

## The Long-Run Effects of School Racial Diversity on Political Identity<sup>†</sup>

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*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. (JEL D72, H75, I21, I28, J15)*

Political partisanship shapes the way individuals see and interact with the world. In the United States, 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, Huber, and Washington 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, Palmquist, and Schickler 2002; Jennings, Stoker, and Bowers 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

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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 US 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 online 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, Deming, and Rockoff 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 (that is, 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 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).<sup>1</sup> 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

<sup>1</sup>The omitted category in this specification is composed of students who registered as Democrat, registered as unaffiliated, registered as one of three other officially recognized parties, or remain unregistered.

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.<sup>2</sup> Our results are consistent with this prediction.<sup>3</sup> 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 for the Democratic Party is a well-understood norm for Black Americans, the predominant minority group in our setting (White and Laird 2020).<sup>4</sup> 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 find only weak and indirect evidence in line with this prediction.<sup>5</sup>

While intergroup contact is a leading mechanism in our context, two additional potential mediators have been documented in this setting. Specifically, Billings, Deming, and Rockoff (2014) found that changes in school racial composition due to CMS rezoning 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 (for example, 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, Milstein, 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

<sup>2</sup>Since 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.

<sup>3</sup>Notably, 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, Kaufman, and Sands (2019); Condra and Linardi (2019); and 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, Wagner, and Zweimüller 2017; Dustmann, Vasiljeva, and Damm 2019; Tabellini 2020), though others have found more nuanced results (Mayda, Peri, and Steingress 2018; Steinmayr forthcoming).

<sup>4</sup>In our sample, 84 percent of minorities are Black.

<sup>5</sup>Section III discusses the interpretation of our results in more detail.

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 experiment in Indian schools and found that increased contact across economic status generates short-run increases in tolerance and out-group friendship. In the United States, Carrell, Hoekstra, and West (2019) find 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. Mousa (2020) and Lowe (2020) also provide experimental evidence of positive impacts of religious- 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.<sup>6</sup>

Last, we contribute to an important literature studying the effects of segregation and school integration policies. Several prior studies have estimated effects on academic and social outcomes (Guryan 2004; Ashenfelter, Collins, and Yoon 2006; Reber 2010; Johnson 2011; Lutz 2011; Billings, Deming, and Rockoff 2014; Gordon and Reber 2018; Shen 2018; Tuttle 2019; Bergman 2018). 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 noncontiguous areas with different racial compositions. School officials aimed to keep each school's percentage of Black students 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 (*Capacchione v. Charlotte-Mecklenburg Schools*). 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 US census geographies to avoid assignment based explicitly on race. For example, the new boundaries often cut through census block groups. While CMS

<sup>6</sup>A 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).

set neighborhood schools as the default, families could also apply to attend other CMS schools.<sup>7</sup>

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, Deming, and Rockoff (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 (CMS 2020). The data include information on student demographics, academic outcomes (that is, state test scores in grades three through eight, absences, and suspensions), and home addresses 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 online Appendix B provides detailed discussion of the data construction.

The analysis sample is restricted to CMS students in grades three through eight who were enrolled in the 2001–2002 academic year (the period immediately before the change in school zones and the end of busing).<sup>8</sup> 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 received a longer period of “treatment” (that is, the change in school racial composition) as they had 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, Baron, and Banaji 2008).<sup>9</sup> We also require that all students have name and address information—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

<sup>7</sup> Enrollment was subject to capacity constraints.

<sup>8</sup> Our sample definition differs from Billings, Deming, and Rockoff (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.

<sup>9</sup> Our main results are robust, though smaller in magnitude, when including high school students (online Appendix Table A7).

assigned schools and neighborhoods are not affected by the possibility that some families may respond to the boundary redrawing by moving to new neighborhoods.<sup>10</sup>

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.<sup>11</sup> As detailed in online Appendix B, we matched 61 percent of students to a voting record.<sup>12</sup> 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 (US Census Bureau 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.<sup>13</sup> Registered voters may choose one of five recognized political parties or elect to be unaffiliated.<sup>14</sup> 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. Online 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), and 61 percent are registered. Republican, Democrat, and unaffiliated registrations are 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 (for example, the 2016 Cooperative Election Study survey reports shares of 26, 39, and 33 in national data and shares of 27, 25, and 46 in North Carolina; Ansolabehere and Schaffner 2017).

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 twenty-fifth largest school district, behind the Memphis City School District and larger than Fulton County (Atlanta) (Common Core of Data 2000). In terms of race, the non-White share for our sample is comparable to racial enrollment statistics for other southern school districts.

<sup>10</sup>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.

<sup>11</sup>Only birth year (not day or month) is available in the North Carolina voting records.

<sup>12</sup>Note that we match about 2 percent of the sample to voting records in Virginia or South Carolina.

<sup>13</sup>The 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).

<sup>14</sup>Online Appendix Figure B1 reproduces the North Carolina voter registration form.

## 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 I, 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.<sup>15</sup> 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.<sup>16</sup> 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, Deming, and Rockoff (2014) by using the following neighborhood fixed effects specification:

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

where  $y_{izj}$  is an outcome (for example, 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 minority ( $PctMinority_{izj}$ ), a set of covariates  $X'_{izj}$  measured prior to CMS reforms to improve precision, and an old school-zone-by-neighborhood fixed effect ( $\eta_{zj}$ ). 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 I, we define neighborhoods based on block groups using the earliest pre-reform address in CMS records.<sup>17</sup> 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  $\beta_0$ , 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 lived 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  $\beta_0$ . Since we focus on narrowly defined neighborhoods—that is, block groups—our design is similar to the boundary discontinuity approaches utilized in Black (1999) and Bayer, Ferreira, and McMillan (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

<sup>15</sup> Billings, Deming, and Rockoff (2014) provide detailed statistics on school composition.

<sup>16</sup> Online Appendix Figure A2 shows the distribution of student-level changes in the percent minority students between new and old assigned schools.

<sup>17</sup> This assignment approach follows Billings, Deming, and Rockoff (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).

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: online Appendix Table A2 shows that students are balanced on pre-reform characteristics across the newly formed boundaries. Second, in terms of a “first stage,” online 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 rezoning).<sup>18</sup> We find a statistically significant coefficient of 0.25, in line with Billings, Deming, and Rockoff (2014). While there is a strong first stage, there are two reasons why 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 ones 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 and 2, 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  $< 0.05$ ).<sup>19</sup> 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  $< 0.01$ ). Online Appendix Table A4 reports results conditional on being registered to vote (a post-treatment variable) and shows a similar 2.75 percentage point reduction (a 10 percent decrease relative to the Republican registration rate in this subsample).<sup>20</sup> Columns 3–6 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 4) and as unaffiliated (0.57 percentage points in column 6). While neither estimate is statistically significant, they are directionally consistent with weakening Republican partisanship.<sup>21</sup>

<sup>18</sup>We do not estimate instrumental variable specifications for reasons further expounded upon in Billings, Deming, and Rockoff (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).

<sup>19</sup>We do not find detectable heterogeneous treatment effects by gender.

<sup>20</sup>Angrist and Pischke (2009), Nyhan, Skovron, and Titiunik (2017), and Montgomery, Nyhan, and Torres (2018) discuss the bias induced by conditioning on post-treatment outcomes.

<sup>21</sup>To provide a sense of magnitude, online Appendix Table A6 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



TABLE 1—EFFECTS OF ASSIGNED SCHOOL MINORITY SHARE ON PARTY AFFILIATION

	Registered as Republican		Registered as Democrat		Registered as unaffiliated	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. White students</i>						
Assigned percent minority	−0.183 (0.072)	−0.191 (0.068)	0.054 (0.048)	0.071 (0.048)	0.030 (0.074)	0.057 (0.074)
Observations	15,383	15,383	15,383	15,383	15,383	15,383
$R^2$	0.071	0.090	0.080	0.089	0.075	0.088
Dependent variable mean	0.164	0.164	0.136	0.136	0.264	0.264
Controls	No	Yes	No	Yes	No	Yes
<i>Panel B. Minority students</i>						
Assigned percent minority	0.003 (0.020)	0.002 (0.020)	−0.027 (0.084)	−0.028 (0.082)	0.089 (0.048)	0.106 (0.047)
Observations	20,374	20,374	20,374	20,374	20,374	20,374
$R^2$	0.066	0.068	0.077	0.098	0.064	0.075
Dependent variable mean	0.016	0.016	0.476	0.476	0.149	0.149
Controls	No	Yes	No	Yes	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 one of the other three officially recognized parties. Online Appendix Figure B1 reproduces the North Carolina voter registration form. Columns 1, 3, and 5 report results from a specification that controls only for pre-reform school zone by census block group fixed effects. Columns 2, 4, and 6 report 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. Standard errors are clustered at the pre-reform school zone by census block group level.

Panel B of Table 1 reports results for minority students.<sup>22</sup> Here, we find no statistically significant impacts on the likelihood of registering with either of the two major US political parties. The point estimates in columns 2 and 4 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 online Appendix Figure A3.<sup>23</sup> Both White and minority unaffiliated voters are much more likely to participate in

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.

<sup>22</sup> Online Appendix Table A5 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).

<sup>23</sup> North Carolina has semiclosed 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.

TABLE 2—EFFECTS OF ASSIGNED SCHOOL MINORITY SHARE ON REGISTRATION AND POLITICAL PARTICIPATION

	Registered voter (1)	Vote ever (2)	Vote 2010 (3)	Vote 2012 (4)	Vote 2014 (5)	Vote 2016 (6)	Vote 2018 (7)
<i>Panel A. White students</i>							
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)
Observations	15,383	15,383	15,383	15,383	15,383	15,383	15,383
R <sup>2</sup>	0.111	0.117	0.086	0.103	0.081	0.097	0.093
Dependent variable mean	0.573	0.472	0.070	0.333	0.105	0.308	0.206
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Panel B. Minority students</i>							
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)
Observations	20,374	20,374	20,374	20,374	20,374	20,374	20,374
R <sup>2</sup>	0.091	0.106	0.082	0.099	0.084	0.106	0.104
Dependent variable mean	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.

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.<sup>24</sup>

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.<sup>25</sup> 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, estimates are insignificant across each specific election. For minorities, we find an insignificant 0.45 percentage point increase in ever voting.

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

<sup>25</sup> Beyond 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 to those of the registered sample (i.e., 29, 24, and 48 percent as Republican, Democrat, and Unaffiliated, respectively) suggests that demobilization explains only 10 percent of the effect (i.e.,  $(0.29 \times 0.67)/1.91$ ).

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 (for example, a suggestive 3.8 percent decrease for White students in the 2016 election) and the effects on party affiliation are large (for example, 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.<sup>26</sup> Meta-analyses of hundreds of studies largely confirm this prediction (Pettigrew and Tropp 2006; Paluck, Green, and Green 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).<sup>27</sup> Moreover, the link 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.<sup>28</sup> 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, while there is an increase in unaffiliated registration

<sup>26</sup>The 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.

<sup>27</sup>In 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).

<sup>28</sup>The social identity and norms lens could also be applied for White students (Green, Palmquist, and Schickler 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.

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.<sup>29</sup>

Aside from the channel of intergroup contact, it is possible that effects are driven by other outcomes that may mediate political behavior. Billings, Deming, and Rockoff (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, Junn, and Stehlik-Barry 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, McCabe, and Thal 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 cover outcomes only up to the years 2009 and 2013, respectively. This implies that we can consistently study the nonpolitical outcomes only for middle school (grades 6–8) and high school (grades 9–12) students, as we are forced to drop elementary-age students who have missing data on long-run college enrollment.<sup>30</sup>

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, Deming, and Rockoff (2014) suggest that the effects of school diversity on nonpolitical 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.

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

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

<sup>30</sup> For 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.

TABLE 3—EFFECTS OF ASSIGNED SCHOOL MINORITY SHARE FOR MIDDLE AND HIGH SCHOOL STUDENTS

	Party registration			Participation		Crime and education		
	Republican (1)	Democrat (2)	Unaffiliated (3)	Registered voter (4)	Vote ever (5)	Ever arrested (6)	HS graduate (7)	Attend college (8)
<i>Panel A. White students</i>								
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 × 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)
Observations	18,684	18,684	18,684	18,684	18,684	18,684	18,684	18,684
R <sup>2</sup>	0.069	0.055	0.068	0.103	0.101	0.145	0.444	0.336
Dependent variable mean	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
<i>Panel B. Minority students</i>								
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 × 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)
Observations	21,331	21,331	21,331	21,331	21,331	21,331	21,331	21,331
R <sup>2</sup>	0.045	0.088	0.051	0.082	0.090	0.239	0.388	0.259
Dependent variable mean	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 subsample 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.

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 arrest 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, Deming, and Rockoff (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

treatment in terms of changes in school average parental incomes.<sup>31</sup> 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, Hoekstra, and West 2019; Rao 2019; Lowe 2020; 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, Wagner, and Zweimüller 2017; Dustmann, Vasiljeva, and Damm 2019; Calderon, Fouka, and Tabellini 2020; 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.

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<sup>31</sup> Specifically, 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, Deming, and Rockoff (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, and 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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