

The Long-Run Effects of School Racial Diversity on Political Identity*

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Abstract

How do early-life experiences shape political identity? We examine the end of race-based busing in Charlotte-Mecklenburg schools, an event that led to large changes in school racial composition. Using administrative data, we compare party affiliation in adulthood for students who had lived on opposite sides of newly-drawn school boundaries. Consistent with the contact hypothesis, we find that a 10-percentage point increase in the share of minorities in a white student's assigned school decreased their likelihood of registering as a Republican by 2 percentage points (12 percent). Our results suggest that schools in childhood play an important role in shaping partisanship.

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Political partisanship shapes the way individuals see and interact with the world. In the U.S., 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 provide 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 (Campbell et al., 1960; Hess and Torney, 1967; Jennings and Niemi, 1968; Green et al., 2002; 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 that affect later-life political identity.

This paper provides new evidence on the determinants of partisan identity by studying the causal effects of an important and sudden shock to the social lives of youth. In 2002, the Charlotte-Mecklenburg school (CMS) district ended race-based busing and redrew school attendance boundaries. These reforms led to large changes in the racial composition of schools.

We use this setting to examine the effects of changes in the share of minorities in a student’s assigned school on their political party registration measured in adulthood. Exposure to diversity within schools could impact partisanship by influencing preferences or beliefs on race and economic policies that sharply differ between the two major U.S. political parties. For example, prior research has shown that differences in racial attitudes have played a central role in determining partisan preferences in the South since the 1960s (Carmines and Stimson, 1989; Valentino and Sears, 2005; Kousser, 2010; Kuziemko and Washington, 2018). In contemporary surveys, the average registered Democrat and Republican substantially disagree on a number of race-related issues such as the prevalence of racism (see Appendix Figure A1).

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

Our analysis is based on student-level administrative records linked to voting records. The sample consists of elementary and middle school students who were enrolled in CMS prior to school reassignments. All voting records are current as of 2019, when the average student in our sample was 29 years old. Our main analysis presents estimates separately for white and racial minority students.

We find that assignment to schools with higher shares of minorities significantly impacts the likelihood that white students register as Republicans in adulthood. A 10-percentage point increase in the share of minorities in a white student’s assigned school decreases their likelihood of being registered as a Republican by about 2 percentage points (a 12 percent decrease relative to the mean Republican registration rate).¹ These effects are robust to conditioning on registration status (a post-treatment outcome). We find suggestive evidence that white students are more likely to be registered as Democrat or Unaffiliated voters. For minority students, we find a relatively precise null on Republican and Democrat registration and an increase in Unaffiliated registration. For both white and minority students, we see little evidence of changes in voter turnout.

What mechanisms can explain our results? Intergroup contact is a key potential channel. Several theoretical frameworks provide predictions for how exposure to more minority peers may shape party affiliation. For white students, we focus on the “contact hypothesis,” which posits that meaningful contact with out-group members can reduce prejudice toward them (Allport, 1954). This theory suggests that exposure to minority peers should reduce the likelihood of registering as a Republican by weakening “racially conservative” attitudes that have been linked to support for the Republican Party.² Our results are consistent with this prediction.³ For minority students, our analysis can be interpreted through theories that link intergroup contact and social norms. Recent work in political science argues that support

¹The omitted category in this specification is composed of students registered as Democrat, as Unaffiliated, as one of three other officially recognized parties, or those who remain unregistered.

²Since minorities in our sample typically live in low-income households, it is possible that intergroup contact along the lines of economic class may decrease support for economic policies supported by the Republican Party, a point we return to in Section IV.

³Notably, our results stand in contrast to “racial threat” theory (Key, 1949). This framework 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 immigrant inflows tend to increase support for far-right political groups (Halla et al., 2017; Dustmann et al., 2019; Tabellini, 2020), though others have found more nuanced results (Mayda et al., 2018; Steinmayr, 2020).

for the Democratic Party is a well-understood norm for Black Americans, the predominant minority group in our setting (White and Laird, 2020).⁴ This theory predicts that Black children who are exposed to more Black peers in school should have a greater likelihood of adhering to this group norm, thereby increasing their likelihood of registering as Democrats. We only find weak and indirect evidence in line with this prediction.⁵

While intergroup contact is a leading mechanism in our context, two additional potential mediators have been documented in this setting. Specifically, Billings et al. (2014) found that changes in school racial composition due to CMS re-zoning significantly impacted college attendance and arrests for white and minority students, respectively. To assess the relevance of education and crime effects as mediators for the results on political behavior, we examine heterogeneity in the effects of school diversity by student age at the time of the CMS reforms. To do this, we supplement our main analysis sample with high school students. For education and arrest outcomes, we find that the negative impacts of changes in school composition are specific to older children. In contrast, the effects on party affiliation are largest for the younger children. Overall, the pattern of results suggests that impacts of school diversity on education and crime do not mediate the changes in partisanship.

Our analysis contributes to three main literatures. First, we contribute to understanding how early-life factors affect political behavior and party affiliation. The bulk of this literature provides descriptive analysis of social influences such as parents or institutional influences such as schools (e.g., Campbell et al. 1960). Recent work has innovated by placing greater emphasis on identification of causal impacts. For example, studies have found that changes in family income (Akee et al., 2018), education-related interventions (Sondheimer and Green, 2010; Holbein, 2017), and neighborhood relocation (Chyn and Haggag, 2019) have important impacts on later-life voter participation. Fewer studies have produced credible estimates of causal impacts of early-life conditions on *partisanship*. One exception is Healy and Malhotra (2013). They use random variation in sibling gender to show 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 intergroup attitudes has long been suggested (Schofield, 1991), there is relatively little causal field evidence. Recently, Rao (2019) studied a natural

⁴In our sample, 84 percent of minorities are Black.

⁵Section III discusses the interpretation of our results in more detail.

experiment in Indian schools and found that increased contact across economic status generates short-run increases in tolerance and out-group friendship. In the U.S., [Carrell et al. \(2019\)](#) finds that white students who are randomly assigned a Black roommate in their freshman year are more likely to choose a Black roommate in subsequent years. [Mousa \(2020\)](#) and [Lowe \(2019\)](#) also provide experimental evidence of positive impacts of religious-based and caste-based intergroup contact through sports. Our paper complements these findings by demonstrating that a large-scale school policy change that affected intergroup contact can generate long-lasting changes in partisanship.⁶

Last, we contribute to an important literature studying the effects of segregation and school integration policies. Several prior studies have estimated effects on student-level outcomes ([Guryan, 2004](#); [Ashenfelter et al., 2006](#); [Reber, 2010](#); [Johnson, 2011](#); [Lutz, 2011](#); [Billings et al., 2014](#); [Gordon and Reber, 2018](#); [Shen, 2018](#); [Tuttle, 2019](#); [Bergman, 2020](#)). To the best of our knowledge, we are the first to provide credible estimates of the impact of school segregation on political preferences.

I Background, Data, and Sample

A 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. Based on this Court order, CMS had drawn school zones to include 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 suburban, highly white schools.

In the summer of 2002, CMS drew new school boundaries and ended race-based busing. These changes stemmed from a series of court battles culminating in the dismissal of the desegregation plan ([United States District Court, 1999](#)). CMS drew new school boundaries for the 2002-2003 year as contiguous areas around schools, eliminating the satellite zones previously used to bus students from inner-city neighborhoods. CMS made decisions for the new boundaries based on enrollment projections and attempted to minimize the possibility of overcrowding. Attendance zones often had to deviate from using natural geographic features or standard U.S. Census geographies to avoid assignment based explicitly on race.

⁶A few other studies examine long-run effects of intergroup contact through residential location ([Bazzi et al., 2019](#); [Brown et al., 2020](#); [Goldman and Hopkins, 2020](#)).

For example, the new boundaries often cut through Census block groups. While CMS set neighborhood schools as the default, families could also apply to attend other CMS schools.⁷

The new boundaries and end of busing led to a notable increase in segregation at CMS schools between the 2001-2002 and 2002-2003 academic years. As documented in [Billings et al. \(2014\)](#), the proportion of students attending a school with a high concentration of Black students (over 65 percent) jumped from 12 to 21 percent. Correspondingly, the proportion attending a relatively integrated school (35-65 percent Black) fell from 53 to 40 percent.

B Data and Sample

We incorporate data from multiple administrative sources to study school segregation and long-run political 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, academic outcomes (i.e., state test scores in grades 3-8, absences, and suspensions), and home address in each academic year. We link the sample of CMS students to voting records from North Carolina, Virginia, and South Carolina to measure 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 analysis sample is restricted to CMS students in grades 3-8 who were enrolled in the 2001-2002 academic year (the period immediately before the change in school zones and the end of busing).⁸ By focusing on the year preceding the changes, the sample definition is unrelated to enrollment changes that resulted from the policy reform. We focus on younger children (elementary and middle school) for three reasons. First, they receive a longer period of “treatment” (i.e., the change in school racial composition) as they have more years left in public schools. Second, prior work suggests larger treatment effects on college and crime outcomes for older children, complicating interpretation of effects for this group, a point we return to in [Section IV](#). Third, a long literature has highlighted key roles for childhood and early adolescence in the formation of political and intergroup attitudes ([Greenstein, 1965](#); [Dunham et al., 2008](#)).⁹ We also require that all students have name and address information

⁷Enrollment was subject to capacity constraints.

⁸Our sample definition differs from [Billings et al. \(2014\)](#) on two main dimensions. First, they had access to administrative records for a longer time period (1995-1996 to 2010-2011). They thus observe baseline (before the policy change) test scores for more cohorts. Second, their data contain free/reduced-price lunch status, a variable that is no longer provided to researchers.

⁹Our main results are robust, though smaller in magnitude, when including high school students ([Appendix Table A6](#)).

— the former to link to voting outcomes, and the latter for assigning students to schools before and after school boundaries changed. Address information is additionally used to assign pre-reform neighborhoods. Neighborhoods are defined as Census (2000) block groups within Mecklenburg County. 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.¹⁰

We matched the sample of students that met our restrictions to voting records from North Carolina, Virginia, and South Carolina. We matched to Virginia and South Carolina records to guard against out-of-state attrition due to CMS students moving as adults. All matching is based on name (first, middle, and last) and year of birth.¹¹ As detailed in Appendix B, we matched 61 percent of students to a voting record.¹² Matches are unique in 99 percent of cases. This match rate is relatively similar to self-reported voter registration rates in the Current Population Survey’s Voting and Registration Supplement. Specifically, since the average child in our sample is 29 years old in 2019, this match rate can be compared to the self-reported registration rate of 65.7 percent for citizens between the ages of 25 to 34 in North Carolina (U.S. Census, 2018). North Carolina voter records were downloaded from the North Carolina State Board of Elections website in July 2019 and contain party affiliation (recorded from the voter registration application), as well as turnout across various elections.¹³ Registered voters may choose one of five recognized political parties or elect to be unaffiliated.¹⁴ The voting records from Virginia and South Carolina are from L2, Inc., and are current as of January 2019. Unlike North Carolina, neither Virginia nor South Carolina register voters by party – instead, L2 records affiliation using the most recent primary in which a voter cast a partisan ballot.

Our main analysis sample consists of 35,757 CMS students that meet our sample criteria. Appendix Table A1 shows that 48, 43, and 5 percent of students are Black, White, and Hispanic, respectively. The table also highlights that the CMS reforms had considerable impact, as 47 percent were assigned to new schools in the 2002-2003 academic year. For voting outcomes, 51 percent voted at least once in a national General Election (2010-2018),

¹⁰The earliest address for the majority (80 percent) of students is from the 1999-2000 academic year, the first year available in the CMS records.

¹¹Only birth year (not day or month) is available in the NC voting records.

¹²Note that we match about 2 percent of the sample to a voting record in Virginia or South Carolina.

¹³The voter registration file “is a weekly point-in-time snapshot current per the file date/time stamp” (July 20, 2019) including active, denied, and inactive registrants (as well as removed registrants who voted at least once in the prior 10 years). We use the voter history file to measure participation (NCSBE, 2020).

¹⁴Appendix Figure B1 reproduces the NC voter registration form.

and 61 percent are registered. Republican, Democrat, and Unaffiliated registration is 8, 33, and 20 percent, respectively. Note that these estimates are not conditional on race or registration status. For example, 29, 24, and 46 percent of registered white students are Republican, Democrat, and Unaffiliated, respectively. These rates are comparable to survey-based responses for young (ages 18-35), white, registered respondents (e.g., the [CCES \(2016\)](#) reports shares of 26, 39, and 33 in national data and shares of 27, 25, and 46 in North Carolina).

More generally, the sample is 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, 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 comparable to racial enrollment statistics for other Southern school districts.

II Empirical Strategy

We study the effects of school segregation by using plausibly exogenous variation in exposure to minorities stemming from the CMS reforms in the early 2000s. As discussed in [Section A](#), CMS drew new school boundaries and ended race-based busing for the 2002-2003 academic year. Two consequences are key to our research design. First, school segregation increased markedly.¹⁵ Second, students from the same neighborhoods often found themselves living on opposite sides of newly drawn boundaries for schools that had very different racial compositions.¹⁶ To the extent that children and households on either side of the boundary were comparable before the reforms, the subsequent difference in student outcomes can be attributed to variation in the characteristics of the assigned school such as the fraction of minority students.

Formally, we base our approach on [Billings et al. \(2014\)](#) by using the following neighborhood fixed effects specification:

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

where y_{izj} is an outcome (e.g., later-life political affiliation) for student i living in old school zone z and neighborhood j , regressed on the student's new school zone percent minor-

¹⁵[Billings et al. \(2014\)](#) provide detailed statistics on school composition.

¹⁶Appendix Figure [A2](#) shows the distribution of student-level changes in the percent minority students between new and old assigned schools.

ity ($PctMinority_{izj}$), a set of covariates X'_{izj} measured prior to CMS reforms to improve precision, and an old school-zone-by-neighborhood fixed effect (η_{zj}). The set of covariates includes gender, cohort fixed effects, and pre-reform measures of absences, suspensions, and second-order polynomials in math and reading test scores. As discussed in Section B, we define neighborhoods based on block groups using the earliest pre-reform address in CMS records.¹⁷ 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 coefficient from Equation 1, the reduced-form parameter β_0 , 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 . Since 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).

The validity and interpretation of our research design depends on whether students are systematically different on either side of newly drawn school boundaries, and whether students assigned to a school with more minorities complied by attending these schools. First, with respect to the issue of sorting, the institutional background suggests the redrawing of boundaries was unrelated to student and household characteristics. 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, there is no empirical evidence of student sorting: Appendix Table A2 shows that students are balanced on pre-reform characteristics across the newly formed boundaries. Second, in terms of a “first stage,” Appendix Table A3 estimates the effect of assigned school minority shares on the actual shares for schools attended in the 2002-2003 academic year (the first year after re-zoning).¹⁸ We find a statistically significant coefficient of 0.25, in line with Billings et al. (2014). While there is a strong first stage, there are two reasons why

¹⁷This assignment approach 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).

¹⁸We do not estimate instrumental variable specifications for reasons further expounded upon in Billings et al. 2014 (e.g., it would require making strong assumptions on school racial compositions of students who left CMS, as well as on how to scale exposure across different cohorts).

the estimate is less than unity. First, we use the earliest address in CMS records to assign schools due to concerns over student sorting. This practice generates measurement error in assigned schools because families move over time. Second, while neighborhood-based schools were the default, CMS policy allowed families to choose schools other than the one they were assigned.

III Main Results

This section studies impacts of the school segregation natural experiment on partisan identity and voter participation roughly 15 years later. Table 1 reports effects of increases in the share of minorities in students’ 2002-2003 assigned schools on their political party affiliation as of 2019. We begin with results for white students (Panel A). In Columns 1-3, the outcome is an indicator for whether the individual is a registered Republican. The specification used in Column 1 includes no controls for pre-reform student measures. Here, we find that a 10-percentage point increase in the share minority causes a 1.83 percentage point decrease in the likelihood of being a registered Republican (p -value <0.05).¹⁹ Relative to the Republican registration rate of 16 percent, this reflects an 11 percent decrease. Based on the specification defined in Equation 1, Column 2 shows a similar effect of 1.91 percentage points (a 12 percent increase; p -value <0.01). Finally, in Column 3, we condition on the post-treatment variable of registration and find a similar 2.75 percentage point decrease (a 10 percent decrease relative to the Republican registration rate in this registered subsample).²⁰ Columns 5-9 repeat this analysis for the remaining partisan affiliation outcomes to examine whether the decrease in Republican registration is reflected in party switching. We find suggestive increases in both the likelihood of registering as a Democrat (0.71 percentage points in Column 5) and as Unaffiliated (0.57 percentage points in Column 8). While neither estimate is statistically significant, they are directionally consistent with weakening Republican partisanship.²¹

[TABLE 1 ABOUT HERE]

¹⁹We do not find detectable heterogeneous treatment effects by gender.

²⁰ Angrist and Pischke (2009), Nyhan et al. (2017), and Montgomery et al. (2018) discuss the bias induced by conditioning on post-treatment outcomes.

²¹To provide a sense of magnitude, Appendix Table A5 compares our estimates to one of the primary theorized determinants of party affiliation—the partisan identity of one’s parents (Jennings and Niemi, 1968). For white students, the effect of a 10-percentage point increase in the share of minority students is roughly 15 percent of the size of the intergenerational correlation in registering as a Republican (estimated in the subset of our sample with birth records). Moreover, this table provides a type of placebo test, showing null effects of the treatment on parent party affiliation.

Panel B of Table 1 reports results for minority students.²² Here, we find no statistically significant impacts on the likelihood of registering with either of the two major U.S. political parties. The point estimates in Columns 2 and 5 suggest that the effects of a 10-percentage point increase in the share of assigned minority peers are small at 0.02 and -0.28 percentage points for Republican and Democratic registration, respectively. In contrast to these results, we find a significant 1.06 percentage point positive impact on the likelihood of registering as an Unaffiliated voter (p -value <0.05).

While party registration for the two major political parties is naturally translated into partisan identity, Unaffiliated registration requires further inspection. It is possible that individuals may register as Unaffiliated but still support one of the two major political parties. To examine this possibility, we study party-specific voting in the 2016 Presidential Primary Election for our sample in Appendix Figure A3.²³ Both white and minority Unaffiliated voters are much more likely to participate in a Democratic Party primary. The effect is particularly stark for minorities, where 93 percent of Unaffiliated voters who participated in any 2016 Presidential primary did so in the Democratic Party.²⁴

In contrast to partisanship, we find more limited effects on measures of political participation. Table 2 presents results separately for white (Panel A) and minority (Panel B) students. Column 1 shows statistically insignificant effects on voter registration – a 10-percentage point increase in minority share translates into a 0.67 percentage point (1 percent) *decrease* and a 0.86 percentage point (1 percent) *increase* in registration for white and minority students respectively.²⁵ Columns 2-7 examine measures of voter participation. For white students, we see a marginally significant (p -value=0.09) reduction of 1.45 percentage points in the likelihood of ever voting in a national General Election between 2010 and 2018; however, es-

²²Appendix Table A4 shows that we obtain similar results for Black students when we define the treatment in terms of their racial “out-group” (i.e., the share of non-Black students).

²³North Carolina has semi-closed primaries, where voters who are registered with a party can vote only in their party’s primary, but Unaffiliated voters can request a ballot with any party. We use the partisan ballot requested in the primary election, as recorded in the voter history file.

²⁴We find similar percentages in the 2014 (92 percent) and 2018 (94 percent) primaries.

²⁵Beyond the direct interest in mobilization, this result is important for interpreting the measured effects on party affiliation, and for thinking about potential bias from incomplete matching to the registration file. For example, while it’s plausible that reduced registration reflects weakened partisanship, it may instead reflect a reduction in mobilization while holding latent partisan identity constant. One way to investigate this latter possibility is to assume that all individuals induced not to register are latent Republicans. This would suggest that roughly 35 percent (i.e., $0.67/1.91$) of the effect on white students’ Republican affiliation could be explained by demobilization *or* matching failures. Using the more realistic assumption that the latent identities of these unregistered voters are similar those of the registered sample (i.e., 29, 24, and 48 percent as Republican, Democrat, and Unaffiliated, respectively) suggests that demobilization only explains 10 percent of the effect (i.e., $(0.29 \times 0.67)/1.91$).

timates are insignificant across each specific election. For minorities, we find an insignificant 0.45 percentage point increase in ever voting.

[TABLE 2 ABOUT HERE]

An additional question of interest is how our results would translate into election outcomes. Since *actual* vote choice for individuals is unobservable, we cannot directly calculate effects on party vote shares. However, since the effect on voter turnout in individual elections is relatively small (e.g., a suggestive 3.8 percent decrease for white students in the 2016 Election) and the effects on party affiliation are large (e.g., a 12 percent decrease in Republican affiliation), this would suggest a substantial decrease in Republican vote counts (under the assumption that individuals vote with their registered parties). Since the effect of the policy was to *decrease* exposure of white students to minorities on average, this suggests that its overall effect was to *increase* the Republican vote count among white students.

IV Mechanisms

What mechanisms drive the effects on political behavior documented in Section III? Several potential channels are relevant in our context. One leading hypothesis is rooted in intergroup contact theory (Allport, 1954). The “contact hypothesis” predicts that interaction with out-group members can reduce prejudice.²⁶ Meta-analyses of hundreds of studies largely confirm this prediction (Pettigrew and Tropp, 2006; Paluck et al., 2019). For white students, the contact hypothesis is consistent with the idea that exposure to minority peers in schools should weaken racially conservative attitudes that have been linked to support for the Republican Party. The reductions in Republican registration detected in Table 1 support this prediction.

Prior studies provide more limited guidance on the expected political effects of intergroup contact for minority students. While the contact hypothesis is stated in general terms of in-groups and out-groups, the bulk of the literature has not focused on the perspectives of minority status groups (Shelton, 2000; Paluck and Clark, 2020).²⁷ Moreover, the link

²⁶The original formulations of the contact hypothesis predicted prejudice reduction when integrated groups work cooperatively toward a common goal, under equal status, and supported by authorities, though several studies find prejudice reduction even when these scope conditions are not satisfied.

²⁷In a meta-analysis, Tropp and Pettigrew (2005) find a weaker link between prejudice reduction and intergroup contact among members of minority status groups, though this result is potentially limited by the small number of studies in this category. There is also a more recent literature examining the effects of intergroup contact on minorities’ self-reported support for collective action (e.g., protest intentions). While some studies find negative correlations between positive intergroup contact and collective action intentions (Dixon et al., 2012), others find more mixed evidence (Reimer et al., 2017; Hayward et al., 2018).

between the racial attitudes of minorities and partisanship is less well-established. That said, intergroup contact may still affect party affiliations of minorities via social norms.²⁸ For example, [White and Laird \(2020\)](#) argue that support for the Democratic Party is a norm for Black Americans, the predominant minority group in our setting. Their theory would predict that Black students who are exposed to more Black peers in school should have a greater likelihood of registering as a Democrat. We find weak and indirect evidence in line with this prediction. As shown in [Section III](#), there are no detectable impacts on Republican and Democratic Party registration and an increase in Unaffiliated registration for minorities. Partisan identity is more ambiguous for Unaffiliated registrants. However, [Section III](#) shows that Unaffiliated minorities who voted in the 2016 Presidential Primary did so with the Democratic Party. This provides some limited evidence on the political identities of Unaffiliated voters.²⁹

Aside from the channel of intergroup contact, it is possible that effects are driven by other outcomes that may mediate political behavior. [Billings et al. \(2014\)](#) found that the change in school racial composition at CMS schools significantly reduced college attendance for white students and increased arrest rates for minority students. Prior studies have connected both education and criminal justice outcomes to political behavior. For the former, the literature suggests that college could have negative or positive impacts on relevant attitudes. 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](#)). For criminal justice, 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.

To understand the role of education and crime effects as mediators, we study heterogeneity in the effects of school diversity by student age at the time of the CMS reforms. We do this by supplementing our main analysis sample with older cohorts and matching students to college-going and arrest records. In contrast to voter records, the records for college-going and arrests only cover outcomes up to the years 2009 and 2013, respectively. This implies that we can only consistently study the non-political outcomes for middle school (grades 6-8)

²⁸The social identity and norms lens could also be applied for white students ([Green et al., 2002](#)). Exposing white students to more white peers may lead them to create or adhere to a norm that the Republican party is for Whites, and thus be more likely to register as Republicans. This interpretation is also consistent with our findings.

²⁹A caveat for this descriptive analysis is that it pertains to the minority of Unaffiliated registrants who participated in the 2016 primaries (17 percent).

and high school students (grades 9-12), as we are forced to drop elementary-age students who have missing data on long-run college enrollment.³⁰

Table 3 reports estimates from a specification that allows the effects of minority share in one’s school to vary by grade cohort. Specifically, we augment Equation 1 by including an interaction between *PctMinority* and an indicator variable for whether students are in high-school prior to the CMS reforms. A key motivation for this approach is that Billings et al. (2014) suggest that the effects of school diversity on non-political outcomes were larger for older students. With this in mind, we examine whether older students also had larger changes in political behavior. A confirmatory finding would suggest that impacts on education and crime are important mechanisms in our context.

[TABLE 3 ABOUT HERE]

The pattern of results from Table 3 does not suggest that education and crime are important mechanisms. We find that the detectable impacts of school diversity on political outcomes are consistently largest for the younger students. For white students, the main effect in Column 1 shows that a 10-percentage point increase in the share of minorities decreases the likelihood of Republican registration by 1.5 percentage points (p -value<0.01) for middle school cohorts. While the results for the relevant interaction term are not statistically significant, the point estimate is positive and suggests that the decrease in registration is only 0.9 percentage points for older students. In contrast to this pattern for partisanship, the estimates for college enrollment of white students (Column 8) indicate there is no detectable impact for younger students and a significant negative effect for older students. The results for minority students tell a similar story of larger effects on political outcomes for younger children that are not in line with the pattern of effects on arrests outcomes.

Finally, while we discussed mechanisms through the lens of school racial composition, the tight link between income and race in our context suggests some room for nuance. As noted in Billings et al. (2014), there are several ways to characterize the “treatment” of being assigned to a new school due to changes in school boundaries. Our main specification defines the treatment in terms of school racial composition because the policy reform ended the main tool for maintaining racially-integrated schools (race-based busing). Since minority students tend to live in households with lower income, we could have alternatively parameterized the

³⁰For example, third graders in 2002 are the youngest in our main analysis sample. These students were predominantly born in 1992 and 1993, making them age 16 (i.e., not yet college-age) by the end of the college-enrollment data.

treatment in terms of changes in school average parental incomes.³¹ In this way, the intervention could be interpreted as exposure of relatively wealthier white students to relatively poorer minority students. Thus, it could be that an additional causal chain is intergroup contact across economic status influencing attitudes toward progressive redistribution policies that differ between the political parties. While racial attitudes themselves could affect redistributive preferences (Lee and Roemer, 2006), future work that directly measures racial and economic attitudes may be able to make headway in separating these channels.

V Conclusion

This article provides evidence that a key shock to the social lives of youth caused changes in their long-run political identities. Overall, our findings suggest that school environments play an important role in determining long-run political behavior. The reductions in white student Republican registration are consistent with emerging causal evidence demonstrating that intergroup contact can reduce prejudice (Carrell et al., 2019; Rao, 2019; Lowe, 2019; Mousa, 2020). Notably, this result does not align with other recent studies which find that exposure to diversity, under certain circumstances, can generate backlash (Enos, 2014; Halla et al., 2017; Dustmann et al., 2019; Calderon et al., 2019; Tabellini, 2020). Our contrast with these latter studies may be partially driven by our focus on children rather than adults. This would suggest that exposure to diversity earlier in life may be particularly powerful for shaping attitudes that influence political identity.

³¹Specifically, the research design embodied in Equation 1 cannot precisely separate the effect of changes in racial composition from other highly correlated school characteristics (e.g., test scores and parental income). Billings et al. (2014) show results from models using these alternative measures of treatment which are similar to models that focus on racial composition. Relatedly, the mechanisms by which school racial environment affects partisan identity may involve several social and institutional attributes that include 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 factors.

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VI Tables

Table 1: Effects of Assigned School Minority Share on Party Affiliation

	(1) Registered as Republican	(2)	(3)	(4) Registered as Democrat	(5)	(6)	(7) Registered as Unaffiliated	(8)	(9)
Panel A: White Students									
Assigned Percent Minority	-0.183** (0.072)	-0.191*** (0.068)	-0.275** (0.127)	0.054 (0.048)	0.071 (0.048)	0.135 (0.092)	0.030 (0.074)	0.057 (0.074)	0.152 (0.103)
N	15,383	15,383	8,697	15,383	15,383	8,697	15,383	15,383	8,697
R^2	0.071	0.090	0.137	0.080	0.089	0.133	0.075	0.088	0.114
DepVarMean	0.164	0.164	0.287	0.136	0.136	0.237	0.264	0.264	0.461
Controls?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Registered Voters Only?	No	No	Yes	No	No	Yes	No	No	Yes
Panel B: Minority Students									
Assigned Percent Minority	0.003 (0.020)	0.002 (0.020)	-0.013 (0.027)	-0.027 (0.084)	-0.028 (0.082)	-0.134* (0.078)	0.089* (0.048)	0.106** (0.047)	0.137* (0.071)
N	20,374	20,374	13,042	20,374	20,374	13,042	20,374	20,374	13,042
R^2	0.066	0.068	0.097	0.077	0.098	0.117	0.064	0.075	0.116
DepVarMean	0.016	0.016	0.025	0.476	0.476	0.739	0.149	0.149	0.230
Controls?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Registered Voters Only?	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table reports point estimates and standard errors from estimating a model where the dependent variable is a measure of party affiliation from voting records (e.g., an indicator variable taking on value one if the individual is registered with the Republican party, and zero otherwise). The key independent variable is the share of minority peers in the school assigned to a student in the 2002-2003 academic year. In North Carolina, voters can register as Republican, Democrat, Unaffiliated, or as one of the other three officially recognized parties. Appendix Figure B1 reproduces the NC voter registration form. Columns 1, 4 and 8 reports results from a specification that controls only for pre-reform school zone by Census block group fixed effects. Columns 2, 5, and 8 reports results that additionally control for gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. Columns 3, 6, and 9 report results for the sample of registered voters. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table 2: Effects of Assigned School Minority Share on Registration and Political Participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Registered Voter	Vote Ever	Vote 2010	Vote 2012	Vote 2014	Vote 2016	Vote 2018
Panel A: White Students							
Assigned Percent Minority	-0.067 (0.089)	-0.145* (0.084)	0.005 (0.040)	-0.133 (0.089)	0.021 (0.047)	-0.118 (0.081)	-0.044 (0.062)
N	15,383	15,383	15,383	15,383	15,383	15,383	15,383
R^2	0.111	0.117	0.086	0.103	0.081	0.097	0.093
DepVarMean	0.573	0.472	0.070	0.333	0.105	0.308	0.206
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Minority Students							
Assigned Percent Minority	0.086 (0.071)	0.045 (0.071)	-0.017 (0.037)	0.014 (0.069)	0.031 (0.046)	0.013 (0.062)	-0.018 (0.060)
N	20,374	20,374	20,374	20,374	20,374	20,374	20,374
R^2	0.091	0.106	0.082	0.099	0.084	0.106	0.104
DepVarMean	0.645	0.532	0.070	0.445	0.124	0.335	0.192
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports point estimates and standard errors from estimating a model where the dependent variable is a measure of registration or election participation. The variable “Vote Ever” is an indicator variable taking on value one if the individual voted in any national general election from 2010-2018. The key independent variable is the fraction of minority peers in the school assigned to a student in the 2002-2003 academic year. All results are based on Equation 1 which controls for pre-reform school zone by Census block group fixed effects, gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table 3: Effects of Assigned School Minority Share for Middle and High School Students

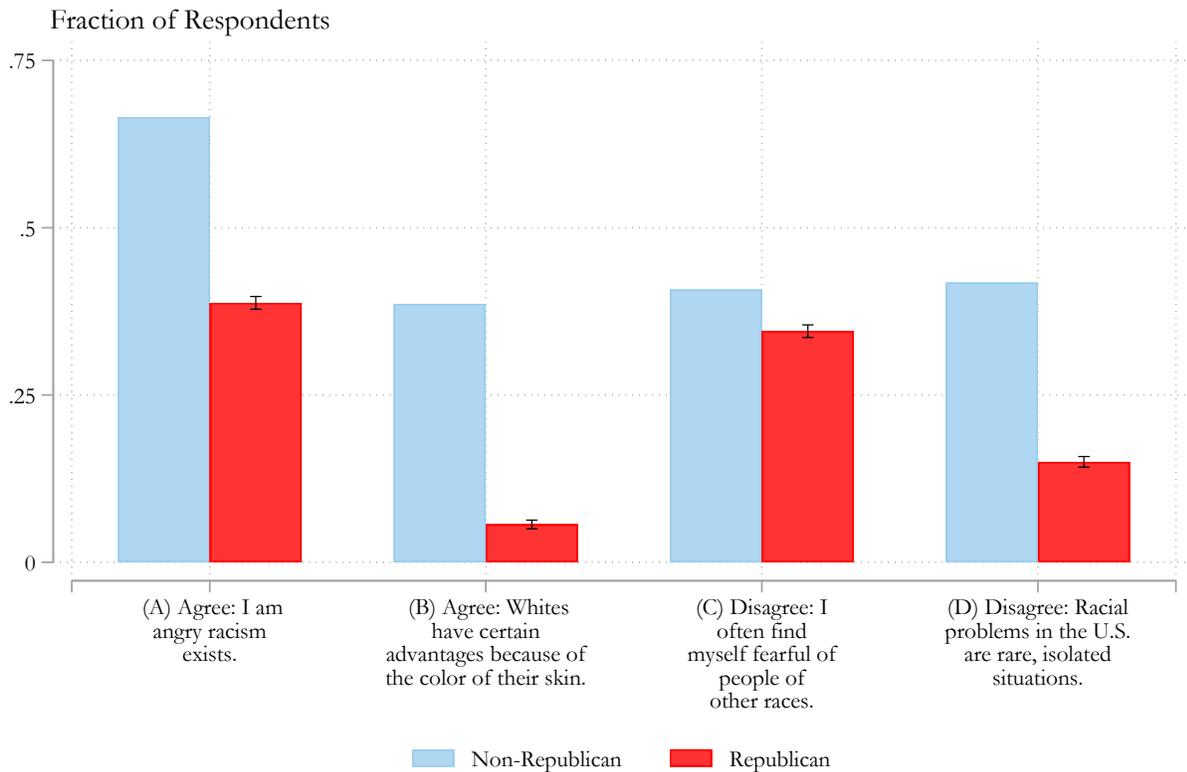
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Party Registration			Participation		Crime and Education		
	Rep.	Dem.	Una.	Reg. Voter	Vote Ever	Ever Arrested	HS Grad	Attend College
Panel A: White Students								
Assigned Percent Minority	-0.151*** (0.052)	-0.030 (0.062)	0.070 (0.069)	-0.115 (0.109)	-0.051 (0.086)	0.013 (0.056)	-0.079 (0.060)	-0.150 (0.106)
Assigned Percent Minority X High School Cohort	0.061 (0.041)	0.004 (0.041)	-0.085** (0.041)	-0.024 (0.057)	-0.010 (0.059)	0.041 (0.033)	-0.049 (0.040)	-0.099** (0.049)
N	18,684	18,684	18,684	18,684	18,684	18,684	18,684	18,684
R ²	0.069	0.055	0.068	0.103	0.101	0.145	0.444	0.336
DepVarMean	0.154	0.124	0.216	0.500	0.421	0.099	0.629	0.509
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Minority Students								
Assigned Percent Minority	0.004 (0.016)	-0.009 (0.072)	0.132*** (0.040)	0.132* (0.071)	0.057 (0.067)	-0.086 (0.052)	-0.034 (0.047)	0.011 (0.057)
Assigned Percent Minority X High School Cohort	-0.012 (0.013)	0.003 (0.049)	-0.049 (0.033)	-0.057 (0.052)	-0.059 (0.053)	0.130*** (0.038)	-0.018 (0.036)	-0.039 (0.046)
N	21,331	21,331	21,331	21,331	21,331	21,331	21,331	21,331
R ²	0.045	0.088	0.051	0.082	0.090	0.239	0.388	0.259
DepVarMean	0.015	0.446	0.123	0.586	0.490	0.253	0.478	0.294
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The analysis in this table is limited to the sub-sample of students enrolled in middle (grades 6-8) and high school (grades 9-11) in the year before CMS reforms. We use this sample because we cannot examine long-run college-going for students in elementary school at the time of the CMS reforms. The table reports point estimates and standard errors from estimating an augmented version of Equation 1 that includes an interaction term between whether a student was in high school and the fraction of minority peers in their assigned school. All results control for pre-reform school zone by Census block group fixed effects, gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. Note that we do not have data on pre-reform test scores for high-school students so we set their pre-reform test score controls to zero and include missing test score indicators in the specification. Standard errors are clustered at the pre-reform school zone by Census block group level.

Online Appendix

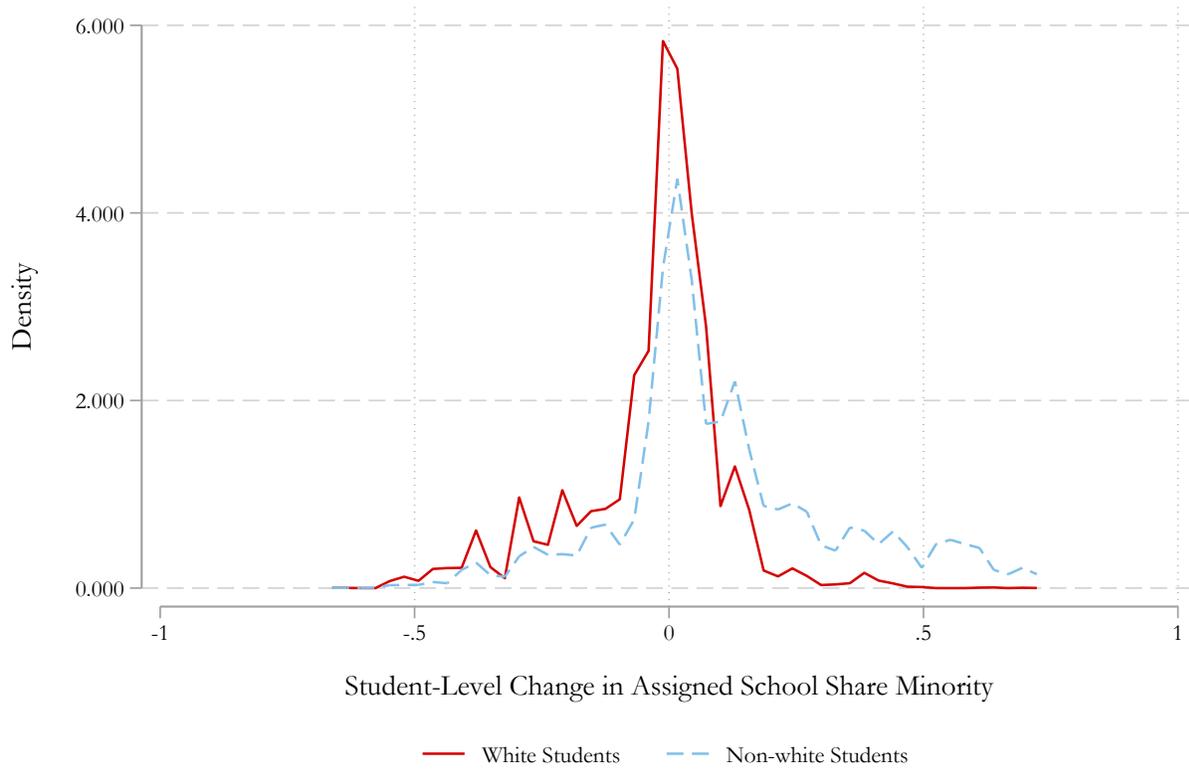
A Appendix Figures and Tables

Figure A1: Racial Attitudes and Party Affiliation



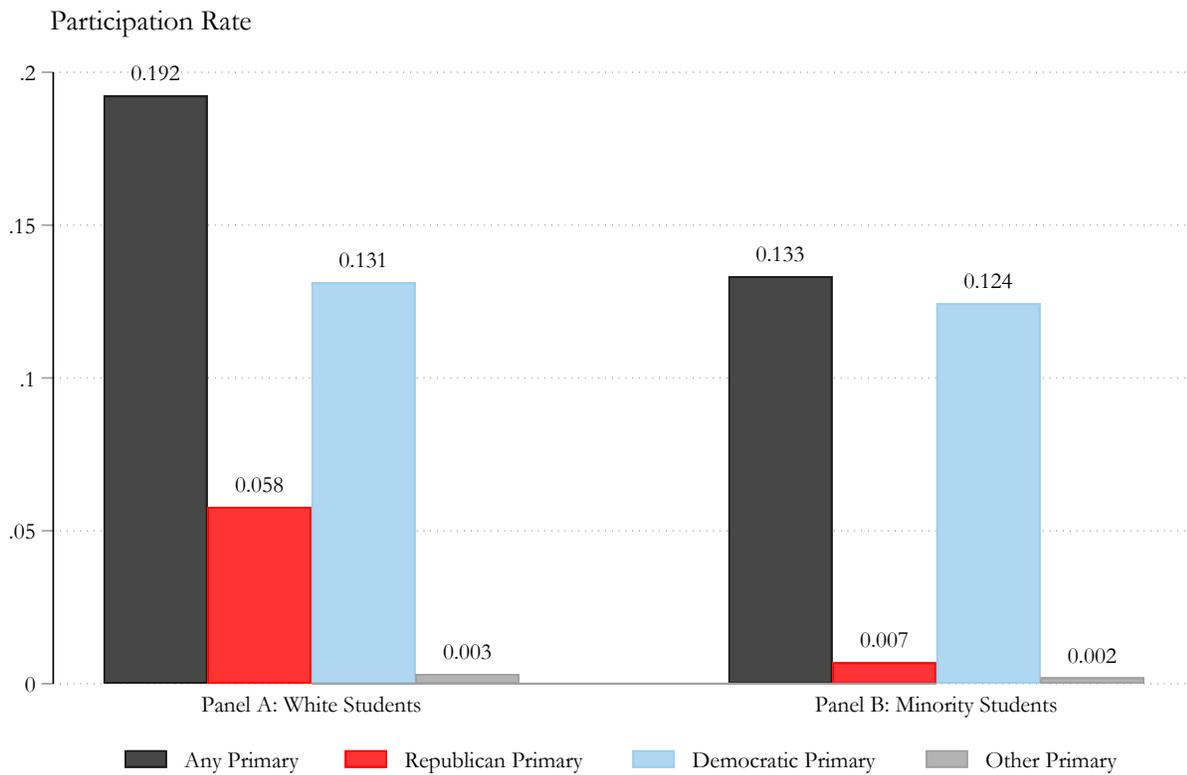
Notes: This table presents an analysis of survey questions from the Cooperative Congressional Election Study’s 2016 wave. In this survey, respondents are asked to report their *agreement* on a 1-5 scale with the statements (A) “I am angry racism exists.” and (B) “Whites have certain advantages because 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 assigned for the 2001-2002 academic year.

Figure A3: Participation in 2016 Primary Elections by Unaffiliated Voters



Notes: This figure presents an analysis of participation 2016 primary elections in North Carolina for Unaffiliated registrants in our main sample. Panels A and B present results separately by race. From left-to-right, each panel reports the likelihood of participating in any party primary (black), a Republican (red), a Democratic (blue) or an “other” (grey) party primary.

Table A1: Summary Statistics for Main Analysis Sample

	(1)	(2)	(3)	(4)
		School Percent Minority (2002)		
	All	< 0.47	0.47 – 0.69	> 0.69
White	0.43	0.63	0.42	0.23
Black	0.48	0.32	0.48	0.65
Hispanic	0.05	0.02	0.05	0.07
Other minority	0.04	0.03	0.05	0.05
Reassigned in 2003	0.47	0.41	0.47	0.54
Registered Voter	0.61	0.61	0.62	0.62
Registered Republican	0.08	0.12	0.08	0.04
Registered Democrat	0.33	0.26	0.33	0.40
Registered Unaffiliated	0.20	0.22	0.20	0.18
Voted, General Elections 2010-2018	0.51	0.51	0.51	0.50
N	35,757	12,439	11,540	11,778

Notes: This table reports descriptive statistics for the main analysis sample. Column 1 provides statistics for the entire sample, while Columns 2-4 report statistics based on the tertiles of the school-level percent of minorities in one's assigned school (2002-2003 academic year). 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. See Section B for further details.

Table A2: Assigned School Minority Share and Student Characteristics

	(1)	(2)	(3)	(4)
	Assigned Percent Minority			
Pre-policy, Avg. Math z -score	-0.000 (0.001)	-0.001 (0.001)	-0.000 (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. Suspensions	0.001 (0.002)	0.006 (0.004)	-0.000 (0.002)	0.003 (0.002)
=1 if Male	-0.001 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.001 (0.001)
=1 if Minority	0.003 (0.002)			
N	35,757	15,383	20,374	21,893
R^2	0.970	0.969	0.951	0.971
DepVarMean	0.621	0.449	0.751	0.630
Controls?	Yes	Yes	Yes	Yes
All Students?	Yes	No	No	No
White Students Only?	No	Yes	No	No
Minority Students Only?	No	No	Yes	No
Registered Voters Only?	No	No	No	Yes

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-level characteristics measured in the academic years observed before the end of school busing and school boundary rezoning (referred to as “pre-policy” measures). 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 A3: First-stage Effects of 2002 Rezoning on School Racial Composition

	(1)	(2)	(3)	(4)
	Actual Percent Minority			
Assigned Percent Minority	0.252*** (0.040)	0.303*** (0.044)	0.217*** (0.053)	0.252*** (0.053)
N	35,046	15,030	20,011	21,486
R^2	0.583	0.617	0.315	0.587
DepVarMean	0.578	0.424	0.694	0.585
Controls?	Yes	Yes	Yes	Yes
All Students?	Yes	No	No	No
White Students Only?	No	Yes	No	No
Minority Students Only?	No	No	Yes	No
Registered Voters Only?	No	No	No	Yes

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 pre-reform school zone by Census block group fixed effects, gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. 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 A4: Effects of Assigned School Out-Group Share on Party Affiliation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Registered as Republican			Registered as Democrat			Registered as Unaffiliated		
Panel A: White Students									
Assigned Percent Non-White	-0.183** (0.072)	-0.191*** (0.068)	-0.275** (0.127)	0.054 (0.048)	0.071 (0.048)	0.135 (0.092)	0.030 (0.074)	0.057 (0.074)	0.152 (0.103)
N	15,383	15,383	8,697	15,383	15,383	8,697	15,383	15,383	8,697
R ²	0.071	0.090	0.137	0.080	0.089	0.133	0.075	0.088	0.114
DepVarMean	0.164	0.164	0.287	0.136	0.136	0.237	0.264	0.264	0.461
Controls?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Registered Voters Only?	No	No	Yes	No	No	Yes	No	No	Yes
Panel B: Black Students									
Assigned Percent Non-Black	-0.008 (0.015)	-0.005 (0.015)	-0.004 (0.021)	0.073 (0.076)	0.066 (0.074)	0.135** (0.058)	-0.071* (0.042)	-0.085** (0.041)	-0.120** (0.055)
N	17,090	17,090	11,817	17,090	17,090	11,817	17,090	17,090	11,817
R ²	0.068	0.070	0.092	0.066	0.086	0.100	0.063	0.074	0.100
DepVarMean	0.014	0.014	0.021	0.532	0.532	0.766	0.145	0.145	0.208
Controls?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Registered Voters Only?	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table reports point estimates and standard errors from estimating a model where the dependent variable is a measure of party affiliation from voting records (e.g., an indicator variable taking on value one if the individual is registered with the Republican party, and zero otherwise). The key independent variable is the fraction of “out-group” peers in the school assigned to a student in the 2002-2003 academic year. Out-groups are specific to each student (e.g., the out-group for Black students are non-Black students). Note that to compare the results in Panel B of this table to that of Table 1, coefficients should be multiplied by -1 (i.e., “Percent Non-Black” is similar to (1 - “Percent Minority”)) – Panel A directly replicates that of Table 1. Columns 1, 4 and 8 reports results from a specification that controls only for pre-reform school zone by Census block group fixed effects. Columns 2, 5, and 8 reports results that additionally control for gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. Columns 3, 6, and 9 report results for the sample of registered voters. Standard errors are clustered at the pre-reform school zone by Census block group level.

Table A5: Comparing Effects of Assigned School Minority Share & Parent Party Affiliation

	(1)	(2)	(3)	(4)	(5)	(6)
	Matched Birth Rec.	Parent Registered	Parent Reg. Republican	Registered as Republican		
Assigned Percent Minority	-0.043 (0.116)	-0.052 (0.149)	0.004 (0.148)	-0.251** (0.108)		-0.253** (0.108)
Parent is a Registered Republican					0.165*** (0.012)	0.156*** (0.012)
N	15,383	7,155	7,155	7,155	7,155	7,155
R^2	0.297	0.306	0.180	0.121	0.165	0.152
DepVarMean	0.474	0.617	0.338	0.185	0.185	0.185
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
All Students?	No	No	No	No	No	No
White Students Only?	Yes	Yes	Yes	Yes	Yes	Yes
Minority Students Only?	No	No	No	No	No	No
Registered Voters Only?	No	No	No	No	No	No
Matched to Parents Only?	No	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results based on matching our main analysis sample to North Carolina birth records (1990-2001). Appendix Section B provides details on the birth record matching process. We link parents (observed in the birth records) to voting records using the same process that we used for children. 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 either of the child's parents are a registered voter or whether either of the child's parents are 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 pre-reform school zone by Census block group fixed effects, gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. 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.

Interpretation: Column 1 shows that we obtained birth record matches for 47.4 percent of white students 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 match 61.7 percent of students' parents to the Voter File, and again Column 2 shows that treatment does not significantly predict matching. In Column 3, we find that the policy did not affect parents' partisanship, suggesting that the policy's effects on students' later life partisanship are not mediated through parents. Finally, Column 4-5 provide parental transmission benchmarks. First, Column 4 re-estimates effects of the policy on this matched sub-sample, and shows 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 Column 5, we estimate that having a Republican parent is associated with a 16.5 percentage point increase in the likelihood of registering as a Republican in adulthood – relative to the mean of 18.5 percent, this is an 89 percent increase. Thus, the effect of a 10-percentage point increase in the percent minority in one's school is roughly 15 percent of the size of the intergenerational party transmission correlation estimated in the same sample (this is estimated by $(0.1 * .251) / .165$).

Table A6: Effects of Assigned School Minority Share by Grade Cohort at the Time of the CMS Reform

	(1) Registered as Republican	(2) Registered as Republican	(3) Registered as Democrat	(4) Registered as Democrat	(5) Registered as Unaffiliated	(6) Registered as Unaffiliated
Panel A: White Students						
Assigned Percent Minority	-0.138*** (0.048)	-0.169*** (0.059)	0.018 (0.049)	0.015 (0.055)	0.030 (0.056)	-0.032 (0.068)
Assigned Percent Minority X Middle School Cohort		0.010 (0.042)		0.006 (0.038)		0.142*** (0.048)
Assigned Percent Minority X High School Cohort		0.068 (0.057)		0.004 (0.053)		0.049 (0.060)
N	26,457	26,457	26,457	26,457	26,457	26,457
R ²	0.069	0.069	0.061	0.061	0.071	0.071
DepVarMean	0.160	0.160	0.128	0.128	0.235	0.235
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Minority Students						
Assigned Percent Minority	-0.001 (0.012)	0.003 (0.017)	-0.004 (0.055)	0.007 (0.065)	0.091*** (0.031)	0.102** (0.042)
Assigned Percent Minority X Middle School Cohort		0.003 (0.013)		-0.017 (0.041)		0.014 (0.031)
Assigned Percent Minority X High School Cohort		-0.009 (0.017)		-0.013 (0.060)		-0.030 (0.043)
N	31,925	31,925	31,925	31,925	31,925	31,925
R ²	0.050	0.050	0.087	0.087	0.059	0.059
DepVarMean	0.016	0.016	0.451	0.451	0.136	0.136
Controls?	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample for this analysis are all students enrolled in elementary (grades 3-5), middle (grades 6-8) and high schools (grades 9-11) in the year before CMS reforms. The table reports point estimates and standard errors from estimating an augmented version of Equation 1 where we include an interaction term for whether a student was in high school and the fraction of minority peers in one's assigned school and an interaction for whether a student was in middle school and the fraction of minority peers in one's assigned school. All results control for pre-reform school zone by Census block group fixed effects, gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. Note that we do not have data on pre-reform test scores for high-school students so we set their pre-reform test score controls to zero and include missing test score indicators in the specification. 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 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 student's 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 2010-2018 general and primary elections (as well as local elections which we do not use). 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-proxied party affiliation variable.³² 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.³³ 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. We also drop 1 percent of the remaining students in our sample who are the only individuals in our sample that live a given pre-reform school zone by Census block group combination. This is because these students will be omitted from our regressions since the neighborhood fixed effects that we use will not be identified. The final sample that we study contains 35,757 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. As mentioned in the main text, we can compare this statistic to Census statistics on voter registration. In 2019, the average child in the sample is about 29 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 expect that some CMS children move out of North Carolina later

³²The 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 measures party affiliation by the most recent primary in which a voter cast a partisan ballot.

³³For 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.

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.

A Using Birth Records To Study Parental Impact on Party Affiliation

To provide a sense of the magnitude of our findings, Appendix Table A5 studies 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 this intergenerational correlation in self-reported partisan affiliation is a useful guide, to provide a more directly comparable estimate to our treatment effects (i.e. using the same empirical specification, outcome measures, and sample) we estimate this parent-child party registration association in the voter file.

This supplemental analysis is based on linking our main analysis sample of 35,757 students to birth records from 1990 to 2001 obtained from the North Carolina State Center for Health Statistics. Importantly, the years covered by the birth records will *not* cover all students contained in our main analysis sample. Specifically, 38 percent of our sample are born before 1990 (the first year of the birth records). To address this limitation, one approach is to obtain parent information for children born before 1990 by identifying their later-born siblings in the birth records. As detailed below, we pursue this approach and show that our analysis of parental impacts is robust to excluding children matched through sibling links.

We link the final sample to birth records in two steps. 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. First, we use this information to link children based on their name and date of birth information. We matched 13,601 students using this approach. In total, there are 22,365 children in our sample born after 1990, which is the first year in the birth records. This implies that we obtain a match rate of 61 percent for all children born after 1990. 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. We do this to obtain information on parents for children who were born *before* 1990. Note that these matches rely on younger siblings (born 1990-2001 in North Carolina) who share the same last name and home address. We matched 2,974 students using this approach, which implies

that we obtain a match rate of 22 percent for all children born before 1990.³⁴ Overall, we linked 16,575 students (46.35 percent) in our main sample to their parents as recorded in birth records. The low match rate is largely due to the fact that, as mentioned above, a large fraction of our sample is born before 1990, the first year of the birth records.

³⁴Note that the results of our analysis of the intergenerational transmission of political preferences in Appendix Table A5 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?
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
Race African American/Black American Indian/Alaska Native Asian Multiracial Native Hawaiian/Pacific Islander White Other
Ethnicity Not Hispanic/Latino Hispanic/Latino

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 Assigned School & Parent Party Affiliation, Robustness (Alternative Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Matched Birth Rec. M1	Parent Registered	Parent Reg. Republican	Registered as Republican		
Assigned Percent Minority	0.035 (0.055)	-0.126 (0.143)	0.019 (0.165)	-0.232* (0.119)		-0.235** (0.118)
Parent is a Registered Republican					0.171*** (0.012)	0.171*** (0.012)
N	26,457	5,774	5,774	5,774	5,774	5,774
R^2	0.526	0.213	0.153	0.139	0.178	0.178
DepVarMean	0.220	0.751	0.414	0.189	0.189	0.189
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
All Students?	No	No	No	No	No	No
White Students Only?	Yes	Yes	Yes	Yes	Yes	Yes
Minority Students Only?	No	No	No	No	No	No
Registered Voters Only?	No	No	No	No	No	No
Matched to Parents Only?	No	Yes	Yes	Yes	Yes	Yes

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 either of the child’s parents are a registered voter or whether either of the child’s parents are 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 pre-reform school zone by Census block group fixed effects, gender, cohort fixed effects, and pre-reform mean absences, mean suspensions, and second order polynomials in mean math and reading test scores. Standard errors are clustered at the pre-reform school zone by Census block group level.