THE LONG-RUN EFFECTS OF SCHOOL RACIAL DIVERSITY ON POLITICAL IDENTITY

Stephen B. Billings
Eric Chyn
Kareem Haggag

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ABSTRACT

How do early-life experiences shape political identity? In this paper, we study how a shock to the social lives of youth affected their party affiliation in adulthood. Specifically, we examine the end of race-based busing in Charlotte-Mecklenburg schools (CMS), an event that led to large changes in school racial composition. Using linked administrative data, we compare party affiliation for students who had lived on opposite sides of newly drawn school boundaries. We find that a 10-percentage point increase in the share of minorities in a student's assigned school decreased their likelihood of registering as a Republican by 8.8 percent. Consistent with the contact hypothesis, this impact is entirely driven by white students (a 12 percent decrease). This effect size is roughly 16 percent of the correlation between parents and their children's party affiliations. Finally, consistent with this change reflecting underlying partisan identity, we find no significant effect on voter registration likelihood. Together these results suggest that schools in childhood play an important role in shaping partisanship.

Stephen B. Billings
University of Colorado
Department of Finance
Leeds School of Business
Boulder, CO 80309
stephen.billings@colorado.edu

Eric Chyn
Department of Economics
Dartmouth College
6106 Rockefeller Hall
Hanover, NH 03755
and NBER
eric.t.chyn@dartmouth.edu

Kareem Haggag
Social and Decision Sciences
Carnegie Mellon University
Porter Hall 208-H
5000 Forbes Avenue
Pittsburgh, PA 15213
kareem.haggag@cmu.edu

1   Introduction

Political partisanship shapes the way individuals see and interact with the world. In the U.S. context, partisanship strongly predicts a range of political opinions and economic perceptions (Bartels, 2000; Gerber and Huber, 2010; Lenz, 2012; Achen and Bartels, 2016). Moreover, recent studies have provided evidence that partisan identity has causal effects on both political behavior and attitudes (Gerber et al., 2010; McConnell et al., 2017; Barber and Pope, 2019).

What are the origins of partisanship? Leading theories posit that childhood experiences play a key role in determining political identity and behavior. Numerous descriptive analyses provide evidence consistent with this hypothesis (Campbell et al., 1960; Hess and Torney, 1967; Jennings and Niemi, 1968; Green et al., 2004; Jennings et al., 2009; Stoker and Bass, 2011). Yet, a lingering concern is that correlational evidence of a link between partisanship and childhood circumstances may largely reflect the influence of omitted variables. For example, exposure to minorities or peers from a different social class during one's youth could reflect unmeasured attitudes or preferences that affect later-life political identity.

In this paper, we use a natural experiment to study the effects of a sudden and important shock to the social lives of youth on their subsequent partisan identities in adulthood. In 2002, Charlotte-Mecklenburg Schools (CMS) ended race-based busing and redrew existing school boundaries. This reform generated large changes in the racial composition of schools. Motivated by a large literature on how exposure and contact with racial out-groups can shape attitudes (Key, 1949; Allport, 1954) and work on the link between racial attitudes and political affiliation (Sears and Kinder, 1971), we ask whether this change to racial context in childhood can affect one's political party affiliation more than a decade later in life.

To identify causal impacts, we compare students who lived in the same neighborhoods and had pre-reform addresses that placed them on different sides of a newly-drawn school boundary. Our approach follows prior work studying schooling and arrest outcomes in this context (Billings et al., 2014) and estimates the effect of being assigned to a school with a higher share of minority students after conditioning on fixed effects for small geographic areas (i.e., Census block groups). This approach reveals causal effects if children on either side of the new boundary are similar prior to the redrawing. Institutional features of our setting support the plausibility of this assumption,

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and we provide statistical evidence showing no evidence of systematic sorting within neighborhoods based on the new school boundaries.

Our analysis of long-run political preferences relies on student-level administrative records linked to statewide voting records from North Carolina. Because children may move from North Carolina later in life, we also link our sample to voting records from Virginia and South Carolina. Our sample consists of 35,988 elementary and middle school students who were enrolled in CMS prior to school reassignments. All voting records are current as of 2018, when the average student in our sample was 28 years old. We present estimate for all students, and separately for white and for racial minority students.

We find that being assigned to schools with higher shares of minorities significantly impacts later-life political preferences for white students, with no impacts on minority students. Specifically, we find that a 10-percentage point increase in the share of minorities in a white student's assigned school decreases their likelihood of being a registered as a Republican by 2 percentage points (a 12 percent decrease relative to the 16.4 percent Republican registration rate).[1,2] These effects are not driven by detectable changes in voting registration.[3] We find suggestive evidence that white students are more likely to be registered as a Democratic or as an unaffiliated voter. For minority students, we find small and statistically insignificant impacts on party affiliation.

To provide a sense of magnitude, we compare the estimated effects of school minority exposure to the correlation of partisan identities of parent with that of their children. A large literature on political socialization has posited parental transmission as the central driver of partisan identity (Campbell et al., 1960; Hess and Torney, 1967). For example, Jennings et al. (2009) find a self-reported party identification correlation of 0.37 between parents and children in their mid-20s. We provide a novel estimate of this intergenerational relationship (previously documented in surveys)

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[1]The omitted category in this specification is composed of students that remained unregistered (43 percent of white students overall), registered as Democrat (14 percent of white students overall), registered as Unaffiliated (26 percent), or as another one of the three officially recognized parties (less than 1 percent). We refer to the latter two categories (Unaffiliated or third party) as "Other" in our analysis.

[2]While our results are robust to conditioning on voter registration status, we do not condition on registration in our primary specification, since this measure is a post-treatment outcome, and thus conditioning on it could create significant bias (Angrist and Pischke, 2009; Nyhan et al., 2017; Montgomery et al., 2018).

[3]We find that a 10-percentage point increase in the share of minorities in a white students' assigned school decreases their likelihood of being registered to vote by a statistically insignificant 0.75 percentage points (p-value = 0.39). If we assume that these unregistered voters had similar underlying partisan identities to white students who were registered voters (i.e. 29 percent Republican, 24 percent Democrat and 48 percent Unaffiliated or another party), then we would expect 11 percent of the 2 percentage point decrease in likelihood of being a Republican would be due to the decrease in registration.

by focusing on the subsample of children who can be matched to a parent as recorded in birth records and then matching these parents to their voting records. In this subsample, we find that having a parent registered as a Republican is associated with a 15.6 percentage point increase in the likelihood that their child is registered as a Republican. This implies that a 10-percentage point increase in the share of minorities in one's assigned school has an impact on Republican partisan identity that is roughly 16 percent of the intergenerational transmission correlation.

While intergroup exposure is a leading mechanism in our context, there is at least one alternative channel that may also be present. Specifically, prior research in our setting suggests that the effects on partisanship could be mediated by the impact of re-zoning on academic achievement. Billings et al. (2014) found that changes in school racial composition due to CMS re-zoning have statistically significant impacts on college attendance for non-minority students. We replicate this analysis using our sample of white students and find that a 10-percentage point increase in the share minority reduces the likelihood of college attendance by 2.45 percentage points (p-value< 0.10). To assess the relevance of this mediator for our partisanship analysis, we look to the existing literature for guidance on the relationship between college attendance and partisanship. On the one hand, the changes in social environment that occur with college enrollment could liberalize attitudes (Nie et al., 1996; Dee, 2004). This case implies that our reduced form estimates understate any decrease in Republican party affiliation due to changes in racial or social attitudes. On the other hand, education may increase future income (Acemoglu and Angrist, 2001), thereby causing individuals to support lower taxation (Meltzer and Richard, 1981; Mendelberg et al., 2017). For example, Marshall (2019) finds that increases in high school education reduce support for the Democratic party; suggesting that the negative effects we find could potentially be mediated through reductions in education. However, a back of the envelope calculation based on this evidence suggests that at most 0.37 percentage points (19 percent) of our main estimates could be explained through changes in college enrollment. Hence, we believe it's plausible that the impacts of increased minority exposure in our setting largely operated by changing (unobserved) attitudes around race and social class.

Overall, our analysis contributes to three main literatures. First, we contribute to studies of the role of early-life and pre-adult factors in determining political behavior and party affiliation. The bulk of this literature is largely based on descriptive studies on the importance social influences such as parents or institutional influences such as schools (Campbell et al., 1960; Hess and Torney, 1967;

Jennings and Niemi, 1968; Green et al., 2004; Jennings et al., 2009; Stoker and Bass, 2011). Recent work has innovated by placing greater emphasis on identification of causal impacts. For example, several studies have employed quasi-experimental methods and found that changes in family income (Akee et al., 2018) and neighborhood relocation (Chyn and Haggag, 2019) have important impacts on later-life voting.[4] Relatively fewer studies have produced credible estimates of causal impacts of early-life conditions on partisanship. One exception is Healy and Malhotra (2013). They use an approach based on random variation in sibling gender and find that young men who have sisters (rather than brothers) are more likely to identify as Republicans. Our findings complement their analysis by demonstrating that partisanship can also be shaped by factors outside of the household.

Second, we contribute to studies of exposure to diversity and social behavior. While a link between school segregation and black-white attitudes has long been suggested (Schofield, 1991), there are relatively few studies that provide empirical evidence of a causal relationship. Recently, Rao (2019) studied intergroup contact in Indian schools and found that increased contact across castes generates short-run increases in tolerance and out-group friendship.[5] In the U.S., Carrell et al. (2019) provide evidence that intergroup contact reduces prejudice by showing that white students who are randomly assigned a black roommate in their freshmen year are more likely to choose a black roommate in subsequent years. This paper complements these findings by demonstrating that a large-scale school policy change that affected intergroup contact can generate long-lasting changes in political preferences.[6]

Last, we contribute to an important literature studying the effects of segregation and school integration policies. Prior studies have focused on estimating effects on student-level academic and economic outcomes (Guryan, 2004; Ashenfelter et al., 2006; Reber, 2010; Johnson, 2011; Billings et al., 2014; Tuttle, 2019; Bergman, 2020). To the best of our knowledge, we are the first to provide

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[4]Several recent studies have relied on randomized control trials to estimate the impact of childhood development interventions. For example, Sondheimer and Green (2010) pooled results from three randomized education interventions find that an increased graduation rates impacts voting outcomes. Holbein (2017) studies an experiment that improved non-cognitive skills and finds impacts on long-run political participation. Our work is distinguished in that the treatment we study does not directly target the skills of children.

[5]Lowe (2019) finds similar impacts when studying caste-based intergroup contact for cricket players in India. He further breaks down the type of contact, and finds, consistent with contact theory Allport (1954), that these positive effects are limited to collaborative contact, with opposite effects produced by adversarial contact.

[6]A few recent studies also examine intergroup contact outside of a schooling context. For example, Bazzi et al. (2019) studied a population resettlement program in Indonesia and found evidence that intergroup contact shapes nation-building. Brown et al. (2020) studied the impact of having a black neighbor during childhood and find important impacts on long-run partisanship using an approach that relies on geographic fixed effects to address concern over selection bias.

credible estimates of the impact of school segregation on political preferences.

2 Background

2.1 Charlotte-Mecklenburg Schools, New School Zones, and the End of Race-based Busing

Since the Supreme Court's 1971 Swann v. Charlotte-Mecklenburg Schools decision, CMS had operated under a racial desegregation order. Following the Court order, school zones in CMS were redrawn to capture non-contiguous areas with different racial compositions. School officials aimed to keep each school's percent black within 15 percentage points of the district average. Racial balance was preserved by using "satellite" zones that bused students from inner-city neighborhoods with high shares of minority students to schools located in suburban, highly white neighborhoods.

For the 2002-2003 academic year, CMS drew new school boundaries and committed to ending race-based busing. These changes stemmed from a series of court battles. In 1997, a CMS parent sued because their child was denied entrance to a magnet program based on race. This case led to a reopening of Swann in 1999, and the CMS desegregation plan was ultimately dismissed (United States District Court, 1999). CMS drew new school boundaries for the 2002-2003 year as contiguous areas around schools, and families were assigned to their neighborhood school by default. Although neighborhood schools were set as the default, families could also apply to attend other schools in the district.[7] Enrollment was subject to capacity constraints, and schools that were oversubscribed had lotteries to determine admission (Hastings and Weinstein, 2008; Deming, 2011).

Figure 1 provides an illustration of the change in school boundaries as a result of the new policy. The top and bottom panels show two school zones in the academic year before and after the 2002 redrawing, respectively. The top panel shows that the zone for Northeast Middle School, a 37 percent minority share school district, included satellites. The bottom panel shows that this zone was changed in two ways for the 2002-2003 academic year: CMS eliminated the satellite zones and redrew the boundary of neighboring Albemarle Road Middle School to reduce overcrowding. Note that the decisions on boundaries were based on enrollment projections and did not rely on natural geographic features. In addition, CMS did not redraw zones based on standard census geographies. For example, the top panel of Figure 1 shows that the Census block group containing

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[7]This neighborhood-based school choice policy was known as the "Family Choice Plan" (FCP).

the Northeast Middle School was previously undivided. The bottom panel shows that the redrawing led some students living in this block group to be reassigned to Albemarle Road Middle School.

The new boundaries and end of busing led to a notable increase in segregation at CMS schools. Figure 2 shows that the proportion of schools that have high share of minority students (over 85 percent) increased notably starting in the 2002-2003 academic year. Correspondingly, the proportion of schools that have a relatively integrated student body – between 35 to 85 percent – decreased.

2.2 Expected Effects of CMS Re-Zoning and School Segregation on Political Preferences

The rezoning of school boundaries and changes in racial composition at CMS schools could impact later-life partisanship through the channel of intergroup contact. This section briefly reviews prior research that sheds light on this hypothesized mechanism. We also discuss prior studies related to other mediators that may operate in our setting.

Three strands of literature provide strong evidence that exposure to diversity during childhood can have impacts on subsequent adult partisanship. First, recent studies have provided causal evidence that circumstances and early-life events matter for political behavior.[8,9] For example, studies have shown long-run effects on voting due to education-related interventions (Sondheimer and Green, 2010; Holbein, 2017), income transfers (Akee et al., 2018) and relocating to less disadvantaged neighborhoods (Chyn and Haggag, 2019).[10] Second, a large literature has studied the "contact hypothesis" (Allport, 1954) which posits that social contact with out-group members can reduce discrimination.[11] In a meta-analysis of 515 studies, Pettigrew and Tropp (2006) find that

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[8]A long prior literature has highlighted a key role for adolescence in the formation of social and political beliefs. For example, one version of this folk wisdom is the "impressionable years hypothesis" which puts forward the idea that the socializing influences experienced in youth have an enduring impact on thinking throughout one's life (Greenstein, 1965; Krosnick and Alwin, 1989). With respect to politics, there has been evidence consistent with this idea in showing that interest in politics is relatively stable and resistant to intervention after late adolescence (Prior, 2010, 2018), bolstering the claim that, "the more important a political orientation is in the behavior of adults, the earlier it will be found in the learning of the child" (Greenstein, 1965).

[9]More generally, prior research has shown that interventions and conditions during childhood have large causal impacts on long-run educational, health and economic outcomes (Garces et al., 2002; Chetty et al., 2011; Heckman et al., 2013; Chetty et al., 2016; Hoynes et al., 2016; Carrell et al., 2018; Chyn, 2018; Bald et al., 2019; Chyn et al., 2019).

[10]This credible causal evidence is more broadly consistent with correlational evidence that suggests a link between early-life circumstances and political behavior (Campbell et al., 1960; Hess and Torney, 1967; Jennings and Niemi, 1968; Green et al., 2004; Jennings et al., 2009; Stoker and Bass, 2011).

[11]While much of the literature on contact hypothesis has studied adults, the work on prejudice reduction interventions has been focused on children. As Paluck et al. (2019) note, "What we know about prejudice reduction comes largely from studies of children or young adults...no studies of ethnic or racial contact include participants over 25 years of age."

intergroup contact typically reduces measures of prejudice. Third, racial attitudes are tightly linked to party identification in the American context, with self-reported racial attitudes being strongly correlated with partisan identity (Valentino and Sears, 2005).[12,13] For example, based on data from the Cooperative Congressional Election Study's 2016 wave, Appendix Figure A1 shows that Republicans are less likely to agree with the statement that whites have certain advantages because of their skin color when compared to Democrats and Independent voters.

Yet, while the literature suggests that exposure to diversity is likely to generate effects, it is unclear whether we should expect positive or negative impacts on Republican party affiliation. Notably, evidence for prejudice reduction—in line with the contact hypothesis—is strongest in contexts in which cooperation is encouraged and there is a common goal supported by authorities (Lowe, 2019). It is less obvious what effects exposure will have in a more general setting such as schools where there may be both cooperative and adversarial contact between groups. Further, the "racial threat" hypothesis (Key, 1949) posits that whites' proximity to black communities can trigger hostile racial attitudes, as whites perceive their status as threatened. Several studies have found evidence consistent with this threat hypothesis, including Enos (2014), Enos et al. (2019), Condra and Linardi (2019), Hangartner et al. (2019). In a related vein, recent studies of the political economy of immigration have found that influxes of immigrants tend to increase support for far-right political groups (Halla et al., 2017; Dustmann et al., 2019; Tabellini, 2020).[14]

Aside from the channel of intergroup contact, the CMS reforms that we study may also have effects on partisanship driven by impacts on other student outcomes that mediate political behavior. Billings et al. (2014) found that the change in school racial composition at CMS schools significantly reduced college attendance for non-minority students and increased arrest rates for minority students. Prior studies have connected both education and criminal justice outcomes to

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[12]Studies within political science and economics have examined the historical connection between racial attitudes and party affiliation. For example, one central topic of inquiry has been the historical exodus of white Southerners from the Democratic party to the Republican party in the 1960s; with several papers using qualitative and quantitative methods to establish the primacy of racial attitudes in that shift (Carmines and Stimson, 1989; Kousser, 2010; Kuziemko and Washington, 2018).

[13]As noted by Hutchings and Valentino (2004) in their review of the role of race in American politics, the "ensuing debate over the impact of racial attitudes on policy preferences has been among the most contentious in all of public opinion research." While there are concerns that such measures reflect distinct ideological commitments that would exist regardless of race, it remains relatively uncontroversial that they predict partisan identity.

[14]Some other recent papers have found more nuanced effects, for example Mayda et al. (2018) find that increases in high-skilled (low-skilled) immigrants are associated with decreases (increases) in support for Republicans. Relatedly, Steinmayr (2020) finds that exposure to refugees passing through border municipalities in Germany increases support for the far-right party, but that sustained contact through hosting municipalities decreases support for the far-right.

political behavior. For the former, the literature has suggested that college could have negative or positive impacts on conservative attitudes of students. On one hand, attending college could liberalize attitudes due to a change in social environment (Nie et al., 1996; Dee, 2004). On the other hand, the increase in income associated with higher education could cause individuals to support lower taxation (Meltzer and Richard, 1981; Mendelberg et al., 2017). Given these ambiguous predictions, it is unclear whether we should expect the reduction in college enrollment to mediate increases or decreases in Republican affiliation among the non-minority students in our setting. For crime outcomes, recent studies provide credible evidence that arrests reduce the likelihood of voting (Gerber et al., 2017; White, 2019), but to the best of our knowledge there are no studies that shed light on a connection to party affiliation.

Overall, prior research suggests impacts on partisanship are likely in our setting, but there is little guidance on the direction of expected effects. Our reduced form analysis provides an estimate of how partisanship is impacted by the combined forces of intergroup exposure and changes in other student outcomes such as changes in education or criminal background. In Section 5, we conduct several exercises to explore the extent to which our estimates reflect the impact of intergroup exposure versus other mediators.

3 Data and Sample

We use records from multiple administrative sources to analyze the impact of school segregation on long-run voting outcomes. Specifically, we use records on all CMS students for the academic years 1998-1999 through 2010-2011 to create a sample of children affected by the 2002-2003 redrawing of school zone boundaries. The data include information on student demographics (e.g., gender, race) and measures of academic outcomes such as state test scores (grades 3-8), absences and suspensions. The data also include information on home address in each academic year. We link the sample of CMS students to voting records from North Carolina, Virginia and South Carolina to obtain measures of political party affiliation and voting behavior. The remainder of this section describes our sample, and Appendix B provides detailed discussion of the data construction.

The sample for our analysis is restricted to CMS students in grades 3-8 who were enrolled in the academic the baseline academic year 2001-2002, the period immediately before the change in

school zones and the end of busing.[15] By focusing on the year preceding the changes, the sample definition is unrelated to any changes in enrollment that resulted from the policy reform. We focus on grades 3-8 since this restriction ensures that students have test scores in the academic year in the baseline period. The sample is also defined by requiring that all students have name and address information. The name information is necessary for linking to voting outcomes, while the address information is necessary to assign students to schools before and after school boundaries changed. Further, we also use the address information to assign pre-reform neighborhoods. Neighborhoods are defined based on block groups within Mecklenburg County from the 2000 Census. Note that we focus on the earliest recorded address to ensure that the assigned schools and neighborhoods are not affected by the possibility that some families may respond to the boundary redrawing by moving to new neighborhoods.[16]

We matched the sample of students in the CMS records that meet our restrictions to voting records from North Carolina, Virginia and South Carolina. Since CMS students may move from North Carolina later in life, we match to Virginia and South Carolina records to guard against out-of-state attrition. All matching is based on name (first, middle and last) and year of birth information.[17,18] The voter records for North Carolina were downloaded from the North Carolina State Board of Elections website on July 27, 2019. This voter file contains turnout across various elections, as well as self-reported party affiliation recorded from the voter registration application. Registered voters may choose from one of five recognized political parties or they can choose to be unaffiliated.[19] The voting records from Virginia and South Carolina are from L2, Incorporated. The Virginia and South Carolina records are current as of January 2019. Unlike North Carolina, both Virginia and South Carolina do not register voters by party – instead L2 models party affiliation by the most recent primary in which a voter cast a partisan ballot.

In addition, we obtain information on the parents of students by matching the CMS sample to

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[15]Our sample definition differs from Billings et al. (2014) due to two main differences in the administrative data provided by CMS. First, they had access to administrative records for a longer time period (1995-1996 to 2010-2011). This implies that they observe baseline (before the policy change) test scores for more cohorts and grades of students. Second, their data contain one additional variable (free and reduced price lunch status) that is no longer made available to researchers.

[16]The earliest address for the majority (80 percent) of students is from the 1999-2000 academic year, the first year available in the CMS records.

[17]Year of birth is available in the NC voting records, but day and month of birth are not available.

[18]As detailed in Appendix B, we matched 61.4 percent of students to a voting record. Matches are unique in 99 percent of cases.

[19]Appendix Figure B1 reproduces the NC voter registration form.

birth records (1990-2001) from North Carolina. Matching is done using information on name, date of birth information and address information. We match any identified parent to voting records using the same procedure that we used for children. This matched sample of children and parents permits us to examine the intergenerational transmission of party affiliation. We use this supplemental analysis to benchmark our main estimated impacts in Section 5.[20]

Our main analysis sample consists of 35,988 CMS students that meet our sample criteria. Appendix Table A1 provides summary statistics. In our sample, 48 percent of students are black, and five percent of students are Hispanic. In terms of the CMS reforms, we see that these had considerable impact, given that 47 percent of students in our sample were assigned to new schools starting in the 2002-2003 academic year.

More generally, the students in our sample are representative of students from large urban school districts in the Southern United States. Just before the end of the school busing policy, CMS was the 25th largest school district, which placed it just behind the Memphis City School District and larger than Fulton County (Atlanta) (U.S. Department of Education, 2000). In terms of race, the non-white share for our sample is broadly comparable to racial enrollment statistics for other Southern school districts.

4   Empirical Strategy

We study the effects of school segregation by relying on plausibly exogenous variation in exposure to minorities generated by a CMS reform in the early 2000s. As discussed in Section 2, CMS drew new school boundaries and ended race-based busing for the 2002-2003 academic year. Two consequences of these reforms are key to our research design. First, school segregation increased markedly (see Figure 2). Second, students who lived in the same neighborhoods often found themselves living on opposite sides of newly drawn boundaries for schools that had very different racial compositions.[21] To the extent that children and households on either side of the boundary were comparable before the CMS reforms, the subsequent difference in student outcomes can be at-

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[20]See Appendix B for further details on matching students to birth records to obtain information on parents. In our final sample of 35,988 CMS students, 46.32 percent are linked to a parent overall, and 47.4 percent of white students are linked. We focus on white students for our benchmark exercise.

[21]Appendix Figure A2 shows the distribution of changes in the percent minority students between each student's new and old school zone address separately for white and minority students. These results show there was considerable variation with some white and minority students being assigned to schools that have very different racial composition relative to their prior attended school.

tributed to variation in the characteristics of the assigned school such as the fraction of minority students.

Formally, we follow Billings et al. (2014) by using the following neighborhood fixed effects approach to study the impact of minority exposure in a student's newly assigned school:

$$y_{izj} = \beta_0 PctMinority_{izj} + X'_{izj}\beta_1 + \eta_{zj} + \epsilon_{izj}, \tag{1}$$

where y_{izj} is an outcome (e.g., later-life political affiliation) for student i living in old school zone z and neighborhood j is regressed on the student's new school zone percent minority ($PctMinority_{izj}$), a set of covariates measured prior to CMS reforms (e.g., test scores) to improve precision, and an old school-zone-by-neighborhood fixed effect (η_{zj}). As discussed in Section 3, we define neighborhoods based on block groups using the earliest pre-reform address in CMS records.[22] We focus on addresses recorded prior to the reform to ensure that the assigned school treatment variable is unrelated to any possible sorting of households in the post-reform years. We cluster standard errors at the old school-zone-by-neighborhood level.

The key parameter from Equation 1 is the reduced-form parameter β_0, which captures the impact of changes in school racial composition (and other factors correlated with this schooling characteristic). Given the inclusion of old school-zone-by-neighborhood fixed effects, the effect of racial composition is identified by comparing children who live on opposite sides of a newly drawn boundary. In neighborhoods where there are no new school boundaries, $PctMinority_{izj}$ will have the same value for all students and will not contribute to the estimation of β_0. Given that we focus on narrowly defined neighborhoods – i.e., block groups – our design is similar to the boundary discontinuity approaches utilized in Black (1999) and Bayer et al. (2007).

As noted by Billings et al. (2014), there are a number of possible ways to characterize the "treatment" of being assigned to a new school due to changes in school boundaries. Equation 1 defines the treatment in terms of school racial composition because the policy reform ended the main policy tool for maintaining racially-integrated schools (race-based busing). Given that minority students tend to live in households with lower income and achievement, we could have conceptually defined the treatment in terms of changes in school peer test scores or parental incomes. Billings et al. (2014)

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[22]This approach to assigning children to neighborhoods follows Billings et al. (2014). A majority (80 percent) of students have an address recorded in the first year in the available CMS records (i.e., the 1999-2000 academic year).

demonstrate that results from models using these alternative measures of treatment are similar to their preferred model that focuses on racial composition. In this sense, the research design embodied in Equation 1 cannot separate the effect of changes in racial composition from other correlated school characteristics. More generally, the mechanisms by which school racial environment impacts partisan identity involve a number of social and institutional attributes that include but are not limited to direct friendships, peer and teacher role models, teacher and administrator instruction and discipline, as well as extracurricular activities and interaction outside school hours. We are also unable to separately identify the role of these specific factors. This is a common feature of other studies of school racial segregation (Guryan, 2004; Lutz, 2011; Johnson, 2011; Billings et al., 2014). That said, our empirical strategy is policy-relevant given that school segregation reforms have often relied on changing school boundaries.

Estimates of β_0 have a causal interpretation if the redrawing of school boundaries was unrelated to student and household characteristics. The institutional background in our setting suggests this assumption is plausible. As noted in Billings et al. (2014), CMS decisions about where to draw boundaries were governed by school capacity constraints and enrollment projections. There was no explicit consideration of resident preferences or characteristics. Further, while households in the CMS system may have known of the segregation-related court proceedings in the late 1990s and early 2000s, they were unlikely to be able to predict the location of a new school boundary within a narrowly defined neighborhood in the years before the 2002-2003 academic year. Finally, the next subsection provides empirical support for this assumption by showing that student characteristics measured prior to the reform are uncorrelated with the share of minorities at one's assigned school.

4.1   Checks for Non-Random Sorting of Students

The validity of our research design depends on whether students are systematically different on either side of newly drawn school boundaries. To provide evidence on this assumption, we test whether student observed characteristics such as race or prior test scores are correlated with the independent variable of interest, the assigned school share of minorities. We do this by estimating a model where the dependent variable is $PctMinority_{izj}$ and the independent variables in the model include student characteristics and fixed effects for old school by neighborhood fixed effects.

Table 1 shows that we find no evidence of sorting across newly drawn school boundaries. Col-

umn 1 reports results using the all students in our sample of CMS students. The point estimates are not statistically significant and consistently small in magnitude. Columns 2-4 shows that we obtain similar results in the subsamples of students who are registered voters, whites, or minorities, respectively. Overall, we conclude that students living on either side of the school boundary in the same small neighborhood are similar in terms of pre-reform characteristics.

4.2 Impacts of School Assignment on Attended School Characteristics and Enrollment

The interpretation of our analysis depends on whether new school assignments affect the actual characteristics of the schools attended by CMS students in our sample. Following Billings et al. (2014), we present evidence of a "first stage" by analyzing the effect of the assigned school minority share on the actual shares of schools attended by students in the 2002-2003 academic year (the first academic year after re-zoning). Table 2 presents results for this outcome based on Equation 1. Importantly, note that our main sample contains 35,988 students who are enrolled in CMS in 2001, and the results in Table 2 are estimated using the sample of 35,272 (98 percent) students who enrolled in the first year after the reforms.[23]

While these results show evidence of a relevant first stage, it is worth noting that coefficients of less than 1 indicates imperfect compliance with newly drawn school boundaries. This can occur in two main ways. First, we use the earliest address in the CMS records to assign schools to mitigate concerns over student sorting. At the same time, this practice implies that we have measurement error in assigned schools because families move over time and may not be in the same residence by the 2002-2003 school year. Second, while neighborhood-based schools were the default, CMS policy allowed families to choose schools other than the one they were assigned.

5   Main Results

In this section, we examine the effects of the school segregation natural experiment on partisan identity 15 years later. Specifically, in line with Equation 1, Table 3 reports estimates of the effects of increases in the assigned share of minorities in one's school in 2003 on one's political party

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[23]As shown in Appendix Table A2, there is no detectable relationship between our main treatment variable, the share of minority students in one's assigned school, and the likelihood of enrolling in CMS in the 2002-2003 academic year.

affiliation in 2018.[24] We begin by examining our primary outcome in Columns 1-4; an indicator for whether the individual is registered as a Republican in 2018. In Column 1, using the pooled sample of all CMS students, we find that a 10-percentage point increase in the share minority causes a 0.70 point decrease in the likelihood of being a registered Republican (p-value< 0.01). Relative to the Republican registration rate of 8 percent, this reflects an 8.8 percent increase. In Column 2, we condition on the post-treatment variable of registration (an issue we will return to below) and find a similar treatment effect of a 0.97 percentage point (a 7.5 percent increase relative to the Republican registration rate in this registered subsample).[25]

Both contact and racial threat theories suggest that the appropriate focus is on how one group's attitudes or identity respond to a change in the presence of an *out-group*. We therefore make a fairly coarse split of students into white versus minority racial groups. In line with this focus, we find no effect of increasing the share minority on minority students (Column 4), and a much larger effect on white students (Column 3) – a 10 percentage point increase in the percent minority causes a 1.95 percentage point decrease in the likelihood of being a registered Republican. Relative to the 16.4 percent Republican registration rate of the white student sample, this reflects a 12 percent decrease.

Our interpretation of the decrease in the Republican registration rate is that contact with more minority students in school reduces the Republican partisan identity of white students. However, the overall decrease comes from several potential margins. First, and perhaps most consistent with our interpretation, in Columns 5 and 6 we see an increase in both the likelihood of registering as a Democrat (0.68 percentage points) and as Unaffiliated (0.50 percentage points). While neither of these estimates is statistically significant, they are directionally consistent with our hypothesis of contact weakening Republican partisanship.

One important threat to our interpretation lies in the fact that registration is optional in the U.S., and thus the effects on party affiliation may partially reflect a decrease in registration likelihood. While it's plausible that a reduction in registering itself reflects a reduction in the strength of one's partisanship, it's also possible that it simply reflects a reduction in political

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[24]Appendix Table A3 reports results from a modified version of Equation 1 where we omit controls for pre-reform student characteristics. These results are very similar to those that we obtain from our preferred specification.

[25]Angrist and Pischke (2009) and Montgomery et al. (2018) discuss the bias induced by conditioning on post-treatment outcomes.

participation while holding constant one's latent Republican partisan identity. To investigate this possibility, in Table 4 we estimate the effects on Registration for white students. In Column 1 we see that a 10-percentage point increase in the share minority has a statistically insignificant negative impact on Registration of 0.76 percentage points. If we make the unrealistic assumption that all of the individuals induced not to Register are latent Republicans, this would suggest that roughly 38.9 percent (i.e., 0.76/1.95) of the treatment effect could be explained by demobilization. Making a more realistic assumption that the latent identities of these unregistered voters look similar to the composition of the Registered sample (i.e. 29 percent Republican) would instead suggest that demobilization only explains 11 percent of the treatment effect (i.e., $(.29 \times 0.76)/1.95$). Columns 2-6 show a slight reduction in voting likelihoods of similar magnitude to the reduction in registration, but all estimates are similarly statistically insignificant.

To provide a sense of the magnitude of our findings in Table 3, we look at how it compares to a one of the primary theorized determinants of party affiliation – the partisan identity of one's parents (Campbell et al., 1960; Jennings and Niemi, 1968). For example, Jennings et al. (2009) use a 7-point Party Identification survey measure and find a correlation of 0.37 between parents and their children in their late 20s in 1997 (they also find a correlation of 0.33 in a binary self-report of Presidential vote choice). While two recent studies estimate the intergenerational correlation in voter turnout using voter files (Akee et al., 2018; Chyn and Haggag, 2019), to our knowledge, there are no estimates of intergenerational partisan identity transmission using administrative data. As described in Section 3, we matched children to a parent using North Carolina birth records. Column 1 shows that we were able to make this match for 47.4 percent of the white student sample, and that the treatment variable (Percent Minority) does not significantly predict whether a student is matched to a parent using their birth record. Of this sample, we were able to match 61.7 percent of students' parents to the Voter File, and again Column 2 shows that treatment does not significant predict this match. In Column 3, we look at whether the policy affected parents' partisanship, and find that it does not, perhaps unsurprisingly suggesting that the policy's effects on students' later life partisanship is not mediated through their parents (consistent with this, Column 6 shows that controlling for this measure does not change our treatment effect estimate). Finally, Column 4-5 provide the parental transmission benchmarks. First, in Column 4, we re-estimate the effects of the policy on this matched subsample, and find a similar effect to the full sample – a 10 percentage

point increase in the percent minority causes a 2.5 percentage point (vs. 1.9 percentage point in the full sample) increase in the likelihood of registering as a Republican in adulthood. In Column 5, we estimate that having a Republican parent is associated with a 15.6 percentage point increase in the likelihood of registering as a Republican in adulthood – relative to the mean of 18.5 percent, this is an 84 percent increase. Thus, the effect of a 10-percentage point increase in the percent minority in one's school is roughly 16 percent of the size of the intergenerational party transmission correlation estimated in the same sample.

5.1 Mechanisms

As discussed in Section 2, a leading hypothesis is that the rezoning of school boundaries and changes in segregation in CMS schools impacted party affiliation of white students through the channel of intergroup contact. Yet, it is also possible that effects on partisanship are driven by the impact that the CMS reforms had on other student outcomes that mediate political behavior. This concern is further justified since previous work by Billings et al. (2014) found that segregation in CMS schools affected schooling outcomes for non-minority students.

To begin understanding the role of alternative mediators, Appendix Table A4 analyzes schooling attainment and arrest outcomes for white students. While our sample differs slightly from Billings et al. (2014) since we study slightly younger cohorts, we find similar results.[26] The change in minority share has no detectable effect on the crime outcomes of the white students (Column 3), but we find some negative effects on educational outcomes; we find that a 10-percentage point increase in the assigned minority share results in a 2.42 percentage point decrease (p-value< 0.10) in college attendance (Column 11).

These results suggest that we need to go further to fully evaluate the role of education as a mediator for white student party affiliation. Specifically, it's necessary to know how their reductions in education might translate into partisan identity. To our knowledge, there do not exist clean causal estimates of the effects of college attendance on party affiliation. The predominant folk wisdom dating back to at least Newcomb (1943) is that college can socialize liberal attitudes, which would presumably translate into less support for the Republican party. Since the treatment in our setting

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[26]As discussed in Section 3, our sample differs from Billings et al. (2014) due to differences in the years covered in the administrative data provided by CMS for this project. Specifically, our sample contains fewer cohorts for the analysis.

reduces the likelihood of attending college, our effects would reflect a lower bound of the effects of racial contact if college enrollment liberalizes political attitudes. However, it's quite possible the received wisdom is wrong. For example, Mendelberg et al. (2017) find suggestive evidence that attending an affluent college can increase support for conservative economic policies. Similarly, Marshall (2019) uses state drop-out laws to instrument for schooling and finds that each additional completed year of high school reduces Democratic party support by 15 percentage points. While the effects of high school and college may be quite distinct, if we assume that college attendance is equivalent to two additional years of high school attendance in expectation, then this would suggest that up to 0.37 percentage points (19 percent) of the effect could be explained through school changes (i.e., $0.37 = 15 \times .024$). While there remains uncertainty in how the decrease in education may translate into voting outcomes, the small effects suggest they are unlikely to explain much of the treatment effect we observe.

6   Conclusion

This article provides evidence that a key shock to the social lives of youth – a shift in the racial composition of their schools – caused changes in their long-run political identities. To do so, we use the end of race-based busing in the Charlotte-Mecklenburg School (CMS) district to compare neighboring students who were assigned to different schools upon the redrawing of existing school boundaries in 2002. By linking school records to administrative voter files, we estimate the effects of this natural experiment on students' eventual political party affiliations more than a decade later. We find that a 10-percentage point increase in the share of minorities in a white student's assigned school decreased their likelihood of being a registered as a Republican by 12 percent (2 percentage points), and that this impact was not driven by detectable changes in voting registration. To provide a sense of magnitude, we use the administrative data to link children to parents, providing us with a novel administrative estimate of a key correlation previously reported in surveys. We estimate that the effects of the racial composition shift (a 10 percentage point increase in share of minorities) are roughly equivalent to 16 percent of the intergenerational party affiliation correlation.

Our approach builds on the idea that intergroup racial contact can have profound effects on the social attitudes of children (Allport, 1954; Pettigrew and Tropp, 2006; Paluck et al., 2019), and

the results are consistent with this idea. However, we also explore impacts of the end of race-based busing on other outcomes that could mediate the effects on Republican party identification for white students. In particular, as the policy affected college attendance, we perform a back-of-the-envelope calculation based on existing studies of partisanship and school. Using these estimates, the evidence suggests that, at most, 19 percent of our main estimates could be explained through changes in college enrollment.

Overall, these findings complement an emerging set of studies that provide causal evidence that circumstance and events in early life affect political behavior. Recent studies have demonstrated that family income, educational interventions and neighborhoods matter for voting (Sondheimer and Green, 2010; Akee et al., 2018; Holbein, 2017; Chyn and Haggag, 2019). Our work is distinguished by the fact that we find evidence of effects on partisanship rather than voter turnout. Our analysis also builds on recent work providing causal field evidence for the contact hypothesis (Lowe, 2019; Mousa, 2019; Rao, 2019). Finally, our analysis may have implications beyond North Carolina. Hundreds of school districts were released from court-ordered desegregation during the 1990s, 2000s, and 2010s, a policy shift that has led to the gradual re-segregation of these districts (Reardon et al., 2012; Fiel and Zhang, 2019). In addition to the adverse effects of school segregation on economic outcomes (Billings et al., 2014), our estimates suggest that these policy changes could have led to important shifts in the partisan identities of Americans.

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7 Figures and Tables

Figure 1: School Rezoning Example: Before and After Boundaries for Two Middle Schools
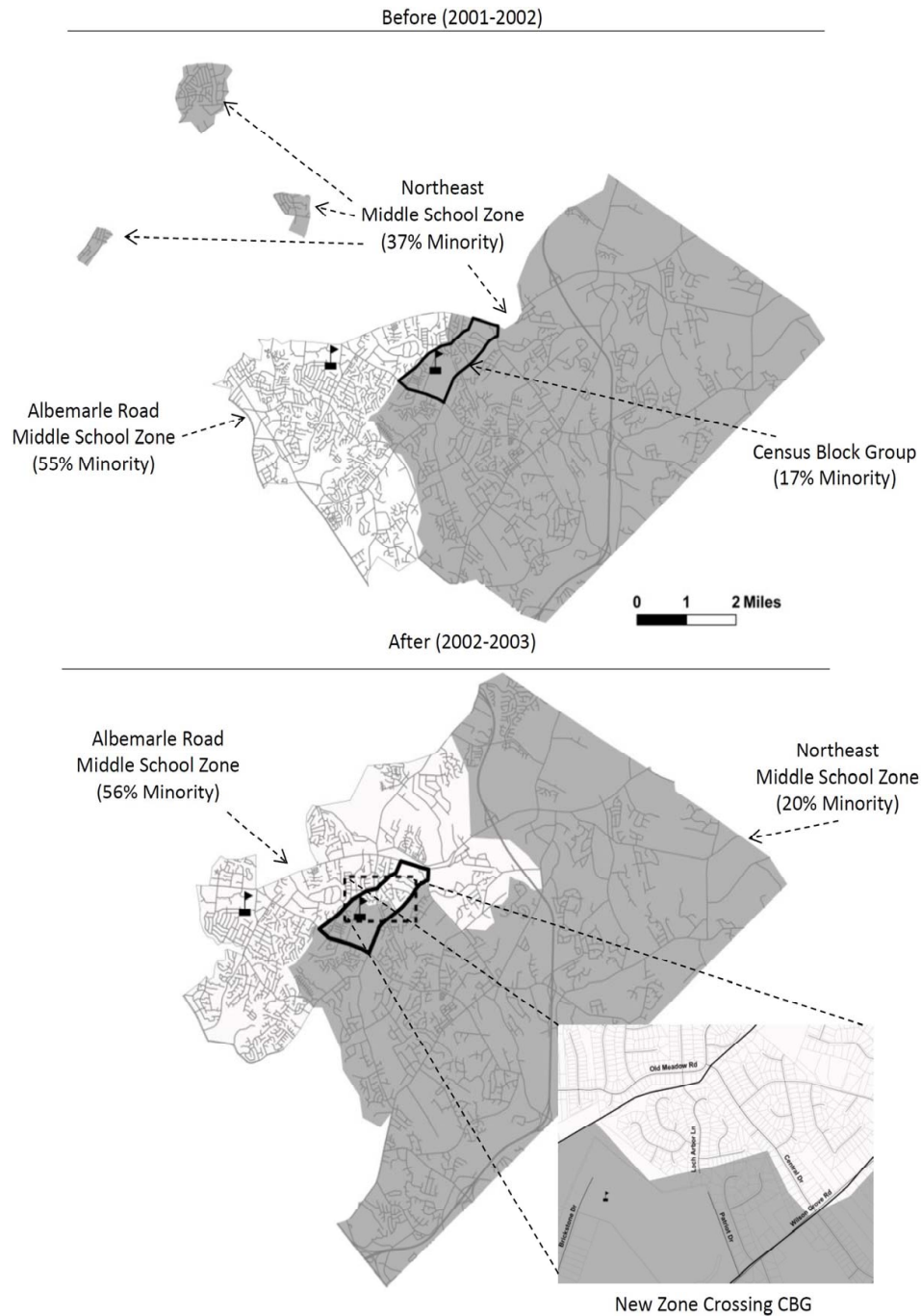
Notes: This figure illustrates rezoning for two middle schools in the Charlotte-Mecklenburg schools (CMS) district. The top panel shows the school zones in the 2001-2002 academic year, immediately before the end of school busing. The bottom panel shows the school zones in the 2002-2003 academic year. Figure is reproduced from Billings et al. (2014).

Figure 2: Effect of the 2002 Rezoning on the School-Level Minority Composition
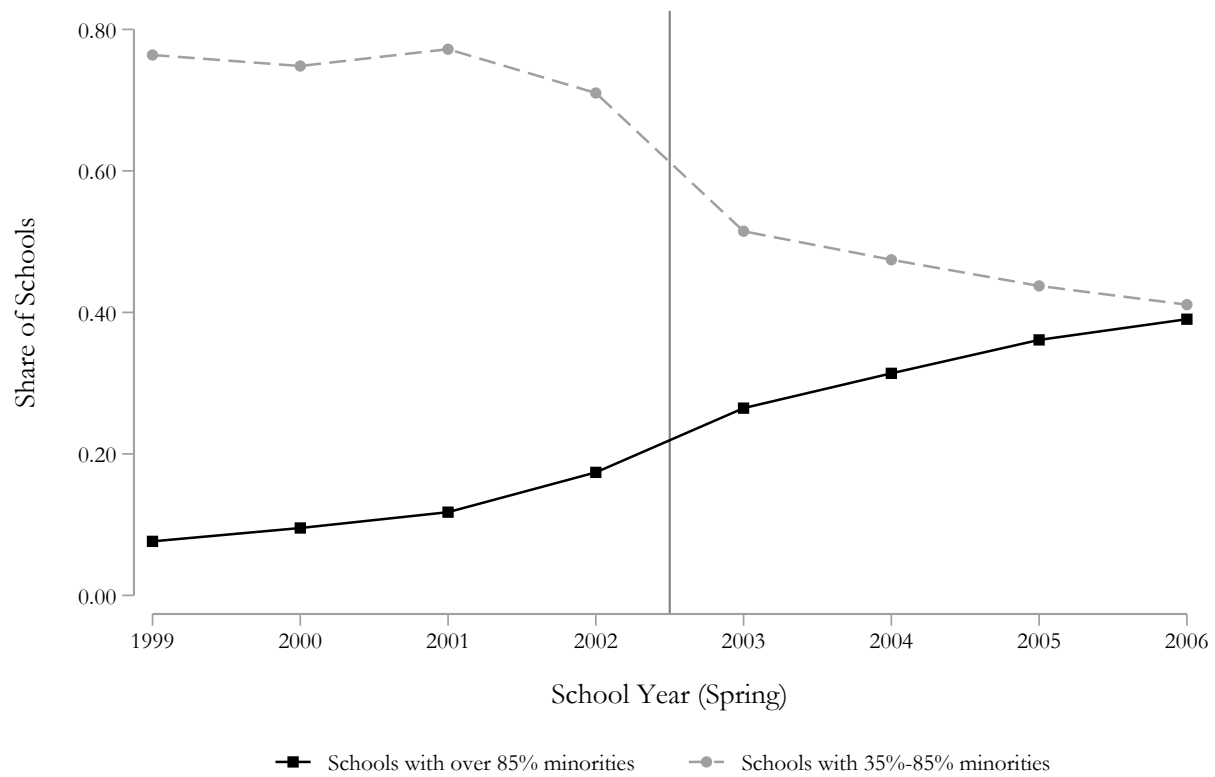
Notes: This figure reports the annual share of CMS schools that have high shares of minority students (above 85 percent) or moderate shares of minority students (between 35 and 85 percent). All elementary, middle and high schools are included in these statistics.

Table 1: Assigned School Minority Share and Student Characteristics

	Y = Assigned Percent Minority			
	(1) b/se	(2) b/se	(3) b/se	(4) b/se
pre-policy, avg. math z-score	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)
pre-policy, avg. read z-score	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
pre-policy, avg. absences	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
pre-policy, avg. suspension	0.000 (0.002)	0.003 (0.002)	0.005 (0.004)	-0.000 (0.002)
=1 if minority	0.002 (0.002)			
N	35,988	22,022	15,383	20,374
R^2	0.970	0.971	0.969	0.951
DepVarMean	0.621	0.629	0.449	0.751
All Students?	Yes	No	No	No
Registered Voters Only?	No	Yes	No	No
White Students Only?	No	No	Yes	No
Minority Students Only?	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports balance test results which examine whether the assigned school minority share (i.e., the key treatment variable of interest) is predicted by student characteristics measured in the academic years observed before the end of school busing and school boundary rezoning. All regressions include pre-reform school zone by Census block group fixed effects. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table 2: First-stage Effects of 2002 Rezoning on School Racial Composition

	Y = Attended School Percent Minority			
	(1) b/se	(2) b/se	(3) b/se	(4) b/se
Assigned Percent Minority	0.250***	0.250***	0.303***	0.220***
	(0.039)	(0.052)	(0.043)	(0.053)
N	35,272	21,611	15,030	20,011
R^2	0.582	0.587	0.616	0.314
DepVarMean	0.578	0.585	0.424	0.694
All Students?	Yes	No	No	No
Registered Voters Only?	No	Yes	No	No
White Students Only?	No	No	Yes	No
Minority Students Only?	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports point estimates and standard errors from estimating Equation 1 where the dependent variable is the share of minority students in one's school during the 2002-2003 academic year (the first year after the end of school busing and rezoning of school boundaries). All columns control for the student's pre-reform school's characteristics (average reading scores, math scores, absences, and suspensions), the student's demographics (race, gender, and school grade in 2002), and pre-reform school zone by Census block group fixed effects. The sample for this analysis is restricted to students who enrolled in a CMS school in the 2002-2003 academic year. The overall enrollment rate for the main sample is 98 percent. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table 3: Effects of 2002 Rezoning on Party Affiliation

	Y = Registered as Republican				...as Democrat	...as Other
	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Assigned Percent Minority	-0.070***	-0.097**	-0.195***	0.002	0.068	0.050
	(0.025)	(0.040)	(0.071)	(0.020)	(0.048)	(0.076)
N	35,988	22,022	15,383	20,374	15,383	15,383
R^2	0.119	0.213	0.077	0.067	0.086	0.085
DepVarMean	0.080	0.130	0.164	0.016	0.136	0.273
All Students?	Yes	No	No	No	No	No
Registered Voters Only?	No	Yes	No	No	No	No
White Students Only?	No	No	Yes	No	Yes	Yes
Minority Students Only?	No	No	No	Yes	No	No

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports point estimates and standard errors from estimating Equation 1 where the dependent variable is a measure of party affiliation obtained from voting records. In North Carolina, voters can register as Republican, Democratic, Unaffiliated, as one of the other three officially recognized parties, or those who remain unregistered. We refer to the latter two categories (Unaffiliated or third party) as "Other" in our analysis. Appendix Figure B1 reproduces the NC voter registration form. All columns control for the student's pre-reform school's characteristics (average reading scores, math scores, absences, and suspensions), the student's demographics (race, gender, and school grade in 2002), and pre-reform school zone by Census block group fixed effects. The sample for this analysis is restricted to students who enrolled in a CMS school in the 2002-2003 academic year. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table 4: Effects of 2002 Rezoning on Political Participation

	Y = Registered	Y = Vote Ever	Y = Vote, 2014	Y = Vote, 2016	Y = Vote, 2018
	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Assigned Percent Minority	-0.076	-0.116	0.058	-0.140	-0.025
	(0.088)	(0.083)	(0.064)	(0.091)	(0.072)
N	15,383	15,383	15,383	15,383	15,383
R^2	0.098	0.104	0.073	0.089	0.085
DepVarMean	0.573	0.481	0.157	0.347	0.255
All Students?	No	No	No	No	No
Registered Voters Only?	No	No	No	No	No
White Students Only?	Yes	Yes	Yes	Yes	Yes
Minority Students Only?	No	No	No	No	No

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports point estimates and standard errors from estimating Equation 1 where the dependent variable is a measure of political participation (voting) for various general election years, as recorded in the July 27, 2019 NCSEB voter file. The variable Vote Ever is a measure of voting in any general election from 2009-2018. All columns control for the student's pre-reform school's characteristics (average reading scores, math scores, absences, and suspensions), the student's demographics (race, gender, and school grade in 2002), and pre-reform school zone by Census block group fixed effects. The sample for this analysis is restricted to students who enrolled in a CMS school in the 2002-2003 academic year. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table 5: Comparing Effects of 2002 Rezoning and Parent Party Affiliation

|	Y = Matched Birth Rec.	Y = Parent Registered	Y = Parent Reg. Republican	Y = Registered as Republican		
	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Assigned Percent Minority	-0.030 (0.118)	-0.030 (0.166)	0.017 (0.157)	-0.251** (0.108)		-0.253** (0.108)
Parent Registered as Republican					0.156*** (0.012)	0.156*** (0.012)
N	15,383	7,155	7,155	7,155	7,155	7,155
R^2	0.228	0.222	0.147	0.121	0.151	0.152
DepVarMean	0.474	0.617	0.338	0.185	0.185	0.185
All Students?	No	No	No	No	No	No
Registered Voters Only?	No	No	No	No	No	No
White Students Only?	Yes	Yes	Yes	Yes	Yes	Yes
Minority Students Only?	No	No	No	No	No	No
Matched to Parents Only?	No	Yes	Yes	Yes	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Column 1 reports point estimates and standard errors from estimating Equation 1 where the dependent variable is an indicator for whether a child matched to a birth record with parent information. Columns 2-6 are restricted to students who matched to a birth record with parent information. Columns 2 and 3 report results from Equation 1 where the dependent variable is an indicator for whether one's parent is a registered voter or whether one's parent is registered as a Republican. Columns 4-6 report results from Equation 1 where the dependent variable is an indicator for whether the student is registered as a Republican. All columns control for the student's pre-reform school's characteristics (average reading scores, math scores, absences, and suspensions), the student's demographics (race, gender, and school grade in 2002), and pre-reform school zone by Census block group fixed effects. The sample for this analysis is restricted to students who enrolled in a CMS school in the 2002-2003 academic year. Standard errors are clustered at the pre-reform school zone by Census block group level.

Online Appendix

A   Appendix Figures and Tables

Figure A1: Racial Attitudes and Party Affiliation

2016 Party Affiliation and Racial Attitudes
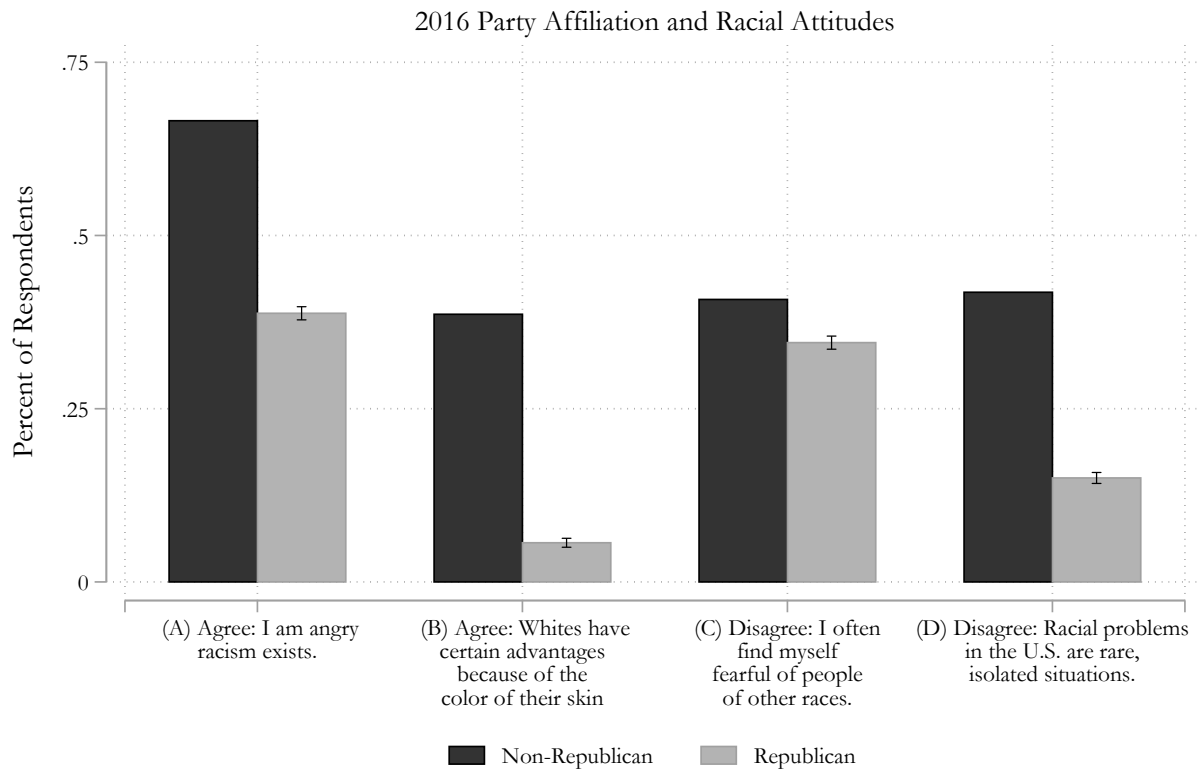
Notes: This table presents an analysis of survey questions from the Cooperative Congressional Election Study's 2016 wave. In this survey, respondents are asked to report their *agreement* on a 1-5 scale with the statements (A) "I am angry racism exists." and (B) " Whites have certain advantages because of the color of their skin." In the results above, we report the fraction of the sample that has the highest agreement (1) with these statements. Respondents are also asked to report their *disagreement* with the statements (C) "I often find myself fearful of people of other races." and (D) "Racial Problems in the U.S., are rare, isolated situations. In the results above, we report the fraction of the sample that has the highest disagreement (5) with these statements.

Figure A2: Distribution of the Change in School Minority Share After 2002 Rezoning
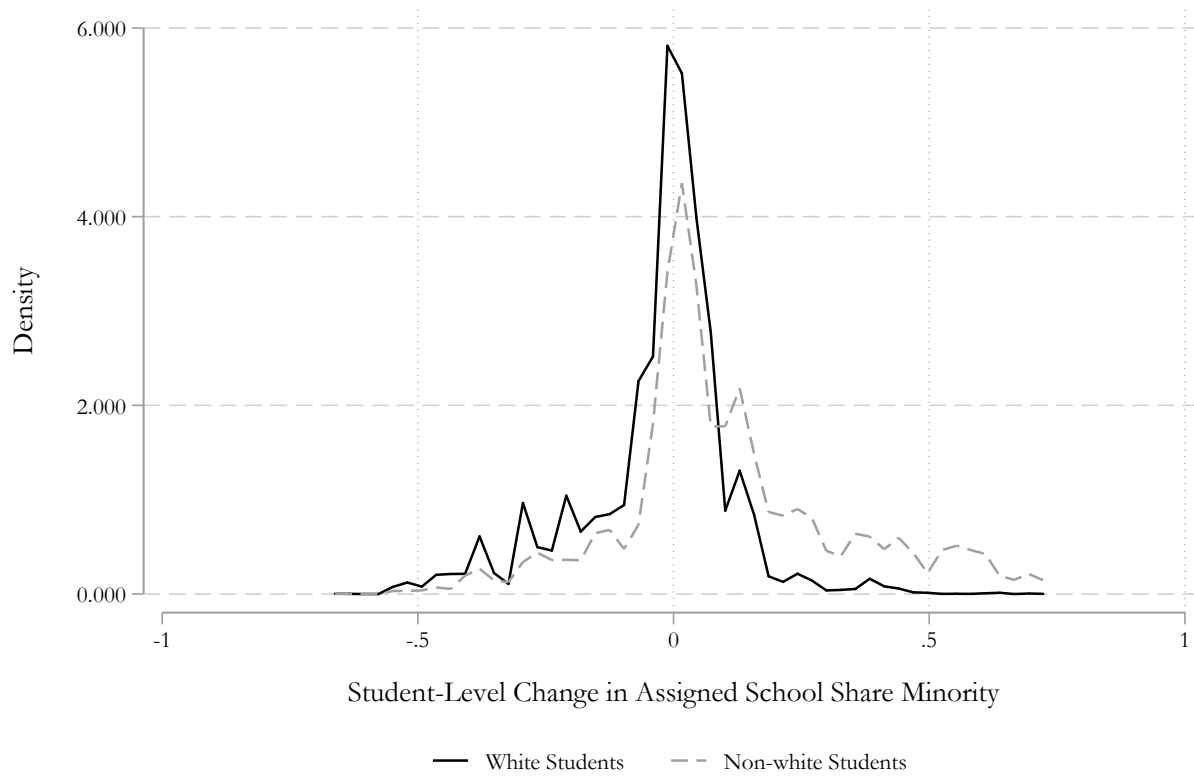
Notes: This figure displays densities for the change in school minority share for each student in our analysis sample. For each student, we compute the change as the difference between the school assigned for the 2002-2003 academic year and the school attended in the 2001-2002 academic year.

Table A1: Summary Statistics

	(1) Avg.
White	0.430
Non-white	0.570
Male	0.496
Grade (in 2002)	5.466
Avg. Math Std. Scores (Pre-policy)	-0.012
Avg. Read Std. Scores (Pre-policy)	-0.015
Avg. Absences (Pre-policy)	7.750
Avg. Suspended (Pre-policy)	0.094
Enrolled in 2003	0.980
Reassigned in 2003	0.469
Registered Voter	0.614
Voted, General Elections 2009-2018	0.499
# Voted, General Elections 2009-2018	1.377
Voted, Primary Elections 2009-2018	0.192
Voted, Any Elections 2009-2018	0.505
# Voted, Any Elections 2009-2018	1.713
Registered Republican	0.080
Registered Democrat	0.330
Registered Other	0.205
N	35,988

Notes: This table reports descriptive statistics for the main analysis sample. Measures of schooling characteristics and voting outcomes are based on administrative records from CMS and voting records, respectively. Note that registered voters in North Carolina (NC) may choose from one of five recognized political parties or they can choose to be unaffiliated. Appendix Figure B1 reproduces the NC voter registration form. We group unaffiliated voters and those who register as a third party as "Other" for our analysis. See Section 3 for further details.

Table A2: Effects of 2002 Rezoning on Enrollment in the Next Academic Year

	Y = Enrolled in 2002-2003			
	(1) b/se	(2) b/se	(3) b/se	(4) b/se
Assigned Percent Minority	0.003	0.017	0.008	0.009
	(0.014)	(0.014)	(0.033)	(0.015)
N	35,988	22,022	15,383	20,374
R^2	0.052	0.076	0.091	0.063
DepVarMean	0.980	0.981	0.977	0.982
All Students?	Yes	No	No	No
Registered Voters Only?	No	Yes	No	No
White Students Only?	No	No	Yes	No
Minority Students Only?	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports point estimates and standard errors from estimating Equation 1 where the dependent variable is an indicator for enrollment in a CMS school during the 2002-2003 academic year (the first year after the end of school busing and rezoning of school boundaries). The control variables in the model are pre-reform averages measures for reading scores, math scores, absences, suspensions and controls for demographics. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table A3: Effects of 2002 Rezoning on Party Affiliation, Robustness (No Controls)

	Y = Registered as Republican				...as Democrat	...as Other
	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Assigned Percent Minority	-0.086***	-0.140***	-0.183**	0.003	0.054	0.026
	(0.028)	(0.046)	(0.072)	(0.020)	(0.048)	(0.076)
N	35,988	22,022	15,383	20,374	15,383	15,383
R^2	0.091	0.155	0.071	0.066	0.080	0.075
DepVarMean	0.080	0.130	0.164	0.016	0.136	0.273
All Students?	Yes	No	No	No	No	No
Registered Voters Only?	No	Yes	No	No	No	No
White Students Only?	No	No	Yes	No	Yes	Yes
Minority Students Only?	No	No	No	Yes	No	No

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports point estimates and standard errors from estimating Equation 1 where the dependent variable is a measure of party affiliation obtained from voting records. In North Carolina, voters can register as Republican, Democratic, Unaffiliated, as one of the other three officially recognized parties, or those who remain unregistered. We refer to the latter two categories (Unaffiliated or third party) as "Other" in our analysis. Appendix Figure B1 reproduces the NC voter registration form. All columns control for the student's pre-reform school's characteristics (average reading scores, math scores, absences, and suspensions), the student's demographics (race, gender, and school grade in 2002), and pre-reform school zone by Census block group fixed effects. The sample for this analysis is restricted to students who enrolled in a CMS school in the 2002-2003 academic year. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table A4: Effects of 2002 Rezoning on Schooling and Criminal Outcomes

	Y = Ever Arrested				Y = HS Grad				Y = Attend College			
	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se	(7) b/se	(8) b/se	(9) b/se	(10) b/se	(11) b/se	(12) b/se
Assigned Percent Minority	0.047	0.050	-0.017	0.102*	-0.062	-0.105	-0.089	-0.083	-0.068	-0.090	-0.242*	-0.027
	(0.054)	(0.057)	(0.069)	(0.060)	(0.064)	(0.077)	(0.128)	(0.083)	(0.069)	(0.096)	(0.141)	(0.073)
N	32,131	19,507	13,695	18,196	17,545	10,485	7,610	9,780	17,545	10,485	7,610	9,780
R^2	0.186	0.190	0.134	0.186	0.274	0.291	0.285	0.276	0.296	0.307	0.310	0.285
DepVarMean	0.183	0.171	0.094	0.250	0.460	0.465	0.530	0.404	0.343	0.359	0.439	0.268
All Students?	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No
Registered Voters Only?	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
White Students Only?	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Minority Students Only?	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports point estimates and standard errors from estimating Equation 1 where the dependent variable is an indicator for ever having an arrest, high-school graduation or enrollment in a four year post-secondary institution. Standard errors are clustered at the pre-reform school zone by Census block group level.

B   Appendix on Data Construction

The analysis in this paper is based on a sample of students in the Charlotte Mecklenburg School (CMS) district linked to voting records from North Carolina, South Carolina and Virginia. The process for creating the sample consists of the following steps:

1. **Cleaning the sample of CMS students:** The sample construction process begins with administrative records that track all CMS school students from 1998-99 to 2010-11. The data include information on student demographics (e.g., gender, race), home address in each academic year, and measures of academic outcomes such as state test scores (grades 3-8), absences, and suspensions. We create an initial sample (prior to matching to voting records) of 36,487 students in grades 3-8 who were enrolled in CMS in the academic year 2001-2002, the baseline year immediately before the change in busing regime, had address and name information (which is necessary for linking to voting outcomes), and had non-missing demographic and previous academic performance measures. We focus on grades 3-8 since this restriction ensures that included students will have test scores in the academic year before the change in busing regime. We use address information based on the 2001-2002 academic year to define the main independent variable of interest. The home address information allows us to identify a student's assigned school in the academic year 2002-2003, which was the first post-busing period. The main independent variable of interest for our analysis is the minority share of students based on one's post-busing assigned school in the 2002-2003 academic year. We also link each students home address in the pre-business academic year (2001-2002) to 2000 Census geographies. We use the block groups from the 2000 Census to define neighborhoods for each student.

2. **Linking students to voting records:** We link the initial sample of 36,487 students to voting registration and history records from North Carolina (current as of July 2019), South Carolina (current as of January 2019), and Virginia (current as of January 2019). The voting records from North Carolina were downloaded from the North Carolina State Board of Elections (NCSBE) website in July 2019. The voting records for South Carolina and Virginia were obtained from L2, Incorporated. Voting data from L2 has been used in prior research (Velez

and Newman, 2019; Yoder, 2019; Enamorado et al., 2019; Chyn and Haggag, 2019). The NCSBE voting records include voter registration for the full state, as well as voter turnout in the 2009-2018 general and primary elections. The voting records from South Carolina and Virginia include voter registration for both states, as well as turnout in the 2000-2018 general and primary elections. The voting records from North Carolina contain self-reported party affiliation. Five political parties are recognized in North Carolina: Constitution, Democratic, Green, Libertarian and Republican. Registered voters may choose one of these political parties when completing a voter registration application, *or* they may choose not to register with any political party and be designated as unaffiliated. In the records from L2, there is an L2-modeled party affiliation variable.[B1] To link the voting records to the sample of CMS children, we use first name, middle initial, last name, year of birth and geography. Prior studies have used name and date of birth information to link administrative and voting records.[B2] Ideally, we would use birthday for record linking, but the North Carolina voting records only contain year of birth. We use geography in our linking process as follows. First, we link all CMS students to the North Carolina records based on first name, middle initial, last name and year of birth. Second, we match the remaining persons who fail to match to voting records from North Carolina to the records from South Carolina and Virginia. Based on the two-step linking, we retain all persons who uniquely matched to a voting record. Note that about 1 percent of the sample of children match to more than one voting record. We remove all children who have duplicate matches in the voting records. The final sample that we study contains 35,988 children who attended 107 different schools in the 2002-2003 academic year. We matched 59 percent of students to a voting record in North Carolina. We can compare this statistic to Census statistics on voter registration. In 2018, the average child in the sample is about 28 years old. Based on the November 2018 election, 65.7 percent of age 25 to 34 citizens were registered voters in North Carolina (U.S. Census, 2018). While our match rate in North Carolina is lower than the statewide voter registration rate, it is important to note that we

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[B1]The voting records from Virginia and South Carolina are from L2, Incorporated. The Virginia and South Carolina records are current as of January 2019. Unlike North Carolina, both Virginia and South Carolina do not register voters by party – instead L2 models party affiliation by the most recent primary in which a voter cast a partisan ballot.

[B2]For example, Baicker and Finkelstein (2018) use full name, date of birth and gender to link data from the Oregon Health Experiment to voting records. Akee et al. (2018) use first name, last name and date of birth to link the Great Smoky Mountains Study survey data to voting records. Holbein (2017) use first name, last name and birthday to match individuals who participated in the Fast Track intervention to voter records.

expect that some CMS children move out of North Carolina later in life. This possibility is the justification for matching the sample to voting records from Virginia and South Carolina as part of the second step in our matching process. We matched about 2 percent of the sample to a voting record in Virginia or South Carolina. Overall, we match 61.4 percent of students in our sample to a voting record.

3. **Linking students to birth records:** We linked the final sample of 35,988 students to birth records from 1990 to 2001 obtained from the North Carolina State Center for Health Statistics. The birth records include information for the names of children and their parents, the student's date of birth, and address at the time of birth information. We link the final sample to birth records in two steps. First, we link children based on their name and date of birth information. We matched 13,678 students (38 percent) using this approach. Second, for the remaining children who were not matched to birth records, we matched to the birth records based on the last name and address information. This matching approach allows us to obtain information on parents for children who were born before 1990, the first year of birth records available to us.[B3] This is because younger siblings (born 1990-2001) who share the same last name and home address will be identified in this approach, allowing us to identify parents. We matched 2,991 students (8.1 percent) using this approach.[B4] Overall, we linked 16,669 students (46.32 percent) to their parents as recorded in birth records.

[B3]Approximately 38 percent of students in the final sample are born before 1990.

[B4]Note that the results of our analysis of the intergenerational transmission of political preferences in Section 5 are robust to focusing only on the subset of students identified in birth records using full name and date of birth (i.e., omitting any student linked to parents through last name and address information alone). Appendix Table B1 reports results for the subsample of children linked to parents only using the approach based on full name and date of birth.

Figure B1: North Carolina Voter Registration Form

NORTH CAROLINA VOTER REGISTRATION APPLICATION (fields in red text are required)

2020.02

06w

1 | Indicate whether you are qualified to vote or preregister to vote based on U.S. citizenship and age.

Are you a citizen of the United States of America?
IF YOU CHECKED "NO" IN RESPONSE TO THIS CITIZENSHIP QUESTION, DO NOT SUBMIT THIS FORM. YOU ARE NOT QUALIFIED TO VOTE
☐ Yes ☐ No

Will you be at least 18 years of age on or before election day?
☐ Yes ☐ No

Are you at least 16 years of age and understand that you must be 18 years of age on or before election day to vote?
IF YOU CHECKED "NO" IN RESPONSE TO BOTH OF THESE AGE QUESTIONS, DO NOT SUBMIT THIS FORM.
YOU ARE NOT QUALIFIED TO REGISTER OR PREREGISTER TO VOTE.
☐ Yes ☐ No

2 | Provide your full legal name.

Last Name _____ Suffix _____

First Name _____

Middle Name _____

3 | Provide your date of birth and identification information.

Date of Birth (MM/DD/YYYY) ___ / ___ / ___

State or Country of Birth _____

NC Driver License or NC DMV ID Number _____

Last 4 Digits of Social Security Number _____

☐ Check if you do not have a driver license or Social Security number.

State Voter Registration Number (*Optional: To locate, check "Voter Lookup" at www.NCSBE.gov.*) _____

4 | Provide your residential address - where you physically live.
Do not enter a P.O. Box or a mail drop location.

Address Number _____ Street Name and Type _____

Address Line 2 (e.g., apartment, lot or unit number) _____

City _____ State _____ Zip Code _____

County _____

Have you lived at this address for 30 or more days? ☐ Yes ☐ No

If "No", date moved? _____

5 | Provide a mailing address.

Do you receive mail at your residential ☐ Yes ☐ No
If "No", you are required to provide a mailing address.

Mailing Address Line 1 _____

Mailing Address Line 2 _____

Mailing Address Line 3 _____

City _____ State _____ Zip Code _____

No Physical Address? If you do not have an address, use the space to the right to illustrate where you normally live or sleep. Write in the names of the nearest crossroads (or streets). Draw an **X** on the map to show where you live or usually sleep.

IMPORTANT: You should also provide a valid mailing address above to permit the board of elections to send you a voter card.

NORTH ⬆

6 | Provide your demographic information (optional).

Gender: ☐ Male ☐ Female

Ethnicity: ☐ Not Hispanic/Latino ☐ Hispanic/Latino

Race: ☐ African American/Black ☐ American Indian/Alaska Native ☐ Asian ☐ Multiracial ☐ Native Hawaiian/Pacific Islander ☐ White ☐ Other

7 | Provide your choice for political party affiliation.

☐ Democratic Party ☐ Libertarian Party ☐ Other _____
☐ Constitution Party ☐ Republican Party
☐ Green Party ☐ Unaffiliated

If you select a party that is not recognized in North Carolina, you will be registered as *Unaffiliated*.

8 | Complete if you are currently registered to vote in another NC county or in another state.
(This information will be used to cancel your previous voter registration in the other county or state.)

Notes: This figure reproduces the North Carolina state voter registration form as of 2019.

Table B1: Comparing Effects of 2002 Rezoning and Parent Party Affiliation, Robustness (Alternative Sample)

	Y = Matched Birth Rec. M1	Y = Parent Registered	Y = Parent Reg. Republican	Y = Registered as Republican		
	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Assigned Percent Minority	0.077 (0.104)	-0.119 (0.145)	0.009 (0.165)	-0.240** (0.120)		-0.241** (0.118)
Parent Registered as Republican					0.171*** (0.013)	0.171*** (0.013)
N	15,383	5,774	5,774	5,774	5,774	5,774
R^2	0.280	0.212	0.152	0.128	0.167	0.168
DepVarMean	0.379	0.751	0.414	0.189	0.189	0.189
All Students?	No	No	No	No	No	No
Registered Voters Only?	No	No	No	No	No	No
White Students Only?	Yes	Yes	Yes	Yes	Yes	Yes
Minority Students Only?	No	No	No	No	No	No
Matched to Parents Using M1?	No	Yes	Yes	Yes	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Column 1 reports point estimates and standard errors from estimating Equation 1 where the dependent variable is an indicator for whether a child was matched to a birth record with parent information using name and date of birth information only (hereafter referred to as "M1"). Columns 2-6 are restricted to students who matched to a birth record with parent information using method M1. Columns 2 and 3 report results from Equation 1 where the dependent variable is an indicator for whether one's parent is a registered voter or whether one's parent is registered as a Republican. Columns 4-6 report results from Equation 1 where the dependent variable is an indicator for whether the student is registered as a Republican. In all regressions, the control variables in the model are pre-reform averages measures for reading scores, math scores, absences, suspensions and controls for demographics. Standard errors are clustered at the pre-reform school zone by Census block group level.