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Backlash? Schooling Reassignments and the Politics of School Desegregation

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Abstract
School desegregation efforts often spark fierce political backlash. This dissent is typically ascribed to families' dissatisfaction with the changes in schooling assignments required to achieve desegregation aims. In this paper we use the empirical context of the Wake County Public School System (WCPSS) to estimate the effect of diversity-driven schooling reassignments on public engagement with educational politics, operationalized as turning out to vote in WCPSS school board elections. Specifically, we combine unique data detailing the geography and timing of school reassignments within WCPSS with rich, longitudinal, individual-level voter registration and turnout data to estimate the effect of living in an area where the district has reassigned students to a different school on voter participation in WCPSS school board elections held between 2001 and 2009. Estimated in a difference-in-differences framework, our results show that schooling reassignments substantially increased the likelihood that a registered voter cast a ballot in subsequent WCPSS school board elections, with these effects disproportionately driven by increased turnout among white voters.

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I. Introduction

The history of American public school desegregation is punctuated with fierce political backlash. The Supreme Court's landmark *Brown vs. Board of Education* decision met with "massive resistance" from state and local policymakers throughout the South (Bartley 1969). In the 1970s and 1980s, opposition to local desegregation efforts was a major driver of local politics in cities and suburban areas across the United States (Delmont 2016; Kraus 2000). More recently, contemporary school diversity initiatives have acted as a flashpoint for local educational politics in many U.S. communities (e.g., Shapiro 2019; 2020; Tucker 2022; Peetz 2019; Goldstein 2019; Lassiter 2021).

This paper supplements the rich historical, qualitative, and journalistic literature documenting backlash to school desegregation with credibly identified estimates of the effect of diversity-driven schooling reassignments—a major component of contemporary school desegregation programs—on voter backlash to those programs. Our analyses draw upon data from Wake County, North Carolina over the period from 2000 to 2010. During these years, the Wake County Public School System (WCPSS) operated one of the nation's most ambitious socioeconomic desegregation initiatives. As part of that initiative, WCPSS annually reassigned a relatively small proportion of students—usually about five percent—to new base schools to maintain the desired degree of socioeconomic diversity in each of the district's schools. We bring together unique data detailing the geography and timing of school reassignments within WCPSS with rich, longitudinal, individual-level voter registration and turnout data. Then, applying a difference-in-differences framework, we estimate the effects of schooling reassignments on voter participation in WCPSS school board elections. In addition to estimating the average effect of reassignment on political participation, our data also allow us to address key questions regarding the extent to which backlash may be sustained over time and across elections, as well as the degree to which backlash is driven by white voters and avoidance of majority-minority schools. We report three main results. First, we show that school reassignments increased voter turnout in school board elections by about 3 percentage points in the first election following the new assignments. This effect represents an increase of approximately 25 percent, relative to the comparison group mean, in these low-turnout elections. Second, we show that reassignment-driven increases in turnout are considerably higher—in the range of 5-6 percentage points—in the second election following reassignment, suggesting that diversity-driven reassignments remain salient in the minds of voters in subsequent years. Third, we find that these effects are disproportionately driven by increased turnout among white voters and exhibit little heterogeneity by voter sex or party affiliation. This result suggests that the political backlash to school desegregation efforts has a strong racial dimension, a suggestion supported by further analysis demonstrating: 1) The effect of reassignment on white voter turnout increases in the share of Black students at the newly assigned base school and 2) Reassignment leads to an increase in newly registered white voters, but has no such effects among nonwhite voters. Our results are robust to different estimators and empirical specifications, including those designed to account for staggered treatment implementation and the attendant potential for effect heterogeneity that may accompany the variation in treatment timing. Together, these results suggest that widely-held concerns about political backlash to schooling assignments, and thus the long-term viability of school desegregation initiatives, are well-founded.

II. The Politics of School Desegregation

School desegregation has played a crucial role in narrowing the racial inequalities that cut across the American social fabric. Desegregation efforts implemented in the wake of the *Brown vs. Board of Education* decision drove improvements in educational achievement and attainment for Black youth, improved their employment and labor market outcomes, and reduced their risk of arrest and crime victimization (Ashenfelter et al., 2006; Card & Rothstein, 2007; Johnson, 2011; Reber, 2010; Setren 2024). Further, existing research suggests that school desegregation initiatives boosted individuals' future civic participation and residence in racially diverse neighborhoods, regardless of their racial or ethnic identity (Bergman 2018; Kaplan, Spenkuch, & Tuttle 2018; Paluk et al. 2019).

Despite these well-documented benefits—especially given the lack of evidence for any negative effects on white youth (Johnson, 2019)—public opinion on school desegregation remains uneven. Surveys conducted in a wide range of contexts across time and place consistently demonstrate that the American public is broadly supportive of the concept of diverse schools (Frankenberg & Jacobsen, 2011; Hochschild & Scott, 1998; Page & Shapiro, 1992; Smith, 1982). But public support for desegregation dwindles, often substantially, when pollsters inquire about specific desegregation policies or the trade-offs required to achieve a meaningful level of diversity (Hochschild & Scott, 1998; Page & Shapiro, 1992; Frankenberg & Jacobsen 2011; Carlson & Bell 2021).[1]

Complementing this survey evidence, both historical case studies and contemporary journalistic accounts demonstrate that policy efforts to desegregate schools often meet with sharp public opposition.[2] While these accounts often depict communities that broadly appreciate the

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[1] Surveys routinely indicate that a large majority of the public—and white respondents in particular—oppose the use of busing to integrate schools (Hochschild & Scott, 1998; Page & Shapiro, 1992; Frankenberg & Jacobsen 2011). Furthermore, only 25% of respondents to a 2017 PDK poll indicated that they would accept a longer commute to allow their children to attend a diverse schools. This finding is echoed in recent work showing that, when asked to select between a pure residence-based school assignment policy and an assignment policy designed to achieve a degree of diversity across schools in the district, a majority of respondents indicate that they prefer pure residence-based schooling assignments, particularly when the diversity aims are race-based rather than based on socioeconomic status (Carlson & Bell 2021).

[2] Several excellent and in-depth historical case studies have explored the politics surrounding some of the nation's oldest and best-known desegregation initiatives, including those in Boston (Formisano 2004), Charlotte (Mickelson,

value of diverse schools and support for school diversity programs, they describe how families organize in opposition to school desegregation efforts that they view as disruptive to their children's schooling. Race plays a central role in many of these political stories. While there is a growing awareness of the disproportionate burdens that school desegregation efforts often place on communities of color (e.g., Thompson 2022), organized reactions to school desegregation have typically been driven by the grievances of white families (Delmont 2016). This white resistance is often understood to be the downfall of school desegregation efforts as opponents exploit cracks in the political or legal edifices supporting such programs to bring about their eventual downfall.

As a whole, prior work suggests that the public is more supportive of diverse schools in theory than in practice. Further, it points to three particular patterns in political pushback to desegregation: (1) Opposition to school desegregation initiatives are most acute among the families and communities whose schooling assignments and experiences are most directly affected by these policies; (2) The backlash to school desegregation is sustained over time, rather than a transitory phenomenon; and (3) The backlash to school desegregation is particularly acute among white families. In this paper, we systematically assess whether these patterns played out in the Wake County context by leveraging WCPSS' use of diversity-driven reassignments throughout the 2000s to estimate the effect of residing in a location affected by a schooling reassignment on the likelihood of turning out to vote in subsequent WCPSS school board elections.

Smith, & Nelson 2015), Louisville (K'Meyer 2013), Memphis (Johnson 2019), and Raleigh (Harwood, 2024; Parcel & Taylor 2015). Perhaps the capstone of this genre of research is Matthew Delmont's (2016) book *Why Busing Failed*, which examines how the politics of busing played out across the country. More contemporary journalistic accounts of diversity initiatives include coverage from New York City (Shapiro 2019; 2020), San Francisco (Tucker 2022), Maryland's Montgomery (Peetz 2019) and Howard (Goldstein 2019) counties, and North Carolina's Charlotte-Mecklenberg County (Lassiter 2021).

III. School Desegregation in the Wake County Public School System

As in many cities, desegregation was a slow process for Wake County schools in the years immediately following the Supreme Court's ruling in *Brown v. Board* (Parcel, Hendrix, & Taylor 2015; Ayscue, Siegel-Hawley, Kucsera, & Woodward 2018). At the time, the county's educational landscape consisted of a mostly white Wake County school district and a majority black Raleigh city school district. However, a series of court rulings and threats of withheld federal funding in the late 1960s and early 1970s ratcheted up the pressure for meaningful desegregation (Mickelson, Smith, & Nelson 2015). After years of contentious negotiations, the county and city districts merged in 1976, creating WCPSS. The newly merged district soon launched the ambitious "15-45" racial desegregation policy, which, for nearly 20 years, held that the racial composition of each school would be no less than 15 percent Black and no more than 45 percent Black. Fearing that this race-based assignment policy would be ruled unconstitutional after a series of judicial challenges to similar policies in the late 1990s, the district redesigned the policy to achieve balance on socioeconomic status and student achievement levels rather than race.

The socioeconomic-based assignment policy, which went into effect in the 2000-01 school year and extended through the 2009-10 school year, set a ceiling of 40 percent of enrolled students eligible for free or reduced-price lunch (FRL) in each school. In addition, the assignment policy set a target of no school serving a student body in which more than 25 percent of students were performing below grade level, as measured by state standardized tests. As an initial step toward achieving these goals, WCPSS divided the county into roughly 1,500 geographic nodes—a node can be thought of as a micro-neighborhood—that each contained approximately 125 students. Using these nodes, the district employed a multi-pronged student assignment policy to achieve its diversity targets.

First, WCPSS assigned each node to a "base" elementary, middle, and high school that served as the default schools for students in the node to attend. Second, the district implemented a controlled choice system, which supplemented base school assignments with a menu of magnet programs and a year-round calendar option designed with diversity targets in mind (Carlson et al. 2023). Third, WCPSS annually reassigned a small number of nodes—and the students within them—to a new base school. Specifically, throughout the period when the socioeconomic-based student assignment policy was in place, WCPSS administrators would annually take stock of every school in the district, considering factors identified in WCPSS' student assignment policy, including the socioeconomic composition, the proportion of students performing below grade level, and the school's capacity. Administrators also considered the need to populate any new schools scheduled to open. In pursuit of these multiple goals, WCPSS administrators would change the base schooling assignments for roughly 2-10 percent of nodes each year. Relying on the expert judgment of district administrators, the reassignments were intentionally distributed across the full geographic and demographic scope of the district. Although reassignment typically affected only a small share of students in any given year, more than 20 percent of elementary school students enrolled during our period of study experienced at least one school reassignment.

Our analyses focus on this third feature of the district's diversity plan: annual schooling reassignments. Parental frustration with reassignment mounted as the decade progressed and, in 2009, a slate of conservative board candidates ran for each of the four contested seats on the district's nine-member board on a platform of abandoning the district's desegregation efforts and moving to a system of neighborhood-based schooling assignments. The slate was successful. All four candidates running on the "neighborhood schools" platform prevailed in their races, tipping the board's ideological balance and ultimately leading to the demise of desegregation efforts in WCPSS.

IV. Data

We bring together data from WCPSS and the North Carolina State Board of Elections (NCSBE) to construct the dataset that underpins our empirical analyses. We first worked with WCPSS personnel to acquire: 1) Annual shapefiles containing the geographic boundaries of every node in the district for each school year from 1999-2000 through 2010-11, and 2) Annual records detailing the base schooling assignments for each node in the district—these records identify, for each school year from 1999-2000 through 2010-11, the base school that WCPSS assigned to each grade in every node in the district. Importantly, the district used a consistent set of node and school identifiers across the time period, allowing us to use these records to create our measures of node reassignment. As we describe in greater detail below, the fact that we observe annual base schooling assignments at the node-grade level accords us significant flexibility in generating our node reassignment measures.

We draw data from NCSBE to identify both the set of voters eligible to cast a ballot in school board elections and their participation in each election that occurred during our study period. With respect to eligible voters, the NCSBE has long snapshotted and publicly archived files containing the set of North Carolina registered voters on each election day—local, state, or federal—as well as on the first day of each calendar year. Each file contains a wide range of information on each registered voter, including their name, race, sex, address, party affiliation, age, date of birth, voter registration date, voting precinct, and a comprehensive list of electoral districts in which the voter resides, including the Board of Education district in which the voter's residence is located.

We collect eligible voters' turnout records from the voter history lookup tool available on the NCSBE website. As the initial step in collecting these records, we used the voter registration snapshots described above to compile the set of voters eligible to vote in any Wake County School Board election between 2001 and 2009.[3] We then wrote a script to automate the process of looking up the voting history—the set of elections in which a voter cast a ballot—of each "ever eligible" voter. The script first entered each voter's information—name, year of birth, and county—into the voter history lookup tool and then recorded each election in which the voter cast a ballot. These records serve as our primary source of information on voter turnout in school board elections.[4]

With these sets of records in hand, we used the following four-step process to create our analytic dataset. First, we assembled the set of voter registration snapshots described above, obtaining all unique addresses from those snapshots. That is, we compiled a list of every address that appeared in any NCSBE voter registration snapshot. Second, using ArcGIS and the set of shapefiles containing annual node boundaries, we geocoded every address appearing in any voter registration snapshot to the WCPSS node in which it is located. We performed this geocoding process separately using each node boundary shapefile from 2000 to 2011, which provided us with the WCPSS node identifier connected to each address in each year our data span.

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[3] Ideally we would be able to collect voter registration and turnout records prior to 2001 to allow for estimation of turnout trends over multiple pre-reassignment elections. However, comprehensive, accessible turnout records are not available from NCSBE prior to the 2000 election.

[4] To maximize the completeness and accuracy of our turnout measures, we complement the turnout records from the NCSBE voter history lookup portal with information from Catalist, a leading political data firm. Catalist has long maintained a national voter file containing registration and turnout records from each state, including those for relevant Wake County School Board elections. To obtain these records, we sent Catalist the file containing the set of voters eligible to cast a ballot in any of the aforementioned Wake County School Board elections. Catalist then matched the individuals in this file to its national voter file and provided us with their turnout records for each of the school board elections. These turnout records were remarkably similar to those in the NCSBE voter history portal, instilling confidence in the completeness and validity of our turnout measures.

Third, we used the voter registration snapshots to identify the set of voters eligible to cast a ballot in each Wake County School Board election from 2001 to 2009 and compiled these election-specific sets of potential voters into a single file. Then, we merged in node identifiers on the basis of address and year, which provided us with the WCPSS node that each eligible voter resided in at the time of each election. Finally, we used voter and election identifiers as the basis for merging turnout records, which indicate whether each eligible voter cast a ballot in a particular election, into the dataset. All told, this process resulted in a dataset with an eligible voter-by-election structure that contained information on node reassignments, voter characteristics, and voter turnout.

V. Measuring Voter Turnout and Node Reassignment

The outcome of interest in our analyses is turning out to vote in WCPSS school board elections held in October 2001, 2003, 2005, 2007, and 2009.[5] The Wake County School Board consists of nine seats, and elected candidates serve a four-year term. Elections for the nine seats were staggered across two electoral cycles, resulting in five seats up for election in one cycle and four in the other. Specifically, seats in board districts 3, 4, 5, 6, and 8 were up for election in the 2001, 2005, and 2009 cycles while seats in districts 1, 2, 7, and 9 were contested in 2003, 2007, and 2011. Naturally, for a given election, we only consider an individual an eligible voter if they reside in a board district up for election in that cycle.

Consistent with WCPSS reassignment procedures, we define our treatment measure at the node level and conceptualize it, broadly speaking, as a node's exposure to reassignment. As

[5] During the study period, North Carolina held school board general elections in October. If no candidate received a majority of votes cast in the October election, then a runoff election between the two candidates with the highest vote totals was held the following month. Our data contain turnout information for elections held on October 9, 2001, October 7, 2003, October 11, 2005, October 9, 2007, and October 6, 2009. At least one seat up for election went to a runoff in 2001, 2005, and 2009. Our data contain turnout information for the runoff elections, but our primary results focus exclusively on the main elections, given the sample restrictions imposed by the runoffs.

noted above, our ability to observe base schooling assignments for each grade in each node accords us significant flexibility in specifying our measures of exposure to reassignment. In our primary analyses we employ what we term a "full elementary reassignment" measure. For this measure, we consider a node exposed to reassignment in a given year if all elementary grades (KG-5) in the node were assigned to a different base school than they were assigned to the previous year.[6] Table 1 presents the number of nodes that experienced a full elementary reassignment (left-hand column) and any reassignment (right-hand column) each year from 2000 through 2010. The table illustrates that the number of full elementary reassignments varies considerably across the time period we analyze, from a low of 20 in 2001 and 2010 to almost 150 in 2007. Although larger in absolute magnitude, the number of nodes experiencing any reassignment follows a broadly similar pattern, with relatively fewer nodes being reassigned early in the decade and more experiencing any reassignment in later years. Nodes could experience more than one full elementary reassignment during the period our analysis spans. Our data indicate that, out of the more than 1,300 unique WCPSS nodes, 678 were exposed to at least one full elementary reassignment, 166 experienced at least two, and 12 underwent three full elementary reassignments. As we detail below, our analyses focus on estimating the effect of a node's first reassignment on voter participation in WCPSS school board elections. Importantly, though, our empirical model also controls for any subsequent reassignments.

[Insert Table 1 about here]

Our treatment specification must reflect the timing of reassignment relative to the electoral cycles described above. Accordingly, for reassigned nodes, we construct a series of

[6] Although we use the full elementary reassignment measure as our primary treatment indicator, we also create measures of "any reassignment"—defined as any grade in a node being assigned to a new base school—and a measure for "majority reassignment," defined as a majority of grades in the node being assigned a new base school.

indicators measuring the timing of each election relative to reassignment. These indicators begin with the second election prior to reassignment and extend through the third election after reassignment.[7] Importantly, our specification of these indicators reflects the nature of WCPSS' reassignment process, which began each fall with WCPSS personnel presenting a draft reassignment plan in a series of community forums. The district used these forums to solicit feedback and, over the next few months, would potentially make some revisions to the reassignment plan. WCPSS finalized its node reassignment plans in the spring, with the reassignments taking effect the following fall. Given the structure of this process, reassignment has the potential to affect voter behavior starting with release of the draft plan—we provide empirical evidence of such an effect in an analysis below. Thus, our we align our treatment indicators with the year when a node was identified as a candidate for reassignment in the district's draft reassignment plan. Separately for each year of our data, Table 2 details the alignment between release of the draft reassignment plan—when a node was identified as a candidate for reassignment—and electoral timing. The top panel of the table depicts this alignment for Board Districts 1, 2, 7, and 9, whose seats were up for election in 2001, 2005, and 2009. The bottom panel provides the same information for Districts 3, 4, 5, 6, and 8, which held elections in 2003 and 2007. To illustrate interpretation, the top panel of the table illustrates that, for nodes identified as candidates for reassignment in Fall 2003, the 2001 election was the most recent pre-treatment election while the 2003 and 2007 elections were the first and second post-treatment elections, respectively. The table also indicates that, for the 2003 cohort of reassigned

[7] We specify the measures of a node's second or third reassignment slightly differently. For those instances of reassignment, we construct measures indicating the number of years between the election and when the reassignment occurred—we include these indicators in our empirical model to flexibly control for any effects of these subsequent reassignments.

nodes, we do not have data from either a second pre-treatment or a third post-treatment election—other cohorts of reassigned nodes, however, allow us to estimate these parameters.

[Insert Table 2 about here]

Table 3 presents summary statistics for our analytic dataset. The table illustrates that, across the five elections we analyze, the average turnout rate was just below 12 percent, a figure consistent with the low turnout levels typically seen in local elections (Anzia 2013). The near-doubling of the share of observations from the 2001 to the 2009 election reflects the rapid population growth of Wake County during this time period. In terms of partisan composition, 42 percent of eligible voters were registered with the Democratic party, about 35 percent were registered Republicans, and the remaining 23 percent were unaffiliated with either of the two major parties. More than three-quarters of eligible voters across the five elections were identified as white in the voter registration records while about 17 percent were identified as Black and five percent as another race. Notably, the racial composition of the electorate differed from WCPSS student enrollment during this period. While more than 75 percent of voters identified as white, less 60 percent of WCPSS students held that identity. And more than a quarter of WCPSS students were Black, but just more than 15 percent of the electorate shared that racial identity. These disparities are consistent with research from other contexts that documents a "democratic deficit" in education governance (Kogan, Lavertu & Peskowitz 2021).

Turning to electoral outcomes, the average eligible voter could cast a ballot in a race where the winning candidate prevailed with, on average, about two-thirds of ballots cast, which translates to about 4,100 votes. The average eligible voter had an incumbent on the ballot just less than half of the time, and the incumbent won in about 30 percent of cases, implying that incumbents won about two-thirds of the races in which they were involved. Finally, Table 3 shows that about 16 percent of the sample consisted of voters with their first post-reassignment opportunity to vote in a school board election. The table also presents sample means for each of the other indicators measuring the relative timing of elections and node reassignment.

[Insert Table 3 about here]

VI. Empirical Strategy

We estimate the effect of node reassignment on voter turnout in school board elections in a difference-in-differences framework. Conceptually, our design compares, for reassigned nodes, voter turnout in the pre-reassignment period to turnout in the post-reassignment period, and then benchmarks that difference against turnout levels in non-reassigned nodes. We implement this design via a model of the following form:

$$Y_{int} = \sum_{e=-2}^{e=3} (\tau_e T_{inte}) + \left(\sum_{r=2}^{r=3} \left(\sum_{k=-10}^{k=10} (\delta_{kr} S_{intkr}) \right) \right) + \beta X_{int} + \mu_n + \sigma_t + \varepsilon_{int} \qquad (1)$$

where *Y* represents voter *i* who resides in node *n* turning out to vote in the school board election held in calendar year *t*. Our treatment specification, represented by the $\sum_{e=-2}^{e=3}(\tau_e T_{inte})$ term, is a matrix of dummy variables indicating the timing of elections relative to node reassignment. As noted above, these indicators extend from two elections prior to reassignment through three post-reassignment contests. We specify the reference category among the set of dummy variables to be *e* = -1. As a result, τ_e represents the estimated effect of reassignment on a voter's probability of casting a ballot in a school board election, relative to their likelihood of voting in the most recent pre-reassignment election. The second term on the right-hand side of equation (1) controls for potential effects of a node's second or third reassignment. Specifically, for each of these reassignments, which we index with *r*, the model contains a matrix of dummy variables indicating the number of years relative to reassignment, indexed with *k*. The model additionally includes a vector of observable voter characteristics, X_{int}, a node fixed effect, μ_n, an election (i.e.

year) fixed effect, σ_t, and an error term ε_{int}. We estimate this model via ordinary least squares with standard errors clustered at the node level. In addition to estimating this model for the full sample, we also estimate it separately by voter race, sex, and party identification. These specifications shed light on whether the effects of reassignment vary across politically relevant subgroups.

Our approach to estimating the effects of reassignment has multiple appealing features. First, our treatment specification formally assesses whether voters in the treatment and comparison groups exhibit differential trajectories in electoral participation in the years leading up to reassignment, a partial test of the parallel trend assumption that underpins our identification strategy. Second, estimating τ_e separately for each school board election sheds light on the potential for temporal heterogeneity in the effects of reassignment. It allows us to assess, for example, to distinguish between an immediate, short-term effect of reassignment on voting behavior and an effect that is sustained or grows in subsequent elections.

As in many applications, our treatment is administered at a higher level, nodes, than our unit of analysis, eligible voters. This difference introduces the potential for compositional change to be responsible for any observed effects. That is, it is possible that reassignment induces residential mobility, which could result in substantively different sets of registered voters residing in the node during pre- and post-reassignment elections. And the different sets of voters, rather than any (de)mobilizing effects of reassignment, could produce any estimated impacts. Although residential mobility might reasonably be considered a mechanism by which the effects of reassignment operate, it is undoubtedly a substantively different process than reassignment changing individual behavior—either spurring voters to the polls or inducing them to stay home.

Accordingly, after presenting our primary results, we conduct a series of analyses designed to generate evidence to help adjudicate between different potential mechanisms.

A recent econometric literature suggests that designs like the one described above, which leverage variation in treatment timing to identify treatment effects, may yield biased estimates when average treatment effects are estimated using standard two-way fixed effects (TWFE) regressions (such as the model outlined in equation (1)) and treatment effects are heterogeneous across treated cohorts (Baker et al., 2022; Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021). Contaminations to the dynamic treatment coefficients are a result of the TWFE estimation strategy itself (Sun & Abraham, 2020) and are driven in part by early treated units inadvertently serving as controls for later treated units. To assess whether our results are sensitive to our use of a TWFE estimator, we also apply the doubly robust difference-in-differences estimator proposed by Callaway and Sant'Anna (2021). Conceptually, the Callaway and Sant'Anna (2021) approach estimates the average treatment effect on the treated (ATET) for each cohort (i.e., nodes treated in the same calendar years) at each event time (i.e., election relative to reassignment). This approach relies on a "cleanly" identified set of comparison units including not-yet-treated and never-treated nodes, thus avoiding the potential sources of bias associated with TWFE estimators identified by the recent difference-in-differences literature. These cohort-by-event-time parameters are then aggregated across cohorts by event time to produce election-specific estimates.

VI. Results

Table 4 presents the results of estimating equation (1) for the full sample, as well as separately by voter race, party identification, and sex. The full-sample results, which we present in the first column of the table, illustrate that voter turnout probabilities in the second pre-reassignment election do not differ from those in the election immediately preceding reassignment—the coefficient estimate for the second pre-reassignment election is insignificant and close to zero. This provides evidence in support of the parallel trends assumption underpinning our identification strategy.

The main takeaway from Table 4, however, is that node reassignment produces statically significant and substantively large increases in the probability that eligible voters cast a ballot in post-reassignment elections. For example, node reassignment increased the likelihood of voter turnout in the first post-treatment election by about three percentage points. Although seemingly modest in magnitude, Table 3 illustrates that mean turnout rates in the comparison group are only about 12 percent, meaning that the estimated effect represents an increase in turnout of approximately 25 percent. The estimate for the two subsequent elections—the second and third elections after reassignment—are even larger, with reassignment increasing the probability of voting by about five percentage points in the second post-reassignment election and by nearly nine percentage points in the third. We note, however, that the coefficient estimate for the third post-reassignment election should be interpreted with some caution as it is only informed by the three earliest cohorts of reassigned nodes.[8]

[Insert Table 4 about here]

The second panel of Table 4 presents estimates separately for white and non-white voters. For both groups, the results again indicate no evidence of differential pre-treatment trends in

[8] In further analyses we estimate equation (1) over an analytic sample containing the three earliest cohorts of reassigned nodes. Results from this sample shed light on whether the large effect in the third post-reassignment election is an election effect or a broader cohort effect. For these cohorts, if the estimated turnout effects for the first and second post-reassignment elections were similar to the effects in the third post-reassignment election, then it would suggest that something about reassignment of these cohorts was responsible for the substantial effect in the third post-reassignment election. However, the analysis indicated that turnout effects for the first and second post-reassignment election were broadly consistent across cohorts, suggesting that the large effect for the third post-reassignment election is a timing effect, rather than a cohort effect.

turnout. The results make clear that reassignment significantly increases turnout in the first three post-treatment elections for both groups of voters. However, in each election the magnitude of the effect for white voters is more than twice as large as the effect for non-white voters. For example, in the first post-treatment election, we estimate reassignment to increase turnout among white voters by 3.4 percentage points, but the effect on non-white voters is just 1.6 percentage points. Similarly, in the second election following reassignment, white voters exhibit a turnout increase of nearly six percentage points while the effect among non-white voters is 2.5 percentage points. And the disparity for the third post-treatment election is even larger, with reassignment increasing turnout among white and non-white voters by 8.8 and 2.3 percentage points respectively. For all three post-treatment elections, tests of equality reject the null hypothesis of no difference in the effect between white and non-white voters at $p<0.001$. Together, these results make clear that the full-sample results are primarily driven by white voters and are consistent with backlash to diversity-driven schooling reassignments.

The third panel of Table 4 presents results separately by voters' partisan affiliation—it presents results for Democrats, Republicans, and voters unaffiliated with either of the two major parties. We highlight four takeaways from this set of results. First, all estimates for the second election prior to reassignment are again close to zero and insignificant, indicating no differential trends in turnout between treated and comparison nodes in the time leading up to reassignment. Second, the results make clear that reassignment increased turnout among all three groups in all three post-reassignment elections, with the magnitude of the effect increasing in each post-reassignment election. Third, the estimated effects of reassignment on voters with a partisan affiliation—either Democrat or Republican—are larger than the effects among voters unaffiliated with either party. This pattern holds for each of the three post-reassignment elections we analyze and, for each election, tests of equality reject the null that the estimates for the three groups are equal to one another. Fourth, the estimated effects for Democrats and Republicans are broadly similar, although the effects for Democrats are slightly larger than those for Republicans in the second and third elections following reassignment.

The fourth and final panel of Table 4 presents results separately for male and female voters. The estimated effects for the two groups are remarkably similar to one another, as well as to the full sample results in the first column of the table. For each post-reassignment election, tests of equality are unable to reject the null hypothesis of no difference between the estimated effects for the two groups, indicating no meaningful heterogeneity in the impacts of reassignment on turnout of female and male voters. Together, the results in Table 4 tell a clear story, one where reassignment leads to a large increase in the likelihood that voters, particularly white voters, head to the polls to cast a ballot in subsequent school board elections. Further, the results make clear that these effects are not limited to the first post-reassignment election as the effects persist, and even increase in magnitude, in the second and third elections following reassignment.

As noted above, the TWFE estimator we use to produce the results presented in Table 4 may yield biased estimates when treatment implementation is staggered across time—as is the case with WCPSS node reassignments—and effects vary across treated cohorts. Callaway and Sant'Anna (2021) propose an estimator that addresses this issue by estimating the ATET separately for each treatment cohort at each event time. In our context, and consistent with the prior analysis, we define treatment cohorts in terms of a node's reassignment relative to its next electoral opportunity. For example, if a node was reassigned in 2003 and was also located in a board district that was up for election in fall 2003, then that node would be a member of the 2003 treatment cohort. However, if a node was reassigned in 2003 and the next election for that node's board district was fall 2005, then the node would be assigned to the 2005 treatment cohort.

[Insert Table 5 about here]

Table 5 presents the ATETs for the 2005, 2007, and 2009 treatment cohorts in the 2003, 2005, 2007, and 2009 elections, as generated by the estimator proposed in Callaway and Sant'Anna (2021). The absence of 2001 and 2003 treatment cohorts is attributable to the inability to observe a pre-treatment baseline for these cohorts. That is, estimation of ATETs for a given treatment cohort requires a pre-treatment measure of turnout. In our case, the 2001 and 2003 treatment cohorts would require measures from 1997 and 1999, respectively. Unfortunately, the electoral data do not extend that far back, preventing us from obtaining a pretreatment baseline for these cohorts. Consequently, these cohorts are considered "always treated" and ATETs for them are not estimated.

The primary takeaway from Table 5 is that, across all three cohorts, reassignment has no impact on turnout in pretreatment elections but statistically significant and substantively large effects on turnout in elections following reassignment. For example, for the 2005 cohort, reassignment is estimated to have increased turnout by four percentage points in the 2005 school board election and 5.5 percentage points in the 2009 election. Similarly, for the 2007 and 2009 cohorts, reassignment is estimated to have boosted turnout by three and two percentage points, respectively, in the elections held in those years. Together, Table 5 supports the conclusions drawn from the results in Table 4—no evidence of pre-reassignment trends in turnout, but statistically meaningful and substantively large turnout impacts in post-reassignment elections.

[Insert Table 6 about here]

Table 6 aggregates the treatment cohort-by-election ATETs presented in Table 5 to present a single ATET for each of three elections: 1) The election prior to reassignment, 2) The first election after reassignment, and 3) The second election after reassignment. The first column of the table presents the full-sample results while the second, third, and fourth panels mirror the structure of Table 4, presenting results separately by voter race, party identification, and sex, respectively. Reassuringly, the full-sample results again show no turnout effect of reassignment in the election prior to treatment—the point estimate is statistically insignificant and very close to zero. By contrast, the estimated effects for the first and second post-reassignment elections, at three and six percentage points, respectively, are statistically significant and substantively large. Overall, the results in Table 6 are remarkably consistent with those in Table 4 and provide further evidence that diversity-oriented changes in schooling assignments spark substantial increases in the likelihood that voters in affected neighborhoods head to the polls.

The subgroup results in Table 6 are also quite consistent with their analogs in Table 4, and we highlight four specific features of these results. First, across all the groups we analyze, no estimates for the election prior to reassignment approach statistical significance, instilling further confidence in the validity of our empirical design. Second, mirroring the results presented above, Table 6 shows that, for both the first and second post-reassignment elections, the estimated effects on white voters are about twice the magnitude of the effects for non-white voters. For white voters, we estimate reassignment to increase turnout in the first and second post-reassignment elections by three and six percentage points, respectively; the analogous estimates for non-white voters are in the range of 1-2 percentage points. Third, the results broken down by voters' party affiliations again show little meaningful difference in the effects between Democratic and Republican voters, but the effects among these two groups are about twice the size of the impacts among unaffiliated voters. Among voters affiliated with either the Democratic or Republican party, we estimate reassignment to increase turnout by three and six percentage points in the first and second post-reassignment elections, respectively. The comparable estimates for voters unaffiliated with either of the two major parties are only about half the magnitude. Fourth, there is little evidence that reassignment impacts male and female voters differently—the estimates for the two sexes are again similar in both size and significance.

Taken as a whole, the results we present in Tables 4, 5, and 6 serve to both support the validity of our empirical strategy and demonstrate that reassignment boosted turnout in Wake County school board elections, with the effects among white voters primarily driving the observed impacts.

VII. Supplemental Analyses and Robustness Checks

Our main results make clear that diversity-driven school reassignments produce statistically significant and substantively large increases in the probability of registered voters casting a ballot in school board elections. In this section we present a series of analyses designed to shed further light on potential heterogeneity in these effects, as well as on potential mechanisms by which they may operate. Specifically, we present analyses that assess: 1) Whether the effects of reassignment vary by the timing between a node's reassignment and its next electoral opportunity; 2) The sensitivity of the estimates to inclusion of a voter fixed effect; 3) The effects of reassignment on voter residential relocation and new voter registrations; and 4) The role that characteristics of a node's assigned base school play in mediating or moderating the estimated effects of reassignment. Together, these analyses provide valuable nuance and context to our main results presented above.

Timing of Reassignment vis-à-vis the Electoral Cycle

The Wake County School Board operates according to a four-year electoral cycle and reassignments can occur in any year of the four years. Table 7 presents the number of node reassignments relative to the timing of its next school board election. To illustrate interpretation, the first row of the table indicates that, for reassignments taking effect in Fall 2000, no nodes had an election that fall—school board elections only occurred in odd-numbered years—but 24 reassigned nodes had their next election the following fall (i.e., 2001) while 52 nodes waited three years until the next election (i.e. 2003). Reassignments occurring in odd-numbered years, by contrast, took effect in the months immediately prior to the October election. For example, for reassignments taking effect in 2007, voters in 79 nodes could go to the polls that fall while voters in 64 nodes had their next electoral opportunity in 2009. More generally, Table 7 illustrates that the distribution of node reassignments relative to the timing of its next school board election contains significant variation, and it is possible that the effects of reassignment vary according to this timing.

[Insert Table 7 about here]

To investigate this possibility, we construct a series of indicators measuring the number of years between release of the draft plan first identifying a node as a candidate for reassignment and the node's next electoral opportunity. We then include these indicators in a model of the form:

$$Y_{int} = \sum_{r=1}^{r=3}\left(\sum_{k=-3}^{k=3}(\delta_{kr}T_{intkr})\right) + \beta X_{int} + \mu_n + \sigma_t + \varepsilon_{int} \qquad (2)$$

where *Y* represents voter *i* who resides in node *n* turning out to vote in the school board election held in calendar year *t*. For each full elementary reassignment *r*, which ranges from one to three, the treatment specification is a matrix of dummy variables indicating the number of years relative to reassignment, which we index with k. So, when k = 0, T_{intkr} is a dummy variable indicating that the election was held the fall when the draft plan that would ultimately lead to the node's reassignment was released. When k < 0, T_{intkr} indicates that the draft plan leading to reassignment of the node would be released in k years. And when k > 0, T_{intkr} indicates that the draft plan leading to reassignment was released k years ago. For each instance of full elementary reassignment, we specify the reference category among the set of dummy variables to be k = -1. As a result, δ_{kr} represents the effect of reassignment on school board election turnout relative to the year before release of the draft plan resulting in a node's reassignment.[9] By estimating separate turnout effects for elections held the year that the draft reassignment plan was released, the year that reassignment took effect, and each of the two years following reassignment, these estimates shed light on the potential for the effects of reassignment to vary according to the timing relative to the next electoral opportunity.

[Insert Table 8 about here]

Table 8 presents the results from estimating equation (2) for the full sample and for each of the subgroups examined above. The results indicate relatively little heterogeneity in the effects of reassignment according to its timing relative to a node's next electoral opportunity. The full sample results in Table 8 make clear that release of a draft reassignment plan boosted turnout in elections held that fall by more than two percentage points. Turnout effects for elections held in subsequent years—the fall that reassignment took place and each of the two years following that—are broadly similar, in the range of 2-3 percentage points. The lack of heterogeneity in the full sample results is reflected in the subgroups we analyze. Although the size of effects varies

[9] Consistent with equation (1), the remaining contents of the model consist of a vector of observable voter characteristics, X_{int}, a node fixed effect, μ_n, a calendar year fixed effect, σ_t, and an error term ε_{int}. We estimate this model via ordinary least squares with standard errors clustered at the node level. We estimate the model for the full sample, as well as for each of the subgroups examined above.

across groups—for example, the effects among white voters are larger than the effects among non-white voters—Table 8 shows relatively little within-group variation in those effects by the relative timing of reassignment and a node's next electoral opportunity.

Together, these results suggest that voters induced to turn out by reassignment are relatively insensitive to the timing of a node's next electoral opportunity—they are about as likely to vote in an election held the fall that reassignment takes effect as they are in one held one or two years down the line.

Specification Containing a Voter Fixed Effect

The specification of our primary empirical model contains a node fixed effect. Although this fixed effect corresponds to the level at which reassignment occurred, it leaves open the possibility that some portion of the estimated effect of reassignment in our main results is driven by compositional change in reassigned nodes. For example, it is possible that families moving to a node in the years following reassignment have a high underlying propensity to vote in school board elections. To address this possibility, we estimate a variant of the specification presented in equation (1) where we replace the node fixed effect with a voter fixed effect. Although this specification mitigates validity threats posed by compositional change, it will almost certainly understate the effects of reassignment and its results, therefore, should be considered lower bound estimates for the effects of reassignment.[10]

[Insert Table 9 about here]

[10] Estimates from this specification will not reflect a scenario where reassignment spurs residents to register to vote and cast ballots in all observed post-reassignment elections. Such a scenario is best illustrated by reassignments occurring relatively late in our analytic period, where there may only be a single post-reassignment election. Individuals induced by reassignment to register and vote in this election will not inform the coefficient estimate for the effect of reassignment—the empirical model will be drop them as a "singleton" observation. Similarly, individuals moving from outside of Wake County into a previously reassigned node may be convinced by their neighbors to cast a ballot in the district's board elections. Conceptually, their vote should be reflected in the estimated effect of reassignment. Empirically, a model containing a voter fixed effect would not reflect these voters' turnout in the estimated impacts of reassignment.

Table 9 presents results from the model containing voter fixed effects. As expected, the estimated effects of reassignment are smaller in magnitude than their analogs from the model containing node fixed effects. However, the estimates remain positive, statistically significant, and meaningful in substantive magnitude. The full-sample results indicate that reassignment increases turnout by about 1.5 percentage points in the first post-reassignment election and by about 2 and 3 percentage points, respectively, in the second and third elections following reassignment. The subgroup results in Table 9 are also consistent with our main results. The estimated effects for white voters are larger than those for non-white voters and there is no meaningful difference in the estimates for male and female voters. Together, the results in Table 9 serve to further support the results of our main analyses, as well as the substantive conclusions drawn from those analyses.

Effect of Reassignment on New Voter Registration and Residential Mobility

Although the estimates from the specification containing a voter fixed effect are positive and significant, they are smaller in magnitude than our primary estimates. This difference suggests that some portion of the estimated effect of reassignment is attributable to newly registered voters casting a ballot, compositional change, or both. We conduct two analyses to more directly assess the plausibility of these potential mechanisms. First, we estimate the effect of reassignment on new voter registration. We do so by taking advantage of information in our data specifying each voter's registration date. Using this information, we define a new voter as one who first registered to vote between 2000 and 2010, the period during which WCPSS reassignments occurred and could thus potentially spur a citizen to register to vote. We define this measure at the voter level—a given voter is either newly registered or not. Approximately one-third of voters in our data hold this classification. After generating this measure, we estimate a variant of equation (1) in which we specify it as the outcome measure.

[Insert Table 10 here]

We present the results of estimating this model in the left-hand panel of Table 10. The first column of the panel presents the full-sample results while the second and third panels present results for white and non-white voters, respectively. Across all three groups, and mirroring our main results, there are no differential pre-reassignment trends in new voter registration rates across treated and comparison nodes. In the post-treatment elections, by contrast, the full sample results demonstrate that reassignment increases newly registered voters residing in the node by two percentage points in the first post-reassignment election and four percentage points in the second and third elections following reassignment. The results make clear that the increase in newly registered voters in reassigned nodes is driven entirely by white voters, which is again consistent with political backlash to school diversity efforts. Considered alongside our earlier findings, these results provide substantial evidence that the effects of reassignment operate via the mobilization of white voters, both in terms of spurring turnout among already-registered voters and prompting citizens to newly register in order to cast a ballot.

Although our analyses indicate that the effects of reassignment are primarily driven by mobilization, it remains possible that node compositional change is responsible for some portion of the impact. To more directly assess this possibility, we leverage the fact that our data contain time-varying information on voter addresses. We use this information to generate a measure indicating whether we observe a voter moving over our period of analysis. We consider a "moving" voter as one we observe at two or more unique addresses between 2000 and 2010.

About 20 percent of voters in our sample fit this definition. After creating this measure, we specify it as the outcome in a model of the structure presented in equation (1).

The right-hand panel of Table 10 presents the results from estimating this model, first for the full sample and then separately for white and non-white voters. The full sample results show no significant differences between treatment and comparison nodes in the proportion of eligible voters with an observed move—this result holds across all elections we analyze. Results for white voters are broadly similar to the full sample results, an unsurprising pattern given that white voters account for more than three-quarters of observations in our data. Together, these results suggest that voter mobility, and compositional change more broadly, is unlikely to serve as a mechanism by which the effects of reassignment operate, at least among the full sample and white voters.

The results in the right-hand panel of Table 10, however, tell a different story for non-white voters. Indeed, they indicate that reassignment results in nodes containing larger shares of residentially mobile voters, particularly in the second and third elections following reassignment. More broadly, the results in Table 10 suggest that different groups of voters may respond differently to reassignment, with white voters exercising political voice and non-white electing residential exit in the wake of reassignment (Hirschman 1970), We return to this topic in more detail in the concluding section.

The Role of Base School Characteristics

Our results to this point make clear that the effects of reassignment on voter turnout is a story of voter mobilization, but they provide relatively little insight into the particular features of the reassignment process responsible for that mobilization. For example, the turnout increases spurred by reassignment may be due to logistical inconveniences caused by the reassignment, such as disruption of carpool arrangements, longer bus rides, or a less convenient location relative to parents' workplace. Alternatively, the turnout boosts may be attributable to voter reactions to specific characteristics of the newly assigned base school, such as its status as a recently constructed school lacking a track record in the district, the racial and ethnic composition of the student body, or its level of student achievement. Although we are unable to directly observe voter reactions and attitudes, we can shed indirect light on the issue by analyzing the degree to which base school characteristics mediate or moderate the turnout effects of reassignment.

As the first step in these analyses, we identified, separately for each year our data span, the WCPSS-specified base elementary school for each node in the district. Then, drawing upon a longitudinal school-level dataset containing annual information on all WCPSS schools, we merged in several characteristics of the base elementary school connected to each node: 1) An indicator that the assigned base elementary school was newly constructed, 2) Measures of the proportion of base elementary school students who are white, Black, Hispanic, and Asian; 3) The distance in miles between the geographic centroid of the node and its assigned elementary school, and 4) The average reading and math achievement levels at the school. Together, this process added—for each election from 2001 to 2009—time-varying information on the base elementary school associated with each voter's residential address and observable characteristics of those base elementary schools.

We conduct two sets of analyses with these data. First, we add base elementary school characteristics as covariates in the model presented in equation (1) to assess the degree to which these characteristics mediate the effects of reassignment. If the effects of reassignment are purely a function of voter reaction to base school characteristics, then conditioning on those characteristics should reduce, or even eliminate, the effects of reassignment.

[Insert Table 11 about here]

Table 11 presents the results from estimating these models. It is immediately apparent that conditioning on observable base school characteristics has no meaningful impact on the estimated effect of reassignment. Across all specifications, the estimates are remarkably similar in sign, significance, and magnitude to our primary results in Tables 4-6. Substantively, this implies that the effects of reassignment are not driven by voter reaction to the observable characteristics of their newly assigned base school.

Although base school characteristics do not mediate the effect of reassignment on voter turnout, it remains possible that they are a source of heterogeneity in those effects. Our second set of analyses examine this possibility by estimating a variant of the specification in equation (1) where we interact each base school characteristic with the indicator for the first post-reassignment election. In addition to estimating this specification for the full sample, we also estimate it separately for white and non-white voters.

[Insert Table 12 about here]

Table 12 presents the results for the terms contained in the interaction described above. The top panel of the table presents results for the full sample while the middle and bottom panels present results for white and non-white voters, respectively. In each panel, the top row of the table presents the main effect of the base school characteristic noted in the column header while the middle row presents the main effect for the indicator of the first post-reassignment election. The bottom row presents the coefficient estimate for the interaction between the two terms, which can be interpreted as the change in the effect of reassignment on voter turnout as the base school characteristic goes from zero to one.[11]

The full sample results show no significant variation in the effects of reassignment according to the racial or ethnic composition of a node's newly-assigned base school. The interactions between the indicator for the first post-reassignment election and each of the four school composition measures fail to reach conventional levels of statistical significance. The results do, however, provide evidence pointing to heterogeneity in the effects of reassignment according to the distance between voters' nodes and its elementary school. In particular, each additional five miles that families must travel is estimated to increase the turnout effects of reassignment by one percentage point.

When we restrict the sample to white voters, our results provide some evidence that the effect of reassignment systematically varies according to the base elementary school's racial/ethnic composition, particularly the share of Black students in the school. Specifically, the results indicate that reassignment boosts the probability of turnout by a bit more than two percentage points when a white voter's node is reassigned to a hypothetical base elementary school with no Black students—the main effect for the reassignment indicator is 2.3 percentage points. The estimated effect approximately doubles—to nearly 4.5 percentage points—if the node was reassigned to a base elementary school where Black students accounted for half of the school's enrollment. This result, coupled with the statistical insignificance of the three other measures of school racial and ethnic composition, is consistent with white families' backlash to

[11] We also estimated specifications where we interacted the respective measure of school racial or ethnic composition with the indicators for the first, second, and third elections following reassignment. The estimates for these three interactions, which are available upon request, were similar in magnitude, both to one another and to the estimates in Table 10. However, these three interactions were highly collinear, resulting in the point estimates being estimated with relatively little precision.

WCPSS' school desegregation program cited in prior journalistic and anecdotal accounts of the program's demise.

Among nonwhite voters, the race/ethnicity results in Table 12 provide some evidence that the effects of reassignment increase in the share of Asian students comprising a node's newly-assigned base school, although the interaction is estimated with relatively little precision. By contrast, the impact of reassignment significantly declines as the share of Hispanic students in a node's assigned elementary school increases. Further, the estimates for this voter subgroup make clear that the heterogeneity in the effect of distance observed for the full sample is disproportionately driven by nonwhite voters.

VIII. Discussion and Conclusion

Throughout U.S. history, efforts to diversify the nation's public schools have consistently sparked organized opposition. The literature is replete with historical and journalistic accounts of this blowback. Our study aims to contribute to this literature by providing systematic evidence on: (1) The effect of schooling reassignments—a major component of contemporary school diversity initiatives—on voter turnout in school board elections, (2) The extent to which this political reaction is sustained over time, and (3) The degree to which increased political engagement is driven by white voters. We provide this evidence through analysis of data detailing the geography and timing of school reassignments within WCPSS combined with rich, longitudinal, individual-level voter records.

Applying difference-in-differences techniques, our results show that school reassignments increased voter turnout in school board elections by about 3 percentage points in the first election following the new assignments and by nearly twice that amount in the second post-reassignment election. Further analysis provides evidence that these effects are disproportionately driven by white voters and that, for this group of voters, the magnitude of the impact of reassignment increases in the share of Black students enrolled in the newly assigned base school. We further demonstrate that reassignment increases the share of newly registered voters in a node, with the effect wholly attributable to newly registered white voters. In short, our results provide clear and convincing evidence of substantial, sustained, and racially motivated backlash to WCPSS' school diversity efforts.

These findings raise important questions about the political viability of school desegregation efforts in the contemporary era. Although we lack data on mobilized voters' candidate selections, our findings strongly suggest that WCPSS's 2000-2010 school reassignments contributed to the backlash that ultimately led to the demise of the district's longstanding school diversity efforts. Such a conclusion is further reinforced when we consider our findings alongside both the national historical record and the steadily increasing electoral success of candidates opposed to school reassignments in Wake County during the study period. Put differently, our results indicate that a major policy lever that districts can, and arguably must, pull in pursuit of diversity goals spurs patterns of political engagement that threaten the long-term viability of those aims. This is a troubling conclusion to reach, given the robust body of evidence suggesting that school desegregation efforts help to narrow longstanding racial inequalities by benefitting young people of color without negatively affecting white youth.

While it is impossible to know how well these findings generalize, several aspects of the WCPSS policy context might have been expected to minimize backlash. First, rather than organizing its school assignment practice around racial diversity, WCPSS sought to achieve diversity in terms of socioeconomic status and academic performance. It is reasonable to think that such a framing may minimize any racial dimension of schooling reassignments in the district. That does not appear to be the case, however, perhaps owing to the district's socioeconomic-based school assignment policy coming on the heels of its "15-45" policy, which sought to achieve diversity on the basis of race. Second, WCPSS explicitly structured its policy to minimize school reassignments. As we described, reassignments typically only affected 5-10 percent of nodes in a given school year and, even then, many of the reassignments were designed to accommodate the district's rapid growth and populate newly built schools. Third, although residential segregation complicated school desegregation efforts in WCPSS, as it does in other communities across the U.S., Wake County's distinctive patterns of development created a "checkerboard" pattern of desegregation that allowed the district to reassign students without dramatically increasing the distance between students' homes and their assigned schools (Domina et al., 2021). Fourth, WCPSS complemented its school reassignment policy with a controlled choice system that allowed families to select among calendar and magnet options, in addition to their assigned base school. Prior work shows that white and Asian families disproportionately took advantage of these options, suggesting that these school choices may have served as a "pressure release valve" that mitigated at least some political opposition to school desegregation (Carlson et al. 2023). Together, these features suggest that WCPSS worked to construct an optimal context for the long-term political viability of its school diversity initiatives. It is therefore striking that WCPSS's school reassignments engendered a pronounced and sustained backlash, culminating in the 2009 election of a slate of board candidates who followed through on their campaign promises to enact "neighborhood schools" assignment policy.

In addition to its relevance to contemporary policy discussions, our work also carries several implications for research into the politics of desegregation. Most basically, this paper provides among the first direct, systematic evidence on politics surrounding a major component of contemporary school diversity initiatives. In doing so, it provides a nice complement to work in topically adjacent spaces, such as the large literature describing responses to school desegregation initiatives in the wake of *Brown v. Board* (see Reardon & Owens 2014 for a comprehensive review), or the set of studies that estimate the civic effects of particular education policies or programs (e.g., Gill et al., 2020; Carlson, Chingos, and Campbell 2017; Hastings et al. 2007; Cohodes & Feigenbaum, 2021), including school desegregation (Billings, Chyn & Haggag 2021), on students later in their lives.

The analyses we present above answer several important questions, but they raise many others that could usefully be addressed in future research. First, our results suggest that the political response of white voters respond to reassignment—and perhaps school desegregation efforts more generally—differ from the responses of nonwhite voters. In particular, we provide clear evidence that white voters respond to reassignment with political voice nonwhite voters may be more likely to respond with residential exit from reassigned nodes. Although in-depth inquiry into these potential differences is beyond the scope of the present paper, it is a natural topic for future research on the politics of school desegregation.

More broadly, future work would do well to examine other dimensions of the politics of desegregation. Our work engages voter response to reassignment, but there are many additional dynamics ripe for analysis. For example, what does candidate entry look like in response to reassignment, or school desegregation more broadly? Are there any effects on the platforms on which candidates run? Do dimensions of school diversity initiatives beyond reassignment evoke political responses? Do the effects of reassignment on turnout in school board races spill over and spur increased turnout in other political contests? These sorts of questions are natural candidates for future inquiry, and such analysis will be necessary if we hope to gain a more comprehensive understanding of the politics of contemporary school diversity initiatives.

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Tables and Figures

Table 1. Number of Node Reassignments by Year

	Full Elem. Reassignment		Any Reassignment	
Year	**No**	**Yes**	**No**	**Yes**
2000	936	71	776	231
2001	996	20	818	198
2002	986	43	839	190
2003	1028	29	931	126
2004	1048	92	892	248
2005	1152	52	1078	126
2006	1131	116	898	349
2007	1157	146	934	369
2008	1202	119	1006	315
2009	1234	87	963	358
2010	1306	23	1020	309

Source: Author's calculations

Table 2. Timing of Elections Relative to Node Reassignment, by Board District and Year

Year of Draft Reassignment Plan	Two Elections Prior	Election Prior	First Election After	Second Election After	Third Election After
	Board districts 1, 2, 7, 9				
1999	NA	NA	2001	2005	2009
2000	NA	NA	2001	2005	2009
2001	NA	NA	2001	2005	2009
2002	NA	2001	2005	2009	NA
2003	NA	2001	2005	2009	NA
2004	NA	2001	2005	2009	NA
2005	NA	2001	2005	2009	NA
2006	2001	2005	2009	NA	NA
2007	2001	2005	2009	NA	NA
2008	2001	2005	2009	NA	NA
2009	2001	2005	2009	NA	NA
	Board districts 3, 4, 5, 6, 8				
1999	NA	NA	NA	2003	2007
2000	NA	NA	2003	2007	NA
2001	NA	NA	2003	2007	NA
2002	NA	NA	2003	2007	NA
2003	NA	NA	2003	2007	NA
2004	NA	2003	2007	NA	NA
2005	NA	2003	2007	NA	NA
2006	NA	2003	2007	NA	NA
2007	NA	2003	2007	NA	NA
2008	2003	2007	NA	NA	NA
2009	2003	2007	NA	NA	NA

Table 3. Summary Statistics

Variable	Mean
Voted	0.118
2001 Election	0.134
2003 Election	0.178
2005 Election	0.200
2007 Election	0.234
2009 Election	0.253
Age	45.806
Democratic Party	0.420
Republican Party	0.348
Unaffiliated/Other Party	0.232
Black	0.174
White	0.773
Other Race	0.053
Female	0.539
Male	0.461
Winning Candidate %	66.674
Winning Candidate Votes	4102.082
Incumbent Involved	0.460
Incumbent Won	0.304
Two Elections Prior to Reassignment	0.020
Election Prior to Reassignment	0.091
First Election After Reassignment	0.164
Second Election After Reassignment	0.119
Third Election After Reassignment	0.050

Source: Author's calculations

Table 4. Coefficients and standard errors for indicators of election relative to node reassignment from OLS regression predicting turning out to vote in Wake County School Board election

Election Relative to Reassignment	Group							
	Full Sample	**White**	**Non-white**	**Republican**	**Unaffiliated**	**Democrat**	**Female**	**Male**
Second Election Prior	0.001	-0.001	0.006	0.009	-0.005	-0.004	0.003	-0.002
	(0.009)	(0.010)	(0.009)	(0.011)	(0.007)	(0.012)	(0.009)	(0.010)
First Election Prior	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED
First Election Post	0.032***	0.034***	0.016***	0.034***	0.021***	0.034***	0.032***	0.032***
	(0.004)	(0.005)	(0.004)	(0.006)	(0.005)	(0.005)	(0.004)	(0.005)
		test of equality: *p*=0.001		test of equality: *p*=0.005			test of equality: *p*=0.905	
Second Election Post	0.053***	0.058***	0.025***	0.049***	0.039***	0.062***	0.053***	0.052***
	(0.008)	(0.009)	(0.006)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
		test of equality: *p*=0.000		test of equality: *p*=0.004			test of equality: *p*=0.901	
Third Election Post	0.078**	0.088***	0.023**	0.075***	0.061***	0.087***	0.079***	0.075***
	(0.011)	(0.013)	(0.010)	(0.014)	(0.012)	(0.012)	(0.012)	(0.012)
		test of equality: *p*=0.000		test of equality: *p*=0.076			test of equality: *p*=0.531	
N	1,009,331	778,726	239,605	351,076	234,074	423,705	542,736	463,862
N Nodes	1,350	1,342	1,328	1,335	1,343	1,346	1,349	1,346

Notes: *$p<0.10$; **$p<0.05$; ***$p<0.01$. Estimates in each column are from a single regression estimated via ordinary least squares (OLS). Each column presents estimated coefficients and heteroskedastic robust standard errors clustered by node (in parentheses below coefficient) for indicators of election relative to a node's first full elementary reassignment. In addition to the indicators presented in the table, each regression contained indicators for four or more years prior to the node's first full elementary reassignment and four or more years after that reassignment. The regression also contained full sets of indicators for a node's potential second and third full elementary reassignments, as well as fixed effects for node and election.

Table 5. Estimated average treatment effects on the treated and standard errors for node reassignment from heterogeneous treatment effects regression predicting turning out to vote in Wake County School Board election, by reassignment cohort and election

	Election			
Cohort	**2003**	**2005**	**2007**	**2009**
2005	NA	0.040***	NA	0.055***
		(0.008)		(0.011)
2007	0.008	NA	0.028***	NA
	(0.046)	0	(0.007)	
2009	NA	0.007	NA	0.021***
		(0.010)	0	(0.005)
N	855,481			
N Cohorts	4			
N- Never treated	616,045			
N- 2005 cohort	87,640			
N- 2007 cohort	67,023			
N- 2009 cohort	84,773			

Notes: *p<0.10; **p<0.05; ***p<0.01. Results from a single regression predicting turning out to vote in a Wake County School Board election using the estimator presented in Callaway and Sant'Anna (2021). Each cell presents the estimated average treatment effect on the treated (ATET) for a node's first full elementary reassignment for a given reassignment cohort in a particular election. Standard error clustered by node in parentheses below ATET estimate. In addition to the treatment measure for a node's first elementary reassignment, each regression also contained variables indicating the number of years after a node's potential second and third full elementary reassignments.

Table 6. Estimated average treatment effects on the treated and standard errors for node reassignment from heterogeneous treatment effects regression predicting turning out to vote in Wake County School Board election, by sample group and election

	Group							
Election	**Full Sample**	**White**	**Non-white**	**Republican**	**Unaffiliated**	**Democrat**	**Female**	**Male**
Election prior to reassignment	0.007	0.010	0.012	0.014	0.010	-0.007	0.002	0.013
	(0.019)	(0.020)	(0.010)	(0.014)	(0.010)	(0.038)	(0.022)	(0.017)
First election after reassignment	0.029***	0.028***	0.015***	0.029***	0.016***	0.031***	0.030***	0.027***
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Second election after reassignment	0.055***	0.060***	0.024***	0.059***	0.034***	0.063***	0.057***	0.054***
	(0.011)	(0.012)	(0.007)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
N	855,481	665,180	190,119	297,132	195,124	362,136	459,010	393,928
N Cohorts	4	4	4	4	4	4	4	4
N- Never treated	616,045	494,925	121,209	217,822	141,677	256,311	329,153	285,425
N- 2005 cohort	87,640	58,269	29,216	27,226	18,781	41,266	47,860	39,146
N- 2007 cohort	67,023	43,675	23,251	19,787	14,683	32,455	36,774	30,099
N- 2009 cohort	84,773	68,311	16,443	32,297	19,983	32,104	45,223	39,258

Notes: *$p<0.10$; **$p<0.05$; ***$p<0.01$. Results in each column from a single regression predicting turning out to vote in a Wake County School Board election using the estimator presented in Callaway and Sant'Anna (2021). Each cell presents the estimated average treatment effect on the treated (ATET) for a node's first full elementary reassignment for a particular election relative to reassignment. Standard error clustered by node in parentheses below ATET estimate. In addition to the treatment measure for a node's first elementary reassignment, each regression also contained variables indicating the number of years after a node's potential second and third full elementary reassignments.

Table 7. Number of Full Elementary Reassignments Relative to Timing of Next School Board Election

Year of Node Reassignment	Election Year	Election in One Year	Election in Two Years	Election in Three Years
2000	0	24	0	52
2001	13	0	12	0
2002	0	25	0	17
2003	29	0	1	0
2004	0	54	0	35
2005	33	0	17	0
2006	0	57	0	57
2007	79	0	64	0
2008	0	47	0	NA
2009	59	0	NA	NA
2010	0	NA	NA	NA

Source: Author's calculations

Table 8. Coefficients and standard errors for indicators of time relative to node reassignment from OLS regression predicting turning out to vote in Wake County School Board election

Years Relative to Draft Reassignment Plan	Group							
	Full Sample	White	Non-white	Republican	Unaffiliated	Democrat	Female	Male
3 Years Prior	0.002	0.004	-0.006	0.003	0.004	0.001	-0.001	0.006
	(0.009)	(0.009)	(0.010)	(0.011)	(0.009)	(0.010)	(0.009)	(0.009)
2 Years Prior	0.000	-0.004	0.000	-0.007	0.006	0.001	-0.006	0.007
	(0.010)	(0.011)	(0.010)	(0.012)	(0.008)	(0.012)	(0.010)	(0.010)
1 Year Prior	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED
Year of Draft Reassignment	0.023**	0.017*	0.020**	0.017	0.020**	0.026**	0.020**	0.025***
	(0.009)	(0.010)	(0.009)	(0.012)	(0.009)	(0.010)	(0.009)	(0.009)
1 Year Post	0.029***	0.030***	0.015	0.028**	0.026***	0.031***	0.025***	0.035***
	(0.008)	(0.009)	(0.010)	(0.011)	(0.008)	(0.010)	(0.009)	(0.009)
2 Years Post	0.023**	0.021**	0.011	0.023*	0.017**	0.024**	0.020**	0.027***
	(0.009)	(0.010)	(0.009)	(0.012)	(0.008)	(0.011)	(0.009)	(0.009)
3 Years Post	0.034***	0.034***	0.020**	0.032***	0.017***	0.041***	0.031***	0.037***
	(0.006)	(0.007)	(0.008)	(0.008)	(0.006)	(0.008)	(0.007)	(0.006)
N	1,003,944	776,781	227,163	349,597	232,733	421,144	539,235	461,997
N Nodes	1,350	1,341	1,328	1,333	1,343	1,346	1,349	1,346

Notes: *p<0.10; **p<0.05; ***p<0.01. Estimates in each column are from a single regression estimated via ordinary least squares (OLS). Each column presents estimated coefficients and heteroskedastic robust standard errors clustered by node (in parentheses below coefficient) for indicators of time relative to a node's first full elementary reassignment. In addition to the indicators presented in the table, each regression contained indicators for four or more years prior to the node's first full elementary reassignment and four or more years after that reassignment. The regression also contained full sets of indicators for a node's potential second and third full elementary reassignments, as well as fixed effects for node and election.

Table 9. Coefficients and standard errors for indicators of election relative to node reassignment from OLS regression containing voter fixed effect predicting turning out to vote in Wake County School Board election

Election Relative to Reassignment	Group							
	Full Sample	**White**	**Non-white**	**Republican**	**Unaffiliated**	**Democrat**	**Female**	**Male**
Second Election Prior	0.006	0.005	0.002	0.016	-0.000	-0.002	0.007	0.004
	(0.011)	(0.012)	(0.010)	(0.013)	(0.009)	(0.014)	(0.011)	(0.013)
First Election Prior	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED
First Election Post	0.015***	0.014***	0.010***	0.016***	0.011**	0.017***	0.014***	0.016***
	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Second Election Post	0.018***	0.017**	0.013**	0.011	0.018**	0.027***	0.017**	0.019***
	(0.006)	(0.007)	(0.005)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)
Third Election Post	0.033***	0.038***	0.005	0.028**	0.037***	0.038***	0.034***	0.033***
	(0.010)	(0.012)	(0.009)	(0.013)	(0.011)	(0.012)	(0.010)	(0.011)
N	808,020	641,392	166,293	287,863	160,051	335,466	435,630	371,171
N Nodes	1,350	1,339	1,328	1,328	1,333	1,341	1,348	1,345

Notes: *$p<0.10$; **$p<0.05$; ***$p<0.01$. Estimates in each column are from a single regression estimated via ordinary least squares (OLS). Each column presents estimated coefficients and heteroskedastic robust standard errors clustered by node (in parentheses below coefficient) for indicators of election relative to a node's first full elementary reassignment. In addition to the indicators presented in the table, each regression contained full sets of indicators for a node's potential second and third full elementary reassignments, as well as fixed effects for voter and election.

Table 10. Coefficients and standard errors for indicators of election relative to node reassignment from OLS regression predicting a voter's observed residential relocation and registering to vote during the 2000s

	Observed Move			Registered During 2000s		
	Full Sample	**White**	**Non-white**	**Full Sample**	**White**	**Non-white**
Second Election Prior	0.005	0.005	0.005	-0.007	-0.011	-0.004
	(0.006)	(0.006)	(0.013)	(0.011)	(0.011)	(0.017)
First Election Prior	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED
First Election Post	-0.005	-0.007	0.012	0.022***	0.018***	-0.006
	(0.005)	(0.005)	(0.008)	(0.006)	(0.006)	(0.008)
Second Election Post	-0.001	-0.010	0.053***	0.043***	0.039***	-0.012
	(0.009)	(0.009)	(0.013)	(0.010)	(0.011)	(0.010)
Third Election Post	-0.009	-0.022*	0.064***	0.041**	0.037**	-0.019
	(0.014)	(0.013)	(0.020)	(0.017)	(0.018)	(0.017)
N	1,009,331	778,726	230,605	1,009,331	778,726	230,605
N Nodes	1,350	1,342	1,328	1,350	1,342	1,328

Notes: $*p<0.10$; $**p<0.05$; $***p<0.01$. Estimates in each column are from a single regression estimated via ordinary least squares (OLS). Each column presents estimated coefficients and heteroskedastic robust standard errors clustered by node (in parentheses below coefficient) for indicators of election relative to a node's first full elementary reassignment. In addition to the indicators presented in the table, each regression contained full sets of indicators for a node's potential second and third full elementary reassignments, as well as fixed effects for election.

Table 11. Coefficients and standard errors for indicators of election relative to node reassignment from OLS regression predicting turning out to vote in Wake County School Board election

Election Relative to Reassignment	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Second Election Prior	0.001	0.001	0.003	0.001	0.004
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
First Election Prior	OMITTED	OMITTED	OMITTED	OMITTED	OMITTED
First Election Post	0.033***	0.033***	0.031***	0.031***	0.032***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Second Election Post	0.053***	0.054***	0.052***	0.050***	0.050***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Third Election Post	0.078***	0.079***	0.077***	0.074***	0.073***
	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)
New school indicator	X				X
Distance to base elementary school		X			X
Mean school achievement			X		X
School racial/ethnic composition				X	X
N	1,009,331	997,290	1,009,331	1,009,331	997,290
N Nodes	1,350	1,340	1,350	1,350	1,340

Notes: *p<0.10; **p<0.05; ***p<0.01. Estimates in each column are from a single regression estimated via ordinary least squares (OLS). Each column presents estimated coefficients and heteroskedastic robust standard errors clustered by node (in parentheses below coefficient) for indicators of election relative to a node's first full elementary reassignment. In addition to the indicators presented in the table and the measures denoted in the bottom panel of the table, each regression contained full sets of indicators for a node's potential second and third full elementary reassignments, as well as fixed effects for node and election.

Table 12. Heterogeneity in the effect of reassignment on voter turnout in the first post-reassignment election by the racial and ethnic composition of a node's base elementary school, full sample and by voter race

	School Racial/Ethnic Composition					
	Proportion Asian students	**Proportion Hispanic students**	**Proportion Black students**	**Proportion white students**	**Distance**	**Mean School Ach.**
	Full Sample					
School characteristic	0.029	0.081***	-0.029	-0.015	0.001	0.025***
	(0.040)	(0.031)	(0.022)	(0.017)	(0.001)	(0.009)
First election post	0.031***	0.036***	0.026***	0.035***	0.026***	0.031***
	(0.005)	(0.007)	(0.008)	(0.007)	(0.006)	(0.004)
School characteristic X	0.041	-0.027	0.023	-0.004	0.002*	-0.006
First election post	(0.078)	(0.031)	(0.019)	(0.013)	(0.001)	(0.010)
N	983,410	983,410	983,410	983,410	997,290	1,009,331
N Nodes	1,314	1,314	1,314	1,314	1,340	1,350
	White Voters					
School characteristic	0.045	0.078**	-0.033	-0.013	0.000	0.031**
	(0.048)	(0.037)	(0.026)	(0.020)	(0.010)	(0.009)
First election post	0.033***	0.036***	0.023**	0.042***	0.031***	0.033***
	(0.006)	(0.008)	(0.009)	(0.009)	(0.008)	(0.005)
School characteristic X	0.043	-0.017	0.041*	-0.014	0.001	-0.006
First election post	(0.094)	(0.036)	(0.024)	(0.016)	(0.002)	(0.012)
N	758,883	758,883	758,883	758,883	769,938	778,726
N Nodes	1,306	1,306	1,306	1,306	1,332	1,342
	Non-white Voters					
School characteristic	0.001	0.072***	-0.017	-0.013	0.000	0.011
	(0.033)	(0.024)	(0.017)	(0.015)	(0.001)	(0.007)
First election post	0.012***	0.028***	0.023***	0.006	0.009	0.015***
	(0.005)	(0.007)	(0.008)	(0.007)	(0.005)	(0.004)
School characteristic X	0.100	-0.081**	-0.021	0.023	0.002*	0.001
First election post	(0.069)	(0.034)	(0.019)	(0.015)	(0.001)	(0.009)
N	224,527	224,527	224,527	224,527	227,352	230,605
N Nodes	1,295	1,295	1,295	1,295	1,317	1,328

Notes: *p<0.10; **p<0.05; ***p<0.01. In each panel, estimates in each column are from a single regression estimated via ordinary least squares (OLS). Each column presents estimated coefficients and heteroskedastic robust standard errors clustered by node (in parentheses below coefficient) for main effects and interaction between the measure of school racial composition indicated in each column header and an indicator for the first election following reassignment. In addition to these measures, the regressions also contained indicators for a node's second election prior to reassignment as well as the second and third elections following reassignment. The regression also contained full sets of indicators for a node's potential second and third full elementary reassignments, as well as fixed effects for node and election.