non-partisan and contests are staggered, such that only a subset of the seats on a board are contested in an election year.

We link the election results with an annual panel of school boards. We assembled the panel by contacting each North Carolina school district to obtain their historical records, yielding the names of school board members by year for 109 school districts.[7] This link between the election records and our board panel is important because the election results themselves do not identify which candidates won the contest, only the votes received. The school board panel thus allows us to identify the winners and losers of each contest. The panel also provides the names of those members of a school board who are not participants in an election for a given year.

To determine key characteristics of school board members and candidates, we merge this linked sample with the North Carolina voter registration database (obtained from the NCSBE). The voter data contain the full name, voter identification number, address, age, political party, race and ethnicity, and voter history (among other variables) of all registered voters. To run for office, candidates are required to register in their district's county. As a result, we link candidates and board members with their voter registration record through a two-step procedure: We first identify a set of potential matches for each candidate using a within-county fuzzy match based on their name. We then manually rule out false matches by cross-referencing remaining covariates with information from online sources, such as current and archived school board websites and media articles. If there is unanimous agreement across the remaining potential matches in terms of a characteristic, we assign that characteristic to the entry. Based on these two steps, approximately 74% of the entries in the linked election records are matched with the North Carolina voter registration database.

Table 1
Alamance-Burlington school board election in 2008.

Name	Votes	Winner	Party
Steve A. Van Pelt	26,093	1	Dem
Patsy Simpson	24,217	1	Dem
Jackie S. Cole	21,504	1	Non-Dem
Mary Frye Erwin	21,305	1	Dem
Dee Ann Mahan Cobb	18,573	0	Non-Dem
Ernestine Lewis	17,226	0	Non-Dem
Heather Sorell	11,742	0	Non-Dem
Don Williamson	10,594	0	Missing
Lyle G. Payne	9468	0	Non-Dem
John Riffe	8163	0	Non-Dem
Write-ins	456	0	.

In our analysis, we focus on the political identity of school board candidates who are narrowly elected. There are two reasons for doing so. First, determining how school boards affect student segregation across schools requires observing a characteristic of school board candidates that predicts preferences over such segregation. The recent controversy over school board elections and school assignment plans in several districts (for example, the Wake County school district in North Carolina, as documented in Parcel and Taylor, 2015) suggests that partisan division would satisfy this criterion. The second reason is that our regression discontinuity design requires a characteristic that generates a sufficient number of "treatment" and counterfactual cases across elections. While the race or ethnicity of candidates is also likely to predict preferences regarding segregation, we observe many fewer contests in which a marginal difference in vote margin would switch the race or ethnicity of the winning candidate.[8]

The construction of the sample we use for our analysis is best illustrated by way of example. Table 1 presents the election results for the 2008 election in the Alamance-Burlington school district. In that instance, four candidates won seats on the school board in an 'at-large' contest that included ten total named candidates in addition to write-ins. To establish this fact, we obtain the candidate names and vote counts from the NCSBE election results and ascertain which candidates won by comparing the names with a list of school board members for Alamance-Burlington in 2009 (the year following the election), using our manually-constructed board panel. We then match each candidate to the voter registration file to determine his or her party affiliation. As shown in the table, we successfully match all but one candidate (Don Williamson) for this particular school board election.

Table 2 reports characteristics of the election candidates who are matched with the voter registration database in our sample. It reveals that 52% (569 of 1094) are registered as Democrats, who are more likely to be female and substantially more likely to be black, relative to their non-Democratic counterparts. It is also worth noting that three-quarters of the matched candidates who are not registered as Democrats are registered as Republicans, with the balance identifying as unaffiliated or being registered with a third party.

To calculate the running variable for 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 vote percentages for the pair and assign a positive (negative) sign if the winning candidate in the pair

[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, have a higher income, be more white and be more conservative than the average resident of their district (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 et al., 1986). These analyses, however, are unable to credibly rule out omitted sources of bias.

[6] Unfortunately, the State Board of Elections does not have records for school board elections prior to 2008.

[7] In total, we obtained over 5700 unique board member names between the years 2000 and 2014, though some districts were only able to furnish an incomplete panel of board members. The link with the assembled panel covers approximately 88% of the school board election contests in North Carolina between 2008 and 2012.

[8] For instance, only 29 black (as opposed to non-black) school board candidates are elected in contests within a vote margin of 30 points, as compared with 82 Democratic candidates. In the Appendix, we present results that examine the effects of an additional black school board member for comparison purposes.

Table 2
Election candidate characteristics.

	Non-Democrat	Democrat
Female	0.36	0.43
Black	0.03	0.41
Republican	0.75	0
N	525	569

Notes: 1094 election candidates.

Table 3
Electoral contest characteristics.

	Mean	SD	Min	Max
Election year	2010.1	1.6	2008	2012
Cycle length	2.05	0.32	2	4
# winners	1.54	0.98	1	5
# candidates	2.91	2.39	1	17
Vote margin	20.74	62.82	−100	100

Notes: 482 electoral contests.

is a Democrat (non-Democrat). The margin thus reflects the distance, in percentage points of the total vote, from switching the political identity of the marginal winner of a contest. With reference to the example presented in Table 1, the vote margin is calculated as the difference in the vote share for Mary Frye Erwin (the marginal winner of the contest) and Dee Ann Mahan Cobb (the marginal loser of the contest): 1.61 percentage points. The process of constructing these pairs for each electoral contest is designed to ignore and skip over the most popular loser if her political affiliation is missing (unmatched) or she and the least popular winner share the same political affiliation.[9] We obtain this running variable for 482 electoral contests (matched also to district variables, described in the next subsection) across 105 of the school districts in North Carolina.[10]

Table 3 summarizes the electoral contest characteristics in our sample. Our records span the 2008–2012 election years, with most contests being held in even-numbered years. On average, there are slightly fewer than two electoral contests per district in an election year.[11] For the vast majority of North Carolina districts, elections occur every two years (which we refer to as the cycle length). As previously mentioned, some electoral contests contain multiple winners, with an average of 1.54 per contest. The average electoral contest also contains 2.91 (non-write-in) candidates. The Democratic vote margin variable that we create spans −100 to 100 with an average of about 21 percentage points over the 482 electoral contests.

2.2. School district characteristics and outcomes

To connect school board elections 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.[12] Our focus on elementary schools is motivated by them serving smaller attendance zones (compared to middle or high schools) and transportation costs being more salient for younger children. Both factors increase the scope for boards to influence school segregation through attendance zone adjustments.

We measure segregation for each district-year combination using dissimilarity indices that exploit student information provided by the NCERDC. For characteristic z (e.g., economic disadvantage), the dissimilarity index for a given district in a given year is computed using

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

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 encrypted geocoded addresses of students (also obtained from the NCERDC) to compute dissimilarity indices over Census block groups (as opposed to schools).[13] Beyond its frequent use in the literature, the dissimilarity index has attractive properties for quantifying segregation. Bounded between 0 and 1, the value of the index can be interpreted as the share of characteristic z students that would need to be reallocated in order to equalize their share across the k units.

We compute dissimilarity indices by year and district for black and economically disadvantaged students. Focusing on these dimensions allows us to examine whether the political composition of a school board causally influences student segregation across schools under its purview, revealing information about the objectives of school boards from their behavior. For instance, school boards of different political compositions may prioritize addressing different dimensions (racial or economic) of segregation.[14] It may also be the case that particular school board member types choose not to offset household sorting, revealing a limited preference for equality in general (relative to the costs imposed by economic or political constraints, or to other objectives).[15]

We match the panel of district variables with the election results to construct 'school boards,' each of which is defined as a district-election year pair. Each board is then summarized by (1) district characteristics measured in the year of the election (which are predetermined), and (2) district variables measured in the year of the *next* school board election in the district, which are outcome variables influenced by the board. For our example case, the school board is 2008 Alamance-Burlington, which is matched to district characteristics for the 2007–2008 school year and, because the district holds elections every two years, the 2009–2010 school year. 2010 Alamance-Burlington is then a distinct school board that, although sharing some common members, succeeds the 2008 board. Due to

[9] For example, if Dee Ann Mahan Cobb were unmatched or registered Democratic in our example, we would instead calculate the running variable using Ernestine Lewis, the next most popular loser. We treat the mass of write-in candidates in each contest as an opposite-type loser by construction. The running variable is undefined if there are no winners for which we are able to match information from the voter data.

[10] The contests that comprise our final sample represent about 73% of the electoral contests linked to our board panel and 65% of all school board contests obtained from the NCSBE between 2008 and 2012.

[11] Across 257 district-election year combinations, there are a total of 482 electoral contests for which we are able to construct the running variable.

[12] Economic disadvantage is defined as meeting the federal income eligibility guidelines for receiving a free or reduced-price lunch, which are that a student's household income must not exceed 130% (free) or 185% (reduced-price) of the federal poverty threshold.

[13] To maintain student anonymity, the geographical resolution of residential addresses is at the block group level. A Census block group contains between 600 and 3000 people and is typically assumed in the literature to represent a local neighborhood. The average school district in North Carolina includes approximately 46 block groups.

[14] 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.

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

multiple simultaneous electoral contests in a number of districts (e.g., some districts elect members by ward or sub-district), our final sample contains 257 school boards across the 105 districts. These school boards represent 83% of all school boards in North Carolina districts for which we obtained election results between 2008 and 2012.

Table 4 summarizes characteristics and outcomes for the school boards in our sample. The statistics reported correspond to the 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% black and in which 63% 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% 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.

Table 4 also summarizes our primary outcome variables of interest, which are racial and economic segregation across schools. Districts exhibit a significant degree of school segregation along racial lines on average: to equalize the within-district share of black students across schools, the average district would need to reassign 30% of black students to new schools. There is also a high degree of heterogeneity in the segregation measures: some school boards preside over highly integrated districts, while other districts are acutely segregated. In terms of economic school dissimilarity, the average district would need to reassign 22% of economically disadvantaged students to equalize their share across schools, with similarly large variability across districts.

As an alternative way to understand the variation in our data, Fig. 1 depicts the geographic distribution of selected characteristics across districts. Geographic variation is substantial, whether according to student proportion by race or affluence, or residential segregation along either of those two dimensions. Thus, there is likely to be nontrivial overlap between districts in North Carolina and those throughout the United States, lending some degree of external validity to our analysis of school board composition on school segregation.

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) measured at the end of an election cycle (indicated by τ), T_j is the treatment status of the school board, and W_{j0} is a vector of observed covariates that includes the district characteristics measured at the beginning of the election cycle.[16] As discussed earlier, we consider treatment status based on the political composition of the school board: the effect of an additional Democratic school board member.

Table 4
District characteristics and outcomes.

	Mean	SD	Min	Max
% Black	0.26	0.22	0	0.95
% Economic disadvantaged	0.63	0.14	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	0.30	0.16	0	0.75
School segregation – economic disadvantaged	0.22	0.13	0	0.58

Notes: 257 school boards (unique by district-election year). Residential segregation measures are missing for one board.

Obtaining credible causal estimates of the primary parameter of interest (γ) depends on addressing probable sources of endogeneity. In particular, estimating Eq. (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 ($cov(Q_j, T_j) \neq 0$). Problematic candidates for Q_j include variables related to voter preferences, neighborhood sorting and geographic concentration, any of which may be correlated with both board composition and segregation across schools.

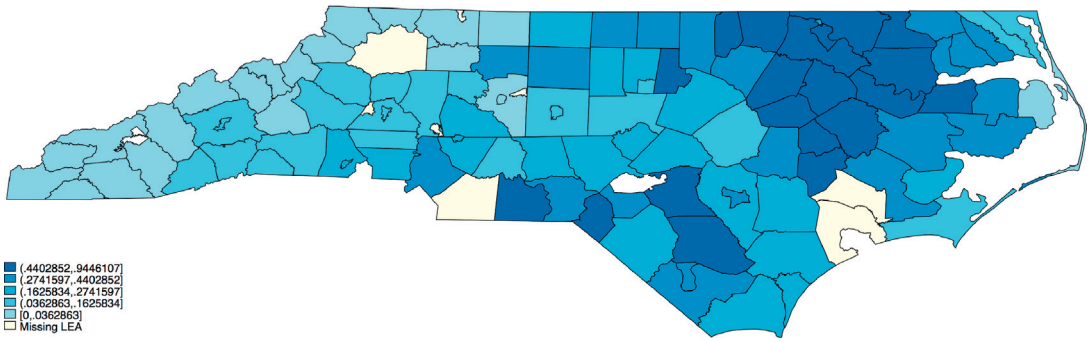
The direction of bias implied by the omission of such endogenous variables will clearly be determined by the sign of the associated correlations. For example, if people who reside in less segregated neighborhoods politically align more closely with Democrats, then omitting a measure of neighborhood segregation from the regression would lead to downward-biased estimates (assuming that neighborhood segregation is correlated with segregation across schools). While directly controlling for residential dissimilarity (i.e., by including it in W_{j0}) might address this particular case of bias, several endogenous variables are likely to be unobserved which, by definition, cannot be controlled for within a standard regression framework. For instance, it could instead be the case that Democrats are more electable in cities, where denser populations are more geographically concentrated by type (e.g., race or economic disadvantage), making it difficult to implement desegregationist policies (relative to suburban or rural settings) and leading to an estimate of γ that is biased upward.

The extent to which segregation reduction efforts are constrained would depend on 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 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 two key factors, assuming that infrastructure and organizational constraints imply an optimal school size well below total district enrollment, resulting in multiple schools per district.[18] The first factor is that transporting students to schools is costly, according to the distance between a student's residence and her assigned school.

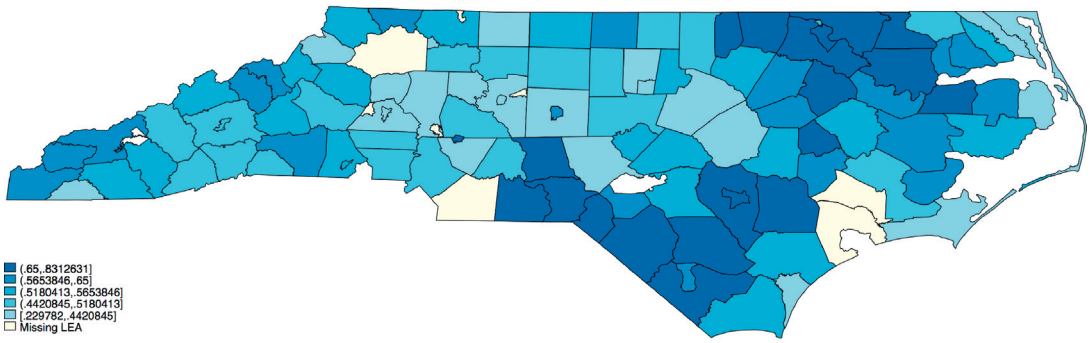
[16] To be concrete about the subscripts, in the context of the 2008 election in Alamance-Burlington, j represents the 2008-Alamance-Burlington combination, and τ and 0 correspond to the 2009–2010 and 2007–2008 school years, respectively.

[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.

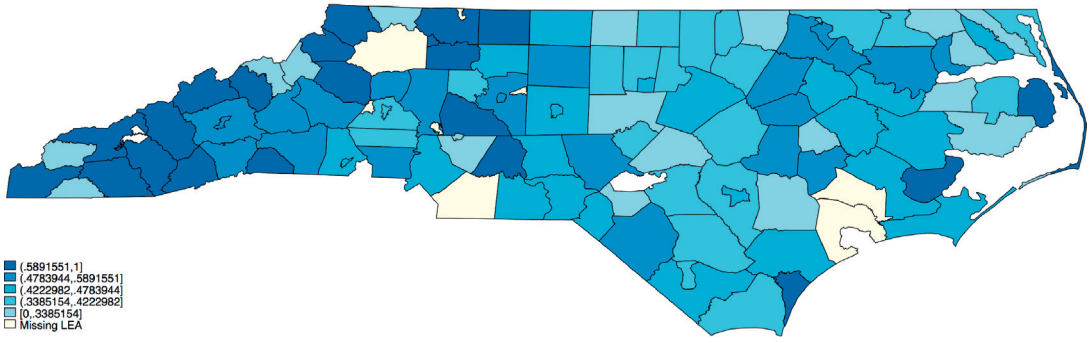
[18] An obvious example of an infrastructure constraint is building size, while an example of an organizational constraint is the potential deleterious effects of having to manage too many classrooms within a school.



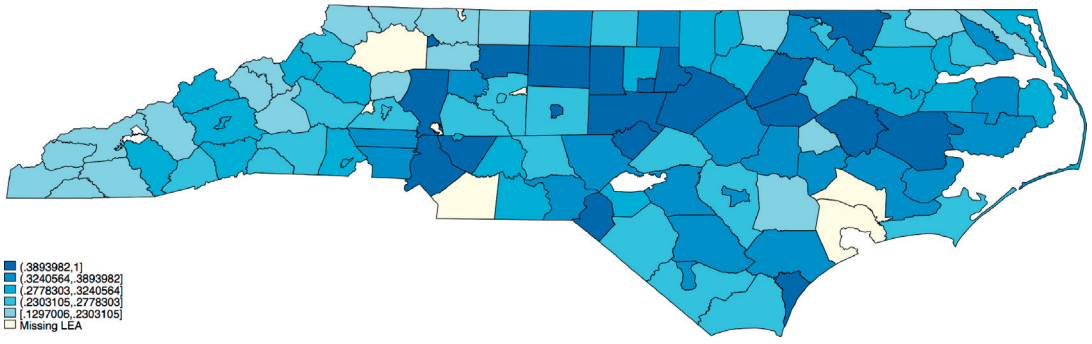
(a) Percent black



(b) Percent economic disadvantaged



(c) Residential segregation - black



(d) Residential segregation - economic disadvantaged

Fig. 1. Geographical variation – districts characteristics. *Notes*: The figures show the geographic distribution of selected district characteristics.

The second factor 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 adjustment 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 Eq. (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: $cov(Q_j, \epsilon_j) > 0$. Upward bias then follows if $cov(Q_j, T_j) > 0$, as in the example of concentrated cities.

Our solution to the problem of unobserved endogenous variables is to implement 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 Democratic vote margin, we identify the political identity (Democratic/Non-Democratic) 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, which is to estimate the following specification:

$$Y_{j\tau} = \pi + \theta D_{ij} + f(x_{ij}, D_{ij}) + \kappa Z_{ij0} + \nu_{ij}\,, \tag{2}$$

where $Y_{j\tau}$ is the segregation outcome (as before), Z_{ij0} includes observed district and electoral contest characteristics, and x_{ij} is the running variable. Estimation is carried out via local linear regression, using an optimal bandwidth given by Calonico et al. (2014), and we cluster standard errors at the board (j) level.

The causal parameter of interest is θ. The resulting estimate $\hat{\theta}$ should be free of the types of bias discussed above, identifying the effect of an additional Democratic board member on the degree of segregation across schools. We can compare it to a naive estimate of γ in Eq. (1) in order to assess the direction and magnitude of bias. While our empirical strategy does not require observable characteristics Z_{ij0}, as such controls should not be necessary if the validity assumption holds, we also estimate specifications with controls as including them has the potential to reduce the variance of our estimates.[21] We assess validity by separately replacing the dependent variable $Y_{j\tau}$ in Eq. (2) with Z_{ij0} and Y_{j0}, and alternatively estimating the local linear regression for counterfactual placebo thresholds, where no effects should exist.

In addition to our main analysis, we are able to adapt our procedure to analyze the mechanisms behind any detected changes in segregation. In particular, we alternatively replace the dependent variable in Eq. (2) with a measure of attendance zone boundary changes (detailed in Section 5), as well as various measures of subsequent residential sorting and differential selection between traditional public, private and charter schools. This allows us to causally examine a key channel through which school boards alter segregation and consider household responses that result.

Fig. 2 depicts the extent of narrowly-decided electoral contests in our data. In particular, the panels highlight the school districts in which at least one close election takes place in one or more of the election cycles. Panel (a) defines close elections as those in which the difference in vote share x_{ij} between the Democrat and non-Democrat is less than 5 percentage points, while the definition for panel (b) is that the difference is less than 10 percentage points.

These patterns are also summarized in Appendix Table A.1, which compares the subsamples of 'close' election school boards to the full sample in terms of characteristics and outcomes. The table reveals a few minor differences between boards that experience a narrowly-decided contest (which provide the identifying variation for the effects we estimate) and the larger sample of boards. For example, the average board with a vote share within the 5 percentage point bandwidth is 22% black (compared with 26% for the full sample) and has a racial school dissimilarity index of 0.32 (compared with 0.30 for the full sample). In conjunction with Fig. 1, it is apparent from Fig. 2 that there is substantial geographic variation in close elections, with districts represented that vary in the share and residential segregation of black and affluent students.

4. Results

The results of our regression discontinuity analysis are presented in three parts. In the first subsection, we provide evidence of treatment, both at the electoral contest and school board levels. In the second subsection, we determine the causal effect of an additional Democratic board member on racial and economic school segregation. We also interpret our estimates with respect to the prior literature and contrast them with analogous non-causal estimates using a simple ordinary least squares approach. In the third subsection, we assess the validity of our causal claims, testing for discontinuities in election and district covariates, pre-treatment variables, and post-treatment outcomes at counterfactual placebo cutoffs.

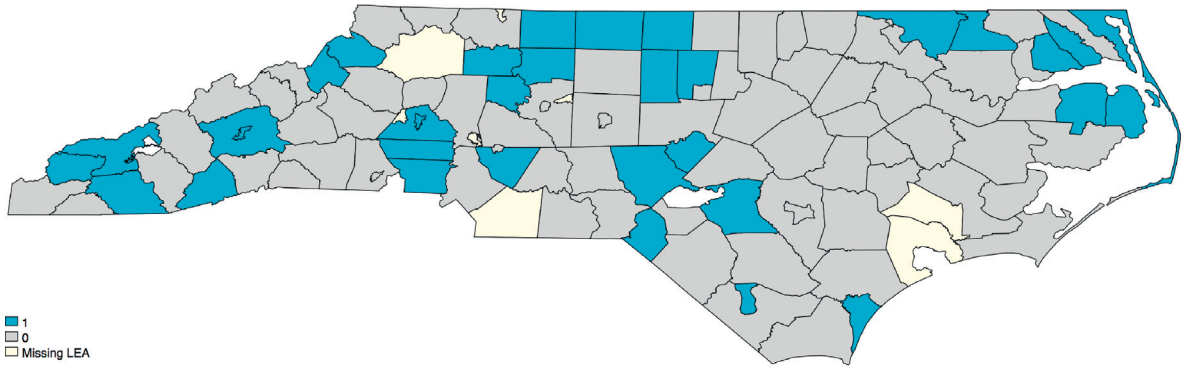
4.1. Evidence of treatment

Before presenting the main causal effects, it is important to establish that the vote share of candidates in election contests affects the outcome of those contests and the composition of school boards. Fig. 3 first reveals the discontinuous effect of a Democrat winning a contest on the proportion of Democratic 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 Fig. 3 shows. Panel (a) features a sharp

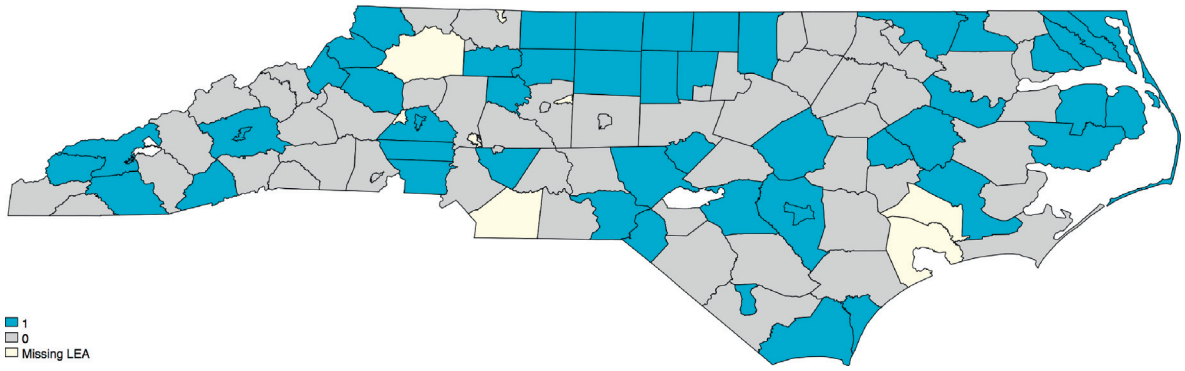
[19] For reasons discussed at the end of subsection 4.2, such bias is also unlikely to be addressed by the inclusion of fixed effects.

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

[21] In particular, pooling electoral contests as in Eq. (2) is likely to generate heteroskedasticity.



(a) $|x_{ij}| \leq 5$



(b) $|x_{ij}| \leq 10$

Fig. 2. Geographical variation – districts with close elections. *Notes*: The figures show which districts feature close electoral contests. Panel (a) defines close elections as those in which the difference in vote share x_{ij} between the Democrat and non-Democrat is less than 5 percentage points, while the definition for panel (b) is that the difference is less than 10 percentage points.

jump from 0 to 1 at the threshold, while panels (b) and (c) display discontinuities of approximately 50 and 40 percentage points in Democratic share, respectively.

These discontinuities in electoral outcomes translate into a discontinuity in the school board composition.[22] Panel (d) of Fig. 3 details a strong treatment on the proportion of Democratic school board members, suggesting a discontinuity of approximately 35 percentage points. This result establishes the relevance of our regression discontinuity design: a narrow election of an additional Democratic candidate has significant implications for the political composition of the seated school board.

In the Appendix, we further examine whether the narrow election of a Democrat has implications for the racial or gender composition of the school board. Fig. A.2 shows a small (but statistically insignificant) discontinuity in the share of board members who are black, while Fig. A.3 displays no discontinuity in the share of female board members. Notwithstanding these supplementary findings, for the purposes of interpreting estimates of θ in Eq. (2), we do not take a stand on the exact political channel of representation through which a narrowly-elected Democratic candidate influences outcomes.[23]

[22] The distinction between the electoral and board level being that the latter includes both members who are and are not (as their term has not expired) on the ballot of an electoral contest in a given year. So the board-level treatment depends on the composition of both sitting and newly-elected members.

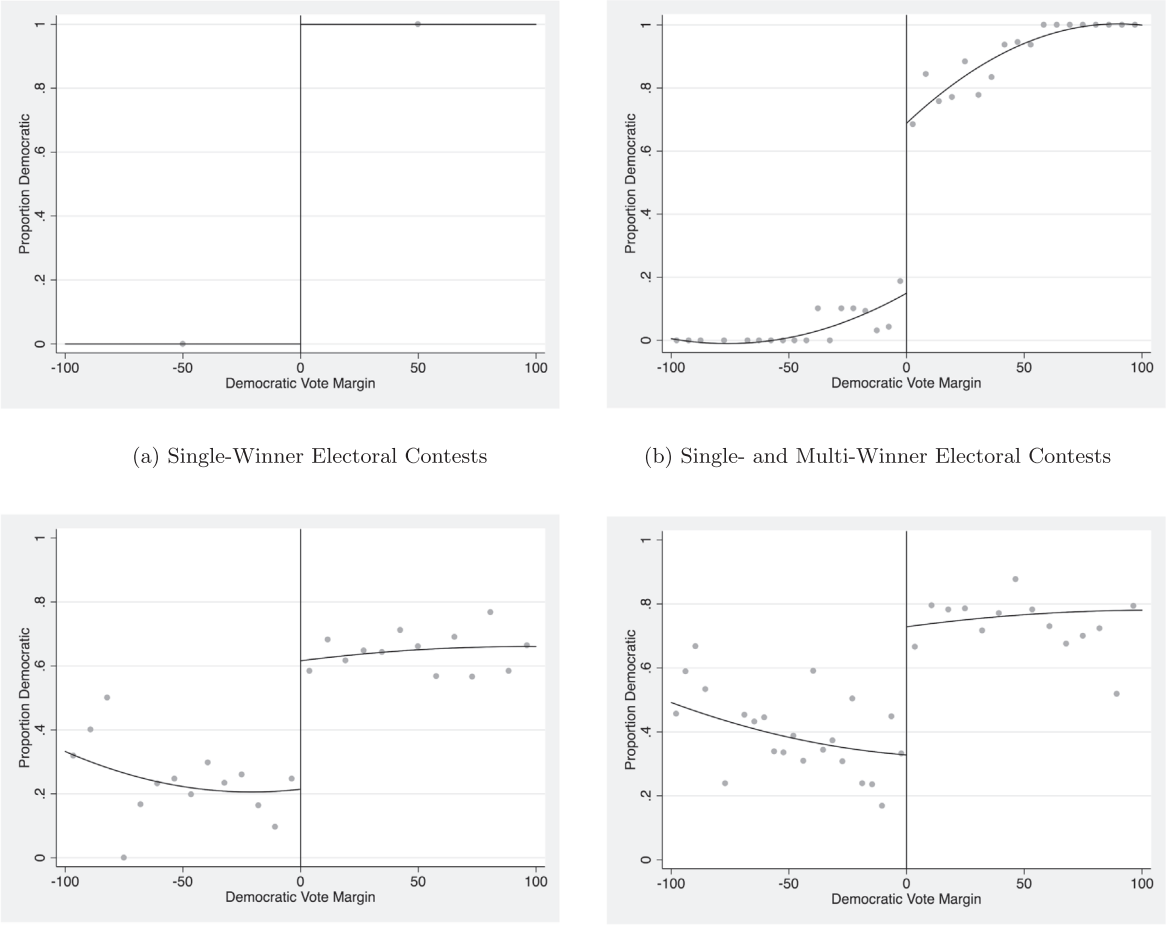
[23] In this regard, our approach is similar to Anwar et al. (2012), who examine the effect of a randomly-assigned black member to the jury pool on racial disparities in convictions.

4.2. Causal effects on school segregation

In Figs. 4 and 5, we provide visual evidence of discontinuities in racial and economic segregation, respectively. Based on data at the electoral contest level, panels (a) and (b) in Fig. 4 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. This is consistent with Democratic board members preferring to reduce racial segregation of students across schools more than their non-Democratic counterparts. Based on panels (a) and (b) in Fig. 5, 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 estimates of the causal impact of an additional Democratic board member on segregation across schools along racial and economic lines, respectively. In each case, the estimate from a local linear regression without controls is reported for three bandwidths. Column (1) uses the optimally-selected bandwidth from Calonico et al. (2014) (henceforth 'CCT'). Columns (2) and (3) then report the estimate for a bandwidth that is two-thirds and one-third of the optimal CCT value, respectively. Columns (4)–(6) report robust bias-corrected estimates. We include controls in column (5), while column (6) additionally restricts the sample to those districts which contain more than one elementary school.[24]

[24] This is motivated by the concern that, absent the opening of a new school, school segregation is zero by construction for districts with only one school.



(a) Single-Winner Electoral Contests

(b) Single- and Multi-Winner Electoral Contests

(c) Election Winners Across All Contests

(d) School Board Composition

Fig. 3. Evidence of treatment. *Notes*: Each figure is created by plotting the average of the proportion Democratic within each bin of the Democratic vote margin on either side of the margin threshold, and fitting the data using a quadratic polynomial without controls.

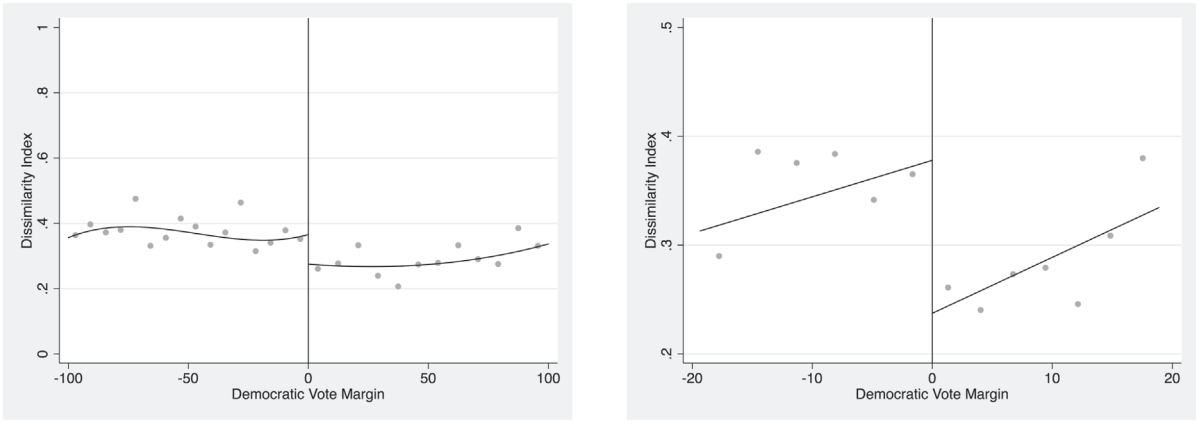
Confirming visual evidence from Figs. 4 and 5, we find large and statistically significant negative effects for segregation along racial lines, but small and statistically insignificant effects for segregation along economic lines. With respect to the effect on racial segregation across schools, the local average treatment effect in column (1) implies that an electoral win by a Democrat results in an approximately 8 percentage point reduction in the black dissimilarity index across schools within a district. The magnitude of the effect is essentially unchanged for the robust bias-corrected analogue (column (4)) of the conventional estimate. By way of comparison, the average proportion of black students who would need to be reallocated to equalize their within-district share across schools is 0.30. Columns (2) and (3) demonstrate that the effect is robust when narrowing the bandwidth around the cutoff, and columns (5) and (6) show that robust bias-corrected estimates remain negative and statistically significant when including controls and restricting the sample to districts with more than one elementary school.[25]

To place our results regarding racial segregation in context, Guryan (2004) and Reber (2005) find an approximate 25 percentage point long-run reduction in such dissimilarity following court-ordered desegregation, while Lutz (2011) and Reardon et al. (2012) find a 1 to 2 percentage point increase each year following the release from such court orders. When comparing it to the prior literature, it is important to highlight two key aspects of our estimated causal effect on racial dissimilarity. First, the 8 percentage point reduction represents a very short-run effect, typically occurring two years following an election. In the long run, much of it may be undone by household re-sorting. Second, our estimate reflects the effect of a Democratic member *relative* to the non-Democratic counterfactual. If non-Democrats are passive, allowing residential sorting to potentially increase segregation over time, then the identified partisan difference would be larger than the (unidentified) level effect for the Democratic member. This would also be true if non-Democrats take actions to increase segregation.[26]

To assess the extent to which a regression discontinuity design is necessary for identification, we present our local average treatment effect estimate alongside a naive ordinary least squares analogue in Table 7, both without and with district controls. In the absence

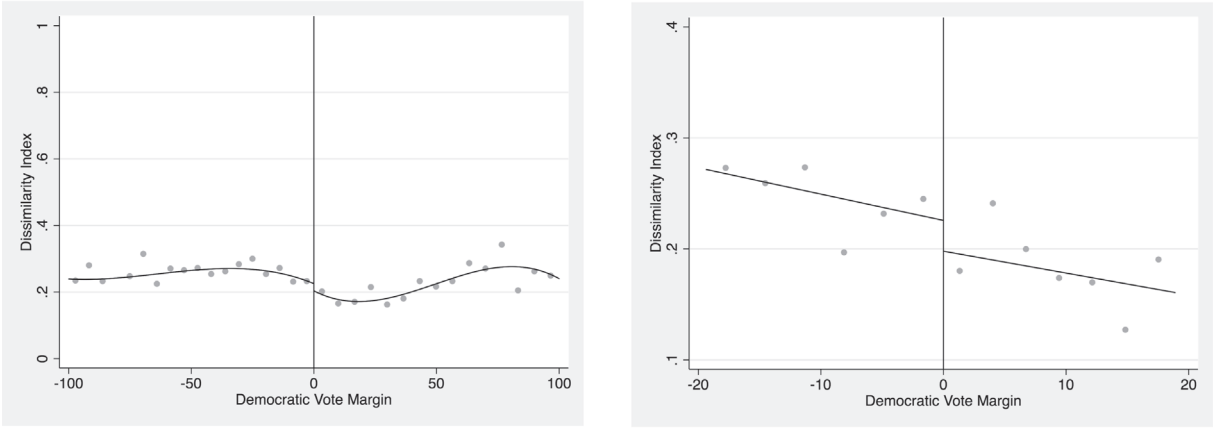
[25] Although we do not observe many narrow electoral discontinuities in the race or ethnicity of candidates, we nonetheless present results in the Appendix of the effect of an additional black school board member on school segregation for comparison. Table A.3 reveals no statistically significant evidence for effects on either racial or economic segregation across schools.

[26] Another point of reference for our results is a recent white paper that proposes attendance zones to reduce school segregation in Lenoir County, NC high schools (Joyner et al., 2016). The proposed zones, which are contiguous, would reduce the non-white dissimilarity index across schools in Lenoir by around 29 percentage points.



(a) Global Quadratic Fit (b) Local Linear Fit

Fig. 4. Causal effect of additional Democrat on racial school segregation. *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 bin of the Democratic 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 bin of the Democratic 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.



(a) Global Quadratic Fit (b) Local Linear Fit

Fig. 5. Causal effect of additional Democrat on economic school segregation. *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 bin of the Democratic 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 bin of the Democratic 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.

of controls, the effect of an additional Democratic board member under the OLS specification is a 2 percentage point reduction in racial school segregation. This is upward biased from the causal 8 percentage point local average treatment effect reduction, though we are not quite able to reject the null that these estimates are statistically indistinguishable, given a p-value of 15.3% using a cluster-robust Hausman test with 5000 bootstrap repetitions. It is notable that controlling for district characteristics (as one might attempt in the absence of a more sophisticated identification strategy) does not address the bias that arises under the OLS procedure. The upward bias remains and the RD and OLS estimates are now statistically distinguishable, with a p-value of 4.2%.[27]

It is worth noting that an OLS specification with district fixed effects is unlikely to address the endogeneity issue given our sample (as an alternative to the RD approach) for two reasons. The first reason is that fixed effects absorb most of the variation in school board composition, since we only observe (at most) three election years per school district. The second reason is that school board composition is measured with error (due to imperfect matching of school board members to the voter registration database), resulting in attenuation bias that is likely to be exacerbated by the inclusion of district fixed effects. Consistent with these issues, the point estimate with district fixed effects (available upon request) is attenuated toward zero and estimated very imprecisely relative to the OLS specification without them.

4.3. Validity

Our results show that electoral contests generate a discontinuity in the proportion of Democratic school board members which, in turn, causes a reduction in the degree of racial segregation across

[27] Defined in Table 7, we use only district controls for the comparison to reflect the characteristics available to the econometrician for estimating the simple OLS specification in the absence of electoral data. However, this subset restriction does not matter in practice, as the difference between RD and OLS estimates continues to be statistically distinguishable (p-value of 0.031) when using the full set of controls defined in Table 5.

Table 5
Causal effect of additional Democrat on racial school segregation.

	(1)	(2)	(3)	(4)	(5)	(6)
Vote margin > 0	−0.078*	−0.118**	−0.114*	−0.083*	−0.109**	−0.096**
	(0.042)	(0.051)	(0.066)	(0.049)	(0.045)	(0.045)
No. obs.	482	482	482	482	482	454
Robust bias-corrected	N	N	N	Y	Y	Y
Controls	N	N	N	N	Y	Y
Sample	Full	Full	Full	Full	Full	$N_{sch} > 1$
BW	36.54	24.36	12.18	36.54	28.50	21.64

Notes: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014). We explore robustness by computing estimates for two-thirds optimal bandwidth in column (2) and one-third optimal bandwidth in column (3). We report conventional estimates in columns (1)–(3) and robust bias-corrected estimates (according to Calonico et al., 2014) in columns (4)–(6). Controls in columns (5) and (6) include an indicator for whether the electoral contest features multiple contested seats, the total number of seats contested across all elections, the proportion of missing board members (which is dictated by our ability to match candidates to the database of registered voters), the proportion of board members not involved in an election who are black and who are Democrats (and the proportion of such Democrats who are missing), pre-treatment district enrollment, and the pre-treatment proportion of white, black and economically disadvantaged students. Standard errors are clustered by board (district-year).

* Indicates statistical significance at the 10% level.
** Indicates statistical significance at the 5% level.

Table 6
Causal effect of additional Democrat on economic school segregation.

	(1)	(2)	(3)	(4)	(5)	(6)
Vote margin > 0	−0.038	−0.029	−0.047	−0.027	−0.014	−0.001
	(0.035)	(0.044)	(0.058)	(0.041)	(0.029)	(0.031)
No. obs.	482	482	482	482	482	454
Robust bias-corrected	N	N	N	Y	Y	Y
Controls	N	N	N	N	Y	Y
Sample	Full	Full	Full	Full	Full	$N_{sch} > 1$
BW	31.72	21.14	10.57	31.72	27.98	23.19

Notes: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014). We explore robustness by computing estimates for two-thirds optimal bandwidth in column (2) and one-third optimal bandwidth in column (3). We report conventional estimates in columns (1)–(3) and robust bias-corrected estimates (according to Calonico et al., 2014) in columns (4)–(6). Controls are the same as those defined in Table 5. Standard errors are clustered by board (district-year).

schools within a treated district. The ability of our research design to produce causal estimates rests on the assumption that validity holds. While not directly testable for unobserved characteristics, we are able to provide three types of evidence based on observables to lend credence to our causal claim.

Our first type of evidence comes from examining whether any election or district covariates are discontinuous at the voting margin threshold. For example, there should be no discontinuity at the threshold in the composition of school board members who, due to staggered terms, are not involved in an election. Table 8 reports regression discontinuity estimates for seventeen covariates across two panels and none of the estimates are significant. Panel A reveals that a Democratic win is not associated with the number of winners of an electoral contest, the number of candidates, or the proportion of missing candidates (which is dictated by our ability to match candidates to the database of registered voters). Importantly, we also find no discontinuity in the partisan or racial makeup of board members not participating in an election (Prop. uncont. Dem, and Prop. uncont. Black). In panel B, we examine discontinuities in pre-treatment covariates, including the racial and economic composition of the district, district enrollment, the number of traditional public and charter schools in the district, and residential segregation along racial and economic lines. Across all estimates, we find no evidence of a discontinuity.[28]

The second type of evidence for validity entails testing whether any pre-treatment outcomes (prior to an elected school board's tenure) are discontinuous at the threshold. Table 9 reports regression

Table 7
Comparing causal RD to non-causal OLS – racial segregation.

	(1)	(2)
RD	−0.078*	−0.102**
	(0.043)	(0.045)
OLS	−0.020***	−0.015***
	(0.003)	(0.004)
$H_0 : \beta_{RD} = \beta_{OLS}$	0.153	0.042
Controls	N	Y

Notes: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014). District controls include the proportion of missing board members, pre-treatment district enrollment, and the pre-treatment proportion of white, black and economically disadvantaged students. Standard errors are clustered by board (district-year). Equality of coefficients is tested using a cluster-robust Hausman test with 5000 bootstrap repetitions.

* Indicates statistical significance at the 10% level.
** Indicates statistical significance at the 5% level.
*** Indicates statistical significance at the 1% level.

[28] Motivated by potential small sample bias in the dissimilarity index (Carrington and Troske, 1997; Gentzkow et al., 2016), we also check for a discontinuity in the average school size in a district. On the full sample, we find a discontinuity at the threshold of 53.2 students that is significant at the 5% level. Further investigation reveals that this result is driven by a handful of districts with only one elementary school for which the vote margin falls just to the right of the threshold. When we restrict the sample to districts with more than one elementary school, we find an insignificant coefficient of 26.8 with a standard error of 21.5, indicating any such bias is not discontinuous across the threshold.

Table 8
Validity – testing for discontinuities in covariates.

Panel A: Election and district covariates		
# winners	# candidates	Electoral contests
−0.178	−0.356	0.195
(0.328)	(0.901)	(0.381)
Year	# board members	Prop. board missing
−0.238	−0.309	−0.070
(0.450)	(0.434)	(0.059)
Prop. cand. missing	Prop. uncont. Dem	Prop. uncont. Black
0.015	0.081	0.052
(0.047)	(0.118)	(0.083)

Panel B: Pre-treatment covariates			
% White	% Black	% Econ. disadv.	Enrollment
−0.030	0.045	0.041	−544
(0.054)	(0.059)	(0.039)	(1002)
# schools	# charters	Racial res. seg.	Econ. res. seg.
−4.49	0.718	0.001	0.008
(3.18)	(0.794)	(0.047)	(0.028)

Notes: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014). Standard errors are clustered by board (district-year).

discontinuity estimates for period 0 (the school-year prior to the election) outcomes and the growth in segregation from two periods prior to period 0. The results argue against our findings being driven by trends in segregation. The estimate for the pre-treatment level of racial segregation in column (1) is both statistically indistinguishable from zero and smaller in magnitude than its post-treatment counterpart (reported in Table 5). The inclusion of election and district controls reinforces this finding: doing so attenuates the pre-treatment estimate toward zero (see column (2)), such that the difference between pre- and post-treatment estimates is significant. All estimates for the growth of segregation are also small and insignificant, further ruling out confounding pre-treatment trends.

Table 9
Validity – testing for discontinuities in pre-treatment outcomes.

	Racial sch. seg. (period 0)		Econ. sch. seg. (period 0)	
	(1)	(2)	(1)	(2)
Vote margin > 0	−0.066	−0.017	−0.048	−0.011
	(0.042)	(0.039)	(0.038)	(0.031)
BW	34.02	35.34	28.34	23.41
	Δ Racial sch. seg. (−2 to 0)		Δ Econ. sch. seg. (−2 to 0)	
	(1)	(2)	(1)	(2)
Vote margin > 0	0.013	0.020	−0.003	−0.010
	(0.026)	(0.036)	(0.014)	(0.017)
BW	27.60	26.94	24.83	25.66
No. obs.	482	454	482	454
Robust bias-corrected	N	Y	N	Y
Controls	N	Y	N	Y
Sample	Full	$N_{sch} > 1$	Full	$N_{sch} > 1$

Notes: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014), indicating the causal effect of an additional Democrat on different pre-treatment dependent variables of interest. We report conventional estimates without controls in column (1), and robust bias-corrected estimates (according to Calonico et al., 2014) with controls and the restriction that districts must have more than one school in column (2). Controls are the same as those defined in Table 5. Standard errors are clustered by board (district-year).

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. Appendix Fig. A.1 presents regression discontinuity estimates using placebo Democratic vote margin thresholds placed every 4 percentage points between −40 and 40 percentage points inclusive. For both racial and economic segregation across schools, there are no instances in which we can statistically reject that the resulting placebo discontinuity estimate is equal to zero.

5. Mechanisms

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

5.1. 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. Drawing attendance zones is a key responsibility of school boards, a motivation behind work that studies the location and shape of attendance zones (Saporito and Sohoni, 2006; Saporito and Riper, 2016; Monarrez, 2017).[29] Adjustments to attendance zone boundaries typically arise as a consequence of growth and sorting patterns within school districts, particularly in response to overcrowding or underutilization of particular schools (for instance, the opening or closing schools would entail a change in attendance zones). Attendance zone boundaries may also be adjusted for reasons directly related to school segregation; most notably in the case of ensuring compliance with court orders, or in service of other district objectives, such as implementing public school choice mechanisms. As the preferences of school board members likely influence whether and how such adjustments are made, we construct a new measure of boundary changes (detailed below) to examine whether school boards affect school segregation via attendance zone policy.

5.1.1. Obtaining a measure of attendance zone shifts

To analyze how boundaries change over time, it would be ideal to possess a reliable panel dataset of geocoded boundaries for all districts in North Carolina, but such dynamic information is difficult to procure.[30] Fortunately, information provided in the NCERDC dataset

[29] To the degree that other board policies and actions (such as the allocation of teachers and resources across schools or magnet tracks within particular schools) influence patterns of residential sorting, these could indirectly also affect school segregation.

[30] The School Attendance Boundary Survey, conducted by the National Center for Education Statistics (NCES), contains information on attendance zones for most school districts in the United States, but only for the 2013–2014 school year. 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.

contains the elements necessary to infer boundary shifts without observing the boundaries themselves. In particular, the data contain information about where a student 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. It 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 associated with 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:

$$\phi_{kjt} = \frac{1}{2} \sum_{s \in \mathcal{S}_j} \left| P_{ksjt} - P_{ksjt-1} \right| . \quad (3)$$

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), suggesting no change in the boundary. We then aggregate ϕ_{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, ϕ_{jt}, where the aggregation procedure is described in the next sub-subsection.

It is worth noting that the ϕ_{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.[31] 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 ϕ_{jt} (that is unrelated to school attendance zone shifts) may be introduced through year-to-year changes in cohort size. We thus use the variation in ϕ_{kjt} over time to normalize it within each school district to strengthen the signal contained within our measure.[32]

Table 10
Causal effect of additional Democrat on attendance zone shifts.

	Period 1 treatment	Period τ treatment	Period 0 falsification
Panel A: Full sample ($N = 482$)			
Vote margin > 0	0.263*	0.224*	0.028
	(0.156)	(0.133)	(0.105)
BW	17.87	29.63	29.54
Panel B: $N_{sch} > 1$ *subsample* ($N = 454$)			
Vote margin > 0	0.221	0.253*	−0.006
	(0.173)	(0.132)	(0.104)
BW	19.54	28.81	30.37

Notes: Regression discontinuity estimates are computed without controls using a local linear regression with optimal bandwidth given by Calonico et al. (2014), indicating the causal effect of an additional Democrat on attendance zone boundary shifts in different periods. All reported estimates are robust and bias-corrected (according to Calonico et al., 2014).

* Indicates statistical significance at the 10% level.

5.1.2. Causal effect 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 ϕ_{kjt}, obtaining ϕ_{jt} for years $t = 1$ and $t = \tau$ (as well as $t = 0$ to assess validity) of the school board's tenure. We then replace the dependent variable of Eq. (2) with ϕ_{jt} for a given t and determine the effect of an additional Democratic school board member on attendance zone shifts by estimating θ.

Before presenting estimates from our boundary shift analysis, it is worth briefly discussing how they are related to the main causal effect. Recall our finding that an additional Democrat causes a reduction in segregation across schools. 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 distinguish between these non-Democratic objectives, we view them as having a relative preference for greater segregation (in other words, the economic and political costs of segregation are lower for non-Democrats). As a result, we are able to use the effect of treatment on ϕ_{jt} to distinguish between two rival hypotheses about how they might accomplish this objective. 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 suggests which type of non-Democratic behavior prevails.

The results of our boundary analysis are presented in Table 10. We examine attendance zone effects for the first period post-treatment (period 1), the last period post-treatment (period τ), and – to assess validity – the pre-treatment period (period 0). Panel A reports the causal estimate of the effect of vote margin threshold crossing on attendance zone adjustment for the full sample. The period 1 and period τ estimates reveal that the narrow election of a Democratic (relative to non-Democratic) board member causes an increase in our measure of attendance zone shifts. The increase is between

[31] 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.

[32] We also drop block group panels from the sample for which fewer than 100 elementary school students' addresses are ever observed. This removes around 450 block groups (of over 5200) from the sample that account for less than 5% of all block group-year observations.

Table 11
Causal effect of additional Democrat on household response.

Panel A: All school districts

	Racial res. seg.	% White	Private share	Charter share
Vote margin > 0	−0.017	−0.023	−0.024	0.010
	(0.058)	(0.053)	(0.039)	(0.011)
No. obs.	481	482	482	482
Robust bias-corr.	N	N	N	N
Controls	N	N	N	N
Sample	Full	Full	Full	Full
BW	26.05	30.72	28.47	29.17

Panel B: School districts without charter schools

	% White		Private share		White private share	
	(1)	(2)	(1)	(2)	(1)	(2)
Vote margin > 0	−0.015	−0.003	−0.002	0.033	−0.003	0.086
	(0.100)	(0.008)	(0.053)	(0.041)	(0.126)	(0.073)
No. obs.	256	235	256	235	256	235
Robust bias-corr.	N	Y	N	Y	N	Y
Controls	N	Y	N	Y	N	Y
Sample	Full	$N_{sch} > 1$	Full	$N_{sch} > 1$	Full	$N_{sch} > 1$
BW	20.27	27.34	31.26	29.79	21.30	22.91

Panel C: School districts with at least one charter school

	% White		Charter share		White charter share	
	(1)	(2)	(1)	(2)	(1)	(2)
Vote margin > 0	−0.026	−0.023***	0.023	0.031**	0.036*	0.040**
	(0.087)	(0.007)	(0.020)	(0.016)	(0.021)	(0.016)
No. obs.	226	219	226	219	226	219
Robust bias-corr.	N	Y	N	Y	N	Y
Controls	N	Y	N	Y	N	Y
Sample	Full	$N_{sch} > 1$	Full	$N_{sch} > 1$	Full	$N_{sch} > 1$
BW	29.84	17.76	28.19	23.46	29.51	20.38

Notes: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014), indicating the causal effect of an additional Democrat on different household responses. We report conventional estimates without controls in column (1), and robust bias-corrected estimates (according to Calonico et al., 2014) with controls and the restriction that districts must have more than one school in column (2). Controls are the same as those defined in Table 5. Standard errors are clustered by county for the share outcomes and by board (district-year) for all other specifications.

* Indicates statistical significance at the 10% level.
** Indicates statistical significance at the 5% level.
*** Indicates statistical significance at the 1% level.

0.22 and 0.26 of a standard deviation in the magnitude of attendance zone shifts experienced by students in a district. Moreover, we find no evidence of a causal effect on pre-treatment attendance zone adjustments in period 0. Panel B reports analogous estimates for the subsample of districts with more than one elementary school. The point estimates are qualitatively similar, though the period 1 treatment is no longer significant at the 10% level. Given the main result in Table 5, the positive effect on attendance zone adjustment is consistent with non-Democrats tending toward inaction (if Democrats act to reduce racial inequality), potentially allowing residential sorting to increase segregation without substantial intervention.[33]

5.2. Household response

Given causal evidence of Democratic board members reducing segregation across schools and altering attendance boundaries to do so (relative to their non-Democratic counterparts), we now consider the potential for offsetting effects through household choices. There are several channels through which households may respond to board actions, perhaps differentially by race or some other characteristic. First, households could move within their district to select an alternative school attendance zone. Second, they could move to a different public school district. Third, they could opt out of the traditional public system altogether by enrolling their children in private schools (as Baum-Snow and Lutz, 2011 shows occurred for white students following court-ordered desegregation) or in charter schools.

While we cannot isolate all such channels, we examine the household response in Table 11. In panel A, we first examine the effect of an additional Democratic board member on our measure of racial residential segregation to investigate within-district moves. The associated point estimate is statistically indistinguishable from zero.[34] One might expect that household responses along such a margin to be muted within the horizon we consider given substantial costs to

[33] In Appendix Table A.2, we examine differential effects on the opening or closing of public schools, which would also register as an increase in our measure of attendance zone shifts. We do not find evidence that the narrow election of a Democratic board member affects the opening or closing of schools.

[34] It is possible that board actions engender within-district household moves without affecting residential segregation. To that end, we also analyze year-to-year within-district changes in the block group of a student's residence and find no evidence of an effect. The associated estimates are not reported here, but are available upon request.

moving residences. Further, board actions may create uncertainty about future attendance zone boundary changes elsewhere in the district, likely reducing the relative value of moving within the district as a response.

We turn next to the share of students attending traditional public schools in the district who are white. Panel A of Table 11 reports a negative but statistically insignificant point estimate for the effect of an additional Democratic board member on that outcome.[35] We then examine whether an additional Democratic board member has implications for households leaving the traditional public school system for private or charter schools. To do this, we compute the county-level enrollment shares of students attending private and charter schools, respectively.[36] The point estimates suggest a 2 percentage point reduction in the share of private enrollment and a 1 percentage point increase in the share of charter enrollment, but are statistically insignificant.[37] Thus, the findings in panel A suggest a limited overall household response to Democratic-induced attendance zone changes within the horizon we consider.

In panels B and C of Table 11, we then examine heterogeneity in movement to private and charter schools by whether the district does or does not have charter schools (at period 0). This is motivated by the likelihood that exercising a response to leave the traditional public school system will depend on the alternatives available to households. In panel B, we consider household responses (including effects on the private enrollment share) for the subset of school districts without a charter school. Once again, we find no statistically significant evidence of an effect on the share of public school students who are white for these districts, but find point estimates that are somewhat suggestive of households exiting to private schools. When including controls, the estimate suggests a 3 percentage point increase in the share of students attending private schools (from an average of 9% for these districts), while the final column suggests a larger increase in the share of white students attending private schools. However, these estimates are not estimated with precision.

In panel C, we alternatively limit the analysis to school districts with at least one charter school. Given that charters do not charge tuition (unlike private schools), exiting traditional public schools for charters may be more viable for households in districts where such choice is available. Examining the fraction of students attending public schools who are white, we estimate an approximate 2 percentage point reduction, which is statistically significant with controls and indicative of 'white flight' in districts with choice. For the average district with charters, this effect translates to about 117 fewer white students.

While suggestive that white households differentially transfer their children to charter schools in response, this result is also consistent with white households moving out of such districts. To disentangle the effect, we analyze the impact of an additional Democratic board member on the share of students attending charter schools. The results indicate that charter share increases by up to 3 percentage points for this subset of districts (the estimate is statistically significant with the inclusion of controls). This is compared to an average charter share for such districts of about 7%. We then examine whether it is the share of white students attending charter schools that explains this result. Estimates indicate about a 4 percentage point increase in the share of white students that attend charters, suggesting that white households differentially respond to board actions in the short run by leaving the traditional public system for charter schools where such an option is available.

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 board 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 part 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 2012 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-Democratic counterparts) Democratic board members decrease racial segregation across schools: an electoral victory that places an additional Democrat on a school board causes a reduction in the black dissimilarity index across schools of approximately 8 percentage points. This estimate is significantly more pronounced than what would be obtained by a naive OLS approach.

To establish a key mechanism underlying these effects, we then constructed a novel measure of attendance zone shifts. Our results indicate that such shifts are more numerous when an additional Democrat (relative to non-Democrat) is elected, which is consistent with them adjusting attendance zone boundaries to realize reductions in segregation across schools. These results establish that school boards causally affect the composition of peers across schools.

Finally, we investigated whether board efforts to reduce segregation cause households to re-sort across neighborhoods and schools in response, which can potentially offset or increase segregation across districts and between traditional public, charter and private systems. We find little evidence for such responses overall within the horizon considered; though our results do suggest that, in the wake of school board efforts to reduce school racial segregation, white families differentially leave the traditional public school system for charter schools where the alternative is available. Based on these findings, it would be interesting to examine longer-run household responses and the dynamic impacts of school board decisions on schooling inputs as additional data comes online. The political economy of school board behavior, largely abstracted from in this paper, is also an area worth investigating.

Taken together, our findings underscore the central role that school boards play in allocating students to schools, with likely implications for the production of learning and social inequality more generally. Understanding how school boards may influence human capital accumulation is of key policy interest and an important direction for future work.

[35] In Table A.2, we also examine the effect on the overall enrollment level and do not find evidence of an effect.

[36] We use data from the NCES Private School Universe Survey for private school enrollment and use interpolation to fill in missing data in odd years. We compute county-level shares (total enrollment of private or charter students over total enrollment across public, private, and charters) given the difficulty of assigning private schools to a specific school district in cases where multiple school districts belong to the same county. Standard errors are clustered by county.

[37] In Table A.2, we analyze whether there is a discontinuity in the number of private and charter schools in the county and district, respectively, but find no evidence of such effects.

Appendix A. Appendix tables

Table A.1
Descriptive statistics for different samples.

	Full sample		$\lvert x_{ij}\rvert \leq 10$		$\lvert x_{ij}\rvert \leq 5$	
	Mean	SD	Mean	SD	Mean	SD
Panel A: Electoral contest characteristics						
Election year	2010.1	1.6	2010.0	1.6	2009.9	1.7
Cycle length	2.05	0.32	2.07	0.36	2.08	0.40
# winners	1.54	0.98	1.98	1.21	2.13	1.20
# candidates	2.91	2.39	4.39	2.95	4.90	3.08
Vote margin	20.74	62.82	6.02	42.13	−1.77	36.42
No. obs.	482		119		72	
Panel B: District characteristics and outcomes						
% White	0.56	0.22	0.59	0.19	0.59	0.20
% Black	0.26	0.22	0.23	0.19	0.22	0.21
% Economic disadv.	0.63	0.14	0.60	0.14	0.60	0.13
Res. segregation – black	0.46	0.17	0.43	0.19	0.43	0.19
Res. segregation – economic disadv.	0.34	0.11	0.33	0.10	0.34	0.09
Sch. segregation – black	0.30	0.16	0.31	0.17	0.32	0.19
Sch. segregation – economic disadv.	0.22	0.13	0.21	0.13	0.21	0.14
Enrollment	2780.1	4035.1	2867.1	3058.0	2779.1	2897.5
# schools	11.47	12.91	11.75	11.21	11.42	10.50
# charters	0.83	1.60	0.71	1.04	0.80	1.10
Private share	0.13	0.11	0.14	0.11	0.15	0.12
Charter share	0.03	0.05	0.02	0.04	0.04	0.04
No. obs.	257		69		45	

Table A.2
Causal effect of additional Democrat – exploring alternative mechanisms.

	# schools	School opening	School closing
Vote margin > 0	−3.27	−0.037	0.116
	(3.52)	(0.072)	(0.098)
BW	28.34	25.29	18.23
	Enrollment	# private schools	# charter schools
Vote margin > 0	−170	−1.90	0.10
	(1167)	(2.50)	(0.49)
BW	17.51	19.98	17.51

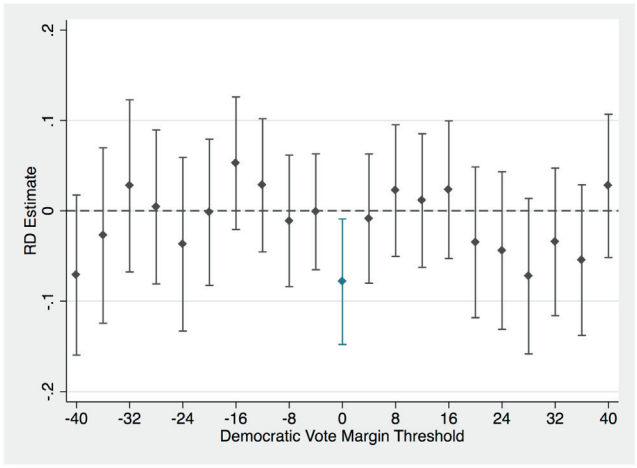
Notes: Regression discontinuity estimates are computed without controls using a local linear regression with optimal bandwidth given by Calonico et al. (2014), indicating the causal effect of an additional Democrat on different dependent variables of interest. All reported estimates are robust and bias-corrected (according to Calonico et al., 2014), based on the sample of 482 electoral contests. Standard errors are clustered by board (district-year).

Table A.3
Causal effect of additional black board member on school segregation.

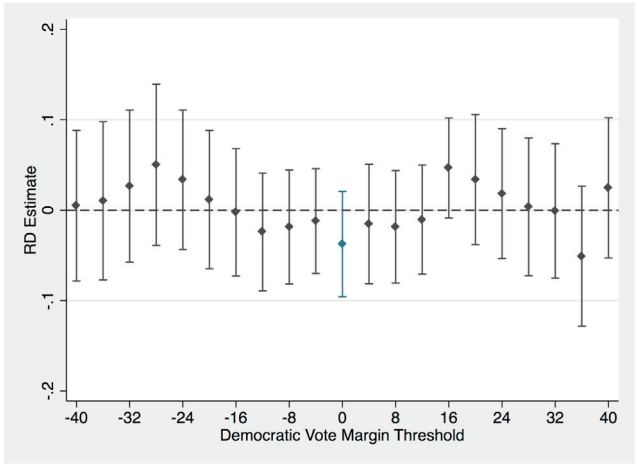
	Racial	Economic
Vote margin > 0	0.042	0.028
	(0.052)	(0.049)
No. obs.	454	454
Robust bias-corrected	Y	Y
Controls	Y	Y
Sample	$N_{sch} > 1$	$N_{sch} > 1$
BW	17.26	18.38

Notes: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014). We report robust bias-corrected estimates (according to Calonico et al., 2014), using the same controls as those defined in Table 5. Standard errors are clustered by board (district-year).

Appendix B. Appendix figures

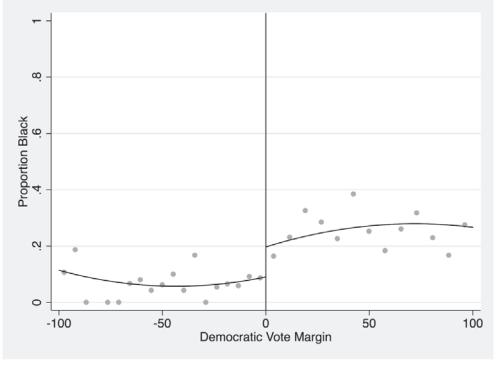


(a) Racial Segregation Across Schools

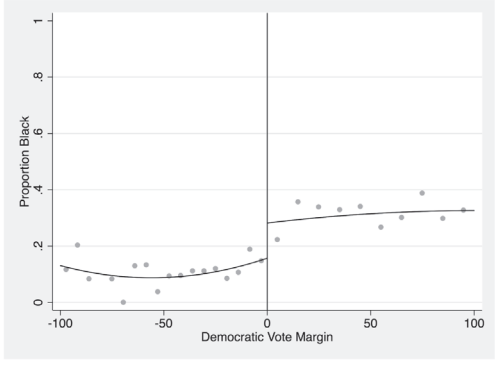


(b) Economic Segregation Across Schools

Fig. A.1. Validity – testing for discontinuities in segregation at placebo breaks. *Notes*: Regression discontinuity estimates are computed using a local linear regression with optimal bandwidth given by Calonico et al. (2014). All placebo breaks use the optimal bandwidth for the true threshold. We plot 90% confidence intervals using standard errors that are clustered by board (district-year).

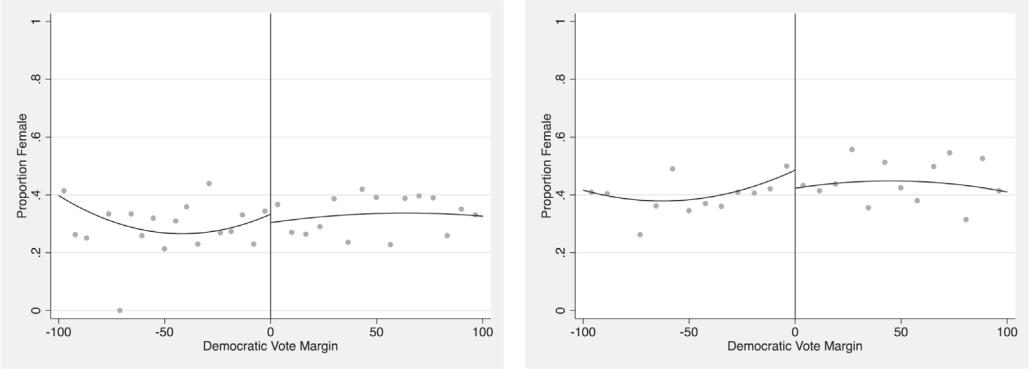


(a) Election Winners Across All Contests



(b) School Board Composition

Fig. A.2. Evidence of treatment – school board racial composition. *Notes*: Each figure is created by plotting the average of the proportion black within each bin of the Democratic vote margin on either side of the margin threshold, and fitting the data using a quadratic polynomial without controls.



(a) Election Winners Across All Contests (b) School Board Composition

Fig. A.3. Evidence of treatment – school board gender composition. *Notes*: Each figure is created by plotting the average of the proportion female within each bin of the Democratic vote margin on either side of the margin threshold, and fitting the data using a quadratic polynomial without controls.

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